Mathematics and the social order

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Abstract

> The sociology of knowledge (a subfield of the discipline of sociology introduced by Karl Mannheim) famously excluded mathematics from its purview. In this essay, I seek to argue that there indeed are some ways in which mathematical ideas can be analyzed sociologically, especially if we employ some of the insights of philosophers, especially the later Wittgenstein.

PREAMBLE

This paper is produced to celebrate the career of my former colleague, Professor Michael Lynch, whom, as Chairman of the Department of Sociology at Boston University, I was able to attract to join our faculty. (He later left for the Ivy League Cornell University).

> The architect and the engineer have their tables of trigonometric functions, logarithms, and the like. But they do not need to remember that these are calculated from a large number of terms of certain appropriate infinite series. They draw freely from an inexhaustible reservoir of mathematical curves and surfaces, but they do not need to be conscious of infinities (Zeppin 2000, 5)

EXTENDING THE SCOPE OF THE SOCIOLOGY OF KNOWLEDGE

In a previous work, I addressed the so-called 'sociology of knowledge' (Coulter 1989). There, I developed a critique of the work of Mannheim and Bloor, both of whom sought to advance a thesis which could describe the bases of all forms of knowledge in social life. My critique of both was concerned with their causal preoccupations, not with their ultimate goal, viz., their notion of an all-embracing conception of a sociology of knowledge. Bloor, in further work, sought to plug a space left open in Mannheim's original work—logic and mathematics. In an essay published several years ago, I tried to address the issue of logic and language within an ethnomethodological orientation, but in that essay I spent only a few paragraphs on mathematics (Coulter, 1991), surely a major component of human knowledge. However, I neglected in that essay and in my book on mind to address the residue which Bloor took up in stark terms: the prospect for a sociology of mathematics. Here, I will attempt to deal with that lacuna. In this brief essay, I wish to distinguish sharply between what could be termed 'a sociology of mathematicians' (an elaboration of the history of that discipline which could be thought of as an interesting extension of the sociology of the 'professions', a well-established component of contemporary sociology) and a sociology of *mathematics as a discipline*, and, in pursuit of this objective, I seek here to address the following problem: does the Mannheimian exclusion of mathematics from the purview of the sociology of knowledge hold up? This entails a detailed consideration of what to many has been an abiding puzzle for what could be meant by a 'compulsion' which is *non-physical* in nature? How, say, could a bit of mathematics 'force' or 'compel' a further move in mathematics (or in anything else)? A cognate problem, often termed that of '*logical* compulsion', has been addressed before by several philosophers and logicians, most successfully in my view by J. F. M. Hunter (1973) who concerned himself with an elucidation of what he felicitously termed 'the formalist complex'. He wrote:

> We look the wrong way upon the formalist complex: we think we have had to acquire it because of the nature of and the relationships between formal concepts; whereas I am suggesting that it is because we have the attitude that we are disposed to attribute to formal concepts that clarity and definiteness of relationship that anyone will tell you is their hallmark. We *create* that clarity and definiteness by our absolute refusal to tolerate difference of opinions in formal contexts, by drilling students remorselessly in definite ways of proceeding, and by exuding and encouraging anxiety about the correctness of results, and double-checking to be sure. (177, italic in original).

In these comments, Hunter is clearly (and explicitly) drawing upon some of Wittgenstein's observations in his *Remarks on the Foundations of Mathematics* (1956, hence: *RFM*) where he insists that interpersonal drill and training lie at the basis of the acquisition of any mathematical capacity or skill: the apparently transcendental authority of mathematics is first of all personified in the *authority* of the teacher/instructor. Children, students, who refuse to accept (or to succumb to) the mathematical instruction(s) of the teacher cannot acquire the relevant mathematics: however, that many students do accept the rigorous strictures embedded in their instruction, *refrain from insisting* upon the free reign of their (perhaps *contrary*) opinions or judgments, is the social fact which assures the authority of the mathematics. In the *RFM*, Wittgenstein writes:

> Now we talk of the 'Inexorability' of logic; and think of the laws of logic as inexorable, still more inexorable than the laws of nature. We now draw attention to the fact that the word 'inexorable' is used in a variety of ways. There correspond to our laws of logic *very general facts of dolly experience*. They are the ones that make it possible for us to keep on demonstrating those laws in a very simple way (with ink on paper for example). They are to be compared with the facts that make measurement with a yardstick easy and useful. This suggests precisely these laws of inference, and now it is we that are inexorable in applying these laws. Because we 'measure'; and it is part of measuring for everybody to have the same measures… (36e, italics added).

