

- (1996b) "The Impossibility of Naturalism: The Antinomies of Bhaskar's Realism," *Journal for the Theory of Social Behaviour* 29: 267–88.
- Locke, J. (1961) *An Essay Concerning Human Understanding. An Abridgment*, London: Dent.
- Magill, K. (1994) "Against Critical Realism," *Capital and Class* 54: 113–36.
- Outwaite, W. (1987) *New Philosophies of Social Science: Realism, Hermeneutics and Critical Theory*, Basingstoke: Palgrave Macmillan.
- Rend, I. A. (2011) *Interpretation and Social Knowledge: On the Use of Theory in the Human Sciences*, London: University of Chicago Press.
- Roberts, J. M. (1999) "Marxism and Critical Realism: The Same, Similar, or Just Plain Different?" *Capital and Class* 68: 21–49.
- Sayer, A. (1992 [1984]) *Method in Social Science: A Realist Approach*, 2nd edn, London: Routledge.
- (1998) "Critical Realism and the Limits to Critical Social Science," *Journal for the Theory of Social Behaviour* 27: 473–88.
- (2000) *Realism and Social Science*, London: Sage.
- Stones, R. (2001) "Refusing the Realism–Structuration Divide," *European Journal of Social Theory* 4: 177–97.
- (2005) *Structuration Theory*, Basingstoke: Palgrave Macmillan.
- Trigg, R. (1989 [1980]) *Reality at Risk: A Defence of Realism in Philosophy and the Sciences*, 2nd edn, Hemel Hempstead: Harvester Wheatsheaf.
- (1993) *Rationality and Science: Can Science Explain Everything?* Oxford: Blackwell.
- Vandenberghe, F. (2005) "The Archers: A Tale of Folk (Final Episode?)," *European Journal of Social Theory* 8: 227–57.

24

OBJECTIVITY

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If we look at the various contexts in which objectivity is pursued (scientific inquiry, social debates, policy decisions) we immediately realize that, by referring to this concept, there is often a chasm between what is ideally aimed at and what can be sensibly argued for and/or achieved in practice (different practices).

Traditional empiricist epistemology tells us that to be objective is to represent the world the way it really is. This is sometimes referred to as 'the view from nowhere' (Nagel 1986), which entails looking at the world in a way that transcends individual experience, perception, or perspective. The underlying assumption is that our mind is such that, by either association of ideas (e.g. Locke) or by means of innate rules of reasoning (Descartes), it is able 'mechanically' and without any creative additions to reproduce existing states of affairs. A crucial corollary to this assumption is, of course, that there is a way that the world is, independently of how we view it, or what we make of it—a way that our mind can faithfully capture.

The domain of inquiry that best suits this scenario has long appeared to be the natural sciences. There is a long tradition—from Bacon to Hume to Galileo and Descartes—that claims that there are facts in nature that natural science is well equipped to investigate. The empirical methods of science successfully lead to the discovery of the natural laws that describe those facts with accuracy and explain them. The results of scientific inquiry are then the closest kin to offering a 'view from nowhere'. So called 'mechanical objectivity' established itself as the ideal image of scientific representation: "the image, as standard bearer of objectivity, is tied to a relentless search to replace individual volition and discretion in depiction by the invariable routines of mechanical reproduction" (Daston and Galison 1992, 98). Mechanical objectivity also entailed a particular type of scientist: "long on diligence and self-restraint, scant on genial interpretation," someone who is willing to "let nature speak for itself" (Daston and Galison 2007, 120–21).

Historically, traditional empiricist epistemology and the natural sciences have become the models for defining objectivity in the sense just suggested, as well as for dictating the standards by which to assess whether, and how far, domains and procedures of inquiry are (or can be) objective. The social sciences—a later addition to the field of science—are enmeshed in this joint scenario. They were evaluated in comparison with the natural sciences, and this portrayed them from the start as 'lesser' sciences, bound to imitate the well-established paradigm of the natural sciences. In particular, by modeling social facts on natural facts, they were asked to embrace an epistemological view of objectivity that does not necessarily or appropriately fit

the social domain. Indeed, the social sciences, besides being penalized by this comparison, were also somehow cheated: the traditional epistemological framework of knowledge as embraced by scientific method was put under severe critical scrutiny in the second half of the 20th century, and at least by its most radical critics it was declared altogether untenable. The image of science that social science was meant to imitate gradually lost at least some of its paradigmatic appeal.

That the social sciences have been caught in this comparison, and with what consequences, can be seen from two of the main debates where the issue of objectivity has been traditionally raised and discussed: the real/constructed debate, and the fact/value debate. In this chapter I will first retrace the general features of these two traditional debates and bring out the meaning of a concept of objectivity for social science formulated within the epistemological limits and constraints set out by these debates.

