CONCLUSION

"The decisive action of reason is almost always confused with monotonous recourse to the certainties of memory."
Gaston Bachelard (1936).

Since the 18th century, one phenomenon – the degree of regularity of the proportion of the sexes at birth – has contributed elements of support to various developments such as the calculus of probabilities, administrative statistics, the moral and social sciences, the statistics of variability, post-Darwinian biology and even Durkheimian sociology. The staying power of the object has lent itself to this. But, as Edgeworth indicated as long ago as 1892, its avatars in the various sciences do not coincide. We should add that, over three centuries, they have often crossed paths, sometimes keeping a fair trade going, but also laying themselves open to other objects being smuggled in. In this book, we have attempted to explore the phenomenon itself through a history of the way it has been understood by scholars – a history that is a reconstruction of the forms taken by the rational control of technical and conceptual operations, as faithful as historiography will allow us to be in the concrete, contextual conditions of long-standing objectivizations, and not governed by a teleological view of discovery or of science.

As we come to the end of what might therefore be described as an epistemological and historical deconstruction of human sex ratio at birth, the various pathways that we have followed through what seems in retrospect to be a history of the relationships between certain areas of mathematics, biological sciences and social sciences have given us a panoply of means that we have finally been able to recombine effectively with new reconstructions in mind. The special scholarly activity of analysing something in order to reshape it is certainly nothing new! Even before Galileo, Jupiter was known, optical lenses and telescopes were produced,

E. Brian and M. Jaisson, The Descent of Human Sex Ratio at Birth, 177–185.

sidereal movements were observed, variants of the world system had been developed... But it was a particular strict combination of these pieces of knowledge, these forms of expertise and these objects that allowed him to identify the satellites around Jupiter and, through this empirical observation, to substantiate Copernican theory. The new elements that we would like to highlight lie elsewhere. Running counter to routine narratives that skim over three centuries of research and theories of science yet care little for the conditions of its historicity, we have attempted to take a serious look at the complexity of historical times and of the actual loci of scientific work. We have highlighted them using analytical instruments drawn from the social sciences and from the history of sciences. Our close deconstruction from the standpoint of the current state of critical thinking on conditions of scientific knowledge and our concluding outline for a well-founded possibility of reconstruction therefore mean not only renewing our links with the oldest traditions of scholarly thinking, whose wellsprings undoubtedly remain hidden beneath the secrets of the scholar's craft, but also proposing a form of "well-tempered" reflexivity in today's work of objectivization. In this instance, current research on the distribution of the sexes at birth mobilizes many fragments which cannot be combined in any relevant way without thinking deeply about the coherence of the objects, the instruments – in this case, statistical ones – and the theoretical systems. Nothing would be worse than to let oneself be carried along by these routines which, for example, could lead one to use a given index, a particular statistical test or a given microeconomic model, without allowing one to foresee that these techniques may blur the phenomenon rather than providing tools for its analysis.

At least three conditions favour this movement from a series of critical deconstructions towards an attempted reconstruction. In the first place, the position that we adopt is not one that overarches the other sciences in epistemological terms – the kind of position that, in the past, would have been defended from either a normative or a critical point of view. Since the early 20th century, there have been numerous interactions between philosophy, the mathematical and natural sciences and the social sciences. As a consequence of that century, these interactions have, in one way or another, given thoughtful specialists long collective experience of intense, always somewhat fraught cross-disciplinary discussions.[1] In starting from an analysis of the history of these tensions, we take that for granted. A second

[1] Published in French since 1900, the *Revue de synthèse* has helped to animate and record these transactions, transfers and tensions. Its archive collection is available online from the National Library of France (gallica.bnf.fr). Several recent issues have been directed

factor arises from the considerable change that has occurred in the state of historical knowledge about the forms that used to be taken by scholarly research, about its most concrete conditions and about the ways in which works have circulated between the sciences and between languages. Thus it is much easier nowadays than it was 30 years ago to come straight to the point of the tensions between mathematicians of the probable in the late 18th century, to follow the pathways of Charles Darwin's writing, or to track the inaccuracies that accompanied the spread across Europe of a particular figure or a remarkable conclusion. During the same period, the pre-disciplinary history of the social sciences (Heilbron, 1990) developed significantly, providing as food for thought some elements of a fairly balanced treatment of the different disciplines concerned, before the major turn institutionalizing them in the universities in the late 19th century. Finally – and this last factor seems to us no less decisive than the others, although it is perhaps not as much discussed – the concrete conditions of the work of empirical objectivization have profoundly changed (Lepenies, 1985, 2003). Süssmilch and Condorcet could do no more than recommend that numerical tables should be drawn up. Fourier's and Quetelet's priority was the organization of administration so that counting would be carried out in a satisfactory manner. The only starting-points available to later authors were these very slightly improved registers, and figures compiled by their contemporaries who were administrative statisticians. The latter spent most of their time producing figures, using a form of administrative organization that was impressive but still imperfect. They published tables that they wanted to be viewed as definitive. Darwin, Durkheim, Lexis and Gini in turn wore themselves out poring over this material, and sometimes criticizing it. They managed to free themselves from dependency on it only through exceptional conceptual efforts. Max Weber and Maurice Halbwachs, each time they sought to explore the phenomena they were studying by any routes other than citation of extracts from collections of statistics, spent hundreds of hours going through sources in great detail, filling in and classifying index cards, and then extracting a few convincing figures from them.

Yet nowadays, if we are willing to mobilize the documentation and apply adequate processing methods to it (although it is true that this can frequently be fairly unrewarding), we have within easy reach most of the material compiled over three centuries and the mass of commentaries on

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at promoting thinking about this long collective experience: "*Henri Berr et la culture du XXe siècle*", No. 1–2, 1996; "*Éléments d'histoire des sciences sociales*", No 4, 1997; "*Actualité et épistémologie*", No. 1, 1998; "*Sciences et philosophie au XXe siècle. L'École de Zürich et le programme surrationaliste*", No 2, 2005.

it, as well as the panoply of technical methods perfected since then, which we can use to analyse it. Current forms of academic work, the role that documentation by electronic media can play in it, and the powerful means of processing and calculation offered by personal computers allow us – notably in the social sciences – to combine, check and renew our efforts towards elements that were previously, in earlier forms of the division of labour of objectivization, dispersed across various sites. Thus a new form of academic division of labour is being outlined. It gives the scholar, at the scale of the practical conditions in which his thinking takes place, a laboratory for the "decisive action of reason" (Bachelard, 1936),[2] unlike the routines of memory associated with older divisions of labour. Figures, methods and conclusions that used to be adopted as if we had to make the best of them, can now be put to the test; the necessary historicity of their formation and their circulation can be measured; and finally we can use these early results as foundations of new terrains for empirical research.

Let us consider that only humankind has attempted to be systematic in counting itself for a few centuries. But even in this case, which is so favourable, it would be futile to expect the same methodological qualities from indices obtained by such different routes as conjectures about conception, for the primary sex ratio; empirical compilations of birth records, for the secondary sex ratio; or even censuses and their demographic extrapolations, for the tertiary sex ratio. Therefore, human sex ratios at birth offer a singular empirical case: an index which has incorporated three centuries of consistent collective efforts to establish it. Yet it is fairly easy to highlight the gap between the ideal of such a ratio and the concrete conditions for its establishment from one country to another, both nowadays and in the past, or even from one era to another. Therefore it would be futile to limit ourselves only to this ideal – and equally futile to settle for the disappointment that this suggests. We need to take as established fact – and therefore as a thing, as Durkheim would add – records made, possible compilations and calculations, imperfections in procedures, their variations, and the over-abstraction that the calculuses presuppose. The persistent divergences between the sciences, indicated by Edgeworth, no longer seem to be the tokens of a conflict of idealized faculties, but to be the indicators of a

[2] This phrase, like the quotation in the chapter heading, comes from Gaston Bachelard's article, "Le Surrationalisme" (1936). Various elements extending this position are indicated in these issues of the *Revue de synthèse*: "*Pensée des sciences*", No 1, 1999; "*Objets d'échelles*", No 1, 2001; "*Sciences et philosophie au XXe siècle. L'École de Zürich et le programme surrationaliste*", No 2, 2005.

new empirical topic to be grasped at the limit of the capacities of humankind to objectivize itself.

As soon as this perspective is set, it is possible, as we have noted, to perceive a recurring theme in the overall understanding of the phenomenon among scholars who have commented on it. Those have sought to show its dynamic have stressed its *oscillatory nature around a near-equilibrium of the two sexes*, with this feature being justified differently each time. Nowadays, specialists have in mind the wording of the principle that Fisher considered he had proved. Historians of biological doctrines are able to find prototypical formulations of this "principle" in Darwin's body of work and in Düsing. Yet we have seen that Darwin, Düsing and Fisher did not envisage the phenomenon in the same way. Each viewed this schema of a trend towards balance in a different conceptual framework: a complex combination of natural selection and sexual selection in Darwin; a re-establishing of physiological balance for Düsing; and the action of natural selection on parental expenditure for Fisher. Other instances can also be found. The cycles highlighted by Halbwachs are an example of this, although admittedly there is no idea here of underlying adjustment in the end. And very much earlier, Condorcet, in a manuscript that remained long unpublished, indicated a moral mechanism peculiar to humankind, whose effect would be the re-establishing of a balance between the sexes at birth. In other words, each time, the same morphology of the phenomenon is expressed and, each time, this expression proceeds from a different conceptual framework specific to a particular moment in the history of relations between natural sciences and moral sciences. Similarly, when these different formulations, and the proofs that have been advanced in their support, are compared, none of them seems completely satisfactory.

A second recurring theme is worthy of attention. An inference drawn from a ratio presupposes a more or less explicit and more or less controlled conception of the *variability of the measured phenomenon*; and the diversity of these concepts seems to have governed the main differences between the academic constructions witnessed over three centuries. Is it a physical variability, an intrinsic uncertainty, measurement errors, or even errors in approximating? From a strictly technical point of view, this ceased to be very important after Quetelet, who – undoubtedly faithful to Fourier – set the seal on a powerful analogy between physical variability and error dispersion, and in doing so led to a normalization in the treatment of both, based on the hypothesis of a Laplace-Gauss distribution. It really must be admitted that the teaching of statistical methods nowadays remains faithful to Quetelet,

even though it is most often ignorant of their origin: what does the nature of the dispersion matter, provided we have the central trend? Yet we must also acknowledge that the analysis of the phenomenon and any possible resulting reconstruction depend on how variability, uncertainty, errors and approximations are constituted in their empirical and technical operations.

