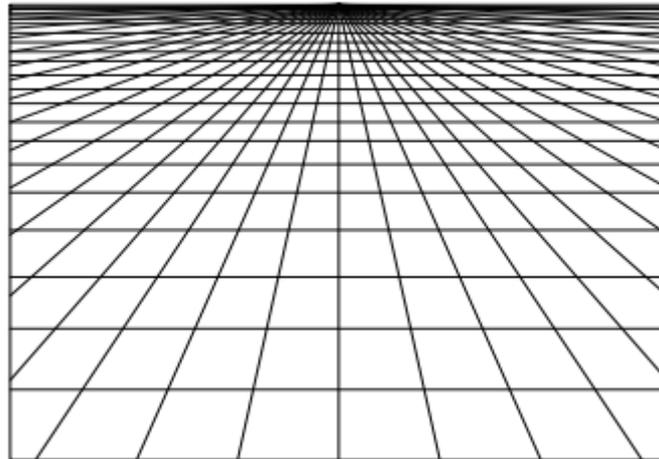




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Scientific economics, expertise and policy making

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Abstract

In the aftermath of the financial crisis that struck in 2008, Europe is still in the midst of a grave economic downturn, and some of the most immediate issues facing policy makers revolve around how to respond to the current economic turmoil. One recurrent response has been to increase the power of technocratic institutions, as well as appointing people with economic expertise to positions of high power. These experts derive their legitimacy, not from democratic principles, but from the source of their expertise, which seemingly provide the objectivity and neutrality needed in order to cope with the problems at hand.

In this thesis I use the current economic crisis as a backdrop for discussing the relationship between economic science and politics. I begin by looking at how scholars in the field of science and technology studies consider the relationship between science and politics. As is shown, they reject the sharp demarcations between a scientific and a political realm, arguing instead that the two are deeply embedded. By drawing on central STS concepts, as well as historians of economic thought, I proceed to make a similar argument for the relationship between economic science and politics. Based on these discussions I end the thesis by looking at the problematic role of economic expertise within such a framework. As is shown, an appreciation of the blurred lines between economic and political work require us to reconsider the technocratic assumptions underlying current responses to the crisis.

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Introduction

Confronted with the worst economic crisis since the great depression of the thirties, some of the major issues in Western political debates currently revolve around how to correctly respond to it. One recurrent response has been to appoint so-called technocrats to positions of high power, under the assumption that in face of great uncertainty, we need to put trust in the ability of experts to deal with the situation at hand. Perhaps the most symptomatic of this response has been the appointment of Mario Monti and his technocratic government in Italy, consisting largely of professional economists and bankers. The tendency, however, is far more widespread. Lukas Papademos formed a similar government in crisis-struck Greece, and the increased power of such institutions as the European Central Bank and the IMF in influencing European politics all point to a strengthened reliance on a specifically economic source of expertise in forming political agendas. One of the most recent and immediate examples of this tendency is the European fiscal compact (formally labelled “the treaty on stability, coordination and governance in the economic and monetary union”), signed on March 2. by all European member states with the exception of the Czech Republic and Great Britain, which in effect exempts large parts of economic policy from democratic and national control.

In the face of grave economic downturns and fiscal turmoil, these technocrats seem to offer courses of action based on sound analyses and neutral objectives. While the (largely neo-liberal) medicine they prescribe may be harsh, they at least seem to provide a diagnosis and a medicine. On the face of it, it does make sense to turn to economists for advice when the economy is in trouble. However, as critics point out, the effect of the medicine prescribes is highly contestable. Also, a paradox remains in putting all trust in the expertise of those who, prior to the collapse, frequently claimed that another major economic recession would not occur because we now know too much about how the economy works. This is especially problematic because economics (as opposed to the “lesser” social sciences like sociology or

anthropology) allegedly occupies the throne of the social sciences precisely because of its ability to make predictions and calculate results in a manner analogous to the natural sciences. Economics, much more than other social sciences, enjoys a *scientific* status, and this no doubt adds to the apparent neutrality and objectivity surrounding economic experts as well as to the force of their argumentation. Consequently, the tendency towards technocratization – or perhaps we could say economization – of politics we are now witnessing is often described as a “responsible” reaction to the calamities facing many different countries. The experts come across as scientific, neutral, disinterested and objective - the diametrical opposite of the stereotypical politician, who is said to be full of self-interest and much too willing to compromise. The case of Italy again shows this most explicitly, where Monti’s government was formed on the condition that it was to include no politicians.

This stereotype of an objective and unbiased technocrat and the self-interested politician is reminiscent of the popular conception of the opposition between science and politics. But is this popular conception really viable? Are the courses of action prescribed by the experts derived from scientific certainties? Are scientists in general and economists in particular really all that unbiased and disinterested? And furthermore, is it not naïve to trust in the neutrality of the scientist when dealing with controversial political issues? These questions have been at the heart of the tradition of science and technology studies (STS). Without accepting a post-modern relativism, these studies have often pointed to the social character of science, and problematized the notion that science left to itself will provide the best courses of action. In fact, on their account, the notion of “science by itself” is a problematic one. However, their focus has largely been on the hard sciences, and in the thesis below I want to argue that much of what STS says about science is equally applicable to the case of economics, and that similar arguments can be deployed in order to make sense of how economic expertise has gained its current status. The studies have been occupied with how

science not simply describe features of reality, but simultaneously creates and sustains identities, institutions and discourses, which in turn render aspects of reality thinkable. Further, once these aspects are rendered thinkable, we are able to act on them. My claim is that the economic science has proven extraordinary effective at doing precisely this, which in turn gives it and its practitioners the legitimacy and power it currently enjoys. In order to see how this is the case, I will argue that STS offers useful vocabularies, concepts and insights. As will be shown, science studies have emphasized that the opposition between a neutral and universal science and messy politics must be radically rethought as the demarcation between the two is far less watertight than is commonly taken for granted. A similar claim, I contend, can be made for the division between economics and politics.

In order to get at this I will begin by giving a brief account of how writers in the field of science studies consider the distinction between science and politics. I will then proceed to problematize the notion of a “pure” economic science both from the point of view of its subject matter and from the point of view of its form of explanation. Here I will draw on the antecedent discussion, as well as some of the great historians of economic thought in order to shed light on both how contemporary economics has gotten its technical and mathematical form as well as how it has been able to expand to areas quite outside its original subject matter. Having problematized the strict separation between economics and politics, I will proceed in the last chapter to discuss how such a conception has consequences for the role of economic expertise in relation to policy making. Whereas a recurrent response to the crisis has been to leave more and more matters of economic policy making directly to those with the relevant expertise, I will argue that such responses are based on the typically technocratic assumption that one can unproblematically move from objective knowledge to preferable policies; an assumption that science studies require us to abandon.

1.0 Science and the Political: an STS Perspective

Before going on to discuss how one can interpret the rise of the economic science within a framework of STS vocabulary, let us turn to some assumptions, insights and findings from the field of STS that will guide the subsequent discussion. For the purpose of this thesis, the central issue to be discussed is how writers in the field consider the relationship between science and scientists on one hand, and politics, society and social order on the other. While studies in the field are highly heterodox, certain common features can be identified. First, based on numerous empirical studies, they reject the view that scientific progress can be sufficiently understood by simply turning to the inner workings of a universal scientific rationale. Secondly, they problematize a priori distinctions and categories in human affairs, arguing that one need to look at *how* these distinctions emerge. This, in turn, should be analyzed by means of studying the *practices* of the agents involved which entails looking at both social and material arrangements when trying to analyze how knowledge is produced, and further, how the production of specific kinds of knowledge are always occurring in, but not reducible to, specific historical contexts. In this chapter I will go on to discuss these insights in somewhat more detail before proceeding to show how some of the vocabulary is equally applicable to the case of economics.

1.1 Science is Social

Studies in science and technology generally militate against the by now largely common-sense notion of science which claims that science is a specific form of method that allows scientists to acquire indisputable knowledge by direct confrontation with the natural world. Science, understood in this way, is a systematic method for acquiring truth. The natural world is rendered visible through the sciences, which in turn describes how reality *really* is.

Scientists are objective and neutral observers, and confronted with the same evidence different scientists should agree on the diagnosis of the phenomenon at hand. Technology, on this account, consists in application of science. Consequently, technological limits coincide with the current scientific knowledge.

