Open Empirical and Methodological Issues in the Individualism-Holism Debate

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I first briefly argue that some issues in the individualism-holism debate have been fairly clearly settled and others are still plagued by unclarity. The main argument of the article is that there is a set of clear empirical issues around the holism-individualism debate that are central problems in current social science research. Those include questions about when we can be holist and how individualist we can be in social explanation.

1. Introduction. The debate over methodological individualism and its alternatives has a long history, and the debate continues (Zahle and Collin 2014). I believe that some issues in the debate have been fairly clearly settled and that others are still plagued by a lack of clarity. However, I want to argue that there is a set of identifiable distinct empirical issues around the holism–individualism debate that are central problems in current social science research, and in that sense the individualism–holism debate is very much a live issue. In what follows I briefly discuss the settled and muddled issues and then devote the majority of the article to outlining some key substantive and methodological social science questions currently under investigation that reflect the individualism–holism dispute.

2. Settled Issues and Continuing Confusions. Methodological individualism has been associated with a number of different theses that often have not been clearly separated (Kincaid 1996, 1997, 2004, 2014). There is not space here to do a full accounting, but for my purposes it is useful to distinguish ontological, epistemological, and explanatory claims. "Social entities are composed of individuals" and "social entities do not act independently of individuals" are example ontological claims. Epistemological claims would include the thesis that macrosociological claims about social entities cannot be con-

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firmed without providing the underlying individual detail. The theses that any explanations in terms of social entities can and must be fully capturable in terms of explanations that are solely about individuals are examples of explanatory claims, claims that are reductionist in nature.

One set of relatively settled issues includes the ontological claims that society is composed of individuals and that the activities of social or aggregate entities such as firms or governments are dependent on the actions of individuals. Few would now deny these individualist strictures in at least some formulation. I describe these questions as "relatively" settled for several reasons. It is not true that society is composed only of individuals, for the built environment—our collective capital, as it were—is absolutely fundamental to most existing societies and yet ignored in ontological formulations of individualism. Humans are a paradigm case of niche constructors. These issues are also not entirely settled because the idea that society depends on individuals can be formulated in different ways: as a claim that individuals and their relations are necessary for social entities, or as the stronger claim that they suffice—that social entities and their actions supervene on facts about individuals, with there being different (e.g., local vs. global) kinds of supervenience claims.

Reductionist explanatory claims are also relatively settled. A standard argument for the claim that social theories can be reduced to individualist ones is that since society is composed of individuals, it can fully be explained in terms of them. Arguments of this general form—X is composed of Y, and therefore X can be explained in terms of Y—were first attacked by Fodor (1974) and then more rigorously undermined by Hellman and Thompson (1976) and put in intuitive terms by Putnam (1981). An ontological fact about composition (or other dependence relations) on its own entails nothing about our explanatory resources. Once it was realized that individualist claims about the inevitability of reduction are a version of the general fallacy that composition entails explanatory reducibility, such arguments for reductionist versions of individualism largely disappeared among philosophers. Unfortunately, social scientists have not fully gotten the message. Thus, Demeulenaere (2013, 182) says that "1. Social life only exists in virtue of the actors who live it. 2. Consequently a social fact of any kind must be explained by direct reference to the actions of its constituents," quite explicitly claiming that composition entails some kind of reducibility. This is one sense in which this reductionist issue is only relatively settled.

Of course, just because reduction is not guaranteed by facts about composition does not mean that it cannot happen. While there may be plausible general considerations suggesting that reductions of social explanations to accounts in terms of individuals are likely to fail—for example, multiple realizations are likely and individual explanations are likely to depend on social

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context—more settled is the fact that there are few if any even relatively successful social science reductions on the books (Kincaid 1996). Social scientists espousing a reductionist form of individualism often produce theories that in practice violate the reductionist ideal, for example, economists espouse individualism but explain in terms of social entities such as corporations and representative collective agents.

These relatively settled issues contrast with recent trends, especially among social scientists, to propose a mechanistic form of individualism as a third way between reductionism and holism. I doubt that an individualism put in terms of a need for mechanisms advances the debate, for several reasons. The first reason is that the notion of mechanisms and the role they are supposed to play can be understood in so many different ways—meanings that are largely unspecified in the literature—that it is doubtful that clear theses are at issue. At the very least, we need to distinguish whether we believe that mechanisms

1. are needed to explain or to confirm;
2. are horizontal (mediating connections) or vertical (grounding or composing in nature);
3. are essential or just useful; and
4. are needed to establish the existence of a causal relation or the size of causal effects (Kincaid 2012).

