

# WHY EVOLUTIONARY BIOLOGY IS (SO FAR) IRRELEVANT TO LAW

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*We argue that as the actual science stands today, evolutionary biology offers nothing to help with questions about legal regulation of behavior. Evolutionary accounts are etiological accounts of how a trait evolved. We argue that an account of causal etiology could be relevant to law if (1) the account of causal etiology is scientifically well-confirmed, and (2) there is an explanation of how the well-confirmed etiology bears on questions of development (what we call the Environmental Gap Objection). We then show that the accounts of causal etiology that might be relevant are not remotely well-confirmed by scientific standards. We argue, in particular, that (a) evolutionary psychology is not entitled to assume selectionist accounts of human behaviors, (b) the assumptions necessary for the selectionist accounts to be true are not warranted by standard criteria for theory choice, and (c) only confusions about levels of explanation of human behavior create the appearance that understanding the biology of behavior is important. We also note that no response to the Environmental Gap Objection has been proffered. In the concluding section of the article, we turn directly to the work of Professor Owen Jones, a leading proponent of the relevance of evolutionary biology to law, and show that he does not come to terms with any of the fundamental problems identified in this article.*

## Contents

- I. Introduction: The Next Interdisciplinary Wave?
- II. Why is the Causal Etiology of Behavior Relevant to Law?
  - a. Problems for the Plasticity of Behavior Rationale
  - b. Problems for the Discovery Rationale
- III. Does the Existing Science Support the Preferred Etiological Accounts?
  - a. There's More to Evolution than Natural Selection. A Look at the Actual Biology

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- b. Best Explanations, or the Problem of Ontological Parsimony
- c. Why it Matters whether Natural Selection is at Work
- d. Levels of Explanation
- IV. Professor Jones's Defense of the Relevance of Evolutionary Biology to Law
  - a. Is There a "Human Behavioral Biology"?
  - b. Evolution and Natural Selection
  - c. The Case of Rape
- V. Conclusion

## I. Introduction: The Next Interdisciplinary Wave?

Evolutionary biology—or, more precisely, two (purported) applications of Darwin's theory of evolution by natural selection, namely, evolutionary psychology and what has been called "human behavioral biology"<sup>1</sup>—is on the cusp of becoming the new rage among legal scholars looking for "interdisciplinary" insights into the law.<sup>2</sup> Will

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<sup>1</sup> Owen Jones, *Evolutionary Analysis in Law: Some Objections Considered*, 67 Brooklyn L. Rev. 207, 209 (2001). We shall use the terms interchangeably throughout much of the discussion, though Professor Jones prefers "behavioral biology," though more for rhetorical than scientific reasons, as we discuss in Part IV. In fact, the only candidates for *human* behavioral biology that figure in the legal literature turn out to be hypotheses commonly associated with evolutionary psychology.

The term 'evolutionary psychology' is, itself, used in both broad and narrow senses. In the broad sense, it refers to any study of human cognition from an evolutionary perspective. Under this usage, Darwin himself was an evolutionary psychologist. The term also has a more narrow denotation, describing a group of researchers including Professors Leda Cosmides, John Tooby, David Buss, and their students and allies. The narrow sense is sometimes called the "Santa Barbara School" of evolutionary psychology. These researchers defend a picture of the human mind that is composed of a large number of modules, each acted on individually by natural selection. The legal scholars discussed in this article seem primarily interested in the narrow sense of the term, hence we will use it in this way as well.

<sup>2</sup> See, e.g., E. Donald Elliott, *Law and Biology: The New Synthesis?* 41 St. Louis U. L. J. 595 (1997); Owen D. Jones, *Evolutionary Analysis in Law: An Introduction and Application to Child Abuse*, 75 N.C. L. Rev. 1117 (1997) [hereafter Jones, *Child Abuse*]; Kingsley R. Browne, *An Evolutionary Perspective on Sexual Harassment: Seeking Roots in Biology Rather than Ideology*, 8 J. Contemp. Legal Issues 5 (1997); John O. McGinnis, *The Human Constitution and Constitutive Law*, 8 J. Contemp. Legal Issues 211 (1997); Brian Kennan, *Evolutionary Biology and Strict Liability for Rape*, 22 Law & Psychol. Rev. 131 (1998); Charles C. Crawford & Marc A. Johnston, *An Evolutionary Model of Courtship and Mating as Social Exchange: Implications for Rape Law Reform*, 39 Jurimetrics J. 181 (1999); Martin L. Lalumiere & Vernon L. Quinsey, *A Darwinian Interpretation of Individual Differences in Male Propensity for Sexual Aggression*, 39 Jurimetrics J. 201 (1999); Deborah W. Denno, *Evolutionary Biology and Rape*, 39 Jurimetrics J. 243 (1999); Cheryl Hanna, *Sometimes Sex Matters: Reflections on Biology, Sexual Aggression, and Its Implications for the Law*, 39 Jurimetrics J. 261 (1999); Owen D. Jones, *Sex, Culture, and the Biology of Rape: Toward Explanation and Prevention*, 87 Cal. L. Rev. 827 (1999) [hereafter Jones, *Biology of Rape*]; Martin Daly & Margo Wilson, *Family Violence: An Evolutionary Psychological Account*, 8 Va. J. Soc. Pol'y & L. 77 (2000); John Monahan, *Could Evolutionary Biology be the Next Law and Economics?*, 8 Va. J. Soc. Pol'y & L. 123 (2000); Owen D. Jones, *Law and the Biology of Rape: Reflections on Transitions*, \_\_\_ Hast. Women's L.J. 151 (2000) [hereafter, Jones, *Reflections*]; Russell

“law and evolutionary biology” have the lasting power and impact of, say, law and economics, or will it go the way of deconstructionism and Critical Legal Studies (CLS), both of which faded from the scene in roughly a decade or less? We shall argue that, if the scientific merits have anything to do with it, the “law and evolutionary biology” fad should have a shelf life at least as short as deconstruction’s.

Indeed, with evolutionary biology, we may already have hit the excesses to which the legal academy is sometimes prone. Professor John McGinnis of Northwestern University School of Law, for example, tells us that evolutionary biology supports the vision of human nature accepted by the Founding Fathers,<sup>3</sup> a claim so preposterous that it could not actually be uttered in front of anyone who knew anything about the relevant science.<sup>4</sup> Examples like this, alas, could be multiplied.<sup>5</sup> Not since the heyday of the

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Korobkin, *A Multi-Disciplinary Approach to Legal Scholarship: Economics, Behavioral Economics, and Evolutionary Psychology*, 41 *Jurimetrics J.* 319 (2001); Jeffrey Evans Stake, *Comment: Can Evolutionary Science Contribute to Discussions of Law?*, 41 *Jurimetrics J.* 379 (2001); Jeffrey Evans Stake, *Pushing Evolutionary Analysis of Law*, 53 *Fla. L. Rev.* 875 (2001); Erin Ann O’Hara, *Brain Plasticity and Spanish Moss in Biolegal Analysis*, 53 *Fla. L. Rev.* 905 (2001); Erin Ann O’Hara & Douglas Yarn, *On Apology and Consilience*, 77 *Wash. L. Rev.* 1121 (2002); Lindsay A. Elkins, *Five Foot Two with Eyes of Blue: Physical Profiling and the Prospect of a Genetics-Based Criminal Justice System*, 17 *Notre Dame J.L. Ethics & Pub. Pol’y* 269 (2003); Christopher Marlborough, *Evolution, Child Abuse and the Constitution*, 11 *J.L. & Pol’y* 687 (2003); Erin Ann O’Hara, *Apology and Thick Trust: What Spouse Abusers and Negligent Doctors Might Have in Common*, 79 *Chi.-Kent L. Rev.* 1055 (2004); Neel P. Parekh, *When Nice Guys Finish First: The Evolution of Cooperation, The Study of Law, and the Ordering of Legal Regimes*, 37 *U. Mich. J.L. Reform* 909 (2004); Morris B. Hoffman & Timothy H. Goldsmith, *The Biological Roots of Punishment*, 1 *Ohio St. J. Crim. L.* 627 (2004); Bailey Kuklin, *Evolution, Politics and Law*, 38 *Val. U. L. Rev.* 1129 (2004); Owen D. Jones & Timothy H. Goldsmith, *Law and Behavioral Biology*, 105 *Colum. L. Rev.* 405 (2005) [hereafter Jones & Goldsmith].

<sup>3</sup> John McGinnis, *The Original Constitution and Our Origins*, 19 *Harv. J. L. & Pub. Pol.* 251 (1996).

<sup>4</sup> Most evolutionists and philosophers of biology believe that traditional notions of “human nature” cannot be made consistent with or grounded in evolutionary accounts of human populations. The *locus classicus* of these points is Ernst Mayr’s work, especially *ANIMAL SPECIES AND EVOLUTION ( ) AND Typological versus Population Thinking* [citation]. Further elaboration of the view can be found in David Hull, *On Human Nature*, in *PHILOSOPHY OF BIOLOGY* 383 (D. Hull & M. Ruse eds., 1998). A more radical critique of notions of evolutionary accounts of human nature can be found in JOHN DUPRE, *THE DISORDER OF THINGS* (1993). Philip Kitcher’s essay *Essence and Perfection*, 110 *ETHICS* 59 (1999) contains a very lucid discussion of the complexities of this issue.

<sup>5</sup> So, e.g., Professor Todd Zywicki of George Mason University accuses “the left” of “hav[ing] their own ‘religious’ beliefs when it comes to scientific questions,” just like the religious right that attacks

Critical Legal Studies infatuation with Richard Rorty and Thomas Kuhn (not to mention Nietzsche!) has another discipline been so mangled in the pages of the law reviews.<sup>6</sup> And as with this last interdisciplinary disaster, the purveyors are typically politically motivated (though this is, so far, a fad of the right, not the left.)

We shall argue that as the *actual* science stands today, evolutionary biology offers nothing to law—more precisely, it offers nothing to help with questions about legal regulation of behavior, which shall be our focus here. Only systematic misrepresentations or lack of understanding of the relevant biology, together with far-reaching analytical and philosophical confusions, have led anyone to think otherwise. Professor Owen Jones of Vanderbilt University School of Law has been the most aggressive and systematic purveyor of the misrepresentations and confusions (though not, it appears, for any political reasons), so his work will be a major target of the analysis that follows.

On the face of things, law and evolutionary biology seem to be mismatched from the start. Evolutionary accounts are *etioloical* accounts of how a trait *evolved*. Why should law care about how behavioral traits evolved? Law must be concerned, to be sure, with what people do and what they are likely to do; but why should law care about the *genesis* of behavior?