This nicely dovetails with the following remark: "I want to say: it is essential to mathematics that its signs are also employed in *mufti* (*im Zivil*). It is the use of mathematics outside of mathematics, and so the meaning of the signs, that makes the sign-game into mathematics."[1] (*RFM,* IV 2, para. 257). Some initial hints may be garnered by inspecting a few subtle paragraphs in the *RFM* such as this one: "Test the justification of this expression in this way: 'You cannot survey the justification of an expression unless you survey its employment; which you cannot do by looking at some facet of its employment, say a picture attaching to it'" (*RFM*, 18). Having previously foregone his earlier commitment to a 'picturing' conception of meaning, this remark dovetails well with his major arguments of the *Philosophical Investigations* (hence: *PI*) about the ways in which appeals to proper use (employment) comprise answers to issues of what something might mean (where 'meaning' here is restricted to the concept of 'intelligibility', and not to a cognate such as: 'personal significance'). Continuing:

> The drawing of a Euclidean proof may be inexact, in the sense that the straight lines are not straight, the segments of circles are not exactly circular, etc., etc., and at the same time the drawing Is an exact proof; and from this It can be seen that this drawing does not—e.g.,—demonstrate that such a construction results in a polygon with five equal sides. (*RFM*, 160e).

Harking back to a comment in the *PI*, Wittgenstein here alludes to the fact that 'exactness' is purpose-dependent (*PI*, para. #88). Further on in the *RFM* we read: "A rule qua rule is detached, it stands as it were alone in its glory, although what gives it its importance is the facts of daily experience".

MATHEMATICS IN MUFTI

One of Wittgenstein's favorite analogies throughout his later writings was the game of chess. Chess is characterized by strict constitutive rules (e.g., a bishop may only be moved along squares of its own color, etc.). However, such rules cannot determine winning moves, only game-acceptable ones. Using this analogy, LW states: "... if we agree [on the proof of a mathematical proposition] then we have only set our watches, but not yet measured any time". (*RFM,* V para. 2) I think that this may be interpreted as follows: there is no empirical content to any mathematical proposition, unlike the fact that if I can tell the time by consulting my watch (as long as it is working—an important point here), I can derive an empirical proposition, e.g., It is currently 4.56 pm. LW further comments: "so it is not the ratification [of the result of a calculation—JC] by itself that makes it calculation but the *agreement of ratifications*" (*RFM*, 164e, italics added). These comments never implied a consensual or merely conventionalist conception of the nature of mathematical theorems and proofs. So what do they imply? Recall LW's distinction in the *PI* between agreements in definitions and agreements in judgments. Here, the issue has become starker: what is such a difference in the current context? In para. 208 of the *PI*, LW wrote:

> Then am I defining 'order' and 'rule' by means of 'regularity'? How do I explain the meaning of 'regular', 'uniform', 'same' to anyone? ... I shall teach him [a neophyte – JC] by means of *examples* and by *practice*.

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1 *Mufti*, and *im Zivil* here mean something like: 'non-specialist', 'in ordinary, non-technical civilian life'

> … and also to continue progressions. And so, for example, when given: …… to go on: ………….. I do it, he does it after me, and I influence him by expressions of agreement, rejection, expectation, encouragement. I let him go his way, or hold him back; and so on. … We should distinguish between the 'and so on' which is an abbreviated notation. 'And so on *ad inf.* [*Infinitum*—JC] is not such an abbreviation. The fact that we cannot write down all of the digits of pi is not a human shortcoming, as mathematicians sometimes think.

Indeed, insofar as computing an expansion of *pi* to *some* determinate number *n* is concerned, this is certainly not the same as, e.g., computing *the* expansion of *pi simpliciter* (I owe this insight to Professor P.M.S. Hacker). In our schoolrooms, we probably have gotten as far as this: 3.1415, and little further! That it *could be expanded further* is certainly true, but then so could any other formula which has ad *infinitum* extended to a formula which expresses it.

