Can There Be A Feminist Science?

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This paper explores a number of recent proposals regarding "feminist science" and rejects a content-based approach in favor of a process-based approach to characterizing feminist science. Philosophy of science can yield models of scientific reasoning that illuminate the interaction between cultural values and ideology and scientific inquiry. While we can use these models to expose masculine and other forms of bias, we can also use them to defend the introduction of assumptions grounded in feminist political values.

The question of this title conceals multiple ambiguities. Not only do the sciences consist of many distinct fields, but the term "science" can be used to refer to a method of inquiry, a historically changing collection of practices, a body of knowledge, a set of claims, a profession, a set of social groups, etc. And as the sciences are many, so are the scholarly disciplines that seek to understand them: philosophy, history, sociology, anthropology, psychology. Any answer from the perspective of some one of these disciplines will, then, of necessity, be partial. In this essay, I shall be asking about the possibility of theoretical natural science that is feminist and I shall ask from the perspective of a philosopher. Before beginning to develop my answer, however, I want to review some of the questions that could be meant, in order to arrive at the formulation I wish to address.

The question could be interpreted as factual, one to be answered by pointing to what feminists in the sciences are doing and saying: "Yes, and this is what it is. " Such a response can be perceived as question-begging, however. Even such a friend of feminism as Stephen Gould dismisses the idea of a distinctively feminist or even female contribution to the sciences. In a generally positive review of Ruth Bleier's book, *Science and Gender*, Gould (1984) brushes aside her connection between women's attitudes and values and the interactional science she calls for. Scientists (male, of course) are already proceeding with wholist and interactionist research programs. Why, he implied, should women or feminists have any particular, distinctive, contributions to make? There is not masculinist and feminist science, just good and bad science. The question of a feminist science cannot be settled by pointing, but involves a deeper, subtler investigation.

The deeper question can itself have several meanings. One set of meanings is sociological, the other conceptual. The sociological meaning proceeds as follows. We know what sorts of social conditions make misogynist science possible. The work of Margaret Rossiter (1982) on the history of women scientists in the United States and the work of Kathryn Addelson (1983) on the social structure of professional science detail the relations between a particular social structure for science and the kinds of science produced. What sorts of social conditions would make feminist science possible? This is an important question, one I am not equipped directly to investigate, although what I can investigate is, I believe, relevant to it. This is the second, conceptual, interpretation of the question: what sort of sense does it make to talk about a feminist science? Why is the question itself not an oxymoron, linking, as it does, values and ideological commitment with the idea of impersonal, objective, value-free, inquiry? This is the problem I wish to address in this essay.

The hope for a feminist theoretical natural science has concealed an ambiguity between content and practice. In the content sense the idea of a feminist science involves a number of assumptions and calls a number of visions to mind. Some theorists have written as though a feminist science is one the theories of which encode a particular world view, characterized by complexity, interaction and wholism. Such a science is said to be feminist because it is the expression and valorization of a female sensibility or cognitive temperament. Alternatively, it is claimed that women have certain traits (dispositions to attend to particulars, interactive rather than individualist and controlling social attitudes and behaviors) that enable them to understand the true character of natural processes (which are complex and interactive).' While proponents of this interactionist view see it as an improvement over most contemporary science, it has also been branded as soft — misdescribed as non-mathematical. Women in the sciences who feel they are being asked to do not better science, but inferior science, have responded angrily to this characterization of feminist science, thinking that it is simply new clothing for the old idea that women can't do science. I think that the interactional view can be defended against this response, although that requires rescuing it from some of its proponents as well. However, I also think that the characterization of feminist science as the expression of a distinctive female cognitive temperament has other drawbacks. It first conflates feminine with feminist. While it is important to reject the traditional derogation of the virtues assigned to women, it is also important to remember that women are *constructed* to occupy positions of social subordinates. We should not uncritically embrace the feminine.

This characterization of feminist science is also a version of recently propounded notions of a 'women's standpoint' or a 'feminist standpoint' and suffers from the same suspect universalization that these ideas suffer from. If there is one such standpoint, there are many as Maria Lugones and Elizabeth Spelman spell out in their tellingly entitled article, "Have We Got a Theory for You: Feminist Theory, Cultural Imperialism, and the Demand for 'The Woman's Voice,'" women are too diverse in our experiences to generate a single cognitive framework (Lugones and Spelman 1983). In addition, the sciences are themselves too diverse for me to think that they might be equally transformed by such a framework. To reject this concept of a feminist science, however, is not to disengage science from feminism. I want to suggest that we focus on science as practice rather than content, as process rather than product, hence, not on feminist science, but on doing science as a feminist.

