Interaction: A Case for Ontological Pluralism[*]

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Philosophers of biology have recently been worried about the question: what is a biological individual? This worry is prompted by the new salience of the microbiome in biology and medicine. How should we conceptualize the relationship between individual organisms like birds or mammals and the microscale life forms – millions of bacteria – that inhabit their bodies and perform functions necessary for their survival? Are those life forms biological individuals? Or does their dependence on a host make them something less than a full-fledged individual? But, if the host bodies are equally dependent on the microbiome, in what sense could they count as individuals? How should we then define full-fledged individuality in order to encompass those entities we want to include and those we want to exclude? C. K. Waters takes the pluralist-pragmatist view, arguing that we should not ask what biological individuals are, but how the concept is deployed, what work it does, in different biological contexts (Waters 2018). There is not one thing that biological individuals are, but different contexts require different distinctions and boundaries. But there is another question we might ask: why do we care about defining individuality in a metaphysically robust way? This is a question that deserves a genealogical answer: how did individuality come to play such a key role in our various analytical endeavors? Put differently: why do individuals, their behavior, and their properties constitute the subject matter of our investigations?

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In this essay, I intend to motivate this question somewhat, rather than answer it directly. My aim is to show how a focus on individuals in some areas of analysis unwarrantedly limits the scope of investigation in those areas. Most often, when individualism is under critique, the alternative is presumed to be group level analysis. Are groups as real as the individuals that constitute them? Are all groups simply aggregations of individuals, and all group properties, simply aggregates of individual properties? These, too, are questions I don't intend to answer. Instead, I want to draw attention to another ontological domain altogether: the process or event domain, focusing particularly on interaction. My contention is that interactions should play as fundamental a role in analysis and investigation as individuals and their states and properties do. This essay is, then, a plea for ontological pluralism.

My strategy is to first explain what I mean by ontological pluralism and propose a minimal criterion for inclusion of a category in our ontology.[†] Then I will discuss two contexts in my own (philosophical) research where giving interactions the same status as individuals increases the scope of investigation. Following this, I will draw attention to some areas in natural science where interactions meet the criterion I have specified. Here I draw on and am inspired by work of Evelyn Keller. I will conclude by suggesting the advantages of the kind of ontological pluralism I am advocating.

1. Some considerations about ontology.

We might mean at least two things by ontological pluralism: a plurality of ontologies or an ontology that is not driven by reductionist or unificationist aims. My interest is really the second of these. It might be better termed "ecumenical ontology." Rather than trying to identify what is really real (to which any other purported kind of thing can be reduced), an ecumenical

[†] I am neither a metaphysician nor an ontologist. My thinking here draws on my studies of scientific inquiry and reasoning.

ontology will be characterized by a criterion that can be satisfied by different kinds of thing. The tricks are first to articulate a defensible criterion and then to think about what satisfies it. If there are multiple defensible criteria that are not versions of or reducible to a single criterion, they would give rise to a plurality of ontologies. As I will not try to argue that the criterion I articulate is the only proper criterion, I am obviously open to ontological pluralism in the first sense as well.

Philosophy of science was (and in some quarters still is) riven by debates about scientific realism. These debates can occur in two registers. In one the question is: do the unobservable, unmeasurable entities referred to in scientific theories exist (or really exist)? In the other, the question is: can we know that the unobservable, unmeasurable entities referred to in our theories really exist? In the first register we must make a judgment about the reality of things like muons, electrons, genes; in the second, we need make no judgment about their reality, only about our access to those entities should they exist. The important point here is that both ways of asking about theoretical entities presuppose that there is a set of entities or phenomena about which we can say that they do exist. These are measured or observed phenomena whose properties and behaviors we are investigating and explaining by means of theories that appeal to unobserved or unmeasured phenomena. This may smack a little too much of the discredited observable-unobservable distinction of the 1950s. The point is that at any given time, there are phenomena whose properties, relations, and behaviors we can (or take ourselves to be able to) identify and measure. We take those as real phenomena that we then need to explain. Sometimes the explanations are in terms of other measurable properties, but sometimes we (or the researchers actually performing the investigations) postulate other phenomena whose properties, etc. we can't directly measure, such as protons, genes, gravitational waves, etc. Their postulated properties and behavior would explain the phenomena in which we are interested if the postulations were true, but our only access to them is through the phenomena we are trying to explain or predict.

