WHAT’S WRONG (& RIGHT) ABOUT EVOLUTIONARY PSYCHOLOGY?

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ABOUT THIS VIEW OF LIFE

TVOL is an online magazine that reports on evolution the way that Darwin imagined it— as a theory that applies to all aspects of humanity in addition to the rest of life. TVOL makes modern evolutionary science accessible to the public on topics that are vital to our personal and societal wellbeing, including health, education, environment, economics, politics, culture and the arts. It shows what Darwin meant when he wrote “There is grandeur in this view of life...”
I recently spoke at a symposium on cognitive and evolutionary approaches to human culture, in which the topic of evolutionary psychology (EP) came up during the final panel discussion. One by one, the other symposium participants rolled their eyes and explained their distaste for EP to the audience.

I was not surprised, but many members of the audience must have found the disparaging remarks of the experts on the stage bizarre. Weren’t they evolutionary psychologists? What else would you call a group of people who study human culture from a cognitive and evolutionary perspective?

Welcome to the murky world of EP. It’s not just murky for the general public. It’s murky all the way up to the top. That’s why This View of Life chose to feature a number of articles on EP to clarify the subject for everyone, experts and lay public alike, which have now been brought together to form this Special Edition.

The perspectives featured in our Special Edition are more diverse than any other collection on the topic to our knowledge. We have included proponents, critics, and scientists who avoid using the label “Evolutionary Psychology”, despite the fact that they study psychology from an evolutionary perspective.

All of the articles are accessible to a broad audience and also link to the scientific literature for those who wish to learn more. Our Special Edition is therefore likely to be eye-opening for both students who are encountering the subject for the first time and experts who wish to broaden their horizons.

Here are a few introductory comments to set the stage:

**Two meanings of EP:** The face value definition of EP is “the study of psychology from an evolutionary perspective”, but EP is also used to label a particular school of thought that emerged during the 1980’s and 90’s, including an influential edited book titled *The Adapted Mind* that was published in 1992. Many scientists who study psychology from an evolutionary perspective but who disagree with this particular school of thought avoid the term EP for themselves. Hence the spectacle of the symposium participants disparaging EP in front of an audience that didn’t necessarily know the historical background.

**On the importance of defending face value definitions:** The annals of science are full of terms with face value definitions that become stigmatized by being associated with particular schools of thought. In addition to EP, other examples include group selection, sociobiology, and the word evolution itself. I am often advised to use the word “change” rather than “evolution” to avoid triggering negative associations in the various audiences that I address. I take great care to ignore this advice. Avoiding a stigmatized word is a classic example of an action with a short-term benefit and a long-term cost. An immediate negative reaction is avoided but the long-term confusion does great harm. I strongly believe that scientists and scholars have a responsibility to use and defend face value definitions and avoid having them hijacked by particular schools of thought. If we aren’t careful about semantics and the history of ideas, who will be? I imagine that most of my readers will agree with me for the key word “evolution,” but it is equally important for the key term “evolutionary psychology.”

**On the need to avoid stereotyping and us/them thinking:** Scientific schools of thought are as prone to stereotyping and us/them thinking as other human social groups. Some self-described Evolutionary Psychologists worked hard to create an out-group, labeled the Standard Social Science Model (SSSM), in addition to their own in-group labeled EP. They succeeded so well that critics of their school of thought often portray it as a caricature, the way Americans portrayed the Germans and Japanese during World War II. The best way to break the spell of us/them
thinking is to focus on the specific propositions associated with various schools of thought and evaluate them on their own terms. Almost every school of thought has a baby and a bathwater. What’s worth keeping and what’s worth throwing out?

**What’s worth keeping:** I have had a love-hate relationship with the school of thought associated with EP from its inception, which enables me to argue either side depending upon the audience (I became a booster at the symposium). You know that something’s wrong with the field of psychology when the average intro textbook says almost nothing about the major adaptive problems confronted by any species, such as mating, feeding, and kinship. B.F. Skinner and other behaviorists tried to explain too much in terms of operant conditioning. The central metaphor of the so-called cognitive revolution, which largely displaced behaviorism in academic psychology, is that the mind is like a general-purpose computer. That’s not quite right either. Behavioral ecologists interpret the behavior of animals as fitness maximizing, which is inappropriate when the animals are living in a novel environment. Instead, we should be studying the psychological mechanisms that evolved in ancestral environments and how they are manifested in current environments. There is plenty to love about these and other positions associated with the school of thought associated with EP. Let’s keep these babies, even if there is also some bathwater!

**What’s worth throwing out:** You know that something is wrong when the average textbook with “evolutionary psychology” in its title has little to say about learning, development, culture, or morality. Granted that the mind is not like a single domain-general computer, but the idea that it is like hundreds of special-purpose computers (the thesis of massive modularity) isn’t quite right either. It’s true that the mind is not a complete blank slate, but somehow we must reconcile the fact of elaborate genetic innateness with the fact of elaborate open-ended flexibility on the part of both individuals and groups. The fact that some differences among human groups can be understood in terms of individual phenotypic plasticity triggered by different environmental circumstances (invoked culture) is a good point, but need not detract from the importance of transmitted culture as well. The idea of symbolic thought as an inheritance system with combinatorial possibilities as rich as the genetic inheritance system falls within the purview of evolutionary psychology. There is plenty that the school of thought associated with EP needs to grow into.

**All psychology should be evolutionary psychology:** Psychologists who are comfortable calling themselves evolutionary psychologists often point out that all branches of psychology should be studied from an evolutionary perspective, which is already the case for all branches of biology (a point made by Michael Price in one of the articles). This makes great sense, but what does it mean in terms of specific propositions such as those listed above? I have three parting observations:

1) Most psychologists (broadly defined) are not creationists and assume that their ideas are consistent with evolutionary theory, even if they do not explicitly employ an evolutionary perspective.

2) This assumption is frequently unwarranted. Approaching any given branch of psychology from an evolutionary perspective validates some propositions, falsifies others, and perhaps most importantly raises issues that weren’t even on the radar screen. This is how evolutionary theory functioned to organize all branches of the biological sciences, starting with Darwin and continuing to the present. The field of psychology has a lot of catching up to do, in part because the E-word became stigmatized in relation to human affairs early in the 20th century.

3) Scientists and scholars who are studying psychology from an evolutionary perspective—whatever they call themselves—are climbing their own learning curves. The best that a theory can do is outline a number of plausible alternatives, which then must be winnowed by empirical inquiry. That is why it is so important to avoid associating a term such as “evolutionary psychology” with any particular school of thought, which will inevitably become dated as science progresses.

We hope that our Special Edition will contribute to restoring the face value definition of evolutionary psychology: the study of psychology from an evolutionary perspective. We did not instruct our contributing authors to use this definition, preferring instead to sample the meanings that are circulating among the experts. Therefore, some inconsistencies will be on display. It is not as if the experts are on the same page and the lay public is somehow confused. Minimally, our Special Edition will demonstrate that the murkiness extends all the way to the top. Hopefully, it will also clarify the topic of EP for everyone.
THE other night, I was talking with a group of seven graduate students about sexual fantasies. No, not their sexual fantasies. We were talking about some data about sexual fantasies that I and two of the present students had gathered that showed a very strong and reliable sex difference that, as far as we know, no else had ever found.

I won’t tell you what the result was—we’re still working on the project, and it’s not ready for prime time—but I will tell you why I’m relating the story. One of the comments made by a graduate student caught my attention. As we were explaining the results of the studies we had run to that point, the student—I will call him George—remarked that the pattern that we had found was quite interesting. He indicated that the finding was so compelling, in fact, that he didn’t see why we were so focused on finding an explanation for it. The results, said George, stood on their own. Why, in short, ask why?

This struck me because my understanding of the reason that people choose to go to graduate school—to pursue science—is because of an interest in explanations. I myself grew up reading science fiction, and those explorations led me to science because science was a lot like trying to solve a puzzle—Find the Right Explanation—for which there wasn’t an answer key. The point, I suppose, is that I thought—and, more or less, continue to think—that the whole point of science is coming up with—and, it goes without saying, testing—explanations. Why, I wondered, ask “why ask why?”

I don’t by any means intend this to be a slight. People are entitled to be curious—or not—about anything they wish. Certainly, others have articulated the idea that to explain a thing is to take something vital from it—explanations of jokes have this sort of feel—but I, and I think many others, take the reverse view, that explanations only add to our joy in what we observe. (Aside: Is this why museums provide explanations near their displays? And why so many people consume the displays but eschew the plaques?)

This anecdote is just to say what I take to be the added value of evolutionary psychology: it points to explanations. Or, to go further, a key part of the value of evolutionary psychology is that it points to deep explanations.

In any case, Wilson’s piece stimulated a discussion on Edge with a number of other prominent scholars, including Steve Pinker, a well-known exponent of evolutionary approaches to psychology. The dialog was illuminating. Briefly, responding to Wilson’s (skeptical) discussion of what evolutionary psychology as a discipline adds, Pinker claimed that evolutionary psychology adds quite a lot indeed, pointing to a paper that listed novel contributions of the discipline. Wilson replied this way:

“Why, I wondered, ask “Why Ask Why?””

“... Many of them fail the novelty test in that they were well-known phenomena before evolutionary psychology existed, such as, “Sex difference in opposite-sex friendships.” Does Steve [Pinker] mean to imply that it wasn’t until evolutionary psychology took hold in the 1970s and 1980s that we discovered that there was such a phenomenon? For many of the items it is not the phenomenon that is novel but the explanation of it.”
This notion that it is somehow damning for a field—or, perhaps, a scientist—to add “only” a novel explanation is, to my mind, singular. Before Newton, pretty much everyone had noticed that things fall. “All” Newton added was an explanation. To take a more pertinent example, before 1859, it was exceedingly well known that plants and animals came in many different types, each with their own traits. “All” Darwin did was provide an explanation for this well-documented phenomenon.

Wilson continued, adding that “Evolutionary psychologists did not discover these phenomena,” and wonders “how much we need evolutionary theory to explain these phenomena.” In the end, he questions why a field such as social psychology “needs to find explanations outside of its own principles and constructs.”

This short essay isn’t the place to get into the details of the explanatory power of the evolutionary approach to (human) behavior. In some sense, I’m happy to say that it’s possible to catalog, count, and codify human behavior, marvel in its wonder, and complacently settle for measuring, rather than understanding.

But I, for one, am not inclined toward such complacency. Neither do I find satisfying explanations that are shallow, if they are explanations at all, such as frequently-repeated one-word proposals such as “learning,” “culture,” or “plasticity.”

What I want as deep an explanation for our new data on sexual fantasies as possible and, ever since Darwin, we’ve known where to start to look for one.

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Reference
I believe that an evolutionary perspective is the single most important idea for anyone who is interested in human behavior. To paraphrase Theodosius Dobzhansky, when seen in the light of evolution, the study of human behavior is, perhaps, intellectually the most satisfying and inspiring science. Without that light it becomes a pile of sundry facts—some of them interesting or curious but making no meaningful picture as a whole.

For example, people make New Year’s resolutions and, every year, losing weight and living a healthier lifestyle are among the most popular areas for self-improvement. Why?

We could simply add this to our growing list of sundry facts—people want to be thinner and more active. The alternative, however, is to try to understand why people in some societies perceive themselves to be overweight and too sedentary.

The evolutionary perspective suggests that many of us struggle to eat less and move around more because our ancestors were more successful if they ate more and moved around less (or, at least, no more than necessary).

We tend to be overweight couch potatoes because our human nature was shaped in a very different environment. The cartoon version is of hungry cavemen and cavewomen working to survive in a world without cars, restaurants, grocery stores, fridges, or even houses. In such an environment, eating as much as possible and never frivolously wasting calories led to survival and reproduction.

The evolutionary perspective argues we are not built to derive the most pleasure from low calorie foods. Furthermore, most of us are not going to instantaneously want to jump off the couch and hit the trail for 5 miles or more.

In the areas of diet and exercise, our toughest battles are often with ourselves or, more precisely, with our evolved ancestral mechanisms.
To stake out a clear and controversial stance, I believe that understanding the evolutionary nature of our battle for healthier lifestyles is more important than weighing our food, monitoring our miles on our new iWatch, or any of the myriad helpful steps to better diet and lifestyle.

I believe the evolutionary perspective holds the key to many other human struggles including substance abuse, financial difficulties, violence, and even friendship. (By way of disclosure I have co-written a book with Jay Phelan, Mean Genes, that makes this argument; indeed the line above, “our toughest battles are with ourselves,” is taken from that work.)

Do we owe this insight to Evolutionary Psychology? No and yes.

There are precursors to evolutionary psychology going all the way back to Plato, who wrote, man “is a tame or civilized animal”. More than two thousand years later Darwin wrote, “He who understands baboons would do more toward metaphysics than Locke.”

More recently, John Bowlby noted that most modern humans live in an environment that is very different from that of our ancestors. Bowlby wrote: the Environment of Evolutionary Adaptedness (The EEA) is “the one that man inhabited for two million years until change of the past few thousand years led to the extraordinary variety of habitats he occupies today.”

Evolutionary psychology deserves credit, then, not for inventing the idea of a biological and evolutionary perspective, but for transforming the idea into a scientific field.

I consider four works to be among those that constitute the foundation of evolutionary psychology (there are many other great works); On Human Nature by EO Wilson, The Adapted Mind by Barkow, Tooby, & Cosmides, The Psychological Foundations of Culture by Tooby & Cosmides, and Cognitive Adaptations for Social Exchange by Cosmides & Tooby.

Collectively, these four works have more than 10,000 Google citations. Thus, there is now an entire field devoted to understanding the human mind as an evolved organ, shaped by cultural and genetic selection. (To be sure, many of these 10,000 works criticizes the original view.)

Furthermore, even though I write the there is nothing wrong with evolutionary psychology, there are many works that claim this label that are very wrong. At the worst extreme, people who claim to be evolutionary psychologists have been naive, misogynistic, foolish, misguided, and even racist. Of course, we all should condemn bigotry wrapped in bad science, and I do. However, an active debate including criticism is part of the scientific process.

Most importantly, none of the flaws, or even sins, committed in the name of evolutionary psychology alters the facts. Humans evolved by natural selection, and we can better predict and understand our world today using an evolutionary psychology perspective.

February 26, 2015
Evolutionary psychology, like sociobiology or Marxism, has become associated with controversy. Why should it, and why has it? Yes, debates about evolution totter endlessly along, and psychology remains a discipline that sometimes seems orphaned by both humanities and the hard sciences. Why should combining psychology and evolution ignite a confabulation of loathing, fear, and scientific vitriol?

Four reasons, by my reckoning.

First, not only do we (here, a royal ‘we’ of evolutionary biologists like myself) expect very many people to not understand evolution, because it is too simple and mechanistic for our meaning-laden world; we also predict that people should reject evolution because one of its core provisos is that people, you and me, should generally behave so as to maximize their relative fitness. Competition, survival, reproduction, of the fittest? Not me, you? For shame. Evolutionary theory indeed predicts that we should each believe, or at least rationalize, ourselves to be mutualistic, altruistic, and moral nearly to a fault, because that is one of the best ways to get the edge on, or into, our competitors, be they individuals or other groups. So are you a believer now? Evolution is controversial because its very existence seems to attack our core beliefs about our own goodness, and the biggest questions regarding human purpose.

Second, psychology purports to study the brain, but can it do so scientifically, like other disciplines? Will generating questionnaires, and treating humans in modern, novel environments like lab rats, illuminate the inner-workings of the most complicated known structure in our universe? The hard sciences are hard because they are reductionistic—they infer mechanisms, processes, parts that, combined together, explain the workings of whole systems. They conduct controlled, predictive experiments. They have conceptual frameworks built from math and data, not fashion. So armed, they ratchet forward, fact by incontrovertible fact. ‘Soft’ disciplines are soft because they reject reduction, and indeed often claim postmodern
relativity for all. Psychology is a soft science because it cannot reduce—there is no place to go except neuroscience, which would swallow it up with nary a belch, given the chance. Evolutionary biology is historical but also reductionist, in that it specifies the precise set of processes whereby all phenotypes have come to be, and change, and it tells us how to discover what functions they serve. As such, it illuminates all domains of science, from genetic sequence through to human behavior—or at least would, if allowed to by academic practitioners. Psychology is controversial because it is a soft science trying to answer the hardest of question, how the brain works. It can’t.

Third, evolutionary psychology was forged in a crucible of polemic, as specific schools of thought, such as the school of highly-modular fitness-increasing brain functions developed by Leda Cosmides and John Tooby. These researchers staked out strong claims, trained talented students, and attacked intellectually-neighboring tribes. Adopting one side of polarized viewpoints, and sticking to it, remains a highly-effective route to scientific notoriety, even though in almost all such fierce academic battles both sides are partially correct, and both partially wrong. We are a deeply tribal species, and we love observing, or joining in, a good scrap. In this case, though, an entire emerging, integrative field has become conflated with extreme views of how the mind thinks, which has made for inviting targets but distracted from the much more general usefulness of evolutionary thinking. Will psychology eventually be torn asunder, like anthropology has been into post-modern, anti-evolutionary ‘culturalists’ versus mainstream but human-centric and evolution-minded biologists? Will economics? One can only hope.

Fourth, ‘psyche’ indeed means ‘soul’, and for psychologists, the hostile tribes of evolutionary biology threaten to steal it away, and subsume their discipline in its mechanistic, reductionist embrace. The irony here is that if there is any discipline that has no soul—that is, no unifying conceptual framework—it is psychology, which has flitted from one arbitrary, more or less imaginary construct to the next since Wilhelm Wundt began treating introspection as data. Of course psychology has produced deeply fascinating insights over its many years. Of course we need a top-down approach to understanding how the brain works, to meet neuroscience inexorably burrowing up from the bottom. But don’t we need a mind-set that recognizes that the brain and mind have evolved, like finches and opposable thumbs? Any discipline would fight like hell to defend its very existence, or at least resist radical transformation at the hands of competitors. Controversy indeed often leads to scientific revolution, with casualties on both sides.

Evolutionary psychology is like evolutionary anything: it is founded on a way of thinking about how the world works, how it has come to be, and how to understand it. It works by telling us what hypotheses to test, what data to collect, and how to interpret our results. The fires of controversy over this emerging field have generated both heat and light, but better understanding of their sources will, I think, help us to control the flames and put them to better use.

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Reference
EVOLUTIONARY PSYCHOLOGY IS NEITHER MOVEMENTS

often exceed the expectations of their founders, because after the movement catches on, other people inevitably want to capitalize on the popularity or authority of the movement and piggyback their own ideas on to those of its founder. The figurehead of an ancient religious movement may have warned his followers, “Take heed that ye be not deceived: for many shall come in my name, saying, I am Christ…” The figurehead of a later political movement may have been so appalled at what the French were doing in his name that he tells someone, “Je ne suis pas Marxiste.”

Both Jesus and Marx are wary of impostors who speak in their name. We have no record of Darwin actually saying something comparable, but I am pretty sure that if he were alive today, he would be thinking it.

The reason is that every generation of evolutionary biologists has various political ideologies attached to the fairly simple Darwinian propositions that (1) species are genealogically connected and (2) the primary cause of adaptation is natural selection. How do we know this? Because every previous generation indeed has tethered their ideologies to their Darwinism.

Ernst Haeckel, for example, maintained the subhumanity of non-European peoples, and saw evolution as effectively the progression from an amoeba to the Prussian militaristic state. You may not like his view, but he was the leading Darwinian in Germany, and he wrote with authority on the subject, so that if you rejected his theory, his followers would accuse you of being anti-evolution. Eventually, his ideas became inspirational to the German officers in World War I.

By the end of the 19th century, Karl Pearson could casually invoke evolutionary biology in support of genocidal colonialism, explaining that “a capable and stalwart race of white men should replace a dark-skinned tribe which can neither utilize its land for the full benefit of mankind, nor contribute its quota to the common stock of human knowledge.” Once again, the issue is not the political idea itself, but the scientific authority with which it is espoused. And once again, if you disagreed with it, especially with the aid of over a century of hindsight, but Karl Pearson was the leading evolutionary geneticist in England. What are you?

And a generation after that, the leading human geneticist in America explains that urban crime is a holdover from Homo erectus, because “the traits of the feeble-minded and the criminalistic are normal traits for infants and for an earlier stage in man’s evolution.” And once again, if you chose to challenge the veracity of that statement, you would be accused of being anti-Darwin.

That is the historical intellectual context within which I see evolutionary psychology. It’s presumably better than creationist psychology, but nobody practices creationist psychology—so presumably the word “evolutionary” is doing a bit more work here than it may seem at first blush. Indeed, the word seems to encode, in this context, a series of propositions that most people actually working in human evolution believe to be false, if not ridiculous. Foundationally, where students of human evolution have generally emphasized the adaptability of the human mind, evolutionary psychologists have rather attempted to call attention to the adaptedness of the human mind.

From these opposed starting points, other divergences quickly accumulate. For example, the idea that there is an instinctual “human nature” that is analytically separable from human culture. Whether or not you believe it, the idea has far stronger roots in Aristotle than in Darwin. But what our knowledge of human evolution tells us is that even our most fundamental evolutionary instincts, walking and talking, are also learned and highly cultural. Moreover, any familiarity with the history of the subject can show that assertions about “human nature” have a great deal of political valence. They consequently must endure high degrees of scrutiny to be taken seriously; the propositions that regularly emerge from evolutionary psychology tend to wither under the merest criticism. My personal favorite is the claim that 37 different cultures attest to the divergent features that men and women
prefer to find when mating, which can now be safely ascribed to nature—until you control for gendered economic inequality, at which point the apparent divergence disappears\(^5\). It wasn’t nature at all; it was history and sloppy scientific reasoning. My second personal favorite is the presumptively evolved disposition for men to be attracted to women with a waist-to-hip ratio of 0.67, the same as that of the stereotypical 36-24-36 Hollywood starlet. Again, naively cross-culturally supported, until you try to control for familiarity with Hollywood. Then it breaks down quickly.\(^6\) Again, history and sloppy scientific reasoning; what passes for cross-cultural generalization in evolutionary psychology tends to appall scholars actually familiar with cross-cultural analyses.\(^7\)

Another problematic idea to students of human evolution is the broad assumption in evolutionary psychology that an evolutionary explanation for any particular feature is ipso facto an adaptive explanation. But again, our knowledge of human evolution tells us that (1) non-adaptive or even maladaptive traits can evolve under appropriate demographic conditions (notably, small population size); (2) those were precisely the conditions under which the great bulk of human evolution occurred; and (3) origin and modern use do not map well onto one another, for either biological or cultural traits. Consequently, there is not the slightest reason to think that any specific feature has to have an adaptive explanation, much less that we have a reliable method for ascertaining it. While of course there are features of the human form that are probably the result of adaptive selection—for example the distinctive shape of the human pelvis in relation to the vertical posture of our ancestors—the human mind seems to be characterized by the opposite condition—adaptability, not adaptedness.