However, it will also be pointed out that these debates illustrate attempts at readjusting the concept of objectivity in such a way that might better fit it to the social domain—enough of an admission that the ideal standard is at the very least insufficiently descriptive within this domain (and possibly more generally, across the board of scientific disciplines). Reflecting on objectivity along these readjusted lines makes us ponder on what of the traditional concept can be retained and what ought to be dropped—to the point of doubting whether any concept of objectivity can remain at all a meaningful and purposeful concept in contemporary discussions. As a starting point for objectivity still plays a central role, for example in the ongoing debate on the use of science, natural and social, in policy making (the so-called 'evidence-based policy and practice' movement), or in the contentious disputes concerning the reliability of so-called expert opinion. In the second half of this chapter I will look into what is expected of a concept of objectivity when it enters these more practice-oriented debates, and whether it still makes sense to invoke such a concept. We will see that, *pace* traditional epistemology, if objectivity is believed to have some use in the realm of social practice, it cannot ignore contexts, interpretations of contexts, and human judgment.

1 Received Views and Issues Redressed

1.1 Objectivity and Man-Made Facts

One way of questioning the objectivity of social science is by questioning the reality of the facts and phenomena it investigates. Surely an objective science should deal with 'real facts', facts whose existence can be discovered by our theories, inquiries, experiments, etc. Ontological objectivity, taken in this sense, entails that the objects of the material world, the world of nature, ought to be 'out there' (in some way to be qualified), and that they do not need human intervention in order to exist. Social objects, instead, seem to exist precisely because humans make them what they are, in the forms and types that we are acquainted with in our societies: money, marriages, race, political leaders, etc. Social facts owe their existence to societal configurations and choices, not to natural law. It is our actions, our inquiries, our theories, or our history that contingently and contextually make up social facts. Sometimes this idea is expressed by saying that social facts are 'invented' by humans. Needless to say, if we follow this line of argument, a science of invented facts barely qualifies as being 'objective', let alone as being a science *tout court*.

The discovered/invented divide has been traditionally used to mark a substantial difference between the natural and the social sciences, and to claim that only the former can be objective, as they deal with real objects (objects that are not an effect of their own creation). As is

well known, the argument concerning the invented nature of man-made facts was widened also to include the reality of natural facts. Natural facts are as invented as the social facts, it was claimed, therefore the objectivity of natural science is on a par with that of social science, i.e. itself an invention.² However, even if we are prepared to maintain a distinction between the two domains and argue for an ontological difference between natural and social facts, this does not entail that objectivity is an out-of-reach goal in the social domain of inquiry. It does, though, take some domain-specific further reflection. A few examples will point in this direction.

According to Searle (1995), there is indeed a significant ontological difference between natural and social facts (or 'brute' and 'institutional' facts, as he refers to each of the two categories). Natural facts, unlike social facts, exist even without us: mountains are there, whether we represent them or not, whether we climb them or not. Natural facts, as he puts it, are ontologically objective. Social facts, instead, exist only if we represent them: in this sense, they are ontologically subjective. Yet, they qualify as objective entities, though only in an epistemic sense. What Searle means is that such facts owe their 'objective existence' to a collectively sustained recognition of their existence, namely to human agreement. More specifically, he argues, these facts might have some physical or material existence (for example, a piece of green paper in my pocket) but this is not enough nor appropriate to grant objective existence as social facts (e.g. being a five-dollar bill). In order for these facts to be acknowledged as objective kinds of entities (e.g. money), their physical existence needs to be represented and used in a particular way by one/more classes of individuals who agree about their specific representation and use. In this way, an ontology of objective social facts becomes possible, and so—we might add—is an objective science that deals with this category of facts (facts that are at the same time ontologically subjective and epistemologically objective, in Searle's terminology).

But there is more. If we focus on the mechanism of representation, a further difference can be brought up as regards natural and social facts. In describing the objects of the natural and the social world, Ian Hacking famously made a distinction between 'indifferent' and 'interactive' kinds (Hacking 1999, 103–6): "The classification 'quark' is indifferent," he explained, "in the sense that calling a quark 'quark' makes no difference to the quark." With social or human kinds, the situation changes. When, as it happens in the social sciences, people and people-related facts become an object of study—child TV viewers, criminals, women refugees, racism referred to black and white people, etc.—they 'interact' (accordingly or contrastively) with the ways they are classified, described, or represented. People themselves also often experience who they are and what they do in the world according to ongoing classifications and descriptions. There is a feedback, or 'looping' effect involved with classifying people, which does not occur in the case of natural kinds, and which uplives the representational mechanism of social-kind making. Kinds of people may change, because the people classified as being of certain kinds might themselves change as a consequence of being so classified. The targeted referents of social scientific inquiries are, so to speak, constantly (at least in principle) on the move.

