

TOUCHSTONES OF HISTORY:

Anscombe, Hume, and Julius Caesar

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Skeptical worries aside, we seem to know a good deal about history. But how do we know what we know about history? How do we know that Julius Caesar was assassinated? At first blush, the answer seems obvious: most of us know whatever we do about history because we read it in history books. This, no doubt, is how we acquired our historical beliefs. But the question is not: why do we *believe* that Julius Caesar was assassinated? It is: how do we *know* that he was? Or if 'know' is too strong, at least: what makes it reasonable to believe that he was? At issue are the epistemological underpinnings of the discipline of history. In a searching paper called 'Hume and Julius Caesar', Elizabeth Anscombe argues that recorded history has a more intricate structure than we might suppose.¹ She contends that the discipline of history is grounded in statements about the past that can be neither supported nor undermined by historical investigation. Such statements serve as touchstones. They constitute the standards against which other historical claims are judged. Anscombe does not use the term 'knowledge'. She may in fact think that our most fundamental historical commitments are not knowledge at all. Her project in this paper might better be put as delineating the epistemological stratigraphy of historical understanding.

How does an inscription in a present day history book afford epistemic access to the historical event it reports? A seemingly straightforward empiricist answer is inferential. We reason back along the chain of record from the current inscription to the event in question. Such an answer is found in Hume's *Treatise*, the initial focus of Anscombe's discussion. Hume maintains that any well founded belief about matters falling outside our experience or memory must be connected to our present experience and memory by a causal chain that affords epistemic access to those matters. He invites us

To chuse any point of history, and consider for what reason we either believe or reject it. Thus we believe that Caesar was kill'd . . . on the *ides* of *March*; and that because this fact is established by the unanimous testimony of historians . . . Here are certain characters and letters . . . the signs of certain ideas; and these were either in the minds of such as were immediately present at that action; or they were deriv'd from . . . testimony . . . and that again from another testimony . . . 'til we arrive at . . . eyewitnesses and spectators of the event. 'Tis obvious all this chain of argument or connection of causes and effects is at first founded on those characters or letters, which are seen or remembered.²

1 G. E. M. Anscombe. 'Hume and Julius Caesar,' in *The Collected Philosophical Papers of G.E.M. Anscombe, volume 1: From Parmenides to Wittgenstein*. Minneapolis: University of Minnesota Press, 1981, pp. 86-93. Page numbers in parentheses refer to this work.

2 David Hume *A Treatise on Human Nature*, L. A. Selby-Bigge, ed. Oxford: Clarendon Press, 1928, pp. 82-3. Hume gives a more textured account of testimony in 'Of Miracles' in his

The inference is evidently an inference to the best explanation: We read a statement in a historical text, reporting that Julius Caesar was assassinated. We consider why the history book says such a thing. The best explanation of the current text's including this information is that the historian had it from a reliable source -- either an eyewitness, or another historian who had it from a reliable source, who was either an eyewitness or another historian who had it from a reliable source . . . and so on, until the chain terminates in an eyewitness report. What grounds our belief that Julius Caesar was assassinated then is faith in the historical record -- confidence that it is a chain linking reliable sources and terminating in an eyewitness report.

Two points about this account are worth noting: First it is linear. For each individual historical belief, we are supposed to have confidence in a chain of record linking that belief to the event it reports. Second, it is egalitarian. All historical statements are supposed to be supported in the same way. Let us call these the linearity and egalitarian assumptions.

On the face of it, Hume's account seems plausible. We disbelieve a historical claim if we consider the relevant historical record corrupt or the sources unreliable. We suspend judgment if we think that the historical record might be corrupt or unreliable. And we are, no doubt, wrong to believe a historical claim if there is no suitable chain of record. Still, Anscombe has a telling objection to the Humean picture. *That's not how we do it* (88). It is not the case, she contends, that we believe that Julius Caesar was assassinated because we trust the chain of reliable histories that links us to the event. Rather, we consider a historical chain reliable precisely because it gets the facts about Caesar right. Being right about Caesar is a touchstone against which we measure histories of Rome. That is because we know that Caesar was assassinated better than we know anything about the intermediate links in the chain of record.

She is right. No one thinks, 'Well, Johnson is a responsible historian. He would not have said it, if he did not have a good source. And he would only consider a good source someone who in turn had a good source, and so on. Therefore, I will take his word for it: Julius Caesar was assassinated.' It is simply not the case that we have more confidence in the reliability of historians than in facts like the fact that Julius Caesar was assassinated. As Anscombe puts it, "If the written records that we now see are grounds of our belief, they are first and foremost grounds for belief in Caesar's killing, belief that the assassination is a solid bit of history. Then our belief in that original event is a ground for belief in much of the intermediate transmission." (88)

Empiricism contends that perceptual deliverances (or impressions of sensation or sense data or what have you) are independently credible and provide the basis for all factual knowledge. This is why an empiricist account of history grounds acceptable historical statements in eyewitness reports. But such an account tacitly assumes that the authority of first personal perceptual deliverances carries over to third personal cases. This assumption is dubious. It is one thing for me to take my perceptual deliverances to be independently credible. It is quite another for me to take someone else's report of her perceptual deliverances to be independently credible. Whether or not the former is reasonable, the latter is not. For my informant may be untrustworthy. Anscombe's point is that we trust eyewitness reports not merely because they present themselves as such but because they accord with things we

Enquiry Concerning Human Understanding, ed. Eric Steinberg. Indianapolis: Hackett, 1977 .

antecedently accept. Rather than being independently credible, they require and sometimes receive corroboration.

