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## (Explaining)<sup>2</sup>

### John Levi Martin talks explanations, causality and social aesthetics

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**DV:** Could you perhaps start by giving us a sense of how *'The Explanation of Social Action'* came about? Those acquainted with your earlier work will recognize some familiar topics, including (but not limited to) your papers on totemic logic and classification (Martin 2000), the concept of the 'authoritarian personality' (Martin 2001) or your writings on field theory (Martin 2003; Martin & George 2006). How does this earlier work lead up to the arguments formulated in the book?

**JLM:** At core, I am a political sociologist who focuses on beliefs. Starting from Marx, and then Alvin Gouldner, I was thinking in conventional class-based, sociology of knowledge when as a graduate student I was introduced to Bourdieu. While it was my advisor-to-be, Ann Swidler, who first assigned Bourdieu, it was my collaborator-to-be, Matt George, who

showed me the Bourdieu that excited me. I think it might have been like when Marx first met Hess, Ruge and the other Young Hegelians, and learned to see Hegel in an exciting way. I thought that Bourdieu solved some of the problems that the sociology of knowledge had not surmounted regarding the connection between social position (interests) and beliefs or subjectivity. The problem was, however, that in the US, the interpretation of this aspect of Bourdieu's work was at best inaccurate and at worst a travesty. Our idea was that 'habitus' was something that Bourdieu invented out of his own head, and created as an intervening variable between class position and action or subjectivity. It was, however, a 'black box' that we knew nothing about. The notion that *habitus* was a long established concept that had a direct relation both to the processes Bourdieu was describing and to the theoretical tradition he was beginning from, and that this implied that *habitus*, far from being some analytic black box, was among the most robust facts in our personal experience (as it denotes each person's 'thang' or way-of-being) well, this was totally foreign to most Americans.

As a result, there were a few things that were pivotal to Bourdieu's contribution but basically ignored by most of his American followers, or only repeated in the form of catch phrases. One was his approach to reflexivity, which required accurately positioning the researcher in a set of relations with others, including but not limited to the subject of inquiry. Another was the relation of action to the body. Strangely enough, in the theory world, with some clear exceptions (most importantly, Wacquant [2004]), social theorists who talk about the body seem unwilling to talk in anything less than the most rarified abstractions, belying the phenomenological basis of Bourdieu's approach. Finally, as I worked more seriously on the notion of field theory, it became apparent how consistent Bourdieu's approach was with the core of this approach, but in his writings one only sees the tip of the iceberg.

So what connects those articles to ESA is the interest in taking seriously the idea that what we mostly do is classify bodies, and that bodies differ in meaty ways, as you of course know, you wrote an excellent piece on this yourself (see Vandebroek 2014); the interest in connecting field theory to its phenomenological basis; and the belief that our analysis of beliefs was fundamentally compromised by a lack of the kind of sociological reflexivity sketched by Bourdieu. *The Authoritarian Personality* is an amazing resource for working through the pathologies that come from a non-reflexive political psychology; it's

much harder for us to uncover these biases in more contemporary work, because the assumptions don't stand out, they are so naturalized.

So something that began as a critique of the classical sociology of knowledge (some of which is preserved in chapter 4 of ESA) dovetailed with an exploration of a coherent phenomenology (some of which is preserved in chapter 5 of ESA), the first of which I had started in graduate school (along with the work on field theory, some of which is preserved in chapters 7 and 8 of ESA). Hence it made sense that they called out for each other and became a book. The reason the book didn't come out for a decade after these parts started to come together was that I wasn't convinced that Bourdieu really had done what he thought he had, namely to formulate a way in which social science could be rigorous, empirical, and more justified than the folk explanations it might study, without falling into the 'mutually assured destruction' that Mannheim had uncovered in the classical sociology of knowledge. I began by pursuing the phenomenological approach of the first generation of field theorists, who had grounded at least the core of their answer to this problem in an acceptance of naïve realism (though they were by no means naïve, and Köhler coupled his *phenomenological* naïve realism with a neo-Kantian acceptance of the trans-phenomenal, being able to link the pair via his principle of isomorphism).

I patiently tried to work this through consistently, and when I did this, I found (in part thanks to others!) that I was wrong in many particulars. In some places, my cognitive science had been hasty. In some cases my readings of theorists had been superficial or tendentious. In some cases, my logic just didn't add up. I had wanted to give a vigorous defense of naïve realism as a coherent phenomenological grounding for a revised theory of action that would rehabilitate actors' accounts. I found that this was impossible, and figuring out what *could* be rehabilitated (experience) took some time, even with the help of Dewey's work. Once things seemed to be falling into place, it turned out that I could speak to something that I'd always found of interest, namely the relation of causality to human action. (The first thing I published, a comment in the journal *Sociological Theory*, was about this, and laid out in compressed form the basic idea that I would still defend today. See Martin 1995) It was like doing a jig saw puzzle, in the sense that there is something like what we call a phase transition in physics, or a percolation process, in which suddenly, past a certain point, instead of being harder and harder, everything gets easier and easier because it really is fitting together.

**DV: The publication of *'The Explanation of Social Action'* follows on the heels of another impressive volume, namely *'Social Structures'* (Martin, 2009). While similar in style, both books offer an in-depth treatment of quite different topics. Could you perhaps help us understand the relationship between them and how they relate to your broader research-interests?**

**JLM:** Well, in terms of the overall argument, both sit together in the same general sense of a vocabulary for sociological investigation. I see a plausible vocabulary for the quasi-structural analysis of social formations in which interactions aggregate into particular relationships, and these relationships crystallize into concrete local structures. That's what SS was about, and I end that by briefly sketching the transition to institutions. A local social structure has implicit principles of action (in the sense of relationship formation and dissolution), and when actors form subjective representations of these objective principles, we speak of the development of an 'institution.' Institutionalization allows a kind of non-local action that is otherwise very difficult to coordinate, and even further, it allows for the alignment of institutions, which is one way of understanding what a 'field' is. Or rather, it's by definition what an *institutional* field is; we can also speak of social-psychological fields, as did, say, Lewin and Koffka, but I think in sociology, a field *is* that set of aligned and melded institutions. ESA tries to make that point by tying the regularity of fields, both for actors and for analysts, to this process of institutionalization.