For example, a line can be traced from Laplace to Fréchet, via Poisson, Borel and Gini, among others. This is the line of a strictly formal conception of the variability of the sexes and of the sex ratio, in different accounts of the mathematical theory of the calculus of probabilities. From this strictly formal point of view, there is little *a priori* importance in causes, errors or approximations. We merely need to note that there are two types of registration of a birth (the two sexes) and that it is impossible to allocate *a priori* a future birth to one or the other of these. Once this uncertainty about each case, which is supposed to be intrinsic and consistent, has accumulated in a certain number of observations, it allows us to infer the probability of one or the other of the two sexes for a future birth with some rigorously established degree of certainty. For each elementary calculation, the variability in frequency of one sex at birth then fluctuates in a specific way, and its measure can never be taken as fixed: it is always known to be bounded within an interval (a limited one, of course), and only in probability. This *probabilistic* schema (in the sense that it results from the mathematics of the probable) consists of accepting a necessary chance and of drawing mathematical conclusions from it. So in no case is there any question of removing the purely epistemic variability of the phenomenon. What is more, this explains its great regularity; and Borel was able to relate the empirical imperfections occasionally observed back to an impoverished physics.

Another line starts from Fourier and Babbage, then runs via Quetelet and on towards Lexis and Halbwachs. In this line, the variability of the phenomenon is no longer solely epistemic in nature, but physical. This other point of view makes it important to improve recording and measures and to perfect the treatment of errors. This is the pathway of *descriptive statistics*, which has most often appeared to be a lower priority for mathematicians of the probable. Although in the late 18th and early 19th centuries, poverty of sources made Condorcet, Laplace and Fourier promoters of what may seem to us to be a combination of the probabalistic schema and empirical statistics, the success of Quetelet's simplifications, the development of offices of administrative statistics and the proliferation of statistical publications gave *la statistique* a great deal of autonomy from the grip of mathematicians in

the main Continental European countries involved. So much so that this corpus of statistics served as empirical material for the new specialisms that appeared in this era, sociology and demography. In general, for this descriptive statistics, any variation is better than none, insofar as the sources are solid and the differences measurable, since the epistemic uncertainty that the calculation entails is not taken into consideration.

A third line starts from Darwin, goes via Galton, branches off towards Düsing and leads on to Fisher. This time the observable variability of the sex ratio at birth is the very fact that must be taken into account. So it is the physiology, the mechanism of reproduction, or the system that characterizes reproduction that must be grasped. As we know, the *statistics of variability* that resulted from this, from Galton's work onwards, brought something very new to the toolkit of those carrying out the calculations. From the mid-20th century, it became the most generally accepted standard for statistical training in biology, economics, sociology and demography.

Let us consider, by way of example, the proportions of boys at birth, S_i, for several consecutive years. From a strictly *probabilistic* point of view, each value means nothing unless it is accompanied by an indication of its own variation: in this case, a confidence interval considered for a certain probability level. The value of S_i can therefore never be taken as certain. In contrast, from a *statistical* point of view, as applied in the calculation, each annual value is viewed as established each year, leaving aside any possible errors, approximations or uncertainties. The use of a statistical method then consists in relating one part of the variations from one year to another to the number for the year, or else to another index that has annual variations, and then in accepting that the remainder of the variability looks "purely" random.

There are two completely different schemas here: firstly, a hypothesis of intrinsic uncertainty from which conclusions can be drawn; secondly, an empirical variability that is decomposed through calculation while keeping the thing we are endeavouring to grasp to something like chance. In the schema that we are describing as *probabilistic*, a chance that is a constituent element of the phenomenon is accepted as such. In the schema that we are marking with the word *statistical*, it is a residual chance, even a discredited one, that measures the variability remaining unexplained once an analytical technique has been applied. It is clear that although these two schemas differ, they are very close in several ways. In the first place, the historical pathways of their formation have crossed and recrossed for three centuries. What is

more, up to a certain point, each of them can be conceptualized through the other. A theoretician of the probable will be able to explain important areas of statistical techniques – but not all of them, and not uniformly. A statistician may take the view that certain pillars of probabilistic thinking – for example, the binomial schema that is at work in the analysis of sex ratio – can be sufficiently explained in terms of frequencies.

In the case of the calculation of human sex ratio at birth, a review of three centuries of tensions between these two conceptual schemas also suggests that the relationships between them, as viewed by scholars and applied in their work, have been through several regimes of conditions that made them possible. The concept of a *pre-Quetelesian* regime seems to describe conditions where neither reference to a central value nor analogy between the structure of variability of the phenomenon and the structure of the errors that its measure involves is predominant. Reading D'Alembert, Condorcet, Laplace, Fourier or Babbage without considering them as scholars of such a particular era would lay us open to dangerous anachronisms. Restoring this characteristic would give their texts a depth that historians of the sciences have too blindly reserved for Laplace and too rarely accorded to the other four. The *Quetelesian* regime in its strict sense is better known: in these conditions, the activity of scholars is governed by the normalization of observations according to the theory of the average and of errors, formulated by Quetelet and carried forward by the rise of European statistical institutions. This regime became established during the second half of the 19th century: several authors have already observed a long time ago that the 1840s marked a turning-point in this regard (Daston, 1988; Hacking, 1990; Porter, 1986). But by the end of several decades, the rise of this first form of statistical thinking and the rise of specialized forms of production had had the obvious consequence of highlighting the variability of the phenomenon whose recording had thus been normalized. Most statisticians active after Quetelet were content to reason like him, but using materials whose homogeneity they rejected even though it had been accepted by their predecessor: "average" reasoning was therefore no longer directed at the average man but, for example in the physical anthropology of Paul Broca (1824–1880) and Louis-Adolphe Bertillon, at presumed racial differences (Brian, 1991). So these were instances of Quetelesian thinking in an era which was already no longer exactly Quetelesian. As we have seen, it was in England, and starting from a completely different conception of variability – derived from Babbage and from Darwin, and consolidated in Galton's works, but fostered by Quetelesian statistics – that methods of analysing variance were developed to the point where they would completely

change statistics in Continental Europe in the 20th century. Thus it is possible to describe the conditions enabling these later forms of thinking as a *para-Quetelesian* regime. From this arose the works of authors as different as Darwin, Galton, Lexis, Durkheim and Gini – to mention only those who, in one way or another, have appeared in the course of our survey.

In the *pre-Quetelesian* regime, the distinction between probabilistic and statistical schemas does not make much sense. In the *Quetelesian* regime, these schemas are fused into a single doctrine. In the *para-Quetelesian* regime, the texts of the most attentive scholars demonstrate an epistemological tension between the two schemas. The characteristic features of this tension, as we have seen, depend on the specific conditions of the scientific division of labour. Perhaps a schema of thinking that would assume both the probabilistic variability of a phenomenon and its statistical variability without confusing them even by approximation – that is, viewing them as two epistemic dimensions that are *a priori* distinct, even though it combines them analytically at a second stage – should be described as *post-Quetelesian*.[3] This clarification would at the very least have the advantage of providing, on the one hand, a reasoned extension of earlier schemas and, on the other hand (as in the first five chapters of this book), a framework for the critical comparison of past examples. Beyond this critical use, our reconstruction of human sex ratio at birth in Chapter 6 is an example of the empirical scope of this proposition, whose principle lies not in reducing a detested chance but in *adopting* a stochastic hypothesis developed through the calculus of probabilities – in "setting chance against chance" (Condorcet, 1793–1794 [2004, p. 437]; translation from Philadelphia edition, 1796).

[3] In early-20th-century Continental Europe, Corrado Gini and Maurice Halbwachs were among the very rare scholars we are tempted to describe as post-Quetelesians.

Appendix A
CONDORCET (1743–1794)

Extracts from *Effets sur l'état moral et politique de l'espèce humaine de quelques découvertes physiques*[1] (1793–1794)

Bibliothèque nationale, Paris, ms, n.a.f. 4586 folio 189 recto-209 verso; published in Condorcet 2004, pp. 923–936.

Effects on the moral and political state of humankind of some physical discoveries like the means of producing male or female children, as one chooses, with a certain probability; of producing children without the union of the mother with any man; etc., which may have results acting for or against the continued perfecting of humankind.

[1] This *Fragment*, numbered *10* by Condorcet himself, is part of the manuscript that he prepared during his period in hiding from the Revolutionary Terror, with a view to a work entitled "*Tableau historique des progrès de l'esprit humain [A history of the progress of the human mind]*" (all known documents that relate to this have been published in Condorcet, 2004). The posthumously published prospectus, later entitled *Esquisse d'un tableau historique des progrès de l'esprit humain [Sketch for a history of the progress of the human mind]* (Year III – 1795), was intended to herald the *Tableau* – a vast project that was never completed. Léon Cahen was responsible for the first edition of *Fragment 10*, with slight modifications from the manuscript form, in 1922. The publication remained unnoticed for a long time, although in recent decades commentators have most often made use of mere snippets of its complex reasoning in order to extrapolate anachronistic conclusions from them. Therefore readers are warned against the kind of hasty borrowings to which this translation of some longer extracts may well lead.
Condorcet is here in the Tenth Epoch of the *Tableau*, where knowledge of the history of the human mind gives him hope for humanity's future. In his view, therefore, we find ourselves after the advent of equality of rights, for which the American Revolution (1776) and the French Revolutions (1789 and 1792) seem to have definitively opened a new epoch. He attempts to evaluate the advantages and risks of controlling the number of births; the possible effects of a probable action to control the sex of the children to be born; and, finally, the effects of discovering a method for reproducing without sexual union. The text echoes debates of his time, notably those which accompanied the reception of Lazzaro Spallanzani's experiments (see in Chapter 2, on p. 38). Condorcet

In allowing oneself to hope that one might see humankind achieve moral perfection following the continuous, indefinite progress of the Enlightenment, one sometimes feels hampered by the idea that certain discoveries, of which there is nothing to prove the impossibility, may disturb its course towards this achievement of perfection and produce a revolution in the reciprocal relations of human beings that will force them to seek their happiness in contriving new combinations. I will not pay too much attention to the evils that the discovery of new means of destruction would produce. Woe betide anyone who does not feel that, even in times of corrupt and ferocious morals, humankind still lacks these means much less than the will to use them. I shall not dwell on the futile fear of the dangers that would result from the art of travelling through the air,[2] for it can be no secret that this could not multiply the faculty to do harm without increasing the faculty to defend oneself, that it could not help a guilty man to flee without providing the means to pursue him more easily.

