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Gini's criticisms to the theory of inference: a missed opportunity

Summary - This paper deals with the criticims that Corrado Gini addressed to the so called Anglo-Saxon theories of inference. Such criticisms were based on a Bayesian argument but Gini's view of probability was not general enough to support Bayesian analysis in general. Therefore while the destructive part of the contributions by Gini became very popular in Italy, at least for some decades, the constructive hints hidden in many of his papers, as far as were useful only in a Bayesian approach, were overlooked.

Key Words - Gini's criticisms to inference; Significance theory; Bayes methods.

1. Introduction

Corrado Gini (1884-1965) was surely the most influent statistician in Italy before and after the Second World War. His researches in statistical methodology (actually only one of his many scientific interests) and in particular about concentration and association between variates, gave him a well deserved renown. His contributions in these fields are largely agreed and stimulate even now further studies. See Regazzini (1998) and Forcina and Giorgi (2005) for excellent general reviews and Giorgi (1990), Cifarelli *et al*. (1996), Conti (1993), Montanari and Monari (2008), the special issue of *Metron* (2006) and the present special issue for some of the modern developments. Moreover, his chair at the University of Rome since 1923 assured him, in Italy, together with his prestige, a relevant academic power. About the enormous importance of Gini in the Italian statistical environment see the synthetic portrait of Giorgi (2001) and the well documented book by Prévost (2009).

At the first meeting of the newborn Italian Society of Statistics, in 1939, Gini presented a celebrated relation where he was strongly critical with respect to the theory of significance of Fisher. He advanced similar criticisms later on many other times in various papers and relations to the Italian Society of Statistics, involving also Neyman and Pearson approach, confidence limits and so on. Specific comments on these meetings are given by Frosini (1989). This attitude had a great impact on the Italian statistical environment. For many years, after the war, the study of statistical inference was almost neglected by most of the Italian scholars, since its foundational bases were so severely refused by a recognized authority. It was even suggested (Herzel and Leti, 1977) that this attitude could explain the remarkable success and interest soon reached in Italy by the modern exploratory statistical methodology, which in a sense can be seen as an alternative to classical statistical inference.

The criticisms by Gini were inspired to a Bayesian viewpoint, more in the tradition of classical authors as J. Bernoulli, P. S. Laplace and Karl Pearson than in relation to neo-Bayesian versions, as suggested in particular by de Finetti. Note that Gini knew personally very well de Finetti but apparently he did not paid real attention to de Finetti's approach to the problem. What prevented Gini from proposing a general application of Bayesian methods was his restricted concept of probability. We will come back to the concept of probability according to Gini in Section 4. It is fairly clear that without a notion of probability flexible enough in order to be assigned to any event, including statistical hypotheses, there is no possibility of successfully proposing a Bayesian approach as a general one. Gini himself (1954) mentions an objection raised by Harold Hotelling during an international meeting in Washington in 1947; Hotelling said that, since Americans are followers of William James, they are all pragmatist and need a theory which can be put in practice. I think that this reaction is almost unavoidable, but a careful reading of Gini's papers allows one to find many suggestions that could have helped in a more constructive vein. Therefore in this paper we will focus not only on the Gini's criticisms of the current theories of inference but also on the many constructive hints which had no direct sequel but which anticipated topics which would became popular in Bayesian statistics, for instance conjugate classes, robustness, principle of precise measurement, predictive approach, conditioning on data and so on. In this sense we speak of a missed opportunity, mainly by Italian statisticians. The radical refuse of inferential theories, made by many statisticians, was not in our opinion the most sensible decision to be made, even after taken in serious consideration Gini's criticisms.

Among many papers discussing Gini's work we mention here also Chiandotto (1978), Forcina (1982 and 1987) and Frosini (2005); commemorative papers published just after Gini's death are Castellano (1965) and Boldrini (1966).

