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OBJECTIVITY IN SOCIAL RESEARCH

Live empirical issues in debates over objectivity in the social sciences

Harold Kincaid[1]

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Abstract
Questions of objectivity involve many general philosophy of science issues; when directed toward the social sciences, even more complex issues surface about the status of the social sciences, e.g. can they be sciences as are the natural sciences? This paper does not take on this mass of issues directly, but instead argues for more restricted theses, in particular that questions about objectivity in the social sciences are often usefully seen as local empirical issues. I look at arguments around underdetermination, value ladenness, the indeterminancy or nonquantitative nature of social science categories or attributes, and traditional ontological debates over materialism and idealism. I show that in all these cases some of the key issues about objectivity are specific empirical issues in the social sciences.

Keywords Philosophy of social science · Objectivity · Value freedom · Underdetermination · Utility theory

Questions of objectivity involve many general philosophy of science issues;[1] when directed toward the social sciences, even more complex issues surface about the status of the social sciences, e.g. can they be sciences as are the natural sciences? This paper does not take on this mass of issues directly, but instead argues for more restricted theses. I look at arguments giving a negative answer to the epistemological question whether social science research can be objective and to the ontological question whether social phenomena are objective. I try to show that common general arguments against objectivity in the social sciences are not persuasive. The idea of objectivity is instead often a variety of empirical issues in ways not commonly realized. My task is thus to argue for these two claims for the social sciences.

[1] Douglas (2004) survey a variety of sometimes overlapping but seemingly irreducible common sense notions of objectivity.

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✉ Harold Kincaid
harold.kincaid@uct.ac.za

1 School of Economics, University of Cape Town, Cape Town, South Africa

The paper proceeds in four steps. Section 1 provides a general approach for thinking about arguments concerning objectivity, one I label "contextualist." In the process, it presents some general ideas about what makes for objective social research. Doubts about objectivity in social research based on the holism of testing and underdetermination of theories by evidence is topic of Sect. 2. Section 3 looks at arguments from the alleged value-laden nature of social science. Though the arguments covered in Sects. 2 and 3 do not cover all the various doubts about epistemic objectivity in social research, most challenges to objectivity make some use of these claims. Arguments that social research is not objective from the interpretivist and social constructionist tradition rely heavily on underdeterminism and holism and a denial that science is value neutral. Antirealist arguments in support of an instrumentalist understanding are often motivated by underdetermination claims. The arguments considered are fundamental issues for assessing claims about objectivity in the social sciences.

While most of the paper focuses on epistemological questions about objectivity in the social sciences, Sect. 4 finishes with ontological concerns about objectivity in the social world. There are many different issues that fall into this category. I only address two of those, though they are central: can social reality be quantitative in the way needed for standard scientific investigation and can social reality be mind-independent and thus objective in that ontological sense? My response is again that answering these questions turns on empirical issues in social research.

The overall moral is that objectivity in the social sciences is often much like other general scientific virtues such as simplicity (Sober 1989): it surely is important, but what it comes to, and when and why it matters, is often dependent on empirical specifics. My goal is to establish this general point; I do refer to specific illustrative work in the social sciences along the way, but, of course, on my view debates about objectivity elsewhere in the social sciences will require looking in part at those local details.

1 Section 1: Assumed stances and some useful distinctions

In this section I present a rough philosophy of science stance that fits with and is supported by the points I make in later sections. I also make a number of essential distinctions to help clarify claims about objectivity.

My general stance here is naturalist and also what might be called contextualist (Kincaid 2004). Naturalism as I interpret it is the Quinean view that science and philosophy of science are continuous and that ultimately all issues are empirical, though certainly at different levels of abstraction, and concomitantly, that a priori conceptual analysis may help clarify issues but will not decide them. Objectivity questions are empirical questions.

What does it mean for objectivity to be an empirical issue? There is a long history of naturalism being claimed by many as descriptions of their positions, only for the naturalism to turn out to have little bite (Kincaid 2013). If all naturalism requires is that empirical facts have some relevance, then the assertion is of minimal force. The same situation confronts the idea that objectivity is an empirical issue.

I will not pretend to, nor think it possible to, give informative necessary and sufficient conditions for “concept A is empirical iff…”. However, we can still think of observation statements suitably relativized to a specified language, theory, etc. (cf. Sober 2014) as key and contrast them to logical, conceptual, and a priori elements. These two elements—observational and conceptual—then can be thought of as grounding a continuum, with claims entirely conceptual on one end and entirely observational on the other. To claim that objectivity is empirical is to claim that judging some piece of science to be objective is asserting that it rests on statements that require observation statements, with those closer to the observational end being more empirical.[2]

So, to give an example, consider the empirical status of causal claims. If I assert the causal chain:

(1) $A \rightarrow B \rightarrow C$

Then by the logic of the concept of cause, you might infer that A is independent of C, given (holding fixed) B. Whether we have true statements about the association of A and B is a statement on the observational end of the continuum. Furthermore, the kind of causal relation in (1) whose logics we used is that of a sufficient cause—A and B are each independent causes. There are other causal notions such as necessary cause that do not entail this association. It is an empirical matter—a matter dependent on observations—which causal notion is relevant in any given case. The causal claim (1) is empirical in these senses.

Correspondingly, in Sect. 2 I argue that whether causal claims based on social science data are objective and unbiased depends on specific empirical facts about how social causation works in the case at hand. Note that my specification here does not deny that there may be useful analytic or conceptual content to explore.

