

The “science” cited by James Damore is bogus, and scientists knew it a long time ago

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In August 2017 news broke that a software developer at Google was fired for circulating a controversial memo he’d written. It contained assertions, supposed to be *scientific*, to the effect that women are innately inferior to men in certain respects—but only “on average,” so presumably we mustn’t hate him too much. It then made recommendations to stop striving for too much diversity in the workplace. The press reacted predictably: those who call themselves liberals applauded the punishment of a heretic, and conservatives shook their heads at the intolerance of scientific “truth.”

Which side is right? Below we examine some of the papers cited in the memo, because even a nonscientist can show that they are bogus, either in and of themselves, or in the way in which they’re being used in the debate. Then we’ll look at parts of the media storm itself. The conclusion however is that in the deepest political sense neither side is right, because the problem is much more serious than the irritation has let on, and its implications much more sinister.

The “research”

As Debra Soh put it in the title of her article in *The Globe and Mail*¹ (see below), “No, the Google manifesto isn’t sexist or anti-diversity. It’s science.” In it she asserts that “scientific studies have confirmed sex differences in the brain that lead to differences in our interests and behaviour.” In particular she says that the more a foetus is exposed to testosterone, the more likely it will grow up to prefer things over people, and hence to prefer math, science, and engineering over subjects that are presumably more cuddly. As applied to the Google case, the claim is that women are inferior to men in skills relevant to software engineering, but only “on average,” meaning that, given an arbitrarily large subset of men, it is in principle possible to find a woman who outperforms all of them, but also that, given a specific skill level, more men exceed it than women do. This qualification will turn out to be irrelevant; I spell it out here only because proponents of the claim seem to think it indemnifies them against the charge of unfair discrimination.

Here now are the papers cited by the “scientists,” and why the arguments in them, or the arguments that use them, are irrelevant or faulty. It’s worth noting up front that the kinds of errors explained below were noticed quite a long time ago by *real* scientists who worked at the so-called “top schools” but whose voices have been systematically marginalized. Meanwhile, don’t be intimidated by the technical language; I’ll explain it in simple terms.

¹<https://beta.theglobeandmail.com/opinion/no-the-google-manifesto-isnt-sexist-or-anti-diversity-its-science/article35903359/>

Pasterski *et al.*

One study, by Pasterski *et al.*, is available at <http://onlinelibrary.wiley.com/doi/10.1111/j.1467-8624.2005.00843.x/abstract>. Here is the abstract, which, as will be shown presently, is alone enough to see that the study is botched:

Toy choices of 3- to 10-year-old children with congenital adrenal hyperplasia (CAH) and of their unaffected siblings were assessed. Also assessed was parental encouragement of sex-typed toy play. Girls with CAH displayed more male-typical toy choices than did their unaffected sisters, whereas boys with and without CAH did not differ. Mothers and fathers encouraged sex-typical toy play in children with and without CAH. However, girls with CAH received more positive feedback for play with girls' toys than did unaffected girls. Data show that increased male-typical toy play by girls with CAH cannot be explained by parental encouragement of male-typical toy play. Although parents encourage sex-appropriate behavior, their encouragement appears to be insufficient to override the interest of girls with CAH in cross-sexed toys.

This can be summarized simply. The experimenters took a group of children whose ages ranged from three to ten, some of whom were encouraged more than others to play with “sex-typical” toys, and some of whom have congenital adrenal hyperplasia (CAH), a disorder that virilizes a child prenatally, *i.e.*, makes the child more biologically male than normal (on boys it usually has little effect, but in girls it can make the genitalia ambiguous to the point of “pseudohermaphroditism”). The idea was to see if these three factors—CAH, encouragement, and biological sex—influence the child's preference for sex-typical toys, *i.e.*, the child's “gender.”

To show the study's mistakes, it helps to recast the situation in the standard language of the scientific method, such as we find in high school or middle school science courses. First, there are the three *independent variables* (“IVs”):

- *IV 1*: the sex of the child. This can take two values: male and female.
- *IV 2*: whether or not the child has CAH. This can take two values: the child either does or does not have CAH.
- *IV 3*: how much the child was encouraged to play with sex-typical toys. This can be thought of as taking two values: the child received either normal or extra encouragement to play sex-typically.

So the eight possible combinations of the IVs' values are:

1. A boy without CAH is given normal encouragement to play with sex-typical toys.
2. A boy without CAH is given extra encouragement.
3. Boy with CAH, normal encouragement.
4. Boy with CAH, extra encouragement.
5. Girl without CAH, normal encouragement.
6. Girl without CAH, extra encouragement.
7. Girl with CAH, normal encouragement.
8. Girl with CAH, extra encouragement.

Recall that the experimenters sought to measure how the child's behavior (his or her toy preference) varied with changes in these IVs. Behavior is therefore the *dependent variable* ("DV") and can take two values: sex-typical or sex-atypical.

In order to perform the experiment thoroughly, *the DV has to be measured for all eight possible combinations of the IVs*. Interestingly, however, the experimenters failed to do so. Instead, their abstract reports the following:

- The boys were given normal encouragement, and those with CAH showed no more likelihood of sex-typical play.
- For girls:
 - Those without CAH were given normal encouragement and responded with a certain degree of sex-typical toy play.
 - Those with CAH were given extra encouragement and responded with *less* sex-typical play than the other girls did.

In other words, *the DV was measured only for combinations 1, 3, 5, and 8*. In the case of the boys (combinations 1 and 3) IV 3 was *controlled*, i.e., kept fixed, while IV 2 was varied. For this reason, the finding of no change in the DV does indeed support the hypothesis that CAH does not affect a boy's behavior, as long as he is given no extra encouragement, and assuming that nothing more is wrong with the experiment. (Plenty more is indeed wrong with it, as discussed below.)

But for the girls (combinations 5 and 8) the experimenters did *not* control the experiment. In fact, they botched it in a way that suggests a political agenda. Consider, for example, a researcher with reductionist bias. He or she might botch the experiment by measuring the DV only for combinations 5 and 7, or only for combinations 6 and 8. If the DV showed that the behavior varied with CAH, then the finding would seem to support preconceived notions that behavior is determined by biochemistry alone—the "nature" side of "nature-vs.-nurture."

Conversely, a researcher with social constructivist bias might botch it by measuring the DV only for combinations 5 and 6, or only for combinations 7 and 8. Again, if the DV showed that behavior varied with encouragement, then the finding would seem to support preconceived notions that behavior is learned—this is the "nurture" side of the debate.

Now the correct way to perform the experiment, the way that tries to cancel the researchers' biases, is to measure the DV for all four combinations—5, 6, 7, and 8—and to try to make sense of the results. If they came out, say, as follows

combination	CAH (IV 2)	encouragement (IV 3)	behavior (DV)
5	yes	normal	typical
6	yes	extra	typical
7	no	normal	typical
8	no	extra	typical

then one would conclude that neither CAH nor encouragement determines behavior. The conclusion, if behavior is indeed determined, is that it must be determined by something else; other variables would need to be sought as candidate determiners.

If, on the other hand, the results came out as

combination	CAH (IV 2)	encouragement (IV 3)	behavior (DV)
5	yes	normal	atypical
6	yes	extra	atypical
7	no	normal	typical
8	no	extra	typical

then they would support the hypothesis that behavior is determined by CAH and not by encouragement. “Nature” would in this case win out over “nurture.”

Now consider this unusual scenario:

combination	CAH (IV 2)	encouragement (IV 3)	behavior (DV)
5	yes	normal	typical
6	yes	extra	typical
7	no	normal	atypical
8	no	extra	atypical

It would be a surprise, because although it would support reductionist predictions, it would also support the strange hypothesis that absence of CAH—*i.e.*, normal sexual development—produces atypical behavior. Such a result should immediately make us suspect a mistake, perhaps in data collection. Whether or not that turned out to be the case, the experiment should of course be repeated many times, to be sure that the results are not a fluke. This scenario is included here just to keep your mind alert to “edge cases” and to give you a larger sense of the way of thinking.

