

# Politics, Genetics, and “Greedy Reductionism”<sup>1</sup>

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would like to thank Alford, Funk, and Hibbing, and Hannagan and Hatemi, for agreeing to write critical responses to my article, and I am grateful for the opportunity afforded me to respond.

I begin with the response of Alford, Funk, and Hibbing. An enforced limitation on space requires some selectivity in my response, but I have tried to respond to all of their major critiques.

Alford, Funk, and Hibbing (AFH) begin by implying that I am caught in the “hubris” of a “mind-body dualism” which sees the mind as some kind of mystical entity. Nowhere in my article have I said anything at all regarding “mind-body dualism.” It is not an assumption I argue for, nor is it a premise of any of the arguments I make. Perhaps AFH believe that if genes are not accepted as the ultimate causal explanation for any and all human phenomena, then somehow this implies “mind-body dualism.” It does not.

AFH claim that I err in treating the environment as “exogenous”: “Whether the causal order is [*genes* → *beliefs*] or [*genes* → *physical traits* → *social reactions* → *beliefs*], the underlying cause is still genetic.” To illustrate this point, AFH assert that “negative parenting is typically assumed to be the cause of children’s antisocial behavior, when in point of fact children play an important role in shaping their own environment, in this case by influencing the behavior of their parents.”<sup>2</sup> What AFH are arguing is that children, presumably on the basis of genetic traits, *create* or *elicit* negative parenting, and the effects on children of this negative response on the part of parents should itself be counted as “genetic” (i.e., caused by the genetic traits of the child in question).

This tendency to view gene-environment covariance as a genetic effect is not uncommon among psychologists who undertake twin studies. Thus, according to Bouchard, “[identical] twins tend to elicit, select, seek out, or create very similar effective environments and, to that extent, the impact of these experiences is counted as a

genetic influence.”<sup>3</sup> Suppose that a one-year-old child is cranky and is beaten by its parents. According to Bouchard and AFH, the impact upon the child of its being beaten by its parents is itself *genetic* because, presumably, the fact that the child is cranky is genetic. But of course, not all parents are abusive and beat their children in response to a child’s cranky behavior (genetic or otherwise), and for any given pair of children with similar dispositions, parental responses will be as varied as parenting styles.

But the absolutely fallacious nature of the assumption that the effects of behavior which is a response to a “genetic trait” should itself be counted as genetic can be seen by considering that slavery of blacks was the response of a group of individuals (white Europeans and Americans) to a genetically transmitted trait, i.e., black skin color. Are we to assume then, that the effects upon blacks of their enslavement by European whites were *genetic*, because slavery was “caused” or “elicited” or “created” by the genetic trait of black skin color? Rape is a response of some men to the genetic characteristic of being female. Should we say that the effects upon women of being raped are *genetic*?

If AFH wish to distance themselves from such preposterous (and pernicious) conclusions, then they are going to have to distinguish between two types of “impacts” upon individuals from behavior that is deemed a response to a genetic trait: Impacts that *will* be deemed genetic and impacts that *will not* be deemed genetic. On what basis might they propose to draw such a distinction?

In support of the validity of the Equal Environment Assumption (EEA), AFH reference studies of so-called “reverse zygosity.” These studies, as noted in my article, concern a tiny subpopulation of DZ twins mistakenly thought by their parents to be MZ twins, and purport to show that the degree of correspondence between MZ twins still exceeds that of DZ twins. According to AFH, “the results of these mis-categorization studies are clear: DZ twin pairs believed by their environments to be MZ twin pairs are no more similar than DZ twin pairs believed to

be DZ twin pairs.” *But the results of such studies are not clear.* To repeat what I said in my article:

Most of these studies of “reverse zygosity,” however, relied upon parental accounts of how they raised their twin children many years later. Because of problems with biased impressions, poor memory, and poor reliability, studies that rely upon parental recall of their child rearing practices have been shown to be notoriously unreliable, typically showing reliability measures of only 0.3–0.5.<sup>4</sup>

In addition, studies have shown that parents’ recounts of their rearing practices are often biased to match some ideal of parenting.