In addition to a broad empiricism, naturalism also fits well with a certain kind of “contextualism”[3] which denies that we are ever in the position of evaluating all our knowledge at once and that instead claims about what beliefs are well supported rest on background knowledge that is taken as given and fixed according to context. Since on naturalist grounds we can expect that perfectly general a priori inference rules are hard to come by, judgements about objectivity are likely not be entirely a matter of formal rules, but more piecemeal and local. Scientists and probably the rest of us sometimes thus exaggerate the extent to which our most objective judgements are formal rather than empirical matters. Contextualist ideas suggest that an understanding of scientific progress and scientific virtues thus ideally provides a careful look at historical, sociological and discipline or topic-specific aspects of scientific research. Here it is the latter that is my focus.

Aside from naturalist and contextualist assumptions, there are a series of distinctions which can be helpful for thinking about objectivity in social research. A first is that claims about objectivity can be either epistemological or ontological. Epistemological takes see truth promotion, confirmation, and the like in appeals to objectivity. However,

[2] This picture asserts that there are ways to understand "is empirical" that do not require operationalist definitions, sense data, and other such positivist notions.

[3] By contextualism I do not mean the view in analytic epistemology about the definition of knowledge but rather an antifoundational epistemological view. See Williams (1999) and Kincaid (2004).

there is a tradition, especially important in the social sciences, of taking objectivity debates to be about the nature of social reality and only secondarily about how we come to know it. We will look at both kinds of considerations below and clearly there can be interconnections, but it is important to note that they are prima facie different claims.

Another useful distinction is between kinds of justifications for the epistemic variants of objectivity. Scientific virtues and methods might be justified and clarified from either a teleological or deontological perspective (Berker 2013; Alston 1988; Steup 1988), to use the distinction well known from metaethics. While there are doubts about the distinction, nonetheless it is a helpful start for thinking about epistemic objectivity. Is objectivity a good thing because it maximizes some desirable scientific product or is it an end itself?[4]

In ethics it has been hard to explain why alleged deontological rules are justified, how they handle competing obligations, and why they cannot be overridden by teleological ends. Similar doubts about deontology in epistemology have been raised (Kim 1994; Nottelmann 2007). Not surprisingly, the perhaps majority view among philosophers of science is that good methods are those that promote some sort of truth related end (Kitcher 1995). In analytic epistemology this would be a form of reliabilism. "Truth-related ends" can come to multiple things. Promoting truth simpliciter (most truths, the highest truth to falsity ratio, the fewest falsehoods, etc.) is one reading. Another less ambitious version looks for the promotion of well-confirmed results, justified beliefs, etc. Of course, there are wheels within wheels that can be spun here. For our purposes, however, the general idea that scientific virtues—of which objectivity is one—promote truth related ends will suffice.

Further important distinctions in the literature relevant to objectivity concern what bears the epistemic end goal and how to think of things such as scientific methods, virtues, etc. Beliefs, statements, propositions, models, practices, and more are candidates for the valued ends; methods and virtues may run from clearly formalizable rules to much more general notions such as scientific practices, where the latter may make explicit room for social practices. For both elements of the distinction—what kind of things are ends and methods—there is a further issue about roughly individuals and groups. Is it beliefs, models, etc. held by individuals or scientific communities? And who are the users of methods, rules, etc., again where it might be individual scientists or some complex social entity.

So, given the above my approach is going to ask about objectivity in the social sciences in roughly the reliabilist way where contributions to truth or other relatively noncontroversial epistemological ends are key. Before turning to specific arguments about the social sciences, there are two further ideas that will play a role in the rest of the paper.

I noted above that even if we have some truth linked notion of objectivity in mind, there are various versions that can instantiate that goal. In the arguments that follow one particular realization will get special emphasis because it has strong ties to common aspects of objectivity. Objectivity is often contrasted to bias. Yet, it may be possible

[4] These are justified in analytic epistemology by ultimately appeals to intuition and ordinary language, which is of course is not my route. Nonetheless the distinction is useful.

to say something somewhat deeper in this regard. Both logic and the experience of many a empirical researcher suggests we draw a distinction between objectivity as lack of bias and objectivity in other strong forms. In this regard the perhaps seeming hair-splitting about different kinds of truth promotion has concrete value. It is one thing to be able to, given the best of my background knowledge, eliminate sources of bias, and quite another thing to show that the remaining hypotheses I have are true, probably truer, closer to the truth, or whatever. Empirical researchers make judgments regularly that particular results are produced by biased methods, ones that are unlikely to produce correct or believable results; it is often another step of some magnitude to make the further step that results free of those biases are probably right.

It is worth noting here that judgement about bias like that described above are all-things considered judgements. This follows from holism and the contextualist stance. Social scientists, like all of us, would like to have fixed rules that would ensure bias is eliminated and tell us which hypothesis is most strongly supported. These urges are especially clear in the use of statistical inference in the social sciences. "Reject the null if $p < .05$" is a prime example. In a way some appeals to formal rules can be seen as using deontological criteria for objectivity. However, formal epistemic rules fall victim to the same kind of objections raised against deontology in ethics: we may have other knowledge that overrides our rules, e.g. large utilitarian consequences. The holism of testing or of evidence gives us reason to think that we may find ourselves in a parallel situation in social science research. Although social scientists like to treat statistical rules of evidence as deontological—as telling us what to believe based on the data and formal a priori statistical rules, their faith is misplaced. Statistical rules require empirical evidence to evaluate what they tell us. Despite widespread misunderstanding, p values, for example, alone do not tell us what to believe. A p value tells us p(E/H = resulted from chance). What we want to know is p(H/E), which requires at least that we know p(E/H) and p(H). But these probabilities are a matter of empirical background knowledge which can lead us to reject H even when a p value is small.