Finally, if the results came out like so:

combination	CAH (IV 2)	encouragement (IV 3)	behavior (DV)
5	yes	normal	atypical
6	yes	extra	typical
7	no	normal	atypical
8	no	extra	typical

then they would support the hypothesis that encouragement, and not CAH, determines behavior. “Nurture” would win out over “nature.” But it’s also important to appreciate that the same hypothesis would also be supported if the results are inverted, like so:

combination	CAH (IV 2)	encouragement (IV 3)	behavior (DV)
5	yes	normal	typical
6	yes	extra	atypical
7	no	normal	typical
8	no	extra	atypical

In this case, not only would we find support for determination by the environment alone (“nurture”), but we would call the results counterintuitive, because they would show that extra encouragement in fact *discourages* typical behavior. An explanation would therefore be in order. One possibility is that the children in the sample are rebellious, are somehow in the habit of defying their parents’ expectations. It would of course then be reasonable to repeat the experiment with a completely different sample of children. If the same results obtained, and if they keep obtaining with other samples of children, then we might suspect that something in the culture being studied makes children systematically defiant of their parents’ wishes.

Another possibility, which anticipates the deeper critique below, is that the children are not rebelling, but are tuning in to something *else*, an independent variable other than IV 1, IV 2, or IV 3. Such a possibility would mean that, rather like the first example above, the experiment is searching for the wrong effect.

And now for what Pasterski *et al.* actually did. To be sure, they botched the experiment neither in the naïvely reductionist way, nor in the naïvely constructivist way, but, more tellingly, by measuring the DV only for combinations 6 and 7, like so:

combination	CAH (IV 2)	encouragement (IV 3)	behavior (DV)
6	yes	extra	atypical
7	no	normal	typical

The appearance in this table of all four values of IV 2 and IV 3 makes it look as though all four combinations have been tested. But of course they haven't. Moreover, the speciousness is *convenient to reductionists*, because the first row of this table (combination 6) makes it look as though the experimenters, by giving extra encouragement for typical play in CAH girls, went to the trouble of "cancelling out" any possible environmental influences, whereas they have not in fact done so. To have done so, they should have measured the value of the DV for combination 8 and shown it to us. If the results had turned out like so

combination	CAH (IV 2)	encouragement (IV 3)	behavior (DV)
6	yes	extra	atypical
7	no	normal	typical
8	no	extra	atypical

then the reductionists would lose, because it would show that something about the encouragement itself is causing atypicality. If, on the other hand, the results turned out like so:

combination	CAH (IV 2)	encouragement (IV 3)	behavior (DV)
6	yes	extra	atypical
7	no	normal	typical
8	no	extra	typical

then the reductionists would be supported, though of course they would need to show, by much repetition of the experiment, that the effect is reproducible.

Other lapses

In addition to this rather damning error in design, the experiment suffers other lapses:

- Although the ethnicities of the studied families was recorded, it was not controlled. It would certainly have been interesting to see what effect it might have on the results.
- Questions arise from the classification of certain toys as gender neutral. These were: "a puzzle, a board game, books, and crayons and a sketchpad." But most books are obviously aimed at a specific gender, and even when not so "marketed," are viewed by many as feminine—think for example of the scene of the movie *Seven*, in which Brad Pitt reacts with an angry slur when Morgan Freeman suggests that he read books. Board game groups are known to be dominated by males. And crayons and sketchpads lie on the "artistic" side of the menu, far from, say, "rough-and-tumble play," which is considered gender-typical for boys. The authors make these classifications on the basis of "prior research" (they cite other papers), but this too is a problem, as will be explained below in an excerpt from a speech of Feynman's. More generally, one must wonder about interactions between "gender-typicality" and culture. Scots love their kilts, and I hear it is not unusual for boys in Sweden to knit. One can only wonder how to decide that a given thing is "gender-typical" for the poor brain that has been partly exposed to American culture, partly to Swedish, and partly to Scottish.
- If the children were not informed that they were being studied, then they might have guessed it from the careful preparation of the toys: they were arranged in a circle and alternated type, *i.e.*, no two "masculine," or "feminine," or "neutral" toys were placed next to each other. Obviously one must think of the possibly confounding effects of self-consciousness during such a study.
- The authors note: "The child-alone play session was always conducted first to provide a baseline measure of toy preference for each child." But this washes out possible effects of the ordering. A dependence of the behavior (DV) on this ordering would have significantly altered the study's findings. If hysteresis were found during, for example, an experiment in physics, it would rule out a simple functional relationship between the variables.

- The authors note: “When both parents participated, the order of participation (mother first or father first) was determined randomly.” This again washes out any possible dependence the child’s behavior might have on the gender of the parent.
- The authors note: “On those rare occasions when a child played with a sex-typed toy in an atypical manner, this was not scored as play with that toy (*e.g.*, shooting Barbie with the gun was scored as play with the gun but not scored as play with Barbie).” But *why* should the gun trump the Barbie in this case? And what would the experimenters have done if the child played with the Barbie as if *it* were a gun? Assuming this ambiguity arose as rarely as the authors say, it is not worth saying more about it here, except that the last decade has seen the prominent rise of representations of women in cinema who are physically very aggressive (*e.g.*, Scarlet Johansson in the *Avengers* series, and Angelina Jolie as Lara Croft), so there is again a potential classification problem here.

Note that none of these flaws and problems, neither of definition nor of procedure nor of interpretation, are found in experiments in the real sciences. Unfortunately, as will be explained below, such defective work is not unusual in so-called “behavioral science.”

Technical questions

In addition to the above, certain questions come up as one reads the paper; they are just questions, mind you, and the authors may have good responses to them:

- On p. 268, they state:

A one-way analysis of variance (ANOVA) for age indicated that the mean ages for the four groups were not significantly different, $F(3, 116) = 2.45, ns$.

Now recall that F is a ratio of the mean sum of squares between groups and within groups:²

$$F = \frac{\frac{1}{g-1} \sum_{i=1}^g n_i (\bar{x}_i - \bar{\bar{x}})^2}{\frac{1}{n-g} \sum_{i=1}^g \sum_{j=1}^{n_i} (x_{ij} - \bar{x}_i)^2} \quad (1)$$

where $\bar{\bar{x}}$ is of course the grand average. The F test requires a lookup based on $g - 1$ degrees of freedom (df) in the numerator, so the 3 in $F(3, 116)$ appears to be correct, since there are $g = 4$ groups here. But the df for the denominator is supposed to be $n - g$, where n is the total number of data points, which the authors tell us is 117. Should the F number therefore not be $F(3, 113)$? A glance at tabulated F values³ shows that the difference between $F(3, 113)$ and $F(3, 116)$ is anyway negligible; but even so, if the right value is indeed $F(3, 113)$ then the reader may be confused.

- Table 3 shows quantities that appear to be unnormalized to unity. Should they be? For example, Table 3 states that, when with their fathers, sixteen girls with CAH spent 19% of their time playing with “girls” toys, 43% of their time playing with “boys” toys, and 35% of their time playing with “neutral” toys. But these three percentages add up to ninety-seven rather than a hundred.

Again these are just questions, they may not be particularly good ones, and in any case the authors may have good answers.

²I will not cite any particular reference. Instead, please just consult any of the very large number of available books on statistics.

³See, *e.g.*, <http://www.itl.nist.gov/div898/handbook/eda/section3/eda3673.htm>.

Why the study cannot be corrected

Many scientists—more importantly, many *nonscientists* whose common sense hasn't succumbed to blind trust in so-called "experts"—would of course agree with the foregoing assessment of the study's errors. But sadly even such people often miss the much deeper problems with the study—with the whole field, in fact. To see what I mean, consider again the three independent variables. IV 1 is biological sex, which the study models as having two possible values, male and female. This is hardly a problem, since in the vast majority of people the sex chromosome pair is either XX (female) or XY (male).

But IV 2, which indicates whether or not the child has CAH, is not quite so simple. Consider this quote from Wikipedia:⁴

Further variability is introduced by the degree of enzyme inefficiency produced by the specific alleles each patient has. Some alleles result in more severe degrees of enzyme inefficiency. In general, severe degrees of inefficiency produce changes in the fetus and problems in prenatal or perinatal life. Milder degrees of inefficiency are usually associated with excessive or deficient sex hormone effects in childhood or adolescence, while the mildest forms of CAH interfere with ovulation and fertility in adults.

In plainer language: there are different degrees of CAH, and the disorder manifests itself in different ways in different people. Thus the meaning of CAH varies, both qualitatively and quantitatively. And yet the study treats it as a single two-valued variable. Such a model *coarsens* reality, from a multi-dimensional continuum (if that itself is not already too coarse) to a binary choice. Note that there is no such flaw in modeling, say, the concentration of a solute, or the location of a particle with respect to some coordinate system. Real sciences, such as physics and chemistry, suffer no ambiguity of definition in their variables.

