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"MISTAKES"

ABSTRACT. A suggestion famously made by Peter Winch and carried through to present discussions holds that what constitutes the social as a kind consists of something shared – rules or practices commonly learned, internalized, or otherwise acquired by all members belonging to a society. This essays argues against the explanatory efficacy of appeals to this shared something as constitutive of a social kind by examining a violation of social norms or rules, viz., mistakes. I argue that an asymmetric relation exists between the notion of mistakes and that of the social. In particular, mistakes do not presuppose a concept of the social, but the concept of the social requires prior specification of a category of mistakes. But no such prior specification proves possible. The very notion of a mistake is so inchoate that it makes it impossible to provide the kind of regimentation required for a rule-governed domain. Thus, there may be recognized mistakes even in the absence of a unified system or common knowledge of norms.

Later writers attempt to avoid Winch's over-strong assumption that something shared and internal constitutes the social but cannot. Extending recent work by Stephen Turner, I argue that "the social" is not a domain that is susceptible to lawlike treatment, but rather a heterogeneous, motley collection. For absent the assumption of a shared something, no social object exists to be explained. So, I conclude, we have at present no clear way of marking out the social as a coherent or unified domain of inquiry.

> Different persons growing up in the same language are like different bushes trimmed and trained to take the shape of identical elephants. The anatomical details of twigs and branches will fulfill the elephantine form differently from bush to bush, but the overall outward results are alike (W. V. Quine)

> I conclude that there is no such thing as a language, not if a language is anything like what many philosophers and linguists have supposed. There is therefore no such thing to be learned, mastered, or born with. We must give up the idea of a clearly defined shared structure which language-users acquire and then apply to cases. And we should try again to say how convention in any important sense is involved in language; or, as I think, we should give up the attempt to illuminate how we communicate by appeal to conventions (Donald Davidson)

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Mistakes can illuminate. Think of Freudian slips. But the light they shed goes in no one direction. For example, my daughter Emma, when still a toddler, proudly displayed her counting abilities for me. She correctly enumerated the objects before her, but then, to my surprise, continued to increase the count, pointing again and again to the very same objects. A different case: I happen to watch a "real TV" show, "Blind Date". Cameras roll as each person tries to impress the other. Wincing at one particularly futile effort, I think to myself, "Mistake; that won't work". Indeed, the date is not impressed.

Now Emma's mistake made plain her lack of awareness of a rule or norm that partly constitutes what "counting" is for us. Helping here involves supplying her with a rule (keeping in mind that precious few of us could fully specify a rule set appropriate to codifying counting the natural numbers). But although the person out to impress makes a mistake – wrongly believes an act to be of a type it is not – the mistake reveals no missing norm. Mistakes can occur even in the *absence* of rules, norms, etc.[1]

What argues for an absence of guiding social norms in the "Blind Date" case? Any proposed behavior-guiding principle – again, call it what you will – fails to guide in any intelligible sense unless it excludes some cases, i.e., identifies what would constitute non-compliance or non-conformity – an instance of a mistake. But we possess no theory marking out, e.g., which actions must impress and which not, and experience indicates we can be surprised on this point. Lacking a determinate basis for a prior sorting of acts, adverting to underlying rules to account for "mistakes" becomes vacuous. If I am correct about this asymmetry – rules, norms, practices, etc entail a contrast class of mistakes, but not *vice versa* – there follow some non-trivial consequences for theories of the social.

One implication of this asymmetry creates, I suggest, a special problem for all claims that any social practice, including linguistic ones, must be rule-guided. For the asymmetry highlights the fact that the explananda of social theory often owe their putative existence to prior assumptions regarding the existence of *shared* rules constituting the objects to be explained.[2] Yet if this assumption proves implausible, what then of the objects of explanation? Do the peculiarly social phenomena that rules, norms, practices are invoked to explain still need to be accounted for once one sees that social practices no longer entail a commitment to rules and their kin?

A suggestion famously made by Peter Winch and carried through to present discussions holds that what constitutes the social as a kind consists of something shared – rules or practices commonly learned, internalized,

or otherwise acquired by all members belonging to a society. Some later writers attempt to avoid Winch's over-strong assumption that something shared and internal constitutes the social but cannot. Call this the "Winch problem": if no rules, then no social kinds, i.e., nothing brutely social to explain. The problem arises because Winchian rules create as well as explain the social. I am not contesting the philosophical truism that our theories determine our ontological commitments. Rather, the worry is why putative objects of explanation should survive the demise of the theories of which these objects are artifacts. As go the theories, so should go the objects.

The point here is *not* a skeptical one, either – that we may always incorrectly identify the underlying basis for an observed regularity, social or otherwise.[3] The worry, rather, concerns whether there are social kinds in need of explanation. Consideration of how the possibility of mistakes requires no such kinds, no constitutive principles of any sort, potentially relocates, I hope to show, the explanatory challenge. Specifically, it moves the focus to a too taken for granted assumption that "sameness" of behaviors or "obvious" social kinds unproblematically demarcate what theories about us need explain.

I examine three efforts to preserve the social as an object of explanation which run afoul of the problems sketched above. Section I examines how the problems manifest themselves in Winch's views. Section II details their presence in responses to Stephen Turner's critique of notions such as practices. Section III identifies how these issues fester in David Bloor's proposed sociological theory of meaning.

The three sections map successive efforts to mark the constitution of the social as an object of explanation starting (in Winch) with processes taking place inside the head and moving (in Bloor) to ones occurring without. Although Winch clearly and correctly commits his account to the explanation of error, his views can be shown to lack the resources to provide this. "Externalist" efforts to demarcate the social such as Bloor's cannot repair this failure – the inability to stipulate what would constitutes a mismatch between rule and experience, i.e., a mistake. For Winch and Bloor, however else they differ, both ultimately recapitulate philosophical errors they claim to overcome, offering in the end unmoved movers of meaning in sociological or pseudo-Wittgensteinian dress.

