Choosing expert statistical advice: practical costs and epistemic justification

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Abstract:

We discuss the role of practical costs in the epistemic justification of a novice choosing expert advice, taking as a case study the choice of an expert statistician by a lay politician. First, we refine Goldman's criteria for the assessment of this choice, showing how the costs of not being impartial impinge on the epistemic justification of the different actors involved in the choice. Then, drawing on two case studies, we discuss in which institutional setting the costs of partiality can play an epistemic role. This way we intend to show how the sociological explanation of the choice of experts can incorporate its epistemic justification.

Keywords: experts, epistemic justification, practical costs

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1. Introduction

Sometimes a policy maker has to make a decision on the basis of a statistical figure. Calculating this figure is usually beyond the ability of the policy maker and it is thus commissioned to an expert in an inferential technique. Often there will be more than one expert in a given technique or various experts in different statistical methods to calculate this figure. The epistemic problem in these situations is how the policy maker can remain a novice in statistics and yet make a justified judgment about the relative credibility of rival statistical experts.

In this paper we will examine how practical facts – concerning both the expert and the novice – may affect the policy maker's decision and its justification. We will consider Alvin Goldman's criteria for the selection of experts, showing how practical costs impinge on the epistemic justification of such choices. We will argue first that, among Goldman's criteria, the impartiality[1] of the expert is the most influential one in determining whether the expert's advice will be accepted: it does not only justify the choice, from a normative perspective, but it also (partially) explains why, as a matter of fact, certain statistical experts were chosen. The link between the individual interests driving this choice and its epistemic justification is provided by one *practical fact*: the costs of not being impartial (either for the expert or for the novice) are sometimes high enough to compel a choice and, at the same time, justify it.

[1] We will consider the advice of an expert *impartial* if his recommendation (e.g., a statistical estimate) is independent from his own personal preferences or biases. Teira (2013a, 2013b) discusses how this independence can be actually warranted.

At the same time, drawing on two case studies on the actual choice of statistical experts, we will examine under which institutional circumstances the costs of partiality drive the policy-maker's decision. We want to show here an epistemically grounded agreement is possible, under certain circumstances, if all the concerned parties can appraise the costs of partiality in a similar manner, despite the differences in their respective contexts. In addition, we want to show that our approach allows a more charitable epistemic interpretation of the actual processes in which statistical experts are chosen than the current relativist standard among social scholars of science. Yet, we will also argue that impartiality has not the same traction in every institutional context and it is open to discussion whether some epistemic justification of the choice of an policy advisor is possible beyond our current democracies.

In the following section we will briefly discuss the relevance of practical costs for epistemological analysis and we will present and refine Goldman's criteria for a justified choice of experts by novices. In section 3, we will discuss in this perspective Ted Porter's case study of the choice of the US Army Corps of Engineers as experts for the American administration in the early 20th century, showing how Goldman's refined criteria can successfully deal with it. In section 4 Eric Brian's analysis of the first population estimates in the pre-revolutionary France will illustrate that costs are not enough to appraise the impartiality of an expert's judgment in cases where the specification of the object of study is inevitably value-laden. In the final section we draw lessons from these case studies.

2. Choosing experts in context

The justification of the choice of an expert by a policy maker is just an instance of the type of problems addressed in the growing literature on the epistemology of expert judgment (Selinger and Crease 2006). The selection of statistical experts may not

have been a traditional concern for epistemologists, but sociologists and historians of science have documented and explained a number of such choices. Generally speaking, the choice of a statistical approach is explained as an exchange in which both the expert and the novice somehow satisfy their diverse interests. The traditional division between positive and normative inquiries usually applies here. Positive practical interests do not feature prominently in the epistemologist's account of the novice's choice. Normative epistemic considerations do not play a major role for most sociologists and historians in explaining this decision either. However, it is gradually acknowledged that the connection between justified knowledge and practical interests is perhaps closer than both parties previously acknowledged.

Jason Stanley (2005, 3-5), for instance, lists a number of well-known cases in which we grant different degrees of justification to an epistemic agent depending on her practical circumstances. Given certain evidence, an agent may count as knowing something when the practical costs of being wrong are low for her, but not when her costs are high. Hence, knowledge would be connected to action through *practical facts*: something might count as justified knowledge for an agent (or not) depending on the cost of being right or wrong about one's beliefs. However, there are three different perspectives to appraise these practical facts: the standpoint of the agent who claims to know something; the standpoint of a second agent who attributes knowledge to the former; and finally the standpoint of a third agent who is considering this knowledge's claim or attribution from her own perspective.

