THE POLITICAL SOCIALIZATION OF GENDER: WHAT CONTRIBUTION BIOLOGY?

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Introduction
That men and women differ in their degree of access to and control of political power has long been a significant aspect of political life. Political socialization research has tended to locate the origin of these differences exclusively in the learning process—whereby boys and girls are differentially equipped with the skills and interests necessary for participation as adults (Weissberg, 1974; Dawson, Prewitt, and Dawson, 1977) or children internalize norms of appropriate gender role behavior that are then activated during the adult years (Sapiro, 1977; Jennings and Niemi, 1981). Gender is undoubtedly a social and personal construct, but it is also based in biological sex differences—genetic, morphological, and physiological. Biological factors are a potential area of investigation that has remained relatively unexplored, in part because many political scientists have been reluctant to borrow as liberally from the life sciences as they have from their sister social sciences (Schubert, 1976). The emerging field of biopolitics might have provided a corrective to what has been called elsewhere the “oversocialized concept of man” (Wrong, 1961). However, the concern of many politically conscious individuals with the issue of sexist scholarship has rendered biological sex differences a suspect category. Arguing that scientific discourse does not take place in a political vacuum, many feminists have concluded that the introduction of biological variables can only be detrimental to women because scientific practice reflects popular prejudices.

This article argues that biological factors as well as environmental or learning-based factors should be included in scientific explanations of the political socialization of gender. We do so in part by reviewing conceptual and substantive areas that are relevant to the political socialization of gender. Bearing in mind Sapiro’s admonition (1979:263) that “accusations of sexism . . . are accusations of poor scholarship,” we begin by incorporating feminist perspectives in an examination of the normative assumptions of biopolitics and political socialization theory.

Political Socialization and Biopolitical Theory

Most empirical studies of political socialization have assumed at least implicitly that the salient aspects of adult political behavior are learned. This is not a particularly novel or even surprising observation—several commentators (e.g., Renshon, 1977) have noted and critiqued the assumption that socialization is an exclusively learned process. We do not necessarily wish to question the preeminence of the learning process in socialization research—certainly no more than we would wish to question the preeminence of environmental factors in natural selection. Our purpose is to consider how this dominant paradigm and the debate it has fostered have structured the way in which biological variables have been incorporated into political socialization research.

The dominant emphasis on political learning in political socialization research can be traced to Herbert Hyman’s Political Socialization (1959). Hyman believed that differences learned during the child’s acculturation process were the core of political socialization, but it is important to recognize that his perspective was a sociological one (Mereiman, 1972). Although the sociological emphasis conceived of political socialization as a learned process, ironically, it directed research attention away from the actual process of learning to the content of political socialization itself (Sigel, 1966; Cook and Sciolli, 1972; Renshon, 1977). The sociological perspective, which became the dominant paradigm, considered the child to be a passive receptor of unidirectional environmental stimuli emanating from “agents” of political socialization, e.g., the family, schools, and peer groups.

The short shrift that the sociological perspective gave to the concept of learning produced a number of critiques of this dominant research paradigm (Froman, 1962; Greenstein, 1970; Baker, 1971; Cook and Sciolli, 1972; Dreitzel, 1973). In two review essays, Froman (1962) and Greenstein (1970) presented an alternative view of political socialization—based in what Sears (1975) has called the child’s idiosyncratic personal growth. However, even this perspective reflected a pervasive environmentalism, precisely because individual personality characteristics were also presumed to be learned (Renshon, 1977:17).
Other critics have realized that in order to step back from the presumption of a *tabula rasa*, it is necessary to recall, as Dennis Wrong so emphatically put it for sociologists, “in the beginning, there is the body” (Wrong, 1961:191). Baker, for example, proposed an interactive transactional view of the political socialization process based on the notion that the “individual has specific needs and drives” (Baker, 1971:593). Dreiztel asserted that “of course children cannot be understood as active and possibly controlling partners in an interactive process as long as their congenital dispositions are not taken into account” (Dreiztel, 1973:15).

Bay’s 1965 essay also emphasized human needs. His critique was in general directed against political behavior research, but his democratic concern for individual development and his distinction between basic human needs and wants versus perceived needs is pertinent. Bay emphasized that unless it recognized human needs, political science research might simply focus on want “artificially induced by outside manipulation” (Bay, 1965:48). In general, these critiques emphasized a democratic concern for the individual. However, they introduced biological factors only on an abstract level—with no discussion of instrumentation or the actual inclusion of these factors in political socialization research. Unfortunately, this remains an accurate generalization despite the fact that the editors of two of the most recent anthologies of political socialization studies have called for research into “biologically based dispositions” (Renshon, 1977) and “biopsychological variables” (Schwartz and Schwartz, 1975). Moreover, there are some exceptions—empirical studies employing biological variables have been conducted in such areas as effects of health status (Schwartz, Garrison, and Alout, 1975), drug usage (Jaros, 1972), and hormonal variations (Peterson, 1978; Jaros and White, forthcoming).

It has been difficult to incorporate biological variables because biological determinism has often been counterposed to environmentalism as the only alternative basis available for devising a research strategy. As we will argue, environmentalism and biological determinism are not the only two assumptions available, but if they were, the tabula rasa paradigm would probably be superior—even though it provides a truncated conception of the socialization process. Furthermore, some prefer the tabula rasa assumption because it appears to mesh more closely with concerns for political reform. Finally, if the introduction of a biological model stifies useful original research—for example, because it views some extant sex differences as intrinsic and not modifiable by the environment when in fact they are malleable in character—then such a model will be of questionable value.

Given these difficulties, it is perhaps not surprising that the dominant research paradigm still emphasizes passive learning (e.g., Sears, 1975; Dawson, Prewitt, and Dawson, 1977). However, the current body of knowledge about the socialization process has very serious shortcomings. Six years ago, Weissberg and Joslyn (1977:45) pointed out that although early studies and their conclusions had been called into serious question, “to date, no new body of positive findings has replaced the older knowledge.” (Weissberg and Joslyn, 1977:45). Moreover, despite the increasing methodological sophistication of political socialization research, they point out that “most analyses are still more or less operating theoretically at a level no more developed than when Herbert Hyman wrote *Political Socialization* 15 years ago” (Weissberg and Joslyn, 1977:45). Our reading of the literature suggests that this generalization is still true today. Furthermore, we argue that the dominant research paradigm of environmental determinism has been stultifying innovative research. In order to move beyond this conceptual impasse, we will examine biopolitical theory to develop a more constructive focus on biological factors in political behavior.

According to Wiegela (1979:88), “biopolitics has attempted to blend strands of knowledge from the life sciences and the social sciences in an effort to better understand human political behavior.” In Somit’s words (1976:3), it is “an effort at scientific cross-fertilization—an attempt to draw upon the intellectual and methodological armory of modern biology.” As is typical of most fields of inquiry, and especially of new ones (Wiegela traces the biopolitical orientation to a 1968 essay by Somit), biopolitics embraces a diversity of methodological and theoretical approaches that we will make no attempt to summarize or integrate here (the interested reader should consult Somit, 1976; Wiegela, 1979; Watts, 1981; Losco and Baird, 1982). Instead, we will select some elements of the biopolitical perspective relevant to our purposes.

First, biopolitical theory and research present a fundamental reorienting of political science research. In this context, Schwartz (1976:17) states: “biopsychological inquiry must either change the prevailing paradigms or be incorporated into those paradigms; it cannot be unrelated to those paradigms.” The challenge to political science paradigms is twofold: the biopolitical perspective will render research more scientific while it is at the same time expanding the concept of human nature and the scope of political inquiry. These concerns are brought together in John Wahlke’s 1978 presidential address to the American Political Science Association. Paraphrasing Albert Somit, Wahlke (1979:25) argues that “pre-behavioralism can only be overcome by surmounting our ‘biobehavioral illiteracy.’” Wahlke continues, “Political scientists ignore that knowledge, i.e., of the biobehavioral sciences at their peril; they rest their work on flimsier, less scientific and untestable pseudo-theories and models at the risk of talking utter nonsense about political behavior.”

Biopolitics is not isomorphic with sociobiology, although inferences from evolutionary biology and principles of natural selection have constituted one of its major elements (Losco and Baird, 1982). Wiegela (1979) refers to a division in the biopolitical community: one focus is “evolutionary,” based in ethnology and sociobiology; the other is “physiological,” based in medicine, physiophysics, psychopharmacology, nutrition, and public health, etc. This division gives rise to different emphases: “those scholars with an evolutionary thrust seem to have dealt primarily with mass issues that in some way affect all of mankind, while scholars with a physiological thrust have dealt primarily with small groups or individual human beings” (Wiegela, 1979:4). This distinction may be an increasingly blurred one, as Wiegela suggests, but it is one we will follow, selectively adopting the physiological thrust in our biopolitical perspective.
We adopt the physiological thrust for two reasons. First, the physiological emphasis on the individual is more consonant with research in political behavior. It is our assumption that to speak scientifically about any kind of biological factor, we must be able to refer to a physiological substrate. Without this careful specification of physiological factors, the danger is to fall back on simplistic and biologically deterministic types of descriptions such as Tiger’s (1970) assertion that men are biologically programmed to dominate politics or Wilson’s (1975) assertion that male dominance and leadership have a genetic, biological base. A discussion of the adaptive significance of a behavior at the species level does “not imply anything about the extent to which environmental experiences in the developmental process may modify behavior at the individual level” (Baer and Bositis, 1982:30, and forthcoming). Furthermore, Archer criticizes on logical grounds the common assumption in evolutionary arguments that adaptive traits have left a psychological rigidity in our make-up. “There can be selection for plasticity and flexibility in a trait, so that the fact that a particular difference is the product of selection does not mean that it is inflexible” (Baer and Bositis, 1976:258).

The second reason we adopt the physiological thrust is quite frankly a political one. Sociobiology is controversial. In particular, its treatment of sex differences in political behavior and political power has been seen as sexist (O’Kelly, 1980). Recent efforts have been made to capture the theory of evolution for feminist analyses (Hrdy, 1981; Kay and Meikle, forthcoming). Hrdy, a feminist sociobiologist, emphasizes that early sociobiological interpretations were based on dated primates which had neglected the study of female primates, a lack that her work addresses. Although we agree with Hrdy and with Kay and Meikle that ethological studies of primates and the principles of natural selection are not anti-feminist, the literature of sociobiology is littered with so much sexist underbrush that to include it would require an extensive feminist critique that we do not have the space to present. Furthermore, as noted above, the biopolitics perspective is not limited to extrapolations from ethology, and hence, it is not necessary to so limit ourselves as well.