Thus, the claim that social science needs (individualist) mechanisms is really many different, independent claims, and unless specific versions of those claims are defended, thinking of individualism in terms of mechanisms is not going to be helpful.

I think that it is clear that some of these theses are quite implausible, and leaving the debate at the level of "mechanisms are needed" obscures these confusions. As an example, Elster (1983) claimed long ago that individualist mechanisms are needed to eliminate spurious causal claims, and this assertion has been repeated regularly by others without any careful explication or analysis. If this is a claim about vertical mechanisms (Elster's quick assertions made no distinctions between horizontal and vertical mechanisms), then it seems clearly false. We can have good evidence that flying baseballs break windows without knowing the quantum mechanical details, so this is no general methodological requirement about part, whole, and causation. The general strategy of confirming that balls break windows—the ball was temporarily prior, and other possible causes can be ruled out—is a strategy certainly open to the social sciences and one that, for example, macroeconomics can employ in relating macroeconomic variables while not providing the individualist detail that realizes them. If Elster's claim is about horizontal mechanisms, then the claim is no more plausible. It is not essential to

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identify mediating causes to rule out spurious causal claims, which generally is done by controlling for them, based on our best background knowledge. Again, such approaches are employed, for example, in macroeconomics without finding individualist horizontal mechanisms. Of course, we imagine that having individualist mechanisms of either type might be useful in specific circumstances, but being useful is a much weaker claim than Elster and those reciting him have made.

Another reason to doubt that the mechanistic third way advances the debate is that there is no reason that mechanisms need to be thought of in terms of individuals. If I want a mechanism to explain the connection between macroeconomic variables such as the level of government spending and inflation and employment, I might well supply one in terms of the behavior of firms and households, not individual people. In short, there is nothing about the place of mechanisms in science that dictates the level where mechanisms reside.

Nonetheless, there is an upside to a shift to thinking about mechanisms rather than reduction, although it has not been much developed in the literature on mechanisms in the social sciences. Asking about mechanisms naturally brings up the question of how explanations at the social level and explanations in terms of individuals relate, assuming that the answer is not just the reductionist one, that the latter replace the former. This is one of the open live issues I will discuss below.

Asking how explanations at the two levels relate also points to some different explanatory goals that we might have in social science explanation, which I want to briefly sketch, for they are essential to the discussion of open issues in the next two sections. It is useful to distinguish three distinct targets of social explanation:

1. individual behavior;
2. individual behavior in relation to social organization, for example, in relation to institutions, organizations, and so on;
3. the behavior of nonindividual social entities such as organizations, firms, and so on.

Target 2 exemplifies the mechanistic question concerning how the lower-level individual and the higher-level societal explanations interconnect. Target 1 is the standard explanandum of the behavioral sciences. How much social and behavioral science can focus on target 1 without answering target 2 is at the core of one set of interesting open questions falling under the rubric "how individualist can we be?" (Kincaid 2014). Target 3 asks how and when we can be holist in the first place—what are the scientific standards for deciding when social entities are real, explanatory constructs? This raises interesting questions and is the topic of the next section.

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3. When Can We Be Holist? There are interesting and largely unexplored questions about when it is scientifically compelling to explain in holist terms. In a way, this question is prior to the individualism–holism debate, for if there are no holist explanations, then there is nothing for individualist accounts to have priority over, nothing to be an incomplete or "non-rock-bottom" account in need of individualist help. As I argued above, I do not think that the answer is the general one, that holist accounts are only legitimate when they have individualist mechanisms. But there are important and general scientific questions about providing holist explanations, and these should get much more interest than they have from those whose philosophical and/or social science intuitions tell them that holist explanations are fishy.