Each of these three perspectives can be substantiated from an epistemic standpoint. Stanley, among others, claims that the relevant practical facts concern the subject to whom knowledge is being attributed –this view is known as *subject-sensitive invariantism* (Stanley 2005; Hawthorne 2004). On the contrary, *contextualist* authors argue that it is the practical context of the attributor –rather than the subject being attributed– what should be considered (DeRose, 1992, 1999; Cohen, 1988). In cases of self-attribution, these two positions merge, but they are clearly different in cases of third-person knowledge attributions. A further possibility is *relativism* (MacFarlane, 2011), according to which the relevant epistemic standard is fixed by reference to the practical features of the *context of evaluation*. So, the standard for measuring the truth of an attribution of knowledge would depend on the practical interests and costs of whoever is assessing such attribution.

These three perspectives are crucial for our analysis. We should distinguish the practical facts regarding our statistical expert, those regarding our lay policy-maker and finally the practical facts concerning, for instance, the public bearing the consequences of the policy-maker's choice. We will discuss the epistemic justification of a novice choosing an expert and we may wonder which practical facts are relevant for assessing this choice. But we need to adopt first some epistemic criteria for judging such choice, if that is possible.

It has been argued indeed that this choice is epistemically blind: the novice does not have direct access to the relevant evidence and cannot understand the arguments that support the expert's advice. However, a significant amount of our beliefs would be rendered ungrounded and scepticism about expert judgment would be inevitable. To prevent it, epistemologists such as Hardwig, Burge or Foley claim that there is some sort of *prima facie* justification to accept statements from experts (or testimonial knowledge in general), even if it creates a certain epistemic dependence on their knowledge. This may be a justification derived from the mere assertion of a claim by a speaker (Burge 1993) or a general epistemic right to trust others (Foley 1994).

We side instead with Alvin Goldman who argues for separating the discussion of expert judgment from "foundational" issues about testimony and focus instead on empirical evidence that may justify a novice in believing a putative expert rather than another. Goldman (2001, 92) defines a cognitive or intellectual expert in the following manner:

> [A]n expert (in the strong sense) in domain D is someone who possesses an extensive fund of knowledge (true belief) and a set of skills or methods for apt and successful deployment of this knowledge to new questions in the domain. Anyone purporting to be a (cognitive) expert in a given domain will claim to have such a fund and set of methods, and will claim to have true answers to the question(s) under dispute because he has applied his fund and his methods to the question(s).

Suppose a lay person finds herself in a situation where various experts claim to have different answer to a given question. Goldman claims she would be justified in choosing one if she grounds her choice in information extracted from these five empirical sources:

A) Arguments presented by the contending experts to support their own views and critique their rivals' views.

(B) Agreement from additional putative experts on one side or other of the subject in question.

(C) Appraisals by "meta-experts" of the experts' expertise (including appraisals reflected in formal credentials earned by the experts).

(D) Evidence of the experts' interests and biases vis-a-vis the question at issue.

(E) Evidence of the experts' past "track-records"

In sum, the novice should check which expert argues better and who gets more (independent) support from other experts for either her claims or her status, then ponder this evidence with her track record in a particular domain and any possible conflict of interest that may bias her judgment. Even if Goldman does not explain how to aggregate the evidence from these five sources, the basic intuition seems plainly acceptable: a novice would be justified in accepting an expert's claim if this expert scores highly in A, B, C and E and there is no evidence of him being biased.

Goldman's criteria allow for practical factors to play an indirect epistemic role, as indirect evidence about the justification of the expert's views. When the novice has no direct access to the evidence backing the expert's judgment, she will have to make a decision relying on facts about the reliability of the expert (for instance, information about the expert's impartiality). Practical factors concerning the expert may be taken as evidence for or against her reliability and, therefore, as indirect evidence supporting or advising against the choice of that expert. On the other hand, it has been shown above that practical factors can also play a different role, by setting the contextually relevant epistemic standard for knowledge attributions. In this case, practical factors would not be providing evidence of any sort, but would rather contribute to determining what evidence is required in order to consider that someone knows something. Bear in mind that, depending on the semantic framework adopted, the relevant practical factors for setting this epistemic standard will be those concerning the subject of the attribution, the attributor or the evaluator. So, if we consider an attribution of knowledge to an expert made by a novice, a contextualist perspective will take into account the practical costs of the novice, while a relativist one will focus on the practical factors related to the evaluator of the attribution (which can be some third party, say, the general public).

