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Can Mathematics Be Social? Flexible Representations for Interaction Process and Its Sociocultural Constructions

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Mathematics is needed especially and primarily for helping coax social phenomena sufficiently into view to permit the sorts of reconstruing, manipulation, and measurement on which productive insight depends. I develop this view in two themes, language use and social space-times, along with two morals, and several object lessons, all leading up to the Knotty Future. I conclude with a discussion of pitfalls in this view.

KEY WORDS: network; sociolinguistics; deixis; grammer; identity.

INTRODUCTION

The answer to the question in the title is "Yes," in several different senses—perhaps most obviously in the intricately social nature of the work of mathematicians.[2] But I shall concentrate on how mathematics can both deepen and sharpen analytic insights into social flows—and thereby offer abilities to maneuver the social. I emphasize just two themes. Within each theme I take slants that are particularly sociological and yet insist upon the intertwining of cultural with social. A prefatory section draws out two morals, and later object Lessons precede the "Knotty Future," after which appears a brief summary and conclusion.

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[2]Eric Livingston (1983), himself trained in mathematics, has brought the sceptical eye of the ethnomethodologist to bear on constructing proofs as the central work of mathematicians. Morris Kline (1980), a distinguished senior mathematician, had already bewailed the disproof of proof that has been accomplished within mathematics in the 20th century.

The nub of being social is inducing a new reality, a social reality as to level/arena/space-time that is constructed through and in interaction. That is the central phenomenon and thus the prime target for mathematical aid. This construction first happened hundreds of milennia ago with the first tribes of humans. This construction of the social can be seen as analogous to the new level of reality induced billions of years earlier with the emergence of life among soups of macromolecules. For how mathematics illuminates the latter, see a brilliant yet readable survey of models written by Hofbauer and Sigmund (1988, esp. Part III). Their survey extends as far as game theoretical approaches (Part IV) which are applicable to social action. But all their models can be at least suggestive about the social.[3]

Indeed, a major thread running through earlier mathematical models in sociology has been borrowing not just technique but also construct-by-metaphor from all sorts of engineering and natural science disciplines. Jim Coleman's seminal *Introduction* (1964) concentrated on stochastic processes among discrete states along lines that had been developed earlier for queueing models in operations research and for rate-control calculations in chemical engineering, in which Coleman obtained his undergraduate degree. I early proposed models of kinship systems and of vacancy chains (White, 1963b, 1970a) that were influenced by the solid state physics in which I was first trained. Still other contributors have been trained in technical fields that abut and even overlap with social science, such as demographic and actuarial mathematics (e.g., Nathan Keyfitz, 1968), epidemiology (e.g., N. T. J. Bailey, 1957), ecology (e.g., Joel Cohen, 1978), as well as mathematical statistics especially concerned with pattern in structure (e.g., Ulf Grenander, 1993).

But pride of place in the history of mathematical social science must be accorded to works that helped bring mathematics of several sorts apt for social analyses into the undergraduate syllabi of many colleges, notably works co-authored by mathematician John Kemeny (e.g., Kemeny *et al.*, 1957). This made it possible to recruit cadres of mathematical analysts who could go beyond borrowing the calculus. Calculus remains with us, of course, even when as inapt as it is in mathematical economics, exactly because of the enormous sunk cost in established curricula and technical literatures in mathematics. Recently, however, with the growth both of computing machines able to support simulation and experiment in mathematics, and of an astonishing number of new pure mathematics beyond algebras and topologies, as in combinatorics,[4] modeling approaches to so-

[3]Also this survey reminds us that the wheel has turned back to a previous era in which reading work in German was important to keeping up to date with mathematical sciences.

[4]See, e.g., Beth *et al.* (1993); and for an applied perspective see Cameron (1994). Here and hereafter I shall give specific citations only to works that I think you might find useful currently: I will be neither scholar nor antiquarian.

cial phenomena are becoming much less constrained by inherited technique of long-standing.

