To be a molecular scientist

The negotiation of epistemic and social virtues in 1970s *Nature*'s marketplace

Max Bautista Perpinyà, 2020

Master thesis History and Philosophy of Science

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“Meanwhile, with this programme, as with all programmes, you receive images and images which are arranged. I hope you will consider what I arrange. But be sceptical of it.”

John Berger, Ways of Seeing (1972)

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“What happened to your enlightenment?”

“I don’t know. It wore off.”

Mad Men (2012)

Acknowledgements.

I imagined historical work to be quite lonely, sited in between books, *Nature* volumes and jugs of fresh coffee. Well, it was like that. But I couldn't have done it alone. Throughout this year, many people have come close to my project, we've chatted over what science is, what history should be, they've read my thoughts, we've reorganised them, provided me with images, texts, food —and I would like to thank them.

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As second person to be thanked is someone who not only thought alongside of me and helped me bring my project forward, but that, we've come to realize, are both in the business of white-collar activism. When I first wrote this paragraph, I mentioned that I was happy to have similar historical interests yet politically different positions. Today, I am not so sure of the latter. Chatting with you about it all (existential history, contextualized ethics, lazy historians, mad biologists, individuals and collectives) sparked in me the will to write something worth reading. Knowing your project so closely also made me admire how beautifully complex can history be. I want to thank you for many things, but in the matter at hand, because you are the person who has read the most chapter drafts of this thesis, and always did it so very closely, attentive, critically, and generously. At some point you got used to my writing style (an "acquired taste", you once wrote), which encouraged me to continue writing the way I like to do. For their benefit, people better read me more than once. Yourself, please be patient with my meandering. I promise to get straight to the point next time. Thank you, Martijn van der Meer.

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Thank you to my parents. My dad, because you provided with the image that catapulted me into writing this thesis: your London Fridays reading the latest *Nature* issue, browsing calmly through the Bible-thin pages, lounging on the lab armchair. I haven't given justice in my writing to the peace of this by-gone era of science, the one of thought and reflexion, but I've carried this image along, both as a historical mirage and future desire. My mum, because you've breathed in me the spirit of criticism and given me the examples of why and how words can be so rich. The books in your shelf not only gave me joy, but became the structure to understand the pea in my sleep.

Introduction. On why and how studying *Nature*, epistemic virtues, advertisements, and politics to tell a story about molecular science.

I. The journal as a forum: more than a bound collection of printed facts

The first page of *Nature* of March 22, 1974 echoed the words of a televised interview with a renowned British mathematician. The opening lines read,

> Dr J. Bronowski was talking about job satisfaction. "Scientists and prostitutes", he said, "have one thing in common—they both get paid for doing something they enjoy." Many would question the enjoyment that Bronowski attributes to prostitutes but surely scientists enjoy themselves.[1]

The lead editorial entitled 'Science and Public Pleasure' dealt less on the comparison between sex-workers and science-workers than the first quote seemed to promise. There wasn't any more sex in Bronowski's interview nor in the piece of the scientific magazine – the reference to prostitution was a provocative comparison, a visual stimulation serving as appetizer for a normative commentary on the topic of scientific employment and the relevance of research within society. 'Pleasure' was here not about personal satisfaction, it referred to social recognition, recognition within peers at a "learned journal", in a "paper in *Nature*," as "a review article in *Scientific American*", or as the ultimate public recognition in the mass media. 'Pleasure' and 'recognition' were not just married together, but 'pleasure' became a scientific duty:

> Pleasing as it is to see oneself in print or on television is it not a trifle vulgar, and should the media not be kept at a discreet distance? Surely not. The general public can certainly take delight in things it does not understand. There are many levels on which one can enjoy a performance of *Hamlet* and there are similarly many levels at which a scientific accomplishment can be savoured. Further, the scientist who refuses to have anything to do with the media is letting in the professional commentator, already at one remove from the field. Misrepresentation is then not the commentator's fault but the scientist's.[2]

Lead editorials such as this one were powerful pages in *Nature* during the 1970s. They were the opening section of arguably the most influential journal at the time. These one-

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[1] 'Science and Public Pleasure', *Nature* 248, no. 5446 (22 March 1974): 269–269, https://doi.org/10.1038/248269a0.

[2] Ibid.

page pieces were typically written by the lead editor, although appeared virtually always unsigned. With titles such as 'Science and Public Pleasure', 'From Russia with hope' or 'Twenty-one years of the double helix', lead editorials told readers about the situation of science within history and within present society. With the characteristic British wit, they not only *told*, but were the space were the editor preached his view about where science stood and where it *should* stand in the future.

About a third of every hundred-or-so-page issue of the magazine was not covered by experimental pieces. Bound together, the sections Review Articles, Articles, and Letters to the Editor (later called Letters to *Nature*) where scientists reported on their empirical findings, coexisted with lead editorials like the one above, and all sorts of columns and sections such as News and Views, Correspondence, Book reviews, Obituaries, Forthcoming events, Appointments Vacant, University News, Appointments, Announcements, International Meetings, British Diary, Reports and other Publications, Person to Person, and Newly on the Market, all indexed on a first-page table of contents.[3] Some of these sections are still alive and well today and have been a central part of *Nature* since its inception in 1869 and throughout its history. The magazine was and isn't only a place to present and read about the latest experimental findings, but a place to read and discuss the political sitting of science. From the Aims & Scope section in its web as of October 2020:

> *Nature* is a weekly international journal publishing the finest peer reviewed research in all fields of science and technology on the basis of its originality, importance, interdisciplinary interest, timeliness, accessibility, elegance and surprising conclusions. *Nature* also provides rapid, authoritative, insightful and arresting news and interpretation of topical and coming trends affecting science, scientists and the wider public.[4]

Only recently, historians have started to take these sections seriously, by investigating the debates that take place within them and by attending to the material histories behind the making of those cellulose pages. Melinda Baldwin published in 2015 *Making 'Nature': The History of a Scientific Journal*, where she delivered the history, not only of the journal, but of the scientific community that contributed to its pages. Because, as

[3] In order of appearance, the content of these section whose name is not self-explanatory: 'Review Articles', a summary of experimental work of a heavily research arena; 'Articles', typically two lengthy articles based on experimental original work; 'Letters to Nature', short technical pieces; 'Announcements', including awards, university appointments and other miscellaneous worthy happenings; and 'Person to Person', personal messages offering and seeking local scientific and technical help in diverse projects).

[4] 'About the Journal | Nature', accessed 5 October 2020, https://www.nature.com/nature/about.

we shall see, it wasn't only the staff which authored editorial columns, but also external contributors who did not only adopt a passive position as readers but actively self-reflected in their writings on their condition as scientists.[5]

Scientific journals, and *Nature* in particular, are not only a repository of scientific facts and experimental cookbooks. They contain a rich perspective on the social issues that trouble scientists, and as such are fertile sources for historians to understand how the scientific process is placed socially, politically, and economically. Quite literally, reports of scientific experiments were placed in a specific order – before, after, or in between opinion columns – and with a certain relation of typefaces, line spacing, and referencing style. Even now, when academic publishing has largely moved online, the structure of journal's website *matters*. Baldwin's work is an invaluable open door to begin dealing with the journal as a whole, in its temporal and physical dynamism.

However, something is still missing from my first probe into the structure and meta-content of the scientific journal *Nature*. In fact, it misses both from the voices of the historical actors themselves and from the analysis of historians of science who have investigated the material culture of scientific publishing. There is no mention in Baldwin's *Making 'Nature'* nor in the other monograph on the history of scientific periodicals, Alex Csiszar's *The Scientific Journal. Authorship and the Politics of Knowledge in the Nineteenth Century*.[6] And so, what misses from both the actors' and analysts' discourse on the structure of scientific journals, and of *Nature* in particular? In my enumeration of all the sections that one can find on the table of contents of each issue, and from the description of *Nature* on its website, we find no evidence of one specific element which, despite its hidden indexation, is ubiquitous throughout time, space, and format. Since the first printed issue of *Nature* till now in its website, from the front to the back cover of the magazine, there is something that is largely left behind: we find advertisements. Their content, placement, and production have been for the most part been ignored by historians and sociologists of science. Put bluntly, it seems as if those studying the process of science have been running their analytic browsers with AdBlock Plus. To my knowledge, no study (in history of science or else) has taken the step to investigate the uses and meanings of scientific advertisements as commercial objects targeting the scientific community.[7] As frivolous a topic as this may seem, I argue in this thesis that scientific advertisements deserve historical attention, and that their content and form are as interesting and fertile for analysis as the debates that take place

[5] Melina Baldwin, *Making 'Nature': The History of a Scientific Journal* (University of Chicago Press, 2015).

[6] Alex Csiszar, *The Scientific Journal. Authorship and the Politics of Knowledge in the Nineteenth Century* (Chicago and London: The University of Chicago Press, 2018).

[7] I will attend to the few studies that treat scientific advertisements in their work in Chapter Four.

Figure 1. Two advertisements in *Nature*, both drawing attention to the reader through different methods: by a sociopolitical 'joke' (left, Biopsies advertisement for "Current Literature Alternating Search Service") and by the force of the figure of Charles Darwin (right, Wang advertisement for Wang 2200 mini-computer)

in the lead editorials, News and Views or the Correspondence. Let us take a look at one of them.

On the same issue as the 'Science and Public Pleasure' lead editorial above, an advertisement at the back cover showed a photograph of a woman facing the camera and holding a piece of paper. The largest font in the ad read: "We want to make you 'C.L.A.S.S.' conscious" (figure 1, left). Below it, the reader would find an explanation of the marketed deal. Those expecting a commercial advertisement for awareness of social inequality may have been disappointed. "C.L.A.S.S." stood for "Current Literature Alternating Search Service", and the full-page advert announced a service of scientific informational indexing that bi-monthly would deliver a "computer printout consisting of pertinent bibliographic information" on the areas of interest that the customer would request.[8] Well done, marketing department, you caught our attention.

You are a scientist in the mid-1970s. If socioeconomically themed jokes didn't do it for you, perhaps heroes would call your attention. Wang Electronics knew perfectly well what sold best to a scientist, and that is — another scientist. And who better than Darwin to sell the Wang 2200? In this recurrent advertisement (figure 1, right), Darwin's

[8] Biosis, 'We want to make you "C.L.A.S.S." conscious', *Nature* 248, no. 5446 (22 March 1974): Cover 3.

picture is placed in the top centre and occupies half of the one-page ad. Below, we read a mocking yet revealing comparison between biological creatures and technological objects: both are under enormous environmental pressure and are placed in situations "where obsolescence comes fast and costs dear."[9] Using evolutionary language, the 2200 was marketed as "adaptable and fit enough to survive changing needs and varying demands." In an unashamedly ahistorical way, Wang's ad closes effectively: "Darwin would have loved it."

What does Darwin selling a computer and a British mathematician talking about prostitutes have in common? While this may appear as a joke, this is the central question of my thesis, an exploratory question, if you will. I wondered while reading *Nature* – while holding the heavy issues bound together and flipping through its pages– about what it was like to see advertisements for molecules and reactives – ligases and kinases, synthesases, purifiers and acids, mixers and cell counters, lenses, tumblers and storage cabinets, free catalogues and minus 80ºC freezers, all delivered to your lab door, fast, and importantly, all these objects standardised, ease to operate, and promising to make your life easier. But specially, what was it like for the scientist to see these commercial breaks, not in the context of ground-breaking experiments, but surrounded by political pieces describing, seeking and constructing a public image of the scientific class, news about a decrease in public funds for research and a zealous governmental gaze looking to assess the return on its investment, and a push from all sides for making *your* science 'socially responsible'?

As the reader may have noticed, all the research objects I mentioned above are handled by a specific kind of operator: the molecular scientist. First of all, I'd like to point out to a choice in my terminology. I have decided to use throughout this thesis the formulation 'molecular scientist', and not 'molecular biologist' as was (and is) normally referred to researchers who work with molecular objects. I have chosen this specific wording for two reasons: first, because molecular work since the 1950s was done not only by those trained in the life sciences, and included a great deal of chemists and physicists;[10] and second, I chose this seemingly uncommon formulation to illustrate how 'molecular scientists' is my analytic term which introduces distance with the actor's category of 'molecular biologist'.

I have an interest in describing the experience of this group of researchers, and especially in the context of the 1970s and with *Nature* as the site of my study for several reasons. Since the late 1960s, the tools necessary for the manipulation of molecular components, and especially those for the cutting, copying, and pasting of genes became increasingly available. The possibility to alter the genetic components of virtually any

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[9] Wang Electronics, 'The Natural Selection', *Nature* 258, no. 5531 (13 November 1975): iv.

[10] Dominic J. Berry, 'Making DNA and Its Becoming an Experimental Commodity', *History and Technology* 35, no. 4 (2 October 2019): 374–404, https://doi.org/10.1080/07341512.2019.1694125.

living organisms by the use of these molecular tools quickly translated into the commercial availability of standardised products like the ones I described above. The ability to engineer (almost) at will the genetic make-up of the scientists' model organism by the creation of recombinant DNA molecules, with the objective to observe downstream cellular, physiological, and systemic effects intended to design therapeutic molecular targets, as well as being able to create a profitable biomedical industry around these tools made the 1970s a "recombinant gold rush."[11] Competition between laboratories around the globe, especially between those in the United Kingdom and in the United States of America, surfaced. *Nature* and *Science* became the places where competition was spelled out, both in the form of scientific articles and political pieces. These magazines became household names for molecular scientists that wanted to succeed during this decade. *Cell*, launched in 1974 as "A Journal of Exciting Biology" appeared in response to the increasing specialisation of these molecular fields and for the demand for extra space aside the well-wanted *Nature* and *Science*.[12] Already in the 1980s, scientists looked back with nostalgia to the past decade, describing the molecular community of the 1970s as an "esoteric, high-spirited, driven fraternity that, by good fortune was coming into maturity."[13]

As forums of the community, *Nature* and *Science* became, paradoxically, the emblems of national superiority in a vision of universal science. In *Nature* and in the UK, where this thesis is set, scientists talked about the situation of British science within their borders and across the Atlantic, both as what science could do for Britain and what the state was doing for science. During the early 1970s, there were intense political debates about the financial situation of science in the context of a dwindling British economy with increasing inflation and unemployment, and the impossibility to maintain the high levels of public investment of the decade prior. Besides the exciting and esoteric community built around biomolecules, the 1970s was also the decade were a regimen of accountability of the scientific profession was put into place, and the molecular scientists experienced this first-hand. One of the ways they responded to political pressures was to reassure that public investment in the sciences of the genes and proteins could return profit with the creation of a commercial industry based on its techniques. The molecular class positioned scientific work as a stronghold of the nation's economy.

This discussions happened within the journal, but let us not forget that the journal is not a static object. While the heavy, leather-bound volumes joining the spine

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[11] Stephen S. Hall, *Invisible Frontiers: The Race to Synthesize a Human Gene* (Tempus Books of Microsoft Press, 1988/2002), 39.

[12] Benjamin Lewin, 'A Journal of Exciting Biology', *Cell* 1, no. 1 (1 January 1974): 1, https://doi.org/10.1016/0092-8674(74)90147-0.

[13] Hall, *Invisible Frontiers*, 35.

of about eight issues accumulate dust in today's archives, the journal was a soft, paperback flexible magazine before scientific communication moved online. The flexibility was material, but also in its authorship, leadership, interests, colours, fonts, and margin spacing. Needless to say that in its dynamism, *Nature* didn't cease to be scientific, as it didn't stop being social, political, and economical.

This is a story about how scientific advertisements and editorial pieces constructed a *Nature* that was fluid and multifaceted. With the advertisements as snapshots and the editorial pieces as spelled-out political fights, my objective with this thesis is to tie the image to the text. I hope that by tying a knot I am able to narrate a story of scientists, their imagination, their molecules, their place in society, their promises, and their own selves.

II. Images and texts as illustrating epistemic virtues

The images of the advertisements seemed to say something about how experimentation was carried out, and what was the good way of doing so. At the very least, they seemed to say something about what a good machine was like. For the most part, this thesis has been an attempt of figuring out what advertisements mean, and what their relation to normative notions of science. In short, I have moved from a representationalist to a contextualised interpretation of advertisments: from saying that their content *represents* certain epistemic, moral, and social dimensions of science, to saying that in order *to be read like that*, a thick context is needed. I will elaborate on these issues (what images mean and how should historians understand them) in Chapter Four. In this section, I will do something more simple. I hope to introduce the reader to the historiographical positioning of my argument. Given the lacuna of historical and philosophical studies of advertisements *targeting* scientists, I have termed this group of visual objects simply 'scientific advertisements.'

Scientific advertisements stand at a relatively uninhabited yet fertile historiographical crossroads. In this section, I place myself within a rich theoretical and empirical background, which has inspired me to design ways by which to look at these visual objects. I draw mostly from two research programmes: from the work on the history of journals, and from that in historical virtue epistemology. These diverse approaches to the world of visual objects ('visual' understood here in a broad sense) allow me to, in all modesty, present my source material as fertile for the study of the relationship between media formats, scientific practice, the economies of research, and the politics of technology. I explore the ways by which historians of science can look at these objects. These ways of seeing ramify in a variety of research questions. At the centre of this gaze into scientific advertisements, I aim to answer: what are these objects, and what did they mean to the people holding them?

The recent work in the history of journals and scientific publishing serve as backbone for the understanding of the media in which my sources lay. Within this nascent field that promises to bring some historical awareness to current discussions on the problematics of scientific communications, two works are key. I have already introduced Baldwin's *Making 'Nature'*, and of the second one I have only mentioned its name. Alex Csiszar's *The Scientific Journal* is the first monograph on the history of the specialist scientific press. His contribution reveals the interlacing of the political and epistemic status of what came to be the epitome of scientific media.[14] Situated in Victorian Britain and post-revolutionary France, Csiszar displays a keen interest on how the new printed periodicals forced the academy to rethink its relationship with society.

Csiszar's book tells the story about how the worlds of academics and journal publishers were in fact, different worlds. With an emphasis on the Royal Academic Societies of France and the United Kingdom, he illustrates the historical contingency of scientific media by showing how the men of science of the early nineteenth century disdained publishing their science in short journal articles instead of the massive leather-bound treatises and transactions that they were used to, but how, by the end of the century, academic publishing had become synonymous with journal publishing, displacing books as the preferred media of the new profession.[15] Then, Csiszar goes a step further by showing that while the distance between journals and academics was large, it was even more so between *commercial* publishers and the Royal Societies. He traces a similar movement during the century as with journal publishing per se, through which publishing science in a for-profit press or becoming a paid author became increasingly (not without tensions) a more recognised and respected way of transmitting about one's scientific studies.[16]

In his chapter 'Meeting the Public', Csiszar narrates the "consequences" of the appearance of the commercial press at the beginning of the nineteenth century "on conceptions of legitimate scientific judgment."[17] He documents the rise of the commercial scientific journal as a whole, with the birth of the short *Proceedings* format in detriment of long pieces of the Societies' *Transaction*. While his chapter does not attend specifically to the commercial content *within* scientific journals as I try to do here with the scientific advertisement, his chapter illuminates the ways by which commercial ventures interact with public credibility of science *through* the particularities of media format.

The historiography of science does justice to this historical trend of the nineteenth century by which commercial scientific journals were settled as respectable

[14] Csiszar, *The Scientific Journal*.

[15] Csiszar, 'The Press and Academic Judgment', in *The Scientific Journal*, 23–66.

[16] Csiszar, 'Meeting in Public', in *The Scientific Journal*, 67–118.

[17] Ibid, 68.

and even sough-after venues for scientists to announce their discoveries. Baldwin's *Making 'Nature'* takes up the relay and illustrates the continuous transformation of the journal as venue of communication for the scientific community. The book illustrates how this commercial enterprise became a sort of rendezvous point, first as the "essential reading for British men of science" at the end of the nineteenth century, passing through the journal's popularity for physicists working in radioactivity at the turn of the twentieth, and by the 1950s, *Nature* having become the press of choice for scientists in the fields of geophysics and nucleic acid research. Through the 1950s and 60s, internationalisation was one of the objectives of the London press, and the journal became a venue of scientific discussion that extended beyond British borders. It was not only by widening its subscription base outside the United Kingdom that *Nature* established itself as an international forum, but by the constant increase of non-British contributors wishing to publish their articles.[18]

The success of *Nature* as the world's most prestigious science journal can be seen also as the success of the commercial press in the making of the scientific community. Baldwin rightly emphasizes how *Nature* was more than a bound collection of printed facts: "*Nature* is an important publication not only because of the famous papers printed in its pages but also because of its significance as a place where scientific practitioners have worked to define what science is and what it means to be a scientist."[19] *Making 'Nature'* is not only a history of the journal, but of the everchanging, heterogenous scientific community that contributed to its columns. Serving as a forum, *Nature* was the point where the community self-reflected. By looking within, the historian does not only get a view of the editorial line of *Nature*, but taps onto the self-identity of the liquid community that inked its paper pages.

I shall carry Baldwin's methodological approach in referring to this community. She says that "the journal itself had no agency, and that it is more useful to consider *Nature* more as a dynamic forum than a self-contained unit or an unproblematic representation of science's workforce."[20] As such, Baldwin takes an approach that I take to heart: "I have tried to avoid using '*Nature*' as the subject of a sentence—for example, '*Nature* argued this'—but when I do, it should be taken as shorthand for '*Nature*'s editors and contributors'."[21]

As Baldwin makes clear, studying *Nature* means attending to a question of identity. The identity of the forum can be probed by looking at pieces like lead editorials, the News and Views section, or the Correspondence columns; where contributors of all kinds (editors, scientists, policy makers, business owners) poured their opinions,

[18] Baldwin, *Making 'Nature'*, 15, 16, 151, 165.

[19] Ibid, 5.

[20] Ibid, 9.

[21] Ibid, 9.

suggestions, and political demands. As well, I attend in my thesis to this question of identify not only in the editorial pieces, but in the advertisements placed besides them. Neither in Baldwin's study of the collective self of *Nature* and neither on Csiszar's account of the politics of the scientific journal, we find any description or analysis of advertisements. As such, I aim at transposing the question of identity of Baldwin with Csiszar's interest in describing the political and "consequences" of media, using not just editorials, but the image of commercial research objects. By using the scientific advertisement, I wish to resuscitate this unindexed piece of literature.

Above all, my question is one of *identity*. This crucial aspect introduces my second gaze through which to study advertisement and editorials and my historiographical positioning. The question of identity – understood here as the identity of the fluid scientific community of *Nature* – that I pose in this thesis embraces the work in the field of historical virtue epistemology of the recent decades. I understand this approach to rely on the philosophical proposal that the epistemic and ethics of any practice (scientific practice, in this case) hinge on one another. Put in another way, that meditations on ways of knowing about the world are inseparable from a reflectivity about the one who knows, and that in short, epistemic worries and praises don't do so without moulding the self. Put bluntly, virtue epistemology describes what is it like to be a good knower; and the historical work in this field has illustrated the situatedness of this co-construction between knowledge making and knowledge makers.

The advantage of approaching history with such outlook is that it prepares the gaze by which to interpret your sources: reflections about what constitutes a good approach to knowing pivots on the condition of the knower and its placement in society. As I am interested in describing the self-identity of the community of *Nature* and the social worries that they reflected upon, taking a virtue epistemology framework allows me compare identities not only through the epistemic discussions, ethical discussions, and political discussions as *separate* conversations, but as fluid heterogeneous identities displaying different sides of their self.

Historians Lorraine Daston and Peter Galison have written extensibility and showed the potential of virtue epistemology. In *Objectivity*, they historicize one of the major epistemic preoccupations of modern science: objectivity.[22] Narrating the history of objectivity goes along with narrating the history of how the scientific self was constructed within a regimen of ethical imperatives. "To constrain the drawing hand to millimeter grids or to strain the eye to observe the blood vessels of one's own retina," Daston and Galison write, "was at once to practice objectivity and to exercise the scientific self."[23] In their quest to show how "ways of seeing" are intimately interwoven

[22] Lorraine Daston and Peter Galison, *Objectivity*, 1st ed. (Zone Books, 2007).

[23] Ibid, 38.

with ways of being, they suggest a powerful theoretical construct: 'epistemic virtues'.[24] This marriage of scientific practices with prescriptive obligations situates 'the self' as a central explanatory element in the field of historical virtue epistemology, and the authors argue, in the history of science. Quite literally, their chapter 'The scientific self' is found in the midst of *Objectivity*, and from this section, we can read how epistemics and ethics are interwoven at the level of the self:

> The term 'epistemic virtues,' with its ethical overtones, is warranted. Ethos was explicitly wedded to epistemology in the quest for truth or objectivity or accuracy. Far from eliminating the self in the pursuit of scientific knowledge, each of the epistemic virtues depended on the cultivation of certain character traits at the expense of others.[25]

Daston and Galison's analysis of the historicity of objectivity attempts to show that "there was nothing inevitable about the emergence of objectivity".[26] However, they are cautious in choosing *which* factors explain how objectivity was constructed. Their view is that historians cannot explain the rise of objectivity by invoking only technical and social developments alone, such as the invention of the photograph, or 'the period of revolutions' (the French, the Industrial, and the Second Scientific Revolutions). Instead, they look for a "superficial" answer, one precisely that requires the mirror image of objectivity: subjectivity. In rejecting the pejorative sense of 'superficiality', Daston and Galison advance the critics by saying that "superficiality is, in a certain sense, exactly the point," and they continue, "the kind of explanation we are after is indeed superficial, in the etymological sense of lying on the surface of things rather than hiding in conjectured depths."[27]

Objectivity is focused on the developments in western science from about the eighteenth to the beginning of the twentieth century and draws a grand sketch of epistemology and human action. To illustrate how the explanatory structure of 'epistemic virtues' functions, let me briefly illustrate one of the major transitions that *Objectivity* covers. The practices of 'mechanical objectivity' of the nineteenth century "meant cultivating one's will to bind and discipline the self by inhibiting desire, blocking

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[24] The use of 'ways of seeing' in Objectivity (page 369) is (most likely) a reference to the BBC show Ways of Seeing (1972) by art critic John Berger, inspired largely by Walter Benjamin's 'The Work of Art in the Age of Mechanical Reproduction (1935). This allusion has also been pointed out in Martin Kusch, 'Objectivity and Historiography', *Isis* 100, no. 1 (March 2009): 127–31, https://doi.org/10.1086/597564.

[25] Daston and Galison, 204.

[26] Ibid, 197.

[27] Ibid, 205.

temptation, and defending a determined effort to see without the distortions induced by authority, aesthetic pleasure, or self-love".[28] Then, the initial Victorian craze for mechanically-obtained image began to morph as the times of blind trust in the image were beginning to fade, and the optimism for the scientific machine – as technology and will-less self – to obtain unmediated images began to crumble. Another set of epistemic virtues besides 'restraint' and 'detachment' began to appear at the end of the nineteenth century, as the scientist was increasingly posed as more than a recording machine: interpretation was needed. The need for a 'trained judgment' was a manifestation that the will-less self could not in fact be so, and this established a new relationship between scientists and their recorded images: a profound technological and theoretical knowledge was needed to see. Those using images, like radiographers, geographers, or anatomists could not rely only on the abilities of their technical medium in stopping time, changing perspective and crossing boundaries of the visible. At the turn of the twentieth century, the ability to judge depended much more than on the machine alone. The "guided experience" of the skilful self was a prerequisite to see what was in front of oneself.[29]

In the field of historical virtue epistemology, other kinds of works have been inspiring for my research. In the edited volume *Epistemic virtues in the Science and the Humanities* (2017), editors Jeroen van Dongen and Herman Paul follow Daston and Galison's call for treating epistemic virtues –paraphrasing the authors of *Objectivity*– literally, not metaphorically.[30] Their attempt of tracing 'histories of the selves' of several scholars and their epistemic virtues was motivated by their goal of incorporating virtue epistemology within current trends in the history of science. In specific, they aimed for a *cultural* history of epistemic virtues, in which historians "examine, for example, how epistemic inquiry was justified and conditioned in political, religious, or aesthetic terms, what sort of contexts it was supposed to require and what kind of meaning science had for different audiences."[31] Their multifaceted volume contains histories of the self of naturalists, linguists, physicists, historians — scientists and humanists alike.

I draw two points of departure from the programmatic macro-history of *Objectivity* and (only in a minor way, as we shall soon see) from the micro-histories of *Epistemic virtues*, both relating to the *scope* of their studies. First, I don't draw here neither a grand-durée history nor one of specific persons. Rather, I see my thesis as a

[28] Ibid, 184.

[29] Ibid, 359.

[30] Jeroen van Dongen and Herman Paul, 'Introduction: Epistemic Virtues in the Sciences and the Humanities', in *Epistemic Virtues in the Sciences and the Humanities*, ed. Jeroen van Dongen and Herman Paul, 1st ed., vol. 321, Boston Studies in the Philosophy and History of Science (Springer International Publishing, 2017), 1–10, https://doi.org/10.1007/978-3-319-48893-6_1; Daston and Galison, 34.

[31] Dongen and Paul, 'Introduction: Epistemic Virtues in the Sciences and the Humanities', 4.

snapshot, a piece of a puzzle, a picture of 1970s molecular science brushed with broad strokes. I attend to the identity of a dynamic community, and as such, the self that I have in front of me is an abstract, idealised self, distilled from a spacious study of the social issues that the magazine and its community thought were important. I think it is important to draw such broad picture without an obsession to nuance because it allows us to characterise the identity of a decade. It is important to recognise the repeating virtues that appeared in the different debates, and the social and institutional mechanism that constructed those identities. Identiti*es* in plural, because as we shall see, the 1970s scientific self was a man with many faces.

I situate the most salient epistemic virtues of the 1970s molecular science, and I restrict my analysis to this decade because I see it as a key moment in the history of western science. As we will see through the chapters, the pressures from public offices to the scientific community forced scientists to re-evaluate their practice and how it related to social needs, with many solutions and identities to defend. The introduction of peer review in both the publication of articles and the revision of grant proposals as a *required* mechanism of social evaluation was institutionally set up in the 1970s not to safeguard scientific quality, but as a manner of fencing of criticisms of parochialism by state actors.[32] While I will not indulge in how the introduction of the peer review system was a political tool within a regimen of accountability, I explain the debates that made its implementation possible: a demand for science to deliver marketable results, mostly in the form of technological development. Peer review and concrete evaluative results are two aspects of the accountability problem of science which were debated and implemented as policy throughout the 1970s, and which responded to the self-image of scientists as they projected it to their public.

This aspect of accountability brings me to my second point of departure, also related to the *scope* of historical virtue epistemology. I humbly offer my thesis as enrichment of the present historiography by proposing to expand the *objects* that the concept of 'epistemic virtues' explains. I aim at broadening the reach of this philosophical tool by using it not only to discuss the entanglement of ethics and epistemics in the context of scientific *experimentation*, but to cover also the situations *outside* the laboratory, namely, how scientists negotiated their place within society. The cases presented in *Objectivity* represent a narrow process of science, namely the practices of experimentation. As such, 'epistemic virtues' are almost exclusively used to explain how "practices of scientific observation and attention" are interwoven and co-constructed with ethical norms.[33]

The case in the edited volume of van Dongen and Paul is different, and some of the contributing chapters do go beyond the experimental part of science, and attend to

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[32] Baldwin, *Making 'Nature'*, 180.

[33] Daston and Galison, *Objectivity*, 38.

the rich self-reflection of some scholars on the condition of being a scholar placed in a social world. Three chapters in particular illustrate what I mean by a 'historiographical expansion of epistemic virtues.' The most salient one is Jessica Wang's chapter, which explores the experience of two American physicists to the post-Second World War scenario and how they confronted the destructive possibilities of nuclear science. She writes a micro-history of the ideal of 'pure science', and how defending physics under these pretexts was not a possibility after the bombings of Hiroshima. Posing science as being good in itself for the sake of science, as Oppenheimer defended, was a moral position that some physicists didn't want to afford anymore. The struggles that some scientists expressed to the alliance between science and claims of national security illustrates a "struggle related to the cultural purpose and legitimacy of both science and scientist," Wang argues.[34] In the face of nuclear annihilation, and in the wake of the Holocaust and the power of eugenic thinking, a collective identity crisis transpired to the Cold War community's cogito: "The guiding principle that 'it is good to find out' no longer appeared axiomatic now that high science and technology had placed human existence in jeopardy.[35] If the epistemic qualities of science no longer made self-evident the value of doing science, then what did?"

The edited volume contains two more chapters where moral judgment is juxtaposed with epistemic worries. In de Bont's contribution, we find the story of early twentieth century naturalists and preservationists who observed and reported on a declining population of wildlife.[36] Their accounts of the difficulties of field research was highly moralised, and the processing of data itself was embedded in ethical reflection. Importantly for what's at stake here, and following the idea that the social value of science was decisive in shaping the scientific identity, de Bont's case shows how the goal of this science – the preservation of species – was the ultimate goal by which this profession should identify and live by.

Finally, Ad Maas makes a daring claim that "the emergence of objectivity was closely connected to the rise of a formally-rule and integrated nation state."[37] Taking the cases of three Dutch historians during the second half of the nineteenth century, Maas shows how the ambition of modernity to innovate both scientific practice and governance went socially and chronologically hand in hand. The practices of establishing social facts and ruling over society were transformed by an introduction of quantifiable

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[34] Jessica Wang, '"Broken Symmetry": Physics, Aesthetics, and Moral Virtue in Nuclear Age America', in *Epistemic Virtues in the Sciences and the Humanities*, 27–47.

[35] Ibid.

[36] Raf de Bont, 'The Adventurer and the Documentalist: Science and Virtue in Interwar Nature Protection', in *Epistemic Virtues in the Sciences and the Humanities*, 129–47.

[37] Ad Maas, 'Johan Rudolph Thorbecke's Revenge: Objectivity and the Rise of the Dutch Nation State', in *Epistemic Virtues in the Sciences and the Humanities*, 173–93.

parameters, where erudition and experience lost its place. Values like 'nationalism' and 'impartiality' co-constructed each other and were highly contested arenas which not only referred to the new ways of knowing and governing, but critically hinged on ways of *being* modern. Maas is explicit about his historiographical positioning in respect to *Objectivity*, and recognises how using 'epistemic virtues' to explain social tension was not the designed use of Daston and Galison:

> while Daston and Galison deem wider societal developments 'inadequate' explanations for 'objectivity' and 'only remotely relevant', I will try to demonstrate their interconnectedness (Daston and Galison 2007, quotations on 35–36; Daston 2014). This is not a far-fetched hypothesis: as the [Dutch] nation's intellectual and moral authorities and as teachers of the nation's ruling classes, professors were part of (or close to) the top ranks of the sociopolitical hierarchy. Several examples in this paper show that their discursive style and professional virtues resembled those of the leaders of the nation and were influenced by changes in the socio-political sphere.[38]

The relevance of the three chapters in *Epistemic Virtues in the Sciences and the Humanities* is both historical and historiographical. Let me start with the latter, which Maas already hinted at in his chapter, with the situation of "wider societal developments" as a satisfactory explanans for objectivity.[39] As well, in her contribution, Wang invites historians of science to attend to the notion of 'pure science' once again, and that while most practitioners readily "dismiss this ideal of pure science as rhetorical window dressing." I am with her in saying that investigating these use of notions does not automatically mean falling prey of their rhetorical power.[40] Instead, Wang argues, looking at how scientists cultivated or disdained the ideal of 'pure science' serves as a point through which to investigate how the identity of the scientific community was constructed around specific conceptualisations of the relation between science and society.

These three contributions to the field of historical virtue epistemology allow me to sketch an area where 'epistemic virtues' as a philosophical concept has barely stood. This is what I call here *social placement of science*: a relatively unexplored territory for historical studies to explain the social tensions of scientific practice as ethically knotted with epistemic commitments.

Complementarily to the historiographical relevance, the historical weight of the three chapters above for this thesis should be clear for those who have paid attention to

[38] Ibid.

[39] Ibid.

[40] Wang, '"Broken Symmetry"'.

their chronological position. Out of the ten chapters in van Dongen and Paul's volume, six chapters are situated in the nineteenth century, and four in the twentieth. Two chapters of the three I discussed here (de Bont's and Wang's) take place in the twentieth century (interwar period and Cold War, respectively), and the other (Maas's) in the late nineteenth *and* with an emphasis on the construction of the *modern* way of science and governance. This appears to me to go beyond simply editorial contingency, and that it reflects on the changes that occurred in this period. Namely, the importance of the social placement of science as deeply interwoven with reflections on the condition of being a scientist. By seeing how the social status of science and the experimental practices themselves interacted, and how, especially from the late nineteenth century onwards, this duo was framed as a matter of public interest and state concern, we can observe that late modern science was characterised by a demand for useful, socially progressive returns on the investments by the state powers, and importantly, the ability to gaze objectively over the expenses of scientists. While the patronage system of science has been in place since science is science, the institutionalised and quantified assessment are a product of the twentieth century, and only institutionalised after the Second World War. The evaluative-managerial regime of science funding was operationalised during the 1970s as peer review, but not alone. The first step was asking from scientists to deliver a marketable product in return on its investment. In this thesis I explain how such a demand was received by the community, and how the scientific self was moulded accordingly.

In specific, I argue that molecular scientists during the 1970s translated their ethical obligations as responsible citizens into the epistemic virtue of molecular vision. This molecular vision was a technical one, which precisely its technicism revealed its virtue: there, at the molecular level rested the world of objective truth and morally undisturbed peace of progress. By attending only to 'good' experimental practices, 'good' science was achieved. This is the paradoxical land of molecular scientists in the 1970s: social contribution came through their molecular technical eye. What 'social' and 'technical' were, however, was still up for grabs: negotiation ensured. The next two sections in this chapter attend to the historiographical literature that has allowed me to think about these two terms and interpret the actors' meanings.

And what about the images? What do the scientific advertisements themselves have to do with these epistemic, social, and ethical considerations (and the intricate relations between them)? I hope to show to the reader in this thesis how the content and form of the advertisements alluded to these dimensions on science, but more importantly, that in order to move from this representationalist notions of what advertisements 'mean', I will argue that their significance as allusions to the epistemic virtues of science is only possible when they are contextualised by the meanings which were negotiated in the most immediate context of the adverts: the magazine itself. In the editorials, the meanings of 'technical', 'social', and 'ethical' were debated and reified.

III. Biology situated politically

When biologists want to display their feathers as socially concerned animals, they say that biology is political and ideological.[41] What's worse, some historians buy into this.[42] But what does 'political' really mean?

In this thesis I take 'political' in its broadest and most descriptive sense: as the administration and governance of the issues that relate to public life, and in this case, to public *scientific* life. From the ancient Greek 'πολιτικός,' I attend not only to the national and state connotations of 'political' (which have a considerable weight on this thesis), but understood in its global sense of πολιτικός: "of, for, or relating to citizens," and "generally, having relation to public life."[43] I thus use 'political' as referring to that which involves discussion on the organisation of the scientific process and its relation with experimental, technical, governmental, commercial, and managerial actors. In my thesis, scientists are citizens. Their management is political.

So, what do we mean when we say that biology is political, and that scientists are placed politically? Let me illustrate this by going into a rabbit-hole of biology's historiography, its disciplinary history, the 'political' commitments of its practitioners, and their ways of being more than pipetting machines.

In the past decades much has been written on the twentieth-century history of laboratory life sciences. This rich historiography allows me to comprehend and interpret the conditions under which the scientific advertisement arose. On the one hand, Evelyn Fox Keller's *The Century of The Gene* and Hans-Jörg Rheinberger & Staffan Müller-Wille's *The Gene* tell a story about "the materialisation of the gene."[44] Their accounts provide a strong conceptual history of the molecular sciences and sciences of heredity, illustrating the different malleable notions of 'the gene', and the different ways in which biologists have reified its meaning so to be able to research and articulate its action.

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[41] Richard Lewontin, *Biology As Ideology: The Doctrine of DNA* (Harper Perennial, 1991).

[42] Maurizio Meloni, *Political Biology. Science and Social Values in Human Heredity from Eugenics to Epigenetics*, 1st ed. (London: Palgrave Macmillan, 2016), https://doi.org/10.1057/9781137377722.

[43] Henry George Liddell and Robert Scott, 'Πολιτικός', in *A Greek-English Lexicon*, Perseus Collection (Oxford: Clarendon Press, 1940, 1999 1843), http://www.perseus.tufts.edu/hopper/text?doc=Perseus:text:1999.04.0057:entry=politiko/s.

[44] Evelyn Fox Keller, *The Century of the Gene* (Harvard University Press, 2009); Hans-Jörg Rheinberger and Staffan Müller-Wille, *The Gene. From Genetics to Postgenomics*, trans. Adam Bostanci (Chicago and London: The University of Chicago Press, 2018).

Complementary to these conceptual histories of the molecular sciences in the twentieth century, we can find other grand narratives of the development of modern biology. One that is explicit about the political nature of the discipline is Maurizio Meloni's *Political Biology*, which reveals the marriage of epistemic notions with its political companions. Meloni's project is to "look at the political implications of human heredity," from the widespread and plural eugenics policies of the early twentieth century, to the molecularisation of the gene and the attempts to 'democratise' biology, up to the new social and political implications of epigenetics.[45] In a similar vein as historical virtue epistemology, the outcome, according to the author, is a "deliberate hybrid that conveys the inextricable, messy inter-connection of epistemic and political events in the relationship."[46]

Meloni claims that he offers a framework in which to understand much of modern biology. He proposes a separation of this discipline into "three different political-epistemic regimes in the long twentieth century of biology" [47] These regimes are both "productive" and "coercive", they "filter and stabilize certain scientific statements (hardening them into accepted truths) and marginalize or silence others as epistemically possible but practically nonviable."

While his 'long' history aims to cover the political articulation of biological thought since the late nineteenth up to the early twenty-first century, one of Meloni's major preoccupations and central turning point appears to be 1945. He effectively divides much of the molecular sciences as before and after the fall of Nazi Germany. One of his most significant claims is that it was not by virtue of separating politics from biology, but by giving a new political meaning to biology, that the post-Second World War molecular sciences advanced as a respectable area of study. In his view, genetics as a discipline underwent several changes in its philosophical objectives and principles: researchers went from thinking in terms of ideal types to population thinking, from realism and essentialism about race to the study of genotypic variation, from fear of genetic-degeneration at the social level to a celebration of genetic diversity. Eugenics, and the publicity of Nazi Germany, forced scientists to rethink their discipline, and how it fitted in a social world. But what was the conceptual key which would paradoxically untangle, and at the same time, tangle, 'science' and 'society' after the Second World War?

Meloni argues that past the dark power of science as represented by eugenics in practice, new meanings of 'technical' were designed, and around this meaning, the identity of a community. Meloni argues that remaining 'pure', 'technical', and 'microscopic' were ways by which scientists could look another way and dodge debates

[45] Meloni, *Political Biology*, 4.

[46] Ibid, 8.

[47] Ibid, 25.

over the social placement of biology. 'Technicism' became, according to Meloni, a tool intended to separate biology from the domain of 'the social' and 'the ethical'.[48] According to Meloni, this intention of separating the social body from the biological body is doomed to fail, and that 'meaning' is always a necessary condition for interpreting and formulating epistemic questions. For Meloni, this 'meaning' is of course, political. Past 1945, 'political' in Meloni's work refers to several processes, such as the political internationalisation embodied in the founding of new economic, financial, juridical, and political institutions and the democratization and universalist ideals under the human rights framework. These grand political restructuring, according to Meloni, was articulated with epistemic notions in biology. The celebration of individuality in this new democratic and globalist age that was supposed to celebrate diversity hinged on the genetic sciences now functioning as "as a rationale for democratic equality," not a tool of eugenic rationalism.[49] The division of biology's ideology as being significantly different between pre- and post-1945, as well as before and after the rise of molecular biology as Meloni does here is highly contested, as we shall see in the following section.

While the framework of Meloni may serve to situate political ideals with trends in research programmes in biology, I am not satisfied with Meloni's understanding of 'political'. I wish to return to a more concrete and localisable meaning of the word: the management of community of individuals. The way research programmes were initiated or halted ("productive" or "coercive" forces of political-epistemic regimes, to use Meloni's wording) was by the patron's wish to sponsor research. Individual scientists and their laboratories, not epistemic wishes, had to be assessed, rewarded, and punished. By 'political' I mean here the ways by which scientists were funded and evaluated, and which, ultimately, dictated who has the resources (money) to perform research and share their ideology.

Going full circle, positioning the political side of science as I have introduced it here in the context of virtue epistemology, transforms my question into a matter of group identity: what are we doing in science, and how do we place our work and ourselves in society? How do we get funding for doing what we do? The scientific community sought answers to these questions, made explicit and negotiated in editorial columns. The argument of Meloni was that a 'technical' gaze emancipated biologists from evil uses of their knowledge, and at the same time, linked them to society through the production of objective knowledge free from political impurities. But, what did 'technical' mean? What was the means to achieve this 'technical' gaze? In the next section I introduce briefly to the historiography of the complicated relationship between the 'technical' and 'social'.

[48] Meloni, 155.

[49] Theodosius Dobzhansky, 'An Outline of Politico-Genetics', *Science* 102, no. 2644 (1945): 234–36 as cited in Meloni, *Political Biology*, 153.

IV. Buying and selling technical solutions to solve social issues

The materialisation of the gene was highly dependent on technical apparatus, like those used to create x-ray diffraction images, or the enzymatic reactions needed to cleave genetic material.[50] As the bodies under study went from cellular to molecular, the laboratory needed fine chemical reagents and enzymes to extract the molecules of life from living bodies. Throughout the second half of the twentieth century, the making of devices for the manipulation was increasingly outsourced to private manufacturers, and the biotechnology industry began having a fundamental role in laboratory biology. This privileged position can be seen by their place in *Nature*, where during the 1970s the great majority of advertisements referred to biotechnology apparatus.

Biotechnology's position wasn't uniquely printed, and their ubiquity in *Nature* isn't the only way their central role in science was manifested. A collection of essays in *Private Science. Biotechnology and the Rise of the Molecular Sciences* (1998) illustrates the many ways by which the commercial industry behind laboratory materials symbiotically interacted with their academic research practice. The volume, edited by Arnold Thackray, addresses notions such as 'private' and 'public' science, 'basic' and 'applied' research, analysing historically the fragility of the borders between these categories, and at the same time, shows how scientists used these concepts and other notions like 'speed', 'universality', 'responsibility' and 'competition' to fence off certain debates, promote others, and enrol financial actors in the pursuit of establishing disciplines and securing funding.

The meaning of 'biotechnology' is now largely associated in the public's imaginary to *genetic* biotechnology. However, biotechnology is almost as old as man, Robert Bud argues in his chapter 'Molecular Biology and the Long-Term History of Biotechnology'.[51] Bud traces the history of biotechnology as a relatively long one which has changes names. While the use of fermentations in food preparation and conservation dates of millennia, the use of living (micro)organisms in the production of goods was industrialised in the nineteenth century. As such, Bud argues, the industrial use of biological organisms, which before the 1950s was called zymotechnology due to focus on enzymatic reactions, was largely associated to the pharmaceutical and agrochemical industries, which, for example, employed yeast to make beer and animal feeds, or

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[50] A most remarkable example would be Rosalind Franklin and Ryan Gosling's 'Photo 51'. Rosalind Franklin and Ryan Gosling, 'Molecular Configuration in Sodium Thymonucleate', *Nature* 171, no. 4356 (April 1953): 740–41, https://doi.org/10.1038/171740a0; For a historical review on the materialisation of the gene, see Keller, *The Century of the Gene*.

[51] Robert Bud, 'Molecular Biology and the Long-Term History of Biotechnology', in *Private Science*, 3–19.

fermented sugars and paraffins to make fuel and protein-rich foods.[52] Importantly, Bud argues that "only in the 1970s did the biotech became to be associated with *genetic* technology" through the framing of the new technology as capable of "solving society's problems." Bud situates three moments that shaped the perception of genetic science as socially productive, which until then had been a fairly abstract discipline without clear applicability: first, the synthesis of a human gene for the first time in 1967 by Arthur Kornberg; second, the successful transfer (known as DNA recombination) of genetic material from one *E.coli* organism to another in 1973 by Stanley Cohen and Herbert Boyer; and third, the moratorium imposed by the National Institutes of Health (NIH) of the United States of America on recombinant DNA technologies from 1974 to 1976. These three moments are for Bud indicative of the discursive approaches that genetic scientists used to secure their discipline and attract funding during the late 1960s and early 1970s: defend themselves from accusations of being socially detached Frankensteinian mad men, and more importantly, showing (at the time, mostly theoretically) the benefits of understanding and manipulating the molecular basis of life. The first two 'key moments' are an example of the image of geneticists as productive, and the NIH moratorium as the image of scientists as an ethical community willing to stop their own work for the good of society. Both these images were, as we shall see in Chapter Two and Three, subject to heated debates.

In another *Private Science* chapter, 'Problematizing Basic Research in Molecular Biology', Lily Kay studies the 1930s and 1960s, with the aim of problematising two dualisms: that there was a significant break between pre- and post-DNA recombination eras, and subsequently, that there is a valid division between 'pure' and 'applied' research. Kay narrates the account of how biotechnology had become by the end of the twentieth century "embedded in a politics of history and meaning: either touted for its promise as a revolutionary technology or, in response to alarm over risks, downplayed as age-old domestication of nature."[53]

During the 1930s and 40s, the 'Science of Man', referring to the research the biological substrate of human life, was modelled on the success of the physical sciences. Associated with eugenic thoughts, Kay argues, molecular biology was presented as the upcoming "means of social control," which promised to "supply instrumental rationality to the managerial sector."[54] During this time, however, genetics was still in the process of marrying biochemistry, and we need to understand molecular biology as their disciplinary offspring.[55] Even after being materialised, the study of genes as DNA was

[52] Bud, esp. 6-9.

[53] Lily E. Kay, 'Problematizing Basic Research in Molecular Biology', in *Private Science*, 20.

[54] Ibid, 22.

[55] Michel Morange, 'The One Gene-One Enzyme Hypothesis', in *A History of Molecular Biology* (Cambridge and London: Harvard University Press, 2000), 21–29.

still done on experimental models quite removed from man. But this didn't mean that practitioners didn't see the biomedical applications that could be developed from their viral, fungi, bacterial investigations. Leading experimentalists such as Max Delbrück, Goerge Beadle, Edward L. Tatum, Linus Pauling, and Joshua Lederberg "readily extrapolated," Kay writes, from these "microorganisms and molecules to humans."[56] And importantly, their projections were financially backed: "scientists and patrons came to share a molecular vision of life. As such, they became producers of a discourse that represented organisms as the genetically directed activity of molecules and viewed the study of microorganisms and proteins as the surest path to understanding and controlling human physiology."[57] Notions that arose in that era in the context of molecular biology such as 'coding', 'information', and 'message' were appropriated from cybernetics, information theory and military command systems.[58] By the 1950s their meaning was being repackaged to signify the power that molecular knowledge entailed in the context of the upcoming applied science of genetic engineering.

Genetic engineering during the 1950s and 60s became the utopian promise of eradication of disease, and molecular knowledge as revolutionising our conception of individual and social health: the vision was one of "biology turning molecular, medicine maturing into an exact science, and social planning becoming rational."[59] The hopes of the new technology were in fact its capability of acting on the individual person but having a social reach: "an emergent medical and social technology," Kay writes, where scientists advocated for a re-conceptualisation of the meaning of eugenics. "The old eugenics was limited to a numerical enhancement of the best of our existing gene pool," Caltech geneticist Robert Sinsheimer argued. "The new eugenics," he forecasted, "would permit in principle the conversion of all the unfit to the highest genetic level."[60] But the 'in principle' was not enough. During the 1960s, public suspicion on the technology grew, and old fears came back. But this time, the distress was caused not by images of concentration camps but by a mistrust on what happened behind laboratory doors. The power of genetic engineering began to be framed in a dualistic manner: by its power to act as medicine's next scalpel, but also by its power to be accidentally or intentionally mishandled. Were scientists capable of behaving ethically at work? And what did it mean to behave ethically? From the mid-1960s onwards, Kay writes, "a counterdiscourse emerged that in the following decades seriously challenged the hegemony of the

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[56] Kay, 'Problematizing Basic Research in Molecular Biology', 24.

[57] Ibid, 24.

[58] Ibid, 27; Andrew S. Reynolds, 'Cell Signaling: The Cell as Electronic Computer', in *The Third Lens: Metaphor and the Creation of Modern Cell Biology* (Chicago and London: The University of Chicago Press, 2018), 114–45.

[59] Kay, 'Problematizing Basic Research in Molecular Biology', 31.

[60] Robert Sinsheimer, 'The Prospect for Designed Genetic Change,' *Engineering and Science* 32 (1969), 8-13 as cited in Kay, 33.

molecular vision of life." The debates ignited the pressure to make science 'ethical', and whether "technical feasibility" should precede or follow ethical considerations.[61] But exactly what 'ethical' or 'technical' were, as we will see in Chapter Three, was debated under different meanings and contexts. The main question, however, was whether scientists were able to govern themselves and manage on their own.

This question is addressed in another chapter in *Private Science*, and whose author, Susan Wright, developed later into a monograph which has a large weight on this thesis. For now, it is sufficient to look at how the chapter 'Molecular Politics in a Global Economy' in the edited volume frames the question of scientific governance. In short, Wright narrates "the rise and fall of genetic engineering controls in the United States and the United Kingdom from 1972, when results of the first controlled genetic engineering experiments were published, to 1982, when controls for research and industrial processes in both countries were largely dismantled".

In explaining how science policy came to be, Wright argues for an intermediate position between rationalist presentism and post-structuralist relativism. On the one hand, "a dominant view, technical reductionism, [which] assumes that the problems posed by genetic engineering were solved though a rational process of assessment conducted by technical experts [...] with privileged access to specialised knowledge in the face of resistance form an irrational public."[62] On the other, the view of "domination by an international scientific elite [which] 'closed ranks' on the concerned public, acting to persuade the latter that the field was safe but aware that definite evidence for this evidence did not exist."[63]

In her middle position, Wright argues, the ethical issues surrounding the technology were reduced to the topic of workplace safety, and safety itself reduced to "a technical question of containment." She frames this technical reductionism and the dismantling of controls to the growing commercialisation of the molecular biology, and concretely, to the literally exponential presence of equity investing on the technologies of DNA recombination, those which were precisely under scrutiny.

> from the demonstration of the bacterial expression of the somatosatin gene in 1977 onward, multinational corporations and venture capital firms began to invest heavily in small genetic engineering firms [...] These investments initiated the field of molecular biology into a new commercial role. Members of the field, formerly cloistered in academe, became equity owners, corporate executive, and

[61] Kay, 33.

[62] Susan Wright, 'Molecular Politics in a Global Economy', in *Private Science*, 84.

[63] Ibid, 84.

> industry advisers. Of course, ties between academe and industry were not new. What *was* new was the extent of these ties and the intensity of their effects.[64]

What is a stake here is not only that deregulation of the technology needs to be explained not as a rational procedure in risk assessment but as the product of political pressure groups with commercial intentions. What is important for this thesis is (also) that those commercial-academic relations reconfigured the identity of scientists. Now that researchers had also become “equity owners, corporate executive, and industry advisers” not only their commitments changed, but what it meant to be a ‘good’ scientist was being redefined and negotiated by written conversation in places like the printed forum.

Concluding remarks

The theme of this thesis is identity, the frame is science policy, and the means is the printed forum. An underlying assumption in my thesis is that the construction of a self-identity by the molecular community was situated within the technical, political, and economical facets of science. That allows me to narrate a story of the identities as portrayed in a visual and textual media of *Nature* as the forum of the community.

Advertisements and editorial pieces, as image and texts, co-construct one another. The advertisements serve me as snapshots of the historical moment, as portraits of idealised personae and visionary laboratories, refined and purified by a for-profit commercial industry. Additionally, I will show also the functional and material aspects of advertisements, and argue that they weren’t simply a marketing tool, but intrinsic objects used in research. This will be the content of the Chapter One, ‘The advertised scientific self. Epistemic virtues of scientific instruments in *Nature*, and the material culture of advertisements (1970-1979).’ The editorial pieces allow me to situate these advertisements in a grander context. I will take two case studies. First, the debates over how science should be placed in society, and the standards by which the different stakeholders of science should assess ‘good’ science and virtuous scientists, many of whom encouraged science to be a productive endeavour worthy of economic patronage and which had to deliver marketable products in return. This will be the content of Chapter Two, ‘The placement of the scientific self in society. Meanings and uses of ‘social’, ‘political’ and ‘social responsibility’ in the context of British science policy, the Rothschild and Dainton reports, and the British Society for Social Responsibility in Science (1971-1972)’. Then, the second case study approaches the topic of ethics *in* the workplace and *of* one’s work. I look at the different approaches to the management of the scientific practice, and the debates that took place when a specific ethical issue

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[64] Ibid.

arose, which related to the safety of the new technologies, leading to debates which threatened the autonomy of the molecular biology community. This will be the content of the final empirical chapter, Chapter Three, 'Regulating 'the self'. The meaning and uses of 'technical' in the debates over the safety of recombinant DNA molecules (1972-1978).'

Following this historical exposition, I will then proceed to the two final chapters. In Chapter Four, I will revise some of my methodology in regard to the relationship between text and image, and thus between scientific advertisements and *Nature*. This chapter, 'Scientific advertisements are a form of art. On what images mean in history, and on the relationship of scientific advertisements to epistemic virtues' is an exercise on self-reflexion, and serves also as a set-up for understanding the relationship between the three empirical chapters, a topic to which I dedicate the final closing chapter. In 'When science buys and sells, ethics arise. Money, virtue, and the identity of the virtuous molecular scientists' I will delve on the relevance of this thesis in particular, and of history of science in general studied from the perspectives of virtue epistemology and the history of scientific journals. I hope however, that the reader perceives the significance between the lines of the upcoming chapters, but it may be worth quickly making explicit the two most salient ones.

First, the historical, sociological, and philosophical investigations of the scientific media is timely, for two reasons. The scientific article and scientific journal, although they played a vital role in the epistemic and political discussions of current science, have received little scholarly attention within the fields of history and philosophy of science, and science studies. The importance of media is aesthetic and political, and as Csiszar stated in his central claim in *The Scientific Journal*, "moments when the norms and forms of expert communication have been most in doubt are precisely those moments when scientific practitioners have sought—or been forced— to renegotiate their public status within a wider political landscape."[65] Today may be considered one of those 'moments', as the credibility of journals as gatekeepers of truth is put into question by scientists themselves. Epistemic issues like publication biases or the replication crisis are profoundly inserted in social and political sceneries, such as career instability in academia, the evaluative-managerial regimes of science funding, and the public legitimacy of research.