In methodological terms, SS didn't spend a lot of time justifying its approach to generalization. I see ESA as giving a more coherent exposition of the logic whereby we can reach generalizations without abstracting away from the phenomenologically recognized qualities of actors' experiences, and I would hope that SS lives up to these strictures reasonably well. There are a few abstractions in SS, but these are basically the dimensions that recur in analytic reconstructions of the space of variation of structures, and I think in most cases, these aren't going to be foreign to the experience of actors (things like individualism, structural elaboration, equality are easy to link to experience, even if the terms I stole from Harrison White—differentiation, involution, dependence—aren't). Sociology has to be about generalization. It has to give us more leverage over the empirical than we have in everyday life, at least sometimes. But I don't think there's any way to justify the castle-in-the-air systems building that counted as theory for a long time. Both SS and ESA are trying to figure out how we can do that, though SS is more of an existence proof and ESA more the logical proof.

**DV:** To pick up your remark on theory as ‘castle-in-the-air systems building’: you attribute the source of much conceptual confusion in contemporary social theory to sociologists’ ongoing infatuation with so-called ‘third-person explanations’ or ‘third-person-causality’ (TPC) which we somehow deem to be intrinsically superior to ‘first-person explanations’. Could you perhaps start by clarifying that distinction and its intellectual sources? You point to the epistemological duo Freud and Durkheim as having been particularly instrumental in popularizing our conventional understanding of explanation...

**JLM:** This distinction is a pretty common idea; a ‘third person’ vocabulary—one which we use in speaking about *someone else*, neither ourselves nor our listeners—is one that need not make any reference to the subjectivity of the actor in question, especially his justifications. Possible families of third-person explanations include *functions* and *causes*, and it is the second of these that is currently of greatest centrality in sociology. But we usually don’t explain our *own* action by its causes (we generally do that only when we need to evade responsibility for our actions). This third person vocabulary then provides a different understanding of what it means to ‘explain’ an action than that in everyday life, where asking for an explanation means ‘calling someone to account.’ A first-person vocabulary is one that makes reference to that subjectivity.

It is already at this point that two common misreadings of ESA pop up. The first is that because I point to serious problems in the use of third-person vocabularies *for the explanation of social action*, I therefore would oppose their use in *any* parts of social science, which would be absurd. There are many questions in social life that can be answered using causal language without instability; while I still think that the *status* of such answers is not quite what some others imagine it to be (for “cause” is an anthropocentric, not mind-independent, category), it is usually the way to go. My point was simply that it becomes unstable when applied to explain *actions*, and that more of sociology than we might first imagine needs to deal with such actions. The second misreading was an assumption that because I found problems with the use of third-person vocabularies to explain social action, I must find the first-person ones acceptable. But I argued that these were not worthy of scientific status. The problem wasn’t that we were picking the *wrong* answer to the *right* question; it is that we had asked the *wrong* question in the first place.

To return to the question—where do these vocabularies come from? *In nuce*, the true sources of this are twain; the first part happens in the seventeenth century, especially in Britain, and it is the rise of a new experimental attitude towards the production of natural knowledge. Although some of the mechanists even then applied this to humans, they tended to assume that their account would overlap with first person ones—thus Hobbes seemed to imagine that mechanical balls of desire and aversion banged around inside the mind and then travelled down the nerves to make the arm move, thus tying first-person (desires) and third-person (causes) vocabularies together. Regarding the second, unfortunately I am not a good enough intellectual historian to give a clear answer. But it seems to lie in France between Comte and Durkheim. The great functionalism that was associated with the first wave of positivism involved a principled rejection of ‘Why’ questions for all issues as anthropomorphic. Thus there was a dismissal of first person vocabularies, and indeed, since Saint-Simon, a confidence in expert knowledge to allow the dismissal of lay ideas. Even more, Quetelet’s mystical interpretation of statistical stabilities as constraint (especially in Buckle’s popularization) was clearly associated with imputing a *constraint* to actors of which they were not aware. But I’m not aware of the functional type accounts being understood to be in a ‘face off’ for explanatory power with first person accounts until Durkheim, probably because of his notion of psychology as his rival in a turf war for human behavior. Durkheim alternated between two visions; in the first, the social was subjectively experienced by the actor, who could accurately report on feelings such as those of moral pressure. But, perhaps to inoculate his arguments against dismissals based on introspection, he did not attempt to make the subjectivity of actors a key mediating test of his arguments. When there was a ‘face off’ between his theory and what actors said, his theory won.

Freud’s role was not large, but he provided crucial reserve troops that could be sent in if the Durkheimian lines weakened under an assault of common sense. In the worst years of the Freudian influence, it was accepted by many social thinkers that actors could be trusted *not* to know their motivations. And thus a theory of action was immune to disproof coming from studying the subjectivity of actors. It does turn out to be true that there is no good reason to think that we can directly explain action merely by querying actors about it. But it isn’t for the reasons that Freud said. The real reasons are amenable to conventional empirical explorations. Freud’s required the injection of arbitrary

authority and an agonistic encounter with the subject, not a promising way to start a social science.

**DV: You argue that this ‘Freudo-Durkheimian’ conception of explanation is closely tied to what you see as an equally flawed (yet widely prevalent) view of social cognition, which you dub the ‘grid-of-perception’-model. What is it about this model that is so inherently problematic and how is it tied to sociology’s obsession with ‘third-person-causality’?**

**JLM:** The grid of perception and the focus on third person causality are potentially independent, but mutually reinforcing; each leads us to downgrade the ‘this-sidedness’ of actors’ cognitive schemes, and hence encourages us to replace them with our own, whether we are talking about reasons for their actions or their subjective orientations to the world. Since people’s subjective orientations to the world *are* relevant for their actions (that’s why people have minds), refusing to take these seriously means doing a bad job. In any case, the problem with the grid of perception model is, first, that it is close enough to being true that the errors are hard to identify. That is, when we interact with the world, we approach it with a mind that is formed. Some of that forming is relatively hard wired, in that pretty much every functioning neural system will develop an orientation to the place and motion of objects, will recognize faces in an upright position, and so on. But lots of it is culturally variable and changes over our lifespan.