Studies in science and technology, on the other hand, “start from an assumption that science and technology are thoroughly social activities” (Sismondo, 2004, p. 10). Likewise, Steven Shapin states in the introduction to his book on the scientific revolution; “I take it for granted that science is a historically situated and social activity and that it is to be understood in relation to the *contexts* in which it occurs” (Shapin, 1996, p. 9). Following Thomas Kuhn (1970) they stress that scientists and engineers are always members of communities, and it is the communities - not abstract, universal laws - that set standards for inquiry and evaluation of truth claims. Far from occurring in a sort of vacuum, science and technology is produced in contexts of more or less established practices and cultures. Looking in detail at these practices scholars have found that scientific practices are not essentially definable by referring to an essential scientific logic. Rhetorical skills, persuasion and intrigues, for instance can play crucial roles in scientific endeavors. These “social” factors most clearly come into play when there are no universally accepted means to determine whether an experiment or finding has been well performed. This insight gave rise to the idea that if science was a social phenomenon it should be studied as such, which was perhaps most influentially articulated in what has been labeled “the strong program”, and particularly in David Bloor’s conception of symmetry. Bloor argued that one should explain scientific successes and failures using the same methods and tools (Bloor, 1973, pp. 173-174). In other words, what constitutes knowledge in a given social and historical context cannot be explained by reference to its truth value. In order to maintain symmetry, sociologists of science should therefore not distinguish between beliefs they hold to be true or false when explaining why people held that

specific view at a particular time. Consequently, it becomes crucial not simply to analyze victorious interpretations with the wisdom of hindsight, and discard the other interpretations as they lose adherence. Instead, one should look at the actual processes and practices by means of which scientific facts are established *as* scientific facts. This, it was argued, could best be studied in cases of controversy and disagreement, where interests, ideology, values and other social and cultural aspects are the most visible. Scientific controversies tend to temporarily open up the scientific “black boxes” revealing frailties in the scientific construction-site to use Latours analogy. A number of such studies have been performed in a variety of scientific fields, and they have shown that the mechanisms, processes, practices and negotiations by means of which scientific controversies have come to an end, have often been ordinary, local and social. They could not be explained either by a distinctive form of rationality or a scientific procedure and logic (Asdal, Brenna, & Moser, 2007, p. 15).

1.2 The Entanglement of the Scientific and the Political

While Bloor and others argued for the need to adopt sociological ways of explaining science, several STS scholars were not content to argue that science is *socially* constructed, at least not without further qualifications¹. Social aspects (like authority, values, ideology etc) certainly played a crucial role, but that is not tantamount to claiming that the content (scientific facts) is explainable merely in terms of a sociological context like “17th century England”. What this form of explanation does is to invert the claims of scientism, substituting a scientific reductionism with its sociological counterpart. However, if one takes seriously the notion of

¹The debate on social constructionism has produced a vast, and often misconceived literature. For a detailed analysis, see Hacking (2001). In denouncing social constructivism here, I mean to say that science is not reducible to, or fully explainable by reference to a stable social context. This has led Latour to argue in favor of “constructivism” as opposed to “social constructivism” (Latour, 2004, pp. 87-120). The first merely states that we give an account of an objective reality by using a range of tools which could possibly fail. Social constructivism, on the other hand means to reduce parts of this reality to “some *other stuff*, the social in which it is “really built”” (Ibid: 91).

reflexivity, “the social” cannot be beyond analysis, and must itself be subject to scrutiny. The latter part of this thesis will be an attempt to expand this line of thought, by looking at how the social sciences in general and economics in particular are effective in structuring the way we perceive reality, and consequently act upon it. In this way studies in STS reject both scientific and technological reductionism as well as social reductionism. Social contexts, interests, values and ideology are not fixed notions. Just as a story of scientific progress must point to more than just a scientific rationale, so too must it contain more than a reduction to a social setting, power-struggles or interests that are beyond analysis. One therefore needs to look at how contestable scientific facts become stabilized, and this includes looking at material arrangements as well as inter-personal relations. In studying these phenomena, scholars in the field have shown how the typically modern dualism between the social and the natural as explanatory categories fails to capture how knowledge is really produced and how power is exercised.

One of the earliest works to take this seriously was Shapin and Schaffer’s historical account in *Leviathan and the air pump* (Shapin & Schaffer, 1996). In it they challenge our commonly held distinction between science and political, and more deeply between nature and society. Instead, they argue, these distinctions, as we know them today, are products of a complex process involving debates, practices, scientific experiments, laboratories and rhetoric that took place in the seventeenth century. In their survey of the debate between Boyle and Hobbes concerning the legitimacy of the scientific experiment in producing certain knowledge, they show how Boyle laid the foundation for a radically new conception of what science was to be. In so doing, the authors argue, a new form of experimental scientific life emerged², one that turned out to form not only how scientists operate, but also how the emerging social order was created in order to respond to it. Because the legitimacy of

²In using the term «form of life» the authors are echoing Wittgenstein. The particular form of life forms its own language game, from which concepts and categories derive their meaning.

experimental science was by no means guaranteed, Boyle needed novel means to persuade his peers. The authors identify three distinct technologies developed in this respect; a material technology (the air-pump), a literary technology (a neutral form of writing that could ensure the transfer of the phenomenon produced by the air-pump to those not directly witnessing); and a social technology (the way in which experimental philosophers should relate to each other and to knowledge claims). In using the term technologies, the authors emphasize that all three were *knowledge-producing tools* (Shapin & Schaffer, 1996, pp. 56, footnote). One must keep in mind that the effect of these debates was not only a new scientific content, but also a new social context. Consequently one cannot explain the former with reference to the latter because neither existed prior to the other (See Latour, 1993, pp. 15-19). That is why the authors conclude that “solutions to the problem of knowledge are solutions to the problem of social order” (Shapin & Schaffer, 1996, p. 332), revealing the intimate association between scientific and political work. They further argue that.

There are three senses in which we want to say that the history of science occupies the same terrain as the history of politics. First, scientific practitioners have created, selected, and maintained a polity within which they operate and make their intellectual product: second, the intellectual product made within that polity has become an element in political activity in the state; third, there is a conditional relationship between the nature of the polity occupied by scientific intellectuals and the nature of the wider polity (Ibid, p. 332).

In Sheila Jasanoff’s vocabulary, these are good examples of what she refers to as *co-production*. Because what was at stake was the credibility of the scientist’s testimonies, and the form of these testimonies changed, a “redrafting of the rules of social order pertaining to the trustworthiness and authority of individuals and institutions” was needed (Jasanoff, 2004b, p. 29). In general, she argues that the purpose of co-production is to “explore how knowledge-making is incorporated into practices of state-making, or of governance more broadly, and, in reverse, how practices of governance influence the making and use of knowledge” (Jasanoff, 2004a, p. 3). In this way, neither science nor the social order can be reduced to each other;

rather, they are co-constituted. My contention in the next chapter will be that a similar claim can be made for the production of contemporary economic and political order. But before we turn back to the world of economics, let us look at certain specific mechanisms that can facilitate this co-production. This will be useful because, as I will argue in the next chapter, these processes are found in more or less the exact same way in the case of economics, and are of key importance in understanding the current power of the professional economist. It will also give us a better understanding of the artificiality of the strict separation between science and politics.

1.3 Laboratories and Centers of Calculation

The difference between a scientist and a politician, Latour has claimed, is that “the politician has no laboratory and the scientist has one” (Latour, 1983, p. 165). This simple feature, he argues, explains the popular stereotypes of scientists and politicians which I mentioned in the introduction. The statement points to a need to rethink both the traditional conception of politics and science. Firstly, he points to the fact that there is no such thing as a “science by itself”; no universal transcendental feature that demarcates science from other fields. The workings of scientists and that of politicians involve many of the same, often quite mundane mechanisms. Scientists are by no means absolute strangers to rhetoric, authority, values, ideology and other social mechanisms, in the same way that politicians are not unfamiliar with deductive rationality. However, the scientist has the distinct advantage of being able to isolate one object or feature and test it extensively by means of trial and error in the laboratory before bringing the result out to society at large. In other words, scientists can afford to fail. The politician on the other hand is forced to act “out there” and has only one shot at a time.

In order to understand the production of scientific knowledge, it therefore becomes crucial to study this production-site in detail. The laboratories contain tools and instruments

both for controlling and manipulating the objects studied. Through such tools as microscopes (or telescopes) scientists are able to make the objects of study human in scale, and thus easier to handle. Also, all manner of tools are available for manipulating, testing, measuring and separating the studied objects. Added to this are sets of what he refers to as “inscription devices”. An inscription device is an “item or apparatus or particular configuration of such items which can transform a material substance into a figure or diagram which is directly usable by one of the members of the office space” (Latour & Woolgar, 1979, p. 51). Whereas the objects of study may be immobile and finite, the documents produced by these inscription devices are highly durable and mobile. Also, importantly, these written documents can be combined and manipulated irrespective of their contextual origins, creating new forms of abstraction. Articles are written using initial diagrams and graphic representations as starting points, and they are compared to or merged with other similar diagrams or documents. In this way, the representations, just as the material objects in the experiments, are put together and translated to form yet other representations that may be at a higher level of generality. Thus, matter is translated into written documents, but written documents, data-set and diagrams are also translated into more generalized forms of representation. What is special about science is thus the specific setup of the laboratory which allows for making mistakes that can be registered and protocolled.

At the same time Latour points to the fact that scientific work is intrinsically political, which in turn is a criticism of the impoverished conception of politics on many standard accounts. The laboratory is an incredibly potent source of political power, Latour argues, and it is there that “most new sources of power are generated” (Latour, 1983, p. 160). Using the example of Pasteur and his micro-biology laboratories, he shows how “in his very scientific work, in the depth of his laboratory, Pasteur actively modifies the society of his time and he does so directly – not indirectly – by displacing some of its most important actors” (Ibid, p.