These early and/or introductory books that I have been naming presuppose relatively little prior math and so can be very useful to someone today seeking accessible orientation and assessment. To guide your possible entry, there is of course no substitute for scanning current overall journals on social modeling to see what is up just now. For sociology, these include the *Journal of Mathematical Sociology, Rationality and Society, Social Networks*; for example in Europe, the *Bulletin* put out at CNRS in Paris and in Japan, the *Newsletter* of the Japanese Mathematical Sociology Group.

I continue by elaborating two morals that derive from my own earlier work in math modeling, before turning to two major themes on today's frontiers.

TWO MORALS

"Corner" Solutions and Cherry-Picking

Existing math mostly engenders tight-fitting mechanisms built from certified parts, which must be fit together to specified tolerances—but that then are guaranteed not to fail, a bit like the build-it-yourself kits that we all buy from discount stores.[5] Given that modeling still mostly retrofits existing math, what are the most fruitful strategies?

FIRST: Look for "corner" solutions. Given a complex mechanism put at your disposal by a previous modeler, likely from another discipline, look for corners within his [*sic*] room of valid solutions that he [*sic*] disdained to sweep up. For example, my market models of the 1980s (White, 1981, 1988) derived from solutions that economist Spence had not bothered about in his brilliantly conceived models of market signaling (Spence, 1974).[6] They were, in fact, literally "corner" solutions in the jargon of calculus, solutions for parameter values where usual smooth continuity did not obtain. Unraveling of social (though implicit) agreements is the phenomenological import of these corner solutions of mine.

I thus adapted Spence's cognitive approach to a social-construction-of-market by producers, construction as interface of sustainable terms of trade (nonlinear pricing). I now wish I had emphasized more that my dis-

[5]The funniest piece that Harold Garfinkel has ever contributed to ethnomethodology (and that, characteristically, is in a still unpublished collection edited by Anne Rawls) is about interpreting and implementing instructions for a do-it-yourself kit on his living room floor just before a party.

[6]These are some of the very few social science models centering on bona fide applications of partial differential equations—the mainstay of mathematical physics. Spence leaves this implicit in his thesis, which was advised by a master of exposition, Howard Raiffa.

covery of and belief in this social construction came from seeing where and how it came apart: from Spence's powerful framing, I could tease out an ineluctable competitive unraveling of the agreement at certain parameter corners. I call this "unraveling" because it starts from an actor in an end-point location and then unzips further, perhaps totally. Maybe emphasizing these "corner" unravelings more would have intrigued more people into empirical canvasing for further development of these models.[7]

My whole book on vacancy chains can be seen as just a "corner" solution to the general (and very difficult) problem of matchings between two populations (White, 1970: esp. ch. 8).

SECOND: Keep the "model" kit easy, keep it simple, so your account can be brief and clear—which is to say "cherry-pick." Herbert Simon is masterful at this. For example, his three-page gem of the late 1950s on the salary size distribution for a population of firms—and thence how the chief executive officer's (CEO's) salary correlates with sales volume—derives from simple tree hierarchies of management with parameter estimates in the form of log ratios—which yield correlations in the observed range.[8] Simon does not allow himself to be distracted from the (possible!—it's a hunt!) main phenomenon by technicalities, possibly because he picked up math late.

To this day, a weekend with one of the great (and hence readable) books[9] together with a good nose for key bits of data can establish you as a cherry picker. For instance, some napkin-scale mathematics can guide your hunches of where some catastrophic jam-up will occur in a social queuing system.[10] Or you can check out possible scopes of the ecological fallacy in various contexts with some simple framing in algebraic equations. Coleman's *Introduction,* mentioned above, is filled with clever 2 × 2 tables

[7]I noticed that mathematical economists were little interested in specific parametric situations. That suggested how ill-rooted their existing theory remains: Onto 17th-century personality psychology, they have overlaid 19th-century physics field theory divorced from any empirical constants of measurement; they seek to avoid measurement through all manner of excuses, rationales, and expedients dressed up as epistemology.