The problem with the grid of perception approach is that, based on eighteenth century theories of vision, it treats our perceptions as consisting of unordered sensations (think, for the case of vision, of color patches of different shapes), which are then rotated and matched to some template so that they can be put in a set of cultural boxes that come from our language, or—as it often seems in sinister social constructionist accounts that use the passive (‘femininity is constructed as...’)—worse, from some crafty clique of scheming elites. What this misses is, first, that the way we get this ‘culture’ isn’t through indoctrination, but our experience of developing in a world that has principles of organization (ones that ‘we’—or at least, our predecessors—put there). Even more, it misses what perception *is*; it is a differential sensitization to the features of the environment that are relevant for action. Our approach seems to imagine that there is a true, pre-processed, ‘unbiased’ knowledge to which all knowledge—better, all knowledge that *other people* might have—can be compared. That’s a natural folk theory, but so is

imagining that someone must have hexed you if your crops wilt. We can't start our science by simply taking that for granted.

**DV: Drawing on somewhat neglected theoretical traditions - the German *Gestalt*-theorists, the developmental psychology of Vygotsky and the Pragmatism of James, Peirce and Dewey - you not only sketch out an alternative model of the way in which actors perceive and interact with their surrounding environment, but also propose a novel way of studying this interaction through an approach you dub a 'social aesthetics'. Could you outline the analytical core of this approach for us?**

**JLM:** The core is to say, first, that the best way of understanding the cognitive components of action is to look not at 'knowledge' (which is a particular social production that has been only completely realized in actors since the development of printing, and the importance of which tends to be overemphasized by the printing classes) but 'taste.' Those of us in the printing classes generally imagine—wrongly—that knowledge as a set of propositions guides (or should guide) action in the way that a minor premise guides a conclusion in a syllogism (with the major premise usually being our values or goals). We accumulate incorrect support for this view by asking people afterwards 'why' they did this or that, eliciting accounts that make use of the same folk theory that we have as actors.

Instead, we need to figure out how we orient ourselves to our environment. Rather than take sight as our root metaphor, take taste; and then think about what it means to have good taste and bad taste. We know that taste is *cultivated*, but that does not involve the arbitrary classification of items into boxes; rather, it involves the differential sensitization to aspects of the world that are there. That's a good exemplar of experience in general; what we experience are the *qualities* of objects, and qualities are (circularly) defined as the potential to induce certain kinds of experience in certain kinds of actors. A wine might have a quality to induce an experience, but only in an actor of a certain cultivation. (And of course, actors can be marvelously wrong in their attempts to translate experiences into knowledge, as when an expert declares that a cheap wine is excellent, merely because it has been poured into a fancy bottle.) The key then is that we apply this perspective to social objects. I really don't like the term 'social aesthetics,' because it sounds 'flaky,' but it is accurate and clear—our job as sociologists is to understand how actors orient to the qualities of the social objects that confront them in organized form.



**DV: Let's turn to these 'social objects' which you see as occupying a quite central position among the entities that populate our everyday environment. You define these objects as 'a crystallization of a set of social relations' (2011: 228). What exactly do you mean by that?**

**JLM:** This has been the most difficult part of this endeavor for me. Here I would turn back to political psychology; a problem that I've always puzzled over is how we have the knowledge to allow for political action. How do we conceptually grasp what a 'political party' is? I believe that we treat it as if it were an object. Although we don't see or hear it as one, when we mentally manipulate it, I think we treat it as an object, even though it is really (as I think all sociologists would agree) a bundle of relations. So in some ways, I think we all understand that bundles of relations can appear as social objects.

Where I think it gets confusing is that while the *organization* is a set of relations connecting *members*, and that doesn't bother anyone, the *social object* as relations transcends the members, and involves, say, the enemies as well. Here is where Marx's point about valuation comes in; the fact that some basket of oranges costs \$12 doesn't have to do with the relations *among* the oranges; it has to do with the relations of the oranges to all other commodities. And yet it seems to be *in* the object. I'm pretty sure this is true for social objects, and that is why when we manipulate these social objects in our minds, they fit together so nicely; because they are reciprocally defined by their relations. But I've been unable to be much more concrete than that, and I don't think it helps anyone to smooth over the rough parts with fancy talk.

Except, perhaps, I would say this. If one wants to study political action, one needs to start with the Schmittian point that it is about friends and enemies. But who *are* our enemies, anyway? The problem with the 'knowledge' approach to social action is that it leads us to assume that actors would need to really *know* things about the objects they act upon, and when it turns out that this is implausible, we assume that they are just 'making things up,' and that their politics is a fantasy. There have been tons of cheap paperbacks written with names like 'the political imaginary' and so on, and it's an easy way to start spewing silly things. But at heart—that *is* the right approach. That is, if you accept, as I do, that political action is based on aesthetic judgment, and you believe that in sociology we are still largely within the Kantian vocabulary of action, as I do, then you have to face the fact that the faculty of the imagination is a crucial intermediary between ourselves and the world, between the senses and the intellect. We also have judgment, a second and

countervailing intermediary, one able to correct the flaws of the productive imagination (or so held most of the eighteenth century work that Kant read). I do not think that we have yet really grappled with what this means, and one of the reasons I've been reading up on certain types of mental disorder is that I think it's very tempting for those of us coming from the ecological psychological perspective to go beyond simply emphasizing the 'this-sidedness' of our sensory powers, which is all well and good. For we also *model* these sensory powers in technically inaccurate ways because we want to define out of the realm of possibility things like the hallucination.

I don't want us to immediately start declaring that other people's views of this or that are hallucinations. But pre-Kantians may have been right—perhaps there is a 'conflict of the faculties' (to take a title of Kant's out of context). The functional and harmonious *outcome* of our perceptual systems may indicate a system of checks and balances, and not a *simple* capacity to 'take in' the qualities of the environment. Pursuing that rigorously may require a bit of fancy talk. But it will also involve close psychological study, and I think we may want to hold off on the fancy talk until we need it.

