1 Introduction

> The roads to human power and to human knowledge lie close together, and are nearly the same; nevertheless, on account of the pernicious and inveterate habit of dwelling on abstractions, it is safer to begin and raise the sciences from the foundations which have relation to practice and let the active part be as the seal which prints and determines the contemplative counterpart.
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> Francis Bacon, 1620 (Book II, 4)

> The fact that science contributes to the social life-process as a productive power and a means of production in no way legitimates a pragmatist theory of knowledge.
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> Max Horkheimer ([1932] 1972: 3)

The prevailing view

In order better to understand the task we have set for ourselves in this study, it is useful first to refer to the prevailing view in response to the question of the conditions that make for the *Power of Knowledge*. The two mottos above, Horkheimer's taken from an essay written in 1932 for the inaugural issue of the *Zeitschrift für Sozialforschung*, and Francis Bacon's from *Novum Organum* of 1620, both endorse a clear separation between questions pertaining to the *truth* and *utility* of knowledge. The distinction between truth and utility is one of the traditional philosophical distinctions in reflections about attributes of knowledge that may make ideas powerful in practice. The year 1932 is symbolic and significant, of course, and Horkheimer's insistence that it is not for societal interests to decide what is true echoes emerging, profound political struggles about the role of science in society. Science and the ideas that materialize from the scientific community are not the handmaidens of power, nor should they be deprived of their proper autonomy. In defending the autonomy of science, Horkheimer ([1932] 1972: 4) also insists that the philosophical perspective that favors a clear separation between the utility and the

truth of knowledge does not lead to a separation or alienation between theory and action (practice).

Horkheimer's position, though using different terms, refers to a primary and a secondary code of knowledge production. The primary code of knowledge production represents truth; the secondary or derivative code refers to the utility of knowledge claims. The implication of the thesis of a primary and secondary code governing knowledge production suggests that the most useful theory is a good theory or truthful knowledge claim. Theory and practice are alienated from each other, but the ruling code in science is truth and not usefulness. Truth controls use. And this is why it is useful for society to allow the autonomy of science.

In a simplified way, one could state that the prevailing view of the relation between power and knowledge can be described as the *instrumental model*.[1] It is characterized by the following principles:

First, the structure and culture of human groups, as producers of knowledge, have no influence on the production of knowledge or on the context of justification. The development of knowledge is driven and determined by the "logic" of science in conjunction with the nature of the world of objects. Second, the use of knowledge is largely independent of the context of application (hence the frequent statement that knowledge as such is value neutral, and can be applied for good or bad purposes). And third, the utilization of scientific knowledge is not impeded by the special circumstances of time, place, and social conditions (sometimes called the *objectivity and rationality assumption*). A scientific body of knowledge is said to be true everywhere.

The model of instrumentality conceives of scientific theory and research as a kind of intellectual tool to be employed in practical situations. Theoretical knowledge, as long as it is true or adequate, is also reliable and useful. Theoretical knowledge alone does not guarantee a successful execution of desired social action, nor does it ensure the value of the means chosen to reach a specific goal. But theoretical knowledge, if utilized, secures a kind of technical relief (*Entlastung*) for

[1] We are aware that the terminology needs clarification. In the philosophy of science before Popper, the *instrumental view* meant Bishop Berkeley's view that scientific theories might work in practice, i.e. they are instrumental, but they are not necessarily true. This was in the early days of modernism, as for many it was sacrilegious to use the concept of truth outside of church scholasticism. However, this view was dominant among practicing scientists, who left the philosophy to the philosophers. Popper (1956), of course, did not sympathize with this attitude, which he described as "science as plumbing." According to Popper, science must never give up the concept of truth. In this book we use the term *instrumentalism* to denote a connection between theory and (successful) application – as expressed, for example, in Kroto's quote above.

actors. Actors are not themselves required to manufacture the knowledge to be utilized, nor is it necessary for them to comprehend the scientific context in which the theoretical knowledge was generated in the first instance. What is sufficient is an adequate understanding of the conditions of application; the desired effect is then guaranteed by virtue of the truth of the theoretical knowledge.