The doing of science involves many practices how one structures a laboratory (hierarchically or collectively), how one relates to other scientists (competitively or cooperatively), how and whether one engages in political struggles over affirmative action. It extends also to intellectual practices, to the activities of scientific inquiry, such as observation and reasoning. Can there be a feminist scientific inquiry? This possibility is seen to be problematic against the background of certain standard presuppositions about science. The claim that there could be a feminist science in the sense of an intellectual practice is either nonsense because oxymoronic as suggested above or the claim is interpreted to mean that established science (science as done and dominated by men) is wrong about the world. Feminist science in this latter interpretation is presented as correcting the errors of masculine, standard science and as revealing the truth that is hidden by masculine 'bad' science, as taking the sex out of science.

Both of these interpretations involve the rejection of one approach as incorrect and the embracing of the other as the way to a truer understanding of the natural world. Both trade one absolutism for another. Each is a side of the same coin, and that coin, I think, is the idea of a value-free science. This is the idea that scientific methodology guarantees the independence of scientific inquiry from values of value-related considerations. A science or a scientific research program informed by values is *ipso facto* "bad science". "Good science" is inquiry protected by methodology from values and ideology. This same idea underlies Gould's response to Bleier, so it bears closer scrutiny. In the pages that follow, I shall examine the idea of value-free science and then apply the results of that examination to the idea of feminist scientific inquiry.

II

I distinguish two kinds of values relevant to the sciences. Constitutive values, internal to the sciences, are the source of the rules determining what constitutes acceptable scientific practice or scientific method. The personal, social and cultural values, those group or individual preferences about what ought to be I call contextual values, to indicate that they belong to the social and cultural context in which science is done (Longino 1983c). The traditional interpretation of the value-freedom of modern natural science amounts to a claim that its constitutive and contextual features are clearly distinct from and independent of one another, that contextual values play no role in the inner workings of scientific inquiry, in reasoning and observation. I shall argue that this construal of the distinction cannot be maintained.

There are several ways to develop such in argument. One scholar is fond of inviting her audience to visit any science library and peruse the titles on the shelves. Observe how subservient to social and cultural interests are the inquiries represented by the book titles alone! Her listeners would soon abandon their ideas about the value-neutrality of the sciences, she suggests. This exercise may indeed show the influence of external, contextual considerations on what research gets done/supported (i.e., on problem selection). It does not show that such considerations affect reasoning or hypothesis acceptance. The latter would require detailed investigation of particular cases or a general conceptual argument. The conceptual arguments involve developing some version of what is known in philosophy of science as the underdetermination thesis, i.e., the thesis that a theory is always underdetermined by the evidence adduced in its support, with the consequence that different or incompatible theories are supported by or at least compatible with the same body of evidence. I shall sketch a version of the argument that appeals to features of scientific inference.

One of the rocks on which the logical positivist program foundered was the distinction between theoretical and observational language. Theoretical statements contain, as fundamental descriptive terms, terms that do not occur in the description of data. Thus, hypotheses in particle physics contain terms like "electron," "pion," "muon," "electron spin," etc. The evidence for a hypothesis such as "A pion decays sequentially into a muon, then a positron" is obviously not direct observations of pions, muons and positrons, but consists largely in photographs taken in large and complex experimental apparati accelerators, cloud chambers, bubble chambers. The photographs show all sorts of squiggly lines and spirals. Evidence for the hypotheses of particle physics is presented as statements that describe these photographs. Eventually, of course, particle physicists point to a spot on a photograph and say things like "Here a neutrino hits a neutron". Such an assertion, however, is an interpretive achievement which involves collapsing theoretical and observational moments. A skeptic would have to be supplied a complicated argument linking the elements of the photograph to traces left by particles and these to particles themselves. What counts as theory and what as data in a pragmatic sense change over time, as some ideas and experimental procedures come to be securely embedded in a particular framework and others take their place on the horizons. As the history of physics shows, however, secure embeddedness is no guarantee against overthrow.

