Mirror Neurons and Practices: A Response to Lizardo


Stephen P. Turner
Department of Philosophy
University of South Florida

Lizardo argues that The Social Theory of Practices is refuted by the discovery of mirror neurons. The book argues that the kind of sameness of tacit mental content assumed by practice theorists such as Bourdieu is fictional, because there is no actual process by which the same mental content can be transmitted. Mirror neurons, Lizardo claims, provide such a mechanism, as they imply that bodily automatisms, which can be understood as the basis of habitus and concepts, can be shared and copied from one person to another. This response to Lizardo points out that the Gallese arguments on which Lizardo relies relate to phylogenetic and universal body movements, not to the learned movements characteristic of practices, and that there is no sameness producing mechanism parallel to the genetic one.

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Omar Lizardo has performed a useful service by opening up a question that deserves discussion, the relation of social theory to cognitive science in a category of inquiry that deserves to be enormously expanded, as I have argued elsewhere (Turner, 2007a; 2007b; 2007e). Lizardo, however, is wrong about mirror neurons and about what they imply for Bourdieu, and misleading about the implications of the contents of The Social Theory of Practices (1994) and my other writings in this area. Mirror neurons, to the extent that they figure in an explanation of the surface or external similarities between people engaged in a ‘practice’, actually undermine Bourdieu’s account, by providing an alternative to the mechanism his account requires. The alternative is both inconsistent with his account and solves problems with the alternative account, as I will explain. Mirror neurons, however, fit very nicely into the account given in The Social Theory of Practices, which, like the mirror neuron literature, focuses on emulation (Tomasella and Carpentier, 2005), and also fit with my later discussion of the implications of the simulation approach to the problem of knowledge of other minds, ‘Imitation or the Internalization of Norms’ (2000; 2002, 58-73).

Lizardo is forced to deny the obvious affinities between The Social Theory of Practices and this literature in his paper, and to pretend that it has something to do with Bourdieu. The mountain he needs to climb is a high one: more than a tenth of the pages in the book are indexed to the concept of emulation and it is explicitly endorsed as a mechanism that can do the work of transmission of practices. In later work, emulation continues to be mentioned, and simulation is discussed directly in a substantial paper (Turner, 2000; 2002, 58-73). In contrast, the terms and their like are not, as far as I know, to be found in Bourdieu. Lizardo, in any event, fails to cite a single instance of their use, and endorses the conflicting language of ‘socialization’. So he must
find a way to discern concepts alien to Bourdieu in Bourdieu’s texts, and to represent The Social Theory of Practices in a way that ignores a large part of its content. Arguing about Lizardo’s interpretation of this will not be my concern here. However, the discussion represents an opportunity to clarify some issues that are important, and have not been addressed in terms of social theory, and should be.

The absence of social theory from the cognitive revolution is a topic of its own. It is an unfortunate historical accident that postmodernism, which operates with an underlying neo-Kantian conception of mind that serves as a surrogate for a concept of mind that is engaged with neuroscience, flourished at the same time that cognitive science emerged as a field. The result was a mutual lack of engagement that has disserved both. The last time that social theory was seriously engaged with the topic of the physical brain and its psychological implications was a century ago, and there is a sense in which we are now simply resuming the conversation where it left off (cf. Turner, 2007b, 364-66). As it happens, the place it left off is directly relevant to Lizardo’s claims about Bourdieu and about my book The Social Theory of Practices. So I will begin with a short historical excursus.

The earliest uses of the term social psychology were by Gabriel Tarde and Charles Ellwood. The term was explicitly understood at the time to contrast to the notion of ‘collective psychology’ or in its German form Volkerpsychologie. Ellwood’s ‘Prologemena’ (1899), the first serious overview of the field, was promptly denounced in Durkheim’s Année Sociologique with the comment that he should have instead concerned himself with ‘collective psychology’, which was the preferred Durkheimian notion (Durkheim, 1900; cf. Nandan, 1980, 63, 65-6). The difference was far more than a matter of semantics. Durkheim thought there was something collective that was real, causal, and produced and interacted with individual psychological facts, such as dispositions. Ellwood, agreeing with Tarde in this respect, but facing the issues in a different way, considered collective usages to be metaphorical, non-explanatory, and unnecessary to explain social life.

The issue with Bourdieu is a variation, and not a large variation, on this core issue. Bourdieu, to simplify only slightly, adheres to a form of collective psychology. For him, individual dispositions and strategies have, to a significant extent, a collective source, which is itself dispositional and strategic. This is a fancy dress form of the idea of ‘group will’, already present in Ludwig Gumplowicz in 1883, who ‘depicted individual values as mere reflexes of social milieu; ethics were the code of the hegemonic classes, and the heroes of history were “only the marionettes who carry out the will of the group”’ (quoted in Crook, 1994, 32). Bourdieu is more elaborate in his formulations of this core idea, and in particular allows more scope for human agency, but logical structure of the argument is the same. It involves a dualism between the collective and the individual:

Social reality exists, so to speak, twice, in things and in minds, in fields and in habitus, outside and inside of agents, And when habitus encounters a social world of which it is a product, it finds itself ‘as fish in water’, it does not feel the weight of the water and takes the world about itself for granted . . . The structuring affinity of habituses belonging to the same class is capable of generating practices that are convergent and objectively orchestrated outside of any collective ‘conspiracy’ or consciousness. In this fashion it explains many of those phenomenon of quasi-teleology which can be observed in the
social world. (Bourdieu in Wacquant 1996, 216).