But nevermind, because these problems are nothing compared to the problems with IV 3, which should, in a sane society, take people's breath away. Recall that it is supposed to measure the amount of encouragement given to the children. Now how, pray tell, is this to be done? The experimenters did the only thing that *can* be done when one tries to force subjective human experience into the objective scientific method. They put parents and children in a room, videotaped them for sixteen to twenty-four minutes, and counted the number of times they heard the parents say something "positive" during that time.

But this is fatuously stupid. For starters, what if the parent says "Good girl!" twice quickly in one breath? Does that count as one instance of encouragement or two? What if the second time swallows up the words, or mutters them? Moreover, the parents and children are also interacting outside the laboratory, so how can the count be reliable? And what about so-called "feedback" that comes from persons other than the parents? Since these children's ages range from three to ten, they are constantly being exposed to messages from other family members, from friends and neighbors, from school, and, perhaps most powerfully, from the media. What kind of idiocy is it to think that this nonstop barrage of influence can be magically waved away by the fiat of a "researcher," and replaced by a thoughtless count of the number of times a formula is uttered in less than half an hour? The authors assure us that the procedures they used "were approved by institutional review boards within the United States and the United Kingdom." But this only means that the stupidity has been institutionalized. As we will see below, this problem was noted by real scientists about half a century ago.

But all of these glaring problems are *still* nothing when we consider one of the deepest and most basic facts about communication—namely, that its real meaning lies in everything *except* what is literally said. As this same legion of so-called experts has gotten our society scientistically to say, "Communication is 90% nonverbal." (Let us overlook for now the inanity of modeling communication as a simple quantity, let alone of assigning a percentage to its nonverbal part.) Take any slur, for example—racial, ethnic, or sexual. Say the word the wrong way and you could incite a riot. But say it under the right circumstances

⁴https://en.wikipedia.org/wiki/Congenital_adrenal_hyperplasia, as of August 20, 2017.

and you could seduce a person into intimacy, might even excite sexual desire. Indeed, if the minimum age of this sample were not as high as three years, one could easily argue that the subjects must be even *more* sensitive to “nonverbal communication,” for the simple reason that they haven’t yet learned the language. No matter: we have the example of dogs. Speak to one in any language, and it will, all things being equal, respond the same way to the same *tone*, to the same *body language*. In fact, if we really need an “expert” to confirm the obvious fact that *nothing* needs to be actually said in order for communication to take place, then we can find one in Hastings, whose book on child sexual abuse [Has94] reveals that a breastfeeding mother can tell that the baby can sense whether she is sexually aroused by the sucking.

So if Pasterski *et al.* and other such “scientists” do indeed hear a parent speak *words* of encouragement to their children, how can they know that it will have the face-value effect? To give another example from their paper:

At the beginning of each play session, participant(s) were brought to the center of the circle [of toys], and told, “You can play with the toys however you like.”

But if the child can really play with the toys however it likes, then there would be no need to *say* so, and indeed no such thing is ordinarily said to a child when it is given toys to play with at home. The saying of it very conceivably alerts the child that something artificial is happening, and that it is being tested in some way.

A not unrelated problem is that, as they report, the experimenters took it upon themselves to rate a parent’s response as “neutral” if she or he attended to the child as it played, or if she or he did not interfere with the child’s play. But such a classification is almost comically arbitrary, because a child might easily interpret an “attending” parent as approving of its play, and might interpret a “noninterfering” one as disapproving of it.

Or again: the very fact that a parent knows that the child is sexually abnormal means that he or she is more likely to be *anxious* about making the child “gender-typical.” Are we really to believe that the child will not tune in to such anxiety, in the parent’s voice, face, or body? Are we really to believe that these “variables,” which are not only unaccounted for but fundamentally *unaccountable*, do not matter, cannot produce counterintuitive effects? And even if such foolery is to be countenanced, the experiment is botched by the very fact that it isn’t *blind*, that the parents actually *know* which kids do or do not have CAH.

Notice that we needed no special training to expose this study’s procedural and conceptual faults. Apart from a dribbling of high school science, all we used was everyday logical thinking. That so many errors were made, at so many levels and of so many kinds, and that some of them should be so disturbingly wrong-headed, has got to arouse suspicions that something more than mere incompetence is at work here.

Finally, take another look at the paper’s abstract, specifically its last sentence. Did you notice those two little words, “appears to”? Their significance will be discussed below.

Bramble *et al.*

Another one of the studies cited by Soh, by Bramble *et al.*, is bogus not necessarily in itself but in the way in which it is cited. Its title is “Sex-Specific Effects of Testosterone on the Sexually Dimorphic Transcriptome and Epigenome of Embryonic Neural Stem/Progenitor Cells.” Sounds fancy, right? The abstract⁵ sounds even fancier. But its essential message is simple: the genes of brain cells in mice are expressed differently at different levels of testosterone, so the male brain is different from the female one.

The problem with this claim—assuming it really is true of mice and assuming it can be generalized from mice to humans—is its irrelevance. We already know that there are sex differences in the mammalian brain.

⁵<https://www.ncbi.nlm.nih.gov/pubmed/27845378>

There are, in case you haven't noticed, sex differences in mammalian bodies. So, since the brains control the bodies, it would be no surprise if they too had sex differences.

The question, rather, is: how do sex differences make women inferior at software engineering or any of its related mental skills? The paper sheds no light on this question, and yet the essentialists—the “nature” side of this “nature-*vs.*-nurture” debate—cite the paper as if it somehow helps to settle it.

Again, note that we needed no special training to understand this study's irrelevance to the question at hand.

Su *et al.*

The reductionists—Soh, Damore, and company—make the claim that prenatal testosterone levels determine “interests,” and their mention of averages of course gives the impression that they respect the influence of the environment on a person's development. One of the papers they cite is by Su *et al.*,⁶ entitled “Men and things, women and people: a meta-analysis of sex differences in interests.” Here is the abstract:

The magnitude and variability of sex differences in vocational interests were examined in the present meta-analysis for Holland's (1959, 1997) categories (Realistic, Investigative, Artistic, Social, Enterprising, and Conventional), Prediger's (1982) Things-People and Data-Ideas dimensions, and the STEM (science, technology, engineering, and mathematics) interest areas. Technical manuals for 47 interest inventories were used, yielding 503,188 respondents. Results showed that men prefer working with things and women prefer working with people, producing a large effect size ($d = 0.93$) on the Things-People dimension. Men showed stronger Realistic ($d = 0.84$) and Investigative ($d = 0.26$) interests, and women showed stronger Artistic ($d = -0.35$), Social ($d = -0.68$), and Conventional ($d = -0.33$) interests. Sex differences favoring men were also found for more specific measures of engineering ($d = 1.11$), science ($d = 0.36$), and mathematics ($d = 0.34$) interests. Average effect sizes varied across interest inventories, ranging from 0.08 to 0.79. The quality of interest inventories, based on professional reputation, was not differentially related to the magnitude of sex differences. Moderators of the effect sizes included interest inventory item development strategy, scoring method, theoretical framework, and sample variables of age and cohort. Application of some item development strategies can substantially reduce sex differences. The present study suggests that interests may play a critical role in gendered occupational choices and gender disparity in the STEM fields.

Look at all those numbers, equals signs, minus signs, and decimal points. Impressive, right? Even the term “meta-analysis” sounds like a fancier kind of analysis. But all it means is that the paper aggregates the results of other studies. So if the latter are as radically absurd as the one by Pasterski *et al.*, or as irrelevant as the one by Bramble *et al.*, then what good is piling the garbage up even higher?

The problems with this paper, as with the others, can be uncovered by taking the idea of “meta-analysis” more seriously. Let's begin with the fatuity of quantifying the qualitative. The abstract says, “The magnitude and variability of sex differences in vocational interests were examined...” No one, it seems, will stop to ask: how do you measure an interest objectively? One could hand out a survey that asks, “Are you interested in X?” and add up the number of yeas and nays. But this is hardly more “scientific” than a vote in parliament. The only way to make the measurement *seem* more scientific is to say instead, “Rate your interest in X on a scale from one to five.” But why five? Why not ten? And what does four mean? Twice as much interest as two? Is that a linear scale, a logarithmic one, or something more exotic?