Logical positivists and their successors hoped to model scientific inference formally. Evidence for hypotheses, data, were to be represented as logical consequences of hypotheses. When we try to map this logical structure onto the sciences, however, we find that hypotheses are, for the most part, not just generalizations of data statements. The links between data and theory, therefore, cannot be adequately represented as formal or syntactic, but are established by means of assumptions that make or imply substantive claims about the field over which one theorizes. Theories are confirmed via the confirmation of their constituent hypotheses, so the confirmation of hypotheses and theories is relative to the assumptions relied upon in asserting the evidential connection. Conformation of such assumptions, which are often unarticulated, is itself subject to similar relativization. And it is these assumptions that can be the vehicle for the involvement of considerations motivated primarily by contextual values (Longino 1979, 1983a).

The point of this extremely telescoped argument is that one can't give an a priori specification of confirmation that effectively eliminates the role of value-laden assumptions in legitimate scientific inquiry without eliminating auxiliary hypotheses (assumptions) altogether. This is not to say that all scientific reasoning involves value-related assumptions. Sometimes auxiliary assumptions will be supported by mundane inductive reasoning. But sometimes they will not be. In any given case, they may be metaphysical in character, they may be untestable with present investigative techniques, they may be rooted in contextual, value-related considerations. If, however, there is no a priori way to eliminate such assumptions from evidential reasoning generally, and, hence, no way to rule out value-laden assumptions, then there is no formal basis for arguing that an inference mediated by contextual values is thereby bad science. A comparable point is made by some historians investigating the origins of modern science. James Jacob (1977) and Margaret Jacob (1976) have, in a series of articles and books, argued that the adoption of conceptions of matter by 17th century scientists like Robert Boyle was inextricably intertwined with political considerations. Conceptions of matter provided the foundation on which physical theories were developed and Boyle's science, regardless of his reasons for it, has been fruitful in ways that far exceed his imaginings. If the presence of contextual influences were grounds for disallowing a line of inquiry, then early modern science would not have gotten off the ground.

The conclusion of this line of argument is that constitutive values conceived as epistemological (i.e., truth-seeking) are not adequate to screen out the influence of contextual values in the very structuring of scientific knowledge. Now the ways in which contextual values do, if they do, influence this structuring and interact, if they do, with constitutive values has to be determined separately for different theories and fields of science. But this argument, if it's sound, tells us that this sort of inquiry is perfectly respectable and involves no shady assumptions or unargued intuitively based rejections of positivism. It also opens the possibility that one can make explicit value commitments and still do "good" science. The conceptual argument doesn't show that all science is value-laden (as opposed to metaphysics-laden) — that must be established on a case-by-case basis, using the tools not just of logic and philosophy but of history and sociology as well. It does show that not all science is value- free and, more importantly, that it is not necessarily in the nature of science to be value-free If we reject that idea we're in a better position to talk about the possibilities of feminist science.

III

In earlier articles (Longino 1981, 1983b, Longino and Doell 1983), I've used similar considerations to argue that scientific objectivity has to be reconceived as a function of the communal structure of scientific inquiry rather than as a property of individual scientists. I've then used these notions about scientific methodology to show that science displaying masculine bias is not *ipso facto* improper or 'bad' science, that the fabric of science can neither rule out the expression of bias nor legitimate it. So I've argued that both the expression of masculine bias in the sciences and feminist criticism of research exhibiting that bias are — shall we say — business as usual, that scientific inquiry should be expected to display the deep metaphysical and normative commitments of the culture in which it flourishes, and finally that criticism of the deep assumptions that guide scientific reasoning about data is a proper part of science. The argument I've just offered about the idea of a value-free science is similar in spirit to those earlier arguments. I think it makes it possible to see these questions from a slightly different angle.