In short, the structuring affinity of habituses generate practices that produce a quasi-teleology that has the effects of a conspiracy or of collective consciousness without being either of these things, and which have a reality both outside and inside of agents to which the agent responds as fish to water, taking it for granted and thus being controlled by it. It is thus the surrogate for the group will, which does the same thing, namely to pursue ends, for example the end of the continuing domination of a class, by directing individual behavior, through habitus, to produce the ends. In this respect Bourdieu is ‘sociological’ in a familiar, functionalist, sense.

In The Social Theory of Practices, which was not directly concerned with Bourdieu but with a form of reasoning about collective objects (1994, 100-116), I distinguished two versions of the idea that there was some sort of collective source of individual mental contents. One involved the idea of shared presuppositions (and its endless variations, such as Fleck’s idea of a Denkgemeinschaft). The other involved embodied knowledge, skills, tacit knowledge, and the like. In a later response to my Bourdieuvian critics (especially Gross, 1998), I constructed a simple 2x2 classification of ideas about the tacit, in which I contrasted individual and social (in the sense of collective) forms of the two families of ideas: the skills family and the presuppositions family (2002, 9).

My argument was not against the tacit as such, but against the collective forms of the idea. Skills and cognitive machinery (such as what I unfortunately called, using the language of Dewey and Hume, habits) that were not collective were not only accepted as a necessary part of the explanation of the sorts of things that collective objects were held to explain, they were sufficient for the purpose, if the explanatory purpose was properly understood. Thus notions like tradition did not go to the bone yard, but could be salvaged by reinterpreting them as something other than the ontologized units I called ‘collective objects’.

The argument itself concerned what I called ‘transmission’. The problematic feature of all ‘collective object’ conceptions was that they required some sort of means of getting from the collective object into the individual that preserved the sameness or unitary identity of the collective object– of what, as Bourdieu says, is taken for granted. To share a presupposition, for example, is to share the same presupposition. But what means of acquiring a presupposition guarantees sameness? There is none, I argued. We might have the same external results, such as a belief in God, but arrived at it on the basis of different ‘presuppositions’. As with the underdetermination of theory by data, where the data cannot guarantee a unique result, the transcendental arguments used in these contexts also could not guarantee a unique result. In the case of tacit knowledge, people get different data, so their individual tacit stuff was not going to be the same anyway, at least in its fine details. Learning history matters, at the cognitive level, because something learned in one way or in one order can produce the same overt behavior as something learned in another way. But this difference does not mean that people cannot communicate, interact, and so forth.

The implications of this argument for social theory were identical to those of Ellwood’s social psychology a century ago. As Ellwood put it, ‘as individual psychology teaches, no two psychical coordinations [a Dewey-Mead term from the 1890s] can ever be exactly alike’ (1899, 663). The message here is very clear. What we acquire through interaction with others is subject to the individual variation in response that occurs in interaction. This is a social conception.
without being a collective one. The ‘social’, for Ellwood, refers to the social process of interaction itself (1901, 741), not to a special realm or substance, such as the ‘water’ to which Bourdieu refers.

Ellwood was a great user of ‘inter’ terms, including not only interaction, but intercerebral and interleaming, among others, to characterize the social process. The resemblance to symbolic interactionism is not accidental. Ellwood was the teacher of Blumer. The difference between these terms and Bourdieu’s language can be seen in the term intersubjective agreement, which is commonly used as an alternative to notions of ‘objectivity’. Intersubjective agreement is functional coordination, to use the Dewey-Mead term, between individual subjectivities. It does the explanatory work that appeals to objectivity do without committing us to the metaphysical implications of the language of objectivity. Applied to practice, these ‘social process’ and ‘inter’ conceptions mean that practices can be no more than that which is sustained through interaction subject to individual variation, and thus practices are not facts outside of individual minds.

For collective object thinkers, something unitary— the group will or its analogue (in Bourdieu’s case practices which are quasi-teleological)— needs to be shared between members of the group (the water in which the fish swim) in order to explain something that is manifested in individual action, feeling, and thinking. And there is a conflict between the idea of a shared, unitary, same thing and the psychological reality of individual variation in the transmission of content between individuals. Bourdieu, with his dualism of individual fish and collective water, is a paradigmatic collective object thinker, despite his many evasive comments about whether the reality in question is subjective or objective. So The Social Theory of Practices could be read, correctly, as a text against Bourdieu.

Bourdieu’s Collectivism

Bourdieu’s main argument has two parts: that practices are a collective, dispositional, and strategic source of individual habitus, which are in turn the source of individual dispositions and strategies. The causal relation is bi-directional: up from the individual through affinities of habitus to the practice, and down from the practice to the individual. To be quasi-teleological, the collective part needs to have some sort of feedback mechanism, as Elster pointed out, from the world to the collective thing, presumably via the individuals (1983, 70). This is a crucial problem for Bourdieu’s (much criticized) larger social theoretical claims, because he wants to account for practices as part of the struggle to reproduce power and domination, that is to say the strategic properties of classes, among other groupings. This might be called the uploading problem. The Social Theory of Practices was concerned, as Lizardo says, with the problem I called downloading: the problem of how something collective gets into individual heads (which is also to say bodies) to produce individual actions. There has to be such a process for Bourdieu, for the simple reason that the quasi-teleology doesn’t work without it. Actions are the actions of individuals carrying out the quasi-ends and must be informed in some manner by the quasi-teleology, which is a collective fact.

