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Commentary

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Biology on Probation: Response to Fine on Evolutionary Psychology and Feminist Studies

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Abstract: I argue that the central issue in Fine's criticism of my paper is not whether evolutionary psychology has critics or whether feminist scholars have "engaged" biological claims, but whether biological explanations are treated as admissible competitors under symmetric evidentiary standards. I contend that her critique repeatedly narrows biological evidence, emphasizes contestation, and leaves acceptance criteria unspecified, rendering evolutionary explanations perpetually provisional. Meanwhile, social constructionist accounts retain default status without comparable falsification standards. Using examples from biosocial theory debates, developmental endocrinology (prenatal androgen effects), labor economics, and comparative anthropology, I argue that strong social constructionism is scientifically inadequate as a primary causal framework for sex-differentiated interests and outcomes. The paper concludes that "critique without exit conditions" functions as epistemic gatekeeping rather than neutral evaluation.

Keywords: Social construction; patriarchy; radical feminism; pay gap

1. Introduction

This article is a response within an ongoing scholarly exchange. In Evolutionary Psychology and the Crisis of Empirical Rigor in Feminist Studies (Defant, 2025), I argued that biological and evolutionary explanations of gendered phenomena are often treated with unusual suspicion in influential strands of feminist scholarship, while social constructionist explanations are frequently granted a more secure default status. Cordelia Fine subsequently published a rejoinder in Sexuality & Culture, arguing that my critique misrepresents feminist studies, underestimates the extent to which feminist scholars have critically engaged biological research, and exemplifies what she characterizes as intellectual imperialism. The present article does not revisit the original argument in full, nor does it attempt to adjudicate all debates within evolutionary psychology, feminist theory, or the broader human sciences. Instead, it addresses Fine's specific criticisms and clarifies the epistemic standards at issue: whether biological explanations are evaluated under evidentiary conditions comparable to those applied to social constructionist accounts.

2. The Crux of the Debate

A substantial body of developmental research indicates that sex-differentiated behavioral tendencies relevant

to later interests, choices, and social organization are influenced by biological processes operating early in development. Work synthesized by Melissa Hines and others suggests that prenatal androgen exposure is associated with measurable differences in sex-typed play, interests, some aspects of spatial cognition, aggression-related tendencies, and preferences that may bear indirectly on later educational and occupational patterns (Hines, 2004, 2009, 2010, 2011, 2020; Hines et al., 2002, 2003, 2015, 2016). Much of this evidence comes from natural experiments, including studies of girls with congenital adrenal hyperplasia, and it is important to acknowledge the usual cautions: samples are often modest in size, effect sizes vary across domains, and the degree to which CAH findings generalize to typical-range variation in prenatal androgen exposure remains debated. Nonetheless, the evidence is difficult to reconcile with accounts that treat sex-differentiated interests and behavioral tendencies as primarily or exclusively products of postnatal socialization. The relevant point is not that biology determines later outcomes in any simple or deterministic way, but that early biological influences appear to be part of the causal landscape that any adequate theory must address.

Importantly, this literature does not rest solely on broad evolutionary inference. It also includes natural-experiment designs, most notably studies of girls with congenital adrenal hyperplasia, that allow researchers to examine the behavioral correlates of atypically high prenatal androgen exposure while partially controlling for many social variables. Findings from this work generally support an association between prenatal androgen exposure and later sex-typed behavior, including patterns of play, interests, and some cognitive measures. These findings do not establish a simple one-to-one pathway from hormones to adult occupational outcomes, but they do challenge accounts in which sex differences in interests and preferences are treated as primarily or exclusively cultural artifacts. Pubertal hormonal changes may also interact with earlier developmental influences and social contexts, contributing to divergence in some traits relevant to risk-taking, competitiveness, and social orientation during the period when educational and career interests begin to take shape.

The relevant issue in Fine's critique is not skepticism toward any individual study; such skepticism is appropriate and necessary. The question is whether skeptical scrutiny of biological evidence is accompanied by a comparably specific causal account of how socialization, norms, and institutions generate the early-emerging patterns at issue. Culture may shape how predispositions are expressed, amplified, discouraged, or redirected, but the extent of that influence is itself an empirical question rather than something that can be assumed in advance. Accounts that treat sex-differentiated interests and behavioral tendencies as primarily products of social construction therefore need to specify the mechanisms by which socialization produces these patterns, especially when they appear early in development and are associated with measurable biological variables. Without such mechanisms, social constructionist explanations remain causally incomplete.

A further difficulty for strongly social constructionist accounts concerns sexual orientation and associated sex-atypical traits. In my original paper, I noted that gay men and women often show, on average, some sex-atypical patterns in interests, behavior, and personality, a finding that is difficult to explain solely through ordinary gender socialization. Sexual orientation commonly emerges early, is observed across cultures, and has generally proven resistant to deliberate efforts at redirection. Historical attempts to change same-sex orientation were not only ineffective in any reliable sense, but often harmful, which further cautions against models that treat sexual orientation as a straightforward product of socialization. These patterns do not establish a single biological cause, nor do they exclude social influences on identity, expression, or lived experience. They do, however, fit more comfortably with biological and developmental models than with accounts that treat sexual orientation and associated behavioral patterns as primarily cultural constructions. Fine's response does not address this line of evidence, although it bears directly on the broader question of whether culture alone can generate stable, early-emerging patterns of sex-differentiated behavior.