These efforts to specify the "meaning" of the social give an ironic twist to Bloor's suggestion that sociology is heir to the subject that used to be called philosophy. For if analytic philosophy was conceived in *Sinn*, i.e., spawned by a union of logic and a metaphysics of meaning, its erstwhile

sociological successors possess a disconcerting resemblance to just this troubled family of philosophical theories.

I

Many are the ways of going wrong. Compare the examples of mistakes already mentioned with one made famous by J. L. Austin.

> You have a donkey, so have I, and they graze in the same field. The day comes when I conceive a dislike for mine. I go to shoot it, draw a bead on it, fire; the brute falls in its tracks. I inspect the victim, and find to my horror that it is *your* donkey. I appear on your doorstep with the remains and say – what? 'I say, old sport, I'm awfully sorry, &c., I've shot your donkey by *accident*'? Or 'by *mistake*'? Then again, I go to shoot my donkey as before, draw a bead on it, fire – but as I do so, the beasts move, and to my horror yours falls. Again the scene on the doorstep – what do I say? 'By mistake'? Or 'by accident'?[4]

Austin, being Austin, could doubtless wring endless nuances and insights from his example. I offer only the following point. If I shoot the donkey at which I aim, but not the donkey I would have liked to have shot, then I make a mistake. I take something for what it is not. Misperceiving and so miscategorizing a natural object is a paradigm case of a mistake. Call mistakes of this type "ground level".

In this regard, Austin's dead donkey represents a "ground level" mistake, while Emma's counting and the "Blind Date" cases do not. Cutting matters this way illustrates differences involved in specifying – to ourselves or others – what a mistake mistakes – what gets wrongly categorized. Ground level mistakes pose no necessary challenge to one's general understanding of how the world works. No categories of understanding need change in light of the mistake; alas, the wrong damn donkey got shot.

In the other cases, correcting that about which one is mistaken – counting, impressing – requires revising how one sees things as cohering.[5] I mean the range of cases to indicate not just how various are the objects about which we make mistakes, but also the deceptive ease with which we move from cases where corrections require some further revision regarding our understanding of matters and cases where they do not.

Consider further in this regard cases where one decides a substantial number of beliefs are mistaken – a Kuhnian paradigm shift, a loss of religious faith, a disenchantment with an ideology. These examples represent cases where, once one decides a mistake was made, changes include beliefs about what, in fact, needs explaining. Absent the requisite political theory, for example, the world no longer need sort into classes determined by their relation to the means of production. Once one cease to hold the theory, what needs explaining changes – the previous explananda do not receive

a different explanation, they cease to be seen as a cohering object, e.g., a class. Likewise, in dropping the Ptolemaic scheme as mistaken, one loses any need for an explanation of retrograde motion, for the phenomenon to be explained no longer exists. Ditto neurasthenia. Women still faint, but they no longer exemplify a medical kind calling for explanation.

I noted first that there can be mistakes in the absence of behavior-guiding precepts of whatever form. But now I add that certain sorts of mistakes invite the conclusion that one was wrong not just in some particular way – misperceiving to which category things belong – but mistaken in the belief that there were any things of the sort. Mistakes ramify, extending beyond instances of miscategorization so as to include beliefs in the very categories and involve further additional unspecified changes of belief as well.

Now it is Winch who, of course, famously insists that social kinds only appear to us provided we first apprehend the rules making them the kind they are. There is nothing natural for Winch about such kinds.[6] Philip Pettit summarizes Winch's "rule thesis" as he calls it as follows: "understanding human action involves seeing the rules or proprieties in accordance with which it is produced, not just detecting regularities in its production".[7] Although Pettit never says just what such "seeing" comes to, he straightforwardly agrees with Winch on this point. "The rules thesis, as he presents it, seems to me to be fundamentally sound."[8] Social kinds, as I call them, exist for Winch and Pettit only insofar as objects are so categorized by us.

Moreover, Winch, strongly endorses the first part of what I initially claimed – positing rules entails a contrast class of mistakes.

> [T]he notion of following a rule is logically inseparable from the notion of *making a mistake*. If it is possible to say of someone that he is following a rule that means that one can ask whether he is doing what he does correctly or not. Otherwise there is no foothold in his behaviour in which the notion of a rule can take a grip; . . . the point of the concept of a rule is that it should enable us to *evaluate* what is being done.[9]

Moreover, in endorsing Winch on rules, Pettit identifies precisely this aspect of Winch's position as an enduring insight. Social kinds are constituted by rules people impose on experience, and such rules require specifying their possible misapplication.

> If a mind is to map things along different dimensions of sameness . . . then there must be a possibility of the mapping going wrong. Thus Winch adds to his initial remarks the observation that the rule-following for which he argues involves the possibility of error: . . . It follows directly from this line of thought that if we are to understand how a person represents things, in particular what they say about things, then we need to know what are the rules or proprieties that govern their thoughts and words. We need to know what would make it right for them to think or say what they think or say, and what would make it wrong. Otherwise we cannot see them as intelligible thinkers or speakers.[10]

But in acknowledging this connectedness of rule and mistake, the asymmetric relation of implication between mistakes, on the one hand, and norms and their kin, on the other, remains unremarked upon and apparently unnoticed. Yet in failing to appreciate the asymmetry, Winch and Pettit do not consider how differently a belief that one is mistaken might impact rule-bound conceptions of the constitution of the social.