156). Just like Boyle's invention of the laboratory as scientific space had repercussions for political, theological and practical issues, so did Pasteur's laboratory intervene in French society in a very immediate way. Because of Pasteur's laboratory work, society was transformed so as to include microbes and microbe-watchers. In this way Pasteur's laboratories "could intervene in the daily details of life – spitting, boiling milk, washing hands – and at the macro scale – rebuilding sewage systems, colonizing countries, rebuilding hospitals – without ever being clearly seen as a stated political power" (Ibid, p. 158). Again, this points to the failure of such sociological explanations which separates the social context on one side, and science, laboratories and individual scientists on the other. Rather than being outside and formed by a society that is, as it were, *sui generis*, laboratory-work is the source of some of the most powerful emerging politics³.

Though the laboratory is importantly political, one must realize how the total artificiality of the laboratory is radically different from the messy nature of the world outside its borders. While it is from this complete artificiality that scientific objectivity is derived, scientists can never achieve the same kind of sterile conditions "out there" except by extending the conditions of the laboratory to the outside, effectively turning society into one vast laboratory. "The moment they get "outside"", Latour writes, "they know nothing, they bluff, they fail, they get by, they lose all possibility to say anything that is not immediately counter-attacked by swarms of equally plausible statements" (Latour, 1983, p. 166). In other words, society needs to conform to the inside of the laboratory in order for the activities performed inside it to be relevant outside. Scientific facts are produced in laboratories under artificially sterile conditions and with specific means of verification, measurement and calculation. Consequently, in order to render the same objects or features visible "out there"

³Latour here also points to the impoverished notion of politics in mainstream debates. In using the term politics here one should not simply think of elections and ballot boxes. Politics should be understood as occurring at all levels of society, and the mark of political action is less the presence of televised party-debates, and more the actual forces which shape society. Thus understood, Pasteur was thoroughly political.

these same means must be translated into society at large. For instance, the same kind of statistical systems must be built on large scale in order to make the objects visible outside the physical boundaries of the initial laboratory. In the case of Pasteur, this is clearly seen in the fact that there would have been no way to see the effect of the anthrax vaccine if there were no health statistics – health statistics that were a direct result of the work inside the initial laboratory.

The case of Pasteur is not a singular case in this respect; the observation can be generalized. The translation of conditions from the laboratory to society requires re-allocations and re-structuring of the whole of society in order to cope with the phenomenon at hand. Again we see how scientific work and social orders are continuously being co-produced, and that what goes on in scientific laboratories or office spaces are genuinely political, albeit by other means. While Latour has studied the scientific laboratory, the same goes for other kinds of *centers of calculation*. What Latour showed in the case of Pasteur and microbiology, I would argue, occur daily in the world of economics. Just like scientists working in their laboratories require society to conform to the inside of the laboratories, so too does economic analysis, in order to attain legitimate results, require society to conform to its models. In the next chapter I will draw on some of the great historians of economy to make a similar case for the co-production of economics and modern social order, before ending the thesis with a look at some of the consequences and normative implications of such a view in discussing contemporary issues of economic policy.

2.0 What is this thing called economics?

As we have seen, studies in the field of STS emphasize that we need to take into account the social aspects of the scientific practice when we investigate how scientific facts are established as facts. In order to understand how scientific knowledge is produced we need to appreciate how different actors work together to form stable networks that ensure the objectivity of their work. This is crucial both in analyzing the workings inside laboratories as well as when scientific work is being translated to contexts outside the original site of production. Far from following logically from a given historical context or an inner scientific rationale, scientific work progresses through a series of controversies which are in turn tentatively closed by a range of what STS scholars call closure mechanisms. Simultaneously, because the objects of scientific work call for extra-laboratorial responses in order to be rendered visible on large scale, science and scientific work is extremely effective in altering, shaping and formatting societies, making scientific work political to its core. By making society conform to the workings of laboratories and other centers of calculation, scientists are able to render visible on large scale that which was previously completely invisible and silent. Once rendered visible, these previously invisible objects come to permeate all levels of society as the objects are given a voice and ways are found to act upon them. Once science in this way is stripped of its essentialist character it comes to resemble other social institutions with its own norms, practices and power relations, and, importantly, deeply entangled in the production of social order.

How, then does this relate to our discussion of the current power of the economic science and its practitioners? As is by now becoming evident, my argument will be that what holds for the relation between science and politics, holds for the current relation between economics and politics. The way scientists extend their laboratories to act on the world outside its borders, I claim, is directly analogous to the way economists extend their economic

objects and principles to society at large, allowing such objects as gross domestic product, rate of inflation, national deficits and so forth to become not only visible, but some of the most important and powerful objects of policy making. Historically, this has required massive amount of institutional construction and rearrangements. It is easy to take for granted the incredible amount of labor that lies behind standards of calculation, standardized gathering of accounting and other practices that are needed in order for abstract economical objects to make sense, and function as policy tools. The best way to come to grips with this is through a historical and conceptual detour, which I will turn to shortly.

There are other reasons why such a detour may prove useful, which pertains to the subject-matter and the syntactical form of economic analysis. We should here note that there are certain important differences between the subject matter of the social and the hard sciences. The physical sciences attempt to describe specific parts of reality by showing how the objects of study behave in a manner that can be represented by formal laws that are supposed to be universally valid. As we have already seen, the practices by means of which these laws are derived at contain immense amounts of labor and creativity, tools and laboratories. Further, the laws and findings are always debatable, and could have turned out differently, something that is most evident in cases of controversy, where the frailties of the scientific edifices are most clearly visible. However, the *behavior* of the objects studied in Newtonian physics obviously diverges from the *behavior* studied in the social sciences. Nonetheless, it has for a long time been the dream of the social sciences to try to make sense of human behavior, but without giving up the universality-claims of the natural sciences. In line with this explanatory conception, economic laws are often represented in mathematical terms which show how the manipulation of one variable necessarily produces an effect in another. However, while economics in particular tends to imitate the natural sciences in terms of its modeling, its object is a very specific kind of human interaction – a sort of interaction

that I will proceed to argue has historically been far from ubiquitous. It is therefore doubly useful to take this detour, both to get to grips with the subject-matter of economic analysis and to get a better understanding of how it came eventually to achieve a scientific status in the contemporary sense of the word.

2.1 *What Economics Describes: the Emergence of Economy*

Contemporary economic theory is often applied in describing near all features of human behavior. Gary Becker, himself a Nobel Prize winning economist, for instance, attempts to make sense of everything from crime and punishment to marriage and family behavior in economic terms like stable preferences, rational choice and utility maximizing equilibrium (Becker, 1997). It is interesting to note that Becker does not attempt to put forth a *theory* of economic rationality. Instead, it is as if he applies economics as a method for describing human behavior in a way analogous to Newtonian physics. Any social phenomenon can best be understood, so the argument goes, by recognizing how the agent(s) calculate the benefits, threats and opportunities of an action based on a set of stable preferences that are consistent over time. People choose among different alternatives by calculating the risks and benefits, and this is true whether we are concerned with buying or selling a product, engaging in different forms of criminal activities, searching for a spouse or raising children. One of the great benefits of this conception of human behavior is that it can be formalized into relatively simple equations (Becker, 1997, pp. 52-55). Economics, on this account, has nothing intrinsically to do with a reflection of systematic features of a system of exchange of goods and services, which has traditionally been the subject matter of economic thought. Rather, it consists in a description (a scientific description) of human behavior which is supposedly universally valid and based on a particular set of axioms. If we grant that human behavior is characterized by calculating different options based on a set of stable preferences, we can

deduce which option grants most utility. Because we will always choose the action that grants the most utility, economics provides a systematic and scientific description of all human behavior. Granted, there will always be minor or major distortions, but these are mainly attributed to such phenomena as obstacles or asymmetries with regards to information, leaving the basic models unscathed. For our purposes this account gives rise to two distinct but closely related problems of economic analysis. The first, which I will immediately turn to, concerns the relation between economics and its subject matter; the second, which I will subsequently discuss, concerns its form of explanation.

2.1.1 Conditions for the Possibility of an Economic Science

A radically different conception of the role of economics is presented by Robert Heilbroner, who defines it as “an explanation system whose purpose it is to enlighten us as to the workings of, and therefore to the problems and prospects of that complex social entity we call the economy” (Heilbroner, 1999, p. 311). Conceptually he thus distinguishes the social order which was later called capitalism, its means of organizing material life called the economy and its explanation system called economics (Ibid, p. 312). For Heilbroner, economics is therefore strictly linked to its subject-matter, the economy. “The economy”, in turn, is intelligible only given a very specific set of social arrangements, and is never completely sealed off from the social and political context in which actors engage in production, trade, marketing and other commercial activities. In his genealogy of the great economists, he therefore emphasizes that their respective insights can only be sufficiently understood in relation to the specific historical and social context, as well as within the intellectual debates in which they wrote and exercised their influence. While they were all concerned with the emergence of capitalism and its various effects, they differed greatly in most all other respects, making a unified theory based on their respective insights impossible. There is a deep reason

for this; the economy is not a fixed and static object that can be sufficiently understood if we only grasp its inherent logic. On the contrary, the means by which we organize material life is itself contingent on a whole range of other social practices deeply embedded in features like law, ideology, religion and political practices.