**DV: To just make things a bit more tangible for our readers, could you perhaps give a classical example of a sociological 'third-person explanation', show its inherent flaws and illustrate how a closer attention to the qualitative nature of perception could produce an explanatory account that is at once more attuned to 'first-person'-perspectives without thereby sacrificing something like a 'causal' explanation?**

**JLM:** I've avoided giving really specific cases, because I figure...why make trouble? That's not a good reason, and your point is a valid one. So let me take the question of political vote choice. And I can give an example. We academics in the US often know that education 'makes' people more tolerant, and basically superior human beings in any way we can measure. And it's for that reason that the educated tend to vote left. This 'explanation' might seem such a caricature that you'll think only idiots would propose this, but it isn't so. It's a necessary conclusion of some of our ways of thinking. First, we imagine that there 'are' traits, but that we can *define* these nominalistically. So if I make a trait 'open mindedness,' I'm allowed to come up with survey questions that 'tap' this trait even without proving that it is a 'real' thing. We think, 'if it predicts variance, you're allowed to treat it as real.' Here's the problem with this: my idea of what is 'open minded,' given that I'm a professional ideological producer, is likely to be bound up with my own self-interest.

It won't be as apparent as if I counted as a point in favor of open mindedness agreeing with the item 'hard working regular folks should pay more taxes to support me and my friends,' but it might as well be.

Now that I've abstracted a social *relation* and fetishized it as a quality of an individual, my next move (as a conventional social scientist) is to investigate what *causes* this trait. I posit that education 'causes' people to be this way. If I speak loosely and don't take this very seriously, I might at worst be only misinterpreting complex comparisons. If I take it seriously, I start worrying about the fact that in the real world, people can (horror of horrors!) *choose* not to get a PhD, when really, we'd want to be able to randomly allocate years of education to people. If we pursue this problem empirically, we can often study things like the difference between graduating as opposed to not graduating high school, or attending a two year community college as opposed to transferring to a four year state college for the last two years, because there are some people who are 'on the fence' and depending on the situation (for example, whether there is funding), they might go one way as opposed to another. But we know we won't be able to compare high school drop outs to PhDs.

So now we are at the point where we can drop education in to a regression predicting vote choice as a potential 'cause.' If the estimate is significantly different from zero, we think we've proven that it is a cause. What have we learned? In the conventional account, the *best* thing we can say is that if we were somehow to kick one person out of school earlier than he wanted, and somehow get another to stay in school, their vote choices would flip. We recognize we usually *can't* say that, but we'd *like* to say that. I think that even if we *could* say this, we'd be saying something pretty dumb, and not what we really wanted to study in the first place, which was about people going about and making their *own* choices.

So I started with the 'fact' that educated people vote left. Actually, in the US, they don't. The *coefficient* for education is positive, but only when you hold income 'constant' (or pretend you do). People with more education in the US tend to vote right, and the reason is pretty clear; they make more money, and people with more money like the right, because the right tells them that it will help them keep that money away from the government. That isn't cutting edge social science, but it is worrisome when 'cutting edge' social science makes us *forget* this. But what else cutting edge social science does, by abstracting and fetishizing action and experience, is to pulverize the social relations that

*should* constitute the object of our study! Sometimes biologists do in fact grind up and centrifuge organisms. But they don't do this *first*, before they know anything about them!

Now here's what Bourdieu might call a structural anecdote. Our national level analyzes of the US show us that rich people tend to vote right, poor people left, and there is a reasonable approximation to a gradient in between these polar positions. I used to live in Madison Wisconsin, a lovely liberal utopia. 'Everyone' there was a Democrat, it seemed. On election day, if you drove around, you might notice that as the houses got smaller and cheaper, suddenly you started seeing the signs for the Republicans. Even if for some reason, all the academics vote left, even if they're in the upper third of the income distribution, why would that make the poorer people in the community *reverse* their normal ('predicted') voting patterns and vote right? *Not to be able to answer this question—easily—is to confess that we have absolutely no idea of what politics is about*, and I think we don't. How could we know something? Only by positioning the parties and the voters in some shared space that links position to qualities, qualities to experience, and experience to action. That's what a social aesthetics would be trying to do.

**DV: You once again mention the importance of returning to the phenomenological relationship between agent and world - i.e. to the *experience* of actors. Such a call is often associated with the humanistic desire to rehabilitate 'the Subject' or re-introduce the 'voice' of those we study. From what I understand, this is not entirely what you have in mind. Could you elaborate on that and indicate how an empirical social aesthetics is different from, say, an 'account of accounts' in the ethnomethodological sense?**

**JLM:** First, I absolutely think we need to understand accounting in the ethnomethodological sense if we are to be able to understand how to wade through the data we get from subjects. However, sometimes we want to do more than just talk about the accounts; we want to try to get at the cognitive components that go into the action (and not just the talk *about* the action, important though that is). Let's take the example from above ('why does person X vote for the right wing party?'). We know that if we ask him *afterwards*, we're going to get an account. But we should be able to learn *something* about how what is in his head *before* the vote is related to the act, shouldn't we? That

means, or so ESA argues, that we need to understand the organized terrain in which objects are positioned, and the qualities of these objects.

Now to be honest, I don't know what was intended when some cultural anthropologists first started talking about reintroducing the 'voice' of subjects. But at least as it entered my field, it had degenerated into analysts confusing accounts and causes. Put another way, it assumed that we could still ask some of the wrong questions ('why did you do this') and use the respondents' answers as if that ended the story; further, by insisting that the author would be true to the voice of the subjects, it required the author distort the words of her subjects to get her own point across.

Absolutely, it's terrible when we ride roughshod over what actors are trying to tell us, and replace it with what we'd like them to say, or, even more disgustingly, what we *wish* were true about them and they *couldn't* know, because of their inferiority to us. But abdicating our own undeserved epistemic authority doesn't mean we need to grant it to actors. The best thing would be if we could hook up their minds to recording devices and download their experiences, and the closer our data gathering comes to this, the better able we are to do a real social science of action.