The strongly constitutive role Winch attributes to rules led commentators early on to worry about ways in which people generally might be said to be mistaken. Alasdair MacIntyre, for one, pressed Winch on just this question. Winch thinks of questions of global mistakes, cases of one world-view squaring off against another, only in terms of inter-cultural conflicts. He resists efforts to use the views of one culture to judge another mistaken, famously denying that the notion of a mistake makes sense unless the categories used to characterize the mistake are those of the culture in question.[11] MacIntyre in turn puzzles as to why the Zande cannot suffer from, e.g., false consciousness and even, *contra* Winch, come to see themselves wrong in some general way, e.g., with regard to their beliefs about magic.[12]

Yet against a whole array of thinkers, Winch maintains that mistakes are purely a local phenomenon.[13] This question of how well Winch accommodates intra-cultural changes of belief, particularly cases where a person concludes they were mistaken in quite general ways, has recently been sharpened by John Horton. Horton presses the Winchian view on just the issue of "whether, and if so how, cultural practices or ways of life can be rejected wholesale as false or illusory".[14] Winch invariably focuses on mistakes above "ground level", e.g., practices such as magic which could only be judged mistaken if, or so he claims, a revised conception of reality or rationality was employed. Winch fails to appreciate how global even intra-theoretic changes of belief can be.

Horton keeps an intra-cultural focus, but reads a Kuhnian problematic into Winch, i.e., asking about global shifts of beliefs which occur within the culture of which one is part. This creates several problems for Winch. For one, there is what might be called the conversion problem, the problem of global but intra-cultural changes of beliefs. How, on Winch's view, do people come to make substantial (massive) belief changes? Given that discordant experiences do not play a straightforward role in belief change at either a local or global level, how for Winch could an individual come to decide he or she was substantially wrong? Winch assigns a constitutive role to the notion of rationality because having some sort of systematic relation to how beliefs get linked does seem basic to the very possibil-

ity of thought.[15] But Winch seriously underestimates, it now seems, how radically systems might be changed by pressures from within.

What Horton perceptively brings out in his exposition of Winch is how, somewhat surprisingly, Winch cannot (for whatever reasons) bring himself to confront cases of massive "insider mistakes", cases where individuals come to the conclusion that wholesale mistakes have been made. Using an example of Winch's own – one of a Christian who rejects Darwin, and a Christian who does not – Horton notes that Winch deals uneasily with a case of cultural insiders driven

> to see the other as mistaken. . . . The man who has come to reject Christianity will not experience this rejection as analogous to developing an enthusiasm for baseball, having earlier lost interest in cricket. He will likely think that what he once believed is now not merely uninteresting but illusory or false. This is what seems not to be adequately captured in Winch's discussion.[16]

The problem here is that it is Winch, and *not* the insiders in the debate, who are hesitant about attributing profound mistakes to some one of the conflicting views.

> Winch seems to be *disagreeing* with both the parties to the dispute that I described. Although they disagree with each other about *what* is the right answer, they agree with each other, but not with Winch, that there *is* a right answer. . . . They would both deny what Winch asserts; and it seems that Winch too is committed to denying their views . . . .[17]

In short, Winch comes to occupy the very position he denies, e.g., Evans-Pritchard. Horton then casts about for reasons why Winch is driven to an impasse regarding attributions of mistakes which is clearly unsatisfactory.[18]

By pressing on Winch's position in the way he does, Horton helps bring to light the vexed relations between social kinds and rules. The old debate about rationality, which is where Horton starts, asks: whose rules (the perspective of which society) should have priority? Horton's investigation illustrates how questions about "whose rules" changes into a question of "what rules". Winch lacks any resource to account for global mistakes as characterized by "insiders". For if he is right about the "internal relatedness" of concepts of the social, it is inconceivable how either massive change should even be possible (though it is), or, if possible, how change can be even relatively localized.[19]

Given the fact of global mistakes, a more plausible position is that Winch is just wrong about the constitutive role of rules for the social. If from Kuhn we learned that conceptual change in science is not structured, not determined by logic or fated by experience, then Horton alerts us that the time has come to read Kuhnian lessons back into Winch. Those lessons impact Winch's view in at least the following ways. Winchian rules

need to preclude certain cases as mistakes. But just such conceptual exclusions makes incomprehensible if not impossible the sort of conceptual changes familiar to us since *The Structure of Scientific Revolutions*. The 'constitutive' and quasi-analytic status Winch attributes to rules lose their claim to plausibility because in the face of global change the mind neither "crumbles" nor can we conceptually "map out" how changes in one set of beliefs necessarily change others.

Mistakes at *any* level need not be evidence for social rules. But without Winchian rules, why start with the assumption that "the social" exists and needs explaining? The problem becomes not why the idea of a social *science*, but why the idea of a *social* science.

II

The foregoing discussion brings to the fore the question of what sense to make of social kinds in the absence of a theory which explains how these kinds come to be. The issue of how a belief in such kinds survive in the absence of any particular theory about them emerges in critical responses to Stephen Turner's important critique of social theory, *The Social Theory of Practices*.[20] Turner here examines how "practice" and related notions function as part of the explanans in contemporary social theory. He questions whether or not this constellation of notions plays well in the causal-explanatory role in which theorists cast it. Turner argues not. I am very sympathetic to, and largely in agreement with, the line of critique he develops.

But what interests me here is a recurrent critical response to Turner's book. The line is this. Turner has some valid points to make regarding some sloppiness found in the ways in which notions of rule, practice, etc. are utilized for purposes of explanation. But, the criticism continues, the content of the explanans notwithstanding, Turner does no justice to the phenomena motivating the notions he questions, viz., the explananda which elicit that notions he finds so problematic. Since causes must be adequate to the effects, the explanation of social kinds or regularities requires something shared that functions to produce those regularities. Whatever the force or legitimacy of Turner's criticisms, the phenomena remain and require explanation.

