# Proving What?

## On the Structure of Proof of Concept Research

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# Abstract

In his *Representing and Intervening*, Hacking (1983) argues that, contrary to common belief, many phenomena which the natural sciences investigate do not exist as such in nature. Rather, they only exist in highly specific, man-made experimental setups, and are as a result created by us themselves. Kroes (2014) has nuanced this by making a distinction between weak and strong creation: The phenomena Hacking talks about are weakly created, because they are not constructed according to an intelligent design. Artefacts like screwdrivers, bikes and scientific instruments are strongly created, because they are based on an intelligent design. While this latter category of objects usually lies outside of the realm of scientific investigation, one can find examples of research in which a seemingly strongly created object is created and investigated. This begs the question of the epistemic value of this type of research: Does the creation of these objects have an effect on scientific theory, for example? If it does not, what is the point of creating such an object *for science*? In this master thesis, I attempt to answer these questions by taking "proof of concept research" as a model for this kind of research. Building on previous work on the subject by Kendig (2016) and Elliott (2021), I introduce a framework to identify the relevant entities involved, as well as a relation between them. Using this framework, I argue that the products of proof of concept research are *both strongly and weakly created.* The epistemic products I identify reflect this duality: On the one hand techniques are developed which are relevant for the creation of artefacts. On the other hand the created object can act as a genuine object of investigation, and theoretical knowledge can be generated accordingly.

# Contents

Abstract

# 1 Introduction

Imagine the well-known story of Newton walking through a garden, and, after seeing an apple fall from a tree, being inspired to develop his theory of gravitation. This story must have made quite the impression on how people believed scientific theorising worked at the time. In fact, it is still a telling story today: In its simplicity, it tells us how the inductivist approach to science involved singular observations which naturally extended into general theories.[1] And while a lot has happened, both within the practices of scientists, as well as in the epistemologies of the philosophers (and sociologists) observing them, science's *raison d'être* is still mainly the generation of knowledge through theorising and modelling practices. This despite the fact that the messiness of scientific practice has been highlighted during the "practice turn" in the 1970s by philosophers and sociologists of science such as Latour and Woolgar, Hacking, and Lakatos, the main *product* of science is the same throughout all of those contexts: It is theoretical knowledge.

Knowledge about what? Although all of the authors mentioned above have discussed the problem of the objects of science (for example, their embeddedness in social contexts), none have tackled the problem as directly as Ian Hacking. In his *Representing and Intervening*, he argues that many of the phenomena which are being observed within the natural sciences (physics, chemistry, and biology) are far from the natural objects we take them for. Instead they are, in fact, man-made themselves.[2] This is due to the fact that the phenomena which scientists observe would never occur in the same isolated way outside of the laboratory. Nature is complex: Its dynamics have to be untangled for us to make any sense of it. This is, according to Hacking, what happens in the laboratory: Through the development of a scientific setup a phenomenon is "created," which can then be observed and can function in scientific theories. This is not to say that our scientific theories are epistemically less valuable, or that the objects of science are "made up." Quite the contrary: For Hacking, the fact that we can make and manipulate phenomena is an argument for realism about those entities. Thus our theories really are about something out there, but that something just does not show itself naturally. Rather, it is knowledge about objects and phenomena which we can recreate in the laboratory and, if we have enough control over them, in technological objects more generally. Thus, the creation of phenomena can be seen as a "nudging" of nature in the right direction: The scientist actively creates only the boundary conditions which are necessary for the phenomenon in question to show itself. To "create" the high-energy particles at the Large Hadron Collider in Geneva, what is actually being built are things like electromagnets, superconductor circuits, and vacuum pumps. Nobody actually constructs the particles which are being accelerated. Peter Kroes has called this type of creation weak creation, as opposed to strong creation in which the object in question is constructed according to intelligent design.[3] An example of the latter

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[1] (Ladyman 2012, ch.1)
[2] (Hacking 1983)
[3] (Kroes 2014)

which Kroes gives is a bicycle: In that case the object in question consists of steel or aluminium in different shapes, put together in such a way so as to make the bicycle "work." Moreover, a bicycle does not magically appear as long as we satisfy certain boundary conditions. Rather, every part needs to be intentionally placed in its right position.

Intuitively, the distinction seems to be between "natural objects" and "artificial objects." The particles in the Large Hadron Collider seem to be naturally occurring. Sure, we have to nudge nature in the right direction, but if we do that, they are just there, and it feels like they always have been there, whether we would have ever satisfied the boundary conditions or not. So if we refer to them in our scientific theories, we are referring to objects which make up the stuff which make the world work: Electrons have always revolved around collections of nucleons, making up the atoms as we know them. And DNA has played a major role in natural selection long before we developed the techniques to isolate and identify it. How different does our knowledge about bicycles seem! In fact, they seem so dependent on our intentions that they can scarcely function in the theories present in the natural sciences. And while there is a great deal to know about the workings of bicycles, perhaps our intuition is right in that they might not realistically function in any scientific processes. That does not mean that no object which owes its existence to strong creation can ever function in scientific processes. In fact, I will argue that there are objects like these which do, indeed, have a place in science. But since these objects are based on intelligent design, their behaviour is intentional. Naturally, this has consequences for what kind of knowledge is being created in any investigation on them, and what kind of influence these objects have on a theory. For now, however, it might be good to give an example of what such a strongly created object in science might look like.

### Induced pluripotent stem cells

In 2012, Shinya Yamanaka received the Nobel price for medicine for his work on the creation of induced pluripotent stem cells (iPS).[4] These cells were remarkable in that they were taken from really any part of the body, where they would have a very specified task and morphology, and that they could nonetheless be prompted to revert back into pluripotent stem cells. Pluripotency of stem cells is useful in medicine, since it means that those cells can develop into any type of tissue; so, for example, a stem cell might be developed into heart tissue to study a patient's heart disease. Or, for example, it might be developed into liver tissue, to study the effect of a certain medicine on a patient's liver. Thus, the technique for creating such induced stem cells was (and is) extremely important for medicine; hence the Nobel prize. It also took a lot of hard scientific work. As Yamanaka explained in his Nobel lecture, the research went, by all standards, exceptionally well[5]. Nonetheless, a lot of scientific work had to be performed in order to develop the techniques

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[4] See Yamanaka (2013).

[5] "The generation of iPS cells was an exceptional experience in my scientific career, in that everything went smoothly. In all other cases, my career has been full of failures." (Yamanaka 2013, 13905)

involved; for example, Yamanaka's group, and other research groups in the field, had to identify what genetic factors were important in creating a specified cell, something which one does not figure out easily due to the enormous amount of possible factors.

But after all, in 2006, Yamanaka managed to create iPS cells from mouse tissue, and in 2007 from human tissue. This was the focus of his research from the very beginning, and it is the result which got him the Nobel prize; his research was so impressive because it resulted in an object with novel and useful properties. The motivation behind his research was some previous work on creating pluripotent stem cells. People had managed to create stem cells through other techniques, but these were either inefficient or impractical for medical use[6]. Thus, the knowledge that generating induced pluripotent stem cells was possible in theory acted as the main motivator for doing the research: it was theoretically possible, now it just had to be done in practice.

As we can see, the motivating force behind this study is not the generation of a theoretical framework. Rather, it is simply the construction of an object. Yamanaka and his team constructed the object based off of an idea about pluripotency in somatic cells. The idea for this object arises within the theoretical context of a field of study, in this case cellular biology. The study was successful due to the fact that Yamanaka's team managed to generate iPS cells, and nothing more. Thus, apparently, the creation of objects is seen as genuine scientific progress. Such a statement does not need to be surprising: It is widely accepted that the development of scientific instruments is *also* a driving force in science. Take the Large Hadron Collider: Its existence represents a major technological feat first of all, but second of all the a big step for science in its potential to push the cutting edge of particle science forward. But while instruments allow us to observe phenomena external to them, this is not the case in research like Yamanaka's: After all, the iPS cells exised for their own sake, not to measure any other phenomenon.

How are we to understand this process within science, in which this kind of "strong creation" plays a major role? If it does not result more theoretical knowledge, then what role does it play in scientific contexts? When is it successful, and what counts as failure? And is there a specific structure we can identify in this kind of research?

With regard to the last question, we can: The strategy taken by Yamanaka can more generally be classified as *proof of concept (POC) research.*[7] This type of research is not new: As we will see, the term "proof of concept" has first been introduced more than 50 years ago. However, philosophical interest in proof of concept research has only really taken up in the last few decades, and still it is a small field: Only a handful of authors have really taken the subject seriously.

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[6] (Yamanaka 2013, 13903–4)

[7] Let me make this clear from the start: Yamanaka did not characterise his research as proof of concept, while he did characterise the works of others as such (Yamanaka 2013, 13907–8). The same goes for many other examples I use in this work. I believe I am justified in characterising my examples as POC research, because I employ more or less explicit requirements for something to be POC research. The term "proof of concept" is one laden with connotations from its usage in varying sectors, however. Some might therefore disagree with the specific definition of proof of concept which I employ and as a result with some of my examples. The plurality of activities referred to as proof of concept makes it impossible to cover all uses by a single definition: However, I believe having a strict definition reduces confusion surrounding the term, and allows effective usage of the term.

Examples which we will discuss later are the works by Catherine Kendig and a group led by Maria Servedio, as well as a courageous attempt to synthesise previous research by Steve Elliott.[8] While these previous authors have made important contributions to this small field, every one of their accounts has some shortcomings: For example, while Kendig identifies some important aspects of POC research, such as her notion of "projectibility" or the formation of new "epistemic categories," her account does not have the rigidity to *explain* why these aspects arise from this kind of research. The same can be said for Elliott's account, which leaves too much room for research field-specific notions of novelty for us to consistently identify research *as* POC research. Moreover, a major distinction between Elliott's account and mine is that Elliott also accepts a notion of nonmaterial POC research. This is due to his indebtedness to Servedio *et al.* and other authors in the field, who all accept POC as a term also applicable to a type of practice in model building. I will disagree with this standpoint later on; At this point, let me just say that, if POC is to be a model for an approach in science at all, I believe we need some specificity in *defining* POC research. After all, if we take POC to be a term referring to many idiosyncratic practices, both within and outside of scientific research, then we lose our grasp on what it could mean to us as philosophers of science. And since I will argue that POC research, when restricted to merely material research, can serve as a model of research which mainly *creates* an object generally, I believe this is the best way to understand POC research.

In any case, POC research is best understood in material contexts (after all, it is in these contexts that POC was first introduced). As such, I will first develop a framework of *material* POC research. Once I will have established that, we will have a solid foundation to consider the possibility of non-material POC research. Once we have established the range on which POC research operates, we can identify the roles this kind of research can play in the broader epistemic context of theory building. To this end, I will employ the following structure: I will first introduce POC research as a type of research which shows feasibility of not-yet-existing objects. Since this target system does not occur naturally, it needs to be created in order to be able to test the theory at all. Moreover, I will argue that this target system is necessarily an object, and that the mental analogue of an object is a concept. As such, we need to start our framework of POC research–a type of research which starts at the mental and ends in the concrete–in the mental world of concepts. To this end, I will discuss what a concept is, and how it arises within a scientific theory. As we will see, all of the research follows from this concept: Thus, the main practical processes of POC research involve the creation of an instance of this concept. This I will discuss later. Interestingly, none of the authors who have written about POC research before have done full justice to either the mental objects which form the basis of this kind of research, nor the process of actually manufacturing these instances. The final part of my framework will have to do with establishing a relationship between the concept and its instance. This is important, because it is the part of the research

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[8] (Kendig 2016), (Servedio et al. 2014), (Elliott 2021)

which is actually being presented on conferences and in research papers.

## 2 The "point" of POC research

Proof of concept is a term used in many fields, and its meaning varies wildly between those fields. Moreover, many researchers and engineers use the term almost idiosyncratically, and depend on their audience to understand the correct meaning of the term. This makes giving a complete definition which encompasses all of the different use-cases of the term challenging at best, and impossible at worst. This has not deterred some to at least provide an open-ended definition, or otherwise a framework to characterise what proof of concept is all about. Notable characterisations have been done by NASA, who introduced the term in 1967 (and obviously was obliged to provide some kind of definition), and more recently Steve Elliott, who has developed a framework of proof of concept research in an ambitious attempt to synthesise the various strands of literature on the subject. As we will see, I will take a different approach in the development of my own framework of POC research. Namely, I will develop an account which is more limited in scope, but by virtue of this more explanatory of that which it does describe. In what ways my framework will differ from Elliott's will become clear over the course of the next few sections. For now, however, I want to provide an initial "sketch" of proof of concept. Such a sketch will lay the groundwork for the later sections of this work, and will unearth some of the terms which we will be seeing a lot in the following sections. Moreover, it will hopefully provide you, my reader, with an initial understanding what proof of concept is all about. Finally, this sketch will help me to set the scope of this work: What counts as proof of concept? What is the difference between proof of concept in engineering settings and science settings? In other words, what is proof of concept *research*? What contexts are we looking at? What contexts are excluded?

This sketch will consist of a number of characteristics of proof of concept research. I will thus not provide a complete definition, but I will identify the aspects which make some approach a proof of concept approach. Since I have claimed that proof of concept differs importantly from conventional research, I will explain what aspects make it so different. As we will see, the scope I ascribe to POC research is more limited than those of other authors who have written about the subject. The reasons for this will be clear after I have developed my characterisation. To start with, however, I will begin defining POC as broadly as possible, in an attempt to cover most of its use-cases. From there, I will limit my characterisation more and more, in order to get to a working characterisation to build my framework on.