To outline the implications of the biopolitical perspective for political socialization, we will briefly turn to the work of Glendon Schubert, a scholar Wiegele (1979:20) describes as “intimately engaged in rethinking the basic structure and research paradigms of political science; his writings have had a powerful impact on biopolitical scholars.” In an early essay, Schubert (1976) develops a view of “politics as a life science.” His position is primarily a physiological one, although he does point out that political scientists should “become better educated in and start facing up to the facts of biological life, including their own life history as a species” (1976:164-65).

Schubert’s conceptualization of biopolitical behavior is explicitly interactive with mutual causation among biological, physiological, cognitive, and other sets of variables. Schubert criticizes both the social science paradigm with its overemphasis on cognitive processes and the biological paradigm with its overemphasis on physiological processes. Each is inadequate to the task of explaining human political behavior. In their place, Schubert proposes a life sciences paradigm which incorporates elements of both. He views political behavior as a causal function of the natural and social environment; biological characteristics, and basic needs mediated by both cognitive and physiological processes. This interactive conceptualization of political behavior is also evident in Schubert’s analysis of human sex differences (Schubert, forthcoming; see also Baer and Bositis, 1982, and forthcoming; Jaros and White, forthcoming).

The incorporation of the concept of basic needs in biopolitics is consonant with the emphasis in political socialization theory on the at least partial biological basis of human psychological motives. However, Schubert’s biopolitical conceptualization expands the notion of human needs to include not only psychological (e.g., self-esteem and self-actualization) and social (e.g., sexual interactions) but also biological needs (e.g., sexual reproduction) and physical/chemical needs (e.g., sleep, water, food, and sensory stimulation). For Wiegele and others, the expanded conceptualization of basic human needs provides the critical normative edge for biopolitical inquiry. Wiegele (1979:8) suggests that:

Political science has often been antihumanistic because it has not adequately taken into account the biological aspects of political behavior. By looking at the entire person as a biological as well as an intellectual and emotional creature, we are assuming a much more humanistic approach. Biopolitics, then, can be viewed as an effort to restore humanism to political inquiry.

In this context, it is ironic that some have characterized the recent turn to biology among social scientists as a search for surrogates for religious and ontological givens (Lowe, 1976; Arney, 1980; Breims, Cerullo, and Stacey, 1981). Humanism arose initially as an intellectual movement opposed to religious other-worldliness. The humanism embodied in biopolitics gives no credence to this type of ontology. In Schubert’s words (1976:164):

Biological theory implies the rejection of the presumption that our political theory as a species began 2,500 years ago in Athens, or (alternatively) as described in ‘naturalistic’ fables (whether optimistic like that of Rousseau or pessimistic like that of Hobbes) or according to the authoritative allocation of values in the even more popular fable of Genesis.

The humanism of biopolitics is based—not in the assumption of biological needs—but in the scientific and empirical explication of real human needs.

Before we leave our heuristic examination of the merging perspectives of biopolitical and political socialization theory, we will note a possible area of conflicting emphases. Both theoretical perspectives presume an interaction between the human organism and the environment. In political socialization theory, this interaction has traditionally been expressed in the context of development. Learned cognitive aspects of political socialization have been viewed as proximate causes of political behavior; biological dispositions have been presented as more distal in influence—indirectly structuring what is learned (Renshon, 1977; Baer and Bositis, 1982, and forthcoming). Biopolitics, however, considers that biological factors have a direct effect on behavior. As Schwartz emphasizes (1976:28), “The assumption stated here... holds that biopsychological variables cannot properly be ignored or minimized because some of their influence on...
political behavior is direct, is not entirely encapsulated or mediated by processes already studied in political science.  "  Schwartz's assumption is related to the emphasis in the biopolitics literature on psychopharmacology (Somit, 1968; Wiegele, 1979).  Jaros (1972), for example, suggests that depressants may "desocialize" the individual.  It is likely that recognizing a direct role for biological factors may be an essential element in revitalizing the concept of learning in political socialization research (Bell, 1968; Cook and Sciolli, 1972), but it is unclear at present what this implies for political socialization theory.  These different emphases may be resolvable in future considerations of political socialization theory and biopolitics; for the present, we prefer to consider the role of biological factors in the development of gender and sex differences in political behavior.  We now extend our merging of theoretical perspectives to feminist thought.

Feminism, Sexism, and Biological Explanations

Academic and feminist circles express considerable concern that biological explanations of human behavior may be inherently sexist (Hubbard, Henfrin, and Fried, 1979).  This concern derives from the historical use of arguments based on (real or presumed) biological differences between men and women (Flexner, 1975) and on their resurgence in recent public debates (Frieze et al., 1978: 191, 208-209).  These concerns are not alleviated by problems of sloppy scholarship.  For example, Baer and Bositis (1982, and forthcoming) note that a number of studies proposing biologically based explanations for extant sex differences ignore areas of political participation where female levels equalled or surpassed those of males.

The concern that using biological variables to explain sex differences in political behavior may reflect sexist values is both palpable and understandable in light of past discrimination.  One encounters these views both in conversation with colleagues and at professional meetings.  Yet it is curious that little in the way of published criticism can be found in political science journals.  Cook (1982: 54) concludes that "the biopolitical model is not inherently sexist, but the way the concepts have been operationalized and in the form of the propositions have not been guaranteed findings of sex differences and female inferiority."  The paucity of published critiques in political science literature is even more curious because the number of critiques of behaviorally based research using psychological or sociological models is so large (Bourque and Grossholz, 1974; Goot and Reid, 1975; Jaquette, 1976; Eshtain, 1979; Evans, 1980).  The lack of published sources places us in the uncomfortable position of criticizing a view that we cannot directly document.

It is important to separate the political issue of sex discrimination from the scientific question of the etiology of sex differences in political behavior.  Science, of course, is not value-free as Sherif (1979) reminds us in her critique of psychological research on women.  Values do influence scientific research, in the choice of the topic of investigation, in the conceptualization of the problem, and in the operationalization of the study.  However, following Sapiro (1979: 263-264), we argue that sexist research is simply poor scholarship: "Scholars cannot make assumptions about women or ignore gender where it is relevant without violating their own canons of research."  It is one thing to view a particular study as sexist in conception or design, it is another to characterize an entire area of inquiry—like biopolitics—as sexist.  It is increasingly unacceptable to articulate environmental determinism (and, of course, biological determinism) as the basis of sex differences.  It is not surprising, therefore, to find feminist journals including articles that seriously consider the involvement of biological factors in the development of sex differences (Signs, Summer and Fall issues, 1980; Women and Politics, June 1983).

The political value of opposition to sexism is helpful in sensitizing researchers and potential critics to oversights or gaps in the scientific explanation of the problem; however, evaluation and assessment of an explanation according to scientific principles must be based on systematic observation and logical inference.  The issue is that of a good explanation versus a bad one—not the "strawman" opposition of a biological versus an environmental explanation of sex differences.  We hope to make this clear: (1) by briefly pointing out that the feminist perspective does not necessitate the exclusion of biological factors because feminists themselves have included biological factors in their own explanations of sex differences; and (2) by contrasting the biopolitical perspective on sex differences and sex roles with the functionalist theory of social roles.

Increasingly, academic feminists are turning to the life sciences for a fuller understanding of sex differences.  In addition to the feminist journals we noted earlier, we also point to the recent collection of articles edited by Parsons (1980), The Psychobiology of Sex Differences and Sex Roles.  This volume developed out of a conference organized by the Amherst-Massachusetts-area Five Colleges Women's Studies Program.  The conference was designed to assist those teaching and doing research in women's studies to incorporate "biological theories of sex-role di- morphism and women's life cycles" (Parsons, 1980:xv).

Two earlier and well-known efforts which attempt a similar incorporation include psychologist Judith Bardwick's Psychology of Women (1971) and sociologist Alice Rossi's (1977) essay on "parenting."  Both of these earlier efforts have been controversial.  Breims, Cerullo, and Stacey (1978) note that "reasonable feminist, research has been established by her earlier call for equality between the sexes (1964), but they argue that her use of biology is a form of determinism and is consonant with the current anti-feminist backlash.  Lloyd (1976: 19) also points to the question of social responsibility in stressing that: "Bardwick ... and others who acknowledge the interactive nature of social and biological variables yet nonetheless stress the latter in order to right what they see as an imbalance, fail to appreciate the danger of their approach.  Although professional colleagues might be presumed to understand, once beyond this charmed circle, misinterpre- tation is very likely."

In assessing the turn toward biology among feminist scholars, it is difficult to avoid the suspicion that the intellectual paradigms and controversies of their respective disciplines structure their explanations of sex differences as much as their feminism does (Griffin, 1978).  However, one of the foremost challenges to social science para- digms is the growing literature produced by the biologi-
cally oriented social scientists—what Wiegela (1982) has termed an "intellectual revolution." In this context, Sherman's (1978:135) observation is pertinent:

In a period when environmental explanations (of sex-related cognitive differences) have been supposed to dominate biological explanations, we have found that biological explanations are plentiful and vigorous. Those writers who championed a biological view, thinking to redress an imbalance in the literature, were mistaken in their perceptions . . . it is now the environmental view that has been neglected.

Because of the immersion of feminist scholars in the "charmed circles" of their respective disciplines, and in order to document the thesis that biological factors are not anti-feminist, we cite the explanations offered by radical feminists. Eishtain (1981) provides an excellent review of radical femi thought. A few examples will suffice to make our point: Shulamith Firestone, The Dialectic of Sex (1972); Susan Brownmiller, Against Our Will (1975); and Marge Piercy, Woman on the Edge of Time (1978). Firestone's feminist theory locates the oppression of women in the tyranny of biology—the biological and physical distinction between the sexes which can be overcome only through cybernetics and test tube babies. In Brownmiller's historical analysis of rape, men dominate women through their fear of attack and rape. Men are motivated to dominate through their (male) lust for power and their physical and biological capacity for rape. Finally, in Piercy's utopian novel, men are injected with hormones so that they also can breastfeed babies. For radical feminists, "patriarchy" rests upon biologically based sex differences that must be eradicated before its demise. The irony is that only the policy prescription distinguishes the radical feminist from the sexist use of biology (e.g., Goldberg, 1974)—either destroy the "natural" or embrace it.