What effects does the way we classify social kinds have on their reality? Where should we look for their reality, if they owe their existence to the way we represent them? Does, or can, a science of social kinds be objective? It has been argued that the reality of social kinds should be assessed at the level of the specific categories the social order proceeds from, and not by comparison with natural-kind categories. Michael Root (2000), for example, claimed that in the social sciences "real taxonomy is less about generalization and more about regulations: we divide ourselves not by discovering our differences but by requiring ourselves to be different." Social categories, in other words, are essentially normative: "extrapolation across all instances is not possible, but

normalization is," in the sense that social categories may prescribe not so much how whatever is classified by means of a certain category is, but rather how it ought to be. This is why these categories are "well made for social regulation" (Root 2000, S633). This does not mean that social categories are 'less real' than natural categories. The social world, just as the natural one, is ordered, but the source of such order is different: as Jon Elster once pointed out, it is an order based on norms and regulations (Elster 1989, 287; also quoted in Root 2000, S635).

Root discusses the example of race. Race, he claims, is like crime. If we had not invented appropriate laws or drawn certain property distinctions, nobody would be guilty of theft. Yet, given that we did, social scientists can classify us along certain categories, provide descriptions and explanations by making use of them, and even try to predict or explain phenomena and trends by means of those categories. Race is biologically real: to be black is to be black, not just to be perceived as being black, or to be believed to be black. And yet, the reality of race as a social category depends "on what we (collectively) have made of race, and ... whether we regulate or discipline each other by race. Should we divide but not regulate by race, we would treat the races but not conserve their reality." So Root concludes: "Laws of nature do not make race real, we do" (Root 2000, S635, S638). As Searle would also say, we add categories to the world, we make 'new things' exist in the world by means of our categories, and we then turn towards investigating these 'things' with the tools and techniques of any science. The real/invented divide is not necessarily a threat for an objective science of the social.

This divide was, however, also taken up from one side in this debate as a reason for doubting whether using science to describe and explain social facts is indeed altogether appropriate. The subject matter of social research is not, properly speaking, social 'facts', but rather human action—a category, or better a 'concept', it is here argued, that cannot be captured by an apparatus of laws and causes. In the late 1950s Peter Winch, informed by Wittgensteinian philosophy, argued that in order to understand what is at stake when individuals act in historical and social contexts we ought to appeal to socially shared rules, meanings, reasons, and interpretations. No social 'science' can achieve this type of understanding. This is a task for a social philosophy.

Asking whether social philosophy can be objective cannot then be answered by invoking scientific methods and their results. Social philosophy is objective when it can grasp the point of, say, a social action in the light of, and on the basis of, community-followed rules of behavior—rules that we all share (more or less critically, or contentiously) when we act, and/or that we try to unveil and understand when others act. To be objective here is a type of intersubjective intelligibility made possible by a domain-specific understanding of the ontology of the social world (Winch 1958).

1.2 Do Values Detract from Objectivity?

Separating out facts from evaluations was one of the epistemological obsessions of empiricist philosophy. Starting from Hume's famous distinction between 'is' and 'ought' (signaling statements of facts, pertaining to how things are, and statements of value, pertaining to how we view things), a long tradition has sedimented on the position that knowledge is objective (and so is the inquiry into knowledge) only if values are sidelined. Values entail perspectives, perspectives are by definition partial, and partiality easily leads to distortion.

Objective knowledge cannot distort how things are. It should instead describe them as they really are. Besides, in this tradition values, by belonging to the realm of subjectivity, express inclinations, attitudes and feelings that cannot be factually justified or rationally disputed.

Knowledge cannot be objective and evaluational at the same time, precisely because objectivity entails justification and rational resolution of conflict among viewpoints (only one out of a range of viewpoints can be the objective one, and for reasons that are both valid and factual). Scientific knowledge has become the prototype of this position, along with tradition. Of course natural scientists have beliefs or make value-judgments. However, their scientific results are independent of those beliefs and values. There cannot be any connection (logical inference) between factual conclusions and the presence of evaluations in the premises of scientists' investigations (see, among others, Hempel 1965, 91). In this sense, value-freedom is at the very essence of 'scientific' inquiry proper, and when social science entered the debate on the status of science and objective knowledge, in the late 19th century, this was the consolidated view it had to confront with. How did social science score *vis-à-vis* this general perspective?