The second point of relevance which I like to stress is the relationship between the three chapters: between the structure of science funding, the ethics of research, and the position of commercial manufactures in scientific experimentation. I argue in Chapter Two that the early debates of the 1970s positioned science as a productive activity, in which funding was perceived as the investing input and marketable products were expected as output. In Chapter Three, I argue that research ethics and any ethical

[65] Csiszar, *The Scientific Journal*, 3.

consideration of molecular work was seen not only as a hindrance, but as epiphenomena of their work. Against claims by historians or sociologists of science that science was or is 'unethical', I would argue that scientists did and do take ethics into account, but they conceive ethics as an emergent phenomenon of the properties of a network of scientists producing concrete products (papers, patents, technologies). As such, these products of the scientific practices are conceptualised to be 'good' *only* in combination. It is only by contributing to the collective making of concrete products that the individual scientists can harvest the moral goods of the scientific practice. When conceptualising ethics as an epiphenomenon, the individual scientist alone has no meaningful access to the realm of ethics. Being a 'good' scientist means, in this framework, trusting the increasingly complex community of researchers, a community that I hope to have shown in Chapter One, did not involve only scientists but manufacturers of objects as well. The first chapter shows the epistemic virtues highlighted in advertisements, as hyperbolised virtues that working scientists ascribed to. One of those virtues was the reliance on international companies to produce standardised objects, articulated with an increasing outsourcing of the making of devices and reactives. This outsourcing is an example of the increasing hyper-specialisation of science, not (only) in terms of subject matter, but also of the kind of work being done. With the making of research instruments being subcontracted to commercial companies, the job of scientists was narrowed to "observing and recording results," as one of the manufacturers put it. In this line of reasoning, the outsourcing was not only of the raw materials of scientific practice, but of the ethical considerations of such work. Seeing ethics as a by-product was the result of the narrowing of the way by which scientists were expected to work and produce. The relevance of this historical description is that any discussions that we may have today about research ethics or the ethical issues of certain technologies (like CRISPR-Cas9) need to take into account the structures by which science is funded and assessed, the work that scientists are expected to do, and their day-to-day practices, like the browsing product catalogues and buying reactives.

My main question in my thesis is: how did university researchers see themselves in society? What did they think they were doing, for who, and why? How was the social place of molecular science during the 1970s defended, and how was the scientific self moulded accordingly? These questions of identity is and placement are driven by analytic curiosity, as the historical virtue epistemology literature envisions them to be, but were also real worries for the historical actors. Coming full circle to the lead editorial 'Science and Public Pleasure' with which I opened this introduction, the *Nature* editor in the shadows wondered of the condition of being a scientist:

One does not, for instance, expect a stock-broker, however personally satisfied by his job, to look for any sort of public acclaim. At the other end of the spectrum are those who offer some sort of pleasure to the general public, not necessarily to their own satisfaction (for example, prostitutes, troubled poets and so on). Where do scientists, and in particular research scientists, fit into this scheme?

Chapter One. **The advertised scientific self.** Epistemic virtues of scientific instruments in *Nature*, and the material culture of advertisements (1970-1979).

With this chapter, I address the question: What can be learn from scientific advertisements? At the outset, I set myself both sceptical and hopeful about the power of these visual objects for telling a story about science. Here I explore *how much can they say* about the meaning of science, and pose questions such as: Are scientific advertisements proxies of what science was like in the 1970s? Can we learn something about what life was like at the bench, from an advertisement of antibodies? The combination of the graphic and linguistic characteristics used in advertisements, purposely designed for engaging in action, highlights elements of the historical moment that other sources may lack. The spirit of advertising agencies, experts in hijacking attention, may come in handy now that I describe what was in fact worthy of attention.

However, I am convinced that the scientific advertisements won't talk by themselves. They will most certainly be missing some of the analytical precision that a historical study requires. For that, I will explore in the upcoming chapters the mirror image of this chapter: which elements outside the advertisements need to be included in order to understand them? Some advertisements leave one puzzled, what do they mean? For example, why did the manufacturer Collaborative Research make a political joke about Cold War politics to sell insolubilized oligonucleotides? To reconstruct the meaning of some advertisements, they need to be contextualised within the place and moment where they were printed. The most proximal context is the magazine *Nature* itself, and specially, the political pieces: the lead editorials, News and Views sections, and the correspondence letters placed alongside the advertisements. I will start to develop this analysis here as a cue for the rest of the thesis.

My analysis in this chapter is synchronic. Advertisements in *Nature* came and went, but many stayed throughout various years. The magazine, however, was printed weekly, and during the 1970s it prided itself of announcing the latest scientific discoveries, and the most recent debates on science policy (among other science-related news). As my overall story in of this thesis is a narrative that uses both advertisements and political pieces, which both have their different tempos, I attend only superficially to chronological happenings. The basis of my analysis here is extracting the elements that are present in the advertisements, attending especially to those elements that repeat themselves frequently in advertisements of different objects and from different companies. Paying attention to recurring elements allows me to sketch patterns of the epistemic and social virtues of molecular biology during the 1970s, through the eyes of for-profit manufacturers of laboratory objects.

In this way, I will first sketch what sort of target the scientists are in the eyes of the biotechnology industry. Then, I will make a larger (and perhaps, more controversial) claim, arguing that ads are particularly important because they reflect more than attitudes *towards* scientists, but that they partially mirror the epistemic virtues that the scientific establishments foster. As literal illustrations of the ideal science, ads convey to the historian the self-identity that the scientific community *projects* about itself towards those who are willing to listen. Those who are willing to listen were the readers of the magazine, in my case the molecular scientists themselves with an interest on radioactive ligands and automatic cell counters. As well, it may be worth noting that not all scientists looking at the ads were able to buy the products listed. The task of managing the budget of laboratories, and assigning which products were interesting to acquire fell on the lab director (principal investigator, as it is called nowadays), and the technical lab manager. However, this aspect of *who* were the advertisements for is only of minor importance in this chapter. The chain of events from marketing, observation, and acquisition is only of tangential interest. This functionalist approach would be daunting, as knowing the commercial effects of the advertisements or the reception by individual scientists may be a nearly impossible task. I am more interested in knowing *what* the ads were saying and *how* they could be read, paying more attention to the context of their printing (the magazine) than the response of the scientific community. In short, I don't attend to the question 'did advertisements accomplish more sales?', I attend only partially to the question 'how do advertisements work?', and focus most of my investigation to exploring the question 'what is a scientific advertising?'. This chapter is highly empirical in character. The reason of why I chose this approach will become clearer after reading the following two, and in Chapter Four, I will lay out my philosophical commitments in my approach to the visual in a theoretical reflection.

A note on the format of this chapter and its intended reading form. I present here advertisements and have commented on them from a variety of angles. As a result, I present an image that emerges from these advertisements: what it meant to be a good molecular scientist in the 1970s told through the eyes of commercial companies. Now, before we begin, it's useful to remember what John Berger said in the 1972 BBC show *Ways of Seeing*:

> remember that I am controlling and using for my own purposes the means of reproduction needed for these programmes. The images may be like words, but there is no dialogue yet. You cannot reply to me. For that to become possible in the modern media of communication, access to television must be extended beyond its present narrow limits. Meanwhile, with this programme, as with all programmes, you receive images and images which are arranged. I hope you will consider what I arrange, but be sceptical of it.

Granted, some of my readers will be able to *reply* to me in different forms (by assigning a numerical grade or by providing feedback in a written form). However, something about Berger's transparency holds today in this context. In *Ways of Seeing*, Berger gave importance to the *image* and the *readings* of European painting between 1400 and 1900. I certainly deal here with a significantly different topic, but I have also given a critical importance to the images of advertisements in the 1970s, and specially the ways of looking at them. I believe that the reader, you, should accompany me in my interpretation of the images. To make this 'critical importance' useful to you and actually articulate it by taking advantage of the freedom of the seeming endless digital format, I have dedicated a page for each of the ads and included no caption. Those are up to you. I highly recommend that before reading my own comment on the image, you look and analyse the image itself. Dedicate some moments to it, take a pause, explore its silence and take a break from my words. It would be expected that our readings don't coincide, because I have arranged the images and their meanings for my own narrative. However, if our readings of these scientific advertisement do coincide, perhaps we could say that our experiences speak something worth saying about them.

I. Capitalised: Bought space, free catalogues

The income that advertisers provided to *Nature* was substantial — it subsidised production costs and helped in reducing subscription rates.[1] The economic support that advertisements gave was reflected in their placement. Since the first issue and for almost the first hundred years of *Nature* (1869-1971), the front face of *Nature* featured a large commercial announcement illustrating images of microscopes, telescopes, cameras, incubators, and a variety of scientific and technical objects for research, educational, and recreational purposes. For these first hundred years, the front cover consisted of just two elements: the masthead and a full-page advertisement. Only in 1971, the advertisement was substituted by an image which referred to the content of some of the experimental articles. Despite advertisements being moved into the inside pages, *Nature* still relied on the commercial transactions of its printed space, and the economic prize of those inner pages raised substantially.

Prices were calculated by the space they occupied, position, and colour. In 1961, a sixteenth page black and white advertisement placed in predetermined non-central pages would cost a minimum of £3,5.[2] For comparison in the same year, the highest

[1] John Hall, 'Science Journals in a Prices Jungle', *Nature* 247, no. 5441 (1 February 1974): 417–18, https://doi.org/10.1038/247417a0.

[2] 'Nature - Revised Rates' (London, 1961), ARC3/M052/Nature/Advertising/1961, Nature Archive, The Archive of Macmillan Publishers, Basingstoke.

price would be that of a whole page front cover advertisement printed in colour (blue and green were more expensive than orange), sold at a rate of £87.[3] The prices rose steadily during the 1960s, and very substantially during the 1970s, with a sixteenth page going from £3,50 in 1961 to £25 in 1975.[4] By 1978, the price for a quarter page had gone up to £132, a 991% increase as compared to 1965, when the price was £12,10.[5]

Parallel to the increase of advertisement rates, their management became more important during the 1970s. From 1932 until 1975, the administration had been outsourced to the firm T.G. Scott, being responsible for advertising and promoting advertising space in *Nature*. The advertising firm would send brochures to account holders and show advertisement rates.[6] However, in 1975, an in-house advertising department was created in Macmillan Publishers, the overarching publishing house who owned *Nature* since its founding.

The process of bringing advertising control into Macmillan was further put into place in 1977, when a new section appeared in *Nature* called 'Newly on the market'. Carrying the same typeface, linespacing, and overall format as the scientific and political pieces of the magazine, this section listed a variety of available laboratory products. It did not mention prices, and the descriptions appeared more neutral compared to the visual advertisements that the manufacturers themselves created. On the first appearance of 'Newly on the Market', we get an impression on the aims of this sober section, and the description of one of its products:

> These descriptions are prepared by the staff of *Nature* on the basis of material provided by manufacturers. The Reader Enquiry Card faces the inside cover. [...] *Sample-handling system for radioimmunoassay. Beckman*. The modular Phase I RIA sample-handling system is designed to improve radioimmunoassays by simplifying procedures for rapid, accurate, inexpensive analyses. Stand-alone instruments, accessories, and packaged RIA kits make up the system, which is available as a whole or in parts. Phase I system includes the Gamma 4000 gamma spectrometer, which is capable of processing up to 400 samples automatically; an on-line DP-5000 microprocessors data reduction system, which stores up to 10 RIA data reduction programs including counts per minute;

[3] Ibid.

[4] 'Mini-Ads', *Nature* 257, no. 5529 (30 October 1975): Cover 3; Alysoun Sanders, 'Email Conversation with Alysoun Sanders, Archivist for Macmillan Publishers and Springer Nature', 23 April - 21 May 2020.

[5] 'Rates' (London, 1965), ARC3/M052/Nature/Advertising/1965, Nature Archive, The Archive of Macmillan Publishers, Basingstoke; 'Mini-Ads', *Nature* 274, no. 5674 (31 August 1978): Cover 3; Sanders, 'Email Conversation with Alysoun Sanders, Archivist for Macmillan Publishers and Springer Nature', 23 April - 21 May 2020.

[6] Sanders, 'Email Conversation with Alysoun Sanders, Archivist for Macmillan Publishers and Springer Nature', 23 April - 21 May 2020.

> the Model J-6 Centrifuge; and an all-in-one pipettor/diluter/dispenser which allows the solid-phase Beckman RIA Kits to be run with one simple pipetting step. Basic to the Phase I System are Beckman double-antibody, solid-phase RIA kits, which reduce incubation time, centrifugation requirements, and chance for error. Circle No. 47 on Reader Enquiry Card.[7]

'Newly on the market' did not survive long, it was in place from the issue of January 6th, 1975 until the issue of November 3rd of the same year. While the format mimicked that of *Nature*'s native content, and the description was quite technical, this section did contain some of the elements which I will bring out in more detail in this chapter: the epistemic virtues of these laboratory objects. From the description of the Beckman radioimmunoassay sample-handling system we can already see some of those virtues that were highlighted by *Nature*'s staff: "rapid, accurate, inexpensive analysis," "processing up to 400 samples automatically," and "one simple pipetting step." Let us return to these epistemic virtues later on —for now, suffice it to say and remember that 'speed', 'cost-efficiency', 'automatization', and 'ease of manipulation' were those virtues highlighted in this 'Newly in the market' entry, and which accompanied the description of many of the ads we will see below.

Space in *Nature* grew to be a privileged position. With the growing subscription base of the journal, the advertisements had the function to attract attention and invite consumers (i.e., researchers) to buy their products (i.e., laboratory objects). This space that manufacturers bought in *Nature* and that I have called 'advertisements' were not only *just* that. In fact, they had a dual function, many of them had a very practical function. They weren't only for advertising itself but had a specific use for scientists wishing to buy the marketed products. As the reader will see in several of the advertisements in this chapter, advertisements contained a fillable form that buyers could cut off from the magazine, fill out with their details and laboratory wishes, and send it by post to the manufacturer. Many of the advertisements, including an advertisement to submit advertisements to T.G. Scott (figure 2), contained this sort of interactive format.[8] The object (an empty form) would be displaced from its original context (the ad) and be repackaged as something else (a filled form). In this way, advertisements gain something beyond their appearance of a frivolous marketing tool. From this dual function, we can envision scientific advertisements in the 1970s as intrinsic elements of research: when they were used as forms, they became part of the mechanism of research itself, as they carried essential information on the material needs of scientists.

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[7] 'Newly on the Market', *Nature* 270, no. 5632 (November 1977): 87–88, https://doi.org/10.1038/270087a0. Original emphasis as bold, instead of italics.

[8] T.G Scott & Son Ltd, 'Order from Nature', *Nature* 249 (5452): xiv.

xiv *Nature May 3 1974*

ORDER FORM NATURE

To: CLASSIFIED ADVERTISEMENT DEPT., NATURE,
T. G. SCOTT & SON LTD, 1 CLEMENT'S INN,
LONDON WC2A 2ED.

Please include the following advertisement in normal/display style setting(......* mm) for......... *insertions under the heading

....

* If display style is wanted, please state required depth of advertisement. 26 mm, 52 mm, 78 mm, etc.

Write or stick your advertisement in this box.

PLEASE STATE CLEARLY WHETHER NAME AND ADDRESS OR A BOX NO. ARE TO APPEAR.

.........lines/mm at.........rate (plus Box No. at 30p if required) making a total cost of £...........

NAME ....

ADDRESS ....

HERIOT-WATT UNIVERSITY

Applications are invited for a

CASE studentship

in the Department of Chemistry. The successful applicant will work in collaboration with Organon Laboratories Ltd. on new amino steroids in a series already established as possessing interesting pharmacological and clinical properties. The project will involve working both in the University and the industrial laboratories.

The University is situated in a parkland campus just outside Edinburgh.

The student, who should have a good Honours degree, will register for the degree of Ph.D. Enquiries should be sent to Dr. M. M. Campbell, Department of Chemistry, Heriot-Watt University, Riccarton, Midlothian EH14 4AS. (1682)

UNIVERSITY OF NEWCASTLE UPON TYNE

ROBERT WOOD FELLOWSHIP

DEPARTMENT OF CHEMICAL ENGINEERING

Applications are invited for the Robert Wood Fellowship tenable in the Department of Chemical Engineering. Applicants must hold a B.Sc. degree (or be prospective B.Sc. graduates) of the University of Newcastle upon Tyne or Kings College in the University of Durham. Applicants for the Fellowship must intend to undertake research in Chemical Engineering topics. The Fellowship will be of the value of £695 per annum plus fees.

Further particulars and application forms may be obtained from the Registrar of the University of Newcastle upon Tyne, 6 Kensington Terrace, Newcastle upon Tyne NE1 7RU. Completed application forms should be submitted to the Head of the Department of Chemical Engineering by June 30, 1974. (1643)

WESTFIELD COLLEGE (UNIVERSITY OF LONDON)

N.E.R.C. RESEARCH STUDENTSHIP

for work on "Water and ion movement in live Sphagnum and peat" is available from October 1974. Peat covers about 2 per cent of the Earth's surface. The ecology of one of the major peat forming plants is to be studied.

The basic award is £695 a year and is under the N.E.R.C. regulations.

Applicants who have or hope to have a good honours degree in BIOLOGICAL, CHEMICAL or PHYSICAL SCIENCES will be considered.

Apply by May 29 to Dr. R. S. Clymo, Westfield College, Kidderpore Avenue, Hampstead, London NW3 7ST. (1671)

LANDSCAPE ARCHITECTURE. RESEARCH STUDENTSHIPS available for honours graduates in land use or natural science subjects.—Apply: Department of Landscape Architecture, The University, Sheffield S10 2TN. (1665)

UNIVERSITY OF EDINBURGH

RESEARCH STUDENTSHIPS IN SOCIAL ASPECTS OF SCIENCE

Studentships are available in the Science Studies Unit, Edinburgh University, for research on Appropriate Technology for Developing Countries; Differentiation in Modern Astronomy; and aspects of the Social History of British Science. Applicants must have (by July 1974) a good first degree in (preferably) science, technology, social sciences or history.

Contact: The Director, Science Studies Unit, University of Edinburgh, 34 Buccleuch Place, Edinburgh EH8 9JT. Please quote reference 5014. (1679)

Figure 2.

Some of the space that manufacturers bought did not only showcase the products themselves, but invited researchers to a free subscription to their product catalogues. These catalogues are part of what is often referred to as 'trade literature', a genre comprising of "discrete publications issued by or on behalf of a company in order to promote the image and/or products of that company."[9] The catalogues advertised in *Nature* were sent by mail without any cost to subscribers. Virtually all manufacturers used this service of mail marketing and pressed hard in their *Nature* ads for readers to subscribe to them. Calling or writing a letter to the manufactures were the typical means of communication for researchers, and they would be sent the catalogues by post. Miles Laboratories, as we will see in detail later, approached marketing with a personal touch. They drew from the testimony of actual persons to promote and encourage trust in the firm, and they positioned themselves as a partner in the research efforts. Featuring a concentrated scientist manipulating a glass pipette, their ad for lectins (a protein family) read, "Our commitment to filling your needs and continuing two-way dialogue with the research community has resulted in the availability of additional lectins. Dr. Moshe Rashi, working with colleagues at the Weizmann Institute, has developed and prepared a wide range of high purity lectins. He is actively interested in learning of your interest in our existing line of new products. For a comprehensive bibliography on lectins, a brochure on our available products or if you have specific interests in any particular application call or write to the office nearest you."[10]

Catalogues weren't static. They mutated with each new element being listed in them and as the manufacturers' power to produce new products grew. Presented as "NEW required reading," Waters Associates ("the Liquid Chromatography People," as they called themselves) illustrated their range of catalogues as literature that scientists needed to be up to date as much as the latest peer reviewed articles.[11]

In this constant mutation, catalogues did not only list products, they contained also prices and the services they offered. The firm The Radiochemical Centre Amersham presented the reading of their catalogues as a necessary requirement for researchers' aims to meet the material and economic realities (figure 3, top left). Their header advertised, "How to make the most of your radiochemicals budget," and the rest of their advertisement for their catalog centred around the financial aspects of research materials: "concentrate first on value for money — exactly the product you need," "having the product delivered precisely when you need it means that there are no costly delays in your work programme," "there is no extra charge but again it can save you both time

[9] Martin Thomson, 'Trade literature: a review and survey,' British Library, Science Reference Library, 1977. p. 10-12 as cited in David King, 'Market Research Reports, House Journals and Trade Literature', *Aslib Proceedings* 34, no. 11 (November 1982): 466–72, https://doi.org/10.1108/eb050863.

[10] Miles Laboratories, 'Lectins', *Nature* 252, no. 5478 (1 November 1974): Cover 4.

[11] Waters Associated Inst. Ltd., 'NEW Required Reading', *Nature* 258, no. 5531 (13 November 1975): v.

and money," "it all adds up to unbeatable value for money."[12] The price of research could be lowered by comparing across vendors, and The Radiochemical Centre Amersham said so explicitly: "Now compare prices."[13]

Money was an issue, but the manufacturers' catalogues were a free entry to consumption. P-L Biochemicals advertised their catalogue as a partner for researchers. In the featured image, a male scientist dressed in a white lab-coat receives the catalogue from what appears to be the hand of a businessman (figure 3, top right). In the header, "Let us give you a complement: a copy of our new catalog."[14] They described it as containing "fine biochemical and valuable technical information" and assured the reader, "you can depend on P-L's superb quality and excellent service to satisfy you biochemical needs at competitive prices."[15]

The advertisement by P-L Biochemicals occupied an eight of a page. Some other manufacturers pressed harder and bought full pages. The Aldrich Chemical Company was one of them, and they were bold in their imagery and description of their catalogues. Their catalogues often contained paintings of the Dutch Golden Age, and in their 1973 edition they featured Jacob Adriaensz Backer's *Democritus and Hippocrates in Abderra*, which shows philosopher Democritus of Abdera sitting in front of a rock and writing in a book while Hippocrates watches him silently behind him (figure 3, bottom left).[16] With this early modern re-enactment of an ancient philosophical scene, the header of the 1973 *Aldrichimica Acta* (as the Aldrich catalogue was called), the header read, "We'll stack our new BIO-CHEMICAL CATALOG against all the others!" and kept on going strong in the body:

> In fact, we don't even call it a catalog. It's a biochemical handbook, packed with detailed data on nearly 4000 compounds. In it, you'll find handling precautions and disposal methods. Physical constants, structures, and purities. Current reference data, and complete pricing information. And speaking of prices: Aldrich Chemical Company's direct-to-you pricing policy invites comparison with any other supplier in the industry. [...] What's more, we promise *and deliver* off-

[12] The Radiochemical Centre Amersham, 'How to Make the Most out of Your Radiochemicals Budget', *Nature* 235, no. 5337 (11 February 1972): Cover 2.

[13] The Radiochemical Centre Amersham.

[14] P-L Biochemicals, 'Let Us Give You a Complement: A Copy of Our New Catalog', *Nature* 254, no. 5496 (13 March 1975): v.

[15] Ibid.

[16] I would like to thank Twitter user @georgianroses for in a fast and selfless internet encounter, having provided with the original sources of Aldrich's cover paintings. Volker Manuth et al., *Wisdom, Knowledge, Magic: The Image of the Scholar in Seventeenth-Century Dutch Art* (Kingston, Canada: Agnes Etherington Art Centre, Queen's University, 1997), 58–59, https://archive.org/details/wisdomknowledgemagic1996.


Cover 2
NATURE FEBRUARY 11 1972
How to make the most of your radiochemicals budget.
(1) Concentrate first on value for money – exactly the product you need, chosen from a top quality range offering the highest specific activities.
(2) Remember the importance of availability and reliable delivery – having the product delivered precisely when you need it means that there are no costly delays in your work programme.
(3) Consider the value of a comprehensive technical liaison service – there is no extra charge but again it can save you both time and money.
(4) Now compare prices. You will find that during a year when costs generally have moved upwards The Radiochemical Centre has succeeded in holding prices throughout the whole of their catalogue range. There are even price reductions in some areas.
(5) Finally, be sure you are on the mailing list for TRC's 1972 catalogue. Notice the number of new, labelled compounds, the comprehensive range of labelled nucleotides, the number of tritiated steroids of high specific activity, and the improved specifications for other compounds. It all adds up to unbeatable value for money. Copies of the catalogue are available.
Our products lead, Our service follows through.
The Radiochemical Centre Amersham
Radiochemicals
1972/3
The Radiochemical Centre Amersham England
Nature March 13 1975
The Centre for radiochemicals
Labelled synthetic corticosteroids at high specific activity
[1,2,4 (n)-³H] Betamethasone 30,000-50,000mCi/mmol TRK.484
[1,2 (n)-³H] Dexamethasone 20,000-40,000mCi/mmol TRK.417
[1,2,4 (n)-³H] Triamcinolone acetonide 30,000-50,000mCi/mmol TRK.485
Full details on request. Write for our complete listing of labelled nucleotides.
The Radiochemical Centre Amersham
Let us give you a complement: a copy of our new catalog
Let P-L Biochemicals new Catalog 104 for 1975 complement your work with its comprehensive listing of fine biochemicals and valuable technical information. Whatever your scientific discipline, if your work calls for the use of biochemicals, P-L's new catalog should be close at hand. You'll find a wide variety of natural and synthetic DNA polymers and RNA polymers plus enzymes, amino acids, carbohydrates, lipids, antibiotics, buffers and, of course, nucleotides and coenzymes. And it introduces a major new listing of affinity chromatography materials. You can depend on P-L's superb quality and excellent service to satisfy your biochemical needs at competitive prices. We invite you to compare — write P-L Biochemicals for your free catalog today.
P-L biochemicals, inc.
Only Iceland has what Iceland has.
Iceland has something special for everyone. It's a wonderland of geysers, volcanoes and waterfalls for the sightseer. It has rugged beautiful rides for the pony trekker. Fat salmon and trout for the angler. And a unique fascination for the ornithologist, geologist and historian.
ICELAND TOURIST INFORMATION BUREAU
Cover 4
NATURE FEBRUARY 16 1973
We'll stack our new BIO-CHEMICAL CATALOG against all the others!
In fact, we don't even call it a catalog. It's a biochemical handbook, packed with detailed data on nearly 4000 compounds. In it, you'll find handling precautions and disposal methods. Physical constants, structures, and purities. Current reference data, and complete pricing information.
And speaking of prices: Aldrich Chemical Company's direct-to-you pricing policy invites comparison with any other supplier in the industry. And that goes for our organic handbook as well (which lists 18,000 compounds, similarly codified).
What's more, we promise and deliver off-the-shelf capability on over 96% of all the chemicals we list!
For more chemists, there's no need for any other source. And now, biochemists can enjoy the same convenience and economy. No wonder our catalog's on top.
Send for Your FREE Copy
Write for the new Aldrich Handbook of Organic Chemicals listing over 18,000 chemicals
IN THE U.S.A.
Aldrich Chemical Company, Inc.
940 WEST ST. PAUL AVENUE MILWAUKEE, WISCONSIN 53233
IN GREAT BRITAIN
Ralph N. Emanuel Ltd.
264 Water Road Wembley, Middlesex HA0 1PY
and
ALDRICH-EUROPE B-2340 Beerse, Belgium
JUST PUBLISHED.
The Source
for more than 18,000 organic and biochemicals, their disposal methods, handling precautions, physical constants, and reference data. But our company is more than a catalog. We're the source for coded data on over 20,000 compounds through our Computer Search service. We're quality control specialists, insuring the purity of our compounds through critical batch analysis. We're responsive researchers, adding dozens of new intermediates to our inventory every month. We're technical publishers, providing thousands of IR and NMR spectra in a dozen bound volumes, and timely information in our quarterly Aldrichimica Acta. For thousands of chemists all over the world, Aldrich is far more than a catalog. Whether your need is technical or chemical, you can rely on Aldrich. The source.
I have not yet received my free copy of the latest Aldrich Catalog/Handbook. Please see that my name is on your mailing list.
Name
Company
Address
City, State Zip
Aldrich Chemical Company, Inc.
940 W. Saint Paul Ave., Milwaukee, WI 53233
Aldrich

Figure 3.

> the-shelf capability on over 96% of all the chemicals we list! For more chemists, there's no need for any other source. And now, biochemist can enjoy the *same* convenience and economy. No wonder our catalog's on top. Send for Your FREE Copy. Write for the new Aldrich Handbook of Organic Chemicals listing over 18,000 chemicals.[17]

The year later, and with 2,000 compounds added to their collection, Aldrich took an even bolder approach to the advertisement of their catalogue. In a larger header font, they wrote "The source." (figure 3, bottom right)[18] This time, the front cover of *Aldrichimica Acta* featured the artwork *The Alchemist and Death*, of yet another Dutch Golden Age artist, Thomas Wijck. In the painting, the scholar appears immersed in work in the laboratory, with a ceiling opening up to a godly realm of thunder and lightning as a young man kneels and prays in front of the Book of Revelation, and Death plays the trumpet.[19] With this image, Aldrich advertised itself as much more than a manufacturer:

> We're the source for coded data on over 20,000 compounds through our Computer Search service. We're quality control specialists, insuring the purity of our compounds through critical batch analysis. We're responsive researchers, adding dozens of new intermediates to our inventory every month. We're technical publishers, providing thousands of IR and NMR spectra in a dozen bound volumes, and timely information in our quarterly Aldrichmica Acta. For thousands of chemist all over the world, Aldrich is far more than a catalog. Whether your need is technical or chemical, you can rely on Aldrich. The source.[20]

Catalogues were thought of as "comprehensive bibliography" and "required reading", using names such as "handbook" and "the source" to illustrate its importance in research. These sorts of advertisements were the space that was bought by manufacturers in order to redirect readers to other spaces: their free catalogues, and from there, to buy the products listed in them. As this brief review of the marketing approaches of several manufacturers for placing catalogues in the hands of scientists shows, competition was fierce, and the marketing departments of these firms worked hard to attract the attention of *Nature*'s readers. Let us now take a look at precisely how they did that by looking at advertisements of the marketed objects themselves.

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[17] Aldrich Chemical Company, Inc., 'We'll Stack Our New BIO-CHEMICAL CATALOG against All the Others!', *Nature* 241, no. 5390 (16 February 1973): Cover 4.

[18] Aldrich Chemical Company, Inc., 'The Source', *Nature*, no. 249 (1974).

[19] Manuth et al., *Wisdom, Knowledge, Magic: The Image of the Scholar in Seventeenth-Century Dutch Art*, 88–89.

[20] Aldrich Chemical Company, Inc., 'The Source'.


Cover 2
Nature August 7 1975
OVER AND OVER AND OVER AND OVER AND OVER AGAIN
the continuous action of the Rotary Tumbler
A vertical rotation mixing machine, designed to mix fluids by a tumbling action around a horizontal axis and suitable for use in incubators or hot rooms, as well as at room temperature.
Mixing is effected by continual inversion of the containers at speeds continuously variable between 10 and 100 r.p.m., enabling the operator to select the optimum mixing action to suit individual requirements. A wide range of interchangeable fittings can be supplied to hold stoppered tubes from 9.5 mm dia. x 65 mm long to 41 mm dia. x 305 mm long.
All fittings are easy and quick to use, the clamps, securing the tubes, slide into position and are retained by two thumb nuts.
For full information please write or telephone
LUCKHAM LIMITED Labro Works, Victoria Gardens, Burgess Hill, Sussex RH15 9QN.
Telephone: Burgess Hill 5348 Telegraphic address: LABRO - BURGESS HILL Telex: 87323 FSI Code Luckham.

Figure 4.


The fire started on the first floor...
...worked its way to the second floor where my Marantz 2270 was, and finally engulfed the third floor. The floors collapsed and fell into the basement where the Marantz remained buried in debris and water until March when the wrecking company came.
While the men were lifting the debris into trucks I noticed a piece of equipment I thought could be the Marantz. I asked the man to drop the load, and the receiver fell 20 feet to the ground.
Out of sheer curiosity, I brought the damaged receiver up to my apartment and after attaching a new line cord to it, I plugged it in. All the blue lights turned on. I connected a headphone and the FM played perfectly. I then tested it with my tape deck, and finally the turntable and speakers. They all played perfectly, too.
Francisco Espina*
Newport, Rhode Island
Unretouched photograph.
Mr. Espina's Marantz 2270 receiver still meets factory specifications. We design all Marantz equipment to perform under extreme conditions for unmatched reliability year after year after year. Like the new Marantz 2275—even better than its incredible predecessor. See the complete line of Marantz receivers, components and speaker systems at your Marantz dealer. He's in the Yellow Pages.
Marantz. Almost indestructible.
marantz.
We sound better.
*Mr. Espina's notarized statement is on file with the Marantz Company. Marantz Co., Inc. guarantees the original registered owner that all parts are free from operating defects for three years from purchase date except tubes which are guaranteed for 90 days. Products are repaired or replaced free of charge during this period provided you bought them in the U.S.A. from an authorized dealer. Naturally the serial number cannot be altered or removed. © 1974 Marantz Co., Inc.
Valley, Calif. 91352. In Europe: Superscope Europe, S.A., Brussels, Belgium. Available in Canada. Prices and models subject
RIJKSUNIVERSITEIT UTRECHT
0015 2714

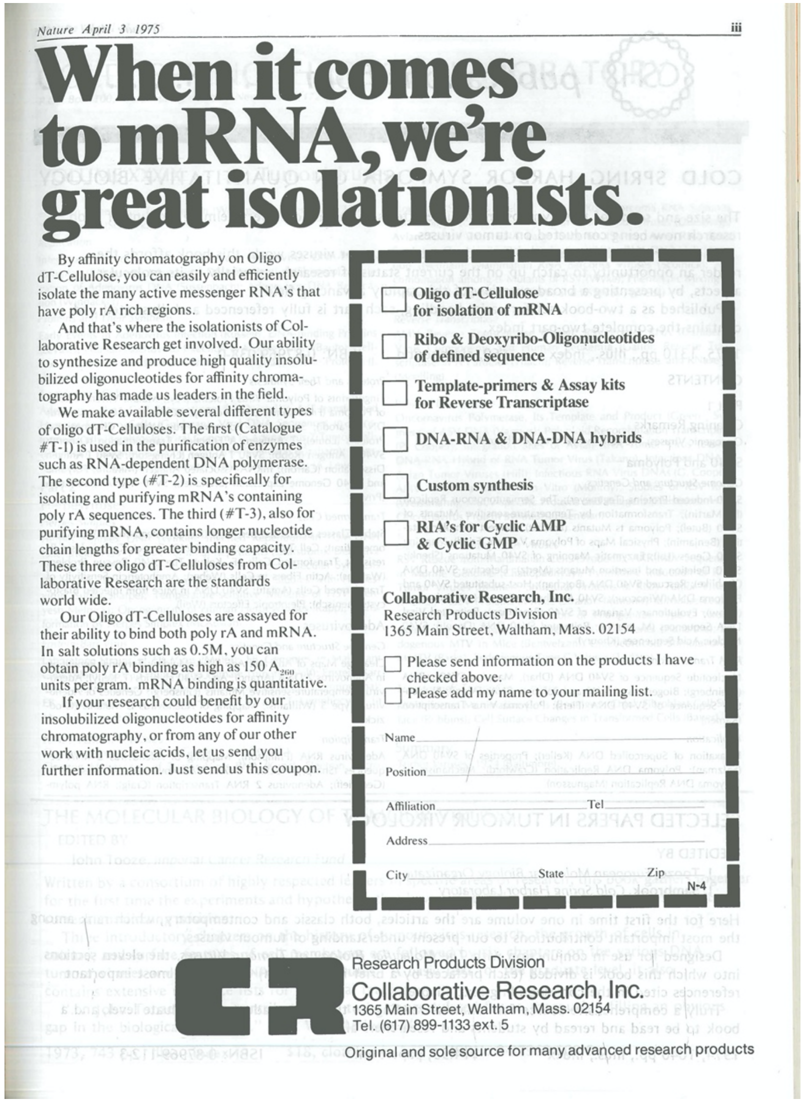
Nature April 3 1975
iii
When it comes to mRNA, we're great isolationists.
By affinity chromatography on Oligo dT-Cellulose, you can easily and efficiently isolate the many active messenger RNA's that have poly rA rich regions.
And that's where the isolationists of Collaborative Research get involved. Our ability to synthesize and produce high quality insolubilized oligonucleotides for affinity chromatography has made us leaders in the field.
We make available several different types of oligo dT-Celluloses. The first (Catalogue #T-1) is used in the purification of enzymes such as RNA-dependent DNA polymerase. The second type (#T-2) is specifically for isolating and purifying mRNA's containing poly rA sequences. The third (#T-3), also for purifying mRNA, contains longer nucleotide chain lengths for greater binding capacity. These three oligo dT-Celluloses from Collaborative Research are the standards world wide.
Our Oligo dT-Celluloses are assayed for their ability to bind both poly rA and mRNA. In salt solutions such as 0.5M, you can obtain poly rA binding as high as 150 A_{260} units per gram. mRNA binding is quantitative.
If your research could benefit by our insolubilized oligonucleotides for affinity chromatography, or from any of our other work with nucleic acids, let us send you further information. Just send us this coupon.
Oligo dT-Cellulose for isolation of mRNA
Ribo & Deoxyribo-Oligonucleotides of defined sequence
Template-primers & Assay kits for Reverse Transcriptase
DNA-RNA & DNA-DNA hybrids
Custom synthesis
RIA's for Cyclic AMP & Cyclic GMP
Collaborative Research, Inc.
Research Products Division
1365 Main Street, Waltham, Mass. 02154
Please send information on the products I have checked above.
Please add my name to your mailing list.
Name
Position
Affiliation
Tel
Address
City
State
Zip
N-4
Research Products Division
Collaborative Research, Inc.
1365 Main Street, Waltham, Mass. 02154
Tel. (617) 899-1133 ext. 5
Original and sole source for many advanced research products

Figure 5.


CUT IT OUT!
FOR ANY TYPE OF GRADIENT — that is all you have to do when you program the LKB ULTROGRAD® gradient mixer. A pair of scissors is all you need to cut the gradient profile for exactly the type of gradient you require. Our technician has just cut three, and he now indicates that he will use the one in the scanning window. When he has set the scanning rate and the duration of the run, he will switch on and the ULTROGRAD will take over—automatically producing the gradient. He can program any type of gradient you like to name, from as many as three liquids at once.
With an optional level sensor, you can also monitor absorbance levels in an eluate and automatically vary the gradient, to provide greater separation of eluted components.
LKB
IN THE SERVICE OF SCIENCE
LKB INSTRUMENTS LTD • LKB HOUSE • 232 ADDINGTON ROAD • S. CROYDON, SURREY. CR2 8YD • TEL: 01-657-0286 SALES AND SERVICE THROUGHOUT THE WORLD: STOCKHOLM, WASHINGTON, THE HAGUE, COPENHAGEN, ROME, VIENNA, PARIS

II. Bold: Marketing strategies

I have identified several formal trends among these advertisements: with a *simple* design, and with a good deal of *humour*, advertisements promoted *the new* as essential in science and technology, and encouraged the laying of *trust* on the manufacturers, and did so by using highly *technical and specialised* language which narrowed and precisely targeted research scientists. Most of the ads that I have seen from this period contain most of these components or a combination of them.

Most of the ads that appeared in *Nature* resemble in layout and style those of its contemporary popular science magazines, such as those seen in *Scientific American*. These kinds of ads are characterised by their simplicity. They are organised into three main sections: a header in bold type, an image occupying the majority of the space, and a longer descriptive text in a smaller type.[21] The header was large and highly descriptive, as was bold and assertive. The image tended to be a photograph showcasing the product. This happened in *Nature*'s ad for a rotary tumbler, as in *Scientific American*'s ad for a radio receiver (figure 4).[22]

This simple three-part design was accompanied by slick and humorous content. Comedy within the ads was commonplace for some manufacturers. *Nature*'s advertisements had sophisticated puns that alluded to the in-jokes of the scientific class, and referred to scientific and politic topics, or a combination of both.

On the innocent side of humour, readers may have found ads with strictly scientific puns, like those of Pharmacia Fine Chemicals. This manufacturer had recurring advertisements placed on the expensive inside covers and running through virtually all 1975 issues, announcing products for the isolation of mRNA and proteins, with some fine onomatopoeia-themed jokes. In one, the header read: "——————SH!!," which a non-molecular biologist may have read as the representation of someone asking for silence, but which its symbol precisely maps onto the function of their product.[23] Biochemists would have easily recognized that '–SH' stands for the thiol chemical group, which is the target to be "isolate[d] and protect[ed]" by the product to be sold, the Activated Thiol-Sepharose® 4B. Another product that Pharmacia Fine Chemicals announced was a

chromatography method used to separate and identify mRNA molecules of interest. The graphic representation of two nucleobases of mRNA, uracil (U) and adenine

[21] I would like to thank Daan Wegener for an improvement of this observation.

[22] Luckham Limited, 'Over and Over and Over and Over and Over Again', *Nature* 256, no. 5517 (7 August 1975): Cover 2; Marantz, 'The Fire Started on the First Floor…', *Scientific American* 232, no. 1 (January 1975).

[23] Pharmacia Fine Chemicals, '——————SH!!', *Nature* 258, no. 5531 (13 November 1975): Cover 3.

(A), whose complementarity as base pairs serves as the mechanism of isolation served also as onomatopoeia of surprise .[24]

Some manufacturers used more daring humour than these elitist onomatopoeias. The firm Collaborative Research, who also sold isolation solutions, run often a politically themed advertisement through the late 1970s. As the Cold War became calmer, humour started to slip through the cracks of the Iron Curtain, and jokes referred both to science and the political environment (figure 5, left). In 1975, the biotech company must have had a creative biologist working in the marketing department. In large letters, occupying a considerable part of the one-page ad, the beholder could read, "When it comes to mRNA, we're great isolationists."[25] This double-entendre, a topical ambiguous reference to isolationism as the international policy of the USA and the USSR can be understood as part of a marketing strategy with its own history. Since the scientific practice and its products began to be commercialised during the late nineteenth-century, businessmen have used the idea that 'controversy makes profit', as they saw that linkage to politically-charged events drove costumers.[26] And indeed, it is not by chance that this 'isolationism' ad ran in a period when virtually all *Nature*'s issues reported at least once on news from the USSR. Political pieces were highly critical of Soviet politics, and displayed sympathy with their scientific Russian comrades, forcedly isolated on the other side of the wall.[27]

Another advertisement which toyed with ambiguity to sensitive topics was LKB Instrument's piece announcing their Ultrograd gradient mixer.[28] The vagueness of the advertisement increases as the analysis deepens, but at first glance, its meaning appears evidently clear. Please, before I comment on it, take a look at the advertisement itself in Figure 5 (right) if you haven't already done so. Dedicate just a few moments and do not try to read the description, as we are trying to mimic here casual browsing through a magazine. What do you see?

What does "CUT IT OUT!" refer to? In the context of the Ultrograd gradient mixer, it refers to the action that is needed to operate the machine: "A pair of scissors is all you need to cut the gradient profile for exactly the type of gradient you require. Our

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[24] Pharmacia Fine Chemicals, 'UUUUUUUUUUUAAAAAAAAAAA', *Nature* 258, no. 5537 (25 December 1975): Cover 2.

[25] Collaborative Research, Inc., 'When It Comes to MRNA, We're Great Isolationists', *Nature* 254, no. 5499 (3 April 1975): iii.

[26] Sadiah Qureshi, 'Meeting the Zulus: Displayed Peoples and the Shows of London, 1853-79', in *Popular Exhibitions, Science and Showmanship, 1840-1910* (Pittsburgh: University of Pittsburgh Press, 2016), 183–98.

[27] Instances of this are Vera Rich, 'Russia Today', *Nature* 254, no. 5497 (1 March 1975): 173–74, https://doi.org/10.1038/254173a0; Vera Rich, 'Coping with an Exodus', *Nature* 253, no. 5490 (1 January 1975): 296–97, https://doi.org/10.1038/253296a0; Mark Azbel et al., 'Soviet Jews', *Nature* 254, no. 5502 (April 1975): 650–650, https://doi.org/10.1038/254650a0.

[28] LKB Instruments, 'Cut It Out!', *Nature* 235 (1972).

technician has just cut three, and he now indicates that he will use the one in the scanning window. When he has set the scanning rate and the duration of the run, he will switch on and the ULTROGRAD will take over—automatically producing the gradient."[29] As the description indicates, this analogue machine required the cutting out of these 'gradient profiles.' The description is also of interest to understand the two figures in the photograph and the action that has just taken place. The description talks about a technician, a male, which supposedly refers to the man in the left-side of the image holding the scissors. Now, what motion is the woman on the right carrying out? And how does it relate to the header, "CUT IT OUT!"?

The woman appears invisible in the description, but I would argue that she is the one who utters the words in the header. With the direction of her movement facing outside of the frame of the photograph as if she was walking out of the scene, and with her sight locked in on the scissors, she might have yelled "CUT IT OUT!". I say yelled because of the bold and capitalised type ending with an exclamation mark. 'Cut it out' is an expression used in its imperative form and uttered when the speaker attempts to make someone else to stop doing something. The phrase is often used in reference to sexual misconduct and inappropriate sexual advances at work. Would she have yelled "CUT IT OUT!" as the sitting man held scissors at the level of her groin? In this view, it seems as though a first glimpse of this ad, combined with the scene depicted in a photograph and the small-typed description, may have been used by LKB Instruments to analytically ambiguously yet clear in a first impression, as a reference to sexual misconduct in the workplace. Everything is possible, "in the service of science."[30]

Humour and controversy went a long way for manufactures to market their products. As they prided on delivering the latest technological developments, 'new' was a ubiquitous word in the world of scientific advertisement. Bolded, capitalised, and underlined, selling 'the new' was vital to sell technologies. Advertisements like the Swiss Wild Heerbrugg wild M11 microscope advertised a "new concept in microscopy", promising "perceptual precision."[31] Similarly, Miles Laboratories had a fascination with the word, as it occupied the largest font of their advertisements, be it for selling products for affinity chromatography, ferritin or restriction nucleases (figure 6).[32]

Selling 'the new' went well with selling 'the best'. The manufactures sold products that were central to the scientists' daily life and designed their ads to engage them emotionally. Phrases like 'Isn't Your Work Too Important … For Anything But the

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[29] LKB Instruments.

[30] Ibid.

[31] Wild Heerbrugg, 'New Concept in Microscopy', *Nature* 254, no. 5501 (17 April 1975): Cover 2.

[32] Miles Laboratories, 'Lectins'; Miles Laboratories, 'New from Miles for Affinity Chromatography', *Nature* 258, no. 5531 (13 November 1975): vi; Miles Laboratories, 'New from Miles. Ferritin. Cationized Ferritin. Antisera to Human Enzymes', *Nature* 255, no. 5509 (12 June 1975): Cover 3.


Cover 2
Nature October 11 1974
THE NEW SUSPENSION MIXER
SM/RT10/2
The new improved Suspension Mixer provides a unique tilting-rotary action, with a variable angle of tilt from 1°–15° from the horizontal. Operation is continuous, with a variable speed range from 10–100 r.p.m. A timer up to 60 minutes and audible warning can be incorporated as an optional extra.
The principal applications are in the controlled and standardised mixing of various types of suspensions, for example—the Coombes, V.D.R.L., and many other serological and related techniques.
The variable tilting agitation with compound action for agglutination, extraction, washing, staining and photographic developments. This unit is capable of regular and continuous operation in an incubator or incubation room. It is also used in many areas of industry—pure research and Quality Control departments.
The Suspension Mixer will accommodate any one of the 'Rotatest' carrying plates and the GWB/F22 viewing lamp.
For full information, please write or telephone:—
LUCKHAM LIMITED Labro Works, Victoria Gardens, Burgess Hill, Sussex, England RH15 9QN
Telephone: Burgess Hill 5348. Telegraphic address: LABRO-BURGESS HILL. Telex: 87323 FS1 Brighton. Code: Luckham
vi
Nature March 13 1975
NEW FROM MILES LABORATORIES, INC.
CHOLESTEROL ESTER HYDROLASE
The enzymatic determination of serum cholesterol has recently been reported (1,2). This method involves the hydrolysis of cholesterol esters to free cholesterol and fatty acids. Cholesterol is then oxidized to cholest-4-en-3-one with the production of hydrogen peroxide. Hydrogen peroxide production is determined enzymatically and followed spectrophotometrically. Cholesterol ester hydrolase and cholesterol oxidase are crucial elements of this system.
Miles Research Products is pleased to offer cholesterol ester hydrolase extracted from bovine pancreas by a method developed in our laboratories. Our preparation has a CEH activity of approximately 0.2 units per milligram of material. It is supplied as a freeze-dried powder containing about 45% phosphate buffer salts and about 20% sodium cholate. The residual proteolytic enzyme content has been specifically reduced to yield a stable product. Further information and assay details are available on request.
Code No. | Pkg. size | Price
36-129-2 | 100 un | $ 30.00
36-129-3 | 1000 un | 200.00
36-129-4 | 5000 un | 800.00
1) Allain, et al.,(1974), Clinical Chemistry, 20, 470.
2) Tarbutton and Gunter, (1974), Clinical Chemistry, 20, 724.
RESEARCH PRODUCTS
MILES
Research Products
Miles Laboratories, Inc.
Elkhart, Indiana 46514
Phone: 219-264-8804
Miles Laboratories, Ltd.
Post Office Box 37, Stoke Poges
Slough, England SL 2 4 LY
NEW FROM MILES LABORATORIES, INC.
RESTRICTION NUCLEASES: HIND III HPA II
These two new restriction nucleases from Haemophilus are the latest to join our growing list of Reagents for Genetic Mapping and Hybridization. These enzymes have no detectable exonuclease III activity against sonicated ³H-DNA.
The Hind III is purified free of Hind II based on gel electrophoresis of the digested λDNA products: Hind II would cause the presence of many additional bands.
Hpa II also cleaves λDNA into many pieces.
For more information about these nucleases, our other Reagents for Genetic Mapping and Hybridization, and our other new biochemical products, call or write the office nearest you and ask for the New Biochemical Supplement 1275.
Description | Code No. | Pkg. Size | Price
Hind III | 39-628-1 | 500 units | $55.00
Hpa II | 39-629-1 | 50 units | $50.00
RESEARCH PRODUCTS
MILES
Research Products
Miles Laboratories, Inc.
Elkhart, Indiana 46514
Phone: 219-264-8804
Miles Laboratories, Ltd.
Post Office Box 37, Stoke Poges
Slough, England SL 2 4 LY
Nature November 13 1975
v
NEW required reading
from Waters — the Liquid Chromatography People
Separation and Identification of Nucleic Acid Constituents
4 pgs. Describes the LC separation of DNA and RNA constituents — purine and pyrimidine bases, nucleosides, and nucleotides. Ask for AN 147.
Drug Levels in Plasma
Drug Levels in Plasma
4 pgs. Step-by-step description of the development of an LC method for clinical assay. Comparison of LC and GC assay results. Ask for AN 160.
Rapidly Determine Carbaryl in Pesticide Formulations
This 4-page brochure describes an LC method for the separation and quantitation of carbaryl, both alone and in mixed formulations. LC results are favorably compared with previously used analytical techniques. Ask for AN 140.
free from
WATERS ASSOCIATES (INST.) LTD.
324 Chester Road Hartford
Northwich Cheshire CW8 2AH England
or WATERS ASSOCIATES
205 Maple St., Milford, MA USA 01757
The Liquid Chromatography People
Circle No. 06 on Reader Enquiry Form.
The Centre for radiochemicals
Cyclic nucleotides:
improve the sensitivity of your assays with these super-hot compounds
[2,8-³H] adenosine 3',5'-cyclic phosphate TRK.498
current batch at 36 Ci/mmol
[8-³H] guanosine 3',5'-cyclic phosphate TRK.392
current batch at 19 Ci/mmol
Full details on request.
The Radiochemical Centre Amersham
The Radiochemical Centre Limited, Amersham, England.
Tel: Little Chalfont (024 04) 4444
In the Americas: Amersham/Searle Corp, Illinois 60005. Tel: 312-593-6300.
In W.Germany: Amersham Buchler GmbH & Co, KG, Braunschweig.
Tel: 05307-4693-97
0442
Circle No. 05 on Reader Enquiry Form.
vi
Nature November 13 1975
NEW FROM MILES LABORATORIES, INC.
FOR AFFINITY CHROMATOGRAPHY
HYDROPHOBIC CHROMATOGRAPHY Kits
In 1972 Shaltiel¹ developed a new chromatographic method of protein separation based on solute-matrix interactions. A family of supports with minor structural variations but significantly different chromatographic properties allows for selective separation. A kit format offers a rapid and simple procedure for selection of the appropriate gel.
Kit I. Alkyl-Agaroses
The kit consists of six pre-packed columns containing 1 ml of an alkyl agarose of the general formula Agarose—(CH₂)ₙ—H, where n = 0, 2, 4, 6, 8 and 10.
Kit II. ω-Amino Alkyl Agaroses
The kit consists of six pre-packed columns containing 1 ml of an ω-amino alkyl agarose of the general formula Agarose—(CH₂)ₙ—NH₂ where n = 0, 2, 4, 6, 8 and 10.
Use of these kits facilitates rapid selection of the most appropriate gel for your system. Each kit contains fully detailed instructions for use. The columns can be re-used continuously. The individual alkyl-agaroses and ω-amino-agaroses are available in 20 and 50 ml packs.
Description | Code No. | Pkg. Size | Price
Hydrophobic Chromatography Kit I. | 62-060-1 | 1 Kit | £7.80
Hydrophobic Chromatography Kit II. | 62-085-1 | 1 Kit | £7.80
Each kit contains 6 columns.
Individual components are available in larger quantities.
¹Er-el, Z., Zaidenzaig, Y., and Shaltiel, S. (1972), Biochem. Biophys. Res. Comm., 49, 383.
SPACER GELS Multivalent Hydrophilic Spacer Gels
In 1973 Wilchek described the preparation and use of poly-L-lysine agarose and poly-DL-alanine poly-L-lysine agarose derivatives. These gels offer the advantages of introducing a multi-point attached hydrophilic spacer arm. Thus, spacer-ligand leaching is reduced and non-specific interactions with the gel minimized.
1. POLY-L-LYSINE-AGAROSE (PL-AGAROSE)
Has free amino groups for coupling to ligands containing carboxylic groups.
2. SUCCINYL-PL-AGAROSE (SPL-AGAROSE)
Has free carboxyl groups to enable coupling to ligands containing free amino groups.
3. POLY-DL-ALANINE POLY-L-LYSINE AGAROSE (PAL-AGAROSE)
Multivalent, hydrophilic spacer gel with amino groups far removed from the matrix.
4. SUCCINYL-POLY-DL-ALANINE-POLY-L-LYSINE AGAROSE (SPAL-AGAROSE)
Carboxyl derivative for coupling to ligands containing free amino groups.
Description | Code No. | Pkg. Size | Price
PL-AGAROSE | 62-070-1 | *50 ml | £21.00
SPL-AGAROSE | 62-071-1 | *50 ml | £28.25
PAL-AGAROSE | 62-072-1 | *50 ml | £45.20
SPAL-AGAROSE | 62-073-1 | *50 ml | £52.00
*Settled gel volume
SPACER GELS Charge-Free Spacer Gels
The preparation of hydrazide agarose by a one step procedure was described by Lamed, Levin and Wilchek in 1973. This gel offers an amine-containing spacer support that is free of charges at neutral pH. Although particularly suited to nucleotide immobilization, it can be further derivatized to increase its versatility. The importance of using charge-free spacer gels in affinity chromatography has recently been discussed by Jost et al. who showed that proteins which bind to butyl-agarose do not bind to butyl hydrazide agarose.
1. ADIPIC-HYDRAZIDE-AGAROSE
Has hydrazide groups on a C₆ spacer for coupling of ligands containing free carboxylic groups or insolubilization of periodate oxidized nucleotides.
2. SUCCINYL ADIPIC HYDRAZIDE AGAROSE
Succinyl derivative with carboxylic groups for coupling to ligands containing free amino groups.
Description | Code No. | Pkg. Size | Price
Adipic Hydrazide Agarose | 62-074-1 | *50 ml | £22.80
Succinyl Adipic Hydrazide Agarose | 62-075-1 | *50 ml | £30.20
*Settled gel volume
for complete descriptions and bibliography, call or write the office nearest you and ask for New 1975 Enzyme Catalog.
RESEARCH PRODUCTS
MILES
Miles Laboratories, Ltd.
Post Office Box 37, Stoke Poges
Slough, England SL 2 4 LY
Phone: Farnham Common 2151
Research Products
Miles Laboratories, Inc.
Elkhart, Indiana 46514
Phone: 219-264-8804
Circle No. 04 on Reader Enquiry Form.

Figure 6.

Best?' are not far from emotional blackmail (appendix A). The manufacturer of this product and this marketing approach was Forma Scientific, and *they* offered salvation: "product safety is our prime consideration."[33] The fear of unfrozen samples wouldn't be a worry for those who would buy from them: "When Forma Scientific builds a biological storage freezer, we start with a heavy-duty steel cabinet bonded to an embossed aluminium liner with a minimum 13 cm foamed-in-place urethane insulation. [...] The dependable safety alarm system with life-time self-charging nickel cadmium batteries is standard."[34] Who wouldn't want anything but the best?

To sell 'the best' to scientists, manufacturers had to establish their credibility in the field. The British radiopharmaceutical company The Radiochemical Centre Amersham was a well-known player in the field, and asserted this fame with confident headers: "Don't take a chance on labelled compounds. Make sure the label's ours" (appendix B).[35] Their advertisements were simple and put a large emphasis on their experience as manufacturers. They placed themselves among the scientific community and their specialist expertise: "we rely on our customers, our technical representatives and our own scientists, to tell us which compounds are needed for new scientific investigations."[36] Amersham's presence in *Nature* in the 1970s was widely felt: they bought advertisement space in almost every issue.

In order to gain a scientist's trust, manufactures had to do better than making engaging jokes, selling 'the new', and promising 'the best'. Ads had to speak the language of the laboratory. The ads of *Nature* were unlike any ad that you could find in *Scientific American*: they were highly technical and specialised and were narrowly targeted to the defined range of scientists who used their techniques. For example, a one-page ad for reverse transcriptase enzymes made by P-L Biochemicals could only be read and understood by a molecular scientist — any physicist or geologist reading the magazine would have just skimmed past after seeing the many chemical combinations that could be used with the manufacturer's enzymes (appendix C).[37] In a more pronounced way, this ad established its scientific validity by citing a paper (published on *Nature*'s competitor, *Science*) alluding to the consensual and open nature of the knowledge behind its technology. In this similar austere and technical style, we find Aldrich Chemical Company's ad for 3-isobutyl-1methylxanthine, which used the bare

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[33] Forma Scientific, 'Isn't Your Work Too Important... For Anything But the Best', *Nature* 258, no. 5535 (17 December 1975): v.

