The Role of Higher Education in Shaping Students' Policy Preferences: Does exposure to the assumptions prevalent in economics and sociology curricula prime attributions of poverty?

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Abstract

A survey experiment was conducted to study to what extent the assumptions about what drives human behavior in neoclassical economic theory and classical sociological theory drive attribution of poverty. Poverty attributions were divided into internal and external attribution. It was hypothesized that respondents primed by the economic assumptions will attribute poverty to the individual while respondents primed by the sociology assumptions will attribute poverty to external causes. No average treatment effect was found. However, significant heterogenous treatment effects were found for the effect of sociology on internal attribution among future social science and STEM students.

Keywords: political socialization, poverty attributions, higher education, experimental research

1. Introduction

Due to the expansion of access to higher education in the second half of the last century, the proportion of highly educated citizens has increased substantially in many parts of the West (Van de Werfhorst & Kraaykamp, 2001; Bovens & Wille, 2017; Ford & Jennings, 2020). As the average level of educational attainment increased, differences in policy preferences between the highly educated and the lower educated have become salient (Stubager, 2009; Stubager, 2010; Stubager, 2013; Bovens & Wille, 2017; Ford & Jennings, 2020). Generally, the higher educated tend to be more supportive of a liberal approach toward cultural issues such as immigration, ethnic diversity, and European integration than the lower educated (Campbell & Horowitz, 2016; Bovens & Wille, 2017).

Interestingly, while attitudinal differences towards economic issues such as taxation and redistribution are marginal and often inconclusive between the higher and the lower educated (Aaldering, 2017; Bovens & Wille, 2017), the same issues call forth very different attitudes between subgroups of the higher educated. (Elchardus & Spruyt, 2009; Huckestein et al., 2018; Damhuis, 2020). Concretely, studies in different contexts have shown that students of the social sciences and humanities are less supportive of conservative economic policies and more supportive of redistribution than students in economics, business administration or law (Ekehammar et al., 1987; Nakhaie and Brym, 1999; Elchardus and Spruyt, 2008; Goossens and Méons, 2016; Fischer et al., 2017; Muheljic and Drace, 2017; Denzler and Wolter, 2018; Van de Werfhorst, 2019; Damhuis, 2020; Lindov, 2020).

Research into public opinion on economic policies suggests that people's policy positions towards economic issues are strongly related to and can even be predicted by their poverty attributions (Bullock et al., 2003; Norcia et al., 2010; Bergmann & Todd, 2019; Piff et al., 2020;

Marquis and Rosset, 2021) or, in other words, their "beliefs about the factors that cause some people to become wealthy and others poor" (Smith & Stone, 1989: 93).

This finding holds true not only for the general population but for university students as well (Griffin & Oheneba-Sakyi, 1993; Bullock et al., 2003; Bobbio et al., 2010; Bergmann & Todd, 2019; Piff et al., 2020) and is further linked to academic discipline. Concretely, economics students have been shown to be more likely to blame the individual for financial hardship and support conservative economic policies while social science students tend to attribute poverty to external causes and support progressive economic policies (Guimond & Palmer, 1989; Guimond & Palmer, 1996; McWha & Carr, 2009).

These differences between the disciplines have, on the one hand, been shown to be due to students selecting into fields that align with their political attitudes and corresponding policy preferences (Ekehammar et al., 1987; Elchardus & Spruyt, 2008; Denzler & Wolter, 2018) and, on the other hand, due to secondary socialization, that is students' socialization into a worldview that aligns with their chosen discipline while studying (Guimond & Palmer, 1989; Guimond & Palmer, 1996; Hastie, 2007; Stubager, 2008; Muhelijc & Drace, 2017; Lindov, 2020). While contact with peers (Mendelberg et al., 2017; Wang et al., 2020; Strother et al., 2021) and faculty members (La Falze & Gomez, 2007; Mariai & Hewitt, 2008; Van de Werfhorst, 2019) might play an important role in students' secondary socialization into a discipline-dependent worldview and public policy attitudes, students' continuous exposure to their chosen discipline's course content appears to exert the strongest influence on the development of students' policy preferences (Guimond & Palmer, 1996; Goossens & Méon, 2015; Fischer et al., 2017; Lindov, 2020).

However, the studies suggesting that course content is a more potent driver of changes in students' policy preferences than peers and faculty members make this claim based on the

exclusion of peer and faculty influence rather than the demonstration of the influence of course content (see Goossens & Méon, 2015; Fischer et al., 2017; Lindov, 2020). Thus, the debate around the effect of higher education on students' policy preferences would benefit from research singling out the effect of course content.

1.1. Research Aim and Research Question

This present study aims to test the effect of course content on students' attribution of poverty and thereby add to the debate around selection and secondary socialization effects and contribute to closing the gap left by studies claiming to have found evidence for the influence of course content on policy preferences by simply excluding other possible explanatory variables for their findings (see Goossens and Méon, 2015; Fischer et al., 2017; Lindov, 2020). More concretely, this research aims to study to what extent the assumptions of what drives human behavior that can be found in economics and sociology curricula affect students' attribution of poverty.

Poverty attributions are not only predictors of policy preferences towards economic issues (Bullock et al., 2003; Norcia et al., 2010; Bergmann & Todd, 2019; Piff et al., 2020; Marquis and Rosset, 2021) but have also been shown to be malleable (McWha & Carr, 2009; Piff et al., 2020; Marquis & Rosset, 2021) and can therefore be considered a potential pathway by which socialization through course content influences policy preferences.

To examine the effect of course content on attribution of poverty, I employ a survey experiment in which I test the extent to which the assumptions about what drives human behavior in neoclassical economic theory and classical sociological theory prime attitudes toward the attribution of poverty. As Neff and Albertson (2020) argue, the discipline-specific assumptions about the world that are implicit in the knowledge students gain in the classroom are decisive in

students' assessment of policies as these assumptions determine which knowledge is prioritized or even paid attention to in the evaluation of a policy. Moreover, if these assumptions remain implicit and, thus unchallenged, their effect on students' beliefs and subsequent policy evaluations remains strong (Dasgupta, 2017; Neff and Albertson, 2020). In other words, it is less the factual knowledge taught in university courses and more the assumptions underlying the factual knowledge and deciding which knowledge is taught, that is believed to affect students' beliefs and policy preferences (Guimond & Palmer, 1989; McWha & Carr, 2009; Norcia et al., 2010; Neff & Albertson, 2020). Thus, by singling out the effect of discipline-specific assumptions, this study can be expected to provide insights into the effect of course content on policy attitudes more generally.

Moreover, albeit economic and sociology university programs usually cover many streams of economic and sociology theory, most globally standardized introductory courses to economics are to a large extent based on neoclassical economic theory (Kirchberg, 2007; Gowdy et al., 2010) and most introductory textbooks to sociology draw heavily on classical sociological theory (Leahy, 2012). While there is currently no text analysis examining how prevalent classical sociological theory is in Dutch sociology textbooks, an assessment of an introductory textbook to sociology that is widely used in Dutch universities confirms that the assumptions of classical sociology, namely the influence of society on the individual, are still prevalent in much of the Dutch sociology course content (Appendix I).

Thus, singling out the effect of the fundamental assumptions on which these neoclassical economics and classical sociology are based, and which students are exposed to when studying in these fields, can therefore be expected to give insights into how general economics and sociology curricula affect students' attitudes.

It follows that the main research question which this research attempts to answer is: *To what extent do the assumptions about what drives human behavior in neoclassical economic theory and classical sociological theory prime respondents' attributions of poverty?*

The main research question contains the following theoretical subquestions.

The first theoretical subquestion reads: *What support exists for priming effects of course content?* This section deals with the assumed mechanism through which course content can affect poverty attributions. To answer the first theoretical subquestion, I will make use of previous studies to argue that priming is a likely mechanism by which course content can affect students' political attitudes.

The first subquestion is followed by: *What is the link between attribution of poverty and academic discipline?* This question will be answered by conceptualizing attribution of poverty and subsequently discussing the link between poverty attributions and two academic disciplines, namely economics and sociology, taking into account the debate around selection and socialization effects.

Lastly, the final part of the theoretical framework will answer the question: *How can course content be expected to prime attribution of poverty?* This part ties the previous two together and will argue for the main thesis put forward in this research which is that assumptions about what drives human behavior in economics and sociology course content prime students' poverty attributions.

After the theoretical framework, I will turn to the empirical part of this study. The question which will be answered empirically overlaps with the main research question and is therefore not reiterated here.

The remainder of this thesis is structured as follows. First, the theoretical subquestions will be answered in the theoretical framework from which the hypotheses are drawn. Then, the empirical strategy, that is the methodology, which is employed to answer the main research question is presented and argued for. Lastly, the results of the empirical strategy are provided, followed by a discussion of the results and a conclusion.

2. Theoretical Framework and Hypotheses

In the following section, I will provide the theoretical framework in which I will (1) discuss priming effects of course content (2) define poverty attributions and link them to academic discipline, and (3) argue that the assumptions about what drives human behavior in economics and sociology course content can prime students' poverty attributions.

2.1. Priming Effects of Course Content on Students' Attitudes

Priming refers to the activation of mental representation which subsequently influence attitudes or even behavior (Engeser et al., 2016). In other words, a stimulus, i.e., prime, makes a concept that is already present in long-term memory more likely to come to mind than other concepts (Doyen, 2014). For a short time after the stimulus, the activated concept takes precedence over other concepts in the evaluation of objects. Thus, the considerations associated with the activated concept are used to assess whatever object or question the individual is presented with. In this way, priming can momentarily influence attitudes (Cassino & Erisen, 2010; Parker et al., 2018).

Priming effects in course content, whether in secondary or higher education, has elicited much interest and generated some interesting results. For instance, Parker et al., (2018) show how

images found in medical textbooks prime students' attitudes towards gender. The authors found that even short-term exposure to stereotypical pictures of men and women increases respondents' implicit gender biases. Similarly, Donovan et al., (2019) find that reading about biological sex differences in humans and plants increases students' beliefs in neurogenetic essentialism. Engeser and Bauman (2014) and Engeser et al., (2016) demonstrate that exposure to achievement primes in real-world textbooks improves students' achievement in subsequent tasks.

All these studies argue that, although the demonstrated effect is only short-lived, consistent exposure in the real world to the same or similar stimulus over the course of an educational program can be expected to have a more long-lasting effect. Thus, this evidence suggests that course content has the potential to prime students' various attitudes (e.g., Engeser & Bauman, 2014; Engeser et al., 2016; Parker et al., 2018; Donovan et al., 2019).

Moreover, while there is currently no study focusing on the priming effects of course content on policy preferences, there is an ample amount of research demonstrating *that* political attitudes can be primed through semantic primes (e.g., Cassino & Erisen, 2010; Dragojlovic, 2011). Thus, because course content has been shown to prime students' attitudes (e.g., Engeser & Baumann, 2014; Engeser et al., 2016; Parker et al., 2018; Donovan et al., 2019) and other forms of semantic that is textual primes have been shown to prime political attitudes (e.g., Cassino & Erisen, 2010; Dragojlovic, 2011), it can be assumed that semantic primes present in course content can prime students' political attitudes.

2.2. Attribution of Poverty and Academic Discipline

Attribution of poverty refers to people's "beliefs about the factors that cause some people to become wealthy and others poor" (Smith & Stone, 1989: 93). Attribution research has

traditionally focused on factors that are distinguished by whether they are individualistic, that is internal to the individual, or structural, that is external to the individual (Griffin & Ohenba-Sakyi, 1993). Some studies identified a third category, namely fate, referring to uncontrollable events as causes of poverty (Pandey et al., 1982; Cozzarelli et al., 2001; Lepianka et al., 2009; Marquis & Rosset, 2021).

