Wrong for the wrong reasons:
lessons on November 9th for the science of human behavior

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This brief note is written at a very particular time, twenty-four hours after some of our best scientists of human behavior failed, utterly, to correctly predict the outcome of the 2016 U.S. presidential election.

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1 Right for the Wrong Reasons

In a high school mathematics exam, you’re asked to show your work: not only to give answers to the questions, but also to present the steps you used to derive them. That’s because (your teacher fears) you can be right for the wrong reasons. When there are just a small number of possibilities—or when the wording of the problem suggests an answer—it’s possible that a guess will be correct, even if it is made on the basis of inconsistent reasoning or even just a whim.

Your high school mathematics teacher knew more than you thought. We’re often unduly impressed when people get the right answer, because we forget the world is largely predictable, or because we’re constantly predicting and forgetting our misses. And sometimes, we go the other

way, and are unduly critical when a prediction goes wrong. Not wearing a seatbelt makes it more likely to die in a car accident—a fact not disproved when someone survives without one. You can get the wrong answer for the right reasons.

That’s why scientists build models: because predictions are not enough. Predictions do a lot of things. They can provide comfort, express moral views, or act as warnings. It can feel powerful to make a prediction, even if it is about something bad. But if a prediction is to advance our understanding of the world, it must be derived from a story about “how things tend to work”—what scientists call a model of the world.

We can see this by looking at how a model works. Say, two days before the 2016 election, you assert that people who are going to vote for Trump are concealing their preferences from pollsters; that’s (part of, or the beginnings of) a model. That model then might say that you ought to correct polls in a certain way, and this correction propagates through a chunk of mathematics, and as a result predicts that Trump will win the election.

Two things happen as a result of this. The first is that you’re making an unusual prediction; as a factual matter, had you actually done this, you’d have differed with most other people in the room. The second is that you are asserting a different view of how the world works, a different view of people talk and act, and how (in this case) talking about who you are going to vote for relates to whether or not you are going to vote, and what happens when you do.

For science, the only conflict that matters is the second one: which model of reality to prefer. The first conflict, over who predicted better, matters only in as much as it helps us resolve the second. Now to be clear, science’s ability to make successful predictions is the source of much of its status in society. And a successful prediction may increase the prestige of the person making it. Predictions matter and are part of the institutional structure of science. But if you don’t understand that their real job is to tell you about models, you will not only misunderstand science (and, indeed, declare many excellent sciences, such as astronomy and evolutionary biology, out of bounds), but you won’t be able to get what you really want, which is better predictions in the future.

In this note, I’m going to make the true role of predictions explicit. A good scientist (I’d say, a scientist who is able to function effectively in whatever community she’s in) knows what I’m about to say intuitively. She might differ in places or in emphasis. She might differ on the domains in which she thinks the story works, but modulo all of that, the story we tell will be roughly the same.

Indeed, many people outside of science will also find these ideas completely transparent. So why talk about it at all? I say that at a critical moment, it is worth slowing things down. Your reward for finishing the piece is at the end it will speed up really quickly and I will say a lot of crazy things (see the table of contents).

2 A Brief Account of Science, in Seven and a Half Paragraphs

Science talks a lot about probabilities. Most scientific papers end up, at some point, saying that some kinds of things are likely to happen, or (conversely) very rare. Some papers even say that something is, perhaps, “70% likely to happen”. In ordinary life when we do that, we’re making a

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1Grokking this was a key stage in the evolution of the sixteenth century ideas of a proto-scientist like Francis Bacon into the beginnings of modern science (in the European tradition) of “Royal Societies”, journals, and discussion groups a hundred years later. It’s related to (but certainly not identical with) the insight of someone like Karl Popper, who said that a theory is scientific—i.e., that we have some hope that it might do all the amazing things scientific theories have done for us in the past—if (and only if) it is falsifiable.
guess or, perhaps, implicitly offering someone odds on a bet. If you’re willing to talk in terms of probabilities, at least for a little while, you can get clear on some important aspects of the scientific process. I’m definitely not saying that talking about probabilities is all of science (though some people have), and I’m certainly not saying all knowledge looks like this (though again some people, perhaps a smaller number, have).