What about the difference in *qualities* of interest. Aristotle was interested enough in physics to write about it rather famously, but how could his interest been anything like Einstein's, given that the latter focused on it exclusively for the whole of his working life. Somehow, because arm lengths, tree heights, and

⁶<https://www.ncbi.nlm.nih.gov/pubmed/19883140>

elephant weights have “magnitude and variability,” one has only to speak of the “magnitude and variability” of a *feeling* in order to elevate the nonsense into plausibility. How can interest—or, for that matter, desire—even be scientifically defined? And why is it assumed that interest cannot change with time, sometimes profoundly? Surely these considerations matter when, say, giving career advice. Meanwhile, note again that none of these problems come up when measuring, say, the strength of a magnetic field.

Problems of definition also infect the paper’s two polarities, “Things-People” and “Data-Ideas.” It is heartily granted that some people prefer stretches of solitude to company, or are particularly fascinated by material or mechanical things. But these coarse dichotomies hide some interesting and counterintuitive truths. What do you think drives a creative person to work away alone, if not the anticipation that he will eventually show his friends a great piece of work? Whom do you think she is talking to in her head, when she philosophizes privately about, say, the nature of space and time, if not to her friends, to figures fondly internalized in her imagination? Conversely, can one be surprised to find the most people-loving among us prattling away about new cell phones, engagement rings, clothes, things, things, and more things? Who can doubt that actors, for example, are “people-persons”? And yet Hugh Grant has said that his greatest joy, one that even arouses him sexually, is to read up about sports cars and their crank shafts. Again, I’m not saying there isn’t some truth to the observation, only that the “psychologists” build upon it with a systematic thoughtlessness.

As for “data/ideas”: before a biologist even goes to work to run a data-collecting experiment, he or she is animated by whole layers and systems of ideas, from those pertaining to the mechanical effects on and between the involved tissues or substances, all the way to those at the most general levels of, say, evolutionary theory. Or, to take an example from politics: before a pollster goes out to administer questionnaires, he or she is immersed in theories, ideas, and expectations, from politics, economics, and sociology. Conversely, a literary critic has to muster evidence for his or her theories and positions, and these have to be collected from various literary sources and “processed” by close readings. So how can anyone tolerate the notion that data and ideas can be separated? And again, even if some objective measure could indeed be found for such things and even if it did show “leanings” to one side or another, who says it will be stable in time for any given person. Moreover, how could the labelling of a person in these ways not *suggest* to her that she continue more deeply along the same “track.” In other words, how can these cooked-up categories, which purport to classify people passively, not exert *self-fulfilling psychological pressures* on them? Note again that none of these concerns touch the inanimate objects of physical science. No amount of government-sanctioned authority can make a planet veer off course.

Undaunted by these debunkings (which were made by scientists long ago; see below), our intrepid psychologists proceed to collect their imponderables into “Holland’s categories.” But the human race, including philosophers, social commentators, artists, parents, and many others, humble and exalted, has been studying its own behavior for hundreds of thousands of years. If these seven categories—“realistic,” “investigative,” “artistic,” “social,” “enterprising,” and “conventional”—were really the first and last word, the fundamental parameters of personality, then why did they wait until the twentieth century to be “discovered”? I am of course not saying that human beings cannot be described. In fact these seven words are perfectly usable adjectives for things, actions, even communities. But why seven? Why not five, or ten? Their mere enumerability has a way of making them seem like scientific truth, the mere expression “seven types of personality” sounding like, say, “eight types of quark.” The difference of course is that objective measurements show that there *are* eight types of quark, whereas objective measurement is *not even possible* in this psychological “science.”

Moreover, like “things-people” and “data-ideas,” this taxonomy also bends toward self-fulfilling prophecy. The gullible (to be kinder one should call them the uncritical), and especially the young, are groomed from day one to listen to messages that come at them from the media, from school, from the government, and most especially from scientific-sounding authorities. So if you give a person a questionnaire, compare his answers to the distribution of other people’s answers, and then tell him he is “realistic,” he will, especially

if he buys into the scientism, simply believe it, proceed to define himself in those terms, and arrange the rest of his life to conform to the expectation that is parading as a prediction. What we have, then, is every possibility of *reversing cause and effect*, a trick that cannot be played on an inanimate object, which is why fields that study inanimate objects—physics, chemistry, *etc.*—can be made into *real* sciences.

And again, even if this taxonomy of personality types were neither obtuse nor self-fulfilling, what makes you think it is stable? That is, who says that a person who appears “artistic” to you in her twenties will not appear “enterprising” three years later? Think of Newton’s transition away from number crunching and into Biblical hermeneutics, or of Leonardo’s later infatuation with engineering at the expense of his painting. The retort of course is that these types are genetically determined. But this assumes the conclusion, amounting to even more reversal. For when a person reacts to such “assessments” typically, *i.e.*, submits to their specious authority and proceeds to define herself in their terms, the effect itself is then taken as confirming a genetic cause. And note how the notion of permanence of type allows the government to *control* its subjects. We’re not satisfied with stuffing you into one of our boxes; we must also be sure it never moves.

Returning to the abstract: after a couple more sentences of dry and solemn scientism, it concludes, “The present study suggests that interests may play a critical role in gendered occupational choices and gender disparity in the STEM fields.” So, all this work, all these papers, all this “meta-analysis,” and the fuss made of it in the media—all just to tell us that men and women have different interests? All to tell us that a person’s interests influence her choice of career? What a show of pedantry! Thank goodness for the “science” of the *Psychological Bulletin*, otherwise we’d never know what we already knew by looking out the window! But in all seriousness: the real sciences tolerate none of this tautological pedantry. And yet in spite of it, these papers, especially their numbers and symbols, have a curious way of making it seem as though something has been *explained*. The irony is that they first have to banish the *obvious* explanation, which is that practically everything in the culture, in the environment, *tells* men and women to be interested in different things. And for their conforming to expectation it rewards them, “differentially.” (Ah, the fetishization of that scientific word!) Instead of calling the results—the *averages*, mind you—predictable from culture, these “scientists” call them evidence of biological determinism.

Meanwhile, note again that the ideas in this study were brought down using only basic logic, an attack that real sciences have no trouble withstanding. And, again, did you notice, in the abstract’s last sentence, those two little words, “may” and “suggests”?

Joel *et al.*

It is interesting to note similar flaws in the one study that Soh cited as a concession to constructivists. It’s by Joel *et al.*⁷ and is entitled “Sex beyond the genitalia: The human brain mosaic.” The authors claim that the brain is a “mosaic,” a collection of “features” each of which can be considered male, female, or neutral, depending on whether they are similar to the averages. They also claim that each person has features of both kinds, *i.e.*, that there is no “internal consistency” in any one person’s brain.

But again this begs the question. Of *course* these kinds of patterns will show up in the brain. How could they not? The brain is immersed in and constantly reacting to a cultural environment that defines gender carefully, anxiously, insistently, founds whole economies on those definitions, bombards people, always and everywhere, with messages that perpetuate, reiterate, and disseminate notions of masculinity and femininity. So why is it surprising that such “conditioning” (the scientific word for cultural shaping) should be reflected in brains?

The authors then claim “robustness” across variations of study, adding, “These findings are corroborated by a similar analysis of personality traits, attitudes, interests, and behaviors.” So we are playing the same strange game, that of trying to quantify qualities. How, for example, do you measure an attitude objectively?

⁷<https://www.ncbi.nlm.nih.gov/pmc/articles/PMC4687544/>

If we hear a test subject say, “I like pottery,” how do we get him to add, “and I like it twice as much as my buddy does”? Present the subject with a discrete set of choices (a popular one is “Agree,” “Disagree,” “Agree strongly,” “Disagree strongly”), and the question arises: how does one extract from it a quantity and a scale (linear, logarithmic, *etc.*)? And again, what makes you think such “values” would be stable in time? If stability does indeed obtain, how do you know that it isn’t due to your own influence on a mind dazzled by the authority conferred on you by institutions, government, and media? Again, the real sciences harbor none of these practical *a priori* absurdities.

In their opening “Significance” section, the authors moreover seem to hope that their findings will stop others from “justify[ing] differential treatment of males and females.” (Unthinkable to replace “differential” with the clearer but humbler word “different”; how else are we to make the stuff smell like calculus?). But they classify their “features” based on averages, which are all that corporations and government care about. Such institutions—think of insurance companies, for example—use statistics (the word itself comes from “state”) whose soundness they are not exactly anxious to investigate, and from them they craft laws and policies which themselves can of course become self-fulfilling prophecies. So, ironically, Joel *et al.*’s claims can in fact be used to aggravate sex discrimination.