The questions about scientific realism can be understood as asking whether the items that feature in scientific theories have the same ontological status as the items they are invoked to explain. This suggests a criterion: that is real that i) can be measured (counted, assigned values in dimensions such as length, volume, weight, …), and ii) is treated as an explanandum in some scientific investigation. Note that this is merely a sufficient condition. It is not a necessary condition, or at least I am not arguing so. My contention is that this criterion can be satisfied by different kinds of phenomena.

Why is it even worth making such a claim? The response brings us to another major question in recent philosophy of science: reduction and reducibility. This debate, too, has been conducted in both an ontological and a representational/epistemological register. Ontologically, the question is whether all phenomena can be understood as decomposable without remainder into aggregations of units which constitute the fundamental level of reality. Epistemologically/representationally, the question is whether our theories can all be organized into a single overarching theory with many derivations, for example, a fundamental physical theory from which biology, chemistry and all other theories can with suitable auxiliaries be derived. So, again there is a presupposition that there is some level of being whose reality is not in question, and against which all other candidates for that status must be compared. But it is not clear what (in the reducibility debate) qualifies anything for that status beyond being non-decomposable. So, the criterion would be X is real if X is not exhaustively decomposable into parts. If one takes the long view, however, decomposability seems to be as much a function of theories and instruments, i.e., the tools of investigation, as of reality itself. What was non-decomposable one hundred years ago is now fractured into multiple constituent parts. In the reducibility debate, the question is whether only one kind of thing can be real or whether multiple kinds of thing can be real, e.g. parts and wholes, atoms and molecules, molecules and organisms, etc. For the advocate of reduction, only one of each pair is a candidate for reality, and to the extent it is decomposable, its reality status is in doubt. As long as reducibility is a defensible ideal, it is not obvious that more than one kind of thing can satisfy a reality criterion.

Interestingly, the reality granting ground of reduction and decomposition is opposite to that of explanation. Reality ends up at different ends of a continuum or on opposite sides of a dichotomy in contexts of explanation and contexts of reducibility: postulated fundamental entities whose behavior drives and explains all else for reduction and the measured and observed that is explained for explanation.. But in both cases, from a metaphysical point of view, the debates seem to be about the same kind of phenomenon: individual entities (whether compound or atomic) and their properties. There is a kind of self-evidence about the presumed ontological priority of individuals. Our world seems composed of individual entities, whether atoms, people, or tables, and their properties. These are the items about whose ontological status we care. But perhaps this is only because we are ideologically primed to perceive the world in this way either by metaphysical commitment or by language. In social science, there is a debate about the reality of groups over and above their members. This debate seems to be asking whether we can grant to groups the same kind of properties we grant to the members of groups. And this question seems equivalent (or nearly so) to asking whether groups of individuals are themselves individuals. I want to argue for the inclusion not of different kinds of individual into our ontology, but of members of a different metaphysical category altogether: interactions. The ground for such inclusion is that interactions satisfy both criteria of reality articulated above, that is, whether one is using empiricist criteria of measurability and eligibility for explanation, or the more metaphysical criterion of non-decomposability, interactions (at least some) count as real.

I will make the case for interactions first by drawing on some recent work of mine in epistemology and on social behavior and then by drawing attention to interactions of various kinds in some natural sciences. Here the point is, first, to show that individualist thinking is constricting, and, secondly, to show that interactions do satisfy the reality criterion. I then want to connect these discussions to some of Keller's points about language and science (Keller *this volume*; 1995) . Here, the point is to give the concept of interaction some philosophical heft.