Parallel evidence comes from labor economics, where a large empirical literature has examined sex differences in preferences, constraints, and tradeoffs relevant to labor-market outcomes. Research by economists such as Blau and Kahn (2017), Goldin

(2014), and others suggests that occupational choice, work hours, risk tolerance, preferences for flexibility, and willingness to enter highly competitive environments contribute meaningfully to observed gender pay disparities. These findings do not settle the origin of such preferences, but they do show that behavioral and preference-based factors are important components of aggregate labor-market outcomes and therefore require explanation rather than dismissal.

Importantly, these economists do not generally treat preferences as socially neutral abstractions; they examine how choices interact with constraints, incentives, family structures, and institutional arrangements. Yet this still leaves a central explanatory question: why do some preferences and tradeoffs differ by sex with such regularity across cohorts and institutional settings? Social constructionist explanations often emphasize the role of norms and expectations in shaping preferences, but they also need to specify how those norms generate stable and recurring sex-differentiated patterns, especially when similar patterns appear across diverse societies and policy environments. Evidence from more gender-egalitarian societies is complex and contested, but it nonetheless raises an important challenge for purely cultural accounts: formal equality and egalitarian norms do not reliably eliminate sex-differentiated interests, choices, or occupational distributions.

Fine cites feminist research emphasizing institutional practices, negotiation norms, and occupational segregation. Such work is relevant to understanding labor-market outcomes, but it does not by itself resolve the question of why men and women often respond differently to similar institutional incentives. Explaining outcomes primarily in terms of culture can risk circularity if preferences are treated as products of social norms, while those norms are inferred from the preferences and outcomes they are meant to explain. Biological and developmental evidence offers a potential independent causal reference point that social explanations should engage directly. Institutional and cultural explanations may identify important features of the environment, but without a more specific account of sex-differentiated predispositions, they remain incomplete as explanations of recurring differences in interests, choices, and occupational patterns.

A related issue arises in Fine's treatment of patriarchy. My argument is not that male-biased leadership or control over resources is morally acceptable, nor that discrimination and coercion have played no historical role. Rather, I argue that some forms of male-biased hierarchy may also arise as emergent outcomes of evolved sex differences interacting with reproductive roles, physical dimorphism, risk allocation, and ecological constraints. This is a causal claim about possible origins and persistence, not a moral defense of the resulting arrangements. Fine's response gives relatively little attention to this distinction between explanation and justification, and it does not address several empirical cases that bear on whether hierarchical sex differences can emerge even in settings explicitly organized around egalitarian ideals.

Anthropological and comparative evidence indicates that sexual divisions of labor are widespread across human societies, even though their specific forms vary considerably by ecology, subsistence system, and cultural context. Men have often been disproportionately represented in high-risk, status-competitive, and externally oriented activities, while women have more often been disproportionately involved in childcare and kin-centered labor. These broad patterns are consistent with evolutionary models emphasizing parental investment, reproductive tradeoffs, risk allocation, and physical dimorphism. They also suggest that hierarchical structures can emerge without requiring a model of deliberate, coordinated oppression as the sole or primary cause. On this account, sex-differentiated social roles may develop from recurring tradeoffs faced by males and females under particular ecological and reproductive constraints, later becoming institutionalized in ways that vary across societies.

Spiro's (1996) analysis of the Israeli kibbutzim provides a particularly important case. The kibbutzim were among the most ambitious modern attempts to weaken traditional gender roles through institutional design: collective childrearing, communal labor arrangements, ideological commitment to equality, and efforts to reduce the dependence of women on the private family structure. Yet despite these unusually favorable conditions for social constructionist expectations, the kibbutzim gradually moved back toward sex-differentiated patterns of work, family life, and leadership. Spiro argues that this reversal was not

primarily the result of coercive exclusion by men, but reflected persistent differences in interests, priorities, and tradeoffs, especially women's greater investment in childcare, family life, and community-internal roles. The case is not merely anecdotal. It is a rare historical instance in which a society attempted, self-consciously and institutionally, to reorganize gender relations along egalitarian lines, only to see many traditional patterns re-emerge. For that reason, the kibbutzim provide powerful evidence that sex-differentiated preferences and reproductive tradeoffs can generate unequal social outcomes even under conditions explicitly designed to minimize conventional gender socialization.

Fine's response gives little attention to this kind of evidence. Instead, patriarchy is treated largely as a normative category associated with oppression, harm, and unjust power. Explaining the emergence of such structures does not entail endorsing them, any more than explaining the evolution of aggression entails endorsing violence. Cases such as the kibbutzim are important because they suggest that sex-differentiated hierarchies can arise, or re-emerge, even where intentional domination is not the most plausible primary explanation. A critique of evolutionary explanations should therefore address these cases directly, rather than treating causal explanation as though it were equivalent to moral justification.

Across developmental biology, labor economics, and anthropology, a common issue emerges. Biological and evolutionary explanations are often asked to meet demanding standards of evidence before being admitted as serious contributors, while social constructionist explanations are sometimes treated as adequate without equivalent causal specificity. Fine's critique does not provide a comparably specific account of how socialization, norms, and institutions generate the recurring patterns discussed above. The asymmetry at issue, then, is not whether biological explanations should be scrutinized. They should be. The question is whether social explanations should also be expected to provide mechanisms, predictive expectations, and conditions under which they could be revised or rejected.

The issue is whether social constructionist explanations are held to the same standards of causal specificity that are demanded of biological and evolutionary frameworks. When influential feminist critiques challenge biological explanations without specifying how social norms, institutions, and expectations produce stable, early-emerging, and cross-contextual behavioral patterns, the result is an uneven standard of explanation. Under those conditions, skepticism toward evolutionary accounts can function less as neutral evaluation than as a form of epistemic exclusion.