But if the perfection of hygiene results in a longer lifespan for human beings, greater fertility and the preservation of a larger number of children; if the perfection of medicine postpones the death of almost all individuals into the oldest ages of life; if this population growth exceeds the limit that the annual reproduction of objects for consumption may reach, will the

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touches on questions debated at length by economists in the Age of Enlightenment, about the ratios of populations to the means of subsistence. The overall thinking that he presents is fostered by a knowledge of practices for limiting births: that such ancient practices were known to a scholar and a gentleman at the end of the 18th century is today attested by demographic historians and historians of demographic knowledge. Condorcet speculates – not arbitrarily, as if to create a utopia, but as a scholarly conjecture – about a world freed from any form of superstition: a world from which equality of human rights would proceed. He dismisses in one paragraph the prospects of powers of destruction and of what we nowadays call crimes against humanity. Given that superstition would be discarded and human society would be conducted through reason thus freed, Condorcet could only conceive of a new authority being able to take charge of population control techniques. For him, there was no room for a state that would embody, if not corrupt, Reason – quite the opposite of the 19th-century, and then the 20th-century, idea. If this happy vision puzzles us, it is because we are applying an anachronistic reading. For Condorcet, the question of the moral impacts of the ability of humankind to affect the probability of the sexes at birth is resolved in the act of procreation between human beings gifted with reason, whether or not it is assisted by technical methods. Thus, seen in retrospect, his thinking may appear liberal.

[2] This echoes the dazzling reception given in France to the invention of aerostats, the Montgolfier hot-air balloon and the Charles hydrogen balloon in 1783.

human race not find it impossible to escape a destruction that is fatal to its happiness, incompatible with the preservation of the order of societies?[3]

It could also be asked what might be [the effect] on humankind [of][4] the discovery of a means of producing a male or female child according to the will of the parents, a means that must in the end be discovered through careful observations. Supposing that this is likely to become a common practice, that it is sufficiently perfected to give the well-founded hope of success, certain to succeed just once in ten attempts for example, would it [not] lead to [changes] in the social relations of human beings, whose consequences could be harmful to the peaceable development of that indefinite perfectibility with expectations of which we have flattered humankind? And would not these relations undergo a much greater revolution if the birth of a child no longer necessarily supposed the union of its mother with a man?

Rarely have philosophers directed their assured gaze upon those objects located between disgust and ridicule, where both hypocrisy and scandal must be avoided. Christian superstition, preoccupied with the chimeras of a mystical purity, has led us into the habit of attaching ideas to these physical actions, which for good or ill may be applied only to their moral consequences. It would show them only from the point of view of shame or of crime, and one had either to make fun of its risible anger or share it. The custom of treating these questions in a foreign language every time it

[3] This passage is somewhat astonishing, as are several others that develop it in the complete *Fragment* (which, it should be remembered, remained in manuscript form until 1922). Condorcet here tackles, explicitly and in great detail, the objection that occurred to Thomas Malthus after reading the *Esquisse* – where, however, there were only rare allusions to *Fragment 10*. Malthus' criticism of the indefinite perfectibility of humankind related precisely to limits on the means of subsistence: population grows by multiplication and "objects for consumption" by addition, so Condorcet's projection would be faulty. Malthus' *An Essay on the Principle of Population* (1798, 1803) caused a considerable stir. For several commentators – most certainly not very attentive to 18th-century debates – it marked the beginnings of modern demographic thinking. As we can see, Condorcet had envisaged this objection and answered it here. There is no doubt that the geometer would have recognized the mark of superstition in Malthus' reaction and would have attributed to a new kind of priesthood the interventions of those who, since the 19th century, have intended to prescribe people's reproductive behaviours. *Fragment 10* is in effect a manifesto that its author would willingly have described as "anti-superstitious". In this respect, it remains somewhat provocative.

[4] Words missing from the manuscript that were re-established for the 2004 edition are shown in square brackets.

was necessary to talk about them seriously,[5] reserving the common tongue for sermons and jests, leaves almost our only choice between expressions either too scientific or vague and obscure, or given over to the common gaiety. |But although Christian superstition has led us, through exaggeration in the opposite direction, to look at objects that are important to the morals and to the happiness of humankind [only] from the point of view of [?][6] or of jokes, it is time to see them from the perspective presented to us by reason,|[7] time to rise above the hypocrisy of customs and the hypocrisy of style, showing objects in their true light and under their own colours. |All the chains of minds, like those of tongues, must finally be broken.| Why moreover would the philosophers who have braved the league of tyrants and priests fear the league of unpleasant jokers and moral hypocrites? Is their courage to find its limit in fear of ridicule and in the anathemas of a false delicacy or an unnatural austerity?[...][8]

Are the very grounds upon which the moralists have based their prejudices as solid as they seem at first glance? Even supposing that there could be a true obligation not to place any obstacle in the way of the possible birth of a being that one believes must be unhappy, is it an increase in the number of births that is the object of this obligation, rather than a growth in the number of human beings who can accomplish its intention and fulfil its duties? Could there be any other aim than that of multiplying well-formed

[5] The issue of languages is one of the main keys to Condorcet's historiographical vision in the *Tableau historique* and in his *Esquisse*. Condorcet suspected all "priests" – officiants of the cults of antiquity, Christian clerics of all periods, scholars and doctors attached to protecting their knowledge – of hindering the achievement of reason. Their favoured instrument was recourse to the use of an esoteric language. It was in response to this that Condorcet, in another *Fragment* relating to the Tenth Epoch – numbered *4* – was to give a method for the formation of a universal language fostered by the philosophical and scientific experience of the preceding centuries (2004, pp. 947–1029).

[6] Illegible word, possibly "jest".

[7] The passages that appear between two vertical bars are earlier variants of the manuscript, which Condorcet himself crossed out, but which we have re-established in order to clarify his meaning.

[8] A passage follows that develops this, dealing with the capacity of humankind to distinguish the necessities of reproduction from the pleasures associated with sexual acts. We should clarify that birth control practices and certain forms of contraception were known in the 18th century. Condorcet indicates the reasons that seem to him to explain the human capacity to operate this distinction, and the causes of superstitions attached to this phenomenon. In the course of his analysis, he mentions birth control, infanticide by exposure of newborn infants in Rome and in China, contraceptive methods known in the 18th century, adultery, prostitution and "those bizarre tastes, those debasing habits that enervate and degrade humankind" (ms folio 194 verso; 2004, p. 928).

beings capable of being useful to others and of making their own happiness? Will the distribution of children in various families, or the distribution of the times of their birth in each family in particular, better fulfil this aim if it is left to chance than if it were directed by will?

Will it be said that the interest of pleasure is necessary in order for humankind to determine to perpetuate itself? But if children are not the object of their fathers' sweetest hope, if their fathers regard them as an inconvenient burden or as beings condemned to unhappiness by receiving life, this would mean that the generations of humankind are destined to appear on the earth only in order to suffer there. And why then would one occupy oneself with the preservation of a race necessarily miserable and foolish? But if the earth offers all human beings the easy means of providing for their needs, if the social pact ensures them such means, if just laws defend them against wickedness and against oppression, why would the desire to have children not be a universal sentiment, [if] it is dictated by nature and approved by reason? [. . .]

We may boldly conclude that the epoch where the ways in which humankind made progress – the epoch of the arts, in short that of a greater wisdom in the distribution of cultivated land and of labour – would render all population growth contrary to the general interest, and where at the same time the lifespan of human beings and the preservation of children would bring about this growth if they abandoned themselves to the impulse of nature, that this epoch would not be, for human beings free of all prejudices, a fatal limit where their coming to perfection would have to halt, where humankind would necessarily have to pass through a state of suffering in order to recommence its course towards a new prosperity, only to come to a halt again before the same obstacle. [. . .][9]

[9] This is a second key to the *Tableau historique*, which derives from the philosophical debates of the Age of Enlightenment. The work is in fact entirely directed at countering *machiavellianism*. This word, as used by Condorcet, does not refer primarily to political calculation, but to the idea that nothing better is to be expected than the current order of the world, and that consequently arbitrary political decisions would be no less just than this order. This machiavellianism is therefore comparable to contemporary political cynicism. In the 18th century, it had to be reconciled with Leibnizian optimism, whose political destiny was to be a form of conservatism (despite the intellectual commotion caused by the Lisbon earthquake of 1755). It was also deeply rooted in a theological view based on the necessary suffering of human beings. For Condorcet, as for Turgot, this system of necessities had been overturned, giving way to a *meliorist* perspective. In his eyes, the belief that human beings are born to suffer results from superstition. In freeing itself from this through the exercise of reason, humanity gives itself the means

In the hypothetical case that one had a means of determining the sex of children at will, at least up to a certain point, would there arise a significant difference in the number of individuals of each sex instead of the almost total equality that exists today,[10] and which then would be the most numerous, and what might be the effects of this disproportion on the social order?

First of all let us examine what grounds could determine a preference for one sex to the other. Up to now, prejudices, much more than reason, have governed the division of labour between the two sexes.[11] This distribution has not even been governed by their almost complete equality of numbers; often there are not enough men to labour, while women remain idle. But we have already examined what the influence of destruction would be on the state of the two sexes. We have seen that especially supposing progress in the art of applying machines to skilled trades, infrequent wars, very long voyages becoming shorter – the necessary consequence of a greater equality of civilization and industry between different peoples – this distribution of labour, left to the will of individuals, would be done in the manner most advantageous to the two sexes according to the ratio of their numbers.[12]

But if a change to this ratio could be mastered, it could then be made with a view to obtaining a distribution of labour more advantageous to the total mass.[13] A distribution exists that is most suitable for each ratio, but between these distributions, the most suitable varying with the ratios, there may be some that are best in themselves.[14] Let us now suppose that the ratio that exists at a given time is less advantageous in regard to the resulting distribution of labour than a certain other subsequent ratio; that in order to move closer to the latter it will be necessary to increase the number of

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of discerning the conditions for its happiness, first among which ranks equality of rights. Respecting rights ensures the movement of humankind towards prosperity.

[10] Condorcet knew Graunt's and Süssmilch's conclusions, as well as the detail of Laplace's work. Each of them, in his own way, demonstrated that the proportions of the two sexes at birth were close, but from a general point of view always slightly in favour of boys.

[11] This is an analysis in terms of division of labour, emanating directly from the economic texts of Turgot and of Adam Smith, which Condorcet knew extremely well.

[12] Here Condorcet is referring to a passage that has not been retained for this translation and which deals with the population in general, without regard to the distribution of the two sexes. In his view, a "constant parity" reasoning would follow the same routes as the more general precedent. He therefore contented himself with an outline of it.

[13] That is, the whole of humankind.

[14] Condorcet's reasoning on the relationship of cause and effect between the division of labour according to sex and the proportions of the two sexes is completely abstract.

women or that of men; it will be more advantageous to give birth to a girl or a boy. Therefore there will always be a subsisting cause that will act to lead to this more advantageous ratio.

In a society where equality of rights would entail the real equality that is the consequence of it, women would nurse their own children. Thus twelve children may be regarded as a limit that they would surpass only very rarely. But the number of those to which a man may give birth is far from being contained within such narrow boundaries. The result of this is that, where the number of individuals is not enough to labour, the quantity of food easily produced would not be sufficient, so one would seek to increase the number of women, even if this were to result in the establishment of [polygamy].