Objects such as marriages, racism, political leaders—unlike molecules, gas temperature, and chemical reactions—arguably entail the very viewpoints and values that identify them as the type of objects they are in the societies where they belong and where they are valued (or devalued). Take those viewpoints and values away (indeed, take humans away) and they would cease to exist altogether, as we saw in the previous section. Asking social science not to deal with values would be just like asking it to be a science with no objects to investigate. Aware of this impasse, supporters of empiricism pursued strategies to have it both ways: keep values at a manageable distance and declare social science something of a science. Here are some alternative ways of achieving this.

Ernest Nagel pointed out that, regarding the fact/value distinction, the social sciences are not in a very different position from the natural sciences. Take for example a sociologist of religion who, in order to analyze his subject matter (religion), is required to characterize a series of distinctions within and about religion—for instance, a distinction between a mercenary and non-mercenary attitude. A distinction of this sort is somehow value-dependent, in that a value-judgment is passed through the terminology here adopted (the term 'mercenary' is normally used with some pejorative overtones). However, this does not make the sociologist much different from a physicist who describes a particular chronometer as inaccurate, or from a biologist who describes anemia as an undesirable condition. In fact, the attitude, commonly shared by both kinds of scientists, of expressing an evaluation (approval/disapproval) of the objects they describe does not prevent, or take over, their capacity for characterizing those objects, that is for making "an estimate of the degree to which some commonly recognised type of action, object or institution is embodied in a given instance" (Nagel 1961, 493-94). In both cases we are able to detect that an evaluation has been imposed on the description of either object, and this shows how there is more than values in that description. Admittedly, says Nagel, isolating the evaluational input might prove more difficult in the case of the social sciences (values are here commonly attached not only to the ends but also to the means of social research; values are often embedded in the very terminology used by the social scientists, etc.), but this is a practical difficulty, not an impossibility in principle.

With this position, the social sciences are put on a par with the natural sciences *vis-à-vis* the distinction (at least in principle). However, even assuming that the evaluational content—despite practical difficulties—can be singled out and put on hold, we are still left with the problem of what to do with the 'exuberance' of values in the social domain. Max Weber gives an articulated answer to this problem by analyzing how and where values play their role in social scientific inquiries, and even before that, by questioning whether altogether different types of values are present in those inquiries.

An important distinction must be drawn, says Weber, between individual, practical evaluations or prejudices (what he calls *value-judgments*) and those values which attribute significance to, and reveal our interest in, what we investigate—that is, values, which make certain objects relevant for us to inquire about (*cultural values*) (Weber 1904). In choosing what to investigate in social inquiry *value relevance* is what social scientists appeal to (they use values, rather than *hard criteria*). So, for example, what makes the emergence of capitalism in Europe an interesting 'object' of sociological investigation is, according to Weber, a particular feature of it, that is, the fact that capitalism has become a systematic attempt to organize human activities in an efficient, calculable, and impersonal way. This is the feature that Weber calls 'internal rationalization'. As a result, capitalism will be studied according to the features deemed *relevant* for an interesting sociological investigation. Or more generally, what qualifies an event as 'socio-economic' is not something that the event itself possesses objectively. Its identification is rather conditioned by "the specific cultural significance which we attribute to the particular event" (Weber 1904, 64). Nonetheless, a value-dependence (or value-relevant) object of inquiry does not exclude the possibility of a value-free investigation. Indeed, once the object of a social inquiry has been identified by means of relevant values, the social scientist is free (and must be free) to proceed in the investigation of this object by making use of the empirical, testable methods of any science. Social science can, in other words, for Weber be 'objective' despite being informed by relevant values.

The problem for Weber is not only to distinguish facts and values, but to distinguish between different categories of values, and allow research to be driven (at least initially) by the 'right kind'. The objects of inquiry for Weber (partially informed by a Kantian viewpoint) are by necessity 'perspectival' objects (he names them 'ideal types'), assembled and identified according to what we value and what makes them interesting: "in the method of investigation, the guiding 'point of view' is of great importance for the construction of the conceptual scheme which will be used in the investigation" (Weber 1904, 84).

Weber's position is important not only in opening a more nuanced debate about the nature and role of values in science (Kincaid et al. 2007; Longino 1990), but also in allowing a re-evaluation of the concept of objectivity as traditionally constrained by empiricist epistemology. Against the backdrop of a strict fact/value distinction the default position is that of keeping facts separate from values. On a Weberian framework we should keep facts separate only from certain types of values. This allows for a further step: deciding what values can stay in (e.g. how to define and control bias), and where in the scientific process they can exert their influence (e.g. "the construction of the conceptual scheme," as we have just read) without detracting from the objectivity of inquiry. At the end of the 1950s the Swedish economist G. Myrdal claimed that if there is a "value problem in social science," this should be made to work for us, and not against us. By this he meant that values should as far as possible be acknowledged in social research, and be given a strategically favorable position—that is, they ought to be turned into means of inquiry rather than being pictured as a detrimental by-product of it (Myrdal 1958).