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evolution by natural selection. Professor Zywicki's evidence for this bizarre claim is resistance to selectionist explanations of mental traits, especially concerning differences between the sexes. See [http://www.volokh.com/archives/archive\\_2005\\_07\\_03-2005\\_07\\_09.shtml#1120775700](http://www.volokh.com/archives/archive_2005_07_03-2005_07_09.shtml#1120775700). Unfortunately for Professor Zywicki, while it is not reasonable, given what we know, to express doubts about Darwin's theory of evolution by natural selection, it is extremely reasonable, given what we know, to express doubts about evolutionary psychology and its selectionist hypotheses about certain cognitive and mental differences between the human sexes. That Professor Zywicki thinks these claims are on an epistemic par just reveals that he has no notion of the actual state of the scientific evidence or of the relevant scientific standards for confirming hypotheses.

<sup>6</sup> See Brian Leiter, *Intellectual Voyeurism in Legal Scholarship*, 4 YALE J. L. & HUM. 79 (1992).

We can break this general question into two more precise questions:

(1) how can an account of the causal etiology of behavior be relevant for purposes of legal regulation?

and, assuming a satisfactory answer to the first question,

(2) does the existing science support the proffered accounts of causal etiology?

We shall argue that the causal etiology of behavior *can*, in principle, be relevant to choices about legal regulation, but that those who purport to offer such accounts of causal etiology are not entitled to an affirmative answer to the second question.<sup>7</sup> And without a defensible answer to the second question, the affirmative answer to the first is merely a promissory note safely ignored by law.

In Part II of the article we identify two grounds for thinking the causal etiology of behavior *might* be relevant to questions of legal regulation—what we call the “Plasticity of Behavior Rationale” and “the Discovery Rationale”—and then survey the various problems that afflict these rationales. We conclude that an account of causal etiology *could* be relevant to law if (1) the account of causal etiology is scientifically well-confirmed, and (2) there is an explanation of how the well-confirmed etiology bears on questions of *development* (what we call “the Environmental Gap Objection”).

In Part III, we show that the accounts of causal etiology that *might* be relevant based on the Plasticity of Behavior Rationale are not remotely well-confirmed by scientific standards. We argue, in particular, that (1) evolutionary psychology is not entitled to assume selectionist accounts of human behaviors, (2) the assumptions

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<sup>7</sup> Different critical issues are raised in Amy Wax, *Evolution and the Bounds of Human Nature*, 23 *Law & Phil.* 527 (2004), though Wax sees more hope in extending extant scientific analyses of human behavior to legally relevant questions than we do.

necessary for the selectionist accounts to be true are not warranted by standard criteria for theory choice, including even the criteria that Professor Jones himself recognizes, and (3) only confusions about levels of explanation of human behavior create the appearance that understanding the biology of behavior is important.

Part IV turns directly to the work of Professor Jones, and shows that he does not come to terms with the fundamental problems afflicting the application of evolutionary biology to law (those detailed in Parts II and III). Although Professor Jones generally adopts a high-handed pose of “taking science seriously,” we argue that, in fact, his work to date has instead obscured the absence of any serious science in support of his conclusions.

We should emphasize at the start three important points about the argument of this paper. First, we are not defending the view that empirical science is irrelevant to the law; indeed, our view is exactly the opposite.<sup>8</sup> The problem here, we shall argue, is that there is no credible science doing any work in the “evolutionary biology and law” fad. Second, we are not denying that human beings and the human brain are products of evolutionary mechanisms, including natural selection. Only the “creationists” deny this, and we are resolute in our opposition to all forms of creationism.<sup>9</sup> That human beings

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<sup>8</sup> See, e.g., Ronald J. Allen & Brian Leiter, *Naturalized Epistemology and the Law of Evidence*, 86 Va. L. Rev. 1491 (2001); Brian Leiter, *Rethinking Legal Realism: Toward a Naturalized Jurisprudence*, 76 Tex. L. Rev. 267 (1997).

<sup>9</sup> We shall use “creationists” in what follows as a term of art to mean both those committed to the literal truth of the Book of Genesis as well as the proponents of “Intelligent Design.” Intelligent Design is simply creationism for those who have consulted a lawyer and a public relations firm; it has nothing to do with science, since the criticisms of evolutionary biology are without merit and the positing of “intelligent design” as an explanatory hypothesis is on a par, epistemically, with the positing of turtles on whose back the universe rests. The Intelligent Design Creationists have stated no empirically testable hypothesis, and posit mechanisms that satisfy none of the standard desiderata of scientific theory construction, like ontological parsimony or methodological conservatism. See, e.g. ROBERT T. PENNOCK, *TOWER OF BABEL: THE EVIDENCE AGAINST THE NEW CREATIONISM* (1999); Philip Kitcher, *Born Again Creationism*, in *INTELLIGENT DESIGN CREATIONISM AND ITS CRITICS* 257 (R. Pennock ed.

and the human brain are products of evolution, however, is irrelevant for law in the absence of sound evolutionary science underwriting specific propositions about the nature and character of humans and their brains. On that latter score, the recent “evolutionary biology and law” fad has come up short.

Third, and perhaps most importantly, in asking whether there is any sound science supporting the claims made about law on the basis of “human behavioral biology” and evolutionary psychology, we are simply holding the proponents of the relevance of evolutionary biology to law to the very standards they profess to adopt.<sup>10</sup> It is familiar, to be sure, that in many domains of legal regulation, rules and policy are made on the basis of far weaker standards than “well-confirmed” or “sound” science. To take one well-known example, the Environmental Protection Agency accepts as a default hypothesis that, when it comes to toxicity, animal studies are adequate for determining toxicity in humans, even though it is increasingly clear that the hypothesis is not supported by the evidence.<sup>11</sup> The reasons for applying such relaxed scientific standards in these and similar contexts is clear enough: risk aversion encourages making rules and policy based on even weak evidence about harms. (If it is toxic in animals, it *might* be toxic in humans, and we don’t ordinarily want to take that chance—or so the argument would go.) It is less clear such an argument will be available to the proponents of law and evolutionary psychology, since many of their arguments promise to increase exposure to risk (especially for criminal defendants or those suspected of crimes), not reduce it, and in ways that are in obvious tension with the presumptions and burdens of proof operative

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2002); BARBARA FORREST & PAUL R. GROSS, CREATIONISM’S TROJAN HORSE: THE WEDGE OF INTELLIGENT DESIGN (2004).

<sup>10</sup> See *infra* Part IV, pp. \_\_\_-\_\_\_.

in the criminal courts. We shall return to this point later, but here it is simply worth emphasizing that, to date, proponents of the relevance of evolutionary biology and law have made their case on the grounds that they are relying on good science. Perhaps, if our argument has its desired effect, they will have to change their argumentative strategy in the future and admit that they are simply proponents of basing policy on interesting, but unconfirmed, speculation. We will raise some questions in what follows as to whether that move is likely to be persuasive.

## II. Why is the Causal Etiology of Behavior Relevant to Law?

Evolutionary accounts of human behavior are accounts of the *causal etiology of behavior*, that is, they purport to tell us what the causal origin is of some pattern of behavior we see before us. Among the most interesting accounts of such behaviors are ones which claim that there was strong *selection* pressure for a particular trait, making such traits *adaptations*, i.e., traits that enhance reproductive fitness. Consider a selectionist account of rape, like that which has found some favor among those who think evolutionary biology relevant to law.<sup>12</sup> Rape seems to be an inexplicable human behavior until we understand its etiology, so the argument goes. At an earlier stage of human evolution, what evolutionary psychologists call “the environment of evolutionary adaptation” (EEA),<sup>13</sup> human males with a greater propensity to rape females left more offspring. The tendency to rape females is taken by these evolutionary psychologists to be a heritable trait. Since raping behavior leads to more offspring, the genetic disposition

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<sup>11</sup> See Committee on the Institutional Means for Assessment of Risks to Public Health, National Research Council, *Risk Assessment in the Federal Government: Managing the Process* 24-27 (1983).

<sup>12</sup> See, e.g., RANDY THORNHILL & CRAIG PALMER, *A NATURAL HISTORY OF RAPE* (2000); Jones, *Biology of Rape*, *supra* n. \_\_.

<sup>13</sup> Leda Cosmides and John Tooby, *Evolutionary Psychology Primer*, <http://www.psych.ucsb.edu/research/cep/primer.html>

to rape will spread through the population. Thus the present day frequency of rape behavior in the population is the result of natural selection.<sup>14</sup>

In Parts III and IV, we will discuss why such arguments should be treated with a great deal of skepticism, but for now we want to ask a different question: namely, even if the etiological accounts are correct, what could be the relevance of such an etiology of behavior to questions of legal regulation? Scholars have thought etiology relevant for two primary reasons. The first pertains to what we will call the plasticity of behavior; the second to the possible discovery of legally relevant pathological behaviors.

First, for the purposes of legal regulation, it is important to know how *plastic* a behavior is (how susceptible to modification), and thus how difficult it would be to change it through legal incentives and disincentives. We shall refer to this as the “Plasticity of Behavior Rationale” for inquiries into the etiology of behavior.<sup>15</sup> Notice that there are two ways law might try to alter a behavior: either by (a) regulating the *cause* of the behavior, or (b) regulating the *behavior* itself. Thus, legal regulators have reason to be interested in both the cause of a non-plastic behavior (its evolutionary origin and developmental pathways, for example) and the behavior itself. It is important to

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<sup>14</sup> More sophisticated scenarios are possible as well. For example, one could argue that the disposition to rape is part of a *mixed strategy* where some small fraction of the population possess the disposition. Of course in this scenario, it is unlikely that the raping disposition could be of much interest to legal regulation because it would affect a very small fraction of the population.

<sup>15</sup> The term ‘plasticity’ will be used as shorthand for *phenotypic plasticity*. (cf. D. Godron, *Phenotypic Plasticity*, in *KEYWORDS IN EVOLUTIONARY BIOLOGY* (E.F. Keller & E. Lloyd eds. 1992). ‘Phenotypic’ refers to observable traits such as height, hair color, and behaviors, almost all of which arise from complicated interactions between genetic and environmental factors. When the variation in a phenotypic trait across a population is largely determined by environmental factors, biologists say that the trait exhibits phenotypic plasticity. Thus, our expression ‘plasticity of behavior’ refers to variation in behavior that is not the result of genetic variation. Phenotypic plasticity is a useful concept in this context because it can be quantified. Highly non-plastic behaviors, i.e. ones whose variation is largely accounted for by genetic factors, are similar to what are often called in legal academic parlance ‘sticky behaviors.’ Such behaviors would be difficult to change by environmental intervention or education and this may be relevant for legal regulation. We prefer the biologists’ terminology because it is quantitative, measurable,

remember, though, that there can be behaviors for which there was strong selection pressure in the past, which are nevertheless plastic. In a recent review article, for example, Anurag Agrawal discusses twenty seven kinds of phenotypic plasticity just among traits involving species interactions.<sup>16</sup> Many of these traits such as stem elongation in plants, trophic specialization in tadpoles, transformation of a prey species into the parasite of a predator, and adjustment of progeny size and quality may well be adaptations (the result of natural selection). Finally, behaviors can also be non-plastic without there being a causal etiology that involved natural selection. For example, those addicted to crack cocaine may, indeed, be strongly disposed to certain behaviors, but the cause of that behavior does not appear to have an evolutionary origin.