The question then becomes: what is the normative position of biopolitical theory toward "natural" biological sex differences? This question can be answered in part by referring to functionalist theory. Parsons and Bales (1955) applied the quintessentially learned concept of roles to the nuclear family in such a way that its current structure seems inevitable. In their view, with the development of industrialization and the shift of productive functions outside the home, the family became a specialized institution to socialize children and meet the personality needs of its members. From research on small groups, Parsons and Bales concluded that all groups, including the family, specialized into "instrumental" and "expressive" roles. Zelditch (1955) extended this view cross-culturally, hypothesizing that the female became the expressive leader in the family because she was biologically attached to the infant. The male, on the other hand, adopted the problem-solving role of the instrumental leader because he was not "disabled" by childbearing or so attached to the infants. Feminists have criticized role theory because it ignores conflict in families and the difference in social power between men and women. "The language of roles conveys not only a sense of the fixed and dichotomous but also an image of separate and equal" (Thorne, 1982: 8).

The normative edge of biopolitical theory is its emphasis on human needs. Assume for the sake of argument that a special biological attachment exists between mother and infant (although this is by no means an established biological fact). Then, this attachment would represent a human need that should be given some priority in social policy as well as in biopolitical explanations of sex differences. The existence of such a need does not mean that human beings could not rationally (or not so rationally) restructure human society to disrupt it. Indeed, history indicates that human societies have often done just that (see, e.g., Badinter, 1981).

Our biopolitical perspective would be critical of such efforts because they deprive both mother and child of a very human source of personal enrichment. Moreover, our perspective in no way assumes that women have fewer or less developed needs in other areas than do men. This hypothetical attachment represents an additional need for mothers, not a quasi-religious support for the social isolation of adult women to a child-bound home (in this context, see Schubert's 1976 discussion of the psychological needs of self-esteem and self-actualization alongside social and direct physical/chemical needs).

Socially, the biopolitical perspective does not require the sublimation of the mother's needs to those of the child. Above all, the critical thrust of the biopolitical perspective is to make political behavior research more scientific in its explanations. Certainly no scientific enterprise is free from value biases. However, if biopolitics can claim a more fundamental grounding in science, then those employing it should be all the more sensitive to feminist critique and refinement. This should be no less true for biopolitics than for behavioral research (Sapiro, 1979).

A Note on Our Definition of Gender

Even a cursory reading of the relevant social science literature rapidly yields the conclusion that there is a great deal of ambiguity and disagreement over the concepts of sex and gender, or alternatively, sex role and gender role. This represents a fairly serious flaw in the research. So long as there is no consensus on the problem to be explained, then the questions concerning the etiology of the problem remain moot.

One of the reasons for the lack of consensus relates to political values. Feminist thought emphasizes change. As Breims, Cerullo, and Stacey (1981:43) point out, "Feminists identified the family as a critical institution in the reproduction of social relationships generally, and decisive for women's subordination. Although male dominated family and social systems are nearly universal, locating the family in history, not biology meant that change was possible." In many cases, this emphasis led to a dichotomy between the concepts of gender and biological sex. For feminists, gender became an ideological system whereby society used anatomical differences as cues to assign individuals to gender groups. Rubin (1975:179), for example, refers to gender as a "socially imposed division of the sexes" and postulates a sex/gender system: "the set of arrangements by which a society transforms biological sexuality into products of human activity" (Rubin, 1975:159). As Schubert (forthcoming 6:7) argues, this postulated dichotomy of sex and gender is problematic in that "sex tends to be understood to imply genetic determinism; while at the same time gender is taken to suggest an exaggerated presumption of cultural determinism" whereas an interactive and transactional view of the problem demands a "common language of discourse for the discussion of both genetic and epigenetic effects."
Schubert prefers to use "sex" as the common term in lieu of "gender," an analytical concept he views as inappropriately borrowed from linguistics.

A second reason for the lack of standard usage reflects the different theoretical paradigms of the social sciences. Psychologists tend to focus on aspects of sex differences relating to personality—particularly one's self-concept of identity and character traits (Bern, 1974; Spence, Helme- reich, and Stapp, 1975). Sociologists, by contrast, concentrate on external norms of appropriate masculine and feminine behavior, rather than internalized psychological dispositions (Mason and Czaika, 1976). The concept of role, of course, is at base a sociological concept drawn from the functionalist theory of social roles. These differing emphases are not absolute. In a recent study, for example, Storms (1979) compared sex-role identity, sex-role attributes, and sex-role stereotypes.

Feminists have criticized the concept of role that is in widespread use. Sociologists Lopata and Thorne (1978) view gender as a pervasive identity that defines the characteristics and structures the roles that society assigns to men and women. For Lopata and Thorne, gender is a complex identity akin to class or race, not a single social role. Some psychologists (e.g., Bern, 1974; Spence, Helme-reich, and Stapp, 1975) view the role concept as biased in its stress on polar personality traits. As an alternative these authors suggest that the androgy nous individual who blends "male" and "female" attributes is more flexible and psychologically healthier.

The concept of sex role is especially problematic when biological factors are included in explanations of human behavior. The sexually dimorphic biological structures of men and women may too easily be correlated with the polar sex-role stereotypes of appropriate masculine and feminine behavior in ways that are consonant with cultural stereotypes, yet are absurd from a scientific viewpoint. For example, Goy and McEwen (1980:54-58) seriously discuss the patently absurd proposition that prenatal hormones influence "gender role behaviors" such as interest in appearance, playing with dolls, tomboyishness, athletic ability, and interest in career versus motherhood. Kaplan (1980) has also criticized this research from an androgy nous perspective.

The concept of sex role is not the only basis for conceptualizing sex differences. Sherman (1978:160) notes the paradox that "even androgy nous perpetuates sexotyping because inherent to the concept of androgy nous is the assumption that some characteristics are male and some characteristics are female." Elshtain (1981b) and Spelman (1982)—who both acknowledge biological sex differences—also take issue with the feminist goal of androgy nous. Spelman criticizes what she terms somatophobia in feminist thought. Elshtain points out that sex differences, rather than being obvious reminders of sexual repression, may, on the contrary, enrich social life. Both scholars stress the distinction between sex differences and a division of labor or rewards based on sex differences. Hence it is important to realize that identifying sex-related differences does not automatically demonstrate their social or political significance when the concept of role is dropped.

The feminist critique and counter-critiques of role and androgy nous highlight the importance of defining the theoretical relevance of sex-related differences. For political socialization researchers, this theoretical relevance must come from the conduct of political inquiry, not from borrowed concepts designed to answer questions drawn from the intellectual paradigms of other disciplines. Political scientists are concerned not with the adjustment of individuals to an interdependent set of social roles, nor with the development of healthy personalities. We are primarily concerned with whether the political behavior and participation of adults, the expression of political interests, and the exercise of political power vary in meaningful ways for men and women.

The concept of role does not suit our purpose, nor is it satisfactory to define the problem empirically by comparing men and women on standard social science variables. If Elshtain and Spelman are correct, then recognizing biological sex differences means that consistent sex-related differences might exist that are of no practical political significance. In hypothesis testing, finding a statistically significant difference between two categories of a dichotomous variable is of no particular importance—especially given a large sample size—unless the difference is substantively and theoretically meaningful. For this reason, focusing just on women or just on a statistically significant sex difference is not a very enlightened exercise.

These considerations suggest the need for a political science definition of the problem. Baer (1980) has proposed the concept of gender as a world view. Gender comprises three independent dimensions upon which sex-related differences have been found. Following Verba's and Nie's (1972) conceptualization of psychological modes of participation, these dimensions structure the extent and type of political activity in which citizens engage. The three dimensions include: (1) psychological involvement in politics (low versus high initiative); (2) tolerance of conflict (low versus high); and (3) orientation to politics (parochial or personal instead of collective or ideological).

This formulation is useful for several reasons. First, this approach does not predetermine the form of the explanation, i.e., learned versus innate. These dimensions are neither standard attitudes in the sense of conscious verbal self-reports of presumed psychological dispositions nor predominantly biological dispositions. Second, the dimensions represent a continuum over which historical and cultural change in the political behavior of men and women can be measured, not a mutually exclusive dichotomy between traditional and nontraditional sex roles. Third, these dimensions are politically relevant, and according to Verba and Nie (1972), they have demonstrated relationships to political behavior. Fourth, this information does not post any normative position on "proper" male or female participation. Gender as a political world view represents a supplementary concept appropriate to political inquiry, not a replacement for the psychological and sociological concepts of sex identity and sex role stereotypes.
Application of Biopolitical Perspectives

Our goal has been to make a persuasive case for the inclusion of biological factors in investigations of the political socialization of gender. Thus far, our efforts have focused on the theoretical underpinnings of this approach to the study of sex differences in political behavior. This effort would be incomplete without some concrete examples of areas of biobehavioral inquiry as applied to gender. We will outline four such areas: (1) hormonal influences on behavior; (2) brain lateralization and cognitive style; (3) biocultural event of motherhood; and (4) physical size and strength differences between men and women. These four areas are certainly not an exhaustive treatment of the subject, nor are they mutually exclusive, at least in the sense of physiological causality. The first three constitute areas of biopolitical research and analysis, the fourth represents a consideration raised by feminist thought. All of the areas are the subject of ongoing research; hence the conclusions we draw will be only tentative.

Hormonal Influences on Behavior

In this section, we will briefly review some possible relationships between relative levels of hormones and behavior. There are two avenues of investigation. First is a linkage between a particular hormone and a specific behavior. These include the linking of testosterone and aggression (Corning and Corning, 1972; Crook, 1973; Deardon, 1974; Davies, 1977, 1980) and estrogen and nurturance (Davies, 1977). The influence of testosterone presumably distinguishes between men and women because men tend to have relatively higher levels of testosterone, and women tend to have relatively higher levels of estrogen. The influence of estrogen is inferred by viewing changing levels of female hormones as a "natural experiment" (Jaros and White, forthcoming) and analyzing these varying levels for a range of possible effects. Hormonal variation occurs during the lunar cycle of menstruation, during pregnancy, and directly following childbirth. Variation in hormonal levels presumably distinguishes men and women because "female" hormones do vary on a regular or fairly predictable basis. Men, on the other hand, are supposed to remain in a steady state physiologically.

