

# Common Errors in Interpreting the Ideas of *The Black Swan* and Associated Papers

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**Summary of the problem discussed in *The Black Swan* (and associated papers):** The problem, simply stated (which I have had to repeat continuously) is about the degradation of knowledge when it comes to rare events ("tail events"), with serious consequences in some domains I call "Extremistan" (where these events play a large role, manifested by the disproportionate role of one single observation, event, or element, in the aggregate properties). I hold that this is a severe and consequential statistical and epistemological problem as we cannot assess the degree of knowledge that allows us to gauge the severity of the estimation errors. Alas, nobody has examined this problem in the history of thought, let alone try to start classifying decision-making and robustness under various types of ignorance and the setting of boundaries of statistical and empirical knowledge.

Furthermore, to be more aggressive, while limits like those attributed to Gödel bear massive philosophical consequences, but we can't do much about them, I believe that the limits to empirical and statistical knowledge I have shown have both practical (if not vital) importance and we can do a lot with them in terms of solutions, with the "fourth quadrant approach", by ranking decisions based on the severity of the potential estimation error of the pair probability times consequence (Taleb, 2009; Makridakis and Taleb, 2009; Blyth, 2010, this issue).

**A more compact summary:** *theories fail most in the tails; some domains are more vulnerable to tail events.*

## FOOLED BY THE PACKAGING

As some of the commentators here should probably realize, *The Black Swan* (Taleb, 2007) is *not* the popularization and repackaging of ideas formulated elsewhere, but the expression of original thought in a very idiosyncratic format --by a member of society who feels entirely free to express his thought in any format he likes, as aggressively as he wishes, and to satisfy whatever sense of aesthetics he feels entitled to have (which in a way is what *academic* freedom is all about).

Given that I am not seeking their paycheck and social company, I do not feel that I owe some sub-academic tradition any compliance with their etiquette and arbitrage-prone metrics; rather I feel obligated to comply with the substance of what academic research should be about.

Alas, the deeper points of *The Black Swan* that were exiled in the compressed technical material in the Notes section in the back of the volume seem to have been chronically ignored, which has pushed me to rephrase them in a more traditional form in a dozen of scholarly papers, in a variety of disciplines. So, while I thank the contributors for their time and interest, I will try to do something more useful than provide a tit-for-tat direct answer to some of the "criticism" by Callahan and Jervis<sup>1</sup>, largely because, with due respect, I do not find their condescending outbursts interesting enough to be worth commenting upon, and also because the papers by Blyth and Runde do so in no small part. Statements like "we political scientists know all of this Black Swan" business rings as hollows as the "we economists know this very well" particularly in front of the economic crisis that started in 2008 --much on that point, later. They just don't see my problem of degradation of knowledge in proportion to the decrease of its probabilities. The only irrefutable statement I've seen in the criticism is that the work is "popular", reflecting an interesting elementary error in reasoning in holding that popular implies non-scholarly: if all non-rigorous, non-scholarly writings aim to be popular, all scholarly writings do not have to be non-popular. Instead I will proceed to providing a correction of common general mistakes made in the interpretation of the ideas of the book for future readers to not fall in the trap.

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<sup>1</sup> One irony: Robert Jervis condescendingly terms the discussion of cognition in *The Black Swan* "adequate if a bit spotty and dated", failing to see that his knowledge of the subject is missing a central element --before my own collaboration with Dan Goldstein (triggered by reactions to my lecture at the 2006 meeting of the Society of Judgment and Decision Making in Toronto), there was *no* research on the perception of the statistical differences in domains (scalable and nonscalable) and the separation of intuitions with respect to fat tails.