Once you think about the special case of predictions as probabilities, a model is just a specification of how likely different things are to happen—in other words, a model is just a bigger set of predictions. Two things have to happen for this to make sense: (1) the model has to specify more than just the particular (possibly one-off) event you’re interested in, (2) the other things it predicts have to all relate to each other.

To make this clear, here are two examples of models that don’t work.

“Trump is likely to win the election”

“Trump is likely to win the election. The temperature in Singapore yesterday was 96°F”

The first fails because there’s no way to judge the model. Either he will win, or he won’t, and we’ll just have to wait. It doesn’t say how this prediction connects to, or depends upon, any other statements or claims that can be tested. The second fails because the precise temperature in Singapore yesterday has nothing to do with the election. If the model gets the second thing wrong, it doesn’t really have any bearing on the first. While formally, it’s possible to write down models like the second one here, a good scientist will immediately say, “oh, you have two separate pieces here: a (dumb) model of the election, and a (dumb) model of the weather in Singapore.” More abstractly: you judge models that predict the thing you care about by looking at the other things they predict and asking if they came true. But this can only work if the other things the models predict have some bearing on the thing you care about.

Here’s a better model:

1. “People vote the way they tell pollsters they will vote. People today are telling pollsters that they’re going to vote for Trump. Trump is likely to win the election” (M1)

The model, which I’ve called model M1, makes lots of predictions—not just about Trump, but about things that happened in the past. It predicts, for example, that if we look at historical polling data, it will be very strongly correlated with election outcomes. Here’s another model

2. “People vote the way they tell pollsters they will vote. People today are telling pollsters that they’re going to vote for Trump, but many of those are young men who won’t show up at the polls. Clinton is likely to win the election” (M2)

M2 not only makes different predictions about the future, it also makes different predictions about the past.

We can look at the past data on elections and see if M2’s “young men” correction is better at getting the outcomes right. If it is (roughly speaking), we then say we ought to prefer M2 to M1; and therefore, of course, we ought to think that Clinton will win. Note that if I look in the history of the data, and end up preferring M2 to M1, but Trump wins, I am “wrong for the right reasons”: I have chosen the model that all the evidence seems to prefer but, for various reasons (both models are wrong, predictions are only ever probabilistic), it failed.

At some point, people noticed how this pattern of argument happened over and over. Then they noticed that it looked a lot like a certain kind of mathematics that had been invented in the

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"As the old joke goes, “the chance of anything happening is 50:50: it will either happen, or it won’t.”
eighteenth century. And then (after two hundred years of work) they turned it into a systematic method. That’s called Bayesian Statistics.

3 Cosmology and the Bayesian Shock-front

There are three things you need to know about Bayesian statistics. The first is that it requires you to specify probabilities exactly—you have to be able to say things like “the probability that this state will vote for Trump is 23%”. That’s not too bad, though there can be a lot of math in combining them together. Whatever you do, don’t think about probabilities in terms of counting how many times something will happen if you try over and over; the Bayesian notion is about what you think and believe, not what happens. Think about probabilities as degrees of belief, which you already do in ordinary life (“Trump is likely to win the election” doesn’t mean “If there were 1,000 elections, Trump would win seven hundred of them”).

This is connected to the second thing you need to know about Bayesian statistics, which is that it allows you to put probabilities on models—to specify the probability that a model is “true”. If that seems strange, you’re not alone. You can think of it like the world “offering you odds” on whether or not the model is true, or (as many people do) specifying the degree of belief you should have in the model being true. Something like this has to happen, because Bayesian statistics is mathematical theory of what you should believe (in certain circumstances). It has to put numbers to degrees of belief in order to even get going.