This also entails that values, despite empiricist veto, can be exposed to rational discussion and to a better understanding of the issues involved. Consider for example some recent discussion in climate change literature concerning how 'to discount' future generations, and how much or how quickly to invest in reducing carbon emission. Here it was shown how different value-laden premises lead to different figures of discount rate. A utilitarian (such as the economist Nicholas Stern) calculates a low rate, whereas someone supposedly driven by a prioritarian moral view (such as William Nordhaus or Richard Toll) projects a much higher

rate (Broome 2008; Cartwright and Montuschi 2014, 131–33). Does this mean that there is no objective figure to attach to the discount rate in this discussion? Quite the contrary, acknowledging the 'guiding points of view' in research, to use Weber's terminology, rather than detracting from objectivity, could fruitfully open scientific inquiry to the advantages of a more inclusive and meaningful discussion.

One way of describing what form this type of inquiry might take is to adopt what Helen Longino calls 'transformative criticism', a methodological strategy that makes the process of scientific inquiry responsive to a plurality of views and criticisms (Longino 1990, 73–74). Objectivity, in a perspective like Longino's, is secured by subscribing to a view of scientific inquiry framed and controlled by public criteria of social scrutiny for the evaluation of evidence, methods, assumptions, and reasoning. It is from this context that objectivity, as the practical goal of a critically aware (as well as accountable) scientific community, can only thrive.

2 From Theory to Practice

2.1 The Objectivity of 'Evidence-Based' Policy

In the age of science, evidence is considered to be the benchmark of objective knowledge. If to know objectively is to prove that a hypothetical claim is true (or false), or to be able to form an undisputed belief concerning the hypothesis, then evidence is the means to achieve this. To know that *p* means that I have the required evidence (to support, to prove, to justify) that *p*. Of course, in order to achieve this, evidence itself is to be objective (that is based on objective knowledge of the facts used as evidence).

In practical contexts, such as the formulation or the implementation of a policy, this role of evidence extends to the realm of decision-making. If we can count on good evidence to prove that, say, a certain intervention will be effective (or stands a good chance to be so), then basing our decision to implement such an intervention on the evidence we have will make our decision 'objective'. Evidence is the means to objective decisions.

The increasingly influential evidence-based policy and practice (EBPP) movement subscribes to this view, and advocates that basing policy on the results of evidence coming from scientific knowledge, including social scientific knowledge, is the key to making policy decisions more objective. With this in mind, governments are more and more willing to fund evidence-based approaches to policy-making; and government funding is increasingly tied to the demand for evidence (Cartwright 2008). There is political interest in assessing, as well as testing "the extent to which policies are 'evidence-based'" (Science and Technology Committee 2006). In discussing its Millennium Development Goals, the United Nations also recommended EBPP because: "evidence-based policymaking refers to a policy process that helps planners make better informed decisions by putting the best available evidence at the centre of the policy process" (UNICEF 2008).

Who could be opposed to this agenda? Consulting well-established empirical facts based on reliable methods, measurements and experiments when we make policy surely makes policy decisions more credible, more reliable, more solid, more corroborated. It even becomes tempting to think that the objectivity of our decisions is a direct consequence of the objectivity of the knowledge used to establish the evidence: the more certain our methods of acquiring evidence, and the better the quality of evidence, the better the decisions. So, a considerable amount of work has been put into how to regulate the use of evidence in the domains of practice. New

institutional rules, structures, and guidelines have been suggested with this aim in mind.⁴ For example, there is a widespread recommendation in policy practice to evaluate evidence according to 'evidence-ranking schemes'.⁴ The idea behind these schemes is that a fixed rank can be given to kinds of evidence, and then depending on how the kinds are ranked, they are positioned in a hierarchy within the scheme. The scheme then "adjudicates" evidence from the very best to the second best, all the way down to the worst (less/least reliable).

Interestingly, if we glance through these hierarchical schemes, it is often the case that best evidence is associated with one particular type of methodology: well-conducted RCTs (and/or randomized control trials) and/or systematic reviews of good RCTs.⁵ There are several reasons why RCTs are pointed at as the 'gold standards' for providing evidence. They have inbuilt assumptions which ensure the results from an ideal trial (their results can be directly deduced from the evidence provided) and control for confounders (i.e. those variables that could interfere with genuine causes). Also, they calculate evidence in terms of probability, so the higher the probability, the better the evidence (and given that all concepts are operationalized, bias is purportedly kept under check). They seem, in other words, to entail some of the features that are deemed essential to ensure an objective outcome: certainty, rigor, quantitative formulation, measurability of results, strict experimental protocols, and absence of subjective distortion. For these appealing reasons they are widely used (or at least invoked) in policy-making projects and pilot programs across a large variety of fields of social intervention (education, crime, poverty, child welfare, etc.).

