Social Theory as a Cognitive Neuroscience

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Abstract:
In the nineteenth century, there was substantial and sophisticated interest in neuroscience on the part of social theorists, including Comte and Spencer, and later Simon Patten and Charles Ellwood. This body of thinking faced a dead end: it could do little more than identify highly general mechanisms, and could not provide accounts of such questions as 'why was there no proletarian revolution?' Psychologically dubious explanations, relying on neo-Kantian views of the mind, replaced them. With the rise of neuroscience, however, some of the problems of concern to earlier thinkers, such as imitation, have revived because of the discovery of neuronal mechanisms, or through fMRI studies. The article reviews the history and discusses the implications of current work for the reconsideration of traditional social theory concepts. It is suggested that certain kinds of bridging work with neuroscience would enable us to answer many questions in social theory that empirical sociology has failed to answer.

key words: cognitive neuroscience, Spencer, practices, Mead, simulation, imitation, connectionism

Social Theory cannot get very far without making generous use of mentalistic or cognitive concepts. Even the identification of the domain of social theory, particularly its articulation as 'sociology', that is to say a special discipline concerned with the social, typically relies on cognitive concepts. Weber, for example, defines social action in terms of the concept of subjective meaning. Durkheim conceives of sociology as itself a cognitive science whose special subject was the collective consciousness ([1895]1982: 40-42). Even rational choice theory is explicitly cognitive and intentional (Elster 1985: 8-10), though it relies not on, so to speak, a naturalistic understanding of consciousness but on the normative theory of rationality contained in decision theory, a fact which raises questions about its credentials as 'explanation'. Present day social theory is the uncomfortable inheritor of all of this theorizing: 'uncomfortable' because many if not all of the terms in which this theorizing has been conducted are no longer in accord with the rest of the rapidly developing body of knowledge about the mind and the brain, and potentially in conflict with it.

Why is this not the pressing intellectual concern of social theory? One reason is that social theorists, especially in the tradition of sociology, long ago abandoned any attempt to be realistic about the mental, either by focusing on discourse or by using, in invisible quotation marks, notions that no longer can be said to have any plausible correlates in actual mental processes as understood by cognitive scientists or neuroscience. A simple example of this process is the much studied subject of collective memory. In its original Durkheimian form the

term had a realistic psychological interpretation: a collective memory is located in the collective consciousness, and its identity as a single thing is guaranteed by its collective status and the causal relation between this collective status and individual consciousness (Halbwachs [1950]1980: 48-9). No one today believes this psychology. What is studied under this heading are such things as public enactments, museums, commemorations, and monuments. But the old vocabulary of, as Clifford Geertz put it, 'Common sense is not what the mind cleared of cant spontaneously apprehends; it is what the mind filled with presuppositions concludes' ([1975]1983: 84), remains as the framework in which such things as collective memory are described.

The older framework is enabled, and sometimes subsumed, by a newer vocabulary of 'discourse,' which represents a further flight from the mental. Geertz played a central role in repackaging this in discursive terms (1973), and his followers and successors, such as William Sewell (2005), repeat these slogans. But they do not bother to explain how minds or brains can be 'filled with presuppositions'. And there is a similar reliance on psychological language in Bourdieu's account of practices, which is framed in terms of dispositions, and a similar disconnection between these terms and psychologically realistic mechanisms. It is perhaps reasonable to say that they have no obligation to do so– that for them all these terms are useful analogies but not attempts to say anything about actual causal processes, or that if they are useful analogies, someone in the distant future will figure out a way to make them square with what other disciplines will discover about the cognitive and the neural (cf. Gross, 1998). But this bravado is hollow, haunted by the fact that what we do know about the mind, limited as our knowledge is, points in different directions.

The retreat in these cases is part of a larger retreat from the cognitive. One cannot fail to be struck by the fashion for sociological approaches which attempt to reduce reliance on mental and cognitive concepts to a minimum, for example in the case both of the conventional 'network theory' found in the United States as well as Latour's actor network theory, and, more interestingly, in the case of 'practice theory' itself, which now tends to focus on assemblages of objects (as in Pickering, 1995 and Pickstone, 2000) rather than supposed mental contents. In each of these cases, the minimum of mentalistic content is very low. Granovetter's 'Weak Ties', for example, are weak in the sense that they involve a minimum of conscious decision making and thus a minimum of belief content (1973). Latour, similarly, flattened the notion of agency to the point that the concept applies equally to clams and micro-organisms (1987). Of course, these enterprises, though they are of some interest as an exercise in seeing how far an analysis can be pushed without appealing to stronger notions of agency and cognition, inadvertently show that weak notions of cognition and agency produce weak explanations and very limited understandings of the kinds of questions about the social world that social theory has traditionally attempted to provide. As I will show later, the terrain vacated by these thinkers is increasingly being taken up by cognitive science researchers. Even such a concept as trust is now in the hands of economists armed with brain-scanning technology (Fehr and Fischbacher, 2005; Fehr et al, 2005). The strategy of minimizing the cognitive is a genuine alternative to engaging with cognitive neuroscience. But only if we choose to push social theory to the far periphery of knowledge, into a ghetto of its own making.