2. Two contexts

a. Social Epistemology

In epistemology, the individual cognitive agent and his/her doxastic states have been the traditional focus of analysis. The prototypical analysand in contemporary epistemology has been the statement: "S knows that *p*." This is in turn understood as involving some or all of belief on the part of S, truth of *p*, and justification of S's belief that *p*. Each of these is the subject of intensive debate. No matter how these components of the assertion are understood, the focal situation remains the individual and their relation to what they believe. Recently epistemology has moved to include social questions, considering the individual cognitive agent in social relations: Alvin Goldman's aptly titled, "Knowledge in a Social World" exemplifies this turn (Goldman 1999). Phenomena like testimony, disagreement, deference to experts involve at least two individuals. In most of this new literature they are approached from the individual's perspective: should I/you believe S; what should be my attitude/response to disagreement? When is deference to experts warranted? How do I tell if an expert is genuine? How do I choose between experts who disagree? These are the new epistemological questions for the individual when their world includes more than the non-social phenomena more standardly presupposed in epistemological set-ups. And, they constitute the subject matter of much social epistemology. But this approach reveals a crucial oversight of traditional epistemology.

Consider disagreement: From the point of view of the traditional, individual cognitive agent centered epistemology, disagreement among peers poses a problem. Many, if not most, writers frame the problem in the first person or in the voice of an individual cognitive agent who believes her or himself to know, or at least to have a (well-) justified belief, that *p*. The framing of the problem is from the point of view of S in "S knows that *p.*" What do I or what does S do when, believing ourselves to be justified in our belief (or in having a high degree of credence in *p*), we encounter disagreement from one whom we take to be an epistemic peer? Why should disagreement be a problem? If an epistemic peer is one with the same data and equal epistemic competence (equal perceptual and reasoning powers), then the peer's disagreement seems to be evidence that my or the original agent's reasons do not support *p* (or the degree of credence invested in *p*. But I believed myself to have good, even decisive, reasons for p. The task of the epistemologist is taken to be to solve this problem for the agent by recommending a course of action.

The epistemologists engaged in these discussions tell us how disagreement should affect the individual's accumulation of evidence relevant to *p*, in ways that preserve the legitimacy of the individual's self-trust. Indeed the point of the discussion seems to be to identify forensic resources available to the individual for rational incorporation of the experience of disagreement back in the smooth cognitive flow. The difference among the philosophers lies in the precise strategies of incorporation that are articulated in each philosopher's preferred epistemological framework. In spite of differences in framework, they seem to share an assumption about evidence – that it is monotonic: if *e* is evidence for *p*, it is not evidence for *q* where *p* if and only if not-*q* – and they seem to be in agreement that the solution involves moving to an epistemological or cognitive metalevel, whether evidence about evidence, explanation of disagreement, beliefs about reliability. The assumption about the monotonicity of evidence makes the phenomenon of disagreement a problem for epistemology. How can two epistemic peers, that is, equals in cognitive abilities, disagree when in possession of the same facts?

Philosophers of science have long known that two or more equivalently informed and reasonable researchers can disagree about the evidential import of the same observable phenomena, the same facts. Pierre Duhem, Thomas Kuhn, Paul Feyerabend all provided illustrations from the history of science (though they disagreed in their diagnoses). But whether diagnosed as underdetermination or theory-ladenness, this feature of the practice of science reveals how the attribution of evidential status to data is a community level practice. Researchers must agree that a given phenomenon is evidence that such and such a state of affairs obtains. Philosophers disagree about the means of such agreement. I have argued that that agreement is achieved through interaction – critical discursive interaction among the members of the research community that sifts through conflicting readings of the data and eventually stabilizes (or rejects) their evidential import. In a forthcoming paper (Longino n.d.), I argue that most approaches in social epistemology take the basic questions of epistemology to be articulated and answered at the individual level. But for the most part, epistemology conducted at that level takes for granted that we know what evidence is. A serious look at the history and practice of science calls this presupposition into doubt, arguing that there can be reasonable differences as to how to characterize the measurable, observable aspects of the domain one is investigating, and reasonable differences as to what hypotheses are supported by observations about whose characterization researchers agree. That is, how to characterize candidates for evidential status and how to characterize evidential status itself are matters to which there is no a priori access and which must be settled by the community in order for research to go forward. These are matters settled through the critical interaction among members of the research community, matters that can change with changes in instrumentation, representational resources, personnel, etc. A social epistemology that limits its purview to individual actions and practices leaves crucial questions, like how evidence gets to be evidence, unanswered.