2. Starting criticisms: the use of the probable error

The long paper (Gini,1939) mentioned above is famous for his violent attack to Fisher's theory, but some more general considerations at its beginning remained somewhat neglected. They are however interesting, both in itself and because of the attitude of Gini in presenting them. He explains that after many doubful reflections on the topic he decided not to comply with the great majority of statisticians who deem always necessary to associate any constant calculated from the data with its probable error or its standard deviation (the probable error, now rarely used, is the semi-interquartile range and, for a normal distribution, amounts to 0.67 times the standard deviation). Even the term "constant" for a statistic belongs to a classical tradition (Aldrich, 2003) foregoing the linguistic and conceptual innovations by Fisher (Fisher, 1922). If a statistic is seen as a constant, it could be difficult understand which variability the standard deviation should measure. This is only an issue of terminology, since it is clear, also in the comments of Gini, that the "sample constant" plays an instrumental role in estimating the corresponding "mass (population) constant". However, speaking of a variable whose realization is the statistic at hand would have advised to speak also of its possible probability distribution, and the effect of this would have been a clarification of the whole setting. In particular Gini emphasizes that the computation of the probable error would usually require the knowledge of some constant of the mass, say σ, and this is only rarely available so that a sample estimate, say σ', must be used. The use of estimates, according to Gini, often entails systematic biases; this is exemplified by the case of the heights of the recruits were σ and σ' measure, in his evaluation of the specific phenomenon, different kinds of variability. Thus, the replacement of σ' to σ makes also useless, or even misleading, the reference to the probable error. In Gini's opinion this argument should show that in general the computation of probable errors is not suitable, as if it was not necessary (or possible) to take into account the accidental variability in examining observational data. My feeling is however that Gini is actually criticizing here a different aspect, namely that the sample under discussion is not a random sample of the mass (population). The general probabilistic framework does not receive much attention in the examples discussed by Gini; often, a normal or a binomial model is implicitly assumed, and the same for the basic assumptions of independent and identical distributions of the sample units. In this example the focus of criticism is probably just on these basic assumptions, and it is surely possible that Gini is right in the concrete example. But in pointing out the irrelevance of probable errors I find a touch of provocation, and a more thorough analysis of the statistical model in its entirety would have been in my opinion more suitable.

A point raised years later by Kendall (1957) is that the Italian School, then largely conforming to Gini's ideas, privileged the analysis in the field of demography, where large numbers were much more common that small numbers; Gini replied (1965b, p. 423) that also many other fields were covered by Italian statisticians, as for instance economy and anthropology, quoting a book of his (Gini, 1955b). Moreover, admitting that the Italian School did not pay much attention to sampling distributions of statistics, he remarked that such researches could be done in the future (Gini, 1962, p. 28). However it is true that fields where samples are typically small, and more easily conceived as random samples, as biology, clinical medicine, technology, agronomy, were rather uncommon in the researches of the Gini's school, and the scholars who were professionally engaged in those fields followed in the years lines entirely separate from Gini's prescriptions. For instance the same 1939 meeting of the Italian Society of Statistics, a scholar (Barbensi, 1939) presented a forthcoming book of his in biometry, belonging to the pure Fisher tradition notwithstanding the stormy climate. Other names, out of the "Italian School" could be mentioned for similar positions in the same period, for instance L. L. Cavalli Sforza and G. A. Maccacaro, whose initial academic training was in medicine. The relative lack of interest for the sampling characteristics of statistical measures by many Italian statisticians of that time does not take value to their researches if one considers the fields that were mostly object of interest but the bad consequence was that the commitment of a large part of professional statisticians in experimental areas was delayed of several years, and essentially involved a younger generation of statisticians.

3. The dangers of statistics

After the initial discussion about the problems connected with the evaluation of the probable error of a constant, *i.e.* of a statistic, Gini in the same 1939 paper remarks that such error is useful in order to determine the probability P that the accidental error which affects the statistics exceed a given threshold and formulates the problem: if such error exceeds the threshold, is there a probability $1-P$ that it not due to pure chance reasons? In his opinion, this is the current interpretation of the significance theory. Some quotations from the most authoritative literature are given, and in particular this passage in Fisher (1932) p. 45 where of course a normal model is assumed: "*Deviations exceeding twice the standard deviation are thus formally regarded as significant. Using this criterion, we would be led to follow up a false indication only once in 22 trials, even if the statistics are the only guide available*". Here Fisher's statement is somewhat misleading: it is true that we have a false indication once in 22 trials in the subset of trials which are ruled by chance only, but the proportion of this subset in the set of all the possible trials is the prior probability that chance only acts, and this quantity is not given. I can agree with Gini's remark that if sample statistics are the only guide available we are not able to identify the probability of being wrong.