The situation of social theory, then, is this: the inherited conceptions of mental life, the conception employed by both classical social theory and later academic sociology, are for the

most part discredited, passé, or irrelevant to any present discussion of cognition in cognitive science. There is no escape from the cognitive through such alternatives as cognitively minimal network theories that is not also an escape from meaningful engagements with the problem of social theory. The problem of cognition, of the mental, and its related problems, such as the problem of the physical properties of the brain and the fact that any plausible mental concepts in social theory need to be realizable in terms of the real features of real brains, cannot be evaded.

But coming to this conclusion does not place us in a position where the problem can be solved by taking a given cognitive conception off the shelf and bolting it into social theory as a replacement for the cognitive conceptions that we have inherited. There is no replacement that does not require changes in the standard usages of social theory. So more is needed: a full engagement with the problems of cognition themselves with the specific concerns of social theory in mind. This is a daunting task. For it requires social theory to engage, as I will shortly show the classical sociologists themselves were willing to engage, with the philosophical and scientific controversies of the present, and to add the voice of social theory to these conversations without expecting that social theory will not be itself radically transformed in the course of the conversation which it enters. If this is a challenge that too few social theorists have taken up, it is striking that social theory once did take these topics up, and the theorists who did were the great losers in the twentieth century. To restart this engagement is necessarily to call into question the achievements of the winners. But it is also, as I will argue at the end of this article, a matter of rethinking the function and tasks of social theory.

Comte, Mill, Spencer, and Baldwin

What happened when social theory was last engaged with these issues? What did early social theory say about the cognitive, and especially about what is nowadays known as cognitive neuroscience? And what happened to this theorizing? In this section I will indicate something about the views of the main players in the period before 'classical sociology'. The main figures in relation to cognition and the brain are Comte, Mill, Spencer, and James Mark Baldwin. Comte, notoriously, was a critic of psychology, regarding it as part of the metaphysical stage, and he left it out of his model of science. His substitute, however, was phrenology. His sometime supporter and debating partner, John Stuart Mill, argued for the foundational role of psychology in the social sciences ([1843]1965: 24-36). But the associationist psychology which Mill assumed would serve this foundational role went out of fashion. Spencer introduced evolutionary ideas into psychology, arguing that such staples of ethical thinking of the time as moral intuitions were the product of evolution (Spencer, 1855, 1857. Cf. Giddings, 1922: 3), an idea expanded on by Baldwin (1894, 1896).[1]

What was this prehistory about? Before Spencer, there was the odd couple of Comte and Mill. Comte, notoriously, was hostile and dismissive about psychology in general, and looked to phrenology as a substitute discipline for psychology. His grounds for this dismissal were in part related to his positivism, his critique of introspection, and his expectations that metaphysical psychological concepts would necessary vanish as mental life was understood in terms of positive law, something he expected to happen in the study of the brain itself (Scharff 1995: 19-44). Phrenology attempted to do this, but unsuccessfully, and was followed by such anatomists as Broca, who paved the way for later understandings. The modern analogue to these ideas is

locationism in the study of brain processes. The assumption of locationism is that one can understand mental functioning only if one understands the distinctive character of mental processes as they are located in the brain and in terms of the distinctive significance for different kinds of mental processes, such as those involving emotion, that are located in particular parts of the brain. Mill's psychology, by which he meant associationism, was the fundamental discipline of the social sciences and the source of the causal force for law-like significance of generalizations in the special sciences. The key idea of associationism is that mental processes, notably learning, result from the association of ideas produced by experience. Today such thinkers as Jerry Fodor denounce connectionism as mere associationism (Fodor and Pylyshyn 1988: 19-20, 34n, 49. Cf. Bates and Elman 1993: 8). Ironically, contemporary cognitive neuroscience has combined associationism with its former rival. The depictions of thought through brain scans typically show electrical clusters in different parts of the brain and depicts thought in terms of these patterns of association or connection.