The third thing you need to know about Bayesian statistics is that it changed science forever. I know this because I saw it happen. When I began as a graduate student in the Princeton Astrophysics program, the cosmologists—i.e., the astronomers who worked on the very largest scales, studying the expansion of the universe—were just learning it. You could, essentially, draw a line across the history of the field, splitting it into before Bayes and after. Before Bayes, there were a lot of great observations that people made. Each one told you something about the way the universe worked—how quickly it took galaxies to form, for example, or how fast the universe was expanding. After Bayes, there were even more great observations made, and now each of them helped tell you about everything else. That’s because the model of how quickly the galaxies were rushing away from each other also made some predictions about how quickly the galaxies formed. So if I got a little bit of data on how galaxies formed, it would contribute to our knowledge of how the universe was expanding. Even if it wasn’t enough to really make much of a difference on its own, it turned out that different observations gave us complementary bits of what we needed.

I was conscious really only during the After Bayes era in cosmology. So when the shock front hit a different part of the field—the community who studied “compact objects”, or black holes—and had a similarly massive effect, I got to watch it. As my horizons widened, and I started to work in different fields, one of the first things I wanted to figure out was “have they discovered

\[3\] Notice something fun that happens: since models themselves talk in terms of probabilities, Bayesian statistics is essentially about attributing degrees of belief to statements about how likely something is to happen: probabilities about probabilities. There turns out to be no vicious circle here, although it can seem like an infinite regress at first. Questions you might have about the details may well be answered in a longer-form piece Bayesian Reasoning for Intelligent People or just drop me a line.

\[4\] An example of this comes from a recent article in Scientific Reports, that noted that one of the major datasets (“Type Ia supernovae”) in cosmology showed at best only weak evidence in favor of dark energy. This was interpreted by some as a claim that a key feature of our modern picture of the Universe was in danger. As many in the community were quick to point out, however, the evidence for dark energy becomes overwhelming if you combine the supernovae evidence with evidence from other sources of data.
Bayesian statistics yet”? Molecular biology and genomics (yes), animal behavior (no), psychology (yes, depending on subfield), political science (yes, depending on subfield), medicine (no).

What happens when the Bayesian shock-front hits your discipline? A few things. As alluded to above, in the cosmology story, you start getting massive returns to scale. Every little thing that people discover—and every new theory that people propose—has ramifications elsewhere. You can move from saying “well, I like the theory you build for Jane’s data, but Mike’s result over here seems to argue against it” to “I like your theory, and Jane’s data support it, but once you add in Mike’s data, I like the other theory better”. So much of how we make progress in science hinges on the subtle shift from she-said-but-he-said. Even before you start combining data, you also get more reliable and reproducible results, because the Bayesian framework avoids the standard fallacies of the statistical approaches it replaces—a freak result, for example, eventually becomes down-weighted over time.

4 James Kanagasooriam’s Tweet

Take a look at Figure 1, tweeted twelve hours ago by James Kanagasooriam, Head of Analytics at the firm Populus in London. Basically, what the figure shows is not only that the polls underestimated the number of people voting for Trump, but that there’s good evidence, on the face of it, that there’s something very wrong with how the pollsters are modeling white people without a college education. The polls underestimated Trump’s support more in states like Kentucky, where there are more of—let’s be clear—the demographic that includes Hilary Clinton’s “deplorables”. In fact, in Kentucky, the polls were 6% or more off, biased in favor of Clinton.

At the risk of stating the obvious, James’ plot is a big deal. The challenge for the next months is how the insights and hints like these that are lying all over the place can be drawn together. Say we take inspiration from James. You sit down to build Bayesian models of the election that tell a story about white people without a college education.

Two things will happen. First, you’ll get better at predicting the next election. Assuming you do the mathematics correctly. The technical theorem says that if you do everything right, at the
to get those predictions right, if only because it is good to know the truth. People look to science in part to make the world a more predictable, manageable place, and the better we know what is going to happen, the better we can plan. To return to the beginning of this paper: prediction matters, and always will.