[34] Ibid.

[35] The Radiochemical Centre Amersham, 'Don't Take a Chance on Labelled Compounds', *Nature* 258, no. 5530 (6 November 1975): xxi.

[36] The Radiochemical Centre Amersham, 'We're Old Hands at New Labelled Compounds', *Nature* 258, no. 5532 (20 November 1975): Cover 2.

[37] P-L Biochemicals, 'Reagents for the Unambiguous Assay of REVERSE TRANSCRITPASE', *Nature* 258, no. 5531 (13 November 1975): viii.

minimal: the chemical structure and formula, its mechanism of action, citation to seven scientific publications, and the price of Aldrich's products. Only those who used it knew that it was indeed "a potent phosphodiesterase inhibitor" and what it was used for.[38]

As exemplified until now, the scientific advertisement could be seen as a piece of advice, an advertised deal, the most rational in the category of marketing objects. They contained information that only a few selected could read and understand. As well, many advertisements had the added function of a fillable and mailable form.

However, could scientific advertisements be thought of as something else? Until now, I've presented the functional and rational side of these ads, and even their seemingly frivolous and commercial nature, product of witty marketing departments. These are two functional ways in which to think of advertisements: for being able to translate material needs into informed decisions, and for carrying messages meant to incite consumption. But aside these *intended* functions, could we think of them as something else and still being worth of historical analysis?

The lack of attention that scientific advertisements have received in the literature in the history of science could indicate that they have not been considered as fruitful sources. But what happens when we take these visual cues seriously? What context must be brought in to understand them? In the next section I begin to take this idea forward and explore different components (form and content) of the advertisements which allude to notions which are external to them, and which necessitate an understanding of the context of the historical moment in which the ads were printed to be able to place a meaning.

This is a first sketch, in which I will attend to the epistemic virtues that the content of the ads referred to. As I mentioned in the introduction, historical virtue epistemology is a productive way of thinking about scientific research as practices whose behaviours and ways of thinking are ethically codified, and which prescribe certain way of being – certain scientific self. In the upcoming section I attend to the epistemic virtues that the manufacturers of objects highlighted of their products. The toughest point of criticism about this approach would be to ask whether the epistemic virtues as presented in advertisements have anything to do with real researchers and the actual scientific community. To know whether the marketed epistemic virtues coincides with the virtues of scientists themselves (which I can already advance already that it does) I will attend in the next chapters to the virtues that were lauded in the political pieces of *Nature*. For now, let us look at the image of a virtuous scientist that commercial companies were trying to create.

[38] Aldrich Chemical Company, Inc., '3-Isobutyl-1-Methylxanthine', *Nature* 253, no. 5491 (6 February 1975): Cover 4.

III. Underlined: Commercial epistemic virtues

In the mid-to-late twentieth century, the microscope was still one of the elementary machines of molecular biologists. While many ads were for biochemical products and enzymes, the microscope as the symbol of laboratory science still appeared strong in 1970s advertisements. Upgraded from the optical microscope, aberration-corrected electron microscopes and electron microscopes were commercialised since the 1940s, and throughout the mid-twentieth century increasingly appeared as vital elements for molecular research. By 1979, the technical complexity of the Philips scanning electron microscope was summarised in their header: "Seeing is believing."[39] Achieving knowledge through visual observation was the main epistemically privileged way of research that Philips marketed. But in its ad for the electron microscope, it associated this trust for the visual with several other epistemic virtues:

> The striking realism of greatly magnified images is the key feature of the results obtained by means of the Scanning Electron Microscope (SEM) – The latest development in electron microscopy. By scanning a three-dimensional surface area of a specimen, this microscope produces images with great depth of field. By increasing or decreasing the power use, it scans minute details or entire areas in much the same way a zoom lens magnifies. And since almost all functions of the Scanning Electron Microscope are automated, even untrained operators obtained results which are easily interpretable and meaningful to the research worker. Fast. [...] The optional ability to visually display the elemental composition of the sample through analysis of X-Rays really confirms that seeing is believing. This is only one way in which Philips' advanced technology is being turned to practical use.[40]

The realism of visual representations (SEM-rendered images, x-rays) to which Philips adhered to, positioned the expertise of the researcher as secondary. In fact, it advocated for a transfer of trust onto "Philips' advanced technology," leaving the scientist to simply admire the almost full automaticity of the machine. Did the ease of manipulation and interpretation, and the speed at which the Philips' technology delivered meaningful results leave scientists indeed as "untrained operators"?[41] Let us return to this question of manipulation and expertise shortly.

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[39] Philips, 'Seeing Is Believing', *Nature* 282, no. 5737 (22 November 1979): iv–v.

[40] Ibid.

[41] Ibid.


IF YOU NEED A NEW ELECTRON MICROSCOPE-
WHY PURCHASE AN OLD ONE ?
When our designers introduced the EM 300 in 1966 they where convinced that its innovations would set the standards others would follow. They were right - as a study of current instruments quickly demonstrates.
Now, with the introduction of the EM 400, they've set the new standards required - now and in the 1980's - to produce better results faster from all kinds of specimens. Just look at some of the innovations which help to achieve this:
● Specimen protection is assured by a 10^{-7} torr vacuum from an ion pump
● Optical versatility is extended by a new 7-lens optical system
● Image distortions are automatically corrected by computer-type lens programming
● Tedious routine operations are automated - so you can concentrate exclusively on the specimen
● Unprecedented stabilities - even in 'difficult' sites - are ensured by new magnetic, mechanical and thermal designs
● Accessories are integrated into a coherent system which optimises the instrument as a base for extended investigations
● The most advanced techniques have been used to facilitate the incorporation of new accessorie enabling the EM 400 to grow int the future.
To get the full story, ask for the EM 400 brochure. You'll be radically surprised.
Philips Industries
Electron Optics Department
Building TQ III-4
Eindhoven, The Netherlands
In U.K.; Pye Unicam Ltd, Sales Promotion Dept, York Street, Cambridge. CBl2PX
VACUUM STATUS
HV2
AIRLOCKS
VACUUM SYSTEM
CAM
GUN
AIR
OFF
ON
FUNCTION SELECTOR
DIFFR FOCUS
MAGNIFICATION
ALIGNMENT CAM LENGTH
BEAM TILT
DEFL
PHILIPS

Figure 7.

Philips' budget for advertising must have been large. Many of the ads, like the one of the SEM microscope, occupied two pages. Watson and Crick's *Nature* 1953 article on the presentation of the structure of DNA also occupied that space. In half that amount in 1975, Philips asked half a question: "IF YOU NEED A NEW MICROSCOPE" (figure 7). As the person is holding their magazine, the eyes move towards the continuation of the sentence on the opposite page: "WHY PURCHASE AND OLD ONE?"[42] The motto is clear, as we have seen before, the new sells, and it renders the old obsolete.

Before we dive into the text describing the EM 300, let me recognize the presentation of the model in the photograph. After all, it occupies over two-thirds of the two pages. It is the first glimpse that the reader of *Nature* would have recognized. We can observe several things in this photograph, especially how it is presented to the person holding the magazine. The angle at which the snapshot is taken renders an almost horizontal line that traverses the two pages, a sort horizon line. This line, slightly elevated on the right side, escapes towards the left, and has positioned the onlooker just above the machine, in a perspective that renders the right side of it larger than the left side of it. In fact, one would have to be very close to admire the EM 300 in such detail and immensity. With this perspective, the beholder is inserted right into the machine, presented with a close up of the many buttons controlling the central shaft of vision, which the operator of the microscope would operate and in front of which he would sit. *He*, because the operator is not invisible. He is represented in the top right insert on the right page. Before we move to him, let me recognize that the main photograph presented that I have just commented upon, that of the forced perspective, would be foreign to any scientist who would operate the EM 300. If you would look at the EM 300, you would not squat to observe the microscope from that distance. This forced perspective that renders buttons and the shiny surfaces of the microscope larger and in first line of sight, is appropriated from sci-fi movies – *2001: A Space Odyssey* (1968) comes to mind.

Let me now go forward to the man, the silent operator, sited and giving us the back. Here he looks through the lenses, concentrated upon the specimen. This insert is, in fact, a presentation of the EM 300 that most human scientists could relate to in their daily perception of laboratory objects. If one would walk into a room, one would never see the forced perspective that appears as the main visual object presented in the advertisement. A 1970s scientist would, in fact, be more accustomed to a vision alike to the top right insert. As the scientist opens the door, he would find his lab partner sitting in front of the central shaft of vision of the EM 300, concentrated, with his back to him.

The concentration of the man in the insert exemplifies the use to be given to the machine. He gives his back because the most important is in front of him. His pose,

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[42] Philips, 'If You Need a New Microscope - Why Purchase an Old One?', *Nature* 258, no. 5532 (20 November 1975): vi–viii.

because *he is posing*, is that of a busy man carrying out technical manipulations with minute research objects and innovative technology. The figure of this posing man relates and is narrated in the secondary part of the advertisement. Secondary, yet informative and of special interest for me to illustrate how the EM 300 was understood by Philips, and specially, how Philips intended to present it to scientists browsing *Nature*. There are two passages I'd like to remark. First, the first words say "When our designers introduced the EM 300 in 1966 they were convinced that its innovations would set the standards others would follow. They were right – as a study of current instruments quickly demonstrates."[43] These phrases showcase the way competition in the biotech industry was illustrated and resolved: innovation, addition, shared standards. Innovations were sold as raising the standard, a standard that *had to be shared* among competitors to stay afloat. Innovation was always to be expected, and objects needed a progressive attitude: "The most advanced techniques have been used to facilitate the incorporation of new accessories enabling the EM [300] to grow in the future."[44] However, innovation on its own isn't enough. It needs to be adopted by other members of the community, including scientists, to form part of the social consensus on the technology's capabilities. As such, the marketing department made sure to include that their competitors were following closely behind the EM 300.

The Radiochemical Centre Amersham also prided itself of shared infrastructures. As an international company, they claimed to offer the same products through several countries and yet a local service. Their two-page ad, 'The Radiochemical Centres,' featured a representation of half the Earth's globe, showing the northern hemisphere. Indicating all the centres where they held stock, they wrote:

> We concentrate on being a truly international operation; our two associate companies and 30 distributors form the largest and most widespread radiochemical network in the world. [...] Our customer service teams always provide prompt technical advice, and our most experienced scientists will often visit your laboratories to discuss particular problems in the use of radiochemicals.[45]

Why is there a focus on the international extension of a single company? Why would that be important for a local scientist? The making of standard techniques, and thus of universal agreed-upon products with agreed-upon stable characteristics is a process in itself, which can be globally referred to as reification. For a scientist to know that the

[43] Ibid.

[44] Ibid.

[45] The Radiochemical Centre Amersham, 'The Radiochemical Centres', *Nature* 251, no. 5472 (20 September 1974): vi–viii.

stability of his product shared across the world was an increasing demand of experimental researchers in the 1970s. During this decade, the efforts of standardisation were increasingly implemented, with different sets of actors being highly important, among which private companies and scientific societies played a vital part. Historians Alberto Cambrosio and Peter Keating have shown this in the case of the standardization of monoclonal antibodies during the late 1970s.[46] They have illustrated how standardization was constructed through the establishment of shared infrastructures, and the adoption of common terminology and techniques, leading to the reification of laboratory materials. In the cases here —that of EM 300 with Philips' innovations which set "standards others would follow," and that of The Radiochemical Centre Amersham's visual display of "the largest and most widespread radiochemical network"— we can observe how the efforts of standardisation were marketed, and how consensual standards were shared not only within firms, but across competitors. In this way, reification of laboratory materials was established.

Innovation was not the only key discursive element to sell laboratory devices. Another one that surfaced often in 1970s advertisements and that I would like to explore in more detail, is the ability of technical objects to make the life of the scientist *easier*. The apparatus made tedious manipulation of molecular objects comfortable. For example, the EM 300 was designed with the activity of the scientist in mind: "Tedious routine operations are automated – so you can concentrate exclusively on the specimen."[47] Designing and projecting the microscope as easy-to-use was not uniquely Philips' concern. In fact, within the microscopy trade, comfort was a defining feature of the devices. The opening sentence for Olympus' optic microscope LHA was "The microscope that works for you in multiple ways" (figure 8, left).[48] The modularity of the microscope made it adaptable to the needs of the microscope: lenses could be interchanged, magnification could "jump right from 28x to 2000x," and plan achromatic objectives guaranteed "color and spherical aberration and field curvature almost non-existent."[49] Olympus' advertisement for the LHA made it clear that manipulation of the microscope was necessary, but had to be easy. Now, it's important to demarcate that the comfort that the device would concede did not mean that expertise wasn't necessary. Quite the contrary, even to *understand the advantages* of the LHA one needed to be knowledgeable about optics. To understand how to read the exposure meter or set the

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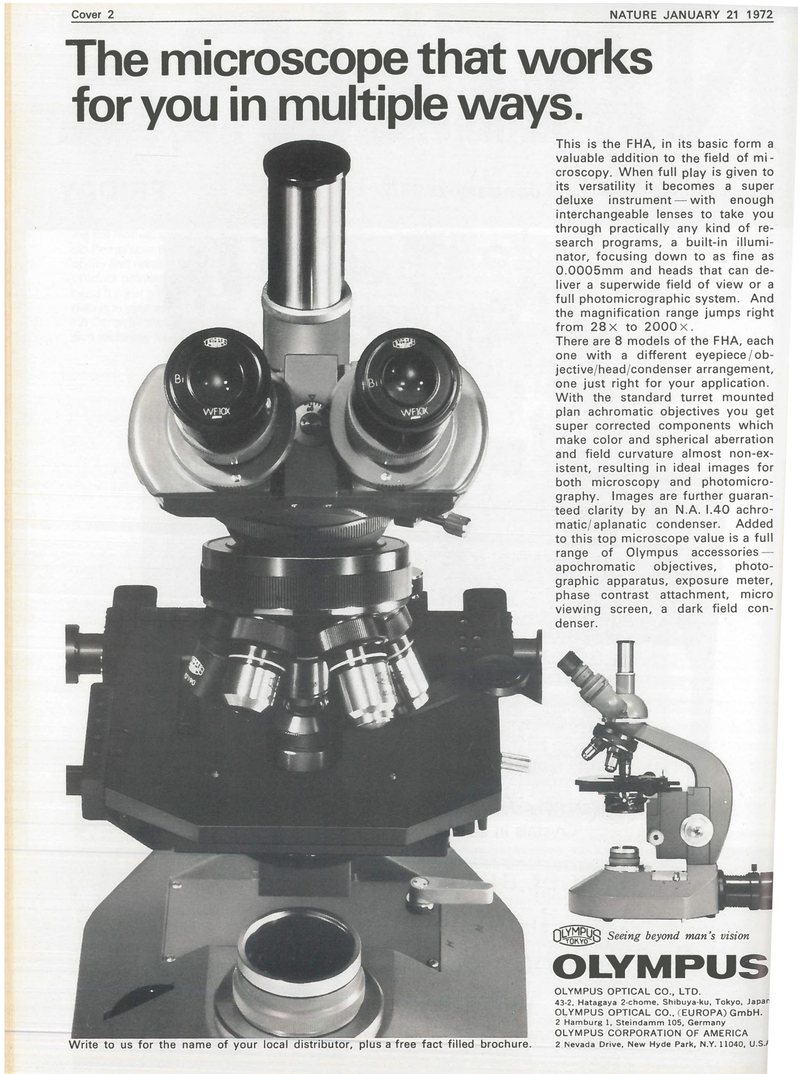
[46] Alberto Cambrosio and Peter Keating, 'Monoclonal Antibodies: From Local to Extended Networks', in *Private Science*, 165–81.

[47] Philips, 'If You Need a New Microscope - Why Purchase an Old One?'

[48] Olympus, 'The Microscope That Works for You in Multiple Ways', *Nature* 235, no. 5334 (21 January 1972): Cover 2.

[49] Plan achromatic lenses are a further development of achromatic lenses. The latter can only correct two wavelength aberrations of two colours (red and blue, typically), while planed achromatic lenses are able to make multiple wavelength coincide in one focal point. Ibid.



Cover 2
NATURE JANUARY 21 1972
The microscope that works for you in multiple ways.
This is the FHA, in its basic form a valuable addition to the field of microscopy. When full play is given to its versatility it becomes a super deluxe instrument — with enough interchangeable lenses to take you through practically any kind of research programs, a built-in illuminator, focusing down to as fine as 0.0005mm and heads that can deliver a superwide field of view or a full photomicrographic system. And the magnification range jumps right from 28× to 2000×.
There are 8 models of the FHA, each one with a different eyepiece/objective/head/condenser arrangement, one just right for your application.
With the standard turret mounted plan achromatic objectives you get super corrected components which make color and spherical aberration and field curvature almost non-existent, resulting in ideal images for both microscopy and photomicrography. Images are further guaranteed clarity by an N.A. 1.40 achromatic/aplanatic condenser. Added to this top microscope value is a full range of Olympus accessories — apochromatic objectives, photographic apparatus, exposure meter, phase contrast attachment, micro viewing screen, a dark field condenser.
Seeing beyond man's vision
OLYMPUS
OLYMPUS OPTICAL CO., LTD.
43-2, Hatagaya 2-chome, Shibuya-ku, Tokyo, Japan
OLYMPUS OPTICAL CO., (EUROPA) GmbH.
2 Hamburg 1, Steindamm 105, Germany
OLYMPUS CORPORATION OF AMERICA
2 Nevada Drive, New Hyde Park, N.Y. 11040, U.S.A
Write to us for the name of your local distributor, plus a free fact filled brochure.

A PLUG FOR OUR β-COUNTER
Anyone on your staff can operate the LKB-Wallac Liquid Scintillation Counter as a routine laboratory instrument. All they have to do is pick out two plug-in boards programmed for the particular samples, plug them in and press the Start button. The counter does the rest, and even gives a confirming printout of the selected parameters.
You avoid repeating all the work of selecting windows and calculating optimum parameters each time you change from one type of sample to another. Do it only once. The information is stored on the plug-in boards ready for use on the next occasion.
The LKB-Wallac system can be used both for single and double labelled samples, to give printouts of CPM and true DPM. An extra unit, which plugs in on top of the main control panel, converts the instrument for multi-user and double labelled operation.
LKB
LKB Instruments Ltd.
LKB HOUSE · 232 ADDINGTON ROAD
S. CROYDON, SURREY CR2 8YD · TEL: 01-657-0286

Figure 8.


IF YOU MAKE YOUR OWN DNA LIGASE, I CAN SAVE YOU A LOT OF TIME.
I know you have enough to think about, without having to prepare your own molecular biochemicals.
I'm Dr. Mary Ann Osuch for Miles Research Division. Because your time is better spent observing and recording results, we at Miles have made some important additions to our line of molecular biological products.
To our group of individual tRNA species and RPC-5, we've added two Leucine tRNA species, 1 and 4, Lysine tRNA and RPC-6. The components for protein synthesis studies, ribosomes, Poly U and viral ribonucleic acids, now includes Elongation Factors G and T. We've also expanded our DNA repair reagents, DNA polymerase, ML DNA, T2 DNA and ØX 174 DNA, to include DNA Ligase, Polynucleotide Kinase and Amino Acyl-tRNA Synthetase.
You'll find these new products offer many advantages, such as level of purity and specificity as shown in the Chemical Credentials which we will send you on request.
For the credentials and catalog describing our molecular products, or if you need help with a problem, send this coupon or call our closest office.
Send me your catalog for Molecular Biological Products and complete credentials on the following products.
Name
Title
Institution
Address
City
State
Zip
Telephone
RESEARCH DIVISION
MILES
Miles Laboratories, Inc.
Post Office Box 272
Kankakee, Ill. 60901 U.S.A.
Phone: 815-939-4417
Miles Laboratories Ltd.
Post Office Box 37, Stoke Poges
Slough England SL 2 4 LY
Phone: Farnham Common 2151
Miles-Yeda Ltd.
Post Office Box 1122
Kiryat Weizmann,
Rehovot, Israel
Phone: (03) 95-29-22

Figure 9.

phase contrast attachment to the right levels required a skilled understanding of microscopy. It was the operation that which was simplified.

Another manufacturer that presented this 'easiness factor' as the main selling point was LKB Instruments. As an accessory to their β-counter (a cell-counting machine), the LKB-Wallac Liquid Scintillation Counter was an electronic plug-in board that recorded and stored cell-count information (Figure 8, right).[50] The advertisement presented a white-coated woman holding and observing one of the plug-in boards. Together with this female image, the 'easiness factor' was mentioned explicitly:

> Anyone in your staff can operate the LKB-Wallac Liquid Scintillation Counter as a routine laboratory instrument. All they have to do is pick out two plug-in boards programmed for the particular samples, plug them in and press the Start button. The counter does the rest, and even gives a confirming printout of the selected parameters.
>
> You avoid repeating all the work of selecting windows and calculating optimum parameters each time you change from one type of sample to another. *Do it only once.* The information is stored on the plug-in boards ready for use on the next occasion.[51]

The comfort that was offered to the operator by the use of the LKB-Wallac was because it reduced the number of material and cognitive manipulations necessary to perform the cell-count. As Philips had sharply put it, "Tedious routine operations are automated – so you can concentrate exclusively on the specimen."[52]

Out of this concern for the 'ease', we can sketch here a sought-after element of the scientific practice: an epistemic virtue. The science promoted by Philips, LKB Instruments, and Olympus was one of thought and concentration, in which the repetitive cognitive and material manipulations were thought of as hindrances. As LKB Instruments had put it, "calculating optimum parameters each time you change from one type of sample to another," were presented as interruptions of the optimal rate of discovery.[53] The electronic weighting company Oertlin+Cahn would call this easy manipulation of small quantities of research objects, "micrograms without tears."[54]

This virtue was one which prioritised 'thought', and the machines were supposed to free scientists from the tediousness of operating with minuscule target objects

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[50] LKB Instruments, 'A Plug for Our Beta-Counter', *Nature* 241/2 (1973).

[51] Ibid. Original emphasis as bold, instead of italics.

[52] Philips, 'If You Need a New Microscope - Why Purchase an Old One?'

[53] LKB Instruments, 'A Plug for Our Beta-Counter'.

[54] Oertling+Cahn, 'The New Electronic Weighing Family', *Nature* 252 (1974): ii–iii.

(molecules, cells) by the virtue of optimally designed devices. This science had a clear message in the terms by which the scientist had to think. Again LKB, was concise and explicit in their discourse about this sort of science: "THINK SMALL."[55] The LKB Microcalorimter was designed to produce data of heat exchanges (e.g., enthalpy, entropy) of the micromolecular systems (biochemical and chemical) under study, and while to *understand* the applications of the LKB instruments you needed to be an expert on the topic ("determination of bonding energies and stability constants for protein interactions and ion-ligand reactions", "interactions of small molecules with surfaces, and high molecular weight materials"), the *manipulation* of the apparatus was fairly simple: "No longer is there a lack of reliable, simple-to-use instruments. The LKB Microcalorimeter gives you a tool which makes it possible to gain an insight into processes which, so far, have been difficult to study."[56] The 'science of thought' needed the simplification of the processes of manual operation. As we will see in the following section and chapters, the thinking, paradoxically, was *not* small.

The machines weren't the only ones able to make scientists' life easier (figure 9). Let's remember that in the scientific networks, human agents are actors too. Take Dr Mary Ann Osuch, from Miles Research Division of Miles Laboratories. She's from the industry sector and she knows the needs and wants of you, the academic scientist, browsing through a *Nature* issue in 1973:

> I know you have enough to think about, without having to prepare your own molecular biochemicals.
>
> I'm Dr. Mary Ann Osuch for Miles Research Division. Because your time is better spent observing and recording results, we at Miles have made some important additions to our line of molecular biological products.
>
> To our group of individual tRNA species and RPC-5, we've added two Leucine tRNA species, 1 and 4, Lysine tRNA and RPC-6. The components for protein synthesis studies, ribosomes, Poly U and viral ribonucleic acids, now includes Elongation Factors G and T. We've also expanded our DNA repair reagents, DNA polymerase, ML DNA, T2 DNA and ∅X 174 DNA, to include DNA Ligase, Polynucleotide Kinase and Amino Acyl-tRNA Synthetase.[57]

[55] LKB Instruments, 'Think Small', *Nature* 241/2 (1973): Back Cover.

[56] Ibid.

[57] Miles Laboratories, 'If You Make Your Own DNA Ligase, I Can Save You a Lot of Time', *Nature* 241/2 (1973).

The woman in the picture is Dr Osuch, presented in a laboratory of Miles Laboratories. In the foreground, the glassware, although distinguishable, is seen out of the focal plane. Just behind it, Dr Osuch appears. She is wearing a pair of hexagon glasses; her mouth is open, talking joyfully to someone, perhaps a colleague within the Research Division. Dr Osuch appears as the face of Miles, in which the chemicals are important, of course – they are set in the foreground – but they still appear out of focus, because they, of course, don't get designed, produced, and delivered by themselves. For that, we need human actors. Those working in Miles, like Dr Osuch, are the ones who are going to make your life easier. Once we go beyond the glassware and the technicism, here is where the professionalism of Dr Osuch meets the personal service of a concrete employee: here Mary Ann comes in sharp, and she has a clear message for you: "IF YOU MAKE YOUR OWN DINA LIGASE, I CAN SAVE YOU A LOT OF TIME."[58]

Miles produced an image of his company around the theme of dedicated personal service. As we saw in the beginning of the chapter in the case of Dr Moshe Rashi and his lectins, and here with Dr Mary Ann Osuch, Miles took a personal approach to marketing. They often featured their employees, such as Dr Chung, who appeared as the image of the advertisement "Miles is specific."[59] They framed themselves as willing to help, with headers such as "When you need to know enzymes Miles can help!" featuring Peter Copper, Director of Marketing and Richard Rollins, Sales Manager, whose images were shown in a seemingly candid photograph of the two in a meeting while Peter was holding a half-smoked cigarette.[60] The importance of this approach is that it highlights the importance of manufacturers as partners in research: "If you have a problem or question just call or write to the office nearest you for personalized assistance. We are here because no problem is small enough until it has been solved."[61]

Time was a precious material to work with in 1970s molecular biology. Difco, part of BLT (Baird & Tatlock London), whose motto was "Complete Laboratory Service," manufactured microbiological reagents and media, and promised to have them "delivered to your bench, *quickly*."[62] The epistemic virtues of Difco were clearly signalled: "speed, convenience, reliability … and remember that Difco offer [sic] the only *complete* line of culture media available in the U.K."[63]

In this quick survey, we've seen the epistemic virtues highlighted by advertisers of scientific objects: innovation, standardisation, simplified operation, comfort,

[58] Miles Laboratories.

[59] Miles Laboratories, 'Miles Is Specific', *Nature* 248, no. 5445 (15 March 1974): xi.

[60] Miles Laboratories, 'When You Need to Know Enzymes Miles Can Help!', *Nature* 248, no. 5446 (22 March 1974): v.

[61] Ibid.

[62] Difco, 'Microbiological Reagents and Media Delivered to Your Bench Quickly', *Nature* 217, no. 5125 (20 January 1968): xiv. Original italics.

[63] Ibid. Original italics.

professionalism, and speed. Who could achieve those godly virtues in their lab? The instrument-makers Perkin-Elmer advertised time-saving in its header: "Food analysis. 60 secs or 60 minutes. The choice is yours."[64] The machine advertised promised a great reduction in the time spent analysing the presence and quantity of arsenic in foodstuffs, but it was the head of the department who had to cash in for their Atomic Absorption Spectroscope. The choice of a fast science was left to the individual. But for whom was science done?

IV. Superscripted: Introducing social responsibility

The manufacturers aimed to win the scientific community's trust by associating themselves with the values that scientists demanded in their practice. However, manufacturers did not only place their technologies as practical solvers of experiments but also as embodying the social values that science professed.

Coulter Electronics alluded clearly to the social importance of a technologically equipped lab in their *Nature* ads. One ad in a June 1975 back cover (an expensive placement) displayed the header: "If your Haematology lab is busy, cost conscious and forward thinking you need the Coulter Model S" (figure 10, left).[65] These elements ("busy", "cost conscious", and "forward thinking") are those of a laboratory, not only of the individual scientists, although it may apply to individuals as well. Ads were directed to those who made decisions on the lab's budget, people whose lab was *theirs*, the directors.

In looking at this ad, we can get a glimpse on more epistemic virtues of the 1970s science. Coulter sold the elements that made up the imaginary of ideal science, and which reflected the self-image of laboratory directors: hard-working, economically efficient and mindful, and progressive. These are virtues of laboratories embedded in a social world, and placed between the researcher and the grander societal worries, we find the technology. The lab-coated man in the image isn't just looking at the machine measuring capillary samples at a rate of "seven parameters in 43 seconds" —he is also soberly looking at the grand scheme of things, at how his research is saving lives and making a societal difference.[66] Ads placed themselves in between the narrow target of the scientific community and the grand context of society. Putting it in another way, the scientist's contribution to society was achieved through his expert manipulation of the

[64] Perkin-Elmer, 'Food Analysis. 60 Secs or 60 Minutes. The Choice Is Yours', *Nature* 248, no. 5444 (8 March 1974): ii.

[65] Coulter Electronic Limited, 'If Your Haematology Lab Is Busy, Cost Conscious and Forward Thinking You Need the Coulter Model S', *Nature* 255, no. 5509 (12 June 1975): Back Cover.

[66] Ibid.

technology. The technology was there to aid the scientist in achieving a positive social influence.

Labex International compacted into one page the realities of experimental science as a social activity of the 1970s (figure 10, right). Much can be said about this one page. The first three sentences/paragraph can be read at a *staccato*:

> Scientists are demanding increased accuracy, higher sensitivity and easier retrieval of results in their routine laboratory work.
>
> Manufacturers are producing newer and more sophisticated equipment to keep one step ahead of those demands.
>
> Labex International 75 brings the two sides together at the largest specialised and most comprehensive exhibition in Europe of everything that's new in the laboratory world.[67]

Labex International was an exhibition of the latest laboratory equipment, an event where scientists and technicians "browsed among sunshine recorders, robotic liquid-handling systems, electronic balances, microscopes and telescopes."[68] The edition of Labex 1975 in London was no other, there would be "on-the-spot-demonstrations," where technicians would demonstrate the workings of "tomorrow's technology."[69]

The firm who owned Labex was Industrial and Trade Holdings (ITF), and it organised these kinds of events in several places throughout the world. The aim of these exhibitions was to give visibility to commercial enterprises, be it of companies selling jeans or spectrometers. ITF was based in Birmingham but arranged exhibitionary complexes throughout the world: IMBEX '77 (the International Men's and Boy's Wear Exhibition), PAKEX '77 (the International Packaging Exhibition), IPHEX '76 (the International Pneumatics and Hydraulics Exhibition), or HEVAC '76 (the International Heating, Ventilating and Air Conditioning Exhibition).[70] In their hometown newspaper, ITF stated their mission: "Together we will put industry on the map."[71]

As part of ITF, Labex International was one of those events were exhibitors exposed their products, hosting thousands of attendees in each gathering (however, "open to members of the trade only") and including daily seminars during the 3 to 4 days

[67] Labex International, 'Tomorrow's Technology', *Nature* 254, no. 5496 (13 March 1975): ii.

[68] Bob Beale, 'High-Tech: We Make It but Can Not Buy It', *Sydney Morning Herald*, 13 March 1975.

[69] Labex International, 'Tomorrow's Technology'.

[70] Industrial and Trade Fair Holdings, 'Together We Will Put Industry on the Map', *The Birmingham Post*, 2 February 1976.

[71] Ibid.

that the exhibition lasted.[72] In Birmingham at least, the members of the trade were not only scientists, but businessman also. An advertisement in the 'Business Post' section of the Birmingham post (which already gives clue to the sort of reader looking at it) presented Labex International as an opportunity to sell products and get to known those "members of the trade."[73] As we can see from this excerpt, the exhibition of Labex International was an advertisement in itself:

> On Thursday, March 3, 1977, The Birmingham Post in co-operation with Industrial and Trade Fairs Limited, will publish a special tabloid supplement. [...] Here is your opportunity not only to attract the attention of our influential business readers in the Midlands, through advertisement but to take advantage of the international interest which this Exhibition will be certain to generate. The supplement will be circulated at the [National Exhibition Centre] complex during the Exhibition – an unofficial catalogue for the Laboratory Apparatus & Materials industry. Advertise and you will be in good company.[74]

Labex advertised itself differently in *The Birmingham Post* as it did in *Nature*. For the 1975 edition of the exhibition, the Labex International's main selling point were the "on-the-spot-demonstrations [of] everything from the most sensitive electronic instrumentation to laboratory glassware."[75] But this wasn't all: wandering among laboratory objects, collecting promotional pens and coffee mugs, scientists may have also found a more sombre exposition.

> Concurrent with Labex a special forum will be convened in which three Nobel prizewinners will present their own views on "The Social Responsibility of Science". In addition, a series of discussion meetings have been arranged each day by learned societies and institutes.[76]

In this event, "sophisticated equipment" became framed as indispensable for scientists to address their research questions, and manufactures framed themselves as essential in effectively enabling the so-called social responsibility that scientists promised.

What is "The Social Responsibility of Science," and why did Labex refer to it within quotation marks? As I announced at the beginning, advertisements are quick and condensed snapshots of the historical moment, but that more often than not, need

[72] Labex International, 'Tomorrow's Technology'.

[73] Industrial and Trade Fair Holdings, 'Together We Will Put Industry on the Map'.

[74] Ibid.

[75] Labex International, 'Tomorrow's Technology'.

[76] Ibid.

context to be understood. To understand what Labex meant by these words, we may turn to editorial pieces discussing precisely this topic, a topic whose name became a household name in *Nature* and British science, and a concept that was more like a proper noun (thus the capital letters and quotation marks) that was fuzzy enough to be employed as a powerful rhetorical tool for appropriate moral behaviour. As I will show in the next chapter, 'responsibility' had several meanings and political uses.

Concluding remarks

Scientific advertisements are a heterogeneous array of literature in form, content, function, and aims. Here, I have shown a new avenue to study how scientists were seen and targeted by for-profit manufacturers. As well, I hope to have shown the intimate relation that these firms tried to establish, or indeed, established with the research community.

This intimate relationship was nurtured by *Nature* itself. The 1970s were a time of profound restructuring of the magazine's relation to paid commercial space and its management. The price per square inch increased together with an internalisation of the mechanisms of their editorial control. This process of internalisation culminated with the shortly lived 'Newly on the market'. This section is a genre that stood between the commercial intent in terms of content and *Nature*'s native scientific form in terms of appearance.

This hybridism was characteristic of advertisements. Not only were they the visual equivalent of sound bites designed to attract attention by marketing departments. Many of them could be employed as forms by cutting them out of their original context (the magazine) and refunctionalizing them as objects that carried vital information on the material needs of research laboratories. The intimate dependence of scientists on manufacturers is quite obvious throughout the 1970s decade. Not only in the production of research objects themselves, but on the production and distribution of technical literature necessary for the reification into universal properties: the catalogue. This kind of trade literature consisted not only of a listing of available products and their pricing, but featured also their vital technical details. While I have not analysed here the content of the catalogues themselves, there is historical evidence that invites to their study. The reification of epistemic objects by the establishment of shared networks and infrastructures was a vital process in the biological sciences during the mid-to-late twentieth century involving of a variety of stakeholders. Cambrosio and Keating, in their 'Monoclonal Antibodies: From Local to Extended Networks' have shown how standardisation was a shared "effort" of researchers, scientific societies, and commercial ventures.[77] The establishment of "reference reagents," for instance, was not due to their intrinsic homogeneity of their properties alone, but a contingent product of human intervention. From their study of antibodies, they argue that "it was not because [monoclonal antibodies] were such a powerful tool that they led to standardization, but rather that they were translated into such a powerful tool precisely because they were seen from the very outset as a potential vehicle for standardization."[78] It is left to be investigated how product catalogues contributed to these shared efforts to establish standards in molecular biology.

One approach to selling machines was their framing as 'new'. This word was often stressed in the advertisements, and the firms prided themselves of putting in the market the latest technological developments. Together with selling 'the best', 'the new' was a ubiquitous characterisation of laboratory objects. The importance of this is not only about how scientists were targeted rhetorically, but illustrates how technological innovation was linked to the efforts of standardisation. As shown in the case of the EM 300 microscope, innovation was only part of the technological process: convergence between companies on shared standards was paramount. The consensual nature of innovation and its relationship with technological reification needs to be further investigated by posing questions about the relationship between companies, and how knowledges and objects were shared among competitors.

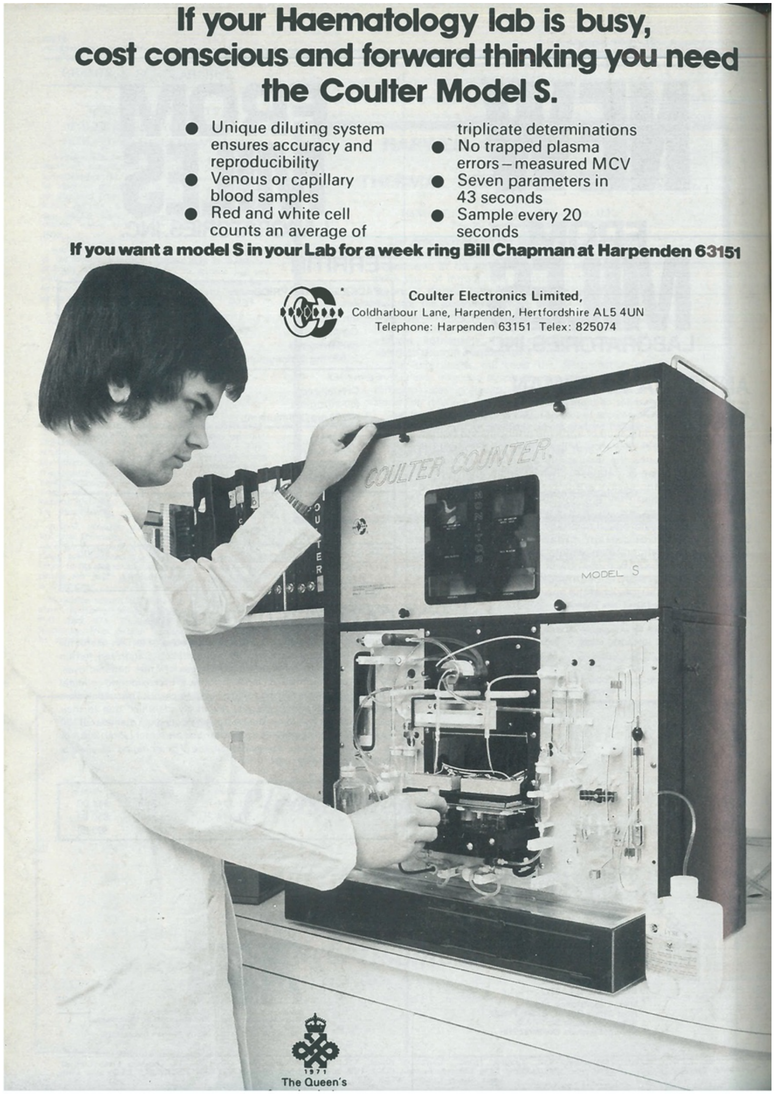
Another theme to which I have devoted significant space in this chapter is the topic of epistemic virtues. I have reviewed here how and which of these virtues were highlighted in the presentation of laboratory objects in their advertisements. By doing so I hope to have sketched a commercially distilled image of some of the virtues of the practice of 1970s molecular science. This practice was one of profound visual realism, with advertisements for microscopes being bold in their assertions that "seeing is believing." While this usual naiveté may come to no surprise to historians of science, I hope to have made clear here that the epistemic certainty that was given to the images was intimately related to the design and advertisement of research machines. Put concretely, the faith in the visual relied on trust in the complex technology behind the apparatuses, like spectroscopes and microscopes. The design of these devices was done with 'ease of use' as a precondition, as many of the advertisements I showed here can attest. Reliability on the visual was thus tightly linked with simplicity in the operation of machines. This relation between the prescribed behaviour of the scientists and their epistemic faith positioned scientists not as the people who would open the black boxes of the technology. Instead, the simplicity of operation would invite them to concentrate on the specimen under study and the empirical examination of its properties as provided by the devices. As Dr Mary Ann Osuch from Miles Laboratories put it: "your time is better

[77] Cambrosio and Keating, 'Monoclonal Antibodies: From Local to Extended Networks'.

[78] Ibid.

spent observing and recording results."[79] Let us move on now to other practices that scientists did: self-reflect and self-protect.

[79] Miles Laboratories, 'If You Make Your Own DNA Ligase, I Can Save You a Lot of Time'.



If your Haematology lab is busy, cost conscious and forward thinking you need the Coulter Model S.
● Unique diluting system ensures accuracy and reproducibility
● Venous or capillary blood samples
● Red and white cell counts an average of triplicate determinations
● No trapped plasma errors – measured MCV
● Seven parameters in 43 seconds
● Sample every 20 seconds
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Telephone: Harpenden 63151 Telex: 825074
COULTER COUNTER
MODEL S
1971
The Queen's

ii
Nature March 13 1975
TOMORROW'S TECHNOLOGY
Scientists are demanding increased accuracy, higher sensitivity and easier retrieval of results in their routine laboratory work.
Manufacturers are producing newer and more sophisticated equipment to keep one step ahead of these demands.
Labex International 75 brings the two sides together at the largest specialised and most comprehensive exhibition in Europe of everything that's new in the laboratory world.
On-the-spot-demonstration
Everything from the most sensitive electronic instrumentation to laboratory glassware will be on show – with technical experts there to explain the products and give on-the-spot demonstrations.
As well as the many instruments which will be on show for the first time, there will be a complete range of well-established instruments, many of which have been modified and up-dated especially to meet the Labex deadline.
Concurrent with Labex
A special forum will be convened in which three Nobel prizewinners will present their own views on "The Social Responsibility of Science". In addition, a series of discussion meetings have been arranged each day by learned societies and institutes.
Complimentary tickets
This is the event of the year for the laboratory worker – complete the coupon below and send it off today.
Your complimentary ticket will be a passport to tomorrow's technology.
Earls Court, London. 14-18 April 1975. 10am-6pm daily.
U.T.P. Exhibitions Ltd, 121 King Street, London, W6 9JG Tel: 01-741 0303 (10 lines)
To: U.T.P. Exhibitions Ltd, 121 King Street, London, W6 9JG.
Please send me complimentary ticket(s) to Labex International 75, and full details of the Discussion Meetings and the Labex Forum.
Name
Position
Company
Address
Future technology in action.
Labex INTERNATIONAL
1975

Figure 10.

Chapter Two. **The placement of the scientific self in society.** Meanings and uses of 'social', 'political' and 'social responsibility' in the context of British science policy, the Rothschild and Dainton reports, and the British Society for Social Responsibility in Science (1971-1972).

This chapter describes the meaning of 'social responsibility of science' in the context of the early 1970s debates over the funding of British science. I will show how this concept was used by various groups (scientists, scientists-activists, government officials, magazine editors, industrialists) and mobilised politically by these different parties who had obvious different social ambitions. We will see how 'social responsibility' had quite different meanings for the people involved in this discussion, but that more often than not, politically distinct groups had shared conceptual assumptions. These shared assumptions will help me describe the ground in which 1970s discussions of science policy were established. I deal here with the specific debates on British science policy that took place mostly between 1971 to 1972, but which had resonance in *Nature* throughout the whole 1970s decade.

I will argue that the different meanings and discursive characteristics of 'social' and 'political' did not only differ between the different actors of the debate, but at times coincided. The common meanings and assumptions of 'social responsibility' invite us to look at the contexts that allowed for these assumptions to be shared. Put simply, understanding the common ground between the different parties will allow us not only to know why certain team won over the other in the political arena, but which game they were playing.

As we are dealing with a dialectic battle, it will be important to understand one the fields where this game was set: in my case, *Nature*. As such, I will characterise where the sympathies of the editors of the magazine appear to lay, arguing that they aligned particularly well with a relatively narrow conceptualisation of the term 'social responsibility'.

A note on methodology and historiography is due. Since the terms 'social', 'political' and 'social responsibility' have such different meanings depending on who utters them, the historian as investigator of these speeches needs a stable analytic tool. As such, I attend here to something that was evenly shared among the different parties involved in this chapter, and that it is in fact a universal category of science: its placement within society. Unescapably, science is and has always been an activity done by humans, and whose activities were costly and had to be afforded by someone. Be it sponsored by individual wealthy patrons as in the early modern period, by an increasing involvement of state in the nineteenth century, or by for-profit companies in the twentieth, scientific work is always funded by someone that has to be pleased with what the scientist deliver, establishing thus a system of scientific patronage. As a group of Danish historians of science have recently illustrated, there is a clear "empirical fact that money is everywhere in science."[1]

This system of patronage is not only a mechanism of funding, but a mechanism for the production of knowledge commodities, be it books, devices, predictions, medicines, or peer reviewed articles. As such, scientific knowledge production is always situated within the reward system of science, and its products can be thought as "unit[s] of work."[2] Understanding scientific knowledge production as part of a system of patronage and production of knowledge goods situates the practice as always set in a certain economic, political, and social setting, and stresses that science does not exists without it.

Science is always *placed*. In a way, given the inescapabilty of this placement, one could well argue that the sentence 'science is placed' assumes an actual division between science and the rest of the social world, a division that I don't want to afford, and which has repeatedly been shown by historians of science to be inexistent. However, we could say that something was in actual fact, *placed* – the scientific self. If we take 'the scientist' to be a well-defined category as 'those who do research,' and focusing here on academic research, we can situate the scientist in a localised placed: the university. This institutional demarcation may not be to the liking of all and may leave certain researchers as non-scientists (in the case of natural philosophers in the early modern period, industrial scientists in the twentieth century, or the independent scholars today). But the category of 'scientist' as bound by its institutional workplace is sufficient and effective in analysing what is at stake here: describing what is was to be a molecular scientist in the 1970s, and the epistemic virtues to which this social group ascribed to. As such, let me reformulate the sentence as posed it before: *the scientific self is placed*. Let us explore the society and politics that this self inhabited.

This placement I investigate here is set around the debates over British science policy in the early 1970s. In the first section I describe the proposals of Victor Rothschild

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[1] Casper Andersen, Jakob Bek-Thomsen, and Peter C. Kjærgaard, 'The Money Trail: A New Historiography for Networks, Patronage, and Scientific Careers', *Isis* 103, no. 2 (1 June 2012): 310–15, https://doi.org/10.1086/666357.

[2] I employ here the term 'reward system of science' not in the purely Mertonian sense of 'recognition', but with an emphasis on the financial aspect of the patronage system, as described recently by Remo Fernández Carro. Robert K. Merton, 'The Reward System of Science', in *The Sociology of Science. Theoretical and Empirical Investigations* (Chicago and London: The University of Chicago Press, 1973), 281–412; Remo Fernández Carro, 'What Is a Scientific Article? A Principal-Agent Explanation', *Social Studies of Science*, 27 August 2020, 0306312720951860, https://doi.org/10.1177/0306312720951860.

and Frederick Dainton, who differed considerably but which, at core, had the common assumption that science should be productive, and that the nation's budget should be tied to scientific research, as much as scientific research should be tied to the nation's goals. Both these proposals contained implicit and explicit conceptualisation of what 'social' and 'political' meant – the settings in which the scientific self was set. In the second section I explore how these proposals were debated by scientists themselves, who argued about the kind of scientific self that emerged in the kind of societies that Rothschild and Dainton proposed. In the third section I explore where the sympathies of *Nature* laid by probing into the meanings of 'political' and 'social' as used in the editorial pieces. To contrast the magazine's position, in the fourth section I attend to the alternative meanings of these words by the British Society for Social Responsibility in Science (BSSRS), and what *Nature* thought of this scientific-activist organisation. To finish, I discuss how the BSSRS's conception of 'political' and 'social' differed from and had similar assumptions to those of Dainton, Rothschild, and *Nature*, and the kind of scientific self that emerged from their proposal, and how the set of virtues of scientific research should be expanded and operationalised.

I. Rothschild and Dainton both wanted science to produce things: discrete versus continuous understanding of science's objectives

Victor Rothschild and Frederick Dainton each headed one of two teams that undertook government-issued investigations on the status of British science budget. Both of their reports were published together in a government Green Paper, *A framework for government research and development*, in November 1971.[3]

The aims and conclusions of both reports, as well as the men who drafted them, were different. Victor Rothschild had been a scientist and bomb disposal expert during the Second World War. Since 1963 he had been the Head of Research of the petrochemical company Shell, work that he continued until his appointment as the head of the newly created government think tank, the Central Policy Review Staff. In 1970 Rothschild was commissioned by the Labour government of Edward Heath to "think the unthinkable": to assess current public spending in scientific research across all departments, and to prepare a long-term strategy as a recommendation for changes in

[3] Victor Rothschild, 'The Organisation and Management of Government R&D', in *A Framework for Government Research and Development* (HMSO London, 1971); Frederick Dainton, 'The Future of the Research Council System', *A Framework for Government Research and Development*, 1971.

national policy.[4] His report, 'The organisation and management of government R&D', argued for an increased utilitarian and accountable national science policy.

Frederick Dainton was a graduate of Oxford and Cambridge, Professor of Physical Chemistry at Leeds since 1950 and fellow of the Royal Society. In 1970 he was appointed with two tasks: Professor of Chemistry at Oxford and chair of the Council of Science Policy, in the latter charged with the task of investigating the "effectiveness of research funds channelled by Research Councils."[5] His report, 'The Future of the Research Council System', proposed a system of layered funding based on the different objectives of the research and, as we will see, with a highly functionalist and holistic interpretation of science.

Shortly after the publication of the Green Paper, at a distance, smoke could be spotted.

Rothschild's report caught fire much quicker and gained the attention of many scientists. In December, the debate seemed to promise flames ('Some Smoke But No Fire So Far'), but by February 1972, the mainstream scientific community who published in *Nature* had already lowered its head and assented in favour of Rothschild's recipe ('That Was the Debate, That Was').[6] How did it burn up so quickly?

Rothschild's report created a stir among scientists, and *Nature* became a heated battleground in the debate for interpreting, accusing, and defending the Rothschild's proposition. Scientists, journal editors, industrialists, and politicians contributed with pieces, published in *Nature*'s first pages. To understand why the report created such a commotion, we must consider its mission and ideology. The principal recommendation of this report was a radical restructuring of the way science was funded and assessed, based on an infamous "customer-contractor principle:"

> 1) Applied R and D is done by somebody. I called that somebody the contractor, but the contractor could equally well have been called the scientist, the engineer, the mathematician, the research worker, the boffin, or any other word or phrase used to describe or identify that somebody who does the R and D.
>
> (2) Applied R and D is an activity with a potential or actual application. Otherwise the adjective "applied" would not be used.

[4] Neil Calver and Miles Parker, 'The Logic of Scientific Unity? Medawar, the Royal Society and the Rothschild Controversy 1971–72', *Notes and Records of the Royal Society of London* 70, no. 1 (20 March 2016): 83–100, https://doi.org/10.1098/rsnr.2015.0021.

[5] Dainton, 'The Future of the Research Council System'.

[6] 'Smoke But No Fire So Far', *Nature* 234, no. 5327 (3 December 1971): 239–40, https://doi.org/10.1038/234239a0; 'That Was the Debate, That Was', *Nature* 235, no. 5337 (11 February 1972): 293–95, https://doi.org/10.1038/235293a0.

> 3) An application is a use which in turn requires that there is a user, who could equally well be called a customer*, a representative of a customer or user, or even a customer or user surrogate.
>
> *While usually (and ideally) the customer commissions applied R and D and pays for it, one can easily envisage cases in which the objective which requires R and D was achieved before a customer had paid for it. As an example, a research establishment might approach a potential customer, saying "We have discovered an economic replacement for the additive lead tetraethyl in petrol. We would like to sell it to you at a price which will include the cost of the R and D we did to achieve this objective."[7]

Rothschild's proposal, mimicking the structure of a logical argument, envisioned science as applied or potentially applied knowledge, and to the disturbance of the readers of *Nature*, it cared very little about so-called basic research. That was the main point of contention. The interpretation of those scientists who identified themselves as 'basic researchers' was that their research was in danger of getting defunded. *Nature* had established its reputation for featuring the latest of this sort of science, which went through different names ('pure,' 'fundamental,' 'basic'). Now, the focus on "applied R and D" could mean a tighter control by the government, and a weakening of the Research Council system, which until now had been a powerful insulation mechanism for basic research funding.

Rothschild's proposal was a major attempt at restructuring the funding channels of British science. British scientists were largely funded through the Research Councils (RCs), which had acted as an institutional buffer. The RCs had been in place since the early decades of the twentieth century, and by the 1960s had solidified into several governmental entities that funded research of specific disciplines. By the time the Green Paper was published in 1971, there were five Research Councils: the Medical Research Council (MRC), established in 1920; the Agricultural Research Council (ARC), established in 1931; and the Science Research Council (SRC), Natural Environment Research Council (NERC), and Social Sciences Research Council (SSRC), all three established in 1965.

Since the 1950s, British science had been for the most part (in financial proportions), military, with 74% of the nation's budget for research and development being allocated to this sort of investments.[8] The British university grew since the end of

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[7] 'Self Evident— an Explanation by Lord Rothschild', *Nature* 235, no. 5337 (11 February 1972): 296–296, https://doi.org/10.1038/235296b0.

[8] Wright, Susan, *Molecular Poltics. Developing American and British Regulatory Policy for Genetic Engineering, 1972-1982* (Chicago and London: The University of Chicago Press, 1994), 34.

the Second World War, but throughout the 1960s a soaring inflation and unemployment, and stagnating growth of the national economy, caused governments (Labour and Conservative alike) to attempt a tighter control over science policy. The manifesto of Harold Wilson's Labour government, which came to power in 1964, positioned science at the core of a socialist revolution, with an emphasis both on increased support and its public control.[9] Despite this political hope, and in the context of deep economic difficulties, by the 1960s scientists and politicians alike saw how "by almost every measure, Britain was behind European counterparts."[10]

The proposal of Rothschild endangered the autonomy of the RCs because it suggested that it should be the 'customer', that is, the governmental department who was interested in research being conducted, not the RCs, that should fund specific research. Under Rothschild's proposal being established, the customer would be "the government departments most likely to benefit from the results."[11] The scientists, as contractors, would be in direct cont(r)act with their patrons.

Let's take a table from the Rothschild's report, which illustrates the proposed financial restructuring of science funding (figure 11). For the year 1971-72, the Research Councils were to be financed by the Department of Education and Science (DES) with

U.K. Research Council Expenditure on R & D 1971–72 (£ million)

Research Council		Current DES Estimates	Rothschild Proposals		
			(a) Paid by DES	(b) Paid by Depts.	
		(1)	(2)	(3)	
(SRC)	Science	50.9	50.9	0	
(MRC)	Medical	22.4	16.8	5.6	(DHSS)
(ARC)	Agricultural	18.7	4.2	14.5	(MAFF)
(NERC)	Natural Environment	15.3	7.7	7.6	(DOE, DTI, MAFF)
(SSRC)	Social Science	2.2	2.2	0	
	Total	109.5	81.8	27.7	

Figure 11. Rothschil's proposal for restructuring UK's budget on science. DES: Department of Education and Science, DHSS: Department of Health and Social Security, MAFF: Ministry of Agriculture, Fisheries and Food, DOE: Department of the Environment, DTI: Department of Trade and Industry. Taken from Dobbs (1972). "Lord Rothschild has also proposed a transfer of about a quarter of the present DES science budget to other government departments (Table 2)" (347 Dobbs)

[9] Ibid, 32–36.

[10] Neil Calver, *The Royal Society and the Rothschild 'Controversy' 1971-1972*, Public lecture audio recording (The Royal Society, London, 2013), https://royalsociety.org/science-events-and-lectures/2013/rothschild-controversy/.

[11] 'Smoke But No Fire So Far'.

£109.5 million. Under Rothschild's system, there would be a transfer of about 25 per cent of this annual funding (£27.7 million) to four government departments which had or could have interests in the research, projected to be sponsored now by the DES. The "customers" would now be the Department of Health and Social Security, the Ministry of Agriculture, Fisheries and Food, the Department of the Environment, and the Department of Trade and Industry. Not all budget transfers would be equal under Rothschild's scheme, however. The Science Research Council and the Social Science Research Councils would continue to receive its £50.9 and £2.2 millions from the DES, respectively; whereas the Agricultural Research Council, for instance, which was receiving £18.7 million from the DES would suffer a reduction of over 75% of its budget, transferring 14.5 million to the Ministry of Agriculture, Fisheries and Food. Biological research with a potential medical application was typically considered 'basic science', and the administration of its funding fell under the Medical Research Council. Under Rothschild, 25% of the MRC's budget would be transferred to its new interested customer, the Department of Health and Social Security.

Scientists were worried about an expected loss of autonomy in their work. However, as we will see, not all who contributed to the debate rejected the proposal on its entirety. For the most part, however, it was agreed that Rothschild's proposal would close the distance between government and their work. Jeffery Leigh, a chemist at the School of Molecular Science at the University of Sussex feared that the commercial basis of the customer-contractor principle would "introduce stress and insecurity" in scientists' job conditions.[12]

Leigh's response to the proposal was, however, very moderate. He did not fully disagree with some of Rothschild's main tenets. His main aim was to salvage the RCs while still implementing the customer-contractor principle. For that, he proposed a unitary RC "dealing with all the sciences" that would oversee the nation's funding and its relationship with the needs of the state.[13] Leigh's counterproposal was itself constructed through commercial lenses and had 'efficiency' as its most salient virtue. He envisioned this single RC as an entity modelled on a "large organization."[14] The addition of this overarching agency, Leigh wrote, "does not necessarily promote inefficiency." Leigh attempted to shoothe readers with an example: "The Shell Oil Company seems comparatively effective in spite of its size."[15]

The reformist attitude of Leigh was not the only response to the costumer-contractor principle. Other suspicions went deeper and were not unwarranted. They

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[12] Jeffery Leigh, 'Proposal for a Constructive Response', *Nature* 235, no. 5337 (11 February 1972): 303–303, https://doi.org/10.1038/235303a0.