First of all, I see POC as consisting of two parts: It is a creative process on the one hand, and a mode of analysis on the other. That which is being created at the earlier half of the POC process will be analysed in the latter half. As a mode of analysis, it thus analyses that which it has created itself. The nature of that which is being created and analysed (e.g. if the object can only be a material object, such as a biological structure, or can also be non-material, such as a computer program) will become clear at a later stage. For now I want to keep things broad, in order to be able to include additions from the other authors who have worked on this topic. Although we are leaving the object which is being created mostly undefined, it should have a name for us to refer to it. I will call the created object the *prototype*, for a number of reasons: The term is neutral with regard to the nature of the object, and it stresses both the novelty and the artificiality of the object. Moreover, it is a term which is also used by Elliott. And although our approaches differ importantly from one another, we nonetheless have a similar understanding of the general role of the prototype, whatever it might specifically consist of.

## 2.1 Feasibility

In its broadest sense, the analytical part of proof of concept consists of an analysis of feasibility of a method for creating a not-yet-existing prototype. This latter term is hard to define itself, but let me still try to do so here. Feasibility entails that an approach leads to a prototype which meets all of the requirements we ask of it. Among these requirements we find, most importantly, *possibility*: That is, since POC deals with objects which do not exist yet, the main function is to prove that it can exist. Or the possibility to use an existing object in a novel way. But it goes further than that: Rather than merely possibility, feasibility might also include certain values we ask of our prototype. A certain method might be feasible not only when it is possible, but also when it is economically viable, in the sense that implementing the method is not too costly. Or whether the prototype is easy or safe to use; Generally any requirement can become a condition for feasibility. When the term "proof of concept" was introduced by NASA in 1967, the main goal of this approach was to establish the feasibility of implementations of ideas generated in a laboratory environment:

> Proof of concept is a technology demonstration phase in order that the feasibility of certain research concepts may be explored in a real-world situation as opposed to the laboratory or wind tunnel environment in which the concepts were initially developed.[9]

In this setting, the test of feasibility was intimately connected to the development of new technologies: Thus proof of concept was later included in a broader framework of technology development as a technology readiness level (TRL). Each of these TRLs, which are still used today, denote a step in a development process which starts at TRL 1 ("basic principles observed and reported" and ends at TRL 9 ("actual system 'flight proven' through successful mission operations").[10] Thus, for NASA, feasibility includes any property which is necessary for the

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[9] See (United States. Congress. Senate. Committee on Aeronautical and Space Sciences. 1967, 163). Although this quote implies that the approach checked the feasibility of the "research concepts" (i.e. the initial ideas from which the implementations were developed), in reality the considerations were often costs, safety of use, and ease of use (see also (United States House of Representatives 1969)). Since these considerations do not apply to abstract ideas, I conclude that it is the feasibility of the *implementation*, rather than the *concept*, which is being tested for.

[10] See (Mankins 1995) and (Olechowski, Eppinger, and Joglekar 2015)

successful development of a technology. What these properties are is, of course, dependent on the context of any particular technology.

We can recover these connotations of technology development in the goals of "proof of concept centres" which have become part of more and more university organisational structures over the past few decades. These centres are facilities which are spatially and organisationally close to universities, where research findings from those universities are being developed to the extent that it might lead to a viable business plan, and consequently to more revenue for the university[11]. In these settings, too, the point of POC is to bridge the gap between scientific research and marketable technologies[12]. Since the findings of scientific research often requires a substantial investment to be developed up to the point where the technology becomes profitable, the university can provide this investment. In return the university gets a share in the company producing the technology, and thus shares in the returns.[13] As such, the requirements of prototypes in these contexts are related to their economic viability and their marketability. It represents a move *away* from the laboratory setting, and into engineering contexts.

This does not mean that POC always refers to a move away from research. It can just as well be attempted in a scientific setting. In that case, the focus is on *epistemic* requirements, an important one of which is that of *scientific novelty.* To be scientifically interesting, the prototype needs to, through its behaviour, "tell us something new" about our theories. For example, one might create a new alloy with properties unlike those of any other alloys known to man, but as long as the creative process or existence of that alloy does not challenge or confirm existing metallurgic theories in any significant way, that alloy and its creation will not be scientifically interesting. The exact meaning or requirements of novelty itself is quite hard to pin down; Elliott has contented himself with the statement that researchers themselves determine novelty according to the norms of their discipline. This does not help us much in the sense of distinguishing *scientific* novelty from novelty in engineering settings. For a certain approach or object might be novel for a certain field of engineering; Say, for example, that the alloy that was created allows for the creation of metal joints with exceptional properties, but due to those same properties they need to be processed and applied in a specific way. This would provide a lot of new and interesting challenges, and can thus be said to be novel to the engineer. However, as long as there already is a scientific explanation for the alloy's behaviour, it will not be novel to the scientist. Thus, if we are judge when a POC approach is scientifically interesting (i.e. when it is POC *research*), we need a more extensive account of novelty. In my own framework, I will try to develop such an explanatory account of novelty in POC research. For now, however, let us take novelty as something which can be recognised by researchers

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[11] See Maia and Claro (2013) and Bradley, Hayter, and Link (2013)

[12] See Gulbranson and Audretsch (2008)

[13] This development is interesting in its own right, and I believe it can be tied to broader developments in the organisational structures of universities in the past decades. For example, Nash (2019) speaks of the "marketisation" of universities, and the prominent role the economic contribution of universities plays in the contemporary policy making.

involved in POC research, and that it is an important requirement for POC to be scientifically relevant. One might be able to identify more specifically epistemic requirements besides novelty, but I will leave it at this for now. What should be clear, however, is that POC does not necessarily refer to an approach in scientific research, and that it are the requirements which prototypes are held to which make it into scientific research. As I will develop my account in more detail, and set up my framework this distinction will become clearer. For now, however, let us take a look at the notions of feasibility we find in the works of other authors who have written about POC research.

### 2.1.1 Feasibility in the literature on POC research

Owing partly to the many-sidedness of the term "feasibility," we can recover this meaning in most of the literature on POC research. For example, we can find related notions in the work of Catherine Kendig, who has characterised POC research within the field of synthetic biology.[14] For example, she characterises POC research as a *proof of possibility*.[15] To her, POC research shows that "the causal connection hypothesized, structure proposed, function suggested, or methodological approach taken in the research obtains in [experimental practice]."[16] Thus, for Kendig, proof of concept shows that the approach meets the most basic requirement: That the prototype exists. Elliott pays homage to Kendig in his general framework of POC research, but takes it a step further: He claims that POC research shows that "prototypes achieve functions or results desired of them," in which the prototypes here are the objects of analysis.[17] This is, of course, completely in line with what I have said about POC research: The "functions or results" we desire can be reworded into the requirements we set on our object of analysis. These requirements can involve mere existence as in Kendig, but also ease of use, or cost-effectiveness. However (and here we get a bit more specific), Elliott makes a distinction between "physical prototypes" and "prototype theories and models." He identifies a notable difference between the two, namely, that theories and models are mainly tested for their epistemic functions or aims.[18] This view on proof of concept in non-material contexts, specifically in the context of model building, can be traced back to some authors which Elliott references. Most notably, he refers to a research group led by Maria Servedio, who have written about proof of concept in model building practices in the field of evolutionary biology.[19] Other important authors are Anya Plutynski and Axel Gelfert, who each have also made a similar point: The models which are created in POC processes are tested for their epistemic usefulness mainly.[20] Now this does not have to be too surprising. After all, most of the requirements we apply to scientific models (and in any case the most important requirements) will be epistemic:

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[14] (Kendig 2016, 740)

[15] (Kendig 2016)

[16] (Kendig 2016, 737)

[17] (Elliott 2021, 264)

[18] (Elliott 2021, 264). It should be noted that Elliott himself does not put much emphasis on this difference.

[19] See (Servedio et al. 2014)

[20] See (Plutynski 2006) and (Gelfert 2018). It should be noted that both of these authors speak of "proof of principle" rather than proof of concept. However, their accounts are largely consistent with those of Servedio *et al.* and Elliott. Thus I am approaching their work as an addition to the body of work on proof of concept research.

A scientific model which does not correctly represent nature, and cannot be used to articulate reasonable expectations of nature, can hardly be used as a scientific model at all. Thus the notion of feasibility can perfectly well be applied to cases of model building. And since model building also consists of a creative process (after all, a model is logically or mathematically constructed), there are quite some analogies which can be drawn between the proof of concept approach as introduced by NASA, and the process which is described by Elliott, Plutynski and Gelfert. Thus, despite their many differences, it is not too surprising that Elliott has let his framework cover both physical prototypes and prototype theories and models. However, we will see in the next section, when I dive deeper into the nature of these prototypes, we will see that this synthesis does more harm than good. The reason for this are some important epistemic differences between the physical POC research as described by NASA and Kendig, and the POC research into models and theories as described by Elliott, Plutynski, and Gelfert.

## 2.2 Justification

The focus of POC is, however, not only on the analysis of feasibility. For rather than just determining whether a method yields a prototype with certain properties, POC also provides justification for the idea which forms the basis for the creation of the prototype. This idea is a *concept.* I will go more in-depth into what those concepts are and where they come from in my framework, but at this point, let us understand by concept *any mental representation of a potential prototype.* That is, if one is to create a prototype, one has to start with a concept of that prototype, before working out a method to create the prototype itself. In return, if the created prototype possesses the properties desired of it, then it justifies holding on to the concept of that prototype. This might seem obvious, but it is an important nuance of the meaning of the term proof of concept. To make this point more clear, let us go back to NASA's original definition: It states that POC makes it so that "the feasibility of certain research concepts may be explored in a real-world situation as opposed to the laboratory." We thus have two contexts: The real world, and the laboratory. If feasibility is shown, then that means that the "certain research concepts" are applicable in the real world. What it also means, is that those research concepts are relevant not only in the laboratory, but also in the field. This, in turn, would also provide an impetus to further develop those "concepts" in the laboratory context, because they are, after all, applicable in the field.

Kendig discusses justification in POC research in terms of the notion of *projectability.* She explains it as the assumption that "if it works here, it will also work in all cases like this."[21] Since research concerns itself with generalisable knowledge rather than specifics, the results of a scientific POC analysis should be applicable to a more general category of cases. As such, the behaviour of the prototype not only tells us something about the specific construction or context in which it operates, but it is rather related to more general dynamics which can be generalisable and

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[21] idem.

knowledge of which can be applied to similar objects or situations. In other words, the behaviour of the prototype provides justification for the underlying concept, and through this, the concept can be applied to a broader range of objects and phenomena. Now, what is being justified and where findings can be projected to remains very vague in Kendig's account. She characterises projectability as an assumption held by the researchers performing the research; Hence why Elliott, in his interpretation of Kendig, has called it the "projectability assumption."[22] Thus, rather than a property of the research itself, it is rather a state of mind of the scientists conducting the research. If that were the case, the best we, as philosophers of science, could do is to identify at what times researchers hold this assumption. However, much like with the notion of novelty, we can do better; And as we will see, using my framework I will develop an explanatory account of projectability as well. In my case, it will not so much be a mental property of the researchers performing POC research, but rather a property of the prototype itself.

As we have seen, POC research goes two ways: On the one hand, it provides an analysis of the feasibility of a certain method. More importantly, it provides a proof of possibility of the prototype which arises from that method. On the other hand, POC research has something to say about the concepts underlying this prototype. It thus provides both information about concrete prototypes, and their abstractions in the form of mental elements. About the former, I have explained that they are held to some requirements, many of them practical (especially in engineering settings), but also epistemic, like *novelty*. About the latter, I have said that it provides justification, specifically in the form of *projectability*. Both the notions of projectability and novelty deserve more attention, which we will get to later on.

For now, however, I want to start limiting the scope of POC research. Until now I have just denoted some general characteristics of POC, and provided some loose specification in how we can distinguish POC research from POC in engineering settings. I have also tried to identify these characteristics in the literature on POC research, and shown how the otherwise heterogeneous literature nonetheless shares some common features. Having established those common features, I will now start to limit the scope of what POC research is all about to me. For as we will see, I will maintain a more limited definition of POC research, in order to get to the core of what makes POC in research so special. This limiting factor mainly has to do with what practices I see as examples of POC processes. Until now I have left unspecified what prototypes can be the result of POC research. I have provided some examples, but no specific outline of what can count as a prototype and what not. Can anything be a prototype in POC research? Certainly not. Then where do we draw the line? In the next section, I will start by distinguishing between material objects and non-material objects as prototypes. Due to POC's origin in the material contexts of NASA, an obvious suggestion would be to permit material prototypes and exclude non-material ones. But as we will find out, the distinction is a bit more nuanced.

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[22] (Elliott 2021, 260–61)

# 3 Limiting the scope

I have already argued that the proof of concept approach consists of two parts: One is a creative part, in which a prototype is developed. Another part is an analysis of this prototype in order to establish its properties. This is one way to think about the organisation of a POC project. There is also another way to think about POC, and this will be more useful to us in what follows. We can think of POC as a flow from the abstract towards the concrete: One starts with an abstract idea, which I have already called a *concept*, and then realises this concept in some more concrete way.[23] Due to its construction, one can check the prototype for the properties which I have discussed in the last section; After all, there is no cost-effectiveness of an abstract idea. And since the justification methods like Kendig's notion of projectability depend on the existence of a prototype, we can see that this construction is an important aspect of POC approaches. Upon closer inspection, however, one finds that there are two notions of prototype construction in the literature. Which of these notions is applied corresponds, as it happens, more or less to the choice of subject matter by any particular author. To clarify this, I will characterise the works of Kendig and Servedio *et al.* according to the abstract-concrete axis I have just proposed, in which the research starts with an abstract entity and ends with a concrete one.