A second thing will happen. When you write down your model, you’re actually writing down a whole family of models each of which says something different about this crucial demographic. When you do all that work to test them—just like I asked you to think about choosing between M1 and M2, above, but now for real—you will be doing more than just getting better predictions.

You will, also, be learning about the nature of elites and non-elites, class, and race in America. In fact, even more: if you do the work to hammer out a model, it will almost certainly make predictions everywhere. You’ll discover, perhaps, that what you’re doing will also, with a bit of effort, make economic predictions. Right or wrong, those economic predictions will feed back into the fundamental social science question, and one of the most important (surprise) policy questions of the next decade: how to handle a transition to a truly cosmopolitan society.

5 What we need: a Bayesian Social Observatory

In the original version of this piece, I had an extensive discussion of failures of the Bayesian models that people used to make their election predictions. I focused in particular on Sam Wang’s work for the Princeton Election Consortium, because I had come to understand it quite well. I felt, for example, that Sam’s method artificially inflated his uncertainty, and that he was representing his predictions as less certain than his models said they were. That’s a problem, because if you reason to believe your models are too certain, it means you have reason to believe your models are wrong and you should fix them.

That was the original version. But it was completely unfair, because the reason I was able to write that critique at all was because Sam had done the work of making his methods transparent and open. It’s a difficult thing to do, and even there I had to be aided by a colleague who had happened to hear Sam give a lecture in person and could update me on the most recent iterations very worst, on average, if you add something to the model you’ll do no worse than before. That doesn’t mean you should throw in everything and the kitchen sink to a model because (1) it’s hard to do things right and people are always screwing up even the simplest models, and (2) the theorem only works on average, meaning that sometimes you’ll do a lot worse. Variance can (and usually does) go up, in other words. A special case of this is called the bias–variance tradeoff. It’s also important to note that I might be mis-reading James’ plot altogether. It’s possible that this relationship is something you’d find even in the absence of a demographic bias in polling error, and could be driven, for example, by an effect I overlooked that means that just naturally, if $X$ gets more votes in the election, the polling error for $X$ is going to be biased against them. I’m not sure how that could happen, but you’d want to think that through, and I mention it in part to make clear just how difficult and hard the actual work of doing this is.

6 Another way to phrase it was that he added something to the model to get a new model that had higher uncertainty, but—to be frank—even though he presented some good evidence for it this modification, I didn’t believe the underlying explanation for why the term appeared. And it bothered me also that his addition made no new predictions on the big things: in particular, as far as I could tell, it couldn’t ever reverse the outcome of an election—which is exactly, as it turned out later, what needed to happen. You can read a version of this (written before we knew the predictions were going to fail) in Section 9 of [Bayesian Reasoning for Intelligent People](#).
that didn’t appear in the scientific literature. Sam is light-years ahead of many of his colleagues, who are doing a huge amount of work, but failing to give it back to the community in a way that can make us all better at science.

This is particularly critical because in fact many purely political science models did much better than the polls. In August, Vox Media published an article pointing out a very strange divergence, between the polling data and models from the peer-reviewed political science literature. The six political science models relied solely on “fundamentals”, i.e., facts about the political and economic conditions of the country, and not how the candidates themselves were doing. They were combined by Jacob Montgomery and Florian Hollenbech using Bayesian methods where the predictions of each model being weighted by the degree of belief given performance on past predictions. Individually, the six models split: three predicted a Trump win, three a Clinton win. The models that predicted a Trump win did better on past data, however, and so the prediction of the combined set was for Trump.

One of the main lessons that the current polling sites draw is the failure of the polls themselves. If we are counting up the number of times elite organizations, of the best and brightest, failed to predict a resurgence of popular, right-wing anger, another two data points would be, in addition to the failure to predict Trump’s nomination, the failure of both polls and prediction markets to foresee Brexit, and the 2015 failure of pre-election polling to predict the defeat of the British Labour party. The post-mortem report was clear where the fault lay:

the primary cause of the polling miss in 2015 was unrepresentative samples. The methods the pollsters used to collect samples of voters systematically over-represented Labour supporters and under-represented Conservative supporters.