Finally: “Our study demonstrates that, although there are sex/gender differences in the brain, human brains do not belong to one of two distinct categories: male brain/female brain.” So the elaborate and supposedly impressive diagrams and statistics filling the pages of this article, which was published in the hallowed *Proceedings of the National Academy of Sciences*, confirm what we already know by pulling our noses out of our books and looking out the window. And yet it gives the curious impression that something has been explained.

“And this is *science*?”

If you’re getting depressed by all this institutionalized idiocy, then get ready for worse. For starters, observe that Pasterski *et al.* are affiliated with highly regarded institutions—UCLA, University College London, City University in London, *etc.* Considering that the paper was actually published, it also presumably went through a process of peer review, of scrutiny by a handful of anonymous colleagues drawn from a pool of “behavioral scientists” stretching across the globe. That they approved the paper for publication raises obvious questions about *their* competence too. It is interesting that none of these professors, nor their supervising administrators, was fired or demoted for such lapses.⁸

Worse, the lapse is not surprising, for the habit of radical error and incompetence in these pseudoscientific departments was already noted half a century ago, by a number of scientists who went to the trouble of raising alarms about it. They include Ruth Hubbard [HW99], Richard Lewontin, Leon Kamin, Steven Rose [RSL84], Stephen Jay Gould, and, in his own way, William Byne. As for the problems of peer review, Michael Crichton was disturbed enough by them to speak out as well.

One of the critiques was made by Richard Feynman, the celebrated Nobel physicist, during his graduation speech at Caltech in 1974. It was then published as an article entitled, famously, “Cargo Cult Science” [Fey], giving rise to the phrase “cargo cult,” which in an irritating irony has come to be misused today, precisely by the scientistically pretentious (see below). In any case, it’s worth reading every word of it, and in spite of Feynman’s rough English worth *rereading* every once in a while. We summarize it briefly, focusing on a couple of its main points.

It begins with Feynman’s complaint of feeling “overwhelmed” by how much “junk” is out there, by which he means such nonsense as ESP, astrology, various mysticisms, and other popular theories that are

⁸Whereas Ward Churchill, after publishing a politically controversial opinion of the terrorist attacks of September 11, 2001, was summoned to a tribunal that promptly fired him for academic incompetence, even though the errors in his scholarship were so few and small that Stanley Fish called the witch hunt by its name.

either not falsifiable or have already been falsified. He then mentions education, child-rearing, the treatment of crime, the proliferation of theories in those fields, and the ironic worsening of results:

Yet these things are said to be scientific. We study them. And I think ordinary people with commonsense ideas are intimidated by this pseudoscience. A teacher who has some good idea of how to teach her children to read is forced by the school system to do it some other way—or is even fooled by the school system into thinking that her method is not necessarily a good one. Or a parent of bad boys, after disciplining them in one way or another, feels guilty for the rest of her life because she didn't do "the right thing," according to the experts.

So we really ought to look into theories that don't work, and science that isn't science.

Note the important finding here: whole institutions, whole networks of credentialed pseudoscientists, distributed in highly funded departments on university campuses, are founded on "science that isn't science." He moves on to "the efficacy of various forms of psychotherapy." Again overwhelmed by the sheer mass of nonsense, he adds, "I have so much to tell you that I can't do it all. I will have to limit myself to just a few little things...Maybe I will give a series of speeches next year on all these subjects. It will take a long time." And then he explains what has become the title of the speech:

In the South Seas there is a Cargo Cult of people. During the war they saw airplanes land with lots of good materials, and they want the same thing to happen now. So they've arranged to make things like runways, to put fires along the sides of the runways, to make a wooden hut for a man to sit in, with two wooden pieces on his head like headphones and bars of bamboo sticking out like antennas—he's the controller—and they wait for the airplanes to land. They're doing everything right. The form is perfect. It looks exactly the way it looked before. But it doesn't work. No airplanes land. So I call these things Cargo Cult Science, because they follow all the apparent precepts and forms of scientific investigation, but they're missing something essential, because the planes don't land.

Now it behooves me, of course, to tell you what they're missing. But it would be just about as difficult to explain to the South Sea Islanders how they have to arrange things so that they get some wealth in their system. It is not something simple like telling them how to improve the shapes of the earphones. But there is *one* feature I notice that is generally missing in Cargo Cult Science. That is the idea that we all hope you have learned in studying science in school—we never explicitly say what this *is*, but just hope that you catch on by all the examples of scientific investigation. It is interesting, therefore, to bring it out now and speak of it explicitly. It's a kind of scientific integrity, a principle of scientific thought that corresponds to a kind of utter honesty—a kind of leaning over backwards. For example, if you're doing an experiment, you should report everything that you think might make it invalid—not only what you think is right about it: other causes that could possibly explain your results; and things you thought of that you've eliminated by some other experiment, and how they worked—to make sure the other fellow can tell they have been eliminated.

Details that could throw doubt on your interpretation must be given, if you know them. You must do the best you can—if you know anything at all wrong, or possibly wrong—to explain it. If you make a theory, for example, and advertise it, or put it out, then you must also put down all the facts that disagree with it, as well as those that agree with it. There is also a more subtle problem. When you have put a lot of ideas together to make an elaborate theory, you want to make sure, when explaining what it fits, that those things it fits are not just the things that gave you the idea for the theory; but that the finished theory makes something else come out right, in addition.

In summary, the idea is to try to give *all* of the information to help others to judge the value of your contribution; not just the information that leads to judgment in one particular direction or another.

After an important comparison with the dishonesty of advertising, he makes a point that will sound familiar in light of our own debunkings above:

A great deal of their difficulty is, of course, the difficulty of the subject and the inapplicability of the scientific method to the subject.

Next he uses the history of measurement of the electronic charge to illustrate confirmation bias and the discarding of valid data that doesn't agree with that of an authority (though he adds, "We've learned those tricks nowadays, and now we don't have that kind of a disease," a claim that may have been true of his colleagues in 1973 but is very doubtful today).

But this long history of learning how to not fool ourselves—of having utter scientific integrity—is, I'm sorry to say, something that we haven't specifically included in any particular course that I know of. We just hope you've caught on by osmosis.

The first principle is that you must not fool yourself—and you are the easiest person to fool. So you have to be very careful about that. After you've not fooled yourself, it's easy not to fool other scientists. You just have to be honest in a conventional way after that.

He then warns about "fooling the layman," particularly in order to secure *funding*, and, using examples from the oil and tobacco industries, stresses the importance of not being "used" by politicians and other interests who want a particular outcome, whether or not the facts bear it out.

Not surprisingly, the longest part of the talk is devoted to the exposure of psychology. It is worth quoting at length:

Other kinds of errors are more characteristic of poor science. When I was at Cornell, I often talked to the people in the psychology department. One of the students told me she wanted to do an experiment that went something like this—I don't remember it in detail, but it had been found by others that under certain circumstances, X, rats did something, A. She was curious as to whether, if she changed the circumstances to Y, they would still do A. So her proposal was to do the experiment under circumstances Y and see if they still did A.

I explained to her that it was necessary first to repeat in her laboratory the experiment of the other person—to do it under condition X to see if she could also get result A—and then change to Y and see if A changed. Then she would know that the real difference was the thing she thought she had under control.

She was very delighted with this new idea, and went to her professor. And his reply was, no, you cannot do that, because the experiment has already been done and you would be wasting time. This was in about 1935 or so, and it seems to have been the general policy then to not try to repeat psychological experiments, but only to change the conditions and see what happens.

Recall that Pasterski *et al.* (above) cut the same corner, by citing "prior research" to justify their gendering of toys. Feynman interjects a concern about similar corruption in physics itself (see below). He continues:

All experiments in psychology are not of this type, however. For example, there have been many experiments running rats through all kinds of mazes, and so on—with little clear result. But in 1937 a man named Young did a very interesting one. He had a long corridor with doors all along one side where the rats came in, and doors along the other side where the food was. He wanted to see if he could train the rats to go in at the third door down from wherever he started them off. No. The rats went immediately to the door where the food had been the time before.