[8]Herb Simon was saying the economists were out to lunch with their claim that CEO pay reflected "marginal value"—since instead, it reflected, precisely the logic of layering social hierarchies. A corollary moral is that proper embedding is crucial with these basic, elementary math models: His student, economist Oliver Williamson, went on to base a career on this note Herb dashed off, weaving together around it the perspectives of Simon and of Ronald Coase. Whereas I would wager that several others (including certainly myself as a graduate student at Princeton) had derived the same core equation but had not the sense or encouragement to see it as publishable—or as relevant to contemporaneous controversies about CEO pay.

[9]For example, William Feller's (1968) or Anatol Rapoport's (1983)—not to mention more workmanlike surveys like Kim and Rhoush (1980) or texts like Leik and Meeker (1975), which are not colorful enough for weekend reading, at least by me.

[10]However, and alas, more detailed modeling of specific systems quickly becomes horrendously difficult, despite imaginative new approaches (cf. e.g., Robertazzi, 1994).

from sociological vision converted into calculable little sets of linear differential or difference equations, often yielding closed-form solutions that you can carry around in your head.

When you see an author importing big layers and tangles of technicalities, beware! Demand specification of payoff in phenomena, not in involutions of previous technicalities. I blew this rule in my snide critique (White, 1952) of Coleman's early *Sociometry* paper on James' earlier data on size distributions of freely forming groups—yes, Jim cut corners on the math, here as always, but my (painfully complex) correction to his stochastic system model did not predict significantly different phenomena. Coleman was the better cherry-picker.

THEME 1

Language Use

My first theme of the current frontier concerns languages. The linguist Zellig Harris long ago had insisted, in some detail and specificity, that mathematics was itself a language that was not all that different from natural languages.[11] This depended, however, on his presupposition that the prime function of languages was referential; so more recent refoundings of linguistics as analyses of the constitution of context and genre in discourse (Auer and Di Luzio, 1992; Goodwin and Duranti, 1992; Levinson, 1983; Perinbanayagam, 1991) mute Harris' claim. But, on the other hand, recognition of the enormously and increasingly diverse scope of the kinds of searches that we call mathematics, a scope beyond Harris' rather prim view of mathematics as analysis and geometry, can suggest additional kinship between mathematics and language.

My theme proper is that mathematical and interpretive approaches should become indispensible to one another, partly because of this increasing scope and flexibility of mathematics. Interpretive traditions depend for achieving *their* best on painfully achieved explicitness of self-reflexivity in language use. Until recently, except for some formal logic from philosophers and some computer modeling, there have been few aids in this, and almost no notion of any aid from mathematics. That is changing, and going

[11]For his final version, see Harris (1991). But as a not unsympathetic linguist said to me, there was a good deal too much banging of pots and pans for the small amount of actual cooking. Much the same verdict in the same era applies to the *Foundations* (1947) of Paul Samuelson, who also transferred whole formal schemes from other settings with little attention to actual phenomenology.

far beyond computer processing of words and word order, important as electronic text parsing is.

It is equally evident that, in avoiding and sidestepping the interpretive—and thus any direct access to construction of social reality—*mathematical models have come to an era of decreasing returns to effort. Another way to say the same thing is that interpretive approaches are central to achieving a next level of adequacy in social data*—which is to say to make the sort of transition as made between the *Literary Digest* poll about Franklin Delano Roosevelt to modern sample surveying (Singer and Presser, 1994).

Languages especially deal with combinations in presentations and interactions, as well as in reference. The mathematics of combinatorics, although as yet somewhat inchoate and lacking in theoretical depth, is rapidly growing (e.g., Beth *et al.,* 1903). Its branches include, beside design of combinations, analysis of networks and codes. It is important as conceptual substratum for the more familiar mathematics for statistical inference and for stochastic processes (Feller, 1968). Linguistics will go nowhere without both the combinatorics and the tangling of processes in time (cf. Hopper, 1992, 1995).

One effective application of mathematics to language is for glottochronology, the question of how many centuries ago separate languages like English and German branched from the same base. About 80% of the most basic descriptive words in a language prove to survive with only modest modification for a milennium, and from elaborations of stochastic models of the process of the word "death" one can derive separation estimates that seem to fit observed cases. And this can all be carried out with very elementary mathematics (Goldberg, 1983: ch. 5).

