Received: 23 June 2021 | Revised: 21 August 2021 | Accepted: 13 September 2021

DOI: 10.1002/jhbs.22145

ORIGINAL ARTICLE

WILEY

Kiær and the rebirth of the representative method: A case-study in controversy management at the International Statistical Institute (1895–1903)

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Abstract

Anders N. Kiær (1838–1919), the director of Norway's Central Bureau of Statistics between 1877 and 1913, was the foremost promoter, at the turn of the 20th century, of the rebirth of what came to be known as the "representative method" or sample survey. His advocacy of a methodology that had been abandoned at the beginning of the 19th century in favor of complete enumeration (the census) provoked a controversy at the International Statistical Institute (ISI) when he first presented it in 1895. Yet, it was "recommended" in fairly short order, by 1903. This was the result of a convergence of factors that prevented the dispute from degenerating into a full-blown conflict and facilitated continuing the discussion while preventing a potential break-up of the association. To understand how this came about, the paper examines (1) the role of the historical background from which the ISI emerged; (2) the epistemic beliefs that informed the ISI members in their daily professional practice; (3) the social structure of the ISI and its "ethos"; (4) the professional standing Kiær enjoyed within the international statistical community. This is a case-study in the sociology of how and why some scientific practices initially seen as "dangerous" gain acceptance and become part of science's lore.

KEYWORDS

census, epistemic beliefs, International Statistical Institute, representative method, scientific controversy

1 | INTRODUCTION

This study seeks to understand how a practical idea (the "representative method," i.e., human population sampling), nearly rejected when first introduced, was eventually accepted. The site where this occurred is a scientific society: the International Statistical Institute (ISI). It took five meetings, from Bern in 1895 when it was deemed "very dangerous" to Berlin in 1903, for the application of the representative method to be "recommended." What factors hindered its adoption and what others facilitated its eventual integration into the epistemic framework of the ISI? What were the social controls that prevented the controversy from degenerating into a sectarian conflict?

This study examines what happens when an innovation is thought to be contradicting established norms of practice. It explores how a novel concept introduced into a generally hostile epistemic environment was able, nevertheless, to propagate. The paper is also an investigation of a controversy. It is common for conflict to arise in a scientific community when a new idea is proposed by one of its members.[1] Just as commonly, a dispute between opponents and proponents can become heated, which sometimes leads to the formation of two entrenched camps. The conflict is resolved when the novelty is either convincingly overturned or is adopted by the community or when the antagonists split into separate schools of thought. This study illustrates that the resolution of a controversy can depend on more than substantive arguments. It shows how factors seemingly irrelevant to the epistemic issue at hand (the social and institutional context in which the idea is introduced) can play an important role and help pave the way to a resolution, and how these elements spared the ISI a divisive outcome.

First, the study examines some general aspects of controversy in science and how they relate to the debate over the representative method. Next, it describes the contextual factors that formed the environment within which the dispute took place. The fourth section begins with a brief chronology of Kiær's advocacy, followed by an assessment of the substantive debate. The conclusion summarizes and expands on the arguments presented.

2 | CONTROVERSY AND DISPUTES IN SCIENCE

Deviance, controversy, rewards, and punishment occur in the house of science as they do in any other human collective. Conflict in science has been studied by numerous scholars (e.g., Farrall, 1975; Gieryn, 1983; Hagstrom, 1965; Merton, 1957; Pinch, 2015).[2] Dissensions arise when a lack of consensus on some issue comes about within a community. Some are short-lived (e.g., cold fusion), others are protracted (e.g., Volta and the electrophorus). Priority of discovery or invention (e.g., the calculus), a violation of an ethical norm or deviant behavior (e.g., scientific fraud), divergent interpretations of experimental results (e.g., Pasteur v. Pouchet), all have been causes of disputes. The way these conflicts play out and are resolved vary considerably: sometimes the novel idea is accepted (e.g., sampling), sometimes it is rejected (e.g., phrenology). Furthermore, substantive disputes are not always the cause of conflicts in science. Disagreements over priority, for instance, have nothing to do with theoretical or methodological issues (e.g., Chuprov and Neyman).[3]

But not all disagreements within a community develop into a conflict. The debate over the representative method is one of those. This type of controversy has not received as much attention in the scholarly literature on science as the more contentious ones. Perhaps it is because they lack the drama associated with their siblings. Whatever the case may be, the current essay is a contribution to the social study of these ephemeral, yet substantive disputes whose emotional intensity is subdued.

Some have argued that the training budding scientists receive "blind" them to any novelty (Mulkay, 1969, p. 29). Is conformity a more potent force than originality? If that were the case, no "new" ideas would come about since everybody would be "stuck" in the existing faith; there would be no disputes. In fact, just as it occurs in the wider society, there are in science iconoclasts and their followers—as there are conformists. During their training scientists are exposed to the history of their discipline and they learn that science "progresses" thanks to those who were not "committed" to existing theories (Hagstrom, 1965, p. 12). The apprentice scientists are told, for instance,

that had it not been for Copernicus they would still be living in a geocentric world. Thus, they see that there is merit in questioning the cognitive "status quo," as long, of course, as one is on the "winning" side.

Generally, acts or ideas that deviate from current customs call for some sort of corrective action against the culprits. Some deviances are perceived as an existential threat to prevailing norms, whereas others are not. This leads to different forms of response: punitive or assimilative.[4] Both of these were present in the debate on the representative method. As we shall see, there were those who rejected the idea and described it in the most damning terms ("very dangerous"); and there were those who believed it could be integrated into the statistician's epistemic culture, although in a role that deferred to the "gold standard" (i.e., the census).

One way the scientific community ensures that deviant ideas are not given the limelight is to prevent them from being published in journals. In the conflict between Mendelians and biometricians, after publishing, without enthusiasm, a paper by the biometrician Karl Pearson (1857–1936), the Royal Society, dominated by members of the opposing camp, asked him "to divide future papers into two parts, a biological part and a mathematical part, so that they could be published separately" (Farrall, 1975, p. 289). This did not sit well with Pearson and convinced him that subsequent papers of his would not get printed. As a result, he founded his own journal in 1901, *Biometrika*, with the support of Francis Galton (1822–1911). In return, the pages of this new publication were "effectively closed" to Mendelians (Farrall, 1975, p. 295). Two decades later, Pearson would be the one dishing out now the same medicine he had then found hard to swallow. He turned down a paper by R. A. Fisher (1890–1962) for publication in *Biometrika* with the following comment: "Under present printing and financial conditions, I am regretfully compelled to exclude all that I think erroneous on my own judgment, because I cannot afford controversy" (Pearson, 1968, p. 453). These examples appear to be clear violations of the putative "ethos of science" according to which the evaluation of a scientist's contribution is based on "detached scrutiny" (i.e., standards that eschew personal likes and dislikes) informed by "empirical and logical criteria" (Merton, 1973, p. 277).[5]

In contrast, nothing of the sort occurred during the time the representative method was on the ISI's agenda. In fact, the very opposite took place. One of Kiær's most vociferous opponents at the Bern meeting, Herr Doktor Georg von Mayr (1841–1925), characterized it as "very dangerous" (IIS, 1896b, p. XCIV).[6] Yet, the very same individual, who was the editor of the *Allgemeines statistisches Archiv* (General statistical archive), the flagship journal of German statisticians, published an article by Kiær on the representative method, a mere 4 years after the conference in the Helvetic capital (Kiær, 1899a). If it is indeed the case that professional journals are "the main institutionalised mechanism whereby scientific conformity is maintained," then publishing Kiær's article was a major departure from this rule (Mulkay, 1970, p. 16).[7] Yet, there is no question that Kiær's idea was greeted as "subversive" to the ISI's "basic commitment," which was complete enumeration. As an illustration of what has been referred to as the "process of resistance in science," Kiær's opponents were committed to preventing its adoption by getting it off the agenda of the ISI conferences and thus putting an end to the debate (Duncan, 1974, pp. 111, 120). We shall see later what elements came into play to weaken this "resistance."