In fact, students of human evolution have found it difficult to detect any influence of selection acting even upon the shape of the human face.\(^8\) The assumption that selection would tightly constrain any particular human behavior—given the flexibility of human behavior compared to the flexibility of human faces—simply does not easily harmonize with what we know about human evolution. Consequently, any work that posits an adaptive explanation for a feature, but calls it an “evolutionary” approach—much less “the” evolutionary approach—is likely to be of greater value to the study of rhetoric or narrative in human evolution than to the study of human evolution itself.\(^9\)

Is religion an adaptation or an exaptation? There are an awful lot of prior assumptions packed into that evolutionary psychology question. Of course, we have learned a lot about religion, both functionally and cross-culturally. First, there is (cross-cultural) difficulty in bounding or defining “religion” rigorously, since at the very least, magical thought is ubiquitous.\(^10\) That in turn suggests that “religion” is a reification, and is only a “thing” in a very narrow and localized sense.\(^11\) Second, what we experience as religion is complex and has social, intellectual, emotional, and normative aspects. There is no firm reason to consider any particular aspect primary or elemental; its moral, affective, rational, and social aspects presumably coevolved with one another. Religion is consequently easily seen as neither an adaptation nor an exaptation; it’s both. That is to say, to scholars of religion and to scholars of human evolution, the question of whether religion began as a property that spread over generations because it directly benefitted its possessors, or whether it was a byproduct of something else beneficial, is just a very naïvely framed question. After all, we don’t even really know if bipedalism was an adaptation or an exaptation; we like to think that it arose in the latest Miocene as a good way of getting from point A to point B on the ground, but there are people who steadfastly believe that it arose as a consequence or byproduct of persistent wading and swimming, and it’s hard to prove them wrong.

And finally, I can’t shake the feeling that the methodologies I have encountered in evolutionary psychology would not meet the standards of any other science. For a notable example, it is apparently a revelation to evolutionary psychology that one cannot readily generalize about the human condition from a sample of humans that is Western, Educated, Industrialized, Rich, and Democratic. Perhaps this was news in psychology—creationist, evolutionary, or otherwise—but, sad to say, everybody else who works with cultural diversity knew that a really long time ago.
All Psychology Is Evolutionary Psychology

The human brain, just like every aspect of every organism on the planet, is the product of evolution. If you accept that evolution is true, you can’t avoid that conclusion. That’s why I often get confused when I hear reasonable people being broadly dismissive of evolutionary psychology (EP).

EP is simply an approach to psychology that explicitly acknowledges evolution as the designer of brains. This approach may sound non-controversial in principle, at least among those who accept evolution. Nevertheless, many non-creationist critics find plenty of reasons to object to EP, or at least to what they consider EP to be. For examples of some such criticisms see Ed Hagen’s Evolutionary Psychology FAQ.

Because many critics of EP would say they accept evolutionary theory more generally, I assume that in criticizing EP they don’t mean to imply that the brain wasn’t designed by evolution. I think they often instead intend to criticize some specific EP hypothesis or result, or some particular approach to doing EP that they are treating as though it represented the entire EP enterprise. For example, they may object to an EP prediction of a biologically-based difference between men and women, or to an EP finding that suggests that human nature is adapted for physical aggression under certain conditions. They may dislike an EP approach that expects the brain to be composed of an implausibly-large-seeming number of mental modules, or that is based on overly-speculative-seeming assumptions about what the environments of our evolutionary ancestors were like. The problem is, although these critiques are often triggered by a specific perceived implication of EP that is regarded with incredulity or disapproval, critics don’t always restrict themselves to challenging only this specific implication. Instead they regard their objection as a reason to attack the entire field of EP.

In any scientific field, critiques of particular hypotheses or approaches are essential for moving knowledge forward, and so such critiques are as welcome in EP as in any other field. However in order to be productive, such critiques do need to focus on specific targets (such as on a hypothesis that is seen as being less predictive than
an alternative hypothesis). Any critique that broadly dismisses the whole EP enterprise—that is, the whole notion that we can use evolutionary theory to understand the brain—is taking a position that is, intellectually and scientifically, very difficult to defend. What is the explicit alternative to ‘evolutionary’ psychology? Creationist psychology? Non-evolutionary psychology? Anti-evolutionary psychology? And if some such ‘un-EP’ approach is the correct way to do psychology, what are the rules of this approach? Would the cardinal rule be that it’s fine to study the brain (and brain products, like the mind, behaviour, and culture), as long as we never acknowledge or identify the process that designed the brain?

There is more than one way to do EP, and advocates of different EP approaches frequently argue, often productively, about the merits of their preferred approach. There are also many competing EP hypotheses about how evolution built various aspects of the brain and human nature. These debates may center on issues such as “is trait X an adaptation, a by-product of an adaptation, or just a random effect of the evolutionary process?” Or, “if trait X is an adaptation, then what adaptive problem did it evolve to solve?” Such debates are necessary for generating knowledge and moving the field forward. But conflict that targets the field of EP as a whole—that attacks the whole notion of using evolutionary theory to illuminate psychology—is not just unproductive but also intellectually reckless and irresponsible. If a critic is seriously proposing that knowledge of evolution cannot enhance our understanding of the brain, then he or she needs to be clear about why this proposition would be true. Is the critic proposing that human neural tissue, unlike every other kind of organismal tissue, is immune to the process of natural selection? If so, this is a radical scientific notion. It would be one of the greatest discoveries ever and the most important advance in biology at least since the discovery of DNA. It’s an extraordinary claim that would require extraordinary evidence before it could be taken seriously.

Of course, I doubt that many critics of the general EP enterprise would really claim to have ‘discovered’ that the human brain has been excluded from the laws of evolution. Nevertheless, such critics should be aware that in attacking this general enterprise, they do seem to be positioning themselves in defense of this indefensible claim.

My own view on these matters jibes with that expressed by other evolutionary psychologists: ‘evolutionary psychology’ is in fact a redundancy, in that all psychology is evolutionary psychology. I mean this in the same sense that all anatomy is ‘evolutionary anatomy.’ Any approach to human anatomy would be impotent unless it assumed that organs have specific functions that promote (or that in the evolutionary past promoted) the organism’s survival and reproduction. Anatomists understand, for example, that the heart functions to pump blood and the intestines function to extract nutrients from food. And when it comes to accounting for function scientifically, there is only one game in town: natural selection. No other known process can build a functional organismal trait (that is, an adaptation). So regardless of whether you accept evolution, you can’t do anatomy without studying organs that are evolutionary adaptations, and you can’t understand these organs without at least implicitly invoking evolutionary principles like functional specialisation for survival and reproduction. Since human neural tissue has been sculpted by the same evolutionary processes as all other tissue, these same principles apply to the study of psychology. If we’re doing psychology, therefore, then we’re also doing evolutionary psychology: we’re trying to understand evolved adaptations—and their mental, behavioral, and cultural products and by-products—and our ability to do so is enhanced through the invocation of evolutionary principles.

March 26, 2015

Reference

1Here I’m paraphrasing some online comments made recently by psychologist Michael Mills of Loyola Marymount University, USA.
I therefore rarely have the opportunity to discuss the major debates in our field—the modularity of the brain, individual versus group selection, genetic versus cultural evolution. In the eyes of many social science colleagues, these would be academic trivia compared to the far greater concerns about reductionism, social Darwinism, genetic determinism, functionalism, and the suggestion that social phenomena might have biological roots. These same fears come up again and again in seminars, conferences, and journals.\textsuperscript{2–5} I am always fighting a rear-guard action against these fundamental concerns, which we’d like to think we left behind a long time ago.

This lag in catching up with the modern status of evolutionary theory might not matter if scientists and social scientists worked on different planets and did not compete for funding or intellectual territory. But in fact we do share university campuses, pages of the big interdisciplinary journals, finite grant budgets, and seats at media and public debates. So these common misconceptions matter a lot. Moreover, much of the public, key opinion leaders, and most politicians, have been trained in the humanities and social sciences, not the sciences. We are a minority group, and seem to have lacked sufficient unity and organization to gain ground against firmly entrenched ideas about human nature and human psychology.

My big concern about evolutionary psychology, therefore, is not so much about our disagreements within the field (important though they are to resolve in the long term), but what the disagreements might have prevented us from achieving as a more united front, and what our internal disagreements signal to those who perceive themselves to be on higher ground in the first place. We must tackle internal debates, but not at the expense of presenting the appearance of major holes in evolutionary theory itself (which is an amazingly common view). Among social scientists, evolution (and sometimes science in general) continues to be so badly understood that we must, first of all, make sure we sell a message of unity on the things we do agree on and, secondly, continue to carefully explain fundamental things about evolution, boring as it may have become.

What is sorely needed is an emerging consensus on points of agreement, and how those agreed elements refine our understanding of human nature (and which views are consistent with this body of knowledge, and which contradict it). I think we’d be surprised at the extent to which we do agree, especially in relation to the dire level of understanding of evolution among important audiences. Things we take for granted, and thus barely even bother to mention, are often badly misunderstood or not known at all—such as Tinbergen’s four questions (something so fundamental that no one has even needed to mention it in this debate so far), the concepts of behavioral ecology, behavioral plasticity, and frequency dependence (adaptive traits are contingent on the social and physical environment), or the interactions between nature and nurture (it would be impossible to find a biologist who thinks that human behavioral traits are 100% genes and 0% culture, but it’s quite easy to find social scientists who believe the reserve—on this and other points, they are often extremists).

The common knee-jerk reaction against evolutionary approaches to human behavior in general is worrying enough, but the especially vicious knee-jerk reaction reserved for evolutionary psychology (for all the reasons
previous commentators have raised), has even greater costs that may appear at first glance. This is because evolutionary psychology is only a small part of the contribution that evolution—in general—has for major challenges of the 21st century. For example, even a creationist would find all sorts of evolutionary concepts and tools useful for researching social phenomena, from biomimicry (copying ideas and designs from nature), ecology, evolutionary game theory, population dynamics, behavioral genetics, cultural evolution, and so on. We’re only one species on a planet of many millions, and with a 3.5 billion year history of evolutionary innovations for the problems of competition, conflict, cooperation, and survival. There are many fundamental behavioral, organizational, and mathematical patterns in nature, before we even get to psychology, which have important implications for understanding—and sometimes fixing—our own social challenges. Social scientists can be wary of this whole range of approaches in part because of the taint of evolutionary psychology, and the heated and sometimes apparently fatal debates that shake the field. ⁶

So what is the emerging consensus on the impact of evolution on human behavior a century and a half after Darwin? Like all sciences, we have lively debate, and differences of opinion. But also like all sciences, this grinding process produces some hard kernels of truth that withstand the test of time. We are accumulating a great amount of empirical data, a series of supported and falsified hypotheses, and a body of common knowledge. In our case the problem is that this common knowledge appears to be obscured by a particularly heavy fog of war. But to start cutting through it, an initial list of 10 points of consensus might look something like this:

• Human beings evolved, like other animals.
• The human brain is a product of natural selection, like other organs.
• Many human traits are adaptations, just as they are in other animals (and this can be tested empirically for a given hypothesis).
• Traits have causes in proximate physiological mechanisms, developmental processes, and phylogenetic legacy, as well as being adaptations “for” something.
• Natural selection has lead to some universal human traits.
• Natural selection also explains considerable variation in human traits (contingent adaptations that depend on the environment).
• Trait variation does not undermine predictive power, it increases it.
• Genes can affect behavior, just as they can affect physiology.
• Cultural traits are subject to an evolutionary process, just as genes are.
• Social Darwinism was bad social science, not bad science.

We could just get on with the science, and leave the points of consensus to emerge on their own and trickle down to other disciplines and the public in their own time. But the remarkable extent and persistence of misunderstandings of and hostility to evolution (and evolutionary psychology in particular) suggest that we should actually intervene. We need to consolidate the huge collective gains that lie behind us, as well as pushing forward the frontlines in our own different directions. It seems vital to accelerate the understanding of core points of consensus about human evolution.

Since the famous Oxford debate on evolution in 1860 between Thomas Henry Huxley and Bishop Samuel Wilberforce, I would say that the impact of evolution on the social sciences has been extremely limited, if not zero. Of course, it is not all a bleak picture. There are signs of some remarkable engagement efforts emerging and, lest anyone think science has all the answers, I can personally attest to vital insights that flow from the humanities and social sciences the other way. We would be equally remiss to ignore or misunderstand those. My own appointment as, at least originally, a biologist to a chair in international relations also represents some remarkable changes going on even in traditional places like Oxford. But while there are steps forward, it is a slow walk, and against waves of gremlins from the past. We must pool our resources to address this problem, as well as enjoying the fight among ourselves. My original title was “What Isn’t Evolutionary Psychology?” Evolutionary psychology isn’t social Darwinism, genetic determinism, or political agenda setting. The problem—our problem—is that many people think it is.

May 6, 2015

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Psychology is the scientific study of human behavior and mind. That is not news. But what might not be so obvious is that most psychologists favor a certain portion of the lifespan, namely adulthood. The psychological literature is saturated with studies of adult participants, elegant theories about adult individuals and groups, and treatments for psychological disorders during adulthood. And most areas of psychology—such as social, cognitive, personality, and industrial/organizational—focus exclusively on describing, explaining, and predicting the behavior and mind of adults.

Even many developmental psychologists—including those who populate their labs with infants and young children—focus on discovering connections between early experiences and adult behaviors. Some work to establish causal links between individual differences in early behaviors and certain adult characteristics. Others test interventions designed to have some future payoff during adulthood. Still others study adult reports of the past to catalog childhood experiences that can predict the potential for troubles or successes during the adult years.

The same focus on adults pervades evolutionary psychology. Most theorizing and research in this area centers on understanding the evolutionary origins of traits seen in adulthood. The major difference between evolutionary psychologists and the rest of the field is that evolutionists are interested not only in proximal causes but also ultimate causes. So rather than studying merely what personality factors are linked to depression or what sorts of early relationships can result in later depressive symptoms, an evolutionist also might focus on understanding depression as an adaptation that brings benefits to adult functioning.

To begin to understand why psychologists direct so much attention towards adults, we have to consider how most individuals view development. Most see it as a progression toward a target of adulthood. Underlying this belief is a tacit assumption that adulthood is the period of life where the real action of humanity takes place. For evolutionary psychologists, this focus on adulthood makes especially good sense because reproduction—the essential process in evolutionary explanations of behavior—goes on only among adults.
One point, however, that is often missing from the thinking of evolutionary psychologists is that to become reproductive adults, we must first survive infancy and childhood. There is, therefore, every reason to believe that natural selection acts as much on the early portions of the lifespan as it does on adulthood. In fact, high levels of infant and childhood mortality for most of our evolutionary history suggest that selection may have had its greatest effects on the early stages of development.

This view of adulthood as the goal of development also carries with it the assumption that development is a linear process of maturation, beginning with the simplicity and inefficiency of infancy and ending with the complexity and efficiency of adulthood. From this vantage point, children are merely unfinished and unsophisticated versions of adults, and childhood is simply a necessary period that individuals must get through on their way to adulthood.

If you’ve ever spent an afternoon with a preschooler, children can seem like inefficient and inept adults. They can’t tie their own shoes, make their own sandwiches, or balance a checkbook. They are easily distracted, exceedingly irrational, and shamelessly sloppy. These everyday experiences can make childhood—the mayhem and mess of it all—feel like something that must be endured in order to get to adulthood.

As the result of these sorts of conclusions about childhood, many professionals and laypeople feel the sooner that children get to adult levels of functioning, the better. This belief drives the push among parents to send their children to academic preschools or afterschool supplemental education programs, and the successes of marketers who sell toys, games, and other products designed to “boost brain growth.”

**From Caterpillar to Butterfly**

There is, however, another way to look at development that does not conceptualize it as a linear progression from the immature to the mature. Consider the metamorphosis of caterpillars to butterflies. The caterpillar is not an immature form of butterfly. Rather the caterpillar is an animal with a set of distinct traits and behaviors that are adapted to its present life as a caterpillar and not its future life as a butterfly. If you were to line these two animals up side by side and look closely, you’d see that the characteristics of the caterpillar are every bit just as refined and complex as those of the butterfly.

Florida Atlantic University evolutionary developmental psychologist David Bjorkund argues that human development is better characterized as a metamorphosis, like caterpillars becoming butterflies, than as linear growth from the immature to the mature. Just as the caterpillar has its own complex organization adapted to the environment in which it lives presently, so does the child.

In an evolutionary developmental framework, children are not amateur versions of adults focused on gaining adult perfection and complexity. Instead, children are a form of our species that are uniquely adapted to the physical, social, and cognitive demands of the environment in which they find themselves. Children and adults have different, though equally robust and sophisticated minds and behaviors, designed for different evolutionary functions.
**Childhood as an Adaptation**

When you look at childhood through this lens of evolutionary developmental psychology, it becomes clear that not all of the characteristic of infants and children are preparations for adulthood. Some function to adapt children to their immediate environment, not some future one. When such characteristics are no longer needed, they disappear. From this perspective, all of the mayhem and mess of childhood is not a necessary evil but rather play an adaptive role in children’s lives.

We can easily look to research in developmental science to find illustrations of seemingly immature adaptations that fade when they no longer serve a function. The most obvious occur early in life. For instance, the placenta provides food and oxygen during the prenatal period. At birth, an extreme transformation occurs and newborns eat and breathe in an entirely different manner. Likewise, human infants have a set of automatic behaviors, such as the sucking and rooting reflex, that promote survival in the early weeks but would be terribly embarrassing if they persisted into adulthood.

Our early motor and sensory limitations also are adaptive. Compared to the rest of the animal kingdom, human newborns are helpless. Halfway through the first year of life most animals can fend for themselves, while we humans cannot even crawl across the room. Developmental biopsychologists Gerald Turkewitz and Patricia Kenny suggest that our early inability to independently locomote is adaptive because it prevents infants from wandering far from their mothers and therefore enhances their chances of survival during this period. However, if crawling were our only form of locomotion during the later years most of us wouldn’t survive long enough to reproduce (or at least would have trouble attracting a mate).

Likewise, newborn’s poor eyesight is adaptive because it works to reduce the amount of visual information that they have to deal with and consequently means that other developing sensory systems, such as hearing, do not need to compete for real estate in the brain. Supporting this claim are experiments from animal labs, such as Robert Lickliter’s, that demonstrates that earlier-than-expected visual stimulation can interfere with the development of the auditory system.

There also are examples throughout childhood. For example, preschoolers often misattribute others’ actions as things they themselves did. This bias facilitates children’s learning because attributing their own as well as others’ actions to a common source (themselves) produces easily retrievable memories. Supporting this idea are findings that children who collaborate with adults on a spatial memory task (e.g., placing furniture in a dollhouse) later make more attribution errors (e.g., saying: “I put the table in the kitchen” when the adult had) but also remember more items in their correct locations compared to children who carry out the task independently.

Another example is children’s common thinking that they are more skilled than they really are. For instance, 3- but not 5-year-olds who overestimate their imitative skills are more verbally advanced than better estimators. This is because children who overestimate their own abilities attempt more challenging tasks and persist longer than more realistic children, and this increased persistence boosts learning. If you’ve ever seen a preschooler on the dance floor at a wedding reception you know what I’m talking about. However, overestimating dancing skill becomes increasingly less charming with age and likely would be costly if retained at 3-year-old levels in adulthood.

The major significance of this analysis for evolutionary psychology is the recognition that natural selection operates at all stages of the lifespan not just on adult traits. As such, evolutionary psychological explanations must include an appreciation for the possible adaptive value of any given behavior at a specific time in development. Childhood behaviors cannot be characterized as immature versions of adult behaviors; and adult-like behaviors may not always adaptive during childhood.
The Role of Development

In addition to the idea that adaptations occur throughout the lifespan, a second significant insight that a developmental approach brings to the evolutionary table is the recognition that all human characteristics not only have an evolutionary history but also a developmental history. Adaptations do not simply materialize at various points in the lifespan but rather must develop from something else. More specifically, at the core of the evolutionary developmental psychology framework is the concept of probabilistic epigenesis: the idea that development emerges from continuous interactions between all levels of biological and environmental factors, from genes to culture, and at all times scales, from milliseconds to eons.

This characterization of development as involving all levels of causation shifts a few things for mainstream evolutionary psychology. Most notably, it broadens the conception of the environment. Evolutionary psychologists most often discuss environmental influences in terms of inherited psychological mechanisms that trigger certain patterns of behavior. Generally these mechanisms are characterized as innate machinery programmed by evolution.

Surprisingly even some development psychologists make claims of innateness. For example, Harvard University’s Elizabeth Spelke proposes that “humans are endowed with a small number of separable [core knowledge] systems that stand at the foundation of all our beliefs and values. New, flexible skills, concepts, and systems of knowledge build on these core foundations.”

Claims of endowments, core knowledge, or other traits described as innate, however, are untenable from an evolutionary developmental perspective because such claims devalue developmental processes. Core knowledge theorists, for instance, do not characterize their proposed systems as emerging from epigenetic processes but rather as adaptations that have been programmed by evolution to appear at a certain point in the lifespan (i.e., infancy). From an evolutionary developmental perspective, proposing that machinery is the product of evolution does not remove the need to explain developmental processes, because all characteristics are shaped through development and not programmed before development.

Core knowledge theorists and others who make claims of innateness assert that such claims are justified when there seem to be no apparent prior experiences that could account for observed behaviors. To illustrate, NYU’s Gary Marcus argues that the “reason for believing that something is innate is that there may be no other satisfying account for how a given piece of knowledge could arise.” This is probably the most unsatisfying thing that you could say to an evolutionary developmental psychologist.

The problem with the Marcus style of reasoning is that researchers who make claims of innateness don’t look very hard, or at all, for prior experiences that could account for their observations. This isn’t to say that their research isn’t rigorous. It often is. However, it is common for these investigators to study humans (or other animals) at one point in time (e.g., 4 months of age) and make claims about other points in time (e.g., the prenatal period and the first 4 months of life) without directly observing but merely thinking about these other points in time. This practice sounds to an evolutionary developmental psychologist like: “Four-month-olds can do x. I cannot think of any prior experiences that could account for x, nor have I carried out experiments at younger ages to try to determine whether certain early experiences are necessary for this behavior to occur. Therefore x must be innate.” It seems hard to argue with evolutionary developmental psychologists that it is not irresponsible science to make claims about time points never studied.

A burgeoning body of research provides experimental illustrations of why claims of innateness are unjustified and demonstrates the value of an evolutionary developmental perspective. The most iconic example comes from studies of imprinting. In the 1930s, ethologist Konrad Lorenz showed that ducklings prefer the maternal call of their species immediately after hatching. Lorenz described this preference as an innate behavior programmed by evolution to occur without any sort of experience.
Forty years later, developmental psychobiologist Gilbert Gottlieb demonstrated that Lorenz’ no-experience-necessary interpretation was wrong. Gottlieb developed a surgical procedure that prevented embryonic ducks from vocalizing while in the egg, and incubated the eggs in isolation. Therefore the ducks were deprived of all prenatal auditory experience (i.e., their own, maternal, and sibling vocalizations). When later tested, these devocalized and isolated ducklings did not show the species typical preference. Thus an adaptive behavior once thought to be innate is dependent on a specific experience, namely hearing embryonic vocalizations, at a certain point in the lifespan.