Zellig Harris's own claim did exhibit a bold simplicity characteristic of the best applications of mathematics: Harris said meaning consists in just patterns of cooccurence among words in text (oral or written). He sought verification through correlations with distances of separation using analyses standard to multivariate mathematical statistics, even to assuming normal distributions. This claim does cut through the fog that philosophers emit on the subject of meaning. But there is a need to go beyond Harris' programmatic formulations to investigate the complexities of grammaticality as word order patterns (e.g., Hawkins, 1983) and word order evolution, social (e.g., Hopper and Traugott, 1993) and personal (Radford, 1990).

My main point is that even extensions of Harris' work to grammaticality would miss the central phenomenon of languages, which is the social construction of reality, rather than being reference and meaning (Silverstein, 1976; cf. the lucid overview in Lucy, 1993). My claim is that *mathematics is needed especially and primarily for helping coax the central phenomenon into view sufficiently to permit the sorts of reconstruing and manipulation and meas-*

urement on which productive insight depends. Unfortunately, in the social sciences, so far mathematics has much more commonly been used as a technical adjunct to permit being a bit prissier about phenomena whose main outlines are worked out independently of the mathematics. Game theory is one exception (Fudenberg and Tirole, 1993; for a magisterial survey, see Shubik, 1984a,b; and for more alternative phenomenology, see Leifer, 1991).

I am not saying such coaxing is easy. For example, I have proposed that (White, 1995b,c) hitherto undeveloped combinations of dualities (e.g., Breiger, 1990) together with switchings between speech registers and their social settings are central to languages as providing footings of reality—but as yet without being able to suggest explicit stochastic modeling. An advantage of even seeking such a model is that it encourages one to locate, and to build from possible homology with, phenomena at very different scales, in this case to earlier work on style changes (White, 1994, 1993: ch. 4).

Some bypass the real-time interactive process approach to languages. For example, Peter Abell (1987) works to model narrative plot directly with techniques of modern algebra, to which he has subsequently adjoined game theoretic interpretation. Abbott (1988) and Maines (1993) argue for the generality of narrative approach: see Bakhtin (1986) and Swales (1990) for further substantive foundation. One could analyze just selected grammatical deposits, say politeness conventions (e.g., Agha, 1993), for modeling using techniques such as Carley (1993) offers. Others bypass both narrative and speech to induce phenomenology of modern social life from contrasting measures of charged outcomes of stratification processes of individuals (Breiger, 1995) and of organizations (Mohr, 1994): these view social class biases in these processes as, so to speak, "body language"—social body language.

THEME 2

Social Space-Times

I think there is a hidden reason for the suddenly growing importance of the (new) linguistics. This reason is the disappearance of the human person as a useful construct in this era of scientific theory of social action (see White, 1995a, and also White, 1992). The recent resurgence of "rational actor" models is not inconsistent with my view since *there is little that is specifically human about rational actors.*[12] *Without persons being presupposed as actors, attention*

[12]And indeed imaginative economists are now getting published in major journals, accounts that sidestep person as actor: see Gode and Sunder (1993), who, I might add, had the bracing experience of working with the new experimental economics pioneered by Vernon Smith.

necessarily shifts to confluences of observable processes-in-relations. Out of these emerge actors and locations of social action.

I further argue that space and time become more problematic without persons as their apparently unproblematic anchors. Long ago, Roger Shepard, building from the new perceptual psychology of James Gibson, loosened the hold of the person construct on researchers, perhaps unintentionally, by providing a rich menu of spaces for them unrelated to our "ordinary" physical space-time. Recently, in sociology Abbott (1990) is one of those who have carried this forward with respect to our views of time (and see White, 1992: Appendix 2, for some further literature).

My point is that models and theory generally are deeply shaped by any such changes in the construal of times and spaces, the ultimate framings. This has been working itself out in several literatures in detailed application. For example, in political science, there is a whole new vision of spaces for, and thus the nature of, actors—as individual legislators or parties (cf. the survey in Enelow and Hinich, 1990). Indeed, the study of language has been reshaped also: see for example, De Swaan (1988: diagrams of overlapping petal shapes). A similar process has been going on in the physical science of gels (de Gennes, 1979) and other dense, partly fluid matter, which I have argued (White, 1992) provide helpful guidance.