But this need to increase the population may [not] be encountered in a country that has reached the degree of civilization that we are supposing here,[15] except in extraordinary cases. But if a lesser rate of reproduction is needed, this does not result in a need to increase the ratio of the number of men to that of women. We have seen above that other means exist,[16] and it is difficult to believe that the means of determining the sex at will may be simpler and easier to employ.

If the parents consult the interest of their own happiness or that of their children, this interest will bring them to equality since we are supposing here that reason has led to the disappearance of the institutions that have made independence, freedom, the enjoyment of the rights of humankind, the privilege of one sex alone.

The question therefore amounts to whether the interest of obtaining a more useful employment of the forces of humankind, acting unceasingly and towards a single aim in the midst of other interests that may be opposed to it, must in the end entail a disproportion in the number of individuals. I do not believe so. As women consume less they may, in all employments that may be fulfilled by the two sexes, produce an equal result for the price of an equal expenditure, and in the total mass of labour, of occupations, the portion that can be regarded as the same whether done by either sex is too considerable for there to be any advantage in a disproportion of the

[15] We really are in the "*Tenth Epoch*" here (see above).
[16] This refers to the most varied means then known for limiting births, without regard to their sex.

number, while the portion of labour that it is good to reserve to each sex would suffice to prevent this disproportion from extending beyond certain limits.

But here an objection presents itself. [. . .] Does not each sex have an interest in multiplying the sex that it is not? Men so that, at an advanced age, liaisons with younger women are easier for them, women so that men, limited to fewer objects, are obliged to become less particular about youth. Here I will observe only that, if that which relates to vanity is set aside, the strength of a long attachment, the propriety of ages, the natural repugnance at being only an object of disgust would destroy this same interest of pleasure in beings directed in general by their reason. I shall not stop to consider that the execution of such an enterprise entails the destruction of that equality of rights which I suppose to have been established; because I would be told that the influence of this very interest would be an obstacle to the preservation of that equality and to the progress of reason. If I add also that the sex which would be reduced to a less [great] number in order to multiply its enjoyment would necessarily become the weaker, I could be told besides that there results from this observation an additional interest in seeking to introduce and strengthen prejudices, to employ those means of oppressing the greatest number, which up to now have had across the whole globe such an awful and lasting success. But I shall observe only that there does not exist here any direct interest for those who may introduce this disproportion, that it would not be advantageous to them within their own family, that success would depend also on the will of that of the two sexes which would lose by this disproportion, and finally that this first success, even supposing that one of the two sexes would ignore its interests or lose its rights, is incompatible with the state in which society is supposed to be.

The only result of this discovery therefore would be a means of re-establishing an almost total equilibrium either in the small portions where nature alone would not have established it or in the extraordinary circumstances where she would have deviated from it.[17]

There would be a real danger if one sex had a pressing interest, opposed to that of the other sex, in augmenting the proportion of the number for itself, but that interest does not exist, and consequently force could not

[17] Thus for Condorcet, where one sex ensured numerical growth relative to the other, the ensuing moral effects would lead to a phenomenon of oscillation in the frequency of births according to sex, which would tend towards the re-establishment of near-equilibrium.

preserve a disproportion contrary to the general progress of humankind once chance had established it[18]. [. . .][19]

Supposing the individuals of humankind guided by their reason, and by those sweet affections to which nature attaches their happiness, they would not overstep the bounds of an ability from which the interest of those affections and the need to feel their sweetness must distance them. Supposing a social system[20] founded on entire equality of rights, it would be impossible to misuse this ability by force for the unhappiness of a portion of humankind. This ability would therefore be nothing more than a resource employed only in certain particular circumstances| where it would serve to make amends for injustices|. This ability would serve above all to make more intimate and more dear, by finally freeing them from the yoke of necessity, the same relations to which, at first glance, one would have been tempted to believe it posed a danger.[21] Everything that may contribute to making individuals more independent[22] is a good, even relatively to the happiness that they can give each other reciprocally: the more voluntary it is, all the greater it will become.

[18] Condorcet considers that one sex cannot privilege its own reproduction over the long term. An observed deviation would lead to a restoration of near-equilibrium. His reasoning here is combinatorial, envisaging different predilections according to the sex of the agents and that of the children arising from their acts. In all cases, the dynamics of the sex ratio is to re-establish an equilibrium close to equidistribution.

[19] This passage deals with the possibility of procreation without copulation.

[20] The expression "social system" here does not have the meaning that it would today. In Condorcet's writings, as in those of some of his contemporaries, it means the system of rules accepted by people in association. We would now describe this, presupposing a national unit, as a "constitution" – in the legal sense of the word – along with the texts that derive from it. Condorcet wrote " social order" (in the text above, just after the position of our Note 10). In this instance, the meaning is much closer to that of Durkheimian sociology. The expression has connotations of "divine order", since society was then serving as an ordering principle, in lieu and on behalf of the Divinity.

[21] That is, relations between the two sexes.

[22] To be understood as: free from the obstacles of existence and from superstitions.

Appendix B
CHARLES DARWIN (1809–1882)

Human sex ratio, natural selection and sexual selection
in The Descent of Man *(1871 & 1874)*

1. The limits of actual enumeration (1871 & 1874)

Hence I was led to investigate, as far as I could, the proportions between the two sexes of as many animals as possible ; but the materials are scanty. [...] Domesticated animals alone afford the |means|[1] of ascertaining the proportional numbers at birth; but no records have been specially kept for this purpose. By indirect means, however, I have collected a considerable body of |statistics|,[2] from which it appears that with most of our domestic animals the sexes are nearly equal at birth. [...] It is, however, in some degree doubtful whether it is safe to infer that the |proportion would be the same|[3] under natural conditions as under domestication; for slight and unknown differences in the conditions affect[4] the proportion of the sexes. (1871, Vol. 1, pp. 263–264; 1874, p. 215).

As no one, as far as I can discover, has paid attention to the relative numbers of the two sexes throughout the animal kingdom, I will here give such materials as I have been able to collect, although they are extremely imperfect. They consist in only a few instances of actual enumeration, and the numbers are not very large. As the proportions are known with certainty |only in mankind|,[5] I will first give them as a standard of comparison. (1871, Vol. 1, p. 300; 1874, p. 242).

[1] Replaces “opportunity” (1871).
[2] Replaces “statistical data” (1871).
[3] Replaces “same proportional numbers would hold good” (1871).
[4] “to a certain extent” (1871) cut.
[5] Replaces “on a large scale in the case of man alone” (1871).

2. The empirical variability of human sex ratio at birth (1871 & 1874)

In England during ten years (from 1857 to 1866) | the average number of children born alive yearly was 707,120|,[6] in the proportion of 104.5 males to 100 females. But in 1857 the male births throughout England were as 105.2, and in 1865 as 104.0 to 100. Looking to separate districts, in Buckinghamshire (where |about|[7] 5000 children are annually born) the *mean* proportion of male to female births, during the whole period of the above ten years, was as 102.8 to 100; whilst in N[orthern] Wales (where the average annual births are 12,873) it was as high as 106.2 to 100. Taking a still smaller district, viz., Rutlandshire (where the annual births average only 739), in 1864 the male births were as 114.6, and in 1862 as |only|[8] 97.0 to 100; but even in this small district the average of the 7385 births during the whole ten years, was as 104.5 to 100: that is in the same ratio as throughout England.[9] The proportions are sometimes slightly disturbed by unknown causes; thus Prof. Faye states 'that in some districts of Norway there has been during a decennial period a steady deficiency of boys, whilst in others the opposite condition has existed.' In France[10] during forty-four years the male to the female births have been as 106.2 to 100; but during this period it has occurred five times in one department, and six times in another, that the female births have exceeded the males. In Russia the average proportion is as high as 108.9 |, and in Philadelphia in the United States as 110.5|[11] to 100.[12] |The average for Europe, deduced by Bickes[13] from about seventy million births, is 106 males to 100 females. On the other hand, with white

[6] Replaces "707,120 children on an annual average have been born alive" (1871).
[7] Replaces "on an average" (1871).
[8] "only" not in 1871.
[9] Darwin's footnote, since 1871: "Twenty-ninth Annual Report of the Registrar-General for 1866. In this report (p. xii) a special decennial table is given". See Registrar-General (1869).
[10] See Bureau des longitudes (1867) and Footnote 12.
[11] "and in. . .as 110.5" not in 1871.
[12] Darwin's footnote, since 1871: "For Norway and Russia, see abstract of Prof. Faye's researches, in 'British and Foreign Medico-Chirurg. Review', April, 1867, pp. 343, 345. For France, the 'Annuaire pour l'An 1867', p. 213." Darwin's addition in 1874 : "For Philadelphia, Dr. Stockton Hough, 'Social Science Assoc.', 1874. For the Cape of Good Hope, Quetelet as quoted by Dr. H. H. Zouteveen, in the Dutch Translation of this work (Vol. i. p. 417), where much information is given on the proportion of the sexes."
[13] The table of Bickes' estimates had been published in the *Zeitung für das Gesamte Medizinalwesen* (February 1831) and in the *Mémorial encyclopédique et progressif des connaissances humaines* [*Progressive encyclopaedic chronicles of human knowledge*] (Paris, 1832–1833). They had been discussed by Villermé (1832*c*) and by Quetelet (1835).

children born at the Cape of Good Hope[14], the proportion of males is so low as to fluctuate during successive years between 90 [47.37%] and 99 [49.75%] males for every 100 females.|[15] It is a singular fact that with Jews the proportion of male births is decidedly larger than with Christians: thus in Prussia the proportion is as 113 [53.05%], in Breslau as 114 [53.27%], and in Livonia as 120 [54.55%] to 100; the Christian births in these countries being the same as usual, for instance, in Livonia as 104 [50.98%] to 100.[16] (1871, Vol. 1, pp. 300–301; 1874, pp. 242–243).[17]

3. The indirect effect of sex ratio at birth on sexual selection (1871 & 1874)

I have remarked that sexual selection would be a simple affair if the males |were considerably more numerous than|[18] the females [...]. For our present purpose we are concerned with the proportions of the sexes, not |only|[19] at birth, but |also|[20] at maturity, and this adds another element of doubt; for it

[14] In 1874, Darwin is referring to additions made by his Dutch translator, Hermanus Hartogh Heys van Zouteveen (1841–1891), to his own work, see Footnote 12.

[15] "The average for Europe...99 males for every 100 females." not in 1871.

[16] Darwin's footnote, since 1871: "In regard to the Jews, see M. Thury, 'La Loi de Production des Sexes', 1863, p. 25". See Thury (1863).