Evolutionary developmental theory and research also works to dismantle one of the most common misunderstandings in evolutionary psychology—namely, that if a behavior is evolved then it must be the result of genetic determination. This idea that evolved behaviors are in the genes lingers in the air and turns off mainstream behavioral scientists to an evolutionary approach to behavior even though evolutionary psychology left behind the idea of genetic determinism a long time ago.

An evolutionary developmental perspective swiftly knocks down misperceptions about the role of genes because it provides an appreciation for how genes and the environment interact across multiple levels to produce development, and a model for explaining how evolved machinery is translated into behavior.

The probabilistic epigenetic model puts genes in their place. It does not grant genes a privileged role in development but rather conceptualizes genes as one part the developmental system that requires input from and interaction with other parts of the system to influence mind and behavior. In fact, this approach makes clear that no level of the developmental system is in control. Thus despite media reports of scientists who have discovered evolved genes that determine intelligence, sexual orientation, and athletic ability, an evolutionary developmental model describes exactly why genes in and of themselves never determine anything.

Genes are in no position to determine behavior buried deep inside our cells and multiple levels away from behavior. Of course some of these genes have a lot to do with behavior and to a large extent in some cases. But genes are always expressed in an environment that plays a role in regulating their expression. This is how temperature can alter butterfly camouflage patterns, diet can change caterpillar shape, and social group composition can switch fish from female to male. In each of these examples, genetically identical animals develop differently based on differences in their environments.

The takeaway message for evolutionary psychology here is that if someone is making the case that a certain adaptive behavior is innate or genetically determined, it is only because no one yet has figured out the developmental processes that underlie its emergence. This acknowledgement comes with a very broad conception of experience—a conception that is necessary given a growing body of research demonstrating that sometimes the experiences that shape development are the result of nonobvious processes (à la Gottlieb’s ducks) that do not conform to our intuitions or rational expectations.

Most importantly, findings in the tradition of Gottlieb require evolutionary psychologists to rethink what is inherited from generation to generation, and what is involved in evolution. Such findings also help us understand how most humans (and other animals) develop in a species typically pattern if genes are not driving development. The answer is, in Gottlieb’s words, that evolution involves “selection for the entire developmental manifold.” Thus all animals inherit not only a species typical genome but also a species typical environment.

Probabilistic Epigenesis as a Metatheory for Evolutionary Psychology (or How Probabilistic Epigenesis Will Save Psychology from Neuroscience)

I’ve heard from colleagues in the biology department at my institution that the psychology department will become obsolete as soon as neuroscientists figure out the biochemistry of brain and behavior. I argue that the probabilistic epigenetic model illustrates exactly why such reductionism will never work if we truly want to understand behavior. Thinking that behavior can be reduced to brain activity or gene functioning devalues the role of the environment (e.g., parents, peers, culture, history) and developmental processes. There is
no way to arrive at a deep understanding of behavior and development without taking into account all levels of the developmental system, which include things external to the organism and not in the realm of things that most neuroscientists are trained to study. Examination of behavior and development does not require an evolutionary developmental perspective, but a deep understanding of human behavior will emerge only from multidisciplinary research and from thinking about behavior, brain, and body as emerging from a probabilistic epigenetic process.

Implications for Child Development

An evolutionary developmental framework also has practical implications for discussions of child rearing and education. The most salient is that if childish traits can serve important functions during childhood, then efforts to speed up development to reach maturity quicker might not always be a good idea. In fact, there is growing empirical evidence from multiple fields that faster is not always better. Here is one example: efforts to accelerate early intellectual development by putting preschoolers in academically oriented programs can increase later levels of anxiety about school and problem behaviors, and reduce later motivation for learning, expectations for success on academic tasks, and pride in accomplishments.

An evolutionary developmental perspective helps us understand why findings like these emerge: When parents or educators push too hard and too soon to try to accelerate development, they are attempting to modify a delicate developmental system designed by evolution and firmly in place for millions of years. Just like the developmental system of a caterpillar would not appreciate attempts to manipulate it into producing wings, the developmental system of a child does not benefit from attempts to make it into a business suit or high heels. Rather than thinking about how we can speed up childhood we should ask: why are we bent on doing so?

Parents and educators also should think through the implications of the degree of species atypical experiences in modern children’s everyday lives. Things like formal schooling, manufactured toys, manicured playgrounds, organized sports, and high technology are evolutionary novelties that historically have not been part of the human developmental manifold. An evolutionary developmental framework suggests that parents and educators should consider the implications of these shifts in early experiences in the decisions that they make for children. There is growing work that suggests that some parts of the species atypical lifestyle of modern children are having unexpected side effects, and that children do best in environments that gel with how their brains and bodies have been designed to grow. This perspective suggests that the solution to some common childhood problems might be to design life to work with how evolution has prepared their developing brains and bodies to grow not against it.

June 14, 2015

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A few years ago, I was giving an invited presentation to an audience of mostly sociologists and family studies professors on the topic of evolution and human reproductive strategies. I mentioned that some social scientists hold false beliefs about “evolutionary psychology,” such as the mistaken assumption that evolutionary psychologists think all men are interested in bedding as many women as possible (often called short-term mating), whereas all women are only interested in marrying a single man and staying faithful to him for a lifetime (i.e., long-term mating). When I tried to dispel this common misperception by noting, for instance, that evolutionary psychologists have hypothesized women are just as designed for short-term mating as men are—in some ways even more so such as women’s heightened desires for cues to genetic quality in short-term mates—an audible gasp swept through the conference hall. I kid you not, I could see rows of people who looked genuinely horrified. I was a little taken aback, so I asked an audience member near the front row who had her hand over her mouth if something was unclear, to which she proclaimed, “that’s not the evolutionary psychology I know.” When I tried to explain that women’s evolved short-term mating desires have been studied by evolutionary psychologists since the early 1990s and the topic remains a very active area of inquiry today, heads swiveled in disbelief. My subsequent PowerPoint slides chock-full of studies confirming women’s specially designed short-term mating psychology were falling, I feared, on an auditorium of deaf ears (or blind eyes, I suppose). Alas, this stereotype about evolutionary psychology wasn’t going to change anytime soon.

It seems to me many critics of evolutionary psychology cling steadfastly to false stereotypes of the field, both theoretical and empirical. This is partly because so much evolutionary psychological research has been produced over the last 25 years it is hard for even evolutionary-informed scholars themselves to keep up (for an up-to-date review, I recommend Buss’ new edition of *The Evolutionary Psychology Handbook*). Add to that the methodological breadth of different techniques used by evolutionary scholars to test hypotheses about the adaptive design of the human mind, and it is understandably difficult to know what all evolutionary researchers have been, and currently are, up to as active Darwinian scientists. Perhaps more than other social scientists, evolutionary psychologists use an incredible variety of research methods, ranging from self-report surveys and behavioral field test experiments, to investigations involving genetics, hormones, and neuroscience, to cross-species and cross-cultural comparisons, to ethnographies of foraging societies and computer modeling of artificial intelligences. To be aware of contemporary evolutionary psychology requires broad and deep knowledge of many scholarly disciplines, and a lot of evolutionary psychology’s critics simply do not know what they do not know about the field as it is practiced today.

Beyond simply not knowing about the empirical breadth and methodological richness of modern evolutionary science, many critics exhibit a certain kind of “empirical nihilism” toward any psychological findings even remotely portrayed as supporting evolutionary hypotheses. For instance, when one points to a set of studies that respond to a specific criticism, some critics reply with a “yes, but” attitude and set forth new criticisms requiring more evidence (sort of a serial “moving the goalposts” maneuver). Now, in science extreme skepticism is generally a good thing. For scientists, there are no capital “T” Truths, and every claim about reality is tentatively true with a small “t” and is always adjustable as more evidence is accumulated over time. Sometimes, though, this attitude is more than healthy skepticism about a particular empirical finding and is, instead, clearly an attitude of irrefutable empirical nihilism toward evolutionary psychology studies in particular. As an example of this type of unshakeable empirical disbelief, I list below 10 of the more common “yes, but” criticisms of evolutionary findings on women’s long-term mate preferences. It’s an illustrative (not exhaustive) list of just how impenetrable some scholar’s beliefs are when it comes to considering evidence that our evolved human mind might be something more than a domain-general learning mechanism writing on an asexual, ungendered blank slate.

**Women’s Long-Term Mate Preferences**

Looking across the animal kingdom, one cannot help but notice that members of most species tend to mate
non-randomly. Whether it is peahens preferring peacocks with more elaborate trains or female common chimpanzees preferring males who possess higher social dominance, males and females of most species display adaptive forms of preferential mate choice. Evolutionary psychologists were among the first to propose similar sex differences might exist in human mate preferences. For instance, evolutionary psychologists hypothesized that women may possess specially-designed long-term mate preferences for cues to a man’s ability and willingness to devote resources to her and their offspring. Such cues include a man’s status and prestige, depending on local culture, may involve hunting ability, physical strength, or other locally-relevant attributes, as well as his ambition, work ethic, intelligence, social dominance, maturity, and slightly older age. Not all women desire the highest value long-term mate at all times, of course, but it is expected that women’s long-term mate preferences should be marked by some degree of “special design” that is reliably observable using the methodological richness of modern evolutionary psychological science.

One way to evaluate whether women possess long-term mate preferences for cues to status-related traits is to directly ask people whether they prefer those attributes in long-term mates (via methods such as self-report surveys), and then compare the intensity of responses of women and men. When doing so, psychologists typically evaluate the degree of sexual differentiation using the d statistic, with an observed d value of ±.20 being considered a “small” sex difference, ±.50 is a “moderate” sex difference, and ±.80 is a “large” sex difference. Negative d values typically indicate women score more highly on a particular preference, whereas positive values indicate men score more highly.

Buss and Barnes were among the first to evaluate whether women (more than men) prefer cues related to a man’s ability and willingness to devote resources. For instance, they found women more strongly prefer long-term mates who have a “good earning capacity” (a large sex difference, d = -0.82), “are a college graduate” (d = -0.60), and “possess intelligence” (d = -0.19). Obviously, these findings are not definitive proof that men and women differ in the evolved design of long-term mate preferences. The findings are merely tests of evolutionary-guided hypotheses, and the tests were supportive of specially-designed sex differences existing in human mate preferences. Still, some critics challenge these results, arguing yes, but...

1) Yes, but... that is just one study. One cannot trust the results of just one study. Evolutionary psychologists need to conduct many more studies before I am convinced these effects are legitimate, let alone evidence of evolved psychology. I’m sure many other studies wouldn’t find sex differences in mate preferences.

Actually, most investigations of sex differences in mate preferences have been supportive of these hypotheses (to be honest, virtually all studies have). In 1992, Feingold meta-analytically reviewed the extant literature (including 32 independent samples) on self-reported mate preferences across college students and community samples and found women more greatly desired socioeconomic status (d = -0.69), ambition (d = -0.67), and intelligence (d = -0.30) in potential long-term mates. Numerous additional investigations have since replicated these basic sex differences in long-term mate preferences among college students. For instance, a recent study focused on women’s mate preferences for men with the ability to invest in them, revealing that college women desire a man who has earned his money (compared to other sources), ostensibly reflective of the aforementioned qualities (ambition, work ethic, intelligence), and that this effect is strongest in the long-term mating context.

2) Yes, but... those studies are mostly with college students. People in the real world (e.g., representative samples of adults) won’t display these stereotypical sex differences of youth.

Actually, yes they do. For instance, Sprecher and her colleagues examined sex differences in mate preferences across a nationally-representative sample of the United States and found women, more than men, valued a long-term mate who had a steady job (d = -0.73), earned more than they did (d = -0.49), was highly educated (d = -0.43), and was older by five years (d = -0.67). Young or old, gay or straight, sex differences in long-term mate preferences for status-related attributes tend to reliably emerge.
3) Yes, but…many of those findings are from decades ago. Sex differences in mate preferences are probably not historically stable. They may have existed many decades ago (in the era of Mad Men), but sex differences in mate preferences are surely not present in more recent times. Actually, yes they are. In a cross-generational analysis of the same mate preference questionnaire administered to Americans from 1939 to 1996, both men and women increased their valuing of good financial prospects and decreased valuing ambition/industriousness over time, but the degree of sex differences in these items largely persisted in strength across more than 50 years.

4) Yes, but…that is only when you have people self-report their ideal mate preferences from a pre-chosen list of traits given to them. If you ask them what they really want, say at a minimum, or maybe let them freely design their ideal potential partners, status-related traits aren’t emphasized by women more than men. Actually, yes they are. Researchers have questioned people about their long-term mate preferences using a wide variety of self-report methodologies. Kenrick and his colleagues asked people what the minimum threshold of possessing a particular attribute would need to be to agree to marry a person. Women, on average, required men’s earning capacity to be in the 70th percentile to be marriageable, whereas men required women to be in the 40th percentile (overall $d = -1.41$).

Using another nuanced form of self-report, Li compelled men and women to engage in tradeoffs among various cues when intentionally designing a desirable long-term mate. Women devoted the most of their limited budget toward their mates’ social level (33%), whereas for men social level was of moderate budgetary importance (17%). Across a series of studies, researchers using this trade off paradigm concluded that women, but not men, consider a long-term mate’s social status a “necessity” and not a “luxury.” Indeed, when forced to make decisions with very limited budgets, sex differences in long-term mate preferences are stronger than with typical self-report surveys.

Self-report surveys also reveal men, more than women, appear effective at displaying status-related traits to the opposite sex. Overall, self-report methods (via ratings, rankings, trade-offs, nominations, or open-ended questions) consistently support the hypothesis that women possess long-term mate preferences for cues to a man’s ability and willingness to devote resources.
5) Yes, but...this is only because women are denied access to resources themselves. If women have higher status themselves, they would not prefer men with high status. It’s just basic rationality, not evolved psychology, causing these sex differences in mate preferences for status.

Actually, it is a compelling test of women’s long-term mate preferences for men’s status-related traits (including their ability and willingness to provide resources) to evaluate whether their expressed preferences disappear when women have ample resources of their own. It could be women only prefer cues to men’s ability and willingness to provide resources because women are structurally denied access to resources.

Addressing this alternative explanation, Townsend and his colleagues have found women in medical school and law school are more selective of a future mate’s financial status, not less. Similarly, Wiederman and Allgeier found college women’s expected income was positively associated with their ratings of the importance of a potential long-term mate’s earning capacity. Regan found as women’s mate value goes up, so does their insistence on men’s high status and resources (i.e., they “want it all”; see also). Having higher personal status and resource-related traits appears not to attenuate women’s preferences for cues to men’s ability and willingness to provide resources. Instead, at least in the USA, women achieving high status themselves appears to make their long-term mate preferences for men’s high status even more intense!

6) Yes, but...that is only true in the United States. Americans happen to live in a culture with conspicuous gender stereotypes about mate preferences that the rest of the world does not share. If you look at more gender egalitarian cultures, in Scandinavia for instance, sex differences in preferences for status-related attributes “disappear” (as claimed by Marks).

Actually, no, they do not. Numerous studies have found sex differences in mate preferences for status-related attributes are prevalent across cultures. Lippa conducted an internet sampling of 53 nations and Zentner and Mitura conducted an internet sampling across 10 nations and both studies found 100% of cultures displayed expected sex differences, with women demonstrating especially heightened long-term mate preferences for good financial prospects, social status, ambition, and older age.

Some researchers have found the magnitude of sex differences in mate preferences for status-related attributes shifts from a large/medium effect size to a more moderate medium/small effect size in nations with higher gender egalitarianism. Zentner and Mitura found exactly this pattern of results after placing nations into three groups, low gender egalitarian cultures (within which women valued Ambition-Industriousness moderately more than men, \(d = -0.65\)), medium gender egalitarian cultures (women valued Ambition-Industriousness moderately more, \(d = -0.53\)), and high gender egalitarian cultures (women valued Ambition-Industriousness moderately more, \(d = -0.48\)). Hence, sex differences in the preference for Ambition-Industriousness in long-term mates were reduced (though not by much, and were still medium in terms of effect size) in nations with higher levels of gender egalitarianism.

Most other sex differences in status-related mate preferences also were attenuated from larger to more moderate levels in Zentner and Mitura’s sample of nations that were higher in gender egalitarianism (e.g., Good Financial Prospects went from \(d = -1.04\), to \(d = -0.84\), to \(d = -0.55\); Favorable Social Status went from \(d = -0.67\), to \(d = -0.42\), to \(d = -0.31\)). In most cases, these reductions were caused by women preferring status-related traits less in high gender egalitarian nations, though in many cases men’s preferences for status-related attributes also were reduced in high gender egalitarian nations (which seems counter to the logic of men appreciating women’s status-related traits more as women enter the workforce in high gender egalitarian nations). One thing is clear, sex differences in long-term mate preferences for status-related traits do not “disappear” in gender egalitarian cultures. They may only be moderate in size, but we see them just fine.

Importantly, Zentner and Mitura also found in low gender egalitarian nations, men valued Good Looks only a little more than women, \(d = 0.24\); in medium gender egalitarian nations, men’s valuation of Good Looks was higher still than women’s, \(d = 0.43\); and in the highest gender egalitarian nations, men’s valuation of Good Looks was the
most different from women’s, $d = 0.51$. Thus, contrary to the expectation that gender egalitarianism always reduces sex differences, Zentner and Mitura found sex differences in Good Looks are largest in nations with the highest gender egalitarianism. What!? Actually, these findings are not unusual, as high gender egalitarian nations also exhibit larger sex differences in Big Five personality traits and the Dark Triad traits of Machiavellianism, Narcissism, and psychopathy; in romantic attachment and love styles; in sociopolitical attitudes and personal values; in clinical depression rates and crying behavior; in tested cognitive and mental abilities; and in physical attributes such as height and blood pressure. If the sociopolitical gender egalitarianism found in Scandinavian nations is supposed to produce smaller psychological sex differences, it’s not doing a very good job of it.

7) Yes, but...all these studies showing men and women want different things in potential partners are merely evidence of gendered narratives as measured by self-report surveys. If ever tested in the real world, women would not preferentially choose or be affected by a partner’s status-related attributes more than men.

Actually, there have been dozens of studies of real world mating and mating-related cognition, and almost all find that women do choose and are affected by a partner’s status-related traits more than men are.

Feingold meta-analytically examined what women ask for and what men advertise in public, real-world personal advertisements and found, as expected, women more than men ask for cues to willingness and ability to provide resources (e.g., 27% of women ask for high socioeconomic status compared to 7% of men). Men who advertise such status-related cues actually receive more responses from women, as well. For example, in a study that experimentally manipulated real-life personal ads, ads placed by men noting they were financially successful elicited the most interest, whereas for women physical attractiveness was the key. In a study of Polish personal ads, the top four cues displayed by men that received responses from women were good education, older age, high resource levels, and tall height. In a study of mail order brides from Colombia, Russia, and the Philippines, women universally listed ambition, status, and wealth as among their most desired attributes in a future husband.

In another real-world test of women’s mate preferences for status, Guéguen and Lamy conducted a naturalistic experiment to evaluate whether women’s reactions to a request for their phone number are affected by men’s apparent status (in this case, driving different types of cars). When a potential participant was a few yards away they had a male experimental confederate (one of six male confederates pre-selected for high physical attractiveness) open his car door and look the participant in the eyes and smile. Then he approached her and said: “Hello, my name’s Antoine. I just want to say that I think you’re really pretty. I have to go to work now, but I was wondering if you would give me your phone number. I’ll call you later and we can have a drink together somewhere.” Women approached by a man driving an expensive Audi A5 Ambition Luxury gave their number 23% of the time. Women approached by a man driving a mid-priced Renault Mégane gave their number 13% of the time. Women approached by a man driving a 15-year-old Renault 5 Super Campus (worth only a few hundred dollars) gave their number 8% of the time. Women’s preferences for resource-related cues appear to affect their real-world mating behavior.

Numerous studies of marital patterns also have found women tend to desire (and actually marry) men who are slightly older than they are, regardless of women’s own age. As men get older, in contrast, they tend to desire and marry younger and younger women. Women have been found to preferentially marry higher status men across such diverse cultures as the Kipsigis of Kenya, the Hausa of West Africa, Trinidadians, and Micronesian islanders, among many others. It is true that some speed-dating studies in urban settings find women do not choose higher status men more often as dates, but these studies are limited by having only high status men in their samples (no homeless men allowed) and potentially including those who are interested in short-term mating (women’s short-term mate preferences focus more on gene quality, not status). In speed-dating studies with low status men included, and when the context is explicitly long-term mating only, women do pick higher status men more often for dates.

There also are a wide range of cognitive studies that test for women’s desires for status-related traits without explicitly asking them what they want. For instance, as part of a study ostensibly helping a university develop
a dating service, Kenrick and his colleagues62 experimentally manipulated whether already-mated men and women were exposed to a target date either very high in dominance or very low in dominance. They found women, but not men, were less committed to their current long-term mating partner after being exposed to a high dominance member of the opposite sex. Merely being experimentally exposed to a man with very high dominance lowered women’s commitment to their current mate, and did so without consciously asking women about their preferences for dominance.

Similarly, exposure to physically attractive women appears to evoke in men desires to fulfill women’s evolved preferences, such as increasing men’s attention toward and desires to possess resources and to display ambition, creativity, independence, and risk-taking63, 64, 65. And when exposed to men who are high in dominance, men tend to rate themselves as lower in mate value66 and men’s feelings of jealousy are more strongly evoked67. All of these cognitive processes occur differently in women and men without explicit, conscious awareness of why they are doing so. Surely, to an open-minded scientist these types of non-survey findings should buttress the view that women possess mate preferences for men’s status-related attributes...

8) Yes, but…even though evolutionary psychologists may study real life cognition, emotion, and behavior, they fail to study the most important Darwinian outcome…fertility. If women evolved mate preferences for status-related traits, then women who marry men of high status men should have more children. Evolutionary psychologists haven’t even bothered to look at these outcomes, lazy-headed daisies...

Actually, several studies by evolutionary psychologists have found women who marry higher status men tend to have more children, and to have children survive to an older age. In a study of pre-industrial Finland (from the 1700s), women married to wealthier men had more children and decreased child mortality68. In another study, marrying a man four years older was associated with maximum levels of fertility among women69. Bereczkei and Csanaky70 conducted a study of 1,800 Hungarians over 34 years of age and found women who married older and better educated men tended to have more children. These are important findings, as it is critical that women’s mate preferences for status-related attributes lead to reproductive success, or at least likely did so in our evolutionary past71, 72.
One may also look at the effects of high personal status on men’s versus women’s reproductive success. Nettle and Pollett and many other scholars have found men’s higher level of personal status is related to higher fertility, but the same is much less true (or not at all true) for women’s higher level of personal status. In fact, modern women who have higher personal incomes themselves tend to have fewer children. Jumping Jehoshaphat...yes, but...