The linking of testosterone with aggression, and in turn, with political participation has been the subject of several biopolitical analyses. The idea of a relationship between aggression and political participation did not originate in the biopolitical literature. Gergen and Ullman (1977:415; see also Muller, 1979) point out in their discussion of political socialization of political activism that "it is commonly believed that political participation represents a socialized form of aggression." This model suggests that a biological factor might be involved in sex-related differences regarding the tolerance of conflict and in psychological involvement in politics.

Biopolitical analysts have focused on the experimental biological research that found testosterone to increase aggression in most animals (Conner and Levine, 1969; Barr, Gibbons, and Moyer, 1976). Baer and Bositis (1982) have criticized this focus extensively, pointing out especially that hormonal levels are not sexually dimorphic, but overlapping distributions. If a hormone produces a specific effect, it should do so in women as well as men. Persky (1974) proposes that females may be more sensitive to testosterone than men. Furthermore, aggression as defined in animal research—increased fighting and aggressive sexual behavior—would be counterproductive to success in politics. The testosterone participation model rests upon the similarity of aggression and assertiveness, but these are different motivationally, socially, and behaviorally.

Davies (1977) has proposed that higher estrogen levels in women result in nurturant behavior. Since he does not elaborate, it is unclear how nurturance might result in sex differences in gender, unless it somehow represented a "mothering instinct" that limited women to a private and "unpoliticized" domestic sphere. At this point, very little firm evidence lends credence to this view. Messent (1976: 189) points out that "maternal behavior has long been thought to be dependent on female sex hormones simply because males of many species fail to show it." Of course, the sex-stereotyped aspect of parenting research, which relies on maternal behavior as the baseline against which to compare paternal behavior, is a serious bias (Kaplan, 1980). Unlike the apparently consistent effects of testosterone on aggression across species, maternal behavior among animals is a complex set of divergent behavior—e.g., giving birth, sucking the young, nesting, retrieving the young—which appear to be differentially affected across species by different types (e.g., estrogens, progesterones, and prolactin) and levels of hormones (Messent, 1976). Cofer and Appel (1964) conclude that it is inappropriate to speak of a maternal instinct among animals so it appears to make even less sense to assume one among humans.

Two biopolitical studies have considered the effect of menstrual cyclicity on variance in political attitudes (Peterson, 1978, Jaros and White, forthcoming) but found no relationship. Baer and Bositis (1982) suggest a number of reasons for this. The body of psychological research on the menstrual cycle is riddled with methodological weakness and procedural variability which preclude generalizing to the average woman. This research does not support the assumption that menstruation results in instability in moods or behaviors. Further, the notion that menstruation results in stress is questionable in that stress itself results in amenorrhea. A similar analysis is related to changing hormonal levels during pregnancy and childbirth—it is thought that rapidly falling hormonal levels result in post-partum depression. However, it is unwarranted at this point to infer that this is due to physiological causes rather than the medical intervention in childbirth (Hahn and Paige, 1980).

In reviewing the negative conclusions we have drawn concerning direct hormonal effects on behavior, we would like to offer a more positive construction following Prange and Lipton (1972:226):

In general, the effects on behavior appear to be quite nonspecific. As we have stated, either an excess or a deficiency of adrenal steroids may produce either mania or depression. Nonspecificity is perhaps best illustrated when opposite behaviors alternate . . . if rats, especially females are given any of several antithyroid drugs, certain behavioral cycles appear in a high proportion of animals.
Several of the studies suggest that testosterone increases activity level (Messent, 1976; Gannon, 1981). A higher level of activity increases the probability that any behavior will be engaged in—aggressive, assertive, or nurturant. The direction of behavior is probably structured by the socialization process, not the hormone. Hence, if aggression is more sex-typed for male behavior, then testosterone-induced, higher activity levels might be associated with aggression, but not necessarily so. Testosterone might have a similar effect on women, although sex-typed norms differ. One of the few studies to investigate testosterone levels in women found that they differ, with employed and professional women having higher levels (Purifoy and Koopmans, 1979).

**Brain Lateralization and Cognitive Style**

Evidence suggests that for normal (meaning, in this case, right-handed) persons, the two hemispheres of the brain specialize in different tasks. The left half specializes in verbal tasks (processing items one at a time); the right half, in spatial, holistic processing (processing items simultaneously) (Sherman, 1978; Bryden, 1979; Schubert, forthcoming). The term brain lateralization usually refers to the speed or completeness of the establishment of left hemisphere dominance for verbal analytic functions and right hemisphere dominance for spatial, holistic functions (Sherman, 1978).

It has been suggested that the brain organization of women differs from that of men. Left hemisphere language lateralization has been reported to begin earlier in women (Sherman, 1978; Bryden, 1979), but by adulthood women tend to be bilateral in their brain organization, i.e., verbal and spatial functions are bilaterally organized (Sherman, 1978; Flor-Henry, 1980). Right hemisphere lateralization occurs earlier in men, and prior to adulthood, men eventually surpass women in left hemisphere lateralization as well. Thus, the adult male brain is more lateralized for both verbal and spatial processing (Schubert, forthcoming). Women tend to equal or surpass men in certain verbal processes, and men are reported to be superior to women in performing spatial, holistic tasks.

Lateralization fits very well with themes we developed earlier. It represents a new, potentially valuable area of biopolitical inquiry whose findings have significance for political socialization and gender. Although the meaning and import of brain lateralization studies for biopolitics are still unclear, their potential relevance can be approached in a number of ways. For example, Schubert (forthcoming) argues that humankind should move away from the male-lateralized, confrontationist political style to a more female style emphasizing problem solving through language and verbal skills. On a more mundane level, differences between male and female brain organization are probably reflected in different perceptual styles and approaches to information—another example of variation along one or more dimensions of gender and politics. Finally, the current evidence and methodology relating to brain lateralization are still in dispute. A priori criticisms (e.g., arguments that the testing procedures do not measure what they purport to measure) as well as environmental criticisms (e.g., arguments that learning differences provide more plausible explanations than lateralization differences) have both been raised (Sherman, 1978; Bryden, 1979; Nash, 1979).

Two findings from brain lateralization studies illustrate these points. Women are reported to be poorer spatial thinkers than men, and considerable evidence suggests that women are poorer in mathematics (although it is not entirely clear how brain organization relates to mathematical reasoning) (Sherman, 1978; Fox, Tobin, and Brody, 1979; Nash, 1979). In both of these instances a reasonable case can be made that brain organization affects female performance as measured by the appropriate tests. However, there are other—not necessarily alternative or competing—environmental explanations. For example, child-rearing practices differ for boys and girls (and also among families). Parents set different rules for girls and boys with regard to exploration behavior. Boys are allowed to investigate and get to know a larger terrain than girls (Hart, 1979). Thus, experience equips boys and girls to perform differently on the types of spatial tests used in this area of study. Eventually we may find that brain organization differentiates boys and girls initially but that child-rearing practices reinforce and magnify these differences. If the practices change, the differences might disappear.

Male-female differences in mathematical performance can be addressed by examining a whole host of social and psychological mechanisms that tend to suppress women’s performance especially during and after the teenage years (Fox, Tobin, and Brody, 1979). Evidence suggests that differential performance levels are related to differential rewards and encouragements along with peer pressure. Again we have a situation where a biopolitical explanation might take the character of an initial difference magnified by environmental forces such that the difference was truly substantial. In each of these instances, interactions between biological and environmental factors could account for considerable variation between individuals—and families.

**Biocultural Role of Mother**

We do not need to convince political scientists that motherhood is an important factor in political behavior. Several studies (Flora and Lynn, 1976; Jennings, 1979; Jennings and Niemi, 1981, McGlen, 1980) suggest that the role of mother reduces interest in politics or limits it to a more local range and reduces tolerance for conflict. Biological factors have not been ignored in considering the role of mother (Fraiberg, 1977; Rossi, 1977; Kaplan, 1978), nor is there a lack of scholars who feel that a biobehavioral approach represents an important challenge to an environmental or culturally deterministic approach (Arnay, 1980; Breims, Cerullo, and Stacey, 1981). Because of the extensive literature on this subject, we will limit our discussion to a few salient points.

First, before any mothering instinct is resurrected, consider how pregnancy makes a young woman especially susceptible to resocialization. The physiological capacity of women, but not men, to conceive, gestate, and bear young structures different learning experiences (Baer and Bositis, 1982 and forthcoming). Almost all of the things that give most adults certainty and continuity are suddenly altered in the pregnant woman. Externally, pregnancy is obvious to others and often results in different treatment. Internally, the woman may feel out of touch with her body—her body image is changing. Pregnancy changes aspects of daily routine such as diet,
sleep patterns, intake of alcohol, and wardrobe. Once the child is born, a similar resocialization process occurs so long as the woman stays home with the child—a discontinuity with her previous life. If the literature is correct in postulating a crucial learning stage in generational learning (Cutler, 1975), then the age at which a woman becomes a mother may be politically significant, along with her experience.

Second, the mother-infant bond is qualitatively different from the father-infant bond in a number of ways, whether or not one accepts the notion of a crucial bonding period following birth. The mother and not the father carries the fetus in intimate interaction. The fetus, it should be remembered, is sensitive and responds to noise, light, and movement in constant interaction with the mother. The mother has direct personal knowledge and acquaintance with the fetus from which the father is biologically excluded until the child’s birth. The mother-infant bond is also qualitatively different if the mother breastfeeds the infant. Some feminists reject the significance of breastfeeding because bottle-feeding is available. However, human milk is superior for nurturing human infants to that of any other species (Ogra and Greene, 1982)—there are important biological reasons why mothers continue to nurse their infants.

Third, some biological factors facilitate a mother-infant bond. One is the alert state following birth, in which infants are alert and attentive to the environment for a longer period than they will be for weeks following birth. Another is the physiological process of individuation of the newborn (Kaplan, 1980). A third is the physiological response of the lactating mother to the cry of the newborn. The cry apparently stimulates secretion of oxytocin and invokes the let-down reflex so that the milk flows. In newly delivered women, the milk flow occurs prior to nursing, often at any infant’s cry, and it is a learned response for the let-down reflex to occur independently for each breast and only when the baby is nursing.