There is, however, a second possible reason for legal regulators to be concerned with the etiology of behavior, namely, that understanding the etiology will help us discover and identify non-plastic behaviors that might otherwise escape our attention.<sup>17</sup> We will call this “the Discovery Rationale” for being interested in evolutionary accounts of behavior.

Each Rationale confronts its own set of difficulties.

*A. Problems for the Plasticity of Behavior Rationale*

Four problems afflict the Plasticity of Behavior Rationale.

First, though least importantly, empirical observations of the right kind would suffice to establish that a behavior is not very plastic and thus would be difficult to change by

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and is defined in terms of the variation in populations, the proper locus of any serious discussion of evolution.

<sup>16</sup> Anurag A. Agrawal, *Phenotypic Plasticity in the Interactions and Evolution of Species*, 294/5541 *SCIENCE* 321 (2001).

<sup>17</sup> See, e.g., Jones & Goldsmith, *supra* n. 2 at 432 (“Behavioral biology is one source of comparatively untapped insights that can reveal patterns of behavior useful to law”).

altering the environment, the structure of society, education, etc. Such empirical observations might include finding the behavior cross-culturally, cross-temporally, and under different regimes of regulation. Behavior that persists under these differing conditions may be best explained as non-plastic, and thus how such behaviors evolved would not matter. Professor John Monahan, for example, is impressed that evolutionary psychologists are better able to realize, in the statutory rape context, that “one gender is at vastly higher risk of being sexually victimized than the other.”<sup>18</sup> Yet surely ordinary empirical observations of the sort conducted by social psychologists, sociologists, and public health specialists suffices for recognizing that!<sup>19</sup>

Of course, the fact that non-plasticity might be confirmed by observations of the right kind does not show that an evolutionary account might not also establish non-plasticity: that etiology is not *necessary* does not mean it is not useful because *sufficient*. The next three problems speak to the question of sufficiency.

The second problem is more fundamental. The ability of an etiological account to establish that a trait is *developmentally canalized* (i.e., there will be little phenotypic variation despite environmental variation) is entirely a function of whether the account of etiology is scientifically well-established. Although taken seriously in some circles, the accounts of the evolutionary origins of human behavior offered by evolutionary psychologists (and those legal scholars who follow them) are not accepted by most evolutionary biologists.<sup>20</sup> The consensus view among biologists is that the etiological

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<sup>18</sup> Monahan, *supra* n. 2 at 128.

<sup>19</sup> One suspects court records on statutory rape prosecutions and convictions from any jurisdiction would support the same conclusion.

<sup>20</sup> As Elisabeth Lloyd, a leading historian and philosopher of biology, has observed: “these authors [i.e., the proponents of evolutionary psychology] are considered a fringe group by most evolutionary theorists. This group repeatedly demonstrates its narrow understanding of evolutionary

accounts offered by evolutionary psychologists fall far short of the standards of evidence required to demonstrate such evolutionary hypotheses.<sup>21</sup> One indication of this consensus is that evolutionary psychologists do not publish in evolutionary biology journals. Another is the impoverished perspective evolutionary psychologists take about evolutionary mechanisms and evolutionary theory compared to those current in actual biological research. In a review of the theoretical apparatus employed by evolutionary psychologists, for example, Professors Feldman and Lloyd criticize evolutionary psychologists for focusing their discussions exclusively on selfish-gene and inclusive fitness models:

The immediate problem with [evolutionary psychologists'] representation of evolutionary biology is that inclusive fitness theory comprises a small subset of models used for special purposes in evolutionary understanding ... There are many other components of evolutionary analysis that address both animal and human behavior, and although they are conceptually more intricate than inclusive fitness theory, they may be more appropriate for the exploration of human psychology.<sup>22</sup>

Lloyd and Feldman argue that such a narrow focus is *not* a harmless idealization. For example, they argue that it “focuses all attention on adaptation”<sup>23</sup> even when this is not the most appropriate representation of the evolutionary process, especially in cases of sexual or fertility selection.

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theory itself, and its misinterpretations of some elements of modern evolutionary biology; and they rarely cite mainstream evolutionary theory or genetics, either contemporary or historical.” Elisabeth A. Lloyd, *Science Gone Astray: Evolution and Rape*, 99 Mich. L. Rev. 1536, 1546 (2001).

<sup>21</sup> See the detailed discussion *infra* Part III.

<sup>22</sup> Elisabeth A. Lloyd & M.W. Feldman, *Evolutionary Psychology: A View From Evolutionary Biology*, 13 PSYCH. INQUIRY 150 (2002).

<sup>23</sup> *Id.* at 151.

Third, even a well-established account of a behavior's etiology would not speak to its universality. While natural selection can sometimes ensure a trait is represented in every organism in a population, this depends contingently on facts about the genetic system and the precise ecological interactions leading to the selection pressure in the first place. There are many well-studied instances of strong selection pressures leading to stable polymorphisms<sup>24</sup> including the stability of the sickle cell anemia allele, despite the fact that it is lethal to any human being that is homozygotic<sup>25</sup> for the allele.<sup>26</sup> Thus even demonstrating that a particular trait evolved as a result of natural selection tells us nothing definitive about the frequency of the trait in a currently existing population.

The fourth and final point concerns *development*, not evolution itself. Even if well-established, an etiological account alone cannot demonstrate that the relevant behaviors are so canalized that they cannot be altered. As Philip Kitcher and Leah Vickers have observed:

The most [evolutionary] scenarios could reveal is that there are pieces of DNA which, in the particular environments encountered by our hominid ancestors, give rise to characteristics—competitiveness, coyness, xenophobia, whatever—that

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<sup>24</sup> Polymorphism is a state of a population where multiple forms of a phenotypic trait or gene are being maintained. For example, in human populations, the fact that multiple eye colors are represented is a stable polymorphism. Eye color is probably selectively neutral, but some traits, such as sickle cell anemia are most certainly not selectively neutral, yet natural selection maintains the trait in the population.

<sup>25</sup> Human beings and other diploid organisms contain two non-identical versions of each chromosome. There are corresponding locations or *loci* on each chromosome which, taken together, are responsible for the genetic contribution of a particular phenotypic trait. The possible forms of a gene located at a particular chromosome are called *alleles*, often written using the letters *A*, *B*, *C* and so forth. Capital and lowercase letters are used to refer to alternative forms of an allele. So 'A' and 'a' may be used to refer to the two possible forms of an allele at some particular locus. When an organism has *A* or *a* at *each* locus (i.e. it is *AA* or *aa*), the organism is homozygotic. If it has *Aa* (i.e. *A* on one chromosome, *a* on the other), then the organism is *heterozygotic*.

<sup>26</sup> L.L. CAVALLI-SFORZA & W.F. BODMER, THE GENETICS OF HUMAN POPULATIONS (1971).

proved beneficial in those environments; the scenarios have no bearing on whether, under different regimes of development, those traits would be bound to arise.<sup>27</sup>

We shall refer to this as the “Environmental Gap Objection,” that is, the objection that given the difference in environment between that of our hominid ancestors and that in which we now develop, there is no *prima facie* reason—that is, no reason independent of contrary evidence—to think a trait which is adaptive in the earlier environment would manifest itself in the later one. If the Environmental Gap Objection is correct, it is fatal to the entire project of identifying non-plastic behaviors using evolutionary etiologies. (It would, of course, tell legal regulators to not bother focusing on the origins of behaviors *at all*.)

#### *B. Problem for the Discovery Rationale*

In principle, the Discovery Rationale seems plausible: if thinking about behavior in selectionist terms will help us discover non-plastic behaviors, then that is a good reason to be interested in evolutionary psychology.<sup>28</sup> Unfortunately, it turns out that, to date, there are no known instances of etiological accounts leading us to discover non-plastic behaviors that are relevant to legal regulation. The only one ever proffered—the higher incidence of infanticide by stepparents than biological parents<sup>29</sup>—tells us nothing of legal interest since the *overwhelming majority* (at least 99.98%) of stepparents don’t kill their stepchildren.

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<sup>27</sup> Philip Kitcher & Leah Vickers, *Pop Sociobiology Reborn: The Evolutionary Psychology of Sex and Violence*, in PHILIP KITCHER, IN MENDEL’S MIRROR: PHILOSOPHICAL REFLECTIONS ON BIOLOGY \_\_, \_\_ (2003).

<sup>28</sup> Mayr defends a similar position in responding to Gould and Lewontin’s critique of the “Adaptationist Programme”. He argues that looking for adaptations, but keeping an open mind that none may exist for a particular trait, is a useful way of organizing inquiry. See his *How to Carry Out the Adaptationist Program*, 121 AMER. NATURALIST 324 ( ).

Professor Jones’s treatment of this topic supplies, however, a vivid illustration of how dangerous the “evolutionary” perspective could be if it were taken seriously by lawmakers. According to the data which Professor Jones himself cites,<sup>30</sup> stepparents are anywhere from fifty to one hundred times more likely to kill their small children than biological parents. Unemphasized by Professor Jones, but decipherable from the same and related data sets, is that only a vanishingly small number of stepparents ever kill their stepchildren.<sup>31</sup> True enough, there were six hundred cases of infanticide per “million couples [with children] per annum”<sup>32</sup> in couples with one step-parent, about one hundred times the rate for couples with only natural parents. Since the figures tell us *only* about the probability of a child being killed in a home where a step-parent lives, not who killed the child, this still doesn’t tell us how many stepparents actually killed their children. On this crucial issue, Professor Jones is silent.

Figuring out exactly how many stepparents kill their stepchildren is difficult with the data sets available. However, since the relevant figures only change slightly year-to-year, with some assumptions we can make a reasonable estimate. Let us look at the year 1985 in the U.S., for which data is available. This is also a year that pre-dates the Daly and Wilson study on which Professor Jones relies. According to the U.S. Department of Justice’s Bureau of Justice Statistics, 61.8% of the cases of infanticide that year were attributable to parents and stepparents (that was actually the highest year in the period

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<sup>29</sup> See, e.g., Monahan, *supra* n. 2 at \_\_; Jones, *Child Abuse*, *supra* n. 2 at \_\_-\_\_; Jones & Goldsmith, *supra* n. 2 at 432-435

<sup>30</sup> Jones & Goldsmith, *supra* n. 2 at 434.

<sup>31</sup> Daly & Wilson, HOMICIDE \_\_-\_\_

<sup>32</sup> Jones & Goldsmith, *supra* n. 2 at 434 (434 n. 85 notes that the chart has been “modified” from Daly and Wilson, who describe this in terms of “victims per million years of co-residence”). Note also that one Colorado study found that maltreatment of a child by a step-father was nine times more likely to be reported than maltreatment by a genetic parent. [cite]

1976-2002; the lowest was 49.9%, and the average around 55%).<sup>33</sup> That year, a total of 440 children age two or younger were killed.<sup>34</sup> That means, roughly 272 small children were killed by either their parents or stepparents (the DOJ figures lump those figures together) in the year 1985.