While the current body of literature on poverty attribution lacks clear evidence for what exactly constitutes the fate category, less disagreement exists regarding the role of poverty attributions in shaping policy preferences regarding economic issues (Bullock et al., 2003; Sabbagh and Vanhuysse, 2006; Norcia et al., 2010; Schneider & Castillo, 2015; Bergmann & Todd, 2019; Marquis & Rosset, 2021). In fact, while nearly all studies dealing with poverty attributions and policy positions find a significant relationship between the two (Lepianka et al., 2009; Bobbio et al., 2010; Marquis & Rosset, 2021), many of these studies theoretically claim or empirically demonstrate the influence of the former unto the latter (Bullock et al., 2003; Sabbagh and Vanhuysse, 2006; Norcia et al., 2010; Schneider & Castillo, 2015; Bergmann & Todd, 2019; Piff et al., 2020).

Concretely, believing that the individual is to blame for financial hardship makes one significantly more likely to support conservative economic policies aimed at minimizing state intervention and limiting welfare state spendings (Sabbagh and Vanhuysse, 2006; Norcia et al., 2010; Bergmann & Todd, 2019; Piff et al., 2020; Marquis and Rosset, 2021). On the other hand, attributing poverty to society and causes external to the individual predicts support for progressive economic policies such as extensive welfare state programs and redistribution (Sabbagh and Vanhuysse, 2006; Norcia et al., 2010; Bergmann & Todd, 2019; Piff et al., 2020; Marquis and Rosset, 2021).

The same pattern has been found among students with the additional relation to academic discipline. Concretely, economics students have been shown to be more likely to blame the individual for financial hardship and support conservative economic policies while social science students tend to attribute poverty to external causes and support progressive economic policies (Guimond & Palmer, 1989; Guimond & Palmer, 1996; Bullock et al., 2003; McWha & Carr, 2009; Bullock et al., 2003; Norcia et al., 2010; Bergmann & Todd, 2019; Piff et al., 2020).

However, as is the case with other political attitudes, the reason for the link between poverty attributions and academic discipline has not yet been fully explored. On the one hand, much convincible evidence suggests that students select into fields that align with their policy preferences and poverty attributions. In fact, most studies examining the topic find evidence for selection effects (e.g., Ekehammar et al., 1987; Elchardus & Spruyt, 2008; Denzler & Wolter, 2018).

Yet, many of these studies find additional evidence for socialization effects as students' political attitudes intensify or change in alignment with the dominant views in their field as they progress through their studies (Guimond & Palmer, 1989; Guimond & Palmer, 1996; Hastie, 2007; Stubager, 2008; Muhelijc & Drace, 2017; Lindov, 2020). A promising but so far understudied source of socialization effects is university course content. For instance, Bullock et al. (2003) and Piff et al. (2020) found evidence for the relationship between individualistic poverty attributions and support for neoclassical economic positions suggesting a relationship between the course content taught in higher education economics classes and poverty attributions. Similarly, McWha and Carr (2009) find that social science students are more likely to attribute poverty to societal causes and argue that this is due to their course content focusing heavily on the influence of society on human behavior

2.3. Attribution of Poverty and its Relation to Assumptions of Drives Human Behavior

Given that course content, that is the content taught in university courses, appears to exert a stronger influence on students' policy preferences and poverty attributions (Guimond & Palmer, 1996), than other forms of socialization (Goossens & Méon, 2015; Fischer et al., 2017; Muheljic & Drace, 2017; Neff & Albertson; Lindov, 2020; Brocic & Miles, 2021), it is worth exploring what specifically in the course content could affect poverty attributions and policy preferences. Theoretically, the content taught in university programs can be informational, conveying knowledge and factual elements to students (Goossens & Méon, 2015; Fischer et al., 2017), and normative, that is transmitting values and a worldview, that is a set of assumptions about the world, to students (Muheljic & Drace, 2017; Neff & Albertson, 2020; Brocic & Miles, 2021).

However, factual knowledge and worldview do not coincide independent of each other. Rather, the worldview in which a discipline is rooted determines which knowledge is conveyed to students studying the discipline and how this knowledge is expected to be applied (Guimond & Palmer, 1989; McWha & Carr, 2009; Norcia et al., 2010; Neff & Albertson, 2020). It follows that while it is possible that it is through the conscious acquisition and processing of knowledge and facts that students change their political attitude or the intensity thereof, it is potentially more likely that the assumptions about the world implicit in the knowledge and information taught have a greater impact as students are socialized into these assumptions and might eventually take them on as their own (Dasgupta, 2013; Neff & Albertson, 2020).

Thus, examining the assumptions on which course content is based, and which the same content is thus likely to pass on to students, constitutes a promising path towards identifying what in course curricula affects students' poverty attributions. Given that research has shown how

economics and social science courses socialize students into different, nearly opposing, political attitudes (Guimond & Palmer, 1985; Guimond et al., 1989; Elchardus and Spruyt, 2008; Méon & Goossens, 2015; Fischer et al., 2017; Lindov, 2020; Brocic & Miles, 2021), these two study fields seem the most likely areas in which effects can be expected to be observed.

Economics and social science have in common that they both attempt to explain human behavior (Dolfin et al., 2017). However, their archetypes, in which much of their current forms are rooted, make significantly different assumptions about what drives human behavior (Grunert et al., 2012; Leahy, 2012).

To begin with, while there are numerous branches of economic theory, including flourishing fields such as behavioral economics which assume that humans are social beings and consequentially prone to biased perception and faulty decision-making (Etzioni, 2010), much of the materials used in university economics classes, and thus university course content, draw heavily on a contrasting economic theory, that is neoclassical economic theory (Kirchberg, 2007; Gowdy et al., 2010).

Neoclassical economic theory assumes that human behavior is driven by rational decision-making (Arrow, 1986; Kopcke et al., 2004; Grunert et al., 2012). Having at first focused specifically on individual choices regarding the consumption of goods and services, neoclassical economic theory presumes that individuals will always choose the option that will maximize their own utility (Gowdy et al., 2009; Manner & Gowdy, 2010; Grunert et al., 2012). At the center of this assumption stands the homo economicus, the individual whose decision-making is free of social constraints and who is fully informed about the costs and benefits of an option and uses this knowledge to weigh costs against benefits (Kopcke et al., 2004; Gowdy et al., 2009; Grunert et al., 2012).

While the assumption that individuals weigh costs against benefits to make a choice that increases their personal wellbeing, also referred to as rational choice theory, emerged from economic thinking about consumption, it has long been argued to be the primary explanation for other types of behavior too (Scott, 2000; Hodgson, 2007). The assumption at the base of all forms of rational choice theory is, according to Scott (2000) "that complex social phenomena can be explained in terms of the individual actions of which they are composed." (2). A standpoint which is also referred to as methodological individualism (Scott, 2000). Thus, placing the independent individual at the center, neoclassical economic theory leaves little to no room for structural or cultural explanations of human behavior (Scott, 2000; Kopcke et al., 2004; Gowdy et al., 2009).

While current sociology, much like contemporary economic ideas, hosts a diverse set of theories and branches including the above-mentioned methodological individualism headed by important sociologists such as Weber and Boudon (Udehn, 2002; Coenen-Huther, 2019; Borlandi, 2020; Neck, 2021), much of today's university course content is still rooted in the assumptions of classical sociological theory (Leahy, 2012). In contrast to the core ideas of methodological individualism, classical sociology assumes that human behavior is largely driven by social structures (Kirchberg, 2007; Leahy, 2012; Polavieja, 2014). Concretely, classical sociology assumes that individuals internalize structural social norms, prescribed by their immediate social group as well as macro-level societal expectations, and make decisions in accordance with these norms (Kirchberg, 2007, Polavieja, 2014). By subconsciously making decisions in accordance with social norms, individual behavior is thus motivated and even guided by external influences. In other words, classical sociology assumes that an individual's immediate and extended social environment, that is the immediate peer group as well as the society and culture in which the individual is placed, prescribe the considerations an individual draws on when making a choice

(Harding, 2007; Adamczyk & Hayes, 2012; Polavieja, 2014). These considerations are then used to make a choice and eventually translated into behavior. Thus, in the sociological view, individual decision-making is not at all free from social constraints as homo economicus suggests. Instead, the individual, or Dahrendorf's homo sociologicus (Kirchberg, 2007), is in fact motivated and even guided by the social environment and society at large.

In sum, while both economics and sociology have the ambition to explain human behavior, the two disciplines do so based on fundamentally different assumptions (Kirshberg, 2007). The three key assumptions prevalent in neoclassical economic theory are that human behavior is driven by rational decision-making, free of social constraints, and takes places through the careful weighing of costs against benefits. In classical sociology, in contrast, it is assumed that human behavior is strongly influenced by the social environment and society as people internalize the structural norms prescribed by society and make decisions accordingly.

Thus, following the reasoning of these scholars, I hypothesize that the assumptions about human behavior implicit in economics course content and sociology course content, prime students' attributions of poverty.

Concretely, the hypotheses which this research is testing are:

H1: *The assumptions about what drives human behavior in neoclassical economic theory prime students to attribute poverty to individualistic factors.*

H2: *The assumptions about what drives human behavior in classical sociological theory prime students to attribute poverty to structural factors*.

3. Research Design

In the following part, I will detail and argue for the research design choices I have made in order to study the extent to which the assumptions of what drives human behavior, as can be found in economics and sociology curricula, affect attribution of poverty. I will begin by presenting the research case and the target population, continue by detailing the data collection process including the chosen method and the data collection instrument, and lastly, explain how the data was analyzed.

3.1. Case Selection

3.1.1. Least Likely Case

The Netherlands was chosen as the case for this study as it constitutes a least likely case. Least likely cases are a type of crucial cases which are useful for hypothesis testing (Levy, 2008; Blatter & Haverland, 2012; Halperin & Heath, 2020). Concretely, a case is a least likely case when theory suggests it to be unlikely that a sought effect will be found in the particular case, either because necessary conditions are not fully satisfied or because prior research suggests that many key variables point into the opposite direction of what a researcher hopes to find (Levy, 2008). Thus, if an effect is found in a least likely case, it can theoretically be expected in other, more likely cases too and thus extends and strengthens theory (Levy, 2008; Blatter & Haverland, 2012; Halperin & Heath, 2020).

The Netherlands can be considered a least likely case as the available evidence challenges that socialization effects in higher education exist in the Dutch context (Delis et al., 2017; Kunst, 2020). Concretely, the longitudinal observational studies conducted in the Netherlands found attitudinal differences between students and graduates of the different disciplines but no significant

socialization effect of economics and social science classes on cultural (Kunst, 2020) or economic issues (Delia et al., 2017). Thus, if priming effects of course content can be found in the Netherlands, where there is currently no evidence for socialization (Delis et al., 2017; Kunst, 2020), the effect is likely to be found in contexts with more favorable conditions.

3.1.2. Target Population

The choice of target population for this study is inferred from the theoretical expectations driving the research. Theoretically, the social group expected to be most influenced by the assumptions about what drives human behavior in neoclassical economic theory and classical sociological theory are university students in economics and sociology who are consistently exposed to these assumptions over the course of their studies (Hastie, 2007; Stubager, 2008; Muhelijc and Drace, 2017; Lindov, 2020; Gills and Morgan, 2021).

However, drawing a sample from the student population is likely to pose a threat to this study's internal validity. Concretely, since the effect of course content is precisely the effect this study seeks to imitate, examining respondents who have already been exposed to course content will throw doubts on whether the experimental treatment is responsible for any found effect instead of students' preexposure (Gaines & Kulinsky, 2011). Indeed, "as long as there is some possibility that experimental subjects arrive already having been exposed to the actual treatment being simulated, the experiment estimates not the average treatment effect, but, rather, the average marginal effect of additional treatment" (Gaines & Kulinsky, 2011: 564).

Thus, the more suitable target population for this research are future students, that is final year high school students or high school graduates who will likely begin university studies but have not yet been exposed to university course content. Importantly, since the bulk of research

indicates that selection effects weigh stronger than socialization effects (Ekehammar et al., 1987; Guimond & Palmer, 1989; Guimond & Palmer, 1996; Hastie, 2007; Elchardus & Spruyt, 2008; Stubager, 2008; Muhelijc & Drace, 2017; Denzler & Wolter, 2018; Lindov, 2020), this study will draw a sample of future students headed for a diverse set of disciplines, use randomization to cancel out selection effects and thereby enhance confidence in potential causal relations between treatment and outcome.