How could an assumption as momentous (and counterintuitive) as the EEA rest upon such shaky ground? Are we to assume, on the basis of the potentially biased and faulty memories of elderly parents concerning their child rearing practices, that the debate concerning the validity of the EEA has been solved once and for all? According to what standard of *science* are we to accept this as conclusive evidence of anything?<sup>5</sup>

I now consider what AFH characterize as “*the most important challenge we wish to issue to Charney concerning the EEA*”:

We estimate the heritability of political and social attitudes to be in the .4 to .5 range, leaving .5 to .6 attributable to environmental factors. But these same procedures reveal that party identification is only about .14 heritable, leaving .86 attributable to the environment, so the classical twin design reports a dramatic difference in the heritability of political beliefs and party identification”. . . If violations of the EEA are responsible for reported heritability, Charney must argue that parents of MZ and DZ twins raise their children equally similarly with regard to party identification but differentially with regard to political attitudes.<sup>6</sup>

In response, I focus on the most obvious problem with the “challenge” AFH set before me, or rather, with the obvious answer. AFH appear to treat “party identification” as a *fixed variable*, such that we can say of any given individual that she possesses political beliefs (“ideology”) A and party identification B, and both are supposed to be *invariable*.

Let me highlight two well-known points concerning party identification in the United States: First, party identification in the United States is relatively weak compared to a number of other countries, as indicated by the incidence of ticket-splitting (in the 2000 elections, 20 percent of voters split their ballots by voting for candidates from different parties for president and for the U.S. House of Representatives); the existence of a sizable number of voters who consider themselves Independents (and hence aligned with neither party); and the frequency with which Americans change political parties (a recent Pew poll showed a sharp change in Americans’ political party identification: Democrats now outnumber Republicans 50 percent to 35 percent, as opposed to 2002, when both had 43

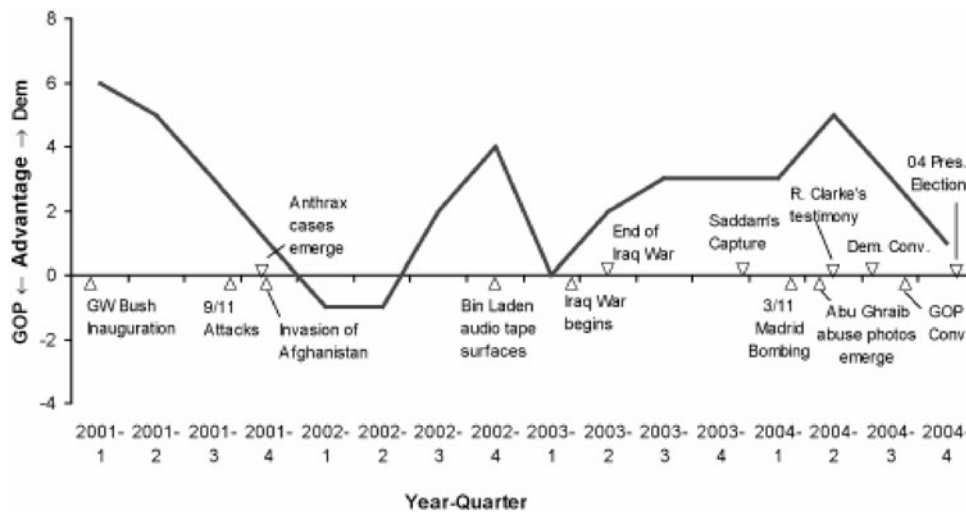
percent). In one study, Brody and Rothenberg showed that fewer than half the 1980 voters were stable throughout their campaign year in their self-description on the National Election Study party identification question.<sup>7</sup> In fact, party identification is so variable in the United States that shifts in party identification can be correlated with specific political events (see figure 1). Second, political parties in the United States exhibit relatively low internal unity and lack strict adherence to an ideology or set of policy goals, allowing for a less strict alignment between ideology and party identification.<sup>8</sup>

Hence, the findings of AFH—high ideological correlation and low party identification correlation—are *exactly what one would expect given the nature of party identification in the United States*. Given the relatively weak and variable nature of party identification, we would expect political ideology (whatever its origins) to be more enduring, more fixed and constant, than party identification (which might differ depending upon when any given individual is asked to identify her party affiliation). MZ twins are more likely to share political attitudes as opposed to party identification because of the relatively weak and highly variable nature of the correlation between political attitudes and party identification in the United States. Parents’ identities are defined much more strongly by their political ideologies (connected, as they generally are, with their moral and religious world views) than by their party affiliations, and for this very reason we would expect parents to be much more concerned to transmit their political ideologies, rather than their party affiliations, to their children. For one committed to an “environmental” explanation of political ideology, what AFH present as the “ultimate challenge” ironically appears to *affirm* the “environmental” approach.