One of the characteristics of many controversies within the scientific community is that they can become quite acrimonious between the principal protagonists and their followers. Hagstrom (1965) suggests that "[d]isagreements are likely to be exacerbated and lead to hostility when participants question each other's technical competence" (p. 265). In 1935, for example, Fisher initiated a conflict with Polish mathematical statistician, Jerzy Neyman (1894–1981), who, as it happens, had studied under Karl Pearson. Fisher, as lead discussant of a paper Neyman read before the Royal Society, disregarded the customary ritual of thanking the speaker in the name of the assembled members. Instead, he chose, without warning, to disparage Neyman as someone not "fully acquainted" with the topic he was discussing (Neyman et al., 1935, p. 154). In 1937, in back-to-back issues of the *Annals of Eugenics*, Fisher went on to denigrate the late Pearson and his statistical legacy (Fisher, 1937). Fisher resented him ever since their little set-to in 1920, mentioned earlier. In contrast, Kiær never experienced anything of the sort during his advocacy of the representative method: no *ad hominem* attacks and certainly no questioning of his "technical competence."

3 | THE CONTEXT

This section describes the social environment in which the representative method was introduced. It identifies factors that impeded its swift acceptance as well as those that made it possible for it to remain on the agenda of the ISI's meetings and, in time, to be recommended. Three areas are examined: (1) the scientific beliefs that informed the professional practice of ISI members; (2) the social structure of the ISI and its institutional norms; (3) the professional profile of Kiær and his standing within the ISI.

3.1 | Epistemic beliefs

During most of the 19th century, state bureaucrats relied on complete enumeration of their country's population to obtain demographic data. The census became the dominant methodology. It met the scientific imperative of the day: that of complete observation. It was seen as a significant improvement over the mathematical "speculations" of Pierre-Simon Laplace (1749–1827). At the turn of the 19th century, the French scientist had calculated the size of his country's population based on a subset of the territory and the number of births therein. His ratio estimator approach was not pursued; instead, most of Europe and America put in place the bureaucratic apparatus to conduct censuses. As the Danish statistician Harald Westergaard (1853–1936) stated: "After Laplace not much was done to develop the theory of representative statistics or of indirect methods of finding the numbers required. This is probably connected with the fact that direct methods of complete enumeration, such as a census covering an entire country, gradually became popular…" (Westergaard, 1916, p. 234). Another statistician, Oskar Anderson (1887–1960), summarized the state of data collection for administrative statistics thus: "As statistical observation was extended in the nineteenth century, indirect methods became superfluous" (Anderson, 1934, p. 367). This cognitive frame of mind is well expressed by a founder of the ISI, Frederic J. Mouat (1816–1897) of the British Royal Statistical Society when he wrote, reflecting on the accomplishments of statistics in the 19th century: "statistical inquiry has introduced order, method, and precision, in the place of speculation, conjecture, and uncertainty" (Mouat, 1885, p. 51). In short, this era represented the "triumph" of the "idea of census," as some historians have put it (Dupâquier & Dupâquier, 1985, pp. 93–102).

Kiær's opponents likely espoused the widespread view among nineteenth century learned circles that "uncertainty arose from imperfect knowledge" (MacKenzie, 1989, p. 117). Hence, Laplace's approach could be qualified as "speculative." By putting in place the bureaucratic infrastructure to implement censuses, governments fulfilled the creed according to which "complete" enumeration would ensure "exact knowledge" (Stigler, 1986, p. 165). As historian Theodore Porter notes: "Nineteenth century statisticians reveled in the existence of government records such as census data, which liberated them from the need for surmise and conjecture—both standard equipment for political arithmetic in the previous century." He adds: "The special merit of statistics was its insistence on accurate and exhaustive enumeration, its exclusion of guesses and approximation" (Porter, 1986, pp. 40, 46). This is the epistemic context within which Kiær introduced his idea.

A novelty is often perceived as a threat to existing beliefs (Rogers, 2003, p. 26). It is hardly surprising then that, at the Bern meeting, his proposal was received with a chorus of opposing voices. It was the very antithesis of the prevalent paradigm (i.e., the census) since it was based on "incomplete" observation.[8] Using a part of the whole (i.e., a sample), the goal of this methodology was to produce a "miniature" picture of the population under study, which would then allow the observer to infer characteristics and attributes for the entire population (Kiær, 1896a, p. 176; Kiær, [1897] 1976, p. 39). The representativeness of the sample was established by making sure it compared favorably on variables known for the population. For example, if, according to the census, a quarter of the population is urban then the same proportion should be reflected in the sample (Kiær, 1896a, p. 178). Based on the most recent census, other demographic variables, such as industry and occupation, sex and age, marital status, and so on, were used for matching purposes. Having thus selected units that exhibited a close resemblance to the

population on the *known* variables, it was then possible to study other characteristics and attributes not recorded by the census ("outcome variables") and come up with estimates for the entire population. As Kiær's stated: "The more points of agreement between the results based on the representative returns and the conventional statistics, the more confidence can be placed on the former even for characteristics not covered by both" ([1897] 1976, p. 56). Later, this mode of sample selection came to be known as *purposive sampling* (Jensen, 1928). Kiær stressed, without further specifying, that the sample should be of "sufficient size" and cover "a sufficient geographic distribution (spread) over the whole country" ([1897], 1976, p. 39).

The system of belief that viewed the census as the only methodology capable of providing a "true statistic" had made room, nevertheless, for another mode of knowledge production: the monograph (or monography).[9] What this technique lacked in breadth, it gained in depth. But its legitimacy was not in doubt. According to Luigi Bodio (1840–1920), a founding member, the Institute's secretary general, and the director of Italy's bureau of statistics, monography provides "the blood, the flesh, the nerves" to the "skeleton" fashioned by complete enumeration. Furthermore, these two modalities of social investigation "complemented each other" (IIS, 1896a, pp. XXXVIII–XXXIX). We will see that Kiær used the monograph as a prop in his rhetorical strategy to convince the ISI that the representative method deserved to be considered a legitimate mean of inquiry.

3.2 | Institutional context

How was the ISI created? What was its structure and its "ethos"? When the Statistical Society of London celebrated its jubilee (1885), one of the items on the reunion's agenda was to explore "the possibility of establishing an International Statistical Association" (Statistical Society of London, 1885, p. ix). To that end, it invited foreign dignitaries in charge of statistics in their respective countries (Statistical Society of London, 1885, pp. xiii–xiv). The conference regarding the creation of an international statistical organization took place after the adjournment of the jubilee meeting. The new organization was given the name "Institut International de Statistique" (ISI).[10] It was not the first time that statisticians had tried to create an international forum to discuss matters of interest to the profession.

The International Statistical Congress (ISC) was the ISI's immediate predecessor. It was the brainchild of the Belgium polymath Adolphe Quetelet (1796–1874). The ISC held nine meetings: the first in Brussels (1853), the last in Budapest (1876). Many statisticians lamented its demise. What they missed most was the existence of an international professional organization that allowed statisticians to exchange ideas. R. W. Rawson (1812–1899), the Institute's first president, would state that the passing of ISC "deprived statisticians of these wonderful opportunities to meet in order to advance science and exchange ideas and experiences, which result in developing and tightening the bonds of friendship and affection that are created so advantageously between men of science" (IIS Institut International de Statistique, 1887a, p. 40). However, other aspects of the ISC were deplored as being instrumental to its downfall: the participation of too many members of the lay public; the high turnover of personnel of the governing body of the ISC which resulted in a lack of continuity in the discussions on various topics of statistical interest; participants to its meetings were overly represented by those (lay and professional) who lived in the city and country hosting the conference; the quasi-official relations the ISC entertained with national governments limited the open discussion of some topics, in turn governments resented the pressures from the ISC to adopt its recommendations. These flaws, the founders of the ISI wished not to reproduce (Neumann Spallart, 1886; Willcox, 1924).