[13] Ibid.

[14] Ibid.

[15] Ibid.

weren't because Rothschild's proposal wasn't new nor radical. Aside Shell (where, as the reader may remember, Rothschild had been the Head of Research from 1963 until he was appointed by the government as the head of the Central Policy Review Staff), the customer-contractor was often compared to the already existing USA's model of science funding. At the beginning of the 1970s, American science had already diluted the image of British capability for coordinated national research.[16] In a post-Second World War scenario, a commercial model of science like the one proposed by Rothschild seemed more competitive in a neo-liberal world.

Some, however, were distrustful and warned the British about what laid over the Atlantic. Harvey Brooks, Dean of the School of Engineering at Harvard University and Chairman of the Committee on Science and Public Policy of the National Academy of Sciences, was invited to comment on the Rothschild's report and the British's reception of it. Brooks wasn't shy to say that he was "struck by the mildness of Lord Rothschild's proposal when compared with American reality," and stated that "the American system has, of course, operated from a formal standpoint almost entirely on the 'customer-contractor' principle since the end of the Second World War."[17] He affirmed that 'American science' had "prospered under it" and found it "difficult to empathize with the cries of anguish which the Rothschild's report has evidently elicited from the British scientific community."[18] But the Harvard Professor wasn't as cold with the British as some of these statements implied, and he wasn't uncritical of Rothschild's model either. In a sharp and blasting enumeration of the status of American science, Brooks wrote:

> I am also struck by Lord Rothschild's apparent obliviousness to some of the evident weaknesses and dangers of the American system, which he seems to admire without attribution. The instability in funding, the effective of lack of concern with the integrity and viability of scientific institutions—especially the universities—the wasteful competition for control over glamorous or spectacular technical programmes, the confusion of technological virtuosity with scientific achievement, the increasing obsession with narrowly conceived "social relevance", sometimes to the detriment of scientific quality, the exacerbation of competitiveness, "grantsmanship", and political manoeuvring in the scientific community—all of these negative symptoms, which are increasingly appearing in the American system, are ignored in the Rothschild report.[19]

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[16] Wright, Susan, *Molecular Politics*, 33.

[17] Harvey Brooks, 'Rothschild's Recipe in the United States', *Nature* 235, no. 5337 (11 February 1972): 301–2, https://doi.org/10.1038/235301a0.

[18] Ibid.

[19] Ibid.

Among many of the pitfalls of 'American science,' the word "social relevance" appeared between quotations. The format used by Brooks illuminates us: it shows us his distrust towards this kind of rhetoric. Putting it in between these marks signifies the lack of truthfulness of a scientist uttering 'my research is socially relevant', and it shows us how the American researcher was aware of the multiple meanings of 'social relevance' and its political uses. What were the different meanings of 'social relevance', or simply 'social'? Flipping the question around: what was the world like, for a scientist that was performing 'socially relevant' research? For Rothschild, this world was an economic world where, as we have seen, science could deliver, for instance, goods for "economic replacement" of products.

Let us move to another world. The world and the 'social' as constructed in the other government-issued report. Dainton's paper was narrower in scope as compared to Rothschilds, as it limited itself to investigate "effectiveness of research funds channelled by the Research Councils".[20] Dainton's Working Group had the objective "to advise [...] on the most effective arrangements for organising and supporting pure and applied scientific research and postgraduate training."[21] The most often noted point of contest between Dainton' and Rothschild's recipe was that the former negated the dichotomous division of the latter between pure and applied research. However, if we take a closer look, Dainton's reasoning was still based on a pragmatic implications of knowledge. Set in a layered spectrum instead of on the extremes, Dainton's paper established three categories instead of two: 'tactical science,' which was employed by the government and industry for "immediate executive or commercial functions"; 'strategic science,' laying as the "foundation for [...] tactical science"; and 'basic science,' which although it had "no application in view," it was assumed that from it would "depend the future ability of the country to *use* science."[22] With a slightly more nuanced approach but still with a utilitarian view of the scientific patronage system, it can easily be seen how Dainton was, from the outset, playing in Rothschild's terrain.

Dainton's paper aimed at keeping the distance between decisions-making of research funding and the government. It did so by promoting a strong RC system as a buffer. In a similar vein as Leigh's proposal of an overseeing "large organization", Dainton advocated for the formation of a central RC governing above the other ones, placing it between government and scientists for increased insulation between government and researchers.

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[20] Calver, *The Royal Society and the Rothschild 'Controversy' 1971-1972*.

[21] From the original 'The future of the Research Council system: report of the C.S.P. Working Group under the Chairmanship of Sir Frederic Dainton,' as cited in E. Roland Dobbs, 'The Organisation and Control of Scientific Research by the United Kingdom Government', *Higher Education* 1, no. 3 (1 August 1972): 345–55, https://doi.org/10.1007/BF01957558.

[22] Dobbs. My italics.

Despite Dainton's proposition starting off with a conceptual disadvantage, his report presented itself as to be philosophically informed. As we will shortly see, it was explicitly framed on Karl Popper's conception of science, and it aimed at politically articulating some of his ideas. The characterisation of the sciences as mutually interconnected within a single funding framework mimicked Popper's principle of unity of science. However, as I pointed out before, Dainton's unity was not free of hierarchy — it conceived the *use* of knowledge as its ultimate goal. As we can see from quotes from the official paper, its description of science and its funding was vibrantly Popperian:

> [T]he Report advocates that the activities of the Research Councils should not be completely independent of one another and of government departments, but "should be coordinated and administered by a Board, which would include as full members the scientific heads of the Research Councils" [...] [The Dainton's report] main arguments against these transfers are that "science is a unity which could only suffer by fragmentation corresponding to the responsibilities of different executive departments;" that "in basic and strategic science neither the devising of programmes of work nor the assignment of relative scientific priorities to each programme can be carried out by non-scientists" and that it is unrealistic to replace "the symbiotic relationship" which at present exists between the Universities and the Research Councils "by a whole series of relationships between the Universities and a multiplicity of Government departments."[23]

Historians Neil Calver and Miles Parker have investigated the philosophical roots of Dainton's proposal, and have located the intellectual influences within the British elite scientific class. This helps us understand the meanings of 'unity' and 'symbiosis' that crowd the Dainton's proposal. One of his supporters, Peter Medawar, publicly defended the RC system at the Royal Society, and did so under Popper's principle.[24] For Medawar, the hypothetico-deductive model was the working mechanism of science, which unified scientists as intellectuals pursuing truth. Its political implementation was the unification and maintenance of the RCs along disciplinary boundaries. Calver and Parker have illustrated the philosophical points of divergence between Rothschild and Dainton. Despite the fact that the defence of the customer-contractor principle needed a division of science into pure and applied, and that Dainton's proposal appeared philosophically denser and in accordance with the scientific community's view of science, Rothschild proposal was more palatable to the government, the latter who wanted to keep their spending in check. While the RC system persisted, Calver and Parker have argued that

[23] Ibid.

[24] Calver and Parker, 'The Logic of Scientific Unity?'

Rothschild won the political battle with strong emphasis towards application-driven research. However, these historians also argue that the philosophical tenets of Popper died only in practice by the exclusion of Dainton's recipe from governmental science policy, but that the unity of science continued to live on as important discursive element for the scientific community.[25]

At the time of the debate, the unity that Dainton professed was not given as much space in *Nature* as Rothschild's proposal. There were many more outraged pieces attacking Rothschild's wild plan to commercialise science than those in actual defence of Dainton's vision. One of the few, 'Dainton Demands a Hearing,' elaborated on the reception of the customer-contractor principle by the Council for Scientific Policy, a committee that, the reader may remember, was led by Dainton himself.

In response to Rothchild, the Council argued that the customer-contractor principle would "lead to a situation which would seriously handicap the scientific resources of this country by making it more difficult to determine coherent policies for scientific research as a whole".[26] Dainton, arguing through the Council' response, said that the RC system and its budget allocation was sufficiently "accountable" to parliamentary control. The £27.7 millions that Rothschild had calculated should be transferred away from the RCs into departmental contractors, Dainton argued, was "entirely wrong."[27]

As I said a few paragraphs above, there wasn't a diametrical opposition between the unity of science and the customer-contractor principle. Something that Dainton himself argued. He saw a large overlap between the two, and bowed down saying that the two proposals were "more than compatible":

> The Council for Scientific Policy welcomes the government endorsement of the research council system and although it is in broad agreement with Lord Rothschild when it says that tactical research should 'be regarded as potentially susceptible to the customer-contractor principle' reaffirms that basic research and strategic research should continue to be done by the research councils."[28]

Despite Dainton's critique of the customer-contractor principle, he did in fact concur with Rothschild in terms of its basic assumption as a valid model of science policy. While Dainton argued that not all science could be marketed and applied, he coincided that *some* could; and that the aim of the 'basic science' was to lay the groundwork for

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[25] Ibid.

[26] 'Dainton Demands a Hearing', *Nature* 235, no. 5337 (11 February 1972): 296–97, https://doi.org/10.1038/235296a0.

[27] Ibid.

[28] Ibid.

applying knowledge, as his layered, philosophically informed model of unity of science funding attested.

Dainton had a socially progressive idea of science, and defended it by making the case that 'science-for-its-own-sake' was a matter of 'human nature' and that it was inherently good for mankind. In a *Nature* lead editorial, he was cited saying, "science [is] necessary to satisfy basic human curiosity and [...] one must not quench curiosity or man will retrogress."[29] This way of argumentation is crucial to understand the debate and its outcome: Dainton coupled an intrinsic human need for knowledge with its inevitable social progress. Further on, he claimed, "science gives man power over his environment and regrettably over his fellow man."[30] Here we find the crux of the matter. In my view, the reason by which Dainton's proposal was unsurprisingly swiped away by Rothschild's recipe is because by linking science with social change while still wanting to retain the 'curiosity' argument, Dainton was playing the same game as Rothschild, albeit worse.

Dainton agreed with Rothschild on the social relevance of science, and did share with him that the ultimate goal of science is a social application, given to it by the power that it can have in controlling the environment and other people. The specific meaning of 'social' was, of course, different in the two men, and the ways in which to achieve it differed too. While Rothschild leaned towards and economic view of the world, Dainton preferred an ill-defined "social progress."

But is important to recognise that in the fight of placing science in society, those who defended the 'pureness' factor by arguing for the need of basic research *for* the ultimate goal of benefiting society (Dainton), had a worse starting point than those that argued for a simplistic model of science-funding and science-benefits (Rothschild).

The kind of patronage that both men aimed at operationalising with their proposals was one which valued the production of goods, be it for a return in financial investment, or for 'social progress'. Now that the game was set, and the scientific self was placed in a patronage system that made him virtuous by his capacity to produce, let us look at how scientists discussed the implications of this productive scientific self.

II. Acceptance of the customer-contractor principle "in principle", identity confusion and disagreement in practice

The lead editorials of *Nature* reflected the editors' acceptance of the main tenet of the customer-contractor principle: that science's ultimate goal was its application. However, and paralleling Dainton's approach, they questioned the pragmatic articulation of

[29] Our Special Correspondent, 'Shadow of Rothschild over Strathclyde', *Nature* 235, no. 5333 (14 January 1972): 71–73, https://doi.org/10.1038/235071a0.

[30] 'Dainton Demands a Hearing'.

Rothschild's recipe, while they generally approved of it "in principle". One of the first commentaries during the debate read, "So why not adopt the Rothschild recipe in its entirety? The difficulty, of course, is not a matter of principle but of practice," and continued "for every objection to the Rothschild recipe, there is a countervailing argument in its favour. And who will deny that it would be extremely valuable for everybody if British government departments were required by circumstances to develop close relationship with scientific research laboratories?"[31] This defence of Rothschild illustrates how the magazine supported the idea of scientists becoming contractors.

Scientists themselves were also ready to become the government' contractors. An event that took place shortly after the publication of the Green Paper shows a first heterogeneous set of reactions of working scientists to the government report and illustrates this readiness. The Council for Scientific Policy convened a meeting in the first week of 1972 at the University of Strathclyde to gather thoughts and opinions of academics on "the problems facing university science."[32] The publication of the Green Paper, however, "overshadowed" the event, and the discussions circled around its interpretation. The first initial reaction seemed to be, taking the words of *Nature*'s correspondent, of partial agreement: "one of the most surprising aspects of this session was the general acceptance of the customer-contractor principle for applied research. Sir Frederick [Dainton] said that this principle was right, a view shared by Sir Brain Flowers," and the correspondent recognised "only one dissente[r]—Professor Patricia Lindop," from the University of London, who could not consider Rothschild's report "a serious document."[33]

The "agreement in principle," however, "did not stifle widespread disagreement with Lord Rothschild's application of the principle," the *Nature* reporter assured, and even Brian Flowers, who had shown agreement with the principle compared the Rothschild's report to a PhD thesis "that contained much good and original work but unfortunately the good work was not original and the original work was not good."[34] Alan Hodgkin, who at the time was the president of the Royal Society, voiced the widespread fear that the Rothschild recipe would diminish the autonomy of the Research Councils. A representative of the Natural Environment Research Council, F.H. Steward, was however only partially sceptical and favoured a tighter connection between government and councils. As these examples identify, *Nature* reported a heterogeneity of responses, a mixed confusion between the Strathclyde attendees. Professor D. Johns from the University of Loughborough was "concerned that if the customer-contractor system was instituted then university departments would become customers so that in the end there

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[31] 'Smoke But No Fire So Far'.

[32] Our Special Correspondent, 'Shadow of Rothschild over Strathclyde'.

[33] Ibid.

[34] Ibid.

would be no need for research councils."[35] While Johns was right that the aim of the Rothschild's report was to introduce policies diminishing the power of the councils, he seemed confused about *who was who* in the customer-contractor system. It wasn't true that the *university* departments would become the customers, but rather they and their researchers would be contractors, while *government* departments would be the customers.

The defence of basic science by the Strathclyde attendees was similar to that of most *Nature* editorials that critiqued the Rothschild report. For instance, in 'How Much Would Rothschild Cost?' the journal editor did not attack Rothschild per se, but questioned how it would work in practice, affirming that nailing down precisely what needed to be researched was not an easy task. To the author of the lead, most likely the editor in chief of *Nature* at the time, John Maddox, argued that assigning the governmental ministries the competence to delimit and specify an area of research was hopeless. The lead editorial read, "the fact is that, as things are, most ministries are ill-prepared to ask questions of the research councils with specific precision."[36] In bringing to the fore the need for expertise as a necessity for knowing what needed to be known, the editor of *Nature* not only positioned scientists as relevant for the management of research funding, but laid a concrete definition of 'basic research': asking the questions "without which sensible answers cannot be produced."[37]This definition of basic science as research which specifies sensible questions so that sensible and socially relevant answers can be produced constituted one of the main critiques towards Rothschild, and the main argument in defence of 'basic science': 'basic science' identified what was worthy of interest. The need for a science that produced a social benefit was not put into question, and rendered *Nature* to admit to "the usefulness of the customer-contractor principle."[38] An intimate contract between statemen and scientists seemed the best way to establish "the strategy for scientific research," which would generate knowledge worth the tax-payers money:

> The essence of Lord Rothschild's case is that in advanced societies such as Britain, taxpayers and the governments, [sic] which represent them, should play a more active and direct part in plotting the strategy for scientific research. In other words, the customer-contractor principle should have brought joy to those who have been urging in recent years that science should be more socially responsive (which is why it is a little odd that the British Society for Social

[35] Our Special Correspondent.

[36] 'How Much Would Rothschild Cost?', *Nature* 235, no. 5332 (7 January 1972): 1–2, https://doi.org/10.1038/235001a0.

[37] Ibid

[38] 'That Was the Debate, That Was'.

> Responsibility in Science has ranged itself in opposition to Lord's Rothschild's proposals).[39]

By placing 'basic research' as the kind of science that should *guide* applied research and generate 'sensible questions', The conceptualisation of 'social' was relatively narrow in *Nature*'s editorials, confined to few areas of science (such as molecular biology) and more importantly, rendering scientists the only group in charge of what was worthy of attention. No wonder that, as we have seen in the citation above, the meaning of 'socially responsive' differed substantially with the so-called British Society for Social Responsibility in Science. A topic and a pressure group to which we will return later. For now, let us take a closer look at what 'social' meant to *Nature*, and extend this investigation to the journal's concept of 'political'.

III. Scientific work is work, but what is 'social' and 'political' to *Nature*?

To understand what 'social' and 'political' meant to *Nature*'s editors and contributors, let me review some of the journal's lead editorials, as they illustrate the interests of the journal as an elite community of scientists and the virtues that this community praised when it came to science policy.

At the verge of the 1970s, with John Maddox as its recently appointed lead editor, *Nature* positioned itself as cautious and explicitly sceptical of cherishing 'technological' as an isolated value of scientific practice. The editors expressed doubts on whether technology was good on itself, arguing that technological research required a specified economic application for its success. A lead in 1967 addressed 'The Technology Gap', a phenomenon that allegedly described the European lag behind the USA's in terms of technological development. Maddox addressed this issue with certain distance, trying to contextualise the debates, which he thought were a "great deal of well-intentioned nonsense."[40] The lead editorial didn't question "the technological feats now being carried out in the United States," and agreed that "the plain fact is that advanced technology is much less obvious in Europe."[41] Maddox, however, was cautious in the simplistic relationship that was too often thrown in the air: technological progress as delivering social progress: "By now there is plenty of evidence that technological developments alone do not bring prosperity."[42] The piece was specifically attacking big-science space

[39] 'That Was the Debate, That Was'.

[40] 'The Technology Gap', *Nature* 213, no. 5071 (7 January 1967): 1–1, https://doi.org/10.1038/213001a0.

[41] Ibid.

[42] Ibid.

programs: the European Launcher Development Organisation (ELDO), which was the first space research project among European countries; and the American Apollo program, who Maddox thought would "prove to be more a drag on the economy than an incentive."[43] This sort of large projects were put into question by Maddox, who said that the £100 million invested in ELDO were "unlikely to add much to the industrial competitiveness of the [European] member countries," and that they could have been better destined to "pedestrian development work—computers or even diesel engines, for example."[44] Referring to "pedestrian development work" as scientific projects which would deliver direct consumer goods, Maddox argued that for technology investment to be profitable, a market that demanded those products was of utmost importance. He highlighted the need for science to be profitable in the context of the early years of the European Economic Community:

> The moral in all these cautionary tales is that lasting technological success depends on the existence of a healthy demand for its products. From this point of view, it is encouraging that the British Government seems to conceive of its European technological community in the context of the Common Market. [...] Mercifully, nobody seems to think that Europe can be turned into an engineers' Aladin's Cave by investing money and effort in spectacular technology.[45]

The 'technology gap' that Maddox dismissed as unimportant in 1967 in the context of big-science space project, was framed as actually problematic in other arenas. The magazine pushed for devoting political interest to technological development only when there was a market that could be capitalised on and customers to supply technoscientific goods to. In 1969, the goods could be, for instance, research instruments and the customers were scientists themselves. An unsigned lead chronicled on the recent report by the Organization for Economic Cooperation and Development, which dealt with the status of scientific instrument industries in Europe, Japan, and the USA. The report described the greater success of the American companies as compared to the Europeans, and the difference was interpreted to be due to a convoluted blend of legal structures, such as the Buy American Act, which made it harder for European companies to sell in the USA, and cultural factors, such as the idea that "the scientific instrument industry in America is genuinely science-based, in Europe is still remains something of a craft."[46]

[43] Ibid.

[44] Ibid.

[45] Ibid.

[46] 'Instruments: Another Technology Gap', *Nature* 221, no. 5178 (25 January 1969): 300–300, https://doi.org/10.1038/221300b0.

In both the lead editorials above, we see how Maddox (presumably) understood the economic value of science as embedded in legal, economic, and political frameworks at both national and international levels. Within these frameworks, the American market qualities were far superior when it came to the scientific instrument industry, and were missing from the European counterpart: "better marketing techniques", "shorter delivery times," "closer personal contacts," "more sophisticated use of market research," "easier availability of capital in the US," and closer "contacts between universities and industry."[47] All these are indeed legal, economic, and political characteristics, but which had a profound ethical undertone. For *Nature* as the representative at top-tier European science, the question was whether these qualities were something that 'the European' science should be looking up to: "There are clearly no easy remedies for the situation, nor indeed any evidence that Europeans *want* the kind of society which generates success on the American scale."[48]

From these snapshots at the verge of the 1970s decade, we can now take the time to look at how the conditions of British economy steered the discussion about science policy in *Nature* in a specific direction. Here we have seen the scepticism of *Nature* toward big-science and in favour of investing on science that had an economic value for citizens. What were some of the continuities and discontinuities through the decade of the 1970s?

The early 1970s, especially compared to the previous two decades, was a moment of abrupt reduction in public expenditure in scientific research. In this economic climate and an increased emphasis on 'applied research', where did the political sympathies of *Nature* lay? What did its editors think when they referred to the 'social relevance' and 'social responsibility' of science? In the early 1970s, *Nature* saw cuts in education and research by the British government, placed in a larger financial recession of the country as a whole. In this economic and political climate, the magazine positioned itself as defending the institution where most of its readers worked. The header of a February 1974 lead editorial read, 'University research in danger.'[49] The threat to academic science came from the recent report by the House of Commons Expenditure Committee, which had reunited representatives from universities, polytechnics, and industry to assess "the value of higher education and doctoral research" and determine the financial strategy of the government in this area.[50] By including representatives of the commercial sector, such as the Confederation of British Industry, the committee heard, unsurprisingly, the case for need of a higher-education that would lead to

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[47] Ibid.

[48] Ibid.

[49] 'University Research in Danger', *Nature* 247, no. 5440 (8 February 1974): 325–325, https://doi.org/10.1038/247325a0.

[50] Ibid.

industrial applications, and importantly, that doctoral training was "negligible" for that.[51] A remark which *Nature* thought was out of place, but that illustrated the social tension between universities and corporations:

> The Confederation of British Industry (CBI) described a "widespread industrial view that the system is producing a body of specialists in science and technology, the relevance and originality of whose research is often questionable". This ill-considered remark, out of place in the committee's terms of reference, epitomises the woeful relationship between industry and most university departments. Industry views PhDs as poor material, PhDs view industry as a poor place for their skills.[52]

The problem was not industry, according to the editors, but that "the half-informed committee" would side only with the reasoning of industrial groups, leaving doctoral training as the end of a culmination of one's career, not the beginning of one. Citing from the report itself, the lead editorial read, "we see [postgraduate education] as specialised training for mature students who normally will have shown talent and determination well above average both in the academic and professional worlds." This, for *Nature*, meant that the committee was discouraging students from entering into a PhD, the rite of passage of Academia.

The worry ran deeper than an institutional fight between industry and academia. *Nature* situated the division between the two as problematic for PhD's themselves, who, studying in universities and being left out of practical problems to solve: "So the PhD would be a narrower qualification, dominated by those in the academic field and even less in tune with societal needs," and the author (likely the editor David Davis) plead, "the government would do well to reject this poorly reasoned report, even though times are hard and relevance is the watchword."[53] The isolation of the ivory tower was still to be defended, even in these hard times.

The tension that *Nature* was facing was hard to solve. In the 1-page lead editorials, many contradictions arose. On the one side, editors vouched for the *continuity* of an isolated university: "Academics protest, quite rightly, that the PhD is not only a training in methods of research but also a means of preserving the continuity of university research—the aim of which is to generate new knowledge."[54] On the same page, they talked about an emerging kind of doctorate studies: the interdisciplinary PhD,

[51] Ibid.

[52] Ibid.

[53] Ibid.

[54] 'Problem-Solving PhDs', *Nature* 249, no. 5457 (7 June 1974): 501–501, https://doi.org/10.1038/249501a0.

which involves students “getting to grips with a real industrial problem which calls for an appreciation of economics, industrial relations and the like.”[55] There were some critiques to this kind of approach to PhD training, but *Nature* was quick in defending it:

> Some may say that to extend the PhD concept too much is to reduce the degree to a certificate of ability to solve problems. But does this matter? Provided that those who are really cut out to do research in the true sense of the word still have a reasonable chance to do it[,] the appearance of more doctors of problem solving (which is all some ‘conventional’ PhDs are at the moment anyways) seems to be much more of a benefit than a threat.[56]

What is important to see here is that *Nature* was defending the status of the university, the continuation of the structures that funded and enabled the ivory tower to continue “to generate new knowledge,” while still being open to the idea of generating knowledge that would be directed to practical use: ‘Problem-solving PhDs’, as the lead editorial was named.[57]

This concern of *Nature*, and in specific of its editor Davis, for scientific employment was obvious during the mid-1970s. In 1975, the journal created a one-timer ‘Manpower supplement’, whose introduction was authored by the lead editor. A collection of articles addressed the topic of the scientific working class in several countries, ranging from the UK to India, including Israel and Japan, among others.

> The purpose of this supplement is to warn that the question of the use of scientific manpower is increasingly not one which can be ignored. It is becoming abundantly clear that the system by which schools supply universities and polytechnics which supply schools, industry, government and themselves is not automatically self-regulating.[58]

The problem of ‘scientific manpower’ was an appropriation from another kind of professional shortage, visible during the early years of the Cold War as a worry of the American industry, which faced problems in recruiting scientific and technical staff after the declining birth rates during the Great Depression and the loss of skilled personnel

[55] Ibid

[56] Ibid.

[57] Ibid.

[58] David Davis, ‘Manpower Supplement’, *Nature* 255, no. 5506 (22 May 1975): 283–283, https://doi.org/10.1038/255283a0.

to the two World Wars.[59] In the *Nature* supplement, the meaning was different, as it dealt with university employment, but they both highlighted some of the same worries: the relationship between industry and academia, and between the state and universities. As we will see shortly, *Nature*'s editors advocated a strong presence of the state in regulating scientific career. This so-called 'regulation' came in two forms: change of the education system, and increased funding. As we will see in the next chapter, 'self-regulation' had a very different meaning as compared to the context at hand.

The problem was a growing realisation that the scientific career as supported by public investment was not sustainable, and that one's path in academia was becoming an increasingly rocky path: "The only things that can be said with much certainty about the scientific profession at present are that a young man entering at 21 cannot expect an interrupted ride to 60, and that the scientific community is going to hear more and not less of the word manpower in the next few years."[60]

The relationships between universities and their patrons were a serious preoccupation for *Nature*, a worry that was sustained from a tension between, on the one hand, the journal's ideal of science: the image of a disembodied and naturally-progressive Science; and on the other, the realities of national and international economics and politics: "scientists, particularly in government and industry, often experience a conflict of loyalties between their employer and the wider, often international, cause of science."[61]

The tension laid precisely on the sticky surface of this "international cause of science" and the people who were actually to perform that cause. The problematic itself was between what the disembodied Science structurally demanded, and the bodies' will for job security: "scientists [...] are increasingly looking for life-long tenure, although science itself proceeds more by revolutions and perpetual rejuvenation."[62] The problem was the "all-growing-old-together syndrome," which arose, according to *Nature*, from a recent historical exceptional point: the large increase in employment for research in the wake of the Second World War, coupled now in the 1970s with a decrease in public funding and the inability of include new recruits:

> In the 1940s, in response to the demonstrable success of scientists in the Second World War and the emergence of the Cold War, many new establishments were formed and many old ones recruited in large numbers. Likewise in the 1960s in Britain (and later in some other parts of the world) many new

[59] Steven Shapin, *The Scientific Life. A Moral History of a Late Modern Vocation* (Chicago: University of Chicago Press, 2008), 101.

[60] Davis, 'Manpower Supplement'.

[61] Ibid.

[62] Ibid.

> universities were built and absorbed vast numbers of young researchers as teaching staff. The degree of homogeneity in both these groups is undoubtedly higher than elsewhere, but homogeneity coupled with a growing seniority is not necessarily the best of environments for research.[63]

The worries regarding the legacy of the university and the academic research within as an employment system continued through the late 1970s. Carrier paths and larger national concerns were perceived by *Nature*'s editors to be intimately linked.

The leading pages presented both alternative career paths for young researchers, such as engineering and applied sciences, as well as political demands to government to changing the university system. Lead editorials such as 'Have you ever thought of going into industry?' are an example these hybrid pieces, which appealed for a remodelling of the relationship between universities, engineering faculties, technoscience corporations, and the government.[64] Taking as a starting point "the feeble productivity performance of British industry" and the plea of Fred Mulley, Secretary of State for Education and Science, who "[had] been urging students to think more of industrial careers and less of academe or the civil service," the staff of *Nature* situated the reason for the disconnect between companies and academia at the educational system itself.[65] Davis (presumably) said that the problem laid in recruitment, and that the gap was present in how research universities and engineering faculties had few exchanges with one another, caused by a lack of cross-teaching between the two, which lead to intellectual disciplinary barriers that were already erected "by students themselves."[66] Davis ended his lead editorial by a giving voice and support to proposals to introduce significant changes in the education system, especially regarding undergraduate studies, and aimed at bettering the British technoscientific standing in the world. The model for these changes was the system of universities of the USA, based on promoting "the student to delay the difficult specialisation decision as long as possible." While "critics claim that this leads to academically light-weight graduates," Davis wrote and with a rhetorically-charged question replied, "and who can say that in the United States industry has not profited from the flow of excellent graduates?"[67]

National industrial development and scientific employment were intimately linked for *Nature*. As we have seen, the British magazine advocated a change of academia based on making research more industrial-based, and the education and

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[63] Ibid.

[64] 'Have You Ever Thought of Going into Industry?', *Nature* 259, no. 5541 (29 January 1976): 257–257, https://doi.org/10.1038/259257a0.

[65] Ibid.

[66] Ibid.

[67] Ibid.

careers of scientists becoming more intermingled with the applied sciences. In addition we have seen how these lead editorials also called for direct involvement of the state to catalyse those changes. In November 1979, and shortly after the elections that had lead Margaret Thatcher to become the Prime Minister for the Conservatives after 5 years of Labour control of government, *Nature* implored the new government for changes in policies relating to scientists jobs, and related the stability of those careers to the strengthening of British economy. By bringing together careers, political decision-making, and national productivity, the lead editorial was successful in linking the interests of scientific workers with the interest of the British state.

Shortly after Thatcher's appointment, a 1979 lead editorial intended to transmit the need for pressing political action: "what is needed, and urgently, is some hard thinking about the nature of scientific careers and what government can do to make proper use of this absolutely central innovative resource."[68] Science was a productive resource, and its workers the necessary labour: "it is utter folly for a government bent on establishing economic recovery to waste its scientific resources — its scientists — through a *laissez faire* policy which both wrecks individual careers and discourages those rising on the ladder."[69] The piece paradoxically combined a distrust on neoliberal market mentality with trust on a meritocratic scientific ranking. "The relation of science to industry," the editor of *Nature* (allegedly) wrote, "must be encouraged."[70] The way of making this happen was to introduce an "apprenticeship scheme" in scientific education that, as we have seen, would reduce the distance between so-called science and so-called technology. This relationship was positioned within the power of the state, and the lead editorial plead for "some central government initiatives" that would be "required to coordinate these efforts within a broad demographic policy for science and engineering."[71] The distrust on self-regulating employment market was explicitly brought forward: "here is a case where science is too important to be left to the scientists, a case where the Prime Minister should exercise her stated aim of coordinating science policy where that is necessary."[72] The responsibility fell on Thatcher, not only because she was the Prime Minister, but because the "benefit" would be "not only of science, but also of the British economy."[73]

As we have seen, *Nature*'s editors were conscious that science needed politics. But did this mean that scientists had to become involved in it? The 1974 meeting of the

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[68] 'Time for a Policy on Scientists' Jobs', *Nature* 282, no. 5734 (1 November 1979): 1–1, https://doi.org/10.1038/282001a0.

[69] Ibid, original emphasis.

[70] Ibid.

[71] Ibid.

[72] Ibid.

[73] Ibid.

British Association for the Advancement of Science (often simply known as the British Association, or BA) clearly expressed the inescapable political positioning of science and the need for scientists to form a pressure group, a position that was backed by *Nature*. Magnus Pyke, Secretary of the British Association and Chairman of Council, told *Nature* that he felt "that the function of the BA is to communicate a policy of social responsibility," that times were new and the "the process of communicating science has changed completely since the early days of the BA."[74] To "communicate a policy of social responsibility" meant having political power by bringing voters on their side: "He stressed," the *Nature* reporter informed, "that it is the aim of the BA to educate members of the public so that they will be placed in a position from which they can exercise their rights as informed voters."[75] The Association *as* a political group had finally put efforts in 'educating the public', a role that *Nature* thought was overdue. However, the Association knew that 'the public' was a heterogenous mass of people with varying interests and potentials to become politically sided with the Association's goals. As such, they knew that "schoolchildren seem to be the part of the population which the BA reaches most effectively."[76]

A few pages later, and also regarding the meeting of the British Association, a piece revealed that 'political' is not only an analytic category, but was an actor's category too. 'Science and politics of molecular biology' was an interview with John Paul, Professor at the Beatson Institute for Cancer Research, and attendee to the meeting. Lauding the triumphs of curative medicine, "biomedical research," Paul said, "had brought the infectious diseases more or less under control."[77] However, "we are now left with a residue of degenerative diseases and the second most frequent cause of death—cancer, which had proved difficult to combat by traditional means." These 'traditional means' had to be equipped with modern molecular biology tools that, in the professor's eyes, would soon deliver stronger solutions for medicine. "We are on the brink of a rapid advance in the understanding and treatment of cancer," the reported quoted, "this will result from the application of the ideas and methods of molecular biology." The political dimension of this hope was increased funding. At the time of the interview, molecular biology funding was 5% of the Medical Research Council budget, which although substantial, it was "very modest financing compared with for instance the United States," Paul said. As such, scientists shouldn't be only researchers, but members of a pressure group to demand larger funding proportions. "Molecular biologists should not hesitate

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[74] 'The British Association at Stirling', *Nature* 251, no. 5471 (13 September 1974): 90–90, https://doi.org/10.1038/251090a0.

[75] Ibid.

[76] Ibid.

[77] Eleanor Lawrence, 'Science and Politics of Molecular Biology', *Nature* 251, no. 5471 (13 September 1974): 94–94, https://doi.org/10.1038/251094a0.

to become involved in the 'political' arena and argue on political and social grounds for an increase in government finance as it is becoming increasingly clear that molecular biology ("that apparently most abstract of biological sciences") is a highly political subject."

Nature clearly conceived science to be a political affair, and in the case of the 1970s, through an intimate relationship between scientific "manpower" and a "feeble productivity performance of British industry". The way by which *Nature* demanded to operationalise this relationship for the better of both scientists as workers, for the better of the nation as a world economy, and for science as an "international cause", was to push political officials for increased funding, and for the academic world to change the training of scientists towards an increased focus on the industrial application of knowledge. As such, *Nature* commented on the situation of the scientific patronage system and aimed clearly at finding the sympathies of the state by alluding to the economical productivity of science. However, was this the only way that one could comment on the scientific patronage system of British science? Could we envision another take of what 'political' meant for scientists?

IV. An alternative meaning of 'social' and 'political' (placed on the margins). The answer of the British Society for Social Responsibility in Science to Rothschild and Dainton, and what *Nature* thought of these 'radical' scientists

At the end of the section where I discussed the Rothschild and Dainton report, I briefly mentioned a group of actors. Let me remind the reader of the exact phrase as published in the 1972 *Nature* lead editorial: "the customer-contractor principle should have brought joy to those who have been urging in recent years that science should be more socially responsive (which is why it is little odd that the British Society for Social Responsibility in Science has ranged itself in opposition to Lord's Rothschild's proposals)."[78]

This citation did not reflect an honest curiosity about why the British Society for Social Responsibility in Science (BSSRS) opposed Rothschild's proposal. It was a sarcastic question aimed at illustrating the seeming paradox of the BSSRS's political positioning to Rothschild's proposal for making science "more socially responsive."[79] The word play simply illustrated the multiplicity of meanings of 'social responsibility', 'socially responsive', or the words that attempted to clarify the relationship between science and society, or as I have called here: the *placement* of science within society. Before we get

[78] 'That Was the Debate, That Was'.

[79] Ibid.

to the society's response and their own account of how should the scientific self be placed, let me present this political pressure group and contextualise how these 'radical' scientists were presented in *Nature*.

As we have seen, *Nature* discarded the position of the BSSRS towards Rothschild's paper within a parenthesis and without giving much commentary to the Society's place within the funding debates. In fact, throughout the debates as appeared in *Nature*, this official sounding yet politically engaged society remained at its margin. Established in 1969, with Maurice Wilkins as president, the BSSRS was born intellectually through several scientists becoming involved in anti-war activism in the context of the Vietnam War. The association of science with military development, such as a chemical and biological warfare was one of the main concerns of the society. The 1970s were a period of heavy activity for the BSSRS: they organised open conferences, formed local research-activism groups, circulated a newsletter, and published their magazine *Science for People*. The topics in which they did research-activism was varied: ranging from how science education was funded and performed, to the dangers of chemical pollutants in factories. They were also concerned with the applications of the upcoming genetic engineering techniques, as well as privacy of data and the place of women in science.

The BSSRS was openly political and pushed for an understanding of science as inevitably placed within national politics. They were especially attentive to the relationship between scientists and their patrons, and how expertise was embedded in political and economic world. From their manifesto we can read: "There are no 'experts' to decide whether supersonic travel is preferable to the disease-resistant varieties of wheat [...] Science and technology serve the interests of those who fund them. And in serving these interests, they help perpetuate them. To a considerable extent, therefore, science and technology have become instruments of state and industrial power."[80]

Nature often labelled this group as 'radical', as this group was "openly red at times."[81] However, historian Alice Bell has shown how the society had a heterogenous formation. Its first meeting took place at the Royal Society with an opening speech by Maurice Wilkins, who had been awarded the Nobel Prize in Physiology or Medicine in 1962. As such, we can safely say that the BSSRS was, in many ways, part of the elite scientific class, and as Bell as shown, "following a long tradition of socialist scientists."[82] However, some of the younger BSSRS members were indeed more 'radical'. The society made itself quite visible as a grassroots activist group after the 1970s meeting of the British Association. During the meeting held that year in Durham, some BSSRS members

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[80] BSSRS Manifesto, ca. 1969–70, as cited in Alice Bell, 'The Scientific Revolution That Wasn'tThe British Society for Social Responsibility in Science', *Radical History Review* 2017, no. 127 (1 January 2017): 149–72, https://doi.org/10.1215/01636545-3690930.

[81] Bell.

[82] Ibid.

flew a banner with the slogan: "Science is Not Neutral." The activist scientists coupled their action with a street performance outside the conference's venue, "acting the effects of the chemical and biological warfare."[83]

Early after the BSSRS's inception, *Nature* was suspicious (if not outright against) of its goals. With a lead editorial titled 'Social Irresponsibility in Science,' the author (most likely the editor John Maddox) opened with "The British Society for Social Responsibility in Science is perhaps more than other new organization in great need of that claim o[f] public attention which the Americans call credibility."[84] The lead editorial argued for the need for a "sober examination" of the "the social problems occasioned by modern science and, more particularly, by modern technology." However, it differed conspicuously with the means of the BSSRS. For example, while the editors of *Nature* thought that a discussion on the social and ethical problems of the nascent field of genetic engineering was needed, they thought that the discussions that the BSSRS stimulated were "muddled and incoherent, jumping from issue to issue."[85] The BSSRS had organised a meeting in 1970 to discuss the field of genetic engineering and the duties of molecular scientists to the ethical issues which the new technologies arose. During the conference, there were debates on topics ranging from the need for more funding for finding genetic markers of cancer, to the problematic ethical and scientific reasons of the genetic basis for racial differences in intelligence; from the controversy over whether males with an extra Y chromosome showed increased violence and aggressivity, to the possibilities of in-vitro fertilisation. The meeting, which *Nature* thought to have provided "very few answers", caused some readers of the magazine to weigh in, with some critical correspondence being published in issues thereafter.[86]

Maddox thought that bringing together such issues, which in his eyes were only loosely connected, meant that discussions over genetic engineering as organised by the BSSRS often "tailed off into discussion of the process of radicalization of science and even some of the familiar problems of Vietnam."[87] *Nature* advocated instead for a closer cooperation between the BSSRS and the British Association, and less "interrupting public meetings" by BSSRS members, referring to the performance-action at the 1970 Durham

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[83] Alice Bell, '"Science Is Not Neutral!" Autumn 1970, When British Science Occupied Itself', *The Guardian*, 8 September 2014, http://www.theguardian.com/science/political-science/2014/sep/08/science-is-not-neutral-autumn-1970-when-british-science-occupied-itself.

[84] 'Social Irresponsibility in Science', *Nature* 229, no. 5286 (19 February 1971): 513–513, https://doi.org/10.1038/229513a0.

[85] 'The Biologist's Dilemmas', *Nature* 228, no. 5275 (5 December 1970): 900–901, https://doi.org/10.1038/228900a0.

[86] C. B. Goodhart, 'The Biologist's Dilemma', *Nature* 229, no. 5281 (15 January 1971): 213–213, https://doi.org/10.1038/229213c0.

[87] 'Social Irresponsibility in Science'.

meeting of the British Association as "public spectacles of various kinds."[88] The lead editorial advocated for recovering the "diplomatic relations between the two bodies," who were thought to have been "severed." But above all, Maddox thought that it would be "proper for the [BSSRS] to show that on some occasions at least it can provide the world not merely with the definitions of problems which it considers to be important but also now and again, with some attempts at constructive solutions." In short, they thought that flying banners such as 'Science is Not Neutral' in front of the scientific elite constituted a problem of credibility for the society's goal. "With so much talk and little work to show, the society has unknowingly as serious a problem on its hands as the British Association."[89]

The BSSRS responded to this *Nature*'s editorial, and their response was also published in the journal. Nine members, including Jerome Ravetz, mathematician, and would-be historian and philosopher of science; Hilary and Steven Rose, respectively philosopher of science and neuroscientist; Jonathan Rosenhead, mathematician and candidate for Parliament for the Labour Party; David Wield, who would become Lecturer in Science, Technology and Innovation Studies; and Maurice Wilkins (the BSSRS head) himself, argued their wish to not associate with the British Association by saying that they had "crucial differences of approach" between the two organisations.[90] These commentators of science, which as we have seen, many would become professionalised commentators in the field of History and Philosophy of Science, and Science, Technology, and Society Studies, positioned the BSSRS as "concerned with analysing the values of modern science, the relationships of science with society, and the ways in which these relations can, were necessary, be changed."[91] They were quick to establish themselves as separate from the elite, "unlike *Nature*, or, apparently, the British Association's council, we do not feel it possible or desirable to divorce the ethical problems of science from," citing *Nature*'s editorial itself, "the processes of 'radicalisation of science'."[92] In response to the accusations of the journal of "much talk and little work," they responded

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[88] Ibid.

[89] Ibid.

[90] Peter Chapman, David Dickson, Shivaji Lal, Jerry Ravetz, Hilary Rose, Steven Rose, Jonathan Rosenhead, David Wield, and Maurice Wilkins, 'Responsibility in Science', *Nature* 230, no. 5288 (5 March 1971): 67–67, https://doi.org/10.1038/230067a0. It may be important to note that the social and intellectual overlapping between the BSSRS and the field of history and philosophy of science during the 1970s decade was significant. In the tenth anniversary of the society, Joseph Needham spoke at the event and celebrated and advocated for socialist politics to which the BSSRS subscribed to. *Nature* described his contribution to the society as follows: "Joseph Needham, who is as close to a patron saint as is possible for a political organisation to have" in Joe Schwartz, 'UK Radical Scientists Ten Years On', *Nature* 282, no. 5738 (1 November 1979): 434–434, https://doi.org/10.1038/282434b0.

[91] Chapman et al., 'Responsibility in Science'.

[92] Ibid.

that the BSSRS had offered proposals for policy in the topics of "herbicides, sponsored research in universities, and the dumping of radioactive waste."[93] Their aim was larger, they claimed, as their ultimate objective was "that of analysis and criticism through which we can mobilize the social conscience both of the scientific community and of society in general."[94]

Now that we've reviewed some of the basic political positioning of the BSSRS, and its reception in *Nature*, let us move into the society's position in regard to science policy. In specific, what was the BSSRS's view of the debates over the structure and funding involving the 1971 Green Paper? Could we consider it an alternative, and how far was it from Rothschild and Dainton's proposals?

As the reader may remember, shortly after the publication of the Green Paper, the government had leaned towards backing the customer-contractor principle and had invited to comment on it and discuss possible ways of operationalising it within a national policy. As one of those commentators that I have brought up here, Jeffery Leigh, had put it, "The government has accepted and endorses the customer-contractor principle, and has invited comment, not on whether it should be put into effect, but on how it should be implemented."[95]

The BSSRS was aware that the British government was setting the possibilities of discourse by inviting "constructive" responses like Leigh's and rejecting "discussions which simply emphasize shortcomings of the Rothschild report."[96] The scientists-activists, however, weren't satisfied with such framing: "the Government's invitation to make comments is restricted to the detailed application of this principle; however, the BSSRS cannot accept this limitation."[97] The BSSRS responded to the Green Paper with a 9-page document, but their voice wasn't re-printed in *Nature*.

The response laid down the BSSRS's political views in the context of the two government-issued reports, both of which were quickly attacked and dismissed. The authors called Rothschild's "perfunctory to a point beyond naivety" and thought of it as "a blunt attempt to apply a simple market mechanism and philosophy explicitly and directly to the conduct of scientific research."[98] Dainton's report was seen as a "diplomatic proposal" that exemplified "the scientific establishment bending before the

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[93] Ibid.

[94] Ibid.

[95] Leigh, 'Proposal for a Constructive Response'.

[96] Ibid.

[97] British Society for Social Responsibility in Science, 'Government Research and Development. Comments on the Green Paper "A Framework for Government Research and Developmewnt" (Cmn 4814, 1971)' (London, January 1972), K/PP178/11/1/24, King's College London Archives (Wellcome Library), https://wellcomelibrary.org/item/b20050069.

[98] Ibid.

wind of governmental displeasure about science's economic unproductivity."[99] His "formula", the BSSRS thought, "lacks the neatness of Rothschild's scheme, but marches several paces in the same direction."[100] With their outspoken discontent with a marketable view of science, the BSSRS critically opposed both its radical expression of the customer-contractor principle and the moderate attempt of Dainton to reconfigure the RCs. The mid-point that the scientific establishment offered as a critique of Rothschild, visible in the *Nature* pieces I documented above, was largely dismissed by the radical scientists and their moderate views were still seen as "unimaginative."[101] Both approaches to policy, the BSSRS denounced, were attempts to preserve the power of science within the elite: "the status quo has been praised complacently."[102]

This much for what the BSSRS did not want and for the points of disagreement with the proposals. But could this group of activist scientists have anything in common with Rothschild' and Dainton's proposals?

To begin answering this question, let me argue that the BSSRS sided more with Rothschild than with Dainton, as the former (alike the BSSRS) demanded a closer politically control of scientific expenditure. While the Dainton's supporters aimed for a professional yet independent science, Rothschild's recipe wanted to eliminate the RCs as buffer institutions. The BSSRS agreed with Rothschild in the anti-isolationism point and argued for "the necessity for a form of social control over applied scientific research which is non-bureaucratic, and more directly responsive to the needs of the community."[103] The BSSRS made use of the response of mainstream scientists to illustrate the "lack of social awareness and sensitivity among scientists," and continued, "while leading scientists extol traditional decision-making by committees of scientists, Rothschild has at least placed science policy where it has always belonged — in the arena of political debate."[104] This was BSSRS's message for mainstream scientists, who they viewed as politically unaware and socially irresponsible. The BSSRS's push for understanding science within the political world ("Science is Not Neutral") aligned them, if briefly, with Rothschild's wishes to making science political. The meaning of 'political', however, was very different. For Rothschild this meant reducing the distance between spending and government control; and for the BSSRS, bringing 'politics' into science meant reducing the distance between science and 'the public'. As I will show below, this 'public' was articulated with a pluralistic, power-laden conception of society.[105]

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[99] Ibid.

[100] Ibid

[101] Ibid.

[102] Ibid.

[103] Ibid.

[104] Ibid.

[105] Ibid.

In fact, the BSSRS's evaluation of science funding positioned 'power' as central analytic category. Their two main axes were 1) that there was an inherent conflict of interest in leaving funding up to the government, and 2) that scientific progress was or could be a contributor to, and not an ally against social inequality. By bringing 'power', and with an accent on state power, to the assessment of science policy, the BSSRS highlighted the bias that could result from centralised governments being the only institutions dictating what should be researched:

> An education authority, for example, has recently obstructed research into the relationship of delinquency and schooling. This illuminates the fallacy in Rothschild's approach. A Government department, and its political head, have a vested interest in *not* sponsoring research which might undermine established policy.[106]

In denouncing the monopoly of state power and its link with for-profit corporations, they drew a picture of the British government as a "corporate state".[107] They asserted, that this "corporate state" should not be the only one sponsoring research, and that while it was commonly seen as "*the* sovereign agency" it should in fact be seen "one social institution among many."[108] Their defence of a pluralistic funding system was based on an understanding of a plural society. A plural society that deserved a plurality of institutions. Based on the ideas of British activist and historian Edward Palmer Thompson, they suggested a radical restructuring of scientific funding, with the creation of institutions modelling the RCs, but with a profoundly different constitution. It is worth citing a large portion of their suggestion:

> [H]ere is a clear role for Research Councils, or some new public institution, to be free, and indeed encouraged, to carry out research on the problems of these groups — the poor, the under-housed, the discriminated against, and many others — who cannot sponsor research themselves. As E.P. Thompson has written (in 'Warwick University Ltd') "There is not, of course, in Britain *one* public, but many different publics, with different demands, needs and values. Hence to respond to social demands does not mean to respond instantaneously to one particular indicator of demands — government policy or the policies of senior industrialists — but to take part, at many different levels in society, in the argument between differing indices of social priority." [...] Our suggestion is, therefore, the establishment of one or more Community Science Research

[106] Ibid. Original emphasis.

[107] Ibid.

[108] Ibid.

> Councils. The C.S.R.C. would receive funds from the Government (and other sources if available). The C.S.R.C. decision-making bodies would consist of scientists (or others) appointed by the Government; scientists selected by the staff of the C.S.R.C.; and nominees elected by grass-roots community groups. To develop a pluralistic funding system, several such councils should be set up eventually [in which] topics could be suggested by community groups, or others.[109]

These "Community RCs" were never set up, but their suggestion was demands closer inspection. The kind of science that the BSSRS envisioned and promoted was very community driven. In the 9-page paper commenting the 1971 Green Paper, they explicitly sketched their ideal science, which would be "both 'critical' and 'socially responsible'."[110] By 'critical science' they mean that it should focus "on problems whose recognition is inconvenient or embarrassing to powerful interests and institutions in our society," thus putting an emphasis on the inherent relationship between power and knowledge.[111]

In their definition of 'socially responsible', the BSSRS further elaborated on this power-knowledge vision. They wrote, "Our society also advocates an attitude of 'social responsibility': a recognition that there is marked inequality of access to that expertise which is essential for the control of our social and natural environment."[112] They also exemplified the difference between critical and socially responsible science: "a scientist may do very 'critical' work in discovering the source of a particular atmospheric pollutant (e.g. a factory stink); but if he merely reports this to his superiors or publishes it in very specialised journals, ignoring the requests of the local community, he is *prima facie* lacking in 'social responsibility'."[113] What it is important to recognise here is that the BSSRS did not deny the capacity of science in changing society, they simply didn't recognise it as politically neutral.

Despite Dainton's naïve defence of scientific isolationism, the BSSRS had shared discursive elements when it came to the capacity of science of exerting change. If you may remember from the first section, Dainton had said, "science gives man power over his environment and regrettably over his fellow man."[114] Parallelly, the BSSRS wrote that 'social responsibility' was "essential for the control of our social and natural environment." But where Dainton saw a 'regret', the BSSRS saw a 'necessity'; while one

[109] Ibid. Original emphasis.

[110] Ibid.

[111] Ibid.

[112] Ibid.

[113] Ibid. Original emphasis as underlined.

[114] 'Dainton Demands a Hearing'.

emphasized the need of a pure and undisturbed science, the other saw the duty of positioning science in a political world and leading social change. They both agreed, however, that science gives power.

V. The socially responsible scientific self as a politically involved expert

The 'social' of the BSSRS referred to a pluralistic, power-laden society. To construct such society, what kind of scientist was needed? Or to ask its mirror image, what sort of scientific self emerged from the BSSRS's claim that "Science is Not Neutral"? What did it mean for the individual scientist to perform science within the power-laden, plural society that the BSSRS envisioned? We have gotten a glimpse of this image from the commentary made of the group to the Green Paper, where they outlined two virtues that the scientists must have upheld: 'critical' and 'socially responsible', thus making the *object* of study and how the research was *communicated* ripe for ethical assessment. Not all research was virtuous, and not all manners of presenting it were either.

The BSSRS continued building up on their project that not all scientific practice was equally virtuous, and in 1972 they published the report, 'The social obligations of the scientist'. The report was signed by the BSSRS's Working Party on Science and Ethics, which was composed mostly by biomedical scientists (a bacteriologist, neurochemist, biochemist, and psychiatrist) but containing as well two moral philosophers and a barrister. It may be important to look at the contents of this document, as it highlights not only how the BSSSR's views on how science as a knowledge-producing activity related to the political realm, but on some of the virtues which they thought scientists should have, and the different ways of operationalising them. As well, we will see that the way by which the BSSRS intended to operationalise the scientific self was not, despite the claims of *Nature* and the more visible street-bound activism, 'radical'. The path that the BSSRS offered to remodel the scientific self was, for the most part, institutional.

The problem that the BSSRS was trying to solve was already a few decades old. They argued that science was, since the atomic bomb and now with issues such as genetic engineering or biological warfare, perceived by 'the public' as an ethically suspect activity. They saw scientists as being looked down, or merely looked at, for the work that they were doing. In a climate of blame and scorn by public opinion, "the scientist becomes troubled about the extent of their moral responsibility." The following proposal, the BSSRS thought, aimed at alleviating and clarifying this responsibility.

'The social obligations of the scientist' was a sort of position paper, which placed itself within several solutions that were being proposed to address the problem, all of which they rejected. First, they objected to developing "something in the nature of a Hippocratic Oath or code of ethics for all scientists," which they saw as a top-down approach which would only set up some "series of pious generalities" unable to aid the individual scientists in their everyday ethical decisions. Another solution which they rejected was to leave "moral and political decision about the social application of scientific work increasingly to scientists themselves." The BSSRS, and this should come to no surprise to the reader, was deeply sceptical of being able to separate 'the scientific' from 'the political'. Finally, they rejected the most drastic proposal of all, "a radical – if not revolutionary – reform of the whole social system." Separating themselves from socialist and communist revolutionaries, they were quick to argue for *reform* instead of *revolution*, and quick also to denounce violence. In view of these alternatives that the BSSRS rejected, the report proposed "something purely pragmatic, quite small-scale in cost and effort, but whose effects could help to bridge an important gap," the one between the scientific and political classes.

Their report situated the BSSRS within a long tradition of a fear of modernity: a society evolving too fast and not being able to adapt, which had moved too quickly from the agrarian to the industrial society, and even more now from the "motor car" to the "jet aeroplane and the nuclear weapon." They feared that the increasing interdependence and interconnectedness of the world made the reach of technoscience increasingly unpredictable. Their worries and their advocacy for institutionally constructing the possibility "to think, talk, and think again for long enough" was translated into an examination of whether the scientific class had any role to play in avoiding the dangers of modernity, and if so, how. "We need to examine what [the moral responsibility of scientists to society] amounts to. To what extent, if any, has a scientist, *as* a scientist, special moral obligation to society, over and above those which he owes in his ordinary capacity as citizen?"[115] In short, was the scientist in a better position to slow the world down?

Just as different social actors (doctors, parents, drivers) have a set of different legal and moral obligations as their social category, the BSSRS argued, scientists should be held responsible *as* scientists. Indeed, the document argued that scientists were accountable because the products of science could have an impact (good or bad) to society, and as experts, scientists were the ones at a better disposition to think about their given impact. As such, the BSSRS's claim that science was political did not drag along with it any postmodern assumption of diluting expertise. Precisely because of their expertise, scientists needed to be held accountable.

> The outcome of scientific work can often have a great impact, for good or ill, to other people. Quite frequently, scientists can predict this outcome earlier, and

[115] British Society for Social Responsibility in Science Working Party on Science and Ethics, 'The Social Obligations of The Scientist' (London, 1972), K/PP178/11/1/22, King's College London Archives (Wellcome Library), https://wellcomelibrary.org/item/b20050045. Original emphasis.

> more accurately than others. Sometimes they can even modify the results. One could claim, therefore, that scientists are in one of those special positions which give rise to special obligations.

Now that the document had concluded that scientists were responsible *as* scientists, what exactly were their obligations? The document was not shy in listing concrete obligations: "(a) to refrain from doing some kinds of work; (b) to do some kinds of work; (c) to contribute to a greater sense of social responsibility among other scientists; (d) to think out the social consequences of their own work and of other work in their field; (e) to inform the public about what is being done, and its likely consequences." Despite the list, some of the obligations were slightly vague. The first two, which related to the virtuousness of refusing or choosing to do certain kind of work, did not explicitly list research areas to avoid or pursue. It did, however, argue that those individual choices were "gestures" that were "socially important" because they "improve[d] the moral climate." By situating the ethical decision on the individual, they were aware of the limited reach of such approach to morality. However, they compared it to the democratic process: "one man's contribution may only be small, and the effect of any change in the moral climate is bound to take time. But then, the casting of one vote in an election can only have an infinitesimally small effect on its outcome," which didn't mean that democracy – and therefore, the individual assessment of the morality of research areas – was not to be pursued.

With point (d), they wanted to encourage in scientists their capacity and will to "foreseeing the consequences" of their work. They thought that it was not "good enough to put on the conventional blinkers, concentrate on the job in hand, and leave the consequences to others." This, the BSRSS argued, was a widespread attitude of academics, and one which fostered an image of isolation of the scientific community "held by many laymen". "Whatever virtues academic remoteness may be thought to further," the authors argued, "it is not a popular quality." In other words, whatever the epistemic virtues that were lauded *within* academia, meant very little *outside* of it.

We have seen how the document attempted to prescribe certain scientific self which had a view in his work as well as in society. As such, to be virtuous meant to have certain 'social responsibility.' Until now, we can recognise that this social awareness was, however, left to individual choice (*choosing* to do or not to do certain kinds of research, *choosing* to think of the consequences of one's work). Indeed, this framing of ethics as being up to the individual was explicitly stated. They said they were "matters for the conscience of individual scientists," and "in accordance with their personal moral values." However, their list also hinted at the need for their moral obligations to be "performed in an organised fashion."