b. Social behavior

In some of the most prominent approaches in behavioral science (classical and molecular genetics of behavior, neurobiology, social-environment oriented research) the focus is also on individual organisms. And, in the sciences of human behavior it is, of course, on human individuals. Behavioral science is focused on variation, how do individuals come to vary in individual properties and propensities. Different research methods permit different specific questions and hypotheses. They might all share an interest in the question: Why do individuals in a given population vary in their expression of some behavior B? Behavior genetics approaches this question by asking: To what extent can variation in B in population P be attributed to genetic variation and to what extent is it attributable to environmental variation? It has developed specific methods to answer this question. Neurophysiology addresses it by tracking correlations between specific neural states identified through various technologies and behavioral states, and trying to rule out neural alternatives that might play a role in the behavioral variation. The methods in these and other approaches (socio-environmental, molecular genetics, integrative efforts) are drawn from methods for studying similarity and variation in non-behavioral properties, like eye color or height, cognitive abilities.

Many of the behaviors studied in behavioral sciences, however, are not purely individual, in the sense of self-concerning behaviors such as brushing one's teeth, but behaviors involving other individuals. They are treated as the individual's behavior with or directed towards others. So, social phenomena like aggression or sexual behavior, which generally involve two or more persons are studied as individual traits: What factors, genetic, neurological, physiological, social, impact the disposition to react aggressively to situations? What factors impact choice of sexual partner or activity? These social behaviors, then, are studied as the properties of individuals are studied. But, there are questions that cannot be answered by looking only or primarily at the properties of the individuals involved in a given social behavior. Consider differences in the frequency, timing, or distribution of a social behavior in two or more geographically separated populations constituted of biologically similar individuals. We could be asking about problematic behaviors like violent aggression or pro-social behaviors like grooming. What is of interest are the differences between the populations, and as I and others have argued, these questions cannot be answered by looking only to the differences among the individuals constituting the populations (Longino 2013, 2019). What we really want to know is what explains the difference in frequency, etc, of certain interactions, not why individuals engage in those interactions. The latter continues to be a reasonable object of research. It is just not the only kind of research question prompted by social behaviors. We can more readily see the full range of questions when we look at the social behaviors as interactions, rather than as expressions of individual dispositions.

In treating social behavior as interaction, behavior is in an important sense depersonalized. We are not asking why individuals do what they do, but what conditions facilitate or obstruct the occurrence of certain interactions, independently of the features of the participants. This approach to social behavior resembles approaches in ecology rather than genetics, neuroscience, or psychology. But ecology, too, has been an arena of debate between individualist, reductionist approaches and non-individualist approaches. It is incumbent on the advocate of interaction to look more closely at particular efforts in the life sciences where interaction is the more apt label for the phenomena under investigation.

3. Biology

Social epistemology and study of social behavior are not the only areas of research where individualist bias has obscured features of phenomena under investigation. Research in many areas of biology reveals the centrality of interactive phenomena to the existence, origin, and persistence of life forms. Study of this research by historians and philosophers also reveals the difficulty researchers have in thinking of our world as composed of anything but individuals and the resistance to modes of analysis that seek explanations at levels other than factors internal to individuals.