[17] The table below shows the 95% confidence intervals that must be read in association with these figures:

	Number of years	Approximated N	Ratio $M/100F$	Ratio M/N	Confidence int. 95%	Lower benchmark	Upper benchmark
England 1857–1866	10	7 071 200	104.5	51.10%	0.04%	51.06%	51.14%
England 1857	1	707 120	105.2	51.27%	0.12%	51.15%	51.39%
England 1865	1	707 120	104.0	50.98%	0.12%	50.86%	51.10%
Buckinghamshire 1857–1866	10	50 000	102.8	50.69%	0.45%	50.24%	51.14%
Northern Wales 1857–1866	10	128 730	106.2	51.50%	0.28%	51.22%	51.78%
Rutlandshire 1857–1866	10	7 385	104.5	51.10%	1.16%	49.94%	52.26%
Rutlandshire 1862	1	739	97.0	49.24%	3.68%	45.56%	52.92%
Rutlandshire 1864	1	739	114.6	53.40%	3.68%	49.72%	57.08%
France before 1867	44	42 500 000	106.2	51.50%	0.02%	51.49%	51.52%
Europe	1	70 000 000	106.0	51.46%	0.01%	51.44%	51.47%

The local figures are irrelevant, even those accompanied by total numbers of live births. Only the variations, including the annual variations, of ratios for England, France and Europe could show relevant differences.

[18] Replaces "considerably exceeded in number" (1871).

[19] "only" not in 1871.

[20] "also" not in 1871.

is a well-ascertained fact that with man |the number of males dying|[21] before or during birth, and during the first |two|[22] years of infancy |, is considerably larger than that of females|.[23] (1871, Vol. 1, pp. 263–264; 1874, p. 215).

So it will be if the more vigorous males select the more attractive and at the same time healthy and vigorous females; and this will especially hold good if the male defends the female, and aids in providing food for the young. The advantage thus gained by the more vigorous pairs in rearing a larger number of offspring has apparently sufficed to render sexual selection efficient. But |a large numerical preponderance of males over females will|[24] be still more efficient; whether the preponderance |is|[25] only occasional and local, or permanent; whether it |occurs|[26] at birth, or |afterwards|[27] from the greater destruction of the females; or whether it indirectly |follows|[28] from the practice of polygamy. (1871, Vol. 1, p. 271; 1874, pp. 220–221).

4. One possible effect of sexual selection on sex ratio at birth: The excess of male stillbirths (1871 & 1874)

The Male generally more modified than the Female. [. . .] The great eagerness of the |males|[29] has thus indirectly led to |their much more frequently developing secondary sexual characters than the females|.[30] (1871, Vol. 1, pp. 271 and 274–275; 1874, pp. 221 and 223).

|Prof. Faye remarks that|[31] 'a still greater preponderance of males would be met with, if death struck both sexes in equal proportion in the womb and during birth. But the fact is, that for every 100 still-born females, we have in several countries from 134.6 to 144.9 still-born males. |During the

[21] Replaces "a considerably larger proportion of males than of females die" (1871).
[22] Replaces "few" (1871).
[23] "is considerably larger than that of females" not in 1871.
[24] Replaces "preponderance in number of the males over the females would" (1871).
[25] Replaces "was" (1871).
[26] Replaces "occurred" (1871).
[27] Replaces "subsequently" (1871).
[28] Replaces "followed" (1871).
[29] Replaces "male" (1871).
[30] Replaces "the much more frequent development of secondary sexual characters in the male than in the female" (1871).
[31] Replaces "In various parts of Europe, according to Prof. Faye and other authors," (1871).

first four or five years of life, also, more male children die than females,|[32] for example in England, during the first year, 126 boys die for every 100 girls – a proportion which in France is still more unfavourable.'[33] |Dr. Stockton Hough accounts for these facts in part by the more frequent defective development of males than of females. We have before seen that the male sex is more variable in structure than the female; and variations in important organs would generally be injurious. But the size of the body, and especially of the head, being greater in male than female infants is another cause: for the males are thus more liable to be injured during parturition. Consequently the still-born males are more numerous; and, as a highly competent judge, Dr. Crichton Browne,[34] believes, male infants often suffer in health for some years after birth|.[35] |Owing to|[36] this excess in the death-rate of male children, |both at birth and for some time subsequently, and owing to|[37] the exposure of |grown men|[38] to various dangers, and |to|[39] their tendency to emigrate, the females in all old-settled countries, where statistical records have been kept,[40] are found to preponderate considerably over the males. (1871, Vol. 1, pp. 302; 1874, pp. 243–244).

[32] In 1871, this passage was not inserted in the quotation. Darwin summarized Faye in these terms: "Moreover during the first four or five years of life more male children die than females". Then he returned to the quotation, starting from "for example…".

[33] Darwin's footnote, since 1871: " 'British and Foreign Medico-Chirurg. Review', April, 1867, p. 343. Dr. Stark also remarks ('Tenth Annual Reports of Births, Deaths, &c., in Scotland', 1867, p. xxviii) that "These examples may suffice to shew that, at almost every stage of life, the males in Scotland have a greater liability to death and a higher death-rate than the females. The fact, however, of this peculiarity being most strongly developed at that infantile period of life when the dress, food, and general treatment of both sexes are alike, seems to prove that the higher male death-rate is an impressed, natural, and constitutional peculiarity due to sex alone."

[34] Darwin's footnote, not in 1871: " 'West Riding Lunatic Asylum Reports', vol. i, 1871, p. 8. Sir J. Simpson has proved that the head of the male infant exceeds that of the female by 3–8ths of an inch in circumference, and by 1–8th in transverse diameter. Quetelet has shown that woman is born smaller than man; see Dr. Duncan, 'Fecundity, Fertility, and Sterility', 1871, p. 382."

[35] "Dr. Stockton Hough accounts… after birth" not in 1871.

[36] Replaces "As a consequence of" (1871).

[37] Replaces "and of" (1871).

[38] Replaces "men when adult" (1871).

[39] Replaces "of" (1871).

[40] Darwin's footnote in 1871: "With the savage Guaranys of Paraguay, according to the accurate Azara ('Voyages dans l'Amérique mérid.', tom. ii, 1809, pp. 60, 179), the women in the proportion to the men are as 14 to 13." (1871). In 1874, the last words were changed to "the women are to the men in the proportion of 14 to 13".

5. A second possible effect of sexual selection on sex ratio at birth: The excess of illegitimate female births (1871 & 1874[41])

|It seems at first sight a mysterious|[42] fact that in different nations, under different conditions and climates, in Naples, Prussia, Westphalia, |Holland, France, England and the United States|,[43] the excess of male over female births is less when they are illegitimate than when legitimate.[44] |This has been explained by different writers in many different ways, as from the mothers being generally young, from the large proportion of first pregnancies, &c. But we have seen that male infants, from the large size of their heads, suffer more than female infants during parturition; and as the mothers of illegitimate children must be more liable than other women to undergo bad labours, from various causes, such as attempts at concealment by tight lacing, hard work, distress of mind, &c., their male infants would proportionably suffer. And this probably is the most efficient of all the causes of the proportion of males to females born alive being less amongst illegitimate children than amongst the legitimate. With most animals the greater size of the adult male than of the female, is due to the stronger males having conquered the weaker in their struggles for the possession of the females, and no doubt it is owing to this fact that the two sexes of at least some animals differ in size at birth. Thus we have the curious fact that we may attribute the more frequent deaths of male than female infants, especially amongst the illegitimate, at least in part to sexual selection.|[45] (1871, Vol. 1, pp. 301–302; 1874, pp. 244–245).

6. Other possible factors in an unequal distribution of the sexes at birth (1871 & 1874)

It has often been supposed that the relative age of the |two|[46] parents determine [*sic*] the sex of the offspring; and Prof. Leuckart[47] has advanced what he considers sufficient evidence, with respect to man and certain

[41] In the 1871 edition, illegitimate births appear as a quick excursus, following the discussion on the variability of the proportion of the two sexes, not the discussion of stillbirths.
[42] Replaces “It is a still more singular” (1871).
[43] Replaces “France and England” (1871).
[44] Darwin’s footnote in 1874: “Babbage, ‘Edinburgh Journal of Science’, 1829, vol. i., p. 88; also p. 90, on still-born children. On illegitimate children in England, see ‘Report of Registrar-General for 1866’, p. xv”. See Babbage (1829) and Registrar-General (1869).
[45] “This has been explained…at least in part to sexual selection” not in 1871.
[46] “two” not in 1871.
[47] Darwin’s footnote, since 1871, with minor typographic alterations: “Leuckart, in Wagner’s ‘Handworterbuch der Phys.’, B[and] iv, 1853, [p]. 774”. See

domesticated animals, |that this is one important though not the sole factor|[48] in the result. So again the period of impregnation |relatively to the state of the female|[49] has been thought |by some|[50] to be the efficient cause; but recent observations discountenance this belief.[51] |According to Dr. Stockton Hough,[52] the season of the year, the poverty or wealth of the parents, residence in the country or in cities, the crossing of foreign immigrants, &c., all influence the proportion of the sexes. With|[53] mankind, polygamy has |also|[54] been supposed to lead to the birth of a greater proportion of female infants; but Dr. J. Campbell[55] carefully attended to this subject in the harems of Siam, and concludes that the proportion of male to female births is the same as from monogamous unions. Hardly any animal has been rendered so highly polygamous as |the|[56] English race-horse, and we shall immediately see that his male and female offspring are almost exactly equal in number. (1871, Vol. 1, pp. 302–303; 1874, p. 245).

7. Natural selection and the rebalancing of the sexes at birth (1871)

On the power of natural selection to regulate the proportional numbers of the sexes, and general fertility.[57] In some peculiar cases, an excess in the number of one sex over the other might be a great advantage to a species, as with the sterile females of social insects, or with those animals in which more than one male is requisite to fertilize the female as with certain cirripedes and perhaps certain fishes. An inequality between the sexes in these cases might have been acquired through natural selection, but from their rarity they need not here be further considered. In all ordinary cases an inequality would be no advantage or disadvantage to certain individuals more than to others; and therefore it could hardly have resulted from natural

Leuckart (1853), which highlighted the direction explored by Notter and Hofacker (1827).

[48] Replaces “to shew that this is one important factor” (1871).

[49] “relatively to the state of the female” not in 1871.

[50] “by some” not in 1871.

[51] Here, in 1871, Darwin did not reference the argument, but turned his attention to polygamy.

[52] Darwin’s footnote, not in 1871: “‘Social Science Assoc. of Philadelphia’, 1874”.

[53] The sentence “According to Dr. Stockton Hough… all influence the proportion of the sexes” replaces the word “Again”, which appeared before the word “with” in 1871.

[54] “also” not in 1871.

[55] Darwin’s footnote, since 1871 : “ ‘Anthropological Review’, April, 1870, p. cviii”.

[56] Replaces “our” (1871).