It is worth making some distinctions around the idea of holist explanations. The social science literature here does not provide a great amount of clarity, and there has been little useful attention from the philosophy of social science as well (Zahle's [2007] discussion on which individual relations are social vs. individual is a fruitful exception). However, a rough first pass is to distinguish institutions from organizations when it comes to holist explanation or the invoking of social entities. Organizations are relatively discreet, identifiable entities such as corporations or businesses, universities, governments, voluntary associations, unions, and so on. These have explicit formal structure. Institutions are a more nebulous and less concrete kind of thing. Norms, social roles, and property rights are paradigm examples. They are in some sense social and in some sense make for holist explanations, but not in the more robust sense in which organizations are.

I want to focus first on holist explanations that invoke organizations, for they can be put entirely in holist terms and thus are most suspect from an individualist point of view. The issue is then whether we should think of collective social entities as real. One plausible approach says that social entities are real if we can have evidence that they stand in causal relations. I suspect that this criterion is not a necessary one for science in general given the unclear standing of causal claims in parts of physics, but I think that it is a sufficient one that is simple enough that we can apply it to the social sciences. There are at least two ways that causal relations might be identified: via measurement and via what Cronbach and Meehl (1955, 283), thinking about psychometrics, identified as "nomological nets." Measurements give us reason to think that a social entity is real if we can show that the best explanation of independent observable social indicators is that they have a common cause in a social entity that causes them. Nomological nets would be the causes of social entities (their properties, states, etc.) and their effects on other social entities.

In the case of quintessential organizations, these criteria certainly are sometimes met by the social sciences. Economics is probably the easiest case. Innumerable studies with substantial data analyze specific markets in terms of

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households and firms. There are various independently measured traits of these entities. Their causal interaction is studied through supply and demand and price mechanisms. A rich empirical literature supports these holist explanations. Another nice social science example is the area called organizational ecology, which looks at the differential forms of survival of organizations in varying social environments (Hannan and Freeman 1989). Here again there are independent measures of organizations and evidence of their causal interaction with each other.

Thus, the case for pure holist explanations in terms of organizations is fairly strong, but then perhaps also fairly obvious. The more interesting and empirically open cases are those that are not clearly about organizations. Social classes and representative economic agents are two interesting cases in point. Classical sociology and classical economics were happy enough to make social classes fundamental to their analyses of the dynamics of society and the distribution of income. Skeptics from the individualist camp ask whether these groupings are anything more than arbitrary and conventional amalgamations of individuals that carry no explanatory weight on their own.

It useful to distinguish two distinct senses in which there might be social classes: classes as types and classes as concrete particulars. Evidence for classes as types would be evidence that there were traits of individuals that put them into separate classes. Evidence for classes as concrete particulars is evidence about specific social entities over specific time periods. The latter sense of class is more clearly the kind of social entity that the holist defends and individualist denies; classes as types of individuals would seemingly be much less worrisome to the individualist and not strong fodder for the holist position.

Whether there are classes in either sense shows how many interesting open empirical issues there are in this aspect of the individualism–holism debate once one drops the urge to find all-or-nothing answers on philosophical grounds. So, to be more specific, take the idea that in advanced modern capitalist societies there is a ruling class. Only the most conspiracy minded think that there is a ruling class in the pure social entity sense of an organization with formal organizational structure and the like. However, it is still an open question whether we can usefully group individuals by treating them as belonging to a social class—the ruling elite—that is real in terms of measurement and nomological networks.

One way to research the measurement question would be to see whether indicators such as individual wealth, income, and political influence, for example, as measured by political contributions and other indicators, were best explained by the existence of a categorical distinction between those belonging to an elite group and those not. There are taxometric procedures, well developed and applied in the study of psychopathology, for example (Ruscio,

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Haslam, and Ruscio 2006), that could address such questions, although they and related latent class analyses have not been put to such uses in the social sciences as far as I know. Thus, here is an interesting set of empirical questions about whether we can be holist, although it would be about a relatively anemic sense of a social entity.

Another related set of interesting empirical questions concerns whether there is a nomological net—whether we can find interesting causal influences of and on a ruling elite. At this point in social science research, there is some substantial evidence, but it is largely case study and ethnological in nature. Domhoff's (2013) *Who Rules America?*, with its multiple expanded editions, is a classic in this regard, but there have been multiple recent confirmatory studies (Gilen 2012). Each of these studies provides evidence about the influence of ruling elites on politics, society, and economics and on the surrounding social environments that influence how those elites behave and thus is evidence that we can explain with these particular social entities. These are social classes in the sense of concrete particulars.