## 3.1 Material contexts

To illustrate her point, Kendig gives an example from synthetic biology, in which cyanobacteria are modified to produce biofuel. In this case, the concept is the "causal hypothesis" that cyanobacteria are able to produce biofuel at highly efficient rates by means of the alteration of its biological pathways. Biofuel production in algae was a well-established field, but production by cyanobacteria had not yet been explored, and the proposal of a biofuel-producing biological pathway in cyanobacteria can be seen as a novel concept. Kendig argues that this concept is proven by means of biologically engineering a type of cyanobacteria which harbours a biofuel-producing pathway.[24] In other words, the "causal hypothesis" which the researchers tried to assess was transformed in experimental practice into the practical question of: "Can we create a cyanobacterium which produces biofuel efficiently?" The main experimental goal became the *actual creation* of a prototype. Kendig herself put it in different words. She rather talked about a "hypothesized theoretical framework (e.g. given input p, q is produced)" which was proven by an examplar experiment which showed that, at least

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[23] In this way we can see the POC approach as a direct opposite to inductive and deductive methods in science (for an overview on inductive methods, see (Ladyman 2012)). Newton, for example, claimed that he would only start from observations, and derive his theories from them in an inductive process. This can be seen as a move from concrete observations to abstract theory. Of course, this is first of all a gross oversimplification, and second of all we have learned over the centuries that scientific methods are vastly more heterogeneous than how Newton and his contemporaries conceived it. Nonetheless, it might be useful to think about science of consisting of two opposing flows of knowledge: One flowing from concrete observations towards abstract models and theories, and another flowing from theory to concretisations. Proof of concept is very strongly an example of the latter.

[24] (Kendig 2016, 739–40)

in one case, p produces q.[25] If we put her elements on our abstract-concrete axis, the **abstract entity** is the "hypothesized theoretical framework" that cyanobacteria are able to produce biofuel efficiently. The **concrete entity** is the exemplar experiment which showed that the created strand of cyanobacteria produced biofuel: Thus, in at least one case, cyanobacteria are able to produce biofuel efficiently. So Kendig sees the abstract as an idea which refers to a class of entities, and POC research realises this idea by creating a single instantiation of such an entity.

Let us look at the relation between these abstract and concrete entities a bit more closely. Even when it is formed, the abstract concept *refers to* this category of (as of yet nonexistent) concrete entities. It can be used to form predictions about how the entities in that category behave, or what properties they have. They can function in hypotheses about the concrete entities, for example in the form of a prediction on the efficiency of biofuel production in cyanobacteria. In other words, the *target system* of the abstract entity (i.e. the system which the abstract entity refers to) is the category of systems like the concrete entity which POC research aims to produce. This relation allows the POC approach to function as a proof of possibility: The existence of the concrete prototype proves that at least one instantiation of the abstract concept is possible. Less has to be said about the way NASA understands POC: Its inclusion into the system of Technology Readiness Levels places POC on a much wider axis which goes from abstract to more concrete, in which technologies at a high TRL are worked out instantiations of research principles which have been developed in low TRL settings. It should be only natural that POC should mimic this structure, and be a step in working out concrete prototypes as instantiations of abstract principles.

## 3.2 Modelling contexts

So Kendig, who writes about POC research which results in material prototypes, characterises the prototype as more or less an instantiation of the abstract concept. Now let us compare this to the characterisation by Servedio et al. (2014), who write about proof of concept in evolutionary biology. As we will see, they identify a completely different practive in science, which confusingly, they also call "proof of concept." To prevent confusion, I will adopt a slightly different vocabulary here, following Plutynski (2006) and Gelfert (2016), and call this approach "proof of principle" rather than "proof of concept." Since we are talking about modelling approaches, the models developed in these context I will call "proof of principle models."

In evolutionary dynamics, many parameters which are involved are biologically complex, and thus challenging if not impossible to vary experimentally. Therefore, evolutionary biologists often have to resort solely to mathematical modelling to test their hypotheses.[26] Servedio *et al.* have written about a certain type of model in this field: the *proof-of-concept model* (or, following my vocabulary, *proof of principle model*). These models are typically mathematically simple models,

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[25] (Kendig 2016, 737)

[26] (Servedio et al. 2014, 4)

which do not necessarily produce quantitative predictions, but rather serve as mathematically explicit chains of reasoning. Servedio *et al.* argue that evolutionary models vary in "degree of abstraction," ranging from highly qualitative models which can produce predictions based on certain input data, to models used for creating abstract understanding of biological processes.[27] A model used for the latter purpose is often verbal in nature and makes use of verbal logic for its chains of reasoning. An example they give of such a model is Darwin's theory of natural selection. Such a model can be very powerful in creating abstract hypotheses about evolutionary processes between various species and across extended time scales, which is something a less abstract model cannot do. At the same time, since the modelled dynamics are very complex, it can be hard to keep track of the validity of the logical claims made, or of the assumptions which are used. To Servedio *et al.*, proof of principle allows for the explication of such verbal models, and lays bare the logical chains of reasoning behind them. Making the proof of principle consists of creating a precise mathematical framework for such a verbal model. This mathematical framework makes previously implicit assumptions explicit, which prompts a re-evaluation and possible removal of assumptions from the model[28]. The verbal model is thus tested through comparison with the mathematical model. Thus, at this stage, neither the verbal nor the mathematical model need to be compared to their target system. Rather, they are compared to one another: "The models themselves are tests of whether verbal models are sound[.]"[29]

Thus Servedio *et al.* have already done my work for me and have placed their models on an abstract-concrete axis, in which the verbal model is the abstract entity and the mathematical model is more concrete entity. Moreover, the mathematical model is an explication of the verbal model, so in a sense it can be seen as an instantiation of the verbal model. However, we run into problems if we compare it to Kendig's case. Remember that in Kendig's case the abstract entity referred to a category of possible systems of which the concrete entity was an instantiation. As such, the abstract entity referred to the concrete entity, and it was this relation between them which allowed POC to function as a proof of possibility. In the case of Servedio *et al.*, on the other hand, both the abstract and the concrete entity refer to an external system which is being modelled. Thus both systems have as their *target system* another system which is outside of the abstract-concrete axis altogether. Let me illustrate this using one of the group's examples. For instance, in one of their examples they discuss a verbal model by Sarah Otto, which is used to explain why many organisms reproduce sexually, even though asexual organisms are able to reproduce at higher rates than sexual ones.[30] A verbal model was put in the form of a hypothesis, namely, that the costs that come with producing both females and males are counteracted by increased genetic variation within the species, which would improve species fitness. The verbal model thus refers to any population with sexual

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[27] (Servedio et al. 2014, 1)

[28] (Servedio et al. 2014, 3)

[29] (Servedio et al. 2014, 2–3)

[30] (Servedio et al. 2014, 4). For the paper, see (Otto 2009)

behaviour, and compares it to populations without such behaviour. However, the development of simple mathematical models served to reveal that the verbal hypothesis was logically flawed and produced unrealistic predictions: Namely, sex does not necessarily increase genetic variation, and even if it does, this increased variation does not necessarily increase fitness.[31] Note that the mathematical model refers to the same target system as the verbal model; As such, the development of the mathematical model produces knowledge about the target system, rather than provide a proof of possibility by its mere existence, as in Kendig's case.

There is thus an important difference between the process Kendig describes and the process described by Servedio *et al.*: While in the former, the abstract concept refers to the concrete prototype, and receives its justification from the mere existence of this prototype, in the latter the abstract concept and the concrete prototype both gain justification (or get refuted, in the case of the verbal model on sex) through being related to an external target system. We can find similar processes to the one described by Servedio *et al.* in other additions on non-material POC models and theories. For example, Plutynski (2006) argues that proof of principle mathematical models in evolutionary genetics establish which conditions influence a general phenomenon and which do not.[32] It is thus a type of mathematical explication of the relevant factors influencing a general process. In this form, such a proof of principle model express "what must be so, for any population that fits (ceteris paribus) the description the model." And Gelfert (2018) has made the similar point that proof of principle modelling practices produce potential representations of a target system.

Thus there are two different practices described in the literature. The first is a type of proof of possibility by means of the actual construction of a single test case, and the second is a type of exploratory modelling designed to identify relevant parameters and check the logical consistency of verbal hypotheses. Is this a problem? Not really; The field Kendig operates in, and the fields Servedio *et al.*, Plutynski and Gelfert operate in are far removed, so the meaning of terms can differ, and depend on the context. That is, when Servedio *et al.* use the term "proof of concept," they might mean something totally different than when the same term is used in the field of synthetic biology, for example. As long as they are used within their own disciplinary boundaries, there is no confusion. Problems arise, however, when one tries to synthesise the two meanings together, as Elliott has attempted in his framework.[33] For the two approaches are mutually exclusive: The abstract concept cannot have both the mathematical model and an external system as its target system at the same time. Moreover, in material contexts, the explication of verbal models does not even make sense, since the prototype does not refer to any external system. Evidence of this mutual exclusivity can be found in Elliott's framework itself. More specifically in "proof of concept demonstrations," in which the success of a prototype is to be demonstrated. He claims that these demonstrations aim to show, in the case of physical prototypes, that they successfully

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[31] (Otto 2009)

[32] (Plutynski 2004, 1205; Elliott 2021, 261)

[33] (Elliott 2021)

perform a function (like a rocket which launches). In the case of prototype models, however, these demonstrations should show that the models achieve epistemic functions (like describing, predicting, or explaining phenomena).[34] This is obviously not compatible. How does the epistemic value of the rocket compare to the epistemic value of a model, for example? What mutual basis do we have to compare them on? If they even have a mutual basis, it is very thin. As such, I propose that we keep these two meanings of POC research separated: They might be called by the same name, but they denote two different processes, which should be evaluated through different means.

## 3.3 From concepts to prototypes

Since I am interested in the processes in science where an object is created without a direct epistemic function of the object, I will now set proof of principle models aside in favour of the proof of possibility-process as described by Kendig. However, this choice requires some more clarifications. For example, since Kendig's work is situated in a context which produces material prototypes, it might seem that I am dismissing the possibility of non-material prototypes in POC research altogether. This is not true. Rather, I am purely limiting my scope to processes which start with an abstract concept which refers to a general category of objects, and during which prototype which falls within that category is developed. This requirement is indifferent to the (non-)materiality of the prototype itself, and thus nonmaterial prototypes are, in fact, possible. I will now show this using an example from early computer science, namely John Von Neumann's creation of his "universal constructor."

### 3.3.1 Von Neumann's universal constructor

In 1948, Von Neumann gave a lecture at the Hixon symposium called "The general and logical theory of automata."[35] In it, he laid our the general model of a self-replicating machine. This machine would float in a sea of parts, which it could use to create a perfect copy of itself. The idea that this model would be able to represent a the copying process of DNA in a reductive way, and possibly pave the way forward towards the creation of artificial life.[36] But Von Neumann soon ran into problems when he tried to actually build such a machine, due the logical cost of supplying the sea of parts. A way out of this problem was provided by Stanislaw Ulam. He suggested using a *discrete* model of cells ordered in a lattice, in which each of the cells had a discrete value, and their value at every time step was dependent on the previous values of their direct neighbours.[37] This approach was more promising due to the fact that it did not necessitate providing a sea of parts to the machine, since it could just replicate itself as a collection of values within the lattice. As such, the general idea of the machine was born: It would live within this discrete model, and replicate

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[34] (Elliott 2021, 264)
[35] (Von Neumann 2017)
[36] (Schiff 2011, 2)
[37] (Schiff 2011, 3)

itself by manipulating values within the lattice. Von Neumann went to work on constructing such a machine, which later became known as Von Neumann's universal constructor, and it was published posthumously in 1966.[38] This publication details every part of the machine, which consists of around 200,000 cells, and shows how it can, in fact, replicate itself. Thus the first example of a self-replicating machine was created, and as such the feasibility of creating such a machine was shown by the machine's actual construction. The discrete model Von Neumann worked was later taken up and became known as cellular automata. Nowadays there exist countless of these cellular automata, each with its own characteristics and uses.[39] Note that the final prototype was completely non-material; The object itself was rather a logical structure, shared in the form of a text which described how every of its parts was constructed and what it did.

We can see that the research of Von Neumann is a type of research which starts with a general, abstract idea, and ends up with a concrete, yet *nonmaterial* object. As such, I take his research as an example of POC research, and conclude from this that nonmaterial POC research is possible. But one might still argue that, although nonmaterial POC is possible, scientific models are nonetheless excluded, due to the fact that models always refer to an external target system. However, this is also not the case: In fact, the literature on proof of principle models I have dismissed before does contain some examples of processes which are genuinely within my scope. An example of this is Gelfert's discussion of the Lotka-Volterra model. This model is is used to model predator-prey relations in an ecosystem, and it was based on observations that a decrease in fishing does not necessary lead to an increase in fish in the ocean.[40] To explain this phenomenon, Volterra started from some basic assumptions about the predator and prey species, and derived two continuous differential equations which managed to model the populations of both the predators and the prey. However, its success in describing predator-prey relations is not the reason why it falls within my scope. Rather, it is due to the fact that the model managed to describe the increase and decrease of discrete populations (consisting of numbers of animals) by means of continuous differential equations. Gelfert himself identifies this as one part of a two-fold proof of principle.[41] The other part was that the model gathered some knowledge about the target system of the model itself; This is the part of Gelfert's account which falls outside my scope. The fact that does fall in my scope is that the model managed to describe discrete dynamics through continuous equations. Volterra was a mathematical physicist[42]: Before constructing the concrete model, he must have had the abstract concept of continuous differential equations modelling discrete dynamics in his mind, otherwise he would not have come up with this specific model. This example thus shows how models can function in POC research in the way I understand it: If the abstract concept of a certain modelling approach refers to a possibility of a certain type of model, and the concrete, worked-out model

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[38] (Neumann 1966)

[39] For two overviews of different cellular automata, see (Schiff 2011) and (Ilachinski 2001).