Thus, one of the first steps taken by the assembled statisticians in London was to elect and induct members to the newly formed association (the original members of the ISI)—including German statisticians, even though all of them turned down the Royal Society's invitation.[11] According to article IV of the new association's provisional statutes, members were to be chosen among individuals "who have distinguished themselves in the domain of administrative or scientific statistics" and specifically, "heads of official statistical bureaux, members of central

Statistical Commissions, Municipal Statistical Bureaux and Statistical Societies, and others of reputation in this branch of science" (Rawson, 1885, p. 322). These measures were meant to protect the ISI from being diluted by the lay public and to ensure that its members had similar scientific concerns and would be dedicated to the Institute's advancement. The ISI wanted to be seen as "an independent scientific academy" whose influence would be based on its cognitive authority (Zahn, 1934, p. 16). Many of the inductees of the newly formed ISI were veterans of the ISC: this common bond was an important inducement to strive to ensure the survival of the ISI.

In addition, according to the revised statutes (article XII) of 1887, a candidate to membership had to be sponsored by five members in good standing (IIS, 1887b, p. 3). There were two categories of members: titular (later called ordinary) and honorary. The first group according to Article IV was limited to 150; that same clause emphasized that it was by no means necessary to reach that number (IIS, 1887b, p. 2).[12] There was no limit to the number of honorary members, but that group never reached thirty during the period under study (Nixon, 1960, p. 124). Article V directed the ISI's Bureau (which administered the Institute's affairs between reunions) to invite non-members who were in charge of statistical services to its meetings (IIS, 1887b, p. 2). This rule represented another barrier against the intrusion of the lay public, and, presumably, encouraged these qualified invitees to apply for membership. They were also granted the right to vote on substantive topics (statistical issues) discussed during conferences, but not on internal matters of the Institute (e.g., elections to the Bureau). At each biennial conference, attending members voted for the officers of the association. There were four elected offices to fill: president, vice-president (of which there were, at first, two then three in 1895), secretary general, and treasurer.

Very homogeneous communities, such as some scientific societies, have been shown to be a fertile environment for the exchange of ideas. Because they share "common meanings, beliefs, and mutual understanding" and form a cohesive network, communication among these "homophilous" individuals is said to be "more effective." A social system characterized by a horizontal structure accelerates the diffusion of an idea. Therefore, the less a social group is vertically structured, the less hierarchy there exists in an organization, the more likely a new idea will diffuse (Rogers, 2003, pp. 19, 36, 305–308).

The ISI was a society of equals. One English member thought that the Institute had succeeded in fashioning a governing structure "without creating an oligarchy out of touch with the members at large" (Baines, 1904, p. 493). Indeed, the elected offices did not confer upon the incumbents some ruling power over the members—everybody was of the same social rank: top officials at various levels of a state's bureaucracy and highly regarded academics. Some of them cumulated both bureaucratic functions and academic responsibilities. In addition, many of these men had reached the pinnacle of their career.[13] This is not to say that differential prestige was absent from this community. Being a founder of the Institute, length of membership, type of membership (honorary or regular), level of participation at the meetings (attendance, presenting reports, being a discussant, being a member of a commission, etc.), being part of the officialdom (president, vice-presidents, etc.) are elements that, no doubt, conferred upon individual members an aura above and beyond their professional status. But this did not alter in any way the essentially homophilous character of the association. Thus, the social structure of the ISI could be described as more "horizontal" than "vertical" (Rogers, 2003, p. 307).[14]

During the years of Kiær's advocacy, the total membership of the Institute was always under two hundred. Attendance at conferences was also under that figure, except for the 1903 meeting in Berlin where it reached 245—a substantial increase compared to all previous ones. Session attendees included invited guests, who always represented the majority of participants: 60 percent on average (Zahn, 1934, p. 22). Only one-fifth to two-fifths of the ISI membership attended the meetings.

Clearly, the ISI was a very exclusive (and all-male) "club." In those days, candidates had to receive a three-quarters majority to be inducted into the organization—that changed to two thirds in 1901 (IIS, 1887b, p. 3; IIS, 1903a, p. 33). Given the small number of conference participants, including invitees, these meetings were very intimate affairs. Attendees worked closely together during these reunions and often belonged to multiple committees. For instance, at the 1899 meeting in Christiania (today's Oslo), Kiær served on five committees (IIS, 1900, pp. 157, 158, 162, 163). During this period, the conferences usually lasted 6 days—with two exceptions: Berlin (1903) 5 days and Christiania 9 days.

As a result of its recruitment policies, the ISI was characterized by a high degree of cohesiveness.[15] Highly cohesive communities can be an unwelcoming environment to a "deviate opinion," which runs a higher risk of being rejected than in less connected ones (Emerson, 1954, p. 688). It would seem, then, that cohesiveness and homophily give rise to two opposing social forces. Because "deviate" ideas can lead to disunity, there is a tendency to want to reject them; because homophily promotes effective communication, "deviate" opinions have a better chance of being heard. We will see what factors mitigated rejection, smoothed the way to acceptance and, thereby, resolved the controversy.

In science, it is an imperative to share one's work and a conference is a stage where that happens (Merton, 1957, p. 655). Unlike other social contexts, there is no obligation, in the scientific world, to accept a contribution (e.g., a paper) simply because it is offered.[16] On the contrary, to be accepted the contribution must meet scientifically based criteria, applied by those who, as representative of the larger community, have been elevated as judges (e.g., peer reviewers, conference discussants). Thus, what occurs here is a *contingent* exchange in which the potential recipient (the judges *qua* the community) is driving the "bargain."[17] Likewise, a presentation is the occasion of a reception that will end up either in accolades or in rejection. At first, as we shall see, a majority of discussants rejected Kiær's idea. Such an incident can lead to controversy which, in turn, can give rise to antagonistic relations that have the potential of destroying the group's cohesion. Hagstrom observes that some disputes in the scientific community evolve into a "vicious circle" that can, sometimes, lead to the formation of hostile "schools" within a discipline. He adds: "Such vicious circles are most likely to develop *when participants do not come face to face*" (1965, p. 266. Emphasis added).

Rogers has argued that "change of strongly held attitudes (…) is accomplished mainly by interpersonal channels" (Rogers, 2003, p. 205). Because of their small size, the ISI's meetings created an environment in which face-to-face as opposed to impersonal interactions reigned, and anonymity could not prosper. To fan the flames of conflict within such a confined assembly could have been disastrous to the functioning of the conferences and to the survival of the organization. Moreover, meeting organizers always made sure to put in place bonding rituals, such as outings and receptions. Of the latter, one of the ISI's vice-presidents and founding member, Emile Levasseur (1828–1911), was convinced that they were both pleasant and necessary (IIS, 1900, p. 24). This intimate environment facilitated casual interactions, on the margin of the official program, that allowed attendees to get to know each other better, and were conducive to the forging of friendships and to negotiations over controversial topics. As one British member, J. A. Baines (1847–1925), remarked:

> Men of every country, whatever their national differences in other respects, are here united by their common interest in a vast field of investigation, and come into intercourse with those whose names have long been familiar to them as masters of their craft, and are thus enabled to improve the occasion, not merely by "talking shop" over the committee-room table, but by exchanging views on things in general side by side at the dinner table, in the reception room, or even in a railway carriage or on the deck of an excursion steamer (Baines, 1904, 497).[18]

These types of communication channels have been shown to be more successful in overcoming an entrenched opposition (Rogers, 2003, p. 205).