9) Yes, but...ancestral men were foragers and could not accumulate wealth, so these mate preferences for “good earning potential” are largely irrelevant to evolved mating psychology. Evolutionary psychology findings are extremely limited because they only apply to modern materialistic cultures.

Actually, it is correct that large masses of “material wealth” were not present in our ancestral past when we lived as foragers, but it is likely ancestral men did accumulate social capital or “status” (from among other things, hunting ability). Several studies have documented this form of male status as being the subject of selective pressures (i.e., high status men—whether that status comes in the form of land, livestock, money, physical prowess, or hunting ability—have more offspring). Evidence of selection for men’s status has been found in many types of cultures, including studies of men’s hunting ability among the Aché, Hadza, and Tsimane. Apicella, for instance, found men’s hunting reputation and upper-body strength both predicted reproductive success among Hadza hunter–gathers.

Moreover, it is important to acknowledge that women’s preferences in modern nations do not seem to be calibrated on money, per se. Instead, women may view money as a proximal cue to the underlying qualities that they have evolved to care about, such as status, prestige, social dominance, ambition, work ethic, and intelligence. So it is certainly true that ancestral men did not accumulate financial wealth, but focusing too much on the importance (or not) of money or wealth across all cultures is missing the adaptive forest for the trees.

10) Yes, but...I know so many people who strongly believe that sex differences in mate preferences simply cannot exist. The idea of evolved sexual desires of any kind are a theoretical impossibility from my point of view! Evolved sex differences in mate preferences have to be just a figment of the imagination of evolutionary psychologists bent on maintaining patriarchy. If the evidence is, on balance, supportive of women possessing long-term mate preferences for men with high status, why do so many post-modernists and social constructionists insist evolved sex differences are not, indeed cannot, be real?

That’s a big question requiring several responses. First, the evidence of evolved sex differences in mate preferences is accumulating, but it is certainly not definitive. Evolutionary psychologists evaluate evidence of psychological adaptation in many ways, including cross-species, neurological, hormonal, genetic, and epigenetic evidence that has not been reviewed here (some examples of such evidence, see). Nothing in science is ever set in stone, and more evidence could emerge that would cast serious doubt about evolved sex differences in mate preferences (though it would take quite a lot to tip the scales against the existence of this particular set of mate preferences). Scientists are skeptical and open-minded, so anything is possible.

Second, it is a mistake to pit post-modernism and social constructivism against evolutionary psychology as though they are in an intellectual death match that only one side can win. This tribalistic, us-versus-them thinking isn’t helpful to science. Much like partitioning the causes of human behavior into nurture versus nature or culture versus biology or learned versus innate, social constructivism versus evolutionary psychology is a false dichotomy that may feel intuitively correct but should not be utilized very often by serious scientists (exceptions include behavioral genetics studies). As insightfully noted by Tooby and Cosmides, “To say a behavior is learned in no way undermines the claim that the behavior was organized by evolution because the behavior was learned through the agency of evolved mechanisms. If natural selection had built a different set of learning mechanisms into an organism, that organism would learn a different set of behaviors in response to the very same environment. It is these evolved mechanisms that organize the relationship between the environmental input and behavioral output, and thereby pattern the behavior. For this reason,
learning is not an alternative explanation to the claim that natural selection shaped the behavior, although many researchers assume that it is. The same goes for culture. Given that cultural ideas are absorbed via learning and inference—which is caused by evolved programs of some kind—a behavior can be, at one and the same time, ‘cultural’, ‘learned’ and ‘evolved’. Mate preferences in humans are certainly to some degree cultural, learned, and evolved. Ultimately, the adaptations of the human mind unearthed by evolutionary psychologists will likely play key roles in explaining precisely how and why human social constructionists have the mate preferences they do.89

Third, some scholars believe, based on strict ideological commitments, that evolved psychological sex differences must not exist90 or even if they do exist, studies of sex differences should be evaluated in ways that favor certain political ideologies over others, such as raising the evidentiary bar for evolutionary psychology hypotheses91. As a consequence of these political beliefs, many scholars chauvinistically dismiss or ignore much of the extant evidence accumulated by evolutionary psychologists. This is a mistake on several levels, not the least of which is that even if evolved sex differences in mate preferences do exist, that does not make them “desirable” or “good” or “inevitable” in any way. Thinking like that is fallacious, it is wrong. Even though humans have likely evolved to be omnivorous, that doesn’t mean we should eat meat. What is natural is not inherently connected to what is desirable and thinking that way is committing the so-called naturalistic fallacy (actually more related to the is-ought problem and appeal to nature fallacy). Instead of this false point of view, evolutionary psychologists take the position that by knowing what our evolved psychological adaptations are, and precisely how they are expressed (e.g., how they are specially-designed and which environments especially accentuate or attenuate their expression), we will be more capable of creating effective tools for altering human behavior in ways we do find desirable. This includes utilizing the socially-constructive psychological adaptations in our mental toolkit to do so. Evolved sex differences are not to be ideologically feared, they are to be scientifically evaluated and, if they exist, knowledge about their special design can be used to more efficiently create the healthy society within which we wish to live92, 93.

Lastly, there are some scholars who are actively deceiving people about empirical findings in evolutionary psychology (e.g., claiming that sex differences “disappear” in egalitarian cultures94). Many of these thinkers spread doubt about evolved mate preferences by alluding to a highly popular study by Eagly and Wood95. People’s memories of Eagly and Wood’s study, however, are often quite at odds with what they actually found, and with the hundreds of empirical findings since.

Eagly and Wood related the size of sex differences in mate preferences for “good financial prospects” to sociopolitical gender equality measures across nations (actual mate preference data came from a large cross-cultural study by Buss96). Eagly and Wood examined four indicators of sociopolitical gender equality and found only one indicator (that’s right, only one of four tests) was significantly linked to smaller sex differences in long-term mate preferences for good financial prospects. Based on that rather meager empirical finding, a generation of scholars seems to have fallen for a “Jedi mind trick” (“these aren’t the sex differences you are looking for”) and have been convinced that sex differences in mate preferences completely disappear in more gender egalitarian nations. Indeed, Eagly and Wood’s study has been cited over 1,000 times and has led to many to believe all psychological sex differences disappear in gender egalitarian cultures. Not true then, not true now.

To the contrary, most cross-cultural studies find nations with the highest sociopolitical gender equality (e.g., Scandinavian nations) exhibit the largest psychological sex differences in the world. You read that correctly. Higher gender egalitarian nations tend to have larger sex differences in mate preferences for Good Looks, in Big Five personality traits and the Dark Triad traits of Machiavellianism, Narcissism, and psychopathy; in romantic attachment and love styles; in sociopolitical attitudes and personal values; in clinical depression rates and crying behavior; in tested cognitive and mental abilities; and in physical attributes such as height and blood pressure97. If sociopolitical gender egalitarianism is supposed to reduce sex differences to the point where they “disappear,” it’s doing a terrible job. In fact, it’s most often doing the exact opposite. Without the constraints of patriarchal sex role socialization, it appears men and women are freer to follow their evolved desires in ways that lead to even greater psychological difference98.
In Sum

In this post, I’ve listed some of the more common “yes, but” criticisms of evolutionary psychologists and the evidence they employ to evaluate the existence of psychological adaptations. Many critics assert evolutionary psychologists rely solely on studies of college students, or unrepresentative samples, or out-of-date samples, or Westernized samples, or use only self-report methods, or ignore fertility outcomes, the list goes on. The focus here has been on evaluating these criticisms with regard to women’s long-term mate preference adaptations for men with status-related traits. In this case, these criticisms appear to be largely mistaken. Indeed, there are many other studies and findings that can be (and have been) used to fruitfully evaluate women’s long-term mate preference adaptations for status-related attributes, including important work on developmental and contextual factors that adaptively alter women’s expression of long-term mating desires99–105. For instance, evidence suggests high mate value women—feminine, agreeable, and attractive women—display the most marked preferences for men with status-related traits106–112, and in some cultural contexts other psychological adaptations appear to overwhelm the expression of women’s evolved sexual desires113.

It should be noted there also exists a very progressive research agenda into women’s evolved short-term mating strategies. Recall earlier that I mentioned many scholars fail to even acknowledge evolutionary psychologists expect women to have an evolved short-term mating psychology. In truth, there is quite a bit of support for several hypotheses about short-term seeking women particularly preferring men who possess cues to “good genes,” cues such as physical symmetry, facial masculinity, vocal masculinity, and other immunocompetence-related and testosterone-related indicators114, 115, 116. Whether it is comparing women who short-term mate versus women who long-term mate, contrasting when the same woman short-term mates versus when she long-term mates, evaluating women’s mate preference shifts across the menstrual cycle (women seem to express short-term mating adaptations more when nearing ovulation117, 118), or observing which men are the most successful at short-term mating, support for evolutionary-informed hypotheses about women’s short-term mating adaptations has been accumulating rapidly119. Indeed, since the 1990s there have probably been more supportive studies testing hypotheses about women’s short-term mating adaptations than long-term mating adaptations. Breaking stereotypes about evolutionary psychology is hard.

Overall, the breadth and depth of evidence for evolved long-term and short-term mate preferences in both men and women is, based on frameworks for evaluating evidence of psychology adaptation, rather strong120, 121. Even so, as I’ve noted earlier, psychological science might accumulate additional evidence that would tip the scales against thinking that mate preference adaptations have been sculpted into our evolved psychology. Inspired by Darwin himself, keeping an open mind and always on the lookout for new evidence (especially evidence against one’s own hypotheses) is what evolutionary psychologists do122, no buts about it.

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WHAT'S WRONG (& RIGHT) ABOUT EVOLUTIONARY PSYCHOLOGY?

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Schmitt, D.P. (2015). The evolution of culturally-variable sex differences: Men and women are not always different, but when they are...it appears not to result from patriarchy or sex role socialization. In Weekes-Shackelford, V.A., & Shackelford, T.K. (Eds.), The evolution of sexuality (pp. 221-256). New York: Springer.


ONE of the most mind-expanding books that you’ll ever read is “Evolution in Four Dimensions” by Eva Jablonka and Marion Lamb. They remind us that evolution is about variation, selection, and heredity, not genes. Genes provide one mechanism of heredity but there are others, including epigenetic mechanisms, forms of social learning found in many species, and forms of symbolic thought that are distinctively human. They provide a concise history of why evolutionary theory became so gene-centric during the 20th Century and how it needs to be expanded to include the other three dimensions.

Eva Jablonka is a Professor at the Cohn Institute for the History of Philosophy of Science and Ideas at Tel Aviv University in Israel. I talked with her by Skype on November 6 2014. Our conversation provides a panoramic tour of evolutionary theory based on heredity, not just genes.

DSW: Welcome, Eva. I’m so pleased to be talking with you.

EJ: Hello, David.

DSW: I want to talk to you about the definition of evolution and the need for it to go beyond genetic evolution. This is the topic of your great book, Evolution in Four Dimensions, which I have adopted as the first text for almost all of my courses. That’s how much I think of it. Let’s begin by discussing your background. What is your training that enables you to write such a book?

EJ: I am a geneticist. I did a PhD in genetics and molecular biology; in fact, on DNA methylation and chromatin structure. Before that, I did a Masters thesis in microbiology. At the same time, I was deeply interested in philosophy of biology. While I was doing a PhD in genetics, I was also writing papers for philosophy of biology journals. I thought that I should combine the two because theoretical biology and evolutionary biology need a very strong conceptual basis. I ended up being in some kind of twilight zone between the two things. For me it was a productive combination.

DSW: Great! Everyone knows that Darwin knew nothing about genes. For him, evolution was about variation, selection and heredity, a resemblance between parents and offspring. Nevertheless, nowadays, whenever you say the word “evolution,” most people hear “genes.” That’s true for a professional evolutionist, as much as for the lay public. How is it that the study of evolution became gene-centric?

EJ: It is related to the strong focus on heredity that is apparent already in the second 19th century, when many theories of heredity were developed. Once evolution became an accepted theory it was clear that one has to think very seriously about heredity. In order to have cumulative evolution, heredity is necessary. Darwin himself had a theory of heredity, which was, in fact, one of the most Lamarckian theories of heredity around at the time! The point is, however, that he needed a theory of heredity to consolidate his theory of evolution, and he did develop one.

The other reason heredity became focal was because of the Industrial Revolution. The population was growing and there was an urgent need to feed people so improvements in agriculture became pertinent. It was clear that breeding and selection were of great importance, and selection must be based on heritable variation. The study of heritable variation was therefore important from a practical point of view.

So there was a huge focus on heredity at that period, and once Mendel’s laws were re-discovered and accepted—it took some time—people equated biological heredity with his hereditary element, the genes. The genes were, at first, just computational entities. Nobody knew what they were. The person who invented the term
“gene,” [Wilhelm] Johannsen, did not think about genes as little particles. He thought about them as processes. He was an Aristotelian, and the distinction between potential and actualization was important for him. The genotype was the (hereditary) potential and the phenotype was the actualization. That’s why when he wrote his famous paper about the gene concept, heredity and the genotype-phenotype distinction, he said that his theory about the gene is ahistorical. Development doesn’t matter to heredity. That was a view that was very quickly accepted. It was compatible with the views of [August] Weismann in the 19th century, who was very much against the idea that acquired, developmental somatic variations can be inherited. Weismann argued that only variations that impinge on the germ-line directly can be inherited.

DSW: Let’s fast forward to the new synthesis. You said that the new synthesis is as interesting for what it left out as for what it kept in. Let’s speak on that theme.

EJ: Many things were excluded, but let me talk here about just two. First, the modern synthesis assumed that the only inherited variations are genetic. Everything else is not really inherited in a biological sense. Therefore, for heredity, all we need to be interested in are genes. But although it is true that genes are very important for heredity, the architects of the synthesis excluded other hereditary factors.

The second thing they excluded is the possibility that development can affect heredity. They said that the hereditary variation, whatever its origin, cannot be influenced by the developmental history of the organism or its ancestors. In this sense variation is functionally random.

DSW: They also, in some ways, excluded the whole developmental process, as if you could map genetic variation onto phenotypic variation and ignore the developmental process that maps one onto the other. Could you elaborate on how that happened?

EJ: There were many reasons for that, conceptual, social-political and experimental. There were experiments that showed that you cannot have selection in pure lines. Johannsen’s experiments, for example. He showed that in pure lines there are phenotypic variations, but selection is ineffective. You cannot select the extremes, for example, and make the tall plants taller over generations through selection. There were, however, at the same time, some experiments that did show selection in pure lines, but they were not very consistent, and people didn’t know how to interpret them.

Other experiments that showed the effect of the environment on organisms, were also ignored. Many of these experiments were published in very good journals but there was no interpretative framework. Unfortunately, some of these experiments were done by charlatans, we always have frauds, sadly, in all domains of science. When the charlatans were exposed, not just the specific experiments, but the whole area became tainted. In the Soviet Union, before and especially during the Cold War, there was this horrible man, Trofim Lysenko, who was a charlatan and very much against Mendelian genetics. He regarded Mendelism a bourgeois aberration. He worked within the Stalinist framework and he caused a lot of harm to Russian genetics. Because of him, the whole tradition that developed in the U.S.S.R. was dismissed as nonsense in the West. But it is wrong to assume that this was indeed the case, that every Russian scientist who discovered that development impinging on heredity was a charlatan.

Some Russian scientists thought that stress, for example, induces heritable variation. They did good experiments but their work was ignored because they became associated with Lysenko, the anti-Mendelian tradition, and the Cold War with its strong anti-Soviet prejudices. They had great tradition of good work in the U.S.S.R., and there were a lot of honest people who worked within a very different paradigm from that then current in the West. Their paradigm was that the hereditary processes are part of the physiology of the cell, and that they are responsive to developmental changes. They did ingenious experiments in aphids, for example, and in silver foxes that showed phenomena that are best interpreted in terms of epigenetic inheritance.
The hereditary variation they found was difficult to classify. It was not completely random, and it was not completely directed. People didn’t have concepts for those kinds of things, and certainly, they didn’t have mechanisms. This was important because once we had molecular biology and an understanding of how DNA is transcribed into RNA, how RNA translated into proteins and especially once we had the central dogma, it seems that developmental changes, which were assumed to affect proteins, cannot be inherited.

There was no molecular mechanism to describe the developmental effects that the Russians and other scientists (in the West) described. Biologists didn’t have a clue how this worked. Maybe, many thought, it’s all an artifact or can be explained in terms of genetic variations. It is not difficult to interpret every complex pattern of heredity in terms of genes. [Thomas Hunt] Morgan once said, “Give me six genes, with a few epistatic interactions, and I’ll explain everything to you.” Every result, can be so explained if you have enough genetic factors and enough interactions among them.

DSW: This is a good opportunity to move to the second dimension of evolution, epigenetics. Now we do have mechanisms, and we can begin to understand how epigenetics counts as an inheritance system. Maybe you could talk about that a little.

EJ: Yes. It’s difficult to decide when it started, but I think modern epigenetics started in the 1980s. It began with the intensification of the study of DNA methylation and the understanding that DNA methylation has a dual role. It is part of the regulatory system and it is a cellular heredity system. DNA methylation is commonly involved in the repression of genes: when there is a lot of methylation in promoter regions there is often transcriptional silencing. On the other hand, it is part of cell memory, so somehow, the patterns of methylation on DNA are reconstructed during cell division. Methylation has these two aspects, developmental and hereditary.

Once you realize that there are these two aspects in development, within the organism, you can think, “Well..., maybe sometimes such variations are also inherited between generations?” This was the insight that Marion and I had. We said, “Well, if you have these mechanisms, is it really true that everything is deleted, completely, during gametogenesis? Biology doesn’t work this way. It doesn’t make sense.” If germ-line cells somehow change their methylation patterns because of certain developmental signals that occurred during development and if these epigenetic marks can be inherited, then we can have the inheritance of developmental variation. This kind of inheritance, we argued, can be adaptive in some types of environment, so it is likely to have evolved.

We reasoned that if this is true we must find evidence for it in the literature. So we went to the old literature of genetics, and we started digging and looking at results that were not explained. We found a lot of those, discovered by very good geneticists, who published in Nature, Science, in the best journals. But they were forgotten sooner or later, at best were mentioned as interesting aberrations. We showed that we can reinterpret these results, not all of them, but many of them, in terms of epigenetic inheritance. We focused on DNA methylation that was then the best understood epigenetic system, although we were aware that other mechanisms are possible.

I want to say a few words about Robin Holliday, who passed away some time ago. He was a great pioneer, a great inspiration for us. He wrote a paper, with John Pugh, about DNA methylation. He and Pugh, and independently Art Riggs, realized that DNA methylation is an inheritance system—a within-organism inheritance system. That was a great insight, and Holliday started working on it, trying to understand a phenomenon such as cancer and aging. Maybe aging is an epigenetic inheritance phenomenon, with epigenetic mistakes inherited within cells, accumulating, and leading to the catastrophe of aging and, eventually, death. People picked this up and started to work on these aspects of within-organism epigenetic inheritance. This didn’t threaten the modern synthesis view of evolution because they thought about within organism cell heredity, not about trans-generational heredity.
Marion and I started to take it outside ontogeny into heredity and evolution. We were basing our initial thoughts on the biological logic of the system, on what we knew about development and gametogenesis. Soon, our conjecture was supported by experiments especially on transgenic organisms, in plants and mice that showed the inheritance of DNA methylation patterns. There were initially very few of those. When we were developing the idea, it was sometime in 1987, there was very little evidence at the molecular level.

DSW: If I understand it correctly, DNA methylation is not common in insects and other non-mammals. Is that right? As important as it is as a mechanism of epigenetics....

EJ: It’s not the only epigenetic inheritance system. We focused on methylation because this is how the study of modern epigenetics started. The fruit fly Drosophila has very little methylation. There is methylation in other insects—in bees, for example, you have much more methylation than in Drosophila. It was clear from the outset that there are several important epigenetic inheritance mechanisms, not just one. It’s not like DNA replication, which is universal. With epigenetic inheritance, you have a whole family of mechanisms. DNA methylation is one of them, but there are many others, and they may play different roles in different groups of organisms, and some may even be lost in some groups. An important epigenetic inheritance mechanisms, that was fully characterize only after 1998 and is now being investigated very intensely, is the one that is mediated by small RNAs. It is responsible for extensive epigenetic inheritance in the nematode worm C. elegans, for example.

What is interesting is that if you are looking at DNA methylation, modifications of histones (which also can be reconstructed within cell lineages) and the small RNA regulatory system, they are functionally linked. They form, together, a very complex system that we don’t understand fully, as yet. We’re getting there.

DSW: Let’s quickly go to the third and fourth dimensions of evolution, to get the whole package on the table. That would be forms of social learning found in many species, and forms of symbolic thought that are distinctively human. I won’t say “uniquely” human. I don’t care about uniqueness. We can say distinctively human. Tell us about those two mechanisms of inheritance, and then we can go on from there.

EJ: Once you start thinking that there are several ways of transmitting variations from one generation to the next, to think about heritable variation in different ways, then you start thinking about additional routes, for example, about information that is transmitted through social learning. As we know, in many animal species, there are such traditions. A lot of people say, “Yes, that’s true, but it’s not quite biology, is it?” But if it is not biology what is it? Many people are very surprised to hear that rats can learn through social learning, for example. But even with rats’ social learning they still are not sure that this transfer of information is, really, biological.

This gets worse when you’re talking about human culture or symbolic culture, that for many people belongs to a totally different sphere. In some ways, symbolic cultural transmission is, indeed, very different from other forms of information transfer. In fact, each of the systems of inheritance has its own distinct features. For example, with behavior and symbolic evolution, you don’t have a big material overlap between generations. When a cell divides, the daughter cell is part of the mother cell. The transmitted information has material continuity. However, when you are thinking about transmission through behavioral means such as imitation, or through symbolic means, you don’t have this material overlap. This has many important consequences.

Because there are genuine and important differences between different types of information transmission, people tend to treat them as if they belonged to completely different worlds and completely different types of discourse. Cultural evolution in animals was not considered real evolution, and cultural evolution in humans, symbolic evolution in humans, wasn’t evolution, it was history, and history is assumed to be something else altogether.
But in a cultural symbolic system the distinctions between development, learning, heredity and evolution becomes very difficult. History and evolution are continuous. It is a very complex system, the most complex because one has to think about all four dimensions of heredity, but this doesn’t mean that culture is not part of the biological realm.