The percentages reported by pollsters failed to correspond to the behaviors of those who actually voted, and this was a major factor in the failure of quantitative social science to provide advance prediction of the election result. Drew Linzer (both an academic, and chief scientist a Civiqs) puts this plainly, and echoes what Sam Wang noted before the election: that the actual election outcome required correlated poll biases on the order of five percentage points. Such biases would be unimaginable under standard models, including ones, like Sam’s, that bootstrap estimates of poll bias from prior elections. Linzer’s original paper describes a Bayesian fit to a phenomenological model of drifting preferences within a fixed population. One of the missing pieces I see in much of this work is the reliance on pollster-reported percentages, rather than the more complicated data that pollsters release which contains clues to the kinds of corrections they apply before reporting their final “P(win)” Monmouth University Polling Institute and Bloomberg are two examples of
polling services that release extensive accompanying data including, depending on the organization, demographics and other respondent-reported opinions. All of this additional data, for a good Bayesian model, provides crucial information on the extent to which a sample (or its post-hoc correction) is representative or not—with just a few sources of good anchoring data, these additional pieces of information create not just an election model, but a model of the public itself.

While it’s fun and instructive to compare and contrast methodologies, however, this is not the place to do it; these papers are excellent reads and give a sense of the difficulties that come from a small-scale operation. An academic sees herself as contributing to the peer-reviewed literature—not to the critical goal of a nation’s self understanding. A newspaper website or a blog sees itself as educating people about statistics, telling stories, engaging with readers, responding to comments, and—I say this without prejudice or criticism—building a brand. Resources are limited. Collaboration is constrained by academic publication priority, by the need to be considered and to speak “only when certain”—or, conversely, to provide information right there and then, for a deadline. These are all field norms; field norms underlie institutions; and institutions matter.

What could you do with unlimited resources and a different set of norms? Or, to put it another way, what could you do if many thousands, or tens of thousands, of academics, statistics nerds, policy makers worked together on a common project?

Such work is fundamentally interdisciplinary. Montgomery & Hollenbech’s predictions were based on a combination of political science acumen—knowing, at the very least, that these models existed, and which ones were respected. They were unusual (in my view) in political science for incorporating more advanced Bayesian techniques in their analysis. But even they, or people inspired by their work, would have had a great deal to learn from, and to contribute to, the hyper-quantitative people coming out of data science.

Put another way, I have never seen more effort, from more people, in more disciplines all focused simultaneously on a single question: what’s the matter with America? People are voluntarily booting up R and making scatter plots and sharing resources without getting paid, a kind of science wiki spread across multiple channels. We’re pouring resources, debate, and discussion into a system but we’re not seeing the payoff because the centralized sources, that we’re all arguing against, or with, or for, are largely opaque. There’s no spec for Nate Silver’s predictions that I can find, no wiki or blog post that writes down the conditional probabilities and specifies the models in the way it should.

There are many reasons for that, one of which is simply incentives. Good, reproducible science appears to clash with commercial imperatives: you can’t give subscribers (or advertisers) privileged access to something you’ve just given away. Paradoxically, it is the extreme value of this kind of work—the fact that an election website like 538 or the UpShot has people pouring in, and is replacing standard shoe-leather journalism—that makes it difficult for it to get better.

This can be overcome. Incentives are not everything. Or, rather, we have a complex set of goals and desires, largely incommensurate with each other and (contrary to neoclassical economics) non-exchangeable, and there’s great evidence in practice—Wikipedia being the obvious example—that people will give to a collaborative activity. I’m not saying that we should copy Wikipedia but I believe someone, like Michael Nielsen, who has thought about these questions of collaborative science a great deal, has ideas.

A second problem appears to be the first-mover problem. There doesn’t appear to be benefit to putting your model online, in clear and transparent Bayesian form—let alone updating it in
response to criticism. I think this is a complete red herring, because the first-mover problem is also the first-mover advantage. Whoever sits down and writes the first open Bayesian model of elections has a good chance of coming to define the field for years, putting their own concerns, questions, and (of course) prestige at the top of the pack.