In this paper, we want to examine how practical costs contribute in two case studies about lay policy-makers choosing statistical experts. Our goal is to discern how the actual practical costs for all the parties involved in the decision allow us to qualify Goldman's criteria, on the one hand. And, on the other hand, we will explore to what extent choices of these sort, contaminated by all kind of interests, can be deemed epistemically justified.

3. Estimating costs and benefits of public works

Ted Porter's *Trust in numbers* (1995) is probably the current standard account of expert statistical judgment in the science studies community. Porter considers different scientific communities as professional bodies (or parts of them) that try to advance their joint interests in response to various social demands for expert advice. According to Porter, in Western democracies at the turn of the 20th century, it wasn't possible for public officials to accept individual expert judgment on the sole basis of personal trust in the members of a scientific elite. The experts gradually became professional bodies whose advice was delivered complying "with impersonal rules and calculations in order to exclude bias and personal preferences". The more social pressure for unified and accountable expert advice, the more likely it was that quantification succeeded among social scientists. According to Porter (1995, p. 8), this social pressure was expressed in normative terms: scientific objectivity answered to "a moral demand for impartiality and fairness" which arose in particular social settings.

We will focus on the seventh chapter in *Trust in* numbers (Porter 1995, 148-190. Simplifying the argument for our purposes, it could be stated as follows: the policy makers are here the United States Congress and the experts are the Army engineers. The former seek a decision rule to authorise or not certain public works and the latter offer them an estimate: a cost-benefit analysis of each project. Other experts contested this estimate. Our question is thus to what extent were the policy makers justified in adopting the engineers' estimate as a decision rule. Let us first put this process in context, before applying Goldman's set of criteria to answer the question.

The American Army Corps of Engineers are the experts. Unlike its French peers (the *Corps des Ponts et Chaussées*), this group of military engineers did not find itself "at the center of an administrative ruling class": they were under constant political pressure to deliver reports about the viability of public works that met the Congress (positive or negative) expectations. According to Porter, quantification grew up here "as

an attempt to create a basis for mutual accommodation in a context of suspicion and disagreement". Cost-benefit analysis, claims Porter, was developed as a quantitative procedure to select water projects in a context of conflict between various government agencies and utility companies.

After 1902, water projects needed to be certified as beneficial by a Board of Engineers for Rivers and Harbors that was established within the Corps. The Board rejected more than half of the projects submitted, judging many as economically unfeasible. By the 1920s approval formally required that the benefits of the project exceeded its costs. With a reputation for rigor, the Corps' estimates went undisputed through the 1930s. After the 1936 Flood Control Act, the American Congress could only authorise publicly subsidised navigation and flood control works if there was a preliminary report of the Corps certifying that their benefits exceeded their costs. The cost benefit ratio was usually undisputed and the integrity of the Army engineers was publicly praised. As a matter of fact, the Corps did not owe its reputation to Congress patronage, but to their performance throughout a century of war, claims Porter. However, in exceptional cases the cost benefit ratio could be turned positive if intangible benefits were quantified and added up.

A battle for these exceptions was fought over the 1940s. The Corps faced utilities and railroad companies, on the one hand, and a couple of government agencies, on the other. The former defended their financial interests challenging the accuracy and impartiality of the Corps estimates. The latter advocated diverging methods to estimate the cost-benefit ratios of dams – those who were losing their properties with the dams found these alternative estimates more favourable to their interests. This opposition led to tighter quantification rules, which were partly relaxed and partly strengthened again in the coming decades, until economists came to replace engineers in the 1960s. Their theoretical approach encompassed "an ever more diverse and recalcitrant array of benefits" that the standard methods could not handle (Porter 1995, 187).

Porter does not analyse a single Congress decision, but rather illustrates with various cases the interaction between experts and policy-makers. We cannot assess the epistemic justification of a given expert judgment adopted by a majority in the Congress. We can only presume certain uniformity in the interaction pattern in all these cases during the period analysed by Porter (roughly 1920-1950) and see to what extent there are elements that might have partially justified the policy makers in each particular

one. Let us seek evidence about Goldman's criteria in Porter's discussion. We will see later how to add them up.