THE NORMATIVITY OF MATHEMATICAL REASONING.

In paragraph 24 of the *RFM*, LW writes: "To say: 'these 200 apples and these 200 apples come to 400' means: when one puts them together, none are lost or added, they behave *normally*" (italics in original). And then there is this ensuing one: "… 'The process of adding did Indeed yield 400' but now we take this result as a criterion for the correct addition—or simply: for the addition-of these numbers." (italic added). In a subsequent paragraph, LW says that "though what interests us is, not the mental state of conviction, but the applications attaching to this conviction", and, further: "The common thing seems to be that by the construction of a sign I compel the acceptance of a sign". However, *who* is 'compelled' here? Certainly not an innumerate person, a mentally-disordered person, a very young child, an autistic child, a pre-literate person, an elderly victim of Alzheimer's, etc. The very invocation of the word 'normally' In this context appears to open up the sluicegates to abject relativism. Does this really portray LW's convictions? He draws back from such a radical inferential prospect in paragraph 65:

> Are the propositions of mathematics anthropological propositions saying how we may infer and calculate?—Is a statute book a work of anthropology telling how the people of this nation deal with a thief, etc.?—Could it be said: 'The judge looks up a book about anthropology and thereupon sentences the thief to a term of Imprisonment'? Well, the Judge does not use the statute book as a manual of anthropology.

There are many ways in which to (mis-)read this paragraph. For example, in his famous paper on *Wittgenstein's Philosophy of Mathematics*, Michael Dummett, the great Frege scholar, writes: "Consider the case of an elementary computation, for example, 'S + 7 = 12'. There might be people who counted as we do but did not have the concept of addition." (1966, 430) But: Who counted as WE do? Then, of course, such folk *would* compute the sum of 5 plus 7 In the same way in which WE do, unless I am misreading the extension of the concept of 'we' here. After all, 'addition' is merely a natural complement to simply counting, and why should 'counting' be restricted to mere enumeration and nothing further? And is *it* even imaginable that there might be some people somewhere who can count but not add? One might also suggest, of course, that enumeration must on occasion involve addition, since, of course, 1 + 1 = 2, and so on…! So, is this sheer enumeration, i.e., that we count 1 and then 2 and then come to

3? Or: is it addition? What is the issue here? Well, it could be this one: Even lacking any explicit arithmetical concept of addition, add we do and always have. When kids learn basic arithmetic, they are taught addition, subtraction, multiplication and division. Later, these building blocks can be extended. The key issue is: how? And in what ways? And, most significantly, for what purpose(s)?

Take a look at this one (*RFM*, para. 15): "The squint-eyed way of putting things goes with the whole system of pretense, namely that by using the new apparatus we deal with infinite sets with the same certainty as, hitherto, we had in dealing with finite ones." Enumeration, then, although obviously central to various sorts of computational procedures, is far from being a defining property of mathematical reasoning. Take the example of 'inference'. We read, in paragraph 17 of the *RFM*:

> 'The mind carries out the special activity of logical inference according to these laws' [laws of inference—JC). That is certainly interesting and important, but, then, is it true? Does the mind always infer according to *these* laws? (italics in the original—JC]. And what does the special activity of inferring consist in?—This is why it is necessary to look and see how we carry out inferences in the practice of language; *what kind of procedure in the language-game inferring is*. [italics added—JC].

Ignoring here the invocation of 'the mind' in this passage, once again, LW appeals to the everyday life of ordinary practices of inferring in order to, as I would put it, de-psychologize the ways in which philosophers have occasionally been tempted to portray it. Whenever he concerned himself with any issues about the acquisition of mathematical as well as logical competence(s) more broadly, Wittgenstein seems to have emphasized, above all else, the *socializing discipline* to which we all as kids learning such formal disciplines have always experienced. Further, we read in *RFM*, 67e: "The calculation takes care of its own application." The calculation *takes care of its own application*. Yes, perhaps, as long as those who are adept at such a calculation are among those hinted at here ... And further on we read this:

> But then doesn't it need a sanction for this? Can it extend the network arbitrarily? Well, I could say: a mathematician is always Inventing new forms of description. Some, stimulated by practical needs, others, from aesthetic needs,—and yet others in a variety of ways. And here imagine a landscape gardener designing paths for the layout of a garden; it may well be that he draws them on a drawing-board merely as ornamental strips without the slightest thought of someone's sometime walking on them. (*RFM*, 166:a)

It has seemed to some, reading remarks like this, that Wittgenstein was espousing a 'sociological' conception of mathematical necessity. Bloor, for example, argues that one can use the *RFM* to correct a lacuna in the sociology of knowledge developed by Karl Mannheim whose program explicitly excluded both logic and mathematics from the purview of his treatment of forms of knowledge as socially constructed (Bloor 1973). The reason for their exclusion was essentially this: logic and mathematics deal with absolute truths, which means that nothing extraneous might even be in the least relevant to understanding their many manifestations.

At first blush, this appears plausible. However, *we do* have a well-established discipline in the *history* of mathematics, so why can't we have a *sociology* of mathematics as its logical extension? We are reliably informed by studies in the history of mathematics, for example, that it was the Persian thinker Al-Khwarizmi who, in the 8th century AD, formulated the concepts of *algebra* and *algorithm* (the latter clearly named after its originator). We can trace the foundations of *calculus* to the work of Leibniz and Newton.[2] However, one must be cautious here lest we be tempted to think that if mathematical *discoveries* are, *au fond*, essentially mathematical *inventions*, that their power to *compel* is illusory. This does not follow, because construing mathematical reasoning as a rule-governed activity does not entail that once the rules of, say, inference, have been established one can proceed to break them *with impunity*. Of course, the compulsion we feel is genuine primarily because we are operating *within* a *conditions-based system* of activity: thus, if we wish to play the language-game of addition, we *must*, cannot do otherwise than, consent to the truth, the necessary truth, that 2 + 2 = 4, and not 5 or some other integer. Mathematical propositions are thus depicted by Wittgenstein as *grammatical* propositions, in his extended sense of 'grammatical'. Grammars of natural languages are surely humanly created, but once one operates within the purview of any particular grammar, one's moves are constrained by adherence to the requirements of that grammar. Mathematics forms a *component of* grammar, but one which is especially stringently enforced amongst its users. One can in the purely physical sense of that word say or *claim* that 2 + 2 = 22, but then one is not doing mathematics, but, rather, playing around with numbers or some such thing. In Oilman's terms:

> Wittgenstein denied that the meaning or structure of our propositions Is the source of the necessity we find in deductive inferences and mathematical calculations, and so rejected the analytic view of necessary truth. If anything he reversed the relation we find in such a view between meaning and necessity. Propositions which we regard as necessary are rules of our language-games; they characterize our language-games. They are formulations of established practices with words, and it is these practices which give meaning to our words. (1984, 95)

In his well-known study of Wittgenstein's philosophy of mathematics, Stuart Shanker (1987, 314) elaborated on the theme that Wittgenstein sought to portray mathematics in other than transcendental terms as it had often been portrayed in earlier philosophical discussions (including by Wittgenstein himself): "To be sure, the motivation for the construction of a new number system is generally either to satisfy some practical need that has arisen (as in, e.g., the construction of logarithms) or else to pursue some mathematical interest that has developed (e.g., the construction of imaginary numbers). Insisting that mathematics depends upon securing agreement among practitioners, LW noted that:

> If constant quarrels were to erupt among mathematicians concerning the correctness of calculations, if for instance one of them were convinced that one of the numbers had changed without his noticing it or his

2 An excellent review of the history of mathematics can be found in Bell (1937).

memory had deceived him or someone else, etc. etc., - then the concept of 'mathematical certainty' would either not exist or it would play a different role than it does in fact. (1992, 2se)

As Baker and Hacker made clear:

This is not to say that the certainty of mathematics is based on the reliability of ink and paper, or on mathematicians' mnemonic powers. But rather, that such facts are preconditions for having and using these techniques of representation. The 'certainty' is located, as it were, *within* the technique, and not in mathematical facts or in the empirical preconditions of the language-game. (1985, 294, italics in original).