As I have said at the beginning of this section, the BSSRS proposed to articulate this scientific self in an institutional manner. The BSSRS itself stood as an example of point (c), by "holding conferences, and the dissemination of newsletters and other publications." Complementarily to this, they designed a stronger institutional action that was to make science a matter of political interest: "[decisions about the social consequences of science], like all other affecting the community at large, must remain 'political' decisions, and must be taken by the organs of government which the community has chosen for itself. [...] In Great Britain, that means Parliament and the Government of the day, advised by the permanent officials of the Departments." Acknowledging that politicians and scientists were "busy men" with many other tasks at hand, the BSSRS proposed an institutional solution: "there is no single institution in this country whose main job is [to communicate between the organs of Government on the one hand and the scientific community on the other]. We think it is high time that it should be *someone's* job." Who was this someone's?

The proposal outlined a middle man between individual scientists and political organs, "a body, organised by the scientific community itself, and expressly charged with the task of informing the public in general, and the organs of Government in particular, at the earliest possible time, of all scientific work likely to have important social consequences for good or ill." The voice of this body formed uniquely by scientists "should not go unheard." The name of such body could be something like "Science and Society Council", and BSSRS's fear of a bleak modern future invited them to predict an apt nickname: "Doomwatchdog." But they were quick to also defend its positive qualities: "it will be at least as much concerned with beneficial as with harmful consequences."

The Council which they envisioned would be an institution which would be scientifically informed of current developments in research "which are likely to have social implications," so that it could make predictions and the "likely effect of that work on society." With this overall knowledge of what was being researched and the possible predictions, the ultimate job of this 'Science and Society Council' would be "to make recommendations to Government, Parliament, and the public" so that "beneficial effects" are promoted and "harmful effects" are restricted.

Individual scientist would participate of the Council's task in two ways. The first one, ideally carried out by all researchers, would be in the form of reporting to the institution of "any work on which a scientists is engaged and which in his view – or that of other whose opinions he think worthy of respect – is likely to have social consequences for good or ill." This reporting was voluntary and was envisioned as the mechanism by which individual scientists would contribute to social issues. It was how the middle ground was struck between focusing only on science, and becoming a full-time activist: "it relieves [the scientist] from having to work out for himself, often with insufficient resources, all the possible consequences of this work, let alone what would be the appropriate social response."

Another way which the individual scientist would interact with the Council was not expected of all scientists. The way the BSSRS envisioned it, however, illustrates the group's attentiveness to power relations in science policy. They argued that all scientists who should work for the Council in an advisory role (such as receiving the reports of individual scientists and predicting the benefits and dangers of research arenas) would do it without economic compensation. As well, they strongly advocated that the Council "should not be sponsored by the Government or by the Parliament, but by the scientific community itself" in the manner of offering its expertise for free. They envisioned voluntary work as to free the Council of the patronage issues which they were accustomed to and often denounced. This proposal illustrates the believe of the BSSRS in the patronage-system of science and the power dynamics at play in scientific funding-production. Their aim was to transcend this system of knowledge production and put expertise and its power to the service of 'the people'.

VI. Concluding remarks

In this chapter I have sketched here the different aspects of the discourse of Rothschild, *Nature*'s community (editors and correspondents), Dainton, and the BSSRS in regards to British science policy in the early 1970s. In trying to study their alliances and their visions for science, I have come to the conclusion that – in this case – the enemies of my enemies are *not* my friends. The ideological relationships between them are complicated and cannot be easily characterized in one dimension. Their relationship is multidimensional. At times, unexpected commitments can be found between seemingly contrasting views, such as Rothschild's and the BSSRS's shared assumption that scientists should not be left alone to their own devices. To try to comprehend such interesting and complex ideological commitments, I have simplified in the following table the discursive elements that I have been picking out and discussing in this chapter.

Scientists, editors, industrialists, government administrators, and the radical scientists of the BSSRS shared the main tenet that science deserved a place in society. What was up for grabs was *which* place it deserved.

Rothschild's recipe modelled itself on commercial companies like Shell and American science, and sold a simple and attractive metaphor: science produces a marketable goods: its management should follow market law. As such, the virtues of the scientific self that emerged from this political proposal were the ability to produce, to deliver "technological virtuosity"[116] and to contribute to the nation's economic prosperity

[116] Brooks, 'Rothschild's Recipe in the United States'.

more than he had received as funding. Customer-contractor science positioned the scientist self as a ‘contractor’, whose virtues arose from honouring this contract. In the eyes of some critics, the implementation of this science policy left two aspects of science on the side-lines: for Dainton and his supporters that was ‘basic research’, for the BSSRS that was ‘the people’. Both these concepts are analytically far from unproblematic, but their political articulation far from ineffective. As I have shown here, they deserve a historical attention.

Political views on science’s relation to society	Rothschild	*Nature*’s editors/ Dainton	BSSRS
Science is political	x		x
Pure science exists and it is good on its own		x	
Science delivers marketable products, and this should be encouraged	x	x	
Science lives in a plural society			x
Science gives power	x	x	x

For Dainton, the customer-contractor principle and the transfer of a quarter of the national science budget to non-academic departments who wanted *something* in return meant two things: it sent ‘basic research’ into financial oblivion and took away the institutional insulation that the Research Council system provided to academics. Despite the seeming adversity, Dainton did have sympathies with Rothschild’s main tenet: science gives power to produce. As such, the formulation of ‘basic research’ as the kind of science able to formulate ‘sensible questions’ and lay the groundwork for “tactical” and “strategic” science, already contained the assumption that the productivity of knowledge was its ultimate goal. As such, the prescribed virtues of the scientific self that emerged from Dainton’s proposal weren’t much different from those of Rothschild. The scientific self that emerged from this conceptualisation positioned him politically insulated, leaving any political involvement of scientists into a narrow crevasse: demanding more money and fighting for more insulation from government. Little more was expected from scientists.

Nature as a forum represented a complicated middle ground. The heterogeneity (and anonymity) of its contributors probably contributes to any philosophical confusion I have shown here. Despite this, the editorial staff did defend repeatedly a series of political positions: science as an inherently political object of national interest, and science’s products as necessary embedded in an economic world of consumers, on the one hand; and the need of a stable and productive academic workforce in touch with their political and economic contexts, on the other. The lead editorial and News and Views section often covered the topic of employment in science, and importantly, tied the problematics of the academic career to the interests of the state, in the case here, to the weight of industrial development for economy of Britain. The scientific self that the journal envisioned was positioned inevitably in a political world, as it made a knot between individual's career choice and the nation's goals. As such, fighting for the rights of the scientific class was a deeply political action, according to the journal. Any other kind of political positioning could simply labelled as 'radical'.

The BSSRS presented itself as a political and social group. As such, the ethical dimensions of the science policy they advocated were explicitly stated: they wanted to "improve the moral climate."[117] The scientific self that they envisioned was positioned as a politically involved expert, whose responsibilities came from the knowledge that he produced. Both the kind of work and how it was communicated were deemed by the BSSRS as fertile for ethical assessment, bound by an understanding of society as having moral plurality that needed to be protected, and by science as being inescapably power-laden. The way they proposed to operationalise the values of 'critical' and 'socially responsible' that the scientific self should possess was by the construction of institutions like the Community Science Research Councils or the Science and Society Council, which were supposed to shift the patronage system of scientists away from the nation's goals and the economic value of knowledge, and by finding a new patron: 'the people'.

In this chapter I have shown how science policy is intimately linked to a prescription of ethical values, that placed the scientific self in a specific relation to other social, economic, and political elements of the world. In fact, we can understand science policy as the way by which these values are operationalised.

[117] Working Party on Science and Ethics, 'Papers of M H F Wilkins'.

Chapter Three. **Regulating 'the self'.** The meaning and uses of 'technical' in the debates over the safety of recombinant DNA molecules (1972-1978).

This chapter characterises the discourse of scientists on the regulatory debates over recombinant DNA molecules. The production of these objects had only been possible through a set of molecular and chemical techniques developed in the late 1960s and especially during the first half of the 1970s that, for the first time, allowed molecular editing of DNA and massive copying of genetic fragments, promising to deliver biomedical solutions at industrial scales. However, some of these 'recombinant DNA techniques', as I will call them from now on, were thought to pose a safety hazard to the technicians operating with them, leading to heated debates on how should scientists proceed: should they stop all research employing recombinant DNA technologies? Should they continue with precautions, and if so, which ones? The knowledge necessary to assess risks was unstable, and the debates were heavily moralised, hinging not only on what counted as valid knowledge, but on what counted as 'good' science.

The convoluted ethical dimension of the debates allows me to take this chapter as a case for investigating a larger issue: how do scientists understand ethics in the workplace? How do they interact with it, and how do they engage with situations that require an ethical stance? These questions stand in the context of the main aim of this thesis: my attempt of drawing an image of what it meant to be a molecular scientist in the 1970s.

In short, I will show here how the discourse of *Nature*'s textual community was very moralising. Despite the image of profound rationality that scientists dressed on, the editorial pieces served as spaces to debate the ethics of science. But ethics played a curious role. When it came to their careers and their success, contributors and editors were quick to establish their privileged ethical-epistemic position within society, and the freedom that was due to them. Here I show how ethics was for the most part seen as an epiphenomenon, as arising *after-the-fact* of the molecular work. Prior to the outcomes of the research, 'technical' was synonym with 'technologically technical', and in the context of the regulation of recombinant DNA molecules and of their synthesis, 'technical' was employed for the most part in reference to the technical knowledge required for the manipulation of biomolecular techniques. Scientific policies were meant to be addressed within the confined area of this meaning of 'technical'. This narrow conceptualisation of 'technical' was challenged by some, who pressured for broadening to other scientific disciplines such as environmental biology in drafting regulatory policies. Some politically active scientists argued for an even larger conceptualisation of the knowledge necessary for assessing the ethics of laboratory work, including collaboration with political stakeholders, such as worker unions, or even the widely used yet ambiguous, 'the people'.

As with the previous lead editorial, I am interested in characterising the playing field where the debates took place. I want to describe the slopes and puddles of the football pitch, get to know the possibilities of the game that made certain team score easier, and caused others to get stuck in the mud. The editors of *Nature* would talk of both the possibilities of medical applications and the biohazards of recombinant DNA technology in an uneven way. While both were hypothetical situations, of the potential medical applications it talked as "exciting areas of research," and named a few such as "correcting genetic deficiency diseases, inserting nitrogen-fixing genes from bacteria into non-leguminous plants so that they would no longer require nitrogen fertilisers, and constructing novel strains of bacteria for such tasks as eating up oil spills."[1] The equally hypothetical biohazards were rather seen as a "remote possibility that, for example, cancer-causing genes could be introduced into a virus or bacterium and cloned."[2] While both well-founded conjectures, *Nature* talked of the hazards as to have "unpredictable biological properties", the "nature and likelihood of which can only be speculated upon."[3] Throughout the debates in the UK and the USA over the regulation of these biomolecules, two things were ubiquitous: the asymmetry between risk and benefits, and more importantly, their framing as a dialectic opposition between the two.

Here I analyse the discourses that took place in the pages of *Nature* between the different interest groups, covering the years 1972 to 1978. My analysis is not chronological. My main interest is in characterising *who* was supposed to deal with the ethical dimension of recombinant DNA technology and *how*. After a historiographical and historical review in the first two sections, I move on to section three to disentangling the elements that were problematic in the debates, such as the position of 'the public' or the mechanisms meant to safeguard the ethical questions in research. Finally, in section four, these elements come back together in the speeches of two speakers at a meeting organised by the largest British scientific trade union, the Association of Scientific, Technical and Managerial Staffs. The Intermezzos, I hope, will serve as a concentrated form of how solutions where proposed, and what (and how) problems arose when science was thought to be dangerous.

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[1] Colin Norman, 'Genetic Manipulation: Recommendations Drafted', *Nature* 258, no. 5536 (18 December 1975): 561–64, https://doi.org/10.1038/258561a0.

[2] Ibid.

[3] Ibid.

I. Historical methods, historiographic foundations: United Kingdom's molecular politics

My historiographical position aims to fill the gap that exists between the histories of the recombinant debate itself and those of *Nature* as a scientific publisher.[4] I characterise here the discourse of scientists as much as how the editors of *Nature* talked about and framed these debates, centred in the United Kingdom.

As a note of caution and for sake of transparency of my historical methods, I would like to point out that I have drawn heavily from the work of historian Susan Wright, author of *Molecular Politics. Developing American and British Regulatory Policy for Genetic Engineering, 1972-1982* (1994).[5] This book is comprised of several chapters, two of which had been previously published as "Recombinant DNA Technology and Its Social Transformation, 1972-1982" and "Molecular Biology or Molecular Politics? The Production of Scientific Consensus on the Hazards of Recombinant DNA Technology," in *Osiris* and *Social Studies of Science*, respectively.[6] During my research project, I began with these two articles as my historiographical backbone. In a rather unfortunate unfolding of events, I realised only after most of my primary source work had been finished, that the book by Wright contained a thorough investigation of what I planned on telling in this chapter. *Molecular Politics* offers a broader angle of analysis, covering more than the *Nature* pieces I discuss here, and including the history not only of the United Kingdom, but also of the USA. A significant proportion of my primary sources are also discussed by Wright. I worked and interpreted these sources before reading Wright's book, and most of the analysis still held even after having read *Molecular Politics'* more contextualised and exhaustive study.

Despite the seemingly ill-fated situation of this duplication, I was able to draw meaningful conclusions. The importance is twofold. First, that while this was not designed to be a 'replication study' of Wright's work, it turned out to be a non-formal yet revealing pseudo-replication, leading me to show that some of the central claims of *Molecular Politics* hold even after a quarter of a century from its publication.

The second issue of importance is significant because it says something about *Nature*'s editorial content as historical source. By reading this material and casually

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[4] I thus stand between two books: Melinda Baldwin, *Making 'Nature': The History of a Scientific Journal* (University of Chicago Press, 2015); and Wright, Susan, *Molecular Poltics. Developing American and British Regulatory Policy for Genetic Engineering, 1972-1982* (Chicago and London: The University of Chicago Press, 1994).

[5] Wright, Susan, *Molecular Politics.*

[6] Wright, Susan, 'Recombinant DNA Technology and Its Social Transformation, 1972-1982', *Osiris* 2 (1986): 303–60; Wright, Susan, 'Molecular Biology or Molecular Politics? The Production of Scientific Consensus on the Hazards of Recombinant DNA Technology', *Social Studies of Science* 16 (1986): 593–620.

overturning the thousands of pages of the magazine of the whole 1970 decade, I selected two aspects as important in order to understand the context in which the debates over recombinant DNA technology took place. My approach in this research had been simply reading *Nature* editorials without a clear aim, hoping to find an under-researched area of study. The two issues that I wanted to bring as historiographical additions to Wright's original two articles (before realising that there was a whole book dedicated to the topic and which also made this points) were 1) that politically militant and unionised scientists had been a significant pressure group within the science policy arena, and 2) that the existing regulatory framework of the scientific workspace and state of funding of university research had laid a meaningful foundation in which to discuss regulation of DNA recombinant molecules. These two issues turned out to also be discussed by Wright in her book, and in a similar fashion to mine. Importantly, I reached these issues before knowing I would be discussing them in the context of the debates over recombinant DNA technology regulation. Wright included these issues to explain the recombinant debates. In short, we reached to the same sources from different beginnings: Wright to explain a historical phenomenon, myself drawn by their repeated appearance in *Nature*. This shows that for a thorough understanding of the history of the regulation of recombinant DNA molecules, these two elements are well-needed. In a more radical vein, I argue as well, that this alignment of sources shows that *Nature* contains material that has high explanatory power as a historical source. While this may not always hold true, it may be a productive heuristic tool to find localised debates within the history of the politics of science.

As my intent here is not to characterise the full scope of the history of the regulation (and deregulation) of recombinant DNA molecules in the United Kingdom, but rather, pay a greater attention to how scientists thought about their ethical duties, I will now bring forward a sort of summary of the main points that Wright sets to be background against which the debates take place. These interlocking elements are the economic, political, and cultural contexts of British science, which deeply influenced (at least, initially) the way in which decision making in the recombinant DNA molecules affair took place. This background is also what I covered in the previous chapters.

Regarding economic background, as I illustrated in Chapter One with the examples of the scientific advertisements, for-profit manufacturers of research materials were ubiquitous in molecular science: from the funding of *Nature* to the making of many of the objects that were needed for experiments in the lab. At the national levels, the private sterling and dollar boosted its place within science throughout the 1970s. In the USA, investment for molecular technologies grew exponentially; while in the UK, the dramatic cuts in public investment and the invitation to businesses to participate by both Labour and Conservatives gave ample room to private investment within research.[7]

At the boundary between 'the political' and 'the economical', we find what I illustrated in Chapter Two: the Rothschild-Dainton-BSSRS controversy of the early 1970s, which reflected a push towards a managerial and utilitarian view of science policy. Paradoxically, the majority of scientists, however, were keener on being insulated from governmental and social arenas. This position was institutionally equivalent to a defence of a strong Research Council system, which ensured the independence of the decision-making in science-policy within the scientific elite. As we will see later, this paradoxical situation between a state that demanded a pragmatic science, and a majority of the scientific community wishing to be institutionally isolated, situated molecular scientists at a curious point when it came to the regulation of their lab technologies.

While the privatisation of research funding increased exponentially during the 1970s, this does not mean that reactionary movements weren't in place. The United Kingdom became heavily unionised during the 1970s, with 55% of its workforce belonging to a union by 1979 (the year with highest membership quotas after a decade of constant increase).[8] Importantly for the case here, unionisation of technicians and scientists was robust as well. The Association of Scientific, Technical and Managerial Staffs (ASTMS), formed in 1968 counted 450.000 members by 1978. The ASTMS had a powerful voice within political debates over workplace safety, using two sorts of approaches: "locally, through negotiation with employers" and "nationally, through pressure for higher safety standards".[9] Wright characterised the ASTMS as a "powerful interest group whose political influence, at least while a Labour government was in office, was substantial."[10] In the remainder of the chapter we will see how this pressure played out, and more concretely, how the ASTMS members and sympathisers understood scientific ethics.

While it is true that the Labour government was willing to recognise the demands of the unions, there were more solid foundations that structurally included their voices. Regarding health and workplace safety, the national configuration that was decisive in steering British regulatory of recombinant DNA technologies, was the Health and Safety Work (HSW) Act of 1974. The HSW Act was a general directive that applied to all workplaces –including research laboratories– in respect to occupational health and hazards. The HSW was implemented along with the establishment of the Health and Safety Executive (HSE), the governmental agency in charge of implementing the HSW Act. The HSE exerted two types of controls: legal and ethical. The legal control was done

[7] Wright, Susan, *Molecular Politics*, 49.

[8] Ibid, 117.

[9] Ibid, 118.

[10] Ibid, 118.

through “statutory regulations enforceable through the courts”, while the ethical control had the form of “codes of practices that were not legally binding.”[11] Importantly, the HSW Act included a novel element, which determined future decision-making processes regarding the regulation of workplace safety, and in the pertinent case here, regarding the regulation of recombinant DNA molecules. That is, the HSW Act provided “rights of employees to participate in decision making on safety issues” through a safety representative.[12]

At a less formal level, and illustrating the cultural backdrop of British research community, we find - without an attempt at quantifying - that a significant proportion of the scientific workforce was politically motivated and socially minded. As I showed in Chapter Two, the BSSRS was a markedly militant organisation of scientists with a critical vision of science. This group and their ideas had a visible (yet not dominant, however) position within the debates in *Nature*. Their formal input in the debates over regulation of DNA molecules was, however, not substantial.

II. ‘The self’ in self-regulation

Molecules and committees

Until now I have presented some of the actors of the debates, but I have not yet introduced a central one: the recombinant DNA molecules themselves. Who are they? And why were they so outspokenly controversial? What were they made of, and why did some fear them? And what kinds of great benefits were they expected from them?

On July 18, 1974, the recently formed ‘Committee on Recombinant DNA Molecules’ released a public letter. This group was composed of eminent researchers within the field of molecular biology and biochemistry; and had met some weeks prior in an invitation-only basis. The invitations had been extended by its chair, the Sandford molecular biologist Paul Berg, himself appointed by the National Academy of Sciences (NAS). The Berg committee, as it was informally called, met at MIT to discuss and draft a public letter that would be addressed to the NAS. Berg had gathered the scientists who contributed to the elucidation of the techniques themselves, such as Cohen and Boyer, viral biologists David Baltimore and Norton Zinder, biochemists Ronald Davies, David Hogness and Sherman Weissman, cancer researchers and gene therapy advocate Richard Roblin III, as well as other personalities of genetic research, such as Daniel Nathans, who discovered restriction enzymes (key for constructing recombinant DNA molecules), and the already legendary James Watson.

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[11] Ibid, 46.

[12] Ibid, 46.

Berg was the author of the by then landmark 1972 gene-splicing experiment publication, in which his team at Stanford University Medical Center described a set of procedures that involved cutting circular plasmid DNA by the bacterial *EcoRI* enzyme, and the subsequent introduction of target genes from *E.coli* into the viral plasmid.[13] The techniques and history are well described, but it may be worthwhile quickly summarising them.[14] The 1972 paper described a set of techniques for the creation of hybrid bacterial-viral genetic constructs, known also at the time as 'recombinant DNA molecules'. In this initial report, Berg's team used a specific bacterial line (*E.coli*), a given viral vector (the simian virus SV40), and two kinds of target genes to be studied (λ phage and galactose operon of *E.coli*). While the target was bacterial gene recombination, Berg was well aware of the possibilities of the generalisation of procedures and the extension of construction of recombinant DNA molecules from virtually any origin: "we have developed biochemical techniques that are generally applicable for joining covalently any two DNA molecules".[15] The techniques described in the 1972 publication were further developed by Herbert Boyer and Stanley Cohen, who inserted the recombinant DNA molecules into *E.coli*, where they would naturally replicate using the native replication machinery, resulting in the generation of multitude plasmids containing the target DNA. By using antibiotic-resistant genes in the DNA hybrids, the scientists could select out those bacteria that had not taken up the construct by using bacterial cultures containing antibiotics such as tetracycline. These techniques allowed the production of large amounts of exact copies of the original target DNA fragment. Accordingly, these techniques are often known as 'DNA cloning'.

After weeks of deliberation and private circulation, the letter was published in the *Proceedings of the National Academy of Sciences* (*PNAS*) and *Science*. The day after, with the indispensable Atlantic gap of the printed press, *Nature* reproduced the letter in

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[13] D. A. Jackson, R. H. Symons, and P. Berg, 'Biochemical Method for Inserting New Genetic Information into DNA of Simian Virus 40: Circular SV40 DNA Molecules Containing Lambda Phage Genes and the Galactose Operon of Escherichia Coli', *Proceedings of the National Academy of Sciences of the United States of America* 69, no. 10 (October 1972): 2904–9, https://doi.org/10.1073/pnas.69.10.2904.

[14] Several of the main human actors involved have written about the intellectual and political legacy of those debates. See, for instance: P. Berg and M. F. Singer, 'The Recombinant DNA Controversy: Twenty Years Later.', *Proceedings of the National Academy of Sciences* 92, no. 20 (26 September 1995): 9011–13, https://doi.org/10.1073/pnas.92.20.9011; Stanley N. Cohen, 'Bacterial Plasmids: Their Extraordinary Contribution to Molecular Genetics', *Gene* 135, no. 1–2 (December 1993): 67–76, https://doi.org/10.1016/0378-1119(93)90050-D; S. N. Cohen, 'DNA Cloning: A Personal View after 40 Years', *Proceedings of the National Academy of Sciences* 110, no. 39 (24 September 2013): 15521–29, https://doi.org/10.1073/pnas.1313397110.

[15] Jackson, Symons, and Berg, 'Biochemical Method for Inserting New Genetic Information into DNA of Simian Virus 40'.

its front page as 'NAS Ban on Plasmid Engineering'.[16] Thus, on July 19, the letter was read by those holding *Nature*, and pointed to the tension between hypothetical dangers and hypothetical benefits: "although such experiments are likely to facilitate the solution of important theoretical and practical biological problems, they also would result in creation of novel types of infectious DNA elements whose biological properties cannot be completely predicted in advance."[17] That was the tension that had to be solved before continuing the experiments, the letter advanced. The scientific interest in these techniques was large, as it permitted the study of gene function (eventually of humans) by the insertion of genes of interest in the viral plasmid and subsequently into the bacterial host, in a relatively straight-forward way.

The use of antibiotic-resistant genes as the method for selecting which bacteria had taken up the constructs was, however, a feared step. The letter hypothesized that it could lead to the colonisation of antibiotic resistant *E. coli* carrying the target genes in the gut of the researchers, leading to unpredictable public health concerns. The letter proposed that "until the potential hazards of such recombinant DNA molecules have been better evaluated or until adequate methods are developed for preventing their spread," scientists throughout the world should avoid performing such experiments, Berg and the rest of the committee plead.[18] They also demanded that a National Institutes of Health (NIH) committee should be formed to evaluate the dangers and guide recommendations.

Now, the timings of publications are important. The day after the letter was released in the *PNAS* and *Science*, the NIH appointed the officially-named 'NIH Recombinant DNA Molecule Program Advisory Committee,' more often known as the 'NIH Advisory Committee' or simply the 'Recombinant DNA Advisory Committee' (RAC). That is to say, that by the time *Nature* readers were reading Berg's plea in July 19, a USA national agency (the NIH) had already listened to them. For Wright, this is indicative of "the true alacrity with which the scientific establishment assumed it alone should address the implications of recombinant DNA research."[19] The Berg committee had been named 'Committee on Recombinant DNA Molecules', while the one appointed by the NIH was the 'Recombinant DNA Advisory Committee.' Without paying much attention to the names and formation of the committees, one could easily think that they were the same. In fact, some of the members overlapped, like David Hogness, who was present in both. At the end, it all stayed in house.

[16] 'NAS Ban on Plasmid Engineering', *Nature* 250, no. 5463 (19 July 1974): 175–175, https://doi.org/10.1038/250175a0.

[17] Ibid.

[18] Ibid.

[19] Wright, Susan, *Molecular Politics*, 140.

The RAC's function was to advise the NIH "concerning a program for the evaluation of potential biological and ecological hazards of DNA recombinant of various types, for developing procedures which will minimize the spread of such molecules within human and other populations, and for devising guidelines to be followed by investigators working with potentially hazardous recombinants."[20] As Wright showed, the committee was *designed* to produce guidelines so research could continue. The RAC was dependent on the NIH's Division of Research Grants, illustrating how the regulators of the molecules would be those who funded its research. The objective was clear, even in the RAC's own words as cited above, the function was to 'develop procedures' and 'devise guidelines,' not to investigate its dangers, let alone the ethical and social implications of the technology. Assessing risks was not a priority: "the RAC paid little attention to sponsoring experiments to assess possible hazards. Risk assessment lagged far behind the drafting of guidelines, and a conference on it with substantial representation form relevant biological fields was not held until roughly a year after guidelines were released."[21]

Scepticism about the RAC's intention is not reserved to Wright's historical analysis. Actors themselves were critical of the committee's homogeneity and its direction. The RAC was formed by its most part by experts on the molecular underpinnings of the technology. In October of 1975, months after the RAC had issued a revision of the guidelines established during the Asilomar conference (an event to which I will return later), a group of 48 scientists who attended a Cold Spring Harbor bacteriophage meeting said that the revised guidelines appeared "to lower substantially the safety standards set and accepted by the scientific community."[22] They attributed such deregulation to the narrow specialisation of RAC's members, and asked more representation of non-molecular scientists, such as animal virology, plant pathology, genetics, and epidemiology. In general, the bacteriophage scientists, who despite the fact that they themselves had an intellectual interest in the technology, could see the conflict of interest in the committee's composition. Accordingly, they asked for a general reformulation of the committee with members who were "not directly involved in cloning experiments."[23] This, only happened later on, in April 1976, when what would be the only non-scientists were added to the committee: Emmet Redford, Professor of Government and Public Affairs at the University of Texas; and LeRoy Walters, bioethicist

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[20] Recombinant DNA Molecule Program Advisory Committee, 'Agenda' (National Institutes of Health, Division of Research Grants, 28 February 1975), https://osp.od.nih.gov/wp-content/uploads/RAC_Agenda_Feb_1975.pdf.

[21] Wright, Susan, *Molecular Politics*, 179.

[22] 'DNA Committee Has Its Critics', *Nature* 257, no. 5528 (23 October 1975): 637–637, https://doi.org/10.1038/257637a0.

[23] Ibid.

at the Kennedy Institute at Georgetown University. However, the only reason these were added was, in the words of one of its scientist members, Jane Setlow, writing to DeWitt Stetten, the RAC's chair: "Like many other present members of the committee, I'm not sure this person [a non-scientist member] could contribute to the deliberations, but I *am* sure that we need one for the purpose of being able to say we have one when there are complaints."[24]

Since the NAS announcement, and in view of the moratorium being called by the leading scientists, the language in *Nature* referred to the pause in research using DNA cloning techniques as an act of 'self-regulation'. The debates that emerged thereafter centred around who had the agency to regulate (to stop, continue, or limit) scientific research. The ethical dimension of these debates was heavily dependent on knowledge about the techniques, a knowledge that had yet to be stabilised. Aside fom the uncertainty and the difficulty of being an expert who could establish ethical guidelines for conduct, other debates attempted to shift the discussion by questioning the location of the knowledge necessary to make ethical claims. Who was the qualified expert? Some argued that 'technical knowledge' referred not only to the DNA cloning techniques themselves but should include also a broader understanding of within biology, and take into account issues about environmental risks and ecological interactions. These debates weren't only about what the facts were, but on how extensive should knowledge be. The meaning of 'expertise', 'technical', 'social', 'responsibility' had multiple, often irreconcilable meanings. Early on, *Nature*'s editors knew that 'self-regulation' was a slippery concept, one in which public health could not rely on in any robust manner. Colin Norman, reporting from Washington to the *Nature* headquarters in London, was aware of the multiplicity of meanings and kinds of implementation that 'self-regulation' techniques had: "it remains to be seen how long the scientific community will go along with this move towards self-regulation."[25]

Since the first public letter voicing concerns about the potential hazards of recombinant DNA molecules appeared in the *PNAS* and the moratorium called by the NIH, it is often assumed that it was the American scientists at the forefront of 'the self' in the race for 'self-regulation'. However, by the time the readers of *Nature* were reading Berg's plea, the UK Medical Research Council and its overseeing agency, the Advisory Board for the Research Councils (ABRC) had already proposed a committee analogous to the RAC.[26] The committee was led by Eric Ashby, and the analogies with the RAC were varied: it was

[24] Jane Setlow to DeWitt Stetten, 4 September 1975, ORDAR, as cited in Wright, Susan, *Molecular Politics*, 165–66.

[25] Colin Norman, 'NIH Backing for NAS Ban', *Nature* 250, no. 5464 (26 July 1974): 278–278, https://doi.org/10.1038/250278a0.

[26] Wright, Susan, *Molecular Politics*, 141.

a molecular technical committee, composed by elite scientists, most of them from biochemistry, cancer research, virology, or molecular biology fields. It would thus not be a surprise that since its inception, the committee was "not [...] set up to make ethical judgments about the use of the techniques."[27] At least, this is what the Ashby committee claimed to be doing, and that their "business" was simply "to assess potential benefits and hazards, after discussion with scientists who are familiar with this branch of biology."[28] Despite their 'technical' focus, the men who wrote the report were conscious of the "wider question" that orbited the narrowly-defined benefits-hazards debate: "how can the social values of the community at large be incorporated into decisions on science-policy?" This question, which was out of scope for a government document, was left to be discussed by "the public," and in that vein they wrote a report "which assumes no specialised knowledge on the part of the reader" to make it "intelligible to people unfamiliar with modern genetics."[29]

The document was organised as follows: a summary, a description of the techniques in question, the potential benefits, the potential hazards, the potential defences against those hazards, and some conclusions and recommendations. The dichotomy benefits-hazards was properly set in place, and while they were both set as "unpredictable," an asymmetry was introduced: "No-one can predict, except in the short term; what benefits might arise from scientific research; one can, however, list the evident hazards." This conceptualisation is akin to and predates what would be typical in governmental reports, science journals and news outlets regarding the recombinant DNA issue: what Wright showed to be a conceptualisation of the benefits as "broadly defined" and of the costs as "narrowly focused on lab hazards." She set the start of this this characterisation at the outset of the Asilomar conference, which took place in the USA months after the Ashby report.[30] As I have shown here, this conceptualisation anticipates that of Asilomar (an event to which I will return below). However, aside from this dialectic characterisation, no guidelines were devised by the Ashby working group.

The Ashby report did not draft regulations nor establish a working committee that would work on it. The impatience was mounting on: *Nature* announced the publication of the Ashby report in January 1975 as 'Amber light for genetic manipulation'. Six months in, the frontpage lead editorial plead 'Forever amber on manipulating DNA molecules?' The British scientists didn't simply wait for the green light: the title of this lead editorial represents the impatience; its content, the political

[27] 'Report of the Working Party on the Experimental Manipulation of the Genetic Composition of Micro-Organisms (Cmnd. 5880)' (London: State of Education and Science, House of Commons Parliamentary Papers, January 1975), 3.

[28] Ibid, 3.

[29] Ibid, 3.

[30] Wright, Susan, *Molecular Politics*, 149.

moves of the scientific elite. The lead editorial reported on a meeting of several scientists with representatives of the Research Councils and of the Department of Health and Social Security. In this reunion that took place in Oxford, in which the scientific press was excluded, government delegates asked the scientists questions such as whether they were planning or willing to perform experiments using recombinant DNA technologies within the next two years. “Considerable numbers” of scientists said ‘yes’ to this question, which mounted “intellectual pressure” to the government to reach a final regulatory solution.[31] In the state of the amber light imposed by the Ashby report and the voluntary NAS moratorium, there were rumours of “impatient scientists [who] were already under way.” *Nature* reported that in this meeting there appeared two groups of scientists, those willing “to get moving again” and “those waiting for more formal guidance on safety.” This division within the scientific community was heated and could attract outsider intervention. What was needed, was an image of self-sufficiency. Let us move now to the Asilomar conference, the reporting of which became the mechanism of how this image of self-sufficiency was built.

How are we supposed to stop these molecules?

When it comes to the history of the regulation of recombinant DNA molecules, the Asilomar conference seems to be a significant pitstop, if not the origin in the myth of ‘self-regulation’. As the Wikipedia article devoted to it currently states:

> The Asilomar Conference on Recombinant DNA was an influential conference organized by Paul Berg to discuss the potential biohazards and regulation of biotechnology, held in February 1975 at a conference center at Asilomar State Beach. A group of about 140 professionals (primarily biologists, but also including lawyers and physicians) participated in the conference to draw up voluntary guidelines to ensure the safety of recombinant DNA technology. [...] The effects of these guidelines are still being felt through the biotechnology industry and the participation of the general public in scientific discourse. Due to potential safety hazards, scientists worldwide had halted experiments using recombinant DNA technology [...] According to Paul Berg and Maxine Singer in 1995, the conference marked the beginning of an exceptional era for both science and the public discussion of science policy.[32]

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[31] ‘Forever Amber on Manipulating DNA Molecules?’, *Nature* 256, no. 5514 (17 July 1975): 155–155, https://doi.org/10.1038/256155a0.

[32] ‘Asilomar Conference on Recombinant DNA’, in *Wikipedia*, 2 February 2020, https://en.wikipedia.org/w/index.php?title=Asilomar_Conference_on_Recombinant_DNA&oldid=938729053. Wikipedia articles about historical events in the history of science are highly relevant, as it may be one of the central sources through which scientists get informed about their institutional and

Despite these grand claims, the main historian of Asilomar within its wider context, Susan Wright, isn't as favourable to the interpretation of the event by the actors of the event, whose discourse is mirrored in the Wikipedia page. She points out in *Molecular Politics*, that the Asilomar conference

> eventually recommended lifting the partial moratorium imposed since the previous July and replacing it with guidelines for genetic engineering research, many of which were strict. The image of this result, beamed across the world by largely positive new reports, was of an international community of scientists idealistic moving to restrict their own research and to anticipate its hazards. What was generously obscured by this image and its emphasis on the novelty of self-regulation was that the proceedings were also designed to enable research to move forward and that this goal was anticipated by the organizers of this meeting.[33]

Since this is not a review of the history of the Asilomar 1975 conference, I point interested readers to Wright's passages on *Molecular Politics.* [34] It is enough to note again her main point: Asilomar was organised *in order to* and with the specific *goal of* resuming research. Guidelines were a way out, the mechanism by which research would continue.

While the British scientists waited in the amber (and as we saw, politically pushed for a green light), the organisers of Asilomar drafted the well-expected guidelines. Finally, on June 5th, 1975, *Nature* had reported the outcome of the Asilomar, and with it, the establishment of guidelines for American molecular scientists. The 'International News' section reproduced a "summary of a report submitted to the Assembly of Life Science of the National Academy of Sciences and approved by its Executive Committee on May 20, 1975."[35] The report framed the need for legislation within "impressive scientific achievements", which promised to "revolutionise the practice of molecular biology."[36] The excitement for new applications was as unknown as the potential

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intellectual history. Despite the "overwhelming sceptical attitude among faculty towards Wikipedia," a group of researchers has written an interesting piece and showed how academic researchers use and value Wikipedia as much as students, and that their sceptical attitude may be simply due to "their colleagues' perceived opinions and practices, and academic disciplines." Perhaps historians of science should spend more time correcting Wikipedia entries on events in the history of science. Eduard Aibar et al., 'Wikipedia at University: What Faculty Think and Do about It', *The Electronic Library* 33, no. 4 (1 January 2015): 668–83, https://doi.org/10.1108/EL-12-2013-0217.

[33] Wright, Susan, *Molecular Politics*, 145.

[34] Ibid, 144–59.

[35] Norman.

[36] 'Asilomar Conference on DNA Recombinant Molecules'.

biohazards: genetic engineering was indeed an "area of biology with many unknowns," thus giving reasons to remain on the safe side of things: "it is this ignorance that has compelled us to conclude that it would be wise to exercise considerable caution in performing this research."

What had begun as a temporary moratorium, continued now as a code of practice, which included both infrastructural, behavioural, and moral restructuring of how work with DNA was to be performed. The outcome of Asilomar recommended were two "reasonable principles," which the NAS approved. These two principles which would guide future experimentation were "(1) that containment be made an essential consideration in the experimental design and, (2) that the effectiveness of the containment should match, as closely as possible, the estimated risk."[37] These two basic pillars were designed to work as interlocking elements of the regulation of scientific practice, in which the generation of natural knowledge both preceded and dictated the conditions by which knowledge is generated. While (1) compels scientists to abide by the agreed conditions, (2) allows and necessitates of scientific knowledge to update and reform those conditions.

Containment included both biological and physical barriers, the former referring to the choice and design of biological creatures used, such as the use of "fastidious bacterial hosts unable to survive in natural environments."[38] Physical barriers, on the other hand, referred to "stringent physical containment and rigorous laboratory procedures", such as "good microbiological practices."[39]

The recommendations set by the Asilomar organizing committee charted the hazards of DNA recombinant technologies in four categories: minimal, low, moderate and high risk. Risk was calculated according to a series of factors within "the available [scientific] information," including the 'novelty' of the "biotypes" generated, native "ecological behaviour" of the strains used and the purported "ecological disruption." As such, risk classification relied primarily on existing phylogenetic classification of the working organisms and the probability that the new organisms created using DNA recombination would be pathogenic to humans and non-human living beings.

The recommendations of the American committee were seeming specific but weren't meant to be exhaustive, nor were legally binding: "the parameters proposed here are broadly conceived and meant to provide provisional guidelines." Safety relied on the researcher's moral integrity and scientific knowledge, as the letter said that "each investigator bears a responsibility for determining whether, in his particular case, special circumstances warrant a higher level of containment than is suggested here."

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[37] Ibid.

[38] Ibid.

[39] Ibid.

The legal dimension was placed a few months after the report by the Asilomar Committee, on December 4 and 5, when the RAC drafted what would be the strictest regulations over the use of the technology in the USA. Colin Norman, reporting for *Nature* from California, saw this draft as “the latest step in a tortuous process of self-regulation by a segment of the scientific community.”[40] This image of self-regulation was necessary, as it was feared that lack of consensus within the community would open the doors for legal intervention. With the RAC recommendations, the reported danger of external intervention seemed already out of view: “it was entirely possible that the matter could be taken out of the hands of scientists, and the technique regulated by legislation—a possibility which clearly bothers many people because it smacks of political infringement on academic freedom.”[41]

In Britain, it wasn’t until the autumn of 1975, a few months after the Asilomar report, that the government decided to move in the direction of drafting and instructing guidelines. Robert Williams, director of the Public Health Laboratory Service, was appointed by the Labour government to form a committee which would develop guidelines. The control would be maintained by the Department of Education and Science, rather than the Department of Health and Social Security, which for Wright is indicative of the British government responding affirmatively to accept “the scientific community’s interest in maintaining peer control over the field.”[42] The committee was formed by molecular scientists (virologists, molecular biologists, geneticists), and did not include non-scientists. Among those present was Sydney Brenner, part of the organising committee of the Asilomar conference, and as we will see, an advocate of a ‘technical solution’ for the hazards of *E. coli*.

Despite that the “interests of the community were clearly to be respected” by the Williams committee, Write writes, union influence was significant.[43] While the ASTMS did not have a formal chair at the committee, they were in close contacts with the Labour government in office, who was willing to listen to them. The vice president of the ASTMS, Douglas Hoyle, was a Labour MP and met with the Minister for Education and Science to transmit the union’s sentiment. Among their demands was that the HSE and the general directives of the HSW Act of 1974 would be taken into consideration. “The efforts,” Wright writes, “bore fruit.”[44]

In August 1976, the Williams report came out, together with a draft regulation. The guidelines were similar to the NAS-endorsed Asilomar recommendations: classification of hazards based on the phylogenetic distance to man, and the imposition

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[40] Norman, ‘Genetic Manipulation’.

[41] Ibid.

[42] Wright, Susan, *Molecular Politics*, 196.

[43] Ibid, 196.

[44] Ibid, 198.

of several categories with assigned physical and biological containment levels. Some issues differed between the Atlantic coasts, however. The Williams report approved the use of the K12 'weak' *E. coli* strain, representing the interests of those scientists willing to use the technology, who advocated for 'technical solutions' for deactivating the bacterial biohazards.[45] Union interests were also enacted, as the Williams paper introduced a mechanism which made notification of the experiments mandatory. Individual experiments had to be notified to an appointed local "biological safety officer", who would pass it onto the national level to the Department of Education and Science, who would review "that the experiment sere conducted according to a recommended code of practice."[46]

Notifying the intention of experimenting was not common among scientists, who thought of the compulsory nature as an unnecessary bureaucratic offense. This aspect was central to one of the most heated printed debates, which took place in *Nature*'s issue of November 4, 1976. The contestants were a molecular scientist and a civil regulator: Michael Ashburner, researcher at the Department of Genetics at Cambridge; and John Locke, Director of the HSE. Ashburner was responding to the HSE's recent regulatory proposal.[47] In view of the two government reports, Ashby (January 1975) and Williams (August 1976), the HSE had drafted some regulations, and had established an agency responsible of overseeing genetic engineering, the Genetic Manipulation Advisory Group (GMAG).

In his letter, Ashburner expressed concern for HSE's proposal, which obligated scientists to notify the GMAG of "intentions" to perform experiments that led to the formation of recombinant DNA molecules. Ashburner was favourable of the Ashby Report (which, let's remember, did not introduce any specific guidelines) and the Williams' Report, which did introduce a code of practice. The newer 1976 draft of the HSE, however, seemed unconceivable to him. While the William's Report recommended an Advisory Group made up of scientists (the GMAG), the new draft proposed to include also non-scientists who would take account the "interests of employees and the general public." In Ashburner's view, the GMAG was already the middle position between scientists and regulators, a position which, in his eyes, should remain with scientist themselves. This was a clear case of demarcating spaces and establishing sovereignty over the political decisions that had to be taken. For Ashburner, political issues had to

[45] I will return to a discussion of these 'technical solutions' at the end of the following section, *Reconstructing 'the self' in 'self-regulation'*.

[46] Wright, Susan, *Molecular Politics*, 202.

[47] Michael Ashburner, 'An Open Letter to the Health and Safety Executive', *Nature* 264, no. 5581 (4 November 1976): 2–3, https://doi.org/10.1038/264002a0; J. H. Locke, 'An Open Reply from the Director of the Executive', *Nature* 264, no. 5581 (4 November 1976): 3–3, https://doi.org/10.1038/264003a0.

be informed by scientific reasoning: "You remove the Advisory Group or any similar body from any central role in the dialogue that must exist between those who do the experiments and those who administer the laws under which they are done. With respect, sir, you do not have the standing in the scientific community required for this job."

Ashburner expressed his disconformity and articulated it as a threat: any attempts to over-regulate the scientific community would turn against the regulators:

> the burden on a scientist communicating with you in advance the protocol of his every experiment, for no obvious reason, would be so great that you would lose the confidence and goodwill of the scientific community. If this were to happen the dangers might be very real since you rely upon this community to draw both real and potential hazards to your attention. Remember that it was the scientists who made possible the 'genetic engineering' experiments who brought their concern to public attention (*Nature* 250, 175).

To Ashburner's profound dissent from the HSE's proposal, John Locke, its director replied in the same issue with a rather defensive letter. He attempted to locate Ashburner's disagreement as a case of misreading the draft and the previous government-backed reports. Lock assured that the Williams Report from that year already recommended a required notification, although as far as I could tell from looking at the legislative papers, it did not.

Locke professed himself sympathetic of scientists' animosity towards forms and regulations: "I entirely agree that neither we nor the Advisory Group should be subjected to 'mountains of forms'." Sympathy ended when it came to the approach to calculate risks. The HSE director attempted to draw on Ashburner's empathy: "I am sure you would agree that we must not devise a definition of what is to be notified which would leave out certain types of work which might prove to present special hazards. It seems to us we must err, if we err at all, on the side of having rather more than we want notified rather than too little." Did all think like that?

Intermezzo I: Legality as understood by scientists

The regulation of recombinant DNA technologies was not isolated from the regulation of laboratory work as a whole, often referred to as 'legislation of biohazards'. The 1970s saw several debates and legal reforms on this front that lead to an increased regulation of scientific practice. On August 2nd, 1974, a month after the announcement of the NAS-backed moratorium, *Nature* took the chance to examine the issue of biohazard legislation. Brian Ford, biologist and science populariser, who had published three years prior "an outline of proposals for biohazard legislation" in the *New Law Journal*, contextualised the debates over the regulation of DNA cloning, arguing "the law does not concern itself sufficiently with the various hazards that can arise when microorganisms are mishandled."[48] Ford recognised that regulation of laboratory work was seen as a top-down, coercive, and simply put, annoying bureaucratic duty. However, it was a chilling surprise to him that work with living organisms was hardly regulated:

> Legal restraints and regulations are the bane of research workers in many spheres. All of us must have felt some kind of annoyance at the filling in of forms or the signing of registers that have the aura of state bureaucracy, but the justification lies in the resulting safety of research. If that is the case, it is odd that legislation covering the handling and use of microorganisms and viruses that are pathogenic to man-kind is virtually nonexistent.

He compared the moratorium called by the NIH with a British example: the push to "make hepatitis a recognised industrial disease amongst hospital and laboratory staff." He saw these two initiatives as "piecemeal short term measures," and argued for "greater controls of a generally accepted legal framework of safety." While it may seem unclear how these two movements (a moratorium for a specific research technique and the legal recognition of a workplace disease) are precisely related, and why he considers them "short term measures" (since they both could lead to long-term legal reform), we must understand his motives as an attempt to develop a regulation of the working materials of biology as whole. He argued that the existing regulation of tightly controlled chemical and radioactive material should be mirrored in biological materials, so as to include animals, plants, pathogens, and all the other organisms used in the biological laboratories. Ford argued that biological matter was in fact more dangerous than chemicals: "in philosophical terms the greatest hazard posed by the lack of controls over pathogens is their infectivity: poisons do not replicate."

His paper in the *New Law Journal*, Ford summarised, "referred to the virus of smallpox (variola) and the occasional accidents that occur when unsupervised or inexperienced staff come into contact with pathogens." To contextualise the dangerousness of the biological matter, he reminded the readers of the smallpox outbreak in London the year prior, a "tragic illustration of the hazard," in which a couple had died from the disease and more than 3000 people had been preventively vaccinated. To this very visible incident, Ford also listed several incidents with biological research objects.

In Britain, for instance, he reported that "children have recently been found playing with discarded culture vessels in the Midlands," and that "contaminated syringes

[48] Brain J. Ford, 'Call for Biohazard Legislation', *Nature* 250, no. 5465 (2 August 1974): 364–65, https://doi.org/10.1038/250364a0.

[had been] casually discarded by a pharmaceutical concern in Kent." In the USA "an underground group planned to immunise themselves against the effects of bacteria that were then to be introduced into the public water supply," and in Australia "a culture of a meningitis virus was stolen (in its incubator) from a research laboratory." Ford didn't tire from listing biomaterial-related health incidents. However, he made clear that not all of them were derived from criminal offenses from 'outside' science, such as burglars or the pharmaceutical industry. Some dealt with scientists themselves behaving in practices that Ford thought to be questionable. Mouth pipetting, a common practice at the time, was beginning to be seen as problematic. Ford reported "a recent example in Britain [which] involved a supposedly non-pathogenic bacterium ingested when the tip of a pipette under suction was momentarily withdrawn from a thin agar culture," and a similar case where a "handling positive sera from syphilitics made a similar error, but did not develop the disease." Ford, himself a scientist, had "seen a technician attempt to open a phial of poly-valent poliovirus, believing the glass to be scored. It was not, and the neck fractured and lacerated his finger."

Regulation at the time was introduced after-the-fact, targeting the accident that had taken place. Ford recognised that "in each of these episodes some safety measures were subsequently introduced to prevent a recurrence," but maintained that, however, "custom-built regulations are no way to prevent accidents entirely." He would have rather have a 'biohazard law', that "would enable general safety measures to be introduced and enforced for the protection of all."

The way in which Ford envisioned this broad legislation of biological material was first of all as a necessary measure for protecting public health. The examples used, such as the public health emergency of the 1973 London smallpox outbreak situated Ford's push for widespread legislation. We can see how 'safety' was seen in context of a kind of 'social responsibility' of scientists, whose behaviour ought to align with the dangers of the materials they work with. The legislation that Ford proposed was framed by two interlocking elements: (1) a classification of pathogens and their associated risks, for which scientific knowledge was needed and for which specific technical skills were required; and (2) the modulation of scientists' behaviour according to consensual agreements. From the application of these two elements it would emerge a picture of a 'responsible scientist,' whose knowledge of the natural (biological) world would have enabled himself to dictate codes of conduct to abide to and by which to generate more knowledge.

Scientific knowledge was not the only necessary knowledge-form, but a more technical knowledge codified by a license: "The status of licensed holders of Schedule A organisms and viruses would be defined in terms of academic training, responsibility and seniority. Less severe restrictions would be placed on laboratories classified for the culture of Schedule B pathogens."

Finally, once the classification and licensed behaviours were established, a rigid code of conduct should be introduced: "standards of safety in laboratories would be laid down and codes of conduct made mandatory. The registration of holders of specified pathogens would doubtless aid the coordination of the research effort, and clear sets of agreed criteria would apply to genetic manipulation of bacteria and viruses, and to the disposal of potentially hazardous materials." Regulation, as we gather from this exposition of a scientists who was undoubtably laying on the side of safety, was concerned mainly as an interlocking of scientific and bureaucratic knowledges.

Now, let us explore the meat of this chapter: how was the 'self' to be construed in case of conflicting scientific and bureaucratic knowledges? Who was responsible, the individual scientist or the community at large?

III. Reconstructing 'the self' in 'self-regulation': picking up the pieces from *Nature*'s editorials and contributions

Is 'the self' up to the task?

Initially, the scientific community expressed agreement with the temporary ban. The earliest report of the American news filled no more than half a lead page, and British scientists were invited to comment on their homologous' debate on the other side of the ocean. The chosen representative was Michael Stoker, director of the Imperial Cancer Research Fund (ICRF). In the section 'What British scientists say…,' Stoker expressed his content, and remarked the myth of self-regulation: he found "encouraging that the very leaders in the field have taken the initiative and have been supported by the [USA] academy," and added, "it is now to be hoped that academics and learned societies in other countries will add their weight, and that international organisations such as the European Molecular Biology Organisation will lend support."[49] The words of Stoker presented the scientific community as aware of the potential benefits of the new technology, but also sensible to the duty in their hands. The promises of recombinant DNA technology were the panacea for the field of molecular biology, which "could, for example, revolutionise the commercial production of substances like insulin, or pituitary hormones. […] to say nothing of the basic knowledge gained." Yet, the technology posed itself also as a "test of self-denial [sic] and social responsibility in the face of strong intellectual temptation." What was this 'strong intellectual temptation', and who was supposed to govern it?

The 'self' had a central yet ambiguous role in the debates over the moratorium. There was a tension that had to be navigated, between the vision of complete scientific

[49] Michael Stoker, 'Molecular Dirty Tricks Ban', *Nature* 250, no. 5464 (26 July 1974): 278–278, https://doi.org/10.1038/250278c0.

autonomy on the one hand, and the need for social agreements on the other. The NAS had called for "self-restraint" — to abide by it meant to leave it up to the individual scientists.

As part of the academic community, publishers had a role to play as well. Early on in the debates, *Nature* weighed in. An unsigned leader —written most likely by the editor of the time, David Davis — replied to David Baltimore, one of the members of the American committee, in his message to editorial boards. The self-restrain in research had to be matched, according to Baltimore, with press censorship. He said that editorial boards "will probably think twice about publishing research papers derived from experiments covered by the embargo."[50] However, *Nature* and Davis did not see the non-legal binding of the moratorium as problematic for publishers, and even argued that it was "quite wrong" to implement "any policy that tried to eliminate from the journal reports of experiments that are believed—even widely believed—to be potentially dangerous." The editor gave two reasons, one being that, in fact, questionable research should be "given publicity", rather than being pushed underground. The other reason advanced by Davis may be more interesting for the present discussion, as it drew a picture of how scientists should deal with moral situations. The editor of *Nature* argued that "asking a referee to remark on the morality of a papers (assuming that it is not a question of legality) cannot but lead to impossible situations." To illustrate this, the editor asked, "should reports of nuclear fission have been called 'potentially dangerous' and in bad taste?" and "is the administration of electric shocks to animals cruel?" Something interesting can be said about this. As *Nature*'s head, Davis argued that scientists shouldn't deal with moral situations neither *directly* nor *individually*. This placed 'science' and 'ethics' into a specific relation. To this aspect of *directedness*, we shall return later. For now, suffice it so say a few words. Davis, as well as many of his companion scientists, argued for a non-direct relation: one in which ethical benefit or injury would be measured after-the-fact, as a sort of epiphenomena arising from scientific work, not as something that had to be taken into account prior to it. The assessment of ethics of animal experimentation had to take into account, Davis implied, the results that it could deliver. With this brief commentary of what I mean by (in)directedness, let me now illustrate how another element of the relation between 'science' and 'ethics' as conceptualised by *Nature*'s editor: the *individuality* of ethical decisions in scientific work.

Davis envisioned individual scientists as separated from ethical decisions in their everyday practices. Publication ethics, intertwined with aesthetic notions ("bad taste" nuclear fission) and animal ethics ("cruel" electrical shocks) weren't a topic to be assessed by individual persons, but by people. The piece argued, "the community can

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[50] 'Should We Publicise Those Experiments?', *Nature* 251, no. 5470 (6 September 1974): 1–1, https://doi.org/10.1038/251001a0.

either legislate against certain experiments or it can attempt to persuade practitioners to desist; if it tries to put the onus on an individual to decide for the community, it can hardly be surprised if the result is often quirky." We see here how moral decision was presented as something not to be dealt with by the individual scientist, but something defined and implemented by 'the community.' The argument of the editor of *Nature* for collective measures collided with the vision of the American committee of asking for individual responsibility to all the players in the scientific community—including editors.

Ethical decision-making wasn't trusted on individuals, according to Davis. DNA cloning technology rose ethical questions that could not "safely be left to a referee with his own personal predilections, or even an editor with his." Reducing complex decisions could not be reduced to the filling up forms, thus any "attempt to do so by putting an X in the box if you believe the experiment should never have been done in the first place" was seen as ludicrous. Davies's distrust for using peer review as a social mechanism to reach moral verdicts contrasts with his trust of peer review to reach scientific verdicts. The year prior, Davis had become the editor in chief of *Nature* and actively worked for the implementation of peer review as a mandatory mechanism for their articles.[51] This attest two things: first, that Davies envisioned science as a communal endeavour in which peer review did serve to reach consensual decisions; and second, that this communal aspect of science was restricted to epistemic issues. Davis saw peer review as valid for epistemic review, and in contrast to his distrust for reaching ethical verdicts, we can see how he operationalised the division between ethics and science using the validity of peer review as a social mechanism.

Something else can be said from Davies citation above. By reducing ethical decision making to personal taste and mere subjective preference (visible in the text of Davis in his association of "a referee with his own personal predilections" as an example of ethical decision-making), Davies was adhering to the basic tenet of the 'emotivist theory' of ethics, a set of historically entrenched philosophical notions conceptualised by philosopher of virtue ethics Alasdair MacIntyre. As Davies's example shows, MacIntyre has argued that the emotivist theorey is the "doctrine that [claims that] all evaluative judgments and more specifically all moral judgments are *nothing but* expressions of preference, expressions of attitude or feeling, insofar as they are moral or evaluative in character."[52] MacIntyre situated emotivism historically, pointing to Western modernity as the root of our inability to address moral situations in a methodical and productive manner: "the culture of moral modernity lacks the resources to proceed further with its own moral enquiries, so that sterility and frustration are bound to afflict those unable to

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[51] Baldwin, *Making 'Nature'*, 180.