(A) *Arguments presented by the contending experts to support their own views and critique their rivals' views*

Confrontation of the Corps estimates must have not taken place until late after its creation, if we accept Senator Guy Cordon's remark (on the occasion of a 1946 Senate hearing on a canal in Louisiana and Arkansas): "this is the first time I have had experience with opponents coming in and controverting facts and making their allegation specific" (1995, 166). A Corps expert was called in to justify the report figures against the objections of the representatives of the Association of American Railroads. Porter gathers evidence that this happened again, at least whenever the opposition was strong enough and succeeded in substantiating their claims against the Corps analysis. Yet, concludes Porter, "it was evidently impossible to for private interests opposing particular projects to discredit its officially sanctioned numbers" (1995, 168)

(B) *Agreement from additional putative experts on one side or other of the subject in question*

Alternative expert estimates were occasionally delivered by other government agencies, e.g, the Soil Conservation Service of the Department of Agriculture. This Service promoted alternative policies for flood control, relying on land management rather than big dams. Its cost benefit methods were consequently less generous with big infrastructures and were preferred by those who considered their interests damaged by the Corps analyses. Following Porter's narrative, all these groups of experts understood that the more unified their advice, the more trustworthy it would be for the policy maker and they soon joined efforts to develop unified cost benefit standards, with scarce success though. This took a toll on the Corps' credibility in Congress in the early 1950s.

(C) *Appraisals by "meta-experts" of the experts' expertise (including appraisals reflected in formal credentials earned by the experts).*

The Corps engineers were recruited among the top graduates of the military academy at West Point, one of the most demanding higher education institutions in the country. The 1936 Flood Control Act required every project to be favourably informed by the Corps before it could be voted in Congress. The Corps engineers officially

became then “the chosen and recognized experts” upon flood control measures. Its expertise was the yardstick that measured everyone else’s competence in the country.

(D) *Evidence of the experts' interests and biases vis-a-vis the question at issue*

Being a military unit, the Corps promoted an image of impartiality regarding partisan conflicts that was publicly acknowledged –Porter collects numerous testimonies in this respect (Porter 1995, 157). Even if their occasional adversaries challenged their impartiality in the assessment of particular project, the interests of these latter were often so obvious that, as Porter puts it, the accusation was self-defeating. Only claims backed up by a different government agency, as legitimately impartial as the Corps, had a chance to prosper. As the King River controversy analysed by Porter (1995, 169-175) illustrates, conflicts between such agencies could not be solved by quantitative estimations alone.

(E) *Evidence of the experts' past "track-records"*

Porter gathers evidence that suggest that the past track record of the Corps was somehow invoked to justify the acceptance of their estimates. One nice example is a 1946 hearing in which a hydraulic engineer had challenged the accuracy of the Corps’ analysis of the benefits of a particular dam with good empirical arguments. His case was dismissed: the committee was “entirely justified” in accepting the Corps estimation, given the many projects they had detected as unfeasible in the past. Though no formal track record of successful estimates had apparently been kept in Congress, the Corps had at least built a reputation of rigor that backed up their figures.

We can conclude then that the members of the American Senate and Congress at least had enough empirical evidence to adopt the Corps’ expert judgment regarding the feasibility of the flood control and navigation projects submitted for approval. In principle, they could be epistemically justified in Goldman’s sense. But there is more to the analysis of Porter than just this evidence.

“Cost benefit analysis was intended from the beginning as a strategy for limiting the play of politics in public investment decisions”, claims Porter (1995, 189). As an estimation technique, it just played an instrumental role in a bargaining process that Porter does not make explicit, but seems to go along the following lines. The American policy makers wanted the Army engineers to deliver an estimate of costs and benefits for public works. Yet they would only believe in estimates justified by general

principles in order to make decisions in Congress less conflictive. The engineers thought that a quantitative technique such as cost benefit analysis met this standard and delivered cost benefit figures. This bargaining process reveals interests regarding gains and losses in both politicians and experts. We read Porter as claiming that the American electorate wanted its legislators to decide with fairness and impartiality about water projects (we may assume they risked their seat if they did not comply). And we assume, by the same token, that the Army engineers wanted to be trusted by these legislators and the American electorate (at the cost of losing their status of experts, whatever the benefits were). This moral pressure on experts and policy makers led somehow to numerical estimates.