Finally, we stress that even if the notion of attachment or bonding becomes established (it remains controversial), in no way can it account for the “mothering” of the 1950s as critiqued by Friedan (1963) and others. Precisely during this period, women returned to the “home,” yet they bottle-fed their infants, and the medical establishment employed every technique available to disrupt any bonding at birth, with drugged labors, routine episiotomies, and the separation of mother and infant at birth (Hahn and Paige, 1980). These considerations suggest the need for an interactive paradigm upon which to base the analysis of the effects of the biocultural role of mother on gender.

Physical Size and Strength

Feminists have generally acknowledged that males tend to be physically larger and stronger than women. Radical feminists such as Brownmiller see this biological difference as critical in the domination of women. Feminists concerned with equal employment rights treat the “fact” of women’s lesser strength as unimportant because brute strength is not necessary to perform most jobs in modern society. We have included this area of biological research because recent research indicates that the female “disadvantage” is much less than previously thought—even by feminists.

At present, there is little question that men are stronger than women in absolute terms, although the magnitude of the differential may vary (Lauback 1967). The greater strength of men may affect the social power of spouses in marriage. It is estimated that about 7 percent of wives have received severe beatings from their spouses at some point in the marriage (Steinmetz, 1978). It is unclear whether women do feel threatened, but to the extent that they do, they are likely to avoid conflict that might escalate to physical violence and to be less assertive in social situations generally. This tendency, of course, relates directly to relevant dimensions of gender.

The biological evidence on the potential capacity for strength and fitness, however, suggests that the physiological “inferiority” of women is not immutable. Males and females do not differ in the quality of their muscle fiber (Bell et al., 1980) nor in their physiological responses to exercise (Wells, 1980; Massicotte, Avon, and Corriveau, 1979). These similarities have led investigators to consider other factors in explaining performance differentials between men and women, e.g., the fact that women have a higher proportion of body fat and, hence, relatively more unproductive body weight to move in physical activity (Cureton, Hensley, and Tiburzi, 1979).

The major biological difference between men and women is in the development of muscle mass in response to exercise (muscular hypertrophy). In males, muscular hypertrophy is associated with strength, whereas in females, muscle girth can decrease as strength develops (Clarke, 1973; Oyster, 1979). The major factors affecting muscle hypertrophy and strength are genetic and method of training (O’Shea, 1976). Testosterone is not related to strength levels in women (Krahenbuhl, Archer, and Pettit, 1978). However, a recent study of male and female cadets indicates that, following training, sex-related strength differences vary by muscle group. The greatest disparity occurs in upper body strength, but little or none occurs in the trunk and leg muscles (Hoffman and Stauffer, 1979). These findings suggest that as women become more physically active and at younger ages, strength differences may diminish further.

Conclusions

We have argued for a serious consideration of biological factors in the explanation of sex differences in political behavior—differences we have conceptualized as gender. Following Glendon Schubert, we proposed that this consideration embody an interactive life science model of political behavior that takes account of human needs and includes both cognitive and physiological processes, and we outlined four areas of substantive inquiry to serve as examples. However, we have saved perhaps the most cogent reason for last. It is simply this. Human sex differences remain an area of continuing public concern, particularly now when sex role norms are in flux. Extrapolations from biological evidence will be advanced in any case. So long as social scientists, and particularly political scientists and feminists refuse to consider biological factors in their research, they abdicate their social responsibility and allow extrapolations to be made that reflect the inadequacies of a primarily biological model.
Two examples will illustrate our point. First, an editorial in the New England Journal of Medicine (DeCherney and Berkowitz, 1982) argues that women should delay their careers until their thirties and raise their families in their twenties because of a study (published in the same issue) indicating that fertility declines after thirty. Second, an editorial in the American Journal of Medicine (Rebar and Cummings, 1981), extrapolating from a study in the same issue, expressed concern that physical activity and athletics may interfere with the reproductive function in girls and women. Both of the referenced studies were based on a single line of research and could be criticized on methodological grounds or countered with the findings of other biological research. In both examples, what primarily concerns us is that the social policy advocated was proposed in total ignorance of other social costs—those, for example, attached to avoidance of physical activity for women and to the timing of early motherhood with resultant inequities in social power between men and women. It is in these areas that biobehaviorally literate political scientists and feminists can contribute to a more humanistic understanding of human sex differences—an essential context for a humanistic social policy.

COMMENTARIES

THE ROLE OF BIOPSYCHOSOCIAL DEVELOPMENT IN THE PROCESS OF GENDER-RELATED POLITICAL SOCIALIZATION

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Baer and Bositis argue that "biological factors as well as environmental or learning-based factors should be included in scientific explanations of the political socialization of gender." The authors present a strong case for the inclusion of biology in the political socialization equation. Their examples of the applications of the biological perspective are particularly effective in illustrating the importance of biology in gender-related socialization.

We commend Baer and Bositis for attempting to integrate issues of gender into the emerging field of biopolitics. This is clearly an area containing a great deal of myth and belief and relatively little data. In particular, assertions have been made in the political arena about the unfitness of women due to their biology. "While I have no desire to embarrass women, there are, in fact, certain times of the month when a woman is emotionally incapable of doing her work." (A Philadelphia police commissioner in refusing a promotion to a woman officer, as quoted in Dan, 1976.) These beliefs should surely be put to empirical test. The articulation of ideas in this article is one step in the right direction.

However, the question posed in the title—"what contribution biology?"—remains, for the most part, unanswered. We agree that adopting an interactionist view of the development of gender-related phenomena is more useful than employing a perspective that emphasizes either learning-environment or biology as the main force behind sex-differences in political behavior. However, the authors have not yet articulated a comprehensive view of the problem. This is not a result of some oversight on their part. It is probably due to the complexity of the issues, the inherent difficulties in any attempt to integrate disciplines, and the recent emergence of the field of biopolitics.

Baer and Bositis are political scientists, and as such, they "are concerned not with the adjustment of individuals to an interdependent set of social roles, nor with the development of healthy personalities [but with] whether the political behavior and participation of adults, the expression of political interests, and the exercise of political power vary in meaningful ways for men and women." Understanding the effects of sex-related differences in political power among adults is a political science issue. Unravelling the process by which these gender differences emerge requires some attention to developmental process. The interactionist approach reflected in the biopolitical perspective that Baer and Bositis outline is a step in the right direction, but their formulation would be strengthened if they integrated research on human development.

Development is best considered as a transaction between the individual and the environment (Sameroff, 1977). Interactive models such as that put forth by Baer and Bositis are static in time, but development, by its nature, is a process in which neither an individual nor the environment remains constant (Petersen, 1980). Since the variables in the developmental equation change, the way in which they interact over time also changes. The individual and the environment exert reciprocal influences on one another, and it is the changing interaction over time between these two changing entities that we refer to with the term "transaction.

The transactional process of development begins at conception. All of us are born with a certain set of genetic potentials that are manifested in our biological and psychological characteristics. These genetic influences on behavior, however, are always indirect; they are ex-
pressed through biochemical and physiological mechanisms and generally are highly influenced by environmental factors. This transaction between genotype and environment continues throughout development, and therefore, genetic influences are not fixed once and for all at conception (Petersen, 1980).

In addition to the influence of genetic factors operating before birth, there is also evidence that prenatal endocrine levels influence later behavior (Money and Ehhardt, 1972; Reinisch, 1976). For example, a critical period for sexually differentiated behavior was first noted in studies of primates and other animals (Harris, 1964; Young, Goy, and Phoenix, 1964). More recent studies have found sex-related differences in testosterone levels in human fetuses, with prenatal levels in males approaching that of adults. Similarly, studies of individuals with anomalous or "accidental" endocrine status and those with drug-induced endocrine status suggest that prenatal hormones do influence some aspects of psychosexual development (Petersen, 1980).

Social and psychological factors also begin to operate at conception. For example, adequate nutrition, which is highly related to socioeconomic status, is essential for optimal prenatal development. Also, research with animals and humans indicates that stress on the mother during pregnancy may affect the psychosocial growth of the child (Petersen, 1980).

After birth, the developing individual is confronted with a broader range of environmental influences. Factors such as socioeconomic status, psychological characteristics, and attitudes become increasingly important. As the child becomes older the array of environmental influences expands to include attitudes and characteristics of friends, neighbors, relatives, teachers, and classmates. At the same time the overriding sociocultural values of society become increasingly relevant. This is especially true for the process of political socialization.

At the onset of puberty, biological and sociocultural factors converge, particularly in sex-role development. The biological aspects of adolescence certainly play an important role in the socialization process; however, little evidence supports any direct, internal influence of pubertal hormones on psychological process, although direct effects are certainly possible. The timing of maturation and the effect of experiencing such a dramatic biological change with a cohort of various stages of puberty are "nonphysical" aspects of puberty that indirectly affect social development. Current evidence, however, does not permit us to separate biological from sociocultural influences at adolescence. In fact, they may be totally confounded and unseparable.

Biological factors are important at other points in the life cycle. Processes such as menstruation, pregnancy, menopause, and the general physical decline associated with aging all have an effect on psychosocial development. However, these biological phenomena have social significance as well, and it may well be that social beliefs regarding biological events have a stronger influence on development than the biological events themselves. For example, some evidence suggests that strong social beliefs regarding mood swings and stress over the menstrual cycle may override any actual hormonal effects (Petersen, 1980).

Just as there are critical biological events in the lives of males and females, there are critical social events of significance in human development, many operating differentially for each sex. The obvious factors among these are marriage (and divorce), births of children, deaths of parents and other loved ones, and children leaving home. These critical events are all embedded within an array of other significant influences on socialization and development, and their effects on adult development are just beginning to be investigated.

Development, then, is influenced by an increasing number of factors from conception to adulthood. Before birth, genetic and hormonal influences predominate, with a relatively small sociocultural contribution. From early childhood through maturity, development is influenced by a complex, open system of factors with a number of transactions occurring among all levels.

In attempting to "define a role" for biology in developmental processes such as gender-related political socialization, we need to keep in mind that biological change is given meaning by the context in which it occurs. A direct effect or simple interaction model cannot deal adequately with this reality. In developmental processes, social factors are certainly as important as biological effects. In fact, no trait is influenced solely by biology or environment. This false dichotomy is perpetuated by the nature-nurture controversy. In the same vein, social scientists need to keep in mind that biological variables are no more real or objective than psychosocial ones, and generally, they are not more easily measured. Both types have reliability and validity problems; we must infer the status of some underlying characteristic from a measurement that contains a degree of error.

