

Some of the “science” cited by James Damore is bogus, and that’s completely unsurprising

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News has just broken 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.” As Debra Soh put it in the title of her article in *The Globe and Mail*¹, “No, the Google manifesto isn’t sexist or anti-diversity. It’s science.” In it, she asserts, “Scientific studies have confirmed sex differences in the brain that lead to differences in our interests and behaviour.” In particular, she says, 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 things that are...well...more cuddly.

Which side is right? In the truest sense, neither is, because the problem is deeper than the irritation has let on, and its implications are more sinister. Here we examine some of the papers cited during the media storm, because even a nonscientist can show that they’re bogus, either in and of themselves, or in the way in which they’re being cited.

Pasterski *et al.*

One botched study, by Pasterski *et al.*, is available at <http://onlinelibrary.wiley.com/doi/10.1111/j.1467-8624.2005.00843.x/abstract>, and its abstract 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 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.

Before discussing the results, let’s recast the situation in the standard language of the scientific method, such as we find in high school or middle school science courses. It will shed light on the study’s mistakes. 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.

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

- IV 3: how much the child was encouraged to play with sex-typical toys. This can take 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. This is the *dependent variable* ("DV"): whether or not the child's preferred toys are sex-typical. So the DV 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, above*. In the case of the boys (combinations 1 and 3), the IV 3 was *controlled* (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 (gender) 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 arouse the suspicion that a mistake has been made, 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 were “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 of this whole line of “research” 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 didn’t botch the experiment in the naïvely reductionist way, and they didn’t botch it in the naïvely constructivist way. The fact that they didn’t test all possible combinations of the IVs (5 through 8, inclusive) shows that they did botch it. But they did so in a telling way, for they measured 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 would 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—that it’s real.

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 the sad fact is that we live in an age in which even such people miss the much deeper problems with the study, if not with the whole field. 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 twenty-third 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:²

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 too scientific an oversimplification) 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. Working our way from smaller to bigger problems, let's begin by considering that word "measure." How, pray tell, can this be done? The experimenters could only have done something equivalent to putting parents and children in an observation room filled with toys, and counting the number of times they heard the parents say "Good girl!" But this is fatuously stupid. For starters, the parents and children are also interacting *outside* the "laboratory," so how can the count be reliable? What if the parent says "Good girl!" twice in a row, 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 softly? What about "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 school, from other family members, from friends and neighbors, and, perhaps most overwhelmingly, 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?

And speaking of age, has nobody noticed that it is in fact a *fourth* independent variable, and is being left *uncontrolled*? What makes this lapse even more ironic is that age is perhaps the most "ontologically stable" variable: unlike the others (even biological sex!) there is absolutely no ambiguity in its definition, no difficulty in measuring, with fabulous precision, how old a child is. (To belabor the point further: even if the child's age were to suffer relativistic twin-paradox modifications owing to super-fast space travel, it would still be well defined in principle and easy to measure in practice.) So why is nobody shocked by the fact that it wasn't controlled?

The authors also presume to know how to classify toys as "sex-typical" or not. It's a good thing they're not doing the study in Sweden, where I hear it is normal for boys to knit. Or in Scotland, where people love

²https://en.wikipedia.org/wiki/Congenital_adrenal_hyperplasia, as of August 20, 2017.

to see them wear kilts. One can only wonder how to decide that a given thing is “sex-typical” for the poor brain that has been partly exposed to American culture, partly to Swedish, and partly to Scottish.

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 not in what is literally said, but in what is *not* literally said. As this same corps of so-called experts has gotten our society scientistically to recite, “Communication is 90% nonverbal.” (For now, let’s overlook the fatuity 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 and you could seduce a person into intimacy, might even excite sexual desire. Indeed, if the minimum age of this sample weren’t three years, one could easily have argued 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 it in remind us that communication doesn’t need *anything* to be said, then we can find it in Hastings’ book on child sexual abuse [Has94], in which she notes that a breastfeeding mother can tell that the baby can sense whether she is sexually aroused by the sucking.

So if our brilliant experimenters do indeed hear a parent speak *words* of encouragement to their children, how can they know that it will have the effect they expect? The very fact that a parent knows that the child is sexually abnormal means that the parent 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*, cannot produce counterintuitive effects? And even if such foolery is to be countenanced, has no one noticed that the experiment is botched by the very fact that it isn’t *blind*, that the parents actually *know* which kids have CAH and which don’t?

Bramble *et al.*

Another one of the studies cited by Soh is bogus not necessarily in itself but in the way in which it is cited. It’s the one by Bramble *et al.*, entitled “Sex-Specific Effects of Testosterone on the Sexually Dimorphic Transcriptome and Epigenome of Embryonic Neural Stem/Progenitor Cells.”

Sounds fancy, right? The abstract³ 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 that it’s *irrelevant*. We already know that there are sex differences in the mammalian brain. 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 answer it.