The right statistical inference rules ideally and allegedly eliminate bias, but the standard practice of significance testing without other information does not do so. Thus, distinguishing objectivity in terms of lack bias from stronger forms of objectivity with a positive truth link is an important instantiation of my claims that epistemic objectivity is contextual and empirical.

Finally, to finish up preliminaries, there is one further quite substantial element required for looking at objectivity in the social sciences that is easy to overlook in postpositivist times. For example, to even state the problems related to underdetermination, discussed below, we need to have claims clear enough to determine relations between them and between them and potential evidence. Much good came from post-positivist philosophy of science after the 1960s that thought positivist formalization requirements and projects were too simple and not true to the science. However, being too simplistic is not the same as being without value. We can all agree that maybe there is not one formulization and one right interpretation of the formalizations scientists give. Yet the weaker thesis that good science should make identifiable and differential claims with discernable differences for the evidence is not a mistaken positivist holdover but a sine qua non for rational inquiry and thus objectivity.

Can the social sciences achieve such goals? Again, that is an open empirical question about the practice of social research. Unfortunately, social science may often fall short in this regard. Critics inside and outside social research have pointed out these shortcomings since the inception of the social sciences. While there has been progress, the problems are still widespread. Let me give two specific illustrations.

First case: prospect theory in behavioral economics. Cumulative prospect theory (CPT)[5] is said by many to be a well-confirmed account of important areas of human decision making and to have displaced the previous dominant expected utility framework. Expected utility theory (EUT) says that individuals maximize the sum of some end, for example wealth, discounted by its probablility of occurring. CPT rejects this account. The most common way of putting CPT in much of literature is that subjects are loss averse—losses count more than gains so they do not maximize simply total wealth—and/or that they overweight small probabilities—they treat expected sums of wealth differently depending on whether they involve small or large probabilities.

However, CPT is considerably more complicated. Much of the literature on CPT does not distinguish its various components and takes evidence that bares only on some components to be evidence for the theory as a whole (see Hofmeyr and Kincaid 2019). So, CPT makes assertions at least about:

- The argument of the utility function: is it wealth or income, or gains and losses relative to a reference point?
- Probability weighting: are some probabilities overweighted or underweighted?
- Stochastic dominance: will decision makers always choose an inherently superior lottery over an inherently inferior lottery?
- Utility loss aversion: are losses treated differently to gains?
- Probabilistic loss aversion: are probabilities assigned to losses viewed differently to probabilities assigned to gains?

These are essential but independent components of CPT (with interconnections of course). Other nonEUT accounts of decision under risk vary in what they say about these questions, but they are lumped together with CPT because they reject some aspects of EUT. As a result, the problem is that appeals to prospect theory are ill defined, thus making the claims asserted about the evidence for “the” theory difficult to objectively assess.[6]

Causal inference provides another illuminating example, this time more general, of obstacles to objectivity from lack of formalization. The use of graphical models and the do operator are a clear case of progress in thinking about causality (Pearl 2009). Prior to these developments both stating and testing causal claims was very difficult. Objective causal claims—ones with some prospect of being truth linked—were correspondingly difficult. Yet this progress likewise shows in a precise way that some or much social theory—how much is a matter for empirical dispute—is still fundamentally unclear.

[5] CPT is a later version of prospect theory building in rank dependent utility to avoid the violation of stochastic dominance implied by prospect theory. These are often run together in the general literature which illustrates the point I am making.

[6] Throughout the discussion I use the word "theory" and "hypotheses" loosely. Nothing I say claims that theories in the traditional sense are essential to science and the contextualism invoked here suggests rather that science is considerably more complex, etc.

This ambiguity shows up in diverse ways. Social scientists often waffle about whether they are making causal claims. They will talk of "determinants," often in the same paper hedging their results with "correlation is not causation" caveats while at the same time making policy recommendations for government action, which makes no sense without causal assumptions. Furthermore, when social scientists make causal claims, it is often not clear which ones they are making. Causes can be necessary, sufficient, and complex mixtures of both. These are different causal pictures requiring different evidence, yet social scientists are often unclear which kind of causality they are invoking. Moreover, even when it is clear what kind of causal claim they are making, they are often unclear what exact causal relations are being asserted. In general, when clear causal models are lacking, clear evidence must be lacking as well.

2 Section 2: Underdetermination arguments

In this section I look at arguments designed to show that the social sciences cannot be objective because of underdetermination and holism of testing issues. My approach in this and the next section is the general reliabilist one that asks if these and other arguments show that social research is unable or unlikely to give truth-linked evidence. Epistemic objectivity as I am using the notion requires some such link.

Standard arguments about holism and underdetermination are familiar: Any test of a theory involves auxiliary hypotheses and thus is a test of the theory and auxiliaries jointly—testing is holistic. Therefore, it always possible to construct multiple theories that fit the evidence and so, data underdetermine theory. A stronger form of underdetermination does not rely on the holism argument but asserts that there may be multiple, different theories which are not modifications of one or the other and which both are equally adequate to the data (Stanford 2017). In both cases, since data underdetermine theory, we have no reason to think the process we use to produce theories give us ones that are true, likely to be true, etc. Put in terms of our reliablist framework, holism entails that testing methods cannot be shown to be truth linked because they do not decide between competing explanations of the data. Thus, decisions about what theories or hypotheses to believe have to be decided by something else other than evidence and correspondingly are not objective results.