There is a tradition of viewing scientific inquiry as somehow inexorable. This involves supposing that the phenomena of the natural world are fixed in determinate relations with each other, that these relations can be known and formulated in a consistent and unified way. This is not the old "unified science" idea of the logical positivists, with us privileging of physics. In its "unexplicated" or "pre-analytic" state, it is simply the idea that there is one consistent, integrated or coherent, true theoretical treatment of all natural phenomena. (The indeterminacy principle of quantum physics is restricted to our understanding of the behavior of certain particles which themselves underlie the fixities of the natural world. Stochastic theories reveal fixities, but fixities among ensembles rather than fixed relations among individual objects or events.) The scientific inquirer's job is to discover those fixed relations. Just as the task of Plato's philosophers was to discover the fixed relations among forms and the task of Galileo's scientists was to discover the laws written in the language of the grand book of nature, geometry, so the scientist's task in this tradition remains the discovery of fixed relations however conceived. These ideas are part of the realist tradition in the philosophy of science.

It's no longer possible, in a century that has seen the splintering of the scientific disciplines, to give such a unified description of the objects of inquiry. But the belief that the job is to discover fixed relations of some sort, and that the application of observation, experiment and reason leads ineluctably to unifiable, if not unified, knowledge of an independent reality, is still with us. It is evidenced most clearly in two features of scientific rhetoric the use of the passive voice as in "it is concluded that " or "it has been discovered that "and the attribution of agency to the data, as in "the data suggest". Such language has been criticized for the abdication of responsibility it indicates. Even more, the scientific inquirer, and we with her, become passive observers, victims of the truth. The idea of a value-free science is integral to this view of scientific inquiry. And if we reject that idea we can also reject our roles as passive onlookers, helpless to affect the course of knowledge.

Let me develop this point somewhat more concretely and autobiographically. Biologist Ruth Doell and I have been examining studies in three areas of research on the influence of sex hormones on human behavior and cognitive performance research on the influence of pre-natal, *in utero*, exposure to higher or lower than normal levels of androgens and estrogens on so-called 'gender-role' behavior in children, influence of androgens (pre- and post-natal) on homosexuality in women, and influence of lower than normal (for men) levels of androgen at puberty on spatial abilities (Doell and Longino, forthcoming). The studies we looked at are vulnerable to criticism of their data and their observation methodologies. They also show clear evidence of androcentric bias—in the assumption that there are just two sexes and two genders (us and them), in the designation of appropriate and inappropriate behaviors for male and female children, in the caricature of lesbianism, in the assumption of male mathematical superiority. We did not find, however, that these assumptions mediated the inferences from data to theory that we found objectionable. These sexist assumptions did affect the way the data were described. What mediated the inferences from the alleged data (i.e., what functioned as auxiliary hypotheses or what provided auxiliary hypotheses) was what we called the linear model—the assumption that there is a direct one-way causal relationship between pre- or post-natal hormone levels and later behavior or cognitive performance. To put it crudely, fetal gonadal hormones organize the brain at critical periods of development. The organism is thereby disposed to respond in a range of ways to a range of environmental stimuli. The assumption of unidirectional programming is supposedly supported by the finding of such a relationship in other mammals, in particular, by experiments demonstrating the dependence of sexual behaviors—mounting and lordosis—on pen-natal hormone exposure and the finding of effects of sex hormones on the development of rodent brains. To bring it to bear on humans is to ignore, among other things, some important differences between human brains and those of other species. It also implies a willingness to regard humans in a particular way—to see us as produced by factors over which we have no control. Not only are we, as scientists, victims of the truth, but we are the prisoners of our physiology.[2] In the name of extending an explanatory model, human capacities for self-knowledge, self-reflection, self-determination are eliminated from any role in human action (at least in the behaviors studied).

Doell and I have therefore argued for the replacement of that linear model of the role of the brain in behavior by one of much greater complexity that includes physiological, environmental, historical and psychological elements. Such a model allows not only for the interaction of physiological and environmental factors but also for the interaction of these with a continuously self-modifying, self-representational (and self-organizing) central processing system. In contemporary neurobiology, the closest model is that being developed in the group selectionist approach to higher brain function of Gerald Edelman and other researchers (Edelman and Mountcastle 1978). We argue that a model of at least that degree of complexity is necessary to account for the human behaviors studies in the sex hormones and behavior research and that if gonadal hormones function at all at these levels, they will probably be found at most to facilitate or inhibit neural processing in general. The strategy we take in our argument is to show that the degree of intentionality involved in the behaviors in question is greater than is presupposed by the hormonal influence researchers and to argue that this degree of intentionality implicates the higher brain processes.