In the year 1985, the U.S. Census Bureau reports that 6,789,000 children under the age of 18 lived with a stepparent, while 37.2 million lived solely with their natural parents.<sup>35</sup> For lack of better statistics, we will assume that means that 754,333 children *two or younger* lived with stepparents that year, and 4,133,333 lived only with their natural parents. Of those approximately 4,887,666 children age two or younger, 272 were apparently killed by their parents or stepparents. In other words, 0.0056% (1 out of every 17,969) children age two or younger were killed by either parents or stepparents that year.

We know that in 1985, 18% of children lived with at least one stepparent. Let's assume, for lack of better data, that the same holds true for children aged 0-2. In that case, 49 of the 272 children killed by a parent or stepparent lived in a family with at least one stepparent. Granting Prof. Jones that stepparents are 100 times more likely to commit infanticide than natural parents with respect to very small children, we will assume that all of the 49 deaths were attributable to the stepparents. Hence 49 or 0.0056% of children living with at least one stepparent were killed.

We don't know how many stepparents had access to those 754,333 children, though given that they were all age two or younger it isn't unreasonable to suspect that

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<sup>33</sup> [www.ojp.usdoh.gov/bjs/homicide/tables/kidsreltab.htm](http://www.ojp.usdoh.gov/bjs/homicide/tables/kidsreltab.htm).

<sup>34</sup> *Id.*

<sup>35</sup> "More Children are Living with Stepparents," *New York Times* (September 1, 1989).

vast majority were the only child two or younger in the household with one stepparent. That would mean that 49 children were killed by 49 stepparents out of a total of 377,167 stepparents. Assuming no stepparent killed more than one, that means 99.98% of all stepparents did not kill their children in 1985. 99.98% of stepparents are utterly innocent. 1 out of every 7,696 are guilty of murder. Whatever a presumption of innocence means, it must surely require the assumption that stepparents are innocent.<sup>36</sup>

Professor Jones does not mention any figures like the preceding. He thinks that since we can tell a selectionist “just-so” story to explain the absurdly miniscule proportion of stepparents who do kill their small children, we are entitled to argue as follows:

[S]uppose we would be willing in the abstract to risk stigmatizing stepparents in order to prevent some infant deaths, but we are as yet uncertain that stepparentage is causally linked to rates of infanticides. Even in the presence of an antecedent belief that infants are at a substantially increased risk of abuse in a home with an unrelated adult male, we might hesitate to act. And even in the presence of data indicating that belief was largely accurate, we may still hesitate, thinking that such a coordination may be coincidental, and that the risk is attributable to as yet unidentified and more palatable phenomena having nothing to do with the degree of genetic relatedness between infant and adult male.

In such a case, evolutionary analysis can make a critical difference by supplying a useful theoretical foundation that helps to clarify probably causal links. That is, evolutionary analysis can detail the *pathway* by which natural selection can favor

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<sup>36</sup> This will be true even in cases where a child has been killed, *unless* we already know that the

condition-dependent male behavioral predispositions that can yield fatal abuse of the unweaned offspring of potential mates—even in the absence of any conscious reproductive motive....This can make legal strategies attentive to the status of the adult male nonarbitrarily and therefore potentially palatable as quite plausibly useful.<sup>37</sup>

Now one might think that either stepparents are worth “stigmatizing”—which may understate what is involved in casting *suspicion of potential infanticide* on the 99.98% of stepparents who do not kill their stepchildren—or they should be left in peace, *based on the actual behavior of stepparents, and nothing else*. If stepparents kill their small stepchildren one hundred times more often than biological parents, but almost no stepparents kill their stepchildren, what is it *exactly* that evolutionary analysis adds to our understanding? It tells us nothing about which 49 stepparents out of 377,167 might murder his or her child. It tells us nothing about why the 377,118 stepparents who don’t kill their stepchildren don’t kill them. “Evolutionary analysis,” even if true, seems to add no useful information whatsoever.

But is it even true? Professor Jones speaks of “evolutionary analysis” supplying a “useful theoretical foundation,” but its utility depends on its truth, on whether the analysis is well-confirmed, whether it really establishes a “pathway.” And as we shall argue in Part III, here the kind of evolutionary analysis Professor Jones advocates has so far failed to deliver legally significant results. It may be true that legal regulations are, like EPA standards on toxicity,<sup>38</sup> sometimes crafted around scientific claims that are not

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killer was one of the parents.

<sup>37</sup> *Id.* at 437-438.

<sup>38</sup> See *supra* n. \_\_\_ and accompanying text.

well-confirmed by the applicable evidentiary standards; but in the case of relying on evolutionary psychology, the law would be opting not for lower evidential standards, but speculation as a basis of policy. (Indeed, if the Environmental Gap Objection is correct, such claims aren't just speculative: they are simply irrelevant.) Given the costs to affected parties—whether “stigmatized” stepparents or “castrated” rapists<sup>39</sup>--it seems quite extraordinary to permit speculation (perhaps *irrelevant* speculation) to drive penalties and stigmata of such severity.

None of this is to deny that there are credible evolutionary accounts of human behaviors or traits. Some of the most famous evolutionary explanations of human traits come in Darwin's own work on the expression of emotions. He explained the etiology of facial expressions as having practical functions earlier in our evolutionary history:

... some expressions, such as the bristling of the hair under the influence of extreme terror, or the uncovering of the teeth under that of furious rage, can hardly be understood, except in the belief that man once existed in a much lower and animal-like condition. The community of certain expressions in distinct through allied species as in the movement of the same facial muscles during laughter by man and by various monkeys, is rendered somewhat more intelligible, if we believe in their descent from a common progenitor.<sup>40</sup>

Notice what Darwin is arguing here. He says that we can take a trait that *we already know to exist* — the facial expressions associated with human emotions — and work out the evolutionary origin of this trait. All of the best evolutionary studies of human behavior and traits have this character: inferences from a well-characterized phenotypic

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<sup>39</sup> See THORNHILL & PALMER, *supra* n. \_\_\_ at \_\_\_-\_\_\_.

trait to its evolutionary origins.<sup>41</sup> Such studies, however, won't help the Discovery Rationale.<sup>42</sup> This rationale requires that we go the other way around. We have to predict the traits that will occur from evolutionary considerations. Careful evolutionary work rarely proceeds in this direction.<sup>43</sup>

### III. Does the Existing Science Support the Proffered Etiological Accounts?

We have seen that evolutionary accounts of the etiology of behavior *might* be relevant to law, *but only if* they are well-confirmed and *only if* we have a response to the Environmental Gap Objection. In fact, the well-confirmed accounts concern behaviors irrelevant to law, while the accounts relevant to law are not well-confirmed by the applicable biological standards. Even if we applied relaxed, non-scientific standards of confirmation, it still turns out that the proponents of applying evolutionary biology to law have no response to the Environmental Gap Objection—indeed, we are not aware that they even recognize it as the fundamental problem it is.

#### A. *There's more to Evolution than Natural Selection: A Look at the Actual Biology*

Many people believe that evolution means “natural selection,” the selection of heritable traits based on their contribution to reproductive success. More precisely, natural selection requires that:

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<sup>40</sup> (Darwin 1872/1965, 12)

<sup>41</sup> See, e.g., L.L. CAVALLI-SFORZA & M. FELDMAN, CULTURAL TRANSMISSION AND EVOLUTION: A QUANTITATIVE APPROACH (1981); W.H. DURHAM, COEVOLUTION: GENES, CULTURAL AND HUMAN DIVERSITY (1991); ROBERT BOYD AND PETER RICHARDSON, CULTURE AND THE EVOLUTIONARY PROCESS (1985); F.J. ODLING-SMEE ET AL., NICHE CONSTRUCTION (2003).

<sup>42</sup> They also will not help the Plasticity of Behavior Rationale, unless the traits in question are both well-established and well-defined. We discuss this issue, *infra* Part IV, in the context of rape.

<sup>43</sup> Of course with a complete characterization of the relevant gene frequencies, the population size, the selection pressures and other evolutionary forces, as well as knowledge about the underlying genetics, quantitative predictions about the course of evolution can be made.

- (1) there are phenotypic variations among individual organisms in a population (that is, variation in the physical and behavioral features of organisms)
- (2) these variations lead to differential reproductive success (i.e., those with certain phenotypes are more successful at passing on their traits to offspring); and
- (3) these variations are heritable, most commonly by genetic pathways.

No evolutionary biologist believes that natural selection is the only mechanism in evolution; indeed, some don't even believe that it should be considered the dominant or primary mechanism of evolutionary change.<sup>44</sup> Remember the problem to which selectionist accounts are responding: genetic mutations are random, and evolutionary accounts seek to explain the persistence of some mutations and thus their associated traits. There are a wide array of non-selectionist evolutionary mechanisms which explain the persistence of traits without natural selection operating directly on those traits. Some examples of especially important ones include:

1. *Genetic Drift*: chance distributions of genetic traits within a population can, depending on the size and structure of the population, have significant evolutionary consequences that have nothing to do with natural selection. In fact, a particular allele, and hence a trait controlled at one locus, can become fixed in a population due to genetic drift alone.<sup>45</sup>

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<sup>44</sup> See, e.g., Stephen Jay Gould, *The Hardening of the Modern Synthesis*, in DIMENSIONS OF DARWINISM 71 (M. Grene ed., 1983); RICHARD LEVINS & RICHARD LEWONTIN, THE DIALECTICAL BIOLOGIST (1985). The controversy is discussed in connection with *empirical adaptationism* in Peter Godfrey-Smith, *Three Kinds of Adaptationism*, in ADAPTATIONISM AND OPTIMALITY 335 (S.H. Orzack & E. Sober eds., 2001).

<sup>45</sup> [add cite]

2. *Asymmetric Mutation Rates*: if allele *A* mutates into allele *a* more frequently than *a* mutates into *A*, *a* will increase in frequency in the population.<sup>46</sup>

3. *Migration*: if the immigration of organisms possessing *A* is greater than the emigration of organisms with *A*, *A* can increase in the population.<sup>47</sup>

4. *Gene-culture co-evolution*: a genetically heritable trait *T* is modified in future populations by a culturally inherited practice that changes the selection pressure on *T*.

5. *Sexual selection*: “the advantage which certain individuals have over other individuals of the same sex and species, in exclusive relation to reproduction.”<sup>48</sup> In other words, differential selection of mates, which leads to differential selection of other traits, often vivid phenotypic ones. Admittedly, the modern literature often treats sexual selection as a subset of natural selection, albeit one with special properties and dynamics.

6. *Genetic Hitch-Hiking*: “changes in the frequencies of neutral traits that are pleiotropically linked [i.e., a single gene affects two or more traits] to other traits that are under selection; or changes in the frequency of neutral genes that are linked on chromosomes to changes in other genes that are under selection.”<sup>49</sup> In either scenario, the hitch-hiking trait will increase in prevalence, even though that trait confers no reproductive advantages.