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A brief prior discussion of the source of the mistake, linked to the practice of labeling, is worth discussing here. Unlike Blyth and Runde (this volume), some reviewers are severely deceived by the packaging of *The Black Swan*. This leads to two classes of mistakes and distortions in the message of the book:

1) **The scanning mistake:** Many readers who have too much baggage (or perhaps not enough) tend, when reading for "work" or for the purpose of establishing their status (say, to write a review), rather than to satisfy a genuine curiosity, to read quickly and efficiently, scanning jargon terms and rapidly making associations with some pre-packaged ideas. This results in the squeezing of the ideas exposed in *The Black Swan* into some commoditized well know framework, as if I partook of the *off-the-shelf-re-warmed-ideas-with-some-new-wrinkle* traditions that give a bad name to academia, or as if my positions could be squeezed into: standard skepticism, empiricism, essentialism, pragmatism, Popperian falsificationism, Knightian uncertainty, behavioral economics, chaos theory, etc. Alas, while connected to it, no, *The Black Swan* is not quite Hume's problem of induction (which, incidentally, is not Hume's); it is not at all John Stuart Mill's "black swan" (which of course is not Mill's -when not capitalized, Mill's "black swan" is a logical, not a statistical or applied epistemological problem); it is certainly not Popper's falsification (the value of negative evidence was more intelligently dealt with by medical empiricists, the likes of Menodotus of Nicomedia, and their later French followers like Victor Brochard; the confirmation bias is well studied in the decision science traditions with no Popper involved); it is not about the dichotomy risk and uncertainty mostly formulated by Laplace in his *Traité philosophique des probabilités*, but generally attributed to Frank Knight (my central problem of the incomputability of small probabilities, on which later, applies to both categories commonly known as computable and incomputable types of randomness); it has something, but not a lot, to do with Austrian Economics (partly because of some of the Austrians' belief in fundamental uncertainty and some compatibility with complexity theory<sup>2</sup>); it is not about the promotion of power laws in the computational sense (I used it as a technical tool and used chaos theory to show the *a posteriori* incomputability of rare events and the fragility of nonlinear modelisation when it comes to tail events and to show that the Mandelbrotian fractals is vastly superior a way of thinking to other methods, in order to probe the limits of statistics, but not a replacement to bankrupt statistical methods which may have no replacement).

Concerning the "Popperian" label, a mistake I might have caused by not being clear enough (or having left the juice to the footnotes), the Popper I advocate the most is the far more unique and original author of the

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<sup>2</sup> Rosser, 2009.

arguments on the built-in limits of forecasting, in his *Poverty of Historicism*, the philosophical version of what statisticians know as *the law of iterated expectations*, not the Popper of the theory of falsification.

2) **The more severe delegitimizing and harmful labeling**, which enters the category deemed harmful by Jon Elster (Elster, 2009). More dangerously, some members of the thinking profession seem to be *only* interested in the packaging, not out of ignorance, but for protective reasons, leading to the outright denigration of ideas that come from outside their circles, regardless of their rigor, typically thanks to the obscurantist practice of labeling. This is very harmful when it comes to risk management --particularly when it stifles warnings without even looking at the data presented (what happened to this author before the crisis of 2008 as the message, as we will see was either ignored or transformed through labeling). It is very bad business for the institution of academia, as it continues to be discredited by players who get into a simple (but costly) parasitic game of publishing cosmetic material of no substance to satisfy résumé building, the equivalent of the *empty suits* we see in large corporations. Worse, this game can lead to the production of some works like those in derivatives research done by Robert Merton and Myron Scholes which increase risks in society and threaten the economic welfare of millions. Yet their work looks impeccable on paper --not following the letter, but rather the spirit of rigorous search for truth, allows me to deliver higher standards of work and help protect society against these errors.

#### **FIRST COMMON MISTAKE: IT IS NOT QUITE "ABOUT THE GAUSSIAN DISTRIBUTION"**

Members of the economics establishment, when addressing the message in *The Black Swan* have limited to criticism to claiming that they, for a long time, have been aware of the fat tails and have been dealing with them, by integrating their fat-tails into models. There are methods that have allegedly been used by Fama, Merton, Engel, Weitzman, and many more. This is both wrong (my argument, proved empirically, later, is that we cannot really work with fat tails because of the self-referential aspect of this inference), and, worse, not the point of *The Black Swan*. One relevant point for sociologists of science: the fact that I made the point clear in the most read statistical publication on the planet, *The American Statistician*, the journal of the American Statistical Association), as well as in scientific-intellectual forums such as Edge.org (with hundreds of thousands of downloads) did not help<sup>3</sup>. I will proceed to rephrase it here, hoping for repetition to have its effect.