However, those very features, that might well depict objectivity in the controlled field of a trial, or indeed in the ideal realm of (social) scientific inquiry, do not necessarily or automatically translate into a matching effect of objectivity in practice. When scientific and social scientific methodological apparatus such as RCTs are used in pursuits such as justifying a policy decision, or handling a dispute over a social cause, there is a danger that the meaning of objectivity that we ideally attach to scientific inquiry (and as intended by using those apparatus) takes over. The result might be overly selective: complex and varied hosts of facts and factors that come from the social domain get ignored, as they do not naturally fit within the strict parameters of, say, an experimental design (e.g. in a trial the efficacy of an intervention is tested under very special conditions). Context, local conditions, individual and social biographies, anecdotal evidence, 'field' knowledge—despite often proving to have an important, if not crucial, role to play—might not receive adequate recognition as they might not be immediately recognized as being relevant, or 'treatable', by rigorous scientific methodology.

This has at least two consequences. In practice, a neglect to address them appropriately might result in poor implementation and meager outcomes.⁶ In theory, a concept of objectivity obtained by sticking to such a restrictively conceived 'evidence base' fails to capture what is at stake in real-world circumstances.

2.2 Is Expert Judgment Objective?

As RCTs come on top of evidence-ranking schemes, almost unfailingly expert opinion appears at the very bottom of these schemes. Indeed, part of the appeal of RCTs is that they are 'mechanical' procedures, and as such they can avoid subjective judgment. Expert opinions, despite qualifications to the contrary, are still 'opinions', and as such they are viewed as the poorest providers of solid evidence. This might appear puzzling. We all do endorse 'expert culture'. For most decisions, actions, resolutions, estimates, choices, in both private and public life, we can and often

do rely on, or at least refer to, some expert, or experts, in one or more relevant fields. Take for example the case of child welfare. In our society, governments delegate different figures of experts to assess children's needs, to evaluate the adequacy of families in meeting these needs and to measure children's development in the context of the institutions they have created. This is how a 'curriculum of childhood' becomes the standard by which children's 'normal' growth is systematized and evaluated (Saraceno 1984). Yet, we also live in an 'expert-worried culture'—or even worse, in an 'expert-dependent culture'. We are surrounded by too many experts (and indeed by too many bad experts) and this is enough to make us suspicious across the board, and often unsympathetic towards the category in general. To use the same example, in child welfare expert opinion is often associated with 'clinical judgment', which is viewed as experience-laden, intuitive, synthetic, individual, and unsystematic.

All in all, what makes expert opinion appealing to us, and makes us resort to experts in the first place—for example the exercise and display of competent judgment on issues and problems that matter, and that we are not able to solve on our own—seems nonetheless to be what makes us most uneasy about those very opinions: a lack of objectivity, and an excess of subjective discretion in how they provide for closure in the search for reliable evidence and reliable answers based on that evidence.

Should experts be trusted, and under what circumstances? To answer this question we often find ourselves resorting to issues more complicated and open-ended than the question itself. Trust might depend on the *authority* of the expert, but as any argument from authority would show, authority is not *ipso facto* an indicator of reliable expertise. Arguments from authority are first of all inductive arguments: to demonstrate that someone has the claimed authority is an empirical question, and often a contested and controversial one. Further, these types of argument can often fall prey to a logical fallacy: the truth of a conclusion cannot be logically (necessarily) inferred from whoever asserts it. This does not entail that the conclusion in question is false, only that the person who asserts it is not a sufficient condition for its truth. What other conditions ought to be granted in view of trusting the authority of an expert? The authority of the field of knowledge an expert appeals to (for example, a well-established scientific paradigm for an expert scientist) certainly is a primary matching condition. But assessing the authority of science, and on what parameters (epistemological, social, pragmatic), is itself a notoriously tangled call. Besides, even after accepting the authority of a well-established paradigm, we are still left with the question of whether the purported expert has sufficient knowledge of the field he/she derives authority from, whether knowledge in that field is all is needed to address the specific problem he/she is asked to deal with, or whether the claims he/she makes on the basis of his/her knowledge are free from conflict of interest, biases, etc.