Anscombe frames her discussion in terms of what we do, not what we should do, vis à vis historical claims. But she evidently considers history, as it is practiced, to be a reputable cognitive discipline. So her views, if correct, have consequences for epistemology. What we do in this case is pretty much what we should do.

Anscombe rejects the egalitarian assumption. She maintains that historical understanding consists of different sorts of beliefs with different sorts of grounds. She does not claim that we never use Humean reasoning. We might, I think, do so for arcane facts. According to Bertrand Russell, the Pythagoreans believed that it is wicked to eat beans.³ He cites Burnet as his source.⁴ I might have doubts about Russell's credentials as a historian, but I have confidence in Burnet. Having no other views that bear on Pythagorean dietary taboos, I accept the claim because I trust the source. But such arcane facts are unusual in that they are neither supported nor undermined by anything else we know. Even if the linearity assumption and the strong dependence on the chain of record hold for such isolated historical claims, it does not follow that they hold generally.

Much recorded history consists of what might be called ordinary historical facts. These are facts like the fact that Galen existed or the fact that the *De Rerum Natura* is authentic. Acceptance of these rests largely on considerations of coherence. The contention that Galen existed meshes with the rest of our understanding of Roman history and with our understanding of the historical record. When it comes to ordinary historical facts, it makes sense to ask: How do we know? Evidence can be adduced to support or to undermine statements of ordinary historical fact. One might think that apart from what I called arcane facts, history consists entirely of statements of ordinary historical fact. We believe that Galen existed because the hypothesis that he existed fits so well with the rest of our knowledge of Roman history. To deny his existence would not just leave a gaping hole, it would discredit many of our views about related matters. We could not, for example, be wrong about Galen and right about Marcus Aurelius.

Anscombe does not deny that mutual support of statements of ordinary historical fact is crucial to much of our historical understanding. She rejects the linearity assumption that each historical fact has a separate chain of record. But, she believes, mutual support is not enough. The case of King Arthur shows why. Bracketing the plainly mythic embellishments, our various views about King Arthur hang together fairly well. The story is gappy, but no more gappy than one would expect of a history of sixth century England. Moreover, we could not be wrong about King Arthur without being wrong about Guinevere. But we could easily be wrong about both. Whether King Arthur existed is a matter of dispute. Mutual support takes us only so far. For we can ask of a constellation of mutually supported claims, 'How do we know any of it?' To answer that requires connecting the constellation to something we know better. Historians are undecided about King Arthur because the coherent story is not sufficiently supported by things they have

³ Bertrand Russell. *The History of Western Philosophy*. New York: Simon and Schuster, 1945, p. 31.

⁴ John Burnet. *Early Greek Philosophy*. London: A & C Black, 1930.

better reason to believe. They are unanimous about Galen because the coherent story is grounded. "One can relate [Galen] to better known historical matters." (91)

A regress threatens. We relate the coherent account about Galen to better known historical facts. What grounds these better known facts? They too, presumably, constitute a coherent account. But if it is legitimate to ask 'How do we know any of it?' when confronted with coherent accounts about Galen, shouldn't it be legitimate to ask it about the account that grounds our knowledge of Galen? The answer has to be 'no'. Maybe we can take one or two steps along the regress, but we can't go on indefinitely demanding grounds. Unless the regress ends, the study of history is futile. At some point, the question 'How do we know any of it?' remains unanswered. If it needs to be answered, skepticism results. In fact, the regress terminates quickly. We know *a* better than we know *b*, and *b* better than we know *c*. But we soon arrive the point where we know nothing better. That, Anscombe believes, is the status of our knowledge of the death of Julius Caesar.

Facts that have this status I call touchstones. Anscombe mentions three such facts: Julius Caesar existed, Julius Caesar was assassinated, and the Latin of Horace, Ovid, Virgil, Cicero, and Caesar is authentic, classical Latin. Touchstones are not, according to Anscombe, entirely immune to doubt, but the only doubt to which they are susceptible is Cartesian doubt. We can perhaps wonder whether we know anything at all about history, but if we know anything about history, we know that Julius Caesar was assassinated.