Evelyn Keller brought this to our attention forcefully in her analysis of the work of corn geneticist Barbara McClintock, her analysis of the work of Christine Nüsslein-Vollhart, and her own analysis of slime mold aggregation. Let us think first about the latter. Slime mold is a generic name for a family of organisms that exist in multiple forms. In an early stage of her career, Keller was interested in using Turing diffusion equations as a way of understanding the general problem of development of differentiated structures from a genetically uniform system. Biologist Lee Segel brought the phenomenon known as slime mold aggregation to her attention.

Dictyostelium discoideum exists as a mass of single unicellular organisms until those cells come together to form organisms consisting of stalks and fruiting bodies. Together Keller and Segel developed mathematical tools for describing the transition of *Dictyostelium*. from one form to the other without postulating a genetic difference in some subset of cells (the "pacemakers") that trigger and govern the movement (Keller and Segel 1970; Keller 1983a). The prevailing view held that these cells have a genetic mutation that enables them to release the chemical, acrasin, thereby triggering the movement of other amoebic cells toward the releasing cell. Instead Segel and Keller developed equations that could model the acrasin diffusion and slime mold aggregation as the outcome of an interaction among otherwise identical cells. This interaction is a response to local changes in parameters that support the persistence of a steady state in a colony of uniformly distributed single-celled amoebae, primarily the exhaustion of a nutrient base. Interestingly, although their work was not at the time widely adopted in the case of slime mold, a quick search of recent literature shows that the Keller-Segel model is itself an attractant, as researchers continue to adopt and adapt it to understand chemotaxis (chemically mediated intercellular interacton).

Keller also drew attention to similar themes in the research of others. Perhaps her biography of geneticist Barbara McClintock is the best known of these efforts (Keller 1983b). Here she showed how McClintock, by trying to understand differences in her samples (corn kernels on a cob, for example) developed models of the interaction among genes and between genes and their cellular and larger environments. Keller was also drawn to the work of developmental biologist Christiane Nüsslein-Vollhardt, who studied gene-protein interactions in development, in particular gene-protein interactions in oocytes (Keller 2017; see also Keller 199). Thus, her work contributes to more general understanding of maternal effects in development. Nüsslein-Vollhardt performed pain-staking experiments showing that such features of *Drosophila* embryos as polarity (differentiation along the head-rear axis) and segmentation could be treated as the outcome of morphogen gradients (in this case, diffusion of the *bcd* protein). These results were extended to other species and genera.

Keller's expository focus is demonstrating what can be gained by resistance to exclusively genetic explanations of phenotypic phenomena. Her target is what she called the master-molecule approach to biological analysis, namely, the effort to attribute phenotypic differences to some governing entity characterized by a unique and stable genome, as suggested by a popularized version of what Francis Crick called the Central Dogma. She saw this as, among other things, one expression of a masculinist androcentrism in science that perpetuates a conception of the world as divided between active and passive objects. We might think of this as the use of metaphors drawn from the experience and point of view of those engaged in science (or those whose perspectives counted in the scientific community -- men) imbued with a sense of its correctness. Keller saw this masculinist androcentrism also in then conventional ways of understanding scientific objectivity, famously distinguishing between static objectivity, characterized by a rigid orientation towards dominance of the object of understanding, and dynamic objectivity, characterized by the ability to move in and out of intimacy with the object of understanding (Keller 1996). There is a consistency in her thought: we need to accept that the world is complex and active and stop trying to shoehorn it into changeless forms. This is not to abandon the effort to understand, still less to abandon the effort to represent relationships mathematically. Rather it is a call for tools of analysis that are capable of expressing the complexity of the natural world.

If we reject the effort to attribute certain phenomena to the action of one agent, there are at least two alternatives. One is to identify another agent, another molecule (a different gene) or perhaps an agent at a higher (or lower) level of organization, a protein (or an amino acid). Another is to look for multiple agents that somehow together account for the phenomenon one is trying to explain. It is the character of that togetherness that interests me. To say that the action of two or more factors is required for the manifestation of the phenomenon is not yet to say what kind of action. It might be joint action, or coaction, or interaction. These distinctions become salient when interaction *per se* is the focus of attention. For the moment, I want to remind the reader of some other biological contexts in which interaction rather than single factor action is invoked.