To further explain this point, Gini dedicates much space to an elementary exposition of Bayes' formula. It may be strange that in scientific society this is deemed necessary, since the topic of "probability of causes" is traditional in the probabilistic literature, but maybe Gini took account of a certain heterogeneity in the public and in any case he preferred not to leave unexplained his main argument. The example with which Gini started is unusual in statistical literature: let us assume that 80% of the swindlers in Rome go at a given hour in a given coffee house; should you give probability 0.80 to the event that the first guy you meet in that coffee house at that hour is a swindler? If you assign a probability of 0.02 to the fact that a non swindler be in the coffee house at that hour and assume that the proportion of swindlers among the Romans is 0.0001, the classical Bayes formula provides a value of about 0.04 that the guy is a swindler, much less than 0.80. Thus a correct probabilistic argument rectifies the confusion between the probability that a swindler go to the coffee house and the probability that the guy you met is a swindler.

This is the kind of confusion run, according to Gini, by the statisticians who use the significance theory: the occurring of an event which is improbable under a given condition does not imply that the condition is improbable given the observation. In formulas (with Gini's symbology), if P_a is the probability that an accidental error has a given intensity and P_s is the probability that a systematic error has the same intensity, then the probability π_a that the error is accidental is given by

$$\pi_a = \frac{P_a}{P_a + f \cdot P_b} \tag{1}$$

where f are the prior odds of a systematic error. Several other examples are also discussed, where the quantity f is different and sometimes can be easily estimated. For instance in the popular game of *Lotto*, where 5 balls are extracted without replacement by an urn containing 90 balls numbered from 1 to 90, after paid the ticket, one wins if was able to predict two or more results. The probability of an exact prediction of 5 numbers is of course very small: is there a reason for accusing the winner of fraud? Of course not because here f, the prior odds of a fraud, is zero (a more recent criminal history suggests that Gini was too optimistic, frauds are difficult but not impossible). In a different occasion, a few years later, Gini approached also in a Bayesian framework a topic which would became a classic in Bayesian literature, that is medical diagnosis (see Gini,1953).

The overall conclusion of this discussion is drastic: *we are not able to measure the reliability of a statistical result or of a difference of two statistical results* (Gini, 1939, p. 204, my translation).

Two other minor criticisms by Gini are worth to be mentioned. When an observed result, t, say, is measured on a continuous scale Gini remarks that the probability of the singular result is infinitesimal, and instead of considering the event $T = t$ it is usual to consider events of the kind $T \geqslant t$ (Gini does not introduce the random variable T, and prefers a cumbersome symbology that we avoid). This entails a modification of the data to be considered, since observing $T = t$ is not the same as observing $T \geqslant t$. The concept of likelihood appeared for the first time just on *Metron* (Fisher, 1921) and was more diffusely explained later (Fisher, 1922) but unfortunately Gini does not enter in deepenings and leaves the criticism at a superficial level. Notwithstanding Fisher's achievements, it was probably too early, in 1939, for discussing the need of conditioning on observed data; all this reminds however the extensive Bayesian literature, started by Berger (1986), showing that, under suitable conditions, P-values strongly underevaluate the null hypothesis just because they elaborate a result which is artificially pessimistic about its validity. The use of tails instead of singular points, even in the discrete case, is justified in more modern expositions of the theory of significance (see *e.g.* Anscombe (1963), Cox and Hinkley (1974)) on entirely different bases, but this is out of our scope.

Another important point touched by Gini in the same paper, though not in an helpful way, is the effect on the posterior probability of the dimension of the sample, whether it allows or not an approximation of the prior with an uniform distribution. Using theoretical arguments he says that nothing can be said in general. Elsewhere (Gini,1943a) he showed appreciation for a research of Bowley (1923) who tried to give conditions to justify such approximation. In that case, though admitting that the influence of the prior distribution decreases as the sample size increases, Gini criticized specific theoretical assumptions, but did not catch the main point, namely that the prior probabilities, in a Bayesian analysis, are essential from a conceptual viewpoint, although a precise elicitation is not always relevant from a practical viewpoint. Approximating the prior with an uniform distribution when the sample is large, as proposed by Czuber, is an *odd idea* (Gini, 1943a, p. 381). Gini reminds in the same paper that the topic received already attention by several authors, including Edgeworth and von Mises, but in his opinion the results were not convincing enough. The principle of precise measurement will wait again some years to be enunciated (Edwards, Lindman and Savage, 1963). It is known however how important was the possibility of avoiding precise prior elicitation at least in the first stage of the modern Bayesian literature, as exemplified by a book by Lindley (1965).