Furthermore, environmental influences not only affect an individual's psychological make-up, they affect her physically as well. For example, stress affects hormone levels (Kreuz, Rose, and Jennings, 1972); the social situation causes changes in testosterone level and manifestations such as aggression; fear of (or belief in) pregnancy can bring on its hormonal concomitants (Yen, Resar, and Quesenberry, 1976); and environment can influence the phenotypic expression of some genotype (Scarr-Salapatek, 1975). Thus, the total system is open to influence, not just its psychological aspects.

Also, interactions occur not just between person and external environment, but between the psychological and physiological within the individual. In addition, it is likely that the relative influence of biological and sociocultural factors will vary for individuals and groups. It is known, for example, that when environmental conditions are optimal their variability is restricted, and genetic factors contribute relatively more to the variance in the trait under investigation. Similarly, if a trait is uniform for all members of a sex, sex itself and its contribution cannot be independently assessed. In fact, sex is clearly more than a simple genetic, or even biological variable.

The call for a perspective in political science research that includes both learning and biology is certainly a step forward. However, we feel that the biopolitical viewpoint needs to be expanded to take into account the myriad of influences that affect development. Political behavior is the result of the transaction over time among biological, psychological, and social factors. The only "constant" in
the equation is change, and that fact calls for an expanded view of the role of both biology and environment in the political socialization of gender. By using a model that includes the spectrum of influences from biological to sociocultural, we increase the likelihood that our research will include or at least consider the variety of influences on development. After all, the question is not whether sex-related differences are biologically based or socioculturally determined. Both are likely to be involved, at least to some extent. The key question is that of their relative influences.

It would be unwise, however, to assume that all of the data relevant to the question of gender differences in power relationships are already extant. Far more rhetoric than data exists on this matter, probably due to the strong beliefs involved. There are clear gaps in knowledge, in part due to the biases of some of the questions posed. A scholar or researcher in this area must be alert to biases in questions or methods that constrain the results obtained. The recent research of Gilligan (1982) on moral development, like that of feminist scholars in other areas, reveals the distortions in theory that result from biases in question formulation and sampling.

A final question. Why has the issue of the biological aspects of gender-related political socialization emerged now? These are historical times in which some politicians are seeking biological explanations for human behavior—because they erroneously view human behavior as immutable. This current tendency, together with the more traditional suspicion of women in politics, make the topic before us extremely controversial. Discussion, therefore, must proceed with great care. Because the topic is so important, it must be approached with the most objective and critical reviews of extant work as well as clear conceptualization and theorizing. We praise Baer and Bositis and Man, we need this notion of total human indeterminancy like a hole in the head. If we are literally nothingness at birth then there is nothing to prevent our being shaped and molded to whatever form a given society desires. This is not a very secure foundation from which to make a case for human freedom or the innate integrity of the human subject.

If we accept the reality of our embodiment, if we acknowledge the fact that we—we human beings—are a species, are animals of a certain kind, that is just the beginning. Then we face a number of daunting questions concerning just how determined we are by our biology, our genes, our being the kinds of creatures we are and none other. So I accept the importance of the authors’ project. But I think, finally, that their attempt to correct a social science that is agnostic or even arrogant with reference to biology falters because their method shares too many of the presumptions of those they critique.

The presumptions to which I refer may broadly be called positivist. That is, the authors are still enamored of the idea of deriving some scientific explanation of human behavior. (The alternative I shall suggest below is an in-

for their beginning steps but urge them and others involved in this emerging area to take a more comprehensive perspective.

RESPONSE TO BAER AND BOSITIS

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Baer and Bositis’s openness in taking biology seriously is welcome and refreshing. For good reasons and bad, social scientists, including those who see themselves as feminist, have suppressed consideration of biological realities. They have tended, implicitly, to accept the notion of tabula rasa—that “blank” creature waiting to be written on by the organized forces of society—that underlies so much political theorizing in the West. As the philosopher Mary Midgley points out in her fascinating book, Beast interpretive account of human thought and action that aims for understanding and probes meaning.) The method understood as scientific, and the authors implicitly endorse it, is one that may broadly be called methodological individualism. One builds from certain rock-bottom particulars—adding them up, or relating them to one another—and the sum of these particulars is one’s explanation of a given phenomenon. “Science,” says Hobbes, “is the knowledge of consequences and the dependence of one fact upon another.” Baer and Bositis implicitly endorse this model, despite the fact that natural scientists long ago gave up on this understanding of causality. So—although they are absolutely correct to argue that “psychological” explanation alone will not serve, nor will social learning arguments—the particular way in which they draw biology into the picture simply affixes it to the multi-causal scene as another variable or set of variables to be concatenated with many others.

There is more to be said on this important epistemological and methodological dispute, but for now these brief comments must suffice. The authors betray their indebtedness to a set of presumptions that finally details...
their larger project, as I understand it, when they speak of political "behavior" being "caused," with biology added in as one of a number of causal factors. Yet behaviorists have never yet been able to "predict" behavior in the way they should if their presumptions are valid and their method holds. (Baer and Bositis sometimes deploy the hard language of "cause." At other times they say "effect on" and "influence"—terms which have a very different epistemological status and would carry different weight in a logic of explanation. To add biology in as just another of a number of multi-causal agents fails to rescue a bipolitical perspective from the general charge of reduc tionism that can, and has, been leveled against positivist approaches to the Geisteswissenschaften. What would a nonreductionist treatment of biology look like in some alternative notion of the human sciences? Baer and Bositis incorporate within their essay the germ of a very different understanding of biology than the one they finally accept. They quote Dennis wrong: "in the beginning is the body" (my emphasis). The key here is the body. Wrong is not referring to disparate biological variables — hormones, brain lateralizations, etc.—but to a body, a whole. Our embodiment, he suggests, is the ground of our being. We are born into the world as a body, not a collection of biological variables that somehow add up to a whole entity. We are essentially embodied selves. If one begins from this recognition of essential embodiment—and there are philosophers who have developed the implications of this recognition including Richard Wollheim, Peter Winch, Maurice Merleau-Ponty—a number of vital and interesting questions move into the forefront of social inquiry that are washed out of the Baer-Bositis approach to biopolitics. I refer to such questions as the inner relationship between our sexual identities, our essential embodiment as male or female beings, and our social identity. The important distinction between the sort of inquiry this would require and the one the authors propose lies in the fact that beginning with essential embodiment grounds inquiry in a notion of a human subject. To approach the body as an aggregate of disparate biological variables that "cause," "influence," or have an "effect on" politics in certain ways is to perpetuate a social science that constructs human beings as objects of a (putatively) scientific inquiry.

The very different epistemological base I am here suggesting sees us as whole beings, importantly self-defining subjects, who, out of our bodies, and within the concrete social locations in which we find ourselves, attempt to create and to sustain self-understanding and meaning—to make something coherent out of experiences. The strongest challenge to disembodied social science, then, will come from making an argument for essential embodiment that rejects the presumptions of positivist inquiry. This seems particularly important if one has feminist sensibilities, for what women have been denied—at least in much political theory—is the status of the subject. A subject is one who understands the world and grasps meaning through her embodied mode. We do not take a stand towards our hormones or our chromosomes; no, it is as embodied beings of particular kinds that we live in and through a complex social world.

The authors’ finally reductive treatment is in evidence as well in their implied notion of politics. Politics gets lost in a kind of blur of styles or types. Proposing gender as an over-all world view loses politics in some possible isomorphism between psychological and political types, and this is quite typical of American social science. To write of "psychological modes of participation" is to lose the citizen. It seems to me that what is required right now is the recovery of a substantive ideal of politics—a res publica—in which human beings, constituted as citizens, engage one another in certain ways not reducible to bipolitical or psychological types or styles or modes. To reduce politics to behavior or correlates from biopsychology (as in "the male-lateralized, confrontationist political style") washes out the purpose and possible dignity of politics. One would never know from this discussion that politics has to do with the way a people structures its collective identity—its hopes, fears, aspirations.

Studies of brain lateralization, perhaps interesting in themselves, cannot serve as the basis of a political theory or vision. Think of Martin Luther King’s "I have a dream," or Gandhi’s "an eye for an eye leaves the whole world blind," or the brave mothers of the Love Canal. What possible explanatory or political purpose would be served by labeling these, or other efforts, as instances of the dominance of male or female lateralization. All the biological variables and psychological variables and socio-economic factors combined do not add up to powerful instances of political vision, action, and courage. To sublimate human beings in some aggregate of processes and interactions and variables is to lose the sense of a moral agent and a public citizen.

A few additional points and questions:

1. The authors never explain what all they intend to pack into "the political socialization of gender." How much causal weight in their overall frame of social-political explanation would this occupy? After all, most men do not hold political power either, and they never have. The one clear-cut male political prerogative, if it can be called such, has been "access" to the dubious privilege of killing or dying for the state.

2. To endorse the claim that sexist research is "simply poor scholarship" is troubling. This could readily become a license to evade unwelcome challenges to one’s feminist dogma. Surely a researcher whose canon of scholarly work was impeccable could come up with an argument or conclusion that might (conceivably) have (possible) sexist implications—depending upon one’s vision of feminism, politics, etc. As the authors themselves make clear, many feminists see any attempt to take biology or the body seriously as a threat—as having sexist implications by definition. Indeed, I daresay certain feminists would regard the Baer-Bositis article as suspect. Does this make it "poor scholarship" because certain individuals, for narrowly ideological reasons, see fit to label it sexist in its implications? Surely not. We can do without such a tidy rule of thumb. It is better to think out and think through these questions rather than to evade them.

3. To remain agnostic on the question of a "special biological attachment" between mother and infant glosses over the important, and growing, literature on mother/infant bonding.

4. What does it mean, finally, to proclaim gender a political "world view"? What view? What world? What politics? What forms of political action and belief follow? We must be wary, I believe, of any attempt to create a Weltanschauung, a mode of explanation that would ex-
plain all the riddles of the universe, as Freud pointed out, on the basis of one overriding hypothesis.