**DV: Now that we're talking about 'data gathering', I would like to shift to the *methodological* implications of your argument. Very often, phenomenological approaches in sociology tend to be associated with so-called 'qualitative' methodologies. In ESA, you are critical of facile equations of the type '*structuralist/quantitative vs. phenomenological/qualitative*' and even argue that some of our most cherished 'qualitative' techniques - like the semi-structured interview - turn out to be quite ill-suited for retrieving the types of 'qualities' that compel social agents to action (something that dovetails with the recent work of Vaisey, 2009). Why is that?**

**JLM:** It's precisely because sociologists haven't been taking ethnomethodology and conversation analysis as seriously as they should. Interviews *can* be great for getting at the qualities that compel us to action—but we have to craft them scientifically to give respondents precisely those tasks that will leave traces of the processes that are involved in qualitative response. That means, for one, showing them examples, having them re-describe them to us, seeing which ones they remember, which dissonant features they

forget, and so on. What is essential is that we don't confuse retrospective accounts with what the mind's *main* job is—not to justify actions-taken to snoopy sociologists, but to figure out what to do *next*. I'm working right now on a book on methods, because I think these principles actually have very helpful methodological implications, ones which dovetail with the practical wisdom that many practitioners have passed on.

**DV: Somewhat counter-intuitively, you contend that a focus on the *aesthetic* nature of social cognition is compatible with the construction of formal, even quantitative models, especially those derived from a field-theoretical approach. Could you explain that further?**

**JLM:** Qualities are objective potentials; for the case of a phenomenology of action, they are potentials to induce a certain form of experience in a certain form of being (one with a particular history of cultivation in addition to its phenotypical potentials). This makes it difficult to compare one quality to another; while we can say that the quality of an iced coffee is more pleasant than that of a rat bite, we find that we run into all sorts of problems when we try to compare most qualities to one another. But experiences of a single quality can vary in *intensity*.

That in itself isn't useful, but it is if we can identify such qualitative experiences with states, places, situations, or anything else that can be treated as a *position*. Because if there is some organization to these *positions*, then we can explore the way the single degree of intensity of a single type of experience varies. The mathematical tools for this exist, and often they are related to field theory or to fluid mechanics. Why? Because if (as argued in ESA) a quality is a 'telling-us-about-what-to-do' associated with an object, in the way that the rancidness of a piece of food tells us to spit it out, then the quality is a *vector*, something that *points* in a direction, a *telos*.

For example, 'insecurity' might be a qualitative experience that (on average) varies over city blocks in a metropolitan area. If we were to hypothesize that people tended to leave insecure areas and relocate in secure ones, we actually have a straightforward model for flow of persons across positions, though we could also study it in other ways. Further, if we were to find that some sort of *action* were predictably related to the experience—for example, let us say that those in positions of insecurity are more likely to cast a vote for municipal leaders from a right party—then each of these relations can also

be seen as a vector, and that's a second way of using field theoretic principles to quantify the study of experience. I'm actually working on that with some colleagues right now.

**DV:** You've repeatedly mentioned the role of field theory. While you share an interest in 'fields' with several other contemporary authors (I am thinking in particular of the recent work by Fligstein and McAdam [2012], see also Fligstein & Vandebroek, 2014), your own approach strikes me as somewhat distinctive. In fact, while most people turn to 'fields' as a means of re-conceptualizing questions of social *structure*, you have perhaps gone furthest in drawing out its implications for our understanding of social *cognition*. Would you agree with that and could you indicate how field-theory can provide the basis for a robust theory of action and cognition? You previously mentioned the work of the *Gestalt*-theorists as crucial to such an undertaking...

**JLM:** That's quite true; only I have to acknowledge that here I am being completely derivative. The consistent field theoretic approach to action was well developed by the Gestalt theorists—Koffka, Köhler, and Lewin—long before the resuscitation of field theory by the great Pierre Bourdieu. These psychologists were focused on vision and the implicit cognition in the process of recognizing patterns in the world. They took the metaphor of a 'field of vision' seriously, because they realized that the elements in this field have a dynamic tension with one another—they were interdependent because people only make sense of the whole using the relations between the parts. Change one part, you change the relations with all the others, and so the 'whole' whole may be changed. (Of course, for this very reason, you can change all the *elements* without changing the whole, so long as the *relations* are preserved, in the way that transposing a melody to a new key doesn't change the nature of the melody.) The reason I think this approach is so important for a theory of action is that action becomes rooted in the qualities of the (phenomenological) environment; what is tortuous to explain using Weberian action theory becomes simple.

The key is that we are able to put into the *environment* a great deal of the principles of *action* on the part of those *in* the environment. The virtue here is that it is the environment that tends to come to us as sociologists well organized. We don't need to worry *quite* so much about our model of the actor, as any model that recognizes the

qualities of the environment has the same implications. Still, there are problems when there is a *dispersion* of the perceived qualities of the environment across persons, as we must expect in many cases.

**DV: One of the things that your work also highlights, is that sociologists often expound their views on the status of culture, cognition and classification in blissful ignorance of even the most basic findings of cognitive science (something that chimes with the work of Lizardo [2007, 2009]. The aforementioned ‘grid of perception’-model is one example of that. Elsewhere (Martin 2010), you have noted how sociological models of ‘culture’ often imply a complexity that seems at odds with the basic processing power of human cognition. Could you indicate how your own work is informed by cognitive research?**

**JLM:** I’m not a cognitive scientist, and I can’t say I now keep up with much in the field, other than diligently studying whatever ‘must reads’ Omar Lizardo notifies me of. And I think that’s basically very good. Cognitive science has been a quasi-field in flux, and the new neuroscientific basis means that things are shaken up again. I think it’s very important not to be overly attached to any new findings of a field whose principles are still in question. We’ve been burned too many times before by adopting the ‘flavor of the month’ in biology or psychology (think of Durkheim relying on Le Bon’s craniometry!).<sup>(1)</sup> So I try to keep an eye on incredibly robust findings: the things that haven’t gone away even after 10 or 20 years. When I wrote *ESA*, I was being influenced mostly by work in the 1990s that hadn’t been disproven. Interestingly, I believe that when you do this, you find that most of the core principles are actually going to be found in the writings of the American pragmatists at the turn of the century! If you read James’s *Psychology* you’re already around a hundred years ahead of most sociological understandings of cognition. And of course, the *Gestalt* theorists also made huge breakthroughs that were pushed to the side in mainstream psychology for a while, but are now largely accepted. I can’t overstate the importance of *Gestalt* psychology for any coherent field theoretic inquiry.