Moreover, focusing on the Dutch educational context, requires considering the particular content of Dutch high school education and its meaning for this research. Concretely, while Dutch high school students have not yet attended university-level classes, this group has likely, at least to some extent, already been exposed to assumptions about what drives human behavior in economics and sociology. Indeed, high school education under VWO includes compulsory social science classes (maatschappijleer) in which the main assumptions of classical sociological theory identified in the theoretical framework are likely brought up (de Jong, 2021). In addition to the compulsory basic social science classes, Dutch VWO students can take optional advanced social science classes as well as optional economics or business classes which further expose some students to the assumptions of interest in this study.

Thus, Dutch pre-students coming from high school have likely been, albeit to a small degree, pretreated. However, given that the whole population has been exposed to compulsory basic social science, this exposure is unlikely to lead to different responses between the experimental groups. Rather, the results should be interpreted as additional, albeit potentially large, treatment effects because respondents did not arrive at the study as clean slates (Gaines & Kulinsky, 2011).

In addition and following Gaines & Kulinsky's (2011) advice, advanced social science and business education in high school will be treated as control variables and included in the questionnaire. Adding this variable makes it possible to explore heterogeneous treatment effects for the subgroups who have been more intensively pretreated than others and examine, for instance, if pretreatment with assumptions from economics affects respondents' reaction to the treatment and subsequent poverty attributions (Gaines & Kulinsky, 2011). Moreover, adding previous exposure as an additional variable makes it possible to account for the variation in attribution of poverty caused by the variation in intensive previous exposure if randomization failed to equally divide pretreated individuals over the experimental groups (Cramer, 2003).

3.2. Data Collection

3.2.1. Method

In order to study to what extent the assumptions of what drives human behavior as can be found in economics and sociology curricula affect attribution of poverty, this present research will employ a between-subject survey experiment. Survey experiments are "studies in which research subjects self-administer a survey instrument containing both the relevant experimental treatment and the outcome measures" (Mummolo and Peterson, 2019: 518). In line with other forms of experimental methodologies, survey experiments generally contain a control group and two or more treatment groups (Gaines et al., 2007; Druckman et al., 2011). Moreover, to be considered truly experimental, survey experiments entail "randomization of one or more features of the questionnaire" (Jilke & Van Ryzin, 2017: 120).

Since the aim of this research is to study the effect of discipline-specific assumptions of what drives human behavior on students' poverty attributions, in other words to establish a cause-

and effect relationship, an experimental design is well suited to reach this aim (Druckman et al., 2011; Anderson & Edwards, 2015). Experiments are the most used method to draw causal inferences as they are more likely than observational designs and other methods to yield reliable conclusions (Horiuchi, 2007; Druckman et al., 2011; Anderson & Edwards, 2015; Jilke & Van Ryzin, 2017). The unique features of true experimental designs, which are considered responsible for the method's potential to lead to causal claims, are a temporal distinction between cause and effect (Horiuchi, 2007; Druckman et al., 2011; Andersen & Edwards, 2015) and randomization of treatment making the "treatment group and the control group equal on average in terms of *all* (observed and unobserved) characteristics" (Horiuchi, 2007: 669).

Concretely, the temporal distinction between the studied cause and its expected effect allows the researcher to assume that the independent variable preceded the dependent variable making a causal relation more likely than in most cross-sectional designs (Gaines et al., 2007; Antonakis et al., 2010; Druckman et al., 2011; Andersen & Edwards, 2015). The temporal distinction between cause and effect in survey experiments is achieved through the order in which participants progress through the survey instrument which is predetermined by the researcher (Jilke & Van Ryzin, 2017).

Random assignment, as the defining feature of experiments, has the potential to reduce the problems associated with drawing causal inference as it allows the researcher to estimate the average treatment effect (Horiuchi et al., 2007; Antonakis et al., 2010; Druckman et al., 2011). In other words, random assignment makes it reasonable to assume that the control group behaves as the treatment group if the latter had not been treated (Druckman et al., 2011). The average treatment effect is estimated by comparing the average outcome of the control group to the average outcome of the treatment group (Gaines, et al., 2007; Druckman et al., 2011).

In addition to leveraging the advantages of experimental designs, survey experiments yield a distinct advantage over other forms of experiments such as laboratory or field experiments. Owing to their origin in traditional survey research, survey experiments can be distributed to a large number of respondents which makes it possible to administer the treatment to a sample representative of the population it was drawn from (Barbaras & Jerit, 2010; Jilke & Van Ryzin, 2017). Thus, survey experiments can be used to draw inferences about larger populations (Gaines et al., 2007; Barabas & Jerit, 2010; Jilke & Van Ryzin, 2017). Hence, as Jilke and Van Ryzin (2017) state "survey experiments combine the internal validity of experiments with the external validity of survey research" (120). Taken together, the advantages of survey experiments, namely high internal validity and generalizability, serve the purpose of this research well which is to establish a causal relationship that is supposed to represent a causal relationship in the real world.

Nevertheless, survey experiments have, as any other method, several drawbacks that need to be addressed in order to fully leverage the potential of survey experiments but also interpret the results they yield (Gaines et al., 2007). First, the treatment in survey experiments is often artificial and created to be powerful warranting the question to what extent comparable treatments can be found in the real world (Barabas & Jerit, 2010; Jilke & Van Ryzin, 2017). Similarly, survey experiments have been criticized for using stylized outcomes such as attitudes to draw inferences about actual behavior also putting into question a study's external validity (Jilke & Van Ryzin, 2017).

In addition, not only treatments and outcomes in survey experiments have been criticized for their lack of resemblance with the real world, also the context in which a treatment is administered might differ greatly from the context in which respondents are assumed to be exposed to the treatment in the real world (Barabas & Jerit, 2010). Concretely, the treatment in survey

experiments is often presented in a clean and distraction-free environment, for instance the questionnaire, which likely enhances respondents' attention but bears little resemblance with the real world in which few environments are free of distractions (Barabas & Jerit, 2010).

A third major limitation of survey experiments concerns the durability of found effects (Gaines et al., 2007; Jilke & Van Ryzin, 2017). Concretely, if the alteration of participants' perceptions or attitudes after exposure is only temporary, that is only for the seconds or minutes in which the experiment occurs, the meaningfulness of such findings for the real-world is arguably questionable (Gaines et al., 2007; Jilke & Van Ryzin, 2017). It is therefore problematic that many experimental studies in public administration claim the alteration of behavior through a treatment without measuring the longevity of the effect (Gaines et al., 2007; Jilke & Van Ryzin, 2017).

Arguably, this present study encounters all these weaknesses of survey experiments. While it is often not feasible to account for these weaknesses in the design, for instance by using treatment exactly as found in the real world or adding potential distraction to the presentation of the treatment, without comprising a study's internal validity, these weaknesses play an important role in how the findings of survey experiments, including the present study, can and should be interpreted (Barabas & Jerit, 2010). They will, therefore, be taken up in the discussion of these study's limitations.

3.2.2 Pilot Study

Pilot studies are strongly recommended in experimental research to test whether the research design works and, if needed, tweak it to better fit the research purposes (McDermott, 2011). Thus, I carried out a pilot study with a convenience sample of students at Utrecht University in early April 2022. Students were approached directly in the cafeteria and asked to participate by

scanning a QR code on their phone which resulted in 22 responses. After collection of the pilot data, I followed McDermott's (2011) advice and asked two students for qualitative feedback. The feedback resulted in some minor adjustments of the questionnaire which will be elaborated on below. In addition, neither of the students inferred the purpose of the treatment, suggesting that demand-effects are unlikely to occur in this research (Mummolo & Peterson, 2017).

3.2.3. Questionnaire

In the following I will explain and argue for the elements of the questionnaire which will be hosted on Qualtrics. The complete questionnaire can be found in Appendix II.

3.2.3.1. Treatment Variable and Conditions

The treatment variable in this present research are the assumptions of what drives human behavior in neoclassical economic theory and classical sociological theory. In order to study whether different assumptions of what drives human behavior have different effects on the dependent variable, that is attribution of poverty, I created two treatment condition and one control condition.

The economics condition consists of a short statement summarizing the key assumptions identified in the literature to be central to neoclassical economic theory. The sociology condition consists of a statement similar in set-up and length to that in the economics condition but consisting of the key assumptions identified in the literature as central in classical sociological theory. Both statements begin with the phrase "Most experts say" which represents how these assumptions are upheld by scholars, teachers, and other forms of authority such as textbooks in the two disciplines.

The control condition consists of a short, general statement which is not expected to significantly bias respondents' results as it highlights that there are multiple opinions regarding what drives human behavior without mentioning any of them. Thus, the statement does not contain a cue or stimuli that could make certain considerations more salient when assessing poverty attributions. If anything, the statement might encourage respondents to think of the plurality of opinions regarding human behavior which is unlikely to lead them to favor one type of attributions over the other.

Treatment A – economics condition

Most experts say that human behavior is driven by rational decision-making independent of social constraints. They claim that individuals will always weigh the costs against the benefits of each option and then choose the option that maximizes their own benefit.

Treatment B – sociology condition

Most experts say that human behavior is strongly influenced by their social environment and society at large. They claim that individuals internalize the norms prescribed by their peers and larger societal structures and then unconsciously use these norms when they make decisions.

Control Condition:

Most experts say that there are many different opinions regarding what drives human behavior.

3.2.3.2. Manipulation Check

As recommended by Kane and Barabas (2018), this experiment employs a factual manipulation check to assess respondents' attentiveness to the experimental treatment. Factual manipulation checks are used in experiments to test whether respondents have read, paid attention, and understood the treatment text (Kane & Barabas, 2018). Kane & Barabas (2018) recommend employing a treatment-relevant factual manipulation wherever possible, that is a question regarding the content of the treatment that can be objectively answered. Further, they recommend placing the factual manipulation check right after the outcome measure to avoid distortion of the measurements.

In this study, respondents were asked to reply to an objective question regarding the treatment placed right below the treatment text before the outcome measure. The question has only one correct answer which paraphrases parts of the experimental treatment and can easily be answered if one has read the text attentively (Appendix II). Although Kane & Barabas (2010) recommend placing the factual manipulation check right after the outcome measure to avoid distortion of the measurements, the manipulation check in this study was placed before the outcome measure. The reason for this placement of the manipulation check is that during the pilot study respondents reported to have forgotten about the text when the manipulation check appeared after the treatment text which would have likely resulted in a many respondents failing the manipulation check although they had been treated (Kane & Barabas, 2010). As a consequence, it is possible that respondents have been treated not only through the treatment itself but also through the manipulation check (Kane & Barabas, 2010). Nevertheless, as the treatment in this study can already be expected to be stronger than the stimuli in the real world which it seeks to imitate, it

does should not inflict additional harm on this study's external validity if respondents are treated through the manipulation check.

3.2.3.3. Dependent Variable and Outcome Measure

The dependent variable of interest in this study is attribution of responsibility for poverty. Attribution research has traditionally focused on factors that are distinguished by whether they are individualistic, that is internal to the individual, or structural, that is external to the individual (Griffin & Ohenba-Sakyi, 1993). Some studies identified a third category, namely fate, referring to uncontrollable events as causes of poverty (Pandey et al., 1982; Cozzarelli et al., 2001; Lepianka et al., 2009; Marquis & Rosset, 2021).

However, while the distinction between individualistic and structural factors as causes for poverty has been largely unquestioned, the fate category remains ambiguous. In fact, Cozzarelli et al. (2001) found no evidence in their work for the fate category and argued instead for the adoption of culture as a third category, an argument which also lacks support by subsequent studies. Building on Cozzarelli et al.'s (2001) work, for instance, Bullock et al. (2003) reported that individualistic causes and culture loaded on the same factors and so did structuralist causes and fate, putting into question both fate and culture as distinct categories. The authors suggest that it is not necessarily an objective question whether a cause for poverty is considered fate or not which makes it questionable to create a category comprising of items measuring the attribution of poverty to fate (Bullock et al., 2003).