6 Bayes vs. “Machine Learning”

Bayesian statistics is not the only tool we have. Some of the most compelling tools we use to make predictions—particularly those coming from Silicon Valley—rely on entirely different ideas. One of the most famous is deep learning, which is essentially an old idea (neural networks) plus large amounts of computer power. Nobody expected them to work as well as they did—least of all, I think, their inventors, who spent some time in the wilderness while computers caught up. Let’s call all those tools, that aren’t Bayesian, “machine learning” (a term that can cover many things).

I’m not talking about those tools here, for a very simple reason: they don’t contain models. Or, rather, they contain an implicit model of the world, but not one that humans can understand. We can feed a machine learning algorithm all sorts of data, huge stacks of it, and it might make very good predictions. But we can never, in those cases, say why the predictions came out the way they did. There’s some work on making machine learning “interpretable”, meaning, on recovering the implicit models that underlie its predictive power, but this is in its infancy. As I noted in a different piece (on the use of machine learning in criminal justice):

The method itself is usually impossible to read, let alone interpret. When we do attempt to represent it in human-readable form, the best we get is a kind of spaghetti code that subjects the information to multiple parallel transformations or unintuitive recombinations, and even allows rules to vote against each other or gang up in pairs against a third. ... advances in machine learning generally amount to discovering particularly fertile ways to constrain the space of rules the machine has to search, or in finding new and faster methods for searching it. They often take the form of black magic: heuristics and rules of thumb that we stumble on, and that have unexpectedly good performance for reasons we struggle to explain at any level of rigor. As heuristics are stacked on top of heuristics, the impact of these advances is to make the rules more tangled and harder to interpret than before.\[12\]

To make progress in science, predictions have to tell you about the model you have of the world. But much of machine learning can’t even tell you what the model is.

Nor, for that reason, can machine learning tools draw (in any real sense) on the wisdom of the qualitative literature, or other scholarly or journalistic accounts. You can’t read an article in political science, or gain an intuition from the field, and use that to alter the way a machine learning algorithm interprets and works with the data. You can only hope it discovers it for itself—and even if it does, you’ll never know. Never say never: perhaps we will have truly interpretable machine learning tools (and friends of mine have worked on them). Perhaps the machines will explain themselves to us! But for now: not yet.

7 History Beyond Numbers:
Chris Arnade, Zeynep Tufekci, Matthew Stoller, Alexis de Tocqueville

In order to play the Bayesian game, people have to be much more rigorous about what their models actually mean. That means that confusions and ambiguities are made ruthlessly apparent. It is amazing to see how often the blocks that stop you in your mathematical work originate in poorly-stated assumptions—some models are born clear, some achieve clarity, and some, mostly Bayesian ones, have clarity thrust upon them. Because of this, it’s much harder for two people to talk past each other when the models are written down in this form. Bayesian statistics is a tool for when models reach a certain stage of maturity, or you want to reap the other benefits of the Bayesian revolution mentioned above.

It is not, or should not, be a theology or a cult. Ambiguity in a theory is not necessarily bad. A crucial stage in the scientific process involves playing about with poorly-phrased models. Models do lots of work, and sometimes they should be judged on other criteria—such as, for example, how much they explain, or how much they illuminate.

Nowhere is this more clear than in the discussion of the state of the American republic. It is fair to say that nobody in the quantitative social sciences knows what is really going on in America, or its sister civilizations in Britain, the Commonwealth, and the European continent. What we can do is construct models that try to capture some aspect of what might be going on, and test them in different ways. Test them collaboratively, openly, robustly, with the best methods we have.

But where do those models come from? Social science is not physics, and the challenge of a Bayesian Observatory is to make sure that that very tempting confusion does not go through. In physics, a model tends to suggest its generalization: we look at the structure of the Standard Model of Particle Physics and notice a gap in the mathematical description that we try to fill. Models come from prior models. In social science, this has almost never happened—with the exception, perhaps, of some pieces of game theory.