To this point Ruth Doell and I agree. I want to go further and describe what we've done from the perspective of the above philosophical discussion of scientific methodology.

Abandoning my polemical mood for a more reflective one, I want to say that, in the end, commitment to one or another model is strongly influenced by values or other contextual features. The models themselves determine the relevance and interpretation of data. The linear or complex models are not in turn independently or conclusively supported by data. I doubt for instance that value-free inquiry will reveal the efficacy or inefficacy of intentional states or of physiological factors like hormone exposure in human action. I think instead that a research program in neuroscience that assumes the linear model and sex-gender dualism will show the influence of hormone exposure on gender-role behavior. And I think that a research program in neuroscience and psychology proceeding on the assumption that humans do possess the capacities for self-consciousness, self-reflection, and self-determination, and which then asks how the structure of the human brain and nervous system enables the expression of these capacities, will reveal the efficacy of intentional states (understood as very complex sorts of brain states).

While this latter assumption does not itself contain normative terms, I think that the decision to adopt it is motivated by value-laden considerations—by the desire to understand ourselves and others as self-determining (at least some of the time), that is, as capable of acting on the basis of concepts or representations of ourselves and the world in which we act. (Such representations are not necessarily correct, they are surely mediated by our cultures, all we wish to claim is that they are efficacious). I think further that this desire on Ruth Doell's and my part is, in several ways, an aspect of our feminism. Our preference for a neurobiological model that allows for agency, for the efficacy of intentionality is partly a validation of our (and everyone's) subjective experience of thought, deliberation, and choice. One of the tenets of feminist research is the valorization of subjective experience, and so our preference in this regard conforms to feminist research patterns. There is, however, a more direct way in which our feminism is expressed in this preference. Feminism is many things to many people, but it is at its core in part about the expansion of human potentiality. When feminists talk of breaking out and do break out of socially prescribed sex-roles, when feminists criticize the institutions of domination, we are thereby insisting on the capacity of humans—male and female—to act on perceptions of self and society and to act to bring about changes in self and society on the basis of those perceptions. (Not overnight and not by a mere act of will. The point is that we act). And so our criticism of theories of the hormonal influence or determination of so-called gender-role behavior is not just a rejection of the sexist bias in the description of the phenomena—the behavior of the children studied, the sexual lives of lesbians, etc —but of the limitations on human capacity imposed by the analytic model underlying such research.[3]

While the argument strategy we adopt against the linear model rests on a certain understanding of intention, the values motivating our adoption of that understanding remain hidden in that polemical context. Our political commitments, however, presuppose a certain understanding of human action, so that when faced with a conflict between these commitments and a particular model of brain-behavior relationships we allow the political commitments to guide the choice.

The relevance of my argument about value-free science should be becoming clear. Feminists—in and out of science—often condemn masculine bias in the sciences from the vantage point of commitment to a value-free science. Androcentric bias, once identified, can then be seen as a violation of the rules, as "bad" science. Feminist science, by contrast, can eliminate that bias and produce better, good, more true or gender free science. From that perspective the process I've just described is anathema. But if scientific methods generated by constitutive values cannot guarantee independent from contextual values, then that approach to sexist science won't work. We cannot restrict ourselves simply to the elimination of bias, but must expand our scope to include the detection of limiting and interpretive frameworks and the finding or construction of more appropriate frameworks. We need not, indeed should not, wait for such a framework to emerge from the data. In waiting, if my argument is correct, we run the danger of working unconsciously with assumptions still laden with values from the context we seek to change. Instead of remaining passive with respect to the data and what the data suggest, we can acknowledge our ability to affect the course of knowledge and fashion or favor research programs that are consistent with the values and commitments we express in the rest of our lives. From this perspective, the idea of a value-free science is not just empty, but pernicious. Accepting the relevance to our practice as scientists of our political commitments does not imply simple and crude impositions of those ideas onto the corner of the natural world under study. If we recognize, however, that knowledge is shaped by the assumptions, values and interests of a culture and that, within limits, one can choose one's culture, then it's clear that as scientists/theorists we have a choice. We can continue to do establishment science, comfortably wrapped in the myths of scientific rhetoric or we can alter our intellectual allegiances. While remaining committed to an abstract goal of understanding, we can choose to whom, socially and politically, we are accountable in our pursuit of that goal. In particular we can choose between being accountable to the traditional establishment or to our political comrades.