5. The authors refer, at one point, to "real human needs." This opens a can of worms—or should—but the authors seem to take the notion as having pregiven, prima facie, force. But one must tax oneself with some tough questions if one brings up the idea of a real or a true need. This presumes false or artificial or bogus needs as some contrast model. How do we distinguish? How do we make the case for those that are real and those that are, somehow, not so basic, primary, authentic? To use the language of real need requires of the thinker that he or she give some explicit attention to the questions I here raise.

**BIOLOGY AND POLITICAL SOCIALIZATION: THE COGNITIVE CONNECTION**

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Political socialization is that subset of learning that is derived from and/or applied to political objects, actors, or situations. As a process, it is dependent upon such factors as the motives and abilities of the participants and the variety of authority or other social relationships among them. It is, or rather, has been, by definition a social, and generally a sociological, process whose structure has given rise to at least five sets of substantive concerns: who socializes, who is socialized, what is communicated, how is the communication accomplished, and what effect does the communication have on the attitudes or behaviors of the recipient. These themes have provided the focus for most research on political socialization (Dennis, 1973:6; Jaros, 1973:23). In their present work, Baer and Bositis begin to raise still more fundamental questions about the nature of learning itself in a political context.

The authors' conceptualization of gender as a world view encompassing involvement in politics, tolerance of conflict, and orientation to politics, which provides the core of the present argument, is directly rooted in cognitive psychology. Essentially, they suggest (and develop a convincing case for) a cognitive-physiological model of gender in political behavior in which various biological factors are linked with cognitive processes to comprise the socialization experience or its product. Although recognition of this potential linkage is an important step forward, the authors do not really demonstrate it. Instead, they offer four marginally interconnected examples of biological variables, each of whose relationship with the gender world view seems straightforward enough, but whose commonalities and explanatory powers the reader is expected to grasp by osmosis. Yet it is the establishing, or at least the identifying, of those very properties that is the most critical and potentially the most valuable exercise that might answer the question, "what contribution biology?"

The problem here arises in large measure because the authors are unduly constrained by the manner in which they have posed their central research question. By using the notion of gender to lead in to a consideration of biopolitical aspects of socialization, rather than deriving applications to gender of whole classes of biological factors that operate on political learning generally, Baer and Bositis overlook a broader line of inquiry that not only promises to be more fruitful overall, but promises as well to make easier and more convincing the specific argument at hand. That line of inquiry would examine more directly the most fundamental links between physiological and cognitive structures and processes.

In particular, such inquiry might focus on identifying the essential points at which physiological and cognitive activity must come together if political socialization, political behavior, or for that matter learning and behavior of any sort, are to be accomplished. Schubert (1976) has referred to these points of interface as "intervening processes" in his conceptualization of a life science paradigm of political behavior. Once identified, each of these linkages could be employed as either a dependent or an independent variable in a variety of hypotheses related to sex, age, maturation, rates of growth, physical traits, or the like. Several potential points of connection come to mind, all of them related to more general questions of how humans think, how they learn, and how they learn to think. Perhaps these are best summarized as a series of cognitive processes which have been identified (or assumed) in various lines of psychological or behavioral theory and research, and for which, if they do operate, there must exist some sort of physiological counterparts. A list of such processes might include the following:

1. **Differentiation.** This refers to the ability to identify, characterize, internalize, and respond to specific bits of information (as distinct from incoming information generally). The social judgment-involved and various balance theory approaches to attitude change (and by extension, learning), for example, are based in part on an individual's ability not only to isolate units of information, but to recognize the degree of discrepancy between new and existing cognitions or in some way to compare the two (Keisler, Collins, and Miller, 1969:155-237; Festinger, 1957; Sherif and Sherif, 1967). This ability to differentiate must be basic to the operation of sensory receptors and neural structures if present notions of cognitive activity have any validity.

2. **Selectivity.** Beyond differentiation, this refers to the ability to discriminate among specific bits of information and to receive and/or respond to them differently. Ex-
amples include selective attention (perhaps akin to the physiological notions of alertness or the presensitizing of certain neural pathways, Posner, 1975) and conation, the selection of appropriate behavioral responses to particular outside stimuli. Again, processes of this order assume the occurrence of certain relatively sophisticated exchanges between incoming and outgoing information, and the existence of a marketplace structure that facilitates them.

3. **Integration.** This process includes the forming of cognitive links between incoming information and existing cognitions, and the assigning of affective evaluations to new information. Social psychologists, for example, speak of selective perception as an integrative process (Krech and Crutchfield, 1971), and neurophysiologists focus on the related issues of molecular or anatomical theories of memory, the operations of synaptic junctions, or the relative balance of dopamine and endorphin associated with a particular cognitive experience (Ettingh et al., 1975; Roberts, 1966; Milner, 1957; Landauer, 1964; L. Stein and J. D. Belluzzi, quoted in Greenberg, 1978).

4. **Access.** This refers to the development, maintenance, and operation of a network of stored bits of information in which specific cognitive addresses can be created and revisited. At the cognitive level, this would include such concepts as selective retention, the directing of new information to appropriate locations in a cognitive "file," and selective recall, the calling up of stored information using the cognitive equivalent of a keyword index to identify relevant items (Tannenbaum, 1955). At the physiological level, various theories of memory again suggest directions of inquiry, though none yet describes how these "keywords" are channeled to particular neuron chains.

5. **Organization.** Here I refer to the higher order structure and operations of relationships among cognitions and affects generally, to the ability not only to connect and address individual cognitions, but to generate from them coherent and systematic attitudes and behaviors defined vis-a-vis some particular object, i.e., to convert internal phenomena into social ones. A central question here is whether we are in fact as organized as we seem, or whether our apparent degree of cognitive organization is a methodological artifact of our search for organization (Hanson, 1980). Livingston (1982) has suggested, for example, that memory works rather like a computer program that advises the brain, "Something biologically significant is happening now. Print (store) everything that is now happening (i.e., save the brain pattern in its entirety)." If applied to activities of a higher order than mere retention, this suggests what is in a sense a less self-flattering, more inductive (or even random) process than the one we normally assume.

6. **Aggregation.** This is the notion that not only do perceptions, cognitions, affects, attitudes, behaviors, and the like exist as individual phenomena, but that in a sense they find one another, that an individual's internal filing system is not only keyword-indexed, but cross-indexed as well. The notions of attitude clusters, belief systems, ideologies, and patterns and classes of behavior, all central to any theory of political behavior or socialization, depend entirely on the existence of some such physiological capability.

7. **Generalization/Specification.** From specific experiences we derive general lessons; from general learning we derive specific applications. Political participation and its relationship with one's sense of political efficacy provide an example. By engaging in one or more specific actions we obtain a general feeling of influence which we then manifest in a number of alternative specific actions (Milbrath and Goel, 1977:57-61). What are the underlying mechanisms that provide for these classes of political learning and subsequently broaden their utility? For that matter, what is the nature of the classes themselves?

8. **Persistence.** The phenomena I have listed above and others as well (motivation, habituation) seem to be long-term in nature, and longevity is a general concern of any inquiry into the nature of political learning (Dennis, 1973:5-6). Yet many of these same phenomena tend to have physiological explanations that are short-term in nature. A case in point is habituation, which at the cognitive level refers to the ability to shut out such "noise" as the saturation advertising of political campaigns, but at the physiological level is explained as the over-excitation of a receptor or a neuron chain (as by expending the available supply of a neurotransmitter such as gaba) which results in a breakdown in the in-transmission of information about contact with a given stimulus. The compatibility of the two notions is not immediately evident.

I have listed these eight processes to suggest that it would be wholly compatible with the conceptual framework from which Baer and Bositis operate to emphasize more general phenomena—(1) that are central to our present cognitive-behavioral notions of political socialization, (2) that require, if they are to be fully understood, the more complete development of a cognitive-physiological notion of political socialization and of learning generally, and (3) that point toward variables that may in fact be found to covary, or equally importantly, found not to covary, with more socially apparent biological factors (of which sex is but one). Such an emphasis might provide both a more productive and a less inherently politicized line of inquiry, and it would also give rise to more fundamental questions about the nature of the socialization process. How and when do these processes develop (a question that Piaget and others have begun to answer, see Peterson, 1981)? Are they applied differently in politics than elsewhere or, as seems more likely, differentially across a variety of political learning experiences? And finally, are these phenomena the mechanisms of learning, or are they themselves learned? The answers to these questions will tell us much about biopolitical socialization.

As Baer and Bositis correctly note, the enduring weakness of much of the extant theory and research on political socialization is that it stops short of examining the physiological substrate that must support cognitive activity. As a result, explanations derived from this work remain incomplete. For their part, these authors have begun to expand the ways in which we conceptualize the learning of politics; they have begun to ask what Holden (1973), writing in a different context, has termed "the right questions." In so doing, they help us to take the essential first step toward more fully understanding one of the most important aspects of political behavior.
AUTHORS’ RESPONSE

The three commentaries provide an interesting array of responses to the position we outlined in our paper. Our basic purpose was to argue for the inclusion of biological factors in the study of the political socialization of gender. Interestingly, all commentators agree that our basic enterprise is a laudable one. However, Asp and Petersen and Manheim propose that our goals would be better served by a more thorough grounding in a general theory or paradigm (whether that of developmental or cognitive psychology), and Elstain views our general approach as perhaps too abstract—one that takes insufficient account of the ‘subject.’ Asp and Petersen emphasize the role of development and make the important point that the interaction between biological factors and the environment will differ over the life span. Manheim correctly notes that the ideas of cognitive psychology predominate in our conceptualization of gender, and he suggests that our goals would be better served by a more complete integration of Piaget’s work, which incorporates a more general set of relationships between biology and cognitive learning.

Each commentator also raises at least implicitly, or explicitly in the case of Elstain, a related set of issues—his or her conception of science and how that conception bears upon the study of gender and politics. Manheim and Elstain present perhaps the more extreme or polar positions on a continuum of viewpoints on the objective nature of scientific inquiry. Manheim’s position is that the primary issue we are concerned with—the impact of sex and gender on political behavior—is best approached from a general scientific theory, with appropriate deductions made in the substantive area of sex differences (as in any other substantive area). With this approach, much of the politicized nature of the inquiry is washed away.

Elstain, on the other hand, rejects the scientific enterprise as a valid method of inquiry. For Elstain, interpretive philosophy provides the appropriate model. He argues this in part because inquiry in general must take account of the subject, but also because for women, the denial of the status of the subject has been the basis of much sexist discourse. In this way, Elstain endorses the importance of feminist criticism and women’s own self-conceptions of their life situations and experiences (the interpretation of the subject) as an integral part of interpretive inquiry.