This last issue in particular brings us to consider a second complex issue: trust has a bearing on the *responsibility* of the expert. Trusting an expert entails trusting that he/she does his/her job according to the rules of the profession (more/other than the rules of scientific method) as well as to the rules of the society where he/she operates—and the two sets of rules do not necessarily comply with each other. 'To be responsible' changes connotation depending on what sets of rules are valued more: on one side it might be felt that the greatest responsibility is towards knowledge, no matter what the consequences (social, moral) of pursuing it might be; on the other, it might be argued that the first responsibility is to society, and that the value of scientific truth does not exclude any other value. What expert would we trust more, then? Answering this question presupposes accepting from the start a trade-off between scientific truth and social good, which is sometimes a difficult call to make when it comes to trust—and even more so when it

comes to trusting experts to give an objective opinion (which is different from trusting the purportedly objective results of an experimental trial). Should experts be consulted at all, and for what reasons? What do they provide that other sources of knowledge and/or evidence do not?

If we look back at evidence-ranking schemes, expert opinions are listed among the means for producing evidence—on a par with methods such as RCTs, cohort studies, etc.—but, given that the standards for quality are settled by those methods, expert opinion by default scores badly for quality: it seems to lack the rigor, systematic design, and deductive structure of those methods. However, as pointed out in the previous section, in practical contexts all these features are rarely sufficient to secure objective results on their own. Once a decision has been formulated, say, to suspend sales of dairy products which contain an excess percentage of dioxin per ml that could be harmful to humans (Douglas 2004), we are still left with the problem of checking what entered into that decision in terms not only of facts and scientific knowledge, but also of how the information that made it to the list of possible evidence was weighed, selected, amalgamated? Besides, what social effects are considered more relevant, more acceptable or unacceptable in formulating the decision in question (public health problems or economic repercussions) depend on value judgments, sensitive to social and/or ethical considerations. In 'putting it all together' we cannot count on unflinching, mechanical 'gold standards'. An authoritative and responsible expert (though with awareness of the problems of definition sketched out above) might be in a position to undertake this task. In this sense good experts are 'aggregators' of knowledge: this is what qualifies their 'expertise' besides and beyond competence in a field of knowledge (Turner 2013, 193).⁵ And if an expert opinion is to prove its objectivity in practice it cannot but be sustained and enacted by reliable subjectivity.

The discussion in the last two sections has the purpose to illustrate that when we move from theory to social practice a concept of objectivity must prove to be up to the challenges posed by complex and often difficult to represent (let alone predict) social domains. The protocols of social scientific inquiry (though with all the qualifications highlighted in the first half of this chapter) fall short of what is needed to make objectivity not only a desideratum of scientific research, but also a practical, and as far as possible achievable, goal for the good of society.

Notes

- 1 The social sciences started appearing in the 19th century, while the natural sciences emerged in the 17th century.
- 2 Supporters and detractors of this argument ignited what became famous in some philosophical and sociological literature of the early 1990s as 'the science wars' (see Koertge 1998; Kukla 2000; Ross 1996).
- 3 Among these institutions are the Cochrane Collaboration (evidence-based medicine) and, modeled on the former, the Campbell Collaboration, which expands the idea of being 'evidence-based' to the fields of education, crime and justice, and social welfare. For details of these two organizations see their web pages at www.cochrane.org/ and www.campbellcollaboration.org/.
- 4 Examples of these ranking schemes can be found in SIGN (2004), or the Oxford Centre for Evidence-Based Medicine Levels of Evidence (2007).
- 5 A randomized controlled trial is an experiment in which investigators randomly assign eligible subjects (or other units of study, e.g. classrooms, clinics, playgrounds) to two groups. One of the groups receives one or more interventions (e.g. a particular treatment). The other does not receive it. If the observed outcome in the group that received the intervention is observed as being statistically significant, then it is concluded that it has indeed been caused by the experimenters' manipulation, i.e. there is a high probability that the intervention actually works. A systematic review is a literature review of all the evidence found in studies dealing with a particular research question that has been screened and has passed the

test for quality (by means of criteria and procedures explicitly defined in advance). Systematic reviews of RCTs include the results of all well-conducted RCTs (according to the quality criteria established for RCTs).

- 6 For examples of the limits of programme implementation see Cartwright and Harde 2012; Seckinelgin 2008; Dehne 2002.
- 7 Douglas's argument is intended to deal with cases when science provides for uncertain evidence (e.g. there is no conclusive evidence about the carcinogenic effects of dioxin on humans). A similar argument can purportedly put forward any time science impacts on contexts of use, no matter how certain scientific evidence is.
- 8 There is a vast literature (literatures) on experts and expertise. For a sample of the issues involved see e.g. Crease and Selinger 2006; Collins and Evans 2007; Cooke and Probst 2006.