3. Addressing Criticisms

Fine correctly notes that evolutionary psychology is not the only evolutionary research program relevant to human behavior, and that evolutionary psychology itself has been subject to extensive internal critique. This point is important, but it does not resolve the issue at stake here. Disagreements within evolutionary science over modularity, adaptationism, developmental plasticity, or the role of culture do not by themselves support strong social constructionist accounts of gendered behavior. Human behavioral ecology, gene-culture coevolution, cultural evolution, and related approaches differ in important ways from some versions of evolutionary psychology, but they generally reject blank-slate models of human psychology, accept evolved biological constraints, and treat culture as interacting with, rather than replacing, evolved dispositions. My argument therefore does not depend on evolutionary psychology being uncontested. It depends on whether biological and evolutionary explanations, broadly construed, are allowed to function as serious causal contributors alongside social and cultural explanations. Fine's emphasis on internal disagreement within evolutionary research is useful, but it should not obscure the fact that many of these alternative frameworks share the very premise that strong social constructionist accounts tend to minimize: human behavior develops within evolved biological constraints.

Fine also emphasizes that evolutionary psychology has been empirically and conceptually challenged for decades, citing a range of philosophical and methodological critiques. That observation is correct, and many of these critiques have contributed to useful refinement within evolutionary approaches to human behavior. But resolving every debate over modularity, adaptationism, evidentiary standards, or the interpretation of particular findings is neither the

purpose of this paper nor a prerequisite for the present argument. No serious research program in the human sciences is uncontested. The relevant issue is whether evolutionary explanations are evaluated alongside competing frameworks under comparable standards of evidence. My concern is that, in some influential feminist critiques, the existence of ongoing debate within evolutionary psychology is treated as a reason to withhold explanatory standing from biological accounts, while social constructionist explanations are not subjected to the same demand for mechanism, prediction, and falsifiability. In that setting, critique can become open-ended: biological explanations are challenged repeatedly, but the conditions under which they would count as provisionally acceptable are left unspecified. This is what I mean by critique without clear exit conditions.

A public example of this dynamic arose during the controversy surrounding James Damore's internal Google memo, which invoked research on sex-differentiated interests as one possible contributor to occupational patterns. The episode is not central to my argument, and I do not rely on it to adjudicate either workplace policy or Google's response. Its relevance is narrower: it illustrates how quickly debates over biological explanations can shift from empirical questions to concerns about harm, safety, or institutional climate. Such concerns may be legitimate in their own domain, but they do not by themselves answer the causal question of whether sex-differentiated interests contribute to occupational outcomes. Fine's critique operates in a scholarly rather than corporate setting, but it raises a related question: whether biological explanations are being evaluated on their empirical merits or treated as suspect because of their perceived normative implications.

Fine emphasizes the breadth of scholars and disciplines encompassed by my critique, suggesting that I dismiss serious scholarship across fields as political advocacy. The issue is not whether scholars should study gender roles, norms, institutions, identities, or inequalities; such topics are plainly legitimate subjects of inquiry. The issue is how competing explanations are evaluated. When biological or evolutionary accounts are treated as suspect because of their perceived normative implications, while social constructionist accounts are treated as the default starting point, inquiry risks becoming asymmetrical. In such cases, the problem is not the existence of feminist scholarship across many fields, but the possibility that certain assumptions about gender, power, and social construction have become so widely embedded that they shape standards of explanation without being made explicit. The breadth of the pattern is therefore relevant, but it should be understood as a claim about recurring epistemic tendencies within influential strands of feminist scholarship, not as a dismissal of all work conducted under that label.

This concern recurs at several points in Fine's response. Rather than directly addressing the cumulative force of the developmental, economic, and anthropological evidence, her critique often shifts attention to the risks, limitations, or possible misuse of biological explanation. Those issues deserve consideration, but they do not answer the central epistemic question: whether biological and social explanations are being evaluated by comparable standards. Attempts to integrate biological and social accounts should not be treated as intrusions into feminist inquiry, but as part of the ordinary process of causal explanation in the human sciences. No field benefits when its preferred frameworks are insulated from empirical challenge, and feminist scholarship should be strong enough to engage biological evidence without treating it as a threat to its moral or political concerns.

4. The Limits of Strong Social Constructionism

Fine's first major criticism is that I misrepresent feminist scholarship by suggesting that empirical research has decisively undermined social constructionist accounts of the gendered division of labor. According to Fine, even if biological or hormonal influences on behavior were well established, this would neither disconfirm social constructionist explanations nor diminish the need for feminist studies. But this response does not answer the narrower argument I am making. The point is not that culture, history, institutions, or norms are irrelevant, nor that feminist inquiry has no legitimate subject matter. The point is that these factors cannot be invoked as sufficient explanations unless they are specified causally and tested against competing biological and developmental accounts. Strong social constructionism, understood as the view that sex-differentiated interests, preferences,

and behavioral tendencies are primarily cultural artifacts, becomes difficult to sustain when biological and developmental evidence is treated as peripheral while social explanations are not required to meet comparable standards of causal specification.