Thus, despite the recurring argument that causes of poverty need to be distinguished by their degree of influenceability, most authors rely on the unquestioned external-internal divide if their study allows for such an arguably simplified approach (Guimond, Bégin et al., 1989;

Guimond & Palmer, 1996; Sabbagh and Vanhuysse, 2006; Bobbio et al., 2010; Norcia et al., 2010; Schneider & Castillo, 2015; Bergmann & Todd, 2019; Piff et al., 2021). Moreover, most studies treat external and internal attribution as two separate concepts rather than dichotomous opposites and thus measure them on two separate scales (Cozarelli et al., 2001; Sabbagh and Vanhuysse, 2006; Schneider & Castillo, 2015; Boeh et al., 2019; Pfiff et al., 2020). Therefore, and given that the aim of this study is to detect differences between groups in regard to the main object to which they attribute poverty regardless of any degree of control, this study will follow the greater bulk of research and focus on individualistic and structural factors as two distinct concepts. In doing so, this study reduces ambiguity from introducing fate as a third category.

To operationalize the two types of poverty attribution of interest, that is internal and external attribution, this research adopts the eight items used by Sabbagh and Vanhuysse (2006) to measure attribution to external and internal factors, namely *failed*, *laziness*, *lack of talent*, *lack of character*, *prejudice*, *limited opportunities*, *bad schools*, and *exploitation*. The measures by Sabbagh and Vanhuysse (2006) are valid and were tested using factor analysis. The items are worded in short statements and are to be assessed on a 5-point likert scale (1= not at all important, 5= extremely important). The wording for the items on the measurement scale is fully adopted from Sabbagh and Vanhuysse (2006). The question used by Sabbagh and Vanhuysse (2006) and adopted here is *"Thinking of persons who are unable to support themselves financially, how important would you say is each of the following causes in holding them back?"*.

The choice for the measures by Sabbagh and Vanhuysse (2006) for the purpose of this research is justified for at least two reasons. First, the items correspond to but are less comprehensive than other measures used in studies researching attribution of poverty (Cozzarelli, 2001; Bullock et al., 2003; Bergmann & Todd, 2019; Bobbio et al., 2010). In other words, Sabbagh

and Vanhuysse's (2006) items are fewer in number and more general than the scale used by most other authors (e.g., Cozzarelli, 2001; Bullock et al., 2003; Bergmann & Todd, 2019; Bobbio et al., 2010) while still referring to the same type of causes. Considering that the target population of this research, future students, has likely not had many touching points with many possible causes for poverty, such as difficult job markets or eroding industries (e.g., Cozzarelli, 2001; Bullock, et al., 2003) keeping the items general might aid respondents in their assessment of the items. Moreover, given that the purpose of this research is to study differences between groups and not to arrive at an accurate representation of attitudes regarding causes of unemployment, it is not necessary to apply a comprehensive measurement scale. Second, Sabbagh and Vanhuysse (2006) tested their scale on a comparable population as is targeted in this research, i.e., university students. Thus, the eight measures have been shown to be answerable by respondents similar to those in this research.

3.2.3.4. Experimental Groups

This study contains two treatments and calls therefore for two treatment conditions. However, as Gaines et al. (2007) and Horiuchi et al. (2007) argue, in order to increase confidence in the results of a survey experiment it is necessary to include a control condition without which there would be no baseline to which to compare any treatment effects. Thus, in addition to the two treatment groups, this study will include a control group who are asked to respond to the outcome measures after having been exposed to a general statement not likely to activate relevant prior knowledge. In line with the experimental approach which this research takes, allocation to the groups will be purely random and take place through the randomizer on Qualtrics.

	Economics Condition	Sociology Condition
Treatment Group 1	x	
Treatment Group 2		x
Control Group		

Table 1: Experimental Groups

3.2.3.5. Control Variables

In addition to the independent and the dependent variables, the study includes a number of control variable. However, instead of covariates which are quantitative variables expected to covary with the dependent variable (Cramer, 2003), this study contains categorical control variables holding information about respondents' membership in subgroups. These categorical variables reflect real subgroups in the population and can be used in two ways. First, these variables can be used to control for their effect in the analysis (Cramer, 2003). Second, categorical variables are suited to estimate heterogenous treatment effects (Brower, 2011; Gaines & Kulinsky, 2011; James, et al., 2017). To that end, the theoretical support for a potential effect of these subgroups on the dependent variable should be strong (Gaines & Kulinksy, 2011).

Thus, the first control variable reflects respondents' *future field of study* and controls for selection effects. In other words, while there is evidence pointing to socialization effects, the existence of selection effects is almost unquestioned (Ekehammar et al., 1987; Elchardus & Spruyt, 2008; Denzler & Wolter, 2018; Guimond & Palmer, 1989; Guimond & Palmer, 1996; Hastie, 2007; Stubager, 2008; Muhelijc & Drace, 2017; Van de Werfhorst, 2019; Lindov, 2020). Thus, this variable can be used to control for the variation in the dependent variable caused by selection effects and can further be employed to study the effect of the treatment on the subcategories of future study.

Second, as mentioned earlier, the target population of this research is likely pretreated (de Jong, 2021). While the majority, if not the whole, sample can be expected to have been exposed to the treatment in basic social science courses, subgroups in the sample might have experienced more exposure, and moreover exposure to assumptions in economics. Thus, the second control variable constitutes *prior courses.*

Third, students' political orientation is also a significant predictor of their poverty attributions (Guimond & Palmer, 1985; Guimond et al., 1989; Elchardus and Spruyt, 2008; Méon & Goossens, 2015; Fischer et al., 2017; Lindov, 2020: Brocic & Miles, 2021). Concretely, left-leaning individuals tend attribute poverty to external causes while, those leaning towards the right are more likely to attribute poverty to internal causes (Sabbagh and Vanhuysse, 2006; Norcia et al., 2010; Bergmann & Todd, 2019; Piff et al., 2020; Marquis and Rosset, 2021). Thus, respondents' *political orientation* is controlled for by asking them which party they would vote for if election was today.

Next *socio-economics background* is strongly correlated with attributions of poverty. Concretely, individuals from lower social classes are more likely to attribute poverty to structural factors than are members of the higher social classes (Guimond et al., 1989; Bendassolli et al., 2015). Given that respondents in this research are likely young and without prior work experience, the questionnaire will ask for the profession of their caretakers to make it possible to control for socio-economic background.

Lastly, *age* and *gender* have been associated with attitudes towards poverty (Guimond et al., 1989; Sabbagh & Vanhuysse, 2006). Concretely, women tend to consider external causes more than men when attributing poverty (Guimond et al., 1989) and older people are more likely to

blame the individual for poverty than younger people (Guimond et al., 1989; Sabbagh & Vanhuysse, 2006).

In addition, it cannot be assumed that all attendees at university open days or student-for-a-day events are pre-students. Indeed, it is likely that students who choose to change their major attend these events together with pre-students. Thus, to be able to distinguish between pre-students and students, another control variable will be included, namely *year of study* including the option for respondents to indicate that they have not yet started. This way, future students can be separated from current students in the analysis.

Lastly, the nature of two control variables, namely *gender* and *political orientation* are considered sensitive and personal questions which means that respondents cannot be forced to make a choice. Thus, for both variables, the following response option will be given "I prefer not to answer.". While this is ethically the right choice, it has important methodological consequences for this study. Concretely, if a large number of respondents on either variable choose not to answer, the confounding effect of that variable might be underestimated.

3.2.4. Sampling

The sampling strategy followed in this research can be considered a convenience sample because the data was collected as was possible rather than by purposeful selection or randomization (Robson & McCartan, 2016). While convenience samples are widely used in experimental research, they limit the extent to which results can be generalized to the population as respondents' characteristics in convenience samples might differ from the population average (Peterson & Merunka, 2014; Robson & McCartan, 2016). Where random sampling is not feasible, the validity

of concrete claims of generalization can only be sustained through replication studies (Peterson & Merunka, 2014).

Practically, data collection took place from mid-April to mid-June 2022 and followed two approaches. The first approach was to visit open days, university campus fairs and trial lectures at Dutch universities to gain access to the target population. The second approach consisted of contacting high schools in Rotterdam, the researcher's place of residence, and asking for permission to collect data among their final year VWO students. All contact with the universities and high schools took place per email between March and June 2022.

However, both approaches proved difficult for two main reasons. First, most open days for Bachelor students took place in the fall, and second, the remaining universities as well as most high schools declined the request.[1] Nevertheless, data was collected at a limited number of occasions by the researcher herself. A complete overview over when and where data was collected is provided in table 2.

	University of Amsterdam	Utrecht University	Erasmus University Rotterdam	Erasmiaans Gymnasium Rotterdam
N	85	20	30	18
Time	Mid-April	Mid-May	Mid-June	Mid-June
Distribution	QR code scanned on personal phones	QR code scanned on personal phones	QR code scanned on personal phones	Anonymous link sent via email

Table 2: Data Collection Strategies

[1] Concerning the former, organizers of open days and campus fairs commonly mentioned two reasons for declining the request. First, organizers could not rule out that attendees would feel pressured to participate in the survey as students might believe that this would improve their chances of being admitted. Second, organizers wanted to avoid possible distraction from their program. Regarding high schools, timing was given as the decisive factor as, at the time of data collection, final year VWO students were either in preparation for, in the middle of, or had just finished their final exams and were therefore unavailable throughout these months. Despite the unfortunate timing of my request, one principal offered to forward the survey to his final year VWO students via email.

As can be seen in table 2, data was collected at four occasions in two ways. At the universities, participants could take part in the survey by scanning a QR code on their phone. Students found themselves either in front of or in a lecture hall while participating in the survey. Since the participants took part in the survey experiment in the environment in which the treatment is thought to naturally occur, this part of the data collection process resembles the process of what Jilke and Van Ryzin (2017) call a "survey-in-the-field" (121). However, the high school students participated by clicking on an anonymous link sent to them via email by their principal. Thus, due to the combination of these two distribution methods, the experiment cannot be considered to have fully taken place in the fields, thus, constituting a hybrid form.

To estimate the number of observations need in order to find an effect, a power analysis conducted prior to data collection suggesting a sufficient sample size of 80 at a significance level of less than 0.05, power of 0.8 and an effect size of 0.2. Since a total of 167 observations were collected, this study can be considered sufficiently powered.

Given the sample strategy outlined above, nature of this convenience sample has important consequences for this study's external validity (Peterson & Merunka, 2014). Mainly, respondents can be considered to differ from the population in two ways. First, most respondents attended trial lectures or open days indicating particular interest in learning more about the university and course. Second, as participation was voluntary, all participants self-selected into the study potentially making them more attentive to the treatment than the average pre-student would have been (Keiding & Louis, 2018).

Lastly, to incentivize participation respondents were offered to take part in a lottery to win a noise-canceling headset after they had taken part in the survey. The sign-up sheet for the lottery was a separate QR code and link and cannot be connected to the survey or any response.

Participation in the lottery was purely voluntarily. The winner of the lottery will be randomly drawn at the beginning of July and receive the price per mail. The lottery serves only to incentivize and is considered independent from the purpose of this research which was communicated to participants before they took part.

3.3. Data Analysis

3.3.1. Data Organization

The complete dataset was imported from Qualtrics in text form and processed in RStudio. As is common in experimental research (Van Thiel, 2014), incomplete observations were removed from the dataset. This choice is justified for one main reason, which is that incomplete observations indicate that participants were not debriefed which is why their inclusion can be considered unethical (Robson & McCartan, 2016). However, if attrition, i.e., nonresponse, is systematic, that is if non-respondents are similar, it could mean, first, that attrition is related to the treatment or the outcome in ways that cannot be measured due to missing background information and, second, that randomization is threatened if attrition occurred unevenly over the experimental groups (Horiuchi et al., 2007; Gaines & Kulinsky, 2011; Bowers, 2011).