**DSW:** No. B. F. Skinner had a classic paper, titled Selection by Consequences in Science. He put his finger on why it is that we need to think of all these things in the same way. It’s the same way that Darwin did; that once you combine variation, selection and heredity, then there’s this plasticity to these entities, which cause them to be molded by their consequences. This is basically what adaptationism is, for genetic adaptationism. The same thing holds, Skinner said, for the plasticity of individual learning. We can say the same for culture. It’s at that level that these things all are a common inheritance system. Of course, in their mechanisms, they become very different, and those differences make a difference. These inheritance systems, as you are careful to point out, are very different from each other, in some respects. Nevertheless, they allow organisms to adapt to their environments, and you get that core part of evolution – adaptationism, basically – that’s open-ended adaptation to change. Let’s talk about what distinguishes symbolic thought from forms of learning found in many species.

**EJ:** Things can be learned in very different ways. When you have symbolic representation and transmission, you have the ability to combine things that do not belong just to the here and now. This is one of the great things that symbols allow you to do. They allow you to think about futures and about imaginary or non-imaginary pasts. You can engineer the future, if you want, through this kind of symbolic communication and representation. You can’t do it with social learning that is not symbol-based. Regular animal social learning is still largely in the here and now. The variation in the symbolic variation is very stable and allows multiple combinations, and the formation of multiple scenarios. Moreover, there are many different ways of symbolizing, and you can combine these ways of symbolizing, too.

**DSW:** Let me play it back in my own words. It is that symbolic thought has the kind of combinatorial diversity that genetically combination has?

**EJ:** Absolutely, it does.

**DSW:** That is different than both epigenetics and forms of social learning. Yes, they are inheritance mechanisms, but they don’t have the combinatorial diversity that genetically combination has and that the recombination of elements of symbolic thought have. Is that a good representation of your views?

**EJ:** I think there are far less constraints on both genetic and symbolic variations, but I think that there are many possibilities for epigenetic and behavioral heritable variations too, more than we usually think. I do agree with you, however, that combinatorial variation is inherent in DNA and in the digital structure of this polymer, and it is also inherent in the symbolic system, which is made up of representations that can be decontextualized. If I say “dog,” I can relate to a dog that I saw, a dog that I will see, a dog I love, a dog I hate; a dog I never saw, a pink little dog.

**DSW:** I can also say, “You’re a dog,” and that will be transferred, basically.

**EJ:** Exactly, and this decontextualization of the single word, this metaphoric usages, allows these enormous combinatorial possibilities. The number of meaningful expressions that you can generate is unlimited.

**DSW:** I want to talk about aspects of symbolic thought that are exceptionally guided. That would include such things as flat-out, intentional planning. We want to do something, and then we choose among alternatives to do that. Also, the scenario building. Human thought is often very future-oriented. For some people, I think this disqualifies them from being evolutionary. I disagree with that. I think that intentional decision-making, for example, is explicitly evolutionary. It’s an explicit variation and selection process. There is also a chance element that goes into what you consider for what you’re trying to do. These forward-directed, intentional elements of human thought, I think, fit within the evolutionary paradigm. They count as evolutionary, for example, but I think other people tend to disagree and want to make some kind of dividing
line between randomness and guidedness. I wonder if you could talk about that. This goes back to Lamarckian forms of inheritance and genetic evolution. Let’s first talk about it in the realm of symbolic thought. What is the role of guided processes, and does that make them non-evolutionary? Or do they remain evolutionary?

**EJ:** If you think that evolution is a change in the constitution of heritable types in a population, why not? Types do not have to be genotypes, they can be epigenotypes, behavioral types, symbolic types. If we agree on that broad definition, I don’t see why change over time with respect to them is not evolution. Symbolic evolution is a very different type of evolution, among other things it is a developmental system that can direct its own evolution. But this does not make it non-evolutionary.

This is not confined to cultural evolution. We need to recall that we have learned that cells are very clever. They have a genetic-engineering kit within each cell, which it can use in all kinds of circumstances. Evolution has led to very clever and sophisticated systems, including systems that can direct their own evolution. There’s nothing wrong with it, and we shouldn’t be surprised at it. In fact, we would be surprised if it didn’t happen, in the long term. I don’t see why it is not evolution. I think that the notion of evolution most people have is very gene-centric. This version may be simple, but it far too limiting. It cannot account for actual evolutionary changes satisfactorily.

**DSW:** That’s how I want to finish up our conversation. There is a lot of shyness in embracing this larger view of evolution. There are different forms of shyness, different forms of reticence. As my experience, I think it’s also yours, that some of our biological colleagues who are doing state-of-the-art work on biological evolution are very reluctant to expand the definition beyond genetic evolution. Even if they can expand it to include epigenetics, then they’re reluctant to include these two other forms, social learning and symbolic thought.

On the other hand, our colleagues in the human-related disciplines, both the humanities and the human social sciences, often, historically, have defined their fields to be apart from biology. Sociology is often defined in a way to be apart from even psychology, not to speak of biology. We have these historical trends, and then we also have the problem that, often, the big complaint about evolution is that it’s biologically deterministic and so on. There’s a reticence on that end to think about such things as symbolic thought as within the orbit of evolutionary science. What are your own feelings about those sorts of things?

**EJ:** I agree with you. I belong to a generation that had to fight very hard to make people accept that the notion of heredity must be extended, and if so, this must also apply to the notion of evolution. Once you have heritable variation of different types, you need think about how these variations and the mechanisms of transmission underlying them work, and how they interact in the real world. You have to think in a very complex way. I am still engaged in arguing with people about that, trying to make them go beyond the gene-centered paradigm on which, I, too, was brought up.

But things are changing. A young colleague of mine told me that maybe I’m a dinosaur, and there is no need for these conceptual arguments. She’s a young and very bright epigeneticist. She said, “Oh, it’s not a problem. It’s just an empirical question. The question is: what exactly is the mechanism? How are things inherited through epigenetic mechanisms, to what extent and conditions, and in what organisms? How do different mechanisms of epigenetic inheritance, methylation, small RNAs, etc. interact? This is what we have to figure out. It’s not a conceptual problem anymore.” Maybe she is right.

**DSW:** We hope she’s right. We’d love to be dinosaurs.

**EJ:** Yes, I too hope she’s right.

**DSW:** How about on the human side, people that are steeped in culture? They appreciate the cultural diversity. They appreciate the fact that who we are is very much a matter of the symbols that we carry, but are unaccus-
tomed to thinking about this as an evolutionary process and, in fact, tend to think about evolution as just genes.
Could you have that kind of conversation?

EJ: There was a very strong reactions against the genetic-based view of evolution by people from the social sciences, who feel that the geneticists’ view is irrelevant to their own work, to their concerns. If it’s irrelevant then all one can say is: “You do your job, we’ll do our job. We don’t have any common ground.”

But if you accept that there is this complexity, that there is reconstruction of phenotypes between generations and this reconstruction uses many developmental resources, the genetic resources, the epigenetic resources and the cultural resources, and all can make a difference to heredity both directly and indirectly, the picture may change. Importantly, since all these resources interact, and they form a system. This system has its own dynamics—its own stability and its own opportunities for change. Once we understand how the system is stabilized, understand the feedback interactions, then we can also understand how one can change it.

If you present things in this way to social scientists—at least, that was my experience—they can accept it, and we can work together. If you tell social scientists, as some biologists do, that culture is just a very thin veneer on what really matters for human nature, they will say, “Well, thank you, very much. We go our way. You go your way.”

DSW: Yes, really taking the concept of cultural evolution seriously is part of what this all about. That’s no veneer.

EJ: No, it’s a very deep, complicated and beautiful process. As you say, it has this future-oriented aspect to it, which is very unique to human, as far as we know, to our symbolic representation and communication system. It makes cultural evolution and cultural history very different from animal culture.

DSW: Here’s my final question, is there any meaningful distinction between biology and non-biology?

EJ: It’s a very difficult question. We all make distinctions. We made a distinction between physics and biology. We make distinctions between biology and psychology. We all make distinctions between different levels of description of phenomena. That’s alright. From this point of view, you have to make distinctions because you describe things at very different levels. If you are somebody who describes things at the molecular level, then it is good to be clear about your domain of research. This is a legitimate kind of distinction.

When we study something like culture, it, too, has its own level of description. But in order to understand this level of description, we have to understand how it is constructed. Biological resources are part of what matters here, so you cannot exclude biology. There is something that I call the “biology of culture,” but it doesn’t reduce
culture to biology. It does, however, incorporate biological factors and biological interactions into the fabric of the cultural system. It means that biological resources are all important. Sometimes, genetic differences are not important for a particular cultural phenomena, while for other cases, they may be quite important.

**DSW:** You said, earlier, that epigenetics had its breakthrough when it became mechanistically well-understood. Everything has a mechanism. Everything has a physical mechanism. When learning and symbolic thought, as the physical mechanism, is better understood – that would neurobiologically and so on, developmentally – then maybe there will be a breakthrough there, in terms of seeing it, more clearly, as an inheritance system.

**EJ:** I think it will help, but I worry about trying to reduce cultural phenomena just to that, just to the neurobiological mechanisms. This is a danger because there is a cultural level of description. Culture is indeed a system of interactions at many, many different levels, that crucially involve neurobiology, but the system is more than sum of its parts and requires its own level of analysis.

**DSW:** Now, that’s where the distinction between ultimate and proximate causation comes in very handy. Not just a proximate description, that’s never adequate for any evolutionary explanation. There will always be a functional explanation, in addition to a mechanistic explanation.

**EJ:** Yes. But I think the distinction between ultimate and proximate, when you’re thinking hereditary variation that are developmentally or culturally constructed is breaking down a bit. Cultural evolution is both proximate and ultimate.

**DSW:** Also, there are levels of description that don’t go all the way down to the neurobiological level. On some of our own work, we’re thinking about, for example, of sacred texts as like a cultural genome. Why is that? There’s high-fidelity replication. Sacred texts are taken seriously. They tend to translate into action. They have many components to them that are deferentially invoked in response to the environment, so that different parts of the Bible, for example, are expressed under different circumstances. You have an expression and a phenotypic consequence kind of thing. That’s a mechanistic description, which is still not all the way down, but we’re finding it very useful to be thinking about culture that explicitly, as an inheritance system, as an evolutionary process.

**EJ:** It’s very interesting to think about the evolution of these texts in this way. We also want to understand how is it that these texts become sacred in the first place. What made them sacred, and how is sacredness maintained? Going back to our DNA analogy, DNA is not just a very stable molecule. In fact it is not very stable at all. It requires all the machinery of the cell to make it stable, to edit away and to get rid of mistakes. It’s the same about the sacred text, there are social-cultural processes that maintain the sacredness of the text or allow changes in it. That’s a very important part of what we need to understand. A good example, are the Mormons. They are very good at modifying things, according to circumstances, and maintaining the core of their religion.

**DSW:** That is an excellent example of this evolutionary thinking applied to our culture. To that, I say, “Amen.” That can be the end of our wonderful conversation.

**EJ:** Thank you, very much.

**DSW:** Thank you!

April 8, 2015
of the most widely cited and discussed articles in evolutionary biology is “The Spandrels of San Marco and the Panglossian Paradigm: A Critique of the Adaptationist Programme”, which was written by Harvard biologists Stephen Jay Gould and Richard C. Lewontin and published in the Proceedings of the Royal Society of London in 1979. Their critique of their own field of evolutionary biology spilled out of the Ivory Tower onto the pages of general intellectual forums such as the New York Review of Books.

Gould died in 2002 but his coauthor is still active. Richard C. Lewontin is a population geneticist by training and pioneered the method of gel electrophoresis among many other accomplishments. His academic books include The Genetic Basis of Evolutionary Change (1974), which I eagerly read as a graduate student. His biological books for the general public include The Triple Helix: Gene, Organism, and Environment (2002) and Human Diversity (part of the Scientific American Library Series; 1982). In his role as social critic and theorist, his books include Biology Under the Influence: Dialectical Essays on Ecology, Agriculture, and Health (with Richard Levins; 2007), It Ain’t Necessarily So: The Dream of the Human Genome and Other Illusions (2001), Biology as Ideology: The Doctrine of DNA (1993), and Not in Our Genes: Biology Ideology, and Human Nature (with Leon J. Kamin and Steven Rose; 1984). Finally, Lewontin has served as a mentor for many PhD and postdoctoral students, in philosophy in addition to biology, including my longstanding philosophical collaborator Elliott Sober.

I talked by phone with Lewontin on March 2 2015. In his mid-eighties, he is still scientifically active and could recall his collaboration with Gould in detail. Our conversation is highly relevant to the “Just so story” critique that is frequently leveled against Evolutionary Psychology.

DSW: I’m so happy to be talking with you and thanks for making the time.

RL: I don’t know if anything I can say to you at this stage has any usefulness.

DSW: I think that is overly humble on your part.

RL: You can’t be overly humble.

DSW: That’s true. Humility is a religious virtue but it is also a secular virtue and a scientific virtue. I couldn’t agree more. I am interested among other things in social history. To get started, you are one of the preeminent evolutionary biologists of our time—many achievements. In 1979 you felt the need to write this article with Stephen Jay Gould that became a classic.

RL: Sorry—which article is that?

DSW (laughs): The Spandrels of San Marco.

RL: I thought it was but I didn’t remember the exact date.
DSW: I wonder if you could tell me—what were the circumstances that moved you and Steve Gould to write this article?

RL: Sure, I can give it to you in detail. I was invited by, I think it was the Royal Society, to come and give a lecture. For one reason or another that I can’t remember, I couldn’t go. So I asked if it would be alright if I asked Steve Gould if he would go in my place—Steve and I were teaching evolution together—and they said sure. So Steve went and he gave a talk from the standpoint of what interested him at the time, which was the notion that some traits arise simply as a structural byproduct of selection on other traits, and he chose to call them spandrels. I did make a contribution to the written version of that article, but most of it was Steve’s.

DSW: Ok!

RL: Now I should warn you about my prejudices. Steve and I taught evolution together for years and in a sense we struggled in class constantly because Steve, in my view, was preoccupied with the desire to be considered a very original and great evolutionary theorist. So he would exaggerate and even caricature certain features, which are true but not the way you want to present them. For example, punctuated equilibrium, one of his favorites. He would go to the blackboard and show a trait rising gradually and then becoming completely flat for a while with no change at all, and then rising quickly and then completely flat, etc. which is a kind of caricature of the fact that there is variability in the evolution of traits, sometimes faster and sometimes slower, but which he made into punctuated equilibrium literally. Then I would have to get up in class and say “Don’t take this caricature too seriously. It really looks like this…” and I would make some more gradual variable rates. Steve and I had that kind of struggle constantly. He would fasten on a particular interesting aspect of the evolutionary process and then make it into a kind of rigid, almost vacuous rule, because—now I have to say that this is my view—I have no demonstration of it—that Steve was really preoccupied by becoming a famous evolutionist.

DSW: So he was trying to grab center stage at every opportunity.

RL: Yeah, I think so.

DSW: Is the Spandrels paper like that?

RL: Well, I made a lesser contribution than he did. Most of the Spandrels paper was written by Steve. There is a section in there, which one can easily pick out, where I discuss the various factors and forces of evolution...

DSW: Yes, I can see that division.

RL: That paper never would have been written by us as a joint paper if I hadn’t asked Steve to go to the Royal Society and give a talk in my place.

DSW: Fascinating! To what extent was this paper motivated, either for you or Steve, by Sociobiology?

RL: Well, I don’t know to what extent it was motivated. Sociobiology was certainly contextually relevant. I think we would have written exactly the same paper if Sociobiology had never come into existence. Looking back, that’s hard to say, but I think the idea was to avoid selective caricatures, the making up of selective stories just because you felt you had to. That is, as you are aware, a very common phenomenon in writing about and teaching evolution. For example, why is blood red? The fact that hemoglobin happens to have that absorption spectrum in the visible is not sufficient for some people, who have to show that it’s a good thing that blood is red because it scares off predators who come and scratch you and stuff like that. So we had a lot of that sort of thing to deal with—what we called “Just so stories”.

DSW: So forget about Sociobiology—basically, you saw naïve adaptationism being a problem in general in the field of evolutionary biology?

RL: Exactly. It was really not Sociobiology itself, but a tendency to try to find, in every instance, some selective advantage for things. We were teaching the students—and Steve was not as keen on that as I was—that there are a whole variety of forces that give rise to observed traits and they are not all directly selected for.
DSW: This interests me very much. I’m interested to know that was the primary motivation for the article, not Sociobiology.

RL: Yeah.

DSW: What’s the right way to do it? When I talk about the adaptationist program, I say that an adaptationist hypothesis is often the best way to start because it doesn’t require much information to know what an organism should be like to be well adapted to its environment. So it’s a good starting point, although certainly not the end point. Then the inquiry can go in a direction where you decide—as with the color of blood, by the way, an example that I use myself—that this is probably not an adaptation. You can arrive at the truth of the matter. But thinking in adaptationist terms is part of the process. So what’s the way to do it right and what’s the role of adaptationist thinking in an appropriate procedure?

RL: Well (laughs), you’re asking me what the right way to do it is. I think the right way is to start with the sentence: “We do not have any hard evidence of the forces leading to the following evolutionary change.” There has to be a prelude to the discussion of evolutionary change to make it clear that although the theory of natural selection is very important and happens lots, there are other forces, or other mechanisms, that lead to change and we are not obliged by being Darwinians and being evolutionists to invent adaptive explanations for all changes. I think that’s where you have to start. Then, as either a philosopher or biologist, ask in a particular case what is the direct evidence, besides the desire that we want to find something, that a particular story is true or not true. Most of the time we’re going to have to say that this happened in the Eocene or the Paleocene and we haven’t the foggiest notion of why it happened. I think the admission of necessary ignorance of historically remote things is the first rule of intellectual honesty in evolution.

DSW: Good. Thank you for saying that so clearly. At the same time, sometimes the past can be inferred with amazing certainty. All the historical sciences are like that, right?

RL: Right. And so, I think that the right general strategy for explanation, writing, and teaching is to begin with some really clear cut cases where we have in our very hands the evidence for a particular causal pathway—a greater reproduction and survivorship of one form versus another—and then move from that to living cases where we’re not quite so sure because we can’t actually count the number of offspring of each type, and so on, to somewhat hazier cases, and then go back to extinct organisms and evolutionary past and say, we could make up a good story, but we don’t know how to show that it’s really true.

DSW: Right. There is the humility again that began our conversation. Although I have to say that it was the lack of humility, in part, that caused the Spandrels article to become so widely read. Steve Gould the showman put that on center stage and it became a three ring circus for a long time, as you know. The adaptation wars and all that. I don’t know whether that is good or bad.
**RL:** All we can say is that sometimes the stories are right and sometimes they’re wrong. One way to deal with this is to take a case where we can in fact—because it’s present and we have the organisms, and we really test what’s going on. Take such a case and put it aside and don’t even mention the result of our determined discovery about it, and make up three stories, all of which sound perfectly biologically realistic and reasonable, and then give the right answer and say now, aside from the necessary observations and experiments—forget the real answer, why should we choose one of these versus another?

**DSW:** Uh Huh. Interesting.

**RL:** What we have to decide is whether we’re going to put behind us certain motivations, one of which is the general motivation to struggle against religious anti-evolutionary views, and at the other extreme to be as individuals successful as evolutionary biologists by giving an explanation of something interesting even when we don’t have the observations. I was raised not as an evolutionist but as a population geneticist.

**DSW:** Right.

**RL:** That’s a big difference.

**DSW:** Why is that a big difference? Let’s clarify that for me. I tend to see it as a small difference. What’s the difference between being a population geneticist and an evolutionist?

**RL:** A population geneticist by theoretical training has certain parameters of population change. That’s become broadened by the realization that there are between population changes and so on, but within a population we’re talking about changes in gene frequency and we have a catalog of the causes: selection, inbreeding, chance, mutation, and so on. Our job as population geneticists is to do the necessary observations of the various things that give us estimates of the strength of those different forces. Now, historically one of the most interesting—now I want to talk a little about the sociology of our science—Theodosius Dobzhansky, my professor and then greatest living evolutionary biologist...

**DSW:** Mr. “Nothing in biology makes sense except in the light of evolution…”

**RL:** Yeah, right. He was a very bad field observer. Theodosius Dobzhansky never, in his entire life, nor any of his students, me included—I would go out in the field with him, actually—even saw a Drosophila pseudoobscura in its natural habitat.
DSW: Right! No naturalistic context whatsoever.

RL: None...at...all. And to this day we do not know anything about the actual habitat of Drosophila pseudoobscura, although by the way, interestingly enough, in more recent years, Tim Prout actually succeeded in trapping pseudoobscura in orange groves, so we don’t even know how much they hang out with cultivated fruit.

DSW: Right.

RL: Now let me go one step further because we cannot understand the development of evolutionary biology if we don’t understand questions of the sociology of academic life. If I wanted to study evolutionary forces acting on some genetic polymorphism in Drosophila, I would go and look for some species of Drosophila where I could actually look at, perturb, and work with the actual breeding sites and egg laying sites and pick up larvae in nature and so on. And in fact there is such a group of Drosophila. They the cactophilic ones. There is a group [of scientists] from Texas and other places that studies the cactophilic Drosophila in an ecologically sensible way of going to the rot pockets and perturbing them, getting larvae out of them and so on. That group never acquired the prestige associated with the Dobzhansky school because—I don’t know why. They were doing what one has to do. That’s why, for example, I try to convince students who are entering evolutionary biology not to study animals at all but to study plants. Plants stay in one place. You can manipulate them. You can move them. Plants are much better than animals for studying things in nature. Yet, plant evolutionary biology is not, for sociological reasons that I don’t understand—I could make up stories—has never had the prestige that animal work has had when it comes to population genetics.

DSW: Right. I think that [there was an] all consuming interest in physical mechanisms as opposed to a more fully rounded approach. I place a lot of emphasis on the classic paper by Niko Tinbergen, “The Methods and Aims of Ethology”, in which he says that you have to ask four questions: Function, History, Mechanism, Development. Are you familiar with that paper?

RL: No, I’m not. Send me a reference to it.

DSW: It’s such a succinct summary of what a fully rounded approach needs to be. Dick, I’d like to spend a little bit of time on Sociobiology and also Evolutionary Psychology, because even though that didn’t motivate the Spandrels paper, it still motivated you to be a critic and Steve too. I wonder if you could bring us back to that point and what you saw as problematic about Sociobiology and then Evolutionary Psychology.

RL: This is what I have been talking about for the last five minutes. This is a branch of academic life that consists entirely, as far as I can see, of making up what would seem to be plausible stories. I would say that’s not what we are in business to do. I don’t know what else to say. Look, when I look at Sociobiology, the book or some of the other books he [E.O. Wilson] has written, it drives me mad. For example, if you read—I’ll take an extremely nasty example because it’s so clear—it is written that aggression is a part of human nature. It says that in the book. It lists features of human nature and aggression is one of them. So then I have said to Ed and others of his school, what do you do about people who have spent almost their entire lives in jail because they refuse to be conscripted into the army? What do you think the answer is? That is their form of aggression.

DSW: Well, OK, that’s facile.