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<sup>3</sup> For instance, in an interview, Kenneth French summarizes the standard position on fat tails: "Taleb is generally correct about the importance of outliers, but he gets carried away in his criticism of academic research. There are

The central idea in *The Black Swan* is that: rare events cannot be estimated from empirical observation **since they are rare**. We need an *a priori* model representation for that; the rarer the event, the higher the error in estimation from standard frequency sampling, hence the higher the dependence on an *a priori* representation that extrapolates into the tails. Further, we do not care about simple, raw probability (if an event happens or does not happen); we worry about consequences (the size of the event; how much total destruction of lives, wealth or other losses will come from it). Given that the less frequent the event, the more severe the consequence (just consider that the 100 year flood is more severe, and less frequent, than the 10 year flood), our estimation of the contribution of the rare event is going to be massively faulty (contribution is probability times effect; multiply that by estimation error) ; and nothing can remedy it.

So the rarer the event, the less we know about its role - -and the more we need to make up such deficiency with an extrapolative, generalizing theory. It will lack in rigor in proportion to claims about the rarity of the event. Hence model error is more consequential in the tails and *some representations are more fragile than others*.

I showed that, for technical reasons, this error is more severe in what I call Extremistan, where rare events are more consequential, because of a lack of scale, or a lack of asymptotic ceiling for the random variable. In Mediocristan, by comparison, the collective effect of regular events dominates and the exceptions are rather inconsequential --we know their effect and it is very mild as one can diversify thanks to the "law of large numbers". For instance Less than 0.25% of all the companies listed in the world represent around half the market capitalization, less than 0.2% of novels on the planet account for approximately half their sales, less than 0.1% of drugs generate a little more than half the pharmaceutical industry's sales – and less than 0.1% of risky events will cause at least half the damages losses.

Furthermore, such point has been proven empirically in Taleb (2009) where I showed, with 20 million pieces of socioeconomic data, that there is no stability in the

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lots of academics who are well aware of this issue and consider it seriously when doing empirical research. Those of us who used Gene's textbook in our first finance course have been concerned with this fat-tail problem our whole careers. Most of the empirical studies in finance use simple and robust techniques that do not make precise distributional assumptions, and Gene can take much of the credit for this as well, whether through his feedback in seminars, suggestions on written work, comments in referee reports, or the advice he has given his many Ph.D. students over the years." Source: [www.dimension.com](http://www.dimension.com).

The problem not understood by finance researchers is that robustness does not come from choice of distribution, but choice of payoff --the point on which the notion of fourth quadrant is based (Blyth 2010, this issue).

estimation of probabilities of tail events from within the data (in other words, the data does not predict out of sample), which made me question the Sveriges Riksbank Prize in Economic Sciences in Memory of Alfred Nobel given to Robert Engle in 2003 for his work on fattening the tails (as well as the five other prizes for people whose work depends on the assumption of thin-tails).

The Gaussian problem is larger than the Gaussian distribution: it is just the most common Platonic representation. I showed in Taleb (2009) how all measures using squares like "variance", "linear regression", "standard deviation", and other parts of the toolkit fail in Extremistan --they are just empty measures.

Let me refine the points of *The Black Swan*. The passage from theory to the real world presents two distinct difficulties: "inverse problems" and "pre-asymptotics".

**Inverse Problems:** It is the greatest difficulty one can encounter in deriving properties from empirical observations. In real life we do not observe probability distributions. We just observe events. So we do not know the statistical properties—until, of course, after the fact. Given a set of observations, plenty of statistical distributions can correspond to the exact same realizations—each would extrapolate differently outside the set of events on which it was derived. The inverse problem is more acute when more theories, more distributions can fit a set a data –particularly in the presence of nonlinearities or nonparsimonious distributions<sup>4</sup>.

+ *The small sample properties of rare events* as these will be naturally rare in a past sample. It is also acute in the presence of nonlinearities as the families of possible models/parametrization explode in numbers.

+ *The survivorship bias effect of high impact rare events*, which I called the Casanova problem in *The Black Swan*. For negatively skewed generators (i.e., with a thicker left tail, in other words tends to produce catastrophes), the problem is worse. Clearly, catastrophic events will be necessarily absent from the data –since the survivorship of the variable itself will depend on such effect. Thus such distributions will let the observer become prone to the overestimation of the stability and underestimation of the potential volatility and risk.