To clarify further the sort of relationship which can obtain between what we call 'necessary' propositions (ones which cannot be shown to be false by any empirical inquiry or experiment) and 'contingent' ones (which could be shown to be false by an empirical Investigation), Vendler proposes the following helpful analogy:

Suppose that while watching a game of chess I see two pawns of the same color standing in the same column. Then I say: 'One of them must have taken an opposing piece in a previous move.' How do I know this? Is it sufficient to say that in all chess games we ever witnessed this correlation held? No, *given the rules of the game*, the relation holds *a priori*; the contrary is not something unusual or unlikely: it is inconceivable. Nor is this an analytic connection in the Kantian sense of the term: any given position on the board is perfectly comprehensible without historical data (think of chess puzzles). One might never realize the connection, but once it is noticed, one sees that it cannot be otherwise. (1971, 255–56)

Saul Kripke made a cognate point in what follows:

They (some philosophers) think that If something belongs to the realm of *a priori* knowledge, it couldn't possibly be known empirically. This is just a mistake. Something may belong in the realm of such statements that can be known a priori but may still be known by particular people on the basis of experience. To give a really common sense example: anyone who has worked with a computing machine knows that the computing machine may give an answer to whether such and such a number is prime. We, then, if we believe that the number is prime, believe it on the basis of our knowledge of the laws of physics, the construction of the machine, and so on. We therefore do not believe this on the basis of purely *a priori* evidence. We believe it (if anything is *a posteriori* at all) on the basis of *a posteriori* evidence... So 'can be known a priori' doesn't mean 'must be known *a priori*'. (1972, 2611)

In a striking remark, Hunter notes that: "What people get from our instruction can be *very indirectly* connected to the Instruction itself." (op. *cit*., 195, italics added) When I teach my child to multiply using certain numbers and functions, I use as an example a certain mathematical expression (usually a very simple one). If and/or when she 'gets the hang of it', she can do 'the same thing again' but with *new* numbers. Learning how to count, add, subtract, multiply and divide as a neophyte sets the stage for as yet *unspecified further* (than the original teachable samples) performances. Chomsky thought that this *must* mean that the inculcation of what he rightly termed a 'generative' capacity required more than socialized drill and training: e.g., *innately* given capacities. However, this betrays a very narrow conception of what 'learning' means. When I teach my kid to ride a bike, her trained capacity is *not* restricted to just *this* bike and to just *this* arena of riding it. (I owe this example to Wes Sharrock). Nonetheless, this does *not* justify the idea that my kid has somehow internalized a 'grammar' in some sort of propositional terms about the ergonomic requirements which enable her to keep her balance (which do indeed describe abstractly what she can now do).

CONVENTIONALISM?

The rejection of Platonistic 'realist' philosophies of mathematical necessity and compulsion does not lead us to embrace any form of 'conventionalism' of the sort which could metamorphose into a Bloor-type *sociologism*. Navigating our way through these thickets leads us to the following conclusion: there has not been a coherent 'philosophy of mathematics' which could specify *independently* of the actual, praxeological instances of mathematical practices, in what mathematical compulsion could consist. One might say just this: mathematics, when orderly, is in order *just as it is*… it needs no *extra*-mathematical justification, foundation nor any other apparatus which might elucidate it: it is, so to say, *self-elucidating*… Fine, but in what sense is this to be assumed? It looks just right as it stands…

In a well-known paper by Greiffenhagen and Sharrock, entitled *Mathematical Equations as Durkheimian Social Facts?*, the following argument is advanced:

> The difficulty in specifying what exactly the question 'Must 2 + 2 =4?' is asking [gives rise to the following question—JC]: Of *whom* is this question being asked? Asked of a professional mathematician, understood as asking 'Is there more than one arithmetic system?', it is mathematically trivial that there are. Asked of ordinary users in the street, unprepared for it, the question may well create difficulties, since it Is not clear in what sense those using the default system understand that it is a distinctive system, let alone what 'other systems of arithmetic' might be. This is not because they are unaware or unfamiliar with 'other systems of arithmetic' (most people are familiar with twelve-hour clocks in which 11 + 3 = 2), but because the sense of the question is unclear. (2009, 127).