Why is this such an important issue? Begin with the relation between these collective things, with their strategies for the reproduction of distinction, domination, and so forth—Bourdieu’s surrogate for the idea of the class determination of consciousness. Here the problem I referred to as the downloading problem is simple: how does the consciousness of the class itself
get into the heads of the individual members of the class? How does something collective get into the heads of individuals, and in such a way that the things in the individual were in some sense the same? No one, including Lizardo, claims that the discovery of mirror neurons relates to this problem. Mirror neurons are means by which individuals relate their mental contents to those of other individuals. They have to do with individual-individual relations, not collective-individual relations.

So, with respect to the crucial issue that *The Social Theory of Practices* raised with Bourdieu, mirror neurons are irrelevant. Mirror neurons might be considered to help with the problem of convergences of *habitus*, or what I called sameness. They have nothing to do with the crucial claim that practices are themselves strategic or quasi-teleological. Lizardo implicitly concedes this, or at least does not attempt to defend anything beyond convergence produced through individuals imitating other individuals. But when one considers the point of the whole Bourdieu enterprise, which is to provide a surrogate for the Marxian theory of the Überbau by replacing the base-superstructure distinction with the field/habitus distinction, saving the convergence of *habitus* part for Bourdieu is like a Marxist saving the superstructure part of Marxism. Explaining social life by reference to superstructural elements is just explaining in terms of commonly held ideas and institutions. There is no room for the Marxian teleology in such explanations: ideas develop and change for reasons other than some underlying cause. Practice explanations that refer only to the supposed commonality, sharedness, or sameness of practices, similarly, do not help with quasi-teleologies. There is no room for Bourdieu’s quasi-teleology of the reproduction of domination (which is only ‘quasi’, presumably, because Bourdieu has abandoned the full teleology of the Marxian conception of history). Acknowledging this amounts to narrowing the difference between us to an issue that is, from the point of view of ‘grand narrative’ social theory, negligible and without interest: sameness as such.

Changing the Subject: What Conception of Practices Are We Considering?

Lizardo argues that the discovery of mirror neurons shows that my objections to sameness or sharing were wrong, and that the discovery vindicates Bourdieu against my critique. A few things need to be made clear about this. As we have seen, sameness is an implicit but necessary claim for Bourdieu, necessary because of the role it plays in linking levels. He does not need to say that individual *habitus*es are in fact identical, just that there is some core or underlying disposition producing overt behavior that is shared, because he needs this shared thing to have the quasi-teleological force that distinguishes domination from stratification that results from the aggregation of individual choices, talents, and dispositions. So there is something of a mystery about what it is exactly that is shared, just as there is a mystery about what *habitus*es are. Lizardo doesn’t solve any of these mysteries, and his claims about them and about Bourdieu himself, as we will see, are contradictory. But he nevertheless claims that mirror neurons are a mode of transmission that allows for the transmission of dispositions and other embodied content in the sense that Bourdieu’s account requires. What mirror neurons do, Lizardo thinks, is to provide direct access to, and to reproduce, in the mind, the embodied dispositions of others, in such a way as to guarantee sameness of content, and that this sameness extends to such things as concepts.
In contrast, I think the mirror neuron literature is crystal clear: that mirror neurons facilitate learning, but are part of the processes of learning in which the individualized history of learning is relevant and variation in individual mental content occurs normally. So the significance of mirror neurons is that they are just another nail in the coffin of Bourdieu’s conception of practices, because they provide a mechanism that supplements and makes more powerful the processes of social learning, which I argued were already sufficient to account for practices. Collectivist practice theory has always traded on the surface or external consistency of action in groups, and claimed that only collective object or collective psychology explanations can account for them. I argued that this claim required downloading from a collective object—a form of transmission that is cognitively implausible.

Here is the conflict: I say learning histories are relevant and individualized; Lizardo says they are not, and that mirror neurons allow the transfer of content of the kind that practice theory has traditionally been concerned with—an important qualification—unmediated by learning history. This is a genuine disagreement which is worth discussing. There are, however, some other issues which need to be clarified before considering this one. Lizardo also thinks that we have a disagreement about the role of explicit teaching. I think this is based on a misrepresentation of my claims and ungrounded in anything I have written, so I will deal with it only in passing. I would prefer to minimize quibbling discussions of the texts—readers can trace his few direct citations for themselves. Lizardo has a point of greater potential scholarly interest, when he argues that The Social Theory of Practices criticizes the wrong kind of practice theory: the presuppositional rather than the embodied kind. This is a more serious misunderstanding. Lizardo says that ‘Practice theory is not synonymous—contra Turner—with explanations of action by way of “paradigms” and hidden frameworks, as long as these last are still conceptualized on the older “cognitivist” language of classical social theory (i.e. Durkheim, 1995[1912], 214)’. Lizardo then suggests that because Bourdieu has an embodied rather than a cognitivist conception of practices he is freed from my criticism of ‘shared premises’ explanations.

The Social Theory of Practices deals both with ‘premises’ and ‘habitus’ reasoning, and makes constant reference to skills and other embodied ‘knowledge’ while making clear that the use of the term knowledge here is metaphorical and problematic. Moreover, the book argues for an ‘embodied’, causal, explanation of practices. Habits are embodied and causal in character. This is a point that has been central to the philosophical critics of the book, who have repeatedly objected to precisely this feature of the argument, on the grounds that practices is a normative rather than a casual concept (Rouse 2002; 2007; cf. Brandom, 1994). My responses to this argument have formed most of my subsequent writing on the topic of practices (1998 [included in 2002, pp.118-44]; 2003; 2005a; 2007c; 2007d).