Maybe alternative explanations of this case are possible, but they are not currently available. For the sake of our argument, we will adopt Porter's at face value: it simply reveals that in adopting the Corps' view, there was a cost to pay for the American policy-makers if their electorate perceived these estimates as partial or unfair. This cost was equally perceptible for the agents concerned (the politicians) and by a third party analysing the case (such as Porter). The interesting point is that these costs contribute to explain (from a sociological standpoint) and justify (epistemically) the choice of our lay politicians: the estimate had to be numerical.

We can interpret this preference for numbers as compliance with Goldman's requisite (D): numerical estimates provide a warrant about the impartiality of the advice. But Porter's account suggests something deeper: the American policy-makers had to pay a penalty for not choosing impartial experts, since their constituencies cared more about criterion D than about the other four. Or, at least, Porter did not mention an explicit price to pay for the policy-makers if they did not consider the quality of the engineer's arguments, their credentials or their past track-record. The assessment of the expert's impartiality is thus the channel through which the practical costs impinge on the epistemic justification of the novice.

This can be immediately interpreted from a contextualist perspective: the costs for the novice of not choosing an impartial expert raise the epistemic standard of the choice. And we can restate Goldman's D clause as follows:

> [D'] The higher the cost for the *policy maker* of not adopting impartial expert judgment, the more demanding her *epistemic standards* for the attribution of knowledge to the expert will be.

Moreover, the novice policy-maker can judge how impartial an expert is according to the same principle: the Army engineers were competing with other experts for the attention of the Congress and presumably they would suffer some costs for losing it. Hence, the policy-maker can also measure the impartiality of her statistical expert for the costs this latter would pay for her biases. In other words, the novice can appraise how justified is the expert in delivering a particular piece of advice considering the practical facts (costs) impinging on it. Hence we should add a proviso to Goldman's clause:

> [D''] The higher the cost for the expert of not providing impartial judgment, the more justified the policy-maker's choice of expert will be regarding [D'].

It is interesting to notice that in this particular case, as analysed by Porter, there is a convergence in the practical interests of the three relevant perspectives: the expert, the novice and the public should appraise each other's costs in the same terms in order to reach an agreement. The Corps engineers offered numerical estimates because they perceived the public pressure on the politicians for such impartial figures and there was a cost for the Corps if they failed to deliver them. The public pressure, on the other hand, creates costs for the policy-maker which impel her to raise the level of justification in accepting the Engineer's advice, and in particular to make sure of the Engineer's impartiality; and a good measure of this impartiality is how big the costs of not delivering impartial advice would be for the Corps. Finally, a third party, the American public, would surely appraise the costs of both the Engineers and the politicians in order to judge their impartiality – after all, the public's interest in an impartial choice of expert is the source of these costs.

We think this coincidence on the epistemic assessment of the expert's advice is important to provide an alternative interpretation to Porter's account. For him, the process would be epistemically blind to the extent that neither the lay politicians nor the public could grasp the statistical foundations of the experts' advice. Impartiality would be mostly an appearance since, as we just saw, all the involved parties had interests at stake. Yet, if we adopt Goldman's approach with the modifications suggested, we may appraise first the epistemic role of impartiality in justifying the choice of the expert. The incorporation of practical costs via D' and D'' allows us to understand the connection between the private interests of the agents and their epistemic justification. For experts and politicians alike, it was in their own interest to be as impartial as possible, because

this is how they would be judged (and eventually punished). The adoption of statistical methods for public policy-making would be, in this respect, more epistemically justified than Porter's own account suggests. But, as we are going to see in our next case study, it can also happen otherwise.

4. Estimating the French population

The second sociological approach to statistical expert judgment we will discuss is presented in Eric Brian's *La mesure de l'Etat. Administrateurs et géomètres au XVIIIe siècle*. Brian analyses here the exchange between French mathematicians and policy makers in the Parisian Academy of Sciences between 1774 and 1789. The policy makers (namely represented by the enlightened economist Turgot, Head of the French Ministry of Finance between 1774 and 1776) sought a scientific estimation of the French population that could be used in the administrative reforms they were undertaking in order to improve the public finances. At the same time, a group of "geometers" in the Academy, led by Condorcet, its perpetual secretary and a friend of Turgot, were trying to promote the application of mathematical analysis to social phenomena, beyond its traditional realm. These mathematicians easily found external support in the policy makers in order to reform the scientific organization of the Academy. In exchange they provided new statistical tools to model the French population according to the new administrative categories these politicians were trying to implement.