Spencer's thought was a development of the combination of associationism and localization, motivated by a general evolutionary point of view, rather than specific attention to the brain or biology. For Spencer, psychology was another place in which the principles he had worked out as part of his larger evolutionary project could be made to apply. The idea of the mind as adapting to the environment, and the idea that the relationship between mind and brain was one that could be understood in terms of the physically differentiated locations in the brain, were novel, and indeed radical. Ironically, they derived from precisely the same source as the ideas of his sociology that were later to become deeply unfashionable: the idea of structural differentiation, and therefore localization, resulting from functional differentiation. 'No physiologist who calmly considers the question in connection with the general truths of his science', he wrote, 'can long resist the conviction that different parts of the cerebrum subserve different kinds of mental action. Localization of function is the law of all organization whatever . . .every bundle of nerve-fibers and every ganglion, has a special duty . . .. Can it be, then, that in the great hemispherical ganglia alone, this specialization of duty does not hold?' (1855: 607-8).

The parallels to his social theory and to later 'structural functionalism' are substantial. But there is a basic difference as well. The organization of the brain could be studied without resorting to fictions and analogies. But while the specific 'duties' of parts of the brain could be inferred from such things as the effects of brain damage or other physical facts about the brain. In the case of social theory, matters were different. The duties and their locations were abstractions. And this led to a dilemma. Either the functional analysis could restate, trivially but at an abstract level, such ordinary facts as 'the state is concerned with the exercise of authority', or it could concern itself with fictional structures, such as the 'cultural' subsystem which was concerned with such fictional functions as 'pattern-maintenance'. The latter was the path that Parsons, Luhmann, and systems theory eventually chose. But none of them could shed the suspicions that they were merely producing concealed trivialities.

'Who reads Spencer now?', Parsons once mockingly asked. The answer is ironic. Spencer appears now as an honored precursor to evolutionary psychology. His influential contribution to social theory was his argument that are most fundamental intellectual dispositions, including what in ethic had been understood theoretically in terms of intuitions given to the moral faculty under faculty of mind theory of mind were in fact the product of evolution and under the continued effect of evolutionary processes, perhaps even including

selection. A related argument was made by Baldwin, an American developmental psychologist whose work was responded to by Cooley and Mead, which is now called the 'Baldwin effect', the idea that learned characteristics could be selected for and thus become second nature to people, in parallel to, and then influencing, inheritance.

Like Gabriel Tarde, Baldwin focused on imitation as a central means of learning, and reasoned that the variation produced by the inevitable imperfections in imitation allowed for a kind of selection (1894, 1896). A social environment composed of models of behavior of a particular kind determined what there was to imitate, as well as providing responses that selected, by producing failure or success, particular behaviors learned in this way. The general idea that even very deep motivations varied socially because they were selected for in a specific local social environment which could have pronounced consequences. If the capacity to learn this behavior was genetically linked, the trait would potentially be selected for in reproduction and become normal for the population, thus turning something learned into something innate– or rather something related to the learning into something innate.[2]

The argument that deep motivations could be selected for 'socially' or by the social environment was a critical step for sociology, because it was simultaneously a means of socializing the mind or making the mental contents which were formerly ascribed to human nature, relative to the social circumstances in which they were established, and at the same time biologizing the mind by identifying mechanisms for the production of biologically evolved features of mental life. Disciplinary sociology, when it developed, preserved the lesson of the socialization of the mind, and increasingly omitted or ignored the biological lesson.

Patten and Ellwood: the Path not Taken

When Simon Patten published *The Theory of Social Forces* in 1896, he built on this background in the light of more recent neuroscience to construct an explicit social theory very different from that of Spencer. He began with a neuroscience model:

> In any network of currents like the human brain the secondary currents are of the greater importance. They bind the various centers into a harmonious whole reflecting sensations from one center to another... the whole mind is thus aroused to a similar activity...the process of reflection separates from each other the elements which are united in the obscure currents. . . . In this way these elements are purified by isolation, and each one is carried to that center most fitted to receive it. (1896: 22)

This is a recognizable formulation of the connectionist model of learning. Patten then went on to make a series of crucial arguments that match closely with current thinking. He compares the mind to society, a notion discussed in the artificial intelligence literature by Minsky (1896), and does so in a way that fits very well with connectionism.

For Patten, the evolutionary forces produced by the pressures of adaptation to specific environmental circumstances parallel and work in terms of similar mechanisms in the mind to select for and strengthen neuronal links as well as to pare the less adaptationally valuable ones. The 'mental mechanism', as he calls it, is purification of these links, through repetition of the 'nervous arc' of neuronal connections. The effect of this strengthening, in the mind, is

understood in terms of Locke and Hume on vividness of ideas. 'Purification' of this unconscious, neuron-level kind, produces clear and forceful ideas.