Bourdieu, however, is another matter. Does he even have an embodied conception of practices? Consider his classic statement of the nature of practices in Outline of a Theory of Practice:

The language of rules and models, which seems tolerable when applied to ‘alien’ practices, ceases to convince as soon as one considers the practical mastery of the symbolism of social interaction—tact, dexterity, or savoir-faire—presupposed by the most everyday games of sociability and accompanied by the application of a spontaneous
This is quite a mouthful, and explains why I didn’t bother to discuss Bourdieu directly in *The Social Theory of Practices*. There is far too much going on here to produce a coherent notion of practice. But, it should be evident that there is plenty of cognitive language here—‘information’, ‘decoding’, ‘precepts’, ‘formulae’, and ‘codified cues’ and of course ‘games’. So Bourdieu does not have a consistently ‘embodied’ non-cognitive conception of practices, restricted to ‘bodily automatisms’ as Lizardo implies. Nor does he abjure the idea of rules—games have rules.

The very luxuriance of this list causes problems for Bourdieu with respect to transmission. For a ‘mass of precepts, formulae, and codified cues’ to be the same in multiple heads, one would need a mechanism by which this sort of thing is reproduced exactly in these various heads. For the things in each *habitus* enabling codified cues to be continuously decoded *in the same way* one would have to have the equivalent of identical secret decoder rings in the brain. And these secret decoder rings would have to be specific to the practice, shared by everyone who engages in the practice, and part of its transmission, not a generalized brain capacity. This is why I called this downloading. The only analogue I could think of to getting all this rule and rule-like material in the same way from one (collective) source to many individuals is the downloading of computer programs from a common source.

What does Lizardo do in the face of the long list of things Bourdieu puts under the heading of *habitus*? He changes the subject. He never says exactly what is left in and what is left out. He seems to want to limit it to bodily automatisms plus the concepts that arise in connection with them. This is no longer the original Bourdieu, but this switch is crucial to Lizardo’s argument. His defense of Bourdieu consists of throwing a large and unspecified part of the cargo overboard. First, as we have seen, the basic point of Bourdieu’s whole project, the quasi-teleological character of practices as machines of domination, gets tossed. Now the bulk of Bourdieu’s notion of practices goes with it. So we are no longer looking at a defense of Bourdieu. We are looking at a defense of something quite different and possibly much more limited—limited to the sorts of things that can be transmitted with the aid of the mechanism of mirror neurons. Ironically, this quite different and much more limited thing turns out to look a lot like the alternative, ‘habits’, picture of practices developed in *The Social Theory of Practices*, because it also, and more consistently, dispenses with cognitive language. The remaining difference is over ‘sameness’.

**Sameness**

Lizardo argues, correctly, that my issue with the standard collective object accounts rests on the problem of sameness of mental content, that I would have no objection to a mechanism that went
from one person to another, and that my objection to the sameness claim was that all the
supposed mechanisms for assuring sameness were implausible. He then provides what he takes
to be a mechanism that avoids all my criticisms. The mechanism, he says, possesses ‘precisely
the neuro-cognitive capacity that according to Turner (1994) belonged in the realm of high-
speculation and/or logical impossibility and incoherence’, namely that it assures sameness of
content.

Lizardo interprets my discussion to mean that I suppose (ex cathedra) that “‘tacit
presuppositions” cannot be transmitted via the practical imitation of conduct, and ... propose that
it is only by the re-translation of practical presuppositions into “public language” of explicit
instruction that a plausible account of the transmission of the “hidden objects” of practices can
be constructed’. This is simply false: my examples, including the ‘American walk’ discussed by
Mauss, which forms a large part of the discussion, are not restricted to cases of explicit
instruction, nor is this stressed. The term I consistently use in the text is emulation, a term which
does not imply explicit instruction, though of course explicit coaching might help someone
emulate. Emulation does, however, operate through ordinary epistemic channels: one can
emulate only what one can see, hear, and so forth. Lizardo then goes on to make the more
interesting claims that my argument ‘presupposes both the conceptual informational sterility of
overt conduct, and thus the strict separation of conceptual content from practical action (patterns
of motor activity) and the related inability of the human neurocognitive system to gather and
process the implicit conceptual information encoded in patterns of overt behavior’. I’ll accept
the first of the two things ‘presupposed’, but the second one is so loaded and potentially
misleading that it needs explanation.

The picture here is this: I say that overt behavior is conceptually ‘sterile’; Lizardo says
that something like conceptual content— not just overt behavior— is transmitted from person to
person in a medium other than overt behavior, with the aid of mirror neurons. There are two
elements to this conflict: the idea of transmission and the idea of concepts and conceptual
content. Conceptual content raises a large set of issues that I have, as noted earlier, discussed
elsewhere, but for present purposes I would be happy to accept the notion of concepts in the
Gallese and Lakoff paper (2005) on which he relies— unlike most of the current philosophical
community— and dispense with the normative one. The problem of transmission, however, needs
to be addressed before explaining this. My response to Lizardo’s idea that conceptual content is
transmitted in some fashion other than through overt behavior is that he has a wrong
understanding of what goes on in the kind of imitation and emulation that is involved in mirror
neurons. Moreover, I will suggest, the right understanding fits with my points about emulation:
the same content is not directly transmitted, as he imagines, but mirrored, that is to say simulated
from within the mind of the person doing the mirroring. To understand this requires a brief
discussion of mirror neurons.

Mirror neurons activate both in the performance of actions and in the perception of
actions. The dual-use character of this system suggests that ‘we recognize someone else’s action
because we manage to activate our own inner action representations using mirror neurons’
(Keysers et al, 2003, 634). How do we ‘manage’ this? According to a standard view of the
matter, ‘converging lines of evidence strongly suggest that our keen ability to perceive the
actions of other people results in part, from the massive experience we have accumulated over
the years in planning and executing self-produced activities’ (Blake and Shiffar, 2007, 56). The
interesting implication of this is that this capacity to perceive actions is to a great extent learned and produced, not from something like our predictive experience with other people as such, but from our own experiences of acting. There is a question, indeed, as to whether the connection between perception and action on which the mirror neuron system depends is itself learned (Hurley and Chater, 2005, 11; Meltzoff, 2005).