As the purpose of economics is inextricably linked with the phenomenon of the economy, Heilbroner argues that it was no wonder that the intellectual tradition evolved as the free market system began to take its holds. Roughly (and oversimplified), Heilbroner distinguishes three forms of social arrangements that have historically provided solutions to problems of human survival. Custom and authority, he claims, are by far the most widespread solutions historically. In the former, important tasks are laid down by tradition. Sons followed their father's trade, and changing trade by far constituted the exception rather than the rule. This was the case for the first 99 percent of human presence on earth, and guided all hunter-gatherer societies. The latter solution, of more recent origin, consists in ensuring economic survival by a single or a set of authorities, who dictates the tasks that need to be performed, as well as who should perform them. In this case, the whip of authority ensures that the necessary tasks get done. Here, one can think of as diverse rules as Soviet Russia and ancient Egypt. That is not to say that these societies did not differ massively and in important respects, but there was no need for economics to make sense of their operations. "Although, the societies of history have shown the most astonishing economic diversity", Heilbroner writes:

although they have exalted kings and commissars, used dried codfish and immovable stones for money, distributed their goods in the simplest communistic patterns or in the most highly ritualistic fashion, so long as they ran by custom or command, they needed no economists to make them comprehensible. Theologians, political theorists, statesmen, philosophers, historians, yes-but, strange as it may seem, economists no (Heilbroner, 1999, p. 20).

The reason for this, he argues, was that there was no puzzle for the economists to explain before the third societal solution was thought of – that of the market system. Of course, there were economic activities and individual marketplaces in most previous societies, but a system

of self-regulating markets was nothing short of unprecedented. No wonder that this transformation has been referred to with such grand terms as *the economic revolution* or *the Great transformation* (Polanyi, 1944). The basic idea of the market system is as simple, and by now well-known, as it was radical; everyone ought to do what is to his or her best monetary advantage, and through doing so, society as a whole would prosper. The radicalness of this system can be appreciated by the simple fact that, except for very small strata of society, personal gain and prosperity had never before been accepted as a legitimate source of action. Even deeper, the market system could not be envisaged in the Middle Ages because of the simple reason that the basic ingredients of labor, land and capital as abstract ideas did not yet exist. “Land, labor, and capital as “agents” of production, as impersonal, dehumanized economic entities, are as much modern conceptions as the calculus” (Heilbroner, 1999, p. 27). Heilbroner thus emphasize the world-building character of the great economists and their theories. Far from being neutral observers of the social realities of their times, they contributed directly to the formation of a new social order. Thus, Heilbroner argues that, “the evolution of their heretical opinions into common sense, and their exposure of common sense as superstition, constitute nothing less than the gradual construction of the intellectual architecture of much of contemporary life” (Ibid, p. 16).

Of course, that is not to say that economic activity had never existed before the invention of market systems. Whereas acts of “barter, truck and exchange”, to use Adam Smith vocabulary, were certainly present in all previous societies, they always played very minor roles, and at no time did they begin to provide for the necessities of life. As economic anthropologists have pointed out, acts of trade tended to follow very different patterns than those laid down by theories of the economic man. Karl Polanyi, whose account I will return to shortly, has shown how trade in traditional societies mostly followed patterns of redistribution, reciprocity and householding (Polanyi, 1944, pp. 43-55). While these principles showed great

variety and were inseparably linked to religion, custom and law, personal gain and prosperity – the very foundation of homo economicus - were seldom, if ever, prominent. Seen in this light, the account of human nature given by mainstream economics seems to be highly anachronistic.

For our purpose, however, it is not the anachronistic feature of the basic axioms of mainstream economics that is of most importance. Rather, what concerns us here is the affiliation between economics as an analytic tool and that which it describes – between explanans and explanandum. As Callon (1998) has argued, this relationship cannot be sufficiently understood by conceiving the one as describing the other because the tools used to describe economic reality simultaneously contribute to shaping that reality. Callon is concerned mainly with the embeddedness of economy in economics. In what follows I will proceed to widen the scope, and look not only at how economics and the market system has coevolved, but also how a co-production has occurred between economics, a market system and various social responses.

2.1.2 The Co-Evolution of State and Economy

One of the great historians of economic thought to have emphasized this dual aspect of the powers of a free market system is Karl Polanyi. In his much celebrated analysis of the transition to a market economy he gives an account that in many respects resemble the co-production account which I discussed in the first chapter. He shows how the modern state and the free market system in effect grew up together. The modern state, he argues, evolved as a way to respond to some of the more destructive features of a purely free market system. Polanyi, of course, does not use the vocabulary of co-production, but his notion of a *double movement* reflects many of the same features. The double movement, he argues, consisted in two opposing principles: “the one was the principle of economic liberalism, aiming at the

establishment of a self-regulating market [...]; the other was the principle of social protection aiming at the conservation of man and nature as well as productive organization” (Polanyi, 1944, p. 132). As an historical account, Polanyi offers an inversion of the standard liberal account where the market system was said to evolve spontaneously as the realization of a natural process, which brought with it a massive increase in wealth and prosperity. The problem with such accounts is that they tend to assume what they ought to attempt to explain. In other words, these accounts are written from within a perspective where one is already in a market system. While it may appear natural for us to do so, it is nonetheless historically fallacious as the market system names an institutional structure which has only existed in very recent history, and even then only partially. Further, as we saw in the previous section, it is only in a market economy that market laws are intelligible. What Polanyi argues is that, while the system did bring with it a massive increase in material wealth, it simultaneously threatened to destroy the social fabric that had been essential to the organization of society until its introduction, and thus called for actions on the part of society to protect itself. As such, he stresses that it was the social reactions to the introduction of the market system that were spontaneous, whereas the free market was deliberately deployed and based on the novel ideas of economic thought.

In his analysis, he defines a market economy as “an economic system controlled, regulated, and directed by markets alone” (Polanyi, 1944, p. 68). Such a system depends on a series of specific assumptions. I will not be able to do justice to his analysis here, but a few remarks concerning the conditions under which such a system could be implemented are in place. First, it depends on human behavior which conforms to the principle of personal gain. Secondly, it depends on markets where the supply of products and services will be equal the demand at a given price. Thirdly, it depends upon a system of money as purchasing power. Given these assumptions, production and distribution of goods and services is ensured by

nothing but their price. But such a self-regulating market, Polanyi argues, “requires nothing less than the institutional separation of society into an economic and a political sphere” (Ibid, p. 71). In order for such a system to prevail, one must consider every element of industry as being produced for sale because otherwise they would not be subject to the laws of supply and demand. However, this does not only include products and the means of production, but also, as was argued above, such features as labor, land and money because these are also essential elements of industry. In other words, markets had to be constructed for labor, land and property, and one of the main arguments in his analysis is that while the extension of markets mechanisms to include these elements follows from the factory system in a commercial society, “to include them in the market mechanisms means to subordinate the substance of society itself to the laws of the market” (Ibid, p. 71). This, he insists, resulted in massive social dislocations and a general dehumanization, as the human subject now became a commodity produced for sale and consequently subject to the laws of supply and demand.

The importance of this account for our purposes is that it shows the role of economics in forming and shaping a labor market. In this way, novel economic principles were both crucial in changing the established social order, while at the same time prompted the need for novel ways to respond, which together was to shape the social and political landscape. Put differently, there is not first a thing we can refer to as “the economy”, and then a science of this called economics - the production of economic knowledge was much more intimately related to the production of social order. This is the key argument in Callon’s account of the embeddedness of economy in economics. However, as I have tried to show here on general grounds, there is a deep affiliation not only between economics as a theoretical tool and the economy as an object of study, but also between economics and that part of societal life that lies outside of the subject matter of economics. To return again to the notion of co-production, we can now appreciate how the demarcation between economics and

politics as well as that between the economy and the state are far less watertight than one may be led to believe. The production of knowledge concerning economic matters had direct results for the problem of how to respond to the materialization of these ideas. Thus, just as Shapin and Shaffer showed how solutions to the problem of knowledge are solutions to the problem of social order in the case of experimental science, so too I hold, can this claim be applied to the case of economics.