The title of the paper mentions "the dangers of statistics". Gini explains in detail in the final part that the dangers of statistics are due to an incorrect use of probability theory. More precisely, taking up a previous paper (1907), Gini distinguishes the *inductive applications* of the probability, where, substantially, one tries to estimate probabilities from frequencies (and Fisher's theory is the major example) from the *deductive applications* of the probability, where one tries to predict frequencies from probabilities. This latter chapter of probability is approved and its utility for statistics is stated, for example in what concerns the prior determination of optimal size of samples.

In the final part of the paper Gini, somewhat joking, confesses that he was uncertain about the title, the alternative being "The sins of statistics". He portrays Statistics as a nice girl, who risks to accept the advances of a bold boy, the Probability, and to lapse into sin. Times are changed since 1939, and now girls and boys would on the contrary be encouraged to pursue their personal sentimental education. In any case things developed in a quite different way from Gini's prescriptions and nowadays the links between statistical inference and probability are clearly indissoluble.

4. Probability

A clarification of the concept of probability according to Gini is relevant here because his idea to do inference through the use of Bayes theorem requires a definition of probability such that any uncertain event can have a probability. Otherwise, the procedure cannot be deemed as a general one, and there would be cases where inference can be made, and other cases where almost no strategy is available. Indeed, this was the problem with Gini's position.

Since the beginnings, Gini was interested in probability, and in various papers he showed a remarkable familiarity with the classics (1949a, 1949b). His basic paper goes back to 1908 (Gini,1908); as we shall see, a certain evolution in his attitude can be identified, but the bulk of his ideas in already well represented in his systematic 1908 paper. Gini proposes a modification to the definition he calls "empirical", which is substantially a frequentist definition: the probability of an event is the ratio between the number of occurrences and the number of observations, provided that the number of observations is large enough. Gini remarks that the ratio is meaningful only if the limitations of space and time are well defined. Therefore his main concept is that probability is a frequency of events in a given class of events (the so-called "collective"). While the determination of the collective is subjective, the probability is objective. The probability is never referred to a single event because without the reference to a well determined collective it would be meaningless. For instance, what is the probability that a son who is expected is male? If the collective is constituted by all the newborns in Italy the probability is 0.515; but in towns the probability becomes 0.512; if moreover the son will be a first born the probability will be 0.517, and so on. Thus it is clear that the probability of the singular event "that newborn will be a male" is undefined, and the choice of the collective is fundamental.

Note that the collective is finite. The possibility of infinite collectives is explicitly refused by Gini (1949a, p. 84) and in a later paper he goes further on by stating that *there is no proposition of probability calculus which cannot be formulated in terms of combinatorics* (Gini (1964) p. 272, my translation).

Commenting on a later but similar paper (Gini, 1949b), de Finetti (1966) remarks that in Gini's framework there is no reason to introduce the word "probability", since the concept is entirely brought back to that of "frequency". However de Finetti appreciates that the probability is related to concrete facts, and not an abstract and unverifiable object. In other terms, the sentence often claimed by de Finetti (*the probability does not exists* – see *e.g.* de Finetti (1976)) is in some sense accepted also in Gini's approach.

Gini's view on probability appear somewhat modified or at least extended in one of his last papers (Gini,1965a). In particular he admits also *open* collectives, that is collectives where at least a part of the events may occur in the future (p. 308). In such cases Gini retrieves the concept of probability as a limit of frequencies, coming very close to the usual modern frequentist approaches. Indeed Gini complains (p. 303 footnote) about the fact that Fréchet and von Mises, who elaborated on the concept of a collective associated to an event to be probabilized, neglected his own 1908 paper.