This capacity is also related to imitation and imagination. It has long been known that certain brain lesions cause individuals to imitate the bodily movements of others. This suggests—though this is still a matter of hypothesis—that the action-perception system works normally in such a way that when the system is activated by the perception of an action, the system prepares to imitate, but the imitative action is inhibited by another mechanism such that the same neurons for action are activated but without action occurring (Gallese and Goldman 1998, 499). This means of inhibition or taking the system off-line is hypothesized to be the one that allows for imagination of actions and thus simulation, understood as a brain process in which the same neurons for action (which are also those activated by perception) are activated without action taking place. Simulation is thought to be an off-line process involving the same neurons as those involved in perceiving an action of another person as ‘minded’ or intentional which thus becomes the means by which we ‘mind-read’.

Learning, and thus learning histories, are an important part of the experimental work on the activation of the perception-action system. Much of this work has involved the difference in perception that results from people’s ability to perform certain actions: dancers can see things about dance moves that other people can’t see (Cross et al, 2006), male and female ballet performers see the typical gender specific dance moves of their own gender better (Calvo-Merino et al, 2006, 1907). There is also research on the visual cues that allow motions and emotions to be identified. What this research suggests is that the cues needed by the system are very modest, and that the preconscious work of perceiving is done largely by the percever constructing perceptions of whole actions on the basis of very limited inputs. Even a very small set of visual cues, for example, allows someone to identify motions and emotions of certain kinds (Tomasello and Carpentier, 2005, 141; Loula et al, 2005; Blake and Shiffrar, 2007).

The typical problems of distinguishing what is learned from what is given as a starting point in the system arise for mirror neurons as well. Some think the whole of mirroring is learned; others think that some basics are given. If they were, they would be useful as a way to account for getting learning off the ground. But disentangling the two developmentally is tough. Consider the smile of a baby. Babies mimic the smiles of parents at a very young age (Meltzhoff and Moore, 1977; 1989). This would be a good start to a process of interactive learning. But of course it is also the subject of a great deal of interactive learning, in which babies get tremendous amounts of feedback in interactions involving smiling, smiling back, recognizing smiles, and so forth. The problem with the idea that everything in the system is learned in this way is the one raised by Lizardo about connectionism: can this mechanism, which depends on massive inputs, account for everything it needs to, or is it too slow and laborious to do so.

Understanding other people is more complicated yet. Mirror neurons have been recruited to support simulation theory. Simulations off-line are like hypotheses. They get tested by the results of one’s actions made on the basis of the simulations or by the fulfillment or non-fulfillment of one’s expectations produced by the simulation. Once the associations are established, we can simulate the feedback process itself. Consider the classic example of
feedback: the thermostat. If we can tell that the level of heat that would switch off the thermostat is about to be reached, based on our simulation of the process, we can avoid overshoting the mark and waiting for the actual feedback system to kick in (Hurley, 2007; Gallese, 2000, 29).

We can’t just ‘read’ off the goals of others, but we can ‘construct’ them (preconsciously) Gallese and Metzinger (2003, 385), by a kind of retrodiction of the target’s mental state, as Gallese and Goldman put it (1998, 497). We construct these retrodictions from our own capacity to simulate, which we apply to what we observe, namely overt behavior, which our simulations enable us to make sense of, or code. Learning the difference between a parental smile of approval and the bogus smile of a salesman or beauty queen is a much more sophisticated achievement, based on a lot of experience that has been assimilated. Mirror neurons are not that sophisticated, but, as Gallese and Goldman suggest, they might ‘represent a primitive version, or possibly a precursor in phylogeny, of a simulation heuristic that might underlie mind-reading’ (1998, 498).

Nothing in this story so far involves anything passing from one embodied mind to another without going through the senses, nor carbon copies of action-perception capacities passing from one mind to another. Sameness, at least with respect to things that are acquired through simulation, is not a reasonable expectation. There is plenty of room for error here, and indeed in the experiments on bodily movement imitations there is plenty of error as well, and also plenty of scope for unconscious learning that corrects errors— even by very young infants (Meltzoff and Moore 1997, 185–7). Nothing in this account conflicts with nor excludes the possibility of imitation of the kind exemplified in brain-damaged subjects for whom the inhibitions to the discharge of action do not work (Gallese and Goldman, 1998, 499). What they imitate is nevertheless determined by what they can perceive as imitable, and if this is not given universally as a starting point, it is a matter of learning, with an individual learning history. So even this imitation is not carbon copying.4

Sharing Basic Neural Formats: The Role of the Universally Given

So what about the idea of what is universally given? Lizardo takes comfort from some quotations from Gallese about the manifold— a Kantian term— and the idea that ‘sameness of content’ is shared with different organisms as a result of ‘modeling the observed behavior as an action with the help of a matching equivalence between what is observed and heard and what is executed’ (Gallese, 2002, 175). The main thing that is shared is the networks of perception and action, which allows for ‘detection’ through unconscious simulation, and this is also the basis for imagining. The question of what is shared with other organisms is a separate question. If what is shared is the Kantian manifold, i.e. some universal set of starting points, such as the act of grasping, this is an argument to the effect that underlying semantic content is a set of given starting points that are shared. But this reasoning doesn’t help with practices, which are by definition not universal, or with concepts, which is a more salient issue for this literature, with similar implications.