The question was, how did the rats know, because the corridor was so beautifully built and so uniform, that this was the same door as before? Obviously there was something about the door that was different from the other doors. So he painted the doors very carefully, arranging the textures on the faces of the doors exactly the same. Still the rats could tell. Then he thought maybe the rats were smelling the food, so he used chemicals to change the smell after each run. Still the rats could tell. Then he realized the rats

might be able to tell by seeing the lights and the arrangement in the laboratory like any commonsense person. So he covered the corridor, and still the rats could tell.

He finally found that they could tell by the way the floor sounded when they ran over it. And he could only fix that by putting his corridor in sand. So he covered one after another of all possible clues and finally was able to fool the rats so that they had to learn to go in the third door. If he relaxed any of his conditions, the rats could tell.

Now, from a scientific standpoint, that is an A-Number-1 experiment. That is the experiment that makes rat-running experiments sensible, because it uncovers the clues that the rat is really using—not what you think it's using. And that is the experiment that tells exactly what conditions you have to use in order to be careful and control everything in an experiment with rat-running.

I looked into the subsequent history of this research. The subsequent experiment, and the one after that, never referred to Mr. Young. They never used any of his criteria of putting the corridor on sand, or being very careful. They just went right on running rats in the same old way, and paid no attention to the great discoveries of Mr. Young, and his papers are not referred to, because he didn't discover anything about the rats. In fact, he discovered all the things you have to do to discover something about rats. But not paying attention to experiments like that is a characteristic of Cargo Cult Science.

There ends Feynman's cautionary tale about psychological research. But also alarming is his mention that the same methodological errors have already infected physics itself, and for the same reason—pressure to secure *funding*:

Nowadays there's a certain danger of the same thing happening, even in the famous field of physics. I was shocked to hear of an experiment done at the big accelerator at the National Accelerator Laboratory, where a person used deuterium. In order to compare his heavy hydrogen results to what might happen to light hydrogen he had to use data from someone else's experiment on light hydrogen, which was done on different apparatus. When asked he said it was because he couldn't get time on the program (because there's so little time and it's such expensive apparatus) to do the experiment with light hydrogen on this apparatus because there wouldn't be any new result. And so the men in charge of programs at NAL are so anxious for new results, in order to get more money to keep the thing going for public relations purposes, they are destroying—possibly—the value of the experiments themselves, which is the whole purpose of the thing. It is often hard for the experimenters there to complete their work as their scientific integrity demands.

So much for Feynman, who, “overwhelmed” by the tidal waves of junk “science,” finally exclaims, “And this is *science*?” Interestingly, the phrase “cargo cult” has itself come to be used wrongly, to refer to a kind of thoughtless, but harmless, pattern matching. When, for example, a software developer hastily incorporates some new tool or technology in her or his stack, reading the bare minimum of the instruction manual to get the thing working without understanding it too deeply, many people call it “cargo culting.” But however shallow your knowledge of the third-party software might be, if your incorporation of it works, then it works; whereas true cargo-cult doesn't work. It is tempting to read this common misuse of the term, even by so-called educated people, as reflecting the direness of real cargo-cult thinking in a culture deeply confused about science.

In any case, I cannot urge you strongly enough to read every word of this short article, to revisit it regularly for the rest of your life, and always to wonder why its alarms still go pointedly ignored.

But the scandal doesn't really stop there, because it involves you and me as well. To understand what I mean, consider that, apart from the high-school biology we used above, the principles of the scientific method don't require any algebra, calculus, or arcane mathematics, and they don't involve jargon more intimidating than “independent variable.” They're based only on elementary logic, which might simply be

called common sense. And yet we grant funds and influence to these pseudoscientists—behavioral “scientists,” “psychologists,” and other quacks—and we look away, trust that they know what they’re doing, trust their internal processes of quality control, trust that the media would never hype up results that are wrong or stupid, and, as if all that were not bad enough, allow jurists and legislators to embed their fatuities in the law. It’s true that those without higher training cannot appraise work done in the real and advanced sciences; subatomic particles, for example, are not part of our everyday lives. But *behavior* has been systematically observed, engaged in, and reflected and commented on, by everyone and for many millenia. How did the twentieth century seduce us into relinquishing the authority of our own experiences to pseudoscientists?

The question turns out to be too deep for this short essay, so the interested reader will need to study some cultural history and criticism, beginning perhaps with Jacques Barzun’s *Science: The Glorious Entertainment* [Bar64]. As related reading, the magisterial *Not In Our Genes*, by Lewontin, Kamin, and Rose [RSL84], should be required for every science major, if not every undergraduate. Hubbard’s *Exploding the Gene Myth* [HW99] makes similar criticisms more concisely and is aimed at a more general audience. Readers wanting a historical sense of the frightening eugenic ideologies driving much of today’s bogus “research” can consult *In the Name of Eugenics*, by Kevles [Kev95]. And Jeffrey Masson, particularly in his *Against Therapy* [Mas94], mounts careful attacks on the constellation of shams known as “clinical psychology.”

The press

Debra Soh

As mentioned in the opening, the above papers were referenced by one of the most confident media responses, by one Debra Soh, whose short piece in *The Globe and Mail* was entitled “No, the Google manifesto isn’t sexist or anti-diversity. It’s science.” Her byline reads, “Debra Soh writes about the science of human sexuality and holds a PhD in sexual neuroscience from York University.” (Notice again the presumption that there can be such a thing as a “science,” a study based on objective measurements and producing deterministic laws, of experiences that are fundamentally subjective. And notice that whole biological subdisciplines—and therefore whole *budgets*—have been established on the presumption.) Ms. Soh states her position at once:

Despite how it’s been portrayed, the memo was fair and factually accurate. Scientific studies have confirmed sex differences in the brain that lead to differences in our interests and behaviour.

Remember those instances of “maybe,” “suggests,” and “appears to”? See how easy it is to banish them if you’re a pundit with a magic wand called “Ph.D. in sexual neuroscience”? She goes on “confirming”:

As mentioned in the memo, gendered interests are predicted by exposure to prenatal testosterone — higher levels are associated with a preference for mechanically interesting things and occupations in adulthood.

A few lines later she cites the study by Pasterski *et al.*, which, as was shown above, is not only seriously broken but also a typical specimen of the orgy of delusion that has come to be called “behavioral research.” Then, citing the paper by Su *et al.* (above), she writes:

Lower levels [of testosterone] are associated with a preference for people-oriented activities and occupations. This is why STEM (science, technology, engineering and mathematics) fields tend to be dominated by men.

This beautifully suggests *why* the nonsense needs to be continually peddled: the more it is believed, the more institutions are relieved of improving the diversity of their workforces, and the less the culture has to examine the myriad, relentless, conscious, and unconscious ways in which it *tells* males and females, as soon as they start existing, to behave in certain ways. Returning to Pasterski's paper, Soh writes:

Similarly, men who are interested in female-typical activities were likely exposed to lower levels of testosterone.

So now Soh makes her own contribution to the "may-game": all you have to say is "likely," and it's science! Then, citing Bramble's irrelevant paper, she writes:

As well, new research from the field of genetics shows that testosterone alters the programming of neural stem cells, leading to sex differences in the brain even before it's finished developing in utero. This further suggests that our interests are influenced strongly by biology, as opposed to being learned or socially constructed.

See that? See how the irrelevant can be added to the stew? All you have to say is "this further suggests." Applause!

And now we come to the bad guys:

Many people, including a former Google employee, have attempted to refute the memo's points, alleging that they contradict the latest research.

I'd love to know what "research done [...] for decades" he's referring to, because thousands of studies would suggest otherwise. A single study [Joel *et al.* (above)], published in 2015, did claim that male and female brains existed along a "mosaic" and that it isn't possible to differentiate them by sex, but this has been refuted by four—yes, four—academic studies since.

Yes, folks, newer is better! Would *you* prefer a 2005 Blackberry to today's iPhone? And besides, the constructivists are "refuted" four times over! Why didn't Ms. Soh tell us sooner that, in science, might makes right? Quick, quick, call the seventeenth-century Vatican and assure them that their geocentrist experts trump that weirdo under house arrest. Ring Hitler and assure him that his darlings who published *One Hundred Scientists Against Einstein* can brush him off when he retorts, "If I were wrong, one would have been enough." Still confident, Soh continues:

Some intentionally deny the science because they are afraid it will be used to justify keeping women out of STEM. But sexism isn't the result of knowing facts; it's the result of what people choose to do with them.