These arguments for underdetermination are not specific to the social sciences. However, they are frequently invoked as reason for doubts specifically about objectivity by social scientists, by philosophers, and by other commentators (e.g. Bardsley et al. 2010). Moreover, there may be reasons specific to the social sciences that make the holism and underdetermination issues especially troublesome.

So, let's look at responses to underdetermination as a general thesis about science. The argument for underdetermination of theory by data from the holism of testing rests on an equivocation. Using the rest of the whole to test a hypothesis does not entail that only wholes are tested. Underdetermination needs the consequent here. Yet is easy to see—as pointed out by Dorling (1979) and Glymour (1980) long ago and others since (Howson and Urbach 2005) that testing can involve multiple claims without it being the case that specific pieces of evidence bear on only the whole and not on any component of the whole. Of course, it may be that the evidence looks inconclusive if we picture the totality of theory being tested by the totality of the evidence. But that is to set up the question in an extremely unrealistic way that in general does not describe a situation we are in. It is to ignore all the complex interrelations among the components of theory and the diversity of evidence we have and thus to hide the differential support some hypotheses may have over others relative to a given subset of the data.

Holism of testing does not entail inability to support some Hs over other if we are in the right circumstances. Put in simplest terms, we need to ask the key questions:

- Does the hypothesis at issue H fit with the evidence?
- Are alternative explanations of that fit plausibly ruled out?

Whether we can answer these questions in specific cases cannot be decided on a priori conceptual grounds and thus it is not inevitable that the data underdetermines what to believe. The question is whether we find sufficient dependencies and independencies in the hypotheses that are issue and sufficient diversity of data types such that we can by substitution and triangulation distribute blame or credit. We know from the start that the evidence does not tell for or against H even though H fits the data if the other assumptions we use to tie H to E—the theory of the test—are implausible or have no other support than E itself. Thus, the holism of testing tells us first that there is almost always one alternative to the maintained H that needs to be ruled out, namely, that the assumptions needed to test H against E are implausible based on what else we know or that they cannot have no other evidence than E itself. The other thing holism suggests is that H is well supported only if there is no other H that combined with the assumptions of the test is consistent or as consistent with the data.

An objective and bias-free test answers these questions in some form. Lack of bias means that we know from the theory of the test—the assumptions and inference procedures used to evaluate the hypothesis given the data—that our process does not have known sources of error. But stronger objectivity results when we know not only that our process do not make avoidable errors but also that they support H over other competing explanations that are not simply the result of error. We want a process that suggests no warrant for mechanisms known to be impossible and warrant for the true one among the possible ones.

A second but related response to underdetermination notes that competing theories can nonetheless share some claims in common. If all the competing models with shared assumptions fit the data but the data cannot decide between the two models, then we have underdetermination of theories as a whole but not underdetermination of all their elements. So global undeterdetermination does not entail local underdetermination. Which elements are supported by which data is again an empirical issue. We have to ask what is the structure of the model, what does it entail and not entail, and how does it then relate to the data at hand.

These debates suggest a moral that is simple and well known but sometimes easily overlooked in social science research, especially in the presentation of results: the route from data to believable hypotheses or theories is generally not an easy or direct one. Claims in science in general and the social sciences in particular that "the data show…" or that "theory X is well supported by the data" often need to be taken with a grain of salt. The holism of testing and the possible underdetermination it allows call for caution unless the needed requirements have been met.

Are there reasons to think that these requirements can never be met in the social sciences? I doubt it. If you think that only experiments can meet the requirements, then you still have to contend with experimental social science as well as with natural science that seems successful while being nonexperimental. If you think that the social sciences are different because they are about meaningful behavior, then you have to reject apparently well established parts of ethology and also you have to ignore parts of social science research that do not seem about meaning (see, e.g., Kincaid 1996). You also have to ignore research, for example, from demography that provides evidence about the influence of climate and pathogens on the growth of population numbers in the Middle Ages. Neither the dependent or independent variable here seems to be about meaning.

Even if the social sciences could meet the requirements for objective or nonbiased results in principle, it may well be that in practice they do not do so. I think that is often the case. Thus, the really interesting issues about objectivity of the social sciences raised by holism/underdetermination issues concern what are the contingent obstacles they raise. We know that they will involve either (or both) practices known to lead to error and practices incapable of distinguishing between different hypotheses that fit a set of data.

How then would we go about making judgments about objectivity in the social sciences where the question is whether underdetermination is a problem? We have to show differences of implications of parts of theory and sufficient relevant evidence to distribute credit and blame. The most compelling case for doing so are perspicuous restatements of the relevant evidence, the theory and the competing hypotheses in each case. Unfortunately, that is a demanding exercise.

To get beyond such detail, our best hope is for broad, general characteristics that separate out the empirical research that seems to avoid the underdetermination problem and that which does not. I look here at some issues for experimental and causal inference in the social sciences in general.

I start with experimental evidence in the social sciences, f of experimental work are to ensure that we are testing the hypothesis at issue and thus that we use the experimental situation to independently establish auxiliaries and control confounders. There are some general skeptical arguments turning basically on the holism of testing and the complexity of social phenomena that conclude that experimental social science cannot avoid the underdetermination problem. Hausman (1992, p. 307) says that because of the Duhem-Quine problem and uncertainties about evidence "it is almost impossible to learn from experience. This is the situation in economics." Bardsley et al. (2010) much more recently say that "economic experiments, by virtue of their complexity, never provide acid tests of individual hypotheses" (p. 95). They explicitly endorse the fallacy noted above of concluding only wholes are tested because testing is holistic and go one to advocate a Lakatosian approach where at best is what can be said is that "research programs" are "progressive." The conclusion is that even in our best experimental social science objectivity is illusive.