RL: I don’t know what you can do about it. If everything can be said to be a form of aggression, even the refusal to be physically aggressive, what kind of science is that?
DSW: Would it be more acceptable to say that aggression is part of the repertoire of human behavior? That leaves it open to be part of the repertoire that we don’t always use.

RL: Before I will allow you to make even that statement, I will insist that you write down how I know whether any particular phenotypic manifestation is or is not included in your definition of aggression.

DSW: That’s fair.

RL: I don’t know what those people would say. But if you are willing to make a clear enough—and most important, and this is perhaps the fundamental contradiction, potentially there has to exist a group of cases of non-aggression. Because if everything by definition can be shown to be aggression then it ceases to be a useful concept in our scientific discussions.

DSW: The problem of explaining everything, and therefore nothing, recurs again and again. What were some of the political implications of Sociobiology that worried you? Misuses of biology, or misuses of evolutionary reasoning—back then and are they still with us today?

RL: My main complaint is not the list of specific manifestations but the underlying claim that there exists a human nature, which then the claimant must give examples of, and so each claimant gives examples that are convenient for his or her pet theory. I think the worst thing we can do in science is to create concepts where what is included or not included within the concept is not delimited to begin with. It allows us to claim anything. That’s my problem with Sociobiology. It’s too loose.

DSW: That brings us to the topic of cultural evolution, which is something that I study a lot and I think that you have thought of a lot. One of the exciting things that I think has taken place is the idea that the study of evolution became too gene-centric over the course of the 20th century. Evolution requires heredity, not genes, and there are other mechanisms of heredity. Culture really is an evolutionary process. That is in part why we are so open-ended, why there’s not a human nature in terms of a fixed human nature because we’re so adaptable. I wonder how much you have thought about cultural evolution in that way and if you have any comment to make upon what studying culture as a genuine evolutionary process with its own inheritance mechanism, including symbolic thought—the evolution of meaning systems—what that does to change the picture of evolution.

RL: I think that the evolution of this thing that’s in our cranium, however it happened, has changed all the rules for the history of the species, for its biology, for everything about it. I mean, rational thought and the kind of communication we have with human language, as opposed to the stereotypical communication of other animals, has really made a fantastic change in the conditions of life and the rates of reproduction of individual types and so on. I would say human evolution is in that sense unique because of the possibility of: a) the details of communication; b) the notion of historical memory; well, everything about human thought. I really do think that if we want to understand evolution, the first species we should keep out of our consideration is Homo sapiens. I’m sorry, but that’s the way it is for me.

DSW: Well, but that’s genetic evolution. What if the principles of cultural evolution, although different in some respects—because another inheritance mechanism needs to be functionally an inheritance mechanism but need not resemble other inheritance mechanisms in detail—what if the basic way of going about genetic evolutionary reasoning also could be employed for cultural evolutionary reasoning? Wouldn’t that put evolution back in the game for the study of human cultural evolution?

RL: Well, let me ask you a question. Why do you use cultural evolution instead of cultural history? Why evolution instead of history? Can you avoid—let me put it another way—can you generally avoid the false similarities, the made up structures that we are criticizing, if we continue to use the word evolution when what we really mean is historical change?

DSW: I’ll actually take you on, on that issue. I’m not sure how much time we’ll have during this conversation. What the mechanisms of human cultural evolution do is adapt human populations to their environments, often with a fine degree
of sophistication. When you just say cultural history you’re being agnostic about adaptation. Cultures adapt to their environments. [Without a history of adaptation] they could no more survive and reproduce in their environments than a genetically evolved species. Just to say history leaves out adaptation, don’t you think?

**RL:** But the problem, here, is that it’s a form of adaptation that hasn’t been studied enough in animals and plants, which is that each change in the species changes what we call the environment, so there is a co-evolution of organism and environment. Historical change in our species has been increasingly the consequence of the organism itself. We’re inventing it all. As the brain grew into what we now have, it became the chief mechanism by which organisms constructed their environment. Look, let me interject here. I think it is extremely important to go to a fundamental issue, which is organisms create their own environments. All organisms make their niches. The whole notion of ecological niche is a very bad notion. There are no niches without organisms. This notion that there is a hole in the world that the organism evolves to fill. The organism by its evolution changes the conditions of its life and changes what surrounds it. Organisms are always creating their own hole in the world, their own niche.

**DSW:** You pioneered the concept of niche construction, which has become a hot topic.

**RL:** I think that one mustn’t see niche construction as a special issue. There are niches and then there is reconstruction of the niches. My claim is a very strong one and I could be wrong: there is no niche without an organism.

**DSW:** I’ll accept that provisionally, but there is such a thing as a purely physical environment...

**RL:** A physical world, oh yeah, but let me tell you one of my favorite seminars that I ever heard. I can’t trace it any more, unfortunately. A guy came to Chicago and gave a talk in which he showed motion picture photographs of all kinds of organisms, plants and animals, using what are called Schlieren optics, which are sensitive to differences in optical density. What he showed was every organism of which he took moving pictures—both plants and animals—have around them a layer of warm moist air—even trees have it—which is being produced by the organisms themselves. So every organism, at least every terrestrial organism (I don’t know about aquatic ones) is by its metabolism producing a layer of warm moist air with certain gases in it that are its immediate environment.

**DSW:** I think that’s probably even more so for aquatic organisms. You make your point very nicely. Each organism is manufacturing its own local environment.

**RL:** Exactly. That’s the wind chill factor. That’s why it gets colder when the wind blows.

**DSW:** Right. But that makes it a complicated evolutionary story. It’s still an evolutionary story, and when you just say history you’re leaving all that out. History seems to me too broad. Sure everything is history but we’d like to say something more specific. If there is a process of adaptation going on, even if it’s one of rapid niche construction and coevolution, that’s still a more specific set of ideas than just plain history, which really does encompass everything and therefore nothing. Don’t we want to use some of those more specific ideas about adaptation and coevolution and niche construction? That’s more than just history!

**RL:** Oh no, I’m with you! If I could convince people to use that notion of niche, not as a fixed thing, but as something that is manufactured by the organism, I would be very very happy. But when I talk to biologists about it, they’re always surprised.

**DSW:** It is still a new idea, in part of course because it’s a complex idea. Complexity is complex, it’s hard to study. We’re always trying to keep things simple, even when we should be embracing complexity in some sense.

**DSW:** What a pleasure, Dick! Thank you so much for this conversation. Have a great day.

**RL:** You too.

March 29, 2015
Debra Lieberman is part of the 2nd generation of evolutionary psychologists. I’m proud to have introduced her to evolutionary thinking when she was an undergraduate student at Binghamton University, when I was still teaching a single course on Evolution and Human Behavior and before I helped to start EvoS, Binghamton University’s campus-wide evolutionary studies program. She obtained her PhD from the University of California at Santa Barbara with Leda Cosmides and John Tooby as her mentors. She is currently an associate professor in the Department of Psychology at the University of Miami in Florida.

Debra’s research is an excellent example of how evolutionary thinking can inform a detailed research program in cognitive psychology. She recently visited Binghamton University to give a seminar in our EvoS seminar series, which was a homecoming of sorts.

DSW: Debra, you are doing the most wonderful work on the cognitive psychology of kin interactions and most recently the psychology of gratitude. You did your undergraduate work at Binghamton and then went on to get your PhD with Leda Cosmides and John Tooby—so you’re the perfect person to talk with about evolutionary psychology. I wonder if you might begin at the beginning, how you got turned on to this, your experience at Santa Barbara, and then fast forward to present. We want to center this on the whole topic of evolutionary psychology.

DL: Let’s see… the beginning. I was always interested in human nature. At Binghamton I was a biochemistry major and when I saw your class, “Evolution and Human Behavior”, I sensed controversy and set sail. We read Homicide and The Adapted Mind—two books that changed my life. It was bizarre to me that it was controversial to talk about humans in the same way that we talk about other critters.

DSW: I remember you coming into my office and venting your anger and frustration at the other courses you were taking and how they didn’t get any of this.

DL: As an undergraduate, you think all your elders are on the same page about what it means to be human and where we all came from. Talking to biologists it is fine to talk about sexual selection and parental investment but—wait a minute—when you’re talking about humans, it’s all “learning” and “culture” and I found this strange. Then you talk to psychologists about relationships and they’d say, “Wait, what is this sexual selection and parental investment? No, we’re humans.” It was a bizarre situation to see that the whole biological world was shaped by these principles, but it just wasn’t applied at all to humans. Strange. So there was controversy—I was in.

At the time there were very few graduate programs that did this. You were pretty much the only one, and then because I read the books, I knew about [Martin] Daly and [Margo] Wilson at McMaster, David Buss at Michigan (back then), Randy Thornhill at New Mexico, and John and Leda at Santa Barbara. Those were the four applications I put in for grad school.

When I met Leda, she told me I’d been accepted and that I should come work with her. I was like “Wow, that’s why I applied!” Santa Barbara was a great experience and I feel fortunate to have joined John and Leda’s lab.

DSW: What was the intellectual climate there? Describe your experience.

DL: As a first year grad student, I took a pathogenesis course in the biology department that I loved, because I was intrigued by the idea that disease organisms could manipulate host behavior. I started to work with one of the biology professors who studied fish that, when parasitized, would swim to the top of the water column where the parasite’s next host, the bird, would eat the fish. I wanted to understand what
the parasite was doing to the neuro-circuitry of the fish that caused the fish to behave this way. So I started in on a project solo, and after a few weeks, John and Leda tapped me on the shoulder and said, “Look, this is interesting, we support you, we’re biologists, but if you’re going to do psychology you need to do more with humans or you should think about another program.”

So immediately I dropped the project and John said that he and Leda really wanted to look at kinship and to understand the cues to kinship. That’s where we started. The “Westermarck effect” was well known — the idea that early association during childhood leads to an aversion later in adulthood. But we wanted to ask, how do individuals figure out who their siblings are? What are the cues? So we developed a huge questionnaire. I started to analyze the data. I came up with a very weak effect of how co-residence predicts sexual aversions. I thought to myself, this is terrible. It was significant but in psychology an effect size of .2 is nothing to write home about. How could something so powerful as an inbreeding avoidance mechanism—if co-residence was really the mechanism—how could I get a .2 effect size? I always thought that if you truly carve nature at a joint, you should see very large effects! We started thinking about other possible cues and this less us to split the sample into older and younger siblings. It totally changed the results. In our data, for people with younger siblings, co-residence no longer predicted sexual aversions, but for people with older siblings, the effect of co-residence was huge. It was a moment of holy crap! A true eureka moment. We talked about it and developed another survey to further test it. That’s what led to our understanding of how siblings recognize each other.

DSW: Which is that it differs because of the information available. Maybe you can summarize those results.

DL: We were interested to know if there was a particular mechanism in the brain that lets siblings know they are related. A very reliable cue to knowing that another individual is your sibling is seeing your mother give birth to and care for it—but that’s only available if you’re the older child. What does the younger child do? The next reliable cue is seeing who your mom invests in over the long haul and that’s what we’ve come to know as “co-residence duration”. This is what happens when you live with someone for a long time and see evidence of shared parental investment. It turns out that if you’re the younger sibling you track parental investment: the longer the co-residence duration, the more certain you can be that the older child is, in fact, your sibling. The older siblings use the cue of watching their mother invest in a newborn. They don’t also use co-residence duration, presumably because of the reliability of seeing mom care for an infant. But in terms of computations, the two cues could have combined—but they don’t appear to.

DSW: It looks like one trumps the other.

DL: That’s right. With this information in hand, we were off to the races. We thought—is this a strange thing that’s happening in Santa Barbara? So I tested it in Hawaii, in Dominica, and working with colleagues we replicated it in Belgium, and in Argentina, so it’s been replicated in a number of places.

DSW: One of the distinguishing features of the Cosmides/Tooby school of evolutionary psychology is massive modularity: that there are many special purpose adaptations to solve the many adaptive problems of life in the ancestral environment. This is a case of an adaptive problem. You want to help your kin but you don’t want to mate with them. Presumably all this cognition came to exist somehow and the supposition is you can’t learn this stuff, it doesn’t even happen repeatedly.

DL: I would say you do learn this stuff. What counts as learning? You’re taking in very specific information from your social environment regarding parental investment in another child.

DSW: Yes, but it’s a very highly structured form of learning, and one that is so context sensitive that it can be different for an older sibling compared to a younger sibling. All of that has to be scripted and the scripting takes place through a process.
of genetic evolution. So this becomes a poster example for the concept of modularity. One of the best examples I know, at least. Am I rendering it the right way?

DL: Yes, I would say so. In my work, I like to put together information-processing models: I think, if I were natural selection, how might I have designed the system to achieve inbreeding avoidance or kin directed altruism? [In the case of sibling detection], you have cues from the environment that are input and our data tell us that they’re not just added together, they’re integrated and that’s suggestive of a [neuro] mechanism that’s doing the integrating and then calculating the degree of kinship.

Is that integrator, that kinship cue integrator, is it specific for siblings? Is it a general mechanism that takes all kinship cues and then estimates relatedness? If so, what are the inputs into this kin detection estimator? Are they specifically sibling cues or is there a separate father detection system, mother detection system, and so forth? These are things we don’t know. Right now, I’ve actually stopped short and not said it’s sibling detection but rather it’s a kin detection mechanism until there’s further evidence that it needs to be split up. The Santa Barbara school of thought is computationally, functionally specific, and until there’s evidence that something can be split off, then it should be retained in a more general system (and by general, I mean functionally less narrow). That’s what we currently see in the kinship system and my work on disgust is the same: Is there a singular disgust? What might that look like computationally?

DSW: That’s a good example because we both know from Paul Rozin’s work that disgust is something that has phylogenetic roots but has been culturally elaborated in humans so that we now feel disgust for all sorts of things. That’s a great example of the middle ground I’m searching for which has both these biological and cultural inputs. Your work gravitates toward this middle ground.

DL: Robert Kurzban and Peter Desctioli have two papers on the evolution of morality, on the mysteries of morality. They thought through how disgust has a flexible relationship with morality and how disgust can lead to such a rich array of norms. My ideas about the relationship between disgust and morality really came from the two of them.

DSW: Great. Let’s now talk about evolutionary psychology. As you know TVOL is doing a multi article theme on it. Talk to me now about evolutionary psychology’s reputation—is it deserved, undeserved?

DL: I strive to understand the scientific gripes people have [with evolutionary psychology], not the personal ones, which have no place in science. I often find that people say John and Leda are wrong because they completely misinterpret or ignore what John and Leda say. Don Symons is fond of saying that you have to understand whom someone is arguing with to understand why they’re writing what they’re writing. John was arguing with cultural anthropologists and Leda was arguing with social psychologists. So their beef was largely with existing strands of academics that didn’t take evolution seriously or didn’t believe there could be structure to the mind. The Adapted Mind will be a book for the ages—love it or hate it! Some have argued that they went too far. I would say that to make a point you have to go to the wall. I personally don’t think they’ve gone too far in their discussion about the Standard Social Science Model (SSSM) of the mind.

DSW: Describe that for our listeners.

DL: It’s a view that the human mind is blank slate and has content independent mechanisms, which means that you can feed these mechanisms with any content and they will operate under any circumstance with equal functionality and produce equally effective behavior. That’s just not the case at all. Non-human animal studies show the insanity of the SSSM. People might be uncomfortable and squeamish with an evolutionary perspective, [thinking] that it might hold them to a certain moral disposition. But you can follow the principles of evolution, apply them to human behavior and still be a good person—still believe the best in humans. In the Adapted Mind, Leda and John say that if you’re concerned about genetic determinism, you should be no less concerned about environmental determin-
ism. Another thing that bugs me is the claim that John and Leda ignore culture. Did these folks not read the subtitle of The Adapted Mind? It is “the generation of culture”. People forget that they were very interested in how we get human culture.

**DSW:** Can you take a few more steps and describe how culture is generated.

**DL:** Sure, but what do you mean by culture?

**DSW:** I would rely on John and Leda’s distinction between evoked culture and transmitted culture. They associate transmitted culture with the SSSM as though people were open vessels and culture is poured into them from the previous generation. Whenever human populations do something different, this could be attributed to transmitted culture. Against that background they made an important point. Since we’re all phenotypically plastic, if you place us in different circumstances then we behave differently because our minds react to our environments. That’s evoked culture. Evolutionary psychology should embrace both of those. If you were to say, evolutionary psychology is about evoked culture and that transmitted culture is something else, I would not agree with that.

**DL:** My own research speaks a lot to culture. If we in fact have representations of who counts as siblings, then it’s not surprising that we have linguistic terms that map onto these very specific representations. I’m told that the Chinese language even has different terms for older sibling versus younger sibling, which is fascinating. We delineate different relationships linguistically and so this enters into our culture, but just looking at the kinship terms without the psychology is just strange, since that wouldn’t give you a full-fledged understanding of kinship. If you started with the psychological adaptations and had an informed model that you can test and understand, you see that there’s a system in all humans that generates representations of different types of kin. And this structures our social interaction and cultures in various ways.

One of the tools I gained at Santa Barbara is to get very specific when discussing kinship, and to ask, what domain are we in? What’s the system? Is this a novel human thing? Maybe there’s not a dedicated system for a particular behavior, or maybe it’s piggybacking on something, or maybe it’s a byproduct of something else. I try and ask all of these questions.

I think evolutionary psychology provides the tools to develop and test the models and to understand the structure of the human mind. It provides predictions about the sort of models out there in the world of culture that you might see. Human culture is not random. There’s a lot of flexibility and variety to be sure, but we tend to observe only a limited set of what’s possible.

**DSW:** That’s a great topic because kinship systems are famously diverse. To quickly cut to an example: the Nuer African tribe were in the process of replacing their neighboring tribe, the Dinka, when contacted by Europeans in the 19th century. Part of the reason is because the kinship system of the Nuer enabled cooperation between the villages. They added an extra tier to their kinship terminology so that you might have someone classified as a kin in some distant village. By virtue of having this kin [which was fictive as far as genetic relatedness is concerned], they combined forces in warfare. Because the Dinka had another kinship system that didn’t extend so far, they couldn’t form as large a fighting force. None of these kin were strictly speaking kin and if they were, their coefficient of genetic relatedness would be low. The Nuer even had a convention of ghost marriage where if the Nuer husband died they’d replace the household with a Dinka male who was captured in warfare. This makes no sense genetically but it kept the social organization intact. These are wonderful examples of kinship systems that go way beyond one based on genetic relatedness. This kind of cultural construction can interface with genetic adaptation and will result in some forms surviving and replicating better than other forms. There is an ongoing process of cultural evolution.

Now I want to go in a slightly different direction, involving another toolkit for understanding these mechanisms. It would be nice to go bottom up through neuroscience. Who is doing that well? Is there anyone you can point to, or is that an area that needs more attention? And how about Leda and John? Are they doing it or encouraging it to be done?
DL: It would be very nice to have the whole story for each psychological adaptation. To have a catalogue of human psychological adaptations and describe the genes that are required all the way through the regions of neural tissue that tend to embody certain functions. I’m interested in describing adaptations at an information-processing level. I’m not as interested in the specific genes or the location. I assume there are genes that associate with kinship systems and they organize neural tissue to do this somewhere. If had multiple lifetimes to completely do it I would explore this.

DSW: I know Evolutionary Psychology was inspired by the work on the cognition of vision, which is massively modular, so on that level it has been the main event for neurobiologists for a long time. I want to end by taking about what EP has to say about sex differences. I know some feminists are critical of EP and that you have an interesting take on it.

DL: What do I think of sex differences? That they exist! I’m impressed more and more about how they exist. Specialized types of sex differences constitute mating psychology. Having been out on the mating market recently, I found myself talking about relationships A LOT. When I would talk to some of my male friends about how to get a mate, I could swear they were speaking English, but it just didn’t compute. It has become even more clear that men and women see very different dimensions when it comes to finding a mate.

On a related topic, in Binghamton I read Camille Paglia and she said something that rang true with me. She pointed out the importance of being responsible for your own actions, and part of that, in my mind is equipping yourself with knowledge and when it comes to sexual abuse and rape, this means knowledge about the other sex. In a perfect world, women could wear what they want, walk the streets naked if they so desired. But we don’t live in that world. Men and women have different psychologies. Understanding psychology would help women understand and navigate the sexual world and also be more safe.

DSW: That puts you in agreement Randy Thornhill and Craig Palmer who criticized feminism about practical things.

DL: I wouldn’t want people to interpret what I say as justifying rape. I am not blaming the victim. The fault is with the person who does harm and forces himself on another person. The question is, could it have been prevented? I think it’s preventable if women have greater knowledge about male psychology. A new chapter for sex ed.

June 1, 2015

References
Until recently, evolutionary psychologists considered behavioristic accounts of human behavior incompatible with evolutionary theory. They characterized B.F. Skinner’s work merely as part of the “standard social science model” and gave it scant attention. But Skinner was in fact an evolutionist who extended evolutionary thinking to the selection of behavior. He argued that the open-ended capacity for behavioral and cultural change was itself an evolved capacity of the organism and an evolutionary process in its own right. In essence, we could study behavioral development according to the same principles of variation and selection by consequences that were involved in genetic selection (Wilson, Hayes, Biglan, & Embry, 2014).

Over the past 50 years, pursuit of this insight has led to considerable progress in our ability to treat and prevent most problems of human behavior (Biglan, 2015). Beginning in the 1960s, research conducted within the behavior analytic field began to show the impact of positive reinforcement on human behavior. That work has contributed to numerous effective interventions for nurturing prosocial behavior in families (e.g., Patterson, Forgatch, & DeGarmo, 2010), schools (Horner et al., 2009), and workplaces (Daniels, 1994).

Selection by consequences can account for most human conflict. Conflict not only results in interpersonal violence, it contributes to the development of most of the psychological and behavioral problems of children and adolescents, including antisocial behavior (Biglan, Brennan, Foster, & Holder, 2004). Recently, studies have identified a link between cardiovascular disease in middle age among people who had faced the stress caused by childhood conflict (Miller, Chen, & Parker, 2011).

Empirical work on the selection of conflict and aggressive social behavior began with the work of Gerald R. Patterson and his colleagues (Patterson, Reid, & Dishion, 1992). They directly observed moment-to-moment interactions in families of aggressive children in an effort to understand why such behavior would develop. The result was an empirically based theory of coercion (Dishion & Snyder).

Comparisons of the interactions in these families with the interactions of families with little aggression showed that the aggressive behavior of the child — and other family members — was selected by its benefit in producing brief respites from the aversive behavior of others. Humans have evolved the capacity to receive reinforcement through the cessation of attack or threat by others. In families with aggressive children, children and adults alike get relatively little positive reinforcement for prosocial behavior; the family is less likely to listen to them, hug or smile at them, play with them, etc. Instead, family members are more likely to tease, criticize, yell, hit, and ignore each other.