Let us try to briefly summarize where we are. As I have shown by drawing on the writings of Polanyi and Heilbroner, economics as a body of knowledge is both produced in conjunction with, and framed within an idea of a free market system. It is in this framework that gave rise to the conception of such “laws” as those governing supply and demand. The power of these ideas produced massive institutional transformations and relocations because markets had to be introduced for all aspects of industry, including the new economic categories of labor, land and capital. Because of this double feature of the market system, Heilbroner and Polanyi both argue strongly for the need to see economics as not being limited to the description of scientific laws governing economic behavior, which in any case is based on a very narrow conception of human psychology. Instead, to return to Heilbroner's definition, if we want to understand the workings of the economy, it is inconceivable to do so without taking into account the specific historical, social and political circumstances within which economic activity takes place. Heilbroner therefore ends his work with a note of warning concerning the scientification of the economic discipline, arguing that it is “ample room for scientific method in analyzing many problems that economists seek to clarify [...] But when it comes to policy recommendations, it is impossible to present economic analyses as if they stemmed unchallengeably from the givens of society” (Heilbroner, 1999, p. 318). While it may be impossible to do so, the current debates concerning economic policy reveal a deep commitment to act as if one could do so. This is in no small part due to the technical character

of present economic theory, where one may be struck by the sheer absence of discussions of such crucial notions as the market and capitalism. Instead, economic textbooks have tended to move away from the world of policy making, and toward enhancing the internal models of their theories. While this absence may limit the robustness of economic analysis, it paradoxically adds to its scientific status. If we accept the basic axioms used in the models, then indisputable outcomes seem to follow. In this way, much of the potency of economics is derived from its form as much as from its content. Let us therefore for a moment turn away from the question of *what* is described and look more at the equally important question of *how* the economic science describes and analyses.

2.2 How Economics Describes: a Science of economy

The radicalization of the economic man exemplified in the works of Gary Becker above, which is evoked to describe features of social reality quite far away from the traditional objects of economic analysis is symptomatic of the trend to focus solely on the abstract principles of their internal models. It is also a testament to the perceived power of the principles evoked. However, that a theory is powerful in no way guarantees its descriptive adequateness. I have argued something to the contrary. Following Callon (1998) I have contended that, rather than merely describing how human behavior really works, the materialization and institutionalization of economic principles tend to produce economic-man like behavior⁴. While I made this argument on historical grounds above, it is useful to look to a slightly different area of economic history to come to grips with how the technical, formalized character of the economic man came to be formulated, and consequently render

⁴ Empirical studies have been conducted which suggest that students of economics tend, unintentionally, to become more selfish and less occupied with the common good (see Wang, Malhotra and Murnighan ,2011). While this study points to a specific group of study, I want to suggest that the tendency the authors detect is mirrored on a much larger scale as economic vocabulary increasingly permeates the whole of public debates.

possible the conception of Becker's calculative agents. This, I will argue is closely related to a theoretical shift in the discipline that has moved away from individual policy issues, and towards a more rigorous and "scientific" understanding of its own practice. As I will argue, it took a long time before economics as a discipline was able to distance itself from its surrounding political and social contexts, and turn solely to the abstract and technical problems posed by its internal structure. However, once these principles and their adherents gathered sufficient momentum, they proved extraordinarily effective in paving the way for policy changes. One may be reminded of the famous claim by Keynes, who stated that:

The ideas of economists and political philosophers, both when they are right and when they are wrong, are more powerful than is commonly understood. Indeed the world is ruled by little else. Practical men, who believe themselves to be quite exempt from any intellectual influences, are usually the slaves of some defunct economists. Madmen in authority, who hear voices in the air, are distilling their frenzy from some academic scribbler of a few years back. I am sure that the power of vested interests is vastly exaggerated compared with the gradual encroachment of ideas (Keynes, 1973, p. 383).

That is, of course, not to say that there are no important insights to be found in economic analyses. Like Keynes I do not wish to dismiss economics as nonsensical, nor that its object of study does not exist. Rather, I hold that we need to appreciate the limits of its insights in order to make critical use of its findings, and the conception of economics as a "hard science" has tended to disguise its limitation. Let us therefore proceed to discuss in more detail how economics came to have the technical and scientific status that it currently enjoys.

2.2.1 Scientific origins: the introduction of numbers, weight and measure

With the successes of the natural sciences over the last centuries there have been numerous attempts to find equally successful ways of explaining and analyzing social phenomena.

While their successes have been less obvious, the strive for objectivity and universality is still prominent in many branches of the social sciences. There is a deep point to be made here

concerning what explanations are to look like in the social sciences. As scientific explanations have come to be identified with a specific method that allegedly grants true statements, the way in which anything is to be explained has come to be identified with the so-called deductive nomological method. Using formal laws, we can deduce what must happen if we know the original state of affairs and the law operating on the object of study. Thus, if we let a rock fall, we can use Newton's laws of motion to deduce how it inevitably has to behave. The explanation is perfectly symmetrical in that we use the same apparatus to explain both what will happen, and what has happened. With this model of explanation as the ideal, the way in which we explain anything requires us to explain why what happened had to happen by necessity. However, as I have already discussed in the previous chapter, this is a bad model for understanding even how the physical sciences operate and progress. Firstly, if Newtonian physics is powerful in describing physical events it is not due to the syntactical structure of the mode of explanation, but because there are specific causal forces operating. In other words, what does the explaining is not the law, but the actual causal mechanisms that, as it turns out, can be represented in formal laws. Secondly, as I discussed in the first chapter, the causes are never mere causes stripped of everything but the causality and open for the scientist to distract from nature. Rather, causes are always murkier, full of perturbations and distortions, which give rise to scientific controversies. This is linked to the third problem of the mainstream conception of science; it leaves no room for either individual or collective action because all events are but realizations of inherent potentials. In this kind of framework, all actors would behave in exactly the same way as another in the same situation. In criticizing this model of explanation I do not mean to say that no general tendencies can be found in studying natural phenomena or collective human behavior; it is merely to say the mode of explanation sketched out above is not only an ideal, but, to paraphrase Latour, it is "an ideal squared: the ideal of an ideal explanation" (Latour, 2004, p. 152). The objectivity even in the natural

sciences, as we saw in the first chapter, is not a product of pure thinking and experiments, but also includes a range of other mechanisms.

However, there is no denying the massive rhetorical advantage of presenting an argument based on “hard facts” represented in numbers and equations, but this rhetorical advantage is itself something to be explained. Of interest is thus not only how scientific knowledge is constructed; equally important is the social basis of authority (See Porter, 1995). The need to quantify is certainly pressing in contemporary economic matters. It is therefore useful to turn again to economic history and look at how it came to achieve its technical, and thus seemingly neutral, form of explanation.

As I already began to discuss above, contemporary economic theory is often presented in thoroughly formalized mathematical terms both at the micro and macro level, with a mode of explanation analogous to Newtonian physics. Histories of economic thought often trace this scientification back to the early economic writers in 17th century England. However, little attention is generally paid to the reasons why it became scientific in the contemporary sense of the word. It is often taken for granted that its becoming scientific was the inevitable outcome of certain key insights which appeared in their primitive forms in 17th century England, and which have by now been refined into more or less complete and robust models. In other words, economics as an intellectual tool became scientific in the contemporary sense of the word because, in a quite literal sense it *is* scientific.

William Letwin, in his account of the origin of scientific economics, argues from a slightly different perspective. He emphasizes that scientific economics arose in close conjunction with the Baconian “new science” and that it was first and foremost developed as a rhetorical tool in a very specific setting. With the biblical authority diminishing in English intellectual debates, arguments on economic policy were left to merchants and individual policy advisers, whom in many cases were suspected of being motivated by self-interest

rather than the common good. In order to escape the charges of self-interestedness a form of argumentation was needed that would be as impersonal as possible, a form of explanation that was found in the detached reason of the natural sciences. “In the search for a way of dispelling the problem of special pleading a scientific method was hit on. The needs of rhetoric brought forward the method of economic theory” (Letwin, 1963, p. 97). In the spirit of Descartes, who saw mathematics as the model for all reliable knowledge, so too did economic writers attempt a Cartesian approach to their study; an objective treatment of trade based on indisputable premises, using arguments of sense expressed in terms of “number, weight and measure”. However, they did so not so much in order to build a scientific theory or gain knowledge, but in order to push forward policy changes, and it was assumed that given the objectivity of the means would necessarily be transformable into good policy. “It is remarkable” Letwin writes:

that the inventors had none of that detached objectivity that goes by the name of “scientific attitude”. They created scientific theories, yet they generally did not do so deliberately, nor did they do it even for the sake of knowledge, but rather their scientific accomplishments were a by-product of their efforts to convince others to accede to certain economic policies (Letwin, 1963, p. ix).

As we saw in the discussion on the scientific disputes between Hobbes and Boyle, the issue of credibility relies not only on deploying impersonal and objective arguments. There is also an important question concerning who’s detached reason is to be trusted. This was the case in the scientific disputes, and Shapin and Schaffer showed how new technologies of persuasion was called for to accept the legitimacy of the novel laboratorial findings. The same problem of authority was recognized by the early writers of trade. Unlike Hobbes and Boyle, however, these writers were seldom intellectuals or academics in universities, but mostly individual merchants. Further because merchants were in general held in very low esteem at the time they mostly wrote anonymously in order not to injure the force of their arguments. Such anonymous writings may be seen as an intellectual technology deployed on the part of the

merchants to affect policy-changes. However, the appeal to numbers, weight and measures called for another technology of perhaps even more persistent and omnipresent character, the statistical apparatus, an intellectual technology that I will return to shortly.