[40] (Gelfert 2016, 59–61)

[41] (Gelfert 2016, 86)

[42] (Gelfert 2016, 59)

proves the possibility of that model, then it does indeed fit in with my understanding of proof of concept.

## 3.4 A note on simulations

This brings us to a somewhat odd entry in the world of scientific objects: Simulations. With regard to simulations, I want to ask: Can a simulated prototype function in POC research just like a material prototype? Simulations are special, since they can be seen as actual experiments, as well as elaborate scientific models. They are models in the sense that they solve a system of equations which represent the relevant dynamics. As such, they represent the world as we *think* it is, not how it necessarily has to be. In other words, "a simulation is no better than the assumptions built into it."[43] However, the dynamics which are simulated are often complex. The equations underlying it might only be numerically solvable, and simulations tend to follow a process over the duration of an extended time scale, which makes it impractical or impossible to model these dynamics by hand. As such, much like experiments, simulations might surprise us despite us knowing what went into them.[44] We might change our system's parameters, or change some boundary conditions, and the system might behave in ways which we genuinely did not see coming. The fuzzyness of the boundary between simulation and experiment becomes even more apparent in the case of general simulation approaches such as Molecular Dynamics simulations, or Finite Element Analysis.[45] Both of these approaches are not geared towards any specific target system, but rather simulate dynamics which are relevant in a wide variety of systems. Moreover, their validity has been shown throughout the years, to the extent that Finite Element Analysis has significant authority in engineering design processes.[46] As for Molecular Dynamics, these simulations are used in lieu of experiments in cases where such experiments are technologically impossible. For example, the folding of proteins is nearly impossible to follow empirically. Simulations nonetheless provide a means to gain knowledge about this process.[47]

The existence of such elaborate methods of simulation begs the question: Is it possible to simulate a prototype and have it function in POC research? If we are confident that our simulation replicates real dynamics correctly, can a simulated prototype take the place of a real prototype? I would say no. The problem is that, no matter how confident we are about our simulations, we can never be completely sure that it corresponds with actual, real-world dynamics. A simulated prototype could never provide the full justification which the abstract concept needs. The proof of the pudding is in the eating; The proof of possibility is in the prototype's *actual*, real-world

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[43] (Simon 1981, 18)

[44] See (Currie 2018) for a recent defence of the argument from surprise, which is the claim that simulation's importance arises from its ability to surprise us.

[45] For an overview of recent usage of Molecular Dynamics simulations, see (Hollingsworth and Dror 2018). For an introduction into Finite Element Analysis, see (Szabó and Babuška 2021).

[46] (Kurowski 2022)

[47] (Best, Hummer, and Eaton 2013)

behaviour, and thus in its actual construction. Of course, simulations might provide evidence that a certain approach might be feasible; This evidence might, in fact, be so strong that researchers are confident that it is possible. It does not, however, provide solid proof that it actually is. To reiterate this point, I would like to cite Currie on the subject:

> The [experimental] study, insofar as it is surprising, is such in virtue of demonstrating that our expectations about the world ... is somehow lacking and in need of re-examination. The simulation study, insofar as it is surprising, is such in virtue of our learning where our ideas take us.[48]

Thus my scope is limited by characteristics of the practice by which the research is done rather than the materiality or non-materiality of the prototype. These characteristics of POC are, first of all, an analysis of feasibility which, at the very least, and most importantly, requires the possibility to actually construct the prototype. Second of all, this creation of the prototype *reflects back* on this concept and the hypotheses which are formed about a broader category of objects, of which the prototype is a member. Thus, by creating the prototype, research is done on the concept. We are now almost ready to dive deeper into this specific process. I will do so later by developing a framework which aims to explain all of the entities involved in POC research, and the relations between them. However, at this point, I would like to consolidate my characterisation until now with a case study. This case study tells of the development of an object with special material properties by Rod Lakes.[49] Although he himself has never characterised this work as POC, I will argue that it does fit the characterisation I have presented here: As such, I argue that we can take it to be an example of POC research.

# 4 Case study – Auxetics

As we have seen in the last section, there are two main characteristics of POC research. First of all is that it consists of a proof of possibility of a prototype. Since the POC is done in a research setting, it also requires the prototype to be novel in some way. In the following, I will take these to be the main requirements for a prototype developed in POC research. The other characteristic of POC research is that the prototype reflects back on the concept or idea of the prototype. In the following, I will show that both of these characteristics are met in a study performed by Rod Lakes.[50] Lakes created the first material which had a property which later became known as auxeticity.[51] His

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[48] (Currie 2018, 656)

[49] (Lakes 1987)

[50] (Lakes 1987)

[51] While the objects of research are called "auxetic materials" or "auxetics" nowadays, we will see that this term did not exist yet when Lakes developed his material. Rather, he knew them as "negative Poisson's ratio materials," and the term "auxetics" was only coined several years later by Evans (1991). However, at the risk of being ahistorical but for the sake of brevity and clarity, I will nonetheless call Lakes' material auxetic, and the property of having a negative Poisson's ratio auxeticity. However, one should keep in mind that Lakes himself had never heard of this term at the time when he did his research.

creation was able to confirm some long-standing predictions about such materials. But before I can show this, I need to clarify the theoretical context in which Lakes performed his research.

### Poisson's ratio

Materials exhibit certain behaviour when you compress them. In general, when a material is compressible, it expands in the direction perpendicular to compression, and shrinks in the direction perpendicular to stretching. For example, pulling on a rubber band makes it become thinner. In 1827, Siméon Poisson used a theory of molecular interaction to derive a value which would characterise this process for elastic rods, a singular value of $\frac{1}{4}$ which later became known as Poisson's ratio. By deriving this value he concluded that all elastic materials must have the same response in terms of contraction or expansion.[52] (Elastic here denotes the property of a material which has an elastic response to forces acting upon it; thus simple metals are all elastic as long as the forces acting upon them is not too high.) A later theory by Augustin-Louis Cauchy and experiments by Woldemar Voigt showed that this was, in fact, false and that different materials might show a variety of responses. Thus, Poisson's ratio could have other values as well.[53] These values were understood to lie between 0 and $\frac{1}{2}$ for a long time, but by 1970, it was derived through the mathematical theory of elasticity and thermodynamics that the limits for this value were actually $-1$ and $\frac{1}{2}$.[54] This meant that materials with a negative Poisson's ratio were theoretically possible, a realisation which was supported by a relatively small number of experiments which had taken place scattered throughout the twentieth century.[55] More specifically, the materials found to be auxetic in these experiments were often crystal structures, and displayed auxeticity anisotropically, meaning that they would only be auxetic in a specific direction.[56]

Roderic Lakes was one of the first ones to create such a material with a negative Poisson's ratio in all directions. To do this, he took a regular (positive Poisson's ratio) polymer foam, which usually has a three-dimensional "honeycomb" structure: The microscopic structure of the material can be seen as a collection of interconnected cells which resemble what would be a honeycomb when it is generalised into the third dimension. By taking such a polymer foam and heating and compressing it, he was able to "collapse" the honeycomb structure, creating a foam which would expand perpendicular to stretching. In doing so, he created the first kind of what would later be known as "auxetic materials," where auxetic denotes the property of having a negative Poisson's ratio.[57] In the article presenting his work, he explained the underlying mechanism which provided the material with its unusual behaviour and provided the exact (negative) Poisson's ratio for his material. Moreover, he reports testing several of the predictions made of such auxetic materials

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[52] See (Greaves et al. 2011, 824). For Poisson's original paper, see (Poisson 1827)

[53] (Cauchy 1828; Voigt 1882)

[54] (Lim 2015, 5; Landau and Lifšic 1975, 12)

[55] Lim (2015) names Love (1927), Hearmon (1946), and Popereka and Balagurov (1970), among others.

[56] (Lakes 1987, 235)

[57] As coined by (Evans 1991).

by the theory of elasticity. For example, the shape of the top and bottom surface of a beam of conventional (positive ratio) material has a "synclastic curvature" (i.e. it assumes a saddle shape) when bent. The surfaces of a beam of auxetic material, on the other hand, were predicted to have an anti-synclastic surface (i.e. more of a dome-shape) when bent. Lakes confirms this as well as some other predictions which were made about auxetic materials based on the theory of elasticity.[58] Finally, he imagines possible applications of these kinds of materials, imagining applications such as shock-absorbers, air filters, and fasteners.[59]

Let us now turn to the question of how this example fits in with the characterisation of POC research I have set out before. As we have seen, the concept of a negative Poisson's ratio material has a long theoretical history. At first, the notion of a negative Poisson's ratio was not even taken seriously due to the overwhelming amount of materials which exhibited a positive ratio. However, a closer examination of the mathematical theory of elasticity allowed people to imagine a material with a negative Poisson's ratio. The comparison with known positive ratio materials allowed people to make predictions of how such a material might behave. At the time of Lakes' research, there was, therefore, a clear idea of what such a material might look like, and what properties it would possess. However, whether these materials would actually behave according to the expectations was uncertain: After all, they could not be proven as long as no such material was available for testing. Moreover, many were not even certain whether these types of materials could be found in the first place.[60] In other words, their "possibility" was still up for debate.

That is, until Lakes created his material. One can immediately see how his research provided a proof of possibility for this kind of material: Since it was now actually created, auxetic materials were no longer a weird prediction by the theory, one which did not have any meaning for any real materials. No, here was a real auxetic material. This is also what made Lakes' material "importantly novel," to speak in Elliott's terms: It was the first time such a material was created. Moreover, it could be created from conventional materials such as the polymer foam which Lakes had used. Thus by creating his material, Lakes had changed auxetic materials from a class of unknown, theoretical entities to actual, existing objects, which one could touch and play around with. Not to mention that it was relatively easy to create; As such, it was not only possible, but his method was also feasible in the sense that anyone could make it, as long as they could get their hands on the type of polymer foam that Lakes used, and knew how to heat it up to a specific temperature. In this sense, Lakes' research is specifically a proof of possibility, and generally a proof of feasibility for auxetic materials. As such, it corresponds to the first characteristic of POC research which I have identified. Moreover, the creation of this material obviously also had a number of effects on the theory of elasticity. First of all, the whole idea of an auxetic material was justified. In other words, while before the limits of $-1.0$ to $+0.5$ for the Poisson's ratio might have been a weird quirk of the

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[58] (Lakes 1987, 1040)

[59] (Lakes 1987, 1040)

[60] "It is believed by many that materials with negative values of Poisson's ratio are unknown." (Lakes 1987, 235).

theory, now there was proof that this was actually the case. Furthermore, all of the predictions of auxetic materials which Lakes tested came from that same theory of elasticity. And similarly to the negative Poisson's ratio, all of these predictions were justified as well through Lakes' research. Thus his research also corresponds to the second characteristic of POC research I have identified: The creation of the prototype reflects back on the concept which circumscribed its development. POC research is a type of research which has consequences for both the prototype and its concept. We can see this in Lakes' research: There is a focus on the material he developed, but also a focus on what the existence of this material and its properties mean for the underlying concept.

Now we have seen that it is possible to characterise certain studies as POC research, and we have already identified some of its importance for both the abstract and the concrete entities involved. We can do better, however. Where do the concepts involved in this kind of research come from, for example? The theoretical development of the negative Poisson's ratio is only one case, but it is not enough to develop a general theory of what these concepts are and where they come from. Moreover, I have not even discussed *how* researchers manage to create their prototypes. How did Lakes manage to create his auxetic material? If he started with the concept of a negative Poisson's ratio, how did he ever get the idea to compress a polymer foam to get there? Finally, how do researchers like Lakes make sure that they actually have a working prototype, and that it exhibits what they want it to exhibit? What makes Lakes identify his object with the general concept of "auxetic materials?" In the following, I will develop a framework in an attempt to answer these questions. Moreover, this framework will help us to understand the epistemic products of POC research more generally.

# 5 The framework

In the following, I will work out a framework to understand and analyse POC research. I argue that POC research can be understood along the lines of two entities and one relation between them. The first kind of entity I will discuss is the abstract entity: the *concept*. I will then discuss how the concrete entities, the *prototypes*, are created according to such a concept. Finally, I will discuss how these prototypes are demonstrated in research. This demonstration will involve the *representation relation*, which connects the concept and the prototype meaningfully to one another.

## 5.1 Concept

To start off with the framework, we will look into concepts. However, before we start, the choice of these entities deserves some clarification. For until now, I have taken the concept to be simply a mental representation of the prototype, whatever that mental representation may be. But what does that mean, exactly? A scientific hypothesis is also a mental representation, but instead I have chosen to use the word "concept." Why is that? To understand this choice we should first understand the difference between propositional mental representations such as hypotheses and concepts. A proposition can be seen as the content of a declarative sentence: It expresses something which can either be true or false.[61] Concepts, on the other hand, do not declare anything by themselves: Rather, they are *constituents* of propositions, the building blocks with which they are constructed.[62] So, while the proposition THE BOOK LIES ON THE DESK can be true or false, its constituents BOOK and DESK cannot; Rather, they are just there. As such, it is rather like a name we give to a category of objects. Similarly, when we construct a new object, it cannot be true or false. If I show my friend the paper plane I just folded out of a sheet of paper, she cannot disagree with the concept PAPER PLANE. She can disagree that we should call this particular object a paper plane; Perhaps I am very bad at folding paper, and the thing I made hardly flies. However, at that stage, the concept PAPER PLANE is already part of proposition THIS IS A PAPER PLANE. Take away the proposition and we are just left with a tag for a certain class of objects: That is what a concept is.