In an essay entitled "Causality in the Social Sciences," Lewis S. Feuer (1954) notes that the loyalty of social scientists to particular theoretical traditions is driven by their adherence to one of a pair of meta-sociological convictions. Feuer calls these meta-convictions *necessitarian* and *interventionist*. Social scientists who adhere to the necessitarian model are persuaded that a predicted state of affairs cannot be prevented from being realized. The interventionist perspective assumes that one can intervene to alter a given state of affairs, but also to block the predicted states from coming into existence. There is an elective affinity between Horkheimer's distinction between the truth and utility of knowledge claims and Feuer's necessitarian and interventionist models. As Feuer (1954: 683) observes, social science theories win allegiance from social scientists not so much because they are able to muster more empirical evidence than competitors, but rather because of the ways they open up possibilities of human action and of human intervention.

In this book we want to revisit and critically consider what appears to many other observers to be the set of self-evident reasons that account for the power of knowledge. The taken-for-granted answer among the public, many policymakers and members of the scientific community is to be found, in the first place, in the conflation of knowledge and truth. What counts as objectivity and truth is subject to historical change, as several studies have shown (Shapin 1994; Daston and Galison 2007).

In modern society, what warrants the conflation of knowledge, truth, and power is mainly linked to dominant norms in the scientific community. These norms postulate that knowledge claims are at their best if they are trans-historical and trans-situational. The decline or loss of context-specificity of a knowledge claim is widely seen as adding to the validity, if not the truthfulness, of the claim. For the citizens of modern society, the solution to the problem of the conditions that give rise to power of knowledge is delegated or relegated to sanctioned procedures of knowledge acquisition as practiced in science, particularly natural science.

We do not question the assertion that knowledge can be powerful. But this assertion does not answer the question of *why* knowledge can be powerful, or for whom knowledge may be powerful. Nor do we doubt that the source of many contemporary powerful knowledge

claims originates within the scientific community. We certainly concur with a growing number of observers that we are living in a knowledge society, and that the command of knowledge is constitutive for authority, inequality formation, participation in civil society, or generally for the identity of individuals.

But it is equally evident that science advances and continues to issue numerous claims that prove to be completely ineffective in practice. What turns such an observation about the "impotence" of science – with respect to many practical social and personal problems – into a most intriguing and puzzling observation is that such ineffective claims are often warranted by the scientific community as in strict conformity with the procedures that otherwise constitute eminent, reputable, and reliable scientific claims. As a result, it is misleading to ascribe to science a kind of boundless power.

One should therefore examine exactly why knowledge is effective in one case but has no effect in another, although in both cases the authenticity of knowledge is guaranteed. Today's standard and rarely doubted response refers to an identification of knowledge with power. This comforting response assumes knowledge to be something so compelling that its power stems from its own genesis (within science!). But in so doing we cannot distinguish between different forms of knowledge on the one hand, and we have no explanation for the successful or unsuccessful application of knowledge on the other. In the first case one assumes that traditional knowledge was too weak to stand up to scientific knowledge. In the second case one seems to rely on the different degrees of veracity of knowledge. Accordingly, knowledge assets would therefore be ineffective because they are theoretically inadequate. We deliberately exaggerate in order to explore the implications of these arguments. We are well aware that there exist approaches that challenge the traditional concept of knowledge and power. This often happens only implicitly; for example, when implementation research asks why a "good" theory was not effective in practice; or when researchers show that technologies quite often work even in the absence of full theoretical explanations (Perrow 1984). What these approaches lack is a systematic analysis of the characteristics of knowledge and power.

Unlike the traditional models of the effect and power of knowledge, we point out that the source of the effectiveness of knowledge does not lie in the process of knowledge production, or in certain norms of the scientific community when it tries to clarify controversial knowledge. Rather, it is crucial that if knowledge is going to be practically relevant, it must include the policy options that should be manipulated in

a certain way so that reality can be brought into line with the relevant knowledge. This means, first, that a lot of knowledge is never put in a position directly to transform reality (and is therefore not in a position to reshape the reality). It means, second, that in order to become effective in practice, a theory need not contain all aspects or variables of the current reality to which it refers. The proposition that only a complex theory is capable of effecting change in a complex reality should be discarded.