Customs and institutions, he argues, are produced in the same way, through evolution, and become part of the subjective environment that shapes the mind. The environment of 'the sensory center', he argues,

> is made up of two parts– the objective environment presented through the original impressions, and the subjective environment created through by the activity of related [neuronal] centers. Each impression from the outer world has added to it certain ideas from the subjective environment and the joint product is projected by the mind and seems a part of the objective world. Customs, habits, institutions and ideals are as objective and real to individuals as any part of the outer world. (Patten, 1896: 53)

Here the experienced objectivity of the social world and its obligations and practices is accounted for without any sort of problematic collective psychology or claim about the objective social mind, as in Durkheim.

The idea of 'social forces' in Patten's title deserves some comment. The assumption of this model is that learning is done in an environment and that the connection with the environment is by way of activity directed at satisfying needs, or survival, as well as satisfying learned pleasures and goods, like altruistic aims. 'Social forces' was a term that referred to the basic psychological motivators that played a specific role in producing social life. They were a main concern of sociological thinkers of this era. Giddings's main theoretical work, on what he called 'social causation', takes up this topic, and it is the topic of Charles Ellwood's *Sociology and Its Psychological Aspects* of 1912, one of the earliest 'social psychology' books. This is a topic which academic sociology largely abandoned.

Ellwood was open to Darwinian insights, but was not, as some later socio-biologists and evolutionary psychologists were, reductivist. But he was not caught up any sort of defense of a Durkheim-like notion of the causal autonomy of the social realm, nor did he take the view that 'the social' constituted some sort of natural entity which could be examined apart from psychological mechanisms that figured in social processes. On the contrary, for Ellwood there was nothing to society that was not there through the ongoing social and psychological processes of human interaction, which he characterized in terms of interlearning (1925: 453-79). [3]

For Ellwood, society is a metaphorical notion (1925: 460). The real causal processes involved in the production of culture, tradition, and so forth, are processes of interlearning, a processes in which we are formed psychologically by the contents and results of our interactions with other people as well as with the physical world. We are driven in particular directions by evolved mental presuppositions and dispositions of various kinds, which may include dispositions to learn particular lessons and to learn about particular kinds of things. There is no inconsistency between this evolutionary and 'interlearning' perspective on social life and the idea that fundamental psychological processes of learning and acting on what is learned are primarily matters of associational psychology which are located in the physical brain and produced by and inscribed in neural connections between various parts of the brain which are specialized in particular kinds of dispositions, responses and so forth.

Ellwood's mechanisms allowed him to be a social evolutionist because interlearning

processes were selective and consequently directional rather than merely random or arbitrary, and Ellwood was also open to the idea that some directionality was imparted on an ongoing basis by evolved instincts rooted in biology. Yet Ellwood said relatively little about cognitive mechanisms themselves. He relied on the relatively unproblematic notion of learning, emphasized the extensive character of learning socially, by which he meant merely learning from one another, but was not himself a psychologist and did not pretend to offer original theories here.[4]

So what happened to the original concerns of Ellwood and his Darwinian view of the mind? Ellwood's views were the victim of a powerful, well-orchestrated generational assault by sociological sympathizers with behaviorism who were fighting against instinct psychology (cf. Bannister, 1987: 141-2, 193-4). Within a few years, 'social psychology' as field had abandoned not only instinct but such notions as imitation, which had informed the competing textbooks of E. A. Ross (1909, 1912) and William McDougal (1921: 332-58) . The effect of this shift was to sideline the problem of the physical reality of psychological facts, which it did for the rest of the century, in favor of an experimental paradigm.[5] Ellwood was caught defending the instinct theory at a time when it was believed, however wrongly, that such things as attitudes were more or less completely malleable. Ellwood then wrote a book on cultural evolution that followed the general lines of his argument about interlearning (1927). But this shift to culture came into conflict with the emerging 'culture concept' promoted by the followers of George Boas: the idea that a culture was a shared set of presuppositions, values, and the like that was a more or less arbitrary selection from the basket of possible human values (M. Mead, 1928: 13; cf. Turner, 2002: 74-107) The few 'evolutionary' thinkers in anthropology who persisted, such as Leslie White, abandoned the kind of larger interests in such topics as the evolution of altruism and altruistic social morality and tried to be as scientistic as possible.