[57] Section heading, 1871, p. 315.

selection. We must attribute the inequality to the direct action of those unknown conditions, which with mankind lead to the males being born in a somewhat larger excess in certain countries than in others, or which cause the proportion between the sexes to differ slightly in legitimate and illegitimate births.[58]

Let us now take the case of a species[59] producing, from the unknown causes just alluded to, an excess of one sex – we will say of males – these being superfluous and useless, or nearly useless. Could the sexes be equalised through natural selection? We may feel sure, from all characters being variable, that certain pairs would produce a somewhat less excess of males over females than other pairs. The former, supposing the actual number of the offspring to remain constant, would necessarily produce more females, and would therefore be more productive. On the doctrine of chances, a greater number of the offspring of the more productive pairs would survive; and these would inherit a tendency to procreate fewer males and more females. Thus a tendency toward equalisation of the sexes would be brought about.[60] [...].[61]

Nevertheless we may conclude that natural selection will always tend, though sometimes inefficiently[62], to equalise the relative numbers of the two sexes. (1871, Vol. 1, pp. 315–316, p. 318).

8. The puzzle of the *production of the two sexes* (1874)

The proportion of the sexes in relation to natural selection.[63] [...] Besides the several causes previously alluded to, the greater facility of parturition amongst savages, and the less consequent injury to their male infants, would tend to increase the proportion of live-born males to females. [...]

[58] Here Darwin finally mentions directly the major question of the imbalance of the sexes at birth in humankind.

[59] This classic example coincides exactly with the human species.

[60] This second paragraph, which in the end was erased from the second edition, is nowadays read by several commentators as anticipating the conclusions outlined by Düsing (1884) and the principle stated by Fisher (1930).

[61] Darwin goes on to discuss various objections that in the end all come down to the same point.

[62] The case of excess male births in the human species results, in Darwin's view, from this inefficiency.

[63] Section heading, 1874, p. 255.

As the males and females of many animals differ somewhat in habits and are exposed in different degrees to danger, it is probable that in many cases, more of one sex than of the other are habitually destroyed. But as far as I can trace out the complication of causes, an indiscriminate though large destruction of either sex would not tend to modify the sex-producing power of the species. With strictly social animals, such as bees or ants, which produce a vast number of sterile and fertile females in comparison with the males, and to whom this preponderance is of paramount importance, we can see that those communities would flourish best which contained females having a strong inherited tendency to produce more and more females; and in such cases an unequal sex-producing tendency would be ultimately gained through natural selection. With animals living in herds or troops, in which the males come to the front and defend the herd, as with the bisons of North America and certain baboons, it is conceivable that a male-producing tendency might be gained by natural selection; for the individuals of the better defended herds would leave more numerous descendants. In the case of mankind the advantage arising from having a preponderance of men in the tribe is supposed to be one chief cause of the practice of female infanticide.

In no case, as far as we can see, would an inherited tendency to produce both sexes in equal numbers or to produce one sex in excess, be a direct advantage or disadvantage to certain individuals more than to others; for instance, an individual with a tendency to produce more males than females would not succeed better in the battle for life than an individual with an opposite tendency; and therefore a tendency of this kind could not be gained through natural selection. Nevertheless, there are certain animals (for instance, fishes and cirripedes) in which two or more males appear to be necessary for the fertilisation of the female; and the males accordingly largely preponderate, but it is by no means obvious how this male-producing tendency could have been acquired. I formerly thought that when a tendency to produce the two sexes in equal numbers was advantageous to the species, it would follow from natural selection, but I now see that the whole problem is so intricate that it is safer to leave its solution for the future. (1874, p. 255, pp. 258–260).

Appendix C
MAURICE HALBWACHS (1877–1945)

Extracts from *"Recherches statistiques sur la détermination du sexe à la naissance"*[1] (1933)

[1933, p. 164; 2005, p. 381][2]

Perhaps statistics really began when it was recognized that the proportion of births of boys – and of girls – remains more or less constant.[3] But why does it do so? This question has continued to preoccupy statisticians. It is a classic problem, and there is all the more reason to examine it again because, despite more than a century of looking for an answer, the solution can hardly be said to have been found.

A secular trend, with an exceptional variation in the years 1918 to 1920

[1933, pp. 166–168; 2005, pp. 383–384]

The proportion of male births is said to be more or less constant. Even so, let us review it over the whole of the last century,[4] or even for up to 120 years, in France. We shall see that it has fallen slightly: an almost imperceptible decrease from one year to the next, from one decade to the next, but never – at least up until the War[5] – a continuous rise. Since this

[1] We are publishing here extensive extracts from Maurice Halbwachs' article "*Recherches statistiques sur la détermination du sexe à la naissance [Statistical research on the determination of sex at birth]*" (1933), on which we give a commentary on pp. 134–136. The author himself drew on part of it again for a section of the text *Le Point de vue du nombre [The standpoint of number]*, which appeared in the *Encyclopédie française* in 1936. Both versions have been recently republished (2005).

[2] Page references are to the original edition (1933) and the recent new edition (2005), where the reader can consult the detail of these arguments in French.

[3] Halbwachs knew, and mentioned, most of the major works published before his time, from Graunt (1662) to Gini (1908). However, he seems not to have known the manuscript of Condorcet's *Fragment 10*, published by Cahen in 1922.

[4] In 1933, the 19th century.

[5] The First World War.

was a slow decrease, it is possible to calculate the proportion of male births on average over fairly extensive periods, and the deviation, also on average, from that proportion. [...]

Let us now consider the period 1891 to 1929: it is striking that, over this thirty-nine years, the mean deviation[6] of 0.491 was very clearly exceeded in the three years 1918, 1919 and 1920, when the deviations were 1.980, 1.280 and 1.940 respectively. This variation appeared and disappeared abruptly: the two deviations immediately before it were 0.430 and 0.210; the two after it, 0.380 and 0.510. The 1919 deviation is almost three times the mean deviation, while the deviations for 1918 and 1920 are both far larger still, at four times the mean. Not since 1891 – in fact, not even since 1811 – have such large deviations been found; and, in particular, none that have been repeated three years running. [...]

Thus, this is the first time since male and female births have been recorded in France that such a marked variation has been discovered. The only comparable irregularity, which appeared 116 years earlier, was a great deal smaller.

If, in ten or twenty centuries, all memory has been lost and no trace remains of the historical, political and military events that took place in France in the 19th century and the first quarter of the 20th century, and yet a statistical report is found showing, year by year, the ratio of births of boys to births of girls throughout that period of 130 years, it would have to be assumed that in around 1802–1803 – in those years themselves or in the years just before them – and, similarly, in around 1918–1919, some major upheaval must have taken place in the physical or social environment, since the balance in the ratio of births of the two sexes, usually maintained from one year to the next, was so clearly disrupted. It is natural that we should immediately think of the wars of the Revolution and of the Empire and also of the Great War that ended in 1918. But how could such events have altered the ratio of the sexes at birth? [...]

[6] This must be understood as the mean of the deviations, in this case taken on the absolute values of the deviations. We should clarify that Halbwachs was very familiar with the mathematical statistics of his time (see Fréchet & Halbwachs, 1924) and that the use of standard deviations and of tests that are common nowadays became widespread in Europe only from the inter-War period, and especially after the Second World War. This paper, presented to a meeting of the leading French statistical society of that era, is evidence of a knowledge acquired before these became accepted, as well as of a reasonable competence.

A cyclical phenomenon?
[1933, pp. 171–172; 2005, pp. 386–387]

We are [. . .] led to ask ourselves two questions:

First, if it is true that, from the beginning of the War, the age difference between spouses became smaller, and if in fact that was the cause of the increase in male births, how does it come about that male births increased (in proportion) only from 1918 onwards and not from 1916? We should note that, in normal times, almost half of newborn babies (48%) are first-born children. Let us assume that these first-born children come from marriages that took place in the preceding year. A marked variation in the proportion of boys among first-born children will become apparent in the total. But, during the War, the number of new marriages – which means the number of first-born children – decreased significantly: in 1915 it fell below a third of what it had been in 1913, and in 1916 it remained below half. First-born children therefore represented a much lower proportion of newborns – barely a fifth. Naturally, variations that affected first-born children disappeared within the overall total. In contrast, the number of marriages increased very quickly in 1917, and even more so in 1918 and 1919 (for 100 marriages in 1913, the figures were 65, 73 and 198 respectively). If we add together children born in 1918 of these unions and children born in the same year to marriages that had taken place in 1915 and 1916 (in the same abnormal conditions of parental age gap), their proportion to the total number of newborn babies reaches and even exceeds its pre-War value. This therefore explains why the variation that applies to them (the higher proportion of boys) appears then, and only then;

Second, if it is true that the reduction in the mean age gap between spouses during the War tended to produce a larger relative number of boys, how does it come about that, after 1919 and in the two following years, when this age gap became much smaller still, the proportion of male births fell, returning almost to its pre-War value? In fact, men got married in large numbers immediately after the War. This still left a very large female population, and a significantly reduced male population. Women were forced to marry increasingly younger men. The age gap between spouses was, and was to remain, much smaller than during the War. However, the proportion of boys among newborn babies did not rise, but fell. Thus, after a change in the distribution of households according to age gap, births of boys increase; a bigger change, in the same direction, does not increase them. Must it be

concluded from this that the age gap between the parents has no influence on the sex of children?

But it could be that the relationship between the two terms is not a simple one, that the increase of the first up to certain point entails a growth in the second, but that past that point it has an inverse effect. This would be a cyclical movement. This is our hypothesis. Can it be directly confirmed?

Study of birth records in the Bas-Rhin *département*
[1933, pp. 175–178; 2005, pp. 389–391]

For every birth certificate that is issued, register office officials complete an index card showing the ages of the father and the mother (in round years), the sex of the registered child, the number of children born to the same mother both since and before the marriage and their sex. These index cards are sent to the General Office of Statistics for France.[7] We were able to get information from the Office of Statistics for Alsace and Lorraine[8] about the cards completed in 1925 for the two *départements* of Bas-Rhin and Haut-Rhin. We confined our study to the Bas-Rhin index cards. From these, we took the number and sex of live births since marriage (including the child being registered on that occasion) and the age of the parents. [. . .]

The index cards were grouped into two bundles, corresponding to registrations of male and female births, and then arranged in increasing order of parental age within each bundle. As we could not copy all of them (there were 36,000), we restricted ourselves to following through the whole series in order of presentation, keeping one index card out of four (the first, the fifth, the ninth, etc).[9] Thus there was a good chance, in proportion, that the cases we kept would number about the same in each category as in all the cases. [. . .] It seems likely [according to our study of these records] that the proportion of male births does not vary with absolute age, but with parental age difference. [. . .] Given the method that we followed (methodical

[7] *Statistique générale de la France* (S.G.F.), now the French National Office of Statistics, *Institut national de statistique et d'études économiques* (I.N.S.E.E.).