This is surprising. Why should we think that doubts about Caesar's death plunge us into Cartesian doubt? That Julius Caesar was assassinated is not only a contingent fact, it is a fact about something that happened a long time ago. It seems to be the sort of thing we could be wrong about. That being so, it appears reasonable to ask, 'How can we be sure that we are not wrong about it?' This does not have the ring of a skeptical question. It sounds like the sort of question that any self-respecting discipline should be prepared to answer. The problem, Anscombe maintains, comes when we try to imagine how we would find out that we were wrong. What would show that? In the *Philosophische Bemerkungen*, Wittgenstein suggests that we might find "something written, from which it emerges that no such man ever lived, and his existence was made up for particular ends".⁵ But, Anscombe points out, the conviction that Julius Caesar really existed is so much stronger than our reasons for believing that the document is truthful, that the clash would discredit the document. Any evidence that seemed to call Caesar's existence into question would immediately be dismissed as misleading. Being so much more certain than any evidence that might be brought against them, touchstones thus are invulnerable to disconfirmation by historical evidence.

If they are invulnerable to disconfirmation by historical evidence, they are equally unsusceptible of evidential support. Again this seems surprising. Surely, one might think, historians could find new evidence that Julius Caesar existed. But the argument for disconfirmation holds for confirmation as well. If we discovered an ancient document which seemed to attest to the existence of Caesar, its content would be evidence that the document was authentic, not evidence that Caesar existed. The so-called evidence does not confirm the fact; its

⁵ Ludwig Wittgenstein, *Philosophische Bemerkungen*, Oxford: Blackwell, 1964, *IV*, §56, quoted in Anscombe, p. 89, where she calls it one of his rare pieces of stupidity.

accord with the fact underwrites its status as evidence.

The indifference to evidence suggests that we could not find out that Julius Caesar really existed. If there is no evidence, how could we possibly find out? Anscombe admits that some people could find out that Julius Caesar existed, but claims that *we* could not. (*We* presumably are adults who were educated in the west.) A child might believe that Shakespeare's play is a pure fiction. He could go to history books and find out that unlike King Lear, Julius Caesar really existed. A Chinese man, who has had little contact with the west, could, Anscombe says, "learn our languages, come to our countries, find out that the corpus of solid historical information belonging to our culture does include this." (91) The investigations of the child and the Chinese man would bring them to our level. They would learn the role of 'Julius Caesar existed' in our understanding of history. They would learn that it is a touchstone. This is something we already know, hence cannot find out. It is, Anscombe believes, an illusion to think that we are simply further along the investigatory trajectory that advances the child's and the Chinese man's knowledge of the matter. For us, the existence of Julius Caesar is settled. There is nothing left to find out, since it is, as she says, 'a solid bit of history'.

Anscombe's point concerns the current status of facts that serve as touchstones, not what made them acceptable in the first place. She does not deny that evidence was ever relevant to the acceptability of the claim that Julius Caesar existed. She does not deny that someone found it out. Eyewitness reports and primary source documents presumably once played a role. Her point is rather that how we or our predecessors came to believe that *p* does not determine the role that *p* now plays in our understanding. To determine that, she thinks, we need to ask what could discredit *p*. If the answer is 'Nothing short of Cartesian doubt,' then *p* is a touchstone.

Touchstones are not just very deep-seated convictions. They anchor our knowledge of history, serving as validatos other historical claims. In Wittgenstein's terms, they are the hinges⁶ on which the study of history turns. Because they are as certain as anything we know about history, they supply the standards against which we measure more dubious historical hypotheses. Anscombe illustrates how this is done. She says, "I was taught, I think, that when Leucetius was first published during the Renaissance, the *De Rerum Natura* was suspected of being a forgery; but its Latinity and the absence of 'giveaways' won its acceptance. This means that there were standards by which to judge. The ancient Latinity of Horace, Ovid, Virgil, Cicero and Caesar was such a standard." (90) To decide whether the *De Rerum Natura* is authentic then, we compare it to works whose authenticity is unquestioned. If the language -- including syntax, vocabulary, style and content -- is sufficiently similar to the language in the works whose authenticity is unquestioned, it is accepted as an authentic ancient Latin work. This seems a methodologically responsible way to proceed. But it raises the question of the status of works we measure by. To assuage doubts about the authenticity of the *De Rerum Natura*, we need a standard to judge it by. The Latin of Horace, Ovid, Virgil, Cicero and Caesar is the standard. Their being the standard, Anscombe says, is "known by tradition and never subject to question". (90) Presumably then, anyone who knows anything about authenticating classical Latin, knows that you test against the Latin of Horace, Ovid, et al.. This may be true. But the question remains: How do we know -- how does anyone know -- that the works attributed to these authors are authentic? To say that their authenticity *is* unquestioned is not to say that their authenticity

6 Ludwig Wittgenstein. *On Certainty*. New York: Harper Torchbooks, 1969, §341.

should not be questioned, much less that their authenticity *could not be* questioned. These are fine words. But, Anscombe pointedly asks, how would you mount the challenge? “To attempt to construct a serious doubt whether we have writings of Cicero -- how could it find a ground from which to proceed?” (90).