Perhaps the most infamous instance of ignoring other participants and possible interaction is the description of the process of fertilization in mammals. Classically (and still in some text books) fertilization is a matter of an active, wriggling sperm cell (or many such cells) making its (or their) way up the vaginal canal until encountering a motionless, waiting egg. The sperm then penetrates the egg and once inside, the two haplotypes merge into one cell and one full set of chromosomes, setting off the process of development. For years observations of egg behavior were ignored by the broader research community. Finally, the role of the egg's cilia in selecting which sperm cell will enter the egg and in facilitating the sperm's passage through the outer covering, the zona, of the egg, have been more widely recognized. The shift in perspective was clearly and wittily described by historian Emily Martin in her article, "The Egg and the Sperm: How Science Has Created a Romance Based on Stereotypical Male and Female Roles." (Martin 1991) Researchers nevertheless continued to find ways to describe the sperm's role in the process as active in contrast to the passive role of the egg. Freed of sexist (and individualist) preoccupations, however, we can see the research on the egg showing that fertilization is not a process in which an active element acts on and transforms a passive element, but an interaction in which two elements act on each other in a process that culminates in fusion of the two cells and their genetic material. Because the first sperm to arrive is not necessarily the successful entrant, incorporating the egg's participation in the process helps make sense of aspects of fertilization that remain puzzling if the sperm cell is the only actor.

Many other life processes are interactive processes, that is, processes in which two or more different kinds of entity combine in an ongoing or episodic way. The various forms of symbiosis, which involves an exchange between two or more organisms that is beneficial to at least one of them, are the most familiar, but one can see other biological processes as interactive as well. Thomas Pradeu has argued that biological individuals are "integrated wholes made up of heterogeneous constituents (including many microbes) that are locally interconnected by strong biochemical interactions and controlled at a systematic level by immunological interactions." (Pradeu 2019, p. 25) New research on the origin of life proposes that catalytic interactions among chemical constituents of the early prebiotic soup were the precursors of metabolism and that cyclic (that is, stably repeating) interactions of this kind gave rise to the metabolic processes that in turn gave rise to self-replicating molecules (Trefil, Morowitz, and Smith 2009).

It would take more analysis of these kinds of case to fully explicate and support the idea that interaction is in some ways and in some instances as or more fundamental than individuals. Some of this argumentation is to be found in recent work of John Dupré, although his focus is on process rather than interaction specifically (Dupré 2012). My contention, in any case, is that interaction is on a par with objects (individuals) ontologically speaking. Here I want to return to the general proposal for thinking of interactions as an ontological category and to bring that into conversation with Keller's concerns with language and science

4. Language and Reality

Keller points to the dual function of metaphor as both helping to entrench ways of thinking and enabling novel ways of representing and understanding natural phenomena (Keller, this volume). Metaphors, especially the productive ones, do more than offer us visualizations of unvisualized phenomena; they express metaphysical commitments as well. Keller has brought our attention to the representations of the gene and genome as stable directors of developmental processes, through the metaphoric language of gene action. The language used to characterize the gene was an outcome of the need to reconcile two conflicting aspects of ascribed gene function: serving as the stable atomic constituent of life and as (actively) directing development. Something truly stable does not change, something engaged in directing a process must change, i.e. do something, in the course of that action. Keller suggests that language that obscured this distinction was important in facilitating research on genes and the genome. I would like to propose that another reason that it was possible to embrace a language that papered over this apparent contradiction is that such contradictory entities have been at the center of western philosophy since the beginning. Plato's changeless forms that are the really real entities, Aristotle's Prime, Unmoved Mover, various representations of the divinity conceived as the *theos* of monotheism, all embody this contradictory aspect.[‡] So do the various conceptions of fundamental material particles, whether corpuscles, atoms, or elementary particles.