The network literature is now so extensive that I cite here only a few works, variously broad, basic, innovative, and compendious, with bias toward multiple networks (Anheier *et al.,* 1995; Boyd, 1991; Burt, 1992; Freeman *et al.,* 1989; Gal, 1979; McAdam and Paulsen, 1993; Milroy, 1987; Padgett and Ansell, 1993; Pattison, 1993; Skvoretz and Fararo, 1995; Wasserman and Faust, 1995; Wellman and Berkowitz, 1988). In my own work, I have emphasized structural equivalence approaches since these seem best adapted simultaneously to interpretive and to process analyses. There are two main alternative lines: first, connectivity studies—see for example, Hummon and Doreian (1989) and papers in Freeman *et al.* (1989), sometimes calling for strength measures of ties and stochastic models. Second, decomposition of networks as populations of triads—see for example, Hummell and Sodeur (1988) and Johnsen (1985, 1986), which opens toward analyses of analogies, for discrete spaces of topological geometry (see below, The Knotty Future).

OBJECT LESSONS

Prototypes and Not

First let me point to some positive prototypes, to models for modeling. The existing breeds of stochastic system modeling cited earlier are unlikely to be of much use in social models, which are for systems in, so to speak, wiggly rubber or gel materials. Instead, models for social systems may need to focus on how they wiggle themselves through interpretative mechanisms. John Padgett (1981) has been brilliant in showing, using the example of Office of Management and Budget budget making/dealing, how occasional catastrophic accumulations or disaccumulations are byproducts just from the amalgamation of other levels' outputs subject to the pretenses of rationality by which we wiggle and interpret. Note that the "interpretive" and the "quantitative" had to be melded together in intimate fashion to achieve this insight. And Padgett (1980) carried this further to interpret previous presidential budget actions in strategic terms. And see Ron Burt (1992) for an analogous 1–2 buildup, but now around network structuration rather than fiscal story lines. Breiger (1995) has recently emphasized the interpenetrations of interpretive with stochastic and structural that have proved necessary across a wide swathe of stratification studies.

Man is not the only social species. The findings of Ivan Chase (1982, 1995) come from a long series of studies which combine modeling and experiment for various social vertebrate species. It is especially fitting as prototype, given my two Themes, because Chase's work at the same time deals with the most basic "language," that of dominance, and it does so in terms of a reconstrual of effective time and space. One could call this the bystander theory of social formation:

> The structural form of a hierarchy can be explained by regularities or "building blocks" of interaction involving two individuals and a bystander . . . the jigsaw puzzle model . . . . In sum, although differences in individual characteristics can be partially predictive of differences in dominance ranks, there is a growing body of analytical and empirical work which indicate the limitations . . . . (233, 237)

Now let me turn to object lessons that are not positive prototypes. Consider examples from my own work. I wasted too long in searching for magic keys in combinatorics mathematics to uncover what I thought should be deep structural embeddings of vacancy chains (White, 1970) according to factional and strata maneuvering.[13] Earlier, I had developed a combinatorial model for intergenerational mobility that to me gave valuable insight into the deep causes of mobility patterns (White, 1963a), but just then Leo Goodman came up with a neat nested set of statistical estimations. I proved to be alone in preferring the causal and phenomenological insights from this (cumbersome) system combinatorics.

But there are positive aspects, not just avoidance lessons to be recognized from this tale. Inventing mathematics is enormously difficult. Huge

[13]I intended to acknowledge this failure in the preface, but Scott Boorman argued me out of that. Combinatorics, along with computer modeling, is now further advanced (cf. e.g., Cameron, 1994; Beth *et al.*, 1988)—and I remain convinced there is an algebra of tenure networks underlying observed vacancy chains.

hunks of time to scan technical literatures are indispensible to us as we search for openings for modeling the new sorts of phenomena that are our charge. Inventing constructs can benefit from similar scans. Look at a recent survey in this area (Breiger, 1990). Chapters 2 (on mobility among committees, by John Padgett), 4 (on vacancy chains in history by Andres Abbott), and 12 (on careers by James Rosenbaum) develop and survey mathematical models, and most of the other chapters use sophisticated statistical assessments. These are payoffs that more than justify scans by myself and many others that did not directly influence developments. As to constructs, a key new one in this area is the Venturi tube formulation of mobility by Shelby Stewman (cf. Stewman, 1988), which came from scans of engineering literature.