It might seem that we have an answer. If the Latin of Cicero, Ovid, et al. are touchstones, we might at least hope to compare them against each other. We could, of course, make such comparisons. But even the discovery of significant differences would not necessarily raise doubts about authenticity. Classicists would more likely conclude that ancient Latin was more varied or flexible than had previously been thought. Suppose this conclusion were somehow blocked. Then perhaps the question of authenticity would arise. But about the authenticity of which works? Do we doubt the authenticity of the Virgil’s *Annead*, or Cicero’s *Philippics*, or Caesar’s *Punic Wars*? Being touchstones, they are on a par. Since none is more firmly established than the others, none can serve as the standard for authenticating the others. If any comes under suspicion, all do. The doubt again turns out to be Cartesian. If we raise the question about the authenticity of the touchstone works, we have no resources for answering it. For our ultimate standards no longer hold.

Anscombe is not here denying that we could entertain Cartesian doubts about history. Her claim is that there are facts about history that we cannot call into question without thereby calling into question the entire corpus of historical knowledge and the entire methodology for establishing historical facts. We can do this if we like, but if we do, we are no longer doing history.

It is widely held that knowledge must be grounded in independently credible beliefs; that is, in beliefs whose credibility does not derive from their relation to other beliefs. Traditionally, epistemologists have held that some intrinsic feature of the beliefs in question or their relation to the knowing subject makes them independently credible. They are, as it might be, indubitable, or self-presenting, or clear and distinct. Such beliefs are held to wear their epistemological hearts on their sleeves. Anyone who considered the matter could tell whether a candidate belief was basic. Anscombe and her fellow Wittgensteinians also believe that knowledge rests on independently credible basic beliefs. But neither their intrinsic features nor their relation to the believer accounts for their status. The child might entertain the hypothesis that Julius Caesar existed, and dismiss it as a fiction. The Chinese man might believe it, but consider it on a par with ‘Galen existed’, an ordinary historical fact about ancient Rome. What makes ‘Julius Caesar existed’ basic, according to Anscombe, is its function in our knowledge of Roman history. As we come to understand ancient history, we realize that ‘Julius Caesar existed’ plays a role that ‘Galen existed’ does not. It serves as a touchstone against which to assess other putatively historical claims. Not only does it figure in the justification for more tenuous historical claims, it belongs to the framework within which we locate events. It is so deeply enmeshed with everything else we purport to know about the subject that to call it into question would undermine the entire enterprise. Not only does the study of history fail to provide reason to doubt it, its immunity to doubt underwrites historiography as we know it.

Although this discussion focuses on history, the argument generalizes. It applies to any area where information is second-hand. If Anscombe is right, not only history, but geography,

current events, science, and ordinary knowledge rest on touchstones which are neither in need of nor susceptible to justification within the disciplines that rely on them.

Philosophers are comfortable with the idea that the other disciplines rest on philosophical presuppositions. So the conclusion that history (or science or sociology or whatever) bottoms out in philosophical questions should not surprise or dismay us. What is unexpected is where Anscombe thinks the bottom lies. We tend to think that philosophy enters the picture when it comes to addressing sweeping questions about, for example, the reliability of methods or the status of broad categories -- perceptual knowledge, knowledge of the past, knowledge of the material world, or whatever. But the factors that Anscombe construes as touchstones are remarkably specific. If someone in a history class were to ask, 'How do we know that Julius Caesar was assassinated?' we would hardly expect the answer to be, 'That's a question for philosophy'.

The worrisome aspect of Anscombe's position is not that history (and by implication, the other disciplines) bottom out in philosophy, but that they bottom out in *Cartesian doubts*. The clear implication is that these are skeptical doubts that cannot be assuaged. She says, "The effect of the hypothesis [that a touchstone is false] is to make a vacuum in which there is nothing by which to judge anything else." (91) If such doubts really cannot be answered, they had better not be raised. The study of history is abortive unless the regress ends.

Inspection reveals that the regress does end. It terminates with facts like the fact that Julius Caesar was assassinated. At that point we take ourselves neither to have nor to need anything more to say. As Wittgenstein says, "If I have exhausted the justification, I have reached bedrock and my spade is turned. Then I am inclined to say 'This is simply what I do'."⁷ The discipline of history, Anscombe believes, depends on accepting the touchstones. It justifies everything else it accepts by reference to them. A contention *p* can be called into question only by considerations that are better known than *p*. And nothing is better known than a touchstone. Having no resources to dig deeper, history's spade is turned. This is simply what historians of ancient Rome do: They ground their accounts in the claim that Julius Caesar was assassinated.

Still, the niggling suspicion remains: Maybe Julius Caesar wasn't assassinated. Maybe he didn't even exist. It is not obvious that we can silence or discredit the worry, merely by construing it as skeptical.