If the first case applies, the treatment effect is unlikely to be representative of the population as the sample differs from the population in important ways (Horiuchi et al., 2007). In the second case, it is no longer possible to draw reliable causal inferences from the study without statistically adjusting for random imbalance (Bowers, 2011). Thus, to see whether non-response was systematic and could bias the result, the available information about the incomplete observations were examined and compared to the total dataset by eye-bawling the data (Wetcher-Hendriks, 2011; James et al., 2014). In addition, observations by respondents who had already studied were

removed as they did not form part of the target population. Lastly, all observations with incorrect answers to the manipulation check were removed as these participants cannot be considered treated (Gaines & Kulinksy, 2011).

Next, all variables were renamed and recoded. As part of the recoding, I collapsed the subcategories of the categorical variables i.e., the control variables, into broader categories to facilitate subsequent analysis (Wetcher-Hendriks, 2011: 15). Having fewer subcategories per categorical variable does not only simplify statistical comparison of the categories and subcategories but also the interpretation of the results (Cramer, 2003; Wetcher-Hendriks, 2011). The eight variables forming the two outcome measures constitute the only interval variables in the dataset ranging from 1 to 5 with all distances between each response option being equal.

To ensure the highest possible transparency about the recoding process and to make it possible to interpret the results more accurately, I created a code book (Wetcher-Hendriks, 2011; Van Thiel, 2014) which can be found in Appendix III.

3.3.2. Descriptive Statistics and Factor Analysis

As is good practice, I carried out descriptive statistical analysis before moving on to inferential analyses (Wetcher-Hendriks, 2011; Van Thiel, 2014; Robson & McCartan, 2016). By doing this, the data could be explored before relationships were tested (Van Thiel, 2014; Robson & McCartan, 2016). In this step, I created frequency distributions for all categorical variables to gain a first impression of the data and spot potential irregularities (Van Thiel, 2014; Robson & McCartan, 2016).

Before computing summary statistics for the outcome variables, the scales constituting the variables had to be created. Thus, I carried out a factor analysis, which is considered an inferential

rather than descriptive statistical analysis (Cramer, 2003), to test to what extent the eight items by Sabbagh and Vanhuysse (2006) reflect the two latent variables of interest, namely internal and external attribution of poverty. Concretely, I applied confirmatory factor analysis which "is used to test the probability that a particular or hypothesized factor structure is supported or confirmed by the data" (Cramer, 2003:28). Confirmatory factor analysis was carried out for the variables constituting the outcome measure to test to what extent they can be combined in the two hypothesized scales, namely external and internal attribution.

In the following step, I computed descriptive statistics for the outcome variables and checked for normality by plotting the variables. The results of the descriptive statistics that are most relevant to this study can be found in the result section..

3.3.3. Inferential Statistics

Inferential statistical analysis "aims to establish whether a certain (theoretically presupposed) relation between two variables is systematic." (Van Thiel, 2014: 128) and is thus necessary to answer this study's research question.

As the factor analysis was presented in the previous section, I turn now to the pivotal point of this study, testing whether exposure to the assumptions about human behavior in economics and sociology prime attribution of poverty and, thus, testing the causal relationship between treatment and outcome. In experimental research, randomized allocation and an overall well-designed experiment are considered mainly responsible in "helping researchers estimate causal effects" (James et al., 2014: 75). Therefore, James et al. (2014) advice experimental researchers to use simple statistical analyses when estimating effects, such as t-tests or analysis of variance (ANOVA).

Thus, given the random allocation of respondents to the experimental groups, the average treatment effect of the treatment on the dependent variable is first tested using one-way ANOVA which "compare[s] means from categories of a single independent variable" (Wetcher-Hendriks, 2011: 192). Given that there are three categories of which the means are to be compared, an ANOVA is more appropriate than a t-test. Thus, I carried out two one-way ANOVA comparing the mean scores between the three experimental groups for each dependent variable.

In addition, and to account for possible random imbalance, I added the control variables to the model and thus carried out multiway ANOVA. Multiway ANOVA has the advantage of being a more sensitive test than one-way ANOVA "in that it is more likely to be statistically significant if the error variance is reduced because some of that variance [in the dependent variable] is now accounted for by the other factors and their interactions." (Cramer, 2003: 180). Thus, if randomization was not successful, multiway ANOVA provides a way to account for the influence of other independent variables on the outcome measure and might thereby uncover a significant effect of the treatment (Cramer, 2003).

Lastly, while the dataset contains demographic variables for all participants, an analysis of covariance (ANCOVA) does not constitute an appropriate analysis for two reasons. First, ANCOVA requires covariates, i.e., additional independent variables, to be quantitative (Cramer, 2003). However, the collected demographics variables in this study are categorical. Second, theoretically the demographic variables are not expected to covary with the dependent variable in as much as they are expected to give insights about subgroups in the sample which might have different responses to the treatment. For both reasons, statistical and theoretical, multiway ANOVA is the appropriate analysis.

3.3.4. Subsequent Analysis

While the average treatment effect is of main interest to this study, examining the subgroups in the population that can theoretically be expected to have differing responses to the treatment can help uncover more specified information about any potential relation between course content and poverty attributions (Bowers, 2011; Gaines & Kulinksy, 2011). Indeed, Gaines & Kulinsky (2011) call for experimental researchers to "avoid thinking in terms of a single average treatment" and consider the "plausibility of heterogenous treatment effects" (559). Heterogenous treatment effects refer to variance in the response to the experimental treatment across subgroups in the population (Brower, 2011; Gaines & Kulinsky, 2011; James et al., 2017).

Thus, following tests for the average treatment effects, I examined the treatment effects in the subgroups which the multiway ANOVA indicated to be significantly related to the outcome variable. Concretely, I computed additional multiway ANOVA including interaction effects between the treatment and each of the significant subgroups to uncover potential significant effects of the treatment within these subgroups (Cramer, 2003; James et al., 2017).

As a last step, I focused on the significant interaction effects between treatment and subgroups uncovered in the multiway ANOVA and calculated their direction and effect sizes. While significant effects in ANOVA are recommended to be further explored using post-hoc tests such as Scheffen's test or Tukey's test, these tests are less suitable to explore interaction effects. Thus, following Cramer (2003), I carried out multiple regression analyses to explore the significant interaction effects in the data.

3.4. Ethical Considerations

To conclude the research design section, I will briefly demonstrate how this research complies with the guidelines on student research ethics and data storage as well as the General Data Protection Regulation (GDPR). However, given that participation in the research is strictly anonymous and no identifiable data of people or organizations was collected, parts of the GDPR do not apply.

Nevertheless, the parts of the GDPR and general ethical research standards applying to quantitative and particularly experimental research were complied with. For instance, participants were informed that participation is voluntary and required to provide informed consent at the beginning of the questionnaire. Further, all data gathered during this research is stored on Qualtrics which is considered a secure environment for survey data.

In addition, experimental research is associated with specific ethical challenges which need to be addressed. First, the true intent of a study is often veiled to avoid biases such as demand effects (Anderson & Edwards, 2015; Mummolo & Peterson, 2017). In the present research, no harm can be expected from veiling the intent but fully informing participants at the beginning of the study might bias the results. Thus, I decided to inform participants that the research studies attitudes towards poverty and included a debrief at the end of the questionnaire. The debrief informed respondents about the true intent of the research and their role in it. Lastly, as in other studies research priming effects of course content, no lasting effects of this experiment on respondents needs to be expected as the effects of priming are generally considered short-lived (Engeser et al., 2016).

4. Results

In the following I will, first, provide an overview over the total sample and explain which and why observations were excluded from the analysis and how I dealt with nonresponse. Then, I will present the descriptive statistics of the control variables for the whole dataset as well as for each experimental group to provide an overview over the sample and its characteristics as well as of potential differences between the groups. Third, I provide descriptive statistics for the dependent variable. Next, I will present the results of the two one-way ANOVA analyses carried out to estimate the average treatment effect testing the hypotheses, followed by the multiway ANOVA. After testing the hypotheses, I present subsequent analyses exploring possible heterogenous treatment effects.

4.1. Sample

4.1.1. Removed Observations

The complete sample consists of 167 observations out of which 54 were removed from the dataset due to nonresponse. Out of the 54 deleted observations 12 ended participation before giving consent and 28 did not finish the outcome measures. Thus, there are only 14 observations whose demographic background information can be compared to the complete dataset. An overview over the full comparison between the background variables and the outcome measures can be found in Appendix IV. No differences stand out between the demographic variables and the answers to the outcome variable of nonrespondents and respondents. In addition, while it appears that respondents were more likely to drop out of the control condition than of either of the other two, this does not seem to have influenced randomization (table 3).

	Respondents		Nonrespondents	
	N	*%*	*N*	*%*
Control Condition	37	41	26	65
Economics	21	23	5	12.5
Sociology	33	36	9	22.5

Table 3: Respondents and Nonrespondents per Experimental Group

Unfortunately, demographic data of 26 incomplete observations is not available. Thus, the lack of systematic difference between respondents and nonrespondents has to be taken with caution. Lastly, I removed all observations of participants who had already studied, resulting in 13 removed observations, and observations of participants who gave an incorrect answer to the manipulation check, leading to 7 removed rows 5 of which in the economics condition. After data organization, 91 observations remained to be analyzed.

4.2. Descriptive Statistics

As is good practice in quantitative research, the results of the descriptive statistical analyses are presented in the following section.

4.2.1. Descriptive Statistics for Background Variables

The majority of respondents was between 18 and 25 years old (n=88) and the rest (n=3) was older than 25 years. Twice as many women (n=60) than men (n=30) participated and one participant preferred not to indicate their gender. Regarding students' future fields of study, most participants did not specify their future discipline (n=24) but an almost equally large number indicated to plan on studying in the STEM fields (n=23), followed by law (n=14), social science

(n=12), and economics (n=10). The remaining observations belong to medicine (n=3), humanities (n=2), political science (n=1), and psychology (n=1).

Regarding political orientation, nearly half of all respondents can be considered left (n=43), followed by a center-orientation (n=12). A tenth of the sample indicated support for a radical left party(n=9) and another tenth indicated support for the right (n=9). One respondent gave their hypothetical vote for a radical right party a and a relatively large number preferred not to answer (n=17). As mentioned earlier, this large number of respondents who did not specify their political orientation might hide potential confounding factors and will, therefore be regarded as a subcategory that can be controlled for. Lastly, the highest number of participants indicated that their parents were occupied in higher management or socio-professional occupations (n=41), followed by intermediate occupations (n=32), routine and manual occupations (n=19), and lastly out of work (n=4).

A complete overview over the frequency distribution of the demographic categories over the experimental groups can be found in table 4.

Baseline characteristic	Sociology		Economics		Control		Full sample	
	n	%	*n*	%	*n*	%	*n*	%
Gender								
Female	17	52	15	71	28	76	60	66
Male	15	45	6	29	9	24	30	33
NA	1	3	/	/	/	/	1	1
Age								
18-25	32	97	21	100	35	95	88	97
25-35	1	3	/	/	/	/	1	1
older than 35	/	/	/	/	1	5	2	2
Future academic field								
Economics	4	12	3	14	3	8	10	11
Social Science	4	12	5	24	3	8	12	13
STEM	14	42	5	24	4	11	23	25
Humanities	/	/	/	/	2	5	2	2
Law	2	6	2	10	11	30	15	16
Political Science	/	/	/	/	1	3	1	1
Psychology	1	3	/	/	/	/	1	1
Medicine	/	/	/	/	3	8	3	3
Other Studies	8	24	6	29	10	27	24	26
Vote								
Radical Left	2	6	2	10	5	14	9	10
Left	17	52	10	48	16	43	43	47
Center	6	18	1	5	5	14	12	13
Right	3	9	1	5	5	14	9	10
Radical Right	/	/	/	/	1	3	1	1
NA	5	15	7	33	5	14	17	19
Occupation of parent with main income								
Higher Managerial	16	48	10	48	15	41	41	45
Intermediate	13	39	6	29	10	27	29	32
Routine and manual	2	9	4	19	10	27	17	19
Out of Work	1	3	1	5	2	5	4	4
Courses in High School								
Social Science	17		17		20		43	
Economics	15		15		22		48	
Business	9		9		8		21	
Philosophy	8		8		8		21	
None	9		9		4		26	

Table 4: Demographic Background Variables. Course in high school is a multiple response variable.