Fine argues that I misrepresent Wood and Eagly by treating their biosocial theory as supportive of my position. The point requires clarification. Biosocial theory does not simply reproduce evolutionary psychology, and Wood and Eagly differ from some evolutionary psychologists over the mechanisms by which sex differences influence behavior. But biosocial theory does not rescue strong social constructionism. Wood and Eagly ground gendered divisions of labor partly in evolved sex differences related to reproduction and physical dimorphism, including pregnancy, lactation, and average differences in upper-body strength. Their disagreement with evolutionary psychology concerns the pathway through which biology shapes behavior: whether primarily through evolved psychological mechanisms or through social role structures that emerge from biological constraints and then shape psychology. In either case, biology plays a foundational causal role. Social roles are not arbitrary cultural inventions, but develop within constraints imposed by reproductive biology, physical dimorphism, ecology, and social organization. Thus, invoking biosocial theory does not undermine my argument. It reinforces the central point: even accounts that emphasize flexibility, social roles, and bidirectional causation still depend on biological constraints that strong social constructionist explanations tend to minimize.

Fine devotes considerable attention to disputing a simple, direct causal relationship between adult testosterone levels and long-term career choices, focusing on Archer (2006), Sapienza et al. (2009), and more recent experimental work. That discussion may be relevant to claims about short-term activational effects of adult testosterone, but it does not answer the developmental question at the center of my argument. Archer's review concerns the challenge hypothesis and the relationship between testosterone and aggression, a framework originally developed to explain seasonal reproductive behavior in birds and later extended, cautiously and controversially, to humans. It was not intended to explain prenatal hormonal organization, developmental trajectories, or enduring sex differences in interests and preferences. Thus, evidence that adult testosterone effects are complex, context-dependent, or limited does not refute the claim that hormones can shape behavioral development earlier in life. The central issue is the distinction between adult activational effects and earlier organizational influences. Fine's critique gives substantial attention to the former while leaving the latter less fully addressed.

What Fine's analysis gives less attention to is the developmental literature on organizational hormone effects. Research summarized by Melissa Hines and others provides substantial evidence that prenatal androgen exposure contributes to sex-differentiated patterns in interests, play behavior, aggression-related tendencies, and some aspects of spatial cognition. These effects are especially evident in natural-experiment contexts such as studies of girls with congenital adrenal hyperplasia, where atypically high prenatal androgen exposure is associated with later behavioral differences. This literature is not beyond criticism: sample sizes are often modest, effect sizes vary by domain, and the generalizability of CAH findings to typical-range hormonal variation remains debated. But those limitations do not make the evidence irrelevant. They indicate that the developmental account should be stated carefully, not that it should be set aside. If early hormonal influences contribute to the formation of sex-differentiated predispositions, then debates over short-term adult testosterone effects address only part of the causal question. The central issue is not whether hormones explain all later behavioral patterns, but whether they are among the developmental factors that help shape the predispositions from which such patterns emerge.

Fine also argues that my reliance on cross-cultural evidence concerning gender equality and STEM participation is misplaced because the evidence is mixed and contested. This criticism deserves serious attention. Richardson et al. (2020), for example, raised legitimate concerns about measurement choices, the use of particular gender-equality indices, and the interpretation of STEM participation as a single aggregate category. Those concerns caution against treating the gender-equality paradox as a simple, universal, or mechanically predictable relationship. But they do not remove the central empirical problem.

Across countries, outcomes differ depending on whether one examines achievement, interests, aspirations, degree choice, occupational choice, or representation in particular STEM fields. They also differ depending on which measure of equality is used. Still, the persistence of sex-differentiated interests and choices in many highly egalitarian societies remains difficult for purely social constructionist accounts to explain.

The broader issue is that uncertainty appears to be weighted differently across explanatory frameworks. In the case of biological or evolutionary explanations, lack of consensus, measurement sensitivity, or variation across studies is often treated as a reason to withhold explanatory standing. In the case of social constructionist explanations, comparable uncertainty is more often treated as an invitation for further theorizing rather than as a challenge to the framework's default status. This is the asymmetry at issue. Debate and refinement are normal features of any serious research program. They should not be treated as disqualifying when they occur in biological accounts, but as routine scholarly complexity when they occur in social constructionist ones.

5. Critical Engagement and the Question of Acceptance

Fine argues that my claim about feminist scholarship and politically inconvenient evidence is "highly misleading," pointing to a substantial body of feminist work that critically engages biological and hormonal explanations of sex differences. She cites, in particular, Jordan-Young (2010), Jordan-Young and Karkazis (2019), Fausto-Sterling (2000), Martin (1991), and others as evidence that feminist scholars do not simply dismiss biological claims, but often scrutinize them closely. This point is important, but it does not answer the claim I am making. My argument is not that feminist scholars fail to read, cite, or criticize biological research. It is that biological research is often kept in a state of permanent critical suspension: engaged, challenged, and problematized, but rarely allowed to mature into an accepted causal explanation even when evidence accumulates across multiple lines of inquiry. Critique is not the same as openness to revision. The relevant question is whether sustained engagement with biological evidence ever leads to conditional acceptance, or whether it functions primarily to preserve the default authority of social constructionist accounts. So long as biological explanations are engaged primarily as objects of critique rather than as possible contributors to accepted causal accounts, feminist scholarship will remain vulnerable to the charge that its standards of explanation are asymmetrical.

The work Fine cites — most notably Jordan-Young (2010) and Jordan-Young and Karkazis (2019) — is careful, detailed, and often methodologically sophisticated. It identifies inconsistent definitions, mixed findings, underpowered studies, and overextended conclusions in parts of the hormone-behavior literature. None of this is in dispute. The question is what follows from these critiques. Identifying limitations and ambiguities in biological research is valuable, but it is not the same as specifying what evidentiary conditions would warrant provisional acceptance of biological explanations. If critique remains open-ended, biological accounts can be held indefinitely at the threshold of acceptance, while social constructionist explanations retain explanatory standing without being required to meet comparable standards of mechanism, prediction, or falsifiability.