[8] Halbwachs was a professor at the University of Strasbourg, in Alsace. The Office of Statistics referred to, which was specifically for the territories regained by France from Germany after 1918, was then the only regional office of statistics in France. Its infrastructure and expertise derived directly from the German experience in the period 1871–1918.

[9] This technique, known as "methodical randomization", was known to German and French statisticians of the day.

randomization), it seems likely that a review of all 36,000 births would have given results very much in the same neighbourhood.[10]

On the other hand, if we start from Category 0 (both parents the same age) – at which the ratio reaches its maximum – and follow the variation either in the negative direction (mother older than father) or in the positive direction (the opposite), we observe that the variation is the same; in other words, we find almost the same proportion of male births for a given age gap, whether it is the father or the mother who is the older of the two. And the same is true (in the three categories that give us an adequate number of male births) whether the households are young ones or older ones. [. . .]

One is sometimes struck by the fact that, in a family with four, five or six children, all are the same sex, and one wonders if there are "families who have boys" and "families who have girls". The index cards we studied were, as we have said, arranged in two bundles – registrations of boys and registrations of girls. It could be assumed that, if there are indeed "families who have boys", there is a good chance that a larger number of them will be found in the first bundle. So, for each of the two categories, we calculated the proportion of boys by eliminating the last birth (in the first category, all these were male; in the second, all female). We found that the proportion of male births was very obviously the same in both categories. We re-did the same calculation using the data from our next, much more extensive survey, and got the same result. Therefore, there is no reason to suppose that, if the first child, or the first two, are boys, the others will be the same sex; similarly for girls. [. . .]

Records of allowances paid to large families in the Bas-Rhin *département* [1933, pp. 178–183; 2005, pp. 391–395]

How could we find out, alongside the number and sex of the children, not just the age of the parents in round years, but their date of birth, in order to be able to calculate the exact age gap, in months, between the father and the mother?

In France, for some ten years, every time a newborn baby is registered, register offices have been recording the father's and mother's dates of birth. But it would take a long time to peruse all these registers. Even if we gave up the idea of finding all the children born to each household (which would

[10] We should note that Halbwachs does not give the confidence interval here, even though, from a technical point of view, he was in a position to do so (see Fréchet & Halbwachs, 1924).

be difficult, except in small municipalities),[11] we would have to look at all births indiscriminately over a period, copying the parents' dates of birth for each one. This would be the best method: but we would need a whole team of researchers. [. . .][12]

Then there were the allowances awarded by the *départements* to large families for some years. This time we were lucky enough to find a large series of records for the Bas-Rhin *département*, from which we were able to draw the following data: 1st the number of children of each sex, for each household with at least four children, legitimate or legitimated, still living.[13] The sex of each child is not indicated, but the forename is. We discarded cases where the forename could be that of either a boy or a girl;[14] 2nd the father's and mother's dates of birth. These are copied onto the form completed by the mayor of the municipality, on his own responsibility. "The undersigned mayor certifies the accuracy of the above declarations, having verified the documents presented and in particular the official family record book." The father's and the mother's birth certificates usually accompany the record of the allowance paid, so – as far as possible – we compared the certificates themselves with the form completed by the mayor and discarded all cases where the two registrations did not agree. It is possible, on the basis of the date of the marriage, which is also recorded, to exclude children born before the marriage, illegitimate children (who anyway are not taken into account when calculating the allowance) and children born to the same mother or to the same father, but in a previous marriage.[15]

Besides, children born to the same mother, but not the same father, do not have the same surname. Where children were born to the same father

[11] Halbwachs did check this for four "small municipalities" in different parts of France; he gave the preliminary results of this survey earlier in the article, in a section that we have not included here. The "best method" that he describes would be developed by others: in France, after the Second World War, it would take the form of the family reconstruction technique used in the Henry Survey in historical demography (Rosental, 1996; Séguy, 2001).

[12] In this era, university academics in France – and especially philosophers, among whom were counted sociologists – did not enjoy research facilities like those which developed after the late 1930s.

[13] There is a bias here. Halbwachs attempts to deal with this objection further on.

[14] This is a new bias, but Halbwachs does not return to this one. As a Durkheimian sociologist, with a close interest in systems of representations, he ought to have been concerned about it.

[15] Implicitly, Halbwachs studied only the couples' legitimate offspring. Here again, the object that he wanted to capture was reduced according to a principle of which, as an author, he was not very critical. In fact, throughout the article, Halbwachs uses a series of arbitrary cross-sections, all capturing the same object from different angles.

in an earlier marriage, this was most often mentioned expressly. This type of study, as can be seen, demands some attention, and is not always free of the risk of error. This was an additional reason to extend our investigation to the largest number of cases that could be obtained.[16]

The Archives Office of the Bas-Rhin *département* gave us access to these records of allowances, with the permission of the prefect. But we cannot describe our study as archive research. This is because, in order to include a large number of cases, we had to restrict ourselves to keeping only the numerical data that we wanted to use from each record; and therefore we ignored, among other items, the date of the marriage, the dates of birth of the successive children and their birth order, as well as the parents' names, place of residence and occupations, and all the information about their financial situation.[17] We started from the oldest files and followed the whole series in order of presentation up to the most recent files, without any break, over a period of about five years. There were certainly some households that we found several times, but with a different number of children, so that we were able to treat them as new cases; moreover, they represented only a very small proportion of the whole. In total, taking only the cases we actually used, our survey focused on 50,561 births for age gaps from −30 months (where the woman was older than the man) to +132 months, or from $-2^1/_2$ years to +11 years, out of about 56,500 that we found for all age gaps.

We devoted more than three hundred hours, spread over a year, to this study.[18] Given that each file gave us on average five births, we would have needed about five times as long if we had had to take the same number of pieces of data from entries in registers, where only one birth is recorded on each occasion. The fact that no one has done the same work before is probably because this source no longer exists, and – when it did exist – no one dreamt of using it for this purpose.

We should now anticipate two objections. Firstly, we confined ourselves to large families and to somewhat older households. But it has long been believed

[16] Loyal to Quetelet, Halbwachs has great faith in a law of large numbers and, like so many of his contemporaries, underestimates the risk of systematic errors – although he does have some idea of this.

[17] Here Halbwachs, as a sociologist, is lamenting the considerable reduction in information resulting from the use of a statistical technique. This topic touches on an issue that is very much alive nowadays in discussions of method in the social history of populations.

[18] Like his German counterpart Max Weber and the specialists of his day, Maurice Halbwachs worked "by hand" and himself performed the most humble tasks required by his science.

that the absolute age of the parents has no influence on the proportion of births of each sex, and the results of our previous survey did seem to indicate this. [. . .] The second objection is more serious. We know that, although more boys are born, more of them also die in the early years, so that the balance is re-established after some time, and there are even slightly more girls in total.[19] Our households, at the time we found them, had at least four children. We do not know the number or sex of those who had died since the birth of the first. But the higher mortality of boys emerges from the fact that the mean proportion of boys, for our whole total, was 101 (to be exact, 100.85) per 100 girls [50.21%], as opposed to 105.5 [51.34%] on average in Bas-Rhin for the four years 1925–1928. This difference does not pose any danger to large numbers; but where we are working on categories that contain only a small number of cases, that is no longer true. There is only one means of negating this cause of error: to increase the number of observations. To compare the results of our survey with others where births (and not children still living at the present time) have been counted, it is necessary to multiply the proportions that we found by 1.04 (the ratio of 105.5 to 101). If we confine ourselves to the current data, the results of comparisons established between them retain all their value, since all the items are equally affected by the same cause.[20]

[. . .W]e should ask ourselves whether they confirm or refute the Hofacker-Sadler theory,[21] so much decried nowadays, and whether it is correct, as those authors believed, that when the father is older male births increase, and when the mother is older, the opposite.

Parental age difference (in years)	Boys	Girls	Number of boys per 100 girls	[95% confidence intervals][22]
−2.5 to 0[23]	3 333	3 348	99.55	[48.66%–51.11%]
0 to 11	22 056	21 824	101.05 [101.06][24]	[49.79%–50.74%]

[19] The idea that "the balance is re-established" is presented here rather simply, and in an intellectual tradition going back to Süssmilch (1741). The phenomenon is actually much more complex. See in Chapter 1 p. 6.

[20] This was how Halbwachs hoped to escape the empirical biases that his sources entailed.

[21] See above, pp. 66–69.

[22] This column, which does not appear in the 1933 or 1936 editions, is ours (on this statistical criterion, see in Chapter 5, pp. 134–136). The same is true for the corrections to Halbwachs' rounding, shown in square brackets.

[23] None of the couples studied consisted of parents born on the same day. Here and on the line below, it must be understood that the case "0" is excluded to within a day.

[24] Rounded incorrectly.

It can be seen that this result conforms to the Hofacker-Sadler law: there is a difference between the two proportions of male births (older father, older mother) of 1.50 in favour of households where the father is the older.[25]

These two statisticians further added that the proportion of male births was higher still when both parents were the same age. Let us take parents where one is no more than a year older than the other as being the same age, and let us calculate the proportion of male births for the following age differences:

Parental age difference (in years)	Boys	Girls	Number of boys per 100 girls	[95% confidence intervals][26]
Below −1	1 329	1 433	92.74	[46.21%–50.02%]
From −1 to +1	4 941	4 765	103.70 [103.69]	[49.89%–51.92%]
Below +1	19 119	18 974	100.75 [100.76]	[49.68%–50.70%]

We can, therefore, say that this second part of the Hofacker-Sadler law is also confirmed,[27] and, with these new total numbers, the difference between cases where the father is older and opposite cases appears much more marked than with the preceding numbers. It is true that, for age gaps below −1 year, we have only a limited number of births. But this is probably the first time that it has been possible to subject the law in question to precise, detailed confirmation. Moreover, we shall see that it corresponds to a very general view of our phenomenon, and that the predominance of boys cannot be explained solely – nor even especially, as these two statisticians believed – by the fact that households where the father is older than the mother are the most numerous. [...]

Relationship between the proportion of births of boys and parental age difference

[1933, pp. 184–191; 2005, pp. 395–400]

From now on, let us confine ourselves to the detailed series of numbers (proportion of male births) calculated according to age differences increasing by six months at a time.[28] [...] No doubt the curve that represents such a

[25] 1.50 is the difference between 101.05 and 99.55. But the conclusion is wrong, as the overlapping 95% confidence intervals in the right-hand column of the table show.

[26] Same principles as in the previous table.

[27] This new conclusion is wrong, for the same reasons as the preceding one.

[28] This criterion for defining age gap intervals is a particular feature of Maurice Halbwachs' work. He returns to its importance at the end of the article.