AFH claim that my referencing a study by Cooper and Zubek<sup>9</sup> that indicates the manner in which heritability of maze-running ability in mice can vary dramatically with different environments somehow undercuts my argument. First, they draw attention to the fact that I admit that the mice in the study “may well have inherited whatever genes are linked to intelligence.” I have absolutely no difficulty in acknowledging such a claim. My argument does not concern the question of the heritability of mouse intelligence, nor does it concern the heritability of human intelligence. My argument concerns the intelligibility of the proposition that *political ideologies* are heritable, a proposition radically different than the proposition that intelligence is (partially) heritable.

Second, my point in mentioning the mouse study was not to warn against overestimating the role of genetics in a given environment (as AFH interpret it), but simply to emphasize the point that high heritability *does not* imply that a given trait is resistant to environmental influence, contrary to what AFH say in their article, and that

**Figure 1**  
**2001–2004 party ID quarterly averages with key events**



Note: All data from PSRA/Newsweek polls with the following exceptions: Second quarter 2001 includes data from Pew's June News Interest Index (6/13–17/01). Third quarter 2001 includes data from Pew's July Favorability poll (7/2–12/01) and Kaiser's August Health News Index (8/2–5/01). Fourth quarter 2002 includes data from Kaiser's December Health News Index (12/6–10/02). Third quarter 2004 includes data from Pew's August Convention (8/5–10/04) and Kaiser's August Health Poll Report (8/5–8/04).

heritability itself can change dramatically with changes in the environment. According to AFH:

Inherited attitudes seem to be demonstrably different than acquired attitudes. . . [A]ttitudes higher in heritability are manifested more quickly, are more resistant to change, and increase the likelihood that people will be attracted to those who share those particular attitudes. . . To the extent that political ideologies are inherited and not learned they become more difficult to manipulate.<sup>10</sup>

Claims of this sort are indicative of a popular misunderstanding of the concept of heritability: High heritability *does not mean inevitability of phenotypic outcome*.<sup>11</sup>

In response to my contention that it seems unusual that those issues that one would associate most with social conservatism in the United States at present—abortion and gay rights—show lower levels of “heritability” than other issues, AFH respond with what can only be described as a *deus ex machina*: “Assortative mating,” based on the observation that conservatives tend to marry conservatives and liberals tend to marry liberals. Then, *proceeding on the assumption that parents are genetically similar*, AFH “correct” for assortative mating, and note that with this “correction,” abortion and gay rights come out among the top five “heritable” issues. What precisely is this supposed to show? Is not the assumption that parents are genetically similar something that must be *proved* rather than *assumed* (particularly when such an assumption allows one to manipulate the data in a manner that is more favorable to one’s hypothesis)? When discussing “assortative mating,”

AFH comment, “while some of this interspousal similarity [as determined by their answers to the 28 items on the Wilson-Patterson index] could plausibly be attributed to persuasion effects taking place after mate choice rather than assortative mating, the levels of similarity are probably too high to dismiss assortative mating entirely.”<sup>12</sup> “Probably too high” according to what standard? Once again, one must ask what standard of *scientific* evidence is being employed here?

According to AFH, I am taken with the “context bound” nature of *words*, and argue that given that *words* like “liberalism” have no meaning for much of the world, liberalism cannot be genetic. This is a parody of my argument, which has nothing to do with *words*, but rather with *concepts* (or “attitudes”), and more specifically, the clusters of complex concepts that comprise a political ideology (and what these clusters of concepts are named makes little difference). When AFH assert, erroneously, that “the package of attitudes held, for example, by conservatives in the United States is remarkably similar to that held by conservatives in other cultures and at earlier times in American history,”<sup>13</sup> they discuss precisely what I discuss. Perhaps they believe, in line with their claims concerning the trans-cultural and trans-historical nature of liberalism and conservatism, that all that has ever changed historically regarding these two ideologies (from their origins to the present) is their *names*.