The other thing I found really important was getting a grasp of the neurology of vision; I know that most readers of *ESA* didn’t see why I thought that this history was so pivotal. But it’s common to say that, since the Greeks, our theory of knowledge has been based on our theory of vision, and a faulty theory of vision—which I find many sociologists



still have—leads to a faulty theory of knowledge. Edward Reed (1996) is so good on this, maybe it should be required reading in all theory classes. After reading that, you'll never again try to argue that human action fits nicely with our experimental vision of causality because (1) sunlight bounces off (say) a cigarette; (2) the photons squeeze their way into your eyes; (3) this triggers the optical nerve and sends impulses into the cigarette recognition element of your brain; (4) you consciously decide to take a cigarette; (5) your brain sends a signal to your hand to pick it up. (The implication is that your action was 'caused.')

This does not describe any mammals of which I am aware, and definitely not human beings. But sociologists (at least those who have never closely observed human action) who want to support our causal ontology rely on this foolishness when push comes to shove.

**DV: Let's turn once more to your intellectual sources of inspiration. You started this interview by mentioning the formative influence of Pierre Bourdieu. While you are resolutely (and impressively) 'pluralistic' in your choice of intellectual sources - which range from Dewey's pragmatism, the phenomenology of Merleau-Ponty, Gestalt-psychology and the Neokantian philosophy of Cassirer, to name but a few - a sizeable portion of your work can indeed be read as an ongoing 'dialogue' with Bourdieu's sociology. Would you agree with that and could you indicate how this dialogue has evolved throughout your work?**

**JLM:** I would say that since I really understood Bourdieu, there hasn't been much change in this dialogue. It was because of Bourdieu that I got interested in field theory, and only when I had really understood its pre-Bourdieuian roots could I truly appreciate what Bourdieu had done. I'm truly impressed at how he was able to re-create the core principles of a field theory without direct exposure to the *Gestaltists*. And then to add a fair amount, and fix problems they had—that's really an accomplishment. And recently I've even become increasingly tolerant of the formulaic nature of so much of his writing, the sing-song repetition and bombast. It isn't just that this might be necessary to get taken seriously in France, but I see it as similar to epic writing from an oral culture; yes there are big chunks of repetitive description (how many times do you need to be reminded that Achilles is 'swift footed,' or that the habitus is 'both a structuring and a structured structure?'). You listen to that stuff for an enjoyable night around the fire, and when you

don't, you learn to speed read through it. He was a great sociologist, and he took on some big empirical projects, which most theorists don't do. That takes a lot of guts.

**DV: As you yourself indicate in the book, your own attempts to clarify the relationship between 'first person'- and 'third person'-explanations shares similarities with the work of French sociologists Boltanski and Thévenot. Not only does their 'pragmatic sociology' address similar issues - the role of judgment, the relation between agents' self-understanding of their own practice and (critical) sociology's account of that practice, etc. - but they also turn to similar sources of inspiration, most notably the work of Dewey. Do you see points of convergence between their work and your own and where does your 'social aesthetics' part ways with their 'pragmatic sociology of critique'?**

**JLM:** Now I've been slowly catching up with Boltanski's more recent work (in part thanks to a flurry of translation). Absolutely, great convergence. My copy of *On Justification* had thousands of tape flags in the notes section, where I think all the great thinking is. The turn towards pragmatism, and the critique of critique, seems the right way to go. Further, his early stuff on finding your way in social space was a real gem, and showed the receptivity to a more ecological-psychological approach.

Of course, to the extent that I am just enthusiastic about his adoption of Dewey, that's just lauding Dewey, and I don't mean to discount the serious work he has put in to go beyond Dewey—including his most recent attempt to re-incorporate a kind of critique. I still come away, however, with the feeling that he is still carrying a lot of baggage he'd be better off dumping. Perhaps one could say that I am glad I don't have his problems, which seems to require that he spend an awful lot of time on things that seem to me pretty straightforward and/or uninteresting. But still, to throw out the Cartesianism of Bourdieu and replace it with pragmatism is just the right thing to do.

**DV: Another French perspective which seems to tackle similar issues - albeit in a manner that seems far less troubled with conceptual clarity than your own - is Bruno Latour's 'actor-network-theory' (see Latour, 2007). In as far as I actually understand what he is saying, Latour's criticism of sociologies that introduce an a priori-asymmetry between actor and analyst, that wield curious definitions of the**

**'social' as 'a particular kind of stuff' (2007: 43) with a distinct causal power and his call to treat 'objects' as full members of the social reality we inhabit, all seem to resonate with the arguments of ESA. Do you see such similarities between his work and your own?**

**JLM:** Latour's the one theorist where I've really made an about face. I used to hate hate hate his stuff. Although I'd still say some of the early work on science was a bit embarrassing, I've learned to love almost all of it. Because it's right. He's a true scientist, that one, and the kind of iconoclast that France is supposedly incapable of producing. And I am delighted at his frontal attack on Durkheimianism of all stripes (and there is a substantial chunk of this still in Bourdieu), and his unapologetically elevating Tarde. Regarding the ANT stuff, I would say, while I love Latour, ANT leaves me cold. Or more accurately, to steal his own phrase, I like everything about it but the 'actor,' the 'network,' and the 'theory.' It seems like one of those helpful recommendations to 'study everything, and its connection to anything else you forgot about the first time.'

Put somewhat differently, to cite Latour himself, you need a place to stand, an Archimedean point, and I don't see where that is in ANT. The general 'moves' in a science of associations are fine, but I think still on the level of a basic epistemology—there should be something *simpler* that we can do in everyday sociology. That said, his conception of how to actually study and navigate these objects is very deep. Some of his earlier work was 'overdone,' and he seemed eager to put himself forward as a very big man indeed. But in the second half of his oeuvre, he developed an understanding that it is easy to dump paradox on a page and sound smart, and very difficult to say something simple and clear about it. And that's what he's been doing. As he stopped trying to be clever, and took a humble and wondering approach to the marvel of the development of objectivity, it turned out that he was a very big man after all. I truly love that guy; a free spirit, a great mind, and a scientist. I'm hoping to take a similar path—to be straightforward and clear about very puzzling problems.