Student Number: 823477

4.2.2. Factor Analysis and Descriptive Statistics for the Continuous Variables

As the dependent variables in this present research constitute scales of several items reflecting broader concepts (Van Thiel, 2014), confirmatory factor analysis was conducted, before descriptive statistics for the dependent variables were computed. Following Cramer (2003), a correlation matrix was created prior to the factor analysis (Appendix V). Following the correlation matrix, a factor analysis was conducted on the correlations of eight items. Two factors were extracted with eigenvalues of more than one. However, item 3 in the original internal attribution factor loaded only weakly and negatively on internal attribution and did not seem related to external attribution. Interestingly, the same value loaded the lowest in Sabbagh and Vanhuysse's (2006) factor analysis, albeit high enough to be included in their model.

Attribution item	Factor loading	
	1	2
Factor 1: Internal Attribution		
Failed to take advantage of educational and training opportunities available to them.	**.39**	
Laziness, little or no ambition.	**.99**	
Lack of native intelligence, ability and talent.	-.13	
Lack of character and willpower.	**.49**	
Factor 2: External Attribution		
Prejudice and discrimination against persons because of their race, age, or religion.		**.61**
Limited opportunities given by society.	−.15	**.79**
Failure of society to provide good schools for many citizens.		**.81**
Being taken advantage of by persons who are better off than themselves.		**.7**
SS Loadings	**1.38**	**2.12**

Table 5: Factor Analysis for eight items. Bold items represent retained items. SS loadings refer to the retained items.

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In the case of this research, the item was removed which increased the eigenvalues and seems to have led to a better fit. The items and factor loadings can be found in table 5. The bold items constitute those that loaded highly on one factor and were thus retained. The first factor seems to reflect internal attribution as three of the four internal attribution items loaded highly on it. The second factor can be considered to reflect external attribution as all four external attribution items loaded highly on it.

After the factor analysis was conducted, the dependent variables were created combining the items as suggested by the factor loadings. Next, the descriptive statistics for the dependent variables were computed. As it is good practice in experimental research, the arithmetic mean and standard deviation for the dependent variable in each experimental group and the full dataset are provided in table 6 (James, et al., 2018).

Further and as illustrated by figure 1, the normality assumption is met by both dependent variables, although a slight skewness to the right can be observed. The skewness value for both variables was below three and the value for kurtosis was below four indicating a normal distribution and, thus, confirming that the normality assumption is met and the variables are suited for ANOVA (Cramer, 2003; Wetcher-Hendriks, 2011).

	Internal Attribution		External Attribution	
	M	SD	M	SD
Control Group	2.48	.81	2.62	.99
Economics Condition	2.57	.65	2.64	.76
Sociology Condition	2.67	.91	2.42	.98
Total Sample	2.57	.81	2.55	.94

Table 6: Descriptive Statistics for Dependent Variables
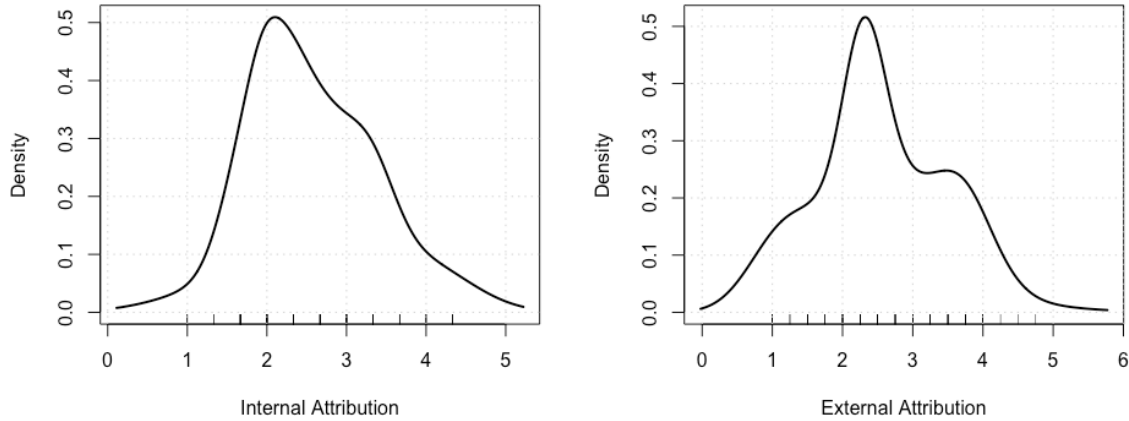
Figure 1: Density Plots for Internal and External Attribution Variables Indicating Normal Distributions

4.3. Analysis and Hypothesis Testing

The one-way ANOVA (table 7) found no significant effect of treatment on internal attribution ($F=0.47$, p $=0.6$) nor of treatment on external attribution ($F=0.5$, p $=0.6$). Consequently, no post-hoc analyses were carried out.

However, before rejecting H1 and H2, I carried out two multiway ANOVA taking into account the variance explained by other variables. No significant effects were found for treatment on internal attribution ($F=0.22$, $p = 0.8$) nor on external attribution ($F=02.37$, $p = 0.1$). Thus, the null hypothesis of no difference has to be accepted and H1 and H2 rejected.

	Predictor	df	Sum Sq.	F	*p*	η
Internal	Treatment	2	.62	.47	.63	.01
	Residuals	88	58			
External	Treatment	2	.89	.5	6	.01
	Residuals	88	77.96			

Table 7: One-Way ANOVA for Internal and External Attribution

However, significant effects were found for gender on internal attribution ($F=3.19$, $p = 0.049$), future studies on internal attribution ($F=2.35$, $p = 0.03$), political orientation on internal attribution ($F=2.85$, $p = 0.02$) and gender on external attribution ($F=4.65$, $p = 0.01$). Therefore, subsequent analyses were conducted to estimate potential average treatment effects within these subgroups.

4.4. Subsequent Analyses

In the subsequent multiway ANOVA, a significant interaction effect was found for treatment and future studies on internal attribution ($F=2.88$, $p = 0.01$) (table 10) indicating that there are significant treatment effects in one or more subgroup of future studies. To identify the significant subgroups and estimate the treatment effects, a multiple regression analysis was computed.

Significant interaction effects were found for the interaction between the sociology treatment and social science future studies ($\beta =2.0$, $p =0.04$) and for the interaction between the sociology treatment and STEM future studies ($\beta =1.48$, $p =0.009$). As both coefficients are positive, the results suggest that, for the subgroups of those planning on studying social science or STEM in the future, the sociology condition led to increased attribution of poverty to internal causes.

Predictor	df	Sum. Sq	F	p	Partial η
Treatment	2	.22	.22	.8	.0
Age	2	2.28	3.34	.1	.08
Gender	2	3.1	3.19	.049*	.11
Future Studies	8	9.12	2.35	.0 *	.26
Political Orientation	5	6.92	2.85	.02*	.21
Parents' Occupation	3	.89	.61	.6	.03
Previous Courses	12.06	14	1.77	.07	.32
Residuals	54	26.21			

Table 8: Multiway ANOVA for Internal Attribution. Significant codes: ***0.001 **0.01 * 0.05

Predictor	df	Sum. Sq	F	p	Partial η
Treatment	2	3.1	2.37	.1	.08
Age	2	.31	.24	.79	.009
Gender	2	6.1	4.65	.01	.15
Future Studies	8	3.62	.69	.7	.09
Political Orientation	5	5.78	1.76	.13	.14
Parents' Occupation	3	1.68	.85	.47	.05
Previous Courses	14	11.1	1.21	.3	.24
Residuals	54	35.44			

Table 9: Multiway ANOVA for External Attribution. Significant codes: ***0.001 **0.01 * 0.05

Predictor (internal)	df	Sum. Sq	F	p	Partial η
Treatment	2	.22	.28	.75	.012
Age	2	3.48	4.58	.02*	.17
Gender	2	2.3	3.03	.06	.12
Future Studies	8	9.12	3.0	.009**	.34
Political Orientation	5	7.74	4.1	.004*	.31
Parents' Occupation	3	1.93	1.69	.18	.1
Previous Courses	14	8.51	1.6	.12	.33
Treatment: Future Studies	8	8.74	2.88	0.01**	.33
Residuals	54	35.44			

Table 10: Table 8: Multiway ANOVA for Internal Attribution with Interaction Effect between Treatment and Future Studies. Significant codes: ***0.001 **0.01 * 0.05

This effect is further illustrated by the boxplots in figure 2 and 3. To compare the effect between the subgroups, a boxplot was created for each subcategory of future study that encompasses respondents in each experimental group. However, it needs to be kept in mind that only for future students of the social science or STEM that received the sociology condition was the effect significant.

Effect	Estimate	SE	95% CI		p
			LL	UL	
Fixed effects					
Intercept	2.6	.23	2.13	3.07	< 2e-16
Treatment Economics	-.21	.38	-.97	.55	.58
Treatment Sociology	-.64	.35	-1.34	.06	.07
Future Studies					
Economics	.73	.49	-.24	1.7	.14
Humanities	.73	.57	-.41	1.88	.2
Law	-.08	.32	-.73	.56	.79
Medicine	-.93	.49	-1.9	.04	.06
Political Science	.07	.78	-1.48	1.61	.93
Psychology	2.04	.79	.48	3.61	.01*
Social Science	-.71	.49	-1.68	.26	.15
STEM	-.6	.44	-1.47	.27	.17
Interaction effects					
E: Economics	.32	.72	-1.1	1.75	.65
S: Economics	-.03	.67	-1.35	1.3	.97
E: Law	-.14	.69	-1.5	1.23	.84
S: Law	.63	.67	-.71	1.96	.35
E: Social Science	1.12	.66	-.2	2.44	.09
S: Social Science	2.0	.67	.68	3.33	.004**
E: STEM	.41	.63	-.84	1.66	.51
S: STEM	1.48	.55	.38	2.57	.009**

*Table 11: Regression Analysis of the Interaction Effect of Treatment and Future Studies on Internal Attribution of Poverty. Significant codes: ***0.001 **0.01 * 0.05*
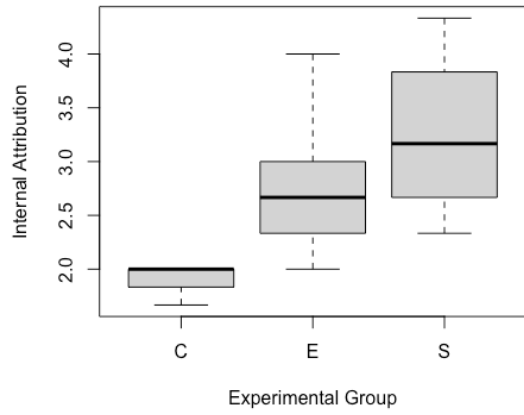
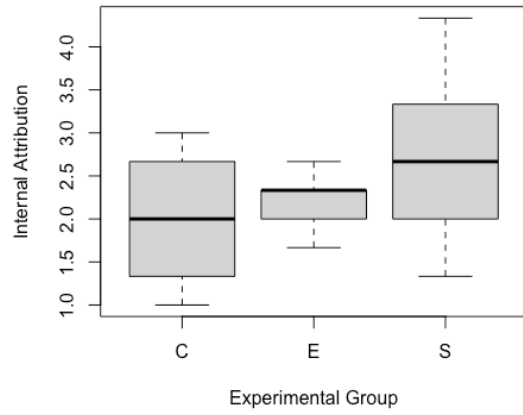*Figure 2: Boxplot for Future Social Science Students*   *Figure 3:Boxplot for Future STEM Students*

5. Discussion & Conclusion

In the following section, I will answer the research question based on the literature and evidence provided by this research. In addition, I will interpret the results in the discussion, followed by a critical appraisal of the research process and suggestions for future research. Lastly, I will reflect on possible practical applications of the findings.