Levasseur told his colleagues, in quasi-Shakespearean tones, that when the time came for conference attendees to return to their respective countries "one feels pangs of sadness" (IIS, 1900, p. 74). His effusive statement served to extol the ideal of brotherly bonds and as a prescriptive reminder of how things should be. In addition, he made explicit a "rule" he believed should guide the deliberations of the Institute:

> Most of the issues we examine are not of a nature that allows their resolution in one meeting. They must be studied for a long time by the committees that you establish and above all by the rapporteurs who are in effect the leaders of these committees, and they must give rise to research, discussions and reports that will shed light upon them little by little (IIS, 1900, pp. 21–22).

In a way, Levasseur's rule, perhaps an expression of what Barber calls the "norm of open-mindedness," which, according to him, is "one of the strongest of the scientist's value", was an attempt at establishing an "institutional mandate" (Barber, 1961, p. 596; Merton, 1973, p. 277). Its function was to be a counteracting force against the rigidity of a belief system (here, complete enumeration). It defined the rights of the presenter and the obligations of his audience; its purpose was to ensure that all members should be allowed to present their ideas and these ideas, however "deviate," to be discussed thoroughly, such that, even if they were rejected, their advocates would feel they had been given a fair hearing. He was reiterating what another founding member, R. W. Rawson, had advocated from the very start:

> Wide differences of opinion may occur regarding proposals submitted; it will perhaps be impossible to reach a perfect agreement on some of these questions; but I request, in all confidence, for the sake of the International Statistical Institute, your generous cooperation, a *full tolerance for all opinions*…" (IIS Institut International de Statistique, 1887a, p. 43) [Emphasis added].

All of the characteristics just described (the memory of the ISC's demise, the structure of the ISI, the small size of its conferences, etc.) helped to shield the ISI's social cohesion from disruptive controversies and to promote solidarity against ascendancy; they encouraged dialogue and prevented divisiveness—animosities seem to have been kept in check, and amity promoted.[19] All these elements were effective means of social control. They assisted in the management and containment of potential polarizing conflicts; they created a favorable environment for Kiær's advocacy of the representative method.

3.3 | Kiær's professional standing

Previous research has concluded that the social standing enjoyed by a messenger, deviant or not, will affect the audience's disposition towards the message (Barber, 1961, pp. 599–600; Rogers, 2003, p. 5; Wahrman, 1970, p. 229). And since, as noted, innovators are often seen as deviants, anything that can mitigate or obviate that perception is likely to facilitate the diffusion of their idea. Presumably, then, a novelty advocated by someone of high repute is more likely to capture the attention of potential users and given its day in court, than if it is presented by someone of lesser standing.

In 1885, Kiær had been one of the founding fathers of the Institute at London. His statistical career began in 1867 when he became the head of the statistical office of the Ministry of the Interior (Kiær, [1897] 1976, p. 8). When in 1877 Norway created the Central Bureau of Statistics, Kiær was appointed its first director; a position he held until his retirement in 1913. Before the founding of the ISI, he had been active with the ISC, attending his first meeting in Florence (1867) (Willcox, 1919, p. 440). At the 7th meeting of the ISC in The Hague (1869), he presented a report on the state of official statistics in Norway and did so again at the next meeting in St. Petersburg (1872). When the ISC created a Permanent Commission (1872), as Norway's representative, Kiær became one of its members, where he rubbed shoulders with future ISI members. For the 9th and, what turned out to be, final meeting of the ISC, he prepared a memorandum on the issue of national revenue statistics. At that same conference (Budapest, 1876), he presented a report on the statistical work taking place in Norway. In addition to this, he attended and presented at other international conferences: the International Congress on Demography in Paris (1878), the International Congress for Hygiene and Demography in Vienna (1887), the International Congress of Customs Legislation and Labor Regulation (Antwerp, 1894), and the International Congress to Combat Alcohol Abuse (Basel, 1895). He was published in a number of scholarly journals not only in Scandinavia (e.g., *Statsøkonomisk Tidsskrif*), but in other European publications (e.g., *Der Arbeiterfreund*), and in the United States (Kiær, [1897] 1976, pp. 57–59; Kiær, 1896b).

At the founding of the ISI (1885), he was appointed to the special committee tasked to draft the statutes that would govern the Institute based on the recommendations outlined in a report written by Franz X. von Neumann-Spallart (1837–1888), a member of Austria's Central Statistical Commission (Neumann-Spallart, 1885). This was a clear acknowledgment of his standing within the fraternity of international statisticians. Aside from one meeting (Rome 1887), he attended every session of the ISI until the onset of WWI (Vienna 1913) and presented on multiple topics (e.g., maritime tonnage, revenue distribution, performing censuses in parts of the globe that had never been enumerated) in addition to the representative method; he was a member of various commissions (e.g., IIS, 1896a, pp. CXIII, CXIV, CXVIII). As a discussant he participated in debates on a multiplicity of issues and, at the St. Petersburg meeting (1897) he was named co-auditor of the ISI's financial accounts (IIS, 1899, pp. 222–223). Furthermore, he was an Honorary Fellow of the British Royal Statistical Society (1874), an associate member of the Statistical Society of Paris, and later would become an honorary member of the American Statistical Association (Royal Statistical Society, 1919).

This compilation of Kiær's contributions to the international statistical community and the recognition he received for his efforts makes it clear that, by the time he introduced his idea of the representative method, he was a mainstay of the set of international statisticians. His high status is one of the factors that allowed him to pursue his advocacy over several meetings of the ISI. The authority he had acquired over the years was another element that protected him from being dismissed out of hand for promoting a "deviant" idea. The prestige attached to his persona made him a particularly credible and formidable advocate within the ISI.

4 | DEBATING THE "REPRESENTATIVE METHOD" AT THE ISI (1895–1903)

This section explores the dispute that took place over the representative method. It begins with a short description of the debate's timeline (1895–1903), then discusses the arguments on both sides of the quarrel. Finally, it assesses the intensity and evolution of the opposition to Kiær's idea.

4.1 | A brief historical sketch

Kiær's advocacy of the "representative method" began in 1895 at the 5th meeting of the ISI in Bern. Although he never failed to mention the topic during the intervening meetings, it was only debated three times: Bern, Budapest (1901), and Berlin (1903).

He submitted a report in French ahead of the Bern meeting, which was printed and distributed to the discussants (Kiær, 1896a). Eight discussants intervened following Kiær's remarks; only three expressed some interest in the idea and thought that it merited further study (IIS, 1896b). Kiær (1899a) described the opposition he encountered there as "sharp" (p. 2). Nevertheless, after a contentious discussion, the conference attendees voted, "by a very narrow margin," in favor of pursuing the discussion at the next session (IIS, 1896a, p. XCVII; Kiær, [1897] 1976, p. 53).

In St. Petersburg (1897), Kiær named the individuals who would be part of a new committee to explore the issue of representative investigations and answered the specific criticisms that had been leveled at his idea in Bern (Kiær, 1899b, pp. 182–183).

At the Christiania session (1899), the representative method was not listed as a topic of discussion. But in a memorandum on another issue, Kiær reminded readers that a committee to study it had been formed at the previous meeting; again, he named its members and stated that it would submit a report at the next session of the Institute (Kiær, 1900, p. 282).

The discussion restarted properly at Budapest in the fall of 1901—this time with eleven discussants, of which two had participated in the Bern debate. Kiær made an oral presentation but did not submit a written report (IIS, 1903b, pp. 66–70). Overall, the discussion was far less contentious than in Bern. In the end, the Institute endorsed *unanimously* the continued study of the representative method—a marked difference from what happened in Bern (IIS, 1903b, pp. 70–78).