**DV:** You started this interview by indicating your background in a more conventional 'class-based' approach to the sociology of knowledge and its grounding in the work of Marx and Gouldner. Given the type of intellectual evolution you have gone through, what room is there in your social aesthetics for a

**concept like ‘social class’? More generally, how does one reconcile a focus on the *aesthetic* nature of social cognition with more ‘traditional’ sociological concerns with questions of power and domination?**

**JLM:** I think Bourdieu gives us a good general model here; he is completely uninterested in the old Marxist project of ‘drawing lines’ around classes, defining who’s in and who’s out. But his whole project turns on the lived reality of class. I think a sociology that doesn’t look at class is a bit like a biology that doesn’t notice that organisms reproduce. Follow the people, follow the relations they establish, and what you are seeing *is* class. And again, let’s see how Bourdieu used an aesthetic approach to social cognition in *Distinction* to understand the nature of class, and used a class approach to understand the nature of aesthetics. The idea of capital as a reified social relation is wonderful, so long as we don’t forget that this is what it is—reified social relations. But we tend to fall prey to just that illusion of solidity we should be studying. For this reason, I’m not that enthusiastic about the focus on ‘capital’ among Bourdieusians; it’s easy to fetishize and turn from *relations* to something *individual*. But capital, like class, is a relation. I see the basic field project as a rigorous attempt to follow through on the core idea of social science, which is that of Marx—to understand the ‘ensemble of social relations,’ as well as the ideational forms to which this gives rise. We might, formulaically, say that we need a new ‘political economy,’ not in the old sense of a nation’s economic policy decisions, but an understanding that most of the social action that we study as sociologists (though not all) is, as verb, political, and what it produces, as outcome, is class.

**DV:** In his recent ‘*On Critique*’ (2010), Luc Boltanski addressed the tension between a ‘pragmatic sociology’ that is respectful of actors’ competences and judgments, but (as a consequence) can provide little in terms of ‘emancipatory’ tools for social critique and various strands of ‘critical sociology’ which do provide such tools, but often do so by relying heavily on the type of ‘*third-person-causality*’ that you yourself criticize in the book. How does your social aesthetics deal with such a tension? Your critique of sociologies that pattern the relationship between *actor* and *analyst* on the model of psycho-analysis - or some of its loosely derived concepts (socio-analysis, misrecognition, etc.) - would suggest you are quite skeptical of ‘critical’ perspectives that propound truths about actors that are

**somehow obscured to them. Is there a ‘critical’ dimension in your approach or do you see the view of the sociologist as ‘a destroyer of myths’ (Elias, 1978: 50 ff.) as ultimately rooted in an over-estimation of the capacities of the analyst and an underestimation of the competences of the actor?**

**JLM:** Yes, I am indeed skeptical of these so-called critical dimensions. As a good Bourdieusian, I look carefully at my friends and I when we are proudly solving the world’s problems, and I can’t ignore how closely our diagnoses and solutions correspond to our objective position. Calling our approach ‘critical’ is the same as calling that of others ‘stupid.’ I think Bourdieu was still too much taken by the image of Sartre to really be consistent here. Plus, he really did come to sociology through a radicalization experience, and I think he wanted to destroy the myths about myths as a way of honoring those outside the tabular culture of the West. Your way of putting it is right, though I would emphasize that it’s less an underestimation of other actors’ competence, and more a shift to the venue of propositional quasi-logical accounting where we have a natural advantage, which we bolster through monologue. We are vastly over-rating our own competence, and perhaps a little bit underrating that of others.

Does that mean that there is nothing liberatory in social science? I don’t take that conclusion. What we are liberated from, however, is largely just error. Like a lot of people, I’m pretty confident that if everyone could only know the *real* story, they’d be sympathetic to my own political and ethical ideas. That means I don’t have to worry about being explicitly critical. The world is critical enough of our ideas.

So yes, I was really pleased that Boltanski has taken this pragmatist perspective on critique. When the French embrace pragmatism and give up the position of the ‘you pay, I preach’ intellectual, you know we’re ready to enter a true millennium of science. And I wasn’t thrilled to see him trying to figure out how to save the day for Good Old Fashioned Critique of Power in *On Critique*. I should really spend more time with this work, but my feeling now is that Boltanski is wading around too much in the folk-theory of actors (with its reification of ‘the’ institution), and stirring in more confusion in the form of the sociological discourse of ‘power’ and ‘violence’ and all those other things that get kids all excited to be sociologists. And so, despite clear thinking that produces some serious results, he’s stuck in the fundamental formal paradox that I discuss in my forthcoming book *Thinking Through Theory* (forthcoming, Norton, 2014), which is simply to accept that any whatness is ‘constraint’—*omnis determinatio est negatio*. And that means one

can always find something to rail against. So I see Boltanski as very carefully and thoughtfully opening up the same pit that sociology just climbed out of, and cautiously climbing back in, hoping that he'll have better luck than the others. If anyone will make it, it will be him, but I'd be concerned.

Now it is important to emphasize that no pragmatist could escape the need for *some* sort of critique, though this would come in the form of a community's self-revision. So as Latour—who was early on the rejection-of-critique movement—has recently realized, while we want to be able to see past the self-aggrandizing and falsely universalized claims of rationalism, we don't want to find ourselves supporting the self-aggrandizing and falsely universalized claims of *other* epistemo-authority systems (though we *do* want to support their right to exist and enter the *polis*). We may have the same enemies, but, to quote Ivan Stang, 'They are *for* braindeath. We are *against* braindeath.' But for a sociologist to think that the world needs him or her to denounce power differentials sounds like a biologist thinking that she needs to denounce mortality.

To bring this back to Bourdieu, for a long time I was confused at why Loïc Wacquant would proudly call himself a 'rationalist,' given his (and Bourdieu's) strong critique of the scholastic bias against practice in rationalism. I think that he and Bourdieu believe that the only way to make a critique is to join rationalism in asserting that there is *always* a 'true' that can be used to tear down the false productions of ideologues. And I admit that he's been doing this to great success. However, I think that we need to take the pragmatist idea seriously, which is that the true is the *outcome* of the process of self-critique of a community. Where there is political strife, there isn't much of a chance of *any* true emerging. And where people don't *want* truth, there's no way of getting it anyway. Where you and someone disagree about the social world, and she doesn't *want* to find out the truth, you can't *force* it on her. (That doesn't mean you can't force *other* things on her, like a legal system, an estate tax, or a long spell in jail.) Truth is an idea that only makes sense for a community of self-critical inquirers. Those who are outside this community don't thereby lose any rights of self-determination.