AFH assert that “no scientifically literate person in this day and age can claim that genes are irrelevant to human

behavior and predispositions.”<sup>14</sup> Limiting what I say to *behavior*, AFH are absolutely right, and nowhere do I make such a preposterous claim. But the problem is that as diffuse a term as “behavior” could cover everything from manual dexterity and the use of language to a belief in the doctrine of the Trinity and preferring the Yankees to the New York Mets. My argument does not concern the question of the “heritability” of everything that could possibly fall under the rubric of “human behavior”; it concerns the assumption that specific political ideologies could be genetically transmitted.

The quoted sentence continues as follows: “yet many people are deeply discomfited by this reality” (similarly, Hannagan and Hatemi assert I am “threatened” by genetic explanations of political ideologies). I find the ascription of psychological motives to me by AFH (as well as Hannagan and Hatemi) an extremely tedious (and somewhat adolescent) form of *ad hominem* arguing. It is easy to play at such a silly game; e.g., many authoritarian personality types have an overwhelming fear of lack of uniformity, multiplicity of explanations, “*contextualism*” (the need to consider particular cultural and historical contexts) and “irreducibility,” and hence are drawn to simplistic, reductionist, “*absolutist*” explanations in which all human phenomena can be reduced to a single, uniform, explanatory variable (e.g., genes, self-interest, rational choice, “economic rationality,” class struggle, God’s plan for humankind). I ascribe no such underlying psychological motives to AFH or to Hannagan and Hatemi, and I would appreciate it if they would return the favor.

According to AFH,

casting the issue as genes competing with the environment, as Charney does in his conclusion (“if genes count for more than environment the phenomena of liberalism and conservatism . . . become utterly incomprehensible”) is silly and misses the point. What we claim is that genes are important to political thought and behavior.<sup>15</sup>

But all that I was doing was summarizing the authors’ own assertions, e.g.,

setting aside the important special case of party identification, we find that political attitudes are influenced much more heavily by genetics than by parental socialization. For the overall index of political conservatism, genetics accounts for approximately half of the variance in ideology, while shared environment including parental influence accounts for only 11%. And in the case of the variance in people’s tendencies to possess political opinions at all, regardless of their ideological direction, genetics explains one-third of the variance, and shared environment is completely inconsequential.”<sup>16</sup>

If AFH find such assertions silly, then we are in complete agreement.

Finally, AFH (and Hannagan and Hatemi) are at great pains to portray me as someone without any real under-

standing of advances in modern genetics. I will simply point out that, as noted in my article, the section that deals specifically with genetics and the methodology of twin studies presents arguments that *are not my own* (I cannot claim credit for them, as much as I would like to). Rather, they are the current arguments of some of the most prominent living geneticists, neuroscientists, and medical researchers (some of whom have provided invaluable guidance), individuals such as Richard Lewontin, Douglas Wahlstein, Jonathan Beckwith, and Annette Karmiloff-Smith.

This “celebrity appeal” is not intended to resolve any controversies, but it is intended to highlight the following: The strongest critiques of the methodology of twin studies (as well as the understanding of heritability on which they rely) at the present time come from prominent geneticists, biologists, neurologists, and medical researchers—*not*, obviously, from political scientists, and *not* from psychologists (with a few notable exceptions). There exists no consensus on these matters among *scientific experts in genetics* engaged in cutting-edge research (among whom neither I, nor Alford, nor Funk, nor Hibbing, nor Hannagan, nor Hatemi can be counted). Any claim to the contrary is manifestly false, and if I succeed in conveying nothing more than this reality to the political science community I will be satisfied that I have accomplished a great deal.

A strict limitation on space will not allow a detailed response to the comments of Hannagan and Hatemi, but inasmuch as they make many of the same claims as AFH in their response, I shall limit myself to two comments.

Hannagan and Hatemi (HH) place much emphasis upon the statistical technique of structural equation modeling (SEM). Advances in statistical methodology can bring with them significant advances in scientific understanding, but what they cannot do is transform a foundationally flawed empirical research technique into a sound one. Let me emphasize the following: As noted in my article, twin studies that employ SEM *rely every bit as much as older twin studies*—and the study of AFH—on the equal environment assumption, unbiased samples, and accurate measurements of the phenomenon being studied. None of the objections raised to the EEA are answered, obviated, or rendered moot by SEM.