Now it should be clear why I have called the abstract entities in POC research "concepts." We also see that a single concept might refer to a lot of different objects, since it denotes a whole category of objects. Thus, we do not just have a "concept" and a "prototype" anymore, as we did in my initial characterisation of POC research: Along with the prototype, there is a whole category of possibly existing and nonexisiting objects. Thus, from now on, I will generally call any object which is realised and which falls within the category circumscribed by a concept an *instance* of that concept. Thus, our prototype is an instance of its concept, but it could be only one instance among a whole array of instances.

Furthermore, since POC concerns itself with the creation of an instance of a concept, and since concepts apparently cannot be true or false, one might start wondering the approach is called "proof of concept" in the first place; After all, if it cannot be true or false, then a concept clearly cannot be proven. Perhaps it is this ambiguity in meaning which has caused the use of the term to be so heterogeneous across different fields. I will not attempt to give an exact answer to the meaning of "proof" in the term "proof of concept" here, because I think it is inherently ambiguous. Some might see it as a proof of feasibility or possibility, while others might understand it as proof of hypotheses in which the concept plays a role. What we *can* define, on the other hand, is the notion of concept itself. In the following, I will attempt to do so by investigating a number of existing theories of concept. Through this, I want to mainly answer the following question: How do the concepts in POC research arise? They are mental representations, usually of objects which exist. But a defining trait of POC research is exactly that these objects do not exist yet. So where do these concepts come from? What determines their defining traits?

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[61] Propositions are specifically *contents* of declarative sentences, because multiple sentences can express the same fact about the world. An intuitive (and often-used) example is that of translation. For example, "grass is green" and "gras is groen" both say the same thing, although one in English and the other in Dutch. See also (Hanks 2009).

[62] See (Margolis and Laurence 2007).

There are many theories of concept out there. Since a theory of concepts inherently spans a space between philosophy, psychology, and cognitive science, the approaches taken in constructing such a theory are similarly diverse. I am not giving an extensive overview of the whole discussion here: Rather, what is important now is to find a theory which will suit our needs. This is not an easy task: For one quickly finds that the concepts in proof of concept studies are wildly different from the ones investigated in a theory of concepts. Take the iPS cells from the introduction of this work, for example: The concept of such a cell must entail highly theoretical and contextualised knowledge, such as *stem cell*, and *pluripotency* and the inducing thereof. The examples typically given in the characterisation of concepts, on the other hand, are *bachelor* or *pet fish*. Whether these examples always amount to a lower degree of complexity would be a stretch; But to grasp these sorts of concepts one definitely needs less highly contextualised knowledge. This I have kept in mind in my review of the available literature. After all, I do not intend to assess the pros and cons of the available theories. Rather, what is important now is to gain some understanding of how concepts play a role in POC research, so the main criterion of any theory is that it will fit to the specific usage of concepts in this type of research.

### 5.1.1 The main theories of concept

Broadly speaking, theories on concepts come in three flavours: There is the classical theory, and then there are probabilistic theories and theory-based theories.[63] The classical theory stipulates that concepts have a definitional structure: A concept, however complex, will be composed of simpler concepts which provide the necessary and sufficient conditions for an entity to be an instance of said concept.[64] As such, a concept should be fully defined. But, going back to iPS cells again, we have seen that many defining traits of such a cell were unclear until the last stages of research, when the researchers were already very close to actually creating the cell. As such we can determine that the “concept” which Yamanaka and his colleagues held was open-ended. In other words, it did not stipulate the necessary conditions for something to be an iPS cell, and as such, the classical theory is not a good fit for our purposes.

Probabilistic theories arose as a response to the classical theory by psychologists, most importantly with regard to how people categorise objects into concepts. For example, psychological research has shown that people can rarely define concepts in terms of necessary and sufficient conditions.[65]. Moreover, there are problems with the demarcation of concepts such as RED, which is not defined by a strict range of wavelengths of light, but rather has vague, subjective boundaries.[66] And there are typicality problems, in the sense that people judge a German shepherd as a better exemplars of the concept *dog* than a Pekinese.[67] This is where probabilistic theories, and most

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[63] (Margolis 1994)
[64] (Margolis 1994, 77)
[65] (Margolis 1994, 79)
[66] (Hampton 2006)
[67] (Carey 2009, 496)

notably *prototype theory* comes in. The idea behind prototype theory is that an entity can be seen as an instance of a concept if it only possesses a sufficient number of properties which members of that category have.[68] As such, a concept does not have to be precisely defined. Rather, the concept encodes a number of properties which are held by a virtual object, which the theory (unfortunately) calls a *prototype*. Instead, I will call this object a *core-prototype*, distinguish this notion from my own notion of prototype (as the object resulting from POC research). The "core" refers to the centre of the cluster of objects which are instances of a concept.[69] The core-prototype is assumed to be the abstract object which represents the different ways in which instances of a concept resemble each other. Moreover, the typicality of an instance is determined by its distance from this core-prototype.

Although the prototype theory takes care of the problem we had with the classical theory (that is, the fact that the concepts involved in POC studies are never fully defined), it nonetheless does not fit our scenario, for the simple fact that *there are no instances of the concept before the research is done*. Thus there can be no "typical" properties of the concept, because there is no single object which can present these typical properties to us. Thus when NASA claimed in 1967 that the concept was developed in a laboratory environment[70], they could not possibly have referred to prior instances of the concept, because the whole point of the proof of concept phase was to develop such an instance. In such a case, when there are no instances of the concept to categorise, what determines the properties the core-prototype should have? In other words, how is one to know the meaning of the concept in the first place?

This is where theory-based theories come in. These theories of concept find their origin in developmental psychology, in that they are first and foremost geared towards understanding how children attain and hold concepts. A major contribution in this field has been by Susan Carrey, and I will be largely following her version of theory-theory.[71] The main idea of this theory is that concepts attain their meaning through their roles within a theory, be it a scientific theory in scientists, or a more rudimentary theory in children. These roles can be relations to other concepts: For example, the concept of DENSITY has the role of being related to the concepts MASS and VOLUME as a division of the former by the latter. Or they can be properties: For example, DENSITY is an intensive quantity (i.e. its value is independent of system size), while MASS is extensive (i.e. increases linearly with system size). Note that these conceptual roles do not have to follow scientific logic: For example, the concept of PERPETUAL MOTION MACHINE is not coherent with the second law of thermodynamics. Nonetheless it does hold its relations to MOTION and TIME.

It should be noted that the "theory" in theory-theory refers strictly to the theory a single person holds. This causes a problem which has been identified by Jerry Fodor and Ernest Lepore with regard to the classical and prototype theories I previously discussed.[72] After all, since the role

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[68] (Margolis 1994, 77)
[69] (Hampton 2006, 80r)
[70] (United States. Congress. Senate. Committee on Aeronautical and Space Sciences. 1967)
[71] (Carey 2009)
[72] (Fodor and Lepore 1992)

of a concept in one person's theory is complex, there is reason to assume that their concept will never have the same content as the concept embedded in someone else's theory. Even more so, one can imagine that the concept a single person holds changes over time. Since concepts change meaning between people and over time, it seems impossible for two people to agree on said concept. Although this instability in meaning has been shown to not be any worse than the theory Fodor himself proposes as an alternative[73], this danger of an holistic account of concepts should still be taken into account.

As such, Carey proposes a theory which ascribes more importance to certain conceptual roles than others to give content to their respective concepts, avoiding the largest force of the criticism. More specifically, only the conceptual roles which take part in *conceptual change* are taken into account. What is conceptual change? As the name suggest, it refers to a process in which concepts attain a new meaning both in children's development and scientific progress. Carey gives examples where children transition to a theory which distinguishes mass and density from a theory in which these two concepts are conflated in the single concept of heaviness.[74]. In science, an example of such a conceptual change is the development of the *vis motrix* (i.e. a precursor of gravity) by Kepler from a conceptual system which did not contain any similar concept to gravity.[75]

Carey argues that an important process in such conceptual change is *Quinian bootstrapping*: It is a process of theory change in which a more highly explanatory account is produced out of primary elements which have less explanatory power. This bootstrapping process happens through a variety of ways. It often involves introducing a placeholder to account for some relation or entity in the theoretical framework in which one is working. Thus Kepler's *vis motrix* was a placeholder entity for some unknown force which is given off by the sun to account for the planet's orbits.[76] In other words, although the entity is not strictly known or defined, the scientist nonetheless needs a name for it: This is the placeholder. Carey relates it to a child learning the symbols "a half" or "heavy": It takes years before these symbols have attained the same meaning as it does for adults.[77] Introducing a placeholder is thus not the same as labelling something which is already known; In such a process, which Carey calls "fast mapping," the concept's meaning is already known, and merely a symbol is attached to it. Giving meaning to placeholders rather involves inferential activities included in the bootstrapping process.

Due to the fact that the placeholder is being introduced within a process theory development, its meaning is necessarily determined by the role it plays within such a theory. And since a concept arises out of such a placeholder, there is an ancestor-descendant relation between these two, which means that the conceptual roles which determine the concept's meaning are descendant from those relations which determined the placeholder's meaning. These relations might undergo

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[73] (Margolis and Laurence 2007, 587)

[74] (Carey 2009, ch.10)

[75] (Carey 2009, 422–28)

[76] (Carey 2009, 532)

[77] (Carey 2009, 533)

changes throughout the process of theory building, and there might therefore be differences between some conceptual roles of the placeholder and the concept. Nonetheless the meaning-determining conceptual roles for a concept will be the same roles which determined the placeholder's meaning.[78]

There are other ways in which we can identify meaning-determining conceptual roles.[79] For example, conceptual roles could be meaning-determining by virtue of their relation to other concepts which indicate conceptual change (this thus excludes roles which relate to concepts which do not change over the course of conceptual change). Another way is identifying those roles as most meaning-determining when they have the largest impact on theory if they were to be changed. Finally, the causality deepest roles could be the most meaning-determining: Thus the causally deepest features of an entity determine whether it falls under a certain concept.

Let us now move from concepts in general to specifically *scientific concepts*, that is, the concepts which arise in scientific theories. Since POC research is an approach in science, the concepts which participate in it are necessarily scientific. Scientific concepts are complex. As such their meaning is presumably determined by a variety of processes, which urges us to take all of the ways of determining meaning-determining roles seriously: Presumably a concept's meaning is determined by a combination of all of them. What has hopefully been established, however, is that conceptual meaning is determined by the fact that they are embedded within a theory. In periods of conceptual change placeholder structures are established, which drive prototype creation, and will eventually result in the establishment of a concept. Also note that, since placeholder meaning is determined by conceptual change, we have a clear requirement for novelty. This concept will derive its meaning from its function within a theory. Let me clarify this with an example. Think of Yamanaka's research, for instance. When he started, there was no "concept" of iPS cells as such, least of all because the technique to create them was not developed yet. However, from previous research it was shown that somatic cells (i.e. the cells he was working with) could theoretically be reprogrammed to achieve pluripotency by using embryonic stem (ES) cells, and thus be able to develop into a variety of specialised cells. Based on this, he hypothesised that some "intrinsic factors" of these ES cells could be used to induce pluripotency in somatic cells.[80] This hypothesis can be seen as introducing a new object: A somatic cell which has been reprogrammed for pluripotency by using some "intrinsic factors" of ES cells. We can see this hypothetical object as a placeholder: It performed all the roles in the theoretical framework which iPS cells would eventually play. But at that point, Yamanaka did not know yet what those "intrinsic factors of ES cells" were: As such, the concept could not be defined yet. Only after these factors were identified and the hypothesis was accepted did Yamanaka name the new pluripotent cells "iPS cells," marking the creation of a new concept.

Until now, Carey's theory has given us an explanation of what determines the content of a

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[78] (Carey 2009, 533–34)

[79] (Carey 2009, 534–35)

[80] (Yamanaka 2013, 13904)

concept, but not what justifies the reference of that concept to an entity in the world. How do we know that the colour of a fire truck is an instance of RED, for example? This problem is especially important in POC research, since in it, new objects are created. As such, concepts do not refer to a collection of already existing objects, rather, whether a newly created object is an instance of a concept has to be determined on a case-to-case basis. As such, it is of special importance to determine what makes someone classify an object as falling under a certain object. As we have seen, the classical and prototype theories provide an adequate means of establishing reference to readily existing objects, but fail in our case, where the object in question is yet to be created. Theory-theories, and especially the one argued for by Carey, do a better job at explaining how a concept is formed without reference to a readily existing object, but does not provide an account of reference once such a concept has been formed. Thus we need an additional account of how this reference is established.

### 5.1.2 How reference is made

Carey's account was psychological rather than philosophical in nature, taking cues from developmental psychology to establish an analogy between the concept-forming strategies employed by children and scientists. The following theory of reference will be even more psychological in nature, providing a descriptive account of how people establish reference in actuality, rather than how reference can be established on strictly ontological and semantic grounds. As we will see, such an account explains how a prototype is identified as an instance of a concept "in the wild," which is important to explain if we are to understand the actual and potential characteristics of proof of concept research.