European Sociology and Interpretation

Continental social theory developed in a different direction, under the influence of ideological anti-individualism and the alternative of the objective group mind (cf. Freyer, 1932), which was typically understood as a shared mental life in which the shared parts were basic concept-forming categories or worldviews. The roots of this idea were Hegelian and neo-Kantian, and were defended by such philosophers as Ernst Cassirer. In its sociological form, it involved the idea that different worldviews were associated with different social groups, and different social forms, attitudes, experiences, and the like, especially those with normative content, were produced and experienced for and by members of the different groups. This is the same kind of analogical thinking that allows one to speak of the premises of the scientific revolution or modern science, and similar topics.

This mode of thinking is so pervasive that it seems beyond challenge. But if we start asking elementary questions about the concept, it begins to look implausible, especially in terms of psychological mechanisms. One can say that these concepts of collective structures, climates of opinion, worldviews, and so forth do get a partial grip on something worth explaining and understanding, without concluding that the explanatory theory implicit in these concepts is or could be turned into a genuine account of cognitive processes. But it is nevertheless not an analogy that can be developed into a psychologically serious account.

The reasoning is psychologically implausible because of the problem of mechanisms and physical realizability. If we think of these presuppositions on the model of a computer program which we must each acquire in order to make sense of one another, we must also suppose something like the following: that there is a kind of server from which we download this program and also download updates we enable us to continue to make sense of one another and update our categorical framework in order to do so. This is a psychological process with no plausible actual psychological analogues. There is no such server and there is no such mechanism of downloading and updating that assures that we each have some sort of common program enabling us to communicate. The whole idea is simply a fiction. There simply are no collective servers of this sort, and no mechanisms of transmission or downloading by which they could operate. The fact that there is no server from which we could download our presuppositions and update them is a decisive reason for rejecting any theory of culture, objective mind, world view, paradigms, and the like that presupposes such a mechanism.[6]

Social Theory and Cognition Today

So where does this leave us? The kind of scientistic social psychology produced after 1925 in the United States produced many 'results' but no significant understanding of the mind.[7] The 'shared presuppositions' model is purely analogical and can't be made into a psychologically realistic account of anything. Yet the thinkers that this kind of social theory and social psychology triumphed over, such as Ellwood and Patten, did not have good answers to a fundamental problem: the gap between the highly generic depictions of processes of learning and basic psychological drives that they employed and the immediate explanatory questions of concern to social theory. Today the gap between social description and neuroscience mechanisms has closed, but only very partially, a point I shall discuss shortly. There are aspects of the gap that cannot be closed, and there is a clearer understanding than there was in Ellwood's day of the reasons that the gap cannot be fully closed.

The problem of generic mechanisms which fail to account for anything specific is perhaps the most important to address, since it is justifiably taken as grounds for dismissing the relevance of these ideas. The most dubious attempts to make highly generic mechanisms account for specific structures tend to be associated with sociobiology, and here the old issues that dogged Ellwood, the misuse of instinct theory, take center stage. McDougall, who was one of Ellwood's psychologist allies, explained in a circular manner: if he had a phenomenon that he needed to explain, for example the building of large structures, he invented an instinct to explain it, and used the fact of the near universal occurrence of the thing to be explained as evidence of the instinct (e.g.1921: 91, 331). E. O. Wilson's explanation of the instinctual basis of religion (1998), or for that matter Dennett's (2006), is not far removed from this. One problem for the social theorist is that these accounts typically overgeneralize the phenomenon to be explained, making all religions seem more similar than they actually are, and, more importantly, fail to acknowledge differences, such as the difference between magical and salvation religions, or the differences between theodicies, that are causally consequential for the topics that social theory typically addresses (cf. Weber [1946]1976a, [1946]1976b). In many cases, there is an overreliance on the notion of sexual selection, which is conceived to operate nearly universally. The need for this mechanism leads Steven Pinker, for example, to reject the contentions of

sociologist and anthropologists that kinship rules greatly restrict sexual selection (1997: 436-38). Yet as anyone familiar with kinship structures and present practices in arranged marriages in much of the world (and historically in Europe itself) knows, decisions to marry and reproduce are governed by innumerable complex local constraints, many of which have to do with social status and economic considerations, and are often largely controlled by parents, so the patterns that emerge bear virtually no resemblance to the abstract biological models on which evolutionary theory relies. There is a lesson here, though one that was well understood by Mill (cf. Turner, 1986: 30-32) that applies more generally– causal complexity, involving the interaction of institutional facts, beliefs, immediate tactical motivations, rational choices, drives, self-concepts, and skills and tacit knowledge– are the norm in explanation. Models of social and historical processes are at best ideal-type simplifications.