Natural selection, genetic drift, mutation, migration, and the other evolutionary forces are also subject to *yet further developmental constraints* that have little to do with variation

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<sup>46</sup> ELLIOTT SOBER, *THE NATURE OF SELECTION* 27 (1984).

<sup>47</sup> *Id.*

<sup>48</sup> CHARLES DARWIN, *THE DESCEN OF MAN*, VOL. 1 256 (1871).

and inheritance. For example, you can not put together humanoid facial structure without creating a chin. Volume scales up faster than surface area, which usually means that moderate increases in surface area will result in large increases in mass. And human beings are constrained by their basic body plan to not evolve exoskeletons, despite the fitness advantage this might create in highway safety.

Now the problem confronting the etiological accounts of behavior should be clear: There are many behaviors for which one could formulate a plausible selectionist hypothesis, but which may be the products of other evolutionary mechanisms. As a consequence, a key issue for evolutionary biologists is to figure out when natural selection was the driving force of a particular evolutionary change. As G.C. Williams reminded biologists, “adaptation [fitness-enhancing evolutionary change due to natural selection] is a special and onerous concept that should only be used [in constructing explanations] where it is really necessary.”<sup>50</sup> Real biologists employ a number of methods to confirm the selectionist hypotheses about behavior, including statistical analyses of current day variation (when present), cross-species comparisons, and direct estimation of the fitness contributions of a trait through experiment and observation in the field.<sup>51</sup>

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<sup>49</sup> STEVEN STEARNS & ROLF HOEKSTRA, *EVOLUTION: AN INTRODUCTION* 344 (2000).

<sup>50</sup> G.C. WILLIAMS, *ADAPTATION AND NATURAL SELECTION* 4 (1966).

<sup>51</sup> For more comprehensive discussions about the standards of confirmation in evolutionary biology, see JOHN A. ENDLER, *NATURAL SELECTION IN THE WILD* (1986); GRAHAM BELL, *SELECTION: THE MECHANISM OF EVOLUTION* (1997); AUSTIN HUGHES, *ADAPTIVE EVOLUTION OF GENES AND GENOMES* (2000); B. Sinervo & A.L. Basolo, *Testing Adaption Using Phenotypic Manipulations*, in *ADAPTATION* 149 (M.R. Rose & G.B. Lauder eds. 19\_\_). For a more introductory discussion, see Chapter 1 of ELISABETH A. LLOYD, *THE CASE OF THE FEMALE ORGASM: BIAS IN EVOLUTIONARY SCIENCE* (2005).

The behaviors actually due to selection, unsurprisingly, turn out to be a subset of all the behaviors which appear to admit of selectionist explanations. Surprisingly, the techniques for confirming selectionist explanations in biology are rarely if ever employed in confirming selectionist explanations in the evolutionary psychology literature. As a result—and this bears emphasis—*such explanations are simply scientifically unconfirmed by the standards of evolutionary biology.*

Consider, for example, the work of leading evolutionary biologists like Mark Kirkpatrick and Michael Ryan who do research on the evolution of mating preferences.<sup>52</sup> These two biologists identify fourteen possible evolutionary mechanisms that account for these preferences, only *seven* of which involve selectionist mechanisms, and only *two of which* ever figure (by analogy) in the evolutionary psychology literature: “males provide resources to females or offspring” and “costs of searching for mates.”<sup>53</sup> As Professors Kirkpatrick and Ryan note, “the primary factors responsible for the evolution of [mating] preferences remain controversial.”<sup>54</sup> Remember that they are talking about the evolution of mating preferences in lizards, frogs, guppies, insects, and birds—not humans!

The danger inherent in assuming simple selectionist explanations is well-illustrated by the preference of some female animals for males “with the most extreme plumage, vocalizations, and displays,” preferences that are so strong in some cases that the male “secondary sexual characteristics have evolved to such extremes that they decrease male

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<sup>52</sup> See, e.g., Mark Kirkpatrick & Michael J. Ryan, *The Evolution of Mating Preferences and the Paradox of the Lek*, 350 *Nature* 33 (March 1991).

<sup>53</sup> See, e.g., DAVID M. BUSS, *THE EVOLUTION OF DESIRE: STRATEGIES OF HUMAN MATING* (1994); GEOFFREY MILLER, *THE MATING MIND: HOW SEXUAL CHOICE SHAPED THE EVOLUTION OF HUMAN NATURE* (2000).

<sup>54</sup> Kirkpatrick & Ryan, *supra* n. \_\_ at 33.

survival.”<sup>55</sup> Now the crude adaptationist, like an evolutionary psychologist, might postulate that females prefer such traits in males because they signal that the male in question will be able to provide resources for offspring and to protect offspring, that he will likely be highly fertile, and so on.<sup>56</sup>

It turns out, however, that in some animals, the selectionist “just so” story is mistaken: “Because females use their sensory systems for other tasks besides mate choice, these systems will often be subject to natural selection for other reasons, such as foraging ability or predator detection, with *the side-effect* that preferences for traits that decrease male survival are likely to be established.”<sup>57</sup> As Professors Kirkpatrick and Ryan note:

One example comes from studies of insectivorous anolid lizards. Their visual system is exquisitely adapted to detect the motion of prey. The male ‘pushup’ courtship display seems to have evolved to match these sensory biases in order to attract the attention of females.<sup>58</sup>

In other words, there is no natural selection in favor of traits that signal high fertility and the ability to provide resources for offspring, as the just-so story would lead us to suppose. Rather, there is selection pressure for phenotypic traits that are detected by a female visual mechanism that is favored by natural selection for wholly other reasons.

This is why, as we noted earlier, the *scientific* question is to distinguish the cases involving natural selection from those involving other evolutionary mechanisms and

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<sup>55</sup> *Id.*

<sup>56</sup> See, e.g., BUSS, *supra* n. \_\_.

<sup>57</sup> Kirkpatrick & Ryan, *supra* n. \_\_ at 36 (emphasis added).

<sup>58</sup> *Id.*

when relevant, to work out exactly which traits or groups of traits selection is acting on and with what intensity.

*B. Best Explanations, or the Problem of Ontological Parsimony*

The developmental pathways giving rise to the behaviors of interest to evolutionary psychologists and lawyers are very poorly understood.<sup>59</sup> We know that these pathways involve a complex and non-linear interaction between genotype and environment, but the subtle details of this interaction are not known. Thus for the relevant behaviors, it will be non-trivial to show that there is a non-cultural, non-learning form of behavioral inheritance (e.g., genetic inheritance) that would make the selective hypotheses of evolutionary psychology possible.<sup>60</sup> In the absence of demonstrating such a mechanism, evolutionary psychology accounts of such behaviors must be treated as, at most, interesting, but unconfirmed, hypotheses.<sup>61</sup>

At the moment there is little information about the genetic basis of behavioral traits that makes the developmental story look simple. But we might think that we can *infer* the genetic basis of a trait, regardless of the complexity of the pathway, if we can show that it is the *best explanation* of the phenotypic traits required by natural selection. Professor

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<sup>59</sup> SUSAN OYAMA, *THE ONTOGENY OF INFORMATION* (1985) is a classic discussion about the complexities of inheritance and development in psychological and behavioral traits. E. JABLONKA & M.J. LAMB, *EVOLUTION IN FOUR DIMENSIONS: GENETIC, EPIGENETIC, BEHAVIORAL, AND SYMBOLIC VARIATION IN THE HISTORY OF LIFE* (2005) is also an excellent discussion of these issues.

<sup>60</sup> Darwin, of course, developed natural selection without knowledge of Mendelian genetics. In fact, his own theory of inheritance was very anti-Mendelian and it took about 70 years to understand how Mendelian genetics gives the (basically) correct causal mechanism for genetic inheritance. Thus Darwin's selective hypotheses were made without knowledge of the underlying inheritance mechanism, only with the knowledge that traits were *heritable*, i.e. that offspring resemble their parents more than other members of the population. This detailed empirical data allowed him to show that in particular cases, natural selection was the best explanation for a particular trait.

<sup>61</sup> Remember, too, that if the Environmental Gap Objection is correct (as we take it to be), then these interesting speculations by evolutionary psychologists will be *irrelevant* to questions of legal regulation. No lowering of the evidentiary bar will matter.

Jones is aware of this argumentative strategy,<sup>62</sup> yet his work is marked by a failure to consider whether the selectionist just-so stories he prefers are really the best explanation.<sup>63</sup> He also shows little awareness of the criteria for “better” and “worse” explanations, mentioning only “plausibility” (which is vacuous without further specification) and “parsimony” (by which Professor Jones may mean what we call “ontological parsimony”). Other criteria familiar from the philosophy of science literature would include: *ontological* parsimony (that is, no gratuitous posits of the existence of things, unless we really need to assume their existence in order to explain observable phenomena), explanatory consilience (the ability of the explanation to unify apparently disparate phenomena), and methodological conservatism (a preference for well-established causal mechanisms and explanatory frameworks).<sup>64</sup> Let us look at just one of these—ontological parsimony (hereafter OP)—in the context of selectionist accounts of human behavior favored by evolutionary psychology.

OP demands that one only populate one’s ontology—that is, the underlying view of *what there is* in the world—with things that are *indispensable* for explaining what is observable. Positing that there is a *genetic basis* for the variation in an observable trait is a significant ontological posit. It requires us to rule out environmental factors, learning, and cultural inheritance, which population biologists have shown to be an extremely

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<sup>62</sup> Jones, *supra* n. 1 at 231

<sup>63</sup> One exception is Jones, *Biology of Rape*, *supra* n. 2 at 941, but the argument, buried in an appendix, is unsatisfactory. [add brief discussion]

<sup>64</sup> For a summary and citations, see Brian Leiter, *Moral Facts and Best Explanations*, 18 Soc. Phil. & Pol’y 79, 81 (2001). See generally, Paul Thagard, *The Best Explanation: Criteria for Theory Choice*, 75 J. Phil. 76 (1978); W.V.O. QUINE & JOSEPH ULLIAN, *THE WEB OF BELIEF* (2<sup>ND</sup> Ed. 1978).

powerful mechanism of evolutionary change.<sup>65</sup> In the absence of direct evidence of its presence, do we really *need* to posit a genetic foundation to explain the observed phenomenon?<sup>66</sup>

Let us consider an example, what Professor Jones calls the “Discriminatory Parental Solicitude” (DPS) theory of infanticide.<sup>67</sup> He writes:

Consistent with the DPS Theory, for example, which predicts that infanticide will increase commensurate with factors indicating that the prospects for a given child to significantly contribute to parental reproductive success are low, deformed or very ill children were frequently killed or abandoned in twenty-one of the thirty-five societies [studied].<sup>68</sup>

Oddly, Professor Jones does not consider that such children are very hard to raise, emotionally and economically, and these facts might also explain the finding. Yet as an alternative explanation, appeal to emotional and economic hardship is more ontologically parsimonious, since it requires us only to make familiar assumptions about material resources and psychological states that figure in numerous other theories in the social sciences. Ontological parsimony, based on the evidence put forward, plainly does not favor the evolutionary psychologist’s explanation.