Anscombe seems to be saying that if we want to understand history, we must take the touchstones on faith. Her language bears this out. I said earlier that she does not say that we *know that* Julius Caesar existed. Rather she says that we *believe in* the existence of Julius Caesar. The locution 'believe in' is much more at home in religious or supernatural contexts than in discussions about mundane facts. We do or do not believe in God, or in reincarnation, or in the divinity of Jesus or whatever. Our children do or do not believe in Santa Claus or in ghosts. Perhaps you can't be a theist if you do not believe in God. The existence of God is then a touchstone of theism. Anscombe seems to suggest that believing in the existence of Julius Caesar is to history as believing in God is to theism. If so, it seems that history is a more dubious cognitive enterprise than we are inclined to think it is. 'Take it on faith' is not what we

7 Ludwig Wittgenstein. *Philosophical Investigations*. Oxford: Blackwell, 1953, §217.

consider intellectually reputable advice. Moreover, even if you cannot be a theist if you question the existence of God, it is perfectly obvious that you *can* question the existence of God. To do so is not an exercise in Cartesian doubt. Reasons can be brought to bear. So if the cognitive structure of history is supposed to be like the cognitive structure of theism, the subject may bottom out in questions that the discipline lacks the resources to answer, but it does not follow that it bottoms out in questions that cannot be answered.

I am not convinced that touchstones are immune even to historical doubt. Let us look at the examples Anscombe mentions. Consider the following alternative to ‘Julius Caesar was assassinated’:

Suppose that while his political enemies were verbally assailing him, Julius Caesar suffered a fatal heart attack. Mark Antony and his followers decided to gain political advantage by putting it about that Caesar was murdered. Perhaps they lied about Caesar’s death. Perhaps they used the term ‘assassination’ metaphorically to label the verbal abuse that they believe led to Caesar’s fatal heart attack. To be sure, there were eyewitnesses. But non-partisan witnesses only saw the event from a distance, not from a vantage point which would enable them to distinguish between Caesar’s collapsing from a heart attack and collapsing from a blow. Like contemporary eyewitnesses, their reports were less reliable than we would like to believe. Be that as it may, people took Mark Antony’s words literally and believed what he said. The rest, as they say, is history.

I made the story up, but it is not utterly implausible. To seriously entertain it as an alternative to the received view we need not advert to malevolent demons, systematic deception, or brains in a vat. But, Anscombe might reasonably reply, the fact that someone can make up such a story does not mean that history has any reason to take it seriously. If history had to discredit every undefeated potential defeater, it could not get started. In order to be worth taking seriously, such a story would have to be backed by evidence or other cogent historical reasons. So the question is, could there be evidence or reasons favoring my story? Anscombe believes not. Even if we found a document attesting to the truth of my account, it could easily be false or misleading. It might be spurious, or it might be political chicanery. But we would hardly take it to refute the claim that Julius Caesar was assassinated. The question, Anscombe insists is: “What would get judged by what?” (89) She considers it obvious that such a document would be judged against the touchstone, and found wanting. Granted, even in the best of circumstances, it is not easy to determine which politicians or pundits are lying (or about what). So the possibility that the document is spurious, false, or misleading is real. But it is not as obvious as Anscombe thinks that the document should be summarily dismissed. Perhaps a lone document would suffer the fate Anscombe describes. But if several seemingly authentic documents with evidently divergent provenances were found, the situation would, I suspect, be different. They might not discredit the claim that Julius Caesar was (literally) assassinated, but they would be likely to give historians pause. They might prompt historians to reassess the evidence -- to look more closely at the primary source documents for evidence of political chicanery or evidence that the term ‘assassination’ was used metaphorically. Such a reassessment could even occur in the absence of new documentary evidence specifically about Caesar. Latinists working on other documents might find reason to take certain locutions or texts, which once been considered literal, to be figurative. The reinterpretation of the evidence about Caesar’s death could simply be a consequence of revisions in interpretation that were justified by the sense they make of other

ancient works. I am not claiming that it would be easy to show that my account is true. My point is only that evidence could be brought to bear. There could be grounds for doubt that Julius Caesar was assassinated.

Could there be grounds for doubt that Julius Caesar existed? This is a bit harder to conceive, but I think it is still possible. I take it that 'Julius Caesar existed' is equivalent to:

$(\exists x) x \text{ is a person} \ \& \ x \text{ is Julius Caesar} \ \& \ [(y) y \text{ is Julius Caesar} \equiv y=x] \ \& \ x \text{ is dead.}$