Despite the fact of complexity, there are transformative implications of neuroscience for social explanation. Consider the problem of the social transmission of skills and tacit knowledge, a central issue in science studies as well as for practice theorists such as Bourdieu. In the recent neuroscience literature there have been reports of the discovery in the monkey's premotor cortex of neurons that enable 'mirroring'. They respond both when the monkey observes the action of another individual and when he performs the action. Evidence suggests that humans have the same mechanism (Gallese and Goldman, 1998). Why does this matter? In the first place, provides an actual mechanism, one that is significantly related to but also significantly different from the mechanisms that have been assumed in the literature. This mechanism, which of course may not be the only relevant one, operates non-linguistically and visually. The neurons enable people to do the things that they see others doing, a capacity different from the capacity to follow verbal instructions or rules. If this is the mechanism of transmitting tacit knowledge, we would know why and in what way personal contact is a condition for acquiring it. The mystery element is gone: if the neurons work on visual material, and produce a capacity to do something similar to what is seen, we need another mechanism to explain other supposed cases of transmission, or we need to question whether there is anything transmitted. The mechanism also points to a resolution of one of the oldest disputes over imitation as a mechanism: whether it was fundamental, or whether the possibility of imitation required a socially created self to do the imitating, as Mead argued. The answer is that the capacity comes first. But it also points to a refutation of one of the standard objections to the idea that the self is the product of interaction. The mechanism does not require a developed self.

To understand this requires a word on the issue of simulation and the 'theory-theory' (cf. Davies and Stone, 1995; Kögler and Steuber, 2000; Turner, 2000). The theory-theory holds that our understanding of other minds depends on our acquiring a theory of other minds, that is, a theory that other people have minds which work in certain ways, which we need in order to predict their behavior and 'understand' them. An alternative to this theory is called simulation theory, and it suggests that we have a relatively basic capacity to think about others, as well as about objects and processes in the world, by 'simulating', either by imitating something we observe or by going off-line and thinking in advance about what would happen if, for example, we jumped off a cliff, or acted toward another person in certain ways. In the latter case we would, in effect, impersonate the person we are acting toward in thought, so as to be able to construct expectations about their response. This is very similar to empathic understanding, and to Mead's taking the attitude of the other. And this has some startling consequences. In the first

place, the discovery of localized neuronal groups that enable imitation *and* simulation brings Mead and such contemporaries as Tarde and Baldwin together, by making these processes basic, rather than dependent on the development of a self or the possession of a theory of mind.

The more important result is that these discoveries transform empathy, taking the attitude of the other, and imitation, from vague and dubious phenomenon that can be observed and described but not explained, into something that can be explained in terms of brain processes (cf. Hurley and Chater, 2005; Hurley, 2006). This on the one hand validates them– especially against the skepticism of positivists about such things as empathic understanding– but on the other hand forces us to ask some serious questions about the concepts that do not fit what is known about brain processes and about the ways in which our received descriptions of phenomenon, such as taking the attitude of the other, do not match up with the neuronal evidence.

The present ubiquity of brain scanning technology also creates a strong presumption about physical correspondence. If one's cognitive models rely heavily on concepts that have no actual analogues in physical processes within the brain that are actually detectable, and other theories do not rely on such concepts, but instead can point to physical analogues in the brain, the general principle that a theory that matches up with known causal mechanisms is better than one that does not would point to a preference for that theory. A simple case in point is Donald Davidson's suggestion that the model of the explanation of action he proposed thirty years ago in which a cause of an action was a reason paired with a pro-attitude, corresponds with the physical conjunction of impulses connecting the amygdala to the cerebrum, does amount to an important point in favor of such a model. And one can imagine a future in which the absence of any such correspondence, for example, between a notion of, for example, normativity, and any specifiable activity in an actual brain location, would count as a grounds for doubting the cognitive causal reality and role of that concept.

In some cases, we are in a position to redefine issues. A significant amount of work has been done recently on 'neuro-economics', using brain scan technology as well as chemical analysis of the role of oxytocin in the brain, on the subjects of trust, altruism, and the punishment of violators of social norms. This research goes to the heart of the Hobbesian problem. But it proceeds in the following way: researchers have attempted, using brain scans and knowledge about the localization of certain mental activities, to link the actions that individuals make in experimental settings to actual brain activity. The results, which identify brain activity in the caudate and dorsal striatum, which are associated with rewards and calculations of reward, are suggestive. People derive rewards from punishing free-riders, and high caudate activity 'seems to be responsible for a high willingness to punish, which suggests that caudate activation reflects the anticipated satisfaction from punishing defectors' (Knutson, 2004; Quervain et al, 2004: 1258); they derive more reward when the punishment they mete out is costless, and that they do no punish blindly, that is without regard to costs. People have, it seems, a taste for punishing. By the same token, they have a taste for co-operation, preferring co-operation with humans to co-operation with computers. There are brain chemistry effects as well. Oxytocin is associated with trust. But it operates by affecting exploitation aversion, not risk aversion. This is in effect the flip side of the taste for co-operation: co-operation is good in itself and exploitation is bad in itself.