Analysis of the moment-to-moment interactions in these families showed that family members would engage in “bouts” of aversive interactions, which ended when another family member escalated their behavior in a way that got the first person to stop their aversive behavior. A child might repeatedly tease or whine while a parent kept telling the child to stop. Eventually the parent might yell or hit the child and the child would stop whining. Because their aversive behavior was occasionally successful at getting others to back off, family members began to select aversive behavior habitually.

Subsequent research showed that marital conflict persisted due to the occasional success of marital partner in getting the others to cease their angry, critical, or argumentative behavior (Patterson & Hops, 1972). And, in work I did with Hyman Hops (Biglan, Hops, & Sherman, 1988; Biglan et al., 1985), we found that the depressive behavior of mothers received reinforcement thanks to the brief respites such behavior produced when the other family members behaved aversively.
Patterson and his colleagues followed the lives of aggressive and non-aggressive children into adulthood. At the time this work began, no one believed that such mundane unpleasant interactions in families could account for the development of life-long criminal behavior. But that is precisely what they found. Children whose aggressive repertoires took shape in these families arrived at school lacking cooperative, prosocial behavior and, as a result, failed to learn and then faced rejection by their peers. Numerous studies have tracked the trajectory of these children as they joined peer groups of similarly deviant children and begin to use drugs, to engage in delinquency, and to have children at an early age. Recent work suggests that this pattern of behavior is consistent with the thesis that in a threatening world, having babies early may be the only means of survival of one’s genes (Dishion, Ha, & Véronneau, 2012).

Understanding the coercion process has contributed to development of a host of interventions that can significantly reduce the burden of problematic human behavior. In my new book, The Nurture Effect, I describe numerous family and school interventions that reduce coercive interactions and increase positive reinforcement for prosocial behavior. These interventions have proven capable of preventing the development of delinquency; tobacco, alcohol, and other drug use; academic failure; and depression.

They do so by making environments more nurturing. The key features of nurturing environments are that they (a) keep coercive interactions to a minimum, (b) richly reinforce prosocial behavior and values, (c) limit influence to engage in harmful or risky behavior, and (d) support a resilient approach to life in which people pursue important prosocial values even in the face of significant challenges, including distressing or discouraging thoughts and feelings.

We can evolve a more nurturing society by widely implementing tested and effective programs for families, schools, and workplaces. But in addition, we need to understand and modify the larger social context that affects families, schools, and workplaces. In my next essay, I will describe the recent evolution of American society, how it has contributed to increased conflict and coercion, and how we can evolve a more nurturing culture.


March 29, 2015
Why do modern people have so few children?

If you’re reading this article, you probably believe that Darwinian theory can shed light on human behaviour. And you probably think that this article is going to be an account of how the theory can explain yet more of the puzzling choices that humans make.

I’m afraid not. This article is about a mystery that Darwinian theory has no ready answer for—at least not yet. It’s the mystery of our own reproductive choices. Darwinists have had a good deal of success convincing the general public that evolutionary theory can provide a means of gaining an understanding of their own behaviour. But we do this by weaving tissue-paper thin scraps of argument to hide the trunk and tusks of the beast in the room.

Why are we producing so few children?

At a superficial level, it isn’t puzzling. Each of us can give our own reasons for not producing a large number of children. For one thing, the kind of effort that would be necessary to raise a large family would leave us little effort to spare for learning about Darwinian theory and reading articles like this.

But we Darwinists aren’t satisfied with superficial explanations so it’s time we discussed this. Today’s humans are able to access vastly more resources than our ancestors but we choose to devote them to purposes and activities that don’t enhance our fitness. Let’s face it; over the course of our lives we invest a relatively small proportion of our time and effort producing and raising offspring or helping our close relatives raise theirs. Our ancestors, or at least most of them, were different; all of them had at least one child that survived to reproduce and many of them succeeded in raising large families in conditions that we can barely imagine surviving. According to one of the basic tenets of Darwin’s theory, we should have inherited the characteristics associated with this reproductive success. So what drove them to put so much effort into producing children and why haven’t we inherited it? Perhaps you have a theory. If so, please share it in the comments.

The desire to mate seems to be an important driver of reproduction in many non-human animals. They aren’t motivated to produce offspring, just to pursue a set of behaviours that resulted in their ancestors’ genes being
passed on to them. Human behaviour suggests we also experience desire to mate—but it is unlikely to be a very important driver of reproduction in humans. It doesn’t fit with our reproductive biology or our behaviour.

Raising a human from conception to independence requires an enormous amount of parenting effort, more than can be provided by its mother alone or its mother and father working together (Hrdy 2009). Conceiving a child when support isn’t available would have been very detrimental to fitness. It follows from this that an uncontrollable desire to mate would have been strongly selected against, certainly in females. Every human population that has been studied has rules which establish responsibilities of parents and their supporters. These rules strongly influence who reproduces and when they reproduce. It’s impossible to know the extent to which our ancestors actually obeyed those rules. No doubt reproductive norm compliance varied from population-to-population, from time-to-time and from individual-to-individual but it’s likely that the most successful people were people like us—people who obeyed most of the rules most of the time.

Our own experience of being human tells us that we’re motivated to pursue goals that seem within our reach and worthy of our effort. Except for the last few generations, our ancestors behaved as if they believed that raising children, as many as possible, was a worthy goal, a top priority. Most young adults today don’t believe this. Why not? What has changed?

Well, many things have changed in the last couple of centuries and this has given social scientists scope to propose many possible “solutions” to the mysterious decline in human fertility. Here is a sample of the explanations offered:

- There was no birth control technology then.
- Women were oppressed then and were forced to have babies.
- Religion taught that it was people’s duty to have many children.
- They needed children to work on the farm and to support them in their old age.
- They expected many of their children to die in infancy or childhood.

The problem with these explanations is that they’re uninformed by Darwinian theory and by the facts gathered by historians and anthropologists studying how people in high fertility populations really behave and what was really going on in different populations when their fertility began to decline. For more information about this, future articles will explore these topics:

- How people in high fertility populations behave.
- The transition to low fertility reproductive behaviour.

Amongst this information must be some clues that will help us to develop testable hypotheses to explain the revolution in human reproductive behaviour that has occurred in the last 200 years.

We need hypotheses that are consistent with Darwinian theory. Those of us who take a Darwinian approach are able to appreciate an important thing about this revolution that most social scientists haven’t recognized: It results in humans starting to make extraordinarily altruistic choices while believing that we’re selfishly following our best interests.
This altruism has had severe fitness costs and differences in the timing of a population’s change of mind about family size have shaped modern history. The new altruism first began to take hold in the people of France toward the end of the 18th century, more than two generations earlier than the rest of Europe (Coale and Watkins 1986). In the middle of the 18th century France’s prospects seemed similar to those of England. Her farms were becoming more efficient and her traders and artisans were finding new ways creating wealth. Her scientists and intellectuals were second to none. And her colonial empire included substantial parts of North America as well as the Caribbean, Africa and South Asia.

By and large, life started to become easier for ordinary Europeans during the 18th century and, in the rest of Europe, people took advantage of this new prosperity. Families boasting of more than eight surviving children were not uncommon. But they were much less common in France. We know this in part because of the research that Jean-Baptiste Moheau (2000) did in his spare time while working as private secretary to the provincial governor in the port city of La Rochelle in South West France. He collected data on births, marriages and deaths and in 1778, when only 30, Moheau published a pamphlet entitled “Recherches et considérations sur la population de la France” (“Empirical Studies on the Population of France and Their Interpretation”). His work revealed that in parts of France fewer children were being born and more dying in infancy. He makes his feelings about this clear in his conclusion. He mostly blames women:

“…rich women, for whom pleasure is the greatest interest and the sole occupation, are not the only ones who regard the propagation of the species as a dupery of olden times; already the fatal secrets unknown to any animal but man have penetrated in the countryside: nature gets cheated even in the villages.”

The “secrets” he refers to are ways of achieving sexual pleasure without the risk of conceiving a child. He also criticises women who don’t breast-feed their babies as his data revealed that infants not fed by their own mothers suffered higher mortality. Moheau predicts that “if these licentious practices, if these homicidal tastes, spread further, they will be no less fatal to the State than the plagues that devastated it in the past.”

To modern ears, Moheau sounds crazy but, in a way, he had a point, a point, which is amply demonstrated by what followed. The practices and tastes that he complained about did spread through France and, while the rest of Europe enjoyed a population boom, population growth in France stagnated. The industrial revolution was transforming people’s lives and throughout the 19th century as young people from the countryside flooded into the areas where they could get work in factories, mines and construction sites. First Britain and then other Northern European countries became manufacturing powerhouses with vast trading empires – but not France, where there were simply not that many people to employ in industry.

The numbers leaving Europe during the 18th, 19th and first half of the 20th century are staggering.

Many Europeans decided to travel further and colonize new territory, displacing and in some cases subjugating the peoples already living there. The numbers leaving Europe during the 18th, 19th and first half of the 20th century are staggering. It’s been estimated that over two million Spanish went to South and Central America. Nearly a million and a half Portuguese settled in Brazil. South America also became the new home of 1.8 million Austro-Hungarians and 3.7 million Italians. North America received 5 million immigrants from Germany, 3.6 from Poland, 2.7 from Scandinavia, 3.2 from the Austro-Hungary, 5 million from Italy and 2.2 million from Russia. Another 10 million Russians colonized Siberia and Central Asia. But the biggest stream of immigrants came from the two large English-speaking islands off the coast of Europe, Great Britain and Ireland. Thirteen and a half million went to North America and another three million went to southern African
and Australia. The fertility and dispersal of people on these two islands goes a long way to explaining why English is now the language of business, diplomacy and science.

The French people’s early adoption of the idea that it’s better to have small families meant that France contributed only a trickle to the river of European immigration. Most North Americans of French ancestry are descended from the few thousand French colonists who settled there in the 17th and 18th centuries.

The great European population expansion (sans France) didn’t last long. By the end of the 19th century small families started to become common in other parts of Europe and, by the 1920s, fertility in many parts of Europe had dropped to the same low levels that we see today. And now, less than a hundred years later, the fertility of almost all human populations is as low as that of Europeans or falling rapidly.

Why have humans stopped competing for fitness? It’s hardly scientific to say that we should stop worrying about why it’s happening and just congratulate our species for being sensible and realizing that failing to curb population growth will be our downfall. And besides, congratulations are hardly in order. As our production of offspring has waned, our production and consumption of many other things has rocketed. Over the last century, the populations that produced the fewest children consumed by far the largest chunk of the world’s resources.

Low fertility alone isn’t going to protect the planet from human desecration but our species’ sudden abandonment of competition for fitness has had other effects. Only a hundred years ago, Europeans were sending their young men off to fight and die for the right of their leaders to claim territory for their nation. In demographic terms, it’s a reasonable thing to do. If populations are expanding, new territory is needed. Records show that most Europeans living at the time did see the First World War as reasonable. The population was still rising rapidly. Many couples had decided to limit the size of their family but there were a lot of reproductive age couples producing offspring. After the war, the idea of family limitation continued to spread and the number of reproductive age couples declined.

Meanwhile, another idea began to spread among Europeans: that military might does not give one people the right to claim ownership of territory occupied by another people. They did fight another huge war and have had a few military skirmishes but the idea has now really taken hold. Most of today’s Europeans have trouble believing that their antecedents could have been so immoral. Most of us believe we must share the planet, not only with other humans but with other species too.

Sharing the territory that we think of as “our own” is not so popular. A lot of Westerners are unhappy about people moving to their country from poorer, less secure parts of the world. These immigrants mostly come from places where the human population had boomed in the 20th century. Like Europeans a few generations earlier, the children of that boom are dispersing and attempting to colonize new territory. Luckily for them, Western countries created many unfilled niches for them to occupy. Despite the brief and tiny “baby boom” that some populations experienced in the 1950s and early 60s, Western couples produced so few children that the economies of many countries in Europe and North American grew much faster than the population. As a result, there were potentially many more jobs than native Westerners to fill them. Some of the immigrants have been highly educated or skilled but many weren’t and they eagerly took unskilled jobs that natives were unwilling to do for the wages being offered.

As they enhance the lifestyle of the natives of their new country, immigrants enhance their own fitness, raising families in the West and often setting aside part of their earnings to help support their relatives back home. This fitness boost has only been temporary, however. Most immigrants to the West arrive with, or soon adopt, the belief that it’s prudent to only have a small number of children. And in their countries of origin, fertility is now low or falling rapidly. According to United Nations estimates (http://esa.un.org/wpp/unpp/panel_indicators.htm), fertility in the Philippines is now at less than three children per
women, down from over seven in the 1960s. Fertility in Mexico and Bangladesh, which peaked at nearly seven in the late 1970s, has now plummeted to just over two. Only in some parts of Sub-Saharan Africa is fertility remaining high.

Without a doubt, life is more comfortable for people who have smaller families. And once people no longer compete for fitness they can strive for sporting or artistic accolades or higher degrees, good jobs and getting papers into Science or Nature. Or they can just take it easy and watch TV. Women need no longer spend the prime years of their lives being pregnant and lactating. Marriage no longer needs to be a longterm reproductive partnership. It can just be the joining together of two people who get pleasure out of being together. If it stops being pleasurable, divorce is possible. And those who decide to have a couple of kids can afford to make their childhood fun and to educate them to increase their chances of having a comfortable and interesting life too.

A person not trained in Darwinian theory might think that the real mystery is why humans took so long to work out that it’s a good idea to have fewer children—or no children at all if times are tough. For Darwinists it’s no mystery. People in the past did get the idea—after all it’s not a hard idea to get. But for the most part, the people who chose that easier life are not our ancestors. Our ancestors were mostly the ones who kept on reproducing and out-competed the ones who had few or no children.

Something kept our ancestors’ noses to the fitness grindstone and whatever it is either didn’t get passed on to us or is ineffective in today’s environment. Because of this, human life today is very different from that of our ancestors and it is continuing to change rapidly. If evolutionary theory is to be of real help in understanding our present behaviour and what our future might hold, we need to get a grip on what it is.

July 12, 2015

References


I interviewed Barb in her office during the summer of 2015.

DSW: Barbara Finlay, welcome to This View Of Life.

BF: Glad to be here!

DSW: This interview will be part of a series of pieces on Evolutionary Psychology and I’m excited to talk with you for three reasons. First, you are editor of Behavioral and Brain Sciences, which is a landmark journal. Second, your own work on comparative brain anatomy, which causes you to be critical not just of Evolutionary Psychology but also what you call human exceptionalism. And third, a particular hypothesis that you have proposed called “The Pain of Altruism.” So, why don’t we launch right into it? Tell us about your background first, a little about your academic training and where you came from.

BF: I got my PhD at MIT after getting my bachelor’s in the most polar opposite place you could imagine, Oberlin College. I was inspired by [David H.] Hubel and [Torsten] Wiesel, who were the first people reporting on the visual cortex. I ended up working on the visual cortex with Peter Schiller at MIT. After about 12–15 months, when we hadn’t discovered how vision worked, I became impatient. I guess that’s what you get to do as a grad student. A lot of people were impatient. It was a depressing period. People thought we would put electrodes into the head and see how vision worked.

DSW: But it proved to be way more complicated.

BF: Yes, that’s true. Then I switched over to doing developmental neurobiology, looking at how the visual system was constructed, which is a somewhat more tractable question. I was at MIT a bit less than 4 years, came right to Cornell as professor, knowing rather little, and started to teach. Fortunately, Glenn Northcutt, a very famous comparative neuro-anatomist from UCSD, showed up for a workshop during my third year here and convinced me that the way to go would be what would be presently called evo-devo, about how development evolved to produce different systems. Evolution could be the basis of understanding development and vice versa. I’ve been working on that ever since, but it didn’t get called Evo-Devo until 25–30 years later.

DSW: When did the term Evo-Devo come about? The late 80s?

BF: More like ’95.

DSW: The question of how Evo-Devo could be new in the 1990s—there’s a story behind that. So now tell us about the journal, Behavioral and Brain Sciences. It has a special status among academic journals.
BF: BBS was founded by Stevan Harnad back in the 70s and it was a unique journal from the start. It modeled itself on an anthropology journal, which was a target article and commentary...

DSW: Current Anthropology?

BF: Yes, but he expanded that considerably. What we look for in BBS, what has always been the key thing, is a target article that has a strong and coherent thesis—that is, an argument of some kind—about how to look at something, what’s the best way to organize empirical data in some domain of inquiry, something to that effect. We distinguish the argument-centered approach from a review paper, where the author’s goal is focused on accumulating and organizing.

DSW: Something a little more groundbreaking.

BF: Groundbreaking is good! You know, novel. We don’t go for controversy per se but these articles just normally generate controversy because they are chosen to be strong points of view. We try to look for arguments that are not hopelessly detailed in their number of postulates.

DSW: A big picture.

BF: Although at the same time, we’re really discouraging of papers that are “my theory of consciousness that I thought up last week in my garret” or things that aren’t heavily empirically supported, no matter how brilliant they might be. So it’s an unusual combination of a strong thesis and a lot of empirical grounding.

DSW: I’ve had two target articles in BBS so I can testify that the review process is grueling—probably the most grueling review process of any academic journal I know. How many reviewers do you send it out to?

BF: We’ve pared it down considerably. I was stunned when Stevan Harnad was training me back in 2001 that he truly wanted to get 10–12 reviews for each article. Both Paul Bloom, my co-editor, and I thought that in the current workload climate, 10–12 are just plain excessive, so we go for 4–6.
DSW: That’s still more than most.

BF: That’s 3x more than most, and not only that, because of the length of the target articles, which will run up to 70 or so pages—12,000 to 14,000 word—the reviews tend to be long as well.

DSW: Any conscientious review would have to be.

BF: And we don’t send all that many out, just because it is so taxing.

DSW: Then there’s the process of getting commentaries. There is a solicitation process, a review process, and you end up with about two dozen commentaries for every target article. So thanks to all that work, BBS is ranked #1 among behavioral science journals and right up there among brain journals.

BF: Yes, and we want to underline too that for organizations that are doing citation counts for impact, the commentaries do not count as citations.

DSW: It’s only the target article, so you’re not padding it.

BF: I think the process itself naturally amplifies the number of citations that an article is guaranteed—25 people [the commentators] have already read your article closely. When does that [otherwise] happen?

DSW: Yes, that’s right! That’s what an impact factor of 25 is! So that makes BBS an interesting microcosm for studying the behavioral sciences from an evolutionary perspective. A while back, I did a survey of BBS. I was in the process of writing a grant proposal for NSF. The first thing I did was look at articles—I think it was between 2000 and 2004, so just when you were coming on—to see the proportion of target articles that were written from an evolutionary perspective. That proportion turned out to be about 30% and I wanted that number to show that the behavioral sciences are starting to be approached from an evolutionary perspective. This was not fringe science, this was not pseudo science—if were, it wouldn’t get into BBS. So that was an interesting number. Then I contacted those authors and I found out about their background. What that showed is that the majority of the authors had not been trained in evolutionary theory—this will not surprise you—they had been trained in some other field and they picked up their knowledge of evolution.

BF: Neither of the editors have been trained in evolution either.

DSW: There you go! The proposal was to fund EvoS, our campus-wide evolutionary studies program, at Binghamton and a sister program at New Paltz, because what’s happening at the level of research is not yet reflected in higher education. That survey was very helpful, we got the grant proposal, and EvoS is a thriving consortium of programs. Against that background, I would like you to share your experience as editor of BBS and the extent to which the articles are incorporating a modern evolutionary perspective—and if it’s changed over the years, because you have 14 years of experience to reflect upon.
BF: Yes, I think I’ve been sitting in the middle of perhaps the largest change. Before I came, there were many notable articles in that domain—your original one, for example, and David Buss’s article on men’s versus women’s preferred mates, cross-culturally—things of that kind. I wrote one myself on brain evolution. I think that around 2000–2005, evolutionary psychology had been established as a particular school of thought...

DSW: Just to put dates on that, the term was coined in the late 1980s. In 1992 the landmark book came out, The Adapted Mind, which really put that school of thought on the map.

BF: Evolutionary Psychology articles tend to be the kind of argument-centered article, not in content but in style, that BBS tends to look for. It is often a thesis about males and females.

DSW: It’s got the big picture part.

BF: We get a lot of Evolutionary Psychology submissions, perhaps too many, I think, given the absolute representation across the entire domain of psychology. There was no other kind of evolution in psychology, because for years, psychology, particularly in its social aspects, had renounced evolution or genetics as a causal explanation—period. Evolution wasn’t included in any kind of definition. The objection was that evolution was too determinist, and quite rightly. At that time, the modern synthesis in evolutionary biology was dominating, and in my view the most interesting human behaviors—altruistic behavior, going to war, religions—were ones that the modern synthesis’ genetically-based evolutionary biology could not even hope to address.

DSW: Right, so if I can just play that back—In my interview with Eva Jablonka, she dwells upon this quite a lot. The study of evolution became highly gene-centric and so therefore gave up the explanatory ability to explain all these cultural things. In some ways, the culture and behavior folks seceded on their own, but in other ways, evolutionary theory went off in a direction that had little to offer for those topic areas.

BF: So the first thing I noticed, I think, was Herb Gintis’s paper on changing viewpoints in sociobiology and how it was time to rethink the nature of the presumed genetic account of behavior—time for the social sciences to start taking biology seriously and time for biology to...

DSW: That was centered a lot on game theory, and so very much steered itself in the direction of social behavior. The evolution of cooperation and that sort of thing.

BF: Starting about that time and ramping up to the present, we are now faced with a true glut of cultural evolution manuscripts.

DSW: That’s really interesting.

BF: I would say that cultural evolution has now replaced consciousness as the number one unsolicited topic.

DSW: I’m very happy to hear that!

BF: Since we need to cover all of cognitive science and anthropology and computation and so forth, we’re forced to become more selective, so we’re having to cut. Paul Bloom and I, dividing decisions between the two of us, cannot coordinate perfectly, so we over-admitted the total percent of cultural evolution manuscripts and might have to pull back.

DSW: One of your most recent acceptances, by John Gowdy and Lisi Krall, is on human economies as super organisms, so how cool is that!

BF: Yes, we’re getting all sorts of things. These papers are all so interesting! We’re right on the uptick of a whole new discipline being discovered, but nevertheless we have to pay some attention to people in perception and computation.

I THINK I’VE BEEN SITTING IN THE MIDDLE OF PERHAPS THE LARGEST CHANGE.
DSW: Right. Next I want to explore the distinction between what could be called “narrow school” and “broad school” Evolutionary Psychology. Broad school is the study of psychology from an evolutionary perspective and narrow school is the school of thought that originated in the late 1980s and took on that name. You were at a workshop at Cornell that I was speaking at, which I have written about. A question about Evolutionary Psychology was asked from an audience member and every speaker at the workshop other than myself rolled their eyes and tried to distance themselves from EP. What’s your take on that?