An unsatisfactory piece of the analysis by Gini is the treatment of subjective probability. In Gini (1965a, p. 306) he admits that opinions may be revealed by bets, but also claims that probabilities so elicitated would not obey the traditional laws of the calculus of probability. Gini neglects here a considerable amount of literature. For instance, a subjective approach is held by Bertrand (1907), in a book that Gini often quotes. In Gini (1943a, p. 279) Edgeworth and K. Pearson are criticized because they give the same probability to hypotheses such that "*they have no reason to evaluate one more probable than the other*" (my translation). It is also impossible that Gini ignored the contributions by de Finetti, and in particular his concept of *coherence* (de Finetti (1931), but quotations could be multiplicated), which guarantees that coherent subjective probabilities obey standard probability rules (a part the issue of complete additivity, not relevant here). Moreover, in the same 1965 paper, Gini mentions the book by Savage (1954) where the subjectivistic approach is set forth in great detail. This is, in my opinion, one of the points where there is some superficiality in Gini's arguments, as noted, for different aspects, by Forcina (1982, section 3). Another quite astonishing example is the quotation of Wald (1950) as if the determination of complete classes of decision functions through the use of formal probabilities – the class of all probability distributions, not a singular distribution to be chosen - was a criticism, and not a completion, of the Neyman and Pearson's approach (see Gini, 1955a, p. 182). A sound outline of Wald's contributions, including comments about the unintentional but suggestive privilege given to Bayes procedures, was easily available at that time (de Finetti, 1951).

5. Again on significance theory and confidence limits

In 1943, at the seventh meeting of the Italian Statistical Society, Gini comes back to the subject of the so-called Anglo-Saxon inferential theories (Gini, 1943a). He starts with a defense of Bayes theorem on the sole basis of the *Lotto* example, and then rejects the objection that his previous criticisms were caused simply by inadequate formulations and did not touch the basic argument. It is interesting that, commenting on an example in quality control, Gini (1943a, p. 361) discusses in detail the meaning of the prior probabilities, which are connected to the reliability of the firm responsible of the production. Nothing can be objected from a modern Bayesian viewpoint. On the contrary, we have here a good example of how to analyse prior information, but how could one translate in numerical terms such a reliability, on the sole basis of Gini's probability concept? Later in the same paper (p. 384) it is remarked that the procedure requires the knowledge of previous samples of the production of the same company, a condition which is remarkably restrictive.

Gini finds the roots of the confusion between the probability that some event occurred by chance and the probability that only chance rules in the classical paper where Karl Pearson introduced the chi-square statistic (Pearson, 1900). Indeed, in Pearson's paper it is explicitly said that the computations are made under the condition of random sampling, while the title, though in a cautious way ("*it can be reasonably supposed*") alludes to the possibility that the results "*have arisen from random sampling*". It is presumable that the text is founded on an informal Bayesian argument, where, however, prior probabilities of an accidental or a systematic cause are not stated, and it is assumed that an extremely small likelihood almost guarantees a small posterior probability. This confusion, according to Gini, characterizes Fisher's theory of significance and the same comment about Pearson is repeated several times (*e.g.* Gini 1955a, 1962 p. 59, 1945-46 and 1966 section XV.1.5).

Gini claims about some agreement of foreign statisticians with the arguments of his previous paper (Gini, 1939). He quotes a personal letter from Greenwood, and explains that even Fisher, in a private communication, recognized the validity of his arguments. There is no doubt that Fisher was fully aware of the differences between the significance argument and the use of Bayes formula, and in this limited sense he could have agreed with Gini. But Gini's criticisms did not even convince Fisher to change the sentence previously quoted (Section 3) from Fisher (1932). The sentence itself remained unchanged in the final edition of the book (see Fisher (1973), p. 44).