The Hobbesian problem assumes that people seek their own security and will do so at the expense of others unless they are constrained by a sovereign, or in Parsons' formulation, social norms. If a taste for co-operation is a basic motivator rooted in the brain, and if exploitation

aversion is also rooted in the brain, the 'problem' is quite different: to satisfy the taste for co-operation without exploitation. This returns the problem of 'social forces' in its nineteenth century form, to a question that can be addressed both in a broadly Darwinian way and in terms of actual motivational properties of individuals, in contrast to its Parsonian form, which arises from considerations about abstract individuals endowed with hypothetical properties. And it allows us to ask whether there is a difference on the level of brain chemistry or localization between such sociological phenomenon as trust, social solidarity, response to charisma, obedience, and the like, as well as to ask questions about what goes on in the brain when a person submits to a norm or rule. Are these things a matter of rationality, fear, childhood emotions, or some combination of these and perhaps other sources that can be localized in the brain, or are they a result of something else?

Yet there is an important limit to what a cognitive science approach can do in social theory. Davidson makes an important point about this when he notes that despite the fact that we know the 'normative theory of rationality' is wrong as a picture of actual human reasoning, we nevertheless can't dispense with it. Even to describe 'biases', such as the well-known failure to preserve the transitivity of preferences, we need the model as a baseline. And this is also true of neuro-economic experiments. They depend on what is known to be a false model of human reasoning in interpreting the results of the game theory experiments they rely on. But there is no alternative.

We can put the point more broadly. In describing beliefs, intentions, and the like we are using 'folk' vocabulary that is extremely problematic, but inseparable from the description of the issues of concern to social theory. To take a simple example, the notion of representative democracy relies on the idea that the representatives have a conscience. But conscience, and, as it turns out, many much more basic terms in our folk psychology, including believe, are highly culturally variable. In *The Greeks and the Irrational* (1959), E. R. Dodds describes the extent to which emotions were attributed to the actions of the Gods. And there is a controversy about whether ancient Chinese used intentional language at all (Chad Hansen, http://www.hku.hk/philodep/ch/). Many terms describing action do double duty as ascriptions of external social attributes, especially the fulfillment of roles, and as assertions about mental states. The notion of responsibility, for example, routinely mixes up that which is going on in the head of the agent and that which is external or social.

In a sense, this is exactly what we should expect: in the face of the immense complexity of mental processes and the particular cognitive capacities that humans have to deal with them, it is not surprising that different traditions solved the problem of description in different ways, with the difference related to the problems of social co-ordination that these descriptions also serve, in addition to providing folk explanations. We should also expect that the evolution of these modes of description will have been path-dependent, so that what seemed like a good theory of the mind in one setting reflected and fit with the pre-existing coordinating beliefs, such as religious beliefs about spirit. But we cannot reasonably expect that the actual, complex, human cognitive processes that these ideas imperfectly describe should match up in simple correspondence– or for that matter in any strict relationship.

There is a more serious problem, made famous by Jeff Coulter (Button et al, 1995; Coulter, 1989), which has served to conceal these limitations on the project of bridging the gap between ordinary language and brain science. Much discussion of brain processes, including,

incidentally, the neuro-economics discussed above, proceeds by ascribing intentional notions, such as 'concerned with reward', to parts of the brain. Strictly speaking, this is no more true than saying that a bicycle wheel is concerned with the surface of the road it travels on. But the practice of speaking in this way creates an illusion of closing the gap with brain processes that is unjustified. We are able to make links– I think of them as strings that connect ordinary language here and there but not everywhere to brain processes– but not to produce correspondence. The possibility of making these links is allowed for in ordinary language itself, which has plenty of terms for physical processes like fatigue that might affect our ascriptions of folk psychology terms, for example when we decide that something done by an agent was unintentional and done out of tiredness.