As an inspiration I will use the work by Stephen Laurence and Eric Margolis.[81] In short, Laurence and Margolis argue that there is a myriad of ways in which people create a reference relation between an object and a concept, and to trace the full process would most likely be a messy endeavour. As such, the account merely stipulates that, when someone encounters a new object, they *will* establish a reference between that object and a concept in their mind. As such, how a reference is made is a problem of cognitive science; A philosopher only needs to accept that the reference is made. One might object to this, and argue that there must nonetheless be some philosophical perspective on what justifies making these references. How do we know whether we are not making false references, identify objects as instances of a concept when they are not? But the philosopher is relieved of this duty by virtue of what is called "psychological essentialism."[82] The essentialist thesis claims that people, from a young age onwards, represent objects as having

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[81] Laurence and Margolis (2002)

[82] See (Carey 2009, 518). Of course one could argue that accepting psychological essentialism smacks of philosophical cowardice: After all, it provides no metaphysical foundations for a theory of conceptual reference (see also [[Medin (1989)). However, a full-fledged theory of concepts is not the aim of this work. Rather, it aims to explain the method of proof of concept research, of which the metaphysical aspects as such are of less importance. Psychological essentialism explains how people form and use concepts; Let us then use this thesis to explain how people (scientists) do and approach proof of concept research.

some “hidden essence,” which determines its membership of a conceptual category.[83] For example, in a famous experiment by Frank Keil, children were asked whether a cow which was made to look and behave like a horse should be categorised as a cow, or as a horse.[84] It was found that, while very young children judged the animals by their appearance, and would thus judge these animals as a horse, children which were older tended to categorise them as cows, arguing on the basis of rudimentary biology.[85] This indicates that older children and adults imbue objects with some kind of essence, a hypothesis which has since been confirmed by other experiments.[86]

How does this help us in explaining the reference between an object and a concept? Well, it shows that people might categorise an object as an instance of a concept based on its “essence.” And since this “essence” is implicit, people are thus not limited to make this reference relation through definitional or probabilistic properties. But that does not fully explain how this reference relation is actually established. Now, of course the mental processes by which this happens lies in the field of psychology. In Carey’s words, elucidating these processes is “the bread-and-butter work on the psychology of concepts.”[87] However, Laurence and Margolis do have a name for these processes: They call them *sustaining mechanisms*.[88] These sustaining mechanisms ensure that the concept-world relation hold. They can take a variety of forms: They can even be learned, and then resemble the relations as described by the classical theory, or prototype theory.[89] But most importantly for us, these sustaining mechanisms can also be social in nature: People refer to an object as an instance of a concept in communities, and will want to have a shared basis on which objects are judged as instances of a concept.[90] In other words, a community will assume that a concept refers on a shared basis, and people will aim to let their conceptual systems correspond to other systems in the community. Notable examples are scientific practices: They make sure that our objects are instances of concepts through empirical means.

## 5.2 Creation of prototypes

Once a scientific concept has been developed in a certain theoretical context, the target system it refers to might not actually exist yet. In such cases one might want to realise an instantiation of such a concept; In other words, one might want to create it. In this section, I will go over this creative process in two parts: I will first give a general account of the creation of objects, which applies in any context where a scientifically or technologically novel object is created. As we will see, the creation of objects always involves techniques on how to create them, and the creation

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[83] (Neufeld 2022)

[84] Keil (1992)

[85] (Keil 1992, ch.8)

[86] (Neufeld 2022)

[87] (Carey 2009, 518)

[88] (Laurence and Margolis 2002, 37)

[89] Laurence and Margolis (2002) argue for this point using a theory of concepts by Jerry Fodor (which I have not discussed here), but the same argument can be constructed using other theories. See also (Carey 2009, 519)

[90] (Carey 2009, 520)

of *novel* objects requires the development of *novel* techniques. What I will discuss in this first part will not only be applicable to the creation of a new object in as in POC research, but for experiments generally in scientific settings. In the second part of this section I will dive more deeply into prototype creation in POC research specifically. I will return to the weak/strong creation distinction by Kroes (2014) which I have referred to in the introduction of this work, and I will argue that prototypes in POC research are in a special position with regard to this distinction. This will allow us to gain a better understanding of their connection to scientific concepts which we will discuss later on.

### 5.2.1 Methodological knowledge

Say one has developed (or otherwise received) a concept of which no known instances exist yet. How does one go about creating such an instance? This process must start with an intention: One *intends* that whatever object one will create will possess some right properties to be identified as an instance of the concept. For example, say I want to create a screwdriver. The required property for something to be a screwdriver is that it can be used to drive screws. As such, I intend for the object which I create to have this property. In practice, this involves deciding whether the screwdriver needs to have a flat end, or one which makes a cross shape, in order to have it screw the right screws. But this is merely an explication of the intention: At this stage, no action has been taken yet. So how do we move from such an intention to the actual creation of a screwdriver? I will argue in the following that it is through employing the right techniques.

A large aspect of science is the creation of instruments and experimental setups. This involves the creation of highly specialised instruments, such as the particle accelerator at CERN, or more small-scale development involving vacuum chambers, cameras, electrical triggers, and the like. Developing such setups requires both using the right techniques and having skill in determining what should be included and excluded in the setup. These techniques are a type of practical knowledge which can be put into sentences and shared verbally or through writing. And a lot of this knowledge is written down in what Davis Baird describes as “instrument cookbooks.”[91] These books contain useful tips and more extensive explanations of issues which one might come across when building a scientific setup or apparatus. Baird shares some examples from a book by Moore, Davis, and Coplan (2009). For instance, the book explains the advantages of using Pyrex glass in the creation of vacuum systems due to its chemical inertness and low thermal expansion.[92] In another part, they explain the mechanics of converting linear motion into rotary motion across a vacuum seal.[93] Techniques like these help the scientist develop their scientific setup. But, in a much broader sense, these techniques also constitute a body of what I call *methodological knowledge*

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[91] (Baird 2004)

[92] (Moore, Davis, and Coplan 2009, 85)

[93] (Baird 2004, 64–65)

which help engineers and scientists "do things."[94] This type of knowledge is not explanatory, but prescriptive. It does not so much tell us how the world works, but it does tell us how to interact with the world. Also note that it is not skill, although it is related to it. Someone might know how one should dice an onion, for example, but that does not mean that they are good at it. In other words, methodological knowledge is the verbalisable aspect of knowing how.

In a sense, methodological knowledge is an essential part of the experimental scientist's daily life: After all, as Hacking has pointed out, experimental science is not so much observing what is already out there, but rather intervening into nature and *making phenomena happen.*[95] Understood in this way, a scientist is not only an expert on a subject due to the theoretical knowledge they possess of it; They are also an expert due to the mastery they have over the techniques and know-how required to observe the phenomena they are interested in. The specific "instrument cookbook" by Moore *et al.* which Baird describes is an excellent example of methodological knowledge which is written down, and applicable to scientific and engineering environments across the fields of physics, chemistry and biology. And this kind of knowledge, Baird argues, differs from theoretical knowledge in the sense that it appeals to pragmatic rather than explanatory needs. Our methodological knowledge tells us how to "[accomplish] certain effects, which, as a matter of fact, have been found to be important in making things."[96] If we want to make a vacuum system, we cannot get there by only using thermodynamic theories, or only using theories of amorphous solids. These theories, while leading us in the right direction, will not tell us outright that Pyrex is a good material to make such a vacuum system. Instead, this is what methodological knowledge is for.

Now, this methodological knowledge allows us to bridge the gap from scientific concept to object fairly easily: If I want to build a vacuum chamber, I can read up on the relevant knowledge, and (with a bit of skill) put the right materials in the right composition so that it makes a vacuum chamber. This is what techniques are all about: They connect our intentions to actions which make sure these intentions are realised. Now which techniques are used differ between between contexts. When starting his stem cell research, Shinya Yamanaka had to read text books in order to learn how to dissect mouse embryos.[97] This is a kind of methodological knowledge which is highly specific to embryonic cell medical research. Obviously, Rod Lakes did not read any medical books in order to get the knowledge he needed: What he presumably did read was a work on the polymeric foaming process. The point should be obvious by now: Techniques are often highly specific towards a certain field of research. And since we have already seen that techniques allow us to realise our intentions, so to say, we can start seeing how the construction of an object comes about.

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[94] In this work, I will strictly refer to knowledge of the techniques used in the creation of scientific setups as *methodological knowledge*. I do this despite knowing that there are alternatives available: For Baird this kind of knowledge is part of a larger category of knowledge called *working knowledge* (Baird 2004, ch.3). In a response to Baird, Sebastian Kletzl has called it "engineer theories" (Kletzl 2014). Neither of these options have the neutrality I am looking for.

[95] (Hacking 1983)

[96] (Baird 2004, 66)

[97] (Yamanaka 2013, 13903)

## 5.3 The development of techiques

This cannot be all, however. For to apply existing techniques is one thing, but it will only help you to realise intentions which already have been realised before. And although you might be able to combine techniques in interesting ways, they generally do not result in novel, interesting behaviour. Lakes knew how to make polymer foams, but the mere creation of a polymer foam would not constitute anything interesting. Rather, he had the intention to create an *auxetic* foam, and for this yet unrealised intention there existed no technique. In other words, to realise the creation of a novel object, one needs more than just existing techniques. Rather, the only way in which such a realisation is possible is through the *development of new techniques*, the development of new methodological knowledge. For example, in his paper, Lakes achieved auxeticity in his polymer foam by heating it up to between 163º and 171ºC and compressing it. But by heating it up to a specific temperature and compressing the foam, his object achieved totally novel properties. Similarly, Von Neumann's approach to making his universal constructor presented an entirely novel method of constructing computational systems. This creation of new methodological knowledge is one of the main epistemic results in the development of any new object, and it paves the way for the creation of other, similar objects.

Now, this does not mean that this development of techniques is straightforward: Rather, as Andrew Pickering has pointed out, more often than not the things we build just do not work the way we want them to.[98] A telling example of his is the development of the "bubble chamber," a detector of fundamental particles, by Donald Glaser.[99] The idea of the bubble chamber seemed to be a relatively straightforward alteration of an earlier device called a "cloud chamber." However, during its development, Glaser kept running into what Pickering calls *resistances*, i.e. obstacles which blocked his process towards constructing a working instrument. Every time such a resistance would pop up, Glaser would deal with it by *accommodating* to it. In other words, he would develop some approach which would circumvent such obstacles and allow him to come closer to his goal of building a particle detector. Now these accommodations obviously affected the design of the detector at times, for example, by the materials it was made from.[100] At other times, however, the accommodations changed the intentions of Glaser himself. For example, while he started off with the intention to create a detector for detecting naturally occurring cosmic particles, along the way he changed his goals to creating a detector which could detect the artificially generated particles of a particle accelerator.[101] This was due to some properties of cosmic particles he could not accommodate to; His device was simply better off in a particle accelerator environment. What Pickering shows us with his example, is that creating an object is not merely a matter of applying

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[98] (Pickering 1995)

[99] (Pickering 1995, 39–50)

[100] More specifically, Glaser changed from high-density ether to lower density xenon when working out the right dimensions for his device. See (Pickering 1995, 44)

[101] (Pickering 1995, 42–43)

old techniques and developing new ones. Rather, the creative process changes both object and creator. And while Pickering speaks about the development of a scientific instrument, I will argue later on that the same goes for the development of prototypes in POC research. Moreover, the creative process Pickering talks about can also bring about knowledge on the phenomenon involved: For example, during a period when his bubble chamber did not manage to detect any of the signals it was supposed to, he reasoned that this was because his bubble chamber was "slower" than the lifetime of the signals these cosmic particles left behind.[102] This, then, placed an upper limit on the lifetime of the interaction between the cosmic particle and the bubble chamber. So while developing the bubble chamber, he had learned something about the particles which that bubble chamber was supposed to measure.

### 5.3.1 The Weak, the Strong, and the POC prototype

Now that we have looked into the process of creating objects in science in general, we can move on to what this means for POC research specifically. In the introduction of this work I have talked about the views of Ian Hacking on the creation of phenomena like elementary particles, and I have argued with Peter Kroes that there is a difference between *weak* and *strong* creation. The difference lies, practically speaking, in where the physical work of construction goes into: In the case of weak creation, not the object of interest itself, but rather its environment is constructed. In this way, the environment may create the boundary conditions for the object of interest to show itself. In the case of strong creation, the object of interest itself is constructed according to an intelligent design. Perhaps the object consists of multiple parts; Then those parts are individually created and organised so as to make the object of interest possible. Think of the example of a bike: Its frame, its steering wheel, and all of its gears need to be individually constructed and organised in an intentional way to make its existence possible. I have called POC research an example of the latter process in science: Its prototypes are created according to a pre-determined design. Lakes could not have created his auxetic foam by creating the right boundary conditions; He had to create it himself, perform the necessary material manipulations to get there. However, we will see from the following that this is not the whole truth. Because although the polymer foam is strongly created, its *auxetic behaviour* is weakly created. I conclude from this that there is no intelligent design of this behaviour. Thus, POC research prototypes are *strongly and weakly created at the same time.* I will elaborate on this point more a bit later. Before I do that, however, I need to further clarify a property of strongly created objects, namely that of the *mark of the author*.

Kroes argues that the intelligent design according to which strongly created objects are produced involves some decision-making process.[103] Since the problem-space in a design problem is usually huge (think of how many designs of a bike are possible!) one can not hope to find the best solution

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[102] (Pickering 1995, 42)
[103] (Kroes 2014)

to this problem. So instead, designers strive to find a satisfactory, rather than optimal solution. For example, one designs a bike which converts leg power to movement at a satisfactory efficiency, rather than the most optimal efficiency.[104] But there are many satisfactory solutions, and thus two designers might come up with two widely varying designs, due to them having taken different decisions during the process. Kroes, then, says that bikes made from these designs bear their author's marks, in the sense that some features of these bikes are due to decisions made by the author.[105] Now, these marks do not have to be so specific that one can identify the author based on them. But they are still due to what Kroes calls the "intentional history" of the object, that is, what choices the author made and why.[106] A weakly created object does not, on the other hand, bear its author's marks. The elementary particles created during collisions in the LHC cannot said to have an author, because their specific features are not the result of any decisions on the part of the experimenters involved.