Taking a comparative and historical perspective, this book poses the broad question of how political decisions relate to scientific knowledge, and in what sense we can actually speak of the power of knowledge. We will do this through some theoretical considerations and the study of empirical data. We shall examine two cases from the early, and one case from the late twentieth century. Race science was a policy-relevant theory and body of research before World War I, and Keynesian economics from the 1920s on. Race science could look back at the heritage of evolutionary theory, with many prominent scholars espousing it, including Darwin himself (Sewell 2010). In Germany it came to be used as a political tool, and provided a legitimating basis for the Holocaust. In the 1920s and 1930s, John Maynard Keynes proposed economic policies to help solve problems of the crisis-ridden British economy. These initially fell on deaf ears, but became dominant after World War II. Keynesian policies were implemented in all developed capitalist economies. At one point, a US president famously pronounced that "we are all Keynesians now."

Late in the twentieth century, concern about the Earth's atmosphere arose, initially instigated by atmospheric scientists. A small group of advocate scientists in the 1970s alerted the world public about an imminent global ecological catastrophe, the depletion of the ozone layer. With the establishment of a largely successful international regime, basic institutional features for a science/policy interaction were established. Several influential actors tried to repeat a similar approach in the case of climate change (after 1988), though so far with little success. While the political success in ozone politics is widely celebrated, climate policy and politics seem to be in a mess. In this book we will particularly examine the institutional framework that was set up to ensure an effective transmission of scientific research into political action through the Intergovernmental Panel on Climate Change (IPCC). Again, as in the case of race science, one could say that the science base was the same, at least at first glance, but the policies implemented by various nations were different.

Within the context of each of the three cases, we introduce a comparative dimension which is useful to address questions of policy variation across national jurisdictions. In the case of Keynesian economics and its impact on the evolution of modern economies, we discuss, among other things, the role of the notion of "complexity" of social phenomena and the extent to which the complexity of the social world has to be mirrored in social science knowledge as a precondition for its practical effectiveness. We also ask what the reasons were for a difference in the appeal of Keynesian policies in different times and places. In the case of climate science, we refer to the role of an internationally unified scientific assessment and the question of why national climate change policies have varied hugely. Similarly to the discourse of climate change, we see how race science was embedded in cultural and political resources of the day and generated knowledge claims that resonated closely with the politics of the time. We will highlight that knowledge production in itself need not be a virtue in modern societies. The national variations will be addressed again as the question arises as to why race science led to the Holocaust in Germany, but not in other countries.

The linear-rational model and its critics

Scholars of public policy, especially in the United States, have been using the instrumental model described above. In a well-known contribution, Harold Lasswell and Abraham Kaplan (1950) depict a linear-rational model of policymaking.[2] It follows an enlightenment model of politics, in which scientific knowledge helps to solve societal problems. If science produces true and valid knowledge, this can be used in the political process, where it produces the "right" political decisions and effectively resolves politically motivated debates. This view has been shared by many authors before and after Lasswell and Kaplan's book was published. The hope was that a science-based solution will be agreeable to warring parties, since it transcends the ideological (metaphysical) differences.[3]

It might be useful to distinguish between two strands in the public policy literature: a rationalist and a pragmatic strand. The rationalist

[2] This model covers technological applications of scientific research as well. Godin (2006) has argued that the "linear model" is a stylized artifact that emerged out of various institutional practices (US government accounting schemes and OECD statistical definitions). It cannot be attributed to single individuals alone, as has been the case with regard to Vannevar Bush (1945), although this is common practice.

[3] Otto Neurath, founding member of the Vienna Circle, put it like this: "Metaphysical terms divide; scientific terms unite" (quoted in Cartwright et al. 1996: 179).

approach tries to base political decisions on the best available knowledge, whereas the pragmatist approach aims at incremental, negotiated solutions that work.