Su *et al.*

The reductionists, as typified by Soh, Damore, and company, is that prenatal testosterone levels determine “interests,” and of course they try to soften the coarseness of this claim with presumably sophisticated

³<https://www.ncbi.nlm.nih.gov/pubmed/27845378>

lip service paid by saying, “on average.” For this purpose they cite a paper by Su *et al.*,⁴ entitled “Men and things, women and people: a meta-analysis of sex differences in interests.” Here’s 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. All it means is that the paper aggregates the results of other studies. But of course, if the latter are as shoddy 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?

Other problems can be uncovered by using *real* analysis. 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...” Will no one stop and ask: how do you objectively measure an *interest*? The only way to make the measurement seem scientific is to present a person with a *scale* and to say something like, “Rate your interest in kayaking, 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. Was Aristotle interested in physics in the same way that Einstein was? Somehow, because arm lengths, tree heights, and 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, as distinct from, say, desire, even be *defined* “scientifically”? And why is there no respect for the fact that interest can change profoundly with time? Meanwhile, note again that none of these problems come up when measuring, say, the strength of a magnetic field.

Similar problems of definition 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 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 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, new 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.

⁴<https://www.ncbi.nlm.nih.gov/pubmed/19883140>

As for “data/ideas:” before a biologist can even get up in the morning to go to work and operate the devices used in an experiment, he or she is motivated by whole layers and systems of ideas, from those pertaining to the mechanical effects on and between the involved molecules, all the way to those pertaining to the “highest” (most general) models of evolution. Before a pollster, for example, can go out and start dispensing 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* be found for such things and even if it *did* show “leanings” to one side or another, 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 not place a psychological *pressure* on its subjects, exerting self-fulfilling forces precisely as they purport to classify passively?

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.” Consider, however, that 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 “research.”

More, this taxonomy too bends toward self-fulfilling prophecy. The gullible, 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 sciency-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. In other words, we have here every possibility of *reversing cause and effect*, whereas that trick cannot be played on an inanimate object, which is no small part of why fields that study inanimate objects—physics, chemistry, *etc.*—are *real* sciences.

Still more: even if this taxonomy of personality types were neither fatuous nor self-fulfilling, what makes you think it is *stable*? That is, what makes you think that a person who appears “artistic” to you in her twenties will not appear “enterprising” ten years later? The retort of course is that these “types” are genetically determined. But this shows even *more* reversal, for when a person responds typically to such “assessments”—*i.e.*, submits to their false authority and proceeds to define herself in their terms—the effect itself is then taken as confirming a genetic cause! And apart from the stupidity of such a notion of permanence—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 reader should note how conveniently it plays into the government’s desire to *control* its subjects. We’re not satisfied with just stuffing you into a box; we must also be sure it never moves.

Finally, after a couple more sentences of dry and solemn scientism, the abstract concludes, “The present study suggests that interests may play a critical role in gendered occupational choices and gender disparity in the STEM fields.” What? All this work, all these papers, all this “meta-analysis,” let alone the fuss made of it in the media—all just to tell us that men and women have different interests? What? You’re telling us that a person’s interest influences her choice of career? What a gorgeous display of pedantry! Thank

goodness for “science!” Thank goodness for the *Psychological Bulletin*, otherwise we’d never know what we already knew by looking out the café window! But in all seriousness: note the magical way in which the impression is given that something has been *explained*. And yet the obvious explanation is not the one the authors have in mind: the environment *tells* men and women to be interested in different things. The culture has prepackaged a set of subtle rewards for people who conform to its notions of gender, and it confers those rewards “differentially” (ah, the fetishization of that scientific word!). Instead of calling the results predictable from culture, our hero-pseudoscientists call them evidence of genetic determinism.

It gets worse

If you’re getting depressed by all this institutionalized idiocy, then prepare for worse. For starters, observe that Pasterski *et al.* are affiliated with highly regarded institutions—UCLA, University College London, City University in London, *etc.* And considering that the paper was actually published, it 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.

But even this is not surprising, because 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 people such as Ruth Hubbard [HW99], Richard Lewontin, Leon Kamin, Steven Rose [RSL84], Stephen Jay Gould, William Byne, and Richard Feynman [Fey], no less. As for the problems of peer review, Michael Crichton was so disturbed by its corruptions that he took the trouble to speak out about it. Of course, all of these alarms have been systematically ignored. (Why? Think!)

And the scandal doesn’t really stop there. It involves you and me as well. To understand what I mean, consider that the principles of the scientific method are based on nothing but common sense. They don’t require any calculus, any algebra, any abstruse terminology (unless we call the word “variable” abstruse), or any arcane mathematics. They require no special training at all. The only skill, apart from high-school biology, that we used to expose the mistakes of Pasterski *et al.* is elementary *logic*, which might better be called simple common sense. And yet we grant funds and influence to these quacks, 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 can’t evaluate the work done in the *real* and advanced sciences; subatomic particles, for example, are not part of our everyday lives. But *behavior* has been observed, engaged in, reflected on, and talked about, systematically commented on, by everyone and for many millenia. How did the twentieth century persuade us to relinquish that authority 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*. Those wanting a sense of the epidemic incompetence in the so-called behavioral sciences can read Feynman’s short graduation speech, “Cargo Cult Science.” And Jeffrey Masson, particularly in his *Against Therapy*, mounts careful attacks on the constellation of shams known as “psychotherapy.”

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