In the social sciences, models come from minds—and, in particular, a very different kind of mind than that of the computational social scientist. Models come from people in the library stacks, or in the policy community, or in the world at large where they do a very different kind of work than the Bayesian framer and tester. They come from people enmeshed not in the thin world of mathematical descriptions, with limited variables describing a small and encompassed space; but from the “thick” world of reading, and writing, and participating in the work of “figuring it out”.

A number of people, myself included, have been fascinated by the work of Chris Arnade, who simply walks around and takes photographs and in a very disordered and jumbled and sincere way tries to make sense of a world that most scholars have no understanding of. You can tell from the titles of his articles, the most popular of which was ‘Why Trump voters are not “complete idiots”’, that Chris doesn’t claim to be doing what he does as scholarship, or a contribution to the peer reviewed literature. But he has more than paid his dues by simply giving his readers—those who do not know the social worlds he’s spending time in—the sensation of strangeness, the sense that, as obvious as it seems from the laptop, the answers to “what’s the matter with Kansas” are not

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13 Although I’ve seen it happen—Bayesian statistics has its subtleties. They just don’t come up that often.
14 It is certainly the case that sociologists and political scientists talk about “building on” the models of others. But the sense in which they do this is very different from the physicist, and despite the (humble) claim that scholars sometimes make to be taking the next, logically ordained step from the giants they stand on, what they are really doing is enlarging, or contracting, the model by force of intuition and will.
trivial, obvious, or staring you in the face.

Another person, very different from Chris, is Zeynep Tufekci, a professor at UNC but also a journalist for the New York Times. Zeynep writes, and tweets, both as a scholar of technology and someone who has experienced and seen the Erdogan’s right-wing authoritarianism in Turkey. What Zeynep has to say is, in part, that what you think, or what it is comforting to think, is wrong. That simple, primarily economic explanations, relying on eyeballed statistics, do not capture the logic of this new rise in authoritarian politics.

And then there is Matthew Stoller, who over the years, in and out of the policy world, has been writing articles and collecting material for books. Matt is not a scholar in the academy, and he has different goals and methods from the person writing a tenure book. But, like a scholar in the academy, he sees what you can see only when you see and look at something, like a historian—his undergraduate training, which we did together—for a very long time. For the last twenty years, I have watched Matt put together, and take apart, theories and explanations of political behavior. It is the work of humanists and scholars, as well as people like Chris and Matt that I think about when I read Isaiah Berlin’s essay “The Concept of Scientific History”. There, Berlin provides one of the few accounts that connects—and usefully contrasts—the work of the scientist and the historian.

Capacity for understanding people’s characters, knowledge of ways in which they are likely to react to one another, ability to ‘enter into’ their motives, their principles, the movement of their thoughts and feelings (and this applies no less to the behaviour of masses or to the growth of cultures)—these are the talents that are indispensable to historians, but not (or not to such a degree) to natural scientists.\(^\text{15}\)

John Von Neumann many not have needed them to invent game theory, but they are the capacities that allowed Alexis de Tocqueville to diagnose the disintegration of the elite during the French Revolution. What Berlin does not go on to say, but what is at this moment completely clear, is that these kinds of capacities, and the work that people endowed with them tend to do, is essential to progress in the sciences.

If those of us who study the human world computationally and mathematically can come together—from time to time, as part of our day’s work—to share and pool our models and accounts, we will succeed only if we pay attention to what’s beyond. There are other ways to understand the world, and they are not simply the places where science is not, or cannot go. They are, in fact, essential for its growth and flourishing.

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\(^{15}\)The Concept of Scientific History, first appearing in History and Theory I (1960): [http://berlin.wolf.ox.ac.uk/published_works/cc/scihist.pdf](http://berlin.wolf.ox.ac.uk/published_works/cc/scihist.pdf)