Consider the following account of concepts in relation to the problem of learning history: ‘Concepts are not given to us by the world, but are products of our attempts, as a species (with a phylogenetic past) and as individuals (with a personal past), to make sense of our worlds’. This is in accordance with my view of the significance of learning histories. But these are the words of
Mark Turner (1991, 152), a collaborator of Lakoff, and one of the major sources of the body of ideas that Lizardo relies on to refute *The Social Theory of Practices*. This quotation sharply distinguishes two things— the universal and the individual. What interests Gallese and Lakoff is something relevant to this distinction: the claim that abstract reasoning and conceptual reasoning necessarily employs the sensory-motor system. They refer to the structuring circuits used in this system as ‘cogs’ (2005, 473) and to the perspective as a whole as ‘structured connectionism’. It is specifically addressed to the Kantian or universal problem of what is given:

From the structured connectionism perspective, the inferential structure of concepts is a consequence of the network structure of the brain and its organisation in terms of functional clusters. This brain organisation is, in turn, a consequence of our evolutionary history— of the way in which our brains, and the brains of our evolutionary ancestors, have been shaped by bodily interactions in the world. (2005, 468)

The idea— very speculative, as they note— is that abstract thought itself employs this same universally human inferential structure and thus this same functionally clustered network structure of the brain. We can think abstractly, in other words, because we can rely on this pre-given organization. This is an answer to the following problem:

From the perspective of neural computation, a human brain consists of a very large number of neurons connected up in specific ways with certain computational properties. How is it possible to get the details of human concepts, the forms of human reason, and the range of human languages out of a lot of neurons connected up as they are in our brains? How do you get thought and language out of neurons? (Lakoff, 1999)

Structured connectionism, his approach, ‘allows us to construct detailed computational neural models of conceptual and linguistic structures and the learning of such structures’ (Lakoff, 1999). This is, in a sense, a response to Lizardo’s worry that connectionist learning and interaction can’t do the job. The way it helps is by giving learning a kind of head start if certain concepts (or a kind of neural organization that amounts to a kind of near-concept) are given phylogenetically rather than learned, they can be a means of a kind of rudimentary human sharing, and they might continue to be used in more advanced forms of thought. But this is an appeal to human universals, not to a mechanism of transmission of the kind required by Bourdieu.

There are two elements here: learning, which is still connectionist in this model, and structure, which is phylogenetic. The sharing here is at the phylogenetic level; the rest of it is learned, and thus subject to the facts of individual learning history. The possibility of learning in such a way that we understand one another may be facilitated by the phylogenetically shared common structural elements. But, as we already know from the studies of dancers, there is plenty that is learned that is not universally shared. Nothing in this discussion tells us anything to the effect that there is any kind of collective fact that is not phylogenetic that is shared in some sort of novel way, not subject to the vicissitudes and variations of learning, and indeed of embodiment itself— bodies themselves vary.

Gallese takes a pass at some very traditional problems of social theory as a way of criticizing the monadic focus of much cognitive science on individual cognition. But he does so
not by postulating some sort of collective mechanism, nor even by making the kind of assertion of identity that Lizardo makes, but rather by locating group identity in the economy of the mind: Identity is important within a group of social individuals because it provides them with the capacity to better predict the consequences of the ongoing and future behaviour of others. The attribution of identity status to other individuals automatically contextualizes their behaviour. This, in turn, reduces the variables to be computed, thus optimizing the employment of cognitive resources by reducing the 'meaning space' to be mapped. By contextualizing content, identity reduces the amount of information our brain has to process. (2003, 518)

To be clear, he does speak, loosely, about sharing of political perspectives and other forms of empathic sharing. But the only mechanism he has in mind for this is the following:

What is common between a neonate who replies to his mother sticking out her tongue with an equivalent behaviour, and the skilled repetition by an adolescent of the piano chords as demonstrated by the piano teacher? Both instances of imitative behaviour are made possible only by the capacity to solve the computational difficulties inherent in any type of interpersonal mapping, due to the different perspectives of demonstrator and imitator ... If I want to reproduce the behaviour of someone else, no matter how complex it is, or whether I understand it or not, I always need to translate my external perspective of the demonstrator into my own personal body perspective. This problem can, however, be overcome if both the actions of the demonstrator and of the imitator share a basic neural format. (2003, 519)

So– the common mental stuff that makes the computational problem soluble in real time is the basic neural format, which is to say, what is phylogenetically shared can be used as a computational starting point that can be attributed to the other party as one imitates, in this case, their action, because it is already phylogenetically shared. Everything else that is shared has to get there the old fashioned way– it has to be learned, and in the case of things that make up interpersonal relations, learned through our use of our own bodies as a model, or through explicit teaching with feedback, or through unconscious feedback. And it works on overt behavior: what Gallese calls ‘my external perspective of the demonstrator, which I need to translate into my own body perspective (2003, 519, emphasis supplied).

What do these claims have to do with The Social Theory of Practices and the follow-up papers of mine that Lizardo cites? The book is specifically not about the Kantian or ‘universal mental content’ problematic. It is concerned, in its discussion of shared presuppositions, with the neo-Kantian form of the problem: with the idea that a particular epoch, society, or discipline shares presuppositions. The Kantian form of the problem can be given, as it is in these texts, a phylogenetic solution. Sharing in this case is a result of a mechanism that actually does copy, namely genes. What mechanism does The Social Theory of Practices appeal to to solve the neo-Kantian problem? Emulation. In short, The Social Theory of Practices appeals to the same kind of mechanism that the mirror neuron literature does, when it is concerned with non-universal mental contents, such as practices.
Lizardo’s Alternative Mechanism: Does it Exist?