We believe that a great many of the hypotheses put forward by evolutionary psychologists have this characteristic: a bit of imagination and a few moments of thought

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<sup>65</sup> See ODLING-SMEE ET AL., *supra* n. \_\_\_ and CAVALLI-SOFRZA & FELDMAN, *supra* n. \_\_\_.

<sup>66</sup> Perhaps we do if, as seems more plausible, a gene-culture co-evolution account of the relevant phenomena works. [cite]. But if such an account works—that is, if it delivers, for example, explanatory consilience unavailable to a purely cultural or environmental account—note that it is highly unlikely to involve the kind of crude assumptions about the genetic bases of behavior so central to the speculative stories Professor Jones relies upon.

<sup>67</sup> Jones, *Child Abuse*, *supra* n. 2.

will suggest many relevant alternative hypotheses that have not been eliminated by the evidence considered. Insofar as these alternatives do not require expanding our ontology by positing new, and heretofore, unknown genes, then the “best explanation” strategy of argument needed by proponents of evolutionary psychology will not work in their favor.<sup>69</sup>

### C. *Why It Matters Whether Natural Selection is at Work*

Why, it might be asked, does it matter whether we can distinguish between selectionist and non-selectionist mechanisms in evolutionary biology? After all traits that are the result of genetic drift (or other non-selectionist mechanisms) can be as developmentally canalized as those that are the products of natural selection, it is just that the former traits confer no reproductive advantages. Here is where it is crucial to remember the *precise structure* of the argument central to the project of evolutionary psychology and those who would apply evolutionary psychology to problems of legal regulation. It will help to spell this out in some detail.

1. If evolutionary biology is to be relevant to law, then we must establish that certain traits are canalized, i.e., that they are non-plastic (and so unlikely to be influenced by legal incentives and disincentives). If they have a simple genetic basis, then they are more likely to be non-plastic and resilient to intervention<sup>70</sup> (ignoring, for the moment, the

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<sup>68</sup> *Id.* at 1201.

<sup>69</sup> More generally, evolutionary psychology seems to suffer from a “problem of explanatory narrowness,” namely, positing the existence of properties (e.g., “a rape gene”) whose “explanatory role is too peculiar or narrow, that is...it only explains one class of phenomena to which it seems too neatly tailored.” Leiter, *supra* n. \_\_ at 82. The classic example is, of course, Molière’s doctor who explains how opium puts people asleep by appeal to its “dormative power.” But positing dormative power suffers from this problem of explanatory narrowness: this “power” is too neatly tailored to the explanandum, and it does no other explanatory work. Explaining rape by appeal to a gene for the rape trait does not look much more promising.

<sup>70</sup> It is not clear that this is actually true. Strong selection pressure may, in some cases, lead to the evolution of further phenotypic plasticity. However, we believe that evolutionary psychologists and their

Environmental Gap Objection), and that should, it seems, have a bearing on regulation. This is the scenario under which evolutionary biology *would* be relevant to law.

2. But we have no account, or even unambiguous evidence, of the *genetic* bases of any of the traits commonly at issue in the literature (e.g., rape, infanticide of stepchildren, human mating preferences, etc.).

3. So the only reason proffered for thinking that certain traits are non-plastic (because canalized) is that we can formulate a selectionist hypothesis about those traits (less charitably, we can tell a selectionist “story”). If the selectionist explanation truly is the *best* explanation, then perhaps we are entitled to infer a genetic basis because we know that genetic inheritance is typically implicated in cases of strong selection pressure.

4. But given the criteria that biologists employ for saying the selectionist explanation is the best explanation—as against, e.g., non-selectionist explanations—we have no reason to affirm (3).<sup>71</sup> Therefore, we have no reason to affirm (1).

5. That still leaves it open to the evolutionary psychologist (or the lawyer relying on evolutionary psychology) to argue that non-selectionist explanations which involve positing a straightforward genetic basis for the trait in question are the best explanation, which would entitle us to (1).

6. But evolutionary psychologists have *no hypotheses* that support (5), i.e., we have no reason to think those are the *best* explanations. Positing genetic drift or pleiotropy, after all, require some knowledge of underlying genetics, and, per 2, we have none here! That means, of course, that all socio-cultural explanations (or, perhaps, complex

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defenders assume that strong selection pressure decreases phenotypic plasticity. For further discussion, see S. Via & R. Lande, *Genotype-environment interaction and the evolution of phenotypic plasticity*, 39 *EVOLUTION* 505 (1985); MARY JANE WEST-EBERHARD, *DEVELOPMENT PLASTICITY AND EVOLUTION* (2003).

hypotheses about gene-culture co-evolution), will be preferable explanations, because they require no ontological posits that we do not already rely on elsewhere, and for which we have independent evidence.

7. As a consequence, we have no reason to think any of the traits in question have a simple genetic basis, and so no reason to think traits are canalized as required by (1).

#### *D. Levels of Explanation*

The fundamental problem, then, for those who would apply the theory of natural selection to problems of human behavior of interest to legal regulation is stark: there is no science supporting the etiological accounts on offer and the “pathways” they posit. There are, to be sure, intriguing speculative stories, but even if they turned out to be true, they would still be rendered irrelevant by the Environmental Gap Objection. Evolutionary biology looks, then, to be of no use for those interested in the legal regulation of behavior.

Proponents of “evolutionary biology and law” will no doubt retort as Professor Jones so often does:

As legal thinkers attempt to design effective methods [for regulating behavior]...they may rely on widely shared but typically unexamined assumptions about how people will respond as law changes....When they do so, legal thinkers tend to display a glaringly obvious, though rarely acknowledged, over-reliance on social science sources...[O]ur virtually exclusive and blinkered reliance upon them is importantly limited.

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<sup>71</sup> See the discussion *supra* \_\_\_-\_\_\_.

Here's why...2) all theories of behavior are ultimately theories about the brain; 3) the brain's design, function, and behavioral output are all products of gene-environment interactions; and 4) social sciences attend only to the environmental half of this genes-environment interaction...<sup>72</sup>

In other words, according to Professor Jones, we can not regulate behavior *without* understanding the brain and what makes it the way it is; thus, even if the current evolutionary accounts of the brain are merely speculative, this does not excuse us from the need to have such an account.

To understand how confused this perspective is,<sup>73</sup> consider what its implications are in the most banal contexts. Suppose some young parents came up with the idea that they would withhold dessert from their children, ages 6 and 8, until the children cleaned up their plates and utensils from dinner. Their idea was that this incentive—"get your dessert *once* you clean up"—would affect the behavior of their children. Although one suspects tens of millions of parents have deployed this behavior-modification scheme quite successfully, according to Professor Jones, this approach is "limited," since the parents do not understand how the brain actually works. It is not enough for Professor Jones to know that certain changes in the environment (for example, withholding dessert) will change behavior (for example, cleaning up after dinner), the parents must also know about the "brain's design, function, and behavioral output."

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<sup>72</sup> Owen D. Jones, *Proprioception, Non-Law, and Biologal History*, 53 Florida L. Rev. 831, 846-7 (2001).

<sup>73</sup> And note that this confusion is not a careless artifact of *one* article, but rather a pervasive feature of his work. In his most recent, lengthy article on the subject, for example, Professor Jones writes, "all behavior, and all the brain activity that perceives and directs it, are fundamentally biological phenomena, rendering the study of behavioral biology manifestly relevant to any deep and current understanding of how and why human behave in ways important to law." Jones & Goldsmith, *supra* n. 2 at 419.

This, of course, is complete nonsense, as every legislative body (and parent) in the world, thankfully, understands. Professor Jones, alas, does not understand it, as he makes clear:

[B]ehavioral biology is essential to any firm grasp on human behavior relevant to law. Law depends on a solid behavioral model. And solid behavioral models must integrate social science perspectives, from sociology, economics, and the like, with life science perspectives, such as those offered by behavioral biology. For it is as coherent to have a theory of human behavior that lacks a sound biological foundation as it is to have a theory of gymnastics lacking a sound foundation in physics. It cannot be done.<sup>74</sup>

That it is done, all the time, is, alas, a problem for claims like these. (Impossibility claims are *always* refuted by what is actual!) Indeed, we know of no book on gymnastics that accords any attention to physics (Professor Jones cites none, of course), and we know of no gymnasts whose skill, or self-understanding, depends on any knowledge of physics. To be sure, we are confident that no gymnast violates the laws of physics, and no “how-to” guide to gymnastics recommends movements that are physically impossible. So, too, we are confident that our young parents inculcating responsible behavior in their children are not violating any biological laws. All behavioral phenomena may have a biological dimension; and all biological phenomena may have a physical dimension. But there is *no evidence* that in order to explain and predict behavioral phenomena you need biology, or that to explain and predict biological phenomena you need physics.

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<sup>74</sup> *Id.* at 872-873.

In some cases, to be sure, insight into biological phenomena may, indeed, illuminate behavioral phenomena, but that depends on the *evidence in hand* not on any general epistemic demand for “consilience,”<sup>75</sup> according to which “all knowledge reduces to basic science,”<sup>76</sup> or, for Professor Jones, at least to biology. But as Jerry Fodor has noted, there is no evidence for consilience as a regulative epistemological ideal:

[T]he case against consilience is induction over its lack of [past successes]. In fact, there are very few examples so far in which it has turned out that the explanatory apparatus of a higher-level science can be paraphrased in the vocabulary of some science further down. To be sure, there used to be a lot of interest in a research programme that the Logical Positivists called 'the unity of science': from astronomy to zoology, all scientific vocabulary was (sooner or later) to be defined in that of basic physics. 'Science is physics plus abbreviations,' so such Positivists said. But hardly anyone believes this any more.

[C]onsilience is...in disrepute because [philosophers take science seriously]. It's attending to how the scientific edifice is actually organised that makes the eventual reduction of the rest of science to physics seem so unlikely....[W]hat one sees when one looks [at scientific practice] doesn't at all suggest a structure that is collapsing into its basement. If the unity of the sciences is true, then there ought to be fewer sciences every day, as basic physics absorbs them one by one. But what's going on

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<sup>75</sup> Not to be confused with the theoretical virtue of explanatory consilience (that is, explaining how apparently disparate phenomena are related) noted earlier as a factor in choosing between theories.

<sup>76</sup> Jerry Fodor, “Look!”, 21 *Lond. Rev. Books* \_\_ (Oct. 29, 1998) (reviewing E.O. WILSON, *CONSILIENCE*).

seems to be quite the reverse: an accelerating proliferation of new disciplines; the damned things multiply faster than college deans can keep up with them.<sup>77</sup>

No one is reducing the theory of gymnastics to physics (no one is even *trying* to do so!), and no one is having much success reducing psychological explanations to biological ones, except in certain subfields of neuroscience. It suffices to explain and predict human behavior that *you actually explain and predict it* in purely psychological terms; there is no reason to be discontent with that level of explanation, and no reason to supplement it, *unless you actually can do so*. But the etiological accounts that have attracted the attention of the “law and evolutionary biology” fad do not deliver, and that is reason enough to forget them. Contrary to Professor Jones, there is no general epistemological demand or need for pseudo-scientific speculation to underwrite otherwise fine regulatory schemes cast in folk-psychological terms, schemes like, “No dessert unless you clean up the dishes.”