A Concluding Cautionary Note

It seems altogether too grand to what I have outlined here a 'perspective' or theory. One of Ellwood's admirers, Harry Elmer Barnes, commented on what he called the sanity of Ellwood's social psychology, by which he meant its reliance on the unproblematically true as opposed to grandiose theories of mind. In particular, Barnes had in mind the humbleness and ubiquity of causal processes on Ellwood's conceptions relied. The present analogue to this sanity is physical realizability. The idea that social theory ought to be physically and computationally realistic and cognitively realistic seems to be a very modest and unproblematic constraint. This is not reductionist in any problematic way, though it is certainly not, and this is an important qualification, an approach that is necessarily compatible with the acceptance of particular descriptions favored by traditional social theory, such as the notion of society. Some descriptions from folk psychology, or from our normative account of rationality, may be permanently indispensable. We cannot even talk about human action as action without them. But social theory has been here before, for example in Weber's methodological writings, and his solution, or some variant of it, is likely to be as good as we will get.

This might be taken to mean that knowledge of the physical brain only provides very broad constraints on social theory, which will continue to focus on the tasks of making social life intelligible. This would not be much of an advance on the thinkers I discussed in earlier sections. We would still be far from questions like 'why was there no proletarian revolution?' or 'what produces nationalism?' But I think this is a misreading of the situation. The question of how beliefs work in the brain, of how they engage the brain and draw on its forces, are precisely the kinds of questions that we can now address, however imperfectly, without relying on mental fictions, such as those I discussed in *The Social Theory of Practices* (1994).

One example can illustrate this point. Weber hinted that charisma could not be fully understood without reference to biology. As Weber's account stands, there is an ideal-type of charisma, but it is difficult to connect to mental processes. A bridge is needed. In a paper on the subject I provided what in retrospect can be read as a possible bridge. I argued that the charismatic leader provided, through his actions and his account of his actions, a radically novel choice situation, which changed the risk perceptions and calculus of the follower (1993; 1995). The leader's message is 'follow me, and a new world of opportunity is possible'. The presentation of this radical choice, which involves new and unknown risks, and is an all or nothing choice, I argued, was the cause of the agitated mental state of the follower, not, as it had

usually been assumed, the other way around, where the agitated state of the follower was the cause of the decision to follow. This now becomes something close to a factual neuroscientific question. Does the brain of the follower of a charismatic behave like a decision-maker faced with a radical choice involving risk, or not?

Similar kinds of questions can be asked, as I have suggested, about solidarity, alienation, anomia, group and grid, and so forth. And with this kind of inquiry, together with the bridging work that would enable us to be more precise about the meaning of these terms, would begin to help us answer questions like 'why was there no proletarian revolution?' The answers are open. There is no particularly strong reason to believe that, for example, nationalism and socialist solidarity engage the mind in the same way. But it would be of great interest to know whether these social phenomena appear in the brain as similar or different, whether they are more like revenge than trust, whether they resemble the reaction to free-riders, or whether they resemble religious experience. Social theorists have asked these questions about the great social phenomena of the age, if not in precisely these terms. 'Empirical sociology' for the most part dismissed such questions, because their methods had nothing to contribute to an answer. Perhaps now some of them can be answered. One hesitates to speak in these terms, but if the promise is even partly fulfilled, this would be a genuine rebirth of social theory.

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Notes

1.The 'classical' social theorists also had relevant views, but in their iconic form as founders of sociology as an autonomous field, their engagement with these issues has been de-emphasized . Nevertheless it is worth recalling that Durkheim cut his theoretical teeth on the criticism of Wundt's psychology of *Volk* (Durkheim 1993: 89-122), and Weber himself performed fatigue studies (see Schluchter 2000: 59-80).

2. For a critique and review of the recent literature, see Griffiths (forthcoming).

3. The similarity with symbolic interactionism that one detects here is no accident. Herbert Blumer was his most successful pupil and Blumer did not differ with Ellwood with respect to what might be called social ontology, denying, as Ellwood did, that there was any such thing as society, but rather on a point about the significance of culture, which Ellwood affirmed and Blumer denied, in favor of the significance of present interaction (Turner 2006), a theme he derived from Mead (1932) For reasons that I will not go into here, this proved to be a dead end, and eventually faded from symbolic interactionism itself.

4. As it happens, the psychologist he knew best, his counterpart at the University of Missouri, Max Meyer, who wrote a remarkably connectionist-like account of learning and emotion complete with an electric circuitry analogy (1911).

5. In the nineteen-thirties Ellwood wrote a reflective essay on Baldwin, who also made imitation central. In the essay which nicely encapsulates the social theory that Ellwood took to be implied by Baldwin, which was close to his own, he discusses the way in which imitation had been supplanted (1936).

6. This is a simple summary of the arguments I present in *The Social Theory of Practices* (1994) and *Brains/Practices/Relativism* (2002).

7. Danziger's *Constructing the Subject* is the best account of the period in relation to psychology (1990). I discuss the problematic character of attitude psychology in two places (2002: 1-22, 2005: 44).