5.1. Answer to the Research Question

This research began by asking the question of the extent to which the assumptions about what drives human behavior in neoclassical economic theory and classical sociological theory prime respondents' attributions of poverty. The theoretical subquestions were answered in three parts. First, previous studies have shown that students can indeed be primed by course content (e.g., Engeser & Bauman, 2014; Engeser et al., 2016; Parker et al., 2018; Donovan et al., 2019) suggesting that course content has the potential to also prime political attitudes. Second, students have been shown to not only select into studies aligning with their political attitudes but also socialize further into their discipline specific attitudes (Guimond & Palmer, 1989; Hastie, 2007; Stubager, 2008; Muhelijc & Drace, 2017; Lindov, 2020) including poverty attributions (Guimond & Palmer, 1996). Based on arguments put forward by previous authors suggesting that the implicit assumptions in course content constitute an important source of socialization effects (Bullock et al., 2003; McWha & Carr, 2009; Neff & Albertson, 2020; Piff et al., 2020), it was hypothesized that the assumptions about human behavior prevalent in economic course content prime students to attribute poverty to the individual (H1) while the assumption prevalent in sociology prime students to attribute poverty to external factors (H2).

One-way and multiway ANOVA revealed no significant average treatment effect between the experimental groups and H1 and H2 were rejected. Moreover, while significant conditional average treatment effects were found for the subgroups of future social science and STEM students, these effects point into the opposite direction of what was hypothesized and thus further disprove the hypotheses (figure 2 and 3).

However, while the hypotheses were rejected, the significant conditional treatment effects for future social science and STEM students make it possible to give a more nuanced answer to the research question. Concretely, assumptions about what drives human behavior in economics and sociology are likely to have a conditional priming effect on students' poverty attributions. While the population of future students as a whole can no longer be expected to be primed by the assumptions, subgroups of future students divided by their future field of study are likely to respond differently to exposure to the assumptions. Concretely, the empirical evidence suggests that future social science and STEM students are more likely to be primed by exposure to the assumptions prevalent in sociology course content than their counterparts from other fields. While the extent to which they can be primed is rather large (β =2.0 and β =1.48), it indicates stronger attribution to internal attribution following exposure to the sociology assumptions. Thus, the extent to which assumptions about what drives human behavior in neoclassical economic theory and classical sociological theory prime respondents' attributions of poverty is non-existent for the mixed sample but rather large in two subgroups.

5.2. Discussion

By singling out the effect of assumptions prevalent in economics and sociology curricula on attribution of poverty (Bullock et al., 2003; McWha & Carr, 2009; Neff & Albertson, 2020; Piff

et al., 2020), this research contributes empirical insights to the theory on socialization effects of course content. Contrary to the hypotheses, no significant effect was found of the assumptions in sociology on external attribution nor of the assumptions in economics on internal attribution.

However, this result needs to be understood in light of one major shortcoming of survey experiments and priming effects more generally, namely the artificiality of the treatment (Barabas & Jerit, 2010; Jilke & Van Ryzin, 2017) and the problem of one-shot exposure (Gaines et al., 2007). In other words, the evidence which this study provides suggests that it is highly likely that exposure to explicit assumptions about human behavior does not have a priming effect on students' poverty attributions. However, as suggested by Neff & Albertson (2020), it is through unconscious socialization in implicit assumptions over a long period of time, that students' policy preferences are affected. Thus, it is possible that the sort of socialization through course content that takes place in the real-world was not captured by the treatment and mechanism employed in this research. Moreover, assumptions are just one component of course content that is theorized to affect policy preferences (Dasgupta, 2017; Muheljic & Drace, 2017; Neff & Albertson, 2020; Brocic & Miles, 2021) and other components might be more strongly responsible for socialization effect, such as factual knowledge and analytical skills (Goossens & Méon, 2015; Fischer et al., 2017).

In addition to the lack of effect of the treatment in the whole sample, heterogenous treatment effects were found through follow-up analyses. While theory might suggest that treatment effects differ across subgroup within gender, socio-economic background, prior education, and political orientation (Sabbagh and Vanhuysse, 2006; Norcia et al., 2010; Bendassolli et al., 2015; Bergmann & Todd, 2019; Piff et al., 2020; Marquis and Rosset, 2021), no such effects were found.

However, and of much interest to the selection versus socialization debate, this research provides empirical evidence that students who select into one field of study do indeed systematically differ from students selecting into another field in the way they can be expected to respond to university course content. Concretely, two subgroups, namely future social science and future STEM students have been shown to respond more strongly to the assumptions prevalent in sociology than other students (see figure 2 and 3). Especially the result for the future social science students is of particular theoretical interest to the theory on selection versus socialization effects as it provides support for the argument that students selecting into social science differ from students selecting into other fields and are, in addition, more receptive to the type of assumptions they will encounter in their future studies (Ekehammar et al., 1987; Elchardus & Spruyt, 2008; Denzler & Wolter, 2018).

However, while future STEM and social science students appear more receptive to the sociology treatment than other students, their response to the assumptions is contrary to the theoretical expectation. Concretely, the found effects contradict the theoretical expectation (see Guimond & Palmer, 1989; Guimond & Palmer, 1996; Hastie, 2007; Stubager, 2008; Muhelijc & Drace, 2017; Lindov, 2020) in that students treated with the sociology assumptions attributed poverty more rather than less to internal causes than their counterparts in the other experimental groups. While, as in line with much of the literature, social science students in the economics group attributed poverty to internal causes more than those in the control group, this finding is insignificant and needs therefore considered to be random rather than systematic (Wetcher-Hendriks, 2011; Van Thiel, 2014).

While it is possible that priming future social science and STEM students with assumptions about the external influence on human behavior makes them more likely to blame the individual

for poverty, there is, to my knowledge, no theoretical support for this finding. Instead, the scale for internal attribution used in this research could provide a possible explanation. Concretely, since the factor analysis revealed that the individual factor loadings on internal attribution were rather low compared to those on external attribution, and the eigenvalue of the internal attribution factor was not much higher than one (table 5), it is possible that the scale used to measure internal attribution was flawed. If the scale did indeed not capture internal attribution to satisfactory degree, the heterogenous treatment effects found in this study need to be put into question.

5.3. Methodological Shortcomings and Limitations

In addition to the type of sample used in the study, the supposed pre-treatment the population had experienced and their effect on the validity of this research, other methodological shortcomings and limitations need to be addressed.

First, and as mentioned earlier, the relatively low eigenvalue of the internal attribution scale used as outcome measure constitutes one methodological shortcoming of this study. While the eigenvalue was high enough to consider the items comprising the scale related (Cramer, 2003), a better suited scale might have led to different results.

Second, albeit this research was sufficiently powered ($(1 - \beta)=0.8$), a larger sample size might have led to a significant effect. Indeed, other studies demonstrating priming effects of course content found small effects with larger sample sizes than the one used in this research (Engeser & Baumann, 2014; Engeser et al., 2016; Parker et al., 2018; Donovan et al., 2019).

Lastly, respondents were exposed to the assumptions in question only through their explicit and brief presentation. A longer exposure, through for instance the task to thoroughly read and summarize a longer text which implicitly introduced the assumptions might have had a different

effect since respondents are more likely to adopt assumptions in the short-term if they mentally worked with the assumptions (Donovan et al., 2019).

Lastly, the generalizability of these findings needs to be discussed. First, the Netherlands constitutes a least likely case for socialization effects to manifest since prior studies were unable to report evidence for socialization in the Netherlands (Delis et al., 2017; Kunst, 2020). Thus, socialization effects of the assumptions in course content might still be found in other, more likely cases (Levy, 2008; Blatter & Haverland, 2012). Especially given that Dutch pre-students have likely been pretreated through compulsory social science in high school (de Jong, 2021) which might have contributed to the small and mostly insignificant effect sizes (Gaines & Kulinsky, 2011), effects might be found in countries where pre-students have not been exposed to the treatment prior to the experiment.

Second, the studied sample consists of a convenience sample collected among volunteers which means that respondents might have been more motivated and attentive to the study than the general population would have been (Peterson & Merunka, 2014). However, given that no effect is found, it is unlikely that less attentive respondents would have led to significant results. Thus, while the use of a convenience sample means that respondents likely differed from the population they were drawn from, the threat which this type of sample poses to the external validity of this study is limited.

5.3.1. Future Research

Thus, future research studying socialization effects of course content should consider a number of issues. First, longitudinal studies are a promising avenue for the study of the effect of course content as it best replicates possible effects in the real world where students are exposed to

course content over the course of several years. Second, other potentially influential components of course content such as factual knowledge and analytical skills (Goossens & Méon, 2015; Fischer et al., 2017) need to be studied and their effect on policy preferences tested. Third, future studies should aim for large sample sizes as many studies that detected priming effects of course content found small effects with large sample sizes (e.g., Parker et al., 2018; Donovan et al., 2019). Third, giving the confounding effect of variables such as gender and vote choice on poverty attribution and policy preferences, it is advisable to use randomization techniques that better account for these confounding factors such as randomized block-designs (Horiuchi et al., 2007). Next future studies are advised to consider other possible mechanisms through which course content affects policy preferences. Lastly, in line with Gaines and Kulinsky (2011) and Bowers (2011), upcoming experiments should consider studying heterogenous treatment effects in addition to the average treatment effect, especially concerning subgroups of future studies.

5.4. Practical Implications

The assumptions individuals hold about what causes poverty have important consequences for the economic policies they favor (Bullock et al., 2003; Norcia et al., 2010; Bergmann & Todd, 2019; Piff et al., 2020; Marquis and Rosset, 2021). Moreover, university graduates are overrepresented in societal leadership positions (Bovens & Wille, 2007), especially in areas where they hold significant influence over public policies (Christensen & Mandelkern, 2021). Thus, if course content does indeed prime the poverty attributions of some students, as shown here for those in STEM fields or the social sciences, these changes in students' attitudes have consequences for policymaking and therefore for society.

Moreover, the heterogenous treatment effects which this study points to provide partial support to Neff and Albertson (2020) who argue that focusing exclusively on one academic field makes it difficult for students to appreciate alternative approaches to public policy. Thus, based on the results of this and future studies, educational leaders should consider intentionally familiarizing students of various disciplines with the teachings and course content of other disciplines. By exposing student to the assumptions prevalent in other fields, they will likely be enabled to take a nuanced position towards attribution of poverty.

5.5. Conclusion

Finally, using a sample of Dutch pre-students, this research studied to what extent the assumptions of what drives human behavior that can be found in economics and sociology curricula affect pre-students' attribution of poverty and thereby contributed to the literature on selection and socialization effects and socialization effects of course content in particular. No significant average treatment effect was found. However, a positive effect of the assumptions prevalent in sociology course content on attribution of poverty to internal factors was found for future social science students and future STEM students.

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Appendices

Appendix I – Extract of Dutch Sociology Textbook

The following extract from a currently commonly used Dutch Sociology Textbook, namely *Grondbeginselen der sociologie* by de Jager, Mok and Berkers (2013), illustrates that the assumptions of classical sociology, relating to the influence of society on the individual are still prevalent in Dutch sociology course content.