BF: EP is an unusually strict concatenation of highly charged concepts. First, the idea that cognition and lots of behavior is best thought of as a bunch of modular separate organs that can be selected independently—the Tooby and Cosmides version of EP—second, that a lot of current behavior can be explained by what original adaptations were for, the environment of evolutionary adaptiveness … well, I don’t want to list every...

DSW: You listed the two most important.

BF: Those two things run so hard against the main two tenets of the research I’ve done and also the department I’m embedded in...

DSW: That’s a good segue, to your own work, so why don’t we just go there.

BF: Inspired originally by Northcutt, I set the question for myself of how you get a anatomically-distributed system in the brain like the visual or olfactory system to get relatively bigger. Suppose a species moved into a nocturnal niche, and would do better to rely on olfaction than vision, and should thus expend more processing on the sense of smell. But olfaction is not just the nose, or a single brain part, there are parts distributed throughout the brain—how would you coordinate that? I was first interested in cell death as a way of sculpting out such functional systems in development. Then I looked at the generation of neurons to see if the numbers in groups of neurons in different anatomical locations, but representing a single functional system, were in some way coordinated. Essentially I was looking for modules that could be selected by evolution, and boy, did I not find any! Instead, to my complete surprise, both at the level of describing adult nervous systems from various species, and neural development in the same animals, everything was predictable from brain size. If I divided the brain into the parts that anatomists like and looked at twelve different parts that all together added up to the whole brain, and then asked how well I can predict the size of those particular parts from the size of the whole, the answer was, with 96% accuracy, you can do that. In other words, if you know the size of the whole brain, you know the size of the mid-brain, the thalamus, the cortex. Then you add in a couple—and I mean a very few—of correction factors, such as the cortex is bigger in primates overall, you reach 99% accuracy. For example, the human cortex is exactly the size it should be for a primate with a brain that’s overall as big as ours. Then I started to look within that for functional systems, such as the visual system. Parts within functional systems also scale predictably with respect to overall brain size. It’s important to realize that the scaling isn’t linear, but allometric. For example, the cortex gets to be an increasingly large proportion of brain size as size increases in any mammalian group, and the human cortex is just where it should be for primates. The cortex can go from about 20 percent to 80 percent of volume, say, between rodents and primates, increasing sharply but very predictably.

DSW: Let me stop you there. I’m familiar with this through your work, but when I first encountered it...

BF: It takes a while...

DSW: ...it does take a while, and it leaves a lot of questions unresolved. One of my favorite books is the Scientific American book titled Evolving Brains by John Allman, which seemed at least when I read it to tell a different story—that when you look at electric fish or some species where olfaction is the primary sensory mode, then this was reflected in brain proportion. Is that not correct?

BF: No, both accounts are true. If you know just brain size you get at 96 percent of the variance—and I’m talking about just mammals now—but then the important 3–4 percent is the olfactory-limbic collection of
structures that co-vary with the olfactory bulbs—the hippocampus and other olfactory cortices. You can have animals where those are particularly large, like rodents and anteaters, or where they’re small, as in primates. Or you can have both the cortex and the olfactory apparatus quite large, as in carnivores, so those things can sort independently.5

DSW: So, yes that happens but it accounts for a very small percent of the variance?

BF: Yeah, but it’s a big amount of tissue.

DSW: Right, functionally is what counts. Does that make your point about modularity less relevant? Are we right back where we started?

BF: The really remarkable thing is the stability of the nervous system compared to the instability of the periphery. You definitely can get new functions into old tissues and the interesting thing is how stable the fundamental anatomical organization of that tissue stays. I was being careful to say before: “anatomically defined areas”, like the primary visual cortex. We actually looked at the relative size of primary visual cortex in diurnal versus nocturnal mammals; we didn’t find any difference.6 Does that mean that visual animals don’t devote more of their brains to vision? They certainly do. How? So for example, in deprivation situations, if a person is blind from birth, or simply not sighted voluntarily for only two weeks, they’ll start to use their visual cortex to read Braille.7 We all impressed ourselves too much by calling the visual cortex the visual cortex, and then the idea naturally arose that if you want to make a visual animal the only thing you can do is make that visual cortex bigger. But if you can put vision in a lot of places...

DSW: I see! Aren’t there experiments where they rerouted the eyes to the olfactory cortex in ferrets?

BF: Yes, but it doesn’t happen evolutionarily.

DSW: But it shows the plasticity of the brain to be able to do that.

BF: This used to be an absolutely heretical thing to say. And boy, did I get a lot of crap for it. But it turns out that unbeknownst me the same thing was happening in evo-devo and the control of body plans...

DSW: OK, nice!

BF: …so what people were discovering when they were looking at gene expression and transcription factors, and so forth, that set up both the invertebrate and vertebrate body plan, the 11 initial segments that organize the body plan, that is, what’s front and back, middle and side were exactly the same for vertebrates and invertebrates organized by the exact same genes. The expectation prior to that time was that evolution is a random walk, and that any part could be under selection, new parts could be selected as necessary.

DSW: These are developmental building blocks that are very conservative.

BF: Yes, and this was written about very nicely in Jon Gerhart and Marc Kirschner’s Cells, Embryos, and Evolution—one of my favorite books—about the conservation of fundamental mechanism, an observation keeps coming up over and over again. They identified a conserved body plan, and the same is true for the brain plan and for fundamental building blocks like oxidative metabolism. The number of neurotransmitters is a conserved set that you can count on two hands.

DSW: But we still have giraffes and anteaters and whales, so the phenotypic diversity hasn’t changed. We’ve learned some things about their development, and those things are surprising in some sense, but the conservative nature of those building blocks has not prevented the diversity that we see.

BF: Yes, so the point of this research is never to say “in fact, mice and humans are exactly the same”. We are not. The question is, what’s the palette of mechanisms that are allowed into a causal explanation of how you
get diversity? When people first started thinking about it, they thought that in order to evolve language, a little chunk of tissue must be designated and committed to be language cortex, to get language input, and to do language related transformations within itself. Now, proposals like that seem much less plausible, since fundamental architecture is so conserved. Now you’d be much more likely to offer an explanation about what information the organism gets early on, what it is motivated to do, and how we can understand language structure in terms of the rather rich variety of computational devices we know to be in every brain.

DSW: I can see that this is important. We can go through a number of specific hypothesis that are associated with Evolutionary Psychology. One is Robin Dunbar’s hypothesis that the larger the group size, the more the brain…can you state Dunbar’s hypothesis—because you can do it better than I can—and then critique it?

BF: Well, Dunbar’s had at least four theories about what a socially adapted brain might be. I think the fundamental thesis is unfalsifiable in a way. Social complexity is likely to be associated with a large brain. So is tool use, elaborate foraging strategies, and any number of different complex abilities—except, interestingly, migration and long-term wayfinding.

DSW: Why not?

BF: Shall I digress on that?

DSW: Sure.

BF: We tend to think of a goose following an elaborate route as doing an amazing cognitive calculation, but what that goose is doing is keeping itself from the harder task of being a chickadee and having to master 3 or 4 or 5 different environments. If you compare the animals who have evolved to follow their environment around, some of the structures involved with wayfinding are larger—but in general they’re more small-brained than the ones who have to be a winter kind of bird and then a spring kind of bird.

DSW: Awesome! So you can be less variable by migrating. I actually wrote a paper on that, in which specialization can evolve with habitat preference. If you can choose your habitat, then you experience a uniform environment, while a generalist has to cope with different environments.

BF: And Louis Lefebvre, who’s one of my favorite scientists in this whole domain, looked at a lot of capacities that are associated with large brains. A study that I cite all the time—so first off, all large-brained abilities co-vary, such as an ability to use tools, or innovate, or do well in laboratory tests of set shifting, and things like that. Those all co-vary, but the real killer observation is that the bigger your relative brain size is, the more likely you are to succeed in invading a new territory. Chickadees spread everywhere, turkeys not so much. Similarly, raccoons but not rabbits.

DSW: So many specific things can select for brain size but the way that happens is that the whole brain has to become bigger.

BF: Yes.

DSW: So therefore you get other capacities?

BF: You get the opportunities to combine all these different kinds of abilities and set them according to your immediate history. You’ve got four really big learning engines in your brain: two kinds of associators, a reinforcement system and an error correction system in the cerebellum. Every species’ brain has all those parts and every part all looks at all incoming information. What a developing animal is motivated to look at ends up as computations that are populating its nervous system, for generalist and specialist species.
DSW: Okay, so I want to nail this down. If Dunbar is correct in a sense, and if you have two primate species that differ in their social complexity, and you try to hold them the same in other respects, then their brains will be different, but how will they be different? Will one be bigger than the other?

BF: As to Dunbar... there are two arguments going on, and remember we’re talking about the details of 1 percent of the variance here, about whether there is any excess brain size to be accounted for in primates with large group sizes. For the first argument, a technical, quantitative one, I’m with what I think is the most of researchers who say there isn’t any interesting “extra” brain to account for—it’s predictable from brain size\(^{10}\). Dunbar says there really is excess difference. Initially, he looked at was plain old brain size. That was the first claim. Then, no, it’s relative brain size, and the last thing was no, it’s a particular chunk of the frontal temporal lobe. So the hypothesis has migrated around as to...

DSW: No shame! That’s what happens! Science progresses! Science progresses!

BF: But the trouble is that there’s no co-varying out of related abilities, the second, non-technical argument. First, the percentage of the variance is tiny, and what else varies in these animals? Do they have larger territories? Do they use tools more? Do they invade new regions more? Are they associated with a particular type of social structure? The list of things that are potential covariates is enormous—you could go on for the rest of the afternoon and I would bet my bottom dollar that we would find other aspects of complex behavior that are going to vary with any aspect of social complexity that you put up, just because the nature of the stuff that Louis Lefebvre has already demonstrated of complex behavioral abilities that intrinsically co-vary. And there’s no way that Dunbar would be able to, or should or could, remove the rest of cognition from social cognition in primates.

DSW: OK, so back to language. Back in the day with Steve Pinker and Noam Chomsky and others, the reasoning was that language is such a specialized adaptation that it had to be reflected by some kind of organ in the brain. This would be another part-by-part selection story and you’re saying “not true”—right?

BF: Yeah, and with a lot of evidence as well. Morton Christensen down the hall here, works a lot on how language evolves to fit the brain and not the other way around and has a number of examples like that.

DSW: Does that mean, if I understand correctly, that different languages can be very different from each other, because historically they are outcomes of cultural evolution and the way a particular population crafts a language might be very different? There might not be a fundamental unity to different languages? Am I right about that?

BF: Yeah, it was really quite strange that...

DSW: A universal grammar is basically what’s being challenged.

BF: Yes—so the idea, and really without much data, was that there was in fact a universal grammar. On its first incarnation it was a great deal more detailed than it ended up becoming. Universal grammar was a proposal, or a hypothesis, about English and some other closely associated languages. It was never an elaborate look for what the nature of the universal grammar might be, across the languages of whole world. It was data-free. If I might be permitted a sexist crack: one of tenets of Chomsky about why you had to have a universal grammar was “poverty of the stimulus.” The idea was that no child could possibly learn language given the inferior descriptive information that any infant got. So, as I’m raising my son and chatting to him, and everybody else is chatting to him, I’m thinking “if this is poverty of the stimulus, I would like to see wealth of the stimulus!”

[lots of laughter]

DSW: At the same time there has to be some scaffolding? That’s true with visual development too.
BF: What universal grammar eventually got moved down to was the ability to use symbols recursively, and not much more. There’s nothing much linguistic about that at all, if anything. So there’s no candidate at this point that I’m aware of for something that’s really about language that is a feature of just human brains.

DSW: That’s amazing!

BF: It’s an empirical question, you could….

DSW: …amazing that there can be something that’s unique phenotypically—that’s beyond question—and yet not based on much that’s unique in the human brain.

When evolutionary psychologists talk about modularity, they start from the outside—in other words, phenotypic modularity. The point they make, that species have adaptations for their particular environments, is true, especially for non-human species. The phenotypic modules must be mechanistically instantiated in some way—sex differences for example, such as why men are more risk-taking than women. Or, to pick a very specific one, the last person I interviewed was Debra Lieberman. Her work examines how kin recognize each other with respect to incest avoidance and helping behavior. It’s a neat trick—to be adaptive, you have to avoid mating with your close relatives but you also need to help them. You have to be positively attracted to them in some sense and repelled by them in another, and how do you recognize a sibling anyway? She has shown, convincingly to me, that the recognition mechanism is different for older siblings vs. younger siblings—based on the quality of information. If you’re an older sibling and you have a younger sibling, you saw it born, you saw your mom nurse it, and that’s good information. If you’re a younger sibling, then you haven’t seen that for your older sibling and so the best information that you have is co-residence—how long have you been living with them. And, again convincingly, as far as I can tell, she shows that there’s a sort of if-then clause. Something happens inside the brain that makes sense from an evolutionary perspective, so it seems that natural selection has been fine tuned enough to evolve those very specific and context-sensitive recognition mechanisms. Now, no one would expect that to be reflected in brain size, and probably no one would expect to see some spot on the brain that does that, but something does that—a circuit? Or what? So the modularity thesis might be correct but we just have to look for it in the brain in the right way. What do you have to say about that?

BF: I would make exactly your argument. There has to be species-specific neural circuitry because of the x, y, and z kinds of evidence you just described. This circuit might not be the little brain part I can draw a line around and say there’s the language area. What we do need to figure out why it is that humans are so interested in learning language and chimps are so wildly uninterested—that’s where I would start. One of the most interesting things to me is, there is a lot of wired-in form recognition, from very basic to very structurally complicated. For a basic example, a loud noise will startle anyone—so that’s a lot of auditory processing that is responded to by orienting to the noise. That’s innate—3D reaching in space and localizing things. That’s all wired in. There are certainly sex differences in activity levels and in many animals, different kinds of innate knowledge, like recognizing foods or some predators. I wrote a paper with Michael Anderson in “Frontiers” in 2013 about the relationship between neuroplasticity and evolution and cognition11. Whenever researchers first described species-specific behavior in neural terms, they wanted to go and find an encapsulated module that contained the motivation and the circuitry. So we come up with a noun for the behavior, we put the noun in the brain… We just have to get a better descriptive vocabulary for how to assemble new capacities out of old parts. Now where I’ve gone with that… I really think that researchers are coming to recognize a major location for evolutionary change in brains and that’s the subcortical motivational and emotional systems. I think that monogamous voles were the greatest piece of explanatory evidence regarding human language that has come down the pike.

DSW: Why is that now?

BF: So, I don’t know if everyone knows…
DSW: You have to give some background—I promise you that not everyone will!

BF: So there are several species of voles with different social systems. I always get the species all wrong and I apologize to my colleagues for that, so I leave the species names vague. Most rodents and most mammals are promiscuous, not monogamous. A few species are socially monogamous, like ourselves. Some vole species that have separated themselves out from the promiscuous hoard and become monogamous. They have done that, not by installing a “this-is-my-husband-the-vole” recognizer in their primary visual cortex, but by a very specific alteration of their motivational system, tying individual recognition to motivation. This is what they’ve done. Sex motivates all species; sex-related organization is found all through these subcortical systems. What the monogamous voles have done is make a necessary connection between a particular animal and the reinforcement of sex. So, they’ve now plugged in their recognition of individual animals via oxytocin or vasopressin receptors into their primary reinforcement circuitry.

DSW: So only the special someone elicits the…?

BF: These voles will work to be with their partners and other animals will not.

Everyone is rewarded by sex—that’s the baseline and these animals continue to be rewarded by sex. They are not completely monogamous. But what does happen is that male and female pairs live together for an extended period of time and raise multiple litters, take care of the litters together, and by so doing, get better offspring survival rates for quintessential reproductive success. This is what the whole brain is about, basically—using the reinforcement circuitry to organize for what you will do and repeat doing. This particular system is changed in these animals and it’s done by just the addition of a very particular connection. The interesting thing about this, compared to the predictability and stability of the whole brain neuron numbers and volumes, is that these neuromodulator systems are just churning around. There’s a lot of individual variability and between-species variability. You can turn a promiscuous vole, or mouse, into a monogamous one by doing a genetic injection of these receptors in the correct place. The late Jim Goodson of Indiana University, who did his doctoral and postdoctoral work here at Cornell did a lot of similar work extending this in birds. Compared to mammals, birds give you much more scope for variation. Different bird species will go from being entirely solitary to being in groups of 50,000. He was able to look at some of the neural circuitry of steps in group preference, steps in territorial size preference. His work suggested all of these preferences could be represented in similar ways: “I feel comfortable when I’m in about an acre of ground and I feel lonesome if it’s two, and I feel anxious if it’s half, and I’ll move and I will change things in order to get to the situation that I prefer.” Once you start to think that way, you can see how much leverage you can get when you’re using these basic motivational processes to imagine how you can construct a nervous system. I think—and I’ve argued in print with Supriya Syal—for a theory of language based on motivational changes. I think it’s the motivational circuitry that’s changed in the context of a large cortex and some ability to speak. What’s different about humans is that we really care more than anything else to influence mommy or caretaker by talking or waving our hands.

DSW: Well, I can’t resist. I didn’t anticipate talking about this. Christopher Boehm’s work and the concept of major evolutionary transitions makes the unique human evolutionary event social control, so that rather than a typical dominance hierarchy, the would-be dominant individual is controlled by the subordinates. That created a sort of enforced egalitarianism that made more cooperation possible. Other primates, including chimps, cooperate to a degree, but members of a group are also their own chief rivals. Basically, teamwork became the signature human adaptation and symbolic thought—many forms of communication, including symbolic thought—are forms of teamwork. They’re low-cost team work but still, a symbol is shared, and there’s something very communal about that. Michael Tomasello’s work—simple things like pointing and shared awareness, all follow upon this major transition. So does that fit into your scheme?
BF: Yes, completely, so you’ve got these interesting things—like we have this obvious morphological specialization of the whites of our eyes, so we can tell where we’re looking. We are very attuned to that—and talk about a subordinate dominating an adult—look at these babies that are getting their mommies to talk to them! So I think this is all part of the same general thing and it doesn’t require adding a cooperation circuit. It says “let me to use my learning machine to understand that thing.”

DSW: Great.

BF: I think that things are moving nicely in neuroscience with Karl Deisseroth and optogenetics—being able to dissect out complex motivational circuitry, which is massively intertwined wires and cells at the base of the brain that no one could know a thing about before. But now, for the first time, those are getting to be dissectible.

DSW: What’s the technique that enables that?

BF: Optogenetics. This guy’s managed to be able to turn off genes or whole cells or neurotransmitters, or make cells fire or not in the brain in response to a light pulse nearby them. So this is uniquely useful in places where there’s complex convergence circuitry. People have been spending so much time looking at the cortex because it’s sort of easy—the dimensions are all spread out and laying flat for you. But the base of the brain—that’s much harder. I think that’s where the action is going to be in this almost roiling motivational circuitry that is so easy to change. To extend the domain of the vasopressin receptor, or to produce it in some different tissue, or any number of these neuromodulators that can change the organization of cognition. That wasn’t part of the explanatory language. There was no way for it to be with classical Evolutionary Psychologists.

DSW: Yeah for sure. It was very much an outside-in kind of thing, not mechanistic. Phenotypic modularity, however it is implemented mechanistically. Well, let’s end up with your own EP hypothesis, “The Pain of Altruism”. Tell us what that is.

BF: So, this is about rewiring another motivational system. The idea is that humans haven’t changed the nature of pain in general but we’ve changed the set of things that are designated pain. I watched a bunch of monkeys who had undergone (in lovely conditions) caesarian sections, where I was looking at retinal development very early on. I was sitting with the recovering mothers and in about an hour these gals would be up and about, banging on each other if you let them and wanting their lunch. I wasn’t a bit like that when I had a caesarian section! So finally I said, “Wait a damn minute!” I was very influenced by a book by Wenda Trevathan called Human Birth: An Evolutionary Perspective, and books by Sarah Hrdy. Human birth is always assisted and in other primates, birth is practically never assisted. So I started to think that there’s a whole class of things, including labor, recovery from trauma, and probably diseases from viruses, where because we can ask for help and get it, these things have been designated painful in an evolutionary sense that aren’t painful in other animals, because there’s no use for them to be in pain. For example, having painful labor for an antelope in Africa would leave that animal incapacitated for hours. Writhing on the ground, crying in pain trying to give birth to an antelope makes them become lunch, not mothers! That doesn’t happen. So if you look at large ungulates giving birth, with large hooves and all kinds of horrible stuff, they show rather little evidence of discomfort. That’s not to say that birth isn’t dangerous—it is! There’s high mortality associated with birth, but that’s not when humans are yelling. They’re yelling when their cervix is dilating, which isn’t dangerous at all. So that’s designating a stretch of particular smooth muscle to be something that’s putting me in extreme distress, making me call for help, to have a better chance of survival in anticipation of the event that happens later. The missing concept in most people’s minds is that pain is to cause action—it’s not just the automatic sensation of damage. Other animals have pain when it’s for something, like get off my foot, stop biting me, or if I possibly can, I should lie down and recuperate. But most animals don’t have that luxury, unless they’re babies. I think we’ve taken a mother-infant behavior module (kind of!) and expanded it.
DSW: So does that imply that in other mammals, because the care-giving relationship is the mother-offspring relationship, that infants would feel pain?

BF: Yes, that is an unfortunate consequence. So yeah, I remember used to think when exasperated with my own babies, “You’re just crying because you want to irritate me!” But maybe they’re crying—I can’t prove or demonstrate this—because the experience of pain keeps the call for help honest. It is an honest signal.

DSW: There are also contextual factors and cross cultural factors. That would fit into the hypothesis.

BF: Essentially, though, I am arguing that there are a number of physiological states that don’t count as pain in other animals.

DSW: Basically what you’ve done—and this is why I called it an EP hypothesis—is that you presented an adaptation hypothesis that our species has certain selection pressures, which are not experienced by other species, to put it crudely. Then there’s the question of how it happens mechanistically. Would you talk about gating mechanisms?

BF: So this would be the same kind of thing where some new class of inputs gains access to the “yeow make it stop” circuitry. All this input does cohabit and it’s not a crazy amount of re-organization to imagine. But how it very specifically would happen I have no clue. I think it’s something that needs explanation and I do think it falls under this changed motivational envelope rather than the redesign of the whole circuit.

DSW: You do get accounts of men in battle not feeling pain.

BF: Yes, that’s an interesting thing. I learned that this is not human-specific at all. It is cross-mammalian. The prototypical human battle story is “I got my leg shot badly, but still I picked up my comrade and got him back to the hospital, and only then I noticed my injury...” It turns out that in most mammals that have undergone some major trauma, the first thing they do is get the hell out of there. Later they try to attend to their wounds.

DSW: And as they’re getting out of there they’re not feeling the pain. So humans are capable of being like other animals in certain respects.

BF: Yes. It’s not a human specific, battlefield specific evolved thing.

DSW: This has been a wonderful conversation—I’m so happy to add it to our comprehensive series on Evolutionary Psychology. Thank you, Barbara!

BF: Thank you!

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References


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