The second objection of HH that I would like to address I consider much more interesting: It is that my objections to AFH are not “scientific,” but rather “philosophical,” and that I illegitimately “critique an empirical work based on the philosophical rejection of the scientific method.” To point out the flaws in a supposedly scientific methodology (twin studies), to point out that it fails to meet the rigorous criteria of scientific knowledge, is hardly to reject

the scientific method, but rather to uphold it. I do not believe that what AFH and HH are doing is science, for all the trappings of science they employ, e.g., an empirical study, the collection of data, analysis of the data using statistical methods. I could perform the exact same study as AFH using a different questionnaire and claim to have determined what percentage of an individual's belief concerning the doctrine of the Trinity is due to genes and what percentage to environment—or to what extent whether one favors the New York Yankees or the Boston Redsocks, or Mercedes or BMWs, or Lowes or Home Depot—is “heritable.”

A twin study which asked MZ and DZ twins whether they shopped at Macy's department store would very likely reveal that this “trait” was partially “heritable” (because MZ twins have greater contact throughout life than DZ twins and tend to live closer to one another, they are more likely to shop at the same stores than DZ twins). Researchers, blind to the more obvious explanations for their findings, might propose a “Macy's gene.” More sophisticated researchers might propose a Macy's personality type, correlated with the phenotype of shopping at Macy's. The results of such a study would doubtless generate spectacular news headlines (and be a godsend as a marketing tool), but if taken seriously would indicate nothing more than the inadequacy and crudeness of the researchers' methodology (as well as their general thinking—or lack thereof—about the plausibility of such things). But such a study would not constitute an addition to—or advance in—scientific understanding.

Parts of my argument dealt with something called “common sense” (and it makes little difference whether or not one wants to call this “philosophy”). Let us recall that in their article, AFH talk of *specific genes* for each of the specific *beliefs* that they associate with the liberal and conservative (or “contextualist” and “absolutist”) phenotypes:

Even if the individual genes involved with absolutism or contextualism tend to move together, this does not mean they always do. Some individuals may carry, say, an absolutist's aversion to out-groups but a contextualist's rejection of a universalistic behavioral code.<sup>17</sup>

To talk of a *gene coded for* the belief that universalistic behavioral codes are improper (immoral?), is like talking about a gene for one's views concerning federalism. To propose a gene coded for one's belief regarding the proper balance between states' rights and the federal government defies common sense (and I will note the extent to which AFH appear to have backed away from absurd claims of this sort in their response to me, which I take as a positive development).

Perhaps it is engaging in philosophical reasoning of a sort to point out that different kinds of explanation are appropriate to different kinds of phenomena, and it is only a misunderstanding of the phenomenon in question that allows one to seek an inappropriate explanation for it.

The same is true in science. Quantum mechanics has very little to tell us about the functioning of the human heart, and if a physicist claimed that the resolution of remaining difficulties with string theory promised greater understanding of the etiology of heart disease, we would have to conclude that he did not know what heart disease was. (Note that this phenomenon, the “irreducibility” of our scientific knowledge about the human heart to our scientific knowledge about quantum mechanics, does not mean that the heart is a mystical phenomenon, or lead to the positing of a “heart-matter dualism”<sup>18</sup>).

If I were asked, why did Napoleon lose the Battle of Waterloo, or why did the German Revolution of 1848 fail, or why did the practice of the racialized slavery of blacks begin in Europe in the sixteenth century, or why did democratic government first appear in ancient Athens, or why did the American Founders turn to ancient Rome rather than Athens for their model of government, and responded that advances in genetics held out the promise of an answer to these questions, the interlocutor would have to conclude that I had no comprehension of what history was. And note that this assertion does not turn history into some kind of mystical phenomenon, or posit a “mind-brain duality,” or any other such nonsense.

That those such as AFH do not adequately comprehend the phenomenon they are supposed to be investigating (political ideologies) is made abundantly clear by their erroneous assumptions about the trans-historical and trans-cultural nature of liberalism and conservatism as distinct political ideologies, that liberalism and conservatism are each accurately defined by the core cluster of attitudes they list as comprising the liberal and conservative “phenotypes,” that the only noteworthy historical change in these ideologies is what they are *named*, and that party identification is a fixed variable at the present time in the United States. It is only greedy reductionism, the ultimate *hubris*, that impels political scientists to so egregiously *mischaracterize* complex phenomena in order to fit them into a reductionist explanatory model (and political science has seen plenty of these in its checkered history).<sup>19</sup> In the words of the renowned geneticist, statistician, and evolutionary biologist Richard Lewontin: “It is a sign of the foolishness into which an unreflective reductionism can lead us that we seriously argue from protein similarity to political similarity.”<sup>20</sup>