In another line of work, blockmodels, the main lessons are not how important it is to invoke powerful mathematics—but how one can and ususally must do this through reliance on others. My original work on kinship (White, 1963b) developed the social implications of work by the great mathematician André Weil, made more accessible by the Kemeny text cited earlier. My phenomenological attempts in the late 1960s to loosen this up to apply to sociometric network patterns depended on mathematical work by John Paul Boyd (for its full development see Boyd, 1991) to find a path to operationalization. A programmer of genius, Greg Heil, created a deep algorithm, which, however, was unable to deal with the shimmer and noise that are ineradicable from social relational "data"—see Themes 1 and 2 above.

One of the main subsequent deepenings, from structural to abstract equivalence in roles, was urged first, and very lucidly, by Paul Bernard, then a doctoral student, but neither he nor I could figure out a mathematical formulation, which waited upon the later work of Christopher Winship. And, similarly, the proper algebraic formulation of structural roles depended on Scott Boorman's contributions. See Pattison (1993) for a comprehensive account of the whole, including work of Michael Mandel and many contributions of hers and of Breiger's and Boorman's. I think a second deepening will come from Frank Romo's idea of omega blocks (cf. Anheier *et al.,* 1995), but this will surely require the statistical genius of a Leo Goodman to exploit, likely in a context of the stochastic models of size distribution that Herbert Simon has helped to pioneer.

THE KNOTTY FUTURE

One of the most remarkable anticipations or previsions in the history of natural science provides not just an analogy but possibly even a direct lead to modeling the central phenomenon of social action: viz., interactive social constructions of reality among us. It is a prevision of knot theory and its application to physics. I had been fascinated with knot theory since reading the introductory text by Crowell and Fox (1963), and I will point out several more Morals en route to the main points.

The Crowell and Fox book was published in a series edited by the aforesaid John Kemeny along with mathematicians Garrett Birkhoff (American superstar of lattice theory—see Freeman and White, 1994) and Mark Kac (Hungarian superstar of combinatorics—see Beth *et al.*, 1993). Knots are a maddening combination both for mathematics and in empirical reality—supreme obviousness combines with supreme difficulty in precise formulation and recognition. After all, experienced fly fishermen [*sic*] just discard tangled line even though the tangle might eventually prove to have no knot at all.

But, as normal, I did not get any mileage out of first fussing with knot theory, not directly—just as I remember getting merely a paper or two (White, 1963a, 1970b—see discussion above) out of spending many months reading up on hypergeometric series in the 1960s. Anyone doing modeling has to keep scanning mathematics. This is an enormous universe, and one whose indexes and abstracting and review services are opaque to all save professional mathematicians—and I suspect even then, if you exclude the superstars, only to "nearby" mathematicians. So one just has to spend time scanning books (which have a higher density of words and more preliminaries than journals) in math libraries—where I usually felt like a chimp scanning supplies in a jungle camp temporarily empty. Even without particular payoffs, small or large, such scanning at least keeps your mathematical tongues loosened. Occasionally you strike paydirt, for examples of which see Boyd (1991).

And, again as normal, I was stupid about knot theory (as about so many other topics in the confusing jumble known as "combinatorics"), or to put it more delicately, I did not have the depth of mathematical insight to see that knot theory connected with other highly technical areas in theoretical physics in which I had already been trained. The distinguished mathematician Michael Atiyah (1990) has now supplied these connections in a marvelously lucid account (with technicalities kept bearable). Not one but two scientific geniuses figure—Lord Kelvin in the last and Richard Feynman in this century:

> Lord Kelvin put forward in 1867 the imaginative and ambitious idea that atoms were knotted vortex tubes of ether. The arguments in favour of this idea may be summarized as follows:
>
> (1) *Stability.* The stability of matter might be explained by the stability of knots (i.e., their topological nature).
>
> (2) *Variety.* The variety of chemical elements could be accounted for by the variety of different knots.