**APlatonicity (Preasymptotics).** Theories are, of course, a bad thing to start with, but they can be worse in some situations when they were derived in idealized situations, the asymptote, but are used outside the

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<sup>4</sup> A Gaussian distribution is parsimonious (with only two parameters to fit). But the problem of adding layers of possible jumps, each with a different probabilities opens up endless possibilities of combinations of parameters.

asymptote (its limit, say infinity or the infinitesimal). Mandelbrot and Taleb (2009) shows how some asymptotic properties do work well preasymptotically (Mediocristan), which is why casinos do well, but others do not, particularly when it comes to Extremistan.

Most statistical education is based on these asymptotic, Platonic properties—yet we live in the real world that rarely resembles the asymptote. Furthermore, this compounds what I call the *ludic fallacy*: most of what students of statistics do is assume a structure similar to the closed ones of games, typically with *a priori* known probability. Yet the problem we have is not so much making computations once you know the probabilities, but finding the true distribution. Much of knowledge problems come from this tension between *a priori* and *a posteriori*.

**SECOND COMMON MISTAKE: THERE IS NO SUCH THING AS AN HISTORICAL "EVENT" IN THE PROBABILISTIC SENSE**

The very definition of an "Historical" event lacks a dimension in Extremistan, particularly when we are talking about probabilistic decision-making (or decision making in the rational choice tradition). In fat-tailed domains, there is no such thing as a *typical* event. In other words, traditional tools lack a significant dimension, much like someone living in 3D space, but only seeing it in 2D. The missing dimension here is the severity of the event.

This absence of "typical" event in Extremistan is what makes prediction markets ludicrous, as they make events look binary. Describing an event as "a war" for decision making or analysis purposes is meaningless: you need to estimate its damage—and no damage is typical. Many predicted that the First War would occur—but nobody predicted its magnitude. One of the reasons orthodox economics has failed us in the crisis of 2008 (and many before it) is the sad fact that the literature is almost completely blind to the point. One of the problems historians should face is that an event is not typical in fat tailed domains. It may be just a simplification -- attributes of an event may not

In Mediocristan, there is such a thing as an event: a car accident, a heart attack, a bankruptcy (for a company), a disease (for a person, not an epidemic). Hence when we discuss them we know what we are talking about; we know whether or not we can deal with them, predict them, mitigate their effect. Getting overweight, getting old is well defined as we know what an overweight person looks like, say between 30 and 60 lbs of excess weight, or an old person, say between 80 and 100, etc. (More technically, Taleb, 2009, discusses the convergence in thin tailed domains of an expectation, conditional on exceeding a threshold, to that threshold,

with the opposite lack of convergence in fat-tailed domain).

So in fat tailed domains, discussing, predicting, hedging an event are not well defined operations, as these events can take a large scale. Take a war. It can kill a dozen people, or lead to millions of casualties. You may be able to predict the occurrence of a war, but you will not be able to gauge its effect! Conditional on a war killing more than 5 million people, it should kill around 10 (or more). Conditional on it killing more than 500 million, it would kill a billion (or more, we don't know). You may correctly predict a skilled person getting "rich", but he can make a million, ten million, a billion, ten billion—there is no typical number. We have data, for instance, for predictions of drug sales, conditional on getting things right. Sales estimates are totally uncorrelated to actual sales—some drugs that were correctly predicted to be successful may have been underestimated by 22 times.

What causes these severe mistakes is that outside the special cases of casinos and lotteries, you almost never face a single probability with a single (and known) payoff. You may face, say, a 5-percent probability of an earthquake of magnitude 3 or higher, a 2-percent probability of one of magnitude 4 or higher, and so forth. The same with wars: You have a risk of different levels of damage, each with a different probability. "What is the probability of war?" is a meaningless question for risk assessment.