#### **IV. Professor Jones’s Defense of the Relevance of Evolutionary Biology**

Professor Owen Jones has been the leading proponent of legal regulation based on a “biological” approach to human beings,<sup>78</sup> and the claims he has sometimes made on its behalf are, as we have just seen, quite extraordinary.<sup>79</sup> Unlike the politically motivated scholars who have jumped on the biology bandwagon,<sup>80</sup> Professor Jones’s work appears

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<sup>77</sup> *Id.*

<sup>78</sup> See, e.g., [cites from earlier]. Professor Jones’s articles are somewhat repetitive, and the same basic content (lengthy reviews of “human behavioral biology”, evolution by natural selection, and their relationship to law) tends to be repeated again and again, often *verbatim*. We have, accordingly, resisted citing to *every* article where the same mistake or misleading statement is made.

<sup>79</sup> He claims, for example, that “the extraordinary growth of behavioral biology renders obsolete any law-relevant model of human behavior that fails to integrate life science perspectives with social science ones.” Jones, *Law of Law’s Leverage*, *supra* n. \_\_\_ at 1143.

<sup>80</sup> See, e.g., McGinnis, *supra* n. \_\_\_.

to be politically uncommitted; his professed commitment is only to good science.<sup>81</sup> One would expect, then, that if anyone confronts the problems we have rehearsed, it would be him. Alas, that expectation would be defeated once one turns to Professor Jones's writings, where he manages to sidestep throughout his work the fundamental problems we have reviewed above. In fact, Professor Jones employs two misleading rhetorical devices, which obscure the extent to which there is no good science in the offing for his claims.

A. *Is There a "Human Behavioral Biology"?*

Professor Jones makes it his regular practice to refer to "human behavioral biology" without calling attention to the fact that *all* the genuine biological literature he cites is not about humans at all, but rather mallards, orangutans, seals, waterfowls, and lesser snow geese.<sup>82</sup> As to the propriety of drawing inferences from this literature to human beings, Professor Jones is mostly silent: only *rarely* does he even mention the fact that his literature of "behavioral biology" is almost entirely about non-humans. In one article, for example, he observes that,

It would be foolish to over interpret this [behavioral biology literature]. Humans are not insects. They have no special anatomical features used only in forced copulations. And plenty of rapists are not sexually desperate. Nevertheless,

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<sup>81</sup> [cite]

<sup>82</sup> See, e.g., Jones, *Biology of Rape*, *supra* n. 2 at 937-938; Jones & Goldsmith, *supra* n. 2 at 426 (making claims about the biology of human behavior without offering *any* citations in support). Of course there are many studies of what may loosely be called human behavioral biology, that are not particularly evolutionary in nature. Neuroscientists, for example, study the neural foundations of cognitive and non-cognitive behaviors.

biologists maintain that there is something important to be learned from scorpionfly behavior about the evolutionary processes that can give rise to sexual aggression.<sup>83</sup> Indeed there are things evolutionists could hope to learn from other animals displaying behavior which is similar to human rape. Since very little is known about the probability of rapists leaving more offspring (which is what is required if natural selection is to explain the trait), cross-species studies could be used to make inferences about the fitness enhancing effect of the posited “rape gene.” Of course, this would require significant work to ensure that additional offspring were actually the result of forced copulation and that human rape behavior was sufficiently homologous to scorpionfly behavior. For obvious reasons, such a study would be difficult to carry out, yet would be necessary for a convincing case.

While less than ideal, a more probative study would compare forced copulation in humans and our fellow primates. One primatologist, however, suggests that such a study is not likely to generate a conclusion amenable to the accounts of evolutionary psychologists. Frans de Waal writes:

In monkeys and apes there is a clear link between power and sex. High ranking males enjoy sexual privileges, and are more attractive to the opposite sex. ... This age-old connection may explain how power and sex get mixed up in the minds of men, and occasionally spin out of control together --- not because men are born to have coercive sex, but because power in general is a male aphrodisiac.<sup>84</sup>

Professor de Waal recognizes that this hypothesis is, as of yet, untested, but his suggestion is in many ways more plausible than the proffered accounts of evolutionary

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<sup>83</sup> Jones, *Reflections*, *supra* n. 2 at 162.

psychologists. For one thing, de Waal's discussion gives a historical, not selective, hypothesis about the origin of rape behavior. When relevantly similar behavior is observed in our evolutionarily closest relatives, serious biologists take this data very seriously.

In most of Professor Jones's work, even "brief" notes of caution about extrapolating from studies of scorpions to people are missing. Elsewhere, Professor Jones offers the following defense of his somewhat promiscuous extrapolation from non-human animal studies:

A prediction-evidence fit cannot alone establish that an ultimate explanation of behavior in other animals explains an evolutionary origin of similar behavior in humans. Given a tight fit, however, it is parsimonious and thus preferable to assume that it does.<sup>85</sup>

This comment betrays, however, a deep confusion over the demands of parsimony in theory-construction, as we have already argued.<sup>86</sup> Parsimony demands that when two hypotheses fit the facts equally well, we choose the one that does not posit any extra mechanisms or entities. But in Professor Jones's example, no serious attempt is made to rule out alternative hypotheses, ones which we know are relevant alternatives given what we know in general about the nature of evolutionary change. Indeed, considerations of parsimony should not even arise here because Professor Jones has not considered all of the relevant alternatives that have actually been offered by biologists, let alone all of the plausible ones that could be generated.

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<sup>84</sup> de Waal's book review in the *Times* (I have it cited below).

<sup>85</sup> Jones, *Child Abuse*, *supra* n. 2 at 1211.

<sup>86</sup> See *supra* \_\_\_\_.

Considerations of parsimony should only arise when the relevant alternatives have been eliminated and the remaining hypotheses appear to explain the phenomenon equally well. But Professor Jones has not come close to making the threshold case for invoking considerations of parsimony.

We should conclude this section, however, by noting that detailed studies about the evolution of human behavior ("human behavioral biology") do exist. One of the most well-studied examples is the coevolution of lactose tolerance and dairy husbandry.<sup>87</sup> The work begins by noting the considerable present-day variation in lactose tolerance across human beings of different ethnicities. Northern Europeans are largely lactose tolerant, for example, while East Asians are largely not.

Theoretical modeling suggests, in turn, that this may be the result of dairy husbandry and lactose tolerance co-evolving. Detailed mathematical models developed by Professors Feldman and Cavalli-Sforza suggest that if some portion of the population can gain a bit of fitness by drinking milk (i.e., they can absorb some of the calories from milk and this leads, eventually, to greater reproductive success) then there will be cultural selection for dairy husbandry. This increase in dairying will, in turn, exert selection pressure on the degree of lactose tolerance in the population. This increase in lactose tolerance can, in turn, lead to further cultural selection pressure for dairy husbandry and subsequently further selection for increased lactose tolerance.

Although we have described this process qualitatively, the cited studies develop detailed mathematical models using two-locus population genetics.

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<sup>87</sup> See M.W. Feldman & L.L. Cavalli-Sforza, *On the theory of evolution under genetic and cultural transmission with application to the lactose absorption problem*, In MATHEMATICAL EVOLUTIONARY THEORY (M.W. Feldman ed. 1989).

Of course, the mathematical model is merely a hypothesis without empirical test. Thus, Professors Feldman and Cavalli-Sforza gathered key data, looking at present day variation in lactose tolerance, the genetic basis of lactose tolerance, and anthropological data concerning the differential adoption of dairying practices by different cultures. The data and the model both suggest that cultures with historically high degrees of dairying, such as those in Northern Europe, had much higher population-wide lactose tolerance than those in areas without much dairying, such as East Asia. These empirical data conform to the predictions of theoretical models which suggest the possibility of co-evolution between lactose tolerance and dairying. A lot of work was required to establish this fairly modest thesis that explains differential lactose tolerance, but this is an example of the kind of cutting edge work about the evolution of behavior that is taken seriously by evolutionary biologists.

While this example is instructive and of great interest to evolutionary scientists, it is of little obvious relevance to the law: the legal regulation of lactose consumption is not, shall we say, one of the pressing legal issues of the day. The real problem for Professor Jones's project is that we know of no studies relevant to questions of legal regulation that are developed with anything approaching the level of scientific rigor characteristic of this *real* instance of human behavioral biology.<sup>88</sup> Professor Jones, although he speaks often of "human behavioral biology," cites none.

#### *B. Evolution and Natural Selection*

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<sup>88</sup> A possible exception are studies of the incest taboo [cite Durham (previously cited) "Coevolution"], but these studies are unlikely to lead to any significant changes to the particulars or justifications of marriage laws because marriage between siblings already illegal, and there is ample non-evolutionary genetic information about disease that results from sibling incest.

Professor Jones employs a second misleading locution to obscure the largely non-existent scientific grounds on which he bases his speculations. He slides repeatedly from the claim that the human mind is a product of “evolution”--a claim that everyone except the creationists accept<sup>89</sup>--to the claim that *natural selection* is the evolutionary mechanism at issue. Many evolutionists would be comfortable saying that natural selection was largely responsible for creating the brain given its complexity and what we know about the evolution of brains across species. But Professor Jones needs something much stronger if he is going to underwrite the claims of evolutionary psychologists. Professor Jones must believe that the brain is made up of many individual traits, what evolutionary psychologists call “Darwinian modules.”<sup>90</sup> These modules, the evolutionary psychologists tell us, are acted on individually by natural selection, such that each one is an adaptation for the ecological challenges our ancestors faced in the EEA.

Unlike the claim that the brain has evolved and selection had a role in shaping the structure of the brain, this latter claim is extremely controversial.<sup>91</sup> More importantly, to simply make this slide is to bypasses the actual scientific question that is at issue, as we saw in the discussion of Kirkpatrick and Ryan on the evolution of mating preferences.<sup>92</sup>

In one article purportedly devoted to consider “objections” to “evolutionary analysis in law,”<sup>93</sup> Professor Jones fails to consider *any* of the objections (or variations on these

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<sup>89</sup> See *supra* n. \_\_ at accompanying text.

<sup>90</sup> Leda Cosmides & John Tooby, *Cognitive Adaptations for Social Exchange*, in THE ADAPTED MIND (J. Barkow et al. eds., 1992).

<sup>91</sup> See, e.g, David Buller and Valerie G. Hadcastle, *Evolutionary Psychology, Meet the Developing Brain: Combating Promiscuous Modularity*, 1 BRAIN & MIND 307 (2001).

<sup>92</sup> See *supra* \_\_.

<sup>93</sup> Jones, *supra* n. 1.