AFH and HH, along with many other political and social scientists, suffer from massive confusion in failing to distinguish between the *reasons* why persons *hold* or *believe in* specific political ideologies (i.e., the answer to the question why does this individual hold the political ideology she does) and *political ideologies themselves*. Whatever the explanation as to why a given individual ultimately holds the political ideology she does (and I will simply assert that I believe the reasons to be potentially infinite, including, e.g., upbringing, emotional appeal, rebellion against one's

parents, attempts to please one’s parents, lazy and uncritical acceptance of the beliefs of those in her surrounding environment, rigorous individual critical reflection and rejection of the beliefs of those in her surrounding environment, religious convictions, anti-religious convictions, etc.), such an explanation does not tell us *what political ideologies are*, how and why they developed at certain times and places, how they were institutionalized in specific political and social practices, and how they developed and transformed over time. One might attempt to explain the “etiology” of persons’ attitudes toward the U.S. Constitution on the basis of “genetic” personality traits, and construct a “pro” and “anti” U.S. Constitution “phenotype.” But a theory that purported to explain *why* any given individual had the attitudes she did toward the U.S. Constitution would not explain *what the Constitution was*, i.e., why it was written, when, and by whom, what political principles it embodied or codified, and why.

Finally, if I engage in philosophy, so do AFH and HH, even if they are not aware of it, or of the profound philosophical assumptions concerning *knowledge* and *reason* that underlie their beliefs regarding the nature of science on the one hand, and political and moral beliefs on the other. I cannot elaborate further on these points here, but will simply end by posing a question to Hannagan and Hatemi. If, as AFH claim, my political views are due largely to my genes, for example, my views—perhaps mediated by genetic personality traits—about Social Security, the proper scope and limits of presidential power, and the war in Iraq, would HH be willing to conjecture as to what percentage of their views concerning *genetics*, e.g., the viability of twin studies, the genetic basis of political ideologies, the soundness of the Equal Environment Assumption, are due to *their* genes. Might they undertake a twin study to answer this question? And if not, *why*?

## Notes

The renowned geneticist Jonathan Beckwith, American Cancer Society Research Professor of Microbiology and Molecular Genetics at Harvard Medical School, has written a response to Alford, Funk, and Hibbing and Hannagan and Hatemi which will appear in a forthcoming issue of *Perspectives on Politics*. His contribution is intended to supplement my response, providing the unique perspective of a *geneticist* in a debate that has been dominated by political scientists and psychologists.

- 1 “Greedy reductionism” is a term coined by Daniel Dennett (1995, 82) to condemn those forms of reductionism that try to explain too much with too little. Use of this expression is not meant to imply an endorsement of Dennett’s thesis.
- 2 AFH 2008, 322.
- 3 Bouchard et al. 1990.

- 4 Charney 2008, 338.
- 5 Imagine if the validity of the theory of relativity rested upon the accounts of Einstein’s mother as to how she raised baby Einstein.
- 6 AFH 2008, 322.
- 7 Brody and Rothenberg 1988.
- 8 For both of these points, see, e.g., Carsey and Layman 2006; Lockerbie 2002; Weisberg 1980, 2002; Brody and Rothenberg 1988; Mutz, Brody, and Sniderman 1996; Norrander and Wilcox 1993; Converse and Pierce 1992; Franklin 1992, 1984; Niemi, Reed, and Weisberg 1991; Franklin and Jackson 1983; Fiorina 1981; Markus and Converse 1979.
- 9 Cooper and Zubek 1958.
- 10 AFH 2005, 164.
- 11 See, e.g., Bailey 1997.
- 12 AFH 2005, 161.
- 13 AFH 2005, 164. If it is not already obvious, the impetus to mischaracterize political ideologies in this manner comes from the desire to fit them wholly within a reductionist genetic explanatory framework that effectively bypasses history and culture. Just as, e.g., the phenotype of hazel eyes, the result of a corresponding genotype, is the same in all times and all places (and can be characterized apart from any specific cultural and historical context), so too, according to AFH, with political ideologies.
- 14 AFH 2008, 325.
- 15 Ibid.
- 16 AFH 2005, 164.
- 17 Ibid., 165.
- 18 Neils Bohr was explicit that his denial of the reducibility of biology to quantum mechanics had no implications for “free will or determinism,” and did not involve a mysticism incompatible with the true spirit of science”; see Bohr 1936, 299.
- 19 Many kinds of “hubris” are exhibited in an article such as AFH’s. Let me note just one other characteristic “ethnocentric hubris”: In characterizing contemporary American liberalism and conservatism as if they were the templates not only of liberalism and conservatism in all of its varied historical and cultural manifestations, but of *all* political ideologies in *all times and places*, they equate the beliefs and behavior of contemporary Americans with the beliefs and behavior of “humankind.” The genetic underpinning they give to such assumptions makes contemporary Americans the paradigm for humanity as a biological species. As a helpful corrective to such hubristic provincialism, I suggest that AFH travel to the Amazon rain forest and undertake an extensive study of the political attitudes of the remaining indigenous tribes.
- 20 Lewontin 2001, 62.