Asp and Petersen endorse a more middle ground along the lines of the position we attempted to stake out in our paper. In their view, science represents an important effort to identify and define causal antecedents of, in our case, human behavior. Yet, their position is not the ‘positivism’ that Elstain criticizes because contemporary social science methodology in no way asserts the goal of a general overarching grand theory valid across both history and cultures. Furthermore, it accepts the critical role of the human subject and human consciousness in scientific research itself, and hence, the value-embedded context of the research process. This middle position provides a role for feminist criticism to refine scientific research, but on the other hand, the subjective and conscious experiences of women do not constitute by themselves a complete picture of gender and its effect on behavior.

Our reading of Manheim’s commentary suggests to us that he has in part misunderstood the primary goal of our paper. Our purpose was not to propose a grand theory of a cognitive-physiological model of gender in political behavior, but rather the more modest goal of trying to convince a wide range of political scientists and feminists to consider biological factors seriously in their research on the political socialization of gender. Our reading of the biopolitical literature suggests that no such theory now developed can comprehensively integrate both the biological factors and the environmental factors. Our purpose was to clear away, if possible, some of the conceptual underbrush which we believe has directed research attention away from a serious consideration of biological research, and then to suggest in an heuristic fashion some potential areas for further research. The difficulty with the specific model that Manheim proposes is that it will only illuminate a portion of the possible areas of politically significant sex differences. For example, the relationship between the cognitive processes that Professor Manheim outlines and political power differences between men and women arising from sex-related differences in physical size and strength remains rather opaque to us. Yet, this is an area of power that political science has been rather blind to. It is feminist thought and criticism that highlights the importance of power relations in the family and among informal relationships, and power relations in these spheres may in turn affect power relations in more formal spheres such as politics.

Manheim also seems to have missed the point of our attempt to depoliticize the study of biological factors in the context of gender research. Our defense of an area of study, however, is a separate issue from our construction of biopolitics as a more humane approach to human behavior and from our argument that social scientists have a social responsibility both in the conduct of their research and in the communication of their findings to policymakers. Two recent policy issues have arisen since we first wrote our paper which illustrate the critical political implications of sex differences. First, the Army has announced that it is restricting occupations open to females based on the determination of a sex-related strength difference (Washington Post, August 31, 1982). Second, publicity is increasing about what one writer has described in Discover as the monthly “Dr. Jekyll/Ms. Hyde” personality alterations due to the prevalence of “premenstrual syndrome” (Angier, 1982). In neither case does current research support these conclusions. Yet, it is interesting that consonant with the more conservative times, these purported sex differences are now highlighted—even contrary to current scientific research. As responsible political scientists, we cannot ignore the value-embedded context of our research, and we cannot ignore the political context, either.

We find much to agree with in the commentary of Asp and Petersen. They emphasize change and development over the life span as a model for analyzing the interaction between biological factors and the environment during the course of individual development. These two aspects of the socialization process are clearly lacking in much political socialization research. Too often adults are viewed as fully formed at voting age and unchanged during the
adult years. Ironically, this is despite the considerable research on voting behavior and elite behavior that suggests just the opposite.

Our analysis of the socialization process did emphasize development, but we did not incorporate development in an active way as Asp and Petersen do. They propose that biological influences tend to be greater prenatally and during the early years and that environmental influences tend to become stronger and more diverse over the life span. We agree with this general outline, however, we take issue with some of their more specific formulations or examples of this general process.

Asp and Petersen suggest that “prenatal endocrine levels influence later behavior,” and then they note that there is little evidence for “direct, internal influence of pubertal hormones on psychological process” during adolescence. Asp and Petersen are evidently following the distinction often made in biological literature between the “activating” effects of circulating levels of testosterone among adults and the “organizing” effects of testosterone on the brain and the genitals during prenatal and neonatal life. We find the import of this distinction somewhat nebulous because normal male development includes both effects. Moreover, although some research among animals has stressed that the organizational effect is an important determinant of behavior in its own right, other research indicates that this effect varies among species, and primate research indicates that it may not be permanent (Rogers, 1976:175).

In particular, we take issue with the general interpretation (e.g., see Baer, 1980, Goy and McEwen, 1980) of the research on human sex-hormone abnormalities, which asserts that, even though prenatal hormones do not affect sexual preference or gender identity, they somehow affect gender role behavior. As we suggest in our paper, the problem with this interpretation is that the descriptions of behaviors are not free of sex-role stereotypes, i.e., stereotypical behaviors that vary among men and women both culturally and historically (e.g., “tomboy” behavior and “playing with dolls”).

It is indeed possible that prenatal hormones influence behavior, but current research does not support this inference (Baer and Bostis, forthcoming). Yet, if hormonal differences between males and females have an effect, it should certainly be most evident during adolescence, and environmental or learning factors should be less influential during that part of the life cycle, contrary to the Asp and Petersen formulation.

Asp and Petersen stress that in the current political context, it is especially important that, as consumers of research ourselves, we be alert to research methods and interpretations in extant research that might constrain possible results. Although our purpose in this paper was to present an heuristic treatment of possible biological influences on development and political socialization, and not a critique of current research, we strongly agree with this important caveat.

We feel constrained to address Elshtain’s commentary in some detail because her criticisms reflect a point of view that is rare in feminist thought and an antiempirical stance which essentially rejects extant empirical and quantitative research as “positivist” and inappropriate to the task at hand. Elshtain opposes positivist and interpretive paradigms as if they were the only two possible types of inquiry. She then endorses the philosophical interpretation, which “aims for understanding and probes meaning,” at the same time characterizing our paper as a positivistic analysis because we employ the language of causality. We fundamentally disagree with her dichotomy, and most adamantly do not consider ourselves positivists.

We would remind Elshtain of the important distinction between language and a philosophical commitment to positivism. The language of causality is a rich one, and one that is meaningful to the audience of social scientists we hope to reach, as indeed it is in ordinary discourse. It would be difficult indeed to “probe for meaning” without some notion of “cause”—Elshtain herself uses the term “determined,” actually a much stronger term than cause. The meaning of terms such as cause is not universal; they derive their sense from the context in which they are used. The context in which we use the term cause is quite different from that of the early philosophers of science who advanced a positivistic position.

Our view of science in no way presumes a positivistic conception of cause and explanation. Elshtain is out of touch with contemporary social science methodology if she really believes that current research ignores human consciousness and interpretation. Of course, the conduct of research and the process of theory development both entail interpretation on the part of the researcher. For Professor Elshtain, however, the more cogent criticism of empirical research is not whether researchers themselves accept their own subjectivity, but that they deny subjectivity to those they study. It is admittedly the case that science seeks to identify objective or causal relationships, not subjective ones. However, this does not mean that science therefore ignores the fact that those individuals it studies are also active, thinking human beings. As one methodologist emphasizes, “the subject is not a passive responder to stimuli and experimental conditions. Instead, he is an active participant in a special form of socially defined interaction which we call ‘taking part in an experiment.’” (Orne, 1969:144). The dignity of human subjects in social research is further highlighted by the emphasis on the intrusiveness of social science research and the importance of ethical and political considerations in the conduct of research even at the lowest level—that of the introductory undergraduate text (e.g., Babbie, 1979: 57-78).

The alternative mode of inquiry that Elshtain endorses is grounded in the notion of the subject, whom Elshtain defines as “one who understands the world and grasps meaning through her embodied mode.” Here Elshtain clearly reveals her bias—what she views as the problem to be explained is the meaning of our sexual and social existence for the individual. Clearly, this meaning will vary among individuals, and studying it is not the same goal as that of science—which is interested in classes of behaviors of groups of individuals. It is important to note, however, that the scientific approach to social and political life does not preclude an examination of the meaning of social and political life to the individual. The scientific mode of inquiry and the interpretive mode of inquiry are simply two different ways (among others) of looking at the same phenomena. In our view, the scientific mode is complementary to the interpretive mode because it informs us of generalities not available to us simply through introspec-
tion. Yet Elshtain rejects the scientific mode as invalid or inappropriate.

We feel that Elshtain has taken this position because she believes that interpretive philosophy and social science research seek to explain the same thing. This fundamental misunderstanding of science is evident when she cites the case study examples of Martin Luther King, Ghandi, and the mothers of Love Canal. Clearly, the explanation of a single individual’s actions via a model is reductionist—but that is not what science seeks to accomplish. Science seeks not to serve as a political theory or vision, but simply to “explain” relationships between classes of variables for groups of individuals according to well-understood criteria of logical inferences, based upon statistical generalities.

A couple of other minor points. Elshtain misses the point of our discussion of sexist scholarship. We are not saying that if someone finds sexist implications, then a piece of scholarship is sexist. A scientific finding may be considered to be sexist, e.g., “passive” females and “aggressive” males, yet the interpretation is problematic because the meaning comes from society, not from the research itself. Like Sapiro, our point in the original discussion was that if the method of research (in this case, behavioralism) makes certain classes of variables important (in this case, sex or gender), and these are then ignored in the research by a specific researcher, then what we have is sexist scholarship and poor scholarship as well.

Elshtain raises the cogent criticism that we did not specifically discuss the research literature on mother-infant bonding. We assumed perhaps mistakenly that this literature would be quite familiar to our audience, and we wished to discuss the biocultural role of mother as an area for biopolitical research apart from the specific controversies associated with the bonding literature. The concept of “bonding” refers to a reciprocal bond between mother and infant, yet the classic research, e.g., by Harlow, Spitz, Bowlby and others on maternal deprivation (see discussion in Davies, 1977:154-159) and by Klaus and Kennel (1976) on the effects of the amount of contact between mother and newborn on maternal behavior examines the relationship as unidirectional, not reciprocal. Further, since hospitals tend to isolate infants from everyone but their mothers and the hospital staff, we lack an important baseline of comparison for father-infant bonding, and even sibling-infant bonding that might occur in a home birth. Given the gaps in the current research, it is important not to ignore learning-based explanations for maternal-infant attachment (Rothman, 1992). It was to address the biocultural role of mother beyond the limitations of the current research that we emphasized other ways of analyzing the bonding phenomena, not in order to remain “agnostic” in the face of important research.

—Denise L. Baer

—David A. Bositis

References