Thank goodness for the Debra Sohs of this world, who show us how to create "facts" by turning blind eyes on radical error and stupidity, blowing away those pesky "maybes," and summing it all up with the word "confirmed." Someone ping the chemists and physicists and invite them to the party.

And finally:

This is exactly what the mob of outrage should be mobilizing for, instead of denying biological reality and being content to spend a weekend doxxing a man so that he would lose his job. At this point, as foreshadowed in Mr. Damore's manifesto, we should be more concerned about viewpoint diversity than diversity revolving around gender.

How refreshing to see an academic explicitly endorse mobbing! But you see, if Damore should not lose his job, then who else should, if not that posse of well-funded reductionists at Harvard, and their counterparts in Canada, who taught him and Ms. Soh the art of pseudoscientific overconfidence?

Soh also wrote a shorter piece in *Quillette*⁹. Her byline there elaborates:

⁹<http://quillette.com/2017/08/07/google-memo-four-scientists-respond/>

Her dissertation used four types of neuroimaging, including structural and functional MRI, to investigate brain differences associated with sexual orientation, paraphilias (or unusual sexual interests), and hypersexuality. You can find her columns in *The Globe and Mail*, *Playboy*, *LA Times* and elsewhere. You can also follow her on Twitter @DrDebraSoh

Look at that little bento box of jargon! “Sexual orientation,” “paraphilias,” “hypersexuality.” Thank heaven we have “scientists” to tell us what it means for desire to be “unusual” (gosh, is that a code word whose meaning the government can stretch to convenience?). Bravo to them, for having now indoctrinated the world into never wondering why the ancient Greeks, Egyptians, and Romans, had no need for such reifications. The next time you find yourself picking your nose too much, ask yourself, gravely, whether you might not have “hyperrhinoexcaviasis” (pronounced haI-pər-raI-no-eks-k ə-VAI-ə-sis). But don’t worry, there’s a gene for it, and Amgen’s working on the cure...so go buy some shares! Meanwhile, check out the technology: “neuroimaging” and “structural and functional MRI.” Golly, if the machine shows a correspondence between your sexual thoughts and the pretty colors on the screen, then it must have uncovered the cause! But if you don’t have access to *Doctor Soh’s* dissertation or fancy gadgets, then don’t worry, because she also publishes in humble-folk forums, such as *Playboy*. Heaven forbid that her “scientific” ideas should influence your sexual behavior.

And finally, the cherry on top. Referring to constructivist research, she writes:

Some of these ideas have been published in neuroscientific journals—despite having faulty study methodology—because they’ve been deemed socially pleasing and “progressive.” As a result, there’s so much misinformation out there now that people genuinely don’t know what to believe.

Oh, the tragedy. Peer review in neuroscientific journals has allowed papers that the good doctor doesn’t approve of. I wonder why, then, anyone with common sense can spot the flaws in the studies she does like. Meanwhile, let us appreciate her heartfelt concern for those who “genuinely don’t know what to believe.” Fortunately for such lambs, she and her fellow experts have shown the way: just keep repeating “likely,” “maybe,” “suggests,” “appears to,” “linked,” and “associated,” and soon everything will be “confirmed.”

Quillette

That *Quillette* article features three more “scientists.” The first is a Lee Jussim. All he did in his handful of paragraphs was repeat what Damore said, nod his head, and decry (quite reasonably, I might add) academic repression at the hands of PC ayatollahs. That’s it; no picking anything apart; just slap Damore on the back for citing the published “research.” And just why should we listen to Herr Doktor Professor Jussim? Why, because his byline shows him to be fabulous! “Rutgers,” “Stanford,” “prize,” “award,” “distinguished contributions.” Perhaps the best is the subtitle of his latest book, *Social Perception and Social Reality: Why Accuracy Dominates Bias and Self-Fulfilling Prophecy*. It “ties that work [on social perception, accuracy, self-fulfilling prophecies, and stereotypes] together to demonstrate that people are far more reasonable and rational, and their judgments are typically far more accurate than social psychological conventional wisdom usually acknowledges.” Hear that, folks? Our gut feelings, shaped constantly and assiduously by the latest propaganda and the oldest social prejudices, are not just “more accurate” but “*far* more accurate” than previously acknowledged. Our “scientist” has “demonstrated” it. So the next time you read *Playboy* and come across one of Doctor Soh’s lectures about what you want out of sex, rest easy: she’s not there to reinforce stereotypes. The next time Jeremy Scahill decries the self-fulfilling prophecies of the so-called War on Terror, brush him aside as “not an expert.” And of course Jussim’s book *itself* can’t possibly be helping to promote stereotypes and self-fulfilling prophecies. After all, he’s at Stanford, and he got all those awards. So the next time a cop pulls you over for being black, pipe down and bow to science.

Genius Number Two is David P. Schmitt. After some political throat-clearing, he commences:

In the case of personality traits, evidence that men and women may have different average levels of certain traits is rather strong.

Wow, folks! What's better than a truism? How about a truism spiced with our favorite hedge-word: "may"! And throw in a "rather" for good measure! Ploughing ahead:

For instance, sex differences in *negative emotionality* (emphasis in original) are universal across cultures; developmentally emerge across all cultures at exactly the same time; are linked to diagnosed (not just self-reported) mental health issues; appear rooted in sex differences in neurology, gene activation, and hormones; are larger in more gender egalitarian nations; and so forth (for a short review of this evidence, see here [links to <https://www.psychologytoday.com/blog/sexual-personalities/201504/are-women-more-emotional-men>].)

That link to the short review of the "evidence" no doubt includes blueprints of the time machine that our pseudonauts used to travel back through all of history and study all cultures in order to claim their impressive "universality," accompanied no doubt by a footnote explaining that the decades-long exportation of Anglo-European culture across the globe, by force or by cinema, snuffing out indigenous cultures and normalizing suit, tie, and the "nuclear family," is an effect of no consequence. But pay attention to the care with which our "personality psychologist" italicizes "negative emotionality." You see, folks, we can't call it "bad feeling." It just doesn't sound *real* enough, doesn't sound like "transversality," "anti-symmetry," or "subspace frequency," doesn't sound like something you can pick up off the shelf and measure with Dr. Schmitt's magic feelings-yardstick. How else can we convince you that the reason men and women have different ways of coping with stress and expressing anger is not that they are so *taught* (to sound scientific I should say *socialized*) but that they have different "neuropsychologies," "genes," and "hormones." By the way, did you catch that cameo by our buddy, the word "appear"? Did you notice its close cousin, "linked"? And these differences emerge at "exactly the same time," no less. Jacques Barzun informs us that in the nineteenth-century it was not unheard of for a girl to get married at the age of nine—in the United States as well. But fear not! Our ingenious "evolutionary psychologists" will deploy a special Riemannian metric to recalibrate measurements of the spacetime continuum, renormalizing the sequence of human developmental stages—heck, throw in a conformal mapping to fine tune the "data"—and showing us that milestones in growth happen "at exactly the same time," and "across all cultures." Read the rest of his opinion for more juicy "truths" about sex differences. But first read his byline and remember: fifty articles and book chapters; peer-reviewed; founder; director; two hundred psychologists; in sixty countries! Look how far you can go if you just smile and stay positive!

Our third babble-wielding guru is Geoffrey Miller, another "evolutionary psychologist," whose longer effusion begins with a little gift, the kind a baby leaves in its diaper:

Among commentators who claim the memo's empirical facts are wrong, I haven't read a single one who understand [*sic*] sexual selection theory, animal behavior, and sex differences research.

Ah, *now* we see our foolish error. We have no respect for *theory*, rather like the Nobel committee of 1921, which conferred the prize on Einstein not for his untested work on relativity but for his *experimentally verified* work on the photoelectric effect. What *Spießbürger* we've turned into. After all, if unproven theory is not science, then why are the string theorists being showered with million-dollar Breakthrough Prizes? What's that you say? Sheldon Glashow left Harvard in disgust at the burgeoning of unverified theory? Well, he has a right to his opinion—but money talks, and anyway he's outnumbered.

Similarly, Miller and his fellow "evolutionary psychologists" outnumber their critics, and what makes them experts is that they can talk theory, all day long. Can you? Can Danielle Brown, Google's VP of Diversity? As our mocha-sipping expert himself snidely remarks, "I was impressed to see that her Michigan State B.A. in Business and her U. Michigan M.B.A. qualify her to judge the scientific research."