In these accounts there is a mix of individual and social detail, in terms of both evidence and explanation. Obviously, facts about individual wealth and income are relevant. Tracing the overlap of members across the boards of directors of corporations, universities, foundations, think tanks, and so on, both provides evidence for the elite class thesis and ties it to individuals. But corporations, universities, foundations, think tanks, and so on, are social entities and likewise an essential part of the explanations that ultimately result in theses about the behavior of a social class.

On the view I am defending here, this evidence for a ruling elite is compatible with there being other explanatory social groupings, for example, different ways of identifying elites. If we stick to the idea that there can be holist explanations in terms of social entities when those entities fall into a web of causal relations, then nothing precludes us from finding social classes or groupings different from Domhoff's ruling elite that also allow us to identify causal patterns. Whether such alternative groupings are competing accounts or orthogonal explanatory patterns is an issue that has to be evaluated case by case, again suggesting interesting empirical individualist–holist issues in current social science research.

It is also an open empirical question whether the kinds of evidence and explanation that can be found for the existence of an elite ruling class can also be identified for other elements of traditional class theories of society. For example, can we find the kind of needed measurement and causal relations that would warrant the existence of a working class as an explanatory concrete social entity rather than as a type to categorize individuals? Most of the work on class in twentieth-century sociology looks for evidence for classes as types of individuals, categorized, for example, by occupation. My guess

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is that the prospects here for social classes as concrete particulars are much dimmer and that there are multiple equally justifiable categorizations of types of individuals in terms of occupational classes, but this is the kind of substantive social science issue that is raised by asking whether we can be holist.

To finish off this section, let's turn to the case of representative agents in economics that I mentioned earlier. It is standard practice in some theoretical approaches in macroeconomics to model the economy in terms of representative agents—to develop models and draw implications from them by assuming that a collection of individuals (and also firms) can be treated as a unitary agent described by the relevant utility or production function. This makes for homogeneous agents and greatly aids tractability, for modeling macroeconomic phenomena with heterogeneous agents is enormously more difficult and makes analytic solutions, which are favored by economists over simulations, generally impossible.

Whether representative agent models have more than tractability going for them is another instance of an interesting "can we be holist?" question. There are at least two broad issues involved. The first concerns the extent to which there is evidence for a nomological causal net based on representative agents. Economists use models with representative agents to make theoretic deductions and to show that they fit with some generally accepted economic facts—what economists call "stylized facts." How probative such evidence is raises standard issues in philosophy of economics about models and economic reality but makes that question more concrete by giving it a definite formulation in terms of a specific version of the individualism–holism debate.

A second concern about the reality of representative agents involves a version of the mechanism requirement. I argued above that individual mechanisms are not necessary but can be useful. However, things are a little more complicated than that. If a holist claim makes essential assumptions about how individuals behave, then obviously that claim is troublesome if there is good evidence that the assumptions about individuals are false. In the case of representative agents in macroeconomics, there are established results showing that only under very restrictive conditions will individual utility functions additively aggregate in such a way that they can be treated as having a unitary utility function (Kirman 1992). Is this a case of a theory presupposing implausible accounts of individual behavior? Or, are representative agents postulated without any pretension that there is some coherent story about how they arise from individual preferences? These are further interesting open questions concerning whether we can be holist.

4. How Individualist Can We Be? The other side of the question "can we be holist?" is how individualist can we be (Kincaid 2014). Recalling the three possible targets of explanation described earlier, this question asks how in-

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dividualist we can be in explaining individual behavior and in explaining the interaction of individuals and institutions or organizations. There are several dimensions that we can use to assess accounts as more or less individualist:

- an account may invoke some reference to social entities versus no reference;
- an account that makes some reference to social entities may invoke institutions versus organizations, with the latter being more social and less individualist;
- an account may invoke a richer set of social entities versus a thinner set;
- an account may give the social entities invoked more or less causal importance relative to individual factors.