Turner, focused as he is on all the problems arising from imputing causal force to practices and their kin, only glimpses in passing that the failure of a certain type of causal account renders problematic as well the explananda those accounts took for granted. An example he discusses exhibits how cause and object disappear together. The case involves Jacob

Burckhardt's classic works, particularly *The Civilization of the Renaissance in Italy*.[21] Turner questions whether or not it is plausible to accept a shared "vision of life" as explaining the artistic and architectural developments Burckhardt studies.[22] But what Turner neglects to emphasize is how the vision is not only is part of the explanans, but required for constituting the explanandum, the relics as constituting a particular kind, as products of a common cause.

A partial appreciation of this point by Turner comes out in the following remarks:

> The existence of the relics is treated as evidence for the existence of an underlying vision. Perhaps the hypothesis is the best explanation – but the evidence used to select between alternative 'vision' hypotheses does not serve to undermine the 'no shared vision' hypothesis. The same problem arises for other cases of collective *explanans* for collective *explananda*.[23]

But the relics require a common factor explanation only if first taken to constitute a "collective phenomenon". In other words, Burckhardt's 'vision' constitutes the object it sets out to explain – the civilization of the Renaissance in Italy.

This point emerges yet again when Turner speculates on how changing the cause – substituting habits for vision – changes what is being explained:

> If acting in accordance with a tradition is acting in accordance with the way of life of a community, and if the way of life of a community includes certain observances, performances and activities and individual habits and mental habits arise through engaging in the relevant performances, nothing need follow with respect to the causal role or status of practice understood as a kind of collective fact. . . . The observances, so to speak, cause *individual* habits, not some sort of collective shared single habit called a practice or a way of life, which one may possess or fail to possess. If this is so, the collective or public facts about traditions or 'cultural systems of meaning' begin and end with the observances or public objects themselves. Everything else is individual – there is no collective fact of the matter at all.[24]

So if, as Turner claims, "there is no collective fact of the matter at all" causally, then the objects of the explanation ceases to have a collective character as well. There is just what individuals did under the circumstances encountered.

I do not see it as a fault of Turner's account that such collective phenomena disappear as possible objects of explanation. I simply insist that it is a consequence of changing the causal story in the way he proposes. Where confusion arises is in retaining objects of explanation even after rejecting the terms which made it possible to speak of those objects.

Consider, in this regard, the question: are social practices objective, something there to be observed? Trevor Pinch, for one, challenges Turner's

doubts about the objectivity of practice talk. But Pinch's answer to those doubts betrays his misapprehension of the relation of object and causes.

Pinch uses the example of Mauss noticing how French women had come to adopt a gait characteristic of American women. Mauss, he writes, notices something to be explained, viz., "that American women walk in a different way and that Frenchwomen start to walk in that way".[25] But in discussing this case, Pinch speaks of 'practice' both as something invoked in the *explanans* – "Turner points out correctly that there are problems in granting causal powers to the practices" – and as *explanandum* – "Turner also correctly points out that Mauss had to use his own cultural membership ... to identify the practice".[26] Pinch finds no problem in asserting that "practices are social in the sense of being shared", meaning here "practice" as an "objective" phenomenon open to observation.[27] These comments makes vivid just how what is to be explained depends upon the prior assumption that there is a common element. But how to specify what needs explaining without bringing back in what Pinch too wishes to deny – a shared mental something?

Can the social object here – the gait – survive as a shared something when what constituted it as a shared something has been rejected? Because he refuses to let the object go, Pinch winds up speaking of the common something that is transmitted:

> Transmission is undoubtedly the hardest thing to study empirically, often because important means of transmission are informal and occur en passant. ... Many practices are specific embodied practices or material practices and neophytes learn to practice these skills in circumscribed institutional environments. In other words transmission is made easier (both to study and for participants) by there being a circumscribed environment ....[28]

Pinch's "explanation" quickly lapses into tautology: learning the practice explains the practice. Thus Pinch brings back in the problems with the *explanans* that he congratulates Turner for being correct to avoid. As long as we have the same term – 'practice' – invoked on both sides of the explanation, no gain results.

Pinch perceives that something is off by attempting to explain practice in this way, but insists that the *explananda* persist as objects of (empirical) investigation: "the issue of how sameness is in fact manifest is again an empirical matter – the solution may be complicated and messy and heavily contingent upon different sorts of circumstances, but this is surely empirical investigation as normal".[29] The Winch problem manifests itself. If social kinds are constituted in a certain way, a way peculiar to our "idea of the social", then once the "idea of the social" goes, so do the kinds. If objects are to be grouped in the first place as social kinds, some other way of picking them out is owed. Again, the problem is *not* that one lets theories

determine kinds, but that the kinds in question are used as justification for theories of a certain sort. *But this is either just to put the theoretical cart before the horse, or it is to hold onto an account of objects even after one professes to have denied the theory constituting those objects.*[30]

Michael Lynch alertly appreciates that the citing of practices occurs in both *explanans* and *explananda*, and follows Turner in rejecting a role in the *explanans* for "non-public collective facts". But, as with Pinch, he denies that this engenders any incentive to shy away from the study of practices qua *explananda*. Turner, he concludes, offers only a cautionary tale regarding "contemporary efforts to theorize practices", and not a reason to think we cannot investigate them.[31] In similar spirit, Pickering congratulates Turner for rightly castigating efforts to account for the seen by the unseen. Pickering proposes focusing instead on "the domain of visible phenomena, and [that] takes the form of the analysis of enduring patterns of practice".[32] (325) My question here is how to cash out the notion Lynch and Pickering appear to retain of "enduring patterns of practice" without assuming objects for which a theory is then needed as explanation.

Other less astute commentators content themselves with simply boggling at the suggestion that Turner succeeds in identifying a problem. There just are, the bogglers contend, collective phenomena in need of collective explanation.