These views are generalizations about entire bodies of experimental work motivated by unwarranted inferences about what can never be done from the fact of the holism of testing. These generalizations also at times border on an unsophisticated skepticism that goes from our inability to eliminate all doubt to the conclusion that we cannot have supported beliefs. However, we have seen that there is no logical reason why complex tests cannot be "decisive": if I wiggle C, and I use another means to hold everything else constant, then wiggles in E are really good evidence, believable evidence, etc. that C causes E.

Experiments in economics try, as do experiments everywhere, to reproduce the above ideal circumstances. And, like experiments everywhere, they may be more or less successful. The game theory experiments Beardsley et al. cite as leaving us unable to draw firm conclusions have problems because there are so many moving elements at once when there is strategic interaction with multiple players, multiple equilibria, learning effects and so on. But not all economic experiments have to be and are saddled with such complexities.

Thus, take for example, research on expected utility and its competitors such as cumulative prospect theory mentioned earlier. After some years of work, the relevant theoretical components and the experiments to test them have become quite refined. The different components and their combinations producing different possible theories and their logical interrelations are well worked out (Hofmeyr and Kincaid 2019; Harrison and Swarthout 2019; and the many references in both). Experimental treatments well aware of the many moving parts and many possible confounds have been refined to the point where it is possible to isolate blame and credit and to reduce the prospects of error. Experiments to isolate atemporal risk attitudes, a key component of most competing decision theories, are particularly compelling. They can measure risk attitudes without having to test all components of competing decision theories and have steadily advanced. Confounds such as hypothetical bias, wealth effects, magnitude effects, sequence effects, and much more have been probed with the standard logic of varying the possible confound and looking for produced distortions holding everything else constant (see the comprehensive survey by Harrison and Rustrom (2008). There is now quite compelling experimental evidence that the key assumptions of CPT—e.g. loss aversion—do not hold in clearly specified decision circumstances (see Harrison and Ross 2017).

So, again, my moral is that assessments of objectivity often depend on local empirical evidence. Work on CPT and competing decision theories in general may well reflect disciplinary fads and biases; yet, some experimental conclusions about decision making under risk are relatively well established.

Recent advances in work on causality also describe some clear situations where we can avoid underdetermination and situations where we cannot and must settle for some form of subjectivism. So, assume we have the following correlations:

1. C and E are correlated.
2. C and M are correlated.
3. M and E are correlated.

Here are two possible causal models consistent with the data:

(a) $C \rightarrow E \rightarrow M$
(b) $M \rightarrow C \rightarrow E$

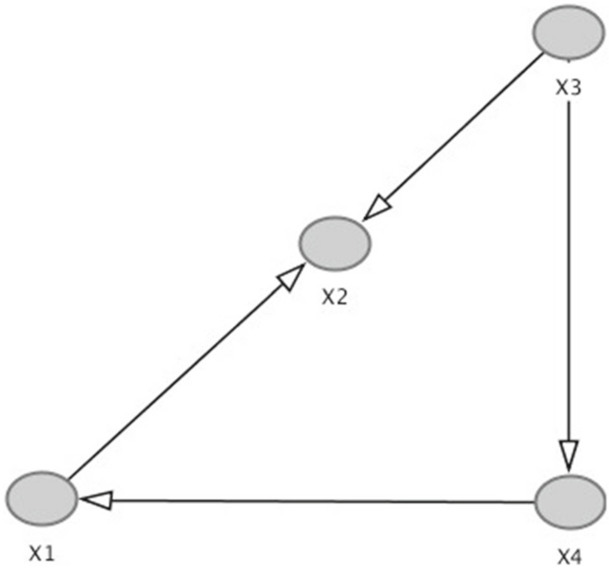

Fig. 1 A causal model where directed arrows represent causal direction

where the arrows represent one-way efficient causes whose direct influence is independent of every other cause in the graph. There are still other models not listed here that are compatible with the data summarized by 1–3. Thus, we have a classic case of theory underdetermined by the evidence.

However, this underdetermination is not inevitable. If we can control—fix the value of M and nonetheless C and E remain correlated, then we know that model (a) is wrong because fixing M would screen off C from E. If we fix or control the value of C and still M and E are still correlated, then we know that (b) is wrong for the same reason. By this general process of proposing models with clear entailments and finding evidence about the value of one factor independently of the others—epitomized in controlling or fixing it—then we can find the true model among those possible.

Moreover, even if we cannot find the true model, we may have good evidence that some parts of the model are well confirmed by existing data. So, consider the model in Fig. 1. It implies that:

(a) $x1 \perp x3 \mid x4$
(b) $x2 \perp x4 \mid x1, x3$

where "$\perp$" means two elements are probabilistically independent (not associated) and where "|" means given or conditional upon. Those implications from the model allow us to test it. However, as we see in Fig. 2, there are other models that have the same implications. The links between X1 and X4 and X4 and X3 that have no arrows mean that there are four other possible models that imply (a) and (b) while still having X1 and X3 cause X2. The model in Fig. 1 is underdetermined by all the conditional independencies in the data between its variables. Yet, nonetheless we