Brian analyses this exchange inspired by Pierre Bourdieu's sociology of science. In his analysis, the political interests of the French administrators are not taken to be constraining unilaterally the choices of the mathematicians. It is admitted that their interests as a group within the Academy of Sciences are pursued autonomously. As a matter of fact, the language of interests is altogether avoided: for Bourdieu, both mathematicians and policy makers act following more behavioral dispositions (*habitus*) than beliefs and desires. Rather than tracing back to their psychological origins the motivations of these French geometers, Brian reconstructs their philosophical and practical understanding of mathematical analysis and focuses on their efforts to implement these views in the Academy.

Hence, according to Brian, in order to explain Condorcet's actions we should understand how he merged three different doctrines. He took from D'Alembert a metaphysical view of calculus that allowed its application to whatever domain. From

Turgot, Condorcet took not only epistemological ideas (about the analysis of the social order), but also practical guidance about how to implement some of them to the governance of France. Condorcet finally learnt from Laplace how to ground probabilities on integral analysis and apply them to infinite domains. Condorcet combined all these ideas in a way that put him in the position to understand what sort of figure the policy makers were after. He then pushed his colleagues at the Academy to use the surveys commissioned by the Ministry of Finance and deliver an estimate for the French population. This was the *Essai pour connaître la population du royaume*, published in 1783-1788 in the *memoires* (reports) series of the Academy. It compiled the figures gathered by La Michodière, another French administrator, together with the analytical papers of the three featuring co-authors of the *memoir*: Condorcet, Laplace and Dionis de Sejour.

The official support for the project allowed Condorcet to reorganize the Academy in conformity with these ideas. In return, the Academy invested La Michodière's figures with scientific authority. The French enlightened opinion around 1780s was apparently more inclined to believe population estimates with the scientific endorsement of the Academy than as mere administrative calculations (Brian 1994,192). The publication of the *Essay* completed the transaction between geometers and administrators. However, the outburst of the Revolution aborted this blooming collaboration.

We can pose again the question of whether the French policy-makers were epistemically justified in accepting the Academy's estimate. Brian compiles good evidence to feed Goldman's criteria. Let us examine first evidence for debate among experts on the estimate (criterion A). Following Brian's analysis of the process, we should notice that the Ministry of Finance did not choose among various estimates of the French population, but rather commissioned one to the Academy, relying exclusively on its scientific authority. Yet, still according to Brian, an alternative estimate provided by the former finance minister Jacques Necker had prompted the allegiance between the Academy and the Ministry of finance. In the *Essay* there are indeed criticisms of the use Necker made of the Ministry surveys in his 1784 *De l'administration des finances de la France* (Brian 1994, 272). However, the French public sided with Necker treatise and ignored the Academy report, even if the

divergence between their figures wasn't huge. It was considered politically suspect, claims Brian, too close to the interests of the Monarchy, despite its scientific authority.

Brian is quite explicit about the monopoly on scientific authority that the Academy exerted in France at that time. In this respect, the scientific credentials of Condorcet and his co-authors could not appear higher (criterion C). Their track record as statistical experts, if we measure it, for their publications in the memoires series (they had not issued estimates for social variables before) seems equally positive (criterion E).

However, despite the scientific authority of the Academy's estimate, the support received from the Ministry of Finance apparently deprived it of any public credibility – warns Brian. A Monarchy sponsored estimate seemed politically loaded and, as a matter of fact, when additional experts issued alternative figures soon afterwards (Brion de La Tour, the Chevalier des Pommelles), they were inspired more by Necker's criteria than by the Academy. Criterion B (additional experts) speaks against the epistemic justification of the admistrator's choice of statistical advice.

But what about the experts' impartiality (D)? It is interesting to notice how, in Brian's Bourdieusian approach, the members of the Academy act in their best scientific interest: they did not compromise on the quality of the estimate in order to satisfy any practical interests. However, even if the French administrators could considered their experts unbiased (as criterion D requires), the adverse reaction of the French public suggests that their population estimate, as such, was not considered impartial. The choice of an expert by the French administrators might appear somewhat epistemically justified, from Goldman's standpoint –after all, it met criteria C, D and E. Yet, the issue at stake was the impartiality of the estimate itself: it seemed too dependent on the interests of the administrators who had commissioned it. This is Brian's more radical point: the object to which the Academy's statistical tools were applied, the French population, did not pre-exist, but was somehow *constructed* in the exchange between scientists and administrators.