This asymmetry is the core issue. In the mode of critique Fine emphasizes, uncertainty in biological research is often treated as a reason to withhold explanatory acceptance, while uncertainty in social explanations more often becomes an occasion for further theorizing. The question is not whether feminist scholars critique biology; plainly, many do. The question is whether such critiques specify the conditions under which biological explanations would count as provisionally acceptable when evidence converges across multiple lines of inquiry. In much of the literature Fine cites, those acceptance criteria remain unclear.

Jordan-Young's critique of early brain-organization theory documents genuine problems in parts of that literature, including inconsistent definitions, methodological limitations, and overextended conclusions. Those criticisms are relevant, but they do not by themselves provide a competing causal framework capable of explaining early-emerging sex differences in interests, play behavior, or preferences. Nor do they fully address later developmental work,

including studies of congenital adrenal hyperplasia, that was designed in part to overcome some of the weaknesses identified in earlier research. The limitation of this mode of critique is that it can restrict the explanatory uptake of biological accounts without supplying an alternative account with comparable causal and predictive specificity. Methodological criticism is necessary, but it should be paired with an account of what evidence would warrant provisional acceptance, or with a competing explanation capable of explaining the same developmental patterns.

My brief characterization of Jordan-Young (2010) was not intended as a comprehensive summary of her work, and it may reasonably be criticized as compressed. But compression is not the same as misrepresentation. The central issue is not whether Jordan-Young identifies real problems in parts of the hormone-behavior literature; she does. The issue is whether such critique is accompanied by clear criteria for when biological explanations would count as provisionally warranted. In this respect, her work illustrates the broader pattern at issue: biological explanations are subjected to sustained methodological scrutiny, but the standards under which they would be accepted as partial causal explanations often remain unspecified.

Fine similarly relies on Jordan-Young and Karkazis (2019) to argue that feminist scholars have challenged testosterone-centered explanations of aggression, risk-taking, and related behaviors. Much of their critique is directed at simplified public narratives about testosterone and at claims concerning short-term adult activational effects. Those critiques may be valuable, especially where testosterone is invoked in overly broad or deterministic ways. But they do not settle the broader developmental question of whether prenatal hormonal influences contribute to sex-differentiated interests, preferences, and behavioral tendencies. Nor do they provide an alternative causal explanation for why some sex-differentiated behavioral patterns emerge early and recur across cultural settings. A critique of exaggerated testosterone narratives should therefore not be treated as a refutation of biological causation more generally. The relevant question remains whether biological explanations are allowed to function as partial causal accounts when the evidence supports them, rather than being held indefinitely in a state of critical suspension.

Fine further objects to my treatment of Anne Fausto-Sterling and Emily Martin, arguing that their work is "deeply empirically engaged" with biological and evolutionary arguments. That characterization requires qualification. Fausto-Sterling's discussion of sex non-binarity draws heavily on intersex conditions and differences of sex development to challenge simplified accounts of sexual dimorphism. Whatever the value of that work as a critique of social and medical classification, it does not by itself overturn the biological point that human reproduction remains organized around two gamete types and a broadly dimorphic reproductive system. This distinction is especially important in Fausto-Sterling's case because her biological training can make cultural and classificatory claims appear to carry the authority of empirical biology, even when the argument being advanced is primarily conceptual or political. Martin's analysis of sperm and egg metaphors likewise offers an influential cultural critique of scientific language, but it is not primarily an empirical contribution to reproductive biology. Indeed, scientists have criticized such arguments when they appear to treat metaphorical descriptions in scientific writing as though they undermine the underlying biological asymmetries associated with anisogamy. These examples therefore support my broader point: culturally informed critiques of biological language may be valuable, but they should not be treated as substitutes for empirical causal accounts of sex differentiation.

My brief references to these authors were compressed because they appeared within a broader synthetic critique rather than a detailed review of feminist science studies. Greater precision in those passages may have been warranted, but that does not resolve the substantive issue. The question is whether culturally framed critiques of biological language, classification, or metaphor are being treated as sufficient to displace biological explanations, while evolutionary and hormonal accounts are required to provide much more detailed causal demonstration before being granted explanatory standing. If so, the asymmetry remains: critiques of biological framing are allowed to carry considerable epistemic weight, while biological explanations themselves are held at the threshold of acceptance.

Fine also argues that my failure to cite certain critical works, including Jordan-Young and Karkazis (2019), constitutes a significant omission. That criticism is useful to the extent that it identifies literature that should be addressed more directly, and I have done so here. But the omission of a particular critical text does not by itself establish misrepresentation, especially in an article concerned with broad epistemic patterns rather than a comprehensive review of every debate in feminist science studies. No synthetic critique can cite every relevant intervention, and the existence of critical engagement with biology does not, by itself, answer the question at issue. My argument does not depend on claiming that feminist scholars are unaware of biological evidence. It depends on examining how that evidence is treated once encountered: whether it is allowed to contribute to accepted causal explanations, or whether it remains primarily an object of continuing critique.

That is the claim I advance, and it is not answered simply by showing that feminist scholars have criticized biological research. The relevant question is whether those critiques specify the conditions under which biological explanations would count as provisionally warranted. Fine's response raises a number of methodological concerns about biological and evolutionary accounts, some of which merit serious consideration. But without corresponding criteria for acceptance, such critique risks reproducing the asymmetry my original paper identified: biological explanations remain subject to continuing skepticism, while social constructionist explanations retain their default explanatory standing.