Charles Lindblom's classic description of public policy as the "science of muddling through" can be read as a pamphlet in favor of a pragmatist approach. Lindblom contrasts the rational approach (which involves a huge information collection exercise plus a systematic comparison of available alternatives of action) with a more modest approach, where the policymaker considers only a few policy alternatives (most of which will be familiar to him from past controversies), relying on a record of "past experience with small policy steps to predict the consequences of similar steps extended into the future" (Lindblom 1959:79). Echoing James March and Herbert Simon's (1958) bounded rationality thesis, Lindblom argues that the first of these approaches is impossible with complex problems, since limitations of time and resources (monetary, intellectual, and informational) are overwhelming. He even suggests that in practice, administrators are advised not to practice the first model, but rather to restrict their consideration of policy alternatives to just a few. It is therefore curious that "the literatures of decision making … and public administration formalize the first approach and not the second" (Lindblom 1959: 80). In other words, practitioners know that they cannot cope with the demands of the rationalist model, but academics are oblivious of this and theorize exactly such a model.

In a later article Lindblom returned to the topic, defending the second approach which he now calls "disjointed incrementalism." His argument rests on the case that we will never achieve a "full picture" or a *synoptic view* of all relevant elements (values, information, factors, causes …) that are prior to a decision. Instead, we have to proceed from a grossly incomplete analysis, but do this in a conscious way. It is of no help to appeal to the ideal of synoptic analysis, as this will lead to worse outcomes compared to decision-makers who are conscious of the limitations and muddle through open-eyed, so to speak. As he put it, "a conventional synoptic (in aspiration) attempt to choose and justify the location of a new public housing unit by an analysis of the entirety of a city's land needs and potential development patterns always degenerates at least into superficiality if not fraud. A disjointed incremental analysis can do better" (Lindblom 1979: 519).

Another line of inquiry has developed the view that politics and science are at odds with each other, mainly due to epistemological reasons and language barriers. The "two communities" model (developed, *inter alia*, by Caplan 1979) casts doubt on the concept of a linear rational model and sees the relation between science and politics as difficult.

The two are characterized by different logics and cultures.[4] While the scientist wants to arrive at the truth, the politician is concerned about power. Asserting the theory of functional differentiation, Luhmann (1984) makes a more basic point about the problematic nature of communication between social systems. Communication between politics and science is problematic, or "highly improbable."

Whenever ideas have become institutionalized in policies and have therefore become real, it seems only natural to think that what has happened had to happen. In other words, the link between knowledge and politics seems unproblematic, even inevitable. It is the task of the historian and critical social scientist to unravel this apparent inevitability. Michel Foucault used the term *discourse* to describe the reality of thoughts and practices enshrined in a specific historical period. He used the term *archaeology* to describe the efforts needed to analyze and deconstruct these discourses. Of course, social scientists know that the roles of academics and policymakers are different, and that actors from these two fields inhabit different epistemic universes. One can therefore assume that it is unlikely that these roles will intertwine easily. Unlikely does not mean impossible, but the possibility of such "meetings" needs to be investigated carefully.

It has been observed by people who were active in both roles that it is nearly impossible to step out of the role one currently embodies – all prior knowledge and empathy with the "other" role notwithstanding. Consider this everyday example: A car driver wants to get to his destination quickly, and therefore endangers a pedestrian crossing the road. The pedestrian could do the same a few moments later after she gets into her car and drives off. Likewise, the car driver whom we saw in a rather reckless manner will eventually revert to his role of pedestrian. Imagine he has just stopped by the roadside and wants to get to the shop on the other side of the road. He will now find himself complaining about irresponsible drivers trying to "knock him over." We are all familiar with such everyday examples which teach us how difficult, perhaps even improbable, it is that "lessons learnt" from one role will swiftly improve one's performance in the other roles. One might say that only accidents or near misses will produce the necessary shocks for a re-examination of routines.

[4] The two communities model has been superseded by policy network approaches (Heclo 1978; Marsh and Rhodes 1992) and discourse coalitions (Hajer 1995; Gottweis 1998). Here, a close exchange of information between actors of different social subsystems is postulated (including representatives from industry, science, administration, and the public). They participate in a public discourse and at times also cooperate within less visible networks in order to influence political decisions. They confront another set of actors who support different interests, values, and political goals.