> The idea that these patterns of practice-regularity could be explained by reference to individual habits and public ceremonies alone simply strain the imagination. That no complete psychological theory capable of accounting for the transmission of the habitus has yet been elaborated is hardly a compelling reason for abandoning the entire idea of collective practices, given their obviousness to all who would but look.[33]

> The sheer volume, variety and persistence of social and political theorizing of some species of enabler – of understanding, of successful interpersonal engagement, of relative cultural homogeneity . . . – reflects the ubiquitous sense that 'there is something there'; and that whatever it is, it is difficult to account for its efficacy in orderly relations across and between individuals, if it is not in some way 'supra-individual' – dare one say 'shared'?[34]

> The weighty accumulated insights of 'the Rest' – however flawed, partial, or 'metaphorical' – have chimed too reasonably or seductively for too long with our experience of 'the social realm', for them to be comprehensively displaced by the – not overwhelmingly novel – arguments of one slim volume.[35]

These complaints suggests just how basic this problem is. Ignoring Turner's proposal to let habit do the work previously done by practices, the problem about mistakes illustrates the need for some prior specification of social kinds. But old theorizing habits die hard.[36] Theorists cling to practices as explananda even after the supporting theory disappears, indeed, in the absence of any specific theory at all.

III

The "Winch problem" – how to specify the social in the absence of a prior theory – can be put as a problem for a theory of meaning. Quine states it this way. "Language is a social art. In acquiring it we have to depend entirely on intersubjectively available cues as to what to say and when."[37] What allows language to be a tool collectively available for collective work? What, in short, stabilizes "meaning"? Winch starts with the insight that what terms for social kinds pick out are not natural or self-evident. Winch correctly appreciates that the social is a paradigm, if you will, of a theoretical kind – human made to the core. If one finds unsatisfactory both Winch's ideational account of how people "share a meaning" for the social and its practice-theory counterparts, what then to say of the "meaning" of such terms?

One strategy, of course, would be to insist that the last question wrongly frames the issue, inviting us to take a philosophically fatal first step. For that question asks for an answer in terms of a theory of meaning, a theory of a shared something. But the Siren song of "meaning" still proves too strong for some. Thus David Bloor works to forge a "sociological" theory of meaning and knowledge. Bloor's exposition has the genuine virtue of being clearly stated and intellectually forthright. The shortcomings of his position, I will maintain, are not his alone, but inherent in any position to "externalize" Winch – to move the focus of meaning from a discussion of "internal" or conceptual relations to one which relocates the "criteria of meaning" in "social space". Any such strategy only shifts the unmoved movers of meaning from inside the head to without, and to no better effect. The externalizing move cannot make good on the claim to provide the shared something which stabilizes "meaning" in a way that allows of a prior delineation of mistakes.[38]

Bloor attributes to Wittgenstein an account wherein "the nature of belief, language, reasoning and action" are "to be made intelligible by showing how they arise from human behavior anchored in its material, biological and cultural setting".[39] The operative term here is "anchored". In proposing to locate fixed points, Bloor takes the first step towards a theory of meaning, of providing on Wittgenstein's behalf a "social theory of knowledge".[40] Regardless of whether Bloor rightly attributes this account to Wittgenstein, Bloor clearly endorses the position he outlines in Wittgenstein's name.

Bloor labels his take on a social theory of meaning 'finitism':

> This is the thesis that the established meaning of a word does not determine its future applications. The development of a language- game is not determined by its past verbal

form. Meaning is created by acts of use. . . . [I]t is constructed as we go along. . . . [W]e are to think of meaning extending as far as, but no further than, the finite range of circumstances in which a word is used.[41]

However the details develop, the constituents of "meaning" move outside the head. Meaning so constituted extends "as far as, but no further than" the "range of circumstances" in which words are used. Moreover, the intent of the view is also quite clear: conceptual bounds are not determinative of meaning.[42] Accounting for how a word comes into and stays in play in a given language-game begins, from this perspective, with a detailing of the real world conditions at work in the process.

So, for example, one might imagine a socio-linguistic account of how, in an American context anyway, a term – e.g., 'boy' – evolves from having an innocuous use to a racially charged one by detailing the social contexts. But such accounts provide no fixed bounds for any term, no delimitation the twists of use that might await. (So, 'bad' comes to be a term of approval, or the phrase "That's nice", appropriately inflected, expresses disdain.)

Problems arise insofar as Bloor imagines 'finitism' names not a chronicle of usages, but a theory of meaning. That he so intends it emerges in remarks such as the following: the "semantics of finitism" is "that words are ultimately connected to the world by training, not by translation".[43] Now the claim that "words are connected to the world by training" strikes me as correct. But how does this constitute a semantics, anything that might reasonably count as a theory of meaning or truth conditions?

Bloor appreciates that finitism, puffed up into a semantic theory, faces an obvious problem, what he terms the " 'problem of wide-open texture' ".[44] The problem is simply a variation of the insight that any viable semantic rules must determine a contrast class. As Bloor puts it, "if we allow ourselves to exploit all kinds of criss-crossing similarities, and if we shift the respect in which things are judged similar, then our groupings would eventually include everything".[45] That is, there would be no saying what counted as a mistake. But, Bloor acknowledges, this consequence represents a *reductio* of any proposed semantic theory. Following Bloor then, I make solving the "problem of wide-open texture" the test of whether or not finitism succeeds as a theory of meaning.

Bloor makes two moves on behalf of cutting down the range within which a word can function, and so avoiding the problem of wide-open texture. The first is to tie the common denominator of word use – "family resemblance" – to paradigm cases. Bloor then relativizes paradigms to specific language games (how these are demarcated he provides no

hint), adding that "ancestral link are tacitly present in the precedents and purposes built into specific language-games".[46]

But, of course, cases are only paradigm if they exemplify "defining" features. What are these? Attempts to answer this question brings to the fore two problems. First, the semantics of finitism was to move away from the notion that word use is determined or defined by prior conceptual boundaries. But in giving the semantics of finitism, Bloor quickly comes back to "ancestral links" that are "tacitly present". If this is not conceptual, what then could it be? With regard to language, "tacitly present" either goes proxy for "implied by the meaning" or a related notion. Locating "tacitness" in behavior does not work, for the tacit is tacit only to and for minded creatures. Either meaning is "built into" minds or it is not. Finitism begins with the claim that it *is not* but ends, it seems, with the claim that it *is*.