Taken together, Fine's two objections point back to the same epistemic issue. In the first, she argues that I overstate the evidentiary force of biological and evolutionary explanations against social constructionist accounts. In the second, she argues that I understate the extent to which feminist scholars have engaged biological evidence. But neither objection resolves the central question: whether sustained critique of biological explanations is accompanied by clear criteria for their provisional acceptance.

6. Complexity, Scope, and Standards of Explanation

Fine argues that my critique fails to meet scholarly norms because it does not adequately engage opposing arguments and because, in her view, it treats complex phenomena such as patriarchy, pay inequality, and occupational segregation in an overly simplified or partisan way. This criticism misidentifies the purpose of the article. I am not offering a comprehensive literature review of feminist explanations of patriarchy, the gender pay gap, or occupational segregation. Nor am I claiming that these phenomena can be reduced to a single biological cause. My focus is narrower: how competing explanatory frameworks are adjudicated, especially whether biological and evolutionary explanations are treated as admissible causal contributors alongside social, cultural, and institutional explanations. The question is whether those analyses are evaluated under standards comparable to those applied to biological accounts, or whether biological explanations are treated as suspect because of their perceived normative implications.

Scholarly norms do not require an author to rehearse every counterargument before diagnosing a recurring pattern of epistemic practice. They do require clarity about standards of evidence, openness to rival explanations, and a willingness to specify conditions under which favored frameworks could be revised or rejected. The pattern I am criticizing occurs when social constructionist explanations are treated as the default starting point, while biological explanations are subjected to heightened skepticism and framed as suspect because of their perceived normative implications. Under those conditions, the problem is not that feminist scholarship studies culture, institutions, or power, but that one class of explanations receives default standing while another must overcome a substantially higher threshold before being admitted as a serious causal contributor.

Fine's discussion of patriarchy illustrates this asymmetry. My argument is not that male-biased hierarchies are morally justified, nor that they have caused no harm. It is that their origins and persistence require causal explanation. If some forms of male-biased leadership or resource control arise partly from evolved preferences, reproductive strategies, physical dimorphism, risk allocation, or sex-differentiated tradeoffs, then explaining those origins does not entail endorsing the resulting inequalities. The same distinction applies elsewhere: explaining the evolution of aggression does not justify violence, and explaining

evolved mating strategies does not justify coercion. Biological explanations may be misused politically, but the possibility of misuse does not make them false or inadmissible. Treating causal explanation as though it were moral endorsement risks obscuring the very question at issue: how such patterns arise in the first place.

A similar issue arises in Fine's treatment of the gender pay gap. Human-capital variables do not explain the entire gap, but neither do broad appeals to institutions, norms, or policy choices resolve the causal question. Such factors may contribute to labor-market outcomes, but they must be specified and tested rather than invoked as sufficient explanations by default. Pointing to a large feminist literature emphasizing institutional practices, negotiation norms, occupational segregation, or policy constraints therefore does not by itself answer my critique. The question is not whether social explanations exist, but whether they are treated as empirically contingent hypotheses or as conceptually privileged starting points. When uncertainty in biological or preference-based explanations is treated as a reason to withhold acceptance, while uncertainty in social explanations becomes an invitation for further theorizing, the result is an asymmetrical standard of explanation.

Fine also suggests that my critique is weakened by the fact that my formal training is in geology and geochemistry rather than feminist theory or psychology. This objection does not address the substance of the argument. I do not claim authority over every subfield I discuss; I evaluate arguments, evidence, and standards of explanation. Cross-disciplinary critique is routine in science and scholarship, especially when the issue concerns evidentiary standards, causal reasoning, and the adjudication of competing explanatory frameworks. Feminist scholarship itself regularly engages and critiques biology, economics, evolutionary theory, psychology, and other fields. Such critique is legitimate when it is grounded in evidence and argument rather than disciplinary possession. My position outside both feminist studies and evolutionary psychology may be an advantage in this narrower respect: I approach the dispute less as a defender of any disciplinary tradition than as a scientist concerned with evidentiary symmetry, causal inference, and the standards by which competing explanations are accepted or rejected. By the same standard, my claims should be evaluated on the coherence of their reasoning and the adequacy of the evidence, not on the field in which I received my doctoral training.

7. Explanatory Pluralism and the Charge of Intellectual Imperialism

Fine characterizes my critique as an instance of "intellectual imperialism," understood as the unjustified attempt to denigrate or impose one framework on domains where it has limited explanatory value. I do not claim that evolutionary psychology provides a complete or sufficient explanation for all gendered phenomena, nor do I argue that biological explanations should replace social, cultural, institutional, or political analyses. My claim is more limited: biological and evolutionary explanations should be treated as admissible causal contributors in the analysis of gendered outcomes. They should be evaluated alongside social explanations according to comparable standards of evidence, causal specificity, and openness to revision, rather than treated as suspect because of their perceived normative implications.

Insisting that competing explanations be evaluated evidentially is not intellectual imperialism. It is a basic requirement of scholarly inquiry. A charge of intellectual imperialism would be more appropriate if one framework sought to exclude rival explanations from consideration without engaging their evidence. My argument is not that evolutionary psychology should dominate the analysis of gendered phenomena, but that biological and evolutionary explanations should not be dismissed as irrelevant, dangerous, or conceptually illegitimate before their causal claims are assessed. Requests for epistemic symmetry should therefore be distinguished from attempts to impose a single explanatory framework. The former asks that rival explanations compete under comparable standards; the latter would foreclose such competition.