Letwin thus points to a crucial, but contingent, affiliation between economics, science and policy making. However, he also stresses that the origin of scientific economic must be seen in relation to the specific and local circumstances, not as an inevitable relationship between the study of trade and natural philosophy. As David Burchell (1998) emphasizes, this can be appreciated by the fact that the association between economics and natural philosophy played out very differently in the cases of England and Germany. The problem of credibility that Letwin proposed was a local and contingent one, due in large part to a crisis in public administration “where public life was dominated by the problem of establishing a credible and authoritative policy viewpoint among the welter of “interest groups” confronting a relatively weak national authority” (Burchell, 1998, p. 205). Thus, whereas Adam Smith and the physiocrats came to dominate English economic debates, the German Kameralwissenschaft showed little of this detached scientific affiliation, which Burchell argues is the main reason why it is left out of much economic history (Ibid, p. 197). We must also keep in mind that the early study of trade, subsequently labeled “political economy” was still Aristotelian in the sense that it was seen as an art of managing a state analogous to the managing of a household. In other words, the study of trade was also the study of politics. So too did Adam Smith define it as “a branch of the science of a statesman or legislator [that] proposes to enrich both the people and the sovereign” (Smith 1991, p. 325, quoted in Letwin 1963, p. 217). “Not until much later”, Letwin continues:

such are the temptations of applied science, did economists feel quite comfortable about ignoring utterly all policy questions, and studying solely the abstract and purely speculative problems set by the internal structure of their theory; only then did economics come to have its present technical character and apolitical name (Letwin, 1963, p. 217).

2.2.2 A Representational Challenge

The rhetorical advantage of the kind of depersonalized argumentation evoked by the early writers of trade has by no means lost its potency in contemporary debates. This tendency is certainly not exclusive to matters of economic policy, but is reflected in most branches of policy making. While being rhetorically powerful, the insistence on quantifiable representation reflects a shift not only in how one talk about issues like economics, but more deeply, it has become constitutive for our understanding of a whole range of social phenomena. In general terms this is what Foucault discusses in his conception of governmentality, who also argued that “the essential issue in the establishment of the art of government in early modern Europe was the introduction of economy into political practice” (Foucault, 1991a, p. 92). Foucault was largely occupied with what we can call the co-production of knowledge and power, where the typically modern way of governing is dependent on an increased focus on classification and standardization. Only by being represented categorically could those represented be controlled and managed. This shift manifests itself in many areas of increased institutional power, which tend in various ways to discipline people’s lives. This was the case for crime and the rise of the modern prison (Foucault, 1991b), our conception of pathological insanity (Foucault, 2001), and in a similar way, I argue, for the economic man of mainstream economic theory.

Economics in particular stands out as the most calculable, and thus the most depersonalized, neutral and objective of the social sciences, but this conception relies on effective representations, numbers, weights and measures, which in turn require huge statistical apparatuses. Thus, if the study of trade could be realistically perceived as neutral and objective in the sense sketched out above; if arguments were to be based on number, weight and measure rather than the contingent sentiments of individuals, then ways needed to be found that would produce these numbers in an objective and depersonalized fashion

analogous to the physician's laboratory. It is here that the history of economic thought becomes closely interwoven with the history of statistical and accounting practices; a relation that has by now become so intimate that it has become hard to strictly distinguish the two⁵. It is by means of such statistical representation that economic objects like GDP, inflation rates and so forth become visible, and, to return to Latour's terminology, become human in size such that we are able to act upon them.

Following Foucault, any kind of intervention first requires effective forms of representation. However, all representations are in a sense *mis*representations (Law & Wittaker, 1988). That is, heterogeneous objects are reduced to homogenous ones, who are made to speak, as it were, with one voice. "They speak for others that have been deprived of a voice, that have been transformed from objects that spoke for themselves into mere shadows of their former selves" (Ibid, p. 179). Further, this kind of representation has the dual effect of both describing and affecting those represented. As Hacking has argued, the defining of new classes of people for the purpose of statistics has large consequences for the ways in which we conceive of others and think of our own possibilities and potentialities (Hacking, 1990, p. 6). What counts as normal becomes the degree to which a person or a group of persons conform to tendencies set by the statistical representation. "Few of us fancy being pathological, so "most of us" try to make ourselves normal, which in turn affects what is normal (Ibid, p. 2). This has great implications for economic sociology because the combination of economic theory and statistical representations produces a situation where what is described is simultaneously produced.

It is within this situation that we must attempt to understand the economic man of textbook economics, and his radicalization with which I began this chapter. It may be tempting, as many sociologists have done, to reject him as a phantasy, as a figment of

⁵ It is, for instance, interesting to note that many of the winners of the Nobel prize in economics are statisticians and mathematicians, not economists by training.

theoretician's imaginations. Callon, contrary to this view, argues that he is very much a real person. However, he is far from the embodiment of a general human nature. He is not the starting point from which a science of man can be constructed, but, on the contrary, a highly situated being, formed, shaped and formatted by that economic science which pertains merely to describe, and the statistical representations of him. "If mathematical economics can be realistic under certain conditions", he writes, "it is not because human behavior is naturally "mathematizable"; it is because the calculative agencies are there to introduce interrelated calculations in decisions and in the formation of actions" (Callon, 1998, p. 50).

It has been the purpose of this chapter to problematize the notion of a "pure" science of economics. Instead, I have argued that we need to appreciate the co-constitutive nature of the form of explanation and that which is explained, which has been a central feature in much STS research. There is a general point to be made here. If we accept this account of the co-production which I have supported here, it becomes impossible to separate the ways in which we know and represent the world from the ways in which we live in the world. "Knowledge and its material embodiments are at once products of social work and constitutive of forms of social life" (Jasanoff, 2004a, p. 2). It has been the purpose of this chapter to show how economics has been extraordinary powerful in precisely this sense.

3.0 Economic Expertise and Policy Making

I began the thesis by asking how the economic expertise has become so powerful in framing and dictating contemporary political life. Throughout the first and second chapter I have suggested that one answer is to be found in the ability of economic principles to produce and sustain forms of reality that have proven extremely resilient. At the same I have problematized the notion of a “pure economics” as a deductive and objective system of argumentation. In the second chapter I made the case that while economic expertise is appealing precisely because of its apparent neutral and objective form of argumentation, there is no intrinsic relationship between the detached reason and the deductive ideals associated with the natural sciences and the study of commercial activities. This I have argued from two distinct but related points of departure. From the perspective of the subject matter of economic analysis, I have maintained that “the economy” is not nearly as clear cut a notion as it is portrayed in contemporary textbooks. This object of economic analysis is never reducible to a set of standard and calculable practices, but is always deeply embedded in social conduct, which in turn are historically situated and contingent. Further, the relationship between economics as an intellectual tool and the economy as an object is not a simple matter of the one describing the other; rather, I have argued that the two are mutually produced. I have also argued from a point of departure of the form of explanation most commonly used in scientific economics, and showed how this form should first and foremost be seen as a rhetorical tool, not as the inevitable outcome of an intrinsically scientific nature of the study of economic matters, if by scientific we here mean the commonly understood version of it as a method for acquiring indisputable truth.

Does this mean that economics cannot be scientific? This is where we must return to our discussion in the first chapter because this question begs the more fundamental one concerning what it means to be scientific; a notion that was problematized in the first part. As

we have seen, studies in STS tend to reject the strict distinctions between science and politics, between nature and society. What demarcates science from other fields of inquiry cannot be adequately comprehended by reference to an essential characteristic or method. Instead they emphasize the practices by means of which actors create and sustain realities. Throughout the first two chapters I have hinted at one specific resemblance between the workings of the scientist and that of the economist. However, as is by now clear, this resemblance cannot be the syntactical form of the mode of explanation, but rather consists in what we can call the scientific and economic practices; a resemblance between the workings of the scientist's laboratory and the economist's office. As economics as an intellectual tradition turned away from political reality and towards enhancing their internal models, measures were needed to make society conform to these models because this is the only way that the conditions and causal explanations set by the models could be realistically applicable to the real world. However, if we return to Latour's discussion of the laboratory as the key distinction between a scientist and a politician, a difference between the scientist's laboratory and the economist's office become apparent. While in both cases inscription devices allow complex objects to be rendered visible and made human in scale, the scientist working in the laboratory is able to isolate and extensively test their hypotheses by means of a series of conjectures and refutations. The economist has much more difficulty in isolating a single feature of his or her hypothesis and subsequently test it without going "wholesale", making the workings of the economist even more like that of the politician. This was one of the main reasons for the immense impact of the turn towards a market economy which Heilbroner called *the economic revolution* and Polanyi *the Great transformation*. Again we must remind ourselves of the artificial distinctions between science and politics, and as we are now able to state, between politics and economics.