One way this could be false is if the uniqueness condition is not satisfied. How could this be? Consider the case of 'Nicolas Bourbaki'.⁸ It looks like the name of a person, but actually it is a pseudonym for a group of French mathematicians who derived some important results in set theory. Perhaps one or another of them appears at mathematics conferences under the name of 'Nicolas Bourbaki' and presents a proof. Even then, the speaker has no more claim to be the real Nicolas Bourbaki than any of the other members of the group. Maybe our descendants will take the name 'Nicolas Bourbaki' to denote the unique person who generated the proofs that appear under the name. They will be wrong, for there is no such person. It is not inconceivable that we are in the same situation vis à vis the name 'Julius Caesar'. Perhaps the name is a pseudonym for a cabal who collectively ruled Rome. One or another member of the cabal may have appeared at political functions under the name of 'Julius Caesar', but the spokesman on any given occasion had no more claim to be the real Julius Caesar than any of the other members of the group. Granted this scenario is a bit harder to swallow than my previous effort, but given the example of Bourbaki, it does not seem an obviously skeptical alternative. Could there be evidence for it? I think there could. Historians might find other ancient cases where tight knit groups adopted the names of individual people. They might look back at the primary sources and find inconsistencies that can be resolved by the hypothesis that in different contexts different members of the cabal functioned as Julius Caesar. Perhaps, for example, the descriptions of his appearance are inconsistent -- some say he is tall, others say he is short. Perhaps the writing styles in the various works attributed to him differ. Perhaps various exploits that had to be dismissed as hyperbolic under the assumption that Julius Caesar was one person become plausible under the hypothesis that the name 'Julius Caesar' denotes the several members of a group. Again, my claim is a modest one. Evidence could be adduced that would undermine our conviction that Julius Caesar, a single human being, existed. The claim does not seem beyond the reach of evidence.

The final touchstone Anscombe mentions is the conviction that the works of Horace, Ovid, Virgil, Cicero, and Caesar are authentic classical Latin works. I suggested earlier that even if we found significant differences in the language used by the several authors, Anscombe would deny that we could non-skeptically question their authenticity. Since they are all on the same level, we wouldn't know which one to doubt. I am not convinced. One principle we might use is 'odd man out'. Suppose, for example, linguistic analysis reveals that the language of Horace diverges considerably from the language used by the other four, but those four diverge little from one another. This would focus doubts on the Horace. Suppose further that the Horace contained syntactical constructions that did not appear in other Latin documents until the 12th century. This would strongly suggest that Horace's works had been misdated. Anscombe might think that such

⁸ Thanks to Amelie Rorty for the example.

eventualities had already been excluded before the facts were elevated to the status of touchstone. But developments in linguistics, literary criticism, even computer analysis of literary texts reveal patterns in literary works that previous scholars overlooked. The conviction that the case is closed looks premature.

Anscombe's position rests on the view that we know some things better than others. Some epistemologists might bristle at the idea that bits of *knowledge* differ in strength, but it seems plain that well-founded convictions do. Even though we are convinced of all of them, we cling more tenaciously to some well-founded convictions than to others. Moreover, we think we are right to do so. We are not embarrassed about being more strongly convinced that Caesar was assassinated than that Galen was an ancient physician. This is all Anscombe needs. Such differences in strength are not idle. They provide a way to adjudicate conflicts. If all well-founded convictions were equally strong, we would have no reason to prefer one to another when they clash. Confronted with a bit of recalcitrant but *prima facie* credible evidence, we would be stumped. We could conclude that either Julius Caesar was assassinated or the newly discovered document is unreliable, but we would have no basis for preferring either disjunct over the other. But because we are much more strongly convinced that Julius Caesar was assassinated, we reject the documentary evidence as unreliable. As I've put it, this is a psychological claim about how we treat our various convictions, but it is more than that. Historiography requires that different weights be assigned to different beliefs about the past. The strength of our convictions reflects the weights assigned. If we didn't rely on the strength of our convictions to adjudicate conflicts, Anscombe believes, we couldn't do history. This may well be true. But it doesn't show that the procedure is reasonable.

Underlying Anscombe's position is the widely held assumption that the weaker conviction cannot override the stronger. This is why she thinks we can have no evidence against the thesis that Julius Caesar was assassinated. Being so much more strongly convinced that Julius Caesar was assassinated, if we were confronted with a document alleging the contrary, we would simply reject the document. She treats conflicts among beliefs like an elimination tournament. Serena Williams plays Maria Sharapova and beats her; Sharapova goes away. Serena Williams then plays Na Li, and beats her; Li too disappears from the scene. She then takes on Tsvetana Pironkova and dispatches her as well. The rivals, being beaten, depart, never in this tournament to be heard from again. The strongest player prevails. But in cognitive clashes, rejected considerations do not, as a rule, obligingly disappear. To merit consideration in the first place, the weaker conviction -- the disputed document, for example -- must have something going for it. It seems authentic, perhaps because of the age of the parchment, the location where it was found, the style of the writing, and so on. It still has those properties. So the question arises: if it is not true, how are we to account for it? Maybe it is a forgery. Maybe it is an authentic Roman work, but a bit of propaganda. Maybe it is wishful thinking on the part of a Roman author, horrified at the idea of political assassination. Each such hypothesis requires backing. We cannot be satisfied with a rejection grounded in a list of 'maybe's. Rejecting a *prima facie* plausible hypothesis has consequences. The reasons for which we originally accepted it, or at least took it seriously, do not just disappear.