The final debate on the representative method before the onset of the Great War took place at the Institute's Berlin meeting in September of 1903 (IIS, 1905). There were only four discussants, all of them new to the deliberations on the topic. There was no outright rejection; on the contrary, all of them supported the report submitted by Kiær and his corapporteur, Paul Mayet (1846–1920) of Berlin.[20] This lack of contentiousness is reflected in the final resolution adopted by the assembled participants. It stated:

> The [Demography and Methodology] section [of the ISI], considering that the correct application of the representative method can, in a certain number of cases, provide exact and detailed observations from which it is possible to generalize, under certain conditions, the results, recommends its application, as long as its attendant report specifies clearly the conditions under which the observed units have been selected (IIS, 1905, p. 133).

Kiær's years of advocacy seems to have paid off. The representative method was now accepted by the ISI's members and integrated into its epistemic culture. The second part of the resolution indicated that the representative method should remain on the agenda and that a report be presented at the next meeting of the Institute regarding novel applications of the method and the value of the statistical results obtained, thus endorsing Kiær's proposal.[21]

4.2 | The debate

In Bern, Kiær argued that in the history of science some ideas had become so dominant that they had obscured other insights which turned out to be of "great importance." The "law of large numbers," in his view, was one of those ideas and it has prevented the "representative method" from receiving the attention it deserves. He went on to say that the latter was particularly well suited when investigating "social issues," whose complexity necessitates the collection of detailed information, which makes it impractical to carry them out on an entire population. Consequently, one should forgo the strict requirements of the law of large numbers (i.e., the census) and adopt "representative enumerations." To illustrate what had to be done to ensure that a representative inquiry yielded a "miniature" replica of the entire society, Kiær discussed two recent studies done in Norway. In conclusion, he expressed his belief that the representative method could have a great influence on the development of statistics and should be studied and discussed by the members of the Institute (Kiær, 1896a, pp. 176–177, 183).

In any community, there are social forces that promote and maintain the cohesion of the group based on socially acceptable behavior and beliefs (Emerson, 1954, p. 688). These are taken for granted and rarely made explicit when everyday life (lay or professional) is routine, but they will be reasserted forcefully when some "deviate" idea appears to question the group's daily practice (and beliefs). In addition, communities that are characterized by their specialized knowledge will find it difficult to incorporate novelties that upend their professional procedures, which have been instilled in practitioners by years of training (Hagstrom, 1965, p. 12). During the time the representative method was debated at the ISI (1895–1903), it was variously disparaged by opponents ("very dangerous", "expedient", and obsolete) (IIS, 1896b, pp. XCIV, XCV; IIS, 1903b, p. 73). Because the information it provided could only represent itself and not the whole, its enemies believed, it was incapable of yielding a "true statistic." One discussant in Bern dismissed it altogether: its very principle was so extremely contrary to the "statistical method" (i.e., complete enumeration) that it should be given no consideration (IIS, 1896b, p. XCV). One

practice that raised the hackles of the orthodoxy was estimation. Mayr expressed his displeasure regarding the current trend among some mathematically inclined statisticians to compute rather than observe. He stated most emphatically that calculation could never replace the "observation of facts." This led him to exclaim in Bern: "We must stand firm and say: no computation when observation can be done" (IIS, 1896b, p. XCIV). Indeed, one discussant, a supporter of Kiær's, who spoke before Mayr, reminded the assembled statisticians that another "calculating" method was already being used: interpolation (IIS, 1896b, p. XCIII). It was a technique developed and promoted by the Danish statistician Harald Westergaard (Westergaard, 1896).

The representative method was assessed on the basis of the norm in place (complete enumeration). As a deviant idea, it represented in the eyes of its opponents an existential threat to the census, the only true expression of "serious statistics" (IIS, 1896b, p. XCV). Another Bern discussant, the Frenchman Emile Cheysson (1836–1910), feared that all the negative comments hurled at the representative method might cast aspersions on monography, which, as mentioned before (Section 3.1), was accepted as a legitimate method, although distinct from, yet complementary to complete enumeration.[22]

To claim that an idea is not deviant is one thing, to convince one's (hostile) audience is another. There are, we are told, three forms of social control when dealing with deviance: apostasy (one renounces one's "wrongful" belief), ostracism (one's idea is banished from, or at least, marginalized by the community), and conversion (the community accepts the "deviate" idea).[23] Kiær's goal, of course, was the latter. A statistician of his stature, who was making valuable contributions in other areas in which the ISI was involved, was not about to give in to the opposition. Nevertheless, there was a distinct possibility that his proposal could be turned down, thus putting an end to the debate. However, this form of banishment would have violated the code of tolerance that founders like Rawson and Levasseur had insisted should prevail at the ISI.

So how did Kiær reply to his opponents? His strategy was to demonstrate that the representative method was not a threat to the prevailing cognitive norms espoused by the ISI. To implement his plan, Kiær took a two-pronged approach.

Kiær appears to have been perfectly aware that the idea of the representative method would upset the sensitivities shaped by the ISI's established beliefs and practices. He anticipated that many of his fellow members would find his idea not only incompatible with the dominant methodological outlook but would be perceived as an attempt to supersede it. Thus, he footnoted the title of the report he presented in Bern with the following proleptic statement:

> In order to avoid misunderstandings that could arise from the use of "enumeration," it will perhaps not be superfluous to point out that this expression is not used here in the meaning of census or ordinary enumeration, and that *the author of this memorandum is very far from saying that a representative enumeration could replace a general census of the population*. We are dealing here with enumerations of a very special type, which provide detailed data that could not be obtained from a general census (1896a, p. 176) [Emphasis added].

This was also his way of proclaiming his conformity to the belief that complete enumeration was the "gold standard" of statistics and to reassure his colleagues that the representative method was not usurping the census's primacy. He would reiterate his obedience to the prevailing orthodoxy in his introductory remarks before the start of the debate in Bern. "It goes without saying," he stated, "that the goal of representative partial enumerations is not to replace general censuses" (IIS, 1896b, p. XCIII). He would repeat this, in one form or another, throughout the years of his advocacy (IIS, 1899, p. 183; Kiær, [1897] 1976, p. 54; Kiær, 1899a, p. 16).

The second prong of his rhetorical stratagem was to assimilate the representative method to another partial investigation that enjoyed the ISI's endorsement: monography, or as Kiær called it "the typical method" ([1897] 1976, p. 53). He did not use this line of argument in the report he submitted to the Bern meeting, but he relied upon it in his introductory remarks before the debate. What triggered this? It was, I venture to guess, something that was

said four days earlier at the conference by Luigi Bodio while making a few short remarks on "statistical monography." He reminded his colleagues that this study methodology is the complement of complete enumeration: the latter can only provide the general outlines of social phenomena; the former yields all the details of "average or typical" cases (e.g., typical families, typical workshops, *etc*.). We saw earlier (Section 3.1) that Bodio defined monography as "the blood, the flesh, the nerves" of the skeleton built by the census (IIS, 1896a, pp. XXXVIII–XXXIX).

Kiær appropriated this anthropomorphic imagery to defend and promote his idea. In his opening remarks in Bern, before discussants had a chance to speak, he stated that the goal of the representative method, just like monography, was to provide the "skeleton" of complete enumeration with "the blood and the flesh" it lacked (IIS, 1896b, p. XCIII). He argued that since monography was recognized as a legitimate methodology so should the representative method—all the more so given that it is related to complete enumeration to a far greater extent than is monography (Kiær, [1897] 1976, p. 53). In other words, the relationship of the representative method to complete enumeration is no different than that which exists between the latter and monography: it is a complement not a replacement. This parity in legitimacy between the two methodologies was restated several times (e.g., Kiær, 1899b, p. 183). He insisted, though, that the two were quite distinct. For example, in St. Petersburg (1897), after acknowledging the usefulness of monographic studies, he pointed out their disadvantages, namely, they do not provide a true miniature of the whole because they fail to account for the variety of cases. In contrast, a good representative method neglects neither types nor variation. Some say, he noted, that representative investigations represent only themselves but not the whole. But in fact, this only applies to studies that are *not* representative (i.e., typological). In contrast, representative enumerations are like a photograph that reproduces, proportionately, the details of the whole (Kiær, 1899b, pp. 181–182).