This raises a further question. If we accept the above account, can it serve as more

than a critique? Does it provide some kind of alternative? While the argument thus far has been made on general grounds, I believe that the antecedent discussion may prove useful in shedding light on contemporary issues as well as offering some general normative pointers towards constructive responses to the crisis at hand. As has become clear by now, the normativity of the account I have been giving points away from a technocratization that has been the most prevailing European response to the current financial and economic calamities. This is where my account converges with a different aspect of the STS literature. While, as I have been arguing, STS offers good analytic tools to make sense of the rise of the economic science, the tradition of STS has also historically been an activist one calling for increased public emancipation and involvement in matters of science and technology. Let us therefore turn away from the discussion of economics as an explanatory system, and towards the ambiguous role of economic expertise within the framework developed above.

3.1 Expertise under the Mark of Complexity and Immediacy

The argument developed in the above chapters, where I have problematized the notion of a pure economic science, has implications both academically and politically. Academically, it provides a way of studying and understanding the development of science in general and economics in particular. However, it also has important consequences for the political role of scientific and economic expertise; that is, for the relationship between (scientific) expertise and policy making. The main purpose of the discussion until now has been to criticize the assumption that absolutely objective and universal knowledge is attainable, and further that it can serve as an unproblematic basis for developing policies. This is the basic assumption behind technocratic argumentation, which holds that it is precisely up to those who possess the relevant knowledge to come up with the best policies. This line of argumentation seems to be doubly compelling when - as is increasingly the case - the matters at hand reach levels of

complexity that almost by definition exclude those who do not possess the relevant technical training.

One of the central problems in contemporary western democracy therefore consists in the apparent contradiction between the policy role of scientific knowledge on the one hand and the principles of democratic decision making on the other (Nelkin, 1979, p. 107). In other words, how can we reconcile the ideals of universality, rationality and scientific curiosity with the democratic ideals of public emancipation and pluralism in a setting where the most controversial and immediate policy issues arise at the very boundary between what we designate by the terms “scientific” and “political”? As policy problems are becoming increasingly complex, and the consequences of action or failure to act come to have far reaching consequences, scientific rationality emerges as an extremely compelling force in answering many of the problems faced by policy makers. During the depression in the early 1930s, the physicist Robert Millikan proposed the following solution to cope with the problems of effective economic policy:

Those in control must either themselves be thoroughly trained in the method of modern correct attack on the problems of economics, finance and government, or must at least be willing to choose as their advisors [...] the ablest most high minded, most competent men in those fields. That alone constitutes the scientific approach to the problem of government. (Millikan 1969, quoted in Nelkin 1979, 107)

The kind of technocratic response proposed by Millikan is deeply engraved in the typically enlightenment ideal of universal rationality, and has been influential not only in matters of economic policy, but in a range of areas in policy making where technical and specialized knowledge play crucial roles. Taking contemporary issues as examples we can think of such problems as those of ecology, global warming, genetic engineering or nanotechnology, all of which are issues right on the boundary between science and policy making. Scientific knowledge, with its universal and apolitical character becomes both the answer to problems, as well as the source of legitimacy most sought for by politicians wanting to implement

specific policies. As such, the logic of scientific rationality has become an incredibly potent form of legitimation also in political argumentation. It has been argued that “much of the history of social progress in the Twentieth century can be described in terms of the transfer of wider and wider areas of public policy from politics to expertise” (Brooks 1965, 68, quoted in Nelkin 1979, 107). As more and more matters of policy making depend on, or is directly or indirectly informed by technical expertise, the temptation may be great when the stakes are high to leave decisions on controversial policy issues to those experts who possess relevant knowledge.

Increased complexity is certainly a present problem in economic decision making, where the globalized economy increasingly causes decisions taken in one place to have ramifications on the other side of the globe. Under these conditions economic forecasting becomes increasingly difficult to assess. The inability of economists to correctly forecast the future, while intrinsic on the account I have given above, is thus drastically increased by the immense complexity of the systems involved in contemporary economic life. Not only such complex financial products like derivatives or currency default swaps, which have been the focus of much attention after the financial crisis struck Wall Street in 2008, but basic economic concepts like money or property have become increasingly difficult to define clearly, rendering the managing and forecasting of these issues equally difficult. In a monetary policy hearing in 2000, the then chairman of the Federal Reserve Alan Greenspan nicely pointed to this feature when, confronted with the failure to predict monetary aggregates, he claimed that the problem of money is that the whole notion is difficult to define, arguing that:

Monetary aggregates as we measure them are becoming increasingly complex and difficult to integrate into a set of forecast. The problem is not that money is not important, but how we define it. By definition all prices are ratios of exchange of goods for money, and what we seek is what that is. [...]. The difficulty is in defining what part of our liquidity structure is truly money (CorporatePersonhood, 2009)

The debate was closed with the rather revealing remark that it is not possible to manage that

which you cannot define, a problem that we noticed in the discussion on economic representation. It is not the purpose of this paper to disregard any attempts to manage, forecast or regulate matters of economic policy. On the other hand, what I want to emphasize is that the kind of problem faced by Greenspan and other people involved in complex economic forecasting is not some sort of anomaly. It is a problem well known not only in economics, but in all areas of intellectual life, the hard sciences included⁶. However, the point is not to downplay these problems, but recognize them as constitutive for robust policy making. As such, they need to be acknowledged, and, as it were, serve as the starting point from where constructive dialogue can emerge, and the popular conception of economics as a neutral and deductive science tends to cloud rather than reveal these problems. Much attention has been paid to this structural problem in STS studies, and in the latter part of this chapter I will turn towards some of these responses, and argue that they may prove useful also in responding to economic matters.

In addition to increased complexity policy makers are more and more confronted with a growing sense of immediacy. Controversial issues call for swift responses, which again add to the need for a secure and well thought out course of action. Again we see the importance of (scientific) expertise in formulating answers. The current economic crisis is certainly no exception to this general statement, and arguments are frequently voiced, often in apocalyptic terms, that “we need to act now”. Such states of emergency, while indeed calling for swift action, simultaneously make it increasingly difficult to pause and think about the nature of the problems, and instead leave decision making to those who have solutions ready at hand. The economist Milton Friedman has argued that:

⁶ The structural problem of this predicament is similar to what Harry Collins and Trevor Pinch (1993) call “experimental regress”. If we judge a forecast by whether or not it gives a good indicator of the direction of the state of the economy, and the forecast fails, there is no way of separating discussions about the outcome from discussions about the forecast.

Only a crisis actual or perceived produces real change. When that crisis occurs the actions that are taken depend on the ideas that are lying around. That I believe is our basic function – to develop alternatives to existing policies – to keep them alive and available until the politically impossible become politically inevitable (Friedman, 1982, s. ix).

Naomi Klein (2007) has taken this quote as starting point to argue that the ideas of Milton Friedman and the free-market frontiers at the University of Chicago have been deliberately deployed as responses to a whole range of crises, from natural disasters to war and military coups, often with quite disastrous consequences for the general public. The conspiratorial aspect of her story is less interesting to this narrative than the picture she depicts of a growing sense of perpetual state of emergency. The political philosopher Carl Schmitt infamously argued that in states of emergency democratic principles must be set aside in order for effective action to be undertaken (Schmitt, 2009), and as Friedman correctly observed, the reactions to crises depend on the ideas of those set to respond to it. These ideas, of course, do not need to be “correct”, as Keynes had noticed. Rather, what is most important is that they are properly institutionalized such that quick action can be undertaken. In many respects this is what has happened over the last few years in European economic life. When the economic crisis first hit, there was no institutionalized response mechanisms in place. However, these are in various forms now beginning to emerge, perhaps most notably in the form of an increased power to the so-called “Troika”, consisting of the European Central Bank, the International Monetary Fund and the European Commission, as well as various newly enacted response programs like the “European Financial Stabilization Mechanism (EFSM) and the European Financial Stability Facility (EFSF).⁷ What is of interest here is the change of role of

⁷ I am, of course, here guilty of a gross oversimplification of the matter. It would require more than a single thesis just to lay out the details of the different responses to the financial and economic crisis. It should also be noted that the mechanisms are by no means uncontroversial, even among professional economists. However, of importance for this thesis is not so much the content of the mechanisms set in play as it is their form of legitimacy, and it seems to me that the responses clearly point in the direction of a general shift towards a situation where important economic policy issues are implemented not in virtue of their democratic constituency, but in virtue of the expertise of those who implement them.

these institutions, perhaps particularly the ECB away from the role of consultants and, increasingly, towards a role of direct policy influence while remaining apolitical in name.