The communications with British and American statisticians were obviously interrupted by the second world war, so that Gini invited at the 1943 meeting of the Italian Statistical Society two German scholars (M. P. Geppert and H. von Schelling) as defenders of Fisher's theory. Their defense (Geppert, 1943 and von Schelling, 1943) was poor (for a good synthesis, see Forcina (1982)) and Gini, quite rudely, observed that while his criticisms had been taken with comprehension by the Anglo-Saxon statisticians, German statisticians, being new to the subject, were not yet sufficiently expert. A point in the reply to Geppert (Gini, 1943b) is worth to be commented on. After a personal and doubtful reconstruction of the significance procedure, Geppert shows that the probability of being wrong is just the P-level, say α. Gini, with an argument based on combinatorial considerations, gives a new proof of formula (1), presented here as the probability of being wrong when the null hypothesis is rejected. Gini does not stress another equivalent reading of formula (1), *i.e.* the fact that it is the probability of being wrong *conditionally* on a significant result (a direct probabilistic proof is trivial). The usefulness of this wording is that it exhibits the fundamental difference between the significance and the Bayesian approaches. The measures are in the first case *prior* measures and in the second case *posterior*, *i.e.* conditional, measures. It is plain that the posterior probability of an error can take all values in between 0 and 1, and is not limited by α, so that the control of the P-value does not guarantee anything about what we can expect after obtained an experimental result. A similar argument could have been raised in a more general setting, letting apart the occasional discussion with Geppert, with reference to the paper by Neyman and Pearson (1933), where they show that in the framework of the choice between hypotheses, the prior (than is unconditional) probability of error in the case of two simple hypotheses with errors of I and II kind given by α and β, respectively, is simply $\max\{\alpha, \beta\}$.

To those who claim that the significance theory has been surpassed by the more modern theory of confidence intervals by Neyman and Pearson, Gini opposes a drastic refusal of the new theory. Of course a Bayesian criticism of the theory, in particular if the confidence level is wrongly interpreted as the probability that the true value of the parameter belongs to the computed interval, is easy to perform. Gini's favourite strategy consists in criticizing the formal *inversion* of the relationship, that is the way of exhibiting probabilities as derived by frequencies starting from theorems which show how frequencies derive from probabilities. This approach is largely rephrased on the procedures of the more classic probabilistic authors, as for instance Laplace and Bernoulli (often quoted by Gini himself) so that for this aspect Gini can be considered as "the last of the classics".

6. Final comments

We reminded that Gini organized most of his criticisms in term of *inversion* of the probability statements. However, this approach is quite limited to the simplest Bayesian procedures or to a purely critical standpoint. The modern machinery of statistical models is accurately avoided by Gini, and this hampers a really general setting of the inferential problem. For instance Gini seems to assume (see *e.g.* Gini, 1966, p. 501) that the matching of frequentist and Bayesian procedures requires always an uniform prior distribution; this is an incorrect generalization of what happens in the discrete case. But this is also a clear consequence of the lack of attention to the possibility of different statistical models, each with its characteristic matching prior. In other fields (concentration, variability, association and so on) Gini's contributions reached a widening of the field, and suggested better general frameworks. In my opinion, in the field of inference this did not happen. There are two points which limited the success of Gini' approach: the concept of probability, not comprehensive enough to support a Bayesian analysis and the focusing on *inversion* which can be useful in a critical vein but does not provide anything from a constructive viewpoint. Nevertheless, when he assumed that objective probabilities were available, his use of Bayesian analysis was often brilliant and anticipatory. Remind the use of the beta class as a conjugate class for a binomial model (Gini,1911), where the hyperparameters were introduced to express prior information, and procedures which can be classified as empirical Bayes procedures, much before that the idea entered the statistical world. The topic was further reconsidered in Gini and Livada (1943), and Pompilj (1951) gave a mathematical reformulation of the Bayesian estimation of the binomial parameter. For these this aspects see also Chiandotto (1978). It must be noted that such contributions of Gini to this specific field, after several years, were internationally recognized (Morris, 1983). In the same perspective of anticipatory contributions, we point out two further issues. The above mentioned paper (Gini, 1911) starts with the formulation of a predictive approach: knowing a past frequency of an event, specify the probability of observing a given number of successes in a given number of future trials. This approach anticipates the modern interest for predictive methods, and could be associated with the well known K. Pearson's claim (1920), that prediction is the fundamental problem of practical statistics. The difficulty of dealing with predictive problems was however not pointed out by Gini in his attacks to frequentist theories, though it could be a good polemic argument. Another interesting aspect is a comment in Gini (1955a, p. 217 and p. 218) about a situation where some "supplementary knowledge" is available, that is were we have partial information about the prior hypotheses and the corresponding probabilities. Then he suggests to proceed as if the probabilities are equal and then control what would happen after introducing suitable variations. Though this remark is done in a very cautious way, there is no doubt this is an early idea of robustness analysis.