[52] Alasdair MacIntyre, 'The Nature of Moral Disagreement Today and the Claims of Emotivism', in *After Virtue. A Study in Moral Theory*, Third Edition (Notre Dame, Indiana: University of Notre Dame Press, 2007), 12.

extricate themselves from those predicaments."[53] What is important to recognize here is not whether Davis' position was 'emotivist' or not, or the historical roots of such position; but that such way of conceptualizing moral assessment, reflected by its dismissal under the pretext of being just "personal predilections" invalidates any sort of systemic analysis of the ethical dimensions of, in this case, recombinant DNA molecules and the associated DNA cloning. Here, reducing ethical issues to personal preference served as a way of dodging them, making individual scientists not only not responsible for them, but unfit to do so.

And here comes (the problem of) 'the people'

With a focus on 'self-restraint', the management of recombinant DNA molecules was for the most part (with the exception of e.g., Davis) situated in the individual. However, these molecules (as well as technology in general) did invoke issues to be solved at the social level. The ethical problem lied primarily in the conceptualisation of 'benefits' and 'risks', and how the technology *could* be a biomedical and commercial miracle, as well as it *could* be a disastrous pathogenic disaster. In this assessment, the relation between technoscience and society was explicitly discussed. *Nature* isn't the best historical source to understand such relation. It is, however, a good source to understand how scientists thought *they* were perceived by society. The news report of the 1974 British Association for the Advancement of Science reproduced the worries of its president, John Kendrew, who perceived a "growing indifference and even hostility towards science" by the public.[54] The report placed the public's resentment of science on a mistaken vision by people who "saw it [science] as the handmaiden of economic growth, as the creator of pollution, as the support for military and as a source of little understood dangers such as genetic engineering and computer data banks, or even as a negation of humanity itself." If this was the backdrop of the technoscience-society relation —or better, of how *scientists* thought that relation was like— the debate over the safety and relevance of recombinant DNA molecules seemed to start from a confrontational standpoint. The background in which the new genetic technologies were assessed was not neutral.

Kendrew's fears of confrontation arose from his perception of scientist's work being misunderstood by an ignorant public. The picture he painted was one of isolated scientists, reclused in an ivory tower doing, paradoxically, important work. It is important to understand that molecular scientists thought of themselves of doing 'pure' or 'basic' research, a kind of science whose social relevance was far away or even unclear, but nevertheless worthwhile. The problem for Kendrew was, accordingly, how science was presented – the *image* of science. While lauding scientists for their "pursuit of

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[53] Alasdair MacIntyre, 'Prologue: After Virtue after a Quarter of a Century', in *After Virtue*, x.

[54] 'B.A.—First the Bad News', *Nature* 251, no. 5470 (6 September 1974): 4–4, https://doi.org/10.1038/251004a0.

knowledge as an absolute good in itself," he argued that it shouldn't be the only one. In a curious conversation across time, directly talking to us, historians, we can see the image that Kendrew thought any non-scientists (even historians of science) had of molecular scientists in the 1970s. The piece read, "much contemporary research might seem to future historians like the minute and pettifogging scholasticism of mediaeval times." We see here, the picture of isolated, uncomprehended scientists working on obscure topics with dangerous methods. The historiography of history of science of biology in general, and genetics in specific, paints, however, a very different picture: one of science being political, economic, and military.[55] The importance at hand is the political force of thinking that the molecular community was an scholastic outcast.

This image had to be flipped, and the solution to a trivial and curiosity-driven science was to frame molecular science as a purposeful science, one directed towards productivity. The relation between science and the public needed reformation. In the context of recombinant DNA molecules and the surrounding public debates, Kendrew argued for highlighting the "useful products [that] could be of great benefit," and for an image of scientists as being in control of the molecular as well as social dimensions of the technology: "scientists must be seen to put their own house in order, or else others would step in and do it for them perhaps in ways which would lead to quite undesired restrictions on what they do."[56] This point —the need for self-regulation to avoid external regulation— has been extensively discussed in *Molecular Politics*. Another important point is to see how Kendrew emphasised the need for scientific knowledge to inform regulation: "even when decisions passed to the social or political forum, it was [sic] absolutely necessary that there should be effective communication of scientific knowledge upon which decisions could be based." This content of the knowledge that Kendrew referred to was not specified, but it did refer to the form: "scientists should take trouble to translate their ideas and discoveries into language which anyone could understand." On the other hand, the content of the knowledge that should inform the ethical discussions could, for example, be one of classification: classification of pathogens dictating classification of risks. This mechanism is alike as the one proposed by Ford (*Intermezzo I*): a legislation framed by two interlocking elements: scientific classification of knowledge based and the modulation of behaviour according to this classification.

A solution: the image of productivity and self-sufficiency

Important discoveries in science did not only appear in the Articles or Letters to *Nature* sections. If ground-breaking enough, they would make it to News and Views, where a

[55] Steven Shapin, *The Scientific Life. A Moral History of a Late Modern Vocation.*, 1st ed. (Chicago and London: The University of Chicago Press, 2008); Meloni, *Political Biology*.

[56] 'B.A.—First the Bad News'.

sort of 'digest' version of the original paper was provided – albeit still technical and remaining inaccessible to non-scientists. On October 1974, as the public debates on the ethical and social dimensions of recombinant DNA technologies had been going on for a few months, scientific developments in the field of genetic engineering were permeated by the social unrest they created. Take for instance, the news report 'Genetic engineering with viruses' written by Oxford pathologist George Brownlee, in which he praised the work of Edinburgh molecular biologists Noreen E. Murray and Kenneth Murray in the paper in the same issue 'Manipulation of restriction targets in phage λ to form receptor chromosomes for DNA fragments'.[57]

The Murrays had been able to "isolate a mutant of bacteriophage λ [a virus which targets bacteria] which can replace the normally used [*E.coli*] plasmid in experiments designed to construct new hybrid DNA molecules."[58] Brownlee wrote of DNA cloning experiments as "very simple in principle and (in brief) involve mixing together in a test tube the donor DNA molecule (the gene to be studied), the receptor (the plasmid) and a 'DNA cutting' enzyme *EcoRI*." The Oxford scientist not only presented the field of genetic engineering as "simple" but wrote of the Murrays' contribution as a "success." The method of DNA cloning had until then been to use plasmids of *E.coli* as donor, and the worry was that using this sort of plasmid could confer antibiotic resistance to the mutant *E.coli*. The Murray's addition was that they had managed to replace the problematic *E.coli* plasmids by the viral phage λ. Brownlee saw it as a "brilliant series of genetic and biochemical manipulations to construct a mutant phage." The Murrays had introduced the tryptophan gene in *E. coli* and managed to achieve biosynthesis and were also "hop[ing] to extend this work", for instance, in the study of DNA insertions in "higher organisms." This presentation of a developing, hopeful, and forward-looking science was followed by a wink to the readers of *Nature*, with an acknowledgment of the historical moment in which the experiments were being done.

> Many readers of *Nature* will know that there is some controversy among scientists about the ethics of performing genetic engineering experiments (see *Nature*, 250, 279; 1974). Part of the concern is about the nature of the particular *E. coli* plasmid used in these experiments which confers on the *E. coli* resistance to the antibiotic tetracycline. Clearly, if one wishes to introduce foreign, and possibly hazardous DNA, it is advantageous to use microorganisms susceptible to antibiotics. Dr K Murray, in a recent lecture to the Biochemical Society, has

[57] G. G. Brownlee, 'Genetic Engineering with Viruses', *Nature* 251, no. 5475 (11 October 1974): 463–463, https://doi.org/10.1038/251463a0; Noreen E. Murray and Kenneth Murray, 'Manipulation of Restriction Targets in Phage λ to Form Receptor Chromosomes for DNA Fragments', *Nature* 251, no. 5475 (11 October 1974): 476–81, https://doi.org/10.1038/251476a0.

[58] Brownlee, 'Genetic Engineering with Viruses'.

> pointed out that phage λ is much safer than the plasmid. It is sensitive to antibiotics and only infects some strains of *E. coli*. There is also evidence that the laboratory strains for which λ is infective do not establish themselves in the human gut. It is thus improbable that hybrid molecules (of whatever type) could be pathogenic.

Extending this work to other organisms represented the image of productivity that Kendrew wanted scientists to appeal to. Also needed was a self-contained and self-sufficient image: science as the answer to science's problems, in this case: biomolecular technology to answer biomolecular technology's problems.

Someone who also was proponent of such solutions was South-African Oxford biologist Sydney Brenner, who had given his testimony to the Ashby committee and, which the reader may remember, had been a member organiser of the Asilomar conference. His 'technical solution' for decreasing the hazards of the technology was similar to that of the Murray's: "using bacteria genetically modified to decrease the chances of their survival outside the laboratory or transfer of their DNA to some other organism."[59] Specifically, his solution involved the use of *E. coli* K12 strain, properly categorised as being part of 'weak strains', which were thought to perish outside the narrow conditions of laboratory buffers, and in addition, to introduce mutations which would "ensure that [the *E. coli*] would die as required outside test tube."[60] During Asilomar, several of these techniques were proposed. Introducing mutations that would make the *E. coli* temperature-sensitive or unable to form new cell-walls would ensure that the bacterial host would die outside test tube. *Nature* reporters who attended the conference were excited about these solutions, lauding the session in which they were discussed as "the most productive session at the conference."[61]

In this section we have seen how the ethical dimensions of genetic engineering experiments were ignored and how the public health concerns were reduced to their molecular technical nature, in which part of the worry —the infection of antibiotic-resistant *E.coli* in the human gut— was proposed to be solved by using genetic engineering itself, such as replacing *E.coli* with phage λ or using the K12 'weak' *E.coli* strain. I mark the *molecular* in 'molecular technical nature' because the editorials of *Nature* often alluded to this sort of solutions. For instance, an editorial at the beginning of 1975 questioned the measures proposed by the British committee in charge of advising and developing safety procedures in laboratories. While the Ashby committee

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[59] Wright, Susan, *Molecular Politics*, 143.

[61] Colin Norman, 'Berg Conference Favours Use of Weak Strains', *Nature* 254, no. 5495 (1 March 1975): 6–7, https://doi.org/10.1038/254006a0.

had proposed physical barriers as well as biological techniques as measures against spread of the molecules, the unsigned leader read, "containment procedures are, of course, important in the context of microbial genetics, but should they be the first line of defence as the working party suggests? [...] one [other] possibility is to 'disarm' potentially dangerous plasmids or bacteria by altering their genetic composition a bit more so that, for example, useful but possibly unsafe strains of *Escherichia coli* could not survive in the human gut."[62]

In short, different methods embodied the different knowledges necessary to stop recombinant DNA molecules from leaving test tubes and petri dishes. Physical containment methods, compulsory notifications were to governmental authorities, and the establishing legal frameworks were proposed and backed by those keen on tighter regulation (William, Ford, Locke). Those who advocated with self-sufficient methods for preventing risks by using biotechnologies themselves (Brenner, Ashburner, the Murrays) were those who argued for reduced regulation because 'the self' and the molecular technical knowledge that came with it was necessary and sufficient. This division seems to have stood the test until now, but there are nuances to such a dichotomy. At the end we will see intermediate and intersecting positions. For now, let us look at what happened when things went wrong.

Intermezzo II: The image of the responsible scientist

In 1978, a smallpox outbreak from a laboratory in Birmingham had led to the death of Janet Parker, a British medical photographer, and the related suicide of Henry Bedson, head of the microbiology department where smallpox was researched. The mother of Parker had also become infected with smallpox through contact with her daughter but recovered. The case would eventually lead to prosecution of Birmingham University by the HSE for "alleged failure to ensure, so far as was reasonably practicable, the health of its employees in the east wing of the medical school."[63] While the charges were later dropped, the Shooter Inquiry, headed by Reginald Arthur Shooter, microbiology Professor, and overseen by representatives of the HSE, the World Health Organisation, and the Trade Union Congress, created a great public stir. The Inquiry established that while Parker herself did not have access to the laboratory where smallpox was handled, the death had resulted from "a major break in containment policy" in Bedson's

[62] 'Amber Light for Genetic Manipulation', *Nature* 253, no. 5490 (31 January 1975): 295–295, https://doi.org/10.1038/253295a0.

[63] 'News in Brief', *Nature* 276, no. 5688 (1 December 1978): 553–553, https://doi.org/10.1038/276553a0.

laboratory.[64] The authors of the report interviewed Bedson and his laboratory workers, and signalled "poor laboratory procedures," such as "the failure to use sealed containers to transport infected materials" or "the practice of passing in and out of the smallpox room during work without changing gowns or gloves or washing hands".[65] The exact cause of death could not be determined, but the Shooter report read, "we feel that the transfer of virus from the laboratory to Mrs. Parker must therefore have occurred by one of three routes: — i. on an air current[,] ii. by personal contact[, or] iii. by contact with contaminated equipment leaving the laboratory."[66] Decades later, the death of Parker and Bedson were still being written in The Birmingham Post with 'shock' and unpredictability as driving narrative elements:

> It was a phone call that killed Janet Parker. Somewhere between dialling the number and putting down the receiver, her fate was sealed. But the conversation had nothing to do with it. So when she got a bit of a cold a few days later, she thought nothing of it. The cold turned into flu. Again, nothing unusual, but flu in August? A bit odd. Then she broke out in a rash. The doctor thought it was chickenpox. Mrs Parker took to her bed. That was on August 11, 1978. A month later, Mrs Parker was dead and Birmingham was in panic. It wasn't flu. It wasn't chickenpox. Janet Parker died of smallpox, a disease more associated with the Indian subcontinent than leafy suburbia. Yet she wasn't its first victim. The smallpox virus Mrs Parker contracted hadn't come from another country. It had come from a lab, the one directly below the room from which she had made her phone calls in the Anatomy Department of the University of Birmingham's Medical School. A lab that was run by Professor Henry Bedson. 'Henry was the kind of man who got quite involved in his work,' says bacteriology expert Professor Hugh Pennington. 'A determined man, who would just sort of get on with things. He was very committed to what he was doing.' What Professor Bedson was committed to doing was finding out more about the smallpox virus and its variants, including white pox and monkey pox.[67]

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[64] 'Report of the Investigation into the Cause of the 1978 Birmingham Smallpox Occurence' (London, Her Majesty's Stationery Office: House of Commons, 22 July 1980), 21.

[65] Ibid, 21.

[66] Ibid, 22.

[67] Campbell Docherty and Caroline Foulkes, 'Toxic Shock; Twenty-Five Years Ago a Disease That Many Thought Was Dead and Gone Reared Its Head in Birmingham: Smallpox. Campbell Docherty and Caroline Foulkes Look Back at the 1978 Outbreak and Ask If It Could Ever Happen Again.', *The Birmingham Post*, 4 October 2003, https://www.thefreelibrary.com/Toxic+SHOCK%3B+Twenty+five+years+ago+a+disease+that+many+thought+was...-a0108504745.

How could the image of a "committed," "determined man" and that of the scientist responsible of "poor laboratory procedures" refer to the same person? Yet once more, and an even harsher defence of Bedson's image, the one by the UK Royal College of Physicians, today still tries to provide a cleaner version:

> His special interest led indirectly to the tragic end of his life. A fatal case of smallpox occurred in a person who worked in another part of the same building in Birmingham. Journalists launched a relentless effort to fix the blame on him and his staff for a breach of technique, and union officials stirred up public fears by confusing the issues with those then arising from genetic manipulation. Harassed as the chosen 'villain' of the tragedy, Henry Bedson's normally stable personality broke down and he took his own life. It could be said that he was a victim of his own dedicated conscientiousness, and of his extreme sense of responsibility.[68]

It may be important to recognise the 1970s as a transitory moment. A time of drafting codes of practices, a time when behaviour in laboratories was increasingly being morally codified by formal and non-formal means, and when molecular scientists were beginning to be aware of the dangers of their work materials. As Brian Ford had mentioned in his crusade to establish an overarching biohazard legislation (*Intermezzo I*), the 1970s were a moment when mouth pipetting or the casual discarding of contaminated syringes were generalised practices. These practices, which only from this transitory period onwards were considered "bad practices", could lead to errors and accidents. Several documents reified these practices by creating bureaucratic and legal categories – in order of increased formality of those discussed here: the Berg letter, the Ashby report, the Asilomar report, the Williams report, the Shooter Inquiry, the HSW Act, the HSE's prosecution of Birmingham University. In short, these attempts to formalise practices and narrow them into 'safe' and 'unsafe' ones allowed for the coexisting of several images of a responsible scientist: the one with an "extreme sense of responsibility" and the ultimate responsible for "poor laboratory procedures."

The reification of the practices into categories such as 'wrong', 'right', 'dangerous', 'safe' was the product of legal and ethical negotiations in the social arena (and not only in *Nature*). And with the reification of these meanings, a conditional relationship was established between different 'ways of being' a scientist, a certain identity to adhere to. The different 'selves' in the debated procured different notions of what counted as 'safe' and 'right', and allow for several identities of Bedson to coexist

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[68] 'Henry Samuel Bedson | RCP Museum', accessed 1 October 2020, https://history.rcplondon.ac.uk/inspiring-physicians/henry-samuel-bedson.

in the same person. Let us look at a specific event, which is relevant because two kinds of scientific self were explicitly spelled out, hinging on the different meanings of ethical and legal categories. This time, however, these selves inhabit two different persons.

IV. The ASTM meeting: two selves, two approaches to regulation.

In November 2nd, 1978, *Nature* carried in its first page news about the previous week's meeting of the trade union Association of Scientific, Technical and Managerial Staffs (ASTMS), the largest labour union of technicians and scientists. The leader served as an introduction and a response to the following pages, all dedicated to the ASTMS's meeting. The 'News' section on November 2nd contained the reproduced the talks of two speakers. First, Sydney Brenner, director of the Medical Research Council at Cambridge, who may the reader remember, had given testimony to the Ashby committee and had been one of the organisers of the Asilomar, where he defended a 'technical solution' to the problem of safety. The second speaker was Jonathan King, Professor of Biology at the Massachusetts Institute of Technology and trade union enthusiast. The *Nature* report also contained an interview with one of the event's organisers: Sheila McKechnie, the ASTMS Health and Safety Officer. The talks of Brenner and King were partially reproduced in six pages, the interview of a *Nature* correspondent to McKechnie was printed in a crammed final page. Before these seven pages, the editors introduced the heated theme. By looking at this editorial introduction, we get a glimpse of how *Nature* talked of the different stakeholders of science's regulation – the committees, unions leaders, technicians, and government officials – who all had a stake in the recombinant molecules' movements.

The scientists' union had gathered "to discuss the social and safety implications of genetic engineering," the leader read, particularly in the context of GMAG, "itself an interesting experiment."[69] The editor deemed GMAG as 'interesting' due to scientists being "in a minority on it," counting with eight representatives and the rest of the committee being formed by four members of "the public interest" plus four designated by the Trade Union Congress, two of which chosen by the ASTMS.[70] *Nature*'s editors didn't fully nor blindly believe the motives of the ASTMS behind organizing the meeting, which they thought of as "not entirely free of self-interest," but as an example of the "expanding white-collar union [...] seeking to impress not just its own members but the world at large with deep concern over health and safety and warn the nascent biological industry."[71] *Nature*'s editors positioned themselves (and by way of a generalisation,

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[69] 'Plenty for GMAG to Do', *Nature* 276, no. 5683 (2 November 1978): 1–1, https://doi.org/10.1038/276001a0.

[70] In fact, scientists (eight) were *not* in a "minority" with non-scientists (eight).

[71] 'Plenty for GMAG to Do'.

attempted to bring with them the majority of scientists) as suspicious of the role of trade unions in science: "research scientists not associated with the union [...] might also have been somewhat uneasy at what exactly was meant by the talk of trade unions getting more involved in the making of science policy."[72] The main source of this 'unease' was the lumping of two debates: genetic engineering on the one hand, and safety in the workplace on the other. The *Nature* piece accused those using the images of science's tragedies (such as the smallpox outbreak three months prior) to pollute the debate over recombinant technology regulations.

While the source of the outbreak had not been a recombinant organism, the disaster did happen in a laboratory, and the ASTMS did not distinguish between the two. Clive Jenkins, ASTMS's national secretary said in an November televised interview (before the publication of the Shooter report) about the Parker-Bedson accident of August:

> Jenkins: What we are aware of is that these hybrids, which were created by former genetic manipulation, after twelve years, they were taken out of the deep freezer in July. And I don't really believe in coincidences, but I know now that it's been said that she died of an unusual strain of viral [inaudible] smallpox, which by the way, wouldn't be in Birmingham if it hasn't been specially imported in.
>
> Reporter: But here we have a medical expert saying categorically that Janet Parker did not die of any unnatural form of smallpox. Can it not be that you are scaremongering?
>
> J: Now, Professor [inaudible] is going to say, that one of the man-made viruses that he and Professor Bedson together made, didn't kill her, I think, obviously we will have to give his opinion immense weight. I also want to know what virus infected her mother, because, if there had been a public inquiry, we could have dealt with the statement now in circulation that the virus that infected her mother, that is being analysed in a different place, is behaving very oddly indeed.
>
> R: What evidence do you got about that?
>
> J: I think, all I can say at the moment is, the entire scientific community is discussing this affair. This is not a localised Birmingham incident now, Birmingham could, after all, if you had one case of smallpox, could have been the centre of smallpox. And that means, a disease eradicated throughout all of the word, could have brought to live, in Birmingham's population. [...]
>
> R: You really think that something terribly dangerous happened in Birmingham?

[72] Ibid.

> J: I think that [the] Birmingham University affair is going to be like Watergate, that is, as you keep turning over the stones, you'll find the situation like this: that work was accelerated, in an unsafe laboratory, which didn't meet the international requirements, dealing with dangerous pathogens, which no longer exist naturally in the human population, and that this may be occurring elsewhere.[73]

However, for *Nature*, bringing the Birmingham case, still "fresh in everyone's mind," into the debates over the regulation of genetic experiments could have unwanted consequences. This, for the editors of *Nature*, was a "slippery path that ends up with recombinant DNA, germ warfare and test-tube babies all being lumped together in the public's mind."[74] In this leader there was not only distrust towards the union, who *Nature* saw as knowingly stirring "the passions" raised by the Birmingham case and connecting them to the safety of genetic experimentation; they were also distrustful of 'the public' in comprehending the difference between the two, and thought of the latter as "imponderable matters." The meeting seemed to have been passionate, and "in the end there was little debate," which for the *Nature* "indicate[d] the level of public understanding of the question," clearly supposed to be deficient.

To *Nature*'s editors there were additions "questionmarks." "One is the problem of industry, which is moving rapidly into genetic engineering," the editor wrote. The problem, he argued, was the lack of representation in the ASTMS meeting. While he complained about the too-much presence of citizens and union representatives, the author of the leader complained about too-little presence of "industrial directors and managers at the meeting." It was a shame, according to the *Nature*'s piece, to hear only one industrialist's voice, who had "made it clear that industry viewed the restrictions on genetic engineering as oppressive," and "would have liked [...] simply a technical committee (which would inevitably rest on an uneasy separation of facts from value judgments)." While we can see that the editor was at least superficially distrustful of the industrialists' position, he saw cooperation between the different stakeholders of science (for-profit companies, unions, and university scientists) as vital. *Nature*, as the textual reporter of the event, argued for a "reasoned dialogue over their [the stakeholders'] hopes and needs," otherwise, the editor thought, "the future for the British biological industry will not be rosy."

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[73] 'ATV Today: 09.11.1978: Smallpox Outbreak', 16mm, *ATV Today* (Associated Television, 9 November 1978), MACE Archive, https://www.macearchive.org/films/atv-today-09111978-smallpox-outbreak.

[74] 'Plenty for GMAG to Do'.

‘Social responsibility’ as scientific freedom

Now that we’ve seen were the sympathies of *Nature*’s editors lay, we can now move to the first of the reproduced talks – that of Sydney Brenner, the Cambridge molecular biologist. His argument was one often employed by those scientists fearing regulation. His talk is especially relevant because it spelled out very concretely the argument for reduced regulation, based on three successive arguments. Let me first briefly schematically reproduce his line of argumentation. The opening argument was a social one, in which Brenner argued that those working with recombinant DNA technologies, “genetic manipulators (if such a profession exists),” as he said, should not be pointed out for doing potentially dangerous research, and specially, that they should not be singled out from among other researchers, like those working with pathogenic bacteria and viruses.

This argument of social fairness was accompanied by and based on a scientific argument. The biologist claimed that it was factually wrong that genetic engineering was dangerous in and of itself, and that it was actually better and safer than other kinds of genetic manipulation because you could design and construct harmless genetic organisms. This biological argument was the main one and rested on the inability of analytically separating ‘naturalness’ from ‘artificialness’.

Finally, from these arguments laid the final one in which he argued that since molecular researchers had sufficient technical knowledge to understand and diminish the dangers associated with DNA modifications (by e.g., deactivating the genetic constructs), scientists and only they should proceed on determining the danger and should be able to decide for themselves.

Now, I would like to illustrate my schematic reproduction of Brenner’s argument with his own words. First, the social argument:

> It is the perception of people in the field [of genetic engineering] that right next door there are people who do much worse things—you know, with the fags hanging out of the mouth, and a cup of tea, doing it any old way they like. Now I think this is quite important, because one should desperately avoid the situation that one branch of science is singled out for the delivery of social punishment. Now I don't want to say this has happened, but I do want to emphasise that this is a kind of connection between science and society that we must avoid: that if scientists have been bad boys, as many people think—science is being questioned—then the genetic manipulators can so to speak carry the can and get six months in category four. That is a psychological situation that must be avoided.

Figure 12. Accompanying image to Sydney's Brenner's talk.

To "carry the can and get six months in category four" referred to the containment risk category four, as recommended in the Williams Report. The sense of urgency in Brenner's words was accompanied by the penitentiary metaphor ("to *get* six months") and its associated image (figure 12).

To be a molecular biologist and to be singled out was not just a matter of social unfairness. It rested on an impossible distinction between 'natural' and 'artificial':

> A researcher can put antibiotic resistance into *Shigella sonnei* with no control; and you have to ask why isn't anybody controlling him? No one's controlling him because he is using natural mechanisms. That is, he's using the mechanisms used in nature to generate new strains—mutation, recombination, even perhaps genes that jump around from one piece of DNA to another; all he is applying to these mechanisms is special selection. He is fishing out of nature events that may be extremely rare and enriching them in a laboratory. [...] What a genetic manipulator does is to do things in vitro. In theory, if you could do genetic manipulation by avoiding the use of test tubes then in fact you would be doing something that was a natural mechanism, and so in theory you might argue that you could escape from the GMAG regulations. I'm not offering that as a possibility, but I just want you to realise that the difference between genetic manipulation and other biology as understood by GMAG is that it is a sort of 'confined area' of genetic construction.

Brenner's worry was that although this distinction between 'natural' and 'artificial' was a "biological myth," it still corrupted legislation in science. In doing so, it violated the freedom of scientists, a freedom that must be protected in the face of the "intellectual" and "social" responsibility towards the scientific process. Of the latter he referred to the "strong scientific onus that we do not enshrine in legislation myths of biology," referring to the natural/artificial division that he had previously questioned. The 'social responsibility' of science, he said, referred to the maxim that "we," without specifying who that that 'we' referred to but possibly alluding to both the scientific community in specific and society at large, "should not be imposing on a subsection of our scientific community and technicians and practitioners controls that appear to them—and objectively—completely extreme compared to what is going on in other fields."

It is interesting that here we see a meaning of 'social responsibility' quite unlike, and in fact, perpendicular to those discussed in Chapter Two. The meaning of 'social responsibility' used by the BSSRS, for instance, lay in the idea that scientists ought to respond to social needs and social demands, as a sort of civil servant. The meaning of Brenner's 'social responsibility' rested on quite another ethical claim, that is, that society ought not to interfere in the decision-making of scientists. They ought not to interfere because the only knowledge necessary to address risk was a technical one, and the only way to reduce danger was through molecular engineering, which in his view, it could "be argued very strongly that it is a way of containing things, of moving them away from organisms that are their targets, and locking them up in other organisms where they can do nothing." Brenner intended to close the black box of safety, and did so elegantly through a very visual metaphor at the end of his talk:

> Years ago when I first started to travel in aeroplanes, with a very crude knowledge of physics and aerodynamics, I used to sit in those DC3s, and I had that marvellous vision with which I could look right into the aircraft engine; and I could see in all detail all the parts going round and round, the spinning of crankshafts and pistons and so on; and then I could actually see those hairline cracks developing. It used to worry me. I think many people look at genetic manipulation with that kind of internal vision. They see lying on the Petri dish the one horror bacterium, the one horror colony, the colony that is going to escape off that Petri dish and create global disaster. I think that that feeling forms one of the most difficult hurdles to cross in trying to do objective risk assessment. And I believe that to be objective is very important. In the past in this field, we have had no more than the balance of example and counter-example. I have sat in on hundreds of arguments which have got into details that even mediaeval theologians could not have reached as to the total number of nucleotide base pairs that should be allowed under section B subsection A paragraph 1.2.

The aircraft engine had to be shut to prying eyes. By using codified language and positioning his work as more complex than unenlightened scholasticism, Brenner interlocked the scientific and the bureaucratic. He attempted to close the box of genetic engineering by being "objective", away from the "hundreds of arguments" that social beings could get into. But not all scientists were willing to go through that path, and not all feared humans.

'Social responsibility' as the "right to be weakened"

In discussions over the ethics and social stance of technoscience, the *technical* standpoint is often contrasted with the *social*, as in *reductive* set in opposition to *inclusive*. But, does such a dichotomy stand the test? Can we envision more complex discourses? How would an alternative to the molecular hyper-technical look? What would a socially attentive assessment of technical discussion look like when spoken by a molecular scientist? While Brenner's talk could be said to be a most explicit spelling of the argument in favour of science's isolation, *Nature* reproduced also the talk of quite another angle. Jonathan King spoke at the ASTMS event, and talked of 'New diseases in new niches,' in which the 'molecular', the 'social', and the 'political' were inextricably intertwined. To understand how the 'molecular' could be 'social', it may be worthwhile to attend to King's arguments in detail.

King was researcher from Brooklyn and Professor of Biology at the MIT. As Brenner, he spoke at the ASTMS meeting. And like him, King wasn't fearful of scientists' expertise and ability to make risk assessment. However, King *was* fearful of the application of that expertise at the level of mass production: "from the research point of view recombinant DNA technology can be employed safely […] but perhaps more relevant here is the new production technology, technology that will be used to manufacture commodities for sale."[75] He brought forward the example of Eli Lilly, the recently foundedx biotech company, which was planning to produce human insulin "through the growth of thousands of gallons of *Escherichia coli* containing human DNA sequences spliced into a bacterial plasmid." The practical application of recombinant DNA technology, that is, the synthesis of *marketable* biomedical products – which was the most commonly praised virtue by both those who defended lesser regulation and those who advocated for tighter directives – was, in the eyes of King, seen as a plausible threat to public health. The likelihood of this threat, and the worries of King, were informed by a specific historical sensibility. Referring to nineteenth century industrialisaiton:

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[75] Jonathan King, 'New Diseases in New Niches', *Nature* 276, no. 5683 (2 November 1978): 4–7, https://doi.org/10.1038/276004a0.

> For example the mechanisation of cotton textile manufacture resulted in a drastic increase in damage to the respiratory tract of the operatives (byssinosis or brown lung). Developments in the German chemical industry—such as the synthesis of the aniline dyes which were used to colour the textiles—entailed the production of potent bladder carcinogens—4-amino-biphenyl and ß-naphthylamine. […] [I]n assessing the risk of cotton dust, we do not examine the effect of cotton fibres on human skin; we examine the effect of cotton fibres on human lungs. Of course, manufacturers in the industry often say "no that's wrong, because the cotton fibres will never get into the workers' lungs". But we know that that it is only if people understand acutely what will happen if those fibres *do* get inside the lung, that action is taken. And knowledge in the past has not been sufficient, it's taken much more action than knowledge.[76]

The key question that King would have liked to have formulated was: 'what happens if cotton fibres contact lung cells?' instead of 'what happens if cotton fibres contact skin cells?' The research question is quite different. The former situated the risk in the worst possible scenario—in a scenario where cotton fibres *do* get out of where they are supposed to be. To King, asking whether the fibres cause problems in the skin wasn't a sensible question to be posed in order to make risk assessment into the safest direction.

The large difference between recombinant technologies handled at the commercial scale and dangerous chemical products was, in the eyes of King, that for chemicals, "the risk is finite," whereas "in the case of bacteria we do have to worry that these organisms […] will move through the ecosystem, transfer for example from the debilitated strains to wild strains of bacteria, and get into strains which perhaps are well-adapted in a particular niche out there," thus opening the possibilities of 'new diseases' (designed through recombinant DNA technology) establishing themselves in unexpected 'new niches'. The dangers of mass-production and the self-replicability of DNA entailed a larger problematic than chemical dyes. The way in which King conceived risk was, however, similar. The biologist aimed to look back and learn from the past:

> Now at this point I wish to clarify and punctuate a very crucial aspect of risk assessment. In trying to assess hazard we must consider what would be the properties, for example, of a wild strain of *E. coli*—even an epidemic strain of *E. coli*—expressing, for example, the human gene for insulin. Now some in the audience will heatedly reply—or they would if I were at home—"but they'll never get into wild strains, they're in debilitated strains, you've got nothing to worry about, you're raising a false spectre". This is of course putting the cart before

[76] Ibid. Original italics

> the horse. The only way that hybrid DNA will be contained, is if people understand that if it is *not* contained there may be problems.

Here we arrive at one of the apexes of King's argument: in order to assess risk, we should be looking at the worst-case scenario: a scenario in which the potentially dangerous products, be it dyes or replicating bacteria*, do not behave as expected*. King, however, was not satisfied, and continued by bringing another element to his argument: susceptibility. And again, by drawing from a historical example. This time he referred to the outbreaks of cholera in nineteenth-century England, and how this case was a perfect example of how living conditions made people either susceptible or resistant to the disease. Taking the example of Manchester, King drew the attention about how the north-west of the country "was rapidly converting from cottage production of cotton textiles to full-scale factory production," and that the industrialization brought with it massive exodus from the country to the cities, "and essentially forced [people] to live in housing not of their own design." King narrated the conditions under which factory workers would live: "miserably crowded, very little light or ventilation, no sanitation, no proper water supply, no means of disposing waste, thus garbage and excrement pollution the waters used for drinking and washing and food preparation." Drawing from a progressive account of public health history, King celebrated the developments of the previous century, and attributed the demise of cholera to the "alliance between progressive scientists and public health people, and—very importantly—the labour movement, which played a primary role in the fight for decent sanitation and public health." What this biologist saw as the key in having fought of cholera, was the improved living conditions of the present, in which "water supplies and proper sewers are considered social necessities rather than individual privileges, [and] decent food is available to most people."

King brought this 'social side' of the story of cholera closer to his biology expertise, and explained it also 'molecularly': "Cholera thrived in these industrial districts because these organisms multiply in the intestine, where they elaborate a toxin; and this toxin binds and penetrates the cells of the intestine, and inactivates a protein of the intestinal cells which is needed in protein synthesis. The organism however goes out in the faeces, and if you live in an area with a contaminated water supply and you drink that stuff—boom —you get cholera." King situated the 'social' and 'molecular' story within an 'environmental' account, placing the textile workers' exposure to lung-damaging fibres as a worsening *condition* for developing cholera. Textile workers in nineteenth-century north-England worked with cotton fibers which would quickly damage their lung tissue, he said, "particularly among operatives of the carding and combing room." This, King reported, made them "unusually susceptible to tuberculosis and pneumonia infection." As well, it was not only a matter of work conditions, but also of the settings in which these population lived. “Given the conditions that they lived in at home, where there was very very [sic] close person-to-person contact, contaminated food, no pasteurisation, etc, there were once again created special conditions for these organisms to thrive,” creating new niches that facilitated the propagation of diseases like tuberculosis and pneumonia.

Once again, King drew parallels between this story and the present. Still within the context of the textile industry, he took the example of North Carolina and New Jersey in the USA, both with a large presence of cotton mills and a high incidence of byssinosis. Byssinosis is lung disease caused by the process of the grinding up the cotton goods, which turns them into a “very fine dust.” This microparticles, King explained, are the primary cause of byssinosis in cardroom workers. When “these people occasionally go to the hospital,” King explained, “they are diagnosed as having anticema [sic] or some obstructive problem in the bronchi.” The typical solution would entail a surgery, and when this is done, they often “pick up a hospital infection, a hospital pneumonia,” which are mostly due to infections by *E. coli*. King brought forward that this was the case of cotton mill workers, but also of cancer patients who go to the hospital, for example, to get chemotherapy. Since they are immunosuppressed, they are more likely to develop infections from *E. coli* strains from the hospital. King did remark that these sorts of infections where “not the spreading epidemic type,” but an example of infection from a “point source, [...] more like the bladder carcinogens coming out of the aniline dye industry.” This ‘point source’ is “where the organisms are surviving,” a reservoir that is waiting from when “somebody comes who’s debilitated or weekend and they pick up the infection.” While not epidemic, infections by hospital *E. coli* seemed to be a major cause for public health concern. The problem was that *E.coli* infections did not pose the same risk for everybody, and to illustrate this, King brought his point home to a more personal level, and within the context of gene cloning: “I don't mean to scare you—I'm now talking about ordinary antibiotic resistant *E. coli* infections—not recombinant DNA.” Recombinant DNA offered the possibility of unknown bacterial characteristics, such as increased antibiotic resistance. Thus, King had gone all the way from the historical cases to the current-day social and medical conditions of textile workers, and later on, cancer patients, in order to sensitize his public for what it means to be “debilitated” or “weakened.” By drawing the voice of the second-person singular, he further brought the sensibilities home, to advance a very explicit point:

> Now these people are weakened—but that's what happens when you go to the hospital, you're in an automobile accident, or maybe a little baby and your immune system hasn't fully developed; we have a right to be weakened, there's nothing wrong with being weakened.

Why would he bring this sort of arguments to the table? What do weakened bodies (factory workers, cancer patients, automobile accidents, babies) have to do the danger of recombinant DNA technology? The worry that King had was that 'new niches' would originate in tissues of weakened people, in which 'new diseases' created in the laboratory (by introducing target 'disease' genes to be studied into bacteria such as *E. coli*) could potentially infect. Thus, 'new niches' referred to tissues of immunosuppressed patients that could be susceptible to the colonisation by bacteria carrying potentially dangerous genes. Recombinant DNA technology made it possible to study gene function of diseases, and while this could prove to contribute to medical progress, King argued that model organisms with recombinant DNA could become 'new diseases' that would, as he tried to show, affect unevenly populations through 'new niches.' For that reason, he was insistent about the importance of not letting recombinant DNA molecules scape the laboratory.

Beyond disease, the danger wasn't limited only to scientists working with *E.coli* to study their etiological genes. This could be the case for the introduction of insulin and somatostatin genes into bacteria in order to produce biomedical products. This was the aim of companies like Genentech or Eli Lilly, and King was worried about the possibilities of their apparently inoffensive genetic material could cause 'new diseases in new niches':

> What about insulin? I don't know what would happen if a newborn [sic] infant picked up an *E. coli* meningitis infection and that bacterium was spooing out [sic] human insulin; but it seems to me there's every reason to be concerned. And I can't tell you what would happen if the genes for somatostatin, another potent hormone which has been cloned for commercial manufacture, were to be put into a wild strain of *E. coli*; again there's every reason to be concerned. And any endocrinologist not in the pay of the company producing it would have to give pause to the uncontained DNA.

As I have tried to explain here, King's concerns with the new technology were varied. They can be summarised as follows: risk assessment must be done thinking of what would happen if those 'things' *do not behave as expected*, including their behaviour in 'susceptible' environments of weakened people. In the context of recombinant DNA technology, those 'susceptible' environments included also 'unexpected' tissues whose colonisation could be made possible by the reshuffling of genetic material.

What solutions did King propose to these complex issues? The main aim for him was that "every effort must be made to see that the genes stay within the debilitated strains; and that these debilitated strains stay within the laboratory." The mentioning of 'debilitated strains' made reference to the *bio*technical solution proposed by Brenner of 'deactivating' the bacteria under study by e.g., using strains that would survive only in highly artificial environments and thus perish in human tissues. But King did not stop with this *bio*technical solution. He continued, "Now this is going to require absolutely the fullest participation of laboratory workers and production workers. And you are not going to be able to do it without strong union participation." His argument was that scientists should not only think of what happens in laboratories and the minute quantities handled there but should also take into consideration the whole process of production of biomedical goods, which trade unions and production workers knew much more.

> If you can ensure that laboratory workers don't pick up these strains whether in their nasopharyngeal passages, or in a cut, or in a urinary tract infection, then you know they are not getting out into the population. The only way we know that can happen is if that sector of the population is fully involved in the safety standards [...] the protection of people from disease involves a tight alliance between essentially progressive scientists and public health people—people who put that as their primary goal— and the trade union movement.

King disliked scientists who were isolationists and wished to slip out of regulations. For that, he recommended international collaboration:

> that has to happen at the international level; because a small sector of the scientific population which is disturbed by this control, which is disturbed by having technicians having a say in the decisions, flies around to international conferences to figure out how to get out of the guidelines, how to weaken the guidelines, how to avoid having trade union participation, and it can be very important that people like ASTMS make contact, for example, with groups in the United States, the oil, chemical, and atomic workers, the French unions, the German unions, the Belgian unions, who represent the same sector of the population, not only to protect themselves but to make sure that in the long run that we're all protected and that the benefits of this new technology— which do exist, and I do believe in them —are realised.

This was the heart of King's argument and his final recommendation. Let's now take a pause, and fast-forward to 2011.

King believed in 1978 that the technology would bring benefits. He continued doing molecular work and is still today a Professor of Biology at MIT. Having a tenured position at MIT since 1979 certainly make him an 'insider' of science, but he did recognise that his plea for scientists to get involved politically was that of an outcast.

The 1970s were, however, ripe for this sort of ideas. In a lecture in 2011, King recalled the times of the recombinant DNA debates within the larger counterculture movements of the 1970s, where scientific personalities such as Linus Pauling or George Wald positioned themselves as "leader[s] of the anti-war campaign."[77] In his 2011 lecture, King talked both about his research and the political place of science and scientists. He thought of himself as one of those "progressive scientists," working in close quarters with politicians: "I had very valuable connections with the Labour movement through Tony Mazzocchi, [Vice-President] of the Oil, Chemical and Atomic Workers [Union]. I actually knew about occupational health and safety issues – I wasn't just spouting."[78] As someone who had been close with union workers and Labour politicians, he hadn't only been vocal in debates over workplace safety. With microbiologist Jonathan Beckwith, they authored in 1974 the piece, 'The XYY syndrome: a dangerous myth' (*New Scientist*) in which they critiqued the association of the extra X chromosome with increased anti-social and criminal behaviour.[79] They called against these reductionist views of genetics by providing a scientific critique of the studies that attempted to draw such associations. They attacked the study design and the statistical interpretation of those claiming to have found the biological basis of the XYY syndrome. In a grander tone, they warned that these studies could be the "opening wedge for programs with more serious eugenic implications."[80] King categorised the XYY affair and the health and safety debates as part of what he termed, "the democratic oversight of science of technology."[81] During his 2011 reflections, he remembered the movements which he was involved with, those …

> … many social issues, all of which really had the character of democratic oversight of science and technology. Some are in sociology textbooks, like the XYY affair. It was, me and Wald and Ruther, [who] we were all excoriated for calling for regulation of recombinant DNA technology. Years later the biotechnology industry said, 'thank god that they set up a system of regulation that allowed us to thrive without worrying about what was going to happen.' […] A group of us organised a campaign called, 'the pledge against the military use of biological research.' We got these signatures by circulating the petitions at scientific seminars. We didn't segregate, [we] went to Cold Spring Harbour, gave

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[77] *A Conversation with Jonathan King* (MIT Biology, 2011), https://youtu.be/d16-zdqJq_8.

[78] Ibid.

[79] Jon Beckwith and Jonathan King, 'XYY Syndrome. A Dangerous Myth.', *New Scientist* 64, no. 923 (14 November 1974): 474–76.

[80] Ibid.

[81] *A Conversation with Jonathan King.*

> a phage talk, and then circulated the petition. You can't do that nowadays, that wouldn't happen.[82]

Interestingly, the reference in this quote to the regulation of biotechnology and its need for progress, coincides with one of the basic tenets of *Molecular Politics*. As the reader may recall, Wright claimed that establishing regulation and drafting it among peers was the lesser evil to be picked: regulation was a mechanism "designed to enable research to move forward".[83] As King recognised, this was later lauded by the biotechnology industry.

In short, King condemned those scientists who saw regulation as a hurdle to their individual research and was very suspicious of their isolation from society. Coming back to his 1978 talk at the ASTMS meeting, he had harsh final words for those scientists who segregated science and politics:

> in the United States we have scientists who in their public statements say 'I'm only interested in the increase of human knowledge', while at the same time they have engaged lawyers to dissociate themselves from NIH funding, and get private funding, so that they can take out the patents. [...] Take two of the venture capital corporations in the United States, Genentech and Cetus Corporation, both in California; the one has backing from International Nickel and the other from Standard Oil of Indiana. Their front men may be research scientists who say, 'we are only interested in the expansion of human knowledge', but the background there is very different. [84]

The appeal to 'pure knowledge', King suspected, was a façade that concealed economic interest. In 2011, he still advocated for a non-segregation of science and politics: "There was never a time in my life that it ever occurred to me that there was a separation between being a working scientist and these larger social issues. [...] All the people that I admired did both."

Concluding remarks

The meaning of 'technical' refers to the set of expertise necessary to perform certain kind of knowledge. This expertise may be theoretical, observational, or practical: they refer to the operations (mental, physical) that are needed to interact with worldly objects

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[82] Ibid.

[83] Wright, Susan, *Molecular Politics*, 145.

[84] King, 'New Diseases in New Niches'.

in order to construct and operationalise functions. As such, each technical knowledge is intimately related to certain kind of approach to these worldly objects: each technical knowledge prescribes attitude, a certain kind of self in this world. The theoretical construct of 'epistemic virtues' accurately addresses the ethical dimension that any such expert technical knowledge is tight too. What I have wished to show with this chapter is how 'technical' knowledge not only prescribes how to interact with the laboratory devices at hands, how to operate them, how to read out its data output, how to clean them, turn them on, prepare them, and interpret them — 'technical' here was tied to certain trust in the output of those knowledge forms and specially, certain responsibility for others in the face of safety risks. 'Technical' here prescribed not only certain epistemic behaviour (certain kind of experimentation) but prescribed also of political action (how those experimentations are ordered in the social world). *Molecular technical knowledge* in the 1970s in the context of recombinant DNA technology tended to be associated with a trust in how those molecules would behave, and certain extrapolation of the molecule's behaviour outside the environment where that expert knowledge was comfortably operating: test tubes, petri dishes, molecular diagrams. As such, the molecular technical experts tended to be trustful in politically organising the world according to the knowledge they generated themselves, and as such, being responsible and accountable for the management of the people operating that knowledge.

However, not all molecular technical experts relied solely on knowledge of the molecules. Some of the actors encouraged a more diverse set of knowledges to address dangers, including scientific knowledges like environmental, but also human knowledges like legal, bureaucratic, political, and historical. What I have tried to characterise here is both how these knowledges were to interact with the scientific knowledges, and how the most fervent proponents of a *molecular* technical saw this sets of descriptive and prescriptive alternatives as annoyances that difficulted freedom of their scientific practice. While this may seem to suggest all too well the notion that scientists did and don't care all that much about ethics, I would argue that they in fact did, and it was precisely by virtue of how science came to be valued that they could dodge all these sets of knowledges and characterise them as nuisances, or even a threat to the 'social responsibility of science'. I will argue in more detail in the Closing Chapter that the situation that I have described here, in combination to that described in Chapter Two, invites us to think about how scientists characterised research ethics. In specific, I will argue that research funding is inescapable from ethical consideration, and that ethics were largely seen as *epiphenomena* of molecular work, as arising after-the-fact of 'technical' research, thought to be productive and a good endeavour in itself.

To find meaningful conclusions of the empirical work presented here, it will be necessary first to put the accent on the methodology of this thesis, and the concrete way by which texts and images interact. In specific, I return now to the process of writing

Chapter Two: what do epistemic virtues have to do with scientific advertisements? Answering this question allows me to answer the final question of the Closing Chapter: what do ethics have to do with science?

Chapter Four. **Scientific advertisements are a form of art.** On what images mean in history, and on the relationship of scientific advertisements to epistemic virtues.

Scientific advertisements are a form of art. This is at least what Susan Sontag would have said if she had accompanied advertisements with her commentary on what it means to interpret visual objects. And I am certainly with her when I write that scientific advertisements are art. But this begs several questions: what do we mean by art, why are they art, and how should one interpret them to say meaningful things about the history of science?

I see this chapter as an answer to these questions. It is important to put my cards on the table and show some of my a priori philosophical commitments when I wrote about adverts (and as we shall see, the editorial pieces as well). In writing this chapter I attend to the call of philosopher Martin Kusch for the need of “reflexivity” in thinking about and communicating our own epistemic virtues as historians of science, especially when we claim to historize the epistemic virtues of others.[1] And in this attitude, I am also with historian of medicine Roger Cooter, who appeals for “the abandonment of the idea that the historian stands as if on Mars,” and that acknowledging one’s pledges for certain virtues means accepting one’s inescapable “epistemic present.”[2] In our condition of twenty-first historians, Cooter argues, we must face and reflect on the epistemic virtues that transpire our discipline, such as the unspoken rejection of the “hierarchic ordering of causes of any historical happening,” who Cooter assigns to our postmodern condition, regardless of whether we consider ourself so.[3]

This chapter was written post-hoc to Chapter One, where I dealt with scientific advertisements. Some of the ideas expressed here were co-constructed while doing the empirical work, but I decided not to include it in that chapter, nor as a sort of historiographical introduction. This chapter is positioned towards the end of the cue, but it is not an afterthought. It’s located according to the chronological moment when it was written, and its position should be seen as an attempt of showing history in the making, and within this thesis as a process of learning, amending, and experimenting. As well, this chapter and its position serves two clear purposes: first, it is an statement of my analytic position towards images and their relation to the text; and second, it is a

[1] Kusch, ‘Objectivity and Historiography’; Martin Kusch, ‘Reflexivity, Relativism, Microhistory: Three Desiderata for Historical Epistemologies’, *Erkenntnis* 75, no. 3 (November 2011): 483–94, https://doi.org/10.1007/s10670-011-9336-5.

[2] Roger Cooter, ‘The End? History-Writing in the Age of Biomedicine (and Before)’, in *Writing History in the Age of Biomedicine*, by Roger Cooter and Claudia Stein (Yale University Press, 2013), 7, 14.

[3] Ibid, 17.

conceptual set-up for the closing chapter, where I will deal with the historiographical relationship between the previous chapters, and the worth of this thesis.

In short, it is 'I' who writes. Following Cooter's steps, to pretend to engage in an unembodied practice of history writing is merely posing as lacking agency or will – a position that particularly historians of science seem eager to take when gazing at scientists but who less often direct upon their own keyboards. This is an attempt to self-reflect, and if all fails, let it simply be a methodological chapter.

In the introduction I claimed that no study in the field of history of science has worked with scientific advertisements as sources to explain the relationship between commercial manufactures and researchers, and that there was virtually no study who even mentioned or used scientific advertisements at all. This *is* true. But there are some studies who have taken this sort of visual objects in their studies. Their approaches were, however, not entirely satisfactory for the study I set myself to carry out. But in drawing methodological departures based on diverging philosophical assumptions, I was able to polish what I thought my approach to the visual would be.

The few authors that take scientific advertisements as *the* central piece to be investigated are not within the field of history of science. They present their studies as assessments of the accuracy of the claims of adverts in respect to the standing scientific literature. This approach is common in studies which delve onto how pharmaceutical drugs are advertised in mass media, and some freely talk of "disconnect" between what is advertised and what is expressed in the expert literature.[4] This approach characterises the scientific literature as a static slate of truths that can either be correctly or incorrectly reported in the non-expert media, and potentially mislead "vulnerable populations" such as anxious and depressed patients seeking pharmacological treatment.

To my knowledge, even fewer studies have been devoted to study advertisement in technical literature itself. One of those instances, dealing again with truth statements in pharmacological advertisements, investigates how drug advertisements in medical journals are presented. The authors claim to assess the accuracy of ads by looking at whether they contained authoritative bibliographic information, and whether what was claimed was exaggerated, false, or accurately reporting scientific studies; and give advice to the medical population: "doctors should be cautious in assessment of advertisements that claim a drug has greater efficacy, safety, or convenience."[5] Their analysis is important, and I believe, invites to a more penetrating reflexivity than the

[4] Jeffrey R Lacasse and Jonathan Leo, 'Serotonin and Depression: A Disconnect between the Advertisements and the Scientific Literature', *PLoS Medicine* 2, no. 12 (8 November 2005): e392, https://doi.org/10.1371/journal.pmed.0020392.

[5] Pilar Villanueva et al., 'Accuracy of Pharmaceutical Advertisements in Medical Journals', *The Lancet* 361, no. 9351 (January 2003): 27–32, https://doi.org/10.1016/S0140-6736(03)12118-6.

previous one. By making explicit that highly trained doctors, and not only "vulnerable populations", are susceptible of being misled by large fonts and impressive perspectival full-colour photographs, the authors highlight that the elite technoscientific class (including here also medical experts) is also a 'public', a "vulnerable population" in the eyes of commercial companies.

As far as I know, only one study in the field of history of science uses advertisements in specialist literature for research objects as source material. And I would like to put an emphasis on my selection of the word, *uses*. Michael Fortun, in his chapter in *Private Science*, 'The Human Genome Project and the Acceleration of Biotechnology' tells a story about the rhetorics of 'speed' in the twentieth-century chase for Big Science. Fortun's contribution is certainly welcome, as he argues that the need for speed was inherently "constitutive" of the Human Genome Project, and critically analyses the political dimension of quick-paced science, both as a demonstration of the competitive nature of contemporary Western science, and with an attention to how scientists experienced this new way of working, some of which were considerable concerned as they were "acutely aware of their precarious and ephemeral position between obsolescence and nonexistence."[6]

Fortun comments on a variety of textual objects. He analyses symposia proceedings, science news, public addresses of scientists, congressional hearings, committee reports, and popularised science books to distil the discourse on 'speed' of scientists, administrators, and politicians. As well, he uses the advertisements in specialist magazines targeting molecular scientists that allude to the notion of speed, such as the one of the company BIO-RAD whose header in the advert read, "InstaGene™ Matrix: PCR-Quality DNA. Fast… Really Fast."[7] How exactly does Fortun *use* advertisements? Let's take a look:

> RFLP [Restriction fragment length polymorphisms] is just one technology in a genomic armamentarium that has grown to include the polymerase chain reaction (PCR), fluorescent in situ hybridization (FISH), souped-up vector vehicles such as yeast artificial chromosomes (YACs), and dozens of other cyborg assemblages, including the centerpiece of speed obsession and production, the DNA sequencer. Such standardized tools and biomaterials for genomics research are now advertised on their basis of their ability to create speed, to reduce the time quotient in the equations of efficiency (see Figures 1, 2).[8]

[6] Michael Fortun, 'The Human Genome Project and the Acceleration of Biotechnology', in *Private Science. Biotechnology and the Rise of the Molecular Sciences*, ed. Arnold Thackray (Philadelphia: University of Pennsylvania Press, 1998), 187.

[7] Ibid, 186.

[8] Ibid, 184.

Opposite side to this quote, a full-page reproduction of the advertisement. Nothing else, no textual elaboration besides single-sentenced figure captions. It is fair to say that Fortun *uses* advertisements. I say that Fortun *uses* advertisements because their function in his chapter becomes simply a visual pointer for his narrative in the text. As if he was a tour guide, Fortun walks the reader down a busy marketplace as he comments on the movements of the local citizens among the grocers. To make the tour more entertaining, he points to the glass window shops, but doesn't consider stopping. Fortun uses the same mechanisms as the marketing departments who first manufactured the adverts to arrest their readers and convince them of the truth of their statements. While BIO-RAD used the visual media to construct trust with the readers of the scientific magazine on the claims of the ad so that they would buy their product, Fortun employs the visual objects to construct trust with the humanists who read *Private Science* on the veracity of *his* narrative, relying on ads as unproblematic representations of the general discourse on 'speed'.[9]

Fortun refuses to dissect or employ any technique to situate the advertisements in the text. Why did he pick those ones? What does an advertisement for Beckman's DNA synthesizer have to do with a USA congress hearing? We will hardly know, and despite being thematically similar, they are only so at the level of the historian's wish to highlight the central aspect of 'speed' in the discourse of modern genetic biology. The ubiquity that he tries to demonstrate is limp from the author who claims to have 'found' it.

What historian Roger Cooter means when he calls for the well-needed self-reflection of the historian is a call for probing into the concepts and commitments that maintain our historiography. In the case at hand: what are the concepts and commitment of our approach the visual? In which intellectual traditions (historical or not) does our way of seeing perform the interpretation? To these questions, Cooter together with historian of the modern and postmodern subjectivity Claudia Stein, have dedicated time to think and research what it means to use images in history-writing.

In their collection of essays *Writing History in the Age of Biomedicine*, we find reprints of several essays, two of which address these questions head-on, and which have helped me in coming to grips with the role of the visual in my own work. In 'Coming into Focus. Posters, Power, and Visual Culture in the History of Medicine,' Cooter and Stein deliver their account of a historiography of the visual in the discipline of history—what they term in their subsequent essay as 'politics of aesthetics'. They address a specific kind of visual: visual posters, giving a particular attention to showing how historians of medicine have treated (and *used*) health posters about sexually transmitted

[9] This kind of argumentation is taken from Roger Cooter and Claudia Stein, 'Coming into Focus. Posters, Power, and Visual Culture in the History of Medicine', in *Writing History in the Age of Biomedicine*, esp. 118-120.

diseases. They situate the politics of seeing in the visual turn in history-writing in the 1980s and 90s, and argue that it wasn't because there wasn't a larger production of visual objects during these decades that historians turned to them, but due to a contemporary wider (and if you will, institutionally external to academic history) "intellectual climate" preoccupied with the meaning and uses of visuality and aesthetics. Cooter and Stein allude to two cultural strands forming this intellectual pot that has permeated academic historians, roughly falling into either Marxist or Foucauldian interpretations of history.