How did the opposition to the representative method evolve and, in time, collapse? In Bern, of the five discussants who opposed Kiær's idea, only two were unconditional in their disapproval. Dr. Heinrich Rauchberg (1860–1938) stated flatly that this issue did not merit any discussion.[24] The other fundamentalist, Wilhelm Edmund Milliet (1857–1931), told his colleagues that "serious statistics" will never assign any importance to it, therefore it should be given no consideration (IIS, 1896b, p. XCV).[25]

Although his bark may have been the loudest, Mayr conceded that representative enumerations might have a "special value," but only if they are fully "enlightened" by "statistical observation" (i.e., complete enumeration). They could be useful, he added, for some limited legislative or administrative purpose. Bodio, who also believed it would be "dangerous" to recommend Kiær's idea, acknowledged, nevertheless, that there could be situations when an administration had to rely solely on approximate assessments, such as the case regarding the number of letters handled by the postal service (IIS, 1896b, p. XCIV).

This slew of oppositional voices in Bern, which verged on the uncompromising, was derided by Dr. Gustav Schmoller (1838–1917).[26] He referred to Kiær's opponents as "fanatics of statistics." The first discussant, Louis Guillaume (M.D., Director of the Swiss Federal Bureau of Statistics, 1833–1924), who supported Kiær, had made a similar appeal. He had urged his colleagues not to be "intransigent doctrinaires" and to examine without prejudice the proposals presented by Kiær (IIS, 1896b, p. XCIV). Schmoller, for his part, asserted, quoting Hesiod's *Works and Days*, that only "fools" do not realize that often one half is more valuable than the whole (IIS, 1896b, p. XCVI).[27] It would seem that his intervention resulted in shaming Mayr into adopting a more conciliatory position. Indeed, he stated: "since, it would appear, that the age of Hesiod is still with us, it is desirable that [the representative method] be thoroughly discussed in the next session [of the Institute]" (IIS, 1896b, p. XCVI)—thereby reaffirming, what I have called, Levasseur's rule.

In Budapest (1901), one of the two rejectionists was Edmond Nicolaï (1849–1927) of Belgium.[28] His argument against the representative method was somewhat novel in that he characterized it as an obsolete 18th century technology. The 19th century, he asserted, had ushered the era of the law of large numbers and hence of censuses. The Institute, he insisted, should further the development of these innovations and discourage the use of the representative method—to do otherwise would be to reverse the march of progress (IIS, 1903b, p. 73).[29]

What the Budapest meeting reveals is Kiær's influence on his colleagues: some of them (in Belgium, Denmark, and Switzerland) were willing to apply the representative method in their work—with mixed results, as we shall see shortly. After Bern, Kiær had had the opportunity to advocate his idea in the journal of one who called it "very dangerous." This could have had a dampening effect on the opposition. Besides, all along, Kiær placated his opponents by telling them that the representative method was not meant to replace complete enumeration, that he did not wish to advocate it without caveats, that it could not be recommended in all cases, that "great caution must be exercised in such studies," and so forth (IIS, 1896b, p. XCVI; IIS, 1903b, p. 77; Kiær, 1899a, p. 15; Kiær, 1899b, p. 183).

In Berlin (1903) only minor concerns were raised and, as we saw, the representative method was officially recommended by the Institute. Here, as in Budapest, Kiær was no longer in a position of having to convince his colleagues of the value of the representative method. The hurdles that prevented it from being considered a legitimate practice had been swept away. Now the discussion centered on its breadth (e.g., areas of human activity in which it could be used), on perfecting it, and on its proper application. One discussant's suggestion that the representative method be improved by use of the probability calculus was a clear indication that the technology was no longer a candidate for rejection but that the time had come for amplification and refinement (IIS, 1905, p. 130).[30]

Replicability can be seen as a form of social control in science and as the embodiment of institutionalized skepticism. It allows the scientific community to sideline potential deviant propositions or theories (and their promoters), and reward originality. When an innovation cannot be replicated, it is written off (temporarily or permanently) and often its author(s) is met with disdain and contempt and suffers a loss of prestige. In the case of the representative method, several of Kiær's Institute colleagues put his idea to the test. The results were mixed in Denmark, but mostly negative in Switzerland and Belgium. Such a situation could have dealt a serious blow to the legitimacy of the methodology, and it could have led to its rejection as an unacceptable topic of debate. But as we saw, nothing of the sort happened: the Institute ended up *recommending* the representative method. What made that possible?

Of course, Kiær rose to the defense. He asserted that these experiments had not been conducted properly.[31] In Budapest (1901), he discussed the studies undertaken by Marcus Rubin (1854–1923) of Denmark.[32] He reminded his audience that Rubin had obtained "good results" in his investigation of births and deaths but "erroneous" ones when investigating illegitimate births. He faulted Rubin for having selected too few districts, even though these were sizeable. He insisted that one would obtain a much better representation by choosing a large number of small entities (IIS, 1903b, p. 68). In Berlin (1903), he expressed the same criticism against Milliet's study on wages conducted in the canton of Bern and the investigation done in Belgium on the livelihood of retired workers reported by Armand Julin (1865–1953).[33] These surveys, he argued, all suffered from the same limitations: they failed to select a sufficient number of units spread out across the territory under study. He reiterated what he called the "fundamental principle" of the representative method: the larger the number of observed localities or units, the more one is likely to discover general characteristics rather than idiosyncrasies. This rule, he told them, should always be kept in mind whenever one wishes to implement a representative investigation or to assess one (IIS, 1905, p. 121).

As the debate shows, the representative method went from being considered as a subversive idea, because some at first feared it would displace the census, to being recognized as a complementary tool to complete enumeration, and therefore legitimate, just like monography. Even those who reported some unsuccessful results did not reject it outright (IIS, 1903b, pp. 73, 76). Apparently, Kiær had convinced enough of his colleagues that the supremacy of the census was not under threat. The representative method could therefore be integrated into the field of statistics of that era.

5 | CONCLUSION AND DISCUSSION

In the history of the sciences, there have been many instances of ideas that were rejected when first presented, only to be later accepted and incorporated into (or become) the mainstream of scientific beliefs (e.g., Wegener and continental drift) (Frankel, 1987). The representative method is one of these instances. This interpretive essay has

attempted to identify elements that made it possible for the representative method to progress through the meetings of the ISI from a shaky start (a "very dangerous" idea, 1895) to being embraced (1903) in <10 years.[34]

Kiær never claimed to be the originator of this methodology, nor did he provide its intellectual pedigree, but he knew it had been abandoned (Kiær, 1896a, p. 176).[35] Quetelet, one of the 19th century's preeminent statisticians and the ISI's "grandfather" (Nixon, 1960, p. 6), had turned his back on the representative method, in its Laplacian version, convinced by a colleague's arguments that it was unreliable. The man in question, the Baron de Keverberg (1768–1841), characterized Laplace's approach as "incomplete and speculative knowledge." He ended his rejection of this methodology by stating: "In my opinion there is only one way of attaining exact knowledge of the population and the elements of which it is composed, and that is an actual and complete census" (in Stigler, 1986, pp. 164–165). Written in 1827, it summarizes nicely the mindset of statisticians who, during that century, were tasked to evaluate the size of national populations and their characteristics. It is a statement to which the ISI members would have subscribed unreservedly. Indeed, when Kiær "revived" it and presented it to the Institute, some saw it as obsolete, rightly discarded earlier in the century and replaced by a state-of-the-art technology: the census. This data collection methodology had a long lineage in Western history. What the 19th century had brought to it was the establishment of a permanent state bureaucracy to implement it at regular intervals. It became the primary norm of practice around which members of the ISI were united. In many ways, the census was their *raison d'être* as statisticians. After all, the state bureaucratic apparatus to which they belonged had been built for the census. In Kiær's view, the exclusive reliance on complete enumeration had happened at the expense of the representative method. This was the "predominant" belief system he had to overcome. He did so by pointing out that the census had nothing to fear from the representative method, in fact, the latter needed the former to be deployed. He argued also that the ISI already embraced a form of partial investigation: monography. He merely asked that the same recognition be extended to representative enumerations.