Often we have no trouble accounting for the *prima facie* plausibility of the considerations we reject. Either we find evidence that the document is spurious, or find a way to account for the

existence of a document alleging something false. A local and limited revision in our belief system usually suffices. We are even willing to tolerate a few cases of what scientists are apt to call ‘undetected background noise’ -- that is, cases where we are confident there is some reason why the consideration is unfounded, but we can’t quite put our finger on it. Sometimes, however, things are not so straightforward. Revisions reverberate. Construing the rejected document as propaganda raises the issue of how we are to read other contemporaneous documents. Should they still all be taken at face value? The idea that there is just one bit of propaganda in an otherwise reliable collection of factual documents is implausible. So we need ask which other documents are untrustworthy and how to tell. Once we raise the issue that some of the primary source material might be propaganda, we may find reason to reconsider a good deal more of what we had previously accepted about ancient history. The rejected document may begin a cascade of revisions that eventually collectively undermine our confidence in a touchstone.

Let me mention a couple of illustrations. Since Anscombe gives no criteria for being a touchstone, it is hard to know whether one has an actual counterexample. But the cases I mention seem to be plausible candidates. In the 19th century, ancient historians were convinced that Troy was mythical. This conviction was, I suggest, a touchstone of history. One could not appeal to facts about Troy to ground other historical claims. Then Schliemann and Calvert discovered Troy. Perhaps initially historians could insist that the archeological site is not really Troy, that it is some other ancient city. But as the dig continued to reveal features described in *The Iliad* that defense weakened. Eventually it became implausible to thunder ‘What would get judged by what?’ and insist that we are more confident that Troy is mythical than that the newly discovered city is Troy. In consequence of the discovery, historians had to revise more than their view about the existence of Troy. Once they concede that the place really existed, they have to consider how much of the story of *The Iliad* has its basis in fact. Bracket talk about the gods or construe it as metaphor. What should they think about the human protagonists? They needed to look back at the evidence in hand, reinterpreting it in light of the fact that Troy is real. Is there evidence for the existence of Agamemnon, Achilles, Hector, Helen? They also had to rethink their convictions about other ancient literary texts. Should we still be confident that *The Oresteia* is pure fiction or the story of Oedipus? The answer is by no means clear.

Consider another case. Historians used to construe the medieval period as the Dark Ages, where learning was eclipsed. This was, I submit, a touchstone of history, a conviction that framed the interpretation of evidence about the period. But scholars became increasingly aware of developments in mathematics, natural science, art and philosophy, which could not be readily accommodated within this framework. The conviction that the period was an intellectual wasteland simply could not stand. The revisions not only changed our understanding of the Middle Ages, but forced a reconsideration of the Renaissance as well. If reason wasn’t dead, it did not need to be reborn. So how exactly did the Renaissance differ from the period that preceded it? Again this is a real historical question that admits of real historical investigation. It does not throw historians into skeptical panic. It sends them back to their task. It is, in history and elsewhere, possible to discover that we have been deeply wrong about something important without concluding that we do not know anything about the subject at hand or how to study it.

The excavation of Troy yields rock solid evidence that forces a reconsideration of the touchstone. No single datum has that effect in the reconsideration of the Middle Ages. Rather,

multiple bits of evidence, any one of which might be dismissed as inaccurate or unrepresentative of the period, collectively make the case. As the evidence mounts, it becomes increasingly implausible that the touchstone is correct. This suggests that we ought not be so sanguine about assuming that weaker considerations never override stronger ones.

Still, Anscombe's rejection of the egalitarian principle strikes me as correct. If all well-founded convictions were on a par, we would be at a loss when confronted with a clash. But one can reject egalitarianism without concluding that history must be grounded in unshakable commitments. I suggest rather that our convictions possess different degrees of epistemic inertia, a cognitively well founded staying power or propensity to resist revision or rejection. Physical inertia is a property of bodies which consists in their resistance to change in uniform motion. The greater a body's inertia, the harder it is to dislodge. When a slight force acts on a body with considerable inertia, the effect on that body is negligible. But a substantial force can alter such a body's course. So can a suitable constellation of individually weaker forces. Hence the analogy.

A commitment's epistemic inertia derives in part from how firmly we are convinced of it. More important is how central it is to our understanding of the subject. That depends on how much else we would have to revise or reject or reconsider if we were to give it up. Some beliefs have relatively low inertia. Although they are genuine beliefs, we would and should have few qualms about rejecting them should new evidence emerge. 'The Pythagoreans thought it was wicked to eat beans' is a belief of this kind. Others are more resistant to rejection. We could perhaps be convinced Richard III was not behind the murder of the princes in the Tower⁹, or that during the American Revolution Benjamin Franklin was a British spy, but it would take some doing. The sorts of considerations that function for Anscombe as touchstones have considerable inertia. It would take a lot to convince us that Julius Caesar was not assassinated. But, I think, we could be convinced.