And there you have it. If we had just paid our dues, if we had only sweated through the years of rigorous, “scientific” training of the “evolutionary psychologists,” we would have known better than to descend to the gaucherie of exposing the logical nonsense in their theories. Do you still dare to doubt them? Then stand back—ye of little faith!—and be silenced by Miller’s numbers: “I’ve taught for 28 years, written 4 books and over 100 academic publications, given 190 talks, reviewed papers for over 50 journals, and mentored 11 Ph.D. students.” And that’s just one professor. Multiply by the size of the international establishment and we have Himalayas’ worth of “research.” Could such a mountain range be made of pure manure? Could the mammoth sums already spent on it and its “applications” to business and society be all for naught? Of course not. That would mean that government has spared no expense to indoctrinate the global population into a pseudoscientific ideology that controls them by prescribing roles and reversing cause and effect. Now why would the state ever want to do *that*?

Brooks

Another source of grim comedy is David Brooks’ response in *The New York Times* (<https://www.nytimes.com/2017/08/11/opinion/sundar-pichai-google-memo-diversity.html>). He soon begins:

Damore was tapping into the long and contentious debate about genes and behavior. On one side are those who believe that humans come out as blank slates and are formed by social structures. On the other are the evolutionary psychologists who argue that genes interact with environment and play a large role in shaping who we are. In general the evolutionary psychologists have been winning this debate.

If they have been “winning” debate, “in general” or in any other nebulous way, then why are you calling it “long and contentious”? Make up your mind. To “win” the debate is to *close* it, as was done for, say, Newtonian physics. It would be much more honest to say that the “evolutionary psychologists” lost it decades ago, and that government, corporations, other interests vested in reductionism, along with their colluding media, have arranged to cover up the embarrassment and keep pseudoscience running for its fantastic expediency to politics and profiteering. Damore was moreover not merely “tapping” into the debate, but forcefully taking one side of it, showing no awareness of its radical absurdities, and only rehearsing its latest platitudes. And whatever the “evolutionary psychologists” actually say, they here represent the belief that genes *determine* behavior. That is the way the belief operates, not only at the level of lay public discourse but also, as I have found from first-hand experience, in professional biologists doing research at so-called “top schools.” No amount of peppering the claims with qualifications, such as “on average” or “play a large role,” can disguise the fundamentally deterministic, if not *fatalistic*, way in which the belief is held and applied.

Brooks continues:

When it comes to the genetic differences between male and female brains, I’d say the mainstream view is that male and female abilities are the same across the vast majority of domains—I.Q., the ability to do math, etc. But there are some ways that male and female brains are, on average, different.

Excuse me? The “mainstream view”?! *You’d say*?! Thank you for reminding us that popular superstition is its own justification! Time to go back to Aristotelean physics—I’d say!

There seems to be more connectivity between the hemispheres, on average, in female brains. Prenatal exposure to different levels of androgen does seem to produce different effects throughout the life span.

Seem? *Seems*?! This is *science*?! For heaven’s sake, have you ever heard a chemist say, “Stop the presses! There *seem* to be six electrons in the carbon atom!”? Brooks then cites some of the “experts” I have exposed above, including Soh and Miller. One rich part is Miller’s “for what it’s worth.” It was decades ago that “evolutionary psychology” was shown to be pseudoscience, so his opinion is actually not worth the paper it

is printed on. But I suppose the reader who is shocked by this assessment can be forgiven. It would after all be the first time that the *New York Times* printed drivel.

Next he preaches a curious kind of sensitivity:

We should all have a lot of sympathy for...the women in tech who felt the memo made their lives harder. Picture yourself in a hostile male-dominated environment, getting interrupted at meetings, being ignored, having your abilities doubted, and along comes some guy arguing that women are on average less status hungry and more vulnerable to stress. Of course you'd object.

Better yet, picture yourself in a world in which pseudoscience is peddled by the government, by the universities, in the media, and in the workplace, and, when it clashes with political correctness, is roundly held up as unfortunately valid (because it *seems* so, because there are a lot of syllables in the words “evolutionary” and “psychologist”) and in which you are therefore, oh so sympathetically, patted on the back for belonging to an “on-average” underperforming gender, and given the “sympathy,” the oh-so-saving consolation, of being told you are one of the *abnormal* ones who might, just might, be not so underperforming. Meanwhile, have another dose of this universal pity. After all, it works miracles on your morale.

As you can see, the scientistic mentality is radically, dogmatically blind to its tendency to reverse cause and effect, blind to the effect of *feelings* on its human object—even as it professes to *appreciate* them. Brooks and his annoying ilk never seem to stop and realize that these backward effects don't work on, say, molecules and electromagnetic fields. No amount of pity will influence the behavior of an electron, but even small amounts of it can undermine a person—and conveniently drive self-fulfilling prophecies.

Next Brooks proposes a kind of legerdemain with “truth”:

What we have is a legitimate tension. Damore is describing a truth on one level; his sensible critics are describing a different truth, one that exists on another level. He is championing scientific research; they are championing gender equality. It takes a little subtlety to harmonize these strands, but it's doable.

Of course subtlety is in hibernation in modern America.

No it is not “doable,” because no amount of “harmonizing” can transform pseudoscientific nonsense into “truth.” Only one “level” should be of any importance here: not the level of political correctness, nor that of radically broken “science,” but the timelessly reliable “level” of conscientiously applying sound and critical sense to every little deductive and inductive step in the thinking of those claiming to be experts. That application often finds them wanting not just in subtlety, but also in basic judgment, and even sometimes in honesty.

Next, regarding Google's VP of Diversity, Brooks writes:

She didn't wrestle with any of the evidence behind Damore's memo. She just wrote his views “advanced incorrect assumptions about gender.” This is ideology obliterating reason.

He is quite right to say that Google responded with typical PC brutality. But he does not go on to say that the company, along with every citizen in the country, should have lived up to its *civic* responsibility to interrogate the “science,” using common sense as we did above, and *show* it to be a Cargo Cult. That would truly have been reason obliterating ideology. Instead what we have is the clash of two ideologies, each using its own classic mode of suppression to marginalize critics.

Then, in a scandalized lament about the “mischaracterization” of the memo, Brooks writes:

Various reporters and critics apparently decided that Damore opposes all things Enlightened People believe and therefore they don't have to afford him the basic standards of intellectual fairness.

Enlightened People! It seems Brooks thinks that double-capitalizing the term will somehow turn intellectual indolence into gold. But if he and his establishment had a genuine interest in “intellectual fairness,” they themselves would not have systematically marginalized the criticisms of anti-pseudoscientists—Hubbard, Lewontin, Gould, Byne, Feynman, *et al.*—from the pages of their own magazines, newspapers, and television documentaries.

Next, a lecture about mobbing:

The mob that hounded Damore was like the mobs we’ve seen on a lot of college campuses. We all have our theories about why these moral crazes are suddenly so common. I’d say that radical uncertainty about morality, meaning and life in general is producing intense anxiety.

I am sympathetic to any complaint of shocking and aggressive hypocrisy in political correctness, on all sorts of campuses. But it’s interesting that Brooks wouldn’t “say” that the influence of pseudoscientists, not least the “psychologists” pathologization of anger and other appropriate feelings, might not be to blame for furthering the moral inversion he laments in mobs.

Next we attack the CEO:

Which brings us to Pichai, the supposed grown-up in the room. He could have wrestled with the tension between population-level research and individual experience. He could have stood up for the free flow of information. Instead he joined the mob. He fired Damore and wrote, “To suggest a group of our colleagues have traits that make them less biologically suited to that work is offensive and not O.K.”...Either Pichai is unprepared to understand the research (unlikely), is not capable of handling complex data flows (a bad trait in a C.E.O.) or was simply too afraid to stand up to a mob.

This must be why Brooks’ rag mobbed Phil Donahue and Chris Hedges out of their jobs. They were impeding the “free flow of information.” And Pichai should somehow have known that the truth is about “harmony” but simultaneously about “tension”—verily a paradox for the subtle. The last time I checked, a “grown-up,” by definition if not also by the Constitution, is one who constantly distrusts authority, including that of the mob of white labcoats. In any case, for all their “Enlightenment,” none of these people seem to understand that the cure is not to abort the discussion but to *deepen* it.

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