Lizardo’s critique comes down to the following: *The Social Theory of Practices* says that learning histories matter, and that they individuate the mental contents of individuals; Bourdieu’s theory of practice denies this; mirror neurons enable mental contents to be widely shared without individuation, so Bourdieu is right and *The Social Theory of Practices* is wrong. In fact the mirror neuron literature makes learning history central and establishes nothing like the copying relation Lizardo claims to find there. This is, indeed, the central implication of the core fact of mirror neurons, that the networks activated by perception and action are the same: ‘that what we see depends, in part, on what we can do’ (Blake and Shiffrar, 2007, 63). Consider the dancers—or a more mundane example, such as Tiger Woods’ golf swing. A professional can see things about that swing that an ordinary person cannot. The process of learning to swing a golf club correctly begins not with perceiving the swing correctly, but with perceiving it crudely, and gradually improving, through a stepwise process, both one’s perceptual capacity and one’s capacity for action (Hodges et al, 2006, 480). Learning history is essential: without having learned enough one cannot even see the details of the swing sufficiently to be coached to correct one’s errors. Learning is slow, because one can see only a bit beyond what one can do (Ericsson, 2006, 694). In this case coaching, or explicit instruction, is essential to achieving at a high level: professional athletes are constantly coached. But proceduralizing, or encouraging conscious attention to an aspect of performance, typically causes performance to decline. And this is characteristic of expert performances. Explicit discussion, however, is part of ordinary practices of all kinds: people gossip, appraise, advise, and so forth. ‘Chess masters do not just play a lot of chess’, as a paper in the experts literature puts it, ‘they read a lot of the chess literature’ (Hunt, 2006, 31).

Where did Lizardo’s critique go wrong? Lizardo has fatally mixed up two things: the Kantian problem of universal conditions of understanding, and the neo-Kantian problem of understanding within a social group. We may put this in terms of the research examples. The Kantian problem is dealt with in the discussion of physical actions like grasping and smiling that are universal, and which, if we take mirroring responses to them to be rooted in the phylogenetically given features of the neural system, can be the basis of interaction. The neo-Kantian problem, however, is the one that corresponds to practices. Recognizing and distinguishing dance moves is a matter of performance, and therefore training: one cannot perceive correctly without the training. And one can of course be trained in different ways to produce the same performance. So different learning histories can produce the same overt result, as I argued in *The Social Theory of Practices*. Congruence in performance, the reduction of individual differences, results from training in the form of extended practice (Schneider and Shiffrin, 1977). It may be that our imitation and conceptualization of dance ‘employs’ the universal sensory-motor system that is the solution to the Kantian problem, and that as humans we have a limited set of shared universals to which we repeatedly recur. That is the Lakoff thesis. But this explains concepts like grasping, not ballet concepts like ‘grand battement en cloche’. Even if our understanding and communication to others of these more recherché concepts ‘employs’ the universal sensory-motor system, it cannot be reduced to it.

What does Lizardo think happens in the relevant cases? Here is the key passage: ‘*any social setting that acts directly on the body for a given collective will necessarily results in the sharing of similar “practical presuppositions” about the world*, and ‘*any ecological and/or
social technologies that serve to modify the body ..., will also result in the ‘transmission’ and ‘embodied simulation’ to other members of the group of similar bodily techniques, and thus the picking up of the embodied concepts embedded in those patterns of practice”. The idea is this: the same kind of sharing of neural structure that happens phylogenetically in response to the Kantian problem happens in a parallel but non-phylogenetic way for the neo-Kantian problem anytime a social setting acts on the body for a given collective. There is no role for training and experience to reduce individual difference, no role for learning or its individuating vagaries. The common water osmoses into the individual. The ‘social setting’ as a unitary fact ‘acts’ on the individual body, to produce sameness of mental content.

This is indeed what an attempt to salvage Bourdieu as a bodily mechanism theorist would require. But it is groundless. There simply is no such mechanism. It is true that if we teach people to perform the same bodily performances, and then teach them the terms that correspond to the differences they have learned in their bodies, it will be easier to do so than to teach the terms to people who have not had the experience. Indeed, this is an implication of the close relation between learning language, learning the world, and acquiring habits that *The Social Theory of Practices* closes with (1994, 121). But this is very different from the mechanism Lizardo wants to establish.

If there were such a mechanism, it would make casual history irrelevant, replace error-prone learning, and overcome the obvious fact that individuals have very different experiences and get very different feedback even in the same social setting. A mechanism that produces sameness simply by exposure is simply beyond plausibility. A little reflection on the ontogeny of skilled performances shows why individual learning history still matters, and produces differences at the neural level detail even for people who are engaged in the same bodily activities—let alone cognitively complex social activities. Consider a simple example: teaching advanced little league baseball players to bat. The professional coach typically begins by asking them to swing. This ‘natural’ swing is what the player has learned unconsciously and with the informal feedback or advice that they have received by non-professionals. Each swing will be slightly different, and wrong: batting is one of those activities that requires continuous coaching, for all but the largely mythical ‘naturals’. The coach will advise the player on how to correct what is wrong. Again, different advice will be given for different swings and problems. Even a ten year old will have many bad habits to correct, though similar problems will crop up with player after player. The acquisition and correction are the learning history, and it will be different— in the sense that the neural connections will be built up in a different sequence and a different way. If the coaching is successful (and the successes are usually only temporary), the player will be able to produce a more or less consistently good swing. The difficulty of producing even this simple physical act, however, is enormous, for the simplicity is deceptive: unlike grasping, there are numerous parts of the body that need to work together to produce the swing.