"In de sociologie proberen we de acties en reacties van mensen te verklaren uit het feit dat zij niet 'alleen op de wereld' zijn. Sociologie defini.ren we als de wetenschap die het samenleven van mensen binnen grotere en kleinere verbanden empirisch bestudeert. In hoofdstuk komen we op deze definitie terug. Hier volstaan we met de opmerking dat kenmerkend voor de sociologische zienswijze is: een manier van denken die alle activiteiten en voortbrengselen van mensen in verband brengt met de samenleving waartoe die mensen behoren, en die al die activiteiten en voortbrengselen ziet in hun afhankelijkheid van de samenleving." (17)

"Wij zagen dat mensen, op grond van het feit dat zij deel uitmaken van de samenleving, beschikken over een geheel van kennis, verklaringen en oordelen met betrekking tot hun (sociale) omgeving. Zij hebben dat geleidelijk, door middel van leerprocessen , verworven in hun omgang met anderen en tijdens hun ervaringen met die anderen. Sociologen vatten dit alles samen onder het begrip referentiekader . Wij ontlenen onze referentiekaders aan onze sociale situatie, die bestaat uit de omstandigheden waarin wij verkeren, groeperingen en netwerken waartoe wij behoren, het milieu waaruit wij afkomstig zijn en het werk dat wij dagelijks verrichten, kortom: de ervaringen die wij samen met de onzen opdoen. Behalve aan deze directe ervaringen ontlenen wij ons referentiekader

gedeeltelijk aan onze opvoeding en ons onderwijs: daarmee nemen wij ook indirect deel aan de ervaringen van onze voorgangers." (19)

"Met nadruk moet nog worden gesteld dat het gedrag van mensen niet uitsluitend wordt bepaald door hun vroegere ervaringen. Anders gezegd: een referentiekader is, hoewel het een zekere mate van stabiliteit bezit, niet onveranderlijk. Als onze levensomstandigheden en daarmee onze ervaringen ingrijpend veranderen, wijzigen zich na verloop van tijd meestal ook onze opvattingen en ons referentiekader. Deze gehele of gedeeltelijke aanpassing van ons referentiekader treedt bijvoorbeeld op wanneer wij door verhuizing, verandering van werkkring, werkloosheid, emigratie of sociale stijging of daling deel gaan uitmaken van andere groeperingen. Dat is ook het geval wanneer bijvoorbeeld door mijnsluiting, inpoldering (vissers) of bedrijfsbeeindiging de bestaansbronnen van een gemeenschap opdrogen en mensen op andere wijze in hun onderhoud moeten gaan voorzien." (20)

Appendix II – Questionnaire

Block 1

Dear participant,

Thank you for participating in this research!

This survey studies attitudes towards poverty among students. It is conducted under the supervision of Dr. Koen Damhuis as part of the research master program 'Public Administration and Organizational Science' at Utrecht University.

The questionnaire consists of different statements concerning which you are asked for your opinion. You are kindly but firmly asked to read all texts and questions thoroughly. It will take roughly 5 minutes to complete the survey. All answers are anonymous and cannot be traced back to you. You are free to discontinue the survey at any point. In that case, your data will not be included in the research.

If you have questions, do not hesitate to contact the lead researcher:

Aylin Weber - a.l.weber@students.uu.nl

Block 2:

I hereby confirm that I am age 18 or older, have read and understood the information provided to me, and give consent to participating in this research.

- I confirm and give consent

Block 3 – Randomized

Q1

Please read the following text carefully:

Most experts say that human behavior is driven by rational decision-making independent of social constraints. They claim that individuals will always weigh the costs against the benefits of each option and then choose the option that maximizes their own benefit.

Which of the following statements corresponds to what you just read? Experts say that...

- people make rational decisions that are socially constrained.
- people make rational decisions free from social constraints.
- people make irrational decisions.

Q2

Please read the following text carefully:

Most experts say that human behavior is strongly influenced by their social environment and society at large. They claim that people internalize the norms prescribed by their peers and larger societal structures and then unconsciously use these norms when they make decisions.

Which of the following statements corresponds to what you just read?

Experts say that...

- people behave the way they want to.
- people's behavior is influenced by society.
- people's behavior is independent of their environment.

Q3

There are many different opinions regarding what drives human behavior.

Block 4

Thinking of persons who are unsuccessful in supporting themselves financially, how important would you say is each of the following causes in holding them back? (rated on a scale from 1 to 5; 1=not at all important, 5= extremely important)

Failed to take advantage of educational and training opportunities available to them.

Laziness, little or no ambition.

Lack of native intelligence, ability and talent.

Lack of character and willpower.

Prejudice and discrimination against persons because of their race, age, or religion.

Limited opportunities given by society.

Failure of society to provide good schools for many citizens.

Being taken advantage of by persons who are better off than themselves.

Block 5

Please provide some information about yourself.

Q1

How old are you?

- 18-25
- 25-35
- Older than 35

Q2

As which gender to you identify?

- Male
- Female
- Non-binary/ third gender
- Prefer not to say

Q3

What is your (future) field of study?

- Business Administration
- Economics
- Geography
- Humanities (e.g., History, Arts)
- IT
- Law
- Medicine
- Natural Sciences
- Political Science
- Psychology
- Public Administration
- Sociology
- Other Social Science
- Technical Studies
- Other Studies

Q4

In which year of your studies are your currently?

- I have not yet started
- Bachelor 1st year
- Bachelor 2nd year
- Bachelor 3rd year
- Master 1st year
- Master 2nd year

Q5

Which of the following subjects did you study in high school? (multiple answers possible)

- Social Science
- Economics
- Business Administration
- Philosophy
- None of the above

Q6

If election was today, which of the following parties would you vote for?

- BIJ1
- BoerBurgerBeweging
- CDA (Christen-Democratisch Appel)
- ChristenUnie
- D66
- DENK
- Forum voor Democratie

- GroenLinks
- Groep Otten
- JA21
- SP (Socialistische Partij)
- Staatkundig Gereformeerde Partij
- PvdA (Partij van de Arbeid)
- Partij voor de Dieren
- PVV (Partij voor de Vrijheid)
- VVD (Volkspartij voor Vrijheid en Democratie)
- Volt
- 50PLUS
- Prefer not to say

Q7

Which describes best the sort of work the main/highest income earner between your caretakes does as a job?

- Higher managerial
- Middle or junior managerial
- Socio-cultural professions (e.g., teacher, journalist)
- Technical expert (e.g., engineer)
- Technical occupation (e.g., electrician)
- Routine or manual occupation (e.g., carpenter, store assistant)
- Office or service (e.g., secretary, nurse)
- Self-employed
- Retired
- Long-term unemployed

Block 4 - Debrief

Thank you for your participation in this research.

In this research, you have been presented with a text concerning opinions about what drives human behavior. The aim of this research was to find out whether reading a statement about what drives human behavior influences attribution of poverty. You have taken part in an experiment in which people received a statement claiming either that human behavior is driven by individual choice, by social and environmental influences, or that there are many opinions regarding what drives human behavior.

Of course, your data is handled carefully and participation in the survey is completely anonymous. For questions concerning this research, you can contact the lead researchers: Aylin Weber (a.l.weber@students.uu.nl), research master student at the University of Utrecht.

This research has been conducted as part of the final thesis at the research master Public Administration and Organizational Science at Utrecht University.

Appendix III – Codebook

Control Variable: age

Survey Question: How old are you?

Response
18-25
25-35
Older than 35

Table 12: Control Variable Age

Control Variable: gender

Survey Question: As which gender to you identify?

Response
Male
Female
Non-Binary
Prefer not to say

Table 13: Control Variable Gender

Control Variable: studytype

Survey Question: What is your (future) field of study?

The different categories are collapsed into broader academic disciplines in a manner following other authors comparing policy preferences between students from different fields (Fischer et al., 2016; Lindov, 2020)

Category	Sub Category
Economics	Business Administration

Category	Sub Category
	Economics
Political Science	Political Science Public Administration
Social Science	Sociology Other Social Science
STEM	IT Technical Studies Geography Natural Sciences
Humanities	Humanities (e.g., History, Arts)
Law	Law
Psychology	Psychology
Medicine	Medicine
Other Study	

Table 14: Control Variable Future Studies

Control Variable: studyyear

Survey Question: In which year of your studies are your currently?

Response
Bachelor 1st year
Bachelor 2nd year
Bachelor 3rd year
Master 1st year
Master 2nd year
I have not yet started

Table 15: Control Variable Year of Study

Control Variable: schoolcourse

Survey Question:

Which of the following subjects did you study in high school? (multiple answers possible)

Response
Social Science
Economics
Business Administration
Philosophy
None of the above

Control Variable: vote

Survey Question: If election was today, which of the following parties would you vote for?

There are several ways in which Dutch political parties can be grouped together, none of which can fully do justice to the Dutch political system (ProDemos, 2013). However, I will follow the suggestion by ProDemos (2013) and group the parties together according to their position on the well-known left to right scale. Note that the numeric codes are arbitrary and do not reflect any ordering of the groupings.

Position	Party
Radical left	BIJ1
Left wing	SP
	PvdA
	GroenLinks
	D66
	PvDD
	DENK
Center	VOLT
	CDA
	50
	PLUS
	CU
Right-wing	VVD
	SGP
	BBB
	JA21
Radical right	GO
	PVV
	FVD
NA	Rather not say

Table 16: Control Variable Political Orientation

Control Variable: parentjob

Survey Question: Which describes best the sort of work the main/highest income earner between your caretakes does as a job?

The values will be recoded according to the three-class version of the European Socio-economic classification model by Rose and Harrison (2010).

Class	Occupation Type
Higher Managerial and Professional Occupations	Higher managerial
	Middle or junior managerial
	Socio-cultural professions (e.g., teacher, journalist)
Intermediate occupations	Technical expert (e.g., engineer)
	Technical occupation (e.g., electrician)
	Self-employed
Routine and manual occupations	Office or service (e.g., secretary, nurse)
	Routine or manual occupation (e.g., carpenter, store assistant)
Unemployed	Retired
	Long-term unemployed

Table 17: Control Variable Socio-Economic Background

Appendix IV – Descriptive Statistics for Respondents and Nonrespondents

Baseline characteristic	Respondents		Nonrespondents	
	n	%	*n*	%
Gender				
Female	60	66	5	71
Male	30	33	2	29
NA	1	1	/	/
Age				
18-25	88	97	7	100
25-35	1	1	/	/
older than 35	2	2	/	/
Future academic field				
Economics	10	11	/	/
Social Science	13	14	1	14
STEM	23	25	3	14
Humanities	2	2	2	5
Law	15	16	1	14
Natural Science				
Psychology	1	1	/	/
Medicine	3	3	2	29
Other Studies	24	26	2	29
Vote				
Radical Left	9	10	/	/
Left	43	47	4	57
Center	12	13	1	14
Right	9	10	1	14
Radical Right	1	1	/	/
NA	17	19	/	/
Occupation of parent with main income				
Higher Managerial and Professional	41	45	1	17
Intermediate occupations	29	32	2	33
Routine and manual occupations	17	19	1	17
Unemployed/Retired	4	4	2	33
Courses in High School				
Social Science	43		1	
Economics	48		1	
Business	21		1	
Philosophy	21		1	
None	26		4	

Table 18: Frequency Table for Comparison between Respondents and Nonrespondents

Dependent Variable	Internal Attribution		External Attribution	
	M	SD	M	SD
Respondents	2.59	.81	2.55	.94
Nonrespondents	.2.86	.84	2.62	.78

Table 19: Descriptive Statistics for Dependent Variables for Comparison between Respondents and Nonrespondents

Appendix V – Factor Analysis

Variable	*M*	*SD*	1	2	3	4	5	6	7	8
1. Internal_1	2.63	1.07	—							
2. Internal_2	2.35	1.18	.39	—						
3. Internal_3	3.22	1.25	.04	-.1	—					
4. Internal_4	2.73	0.98	.17	.48	.19	—				
5. External_1	2.48	1.17	.04	-0.1	0.06	0.01	—			
6. External_2	2.6	1.24	-.15	-.2	.11	.01	.47	—		
7. External_3	2.55	1.16	.05	-0.2	-.02	-.01	.49	.63	—	
8. External_4	2.58	1.11	-.11	-.04	.05	.09	.45	.54	.55	—

Table 20: Correlation Table for Items of Factor Analysis