Second, Bloor intends his account to contrast with overly intellectualized versions of meaning such as he claims to find in Winch. Yet, 'tacit' as noted above, appears to go proxy for "conceptual", for otherwise there is nowhere for the tacit to reside.[47] What alternative is there? Bloor asserts that what "the family resemblance theory does ... is to bring out in a clear and simple way the social and conventional aspects of concept application".[48] Yet finitism as originally stated *denied* that conventional aspects (or anything else) determine meaning. No clue has been provided as to how creatures like us go from some chronicle of instances of use to a language, to forms useful for the interpersonal sharing of information. Without some account of how the semantic trick is turned, nothing worth calling a theory results.

Bloor is quite explicit at some points that whatever the determinants of use (and so of meaning) are, these cannot be cashed out in simple perceptual terms.[49] The basic tension, indeed inherent contradiction, in Bloor's entire project of positing both a finitistic view of language and a semantics based on "use" or "family resemblances" emerges in the following series of remarks: "Bringing the notion of a criterion under the heading of 'institution', and showing how the family-resemblance doctrine highlights the conventional character of concept application, throws us back on our understanding of institutions and conventions".[50] Again, and in light of his denials of the role of perception, Bloor insists (against Winch) "that there is a continuity between our untutored looking and educated observation, and between socially shared concepts and our innate sense of the groupings of things".[51]

But first, whatever "throws us back on our understanding" pushes the process of meaning creation/concept application back inside the head. Yet

that, of course, was exactly where Bloor said the theory should not be. Second, after having denied that "natural" similarity is either necessary or sufficient to explain word use,[52] he then claims that it is certainly necessary for the social that our "shared orientations to the world"[53] requires an "innate sense" of how things group. But if innate, why then assume that the social plays a necessary role? For the finitistic account *starts* on the assumption that the way the world is or has been does not determine how we apply words to it.

Bloor is driven to these difficulties because he appreciates the problem earlier identified in Winch – the analytic relation for Winch between explanans and explananda. Bloor puts matters thus:

> Winch generates intractable problems for himself. He tells us, for example, that to observe anything we must use concepts. But to have concepts, he says, means to have rules, and to have rules means participating in a culture. From this it follow that we cannot participate in a culture on the basis of observation. A relation to society is presupposed by observation, and so cannot be constituted by it. Winch is explicit in drawing this conclusion.[54]

So for Winch the perception of the social cannot be located in the natural. But finitism notwithstanding, Bloor worries that this leaves the world as a place where meaning has no safe harbor, no anchor. Hence, without a biological or "natural" basis, Bloor has no "social" basis for meaning. But even holding aside the fact that Bloor vacillates here, if the basis of the perception of similarity *is* biological, then why must the social have any necessary role in the explanation of human language use?[55]

Bloor's theory cannot resist, as it were, recursion to the mental. The pressure comes in two directions. Although desirous of externalizing the basis of meaning, the world does not offer the sort of anchors a theory of meaning requires. Nature is not so kind. But if no mental markers as well, then no stability, no unmoved movers of meaning. For purposes of providing a "theory of meaning", moreover, appeal to training and paradigms are non-starters. Such accounts reduce to "this is what we do" or "this is what we accept" and as such assume upon but do not explain our practices.[56]

Bloor emphatically rejects Winch's notion of rules as too mentalistic and so essentially anti-Wittgensteinian. But in his desperation to solve the "problem of wide-open texture", Bloor can only anchor his theory in the most unWittgensteinian ways – pushing meaning back into the head by appeal to "tacitness", or resuscitating the myth of the given, in the form of natural fits between percepts and objects. Bloor has made his theory heir to a subject that used to be called philosophy, but not to the branch of philosophical family to which he desires to be related. In any case, he has as yet no theory of meaning and no prospects, given the problems, of developing one.[57]

It cannot be the litmus test of any scientific theory that particular objects be preserved as investigation unfolds. Science (any science) begins with some folk theory about what there is. But it is left to advanced theorizing to determine what the best explanation is of the world as we encounter it. For example, we do not demand of physics that it preserve pride of place for middle sized perceptual objects. Likewise, starting with a folk theory which talks of the social, we cannot require that just these objects be preserved in a final account of how the world works. The folk objects may disappear under the gaze of theory.

What we learn from considering cases of mistakes is just how unformed our understanding is of how mind mixes with the world to form language, of how little we understand how to make sense of our making sense or of even our failures to do so. We perhaps would do better by treating the social as a provisional category. Our mistake just might be not in the specifics of theories of the social, but in thinking that the social constitutes some special realm needing a theory. I offer no brief favoring reduction of what is now called the "social" to something else. Rather, my standpoint is naturalistic and cautionary regarding assumptions too casually made about what needs explaining when examining humans in groups.[58]

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I wish to acknowledge the help and friendship of the late Irving Velody, whose invitation to the conference he organized on the Philosophy of Peter Winch created the occasion for writing this paper. Revisiting Winch's thought after a hiatus of many years served to dispel my skepticism that there was anything left to say about Winch on social science. Going back to that material made me appreciate that his views on the idea of the social remain widely accepted even while his animus towards a science of society does not. I owe much to audiences at that conference (with John Dupré commenting) and at the program for Social Thought and Analysis at Washington University. I would particularly like to thank (without implying responsibility or agreement): Barry Barnes, Bob Barrett, David Bloor, Jim Bohman, John Dupré, Mark Peacock, Piers Rawling, David Stern, Eleonore Stump, Stephen Turner, Joe Ullian, and Alison Wylie. In addition, I was helped by comments from anonymous referees for this journal.