While more clarity would be desirable (and I will be able to further spell out the last metric below), these criteria give us a rough guide to ranking accounts in terms of how individualist they are. There are a great many interesting open empirical questions in the social sciences that can be framed according to how individualist they are in these senses. These questions are, given my preferences for naturalist philosophy of social science that is close to the practice of social research, much more interesting and relevant to the social sciences than traditional holist versus individualist debates over whether social explanations must be reducible or could never be, the conceptual impossibility or necessity of downward social causation, the necessary and sufficient conditions for collective intentions, and the like. In what follows I describe just a few of the many current live debates in social research that are about how individualist we can be.

An active current debate in experimental and behavioral economics concerns the nature and extent of "other regarding preferences" (Bowles 2006; Binmore 2007). There is substantial evidence—both observational and experimental—that individuals sometimes make choices that benefit others at their own expense. The question is what best explains this behavior. The individualist answer (Bowles is representative here) is that there are innate human proclivities to care about the welfare of others—"other regarding preferences"—common to all humans, proclivities thought to be the result of group selection in our hunter-gatherer past, that explain altruistic choices. The alternative view (defended, e.g., by Binmore) is that it is not innate special altruistic attitudes that explain. Rather, it is social institutions—norms, sanctions that enforce them, organizations that produce repeated interactions making norms essential—that explain other regarding behavior. People act in self-sacrificing ways in artificial experimental setups because they bring with them self-interested behavior shaped by the institutions of everyday life. This presupposition explains

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why self-sacrificing behavior decays with repeated play of experimental games. The point is that the main issue in this lively current debate is a question of how individualist we can be.

Another variant of the expanse of individualism goes to the heart of explaining economic coordination. Smith (2008) argues that the strong rationality and information assumptions common in economics are tools economists need to identify equilibria, but that it is a confusion or at least a much stronger assumption that those tools say something about how interacting individual economic agents reach those equilibria. He reports results showing that it is the institutional structure within which agents act that may allow them to find equilibria, and that those institutions allow them to do so while being quite stupid (Gode and Sunde 1993). For Smith (2008, 65), then, a fundamental current research question is, "How much of the performance properties of markets are due to characteristics of the individual, and how much to those of institutions?" This is precisely my question of how individualist we can be.

The above examples illustrate debates over whether individual explanations have to be complemented with explanations in social terms. However, the fourth criterion listed asks how important the social and individual are if we provide a mixed explanation. Furthermore, the examples cited above were relatively informal in terms of the evidence they use to say how individualist we can be. There are recent developments in social research methodology that potentially address questions of relative importance and at the same time provide for a more rigorous testing of claims that individual-level factors must be complemented with social-level ones.

The developments I have in mind come from the use of multilevel statistical models, most importantly multilevel structural equation models. These are approaches that combine different levels of aggregation as independent variables in statistical tests looking for the correlation with a dependent variable. Hence, a prime application of these methods is in the study of student educational outcomes. The outcomes are the dependent variable to be explained. The explanations are multilevel when not only individual characteristics such as IQ but also higher-level factors such as school type (e.g., magnet vs. not), peer groups, community characteristics, and nationality in international studies are included. Thus, these approaches are mixed social- and individual-level explanations.

The exciting thing about these multilevel approaches is that they potentially tell us both whether the social factors are necessary and how important they are. If social factors show up as statistically significant, then we have evidence that they are necessary. We learn about their relative importance vis-à-vis individual factors by looking at the size of the standardized coefficients in structural equation models (which, unlike standard regression practices, can have a clear causal interpretation). Multilevel models are still relatively

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rare (I suspect that an individualist bias is partly to blame), and details still have to be worked out. As answers to individualism–holism debates, multilevel models are much more convincing when we can give them causal interpretations. The current best framework for converting single-level correlations into causal claims is given by structural equation models. However, there are a number of issues with standard maximum likelihood tests of structural equations when variables are multilevel (Shipley 2009). Yet Shipley shows that there are promising ways to test multilevel causal models by looking for the dependencies and independencies among variables in the data that are implied by the postulated causal model. In this specific case, we have some handle on how to describe the relations between social wholes and the individuals that compose them. For many areas, however, it is an entirely open issue how to relate holist explanations in terms of social entities to individualist explanations of their members—we still do not, for example, have a good handle on how to relate the theory of the firm to theories of individual behavior. These are still further quite open issues in evaluating the individualism–holism debate that ask how individualist and holist we can be.

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