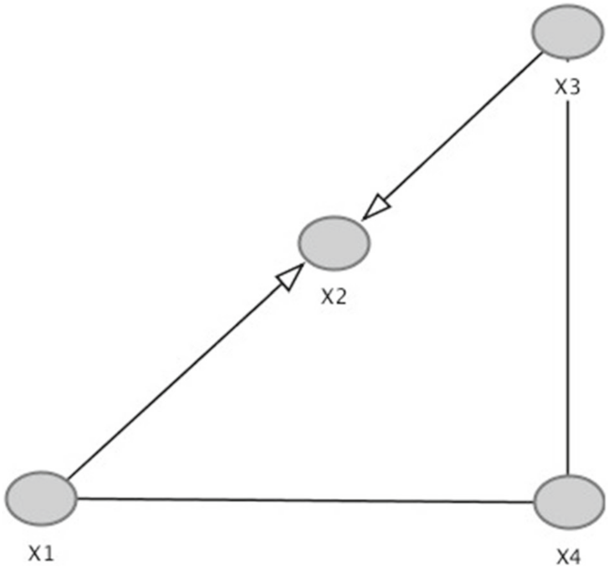

Fig. 2 Models equivalent to that of Fig. 1 in terms of the conditional independencies implied

know that regardless of that global underdetermination, any model that fits the data will support the partial causal relations between X1, X2 and X3.

Thus, we know that in the circumstances described above that in some cases, when we have the right evidence and know the possible causal models, there is an objective answer to what causes what. We also equally know that with the data of (1)–(3) but without the right additional evidence to fix or control variables and without evidence about the possible causal models, we have no reason to think our models are well established. We also know that the model of Fig. 1 cannot rule out the models of Fig. 2. Claiming otherwise is bias and misrepresentation.

These complexities once again illustrate my contextualist point about objectivity. Judgements about objectivity and bias require substantive empirical knowledge that is generally local to problem at hand. In these causal cases, we do have some a priori help as it were from the logical of causal inference, but only then with a specific notion of causation and with significant background causal knowledge.

The same logic used to evaluate the cases just discussed can be extended to indefinitely more complex cases, with structural equation modeling and the explicit use of causal graphs being a key tool. In actual practice, the social sciences often do not do what is needed to handle even the simple cases, but they instead use methods known to ensure biased results. So, the work horse of empirical social science for decades has been multiple regression, with the putative effect on the left-hand side and the possible causes on the right-hand side. It is well known in the causal modeling community that applying such methods untempered by using causal models can lead to predictable mistakes. If a causal mediator M between effect E and its cause C is included in a regression of the form:

$$E = xC + yM + e$$

where is e is an error term and x and y are coefficients ideally measuring effect size, the regression will find that x = 0, because M is screening off C, even though C is in fact a cause of E. This kind of bias is widespread in the social sciences (Kincaid 2012, forthcoming). Yet this failure of objectivity is not inevitable; when, where and how these issues are problems for objectivity are empirical issues.

3 Section 3: Value laden arguments

Assertions that the social sciences are inherently infused with values and thus not objective are rife.[7] A general form of argument for value ladenness is as follows: the evidence plus the rules of scientific method are not sufficient to determine the scientific conclusions drawn, as the holism of testing and underdetermination of theory by evidence shows. However, values and/or social and political beliefs can fill in the gaps and allow determinate conclusions to be drawn. Therefore, values and/or social and political factors are inevitably part of science. Longino (1990) is quite explicit about making this kind of argument, for example, but many other could be cited.

Arguments of this form has have led to widespread assertions that science is value laden. That has been an ill-advised rush to judgement on my view. A first step in seeing why is to do some useful logic chopping on the notion of value laden. A mass of diverse claims is being grouped together under the slogan, though often unexplicitly. The issues are more complex than often noticed. For any claim about value ladenness, we need to note at least the following parameters (Kincaid et al. 2007):

- Are values necessary or contingent?
- What kind of values are involved—epistemic or nonepistemic?
- Are values used in the science or are they implied from scientific results given other value assumptions?
- What part of science are values involved in: determining what problems and questions to pursue, in determining the evidence, in explanation, in fundamental categories, and so on?
- And, for each of the above, if true, what are the implications—here, especially, what are the implications for objectivity?

Given the above formulations, we need to ask whether the argument form cited above supports conclusions about value ladenness and if so, which ones. Arguments that the questions pursued or the value implications drawn are sometimes value laden do not raise strong doubts about objectivity; claims that values are involved in evidence and so on but not essentially are basically claims that bias is possible, a noncontroversial assertion that also does not raise significant doubts about objectivity. Serious doubts do arise if nonepistemic values are essentially involved in evidence, explanation and other core features of science. Yet, even when values are involved in the key

[7] One could be a strong moral realist and think that moral claims are as objective or factual as any other in principle and thus value ladeness is no reason for doubts about objectivity. I will not pursue that line here, though there are more guarded, contextualist versions that might be plausible (Kincaid et al. 2007).

parts of science, the implications are still open, because bias among individuals can actually promote good epistemic outcomes collectively.

There are multiple compelling responses to the argument for value ladenness from underdetermination. The first, of course, is that underdetermination is not inevitable—there are ways and occasions for finding convincing results despite—and indeed, because of—the holism of testing. This shows immediately that arguments for value ladenness have to be concrete, contextual, empirical arguments showing the effects of underdetermination, thus instantiating my main thesis that questions of objectivity are local empirical scientific questions, not large epistemological or metaphysical ones. Secondly, even if underdetermination holds in a specific case, value-ladenness does not follow. Faced with indecisive evidence, a rational response is to withhold judgment and seek further data. Also, nonmoral values can be appealed to as they often are in empirical science. For example, instantiations of epistemic values like simplicity are commonly used in model selection when multiple models are compatible with the data.