NOTES

[1] See related reflections in Alasdair MacIntyre, "The Idea of a Social Science", in B. Wilson, ed., *Rationality* (New York: Harper & Row, 1970), p. 119. I take it, moreover, that while many can count, very few can specify axioms sufficient for that practice. Recognition of mistakes can occur either in the absence of rules or in cases where knowledge of the rules is, at best, partial.

[2] Following Stephen Turner, I take the key element of the "shared" here to be same something that all genuine participants learn – generally spoken of as tacit – which explains social cognition, i.e., how we come to perceive things collectively as the same or as different. What makes social kinds the kinds they are, on the view I intend to criticize, consists of whatever participants *jointly* and commonly acquire that supposedly explains their social behavior. The move here opposes the view that appeal to common "things in the head" can play an explanatory role. See, in particular, Turner's discussion of these matters in "Social Theory after Cognitive Science" and "Throwing out the Tacit Rule Book", both republished in his *Brains/Practices/Relativism: Social Theory After Cognitive Science* (Chicago: University of Chicago Press, 2002). I elaborate on these points in "Why There is Nothing Rather than Something: Quine on Behaviorism, Meaning, and Indeterminacy", forthcoming in *Philosophy, Psychology, and Psychologism: Critical and Historical Essays on the Psychological Turn in Philosophy*, ed. D. Jacquette (Kluwer Academic 2002).

[3] However, as will become clear in my discussion of Turner's work and responses to it, I take this to be an intractable problem as well.

[4] John L. Austin, "A Plea for Excuses", *Philosophical Papers*, 2nd Ed. (New York: Oxford University Press, 1970), p. 185, fn. 1.

[5] I do not wish to make too much of these cases. Dead animals may raise complex issues, as the marvelous Monty Python skit involving returning a dead parrot to a store shows.

[6] For a nice exposition of Winch's position here, and some of the problems to which it leads, see Brian Fay, "Winch's philosophical bearings", *History of the Human Sciences* (2000) 13: 50–62.

[7] Philip Pettit, "Winch's double-edged idea of a social science", *History of the Human Sciences* (2000) 13: 63–77, p. 64.

[8] Ibid. p. 66.

[9] Peter Winch, *The Idea of a Social Science* (London: Routledge & Kegan Paul, 1958) p. 32. See also p. 58; "Understanding something involves understanding the contradictory too". p. 65. For a contemporary view that takes background knowledge, however tacit, to be constitutive of a distinction between correct and incorrect acts, see Charles Taylor, "To Follow a Rule", in *Rules and Conventions*, ed. By M. Hjort (Baltimore: Johns Hopkins University Press, 1992).

[10] Pettit, op. cit., p. 65.

[11] Peter Winch, "Understanding a Primitive Society", in B. Wilson, ed., *Rationality*, p. 91. "But the difficulty is to see what 'correct' and 'mistaken' can mean in this context".

[12] See Alasdair MacIntyre, "The Idea of a Social Science", op. cit., p. 118.

[13] See discussions in the Wilson anthology cited above. Richard Bernstein nicely recapitulates much of this debate in his *The Restructuring of Social and Political Theory* (Philadelphia: University of Pennsylvania Press, 1978).

[14] John Horton, "Relativism, Reality, and Philosophy", *History of the Human Sciences* (2000) 13: 19–36, p. 27.

[15] Davidson exploits a related view in two very different directions, one against the possibility of genuinely untranslatable languages – why call sounds for which we can't find systematic links a language at all, he wonders – and also against skeptical worries about the possibility of massive error.
[16] Ibid., pp. 31–2.
[17] Ibid., p. 32.
[18] Winch opposes a monolithic conception of rationality. Ironically, however, he promulgates a conception of "social rationality" which possesses just the hegemonic and monolithic features he denies to scientific rationality. A central feature in the shift from Winch to Kuhn in this regard involves not just Kuhn's historicizing of scientific rationality, but also his emphasis that scientific thought possesses less structure than, e.g., Winch imagines.
[19] I identify a strong Carnapian strand in Winch's thought (at least the Carnap of "Empiricism, Semantics, and Ontology") in Ch. 9 of my *Meaning and Method in the Social Sciences* (Ithaca, NY: Cornell U.P., 1987).
[20] Chicago, University of Chicago Press, 1994. Turner deepens and develop these themes in a number of essays in his *Brains/Practices/Relativism: Social Theory After Cognitive Science*, op. cit.
[21] London: Phaidon Press, 1965.
[22] *Social Theory of Practices*, p. 34.
[23] Ibid., 35.
[24] Ibid., 99–100. Likewise, Turner complains that "no account of the process of transmission could explain how the same thing got into different people. Dropping the notion of 'sameness', however, reduced the practices to habits". (79) But in "dropping the notion of 'sameness' ", the objects too drop out, disappear as something in need of explanation. That is, referring to practices becomes just a short-hand gesture back to shared behavior, and so not explanatory of it.
[25] Trevor Pinch, "Old Habits Die Hard: Retrieving Practices from Social Theory", *Studies in the History and Philosophy of Science* (1997) 28: 203–8, p. 206.
[26] Ibid., 205.
[27] Ibid., p. 206.
[28] Ibid. p. 207.
[29] Ibid.
[30] I thank Piers Rawling for helping me clarify this point.
[31] Michael Lynch, "Theorizing Practice", *Human Studies* (1997) 20: 335–44, p. 343.
[32] Andy Pickering, "Time and a Theory of the Visible", *Human Studies* (1997) 20: 325–33, p. 325.
[33] Neil Gross, Review of Turner's *Social Theory of Practices, Theory & Society* (1998) 27: 117–127, p. 127.
[34] Paul Acourt, "Ironic empiricism (apparently) versus the demon of analogy", *History of the Human Sciences* (1995) 8: 107–27, p. 122.
[35] Ibid., p. 121.
[36] As Stephen Turner observes, "Norms, in the sociologist's sense, are identifiable only by observing what happens if they are breached". *The Social Theory of Practices*, p. 28. But the possibility of breaching need not entail some norm common to all of us.
[37] W. V. Quine, *Word and Object* (Cambridge, MA: MIT Press, 1960), p. ix.