Hernes has given an account of his personal experience commuting between the worlds of (social) science and politics. He notes that politicians and social scientists show a mutual benign neglect for each other, "politicians funding research but taking little interest in the results; researchers describing the world, but not really expecting much in terms of changing it" (2008: 258). Hernes goes on to construct a typology of the two roles.[5] He suggests that the first step in the work of a social scientist is always an observation in need of explanation, whereas the politician starts with the definition of a political issue that needs to be addressed (and remedied). It is therefore "the aim of the scientist … to explain reality, the aim of the politician to turn something into reality" (Hernes 2008: 262). The politician needs "levers of action" in order to change reality – moreover, a skilled politician should be able to foresee side effects and unintended consequences. Hernes concludes with the remark that the task of the scientist is to "invent explanations and validate them," whereas the task of the politician is to "invent interventions and implement them" (Hernes 2008: 263). It would be interesting to carry the argument one step further and see what happens when scientists (or other non-politicians) try to affect political changes, and are savvy enough to understand the nature of the political process. Following Marx's dictum ("The philosophers have only interpreted the world in various ways. The point, however, is to change it"), many have tried to do so, and not only Marxists. Scientists working in nearly all disciplines, from anthropology to zoology, have made attempts to influence political outcomes through their open or hidden advocacy. So have non-governmental organizations (NGOs) and business organizations, at times working closely with scientists, at times providing knowledge claims themselves.

We can identify a potential overlap between the roles of scientist and politician, revising the role Hernes assigned to the scientist as an exclusively cognitive being. Suppose a scientist knows that politicians want to act and be seen as proactive on an issue. If his or her scientific research indicates "levers for action," and if he or she manages to present the complexity of the issue as manageable, then the chances are much higher that such research will be considered relevant for the policymaker. It would seem that the more scientists understand the nature

[5] Without going into too much detail regarding his typology and some problems associated with it, suffice it to say that he seems to adhere to a rather naïve, Popperian view that scientists would reject a theoretical model if empirical evidence did not conform to it (Hernes 2008: 262).

of the political process, the more they are likely to smuggle research results into political practice, thereby making them effective.

Data and consensus

Knowledge, it should be noted, rarely comes in a form that is unambiguous or simple. To be sure, policymakers may prefer simplicity, and experts sometimes comply with such requests. But even if knowledge is presented at the outset as a simple set of facts, predictions, and recommendations, more often than not it turns out to be complex at a deeper level. This point is apparent in the diversity of policies based on similar, or the same, scientific advice.

We will show how this plays out in our three case studies. With regard to race science, it is obvious that there were vast policy differences across nations despite a common science base. After all, the Holocaust was carried out by one government, not by others. Equally, the case of Keynesianism shows varieties of implementation. To be sure, one could say that there was no overall agreement on the term "Keynesianism." However, the theory entailed policy recommendations drawn up by one idiosyncratic scholar who made specific recommendations to policymakers. Climate change, our third case study, shows how a transnational effort at orchestrating science and policy could not eliminate variations in domestic policies.

Gormley (2007) suggests that economists (in contrast to other social scientists) have been particularly influential in shaping public policy. Writing on the deregulation atmosphere of the United States in the 1980s, he argues that much of the deregulatory push had been advocated by distinguished economists from elite universities, and gained wide acceptance within policy circles. Environmental policy is one telling example where we witness a move away from command-and-control approaches to cost–benefit analysis and market-based approaches. The US Clean Air Act amendments of 1990 introduced emissions trading, an instrument applied to sulphur dioxide (SO_2) reduction. It soon became a favorite in international negotiations regarding climate change. Before that, market-based approaches had been seen with great skepticism, not least by European governments (Damro and Luaces-Méndez 2003; Gilbertson and Reyes 2009). Gormley cites other examples of successful application of policy proposals developed by academics, including those coming from political scientists. However, he notes that there are also many examples where their proposals have fallen on deaf ears. He goes on to offer an explanation for the differing results. In this account, expert consensus is especially important. Proponents of policies armed