These authors continue: "There is a difference between the necessity of adopting a system and the necessity imposed by an adopted system." (128, italics in original). If, for example, one adopted the 'number wheel system' for making computations, then 2 + 2 = 0. If one understands the number wheel system, then the answer to 'what is the solution to 2 + 2?' *must* be '0'. However, this solution in no way affects the mundaneity of the use of the default system in everyday life. Mathematical equations are usable as rules for the transformation of quantities in our lives. Are we then claiming that mathematical truths are true by *convention*? Not at all, according to Wittgenstein. Conventions cannot make propositions *true*, although they play a major (even a constitutive) role in establishing their *intelligibility*. Sense is prior to truth. Nonetheless, in his historical study of the origin of the concept of 'zero', Charles Seife (2001) discusses many variants in the history of mathematical ideas and grounds them in the socio-historical contexts within which they emerged. His is not in any sense a sociological study, but it does elaborate several themes of interest in this discussion. For example, consider the following:

> Thanks to the very nature of numbers—they can be added together to create new ones—the number system didn't stop at three [as it did in the number systems of the Siriona Indians and the Bororo peoples of Brazil]. After a while, clever tribesmen began to string number-words in a row to yield more numbers. The languages currently used by the Bacairi and the Bororo peoples of Brazil show this process In action; they have number systems that go 'one', 'two', 'two and one', 'two and two', 'two and two and one', and so forth.. (Seife, op. cit., 7).

Werner Stark (1967, 162) further reminds us of the socio-cultural diversity exhibited In the use of numbers: he points out that the ancient Chinese ranked the number 12 higher *in their system* of values than the number 13, but this in no way stood in contradiction to their understanding of mathematical calculation: they knew full well that, e.g., adding 1 to 12 makes 13. However, as Stark comments:

> ...when the Chinese said that they preferred 12 to 13... they merely believed that metaphysically a group of 13 is not so propitious as a group of 12. In other words, they were talking metaphysics, not physics, magic, not mathematics, when they placed 12 'above' 13. If we may so express it, their mathematics was like ours, but it was overlaid with magic...

In *The Lectures on the Foundations of Mathematics*, (*LFM* para. 41) LW (1989) remarked that:

> It is of course in the second way [25 times 25 = 625] that we ordinarily use the statement that 25 times 25 = 625. We make its correctness or incorrectness independent of experience. In one sense it is independent of experience, in one sense not.

Yet, of course, such a statement depends upon the idea that when we use mathematics *in mufti*, the equation can still work. But what If we have an (empirical) problem in weighing, say, oats in batches and we cannot make that equation work for us? Could that (and cognate empirical facts) be used to undermine the *pristine* calculus in any way? Here, we return to the notion of 'how things ordinarily are/ and work out'. It was an abiding theme in LWs later works (which I would wish now to speak of as 'sociological' in a minimally disciplinary sense of the term) that many of his examples depended upon some data of *praxis* in ordinary affairs without theorizing any of them to suit an a priori purpose. In *RFM* (20), LW asserts that 'logic' shows us what we could *possibly* construe as a 'proposition', and not *vice-versa*. And now we must ask, once again, who are the 'we' here? The best guess is: 'those who have been successfully trained to follow the rules of inference required to make the correct deductions'. Otherwise, we are speaking of an anarchy which might only be halted by some authoritarian rule *by decree*. Wittgenstein frequently uses terms like 'use', 'usage', 'custom' and 'our natural history' in the context of his considerations of what make 'proofs' the proofs that they are (accepted to be) and what make 'axioms' the axioms that they are (See, *inter alia*, Wittgenstein's

RFM, p. 114). He is far from proposing a conception of necessity or compulsion in terms of 'majority rule', *but* if most people of suitable capabilities did not agree most of the time on what counts as 'proofs' and 'axioms', then these very concepts would be devoid of sense.

COMPUTATIONAL EQUIVALENCES IN COMMUNICATIVE CONTEXTS

In a brilliant essay, Harvey Sacks considered some aspects of lay, everyday uses of numbers, and addressed especially the mundane distinction we make between 'precise' and 'imprecise' numerical references. I here reproduce three striking passages from his paper.