If we relate this to POC research, we can clearly see that its prototypes do have a mark of the author: For example, Lakes' prototype could have been made out of a different polymer, or he could have decided to compress the material even further than he did. In Kendig's example of biofuel-producing cyanobacteria, the researchers specifically used the strain Synechocystic sp. PCC 6803.[107] They could have used a different strain, or opted to pursue a different biochemical pathway, and they would have ended up with a different cyanobacterium. Even Von Neumann could have given his universal constructor a different form. In this sense, these prototypes really are strongly created: They are the creative products of those researchers who made them. On the other hand, their *scientifically relevant* behaviour is weakly created. This behaviour stems from the conceptual roles of the placeholder. Auxeticity and the ability to produce biofuel; Those things are not the direct result of intelligent design, although the objects' design did lead to it. Those properties are rather a direct result of the laws governing the behaviour of the system, in the same way that the ferromagnetism of iron is a result of the way nature works. These properties can be related to the conceptual roles which make the prototype an instance of the scientific concept in the first place: Lakes' material is now called an auxetic material because it exhibits the properties we associate to our concept of auxetic materials. Insofar as the scientifically unimportant features of his material go (its size, its shape, and even its microscopic structure), Lakes is the author of his material. But insofar as Lakes' material is an auxetic material, he is not the author. Another way to see it is the creation of an experimental setup which creates the boundary conditions for Hacking's weak creation. Take the LHC once more, for example. Obviously the LHC has authors (a whole army of them, actually). After all, the LHC was created with the intention to form the boundary conditions to make high-energy particle collision possible. During its creation, choices

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[104] For an discussion on the process of design and the difference between optimal and satisfactory solutions, see (Simon 1981, ch.5)

[105] (Kroes 2014, 389)

[106] (Kroes 2014, 390)

[107] (Kendig 2016, 739)

were made which affect specific features of the LHC, for example the maximum energy at which it can perform collisions. As such, the LHC is fully strongly created. At the same time, the particles which arise out of the collisions done at the LHC are weakly created, for none of them bear the mark of the authors. Thus the LHC is a *strongly* created object which allows for the *weak* creation of particles. Lakes' auxetic foam does a similar thing, but instead of the creation of *external* weakly created objects, some of its *internal* features are weakly created. In this sense, it is the experimental setup and experimental object at the same time.

Now we can move on to the final part of the framework: The relation which is established between (placeholder) concept and prototype. I call this the *representation relation*, because it establishes that the concept is a mental representation of the prototype at hand. Until now, I have assumed that this representation relation has been established as soon as the prototype has been made. In practice, this is not the case; For although a creator might have convinced themselves that they have succeeded in creating an instance of the concept, they still need to convince the rest of the scientific community. This is done through empirical studies, which I will call, loosely following Elliott, *proof of concept demonstrations.*[108]

### 5.3.2 Demonstration of prototypes

Once a prototype has been developed it might establish a concept from the placeholder. However, its relation to this scientific concept is not necessarily established. What still needs to be established is the *representation relation*: The relation which shows that the scientific concept actually refers to the object at hand. In other words, one has to show that the prototype is an instance of the scientific concept. In practice, this means that some properties of the prototype have to be shown to correspond to the distinctive properties ascribed to the concept. This process I will call (following Elliott) *proof of concept demonstrations.*[109] As we know, Elliott distinguishes between physical prototypes and prototype models. The former might be demonstrated through a performance; After all, one can show others that the prototype works by showing it in action. More often, however, these demonstrations consist of chains of reasoning, relating empirical data to theoretical terms. These kinds of demonstrations can be published like any other scientific study, and they can be performed on material and nonmaterial prototypes alike.

At times, such a demonstration is practically pretty straightforward. For example, the Poisson's ratio is defined according to relative height and relative width.[110] Within the theory, these are conceptual terms; A theoretical material does not *really* have a height and width. However, these terms do already refer to properties of objects out there, and thus so does the Poisson's ratio: It can be described to any material for which we can compress and measure the height and width of. In this light, Lakes merely had to measure his material during compression, and then compare the

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[108] (Elliott 2021, 264)
[109] (Elliott 2021, 264)
[110] (Poisson 1827)

relative change in width to the relative change in height (assuming he compressed the material in this direction). All of these measurements are referred to by conceptual entities; Thus the only thing he needed to do was to divide the measured relative change in width by the measured relative change in height. If the value of this calculation was negative, he had succeeded in making a negative Poisson's ratio material.

At other times, however, it takes a few more empirical steps to establish representation. In Shinya Yamanaka's research, for instance, pluripotency is not already related to any observable property of material cells. Rather, conceptually, pluripotency implies that these cells are able to differentiate into different bodily cells. Now, these bodily cells do have properties which can be readily measured. So, Yamanaka showed the pluripotency of these cells by injecting them in living mice and examining the different types of tissue these cells developed into.[111] Through histological examination, his research group was able to identify three germ layers (layers of cells which form in the embryo, and which eventually give rise to all different kinds of cells in an animal's body). Each of these types of tissue has a set of properties which can, in fact, be related to their conceptual representations. In this way, the cells Yamanaka created could be identified as pluripotent cells.

As we can see, the way these prototypes are identified as instances of the scientific concept at hand is by measuring properties which are known to relate to the specifically novel behaviour which the prototype is supposed to have. Note that these measured properties are not the direct result of the designer's choice: Lakes did not intend his material to have a Poisson's ratio of specifically -0.7, nor did Yamanaka intend his cells to develop specifically into the specific they did (although he did intend that they would develop into structures generally). In other words, the properties which are measured are the result of *weak* creation: They do not bear the mark of the author, and are not specifically determined by them. And this makes sense, because all of the strongly created features of the prototype are just that: Strongly created, determined by the author. Lakes could not determine the auxeticity of his material by investigating what polymer his material was made out of. It would be nonsensical; He already knew what it was made out of, and neither would this knowledge help him to determine the degree of auxeticity.

## 5.4 Evaluation of the framework

Until now, I have discussed how scientific concepts arise, how prototypes are created, and how a connection between them is established in POC research. All that is left to do now, is to use this framework to identify the epistemic products of this type of research. Before we do that, however, I would like to take a step back to evaluate the framework so far. Along the way, I will compare the framework with my earlier characterisation of POC research. Do my framework and my earlier characterisation share the same features? Can we even explain some of the features I have identified earlier on, using my framework? To repeat: The two features I have identified are 1. That it

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[111] (Yamanaka 2013, 13905)

constitutes a proof of possibility, and 2. That the creation of the prototype reflects back on the concept. Let us now see if these features can be found in my framework.

**Concepts** As we have seen, the content of a concept arises out of a process of theory change. During theoretical work, the investigation of existing concepts and relations brings about a need to establish a placeholder conceptual structure. This placeholder is defined according to the novel roles it plays within the theory: In other words, it stands in new relations to other concepts in the theory and their interrelations. If the placeholder can be identified with natural objects, such as a type of elementary particles, it becomes a concept for such objects through empirical observation. If it can only be exhibited by an artificial object, it becomes necessary to create that object. The relevant properties of this type of object is determined by the conceptual roles the placeholder plays within the theoretical system: The role which was most involved in the conceptual change will be the most prominent aspect of its behaviour. Moreover, the placeholder gets more defined into a concept through the identification of such an object. The concept which arises out of this process, then, is that of a type of object of which the most scientifically relevant properties are those related to conceptual roles most involved in theory change.

**Creation of prototype** Once a concept is articulated, the POC researcher will attempt to create a prototype which exhibits the type of behaviour which is defining of the placeholder. This creative process amounts to the **proof of possibility**, the first feature of POC research I have identified before. If the prototype is created, then that proves that the relevant properties are legitimate properties to be had by objects out there, and that it is possible to create instances of the placeholder. Since we are dealing with technical objects, we are dealing with a type of strong creation: The design of the object to be created is consciously decided upon, bears the mark of the author, and is potentially changeable. What is not changeable are the scientifically relevant properties it should have: If it is to be an instance of the concept in question, it should exhibit a certain type of behaviour. In this sense these properties are weakly created within the prototype, and are unchangeable. During the creation of the prototype, there are two sources of knowledge: There is the methodological knowledge generated out of the necessity of creating a novel object, and there is the theoretical knowledge about the properties which is generated at times of resistance: When the creative process does not go as planned, finding out the reason why usually leads to some knowledge about the behaviour one is interested in.

**Demonstration of prototype** Once the prototype is created, the POC researcher still needs to establish the representation relation. It is during this establishment that a placeholder is replaced by a concept. This takes place through conventional scientific practices. There are some properties ascribed to the concept due to its role in conceptual change. These are properties which define the category of objects circumscribed by the concept; If one can empirically establish these properties in the prototype, then one can establish the representation relation between the prototype and its concept. Once the representation relation is established, whatever findings one does on the prototype **reflect back** on the concept. Thus the representation relation results in the second characteristic of POC research I have identified. This reflecting back on the concept has an important consequence for the knowledge gained while doing research on the prototype: It means that anything we find out about the prototype will also be applicable to other instances of the concept. So if other instances of the concept will be created in the future, they will also benefit from the knowledge gained on the prototype. In other words, this proves that POC research is *projectable*, as Kendig has argued in her work on the topic.[112] However, while projectability for Kendig is an assumption held by the researchers involved, in this framework it is rather an inherent aspect of the POC research itself.

## 6 Epistemic effects

Now that the framework has been established, we can finally investigate how POC research produces knowledge. We have already seen some ways while I was filling out the framework. One of those ways, and the most obvious one, is through the *development of new techniques.* Since the prototype is inherently novel, new techniques have to be developed to make the creation of that prototype possible. These techniques can be shared like any other type of knowledge as *methodological knowledge.* In fact, techniques are often applicable more widely than just in their original context; As such, the development of these techniques constitutes genuine progress, also for science. Moreover, Andrew Pickering has shown us that empirical knowledge can also be generated during a creative process.[113] Creating an object is hard, especially in science. Even creation based on the most thought-out designs might fail somewhere along the way. It is in these episodes of failure that the scientist can learn something about the phenomenon they want to realise. Why did the object not work? The answer to that question often also contains some empirical knowledge about the phenomenon at hand.

But besides knowledge during creation, there are also ways in which POC research creates knowledge after the prototype has been created, and after the reference relation has been established. In this section, I would like to look at these ways. From the moment that the reference relation has been established, the scientific concept actually refers to a target system *out there.* This means, first of all, that "conventional" research can be performed on the prototype. Whatever knowledge one gains about the concrete object, at least to the extent that it is scientifically relevant, one can apply to the scientific concept. And due to projectability, this knowledge will apply to all future instances of that concept as well. By itself, the notion of projectability in this setting is not very surprising: After all, in conventional science theory is also generalised from individual observations, so there, too, knowledge gained on one individual is projectable to other individuals. However, as

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[112] (Kendig 2016)

[113] (Pickering 1995)

we will see, due to the unconventional nature of POC research, projectability in this context has some surprising effects. First of all, due to its artificiality, POC research has the potential to *create new fields of knowledge*, or new "epistemic categories" as Kendig calls them.[114] These epistemic categories can develop in unexpected ways: For while POC research aims to justify the scientific concepts it starts off from, the prototypes it produces might have unexpected properties. As such, it might create new knowledge in an unforeseen direction. Lastly, these epistemic categories do not just grow by themselves. The creation of a single prototype does not count for much: One might extract some information from it, but just the one prototype does not make an epistemic category. Rather, the category has to *be grown* through the continuous creation of new objects, new instances of the same concept. I will thus propose that POC research starts a sort of *feedback loop* through which new instances are created based on the successes of the previous instances.

## 6.1 Creation of new epistemic categories

Once the representation relation is proven, a prototype really represents a scientific concept. In that sense, whatever research is being done on the prototype will also reflect on the concept; In other words, research on the prototype is projectable. We have already seen this in Lakes' auxetic foam research: Aside from merely measuring the Poisson's ratio of his novel material, he also investigated some theoretical predicitons about the behaviour of these materials, like its shape when bending, or its mechanical strength. These measurements also reflect back on the concept, justify certain conceptual roles or create new ones, and will eventually form the basis for a new *epistemic category.* This notion, introduced by Kendig[115], is another important epistemic role of POC research. As discussed before, POC prototypes are a result of both weak and strong creation. The researcher does not have any freedom in designing the weakly created properties of the prototype, and although they are dependent on the structure of the prototype, they cannot be decided upon. On the other hand, strongly created properties *are* dependent on conscious decisions. Of course, these properties are still limited by pragmatic concerns, and by the fact that scientifically relevant properties have to be realised. There are also some other aspects influencing these decisions. They could be the technical background of a specific researcher, or the compatibility of the prototype within a larger experimental setup. Nonetheless, these strongly created features can be altered without impairing the reference relation. As such, the concept can be realised in any number of ways: It might have completely different strongly created features, as long as it has the right weakly created features, it will still be an instance of the concept, and knowledge will still be projectable to it and from it. This is what underlies an epistemic category. It is a body of research on different scientific objects, all of which are connected by the same basal concept. The field of auxetics is a perfect example of this: While the underlying materials and even working mechanisms which produce auxeticity

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[114] (Kendig 2016, 740)

[115] (Kendig 2016, 740)

differ from material to material, they are still referenced as a single category, on the basis of their property of auxeticity.[116]

## 6.2 Theory building within an epistemic category

Once an epistemic category has been established, it becomes a field of study in its own right. That is, since there are different instances of the concept, each of which have their own properties, theories can be developed which explain these properties. The various properties which were investigated in auxetic solids are one example of this; Thus the different properties *surrounding* auxeticity could be investigated. There are, however, also cases where the novel theory deepened understanding of the constitutive conceptual property *itself.* To show this, I will now present yet another example: The development of a theory on solar cells.