3.2 Distrust and Scientific Authority

While the inclination to leave policy matters to those who claim understanding of the mechanisms at work is great, there is simultaneously a growing distrust and discontent with the idea that science has the answers to questions of policy making. Ulrich Beck noticed this distrust in matters of science and technology, and has illuminated the problem in his work on the risk society (Beck, 1992). Beck argues that from the 1960's people started feeling less optimistic about the promises of science and technology, due in large part to the witnessed catastrophes and uncertainties that modern science and technology brought with it and the social alienation that was produced by it. In this way, the notion of progress in matters of science and technology was gradually and increasingly replaced by a notion of risk, and more specifically a notion of "manufactured risk"; that is, risks that are products of human activities. This revealed a highly ambiguous character of scientific expertise in its relation to policy making. On his account, the role of the scientist is at the same time utterly indispensable and deeply problematic. We need to turn to the specialists because the issues at hand are too complex for non-specialists to even address, but on the other hand we do not really trust the logic of technoscientific rationality to improve the situation, but fear that it might end in annihilation. Beck argues that this criticism is far from an irrational one, but stems from "the *failure* of technoscientific rationality in the face of growing risks and threats from civilization" (Beck, 1992, p. 59).

The same kind of distrust that Beck describes in scientific and technological matters, I contend, is mirrored daily in matters of economic policy, where confidence is also largely waning while the influence of expertise is increasing. I noticed the inability to forecast

economic future above, but the discontent reveals itself in numerous forms - perhaps one of the most visible being the increased anti-European sentiments emerging from within Europe itself. People feel frustrated in their own and their elected official's inability to shape their economic future, as more and more matters of economic policy is being dictated by European technocrats operating beyond what is defined as political. Citizens find themselves in the same paradoxical situation described by Beck, where at once trust is fading and at the same time there is no other viable choice than to turn to the experts. It is very interesting to note that economic (as well as other branches of scientific) expertise thus seems to retain its influence even when the social status of economics and economists is fading, a situation which has been referred to as "the paradox of scientific authority" (Bijker, Bal, & Hendriks, 2009). These writers have noticed that there is no clear correlation between the authority of science and the social esteem of scientists. Instead, they argue that "the cases in which scientific advice is asked most urgently are those in which the authority of science is questioned most thoroughly" (Bijker, Bal, & Hendriks, 2009, p. 1).

3.3 Notes on Normativity

The account I have been giving in the first two chapters gives theoretical support to Beck's contention that technoscientific rationality by itself fails to capture the risks and prospects inherent in modern society, both in areas designated by the term "scientific" as well as in their economic counterparts. Given the account of the production of scientific and economic knowledge sketched out in the first two chapters, and the paradoxical situation of expertise in policy making described above, it seems like there can be no clear cut answers to matters of controversial policy issues. Seen in this light the technocratic response proposed by Millikan seems not only patently naïve, but in many ways a dangerous one. While it may be tempting to adopt an attitude of "leaving it to the experts", who, as it were "speaks truth to power",

such a response is based on the assumptions that objective certainty is attainable, and further that there is a clear correlation between this objective knowledge and the best, most preferable policies, an assumption that we now need to abandon.

What, then, is to be learned from this discussion on the problematic relationship between economic expertise and policy-making? One of the main strengths of economic expertise, I have claimed, is its ability to frame (often highly complex) problems in economic terms, and thus turn complex, heterogeneous problems into technical puzzle solving. While this is indeed a great strength, it simultaneously sets the conditions for the ways in which we are subsequently able to talk about, and respond to, these issues. In other words, once a problem is framed in technical economic terms, this has direct consequences for how we are able to think about these very problems, which in turn tends to exclude those lacking specialized training from entering the discussion. It is important to emphasize that I do not hold that this is necessarily the intent of economists practicing their craft, nor of scientists in general. Rather, I would claim that it is a consequence of a tendency to reduce policy considerations to technical problems.

This tendency is by no means exclusive to economics. In discussing climate changes, Brian Wynne (2010) has argued, I think forcefully, that this issue can no longer be perceived as simply a technoscientific problem. Wynne has observed that terms like “sustainable development” or “generic risk” are often presented by scientists as if they were concrete objects of scientific investigation, masking their epistemological and ethical complexity. They are presented, he continues:

as if their objective structure and limits and *human meaning* can be revealed by science[.] [I]t might be considered natural that citizens would sit back, and wait to be told what they must do, rather than go out and learn as well as take their share of responsibility for what could have been presented as a more complex, multidimensional and inherently indeterminate set of human problems, which citizens and their representatives can and should help define (Wynne, 2010, p. 300).

It is in precisely this sense that I want to argue that such notions as “responsible” or “sound” economic policy is being treated as technical issues for economists to resolve. In the same way Wynne argues that climate change is not simply a technoscientific concern, I would argue that economy is not merely an economic concern; it is also importantly a political and moral one. As such, the first and most immediate normative reaction to be drawn from this analysis is the question of normativity as such. In other words, if we accept that economics is inseparable from politics and vice versa, then the question of normativity ought to be an integral part of the economico-political discourse, and it is precisely this part that tends to be excluded as controversial matters are reduced from complex, heterogeneous ones to technical puzzle solving that can be objectively resolved. At stake is thus not simply how to best respond to various policy concerns - questions which can all too easily be taken up in the epistemic frame of economic orthodoxy - but in a quite literal sense who we are, or rather, who we want to be.

It is, of course, not the intent of this thesis to reject the importance of specialized expertise, nor to suggest that every act of technoscientific experimentation requires public involvement for their legitimacy. In many cases problems are really best left to the experts, and everyone agrees that it is best left to the experts. For instance, few would argue that that a major problem in reconciling quantum mechanics and the general theory of relativity is too little democratic engagement. However, it is very much conceivable that such a reconciliation, if found, will turn out to have great consequences for political and social life, in the same way that nuclear physics turned out to become thoroughly political in the 20th century. It is in these situations, where it becomes utterly impossible to distinguish scientific investigation from political considerations, and where scientific practice comes to directly influence large strata of society, that citizen engagement in science becomes the more crucial. In other words, the problem of non-expert engagement becomes the more pressing as the boundaries between

science and politics becomes the most blurred. However, whereas I have argued that the boundary between economics and politics is blurred beyond recognition, there is suspiciously little room for alternative framing and public involvement when it comes to actual policy making or policy recommendations. This, I have claimed, stems at least partly from the institutional “ownership” by current orthodoxy.

The second normative consideration I want to draw attention to, which is intimately linked with the first, thus revolves around the possibility for democratic participation in a setting dominated by technical discourse⁸. If science strives for universality and consensus, democracy is marked not by the presence of universal consensus, but by contestation. In the face of uncertainty, what might be most needed are alternative narratives; new ways of formulating possible states of affairs (Callon, Lascoumes, & Barthe, 2009). This requires the dual participation of both technical expertise and lay persons who come with their own sources of (often local and contextualized) expertise. By turning controversy into productive forms of dialogue, the boundaries between what constitutes technical problems and those that are social can be constantly renegotiated. The question of reconciling scientific and democratic ideals may thus be an ill formulated one. Rather, this opposition might more constructively be envisaged as an ever ongoing dialogue, where the boundaries are repeatedly renegotiated and reformulated. If, as I have argued, one of the most pressing needs in contemporary economico-political life is the construction of alternative narratives, then spaces need to be constructed where such boundary work can be fruitfully pursued. In the midst of the current economic crisis, this is even more emphatically the case.

⁸ We noticed earlier that the appreciation of the blurred boundaries between science and politics requires us to adopt richer notions of what constitutes political action. In the same way, I use the term democracy here to refer not simply to the electoral-representative system, but to the whole gamut of ways in which citizens are able to shape, change and influence political reality. For a systematic account of such a conception of democracy, see Rosanvallon (2008).

Concluding remarks

It has been the purpose of this thesis to apply an STS understanding of the problematic distinction between science and politics and argue for its relevance in understanding the role of economics in contemporary economic and political life. This, I have argued is particularly relevant due to the current economic crisis, which has tended to move economic policy making away from democratic control, and towards a greater reliance on economic expertise. By drawing on central STS literature as well as economic history, I have problematized the distinctions between an economic sphere, a political sphere and a scientific sphere, arguing instead that these are thoroughly embedded, and that the current institutional structure is by no means an expression of a necessary course of events, but the outcome of contingent (and often local) practices. I have further argued that an appreciation of these blurred distinctions, rather than an insistence on keeping them, may serve as a better starting point from where constructive dialogues can emerge, which may in turn not only increase democratic involvement, but also provide more robust policy making. One should therefore be cautious to applaud the appointments of technocrats as the saviors of Europe; as the embodiment of Rousseau's law giver. While technical expertise undeniably serves an important function, I have argued that the legitimacy of their policy-influence cannot be set by reference to the possession of neutral and objective knowledge. By realizing this, we open up for alternative narratives, and I have contended that one of the most pressing needs contemporary economic-political life might be to construct spaces where the boundaries between technical and social problems may be openly contested.

The line of thought developed above opens up trajectories for interesting further research, both theoretically and empirically. As I have argued that the production of economic knowledge relies on a series of specific material and social mechanisms, it would be very useful to study these in more detail at their actual production sites; that is, both in the

academic world of economic research, as well as in institutions operating on the border between economics and politics. The kind of laboratory work that Latour and others have conducted is in this way also highly relevant in understanding the production of economic knowledge and its relations to political action. Further, as I discussed at the end of the thesis, this line of study opens up for novel ways of understanding political and democratic participation, which I argue call for more theoretical attention.

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