References

- Broome, J. (2008), "The Ethics of Climate Change," *Scientific American* 298 (June): 69–73.
- Cartwright, N. (2008), "Evidence-Based Policy: Where is Our Theory of Evidence?" in A. Beckermann, H. Terens, and S. Walter (eds.), *Philosophy: Foundations and Applications*, Paderborn: Mentis-Verlag.
- Cartwright, N. and Harde, J. (2012), *Evidence-Based Policy: A Practical Guide to Doing it Better*, Oxford: Oxford University Press.
- Cartwright, N. and Montuschi, E. eds. (2014), *Philosophy of Social Science: A New Introduction*, Oxford: Oxford University Press.
- Collins, H. M. and Evans, R. (2007), *Reinventing Expertise*, Chicago: University of Chicago Press.
- Cooke, R. M. and Probst, K. N. (2006), *Highlights of the Expert Judgment Policy Symposium and Technical Workshop*, Washington, DC: Resources for the Future.
- Crease, R. and Selinger, E. (2006), *The Philosophy of Expertise*, New York: Columbia University Press.
- Daston, L. and Galison, P. (1992), "The Image of Objectivity," *Representations*, 40 (Special Issue: Seeing Science): 81–128.
- Daston, L. and Galison P. (2007), *Objectivity*, New York: Zone Books.
- Dehne, T. (2002), "A Dutch Treat: Randomised Controlled Experimentation and the Case of Heroin Maintenance in the Netherlands," *History of the Human Sciences*, 15(2): 75–98.
- Douglas, H. (2004), "Prediction, Explanation, and Dioxin Biochemistry: Science in Public Policy," *Foundations of Chemistry*, 6(1): 49–63.
- Elsner, J. (1989), *The Cement of Society: A Study of Social Order*, Cambridge and New York: Cambridge University Press.
- Kincaid, J., Dupré, J., and Wylie, A. (2007), *Value-Free Science? Ideals and Illusions*, New York: Oxford University Press.
- Hacking, I. (1999), *The Social Construction of What?* Cambridge, MA: Harvard University Press.
- Hempel, C. (1965), *Aspects of Scientific Explanation*, New York: The Free Press.
- Koertge, N. (1998), *A House Built on Sand*, Oxford: Oxford University Press.
- Kukla, A. (2000), *Social Constructivism and the Philosophy of Science*, London and New York: Routledge.
- Longino, H. (1990), *Science as Social Knowledge: Values and Objectivity in Scientific Inquiry*, Princeton, NJ: Princeton University Press.
- Myrdal, G. (1958), *Value in Social Theory: A Selection of Essays on Methodology*, ed. Paul Streeten, London: Harper.
- Nagel, E. (1961), *The Structure of Science*, New York: Harcourt, Brace, and World.
- Nagel, T. (1986), *The View from Nowhere*, Oxford: Oxford University Press.
- Oxford Centre for Evidence-Based Medicine (2007), www.cebm.jr2.ox.ac.uk/docs/Level.html.
- Rorty, M. (2000), "How We Divide the World," *Philosophy of Science*, 67: S628–S639.
- Ross, A. ed. (1996), *Science Wars*, Durham, NC and London: Duke University Press.
- Saraceno, C. (1984), "The Social Construction of Childhood: Childcare and Education Policies in Italy and the United States," *Social Problems*, 31: 351–63.
- Searle, J. (1995), *The Construction of Social Reality*, London: Allen Lane.

- Seckinelgin, H. (2008). *The International Politics of HIV/AIDS: Global Disease, Local Pain*, London: Routledge.
- Science and Technology Committee (2006), "Scientific Advice, Risk and Evidence: How Government Handles Them," Evidence Report, London: House of Commons. www.parliament.uk/parliamentary_committees/science_and_technology_committee/sag.cfm.
- SIGN (Scottish Intercollegiate Guideline Network) (2004), www.sign.ac.uk/guidelines/fulltext/50/comp-evidence.html.
- Turner, S. (2013), "What Can We Say about the Future of Social Science?" *Anthropological Theory* 13(3): 187-200.
- UNICEF (2008), "Bridging the Gap: The Role of Monitoring and Evaluation in Evidence-Based Policy Making," www.unicef.org/cecsi/evidence_based_policy_making.pdf.
- Weber, M. (1904), "'Objectivity' in Social Science and Social Policy," in *The Methodology of the Social Sciences*, ed. E. Shils and H. Finch, New York: Free Press, 1949.
- Winch, P. (1958), *The Idea of Social Science and its Relation to Philosophy*, London: Routledge & Kegan Paul.

Part III

DEBATES