The alternative I am offering is frankly Quinean.¹⁰ A commitment's inertia depends on its place in the web of belief. The more central the commitment, the more tightly woven into the fabric of our understanding, the harder it is to reject. But nothing is in principle immune to rejection, and only differences in degree separate the commitments at the periphery from those at the center. The principle of minimal mutilation favors preserving the commitments with the greatest inertia. But Quine, unlike Anscombe, recognizes that we can have epistemologically good reasons to repudiate even our most central claims. This is something Anscombe flatly denies. She says, "Not everything can be put up for checking. Neurath's image is of a ship which we repair -- and, I suppose, build on to -- while it is afloat: if this suggests that we can go round tapping every plank for rotteness, and so we might end up with a wholly different ship, the analogy is not good. For there are things that are on a level. A general epistemological reason for doubting one will be a reason for doubting all, and then none of them would have anything to test it by." (92) I disagree. Perhaps such massive revisions are not due to 'general epistemological reasons'; but local and parochial reasons mount up. As a Quinean holist I

⁹ See Josephine Tey. *The Daughter of Time*. London: Penguin, 1951, for an excellent argument to this effect.

¹⁰ W.V. Quine. 'Two Dogmas of Empiricism,' in *From a Logical Point of View*. New York: Harper Torchbooks, 1953, pp. 43-44.

believe that philosophy is continuous with the rest of inquiry, so there is no sharp line between historical, historiographical, and epistemological reasons. In any case, the question is not whether there can be general epistemological reasons to reject the touchstones, it is whether there can be epistemologically sound reasons of any sort.

Anscombe's rejection of Quinean holism is grounded in her belief that the touchstones are not just important facts, but standards for the acceptability of other claims. That being so, she thinks that they cannot be repudiated without destroying the enterprise they belong to. Some standards evidently have this character. The standard meter is supposed to be the only object that could not fail to be one meter long. Because it is the standard, however long it is, that is how long a meter is. It is thus the final authority on whether any other object is one meter long. Were we to entertain the possibility that *it* is not a meter long, we would lose our moorings. Not only would we have no basis for judging such a thing, we would lose our grip on what a meter is.¹¹

What enables it to be authoritative? First, it is unique. Because there is only one standard meter, (viz., the bar in Paris) and one standard for being a meter long (viz., being the same length as the bar in Paris), there is no possibility that verdicts yielded by different standards for the same magnitude could clash. Second, units of measure are established by stipulation. Prior to and independent of the requisite stipulation, there is no fact of the matter. The standard meter is authoritative then because it is constitutive. To be one meter long is neither more nor less than to satisfy the standard.

Not all standards have this character. Consider safety standards. No one thinks that for a drug to be safe just *is* for it to satisfy government safety standards. Such standards are not constitutive of the matters they bear on. In judging the safety of a drug, we sometimes think that although current standards have been met, the verdict might still be wrong. Safety standards are not authoritative, but indicative. Their satisfaction affords reason to believe, but no guarantee, that the item being assessed measures up. They tend therefore to be multiple. *Ceteris paribus*, having several indications that a drug is safe strikes us as better than having only one. Moreover, they are revisable. As we learn more about the dangers of steroids, for example, we refine our standards of safety for the drugs. Finally, indicative standards admit of assessment. We can test them against other indicative standards, against the ends we want them to serve, and against our 'intuitions' about the matters they are supposed to assess. Such tests are not of course conclusive. But they supply evidence for or against the continued acceptability of the standards in question.

Anscombe seems to think that the touchstones of history are authoritative standards. If so, nothing is a solid bit of ancient Roman history unless it properly accords with 'Julius Caesar existed' and the like. History is a factual discipline, so the touchstones clearly cannot be constitutive standards. Accord with the touchstones is not what makes a contention a historical fact. Might the touchstones be authoritative without being constitutive? That would require that accord with them be a necessary condition for being a solid bit of ancient Roman history. But it is sheer hubris to claim that a historical statement's failure to accord with a touchstone shows

¹¹ So, anyway, the story goes. In reality even this case is more complicated, since the metal bar expands and contracts with heat. But for the purposes of this discussion, we can accept the idea that the standard meter is a paradigmatic case of an authoritative standard.

conclusively that the event it reports did not occur. And lacking conclusive evidence that it did not occur, we would be unwise to peremptorily exclude it from consideration. Nor do we need to. For the touchstones can be construed as indicative of epistemic acceptability. In that case, accord with them is evidence or reason to believe that a contention is a solid bit of history. Failure to properly accord with them is ordinarily reason to reject a claim. So the touchstones have considerable inertia. But they can be dislodged.

On this account there is no sharp difference between touchstones and ordinary historical facts. There are just different degrees of inertia. No commitment is invulnerable to criticism, revision or rejection, but some are more vulnerable than others. Reasons for questioning a touchstone need neither derive from nor lead to skeptical doubts. They may be generated within the discipline or elsewhere. Psychological evidence about the limited reliability of eyewitness reports or the selectivity of memory could engender a reassessment of previously accepted historical claims. Advances in linguistics or literary analysis could prompt a reinterpretation of historical documents. Discoveries in materials science could provoke a reconsideration of the nature, date, or use of artifacts. Recognition of the reliability of previously discredited sources or the availability of previously ignored sources could call the touchstones into doubt. Anscombe thinks we can't go around tapping every plank, testing for rotteness. Obviously we can't test them all at once, and different tests are needed to assess the strength of different sorts of planks. But no plank is immune to rot, so none should be exempt from testing.