Gini had several followers, some of them strictly obedient to his views. It is remarkable that most of them, if not all, identified a major contribution of Gini to the theory of inference in his recovery of the Bayesian approach.

Castellano (1965, p. 30) writes that "*Gini proved* [...] *how important and unavoidable are prior probabilities in any judgment on the measures deriving from a sample*". Pompilj (1948, 1949, 1951), initially close to Gini, warmly approves empirical Bayes methods proposed by Gini, because they provide, under suitable assumptions, a correct and perhaps approximate solution and even predicts, with reference to such procedures, *"decisive developments on both theoretical and practical sides"* (my translation). Another collaborator of Gini, C. Benedetti claims (Giorgi, 1996, p. 13) that Gini *"discredited the illusion of evading the Bayesian reasoning"*. Unfortunately this Bayesian path was always considered an ideal but unworkable procedure and the diffusion of Bayesian methods in Italy would eventually take quite different roads. It is interesting that, according to Scardovi (2001), Gini himself said that if he had the possibility of saving only one of his papers, he would choose just the 1911 paper, that is (my comment) the paper most anticipatory of Bayesian procedures.

Castellano (1950, 1955, 1960), who was the most influential among Gini's followers, reproposed in a rather polemic way (the title of the first paper mentions an "Anglo-Saxon chapter of the statistics to be revised") the criticisms by Gini without offering an operational general strategy for the inferential problems. A particular unhappy remark (Castellano, 1950, p. 139) is a polemical reply to Kendall, who stated (Kendall, 1949) that when the observation are numerous the prior probabilities "*dwindle into insignificance"*. To this correct remark, Castellano opposed that prior probabilities are constant causes so that they will prevail on the accidental causes. Presumably Castellano derives such argument by the "Principle of prevalence of constant causes" referred to by Gini (1945-46); Gini, however, was dealing there with a general philosophical discussion and the principle cannot replace any serious mathematical argument. In Castellano (1960) it is even said that *significance tests, confidence limits*, *maximum likelihood and similia* are a *very bad service to Science.* As it is clear, this position is entirely destructive and does not give any space to solve or even to admit the problem of inference. Let me remind that Gini explicitly accepted the importance of the problem of inference, yet criticizing Fisher's solution (see *e.g.* Gini,1939, p. 205). Pompilj on the contrary, tried to reconciliate Gini's criticisms and modern inferential statistics introducing the idea of *conformity*. In Pompilj's formulation the significance method is able to measure the conformity of the data to a theoretical model, but not to give the probability that the model is true. Anscombe (1963) remarks that this, more than a criticism to Fisher, is simply a better formulation of theory of significance. It can be presumed that Gini did not disproved Pompilj's formulation of conformity, since the concept appears in a systematic book authored by both Gini and Pompilj (1959). Pompilj was deeply committed to the diffusion of the design of experiments (Pompilj and Napolitani (1954), Pompilj and Dall'Aglio (1959)) and the theory of conformity is the minimum of inferential theory which guarantees for instance to perform an analysis of variance without incurring in logical mistakes. As a personal remind, I can add that to a young researcher asking in the early Sixties for some reference book in statistics, Pompilj suggested the well known book by Cramér (1946)); this confirms that Pompilj's idea was at last not to break away from the current mathematical statistics.

The diffusion of Gini's ideas about inference in the international literature was poor. Many Italian authors regretted this fact (*e.g.* Pompilj (1949), Herzel and Leti (1977)), but, as we tried to show, the lack of a real success was rooted in the contradictions of Gini's approach to inference and in the lack of development of the brilliant insights that Gini nevertheless proposed. Another great Italian scientist, Bruno de Finetti, was similarly ignored for many years, because of the novelty of his approach (I am referring here mainly to de Finetti (1937)). But de Finetti's attitude was extremely constructive, far-off from any rigid prescription and open to dialogue (the present writer stressed this aspect elsewhere (1986)) and, with the fundamental collaboration of L. J. Savage and the contribution of many others, de Finetti's ideas entered, in many different formulations, as a part of the common knowledge in the field of statistical inference. In this way, ideas not far from Gini's ones continued to stimulate thoughtful reflections.

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