(3) *Spectrum.* Vibrational oscillations of the vortex tubes might explain the spectral lines of atoms.

From a modern twentieth-century point of view we could, in retrospect, have added a fourth.

(4) *Transmutation.* The ability of atoms to change into other atoms at high energies could be related to cutting and recombinations of knots.

For about 20 years Kelvin's theory of vortex atoms was taken seriously. Maxwell's verdict [James Clerk Maxwell, the first and master theorist of electromagnetic fields] was that "it satisfies more of the conditions than any atom hitherto considered."

Kelvin's collaborator P. G. Tait undertook an extensive study and classification of knots. (Atiyah, 1990: 5–6).

And indeed Tait's knot publications of 1877–1885 are still cited in Crowell and Fox.

You can already see why I conjecture that knot theory may become central in the future for modeling social phenomena in the currently emerging era of social constructionism. Interactions, ties in sociocultural context, are coming to supplant persons as building blocks—and a person may come to be seen as a knotted vortex among social networks. As mentioned earlier, multiple sorts of spaces, even nonintegral dimensionalities, are now recognized in several natural sciences, and surely this and related viewpoints on temporality that can also be found in social science (e.g., Luhmann, 1982) should encourage analogous developments for sociocultural process.

It is not just persons that can emerge as actors from knot theories of space-times. Consider, for example, the kaleidoscope of networks and corporates in conflict and oscillation as they continually reconstruct the Ottoman Empire as portrayed by Barkey (1993), or in the Medici Florence portrayed by Padgett and Ansell (1993). And there are a number of other theoretical-modeling enterprises moving in this direction, some catalyzed by the Santa Fe Institute (cf. Padgett, 1993).

The mathematics is not easy, not easy at two levels. Knots are about the different ways of embedding one topological space in another. The abstract algebraic characterizations (beginning with group theory of symmetries) developed to handle this lead to painfully difficult calculations. These often concern invariants (e.g., Alexander polynomials for knots) that are preserved under transformations of mappings from one space to the other. Existence theorems are much more common results than are computations subject to measurement! Mathematicians key the breakthrough for knot theory to a mathematician's discovery in 1984 of a new kind of knot invariant (the Jones polynomial), but, though important, this is on the first, technical level.

The really difficult mathematical task is to develop some much broader heuristic perspective for interpreting and maneuvering. This is where our second genius comes in. Long ago, around 1950, physicist Richard Feynman revolutionized the second quantum mechanics—which derives existence and properties of "elementary particles," not just their behavior, from the quantum fields hypothesized in the 1920s. These particles appear and disappear only in and through combining with other such. The derivations were enormously difficult, awkward, complex—unbeautiful. These are real computations of specific existence, not the mathematician's proofs of some woolly "existence." Feynman cut through this morass with rules for drawing diagrams of particle (re)combinations from which integrals could be computed as combinations of (relatively) simple building blocks.

Even a mathematician as gifted as Atiyah criticizes the "*Feynman path-integral* approach. It is not mathematically rigorous, but it is conceptually simple." There is a lesson for any science here! For decades, the major advances in elementary particle physics have rested on this "nonrigorous" approach. This says as much about the value of "mathematical rigor" as it does about Feynman pathintegrals. Social scientists should be emboldened to support any of our young with the technical and phenomenological insights to sketch a path, no doubt a nonrigorous and perhaps a wrong path, to modeling the emergence of sociocultural reality in actors as situated interactions.

DISCUSSION

Mathematics is needed especially and primarily for helping coax the central phenomena into sufficiently clear view to permit the sorts of reconstrual and manipulation and measurement on which productive insight depends. I have arrayed the body of my paper around this statement, which I have already made twice earlier so that this would seem to stand as summary and conclusion. But I shall offer, in a postmodernist spirit, two examples that in part erode the universality of this conclusion and thereby invite some further probing in conclusion.