It might seem strange to many today that, not too long ago, *human population* sampling could have been considered a controversial idea, especially since sampling, in one form or another, had been around for millennia (Rabinovitch, 1969, pp. 440–441). Yet, a few decades after being "recommended" by the ISI at the beginning of the 20th, the sample survey (as the representative method came to be known) would become the primary data collection tool not only of governmental agencies, but also of social scientists who dreamt of a science of society fashioned after the natural sciences, that is, a science that would rest on a solid quantitative foundation. R. A. Fisher, a leading light in the field of 20th century statistics, claimed that it was "more scientific" than complete enumeration (Fisher, 1950, p. 207)—in Kiær's time, ISI members would have been profoundly shocked by such a pronouncement.[36] One prominent American social scientist would proclaim, in the 1940s, that the sample survey was a "scientific tool" that had become "one of the most valuable and powerful research instruments of the social sciences" (Likert, 1948, p. 341). This sentiment was widely shared among American social scientists and policy makers (Allport, 1945, p. 114; Blankenship, 1940, p. 110; Meier & Saunders, 1949, p. 217; U.S. Senate, 1946a, p. 80; U.S. Senate, 1946b, p. 467). In America, starting in the 1930s, the sample survey and particularly one of its derivatives, the opinion poll, would become a permanent fixture in people's daily life (Igo, 2007). Unbeknownst to many, including social scientists and policy makers, Kiær had been the original catalyst.

Aside from complete enumeration, another element that cemented a sense of solidarity in this particular cohort of ISI members, especially the older ones among them, was a shared collective institutional memory of the "traumatic" experience of the ISC's demise. This led many of them to be committed to ensure that the ISI should enjoy a long life. As a result, they were particularly attentive to any incident (such as a "deviate" idea) that might break up the Institute's unity and jeopardize its longevity. Hence the strong emphasis on tolerance in debates and the encouragement to examine and study any topic introduced by a member (Levasseur's rule). To have rebuked Kiær at the very first presentation of his idea (Bern, 1895) would have amounted to a serious loss of face for the contributor and a grave sign of disrespect towards a valued member on the part of his colleagues. It appears that the majority of attendees were not prepared to take that step.[37]

But not everybody was of that frame of mind: for some the representative method was too egregious of a violation of prevailing standards and, therefore, it was more important to uphold what they believed was the statistician's creed (i.e., only a census can yield a "true" statistic), than to comply with the niceties suggested by Levasseur's rule regarding how one should welcome a fellow member's ideas. If this attitude had prevailed, Kiær's proposal would have been abandoned and human population sampling would have had to seek another avenue to acceptance or bided its time.

As we have seen the opposition was composed of fundamentalists, on the one hand, who rejected the representative method utterly and completely (e.g., Rauchberg), and, on the other, of those (like Mayr and Bodio) who accepted that it could be used in exceptional circumstances (mostly of a non-demographic nature: see Mayr's and Bodio's statements reported in Section 4.2). But, as opponents feared, the methodology had a far more ambitious goal than simply being "the blood and the flesh" of the complete enumeration. It rivaled the census: it was doing what the census was supposed to do but could not. Rejectionists of all stripes (Mayr et al.) could not accept that the representative method could do the same job as the census (IIS, 1903b, p. 77).[38]

Kiær (1896a) told his colleagues more than once that the representative method was particularly well suited to study "social issues" (p. 176), which require the collection of detailed information, and thus makes it impractical and "prohibitive" to carry out such an investigation by census ([1897] 1976, p. 38). Kiær was not the only one who brought up the issue of cost. One of his supporters, the Swiss Guillaume, a discussant at Bern (1895), reminded his fellow statisticians, referring to an article written by Harald Westergaard, how much the cost of statistical work (i.e., censuses) was on the increase in all countries (IIS, 1896b, pp. XCIII–XCIV). So did the U.S. Labor Commissioner, Carroll D. Wright (1840–1909), in a letter to Kiær, which the Norwegian quoted at the Budapest session.[39] Wright emphasized the great advantage that could be derived from the representative method in view of the "vast cost" of conducting censuses (IIS, 1903b, p. 70).

Of these two themes, the need for more detailed demographic information, on the one hand, and expense, on the other, the former is a clear indicator of a welfare state in the making. Indeed, Kiær was living in the era of the emergence of this new form of social control. Germany had started (1871) putting in place social insurance policies that covered illness, work accidents, disability, and old age pensions (Braun, 1983). Norway was about to do the same by exploring the feasibility of creating an insurance fund for retirement and disability (Kiær, 1896a, p. 177). To obtain the necessary information (e.g., the number of disabled in the country), Kiær relied on the representative method (Lie, 2002). Thus, the rise of the welfare state with informational needs the census could not fulfill would become in time a catalyst for the wide adoption of the representative method.

The acceptance of the representative method at the ISI resulted from a convergence of elements. It was rejected because it did not fit into the prevailing paradigm—a typical occurrence for new ideas which are turned down when first presented only to be embraced at a later date. Such an incident is common in highly cohesive groups. It has been shown that cohesiveness gives rise to competing forces: on the other hand, it is an unwelcoming environment to novelty because innovations are often seen as threats to the group's unity; on the other hand, the homophilous nature that often characterizes highly cohesive groups promotes effective channels of communication (Rogers, 2003, pp. 305–306). In this case, the latter prevailed over the former because it benefitted from four crucial ingredients: (1) the ISI's climate of intellectual tolerance, which allowed the topic to remain on the ISI's conference agenda and thus gave Kiær the opportunity to sweep away the criticisms (including mixed replication results) leveled at the representative method; (2) the ISI's small size and "horizontal structure"; (3) the institutional memory of the ISC's demise; and finally, (4) Kiær's professional standing. In the end (1903), the ISI was able to reach what one researcher would call an "epistemological pluralism" that allowed different methodologies (i.e., the census, monography, and the representative method) to coexist in harmony (Smolka, 2021, p. 20). Two decades later, the *bona fide* status of the representative method was reaffirmed beyond the shadow of a doubt at the ISI's XVth meeting in Rome (1925): "nowadays there is hardly one statistician, who in principle will contest the legitimacy of the representative method" (IIS, 1926, p. 59).[40]

ACKNOWLEDGMENTS

I wish to dedicate this paper to the memory of my good friend, Phillip ("Phil") R. Berg (1936–2021), he too of Norwegian stock: a man who personified generosity and dignity. I thank the anonymous reviewers and the editor of the *Journal* for their perceptive and very helpful comments. I also would like to thank Ms. Alice Renhoff for helping me to read through German texts I encountered in my research. A very early version of this paper was read before the American Statistical Association's History of Statistics Interest Group at the 2019 Joint Statistical Meetings in Denver, CO.

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ENDNOTES

[1]Before Kiær, the representative method had never been discussed at the ISI (Kiær, [1897] 1976, p. 51; Jensen, 1926, p. 359).

[2]Pinch (2015) is a useful overview of various approaches in the subfield of scientific controversy.

[3]For cold fusion see Gieryn (1999, pp. 183–232); Volta: Pancaldi (2003, p. 111); the calculus: Merton and Lewis (1971, pp. 153–154); fraud: Gould (1981, pp. 234–320); Pasteur-Pouchet: Raynaud (2018, pp. 80–115); phrenology: Gieryn (1983, pp. 787–789); Chuprov-Neyman: Pearson (1927).