So it is wrong to look just at a single probability of a single event in cases of richer possibilities (like focusing on such questions as "What is the probability of losing a million dollars?" while ignoring that, conditional on losing more than a million dollars, you may have an expected loss of \$20 million, \$100 million, or just \$1 million). Once again, real life is not a casino with simple bets. This is the error that helps the banking system go bust with an astonishing regularity. I've shown that institutions that are exposed to negative Black Swans—such as banks and some classes of insurance ventures—have almost never been profitable over long periods. The problem of the illustrative current economic mess is not so much that the "quants" and other pseudo-experts in bank risk-management were wrong about the probabilities (they were) but that they were severely wrong about the different layers of depth of potential negative outcomes. For instance, Morgan Stanley lost about \$10 billion on a certain business linked to subprime bonds, while allegedly having foreseen a subprime crisis and executed hedges against it; they just did not realize how deep it would go and had open exposure to the big tail risks. This is routine. A person who went bust during the crash of 1987 told the author, "I was betting that it would happen, but I did not know it would go that far." In that sense, saying "he predicted the crisis" is meaningless since there is no such thing as a crisis: it can lead to losses of billions, or

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losses of trillions. We need to attach a magnitude to a probability, which makes the probability irrelevant.

Accordingly, Ferguson's methodology (Ferguson, 2006) in looking at the prediction of events as expressed in the price of war bonds is sounder than simply counting predictions --because a bond is priced to cover the probability of an event times its consequences, not just the probability of an event. So we should not focus on whether someone "predicted" an event without his statement having consequences attached to it.

Associated to the previous fallacy, there is the mistake of thinking that the message is that these Black Swans are necessarily more probable than assumed by conventional methods. They are mostly less probable. Consider that, in a winner-take-all environment, such as the arts, the odds of success are low, since there are fewer successful people, but the payoff is disproportionately high. So, in a fat-tailed environment, rare events can be less frequent (their probability is lower), but they are so effective that their contribution to the total pie is more substantial.

The point is mathematically simple, but does not register easily. I've enjoyed giving graduate students in mathematics the following quiz (to be answered intuitively, on the spot). In a Gaussian world, the probability of exceeding one standard deviation is around 16 percent. What are the odds of exceeding it under a distribution of fatter tails (with same mean and variance)? The right answer: lower, not higher—the number of deviations drops, but the few that take place matter more. It was puzzling to see that most of the graduate students got it wrong.

### **THIRD COMMON MISTAKE: THE BLACK SWAN, FOLK PSYCHOLOGY, AND THE PHILOSOPHY OF PROBABILITY**

Many critics have casually questioned whether a particular event, say the Great War, or the September 11, 2001 attack on the World Trade Center, were Black Swans, on grounds that *some* have predicted them. Of course the Sept 11 attack was a Black Swan to those victims who died in it, otherwise they would not have exposed themselves to it. But it was not a Black Swan to the terrorists who planned and carried the attack. The same applies to the crisis of 2008, certainly a Black Swan to almost all economists on this planet, but certainly not to this author who does not think much of the risk management methods used by the economics establishment, as they induce fragility, hence house-of-cards collapses of banks that depend on these methods. (Incidentally, an illustration of the second common mistake discussed earlier, almost all of those who seemed to have "predicted" the event did not predict its depth).

This raises a problem that is worth probing in some depth. The fact that many researchers do not realize

immediately that the Black Swan corresponds mainly to incomplete mapping, or, like Runde (2010, this volume), have to go out of their way to stress the subjective aspect of it, takes us to the historical problem in the definition of probability. Historically, there have been many approaches to the philosophy of probability. It took a while for scientific researchers to accept the notion of subjective probability as formulated by Ramsey (1925) and de Finetti (1937) -- probability as degree of belief, subjective to the observer, expressing it rationally as he wishes under constraints of consistency in decision making (avoiding the violation of "the Dutch book" constraint).

Assuming that "randomness" is not epistemic and subjective, or even distinguishing between "true randomness" and "epistemic randomness" implies that an observer can reach omniscience and perfect knowledge of what is to know and has abilities to compute odds without violating consistency rules -- what is left becomes "randomness", or something by another name that arises from aleatory forces that cannot be reduced by knowledge and analysis. But there is an angle worth exploring: why on earth do we default to the assumption that events are shared by people in the same manner? Why did we ever take notions of "objective" probability seriously? Is it a misunderstanding of the *inverse problem*? In the problems of theory of mind, an underdevelopment of such human faculty to impart a knowledge to others that is different from ours, based on the non-sharing of all information and the partial informational opacity is a pathology that, at its limit, becomes autism (Baron-Cohen et al., 1985). Do we have mild autistic tendencies when we discuss probabilities?