Nonetheless, investigating when nonepistemic values are involved in social scientific results is of nonetheless a very valuable enterprise. Yet, it is valuable only when it involves not some vague appeal to underdetermination but instead proceeds by showing exactly how and where and with what implications they are involved as outlined by the questions listed above. That takes real work, but the payoffs certainly can be valuable. Part of the value comes from realizing that local underdetermination is common in science and that scientists, like most people, tend to clean up a complex story for various reasons. In the face of competing hypotheses consistent with all the evidence and all the background knowledge at a specific moment in time in a particular area of investigation, a rational and defendable response is honesty about what is still up in the air. Keeping scientists honest in this way is valuable calling on my view.

Aside from underdetermination arguments, there are other reasons raised why values must be essentially involved in science. The argument from Rudner (1953) and repeated by Douglas (2000) focusing on inductive risk has gotten much attention. I do not think this argument shows any inherent value ladenness in science and thus not in the social sciences either. The argument is that nonepistemic value assumptions are required to decide when we have enough evidence, precise enough evidence, and so on. These considerations do go to the heart of scientific work since they are considerations closely tied to assessing data.

However, I see no argument here that there are not still two different components—(1) what data you have, what data you could collect, how confident you are in the data, and so on and (2) how do you value (in nonepistemic ways) the consequences of the uncertainties in the data (Douglas at times seems to recognize this). Clearly, making decisions about what data is needed based on such nonepistemic values and not reporting that decision is a kind of bias and value ladenness, but acting this way is not necessary. Nonetheless, pointing out such possibilities is valuable. To repeat my refrain, however, it is an empirical issue when and to what extent such value issues arise and raise doubts about objectivity.

A related argument that is more closely directed at the social sciences comes from Dupre. His argument is related because it suggests that the fact that social science investigations reflect human interests means value judgements are involved at the base of social scientific research. Dupre's argument is much more interesting than the old and answerable claim that values influence what problems we pursue (no doubt a reasonable claim but one that says nothing on its own about the objectivity and value freedom of the answers we give). Rather, Dupre (2007) argues that:

(1) The key classifications we make in the social sciences are classifications that reflect, promote, etc. our interests.
(2) Any classification that involves our interests is evaluative.
(3) Value free social science would be independent of our interests.
(4) Thus, any valuable social science is value laden.

The idea here is clear. Whether we use absolute or relative space conceptions to describe the world is not something that involves our human, nonepistemic interests. However, social science is largely about processes in the world that we have moral, social, political, and other value interests in. How we group and organize conceptually the social world inevitably reflects those interest. Thus, interpretation in social research in terms of values is unavoidable.

This argument is unconvincing as a general reason for value penetration of the social sciences. How I classify much in nature—not just the social world—involves my interests—weeds need to be removed, local indigenous flowers do not. I suspect Dupre would agree. However, then we are pushed back to looking at my original set of questions concerning the parameters of value laden claims. Where in science does this argument show that values are involved and with what effect? Does value involvement in this sense mean results are essentially dependent on contestable value judgements?

It seems not. A concrete example may help (Kincaid et al. 2007). Consider the gross domestic product (GDP), an essential macroeconomic concept. Clearly in some ways it is a category reflecting our interests. Specifically, we have to decide what outputs to count as part of GDP. Standardly, household work is not counted and some have reasonably enough found this objectionable. Yet, if we are clear about such exclusions and inclusions in our measures, we can then go on to see how well those measures predict and explain and quiry how results change if we define GDP differently. Values are involved in our categorizations, but not in ways that inevitably preclude objectivity—we can still have good evidence about how the social world works.

4 Section 4: Ontological arguments: indeterminancy and social construction

In this last section I switch gears from the kind of epistemological issues about objectivity in the social sciences to ontological issues. The point again is to show that there are empirical issues at stake in ways not always recognized. Objectivity is now about what kind of things social phenomena are. No doubt there are interconnections with epistemological issues about reliability and unbiasedness of social research evidence. However, there has been and continues to be an active debate about that nature of social reality: is it an object in the ways natural science entities are or is it "subjective," where that means a matter of ideas, consciousness, perception, and so on? I know there are a variety of issues and different meanings potentially involved here. My goal is not to sort all that out but instead the more limited one of showing that some of these questions are in fact specific empirical questions in social research and that those can be pursued independently of traditional grand, conceptual metaphysical philosophical questions. I look at two types of claims that social phenomena are not ontologically objective—that they are indeterminate in that they do not have objective attributes and that they are not objective in the sense of "material."

One important argument raising doubts that the kinds of phenomena investigated by the social and behavioral sciences are objective comes from Quine. In Word and Object Quine (1960) argued that there is no fact of the matter about individual word meaning and thus no fact of the matter about translation. There is a long debate on what the argument is supposed to be (see Føllesdal 1973; Gibson 1986; Kincaid 1996) that cannot be settled here. The clearest argument would be one from the underdetermination of linguistic meaning or translation by behavioral evidence. From our discussion above of holism and underdetermination, we know how to assess such arguments and know that they cannot be resolved in general, but that they can in specific empirical contexts, as critics of Quine's thesis such as Hellman (1974) have shown long ago. Yet these are epistemological arguments, anyway, and the concern in this section is with ontological claims about the nature of the social. Granted of course that the epistemological and ontological issues intertwine, still the interesting assertion here is that "there is no fact of the matter" about meaning and translation, not that they are sometimes hard to find evidence for. The question is then why there is no fact of the matter.