[38] David Stern provides an extremely helpful overview of how Wittgenstein has been appropriated by sociologists of science in general, and Bloor in particular, in his "Sociology of science, rule following, and forms of life", *Vienna Circle Institute Yearbook: History of Philosophy of Science – New Trends and Perspectives*, ed. by Michael Heidelberger and Friedrich Stadler (Dordrecht: Kluwer Academic, 2002), 347–67.
[39] David Bloor, *Wittgenstein: A Social Theory of Knowledge* (New York: Columbia University Press, 1983), p. 2. Bloor develops but does not essentially modify this view in his "The question of linguistic idealism revisited", in H. Sluga and D. Stern, ed. *The Cambridge Companion to Wittgenstein* (Cambridge: Cambridge University Press, 1996), pp. 354–882.
[40] David Bloor, *Wittgenstein: A Social Theory of Knowledge*, pp. 2–3.
[41] Ibid., p. 25.
[42] Ibid., p. 27.
[43] Ibid. p. 28.
[44] Ibid., p. 31.
[45] Ibid., p. 31. Bloor imagines he is doing this in the service of explicating what Wittgenstein meant by "family-resemblances", and so avoiding appeal to notions of essences and unproblematic properties as much as possible. I leave it to scholars to sort out what here is Wittgenstein and what Bloor.
[46] Ibid., p. 32.
[47] Bloor, "The question of linguistic idealism revisited", p. 374.
[48] Ibid., p. 33.
[49] Bloor, *Wittgenstein*, p. 39; "The question of linguistic idealism revisited", p. 357, 358.
[50] Bloor, *Wittgenstein*, p. 45.
[51] Ibid., p. 176. "How does social participation ever get started? The only way out of the impasse is to accept that there is a continuity between our untutored looking and educated observation, and between socially shared concepts and our innate sense of the groupings of things. That culture depends on such shared orientations to the world is what Wittgenstein assumed all along".
[52] Ibid., pp. 38-41. An especially relevant passage is perhaps worth quoting in full. "If it is objected that we can literally *see* that red and green are different, we must remember that we can see that light and dark blue are different. . . . So even if there are subjectively straightforward resemblances, and perceptually similar features, this carries no immediate implications for the proper application of concepts. Without the sanction of a language-game we cannot even rely on the move from light blue to dark blue being a legitimate one. Nor is there any reason why we should not attach a colour label to what we call a 'red' object, and then promptly apply it to what we call a 'green' object" (40–41).
[53] Ibid.
[54] Ibid., p. 175.
[55] A much more sophisticated and naturalistic handling of the issues surrounding similarity are found in Quine. As Quine notes, there is no need to go so far as to posit some isomorphism between evolutionarily determined perceptual spacings and objective features of the world. It will suffice to note that "[c]reatures inveterately wrong in their inductions have a pathetic but praiseworthy tendency to die before reproducing their kind". (Quine, "Natural Kinds", in his *Ontological Relativity and Other Essays* (New York: Columbia University Press, 1969) p. 126) Quine argues that appeals to the notion of similarity flag an absence of scientific knowledge, particularly causal knowledge. He is critical of the notion of natural kinds not because he thinks we fail to pick out certain items as all of a kind;

obviously, we do. He rejects "natural" kinds because the notion has no explanatory value; the term 'natural' here is uninformative. So too for "similarity". As he remarks, "In general we can take it as a very special mark of the maturity of a branch of science that it no longer needs an irreducible notion of similarity and kind. It is that final stage where the animal vestige is wholly absorbed into the theory. In this career of the similarity notion ... we have a paradigm of the evolution of unreason into science" (ibid., 138).

[56] Bloor's use of Fleck's discussion of the test for syphilis as a case of the "family-resemblance" theory at work is particularly puzzling. Kuhn uses just this case as a starting point for a discussion of wholesale reconceptualiztion – the Copernican revolution writ small. So, what Bloor takes as a case exemplifying semantic stability Kuhn takes as depicting paradigm change.

[57] Barry Barnes asked me for examples of a non-rule bound account of the social. One on which we agree is Erving Goffman's classic study of "closed institutions", *Asylums* (Garden City, NY: Anchor Books, 1961). There Goffman brilliantly details how certain environments function to shape behavior in the absence of specific rules (or intentions) to shape behavior in particular ways. I would also count as a move in this direction Andrew Pickering's *The Mangle of Practice* (Chicago: University of Chicago Press, 1995). I find his account of "performative historiography" (see, e.g., 230–31) most congenial. But while Pickering professes skepticism about the existence of rules (see especially Chs. 6 and 7), he also succumbs to characterizing his own account as a "theory", indeed a TOE – theory of everything. (246–52). But I would have thought that anti-theorizing about the social would go hand-in-hand with his professed antidisciplinarity. For Pickering explicitly denies that his "performative" account is predictive or explanatory (i.e., does not provide determinative reasons or causes). I can only puzzle as to what he means by "theory" for his own case.

[58] Although it came to my attention after I had written my essay, the arguments in this paper resonate with those offered against biological essences by John Dupré in "Sex, Gender, and Essence", *Midwest Studies in Philosophy: Studies in Essentialism*, V. XI, ed. P. French, T. Uehling, Jr., and H. Wettstein (Minneapolis, MN: University of Minnesota Press, 1986), pp. 441–457.

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