A much more serious history of western philosophy and science would be required to fully explore these contentions. Nevertheless, just a cursory survey of our systems of

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[‡] Indeed, these are the worries raised in the great poem of Parmenides, whose interpretation continues to be a subject of debate. See Palmer (2016).

representation shows that we tend to think in terms of independent self-contained agents acting on a receptive or passive object as that which is real and whose properties and capacities it is the task of science to describe and explain. In questioning the self-evidence of this view, I don't deny that individuals are real. But this potted view of why they should figure exclusively in so many of our ontologies at least suggests that we might think twice before ontologically privileging them. If Pradeu is right, after all, mammalian organisms maintain their distinct individuality through the interactions among multiple parts and systems. Of course, the organism as a whole also interacts with its environment, but the interactions from the biochemical to the organic that hold the individual together distinguish the individual from its environment by the density and intensity of those interactions.

My point is not to urge replacing an object-focused ontology with a process- or event-focused ontology. It is rather to urge that interactions be studied for their own sake, just as we study objects, individuals, and kinds of individuals. Interactions invite both the scientific study of specific kinds of interaction and the philosophical study of interaction *qua* ontological category. In biology, it has been common to posit the interaction of genes and environment to explain the development of phenotypic traits as well as the distribution of phenotypic traits. This explanatory move has been seen as part of a more general resistance to reductionist tendencies in (and about) biology. That is, a common direction of explanation of individual properties and differences among individuals is to move to the internal constituents of individual organisms: hormones or (ultimately) genes. However, as long as interactions feature only in explanations, they will be subject to the vicissitudes of debates about scientific realism vs instrumentalism. What the examples I have mentioned show is that interactions can themselves be the target of explanations. There are questions about interactions *qua* interactions. Interactions do not figure only in explanations. As we begin to address differences in the distribution of phenomena among different populations, interactions themselves become the focus, not the individuals participating in them. We may want to ask about the distribution of certain kinds of interaction, about their frequency, about the conditions that facilitate or hinder the occurrence of certain interactions. In these kinds of inquiry, interactions are not posited as explanations of observed phenomena, but are themselves observed phenomena that can be counted and measured and whose characteristics can be explained.

The ecumenical ontology I advocate is not driven by metaphysical concerns, but is a more bottom up enterprise. What do we observe? What questions do we have about what we observe? Keller observes how genetic research has moved from representing the gene as a fixed, stable object to a system for reacting to signals received from the environment. As a reactive system, though, it can no longer be understood in isolation. It reacts to something. And, I would suggest, in that moment of reaction, we could as easily see interaction as action. As research develops it unveils complexities unconceived of in early phases of investigation. In Keller's account, research has moved from considering the gene in agential terms to considering it in interaction with various levels of its context. We will always be constrained by the linguistic resources available, but natural languages are rich enough to offer alternative descriptive expressions than those favored in the mainstream. One of our jobs as philosophers is to attend to the ways in which one formulation may have outlived its usefulness and to ask why it nevertheless persists. Another is to consider the metaphysical and ontological commitments carried by the language we use. We will not truly escape those commitments without an alternative formulation.

I suggest that the language of interaction has the potential to both generate interesting and fruitful scientific research and to avoid some of the problematic consequences of the exclusive language of individual objects. I have argued that this is the case in social epistemology (and epistemology more generally) and in the study of social behavior. A brief survey shows this is also the case in other areas of biology. Philosophically filling out the concept of interaction may make that language more available. This means distinguishing interactions from other kinds of events and processes and classifying them into various kinds: cooperative or competitive, mutualist or parasitic, episodic or continuous, statistical or causal, and so on. For the moment, it must suffice to say that interactions are a kind of hybrid of process/event and object, as they involve objects/individuals exchanging energy in some form or other. I see no reason to think either the objects or the exchange are more fundamental or more real than the other.

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