The same point applies to concept learning: we come to concepts in different ways, through different histories, and this is especially true for concepts understood in terms of the account given by Gallese and Lakoff, in which concepts are understood in terms of their neural connections. ‘Sameness’ in this case is sameness only at the level of functional intersubjective understanding-- not a neural fact, much less one produced by common body experiences. Even less individualized training, such as military drill, is neurally overlaid on differentiated
backgrounds, such as the way recruits walked before they were disciplined by the drill sergeant.

Why would I say that mirror neurons and even the Gallese hypothesis about universal neural precursors to some limited set of embodied concepts are an aid for my account and undermine Bourdieu’s? For one, as noted, the idea that communication employs the universal sensory-motor system reduces the burden of learning—so if it is true, less has to be learned in a connectionist way to produce a distinctive practice by emulation. One need not learn to identify and emulate walking, for example, but might have this capacity to code for and act already wired-in. In learning *grands battements* in ballet, then, one would not start from scratch, but have the head start given by these pre-wired body schema. (I omit the complexities and issues with these notions of ‘concepts’ and ‘schemas’, and with the whole idea of precursors, obviously). Mirror neurons make emulation a real neural process, which eliminates any sense that, like introjection or socialization (or for that matter Bourdieu’s reproduction), this is a purely metaphorical substitute for a genuinely causal account. But the more profound support comes from the genuinely radical feature of the larger discussion of mirror neurons—which comes from its shift from the input side of the story of how we come to be social beings capable of understanding and getting along with others to the individual construction side.

This radical shift—a Copernican revolution in cognitive science, so to speak—completely obscured by Lizardo’s presentation, deserves to be recognized. Consider older notions like Freud’s notion of introjection, which led to the ‘oversocialized conception of man’ in thinkers like Parsons. The introjection of the content of the superego was a case of massive input of a certain kind of data into the unconscious. According to this picture, we are, as individuals, largely constituted by stuff that is crammed into our heads by society. That is, of course, also Bourdieu’s picture. The idea of simulation shifts the emphasis to the individual, who uses himself or herself as a model and means of understanding others. Dautenhahn and Nehaniv (2002) call this the agent-based perspective, which captures the implications of a shift from viewing people as imitation machines to active users of these powers.

Moreover, the idea of mirror neurons pushes this use of one’s own embodied capacities as a model for understanding and imitating others to the very beginning of the human developmental process. Understanding other people is an advanced achievement produced through the building up of levels, subject to error, feedback, and so on. Except, possibly, for simple bodily motions and some as yet unknown array of other universals, it is not there at the outset of the human developmental process. The models of the development of the capacity to mind-read are complex and need not be discussed here (Hurley, 2007). But the processes of simulation they model are, as I have argued, sufficient to account for the uniformities of overt conduct that practice theory is an attempt to account for.

Ellwood was right about this and Durkheim and his heirs were wrong. The social process, with all its opportunities for emulation, simulation, testing, feedback, and learning, is a data rich environment, and human beings, as cognitive science is showing, are powerful emulators and simulators who are particularly good at using simulation to fill in missing data. This feature of simulation is especially important: we can learn so quickly from our social environment because simulation allows us to fill in missing data in social situations. Because we can simulate other people more readily than we can simulate the physical world, we can thus construct, test, and assimilate complex feedback simulative ‘hypotheses’ about this world expeditiously. The interaction between these capacities and this environment is not one that produces clones, or
puppets of the group will, nor does it produce anarchy. It produces skilled interactors with enormous capacities to anticipate, predict, and model the people with whom they interact, to adjust to them, and to learn from their adjustments. This kind of coordination, as Dewey called it, is all we need to account for ‘society’. The burden is on the believers of collective objects to show that it cannot. And advances in our understanding of our cognitive powers such as those involving simulation and mirror neurons makes doing so more difficult.

Notes

1. Tarde proceeded from the relentless application, and conceptual expansion, of the concept of imitation; Ellwood’s approach was an application of the functional psychology of Dewey and Mead of the 1890s to social topics (cf. Ellwood, n.d., Sociological life history). Ellwood is of more than historical interest, but he is also arguably the historical source of the symbolic interactionist critique of the concept of society advanced by Blumer, who was Ellwood’s student. Lizardo complains about the pragmatism of The Social Theory of Practices, a topic I won’t pursue. For a discussion that suggests it ought to be more pragmatist, see Kilpinen (2007).
2. And they are very few. Most of the references are to the book as a whole. But there are a few direct references. After saying that I claim that ‘the transmission of practical skills from one agent to another cannot occur by way of imitation’, he cites 1994, 45. There is nothing of the sort on this page. Moreover, the book repeatedly discusses emulation, which would be an example of this kind of transmission.
3. Mysteriously, however, he uses this same cognitivist language, especially functional presuppositions, himself. This is an important point in the larger history of practice theory, particularly in relation to the term ‘concept’. Oakeshott, as I have noted elsewhere, sought to replace this Kantian language with different terms, precisely to avoid the implications of their use (2003, 2005b).
4. As Nehaniv and Dautenhahn point out, exact copying, even with similar embodiment, is almost never possible: One never has exactly the same agents with exactly the same kinds of bodies in exactly the same settings when the behavior of one agent is said to match that of another, as they must differ at least in their situatedness in time and/or space, not to mention numerous other details (2002).

References


Turner, Stephen (2007e) Practice then and now. *Human Affairs*.


Stephen P. Turner

Department of Philosophy

University of South Florida FAO 226

4202 E. Fowler Ave.

Tampa, FL 33620 USA

turner@shell.cas.usf.edu