I violated my own precepts in proposing models for identities-as-disciplines (White, 1992; ch. 2) that went well beyond any explicit phenomenology or proposed measurement. But it may have been worthwhile. I know of at least one instance of a later investigator trying to provide what is missing (Ocasio, 1994: esp. p. 5). In this same paper, Ocasio shows how to squeeze very important substantive findings out of meticulous mathematical model (event history analysis—Tuma and Hannan, 1984) combined with artful statistical manipulation of carefully collected data. That is the classic and persisting use for modeling, which does indeed make mathematics social.

I violated my own precepts in early network "blockmodeling" studies done by me in collaboration with many others. All this work depended on and derived from "type of tie," yet the phenomenology was so muddy that we just held our noses and went with whatever was provided in the empirical case studies—although we pared those down to the few that had been done most carefully. Much of the other modeling literature for social networks shares this same flaw. But it was works such as these, works flawed by technicism, which partly motivated Mark Granovetter, and I think also Nan Lin and Barry Wellman and Steven Berkowitz and so many others here and in other countries, to work assiduously to clarify phenomenology. Thus even raw mathematics can help us along the road to phenomenology.

And now other ambitious mathematics is being brought to bear directly onto these phenomenological puzzles. For example, Lin Freeman and Douglas White (1994) use Galois lattice theory to greatly clarify, in the course of providing explicit representation of, various interlocking dualities among actors and events and groups (cf. Breiger 1991, ch. 1; a lucid and readable introduction to Boolean algebras can be found in Halmos, 1963). Hummon and Doreian (1989) along somewhat related lines develop an approach that might permit simultaneously discriminating both strength of tie and types of tie.

As large-scale data become available for more structured social organizations, sophisticated tabular analyses of it (logit, log-linear, metric scaling, etc.) can illuminate phenomenology of ties, as shown for example recently by Ronald Burt (1992) and by Shin-Kap Han (1995). These analyses may not seem exactly "mathematics," but then mathematics is a social construction whose exact nature baffles many mathematicians (e.g., Kline, 1991; Weyl, 1949) and whose complexities have led to the creation of metamathematics for keeping track (e.g., Arbib and Manes, 1975). Examples are plentiful in social science of new crossbreeds of "mathematics" with statistics and portrayal-of-measurement (cf. Levine, 1993), but there are other crossbreeds. Computer simulation approaches also often are structured by mathematics and provide an avenue to bold attempts at reconstruing the central phenomena of social reality (for recent technical versions see, e.g., Carley, 1992; Kaufer and Carley, 1993; Leik, 1994; Macy, 1991; Morris, 1993; Robertazzi,1994; Strang and Tuma, 1993; for looser essay versions see March and Olsen, 1976).

It is also well to remember that there are other traditions in human thought, nonmathematical but formal, which build up powerful representations of social reality in setting up very circumscribed subrealities. One example may be theology. But a more obvious example is classic games such as chess, and Go—on which latter see the brilliant analogy drawn by Boorman (1969) to Maoist revolutionary strategy.

I began with a German writing, and I will end with two French contributions that frame a key dilemma. I have chosen to focus this paper on suggesting how to deepen substantive insights using mathematics. The cost has been giving short shrift to the necessity of ascertaining facts, of establishing patterns, through careful empirical work—which always is high in drudgery. In this respect my paper resembles simulation modeling as exemplified in the brilliant pioneering monograph of Raymond Boudon (1973) in and on France. A different approach is to work up models from and in terms of meticulous data, which is the main strategy of Alain Degenne and Michel Forsé (1994) in their superb new review of much of social network analysis. *Their monograph reminds us that I am too glib in my twice-repeated sentence in having the mathematics precede and prepare the way for measurement only subsequently.*

But, although there is a dilemma, it is a fuzzy dilemma: To be able to recognize a "pattern" is to impose a pattern from some sort of prior expectation. That is, we think—just as we refer and perceive—in some language or other. And mathematical languages have proved a valuable option, across the sciences—even though they are subject to the same risks of obfuscatory rhetoric and pompous platitude as Latin or Chinese, and the same temptations to avoid concrete particulars and tiresome details.

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