Fine's appeal to a wide range of social-scientific and humanistic research documenting the social construction of gender systems does not resolve this issue. That such research exists is not in dispute. The question is not whether social factors sometimes influence behavior, but whether invoking them is sufficient to displace biological explanations without meeting comparable standards of causal specification. Evidence for social influence does not show that biological factors are irrelevant, nor does it explain

why biological accounts should face a higher threshold of acceptance than the social explanations offered in their place. Evolved predispositions and biological constraints should be permitted to function as partial causal explanations alongside social ones, rather than being treated as inadmissible whenever social explanations are available.

Much of Fine's discussion also turns on the relationship between explanation and endorsement. When biological or evolutionary accounts are offered to explain some patterns of behavior or preference, they are often treated as though they provide a moral defense of inequality, stigma, or harm. That inference does not follow. Explaining why certain preferences or behavioral tendencies may exist does not entail endorsing every social arrangement that develops around them, nor does it determine how societies ought to respond. Evolutionary psychology is an explanatory framework, not a political program. It can help identify possible causal pathways, but it does not by itself prescribe what societies should value, punish, protect, or reform. This may be why evolutionary psychology brings such an important perspective to the table – it does not have a political objective. Treating biological explanations as suspect because of their perceived normative implications risks shifting debate away from evidence and toward moral association.

Fine's charge of intellectual imperialism therefore returns us to the central asymmetry. Evolutionary explanations are criticized not only on empirical grounds, but also because they are perceived as overreaching, politically risky, or insufficiently aligned with the normative aims of feminist scholarship. Social constructionist accounts, by contrast, are often granted initial explanatory standing even when their mechanisms and falsification conditions remain unclear. Asking that both kinds of explanation be evaluated under comparable evidentiary standards is not an attempt to impose evolutionary conclusions on feminist scholarship. It is an attempt to preserve the conditions of genuine scholarly inquiry, in which competing causal accounts are judged by their explanatory power rather than by their perceived political implications.

8. Explanatory Standards and the Charge of Ideology

Fine's concluding remarks return to earlier concerns about misrepresentation, omission, scholarly objectivity, and what she characterizes as "gender ideology." These criticisms do not alter the central issue. My argument is not that evolutionary psychology provides a complete or sufficient account of gendered phenomena, nor that social, cultural, or institutional explanations should be displaced. It is that biological and evolutionary explanations constitute legitimate explanatory resources and should be evaluated alongside social and cultural accounts under comparable standards of evidence and causal specificity. Treating such explanations as ideologically suspect because of their perceived normative implications does not answer their empirical claims; it shifts the discussion from causal adequacy to political association.

Fine's concluding remarks also suggest that failure to engage every relevant feminist critique amounts to a failure of scholarly objectivity. But these are distinct standards. Scholarly objectivity requires serious engagement with opposing arguments, but it does not require encyclopedic citation or the exhaustive rehearsal of every critical literature before a broader epistemic pattern can be analyzed. More fundamentally, objectivity requires clarity about explanatory commitments, openness to rival hypotheses, and a willingness to specify the conditions under which favored explanations could be revised or rejected. My critique concerns whether the feminist analyses under discussion meet that standard when biological explanations are treated as persistently inadequate while social constructionist explanations retain default standing.

The charge that I advocate a "gender ideology" further illustrates the epistemic asymmetry at issue. Explanatory claims about evolved predispositions or biological constraints are often treated as though they carry moral or political commitments. But explanation does not imply justification. Describing how certain patterns arise does not entail endorsing them, nor does it prescribe how societies ought to respond. If biological explanations are treated as ideologically suspect because of their perceived normative implications, then causal arguments are being judged partly by political association rather than by evidentiary merit. That is precisely the concern at the center of this paper: explanatory frameworks should be evaluated by their causal adequacy, not by whether their implications are considered politically comfortable.

Fine's conclusion therefore returns to the central issue this paper has sought to clarify. Her response emphasizes concerns about scope, tone, omission, and ideological risk, but these concerns do not resolve the underlying epistemic question: whether biological and social explanations are being evaluated under comparable standards. Critical engagement with feminist scholarship is both legitimate and necessary, just as critical engagement with evolutionary psychology is legitimate and necessary. But scholarly inquiry requires that no framework be granted default protection from challenge, and that no line of explanation be treated as illegitimate because of moral association alone.

9. A Word of Caution

A note of caution is warranted. More than three decades ago, feminist writers Barbara Ehrenreich and Janet McIntosh (1997) warned that large segments of feminist and progressive scholarship were drifting toward an outright rejection of biology – not merely critiquing its misuse but treating biological explanation as inherently suspect. Writing in the left-leaning *The Nation*, they documented how entire disciplines had come to assume that "any consideration of biological factors believed to be innate to the human species is completely irrelevant in understanding the nature of human behavior and society," a stance they argued was motivated less by evidence than by fear of determinism and political misuse.

Ehrenreich and McIntosh further cautioned that extreme constructivist and postmodern approaches were beginning to resemble a form of secular creationism: not because they denied evolution explicitly, but because they effectively bracketed evolutionary constraints out of serious consideration while treating culture as infinitely malleable and explanatorily sufficient. Their critique was methodological, not moral. Biology, they emphasized, is probabilistic rather than deterministic, while culture is far less plastic than often assumed. Ignoring either leads not to liberation, but to explanatory failure.

The relevance of this warning has not diminished. Much of Fine's critique proceeds by subjecting evolutionary explanations to sustained skepticism while leaving social constructionist assumptions largely unexamined. The result is not balanced inquiry, but an epistemic asymmetry in which biological explanations remain perpetually provisional and normatively suspect, while cultural explanations function as unchallenged defaults. That is precisely the pattern Ehrenreich and McIntosh cautioned against – and it remains unresolved today.