There were different sets of administrative divisions of the territory (political or geographical), depending on the purposes of each administration. On this basis, different sets of figures were compiled. The Academy chose one particular compilation, La Michodière's, in order to aggregate these figures into an estimate for the population of the country. This aggregation implied, in turn, accepting a new mathematical technique of integration developed by Laplace, together with a philosophical rationale

justifying its application to social phenomena (developed by Condorcet). The Academy estimate of the French population implied thus the expert sanction of a political set of figures (as apt for scientific purposes) and the lay political support to a particular view of mathematical analysis (as the more adequate approach to deliver the estimate). The estimate was somehow conditional on this mutual agreement, brokered through the personal connections of Condorcet in the Academy and the Ministry.

Therefore, *the choice of an impartial statistical expert does not need to make the estimate impartial*: the members of the Academy perhaps had no personal preferences regarding the issue at stake, but their estimate is certainly contaminated by the preferences of the sponsor, since the available data depended on a number of previous political choices. Goldman's criteria assume a world in which a lay jury ponders the merits of the different expert views on an issue that is somehow external to expert or lay intervention. Its relevant features do not depend on their practical interests. But in the Bourdieusian picture drawn by Brian, a group of scientists and a group of administrators trade on how to analyse the French population and every possible judgment about it would be inevitably value-laden, even if the statistical technique applied is, as such, neutral: there was no independent access to the French population and the figures compiled depended on an array of politically loaded decisions.

Moreover, the practical costs do not qualify impartiality as they did in our previous case study. On the one hand, Brian's approach does not explicitly consider costs, mostly because the agents are not acting according to their beliefs and desires, but rather, as we already suggested, on their behavioral dispositions (or *habitus* in Bourdieu's terms). Hence, their impartiality cannot be qualified by practical facts: either they have it or they don't. As the case under discussion illustrates, their subjective attitude would not count much: it would have been the partiality of the estimate, and not the estimator's fairness, what caused the protests. However, leaving Brian's own perspective aside, it is interesting to notice, that institutionally speaking, the decisions of experts and policy-makers alike were relatively cost-free: the Academy had no competition and the French administrators were not publicly elected.

This case illustrates thus how the institutional constrains guiding the choice of experts by lay politicians may impact on its epistemic justification. In a regime where impartiality is not a pre-requisite for governance, Goldman's four other criteria do not seem to have the social traction that allows us to connect individual interests with

epistemic choices. We cannot tell though whether this is just a peculiar feature of this case study or a more general problem.

6. Concluding remarks

With this exploration of the epistemic role of practical costs in the choice of experts, we have tried to bring together the fields of social epistemology and historical sociology of science. On the one hand, we have tried to refine Goldman's criteria for the epistemic justification of novice's choosing experts with the inclusion of practical costs, showing how these latter can qualify such justification through the assessment of the expert's impartiality. On the other hand, drawing on two sociological analysis of the choice of a statistical expert, we have shown first how impartiality seems to be the epistemic criterion with more social traction (among Goldman's five); and then how its relevance crucially depends on the institutional setting where the choice of the expert takes place.

In an American democracy, it is possible to epistemically ground an agreement to the extent that there are individual costs for all the parties involved. In this respect, against Porter, we think that the adoption of statistical methods for public policy-making is epistemically more defensible than most social students of science contend. However, if the institutional setting is an absolutist monarchy, with little completion other than for the king's favor, impartiality loses traction since there is probably little to gain for being fair. Moreover, if there is no independent access to the subject of the expert's advice, his own impartiality will not be enough to guarantee the impartiality of his advice.

Two case studies do not provide a conclusive argument. But we think they at least suggest that the social epistemology of the choice of experts (beyond statistics) can fruitfully explore how practical costs impinge on justification via the impartiality of the experts and policy-makers alike. This approach can also help historians and sociologists to reconsider their own implicit epistemic premises and perhaps appreciate that sometimes the adoption of scientific methods is more epistemically grounded than it may seem at first.

7. References

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