Of the first one, they pick Susan Sontag as scapegoat and intellectual mastermind of what they think to be an overly realistic interpretation of visual objects. Historians of public health such as Allan Brandt, they write, have been blindly following the interpretative path of Sontag. For both authors, they argue, "posters are simply footnotes to bolster a particular historical narrative, whether specific or general. Whereas for Brandt this is the narrative of public health, for Sontag it is the history of Western industrial capitalism."[10] This is what I alluded to when I claimed that Fortun was *using* visual objects. Cooter and Stein's reading of how historians treat the act of seeing as unproblematic is certainly valuable. They characterise Sontag as wishing to see "behind posters, as behind metaphors of disease", beyond the "clouds of propaganda and illusion."[11] This way of seeing as reading against the grain is a position that relies on the assumption that there is always something 'real' behind the curtain, something hidden by a propaganda machine. The problematic with this approach is the seemingly endless process of unveiling truth, an aggressive promethean attitude of revealing what's hidden.[12] Beyond the violence of what could be called 'reading against', the larger problematic, however, is the never-ending questioning whether all that we see is simply the manifest content of our visual dreams. I certainly agree with Cooter and Stein's critical positioning against such ways of seeing, but I have my own concerns about Sontag being singled-out as its intellectual matriarch. More on this later. Let us return to the second strand in the "intellectual climate" of the late twentieth century that, according to Cooter and Stein, proved important for historians to be able to see.

Cooter and Stein argue that due to the suspicions of the Marxists interpretation of history, a new way of looking surfaced, based on Michel Foucault's work on what historical 'discourses' are made of and what they entail. The historians don't hide as

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[10] Cooter and Stein, 120.

[11] Ibid, 120.

[12] I use the word 'promethean' as Pierre Hadot uses to describe this ancient epistemic approach to truth, based on a violent manipulation of matter performed in order to see the mechanicity of the world by which Nature hides its tricks, and opposed to the 'Orphic' attitude, a more contemplative and harmonious approach to the secrets of Nature. Pierre Hadot, *The Veil of Isis: An Essay on the History of the Idea of Nature*, trans. Michael Chase (Cambridge, MA: Harvard University Press, 2008), 91–101.

laying closer to Foucault than to Marx, and are sympathetic for treating the visual world "as any other representation, functioning as so-called 'truth regimes' that produce and sustain systems of power."[13] Their final recommendation is vibrantly in tune with a Foucauldian understanding of representation, where visual images function, just as texts and institutions, as 'regimes of truth':

> Historians seeking to use visual materials need to be aware that any instruction as to their use is a priori discourse laden. The coming into focus of health poster in the 1990s and visual culture in general are something of seeming great importance, something for serious critical engagement, is but a perception of one particular socio-cultural moment [...]. It is a discursive regime, not a universal truth. Historians of medicine should by all means be encouraged to pursue their abandoned visual objects in their field, and treat such objects in terms appropriate to the context of their production. [...] But they should do so with awareness of, and open candor towards, the discourses around the visual from which their approaches derived. [...] In other words, the discourses around the visual need to be seen as just that, and not as objective tools for historical analysis that can be grasped uncritically.[14]

But what does a critical appraisal of visuality entail? How should we *actually* interpret images? Sontag herself had clear ideas about how to go about this. The characterisation of her that Cooter and Stein promote is one which paints her as a realist about the meaning of images, supposedly hiding behind metaphors and symbolism. However, I believe their reading of Sontag is partial, if not outright deceptive. I am of course referring to the omission of the authors of her essay 'Against Interpretation', which nowadays has become a piece of mainstream art criticism, as a cult movie that acts as the rite of passage for the nostalgics of the Cold War American counterculture.[15] Her work, in general, and 'Against Interpretation' in particular, has become the equivalent of a 'celebrity image', as she would have it.

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[13] Cooter and Stein, 'Coming into Focus. Posters, Power, and Visual Culture in the History of Medicine', 134.

[14] Ibid, 137.

[15] Some examples of Sontag as cultural phenomenon are Lauren Elkin, 'Susan Sontag Was a Monster, of the Very Best Kind', *Aeon*, 2019, https://aeon.co/essays/susan-sontag-was-a-monster-of-the-very-best-kind; and Anthony Oliver Scott, 'How Susan Sontag Taught Me to Think', *The New York Times*, 8 October 2019, https://www.nytimes.com/interactive/2019/10/08/magazine/susan-sontag.html; Certainly the most representative example of how countercoulture has been engulfed in the mainstream is the 832-page 2020 Pulitzer Prize winner biography of Sontag, Benjamin Moser, *Sontag: Her Life and Work* (Ecco, 2019).

One can read 'Against Interpretation' as a plea for *certain* kind of interpretation, one at odds with Cooter and Stein's characterisation of Sontag. I choose this kind of reading of the essay. Presented as a *negative* plea, the 'against' in 'Against Interpretation' presents the 'interpretation' in certain historical tradition about seeing. She rejects what she calls the "mimetic theory of art", which she attaches to Plato's conception of art as both useless and untruthful representation of reality. The idea itself of art as representation is what Sontag puts into question, one which, in her view, encourages an attitude towards art based on interpreting the 'meaning' behind the piece, and 'meaning' understood as the intention of the artist's inner world (subjectivism) or a representation of something out in the world (objectivism). Both attitudes are indeed realists approaches to art pieces, and Sontag does well in marking the historical dimension of interpretation in hermeneutic practices of biblical texts.

To reject interpretation is to reject the practice of finding 'meaning' as an ahistorical and unmediated ascription of 'meaning' to the *content* of art. Sontag rejects readings of art such as "What X is saying is …," or "What X is trying to say is …."[16] These phrases, to Sontag, encourage a decontextualised understanding of art. And what does it mean to bring the context in? What does it mean to abandon the project of finding hidden mysterious content (to see behind the "clouds of propaganda and illusion," as paradoxically Cooter and Stein would say)? Sontag writes, "our task is not to find the maximum amount of content in a work of art, much less to squeeze more content out of the work than is already there. Our task is to cut back content so that we can see the thing at all."[17] The thing itself, she says, is the object itself and the function it plays in its contexts. To attend to the thing is to switch our attention outwards, towards the *form*. Moving outwards and taking 'form' as something beyond spatiality,[18] we find the contexts that are needed "to see the thing at all" – its golden or metal frames, its neighbouring piece in the museum or an overlapping memo in an office notice board, its unique print or the multitude of reproductions – if we move outwards, we find the place where art has always been: the frame of history.

But calling for the need of historical context to understand images is of little use to historians. The initial question, after all, was not how or whether history is needed to understand images, but what is the use of images for writing history. It seems as though we've gone full circle: from Cooter to Sontag and from Sontag to Cooter. But not entirely so. As we seem to have arrived to where we started, I would like to recall the task at hand in this chapter, and if the reader will allow my indulgence, to point to a few more

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[16] Susan Sontag, 'Against Interpretation', in *Against Interpretation and Other Essays*, Penguin Classics (London: Penguin Books, 2009), 4.

[17] Ibid, 14.

[18] The discussion of 'form' beyond spatiality appears in Susan Sontag, 'On Style', in *Against Interpretation and Other Essays*, 15–36.

passages of these authors that that further specify what we mean by 'form' of images, and where we are to find it. The task was, in the first place, to make history-writing more transparent in its methodology – a transparency which is needed if we are to self-reflect upon our epistemic virtues and be attentive to our historiographical positioning. 'Form', in all its broadness and heterogeneity, I would argue, is what is needed to do just that.

Beyond the visual turn of the 1980s and 90s, Cooter and Stein write, at the verge of the twenty-first century, historians met yet another element to probe into – spatiality. Also in their *Writing History* essay collection, the chapter 'Visual Objects and Universal Meanings. AIDS Posters, "Globalization," and History' combines their previous attention to the visual turn, which tried to answer the question of 'what do images mean?', to a seemingly new 'spatial turn', which was promised as framework which would bring the attention to how 'positioning' also convenes meaning: their "arrangement", "rearrangement of objects" as embedded in political, ideological, and social histories.[19]

Cooter and Stein's essay addressed 'Against AIDS: Posters from Around the Word,' a "globally themed exhibition" held in a museum in Hamburg in 2006. Their interest was on the 'afterlife' of the posters beyond their initial urban environment, where they were first objects used to promote sexual ethical behaviour. What did they mean, what function did they play in their new context? They ask, how were they transformed in this new exhibition, what new political dimensions did they regain, and which ones had been unreflectively dragged? I won't delve onto their conclusions, but rather look at their methodology and intent, which were a source of inspiration for my approach in this thesis.

Museums, like archives or any other "depositories for images and artifacts," the historians argue, "have particular collecting agendas and particular institutional and intellectual traditions into which new acquisitions are fitted."[20] Their aim in this particular essay was to recover the intellectual tradition that the Hamburg museum was unknowingly embracing and the political position it promoted. To do that, they observed and examined several elements: the disposition of the elements of the exhibition, the chosen title, and the explicit and implicit ethical goals of the exhibition: namely to "incite onlooker to ethical behaviour (safer sex)."[21] As well, they attend to the heterogenous historical contexts in which these posters were first produced, circulated, and consumed; and the political meaning they conveyed now that they were homogenized into the boundaries of a localised exhibition which pretended to sketch an image of a unique 'global' sensitivity to AIDS.

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[19] Roger Cooter and Claudia Stein, 'Visual Objects and Universal Meanings. AIDS Posters, "Globalization," and History', in *Writing History in the Age of Biomedicine*, 138–59.

[20] Ibid, 142.

[21] Ibid, 143.

Beyond the theme of their analysis, what I find remarkable, and I hope the reader has realised, Cooter and Stein focus their attention on titles, arrangement, and historical contexts. These are elements I have also tried to pay tribute in this thesis: titles of advertisements and their arrangement within the issue in the first chapter, and their argument in two historical contexts in Chapters Two and Three.

Beyond titles and historical contexts for production, display, and consumption, what else may be looked at to describe the 'form' of a visual object? Sontag's 'On Style' may have some clues about this. Granted, her piece was meant as a reflection of and normative text in the field of literary criticism, but just as well applies to any 'art' object. In it, we find a description of art, which as I said at the beginning, allows for a broad conception that includes scientific advertisements in its cradle.

In line with her previous attack on readings of art which encourage finding hidden 'meanings', she displays a definition on art based on its sensory materiality (something to be perceived) and its function within a particular historical context. "Art is not only about something; it is something. A work of art is a thing *in* the world, not just a text or commentary *on* the world."[22] In this formulation, we conceive of art not as a piece of propositional knowledge (what art says about something, or what it 'means'), but as part of a given procedural knowledge (what art does, what function it performs).

To move beyond meanings entails, to Sontag, to attend to the object's form – the 'style' as she calls it here. The 'style' is conceptualised as a central dimension of art, not merely understood as a 'decorative' or 'superfluous' aesthetic embellishment, but as the manifestation of both the historical situatedness of the production and consumption of the art object, and the sensory dimension of art as an experience. Art, she writes, has a dual nature, "as object and as function, as artifice and as living from of consciousness, as the overcoming or supplementing of reality and as the making explicit of forms of encountering reality, as autonomous individual creation and as dependent historical phenomenon."[23] Literary critics, in her view, should attend to this dual nature of art: as historically situated objects that perform certain function, and as material objects that are perceived—both belonging to her quest to attend to the procedural dimension of art. This dual nature is accessible to the critic (and by historians wishing to use material objects in their investigations), by the central and mediating role that the 'style' (form) plays.

This functionalist view of art allows for putting to the side much of the talk about the 'meaning' of the content of art. And as such, provides the literary critic and historian a very concrete tool to look at the historical and material situatedness of art objects, now conceived beyond that which is exposed in museums, and broadening to encompass that which has certain 'style' and conveys certain experience. One of those

[22] Sontag, 'On Style', 21.

[23] Ibid, 31.

functions of art is, according to Sontag, to imprint a memory in individual and social consciousness. As such, what literary critics (and us as historians) can look at is the mnemonic characteristics of the art object which permit such imprinting. These mnemonic value of the 'style' of an object can be found if one attends to its repetitive patterns, what she calls the "rhythm":

> One function of style is identical with, because it is simply a more individual specification of, that important function of form pointed out by Coleridge and Valéry: to preserve the works of the mind against oblivion. This function is easily demonstrated in the rhythmical, sometimes rhyming, character of all primitive, oral literatures. Rhythm and rhyme, and the more complex formal resources of poetry such as meter, symmetry of figures, antitheses, are the means that words afford for creating a memory of themselves before material signs (writing) are invented; hence everything that an archaic culture wishes to commit to memory is put in poetic form. […] Thus, form—in its specific idiom, style—is a plan of sensory imprinting, the vehicle for the transaction between immediate sensuous impression and memory (be it individual or cultural). This mnemonic function explains why every style depends on, and can be analyzed in terms of, some principle of repetition of redundancy.[24]

The rejection of 'high' and 'low' art that such broad definition of art entails is not uniquely a plea to do away with elitist conceptions of what's worthy of the category of 'art'. This may have been (one of) Sontag's plea(s), but I take it here with a pinch of salt and with a very opportunistic attitude: to be able to use her framework of literary criticism and her invitation to look at rhythm and repetition, as a sketch of my own framework. Certainly, looking at advertisements as (1) historically situated objects, (2) with a sensorial quality, (3) and with certain 'style' to be analysed in terms of that which repeats; are the three characteristics of why, in all its banality, allows me to call scientific advertisements art. "It remains to be said," Sontag finishes her essay, "that style is a notion that applies to any experience (whenever we talk about its form or qualities). And just as many works of art which have a potent claim on our interest are impure or mixed with respect to the standard I have been proposing, so many items of our experience which could not be classed as works of art possess some of the qualities of art objects."[25]

Now, not all must be easy. And not all of these three dimensions (historical, sensorial, stylistic) are always accessible. To further narrow down my analytic toolbox, I attend once again at the essay by Cooter and Stein about the AIDS exhibition in a Hamburg museum. They are clear on their aims of their essay, and by doing so, point to

[24] Ibid, 34.

[25] Ibid, 36.

some of the difficulties that one may encounter when looking at objects, and some of the questions one cannot expect to find answers to when looking at visual objects

> we are not concerned here with how viewers might have responded to the images or to the exhibitions as a whole (an almost impossible tasks in any case given the uniqueness of individual psychology and experience). Nor are we interested in proving a walk-through critique of the exhibitions. Our main interest is in the historical context of the Museum and how this bears on the politics of aesthetics implicated in its exhibition.[26]

Taking Cooter and Stein's approach, investigating the commercial effects or reception of the scientific advertisements was not my task here. Only marginally I addressed this topic when I talked about the dual nature of adverts as pieces of marketing but also as pieces that can be cut off from the magazine and used to place orders. However, I found no faithful ways of knowing whether scientists in fact used them as such. Strictly speaking, my thesis has not dealt with the success of the adverts in reaching and convincing people, nor on the alleged use of advertisements within the commercial relation between scientists and manufacturers. This would be an interesting (but arduous) line of investigation. In line also with Cooter and Stein, I did not set myself out to critique the advertisements themselves nor how *Nature* chose to arrange their display. This would certainly be a very revealing (but again, nearly impossible) research project, as knowing which advertisements were *not* published (if that was ever so) would give us some insights on the editorial, commercial, or political commitments of the journal.[27]

By contrast, I sympathise strongly with Cooter and Stein's interest for studying instead the "historical context of the Museum and how this bears on the politics of aesthetics."[28] Their 'politics of aesthetics' is, simply put, the idea that aesthetic considerations such as the arrangement of objects and their framing have inescapable political meanings which are historically situated. While it is very true that I don't deal in this thesis with 'politics of aesthetics' per se, I could say that I am certainly interested with the 'politics of epistemics'. In this thesis I have aimed to characterise the epistemic negotiations as they appear in advertisements (Chapter One), and further transplanting the concept of epistemic virtues into the social context (social placement as epistemic

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[26] Cooter and Stein, 'Visual Objects and Universal Meanings. AIDS Posters, "Globalization," and History', 143.

[27] Sanders, 'Email Conversation with Alysoun Sanders, Archivist for Macmillan Publishers and Springer Nature'.

[28] Cooter and Stein, 'Visual Objects and Universal Meanings. AIDS Posters, "Globalization," and History', 143.

virtue, Chapter Two), followed by the ethical dimension of these virtuous ways of seeing that the molecular technologies prescribed (Chapter Three).

Here, 'the text' are the negotiations of the scientific identity as appeared in the editorial pieces, what may be called 'form', 'style', or 'historical context'. By attending not only the rhythmic elements in the advertisements themselves, but the repetitions also in the text as their most immediate framing, I characterise one of the forms of the advertisements to be the *historical context that allowed for the advertisements to be printed and read in a particular way*. Put bluntly, the advertisements do not illustrate or 'represent' the epistemic virtues of 1970s science per se.

What I tried to characterise was the epistemic virtues of the 1970s. My problem was: where to find them? Which sort of historical sources contain these analytic objects? Are they in people, objects, images, texts, or language? At the beginning of my research, I observed that the images of advertisements seemed to allude to some sort of relation between ways of knowing and ways of being a knower, but the specific way in which they related was still evasive. I did not want to fall into the trap of representation, nor writing things such as 'this element of the advert represents this or that,' because this is the sort of thing that Sontag says is 'squeezing' "more content out of the work than is already there."[29] This is much too realist and hermeneutic than what I wish to be. What I tried to argue in this chapter is that the epistemic virtues are not represented in the ads, but that within the adverts *there are* allusions to how the devices and objects are to be used and valued, and how researchers should use them within the context of the laboratory. Above all, what I want to say is that in order to understand advertisements *as* references with certain epistemic and moral acknowledgement -to understand them as such-, can only be done in the context of the magazine. It *is* the journal and the discussion of the texts that which produces the necessary context for these images to be understood as epistemic virtues.

The 'forms' of the advertisements that repeats over and over are the words that nest the images and the golden frame of the magazine. The repeating rhythm is *Nature*. Just as museums have particular intellectual and political structures that guide the display of their pieces and the prearranged meanings by which they are to be understood, the printed journal provides a context in which scientific devices can be seen as the material goods which enable the scientist to reach the epistemic virtues as negotiated in the texts. It is this contextualized display which makes scientific advertisements intelligible, it is the aesthetic sensibility of their politics which makes them art.

In between extracting too much meaning from an image and subordinating them to their form (the text), we must find a middle ground that allows for meanings to appear in our historical narrative, giving justice to the negotiations behind those meanings. In

[29] Sontag, 'Against Interpretation', 14.

my chase, the physical presence of advertisement in the magazine is the product of the epistemic virtues as negotiated in the editorial pieces. This is not to say, however, that the advertisements are not part of the discourse of what being a ‘good’ knower entails; but rather, that the relation between the image and the epistemic virtue is local and negotiable. The texts, which negotiate constantly in a flux what a virtuous scientist is, provide the frame for advertisements to become pieces of the imagined virtuous scientist. The images by themselves lack the meaning to be understood, and in my case, it is the editorials that provide the frame by which the images of the advertisements can be given local meanings— one of them being epistemic virtues. But in all these local textual negotiations of what counted as virtuous science, run profoundly the assumption that Western science is a virtuous activity on itself. Let us look at the virtuousness of 1970s more closely now. Who was the good molecular scientist?

Closing. **When science buys and sells, ethics arise.** Money, virtue, and the identity of the virtuous molecular scientists.

In this thesis I have studied *Nature* as the marketplace of the scientific community, as a site for both conversation and window shopping. My approach to the sources has been quite simple. I tried to recreate the 'browsing' through the forum, being drawn to the sources which alluded to the condition of being a molecular scientist. In my reenactment, an advert for a pyrgeometer to measure solar radiation or a lead editorial on the controversy of a hydrological engineering project at the Nile may have been only of partial interest to me. I sought antibodies and the employment prospects of geneticists in the new biotech industry. Or perhaps this infuriated me, perhaps I even wrote a letter to *Nature* to defend science of commercial impurities.

Be 'I' what may, *Nature* was a quickly rotary printing press. Their weekly delivery ensured radical changes in the topic of the lead editorials. Science flew and the social issues that mattered did too. Some themes were recurrent, of course, like employment in academia. The journal was characterised for its broad, dynamic, and heterogenous content. The journalistic character and internationalist outlook were promoted strongly during the 1970s. John Maddox (editor in chief 1966-1973), who had been science correspondent in a generalist newspaper before, pushed to make *Nature* more in tune with the rhythms of a weekly tabloid.[1] David Davies (editor in chief 1973-1980) gave their recently-opened Washington office more independence, and right from his first lead editorial, he promised more diversity in the reporting of news, "*Nature*'s new gathering facilities around the world must grow in the next year or two, as it is increasingly necessary to understand the scientific scene away from the trans-Atlantic axis."[2]

With this quick overturning of topics, and with the broad themes discussed by editors and contributors, it can hardly be a surprise that the topics I investigated here are heterogenous. Presenting the three empirical chapters discussed in this thesis in a unified conclusion would be deceiving, and a misrepresentation of the diversity of topics in *Nature*. Seeing a causal or even thematic connection *at the level of the sources, the actors involved, or their immediate interpretation* between science as consumption (Chapter One), science as placed in a political world (Chapter Two), and research ethics as epiphenomena (Chapter Three), would be a challenge with no happy ending. A task which I am not willing to afford.

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[1] Baldwin, *Making 'Nature'*, 175.

[2] David Davies, 'Nature in the Future', *Nature* 244, no. 5417 (24 August 1973): 475–475, https://doi.org/10.1038/244475a0 as cited in Baldwin, *Making 'Nature': The History of a Scientific Journal*, 187.

What *can* be said, however, is that the way by which these three chapters interact, and of the themes which I considered, is by alluding to a common meta-discussion. This meta-discussion is one centred on an ethical question: what did it mean to be a virtuous knower? And transposing this question to the context of the self-examination of a collective identity hinging on a shared epistemic object (the molecules): what was it like to be a good molecular scientist? This closing chapter should serve as a space to reflect about what my answer to this question says about science, about history, and about what other people have written.

Being a good molecular scientist during the 1970s meant working with an international heterogenous community of fellow experts, a community which included manufacturers of objects who made your day-to-day manipulations easy, fast, cheap, automatised, and most importantly, *standardised*; being a good molecular knower meant becoming political by demanding more funding and more institutional insulation; being a good molecular investigator meant prioritising observation and recording, attending to your objects and methods and translating your ethical worries and social contributions by trusting the process of science as a communal effort; being a good molecular scientist meant doing relevant research by relying on the work of the community, seeing moral goods as a by-product of a long systematic process interchanging knowledge units between ‘basic science’ and ‘applied science’. Being a good molecular knower meant assuming no individual responsibility.

Most molecular scientists of the 1970s would have disagreed with this caricature. And rightly so. Not only because there were many nuances and alternative propositions (like those of the BSSRS, ASTMS, or Rothschild), but because this is not the characteristics of specific person, but that of an amorphous social body (probably a man, though). These are the values of a community which throughout this thesis, have constructed the image of the *idealised* molecular scientist. As a description of the collective, it is hardly a surprise that they won’t match with one individual person.

Beyond this epistemic impossibility of going from men to man, can I say something about the worth of this thesis? How does it stand in the current literature? Posing the question of worth as a matter of how it fits with the work of others and demanding originality from this relation is a formulation of the construction of value in terms of pursuing the modern ideal of ‘novelty’—developed during the nineteenth century as a symbol of modernity[3] and institutionalised precisely during the Cold War as a product of the political pressure that demanded relevance (in terms of economic viability) from researchers. This demand was answered by the scientific community with the production of written texts (journal articles) to be quantified and assessed according to standardized procedures.

[3] Csiszar, ‘Meeting in Public’, esp. 114-116.

The need to obtain worth of *one*'s work, formulated as a demand for relevance within the product of a *collective* either within academia or beyond its fence has different names – the "public legitimacy of research," Csiszar put it; the "cultural purpose and legitimacy of both science and scientist," Wang called it; "social responsibility of science," as my 1970s actors would have said.[4] I have simply referred to this ethical worry of both scientists and historians as the *social placement of science*. But alas, despite the historical contingency of these demands, one has to justify oneself. Beyond seeking for novelty for its own sake, there are three reasons that make my thesis worthwhile.

First, I would like to emphasize the image created here: the one of scientists as a social group which consumes material goods. In this closing chapter I want to explore the notion of science as a practice of consumption in the light of the historiography of popular science and science popularisation, and what I think my contribution to this field could look like. With these ideas in mind, I would like to reflect on the recent historiographical interest for the economic dimension of science, namely, on the funding of academic research. A seeming 'economic turn' in the history of science, with a particular focus on the economical transfigurations of Western science in post-Second World War and Cold War scenarios, seems a rich unifying approach to the history of science which would explain some of the worries and apparent problems of present-day academia.

The relationship between academia and money brings me to the second element that I think makes my thesis worth it. The notion of science as increasingly organised, bureaucratised, and industrialised profession seems to have played a vital role in the imaginary of scientists themselves, leading to critiques from various intellectual quarters. Trying to understand what happened during the Cold War was a task of sociologists of science then, and of historians of science today. I explore the different positions to a seeming new situation of academia, with an especial attention to the conceptualisations of the place of virtue in late modern science. Against Steven Shapin, I argue for a much more intitmate relationship between morality and epistemics, one much more in tune with Lorraine Daston, Peter Galison, and Michel Foucault, which understands ways of knowing and virtues of being as two sides of the same coin.

My understanding of ethics, and especially, of how scientists conceptualise ethics, leads me to the third point of relevance. I would like to show that ethics were understood as epiphenomena by most of my historical actors in this thesis because of the way scientific funding was structured, which allowed for a conceptualisation of one's work as inherently (though not in a direct and individual way) morally good. This has two consequences: first that in order to make normative claims about research ethics,

[4] Alex Csiszar, 'Introduction. "Broken Pieces of Fact"', in *The Scientific Journal. Authorship and the Politics of Knowledge in the Nineteenth Century*, 4; Wang, '"Broken Symmetry": Physics, Aesthetics, and Moral Virtue in Nuclear Age America'.

we must bring in an understanding of the funding structures and the patronage relationships of science, and second, that the system that was inserted in the period under my study promoted a displacement of individual ethical responsibility.

Science as consumption

In *The Scientific Journal*, Csiszar elegantly recognises the position that academics adopted as objects of display with the introduction of journalists in their meetings as a symbol of 'democracy' and 'transparency' in post-revolutionary France.[5] He has observed the epistemic and political "consequences" of such surveilled position, and the reception by the academics themselves in this new social role. The *Comptes rendus hebdomadaires*, launched in 1835 by the French Academy of Sciences would publish not only the reports submitted by its members, but also reports of the meetings at the Academy. Written by scientific journalists who would attend and record the discussions, the social construction of facts was opened to the public. Such transparency situated scientists as the object of the public gaze: some welcomed it as a sign of accountability, and some disdained being watched and being pressured into delivering eloquent speeches. I believe Csiszar's explanation of this new place of the academics is of critical importance for the historiography of science popularisation, and the conceptualisation of the scientist as a social group which *receives*, instead of the default authorised *producer* of facts. Historiographically, this flips the image of the scientist 180 degrees, from an image as constructor, to a renewed image of the modern scientists also as an object to be consumed, looked at, written about, documented, examined. In my thesis I have taken this inversion to heart. Like Csiszar, I situated the scientist as a displayed object expected to produce goods of national importance in Chapter Two, and unlike him, as consumer of devices and research objects as prerequisite to his own production in Chapter One.

The interest on conceiving the relationship between scientists and 'the public' beyond a dichotomous active/passive, producer/consumer has its roots in the historiography of science popularisation, and a subset of this tradition which investigates the commodities of and around science. In order to understand the relevance of Csiszar's work and that which I presented in this thesis, let me unpack some of the basic notions of the historical work on science popularisation and commodification.

The literature on the science popularisation has a strong tradition in scholars investigating nineteenth-century Britain; and can be traced to social historians of the 1980s and cultural historians of the 1990s, interested in describing the commercial

[5] Csiszar, 'Meeting in Public'.

nature of knowledge – the production, dissemination, reception of its products, including the study of scientific instruments, museums, books, and periodicals. One of the main conceptual contributions of this kind of literature is the notion that science and scientists are embedded in social structures aimed at generating economic profit in different manners, and that these structures were constructed during the nineteenth century and hinged on the phenomenon of the professionalisation of science. Regarding the dichotomy science/public, this sort of historical literature made evident that the cultural meaning of science was malleable, and that the so-called public repackaged and appropriated for its benefit the products of elite academic investigation.

The studies on the commodification of knowledge came to be a strong case supporting the historical contingency of concepts such as 'scientist' and 'lay public'. X2Historians have argued that the professionalisation of natural philosophers during the nineteenth century into a new social category prompted its new practitioners to debate over the "scope and status" of the science and of how expertise and 'the public' related.[6] British historian of science Iwan Morus, for instance, advanced in 1996 that the distinctions between 'expert' and 'lay', or between 'scientists' and 'the public' are themselves products of these Victorian debates:

> The relationship between the popular and the professional has also been viewed as hierarchical: knowledge was produced by the elite and then diffused through the more popular media. A similar distinction has been made between science and technology. Science made knowledge. Technology was a simple matter of application. These distinctions and qualifications are of course the inevitable results of the debates raging [sic] concerning the meaning of science during this period. Boundaries between the popular and the elite, the practical and the pure are always contingent and continually renegotiable. It is an implicit assumption of this paper that such categorizations are emergent properties.[7]

What Morus calls the 'diffusion' of knowledge from either science to the public or from science to technology is rightly called the 'diffusionist model'.[8] Continuing on a critique

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[6] Iwan Rhys Morus, 'Manufacturing Nature: Science, Technology and Victorian Consumer Culture', *The British Journal for the History of Science* 29, no. 4 (1996): 403–34.

[7] Ibid.

[8] The 'diffusionist model of science popularisation' is the explanatory model which poses the production and dissemination of scientific knowledge as two separate processes. This model introduces a binary opposition in which the production is an active process enabled by the rational scientific method, and is deemed as an epistemic right of experts. On the other side, the understanding of scientific concepts by 'the public' is done passively and through simplified versions of the genuine scientific truths. The simplification is mediated by professional scientists or adequate middle-men (science communicators, educators, science journalists). Two seminal works that critique

of this model to explain the relationship of non-experts to scientific products, a seminal work is the 2007 book *Science in the Marketplace: Nineteenth-Century Sites and Experiences*. Editors Aileen Fyfe and Bernard Lightman and the contributing authors set themselves to give justice to 'the public' as more than passive consumers, asserting that that 'the people' "are fickle and selective and they appropriate and adapt what they choose to their own purposes."[9] In creating the notion of 'the marketplace' where objects and experiences are exchanged, they opened a variety of sites for the study of consumption and adoption of scientific products beyond books and periodicals, and including "country houses, museums, galleries of practical science, panoramic shows, exhibitions, lecture halls, and domestic conversations."[10]

From this interest in the social and cultural aspects of science, historians have more recently turned their attention towards the processes of commodification of its material components, such as the production, commercialisation, display, and uses of scientific instruments. A diverse study of the possibilities of this field is the 2017 edited book *How Scientific Instruments Have Changed Hands*, which gathered the recent efforts in "map[ping] out how instruments were made and by whom; who were makers and manufacturers, who were retailers and suppliers, and how their products reached their markets."[11] Of particular interest for my case on scientific advertisements is Joshua Nall and Liba Taub's chapter 'Selling by the Book: British Scientific Trade Literature after 1800.'[12] In their broad survey, reaching until the third decade of the twentieth century, they illustrate how manufacturing firms and men of science interacted with one another in a "symbiotic relationship of collaboration and promotion."[13] In the case of Britain, the authors argue, this close relationship became actively tightened at the turn of the twentieth century, with a noticeable change of tone: "trade and sales literature provide

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this model are those of Bernadette Bensaude-Vincent and Stephen Hilgarner, whose historical and sociological critiques, respectively, have been influential in the field of history of science popularisation and popular science. Bernadette Bensaude-Vincent, 'A Genealogy of the Increasing Gap between Science and the Public':, *Public Understanding of Science*, 31 March 2017, https://doi.org/10.3109/a036858; Stephen Hilgartner, 'The Dominant View of Popularization: Conceptual Problems, Political Uses':, *Social Studies of Science*, 29 June 2016, https://doi.org/10.1177/030631290020003006.

[9] Aileen Fyfe and Bernard V. Lightman, eds., *Science in the Marketplace: Nineteenth-Century Sites and Experiences* ('Nineteenth-Century Popular Science - Sites and Experiences' Conference, Chicago: Univ. of Chicago Press, 2007), 4.

[10] Ibid, 4.

[11] Alison D. Morrison-Low, Sara J. Schechner, and Paolo Brenni, eds., *How Scientific Instruments Have Changed Hands*, *How Scientific Instruments Have Changed Hands*, vol. 5, Scientific Instruments and Collections 56 (Leiden: Brill, 2016), https://brill.com/view/title/33653.

[12] Joshua Nall and Liba Taub, 'Selling by the Book: British Scientific Trade Literature after 1800', in *How Scientific Instruments Have Changed Hands*, 21–42.

[13] Ibid, 35.

compelling evidence for rampant and aggressive commercialisation of the material culture of the era's cutting-edge research work."[14]

Nall and Taub's history of the long-nineteenth century invites scholar to recognise the importance of trade literature such as catalogues and advertisements as "more than just business history." Part of the work in this thesis shows the value of this approach. In Chapter One, I showed the use of and relations of 1970s trade literature with *Nature*, and parallelly, the relationship between manufacturers and scientists. Many advertisements, the reader may recall, weren't of research instruments themselves, but announced free catalogues listing not only products, but also prices, services, and the technical details required for appropriate know-how of the instruments. The catalogue, as a hybrid object between research and marketing, illustrates how commercial manufactures negotiated their position as partners in the research efforts.

My attention to trade literature seems to correspond with the interest of my historical peers. A recently published study on Spanish trade catalogues for scientific instruments shows the value of this kind of historical source. Víctor Guijarro Mora trace some of the "linguistic and rhetorical patterns" of catalogues of instruments used for educational purposes within the early twentieth century in Spain. He pays attention to the ubiquitous association of instruments with the words 'new', 'modern', and 'novel'.[15] He recognises, like me, the use of the "*ad novitatem* argument (that which is new is superior by virtue of being new)" as a commercial tactic.[16] Interestingly, he argues, the use of these 'novelty' concepts rendered two meanings of 'modern': "*modern* understood as current, as opposed to that which is past and the old-fashioned, and *modern* understood as new."[17] Of this second meaning of modern, which is akin to what I have gathered in the *Nature* ads during my first chapter, Víctor Guijarro Mora writes, "the second meaning made sense in a world in which intense scientific activity was taking place and in which results and discoveries were constantly being made, which had to be wisely managed," and he continues "the manufacturer thus showed himself to be an attentive person in contact with scientific production, and consequently the public had no reason to doubt his trustworthiness."[18] The function of 'the new' is very different in his analysis as compared to mine. In his case, 'new' was one of the ways of associating the instruments with the ongoing scientific developments. In my case, may the reader remember from Chapter One, the word 'new' was a ubiquitous word in the

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[14] Ibid, 39.

[15] Víctor Guijarro Mora, 'Retórica y persuasión en los catálogos comerciales españoles de material científico educativo (1920-1936)', *Llull, Revista de la Sociedad Española de Historia de las Ciencias y de las Técnicas* 43, no. 87 (12 October 2020): 181–200.

[16] Guijarro Mora. My translation.

[17] Guijarro Mora. My translation.

[18] Guijarro Mora. My translation.

advertisements for research instruments, that went beyond a marketing tool, and its meaning hinged on the epistemic virtues of standardisation and ease of manipulation of the devices, all in the context of heavy competition between manufacturers and researchers themselves. We saw how novelty was a way for manufacturers to frame their devices as competitive advantages for scientists themselves. While this differences in meaning between Guijarro Mora's study and mine are to be expected, as we are dealing with two geographically, culturally, linguistically, and temporally distinct historical cases, Guijarro Mora's work is a case of a larger theme in the historiography of commodification of knowledge, and as the way by which my study here departs from the majority of these studies. While Guijarro Mora talks about instruments being produced for educational purposes, I addressed advertisements targeting researchers themselves. As such, the largest difference playing a role here is the fact that Guijarro Mora's study, as is the majority of studies within the historiography of science popularization and commercialization, deals primarily with how knowledge products were made, displayed, and used *for educational or recreational purposes*, whilst intentionally leaving in a second plane and as a sort of static background how knowledge products were made, displayed, and used *for research purposes.*

The focus that historical literature on the commodification of knowledge gives to how scientific products were bought, consumed, and advertised for recreational and educational purposes, whilst forgetting how scientific objects were bought, consumed, and advertised for research purposes still falls flat on its inherent attempt of illuminating how science and the public interact without falling prey of "dominant view of popularization."[19] The majority of literature on the so-called history of popular science or science popularisation still depends on the division that that the active production of knowledge and technology lies on the side of the technoscientists, while the passive consumption of those marketed goods is the function of 'the public'. While most historians of this sort would actually argue against this division, the fact is that they have not taken the historical attention to processes within the practices of experimentation themselves, and as a consequence, leave the historiographical literature on the social embeddings of science limping from a missing leg.

This is perhaps a daring comment, but I say so because, for the most part in this literature, science is seen as the producer of the goods, and for the most part is seen as the active producer. While the attempts at showing how the public can be active (by repackaging, reappropriating, decentralizing, or refunctionalizing technologies) are welcome, something still missing. And that is, portraying technoscientists not only as de-facto producers, but as consumers themselves. By shifting the attention to scientists as consumers, I hoped to have given insights into how science is not only a practice that produces, but that consumes. By bringing attention here to how scientists are embedded

[19] Hilgartner, 'The Dominant View of Popularization'.

in an economic model, in Chapter One as *consumers* of objects necessary for their practice, and Chapter Two not simple as *producers*, but individually as cogs in a *communal* effort of science as a productive force of national magnitude.

In line with Csiszar's observation in *The Scientific Journal* that scientists of the modern era were also on the other side of observation and scrutiny, I hope to have added to a more fair and equilibrated vision of the science-public relationship.

Money and virtue in the history of science

The commercial dimensions of science were initially appreciated by historians of science popularisation, where they became productive explanatory elements. Now, past the social, cultural, and material turns that have enriched views onto the relationship between science and 'the public', another seeming 'turn' with money on the mind seems around the corner. An 'economic turn' in the history of science has been gaining attention in the last decade and has been brought forward by the idea that "money is everywhere in science," and that following the trail that it leaves allows historians to draw meaningful characterisations of the social placement of science across disciplinary boundaries, geographic locations, and past the division between industrial and academic research.[20] By looking at systems of patronage beyond the conception of 'patron' as the early modern wealthy individual financing the research of natural philosophers, the economic perspective in the history of science promises to become powerful lenses to describe new themes such as 'ownership' of knowledge, or cast old topics, such as the negotiation of ethical values in research, from a new viewpoint – the changing patronage relationships of the practitioners.

A recent contribution to this economic turn is the 2019 special collection in the *International Journal for History, Culture and Modernity*. Several pieces describe how funding bodies in the late modern period (roughly after the Second World War) have become powerful institutions which dictate who is a valid truth speaker, and how and according to which rules truth-speeches are to be solidified or silenced.[21] Noortje Jacobs

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[20] Andersen, Bek-Thomsen, and Kjærgaard, 'The Money Trail'.

[21] Michael Barany, 'Rockefeller Bureaucracy and Circumknowing Science in the Mid-Twentieth Century', *International Journal for History, Culture and Modernity* 7, no. 0 (2 November 2019), https://doi.org/10.18352/hcm.592; Timo Bolt, 'The Dutch School of Epidemiology. Local and Personal Factors, Funding Agencies and Late Modern Science', *International Journal for History, Culture and Modernity* 7, no. 0 (2 November 2019), https://doi.org/10.18352/hcm.560; Mark Solovey, 'The Impossible Dream: Scientism as Strategy against Distrust of Social Science at the U.S. National Science Foundation, 1945–1980', *International Journal for History, Culture And Modernity* 7, no. 0 (2 November 2019): 209–38, https://doi.org/10.18352/hcm.554; Laura Stark, 'Funding Panels as Declarative Bodies: Meritocracy and Paradoxes of Decision-Making in Modern Science', *International*

and Pieter Huistra introduce this collection and the main historical characteristic of the late modern scientific patronage, which went "from small-scale informal communities to large-scale formal bureaucracies, in which scientific novelty became something that no longer relied on individual ingenuity or serendipity, but on automated systems for carrying out research activities."[22]

The small and growing interest on the rationalisation of scientific patronage in late modernity seems to owe much (if influence is to be measured by citation, that is) to Steven Shapin's *The Scientific Life. A Moral History of a Late Modern Vocation*. The few studies of what I called here the economic turn in history of science seems to coincide in citing Shapin's 2008 book as a long durée acccount of the triumph of the bureaucratisation in the management of science. *The Scientific Life* is regarded as the historiographic backbone of the historical process by which late modern science shifted towards an increased mechanistic organisation to knowledge, but which despite the increase bureaucratisation of science from the late nineteenth century onwards and especially after the Second World War, neither its social production or its practitioners have become the least 'dehumanised' and 'demoralised'. On the contrary, Shapin argues, virtue played and still plays a significant role in the late modern condition of knowledge formation. While *The Scientific Life* seems to be central in the first few historical endeavours into the late modern funding of science, I wish to contest the meaning of this book as it presents itself – namely through the title – as a *history* of science, and the placement that Shapin reserves to virtue in modern science. In drawing points of divergence as exemplified in my work, I wish to specify what 'virtue' has meant in this thesis.

The Scientific Life is presented as a history of science, one that narrates, among other changes, the "social and cultural transition from science as a calling to science as a job."[23] Shapin observes a process of increased organisation and industrialisation of science from the early twentieth century, one that profoundly changed the site of science in a few decades: "The normal site of scientific research by mid-century was not academia but industry," and parallel to this industrialisation of science, the science that did remain in universities, he says, became more organised.[24] The story *The Scientific Life* tells is how rationality came to be embodied in the bureaucratic management of science, and how this process was thought of. The seeming victory of rationality, hailed by many,

Journal for History, Culture and Modernity 7, no. 0 (2 November 2019), https://doi.org/10.18352/hcm.593.

[22] Noortje Jacobs, Huistra Pieter, 'Funding Bodies and Late Modern Science', *International Journal for History, Culture and Modernity* 7, no. 0 (2 November 2019), https://doi.org/10.18352/hcm.584.

[23] Shapin, *The Scientific Life*, 14.

[24] Ibid, 110, 124.

criticised by even more, was based on the possibility of self-governance through objectified self-knowing:

> Late modernity is accounted [by] the triumph of the bureaucrats and the planners, and, by natural extension, of science understood in bureaucratic and planning terms: not just science as knowledge of nature, but science as knowledge of ourselves as natural objects, and, finally, of science as knowledge of science itself—the rational closure of the reflexive circle.[25]

What Shapin describes is the process by which this increased organisation and industrialisation of science went par in par with a critique of such process. This critique, first fronted by what he calls 'social theorists' and 'cultural commentators' in the early twentieth century and shortly after taken up as a discourse of scientists themselves, was based on the image of an increasingly 'demoralised' science, one in which individual autonomy, genius, and creativity were been wiped out at the drum of standardised forms, meaningless interdisciplinarity, and a call for 'relevant' research. This is important. What Shapin argues vigorously is that the critique of the increasing organisation (the idea that science was becoming demoralised) is in itself a historical product, precisely of the early and mid-twentieth century, when some of the earlier sociologists of science attempted at describing (and making normative claims) about the social production of knowledge.[26] The two men whose ideas Shapin aims to contextualize (and by virtue of having become the idiom by which current scientists and some of their commentators still talk, *criticize*) are Max Weber and Robert Merton. These theorists, who promoted a normative image of science as inherently and necessarily 'vocational', 'communal', 'open', 'sceptical', 'individualistic', 'antiauthoritarian' was, according to Shapin, a mistaken description of what science had been and was like. "The passage from the academy to industry was represented [by these theorists and their followers] as a transition from a morally extraordinary to a morally ordinary community, from high to low. Given their socialization, scientists should, and (according to this story) did, rebel against that loss of virtue. That was just their nature."[27] To balance and nuance this story, Shapin contests the essentialism of such image of science by reminding the reader of the little empirical content that the theories of Weber and Merton could actually exhibit.

Much of ink in *The Scientific Life* is spent highlighting how the rationalization, bureaucratization, and industrialisation of the practices of knowledge production went together with critiques of those processes. As science was becoming more organised, the 'social theorists' rose to their pens to frame such organisation as a damaging

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[25] Ibid, 10.

[26] Ibid, 16.

[27] Ibid, 113.

obsession for social rationality in detriment of individual intellectual freedom. The 'cultural commentators' saw the new way of doing science as a loss, and were nostalgic for anything that may have been thought to be 'personal', or even 'humane' in science: "those theorists who identify late modernity with the disappearance of 'the personal equation' from science and industry have, seemingly, won the official academic argument."[28] But, Shapin claims, "the picture of the university as an Ivory Tower of unconstrained scientific inquiry and industry as a regimented, de-moralized, and mercenary Iron Cage did not describe early twentieth-century realities very well and describes early twenty-first-century realities even worse."[29]

Shapin's project is, with his book, to revitalise virtues, and place them right at the center of the process of rationalisation. "The closer you get to the heart of technoscience, and the closer you get to the scenes in which technoscientific futures are made," he observes, "the greater is the acknowledged role of the personal, the familiar, and even the charismatic." In his own words, "much of this book shows how it is that personal virtue, familiarity, and charisma feature in such characteristically late modern configurations as the industrial research laboratory and the entrepreneurial network."[30]

This disconnect between what the so-called theorists and cultural commentators have been saying and what actually happened is what Shapin tries to bring together. His task, he says, is to explain "how it is that personal virtue *still matters* to the making and warranting of late modern technoscience," and correspondingly, "to give some account of why it is so widely said that it does *not matter*."[31] Grand narratives about how we went "from a sacred to a secular world, from trust-in-familiar-people to anonymous trust in impersonal standards and faceless institutions; from virtue to institutional control as a solution to problems of credibility and authority" are all easy and handy stories to tell, consume, and digest.[32] The problem, he says, is that the history of this process is not linear, "unitary, simple, or tidy."[33] This is Shapin's most explicit formulation of the aims of the book, and importantly, he situates his project of revitalise virtues *with and through* a critique of the ideas posed by the so-called social theorists and cultural commentators. By setting in opposition his claim that "personal virtue *still matters*" with a critique of the account that they don't, manifest how this is a book is as much (if not more) a historiographical critique of sociology of science than a historical account of virtues in science.[34]

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[28] Ibid, 10.

[29] Ibid, 18.

[30] Ibid, 5.

[31] Ibid, 13.

[32] Ibid, 14.

[33] Ibid, 13.

[34] While *The Scientific Life* seems to be a history of science, specially by its subtitle, *A Moral History of a Late Modern Vocation*, it is not hard to see how this book is an effort of Shapin *as* historian of

But what is important to distinguish here is that despite Shapin considering virtues such as "the personal" to be the at the "heart of technoscience," his account still conceptualises virtues to arise *from the failure of the rational* to establish itself:

> Late modernity proliferates uncertainties; radical uncertainties mark the venues from which technoscientific futures emerge; and it is in the quotidian management of those uncertainties that the personal, the familiar, and the charismatic flourish.[35]

At this point we get to meat of my argument. Shapin's formulation as displayed above seems to beg the question: how central are then virtues to technoscience? Are they really at "the heart of technoscience" if virtues emerge only when uncertainty proliferates? As I read this, by framing the need of bringing in 'the personal' *only* as an explanation of rational failure (as the need to manage uncertainty), it seems as though the very presence of morality in science is simply a side-effect, a historical contingency that could have been otherwise. I respectfully disagree. Is the co-construction of epistemology and ethics historically contingent, or is *the kinds* of epistemic virtues that their necessary relation what is in fact contingent? It seems to me that Shapin leans too close to the first option, while I would seriously defend the latter. I would strongly argue that epistemic approaches and discussions are *always and necessarily* embedded in ethical discussions, and Shapin's framing of morality as simply a tool which patches the holes that rationality leaves behind, does not seem to make justice to the much richer intimacy between virtue (the moral) and the rational (the epistemic). This 'embeddedness' between the two need some unpacking.

I argue that ethics and epistemology are inseparable because the former is as the mechanism that moulds the self of an agent to attain certain epistemic approach. I believe this to be more productive explanation than seeing ethics simply as a by-product of rational failure. The relation between self and epistemics is much more intimate, "as wax to seal, as hollow imprint," of one another, as Daston and Galison said.[36] In this thesis I have argued for this sort of conceptualisation of ethics, one which allowed me

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science in criticising passé concepts in sociology of science, such as 'pure science' or 'applied science', or empty critiques of scientists losing its creative powers. While it is worth examining those concepts because scientists today still use those concepts, it's important to see Shapin's book in its historiographic and disciplinary light. One which hinges on one of the most commonly held truths of history of science: that there isn't such a thing as 'pure science' because science is always embedded in cultural, political, and social structures that make clear how scientists are never 'independent' or in a disinterested pursuit of truth.

[35] Shapin, *The Scientific Life*, 5.

[36] Lorraine Daston and Peter Galison, 'The Image of Objectivity', *Representations* 40 (October 1992): 81–128, https://doi.org/10.2307/2928741.

to characterise the different epistemic virtues that different groups subscribed to, and the different scientific selves they worked to mould. The image of the virtuous 1970s molecular scientists that I have been distilling throughout my thesis, and synthesized at the beginning of this chapter, involved an array of practices which were considered virtuous: from those practices of the laboratory such as the idea of *belonging* to a diverse community (including manufacturers) which had to *share* standardised practices and materials; to broader practices such as a *refrain* to become politically active beyond keeping correspondence with *Nature* about disappointing budget cuts. At the interface between the political silence and their day-to-day exercises, we find a cult towards *observation* and *recording* of machine-mediated data-gathering. What laid outside the lab of the learned journal, was not of the *individual*'s *responsibility*.

Some advocated strongly for the concentration of the experimenter on the recordings of automated devices, as commercial manufactures positioned 'trust' in their products as the necessary condition for achieving virtue. A virtue that was thought of, for instance, as the ability to translate the experimenter's intensive expertise of the molecular world as social impact of a large community. As we've seen, some would have avidly disagreed on this path to social virtuosity. Activities outside experimentation proper were heavily morally codified as 'relevant' or 'irrelevant', codifying in its wake the practices of experimentation itself. The negotiation of what a good scientist entailed depended not only on his ability to manipulate molecules, but on his social and political positioning. Not getting involved in political discussions and focusing on the 'technical' aspects of research was the position of those, such as Dainton, who believed in a unified image of interconnected layered science whose ultimate goes was applicability (*from* basic, via strategic, *to* tactical). But this image would not have resonated with the virtuousness that other groups would have envisioned a good researcher to have — joining a trade union (BSSRS, ASTMS), writing an lead editorial demanding for more research funding (*Nature*'s editors), or forgetting altogether the 'unapplicable' research (Rothschild) were political alternatives that were all heavily moralised and which their defence introduced ethical cartographies of the different epistemic approaches to knowing the molecular world. Not all knowers were equally virtuous; it was *conditional* on the resonance of their research to a politically demanding social world.

My argument above and in this thesis makes use of the fertile concept of 'epistemic virtues'. I am certainly surprised to see that in *The Scientific Life* little is mentioned of this concept. Granted, *this* specific formulation was only first published in *Objectivity* the year prior to Shapin's publication. But the core assumption of 'epistemic virtues' is not new to Daston and Galison, and Shapin's formulation of how virtues are present in technoscience (as to manage uncertainty) could have benefit strongly from it. As philosopher Martin Kusch has observed, the concept of 'epistemic virtues' as "the idea of using it to bring together ethics and epistemology" does not date to 2007.[37] And as Kusch also points out, Daston and Galison's 1992 heavily cited 'The Image of Objectivity' already contains some of the elements and explanatory model that is used in the book-length explanation of objectivity. Let me unpack some of the notions of epistemic virtues, and how this rich historiography has helped me understand the relationship between ethics and epistemology, diverging in this way with Shapin's.

Of my understandings of what being a knower entails, I owe much to the historical virtue epistemology of Daston and Galison, which itself is reliant on the work of Michel Foucault, and his conceptualisation of what he called 'techniques of the self'. In his 1980-1981 lectures at the Collège de France, Foucault laid out some of the characteristics of this concept which, in his view, played a fundamental role in conceptualising of sexuality, marriage, and sensuality and their relation to broader social structures in Ancient Greece. The technologies of the self, he says, are present in ancient philosophical texts as *tekhnai peri ton bion*, translated as "techniques whose object is life", thus sometimes called 'techniques of life.' These 'techniques' are, according to Foucault's interpretation of the texts, "neither codes, nor exactly prescriptive systems, nor theoretical systems," and still something different to "rules of conduct."[38] Instead, he says, these techniques of the self are descriptive systems that present a *conditional* way of being whose modulation is necessary in regards to a certain life goal. The techniques of self or life are "discourses [...] which present themselves as [...] bottom procedures of constitution of a subjectivity or of subjectivation."[39] In short, they are "not ideologies that attempt to conceal a code," but "the definitions of the conditions on which one will be able to insert as it were the individual's bios, the individual's subjectivity, within a code."[40] In short, these 'techniques' are a shorthand description of a condition: if *x* goal is to be attained, then *b* is how the self must be modulated to attain it. As such, the presence of ethics in the social structure can be found, according to Foucault, in the discourses that present a certain kind of being as *condition* to certain life goal. In my case, I have looked at both scientific advertisements and editorial pieces in which this conditional formulation of what counted as a good scientist could be found.

The reader may remember at this point my position in the previous chapter towards the relationship between the advertisements and the editorial pieces. I argued for a *local* and *negotiable* relationship between them, local because it is only by virtue of the context of the magazine that the advertisements can be printed and understood as conveying descriptions of what 'good' science entails. The relationship is also

[37] Kusch, 'Objectivity and Historiography'.

[38] Michel Foucault, *Subjectivity and Truth: Lectures at the Collége de France, 1980-1981*, ed. Frédéric Gros, trans. Graham Burchell (London: Palgrave Macmillan, 2017), 251.

[39] Ibid, 254.

[40] Ibid, 254.

negotiable: the ethical stance of science was debated in the texts, which invited for different local readings of the advertising images depending on one's position in the negotiation. Taking this idea that the meanings of the texts and images *as* epistemic virtues are local and negotiable, with Foucault's conditional understanding of the 'techniques of life', we can bring the attention to *the self*, and especially, to how the practices of laboratories and the social placement of the scientists mould this self accordingly, depending on the set life goal. To do this, let me review the two main set of sources I used here and their major conclusions.

On the one hand, scientific advertisements. I believe they are important for the task at hand because they draw a line, the surface line between the experimenter and the machine, as Daston and Galison's wax and seal. The moulding of the self through the techniques of self is present in the adverts. Beyond their rhetorical force, scientific advertisements establish the image of the ideal laboratory device, and by doing so they sketch the image of the operator of those devices. By situating the laboratory object, they relegate the laboratory subject to a certain place, and by placing the laboratory object in relation to a specific goal of science, they reserve a given spot for scientists, a "constitution of a subjectivity". The image of the advertisement illustrates the best characteristics of the machine, and sets the standard for the operators who use it and for the manufacturers who want to compete in the market. The innovation of the technology brings along clear instructions on how to read and assess the virtues of the device. And when the object has been bought, the manual of instructions and the product catalogue informs the reader about the knowledge necessary and the manual skills required to manipulate it. The advertisement, by photographing a device, draws the line between what the object is to what the subject should be (see front cover desing).

Now, regarding the editorials, we can say that the negotiations of what entailed to be a good scientist were in fact a set of conditional descriptions in search for a specific 'life goal', to use Foucault's terminology. The discussions were never isolated, and they brought together epistemic and ethical issues, like in the case of what was the best way to regulate recombinant DNA molecules (Chapter Three); and political and epistemic issues, like in the discussions on how research should be founded, which were themselves based on epistemic formulations on the organisation of science (think of the principle of unity of Dainton/Popper, the hierarchical division between pure and basic science of Rothschild, or the inevitable power-laden conception of science of the BSSRS). In short, what Foucault terms as 'life goal' is the ultimate condition by which the self must be moulded and the different social, epistemic, and political dimensions of his consitution. As such, the 'life goal' is akin to the 'social responsibility' in the actor's idiom, the problem of "public criteria of accountability" in Csiszar's words, or as I have called it here, the *public placement of science*. The negotiations that were taking place in the texts are the context necessary to understand the scientific advertisements as images of objects that were epistemically desirable to perform a science that could be considered 'good'.

What I say is quite simple: the conceptualisation of a 'good' scientist did not come about only by virtue of how he experimented, thought, and saw the world; but also as how he positioned himself in the social arena: whether he was or not politically involved, whether he chose to present his research as solving national, industrial, or inequality problems, whether he held a patent of his work or published his work with no fee to readers, whether he engaged with laymen to explain his esoteric work, or whether he decided to publish only in specialist journals. All these *social* alternatives situate the being of the scientist in a conditional "individual's subjectivity." And as such, the tension between these alternatives becomes fertile to be studied from the perspective of historical virtue epistemology, and as a result, putting an emphasis on how epistemic attitudes inescapably hinge with certain social positioning of science and certain way of being a scientist. Telling a story of this epistemic-social crux is telling a story of how identity was shaped and constructed through an ethical gaze. A intrinsic, constitutive ethical gaze.

Ethics as epiphenomena: science as producer displaces ethical responsibility

Daston and Galison, in the first formulation of how ethics and epistemics relate, claimed that not only is the ideal of objectivity historically situated, but that objectivity is a *kind* of morality. They construct and explain a seeming paradoxical relation between epistemics and ethics:

> *Morality* is the salient word here, and with it comes an apparent paradox. How could it be that the very objectivity that seemed to insulate science from the moral—the creed that takes the fact/value distinction as its motto-simultaneously lay claim to moral dignity of the highest order? This apparent contradiction is an artefact of the negative quality of objectivity. It is an ethos of restraint, both external restraints of method and quantification and internal restrain of self-denial and self-criticism. Otherwise put, objectivity is a morality of prohibitions rather than exhortations, but no less a morality for that.[41]

In this thesis I have tried to expand on this paradox that Daston and Galison propose, one which puts the finger on the intimacy between "moral dignity" and claims of freedom

[41] Daston and Galison, 'The Image of Objectivity'.

from values.[42] The paradox that they observe, transposed to my research, would be now formulated as: how could scientists claim to be avoiding 'ethical' discussions regarding their work whilst still claiming to be "determined" and hard-working researchers who, by only focused on the 'technical' aspects of molecules, they could render "responsible" research? What granted scientists the ability of producing worthy science whilst assuming that their research had been freed from value? Where did ethics arise?