**DV: That last remark on a 'community of inquirers' brings me to my final question. In 'What is Field Theory?' (Martin, 2003) you describe what you call the 'passing crisis in Western sociology' by which you refer to the peculiar absence of any profound sense of theoretical crisis within the discipline. An absence that is all the**

**more surprising, since sociologists have perhaps never been further away from reaching a minimal consensus on foundational concepts ('society', 'actor', 'culture', etc.), appropriate methodology or even on what constitutes a good 'theory' or adequate 'explanation'. Now, more than a decade later, we are still largely content with limiting discussion to their own conceptual clubs and technical tribes. In your view, what are some of the key-factors driving this ongoing Balkanization of social theory and which steps, if any, could be undertaken to curb it?**

**JLM:** Well, I'm actually no longer sure that this was so accurate. Maybe the chance to give a little tip of the hat to Gouldner made me think I had more information about the state of the field than I really did. Perhaps I was romanticizing the period of anti-Parsonian *perestroika*. I'm not even sure if sociological theory is balkanized. Without a real historical study, it would be hard to determine what's driving the current dispersion of theoretical traditions. I would say, however, that we see certain cohort shifts, and that I was part of a cohort that had more interest in overarching theoretical allegiances than more recent ones. It might be that as a result, we're seeing less Balkanization in younger groups...in part, because everything is just a 'frame' to be slapped on some empirical results. Don't necessarily think that problems will be solved by breaking up the clubs and tribes—you can end up with everyone all having a happy, lowest common denominator, discussion. As long as we keep our theoretical explorations shallow, chains of reasoning short, and standards of empirical proof low, we can all get along.

I think the real problem here is that I don't think sociology really is enough of a serious science, in the sense that Aristotle would use '*spoudaios*'—not just in the sense of dealing with important matters, but more in the sense of doing what is inherent in its fullest potential to do. Many scientists can be motivated by trivial or selfish concerns—to get a publication, to be cited, to be famous, or whatever—but that doesn't necessarily prevent the science from flourishing, as long as the incentive structure is such that in order to reach the trivial goal, one has to contribute to the more serious one. I'm not sure that sociology is able to support such an incentive system. In the US, I see us actually preferring something that is wrong but easy to grasp over something that is right. And here I'm not talking about people rejecting the right theoretical views or anything one-sided like that. Rather, I mean that our reviewing system seems to privilege familiar and unchallenging work, most obviously in terms of statistics, where our reviewers are just

not up to snuff, but also in theory. The most arrant nonsense can be taken seriously as 'theory' in sociology so long as it is straightforward, simple and fits within our existing way of thinking.

A great learning experience came the day that I fished R. I. MacIver's *Social Causation* (1942) out of the free pile at Powell's Book Store on the corner where I live. This book is 70 years old now, and it's around 70 years ahead of the stuff that passes for theory these days. Our field has the memory of a goldfish, and the biggest problem is that this is a *bad* thing. We'd be better off, that is, if we actually were in a field in which we can forget the old folks, because we've moved on. We aren't. And the things that pass for theory in sociology now are, I think, substantially weaker in conceptualization than things that were being done a century ago. We're repeating some of these basic issues, but with seriously devolved capacities.

My best guess as to what the problem is will sound mean, but it is a self-critique, and only comes because I've been fortunate enough to be able to spend good chunks of the last number of years educating myself a bit. Problem one is that we, as a discipline, don't take the cream of the crop smarts-wise. This is no secret to folks in charge of graduate admissions. That makes doing theory a bit more difficult, though not impossible. Problem two is that the education system in the US has gone to hell in a handbasket. You can graduate college without knowing much about anything in particular, certainly lacking any background in philosophy or math, which means that you may not be prepared for serious analytic thought. I certainly didn't have the sort of education that earlier cohorts did.

I think it's completely admirable that sociological theory has been divorced from authority-mongering and the one-upsmanship of winning theoretical argument by citing more abstruse texts. But I find that we spend a lot of time spinning our wheels over questions that others have worked through tolerably well. It isn't that you need to cite Aristotle or Augustine to show you've read them—it's that we've got kids spending years doing the equivalent of rediscovering the Pythagorean theorem. It's fun when that happens, and there is some good training there, but we'd like to have some progress too. Problem three is that we've largely been schooled in theory by reading authors with a knack for the verbal flourish, like the various post-structuralists. While some of these (especially in France) were out and out frauds, most weren't. *They* had had pretty robust



philosophical educations, so they knew what obvious problems to dance around. We don't, and we fall into one pit after another.

There's a creepy movie *Memento* about a brain damaged man who is trying to avenge his wife's killing, but he can't make new memories, which means that anything he learns in the process of solving this mystery gets put on slips of paper he carries around, or tattooed on his body before he forgets. Re-reading his cryptic notes to himself, he tries to figure out where he is in his investigation, who is a friend, and who an enemy, and so on. And that's not really enough to allow him to undertake something as dangerous to others as avenging a murder. That's us. Those slips of papers are our articles, those tattoos our books. We're wandering around in an amnesiac haze trying to solve a murder mystery that's already been wrapped up.

As for the steps that can be taken to curb it...well, I think the biggest problem has to do with the way in which we think about the relation of theory and research. Theory is, as you say, something that seems more like a tribal identification than something you would really be investigating. We all basically do similar research, uncover relatively unsurprising findings...and then argue about what to *call* them. Theory has been increasingly confused with 'frame,' which is the nauseating word that blind reviewers (and I do mean blind!) use in the meddling that is now confused with peer review. A 'frame' is something fundamentally useless that is used to package a finding that, in itself, wouldn't really command the attention of any reasonable person. It's less a frame and more like a sauce, used to disguise the flavor of rotten food. Lévi-Strauss didn't just slap a lingo on everyday fieldwork—his theory suggested that you had to embark on a huge, difficult, and innovative way of marshaling data. But the idea that a theory would be something that you'd really be working hard to investigate, that it would direct your attention to scientifically crucial places...that seems to have disappeared.

**DV: Well, we can only hope that *'The Explanation of Social Action'* will be able to change that. Prof. Martin, thank you very much for this interview!**

### 3. Notes

1: For an insightful review of Durkheim's fleeting love-affair with craniometry and other forms of proto-sociobiology see Llobera (1996).

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