Karen Offen, writing in Signs: Journal of Women in Culture and Society, gave a definition of feminism that encompasses many of the ideas expressed by other feminists. She defines feminism as both an ideology and a movement for sociopolitical change based on a critical analysis of male privilege, women's subordination, and the unequal distribution of political, social, and economic power between the sexes. In stark contrast, evolutionary psychology emerged as an effort to explain human behavior from an empirical standpoint, unencumbered by explicit political objectives.

References

[1] Archer, J. (2006). Testosterone and human aggression: an evaluation of the challenge hypothesis. *Neuroscience and Biobehavioral Reviews, 30*(3), 319-345.

[2] Blau, F. D., & Kahn, L. M. (2017). The gender wage gap: Extent, trends, and explanations. *Journal of Economic Literature, 55*(3), 789–865.

[3] Defant, M. (2025). Evolutionary Basis of Gender Dynamics: Understanding Patriarchy, the Pay Gap, and the Glass Ceiling. *Journal of Libertarian Studies, 29*(1), 10-27. doi: 10.35297/001c.132320

[4] Defant, M. (2025). Evolutionary Psychology and the Crisis of Empirical Rigor in Feminist Studies. *Sexuality and Culture*. doi: 10.1007/s12119-025-10465-7

[5] Ehrenreich, B., & McIntosh, J. (1997, 6 9). The New Creationism:: Biology Under Attack. *The Nation*. Retrieved from http://cogweb.ucla.edu/Debate/Ehrenreich.html#:~:text=The%20New%20Creationism,Barbara%20Ehrenreich%20and%20Janet%20McIntosh

[6] Fausto-Sterling, A. (2000). *Sexing the Body: Gender Politics and Construction of Sexuality*. New York: Basic Books.

[7] Goldin, C. (2014). A grand gender convergence: Its last chapter. *American Economic Review, 104*(4), 1091–1119.

[8] Hines, M. (2004). *Brain Gender*. Oxford: Oxford

University Press.

[9] Hines, M. (2009). Gonadal hormones and sexual differentiation of human brain and behavior. In D. W. Pfaff, A. P. Arnold, A. M. Etgen, S. E. Fahrbach, & R. T. Rubin, *Hormones, Brain and Behavior* (pp. 1869–1909). San Diego, CA: Academic Press.

[10] Hines, M. (2010). Sex-related variation in human behavior and the brain. *Trends in Cognitive Sciences, 14*(10), 448-456.

[11] Hines, M. (2011). Gender development and the human brain. *Annu Rev Neurosci., 34*, 67-86.

[12] Hines, M. (2020). Human gender development. *Neurosci Biobehav Rev., 118*, 89-96.

[13] Hines, M. (2020). Neuroscience and Sex/Gender: Looking Back and Forward. *Journal of Neurosciences, 40*, 37-43.

[14] Hines, M., Constantinescu, M., & Spencer, D. (2015). Early androgen exposure and human gender development. *Biology of Sex Differences, 6*, 1-10.

[15] Hines, M., Fane, B. A., Pasterski, V. L., Mathews, G. A., Conway, G. S., & Brook, C. (2003). Spatial abilities following prenatal androgen abnormality: Targeting and mental rotations performance in individuals with congenital adrenal hyperplasia (CAH). *Psychoneuroendocrinology, 28*, 1010–1026.

[16] Hines, M., Golombok, S., Rust, J., Johnstone, K. J., Golding, J., & al., e. (2002). Testosterone during pregnancy and gender role behavior of preschool children: A longitudinal, population study. *Child Development, 73*, 1678-1687.

[17] Hines, M., Johnston, K. J., Golombok, S., Rust, J., Stevens, M., & Golding, J. (2002). Prenatal stress and gender role behavior in girls and boys: a longitudinal, population study. *Hormones and Behavior, 42*, 126–134.

[18] Hines, M., Spencer, D., Kung, K., Browne, W., Constantinescu, M., & Noorderhaven, R. (2016). The Early Postnatal Period, Mini-Puberty, Provides a Window on the Role of Testosterone in Human Neurobehavioural Development. *Current Opinion in Neurobiology, 38*, 69–73.

[19] Jordan-Young, R. (2010). *Brain Storm: The Flaws in the Science of Sex Differences*. Cambridge, MA: Harvard University Press.

[20] Jordan-Young, R., & Karkazis, K. (2019). *Testosterone: An unauthorized biography*. Harvard Univeristy Press.

[21] Martin, E. (1991). The egg and the sperm: How science has constructed a romance based on stereotypical male-female roles. *Signs: Journal of Women in Culture and Society,, 16*(3), 485–501. Retrieved from https://www.journals.uchicago.edu/doi/10.1086/494680

[22] Richardson, S., Reiches, M., Bruch, J., Boulicault, M., Noll, N., & Shattuck-Heidorn, H. (2020). Is there a Gender-Equality Paradox in Science, Technology, Engineering, and Math (STEM)? *Psychological Science, 31(3)*(3), pp. 338–341. Retrieved from https://journals.sagepub.com/doi/10.1177/0956797619872762

[23] Sapienzaa, P., Zingalesb, L., & Maestripieri, D. (2009). Gender differences in financial risk aversion and career choices are affected by testosterone. *Proceedings of the National Academy of Sciences, 106*(36), 15268-15273. Retrieved from https://www.pnas.org/doi/abs/10.1073/pnas.0907352106

[24] Spiro, M. E. (1996). *Gender & Culture: Kibbutz Women Revisited*. New York: Transaction Publishers.