The image of this contradiction is embodied in one actor who I have only discussed in passing in Chapter Three. Henry Bedson, may I remind the reader, was the head of the Birmingham University microbiology department where smallpox was being researched. Due to "poor laboratory procedures," a deadly load of the virus left the containment facilities and infected Janet Parker, a medical photographer who worked in a neighbouring laboratory. The suicide of Bedson, who was considered a "committed" and "determined man" illustrates the responsibility he felt he owed for his work. Far from cynically dismissing his sense of "dedicated conscientiousness, and [...] his extreme sense of responsibility," I believe we should see this case as a tragic example of the paradoxical nature of the social placement of 1970s molecular science. As a researcher of the microscopic basis of smallpox, we could see Bedson's commitment to society – his "extreme sense of responsibility" – as his ability and accountability to deliver new understanding of the pathology of the virus. "A determined man, who would just sort of get on with things," as a colleague of Bedson remembered him. But this paradox did not only inhabit Bedson's moral determinacy to finding more about smallpox. It was institutionally expressed in his relationship with the World Health Organisation (WHO), which on the one hand, had recruited Bedson to research several variants of the virus, and on the other hand, was responsible for overseeing laboratory safety policies in his lab. The paradox was explicitly explained, Bedson:

> was in a Catch-22 situation, because [the] WHO wanted him to continue his work but were not happy with the state of the lab, yet because it was due for closure in six months there was no way he would get any money for it. So he just had to get the work done more quickly, which meant using more samples in the lab,

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[42] I talk here of 'expanding' Daston and Galison's paradox of mechanical objectivity of the nineteenth century into the paradox of social responsibility in science in the 1970s, knowingly accepting that late twentieth century western science also followed the cannons of the mechanical objectivity epistemic virtue. As the authors write in *Objectivity*, the rise and fall of the different modalities of objectivity does not imply replacement, but rather 'update' or even 'addition'. "The emergence of objectivity as a new epistemic virtue in the mid-nineteenth century did not abolish truth-to-nature, any more than the turn to trained judgment in the early twentieth century eliminated objectivity. Instead of the analogy of a succession of political regimes or scientific theories, each triumphing on the ruins of its predecessor, imagine new stars winking into existence, not replacing old ones but changing the geography of the heavens." Daston and Galison, *Objectivity*, 18.

> and the more samples you have, of course, the more likely it is that something will happen.[43]

Ethical behaviour, such as enforcing correct safety precautions, was not performed *because* the expected outcome of the work was *already* deemed as ethically good. As a clear example, Bedson's case was representative of 1970s molecular biology. The ethical worries that recombinant technology brought forward (Chapter Three) could only be dismissed because the work that was expected to be derived from them was institutionally established to be good (Chapter Two). It was only on account of the virtuousness that it was assumed of science as the practice which produces applicable knowledge (either immediately as Rothschild proposed, or at some point down the line as Dainton envisioned) that ethical considerations of the process of making of that knowledge could be dodged.

To be precise, my point is that ethical issues were *not* dodged, that they were in fact taken into account as the *outcome* of research. It was only those which were thought to be a hindrance to the delivery that were cast aside. The moratorium on recombinant DNA research, the mandatory notification of the intention of performing research, the safety officer who had the power to veto research, the classification of organisms which banned use of certain animal models, or even the physical containment facilities which posed a barrier between the experimenter's glove and his pipet were measures which in fact were seen as ethically suspect *because* they interfered with a quick delivery of 'good' results.

But the scientists weren't alone. It was by virtue of individual research groups being embedded within the larger community of molecular researchers, itself embedded within the larger biomedical science, that the concrete products of the individual scientists were seen as good. The scientist as a person produced tangible objects, such as articles, predictions, hypothesis, data, talks, seminars, courses, patents, or even a timid column in a generalist newspaper explaining the relevance of his research. These were different ways by which scientists could deliver what was expected of them – 'responsible' science. These products were embedded with the products of the larger scientific community, and as such, delivering socially responsive science. The goodness of research was an epiphenomenon of the community; for the scientist, it was grasped as a contribution to the international discussion. The individual was inserted in a network, and it was only by trusting the process of the community to achieve social consensus on what counted as valid truth statements that the individual could assert his virtues as a socially responsible individual scientist. And even so, the (molecular) academic community was waived from actually delivering so-called applications. As they

[43] Docherty and Foulkes, 'Toxic Shock'.

would have it, science was in the business of providing only the agreed-upon truth statements in specialist literature, it was up to the so-called industrialists and policymakers to enact the value of those objective, value-free truths.

The relevance of understanding that the 1970s molecular science community saw ethics as epiphenomena has an implication for how commentators and managers of the scientific process (not only historians of science, but also sociologists and philosophers of science, and those investigating and implementing research ethics, and devising science policies) are to draw descriptive and normative accounts on scientists' behaviour. The key argument here is that commentators need to put to the test the concepts that the scientific community uses to define its practice, without taking them for granted. In this thesis, questioning how the concepts of 'technical', 'social', 'ethical', and 'political' were used by the actors and whether they were apt for my historical explanations has helped me in bringing together some of the pieces of this puzzle. Given what I have displayed here, I would claim that in order to understand research ethics (including here *all* ethical considerations within experimentation, including workplace safety precautions), one needs to understand them in the context of funding structures. Let me elaborate.

I would like to put the emphasis that ethical considerations *were* present in the debate over the regulation of recombinant DNA technology *as* the promise of the techniques themselves to deliver goods. Despite the scientists' claims to the contrary, who allegedly formed committees that were "not [...] set up to make ethical judgments about the use of the techniques," they certainly did.[44] Posing the techniques as potentially good, their ethical consideration was already in place. Historians of science know not to take the actor's category at face value. In this specific case, it's important for our historical explanations to reconsider both what 'ethical' meant and how it was used. Susan Wright is likely the scholar which has dedicated most work to the case of the regulation of recombinant DNA technology. Her observations and interpretation of the events are still rich today, such as her claims that the committees involved and the conferences organised were, from their outset, "designed to enable research to move forward and that this goal was anticipated," and that there was a reduction of the reduction of the "genetic engineering problem to a technical question of containment."[45] In parallel to these explanations, she claims, this reduction to a 'technical' question obscured the "broader social, ethical, and policy issues" of the technology.[46] These "broader" and "larger" issues, as Wright exemplified, ranged from considerations of the

[44] 'Report of the Working Party on the Experimental Manipulation of the Genetic Composition of Micro-Organisms (Cmnd. 5880)' (London: State of Education and Science, House of Commons Parliamentary Papers, January 1975), 3.

[45] Wright, Susan, *Molecular Politics*, 145; Wright, 'Molecular Politics in a Global Economy', 86.

[46] Wright, Susan, *Molecular Politics*, 146; Wright, 'Molecular Politics in a Global Economy', 86.

use of genetic engineering for biological warfare to the development of genetics as a tool to produce 'designer babies', and some more mundane concerns such as workplace safety or the fear of environmental pollution with organisms containing foreign genetic material. The obscuring of these issues is most certainly true, but I would argue that saying that there was a reduction of the debates to technical considerations to the detriment of broader social, ethical, and political reflexions is only accurate if we take the actor's categories of 'technical', 'social', 'ethical', and 'political'. Granted, the meanings of these concepts are also the common parlance of today, but historians should not be shy in questioning their validity as analytic categories. In an analytic sense, if we are not to take 'ethics' at face value, we shall see how ethical assessment cannot be independently sought from a study of patronage relationships.

What's history for

The Scientific Life is as much about what sociologists had said the scientific life was about than about what the scientific life was actually about. The main critique of Shapin in this book is on the distinction between so-called pure and so-called applied science. As any contemporary historian of science, he is repulsed by the use of such terms in an analytic sense, and therefore, puts into question the "commentators who reckoned that science in industry was a crucial *problem* in the late modern order and that the very idea of industrial science might even be a *contradiction*. The goal of scientific inquiry was Truth; the goal of business was Profit."[47] He puts under historical test such rigid divisions, and he is very much right to label them as being analytically poor. But Shapin's focus is on a particular critique of the industrialisation of science. He writes: "Much of this commentary embedded distinctions between some such notion as 'pure science' and some such notion as 'applied science,' the former presumed to have its natural home in the university and the latter in commercial settings."[48] Pay attention to the initial 'much'. While it is true that such division is naïve and he is right to call it out, and while it is also true that perhaps *most* cultural and social commentary went along these lines, he is forgetting about other kinds of critiques.

On the second chapter I showed how the defence of Dainton of 'pure science' was a watered-down version of Rothschild's customer-contractor principle, and that in fact, shared some of the basic assumptions. The sort of defence that Dainton was putting up relies on the same principles that Shapin is attacking. My opinion is that gripping too hard on the philosophical naïveté and empirical inadequacy of such positions is unproductive. While it may be fair to consider that *most* of the commentary against

[47] Shapin, *The Scientific Life*, 96.

[48] Ibid, 97.

industrialisation and bureaucratisation relied on simple ideas about 'basic' and 'applied' science, I would argue that historians should look beyond the majoritarian discourses, and to bring our attention to silenced, rare, or simply, secondary voices.

And I say so not to promote paternalistic and humanitarian attention to social minorities as a good epistemic attitude in itself. I say it to better history-writing because the consideration to secondary discourses decentres the debates and provides a perspectival point from where to understand complex social tensions. Shapin's *The Scientific Life* reads like a conversation between two, while the situation included three, four, five, and many more speakers. By bringing the secondary actors of the play, we are able to triangulate our subject matter and move beyond the dialogue. In this thesis, I have used the discourse of the ASTMS and BSSRS to perform this function. Their discourses, while not belonging to a so-called minority whatsoever (they were associated with Nobel prize winners and the Royal Society, to begin with, and had direct relationships to *Nature* and the Labour party), were different enough to introduce a triangular perspective from which to better look at the main two positions that monopolised the space of *Nature*. The BSSRS, for instance, pushed for a conception of science policy as inherently power-laden, and of science as an inherently political subject in which individual scientists as well as the research community had duties in their conditions of scientists. Importantly, both the BSSRS and Rothschild agreed that science was 'political', but their meanings differed. Showing an alternative to the meaning and uses of 'politics', we are able to see the worth of triangulating historical analysis. Also, the proposal of a 'Community Science Research Councils' was based, in opposite to Dainton, on a pluralistic understanding of society in which 'the public' should get directly involved with dictating research avenues. The BSSRS scientists' direct work with factory trade unions in relation to toxicity assessment in workplaces, or the ASTMS's attention to the convoluted relationship between workplace safety and workload pressures, provide the historian with actors' discourses that, while being secondary, help in contextualising the debates that were and *were not* present in the mainstream forum. By decentralising the debates, we are able to find who was in the center and how far the field loomed.

Two issues arise from the decentring I performed. First of all, this reminds me to a methodological problem that I had by using *Nature* as a historical source, and an issue that Baldwin encountered too when writing her monograph on the journal. In introducing her approach, she writes:

> A book about an individual journal might reasonably be expected to analyze decisions about which pieces were accepted and rejected. Unfortunately, without an editorial archive it is impossible to do a systematic study of how many or what kinds of articles *Nature* rejected. There are a handful of letters in the

> editors' personal archives that refer to particular editorial decisions and that give us a few insights into the editors' choices, but these hardly provide a complete picture.

As Baldwin, I wasn't here dealing with the scientific articles of *Nature*, nor wasn't interested on what the editors rejected in terms of experimental content. I wanted to investigate the discourse about the negotiations of identity of the scientific community as discussed in *Nature*. By showing positions such as the BSSRS or the ASTMS, and how they were only printed in the margins and contextualised as those of 'radical' scientists, I hope to have been able to bring third discursive vertices into my story, so that we are able to see a more panoramic view.

The second point, regarding the importance of decentralised history-writing, is that by showing what didn't make it to the mainstream floor, we get a glimpse of the alternative proposals that did not become heavy enough to move historical contingency towards their ends. Historical contingency works in two ways. As historians, we normally say that our studies show that we should not take for granted the social structures that we have today because they could have been otherwise, and that it was not written in stone that they were going to be the ones that we have. On the other direction of this contingency, I'd argue, the writing of history *should* encourage political action, not only because writing history makes one realize and embrace the fact that the future is contingent on today's present, but also because we are experts on the social discourses that have and have not shaped social structures. By studying, learning, and communicating the secondary proposals that history seems to have rejected, we are highlighting the roughness of historical contingency, the feeble continuity of today's present, and the fickle direction of the future.

When Roger Cooter stresses the importance of realising and questioning one's own contingency as a historian, he hammers into his readers the idea of our inescapable "epistemic present."[49] Cooter's political worries ooze through the title of *Writing History in the Age of Biomedicine*. He fears that the humanities, and academic history in particular, are being engulfed in a homogenous biomedical epistemic present, and he calls to arms for a "sustained historical unpacking of the episteme in which we write the past to help think the future."[50] Certainly I am with him in his plea for a pluralistic notion of the university, one in which history-writing is inherently radical *because* it tumbles narratives, and does so by deconstructing the epistemic categories of other disciplines in its historically contingent building blocks. The "healthy scepticism of biological

[49] Cooter, 'The End? History-Writing in the Age of Biomedicine (and Before)', 14.

[50] Ibid, 39.

concepts and categories," he says, must be maintained, "for fear of falling into essentialisms deemed hostile to the possibilities for cultural transformation."[51]

The power of history writing must not rely on deconstructive contingency *only*, however. While the fear of epistemic conquest by the biological sciences is a threat that must be dealt with, let us not forget of the *opportunities* that contingency concedes. The scientist is not the episteme, and the problems of today's academia (history included) are ubiquitous: we are categorically all facing the same problems, while perhaps only different in degree. Career instability and systemic inequalities in the process of hiring hinge, *across disciplinary borders*, on conceptions of prestige and institutional status.[52] Also across disciplines, increasing working hours facilitate take-home work conditions that reify heteronormative family structures.[53] The pressure for individual productivity has, also across the board, being turned from the well-known 'publish-or-perish' mechanism, to a 'impact-or-perish' system, in response to the increasing obsession with quantifiable metrics as a representation of so-called impact.[54] Parallelly, discussions of research integrity hinge on the circumstance that research misconduct is more a matter of unfair career competition than a serious epistemic threat to the social consensus on Truth. The watchdogs of science know this all too well, and aim at tracking in all disciplines the complicity between epistemic and ethical rigidity.[55] At the core, the worries of today's academia highlight the idea that the researcher, above all, is a worker. The idea of research as a job, and the conditions in which this takes place, is becoming a contentious matter that stirs heated discussions in all practitioners, and dare I say, is the only thing that actually unites academia.

These widespread worries seem to have found a home in the movement of 'Open Science'. As a scientific movement, it promises to be the solution for epistemic worries such as the replication crisis and a widespread publication bias towards publishing only success stories. However, many of the advocates of Open Science present themselves as articulating and revitalising some of the ideals that, in their mind, are intrinsic to a never-defined 'science'. Some of the naivety of the Open Science movement lies on advocating, for instance, for an open accessibility to specialised knowing as a symbol of "removing access barriers" which would "accelerate research, enrich education, share the learning

[51] Ibid, 10.

[52] Aaron Clauset, Samuel Arbesman, and Daniel B. Larremore, 'Systematic Inequality and Hierarchy in Faculty Hiring Networks', *Science Advances* 1, no. 1 (1 February 2015): e1400005, https://doi.org/10.1126/sciadv.1400005.

[53] Giuliana Viglione, 'Are Women Publishing Less during the Pandemic? Here's What the Data Say', *Nature* 581, no. 7809 (20 May 2020): 365–66, https://doi.org/10.1038/d41586-020-01294-9.

[54] Mario Biagioli and Alexandra Lippman, 'Introduction: Metrics and the New Ecologies of Academic Misconduct', in *Gaming the Metrics: Misconduct and Manipulation in Academic Research* (The MIT Press, 2020), https://doi.org/10.7551/mitpress/11087.001.0001.

[55] 'Retraction Watch', accessed 25 November 2020, https://retractionwatch.com/.

of the rich with the poor and the poor with the rich, make this literature as useful as it can be, and lay the foundation for uniting humanity in a common intellectual conversation and quest for knowledge."[56] Historians of science are used to such naivety, but more often than not, correct it only in specialised historical and philosophical literature. The Open Science movement is precisely the new interlocutor that would be willing to hear our stories. For instance, the 'open' in Open Science is a fertile concept for historians to situate contextually. A lack of self-reflection by the scientific community on the multiple meanings and uses of 'open' seems to have caused the community to shoot itself, delivering business models under the banner of Open Access that perpetuate the inequalities that the movement in its origins tried to remedy.[57]

If the movement of the Open Science is to succeed not only as an epistemic movement but a social one, historians must turn their inquiries to the topic of the scientific community as a workforce, a working community preoccupied not only with explaining the world but making a living out if. As Csiszar has argued, historical moments when "the norms and forms of expert communication have been most in doubt," like in today's worries for publication biases, peer review, predatory enterprises, "are precisely those moments when scientific practitioners have sought—or been forced—to renegotiate their public status within a wider political landscape."[58] The topic of employability, and the academic self as a research worker stands precisely at the mid-point between the producer of "expert communication" and the "wider political landscape".

Historians are the best equipped to illustrate the contingencies of today's academia, the routes of knowledge communication, and the avenues by which scientists establish and maintain patronage relationships. In being able to give meaning to these complex issues, attend to the values at play, and the convoluted relations between politics and science, we should be posing ourselves not (only) as the enemies of the essentialist scientist, as Cooter would have it, but (also) as the allies of a common working class.

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[56] Leslie Chan et al., 'Budapest Open Access Initiative', *ARL Bimonthly* 48 (2002).

[57] Holly Else, 'Nature Journals Reveal Terms of Landmark Open-Access Option', *Nature*, 24 November 2020, https://doi.org/10.1038/d41586-020-03324-y.

[58] Csiszar, *The Scientific Journal*, 3.

Bibliography.

A Conversation with Jonathan King. MIT Biology, 2011. https://youtu.be/d16-zdqJq_8.

'About the Journal | Nature'. Accessed 5 October 2020. https://www.nature.com/nature/about.

Aibar, Eduard, Josep Lladós-Masllorens, Antoni Meseguer-Artola, Julià Minguillón, and Maura Lerga. 'Wikipedia at University: What Faculty Think and Do about It'. *The Electronic Library* 33, no. 4 (1 January 2015): 668–83. https://doi.org/10.1108/EL-12-2013-0217.

Aldrich Chemical Company, Inc. '3-Isobutyl-1-Methylxanthine'. *Nature* 253, no. 5491 (6 February 1975): Cover 4.

———. 'The Source'. *Nature*, no. 249 (1974).

———. 'We'll Stack Our New BIO-CHEMICAL CATALOG against All the Others!' *Nature* 241, no. 5390 (16 February 1973): Cover 4.

'Amber Light for Genetic Manipulation'. *Nature* 253, no. 5490 (31 January 1975): 295–295. https://doi.org/10.1038/253295a0.

Andersen, Casper, Jakob Bek-Thomsen, and Peter C. Kjærgaard. 'The Money Trail: A New Historiography for Networks, Patronage, and Scientific Careers'. *Isis* 103, no. 2 (1 June 2012): 310–15. https://doi.org/10.1086/666357.

Ashburner, Michael. 'An Open Letter to the Health and Safety Executive'. *Nature* 264, no. 5581 (4 November 1976): 2–3. https://doi.org/10.1038/264002a0.

'Asilomar Conference on DNA Recombinant Molecules'. *Nature* 255, no. 5508 (5 June 1975): 442–44. https://doi.org/10.1038/255442a0.

'Asilomar Conference on Recombinant DNA'. In *Wikipedia*, 2 February 2020. https://en.wikipedia.org/w/index.php?title=Asilomar_Conference_on_Recombinant_DNA&oldid=938729053.

'ATV Today: 09.11.1978: Smallpox Outbreak'. 16mm. *ATV Today*. Associated Television, 9 November 1978. MACE Archive. https://www.macearchive.org/films/atv-today-09111978-smallpox-outbreak.

Azbel, Mark, Venyamin Fain, Ilya Pyatetskii-Shapiro, and Viktor Brailovsky. 'Soviet Jews'. *Nature* 254, no. 5502 (April 1975): 650–650. https://doi.org/10.1038/254650a0.

'B.A.—First the Bad News'. *Nature* 251, no. 5470 (6 September 1974): 4–4. https://doi.org/10.1038/251004a0.

Baldwin, Melina. *Making 'Nature': The History of a Scientific Journal*. University of Chicago Press, 2015.

Barany, Michael. 'Rockefeller Bureaucracy and Circumknowing Science in the Mid-Twentieth Century'. *International Journal for History, Culture and Modernity* 7, no. 0 (2 November 2019). https://doi.org/10.18352/hcm.592.

Beale, Bob. 'High-Tech: We Make It but Can Not Buy It'. *Sydney Morning Herald*. 13 March

1975.

Beckwith, Jon, and Jonathan King. 'XYY Syndrome. A Dangerous Myth.' *New Scientist* 64, no. 923 (14 November 1974): 474–76.

Bell, Alice. '"Science Is Not Neutral!" Autumn 1970, When British Science Occupied Itself'. *The Guardian*, 8 September 2014. http://www.theguardian.com/science/political-science/2014/sep/08/science-is-not-neutral-autumn-1970-when-british-science-occupied-itself.

———. 'The Scientific Revolution That Wasn'tThe British Society for Social Responsibility in Science'. *Radical History Review* 2017, no. 127 (1 January 2017): 149–72. https://doi.org/10.1215/01636545-3690930.

Bensaude-Vincent, Bernadette. 'A Genealogy of the Increasing Gap between Science and the Public': *Public Understanding of Science*, 31 March 2017. https://doi.org/10.3109/a036858.

Berg, P., and M. F. Singer. 'The Recombinant DNA Controversy: Twenty Years Later.' *Proceedings of the National Academy of Sciences* 92, no. 20 (26 September 1995): 9011–13. https://doi.org/10.1073/pnas.92.20.9011.

Berry, Dominic J. 'Making DNA and Its Becoming an Experimental Commodity'. *History and Technology* 35, no. 4 (2 October 2019): 374–404. https://doi.org/10.1080/07341512.2019.1694125.

Biagioli, Mario, and Alexandra Lippman. 'Introduction: Metrics and the New Ecologies of Academic Misconduct'. In *Gaming the Metrics: Misconduct and Manipulation in Academic Research*. The MIT Press, 2020. https://doi.org/10.7551/mitpress/11087.001.0001.

Biosis. 'We want to make you "C.L.A.S.S." conscious'. *Nature* 248, no. 5446 (22 March 1974): Cover 3.

Bolt, Timo. 'The Dutch School of Epidemiology. Local and Personal Factors, Funding Agencies and Late Modern Science'. *International Journal for History, Culture and Modernity* 7, no. 0 (2 November 2019). https://doi.org/10.18352/hcm.560.

Bont, Raf de. 'The Adventurer and the Documentalist: Science and Virtue in Interwar Nature Protection'. In *Epistemic Virtues in the Sciences and the Humanities*, edited by Jeroen van Dongen and Herman Paul, 1st ed., 321:129–47. Boston Studies in the Philosophy and History of Science. Springer International Publishing, 2017.

British Society for Social Responsibility in Science. 'Government Research and Development. Comments on the Green Paper "A Framework for Government Research and Developmewnt" (Cmn 4814, 1971)'. London, January 1972. K/PP178/11/1/24. King's College London Archives (Wellcome Library). https://wellcomelibrary.org/item/b20050069.

Brooks, Harvey. 'Rothschild's Recipe in the United States'. *Nature* 235, no. 5337 (11

February 1972): 301–2. https://doi.org/10.1038/235301a0.

Brownlee, G. G. ‘Genetic Engineering with Viruses’. *Nature* 251, no. 5475 (11 October 1974): 463–463. https://doi.org/10.1038/251463a0.

Bud, Robert. ‘Molecular Biology and the Long-Term History of Biotechnology’. In *Private Science. Biotechnology and the Rise of the Molecular Sciences*, edited by Arnold Thackray, 3–19. Philadelphia: University of Pennsylvania Press, 1998.

Calver, Neil. *The Royal Society and the Rothschild ‘Controversy’ 1971-1972*. Public lecture audio recording. The Royal Society, London, 2013. https://royalsociety.org/science-events-and-lectures/2013/rothschild-controversy/.

Calver, Neil, and Miles Parker. ‘The Logic of Scientific Unity? Medawar, the Royal Society and the Rothschild Controversy 1971–72’. *Notes and Records of the Royal Society of London* 70, no. 1 (20 March 2016): 83–100. https://doi.org/10.1098/rsnr.2015.0021.

Cambrosio, Alberto, and Peter Keating. ‘Monoclonal Antibodies: From Local to Extended Networks’. In *Private Science. Biotechnology and the Rise of the Molecular Sciences*, edited by Arnold Thackray, 165–81. Philadelphia: University of Pennsylvania Press, 1998.

Chan, Leslie, Darius Cuplinskas, Michael Eisen, Fred Friend, Yana Genova, Jean-Claude Guédon, Melissa Hagemann, Stevan Harnad, Rick Johnson, and Rima Kupryte. ‘Budapest Open Access Initiative’. *ARL Bimonthly* 48 (2002).

Chapman, Peter, David Dickson, Shivaji Lal, Jerry Ravetz, Hilary Rose, Steven Rose, Jonathan Rosenhead, David Wield, and Maurice Wilkins. ‘Responsibility in Science’. *Nature* 230, no. 5288 (5 March 1971): 67–67. https://doi.org/10.1038/230067a0.

Clauset, Aaron, Samuel Arbesman, and Daniel B. Larremore. ‘Systematic Inequality and Hierarchy in Faculty Hiring Networks’. *Science Advances* 1, no. 1 (1 February 2015): e1400005. https://doi.org/10.1126/sciadv.1400005.

Cohen, S. N. ‘DNA Cloning: A Personal View after 40 Years’. *Proceedings of the National Academy of Sciences* 110, no. 39 (24 September 2013): 15521–29. https://doi.org/10.1073/pnas.1313397110.

Cohen, Stanley N. ‘Bacterial Plasmids: Their Extraordinary Contribution to Molecular Genetics’. *Gene* 135, no. 1–2 (December 1993): 67–76. https://doi.org/10.1016/0378-1119(93)90050-D.

Collaborative Research, Inc. ‘When It Comes to MRNA, We’re Great Isolationists’. *Nature* 254, no. 5499 (3 April 1975): iii.

Cooter, Roger. ‘The End? History-Writing in the Age of Biomedicine (and Before)’. In *Writing History in the Age of Biomedicine*, by Roger Cooter and Claudia Stein, 1–40. Yale University Press, 2013.

Cooter, Roger, and Claudia Stein. ‘Coming into Focus. Posters, Power, and Visual Culture in the History of Medicine’. In *Writing History in the Age of Biomedicine*, by Roger Cooter and Claudia Stein, 112–37. Yale University Press, 2013.

———. ‘Visual Objects and Universal Meanings. AIDS Posters, “Globalization,” and History’. In *Writing History in the Age of Biomedicine*, by Roger Cooter and Claudia Stein, 138–59. Yale University Press, 2013.

Coulter Electronic Limited. ‘If Your Haematology Lab Is Busy, Cost Conscious and Forward Thinking You Need the Coulter Model S’. *Nature* 255, no. 5509 (12 June 1975): Back Cover.

Csiszar, Alex. ‘Introduction. “Broken Pieces of Fact”’. In *The Scientific Journal. Authorship and the Politics of Knowledge in the Nineteenth Century*, 1–21. Chicago and London: The University of Chicago Press, 2018.

———. ‘Meeting in Public’. In *The Scientific Journal. Authorship and the Politics of Knowledge in the Nineteenth Century*, 67–118. Chicago and London: The University of Chicago Press, 2018.

———. ‘The Press and Academic Judgment’. In *The Scientific Journal. Authorship and the Politics of Knowledge in the Nineteenth Century*, 23–66. Chicago and London: The University of Chicago Press, 2018.

———. *The Scientific Journal. Authorship and the Politics of Knowledge in the Nineteenth Century*. Chicago and London: The University of Chicago Press, 2018.

‘Dainton Demands a Hearing’. *Nature* 235, no. 5337 (11 February 1972): 296–97. https://doi.org/10.1038/235296a0.

Dainton, Frederick. ‘The Future of the Research Council System’. *A Framework for Government Research and Development*, 1971.

Daston, Lorraine, and Peter Galison. *Objectivity*. 1st ed. Zone Books, 2007.

———. ‘The Image of Objectivity’. *Representations* 40 (October 1992): 81–128. https://doi.org/10.2307/2928741.

Davies, David. ‘Nature in the Future’. *Nature* 244, no. 5417 (24 August 1973): 475–475. https://doi.org/10.1038/244475a0.

Davis, David. ‘Manpower Supplement’. *Nature* 255, no. 5506 (22 May 1975): 283–283. https://doi.org/10.1038/255283a0.

Difco. ‘Microbiological Reagents and Media Delivered to Your Bench Quickly’. *Nature* 217, no. 5125 (20 January 1968): xiv.

‘DNA Committee Has Its Critics’. *Nature* 257, no. 5528 (23 October 1975): 637–637. https://doi.org/10.1038/257637a0.

Dobbs, E. Roland. ‘The Organisation and Control of Scientific Research by the United Kingdom Government’. *Higher Education* 1, no. 3 (1 August 1972): 345–55. https://doi.org/10.1007/BF01957558.

Dobzhansky, Theodosius. ‘An Outline of Politico-Genetics’. *Science* 102, no. 2644

(1945): 234–36.

Docherty, Campbell, and Caroline Foulkes. 'Toxic Shock; Twenty-Five Years Ago a Disease That Many Thought Was Dead and Gone Reared Its Head in Birmingham: Smallpox. Campbell Docherty and Caroline Foulkes Look Back at the 1978 Outbreak and Ask If It Could Ever Happen Again.' *The Birmingham Post*, 4 October 2003. https://www.thefreelibrary.com/Toxic+SHOCK%3B+Twenty+five+years+ago+a+disease+that+many+thought+was...-a0108504745.

Dongen, Jeroen van, and Herman Paul. 'Introduction: Epistemic Virtues in the Sciences and the Humanities'. In *Epistemic Virtues in the Sciences and the Humanities*, edited by Jeroen van Dongen and Herman Paul, 1st ed., 321:1–10. Boston Studies in the Philosophy and History of Science. Springer International Publishing, 2017. https://doi.org/10.1007/978-3-319-48893-6_1.

Elkin, Lauren. 'Susan Sontag Was a Monster, of the Very Best Kind'. *Aeon*, 2019. https://aeon.co/essays/susan-sontag-was-a-monster-of-the-very-best-kind.

Else, Holly. 'Nature Journals Reveal Terms of Landmark Open-Access Option'. *Nature*, 24 November 2020. https://doi.org/10.1038/d41586-020-03324-y.

Fernández Carro, Remo. 'What Is a Scientific Article? A Principal-Agent Explanation'. *Social Studies of Science*, 27 August 2020, 0306312720951860. https://doi.org/10.1177/0306312720951860.

Ford, Brain J. 'Call for Biohazard Legislation'. *Nature* 250, no. 5465 (2 August 1974): 364–65. https://doi.org/10.1038/250364a0.

'Forever Amber on Manipulating DNA Molecules?' *Nature* 256, no. 5514 (17 July 1975): 155–155. https://doi.org/10.1038/256155a0.

Forma Scientific. 'Isn't Your Work Too Important… For Anything But the Best'. *Nature* 258, no. 5535 (17 December 1975): v.

Fortun, Michael. 'The Human Genome Project and the Acceleration of Biotechnology'. In *Private Science. Biotechnology and the Rise of the Molecular Sciences*, edited by Arnold Thackray, 182–201. Philadelphia: University of Pennsylvania Press, 1998.

Foucault, Michel. *Subjectivity and Truth: Lectures at the Collége de France, 1980-1981*. Edited by Frédéric Gros. Translated by Graham Burchell. London: Palgrave Macmillan, 2017.

Franklin, Rosalind, and Ryan Gosling. 'Molecular Configuration in Sodium Thymonucleate'. *Nature* 171, no. 4356 (April 1953): 740–41. https://doi.org/10.1038/171740a0.

Fyfe, Aileen, and Bernard V. Lightman, eds. *Science in the Marketplace: Nineteenth-Century Sites and Experiences*. Chicago: Univ. of Chicago Press, 2007.

Goodhart, C. B. 'The Biologist's Dilemma'. *Nature* 229, no. 5281 (15 January 1971): 213–213. https://doi.org/10.1038/229213c0.

Guijarro Mora, Víctor. 'Retórica y persuasión en los catálogos comerciales españoles de material científico educativo (1920-1936)'. *Llull, Revista de la Sociedad Española de Historia de las Ciencias y de las Técnicas* 43, no. 87 (12 October 2020): 181–200.

Hadot, Pierre. *The Veil of Isis: An Essay on the History of the Idea of Nature*. Translated by Michael Chase. Cambridge, MA: Harvard University Press, 2008.

Hall, John. 'Science Journals in a Prices Jungle'. *Nature* 247, no. 5441 (1 February 1974): 417–18. https://doi.org/10.1038/247417a0.

Hall, Stephen S. *Invisible Frontiers: The Race to Synthesize a Human Gene*. 1988/2002 Tempus Books of Microsoft Press, n.d.

'Have You Ever Thought of Going into Industry?' *Nature* 259, no. 5541 (29 January 1976): 257–257. https://doi.org/10.1038/259257a0.

'Henry Samuel Bedson | RCP Museum'. Accessed 1 October 2020. https://history.rcplondon.ac.uk/inspiring-physicians/henry-samuel-bedson.

Hilgartner, Stephen. 'The Dominant View of Popularization: Conceptual Problems, Political Uses': *Social Studies of Science*, 29 June 2016. https://doi.org/10.1177/030631290020003006.

'How Much Would Rothschild Cost?' *Nature* 235, no. 5332 (7 January 1972): 1–2. https://doi.org/10.1038/235001a0.

Industrial and Trade Fair Holdings. 'Together We Will Put Industry on the Map'. *The Birmingham Post*. 2 February 1976.

'Instruments: Another Technology Gap'. *Nature* 221, no. 5178 (25 January 1969): 300–300. https://doi.org/10.1038/221300b0.

Jackson, D. A., R. H. Symons, and P. Berg. 'Biochemical Method for Inserting New Genetic Information into DNA of Simian Virus 40: Circular SV40 DNA Molecules Containing Lambda Phage Genes and the Galactose Operon of Escherichia Coli'. *Proceedings of the National Academy of Sciences of the United States of America* 69, no. 10 (October 1972): 2904–9. https://doi.org/10.1073/pnas.69.10.2904.

Jacobs, Huistra, Noortje, Pieter. 'Funding Bodies and Late Modern Science'. *International Journal for History, Culture and Modernity* 7, no. 0 (2 November 2019). https://doi.org/10.18352/hcm.584.

Kay, Lily E. 'Problematizing Basic Research in Molecular Biology'. In *Private Science. Biotechnology and the Rise of the Molecular Sciences*, edited by Arnold Thackray, 20–38. Philadelphia: University of Pennsylvania Press, 1998.

Keller, Evelyn Fox. *The Century of the Gene*. Harvard University Press, 2009.

King, David. 'Market Research Reports, House Journals and Trade Literature'. *Aslib Proceedings* 34, no. 11 (November 1982): 466–72. https://doi.org/10.1108/eb050863.

King, Jonathan. 'New Diseases in New Niches'. *Nature* 276, no. 5683 (2 November 1978):

4–7. https://doi.org/10.1038/276004a0.

Kusch, Martin. 'Objectivity and Historiography'. *Isis* 100, no. 1 (March 2009): 127–31. https://doi.org/10.1086/597564.

———. 'Reflexivity, Relativism, Microhistory: Three Desiderata for Historical Epistemologies'. *Erkenntnis* 75, no. 3 (November 2011): 483–94. https://doi.org/10.1007/s10670-011-9336-5.

Labex International. 'Tomorrow's Technology'. *Nature* 254, no. 5496 (13 March 1975): ii.

Lacasse, Jeffrey R, and Jonathan Leo. 'Serotonin and Depression: A Disconnect between the Advertisements and the Scientific Literature'. *PLoS Medicine* 2, no. 12 (8 November 2005): e392. https://doi.org/10.1371/journal.pmed.0020392.

Lawrence, Eleanor. 'Science and Politics of Molecular Biology'. *Nature* 251, no. 5471 (13 September 1974): 94–94. https://doi.org/10.1038/251094a0.

Leigh, Jeffery. 'Proposal for a Constructive Response'. *Nature* 235, no. 5337 (11 February 1972): 303–303. https://doi.org/10.1038/235303a0.

Lewin, Benjamin. 'A Journal of Exciting Biology'. *Cell* 1, no. 1 (1 January 1974): 1. https://doi.org/10.1016/0092-8674(74)90147-0.

Lewontin, Richard. *Biology As Ideology: The Doctrine of DNA*. Harper Perennial, 1991.

Liddell, Henry George, and Robert Scott. 'Πολιτικός'. In *A Greek-English Lexicon*. Perseus Collection. Oxford: Clarendon Press, 1940, 1999 1843. http://www.perseus.tufts.edu/hopper/text?doc=Perseus:text:1999.04.0057:entry=politiko/s.

LKB Instruments. 'A Plug for Our Beta-Counter'. *Nature* 241/2 (1973).

———. 'Cut It Out!' *Nature* 235 (1972).

———. 'Think Small'. *Nature* 241/2 (1973): Back Cover.

Locke, J. H. 'An Open Reply from the Director of the Executive'. *Nature* 264, no. 5581 (4 November 1976): 3–3. https://doi.org/10.1038/264003a0.

Luckham Limited. 'Over and Over and Over and Over and Over Again'. *Nature* 256, no. 5517 (7 August 1975): Cover 2.

Maas, Ad. 'Johan Rudolph Thorbecke's Revenge: Objectivity and the Rise of the Dutch Nation State'. In *Epistemic Virtues in the Sciences and the Humanities*, edited by Jeroen van Dongen and Herman Paul, 1st ed., 321:173–93. Boston Studies in the Philosophy and History of Science. Springer International Publishing, 2017.

MacIntyre, Alasdair. 'Prologue: After Virtue after a Quarter of a Century'. In *After Virtue. A Study in Moral Theory*, Third Edition., ix–xvi. Notre Dame, Indiana: University of Notre Dame Press, 2007.

———. 'The Nature of Moral Disagreement Today and the Claims of Emotivism'. In *After Virtue. A Study in Moral Theory*, Third Edition., 6–22. Notre Dame, Indiana: University of Notre Dame Press, 2007.

Manuth, Volker, Dianna Beaufort, Jonathan Bikker, David de Witt, Jillian Harrold, Sandra Richards, Axel Rüger, and Jane Russel-Corbett. *Wisdom, Knowledge, Magic: The Image of the Scholar in Seventeenth-Century Dutch Art*. Kingston, Canada: Agnes Etherington Art Centre, Queen's University, 1997. https://archive.org/details/wisdomknowledgemagic1996.

Marantz. 'The Fire Started on the First Floor...' *Scientific American* 232, no. 1 (January 1975).

Meloni, Maurizio. *Political Biology. Science and Social Values in Human Heredity from Eugenics to Epigenetics*. 1st ed. London: Palgrave Macmillan, 2016. https://doi.org/10.1057/9781137377722.

Merton, Robert K. 'The Reward System of Science'. In *The Sociology of Science. Theoretical and Empirical Investigations*, 281–412. Chicago and London: The University of Chicago Press, 1973.

Miles Laboratories. 'If You Make Your Own DNA Ligase, I Can Save You a Lot of Time'. *Nature* 241/2 (1973).

———. 'Lectins'. *Nature* 252, no. 5478 (1 November 1974): Cover 4.

———. 'Miles Is Specific'. *Nature* 248, no. 5445 (15 March 1974): xi.

———. 'New from Miles. Ferritin. Cationized Ferritin. Antisera to Human Enzymes'. *Nature* 255, no. 5509 (12 June 1975): Cover 3.

———. 'New from Miles for Affinity Chromatography'. *Nature* 258, no. 5531 (13 November 1975): vi.

———. 'When You Need to Know Enzymes Miles Can Help!' *Nature* 248, no. 5446 (22 March 1974): v.

'Mini-Ads'. *Nature* 257, no. 5529 (30 October 1975): Cover 3.

'Mini-Ads'. *Nature* 274, no. 5674 (31 August 1978): Cover 3.

Morange, Michel. 'The One Gene-One Enzyme Hypothesis'. In *A History of Molecular Biology*, 21–29. Cambridge and London: Harvard University Press, 2000.

Morrison-Low, Alison D., Sara J. Schechner, and Paolo Brenni, eds. *How Scientific Instruments Have Changed Hands. How Scientific Instruments Have Changed Hands*. Vol. 5. Scientific Instruments and Collections 56. Leiden: Brill, 2016. https://brill.com/view/title/33653.

Morus, Iwan Rhys. 'Manufacturing Nature: Science, Technology and Victorian Consumer Culture'. *The British Journal for the History of Science* 29, no. 4 (1996): 403–34.

Moser, Benjamin. *Sontag: Her Life and Work*. Ecco, 2019.

Murray, Noreen E., and Kenneth Murray. 'Manipulation of Restriction Targets in Phage λ to Form Receptor Chromosomes for DNA Fragments'. *Nature* 251, no. 5475 (11 October 1974): 476–81. https://doi.org/10.1038/251476a0.

Nall, Joshua, and Liba Taub. 'Selling by the Book: British Scientific Trade Literature after 1800'. In *How Scientific Instruments Have Changed Hands*, edited by Alison D.

Morrison-Low, Sara J. Schechner, and Paolo Brenni, 5:21–42. Scientific Instruments and Collections 56. Leiden: Brill, 2016.

'NAS Ban on Plasmid Engineering'. *Nature* 250, no. 5463 (19 July 1974): 175–175. https://doi.org/10.1038/250175a0.

'Nature - Revised Rates'. London, 1961. ARC3/M052/Nature/Advertising/1961. Nature Archive, The Archive of Macmillan Publishers, Basingstoke.

'Newly on the Market'. *Nature* 270, no. 5632 (November 1977): 87–88. https://doi.org/10.1038/270087a0.

'News in Brief'. *Nature* 276, no. 5688 (1 December 1978): 553–553. https://doi.org/10.1038/276553a0.

Norman, Colin. 'Berg Conference Favours Use of Weak Strains'. *Nature* 254, no. 5495 (1 March 1975): 6–7. https://doi.org/10.1038/254006a0.

———. 'Genetic Manipulation: Recommendations Drafted'. *Nature* 258, no. 5536 (18 December 1975): 561–64. https://doi.org/10.1038/258561a0.

———. 'NIH Backing for NAS Ban'. *Nature* 250, no. 5464 (26 July 1974): 278–278. https://doi.org/10.1038/250278a0.

Oertling+Cahn. 'The New Electronic Weighing Family'. *Nature* 252 (1974): ii–iii.

Olympus. 'The Microscope That Works for You in Multiple Ways'. *Nature* 235, no. 5334 (21 January 1972): Cover 2.

Our Special Correspondent. 'Shadow of Rothschild over Strathclyde'. *Nature* 235, no. 5333 (14 January 1972): 71–73. https://doi.org/10.1038/235071a0.

Perkin-Elmer. 'Food Analysis. 60 Secs or 60 Minutes. The Choice Is Yours'. *Nature* 248, no. 5444 (8 March 1974): ii.

Pharmacia Fine Chemicals. '—————SH!!' *Nature* 258, no. 5531 (13 November 1975): Cover 3.

———. 'UUUUUUUUUUUAAAAAAAAAAA'. *Nature* 258, no. 5537 (25 December 1975): Cover 2.

Philips. 'If You Need a New Microscope - Why Purchase an Old One?' *Nature* 258, no. 5532 (20 November 1975): vi–viii.

———. 'Seeing Is Believing'. *Nature* 282, no. 5737 (22 November 1979): iv–v.

P-L Biochemicals. 'Let Us Give You a Complement: A Copy of Our New Catalog'. *Nature* 254, no. 5496 (13 March 1975): v.

———. 'Reagents for the Unambiguous Assay of REVERSE TRANSCRITPASE'. *Nature* 258, no. 5531 (13 November 1975): viii.

'Plenty for GMAG to Do'. *Nature* 276, no. 5683 (2 November 1978): 1–1. https://doi.org/10.1038/276001a0.

'Problem-Solving PhDs'. *Nature* 249, no. 5457 (7 June 1974): 501–501. https://doi.org/10.1038/249501a0.

Qureshi, Sadiah. 'Meeting the Zulus: Displayed Peoples and the Shows of London, 1853-

79'. In *Popular Exhibitions, Science and Showmanship, 1840-1910*, 183–98. Pittsburgh: University of Pittsburgh Press, 2016.

'Rates'. London, 1965. ARC3/M052/Nature/Advertising/1965. Nature Archive, The Archive of Macmillan Publishers, Basingstoke.

Recombinant DNA Molecule Program Advisory Committee. 'Agenda'. National Institutes of Health, Division of Research Grants, 28 February 1975. https://osp.od.nih.gov/wp-content/uploads/RAC_Agenda_Feb_1975.pdf.

'Report of the Investigation into the Cause of the 1978 Birmingham Smallpox Occurence'. London, Her Majesty's Stationery Office: House of Commons, 22 July 1980.

'Report of the Working Party on the Experimental Manipulation of the Genetic Composition of Micro-Organisms (Cmnd. 5880)'. London: State of Education and Science, House of Commons Parliamentary Papers, January 1975.

'Retraction Watch'. Accessed 25 November 2020. https://retractionwatch.com/.

Reynolds, Andrew S. 'Cell Signaling: The Cell as Electronic Computer'. In *The Third Lens: Metaphor and the Creation of Modern Cell Biology*, 114–45. Chicago and London: The University of Chicago Press, 2018.

Rheinberger, Hans-Jörg, and Staffan Müller-Wille. *The Gene. From Genetics to Postgenomics*. Translated by Adam Bostanci. Chicago and London: The University of Chicago Press, 2018.

Rich, Vera. 'Coping with an Exodus'. *Nature* 253, no. 5490 (1 January 1975): 296–97. https://doi.org/10.1038/253296a0.

———. 'Russia Today'. *Nature* 254, no. 5497 (1 March 1975): 173–74. https://doi.org/10.1038/254173a0.

Rothschild, Victor. 'The Organisation and Management of Government R&D'. In *A Framework for Government Research and Development*. HMSO London, 1971.

Sanders, Alysoun. 'Email Conversation with Alysoun Sanders, Archivist for Macmillan Publishers and Springer Nature', n.d. 23 April - 21 May 2020.

Schwartz, Joe. 'UK Radical Scientists Ten Years On'. *Nature* 282, no. 5738 (1 November 1979): 434–434. https://doi.org/10.1038/282434b0.

'Science and Public Pleasure'. *Nature* 248, no. 5446 (22 March 1974): 269–269. https://doi.org/10.1038/248269a0.

Scott, Anthony Oliver. 'How Susan Sontag Taught Me to Think'. *The New York Times*, 8 October 2019. https://www.nytimes.com/interactive/2019/10/08/magazine/susan-sontag.html.

'Self Evident— an Explanation by Lord Rothschild'. *Nature* 235, no. 5337 (11 February 1972): 296–296. https://doi.org/10.1038/235296b0.

Shapin, Steven. *The Scientific Life. A Moral History of a Late Modern Vocation*. Chicago:

University of Chicago Press, 2008.

———. *The Scientific Life. A Moral History of a Late Modern Vocation.* 1st ed. Chicago and London: The University of Chicago Press, 2008.

'Should We Publicise Those Experiments?' *Nature* 251, no. 5470 (6 September 1974): 1–1. https://doi.org/10.1038/251001a0.

'Smoke But No Fire So Far'. *Nature* 234, no. 5327 (3 December 1971): 239–40. https://doi.org/10.1038/234239a0.

'Social Irresponsibility in Science'. *Nature* 229, no. 5286 (19 February 1971): 513–513. https://doi.org/10.1038/229513a0.

Solovey, Mark. 'The Impossible Dream: Scientism as Strategy against Distrust of Social Science at the U.S. National Science Foundation, 1945–1980'. *International Journal for History, Culture And Modernity* 7, no. 0 (2 November 2019): 209–38. https://doi.org/10.18352/hcm.554.

Sontag, Susan. 'Against Interpretation'. In *Against Interpretation and Other Essays*, 3–14. Penguin Classics. London: Penguin Books, 2009.

———. 'On Style'. In *Against Interpretation and Other Essays*, 15–36. Penguin Classics. London: Penguin Books, 2009.

Stark, Laura. 'Funding Panels as Declarative Bodies: Meritocracy and Paradoxes of Decision-Making in Modern Science'. *International Journal for History, Culture and Modernity* 7, no. 0 (2 November 2019). https://doi.org/10.18352/hcm.593.

Stoker, Michael. 'Molecular Dirty Tricks Ban'. *Nature* 250, no. 5464 (26 July 1974): 278–278. https://doi.org/10.1038/250278c0.

T.G Scott & Son Ltd. 'Order from Nature'. *Nature* 249 (5452): xiv.

'That Was the Debate, That Was'. *Nature* 235, no. 5337 (11 February 1972): 293–95. https://doi.org/10.1038/235293a0.

'The Biologist's Dilemmas'. *Nature* 228, no. 5275 (5 December 1970): 900–901. https://doi.org/10.1038/228900a0.

'The British Association at Stirling'. *Nature* 251, no. 5471 (13 September 1974): 90–90. https://doi.org/10.1038/251090a0.

The Radiochemical Centre Amersham. 'Don't Take a Chance on Labelled Compounds'. *Nature* 258, no. 5530 (6 November 1975): xxi.

———. 'How to Make the Most out of Your Radiochemicals Budget'. *Nature* 235, no. 5337 (11 February 1972): Cover 2.

———. 'The Radiochemical Centres'. *Nature* 251, no. 5472 (20 September 1974): vi–viii.

———. 'We're Old Hands at New Labelled Compounds'. *Nature* 258, no. 5532 (20 November 1975): Cover 2.

'The Technology Gap'. *Nature* 213, no. 5071 (7 January 1967): 1–1. https://doi.org/10.1038/213001a0.

'Time for a Policy on Scientists' Jobs'. *Nature* 282, no. 5734 (1 November 1979): 1–1.

https://doi.org/10.1038/282001a0.

'University Research in Danger'. *Nature* 247, no. 5440 (8 February 1974): 325–325. https://doi.org/10.1038/247325a0.

Viglione, Giuliana. 'Are Women Publishing Less during the Pandemic? Here's What the Data Say'. *Nature* 581, no. 7809 (20 May 2020): 365–66. https://doi.org/10.1038/d41586-020-01294-9.

Villanueva, Pilar, Salvador Peiró, Julián Librero, and Inmaculada Pereiró. 'Accuracy of Pharmaceutical Advertisements in Medical Journals'. *The Lancet* 361, no. 9351 (January 2003): 27–32. https://doi.org/10.1016/S0140-6736(03)12118-6.

Wang Electronics. 'The Natural Selection'. *Nature* 258, no. 5531 (13 November 1975): iv.

Wang, Jessica. '"Broken Symmetry": Physics, Aesthetics, and Moral Virtue in Nuclear Age America'. In *Epistemic Virtues in the Sciences and the Humanities*, edited by Jeroen van Dongen and Herman Paul, 1st ed., 321:27–47. Boston Studies in the Philosophy and History of Science. Springer International Publishing, 2017.

Waters Associated Inst. Ltd. 'NEW Required Reading'. *Nature* 258, no. 5531 (13 November 1975): v.

Wild Heerbrugg. 'New Concept in Microscopy'. *Nature* 254, no. 5501 (17 April 1975): Cover 2.

Working Party on Science and Ethics, British Society for Social Responsibility in Science. 'The Social Obligations of The Scientist'. London, 1972. K/PP178/11/1/22. King's College London Archives (Wellcome Library). https://wellcomelibrary.org/item/b20050045.

Wright, Susan. 'Molecular Biology or Molecular Politics? The Production of Scientific Consensus on the Hazards of Recombinant DNA Technology'. *Social Studies of Science* 16 (1986): 593–620.

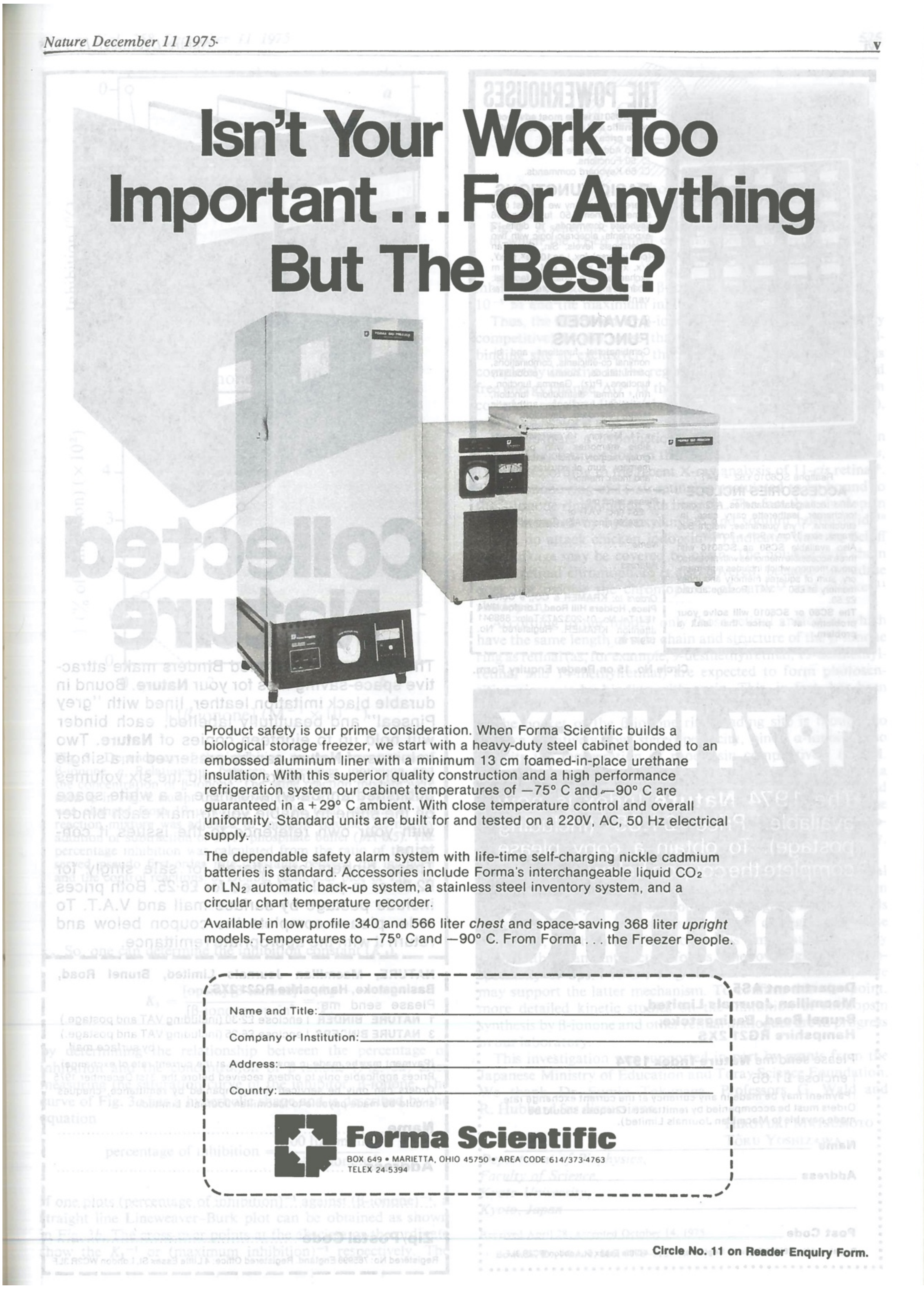
Wright, Susan. 'Molecular Politics in a Global Economy'. In *Private Science. Biotechnology and the Rise of the Molecular Sciences*, edited by Arnold Thackray, 80–104. Philadelphia: University of Pennsylvania Press, 1998.

Wright, Susan. *Molecular Poltics. Developing American and British Regulatory Policy for Genetic Engineering, 1972-1982*. Chicago and London: The University of Chicago Press, 1994.

———. 'Recombinant DNA Technology and Its Social Transformation, 1972-1982'. *Osiris* 2 (1986): 303–60.

Appendices.

Appendix A.


Nature December 11 1975
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Appendix C.

Reagents for the unambiguous assay of REVERSE TRANSCRIPTASE

Reverse transcriptase can be distinguished from cellular DNA polymerases by using appropriate combinations of the following complexes as template primers:

- POLYRIBOADENYLIC ACID • OLIGODEOXYTHYMIDYLIC ACID
- POLYDEOXYADENYLIC ACID • OLIGODEOXYTHYMIDYLIC ACID
- POLYRIBOCYTIDYLIC ACID • OLIGODEOXYGUANYLIC ACID
- POLYDEOXYCYTIDYLIC ACID • OLIGODEOXYGUANYLIC ACID

The most important ones currently being used extensively in screening programs for reverse transcriptase are listed below:

		5 Units	25 Units
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7850	POLY(dA)·d(pT)$_{10}$	38.00	150.00
7878	POLY(rA)·d(pT)$_{12\text{-}18}$	30.00	125.00
7868	POLY(dA)·d(pT)$_{12\text{-}18}$	38.00	150.00
7944	POLY(rC)·d(pG)$_{12\text{-}18}$	40.00	160.00
7943	POLY(dC)·d(pG)$_{12\text{-}18}$	50.00	200.00
7795	POLY(rCm)·d(pG)$_{12\text{-}18}$	80.00	

For an example of assay conditions used for the determination of cellular DNA polymerases and reverse transcriptase see: *Science*, **183,** 867 - 869 (1974).

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Cholic acid [^{3}H(G)]	NET-315
Deoxycholic acid [^{3}H(G)]	NET-454
Glycochenodeoxycholic acid [^{3}H(G)]	NET-481
Glycocholic acid [glycine-1-^{14}C]	NEC-620
Glycocholic acid [cholic-^{3}H(G)]	NET-340
Glycodeoxycholic acid [deoxycholic-^{3}H(G)]	NET-487
Glycolithocholic acid [lithocholic-^{3}H(G)]	NET-482
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S.R.I.5	Solari-Albedometer.
S.R.I.6	Reversing Albedometer.
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Appendix B.


Nature November 6 1975
xxi
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