> Now all numbers could be considered equally precise I suppose, but if you want to do something approximate then you use 15, and if you want to do something precise you use 19. And they're received that way. If you say 'I'll be there at 9.00' or 'I'll be here at 9.30', it's a perfectly acceptable thing to say, and if you arrive at 9.10 or 9.40 it's unnoticeable. If you say 'I'll be there at 9.32', people figure that you're going to be there something closer to 9.32 than they would expect if you'd said 9.30, though 9.30 is just as good a number as 9.32. Besides which, they can ask you why you've given such a precise number (1988, 54).

Further, we read that "number selection for various purposes is not random. 9.00 and 9.01 are not very far apart, but they're quite different sorts of numbers" (ibid., 55). These remarks follow from this one:

> A logician would propose that 'November eleventh nineteen sixty seven' is, e.g., 'the clearer answer' and can replace 'Tuesday'. There are places where It can. There are others where, if you're talking about developing a notion of 'synonyms' which captures how conversation must proceed with respect to equivalences, then they're just not equivalent (ibid., 53).

In addition, note that a computational equivalence does *not* entail 'identity' in the use of a numerical formulation across contexts of usage. We can express a number in one or more words, one or more integers, as a fraction, in decimal notation or in percentage notation. For example, three-quarters (words) can be expressed as ¾, as 0.75, or as 75%. There clearly have to be, in place so to speak, some special communicative purpose which animates a choice between these options. Consider the sorts of circumstances (pedantry aside!) in which one would select the decimal representation over the verbal one, the fraction over the percentage, and so on. Moreover, there are cases in which equivalencies are more subtle. For example, civilians are used to clock-times being reported in 'civilian' time registers, it is 4 pm or 3.25 am: but in military contexts, 4 pm = 1600 (post meridian), and 3.25 am =1525 (ante meridian).

In physics, astronomy and related disciplines, the use of mathematical reasoning has been deeply embedded for centuries, with amazing results. This fact has, on occasion, blinded logicians to the observation that whatever calculi have been adopted were adopted to serve a *special* purpose. None of these disciplinary accomplishments vouchsafe the notion that mathematics is omnirelevant to their varied operations of reasoning. In addition, there is one major arena in contemporary social science within which mathematical computations are assumed to be essential: that arena is, primarily, economics. So, how does mathematics *actually enter* *into* economic calculations? it appears, yet again, that the pragmatics rule the algorithmic, if one is looking at how economic calculations *actually* work in the arenas of the life-world within which they operate. Indeed, so much so, that, according to Anderson, Hughes and Sharrock (1988) the ways in which entrepreneurs actually *make* their computations deviate from any sort of idealized model for any such calculations which might be advocated by a theorist who seeks to apply the models advanced by econometricians. Econometrics is a facet of the discipline of economics which sustains the notion that, unless an economic calculation ls derived from a purely idealized mathematical model, then it must be defective or, at best, it ought to be suspicious. But does econometrics rule the roost, so to speak? In some domains, it has been successful, but only if one adheres to the required idealizations which are required to 'make it work'.

In their work referred to here (Anderson, Hughes, and Sharrock, 1988), it is quite clearly demonstrated that calculations-in-vivo can trump calculations-in-abstracto for certain purposes. Of course, a sociological inquiry into the uses of mathematics in everyday life must pay attention to the varied purposes for which such a resource is deployed: how, and in what way, and for what reason? And this is one major lesson taught by noting the embedding of mathematics within the lebenswelt within which it emerged and still has its potency.

Nothing in the foregoing is meant to disparage the multivariate uses of mathematics. We all of us depend upon it in our quotidian lives. Nonetheless, reifying and transcendentalizing its actual character, as if it were some form of extraneous imposition upon us mere mortals who are merely its recipients (when or if we can grasp its manifold significations) defies the basic fact which I wish to bring to the fore in this paper: mathematics in all of its guises, roles and uses is a social phenomenon, albeit a domain with some very special aspects and so many varied manifestations in our lives.

CONCLUSION

In this paper, I have attempted to argue for the expansion of the traditional foci of the sociology of knowledge to include our knowledge of, and use of, mathematical concepts, In the spirit of David Bloor's path-breaking work. One aspect of this effort, which in some respects comprises a contrast with Bloor's contribution, is my focus upon what could be termed a 'praxiological' approach. It is my hope that what has been argued here can be expanded by those skilled in cognate regions of mathematics as well as the sociology of knowledge.[3]

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