### **FOURTH COMMON MISTAKE: THE PROBLEM OF INDUCTION, CAUSATION, AND COMPLEXITY**

I will simplify here with a functional definition of complexity --among many more complete ones. A complex domain is characterized by the following: 1) There is a great degree of interdependence between its elements, both temporal (i.e., a variable depends on its past changes), horizontal (variables depend on each other), or diagonal (variable A depends on the past history of variable B); 2) Mechanisms are subjected to positive, reinforcing feedback loops resulting from such interdependence, which cause "fat-tails" (i.e., prevents the workings of the Central Limit Theorem that establishes thin-tails under aggregation of elements; in lay terms moves are exacerbated with time instead of being dampened by counterbalancing forces); 3) We have nonlinearities that accentuate the fat tails. So complexity implies Extremistan (the opposite is not true). As a researcher, I focused only on the Extremistan element, ignoring the others except as a backup for my considerations of unpredictability. But complexity has other consequences on the conventional analyses.

A word here about the "induction" problem --it is one step beyond archaic in a modern environment. Simply, in a complex domain, there is no longer such thing as induction v/s deduction (except for a limited sub-set of variables, even then); the entire Aristotelian distinction misses a serious dimension (similar to the one discussed earlier with the a-typicality of events in Extremistan). Even other notions such as "cause" take on a different meaning, particularly in the presence of circular causality. The probabilistic equivalent is the move from a conventional random walk model (with a random variable moving in a fixed terrain, and does not interact with other variables around it) and percolation models where the terrain itself is stochastic, with different variables acting on each other.

Illusions of Causality has been best explained by network theorist Duncan Watts in Watts (2002):

Although they are generated by quite different mechanisms, cascades in social and economic systems are similar to cascading failures in physical infrastructure networks and complex organizations in that initial failures increase the likelihood of subsequent failures, leading to eventual outcomes that, like the August 10, 1996, cascading failure in the western United States power transmission grid, are extremely difficult to predict, even when the properties of the individual components are well understood.

## CONCLUSION

One reminder of the agenda of *The Black Swan* before the reader re-forgets it: rationalism crashes in the tails. Based on that, what should history be?

In the light of the problems presented earlier (lack of definition of an event, difficulty in deriving properties of the generator in some domains, causal opacity in complex domains, weakness of the very notion of *cause*, and, above all, theories fail in the tail), it seems appropriate to revive the ancient notion of *historia*, the recording of *facts without causation*, describing *how* things are, without going overly into the *why*, as practiced in the long tradition of medical empiricists who disappeared and gave way to Galenic and, later, Arabic, rationalism. *Historia* is accounting for facts without causation, nondemonstrative knowledge, a tool of pure empiricism that resists universals. Clearly, it is impossible to resist universals; it is impossible to operate without some element of theory. Even Galen used it as his lame argument against the skeptical empirical and methodist doctors "logos is needed for observation ... observation is impossible without logos". But arguing in favor of rationalism because one cannot observe without a theory misses the point entirely. Empiricism is not about *not* having theories, beliefs, and causes and effects: it is about avoiding to be a sucker, having a decided and preset bias on where you

want your error to be --where the default is. An empiricist defaults to suspension of belief (hence the link between empiricism and the older skeptical Pyrrhonian tradition), while others prefer to default to a characterization or a theory. Accordingly, the entire idea is to avoid the *confirmation bias* (empiricists prefer the disconfirmation/falsification bias, which they discovered more than a millennium before Popper).

*Epilogism* used by the medical empiricist was not inductive or causal, just simple reporting of the history of the process in a raw form (it meant *sensata cognitio*, the report of sense perception). In a way that is identical to what a modern statistician would call nonparametric or "robust", escaping extrapolating distributions, or a physicist would call "phenomenological" presentation, voluntarily limited to the descriptive account of regularities observed.

But I can understand how, as Pomata and Siraisi (2005) explain, how the Scholastics treated it as an inferior form of knowledge, lower in the ranking to *philosophia* or *scientia*. Giving an ancillary status to a field that just records facts is causing the rants I usually hear on the part of historians when discussing their overcausative theorizing.

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