### The Shockley-Queisser limit

Although the concept of generating electricity from sunlight had been known for a long time, and working examples had actually been developed by the end of the 19th Century, these initial "photoelectric plates," which had been made of selenium, boasted an energy conversion efficiency of less than 1%.[117] Their low efficiency made the plates lose popularity quickly, and it was only after the invention of the transistor and its photovoltaic properties that the concept really gained traction.[118] The process through which these solar cells converted sunlight into electricity was predicted and explained by the photovoltaic theory developed by Einstein in 1905.[119] Thus, although photovoltaic plates had existed beforehand, the concept could really take the shape it had due to the theoretical background which was introduced then. Even more so when the transistor was invented, and p-n junction theory was developed by William Shockley.[120] This theory accounted for some characteristic electrical properties of semiconductors, when positively and negatively "doped" semiconductor material (e.g. silicon slightly contaminated by atoms with a positive or negative electric potential, respectively) are in contact with one another. So when the photovoltaic effect was identified in such p-n junction transistors at Bell labs in 1953, research quickly commenced to make solar cells using this type of material.[121] Using the techniques which were subsequently devised (such as putting the energy-harvesting interface close to the surface of the cell, or coating the surface to reduce reflectivity), it did not take long before a solar cell was developed which reached an efficiency of 6%, much higher than previous selenium cells did.

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[116] One need only take a look at the chapters in Lim (2015): "auxetic solids with cavities," "auxetic solids with rigid inclusions," and different auxetic shapes like beams, plates, spheres, and so on.

[117] (Perlin and Jacobson 2022, ch.21)

[118] For the development of the transistor, see Smits (1985). For silicon solar cells specifically, see (Perlin and Jacobson 2022, ch.21)

[119] (Einstein 1965)

[120] (Smits 1985, 22–23)

[121] (Perlin and Jacobson 2022, 353–54)

Thus at this point, we have a concept (a solar cell based on a p-n junction) and an instance of this concept (an–at the time–highly efficient solar cell made of silicon). But this was only the beginning: Over the following years, the technology was developed more and more, and by 1961 the p-n junction solar cell was declared to have "changed from a scientific curiosity to the only reliable long-term power source available to [the U.S.] space program."[122] It should be clear that by now, the "epistemic category" of p-n junction solar cells had been safely established. In this same year that William Shockley and Hans Queisser published their highly influential paper on the "detailed balance limit" of p-n junction solar cells.[123] This paper aims to give a theoretical limit on the efficiency of this kind of solar cells, starting with a few assumptions about them: For example, they are assumed to have the geometry of a flat plate, they operate by absorbing sunlight at an angle of incidence, and their working mechanism depends on the material properties of the p-n junction material. The result is a theoretical limit which should be true for *all* such single p-n junction solar cells, a limit which is still referred to to this day in photovoltaic research.[124] We can identify an interesting transformation here from the abstract concept into concrete instances, and then again into general abstractions in the form of theoretical knowledge. More importantly, however, we can see this development as the emergence of the p-n junction solar cell *as* an epistemic category. At one point in time the first p-n junction solar cell was made: This is where we can identify proof of concept research being done. This is also the start of a new epistemic category, one which would eventually culminate in the Shockley-Queisser limit. The story of the Shockley-Queisser limit shows us how the emergence of an epistemic category deepens our understanding of the underlying concept through an investigation of many different instances of such a concept. Thus, the result of POC research can be the emergence of a new epistemic category, which in turn results in more knowledge about the concept through research on the individual instances.

## 6.3 Unexpected properties

However, the epistemic categories do not always have to correspond to the original concept. In fact, sometimes the created instance has unexpected properties, which either promote novel research into that property, or aid in creating techniques which allow for new fields of research to be opened. An example of this is the usage of the scanning tunnelling microscope (STM) for atomic manipulation, as introduced in a famous paper by Eigler and Schweizer.[125]

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[122] (Rappaport 1961, 1303)

[123] (Shockley and Queisser 1961)

[124] The limit is specifically for solar cells which are knows as *single junction*: They only employ a single p-n junction, and as such, can only absorb light at a single frequency (which is a large reason for their limit on efficiency). Nowadays, many solar cells are multi-junction, and can thus absorb light at different frequencies. This means that the Shockley-Queisser limit does not hold in these cases, and as such it is used as more of a benchmark than a real theoretical limit.

[125] (Eigler and Schweizer 1990)

### Atomic manipulation

The original use of the STM, as developed by Gerd Binnig and Heinrich Rohrer, was to measure surfaces at the scale of individual atoms, by moving a sharp conducting tip at very close distances across a surface.[126] Although the original concept was a device which could perform local spectroscopy on very small length scales, very early in the development process Binnig and Rohrer realised that it could be used for microscopy as well. Since then, this has been the main usage of this device, as indicated by its name. An important feature of this device is that, while it is operating, there is a small force between the tip and the sample that is being measured. Usually this force is kept low, in order to prevent damaging the sample surface.[127] However, one can increase this force by adjusting the position of and voltage on the tip, and thus modify the sample surface on purpose. This usage (which was not, taken strictly, a scientifically relevant property during the development of the STM) had been realised by Becker, Golovchenko, and Swartzentruber and the technique was fully presented by Eigler and Schweizer.[128] The technique has since become a staple in the field of nanotechnology, in which the manipulation of single atoms is employed to create structures on the nanometer scale.[129] Thus while an object (in this case a scientific instrument, but the same goes for any POC prototype) is being developed, unexpected properties might be discovered, on which development and research might continue. The research project starts with the investigation of one concept, but finds out about another concept on the way. In this way, another epistemic category might branch out from the same prototype, revolving around the prototype as an instance of a completely different concept.

## 6.4 Feedback loop

To consolidate these earlier remarks on the epistemic effects of POC research, and as a culmination of what we have discussed in this work so far, I will develop a type of feedback loop here of the research folowing a POC research project. I hope the examples I have shown until now serve as strong enough evidence of its validity; If not, I invite the reader to look at any field of study which concerns itself with human-made objects, be it synthetic biology, much of chemistry, or computer science. The premise of this feedback loop is the following: 1. Articulation of a possible object based on available knowledge, 2. Creation of such object, 3. Investigation of novel object, creation of new knowledge, 4. Repeat. One might see in this a re-iteration of my POC research framework, and they will be right. In fact, it must be so. Since POC research deals with concepts without a target system, this target system continuously has to be created for the field to grow; There simply are no instances of the concept lying around. And since every new object is individually related to the concept, the development of these new objects will, to a large extent, take the form of POC

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[126] (Binnig and Rohrer 1987)

[127] (Binnig and Rohrer 1987)

[128] (Becker, Golovchenko, and Swartzentruber 1987), Eigler and Schweizer (1990)

[129] See (Kolb and Simeone 2005)

research.[130]

So when a possible object is articulated based on the available knowledge, it just needs to be created and empirically investigated. This leaves us with the question how these possible objects are articulated in the first place. Where do these new ideas come from? In the following I propose two pathways: Inspiration by objects, and inspiration by theory.

### 6.4.1 Inspiration through object functioning

First of all, the realised prototypes might inspire someone just through their functioning. In this case, the object itself provides the impetus for the creation of new objects. David Baird has provided an extensive account on this topic in his book *Thing Knowledge*.[131] He has argued that merely seeing a technical object function can bring about an idea of a new object. As an example of this, he takes the story of Thomas Davenport, who, at a time where electric motors were still in their infancy, managed to build a novel electric motor after seeing the demonstration of a powerful electromagnet.[132] Davenport was not well-versed in electromagnetic theory, and thus did not derive his motor from theoretical considerations. But he did know, somehow, to take what he had learned from the electromagnet and apply it to an entirely different object, the electric motor, in order to improve the latter relative to its forerunners. In a reaction to Baird, Sebastian Kletzl has put this inspirational power of objects in more concrete terms.[133] He makes the distinction between (scientific) explanatory theories and *engineer theories*. It is related to the methodological knowledge I have talked about before: The techniques used do not explain why something works, but rather how it works. Davenport definitely knew *how* to build an electric motor, otherwise he would never have been successful in creating one. Presumably, he had some feeling for what inputs were important to get the desired output: For example, a strong electromagnet would create a strong rotational force, which would result in a strong motor. He did need not need to know electromagnetic theory to understand this; He rather knew it through methodological knowledge. Similarly, a researcher seeing the behaviour of a POC research prototype might understand what it takes to improve that prototype. Perhaps someone would see Lakes' auxetic foam and realise that a different polymer would increase the foam's auxetic abilities, not because they knew the microstructure of the auxetic foam, but because they knew what type of material would be fit for the job from a methodological standpoint. Thus, without falling back on theory, one can nonetheless

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[130] Whether every new object created needs its reference relation established is actually not clear-cut. Because what about improvements of existing objects? If Lakes' would have made another set of samples of a slightly different polymer foam, with almost equal properties of his former samples, would his new samples be entirely new objects? Would the reference relation need to be established again? I will side-step these questions here, by assuming that the reference relation does, indeed need to be established every time. If there are times where it does not: No matter, the gist of the feedback loop remains the same.

[131] (Baird 2004). In his book, he argues for the existence of *thing knowledge*, which is a type of knowledge developed during object creation, and is embodied by these objects. Baird's argument is nuanced and somewhat hard to grasp. It is also outside of the scope of this work, so I will not discuss its validity here.

[132] (Baird 2004, 23–24)

[133] (Kletzl 2014)

change or improve a prototype, and thus provide a new instance of the scientific concept. Note that, while this is best understandable in the case of material objects, the same thing can happen with nonmaterial prototypes. Von Neumann's universal constructor is an example of a cellular automaton. And although Von Neumann's work was very theoretically underpinned, one can actually "play around" with other cellular automata like John Conway's "Game of Life."[134] One can create structures which perform certain functions, like expand, move in a specific direction, or even act as logic gates.[135] As such, one does not need to know a general theory of cellular automata to create interesting structures with it, just like one does not need to know the general theory of electromagnetism to create a novel powerful electric motor.

### 6.4.2 Inspiration through theory

The second pathway in which ideas for new objects can be developed is through theory. Theoretical work might suggest the possibility of a new object. I have argued that POC research acts as a proof of possibility and justifies the scientific concept at hand. It should not be surprising, then, that theoretical investigation concerning the concept will continue after the POC research is done. This theoretical investigation might then suggest improvements to the original prototype, or predict new behaviour in alternative structures. In other words, these predictions suggest novel ideas for prototypes, and they can be tested through regular POC research. If these predictions are justified in turn, then this might lead to further theoretical development in the field.

Thus we can see how an epistemic category based on POC research can grow: Starting from the initial prototype, ideas for new prototypes are then suggested either by applying methodological knowledge on the system, or by performing theoretical investigations. Once the idea is there, it can be created and investigated through empirical research. This process creates both methodological and theoretical knowledge, which provides a basis on which the process can start over again.

## 7 Conclusion

I started this work with Hacking's assertion that all phenomena are man-made. We soon found out that this statement could be more nuanced: What Hacking meant is that the environment in which phenomena show themselves is man-made, and that the phenomena are actually just weakly created. On the other hand, there are definite examples of research which actively concerns itself with the creation of man-made objects itself, in which the objects in question are strongly created. This statement, we have found out during over the course of this work, can also be more nuanced. Yes, sometimes scientists actually do strongly create their objects of empirical investigation. However, as we have seen, the behaviour which is scientifically relevant is, nonetheless, weakly created. This

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[134] For an overview of this cellular automaton, see (Gardner 1970).

[135] For an overview of interesting structures, see (Niemiec 2010).

is why it is interesting: Since fully strongly created objects are just the result of our own decisions, their behaviour does not usually surprise us. On the other hand, if we want our objects to exhibit behaviour we do not fully understand ourselves yet, we set ourselves up for surprise. In a way our experimental setups are the same: They are actually made to present us with phenomena, show us exactly those parts of nature or logic which we do not understand yet. If we look at it like this, POC research is not so unconventional after all. It merely concerns those phenomena which can only exhibit themselves within a strongly created object. However, at the same time, this fact makes all the difference. After all, it imbues the research with an extra dimension: That of strong creation. If every instance of a scientific object needs to be constructed, and if that process of construction brings with it its own peculiarities, then the world created by these objects will be fundamentally heterogeneous. Thus the fields of auxetic materials, photovoltaics, and cellular automata are all marked by heterogeneity, although each of their objects is connected through the same behaviour. Moreover, their heterogeneity allows for a large variety of perspectives on that behaviour: In other words, it allows us to investigate the scientifically relevant properties from all sides. On top of that, each of these objects have the ability to inspire us to change them in new ways. In this sense, they are the key to unlocking even more realms of knowledge. So the heterogeneity which POC research creates is not bad; Rather, it is an essential feature of the means by which it produces theoretical knowledge. Moreover, the creative aspect in POC research necessarily brings with it at least one more source of knowledge. After all, this type of research creates novel objects, and novel objects require novel techniques. These techniques become part of a larger body of methodological knowledge. While this is not directly relevant for scientific theorising, it does allow us to create more, different objects. And this is an essential feature of experimental science. As such, the generation of methodological knowledge should not be underestimated.

Finally, I want to highlight a feature of POC research I have scarcely mentioned throughout this thesis, but deserves a mention nonetheless. POC, when it was introduced by NASA, was meant as a part of technology development. It still is; To this day, NASA uses POC as an official measure of technology readiness. And while the literature on POC research has been relatively quiet on this aspect of POC research (me included), it does deserve a mention. Because novel objects might not only be interesting scientifically, they can also be interesting outside of science. And while new methodological knowledge can be used in the construction of a future experimental setup, it can also used outside of science. This is still one of the motivating factors for POC research: The potential practical usefulness of its products. And in this sense, POC research might be the best of both worlds: It combines the creation of knowledge with the creation of technology.

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