[4]On these issues, in a nonscientific context (see Gusfield, 1963, ch. 3, 4).

[5]On the theme that scientific criteria are not always at work when it comes to publishing a contribution in a journal (see Crane, 1967).

[6]Mayr, an honorary member of the ISI, was both a state bureaucrat and an academic.

[7]Although this occurrence may be considered remarkable, it is not unheard of. For a relatively recent example in physics (gravitational radiation), see Collins (2000, pp. 828–829) in which an idea, considered wrong, is still allowed to be published.

[8]I use paradigm to mean the core scientific beliefs that inform a community's daily professional practice.

[9]The monographic method consisted in selecting one or more elements of a population of interest (e.g., working-class families) that were considered "average" or "typical" and perform an in-depth study of all aspects of the family's life: its living accommodations, its furniture, its clothing, its budget, and so forth (e.g., Cheysson & Toqué, 1890, pp. 1–5).

[10]The mission the ISI set for itself was to promote the "progress of administrative and scientific statistics"; and, among other things, to find methods that allow cross-country comparisons; "to promote and foster the general appreciation of statistical science and to stimulate the interest of governments and individuals in the study of social phenomena" (IIS, 1887b, p. 1).

[11]The ISI made it clear that its resolutions would be nonbinding to either members or governments (Neumann-Spallart, 1886, p. 28)—a particularly touchy issue with the Germans. After the London meeting, when German statisticians learned that they had been inducted into the new association, two of them sent a letter to Rawson, the president of the ISI. They were concerned that the ISI would be a repeat of the ISC. Rawson was able to reassure them by demonstrating that the ISI was nothing like the ISC (Neumann-Spallart, 1886, pp. 30–34).

[12]This number was increased to 200 in 1901 at the Budapest meeting (IIS, 1903a, p. 256).

[13]For the period under study, nearly four-fifths of the membership was 50 or older (Depoid, 1964, p. 78).

[14]Another indicator of the equalitarian quality of the ISI, in those days, is the complete absence of awards, prizes, or honors. The Royal Statistical Society, for example, established the Howard Medal in 1873 and the Guy Medal in 1891 (Hill, 1984, p. 138). Usually, an honorific reward is conferred by some superordinate entity to a subordinate one. Perhaps members at the Institute, including those in office, did not see a way to define this subordinate entity.

[15]If attrition (voluntary) is a measure of cohesiveness and <2% (1887–1903) is considered a low rate then this would be another indication of the cohesion that reigned at the ISI (IIS, 1932, p. 232).

[16]Merton tells us that "the institution of science makes scepticism a virtue" (1938, p. 334).

[17]For a different view see Hagstrom (1965) who characterizes scientific contributions as "gifts" (p. 12).

[18]The same sentiment is expressed by another member (Neymarck, 1901, p. 377). Baines was the Royal Statistical Society's Honorary Foreign Secretary and a member of the Institute since 1897.

[19]Indeed, Baines credited the ISI's "admirable *personnel*" for "reducing" "friction" to a "minimum" (Baines, 1904, p. 493). [Emphasis in the original].

[20]Mayet was a discussant in Budapest (IIS, 1903b, p. 74). The fact that Kiær had now a corapporteur can be seen as progress for his cause—he was no longer the lone advocate of the representative method.

[21]The report would not materialize until after WWI at the Rome meeting in 1925. By then Kiær had died (1919).

[22]Cheysson was a devoted disciple and collaborator of his compatriot Frédéric Le Play (1806–1882), the "father" of the monograph.

[23]On this issue see Emerson (1954, p. 688).

[24]Rauchberg, a member of Vienna's Central Commission of Statistics and an academic, proposed that the representative method be struck off the ISI's agenda. His recommendation was narrowly defeated (Kiær, [1897] 1976, pp. 53–54).

[25]But he was open-minded enough to give the method a try, as we shall see. Milliet was the general director of the Federal Bureau of Alcohol of Switzerland in Bern.

[26]Schmoller, an academic (University of Berlin, Academy of Sciences), a bureaucrat (State Council, Kingdom of Prussia), and a founder of the influential society the Verein für Sozialpolitik (Association for social policy), was the leading figure of the German Historical School (Gorges, 1991, p. 318 and passim) and had an international reputation that went beyond the confines of the old continent (Veblen, 1901).

[27]"Fools all! who never learned how much better than the whole the half is…" (Lattimore, 1959, p. 23).

[28]The other opponent was the Russian Nicholas Troïnitsky (1842–1913) who expressed the opinion that the representative method could not possibly be used in a country as vast and diverse as his (IIS, 1903b, pp. 72–73).

[29]Nicolaï (Department of general statistics, Ministry of the Interior and Public Education) was a member of the Institute since 1889 and had attended the Bern meeting. Given the fact that the vote in Budapest is reported to have been unanimous, I assume that even he voted in favor of the continuing study of the method.

[30]The discussant in question was the Frenchman Lucien March (1859–1933) (IIS, 1905, p. 129). In Budapest (1901), Dr. Ladislaus von Bortkiewicz (1868-1931), a professor at the University of Berlin, had already recommended the use of the probability calculus. His focus was on assessing the representativeness of the sample. In his view, there was only one way to make this determination and that is by using probability (IIS, 1903b, pp. 71–72).

[31]As one anonymous reviewer of this paper remarked, this is a very typical reaction in the scientific community. For an example, see Pinch (1994) and p. 93 more specifically.

[32]Rubin was the head of the Denmark's National Bureau of Statistics.

[33]For Milliet's (an opponent in Bern) study see (IIS, 1903b, p. 73). For Julin see (IIS, 1903b, pp. 74–76); he was a member of the Institute since 1895 and headed Belgium's Department of Labor Statistics at the Ministry of Industry and Labor.

[34]One major limitation of this paper is the fact that there are some gaping holes in the historical record. It would have been nice to find out how Mayr came to "allow" a paper on the representative method to be published in his journal. Perhaps an exchange of letters between Kiær and Mayr exists. I do not know. Many questions remain open regarding Kiær and his advocacy of the representative method. It will take Scandinavian scholars to throw light upon this story due, in large part, to the language barrier. Access to Kiær's professional correspondence (not all in Norwegian, I am sure) would be a good start. Another missing piece is Kiær's intellectual journey: how did he become a believer in the representative method. Lie (2002, p. 390) states that Kiær studied in Paris, but gives no reference. When there, did Kiær hear of Laplace's attempts? As far as I know, there is no comprehensive biography of Kiær—certainly not in English.

[35]It had several starts in the 17th and 18th centuries. For the 17th century, there is John Graunt's (1620–1674) estimate of the population of London (1662) (Hacking, 1975, p. 107).

[36]This statement was made at the 26th meeting of the ISI in 1949 (Bern). It shows how fundamentally statisticians' cognitive frame had changed.

[37]One member, Alfred Neymarck (1848–1921) of France, characterized the meeting at Bern as one displaying an increase sense of solidarity among ISI members (Neymarck, 1896, p. 54).

[38]This was eloquently expressed by Bodio at the Bern meeting: "no inductive procedure, no numbers manipulation (…) can replace the effective and true implementation of a population census" (IIS, 1896a, p. XCIV).

[39]Wright was a member of the committee for the study of the representative method, but he never attended any of the meetings during which it was debated (IIS, 1903a, p. 270).

[40]The Rome meeting was the second after the end of WWI. The war resulted in a ten-year hiatus in the biennial schedule of the Institute's meetings: the last prewar gathering was in Vienna (1913), and the first one, postwar, took place in Brussels (1923).

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How to cite this article: Lusinchi, D. (2022). Kiær and the rebirth of the representative method: A case-study in controversy management at the International Statistical Institute (1895–1903). *Journal of the History of the Behavioral Sciences*, 58, 163–182. https://doi.org/10.1002/jhbs.22145