Such accountability does not demand a radical break with the science one has learned and practiced. The development of a "new" science involves a more dialectical evolution and more continuity with established science than the familiar language of scientific revolutions implies.

In focusing on accountability and choice, this conception of feminist science differs from those that proceed from the assumption of a congruence between certain models of natural processes and women's inherent modes of understanding.[4] I am arguing instead for the deliberate and active choice of an interpretive model and for the legitimacy of basing that choice on political considerations in this case. Obviously model choice is also constrained by (what we know of) reality, that is, by the data. But reality (what we know of it) is, I have already argued, inadequate to uniquely determine model choice. The feminist theorists mentioned above have focused on the relation between the content of a theory and female values or experiences, in particular on the perceived congruence between interactionist, wholist visions of nature and a form of understanding and set of values widely attributed to women. In contrast, I am suggesting that a feminist scientific practice admits political considerations as relevant constraints on reasoning, which, through their influence on reasoning and interpretation, shape content. In this specific case, those considerations in combination with the phenomena support an explanatory model that is highly interactionist, highly complex. This argument is so far, however, neutral on the issue of whether an interactionist and complex account of natural processes will always be the preferred one. If it is preferred, however, this will be because of explicitly political considerations and not because interactionism is the expression of "women's nature ".

The integration of a political commitment with scientific work will be expressed differently in different fields. In some, such as the complex of research programs having a bearing on the understanding of human behavior, certain moves, such as the one described above, seem quite obvious. In others it may not be clear how to express an alternate set of values in inquiry, or what values would be appropriate. The first step, however, is to abandon the idea that scrutiny of the data yields a seamless web of knowledge. The second is to think through a particular field and try to understand just what its unstated and fundamental assumptions are and how they influence the course of inquiry. Knowing something of the history of a field is necessary to this process, as is continued conversation with other feminists.

The feminist interventions I imagine will be local (i.e., specific to a particular area of research), they may not be exclusive (i.e., different feminist perspectives may be represented in theorizing), and they will be in some way continuous with existing scientific work. The accretion of such interventions, of science done by feminists as feminists, and by members of other disenfranchised groups, has the potential, nevertheless, ultimately to transform the character of scientific discourse. Doing science differently requires more than just the will to do so and it would be disingenuous to pretend that our philosophies of science are the only barrier. Scientific inquiry takes place in a social, political and economic context which imposes a variety of institutional obstacles to innovation, let alone to the intellectual working out of oppositional and political commitments. The nature of university career ladders means that one's work must be recognized as meeting certain standards of quality in order that one be able to continue it. If those standards are intimately bound up with values and assumptions one rejects, incomprehension rather than conversion is likely. Success requires that we present our work in a way that satisfies those standards and it is easier to do work that looks just like work known to satisfy them than to strike out in a new direction. Another push to conformity comes from the structure of support for science. Many of the scientific ideas argued to be consistent with a feminist politics have a distinctively non-production orientation.[5] In the example discussed above, thinking of the brain as hormonally programmed makes intervention and control more likely than does thinking of it as a self-organizing complexly interactive system. The doing of science, however, requires financial support and those who provide that support are increasingly industry and the military. As might be expected they support research projects likely to meet their needs, projects which promise even greater possibilities for intervention in and manipulation of natural processes. Our sciences are being harnessed to the making of money and the waging of war. The possibility of alternate understandings of the natural world is irrelevant to a culture driven by those interests. To do feminist science we must change the social and political context in which science is done. So can there be a feminist science? If this means is it in principle possible to do science as a feminist?, the answer must be yes. If this means can we in practice do science as feminists?, the answer must be not until we change present conditions.

notes

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1 This seems to be suggested in Bleier (1984), Rose (1983) and in Sandra Harding's (1980) early work.

2 For a striking expression of this point of view see Witelson (1985).

3 Ideological commitments other than feminist ones may lead to the same assumptions and the variety of feminisms means that feminist commitments can lead to different and incompatible assumptions.

4 Cf note 1, above.

5 This is not to say that interactional ideas may not be applied in productive contexts, but that, unlike linear causal models, they are several steps away from the manipulation of natural processes immediately suggested by the latter See Keller (1985), especially Chapter 10.

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