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## References

- Alford, J.R., C.L. Funk, and J. Hibbing. 2005. Are political orientations genetically transmitted? *American Political Science Review* 99 (2): 153–67.
- . 2008. Beyond liberals and conservatives to political genotypes and phenotypes. *Perspectives on Politics* 6 (2): 321–28.
- Bailey, R.C. 1997. Hereditarian Scientific Fallacies. *Genetica* 99: 125–33.
- Bohr, Neils. 1936. Causality and Complementarity. *Philosophy of Science* 4. Address to Second International Congress for the Unity of Science, June, 1936.
- Bouchard, T.J. Jr., D. T. Lykken, M. McGue, N. L. Segal, and A. Tellegen. 1990. Sources of human psychological differences: the Minnesota study of twins raised apart. *Science* 250: 223–28.
- Brody, Richard A., and Lawrence S. Rothenberg. 1988. The instability of partisanship: An analysis of the 1980 presidential election. *British Journal of Political Science* 18 (4): 445–65.
- Carsey, Thomas M., and Geoffrey C. Layman. 2006. Changing sides or changing minds? Party conversion, issue conversion, and partisan change on the abortion issue. *American Journal of Political Science* 50 (2): 464–77.
- Charney, Evan. 2008. Genes and ideologies. *Perspectives on Politics* 6 (2): 299–319.
- Converse, Philip E., and Roy Pierce. 1992. Partisanship and the party system. *Political Behavior* 14 (3): 239–59.
- Cooper, R.M., and J.P. Zubek. 1958. Effects of enriched and restricted early environments on the learning ability of bright and dull rats. *Canadian Journal of Psychology* 12: 159–64.
- Dennett, Daniel. 1995. *Darwin's Dangerous Idea*. London: Penguin.
- Fiorina, Morris P. 1981. *Retrospective Voting in American National Elections*. New Haven: Yale University Press.
- Franklin, Charles H. 1992. Measurement and the dynamics of party identification. *Political Behavior* 14 (3): 297–309.
- . 1984. Issue preferences, socialization, and the evolution of party identification. *American Journal of Political Science* 28 (3): 459–78.
- Franklin, Charles H., and John E. Jackson. 1983. The dynamics of party identification. *American Political Science Review* 77 (4): 957–73.
- Lewontin, Richard. 2001. *It Ain't Necessarily So: The Dream of the Human Genome and Other Illusions*. New York: New York Review Books.
- Lockerbie, Brad. 2002. Party identification: Constancy and change. *American Politics Research* 30 (4): 384–405.
- Markus, Gregory B., and Philip E. Converse. 1979. A dynamic simultaneous equation model of electoral choice. *American Political Science Review* 73 (4): 1055–70.
- Mutz, Diana C., Richard A. Brody, and Paul M. Sniderman, eds. 1996. *Political Persuasion and Attitude Change*. Ann Arbor, MI: University of Michigan Press.
- Niemi, Richard G., David R. Reed, and Herbert F. Weisberg. 1991. Partisan commitment: A research note. *Political Behavior* 13 (3): 213–21.
- Norrander, Barbara, and Clyde Wilcox. 1993. Rallying around the flag and partisan change: The case of the Persian Gulf War. *Political Research Quarterly* 46 (4): 759–70.
- Weisberg, Herbert F. 1980. A multidimensional conceptualization of party identification. *Political Behavior* 2 (1): 33–60.
- . 2002. Partisanship and incumbency in presidential elections. *Political Behavior* 24 (4): 339–60.