*kinds* of objections) raised in Parts II and III of this article. That is, Professor Jones fails to respond to the worries that,

(1) there is little widely accepted *human* behavioral evolutionary biology and what *is* widely accepted involves traits such as dairy husbandry<sup>94</sup> which are irrelevant to law;

(2) the accounts of causal etiology in question are not well-confirmed by the canons of evolutionary biology;

(3) even if they were well-confirmed, they would tell us nothing about whether the genetically-based traits are likely to arise in circumstances dramatically different from those in which they developed (the Environmental Gap Objection);

(4) the central issue among real biologists is identifying when natural selection has operated and when a multitude of other non-selectionist mechanisms were at play.

All these omissions, surprisingly, are in a paper called “Evolutionary Analysis in Law: Some Objections Considered”! Indeed, the paper repeatedly employs the rhetoric noted already, in particular, sliding between the trivial claim that the brain evolved to the controversial (indeed, unsupported) claim that the evolutionary mechanism is natural selection, and strong selection on particular substructures of the brain. For example, Professor Jones writes:

The patterns in which brains process information that lead to behaviors are inevitably affected by evolutionary processes. For there are many more ways for an

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<sup>94</sup> Feldman & Cavalli-Sforza, *supra* n. 87.

organism to meet an environmental challenge disastrously than there are ways to ensure continued survival and eventual reproduction.<sup>95</sup>

Being “affected by evolutionary processes,” however, is not the same as trying to “ensure continued survival and eventual reproduction” as anyone interested in the biological science would know. Similarly, Professor Jones writes:

[G]iven the current state of science and the dictates of logic...we [must] start from the presumption that all human behavior...must in the end reconcile with known evolutionary processes. While there are still debates about details of evolution on the margin..., the theory of evolution by natural selection is no less robust than the theory of gravitational attraction of mass to mass. We may not know by what precise pathways natural selection leaves a big-brained species capable of consciousness. That it has, however, remains undisputed.<sup>96</sup>

Professor Jones says “*all* human behavior...must in the end reconcile with known evolutionary processes.” Does this apply to behavior that has no genetic basis, e.g., fastening seat belts? It would be perfectly reasonable to answer in the affirmative, but this would involve complicating the story tremendously and taking seriously all of the mechanisms of learning and cultural inheritance and the effect these mechanisms have on evolutionary change.

Professor Jones continues: “the theory of evolution by natural selection is no less robust than the theory of” gravity, which is the thesis that only the creationists deny, but that’s also not what is at issue here. What is at issue here is that it is not only not robust, it is not even moderately well-established that natural selection is the sole or dominant

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<sup>95</sup> *Id.* at 215.

force responsible for human behaviors. That we are conscious, as Professor Jones notes, “remains undisputed,” but what role natural selection plays in this fact is not established scientifically.

Despite, then, the rhetorical high-handedness throughout his work— “Only scientific method and rigorous hypothesis-testing can establish which causes what”<sup>97</sup>— the inescapable conclusion is that Professor Jones shows little interest in real scientific method and real practices of theory-confirmation by biologists.

### C. *The Case of Rape*

All the problems we have just canvassed come to the fore in Professor Jones’s review<sup>98</sup> of the widely discredited *A Natural History of Rape* by Randy Thornhill and Craig Palmer, a book that has been called a “fairy tale” by a leading evolutionary biologist;<sup>99</sup> described by a leading philosopher of biology as “pretend[ing] to scientific rigor, when it has none;”<sup>100</sup> and described by another distinguished historian and philosopher of biology as marked by “glaring flaws in [the] science.”<sup>101</sup> Professor Jones, in contrast, finds eight “principal strengths” in the book, and only two weaknesses (pertaining to trivia like “tone”). Oddly, Professor Jones’s discussion of “weaknesses” largely turns into a further *defense* of Thornhill and Palmer—indeed, after talking briefly about tone and stylistic matters, Professor Jones, from his lofty perch, quickly assures the reader:

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<sup>96</sup> *Id.* at 217.

<sup>97</sup> Jones, *Biology of Rape*, *supra* n. 2 at 882.

<sup>98</sup> Owen Jones, *Realities of Rape: Of Science and Politics, Causes and Meanings*, 86 *Cornell L. Rev.* 1386 (2001) (reviewing THORNHILL & PALMER, *supra* n. \_\_\_\_).

<sup>99</sup> Jerry Coyne, *Of Vice and Men: The Fairy Tales of Evolutionary Psychology*, 27 *New Republic* \_\_\_\_ (April 3, 2000). For a more detailed critique, see Jerry A. Coyne & Andrew Berry, *Rape as an Adaptation: Is this Contentious Hypothesis Advocacy, Not Science?* 404 *NATURE* 121 (2000).

<sup>100</sup> Kitcher & Vickers, *supra* n. \_\_ at \_\_.

I find it clear that many commentators fervently believe that [Thornhill & Palmer's] *A Natural History of Rape* must be absolutely riddled with logical and factual weaknesses. I can see how people, new to the subject, might think this. But it is simply untrue.<sup>102</sup>

Unfortunately, it *is* true, as those not new to the subject have observed. Herewith Professor Coyne, an evolutionary biologist at the University of Chicago:

Unfortunately, evolutionary psychologists routinely confuse theory and speculation. Unlike bones, behavior does not fossilize, and understanding its evolution often involves concocting stories that sound plausible but are hard to test...If evolutionary biology is a soft science, then evolutionary psychology is its flabby underbelly...But the public can be forgiven for thinking that evolutionary biology is equivalent to evolutionary psychology. Books by Daniel Dennett, E.O. Wilson, and Steven Pinker have sold briskly, and evolutionary psychology dominates the media coverage of the science of evolution....In view of the scientific shakiness of much of the work, its popularity must rest partly on some desire for a comprehensive 'scientific' explanation of human behavior....

Hence the excitement--and the furor--that has greeted the publication of Randy Thornhill and Craig Palmer's book....Rape, they argue, was favored by natural selection to give sexually dispossessed males the chance to have children, or males with mates the chance to have extra children....Not only does an evolutionary approach [according to the authors] generate new knowledge that could be used to

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<sup>101</sup> Lloyd, *supra* n. \_\_ at 1537.

<sup>102</sup> Jones, *supra* n. \_\_ at 1404.

decrease the incidence of rape; some of the proposals put forth by individual uninformed by evolutionary theory may actually increase it.

[However] to a scientist, the scientific errors in this book are far more inflammatory than are its ideological implications. Like so much of evolutionary psychology, Thornhill and Palmer's book is utterly lacking in sound scientific grounding.

Moreover, the authors use rhetorical tricks that mislead the general reader about their arguments. Once its scientific weaknesses are recognized, *The Natural History of Rape* becomes one more sociobiological 'just-so' story--the kind of tale that evolutionists swap over a few beers at the faculty club. Such stories do not qualify as science, and they do not deserve the assent, or even the respect, of the public.

So, too, Elisabeth Lloyd, a leading historian and philosophy of biology at Indiana University, writes:

[Thornhill & Palmer] want to infer information about the selection pressure from the "design" of a trait, and to assume that the only relevant evolutionary force shaping the trait was natural selection.

This is not the way evolutionary analysis is typically done. First, Thornhill and Palmer simply assume that rape behavior constitutes a single, genuine trait. In fact, the standards of evolutionary biology require that they independently establish this assumption....[E]stablishing that rape is a single trait could perhaps be a problem because of its wide variety of types of occurrences—because, in other words, the majority of rape victims are babies, men, animals, post-menopausal woman, etc.. In addition, Thornhill and Palmer offer no studies of the inheritance of raping behavior.

One also needs to show that the trait *is* an adaptation. There are a number of ways to do this...Thornhill and Palmer do not adhere to any [of these standard modes of confirmation]...In fact, nowhere in the book do the authors present evidence regarding either relevant details of the past evolutionary environment or comparisons with our closest relatives. Nor do they discuss seriously the possibility that rape itself is not a single trait....

[A]ccording to the usual evolutionary standards of evidence regarding demonstration that a trait is an adaptation, Thornhill and Palmer fail rather spectacularly. They *begin* by assuming that rape is a single trait, and that this trait is an adaptation, and they reason backwards from there. Needless to say, this undermines their repeated claims that they are doing good science.<sup>103</sup>

This, alas, is what real biologists and philosophers of biology think, but you would never know this from reading Owen Jones who assures us that the critics of Thornhill and Palmer "simply hadn't any clue about how biology can influence behavior generally" and that it is a mistake to ignore the "basic and scientifically robust knowledge of evolutionary processes" that bears on rape behavior.<sup>104</sup> But, as we have argued throughout, *there is no such knowledge*. This is the misrepresentation at the center of

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<sup>103</sup> Lloyd, *supra* n. \_\_\_ at 1541-42. Professor Lloyd goes on to document Thornhill and Palmer's "ignorance of the actual theory of evolution" (*id.* at 1544). Professor Jones, alas, keeps pace.

<sup>104</sup> Contrast Professor Lloyd's *informed* conclusion:

Based on the weaknesses in their evolutionary biolog...[and] the data fudging and gross misrepresentations of other explanatory approaches...I take it to be the responsibility of educated people to resist Thornhill and Palmer's conclusions about rape. In fact, the only circumstance under which I think this book should be read is one in which a prosecutor is faced with a defendant's lawyer who plans to call one of these authors or their followers to the stand. In that case, the attorney should read the book, then go out and hire a *real* evolutionary biologist as an expert witness.

*Id.* at 1559.

Professor Jones's work. And because it is a misrepresentation, it should be fatal to the "evolutionary biology and law" movement.

## V. Conclusion

Research on the evolutionary mechanisms that account for human behavior is an important area of study, and we hope and expect it will continue. Speculation, of the kind David Buss and other evolutionary psychologists excel at, often plays an important role in the development of science. But to date, it has mostly been speculation, and not credible science—Professor Jones's misrepresentations notwithstanding. That means, so far, that unless we are interested in the legal niceties of dairy husbandry and lactose tolerance, evolutionary biology has nothing to offer to the law.

As Professor Jones himself has noted, "the favored perspective on the causes of human behavior often reflects ephemeral enthusiasms wafted on the politics of the moment."<sup>105</sup> That summarizes we suspect, in a nutshell, the current fascination with "law and evolutionary biology," which permits the patina of "science" to be enlisted on behalf of various hobby horses of the right: people are "selfish," law can't change everything, nature puts limits on utopian aspirations, and the like. Perhaps all of these are true, but right now evolutionary biology offers no support to any of them. But "ephemeral enthusiasms wafted on the politics of the moment" have made the science irrelevant. We hope to remind people that the science is relevant, indeed, crucial, and that, so far, the needed science is not there.

In time, and starting with far more knowledge than we currently have about human psychology and social dynamics, evolutionary methods may be applied to human

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<sup>105</sup> Jones & Goldsmith, *supra* n. 2 at 408.

behavior as rigorously as they are applied to non-behavioral traits and non-human behavior. But in that case, we will already have detailed knowledge about the plasticity of behavior and thus will find out which behaviors are difficult or impossible to change the old-fashioned empirical way.