Quine's intuitive argument seems at times to be that all we observe in the social and behavioral realm is behavior and behavior does not have the characteristic needed to be the kind of objective reality studied by science. This is vague, of course. However, a more concrete instantiation of this idea comes from Michell where the argument is about psychological phenomena and measurement.[8] Michell (1999) provides a history and critique of claims in psychometrics that their subject involves quantitative psychological attributes. Quantitative attributes—objective ones that can be measured—have additive properties. Roughly, if a behavioral characteristic C is objective in this sense, then the qualities of C are such that having 2 units of the quality when added to 2 more units gives us 4 units. Large parts of behavioral science—and though this is not Michell's target, by implication large parts of social science more generally—just do not seem to have such quantitative phenomena as their subject. Instead, the social sciences produce scales and "measurements" that are only assignments of numbers by a rule (Stevens 1946). The conclusion is that much of the observable behavior looked at by the social and behavioral sciences is not quantitative—there is no fact of the matter to use Quine's term.

My response here, of course, is that these sweeping claims are untenable and instead ultimately involve local, empirical issues of various sorts. Are all behavioral and social states or phenomena as Michell describes? What about stress as measured by the level of cortical steroids in the blood? Average individual yearly income? Those seem quite objective in the strongest sense of quantitative attributes.

[8] Michell does not explicitly tie his arguments to debates about ontological objectivity.

Secondly, do we need to accept requirements such as Michell's as necessary for ontological objectivity? There can be much social and behavioral science that seems to study objective phenomena that in a broad sense are quantitative but not necessarily strictly additive. Let me just give one prominent example. Neoclassical economics has long struggled with the psychological underpinnings of the discipline, at least of microeconomics. Early versions flirted with hedonism. However, since the 1950s it has been clear that much microeconomics, especially demand curves, can be accounted for and measured by observed choices. These do not necessarily give cardinal measures, but there are clear axioms that show when and if they provide ordinal measurement. Ordinal measurement is sufficient to ground much empirical work. That work allows cross population comparisons, predictions of market behavior, and so on. These enterprises certainly seem to be about objective phenomena. Of course, revealed preference theory may be a special case, an exception. Nonetheless, that is my point—objectivity in the social sciences often involves a variety of empirical issues.

I finish now with another traditional ontological question put in terms of objectivity. The question goes back to the nineteenth century at least and typically is put in terms of "materialism," "idealism," and similar terminology. While there is again a mass of interrelated issues, ambiguities, and so on in this debate, nonetheless this way of framing social phenomena and social science enterprises clearly has not gone away. Lurking amidst these discussions are once again I think a variety of interesting empirical issues.

Many of the real debates in the social sciences are not about general ontological issues such as materialism but instead about the relative importance of "objective" factors and "ideational" factors (for lack of a better word) in the social world. These elements have to be translated into concrete terms according to context, but once done, the materialism-idealism debate turns into many independent, important and in large part unsettled issues in social science research. In short, one can be a philosophical "materialist" about ontology and still think it is an important question what the relative role is of ideas and other nonideational social factors.

Here are some illustrative examples:

> The role of preferences: If we take preferences as roughly the idealist or subjectivist element and actual market supply and prices as the objective element, then there is an interesting ongoing investigation about relative importance in explaining downward slopping demands curves. Becker (1962) in particular instigated a line of research showing that in models of the right kind individual preferences do not matter—they can be random and the demand laws still holds.[9] An ongoing area of research has followed looking at how far nonpreference, "material" factors go in explaining the law of demand.

> Beliefs and norms versus institutions and organizations in explaining social outcomes: The analytic social ontology project often emphasizes the role of beliefs of special sorts as explaining social phenomena. Much of that work is outside the naturalist framework I have adopted here—Searle for instance

[9] His results are derivations from certain kinds of aggregate models—I do not mean to imply that his results are empirical; they use deduction to show what matters empirically and does not.

thinks you do your ontology first on conceptual grounds and only secondly does empirical investigation begin. We do not have to follow that apriorist approach to see that there are interesting empirical questions here. How far do we get with norms as individual beliefs and how much other social organization do we need to bring in?

Money: Money is a favorite example in the philosophical literature where it is often asserted or implied that mutual beliefs of the right kind explain money. However, there are extremely interesting questions about how much institutional structure is needed for money and how that works (and even what money is). The history of money seems to be one involving power, competing political and financial elites, and other such social organization that has much more material or objective aspects that simply a set of conventions.

Poverty: Accounts of poverty have for many decades disagreed on the relative role of mental versus material factors in poverty. The culture of poverty idea attributes it to beliefs and attitudes of the poor; social structural explanations attribute it to power, social organization, governmental policies, and other intertwined aspects of social structure.

The caste system: For many years the most influential scholarship on the caste system in India has come from Dumont (1980). Simply put, Dumont argues that the caste system is the product of the fundamental Hindu religious notions of purity and impurity. The caste system follows from and is built on these religious concepts. Social science researchers, many, not surprisingly, from the Indian subcontinent itself, have argued that the ideology of purity is secondary and that the caste system rests on a complex set of power relationships involving kin, ancestry, economic status, gender and more. The caste system is objective in a quite material way.

Of course, this section is brief and there are lots of issues lurking. However, it does make a prima facie case that there are interesting, open empirical issues in debates over versions of objectivity in social science that may seem at first entirely conceptual ontological issues.

5 Conclusion

Objectivity in social research can, of course, mean many different things. A standard meaning pursed here concerns methods, practices and other elements of science that advance truth-linked ends. There are alleged general considerations showing that social research cannot be objective in this relatively traditional sense. One such argument works from the holism of testing and a related one from the role of values in social research. As general arguments that necessitate their conclusions I have argued that they fail. However, these arguments do raise considerations that can be relevant depending on local, empirical circumstances. I have sketched some of those, showing that objectivity can be both illusive and achievable. Objectivity can also be an ontological question about the nature of social reality. Those questions also can sometimes best be seen as more specific empirical issues.

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