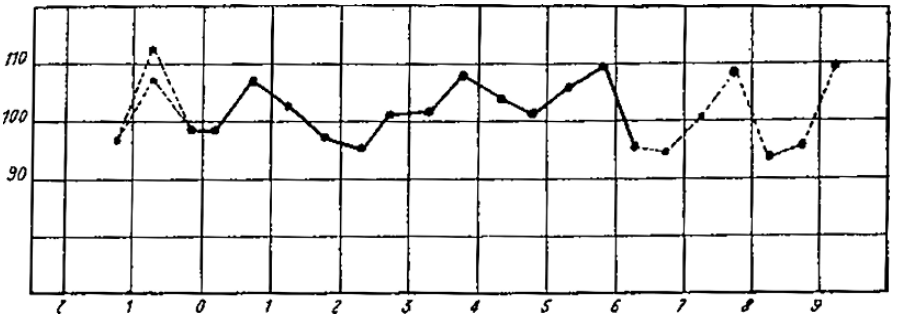
movement includes many irregularities: the maxima are in more or less the same neighbourhood, but one of the three minima is far below the others; on the other hand, [a] first cycle extends across three years, while the two others each extend over only two; and it seems that as we move towards the bigger age differences, the period narrows even more. However, this is a cyclical curve, and, if it is imperfect, that is because we are in the sphere of life whose complexity most likely does not yield to the regularity of mathematical expressions. In any case, it can be stated that the results of a game of chance would not line up in this kind of order.[29] These variations are *real*. There is certainly a relationship between the values taken by the proportion of male births and the age difference between the parents. In the complex problem of how sex at birth is determined, this is the only factor whose effect we have, up to now, been able to recognize and measure.

Let us now recall the hypothesis that we formulated after studying the ratio of male to female births in France throughout the War and up to the most recent years. We noted that the value of this ratio increased abruptly and very markedly in the three years 1918, 1919 and 1920, following the wartime period during which the mean age difference between spouses had become markedly smaller. Was the latter the cause of the former? However, after 1920 and for about ten years thereafter, the mean age difference between spouses became even smaller still. But the proportion of male births – instead of rising – fell, returning almost to the pre-War level. Was there therefore no relationship between parental age gap and this proportion? Or should we instead assume that this relationship was subject to variations such that, two terms being linked, the fall in one to a certain point entails an increase in the other, but once this point is passed, it causes a decrease? We therefore assumed that this relationship perhaps varied in a kind of cyclical function. Direct observation of births, distinguished by sex, according to increasing parental age difference, teaches us that the ratio between the two terms – parental age difference and proportion of male births – does indeed vary as a cyclical function [...]. It seems, therefore, that the facts we have just described, as we were able to observe them, confirm our hypothesis. [...]

Having noted that in 1918, 1919 and 1920 the proportion of male births rose in an apparently abnormal way, reaching values that had not been encountered during the preceding decades, and that nor, moreover, had

[29] Here again, the conclusion is conceptually admissible but lacks any measured criterion. It can be shown that the fluctuations highlighted by Halbwachs remain within an uncertainty band that corresponds to a 95% confidence interval for each age gap group, and that it is futile to try and save the "Hofacker-Sadler law" (see 2005, pp. 172–175).

Ratio of male births to female births
according to parental age difference
(from 50,561 births in the Bas-Rhin *département*)
Courtesy of I.N.E.D. (published in Halbwachs, 2005, p. 396)



The horizontal divisions represent parental age differences in years – positive ones to the right of zero and negative ones, where the mother is older than the father, to the left. The points on the curve are positioned in the middle of the intervals corresponding to six-month increases in these differences (from 0 to 6 months, from 6 months to 12, etc.). The vertical divisions represent the proportion of births of boys per 100 girls. On the left of 0, the lower dotted line corresponds to the values found for a slightly smaller number of births.

any variation of this size occurred during the whole preceding century, we wondered whether this proportion was in line with a corresponding variation in the mean parental age difference. In fact, during the War, the age gap between spouses became markedly smaller. But it diminished even more after 1920, remaining very low until recent years; however, from 1921 onwards, the proportion of male births has fallen, tending to settle at the pre-War level. We can therefore formulate the following hypothesis: when parental age gap decreases within certain limits, the proportion of male births rises; the proportion of male births falls when this limit is exceeded, and when the age gap becomes far smaller still. A series of direct investigations – the last of which looked at 50,500 births, which we were able to classify by sex according to a parental age difference increasing by six months at a time – enabled us to recognize that the relationship between these two terms could be represented as a cyclical function, with the proportion of male births progressing continuously through a sequence of maxima and minima as the parental age difference increases. Thus, our hypothesis seemed to be confirmed.

More generally, there would be good reason to take into account anew the theory – formulated almost a century ago, and criticized and dismissed by statisticians who have studied this problem most recently – that parental age difference exercises a very clear influence on the proportion of male births. Although it was believed that this was belied by the facts, that was because statistics were used that showed this proportion only for parental age differences increasing by five years at a time. In such averaged figures, the real variations disappear: they reappear when these differences are increased by smaller quantities – of one year and, especially, of six months.

The facts that we observed were most certainly limited. Our final piece of research related to a single *département* and looked at only some of the births that had taken place there. However, it seems to us that the only way forward towards a solution is through a precise, limited study, in which we do our best to look beneath averages that correspond to some very large numbers but are confused, to the realities they conceal. [. . .]

The ratio of the sexes at birth is not simply a curious fact. It poses a very big problem. We do not under-rate the importance of the calculus of probabilities and of its applications in the sciences. But, if births were comparable to chance facts, then one of the essential functions of organic life would be subject to purely mathematical or mechanical laws.[30] Of course, in a sense, it is a matter of chance whether a boy or a girl is born, and there is perhaps a profound symbolic truth in the old notion that births were related to the conjunction of the stars.[31] But a given age distribution within

[30] This anti-mechanistic passage echoes the vitalism of Henri Bergson, of whom Maurice Halbwachs was an attentive pupil. However, in the first place it arises from the sociologist's adherence to the conception of the social fact by his master, Émile Durkheim (1895).

[31] Laplace, Quetelet and Durkheim are here in the background, and this "profound symbolic truth" results from the analogy between two combinations of chances, general laws and subjective lack of comprehension. Indeed, Durkheim wrote about one configuration of transmission of totems: "here, the totem of the child is not necessarily either that of the mother or that of the father; it is that of a mythical ancestor who came, by processes which the observers recount in different ways, and mysteriously fecundated the mother at the moment of conception. A special process makes it possible to learn which ancestor it was and to which totemic group he belonged. But since it was only chance which determined that this ancestor happened to be near the mother, rather than another, the totem of the child is thus found to depend finally upon fortuitous circumstances" (Durkheim, 1912 [1979, p. 150]; translation by Swain, 1915/1965).

a group is not a matter of chance; and if this distribution explains, at least in part, the ratio of the sexes at birth, then the birth, in a given society, of a given proportion of boys is not a matter of chance either.[32]

[32] Although Halbwachs' result on the "cyclical" dependence between parental age gap and the probability of the children's sex cannot be accepted, this concluding sentence remains pertinent (see above, Chapter 6).

Appendix D

SEX RATIOS AT BIRTH AND THE CALCULUS OF PROBABILITIES

This Appendix aims to clarify a common thread that joins several technical aspects of the study of sex ratios and to provide a common basis for criticizing them according to criteria derived from mathematics, sociology or biology, or from the history of these sciences. We are well aware that a specialist in each of these different fields might take the view that it is "too much or too little". Nevertheless, this note remains necessary in facing our principal difficulty: the fact that collective memories have differed between the various disciplines for three centuries and, in that time, they have already been subjected to various combinations – so we must give careful consideration to any linkages that we want to construct between them.

1. Two traditional indicators

Two indices that express the regularity of ratios between numbers of births according to sex have run through the literature for a long time.

• The ratio of chances of the two sexes

Graunt (1662) and Süssmilch (1741) used the number of boys per hundred girls in a given place and within a given period (for example, 105 boys per hundred girls). Letting M be the number of male births, F the number of female births, N their total ($N = M + F$), this rate is $100M/F$ (called Γ from now on).

Laplace (1778) also employed the ratio M/F (for example 41/39). Conforming to the older usage of considering the chances of different possible cases, Cournot (1843) interpreted this ratio as the number of *chances* of giving birth to a boy to the number of *chances* of giving birth to a girl.

• The proportion of one of the sexes

This time, looking at the proportion of boys among births, $S = M/N$ *or* $M/(M + F)$ (or, for the proportion of girls, taking $1 - S$). This index was

sometimes used by Laplace (1778), but most systematically in Poisson's work (1830). They both made use of decimal notations (e.g., 0.5125), whereas today we most often use percentages (in this example, 51.25%). This represents the *frequency* of male births. Cournot (1843) recognized here a *probability* in the sense of a value taken between 0 and 1, which expresses the possibility of the birth of a boy.

2. Historical Markers

Since Jakob Bernoulli (1713), Laplace (1778, 1812) and Poisson (1830), who used mathematical proofs that involved sometimes different conceptions of the mathematics of chances, the *frequency* of these observed events has been linked to the *probability that can in principle be assigned* to each occurrence of these supposedly similar events (the birth of a boy, for example). Several 18th-century authors commented on this link. Thus, Laplace set out to measure the *probability* of a *cause* (that is, the greater or less facility of births of boys) by counting the *events* that it produces (that is, actual births).

Between 1772 and 1830, Laplace (1778), Condorcet (1786), Laplace again (1812), Fourier (1821) and Poisson (1830) worked within an area where measurement error calculation and calculation of the regularity of observed frequencies intersected. Quetelet (e.g. 1846) proposed the view that the two questions were analogous – which simplified their solutions, at least for practical purposes.

The calculation of the proportion of one sex among births was the topic favoured for all these discussions. This scenario is sometimes compared to a series of games of heads or tails (with the coin loaded or not), sometimes to a series of blind throws of two types of balls (or of tickets or of beans) placed in the same urn. The proportion of balls of each type (of each colour, for example) could be balanced (50%–50%), imbalanced or unknown. So many stylized experiments helped scholars to make their proposed calculations understood.

From the mid-18th century, scholars became aware that it was dangerous to make a direct comparison between something concrete that could be counted and a game of chance. The works of Laplace and of Condorcet in the 1770s and 1780s, and those of Gauss, Laplace, Fourier and Poisson during the early decades of the 19th century helped to strengthen an *analytical* conception of the calculus of probabilities, based on integral calculus processes and no longer simply on the relationship between possible

cases counted and favourable cases counted. We should add that the mathematics of chances did not end there. In the 20th century, several approaches were taken to renewing them: an axiomatic conception based on the topological theoretic of measure was substituted for certain sections of Laplace's calculus; from the inter-war period, there was a proliferation of mathematical statistics – an approach that cannot always be reduced to the axiomatization of measure; and finally, the more recent development of computerized statistics has sometimes been successful in highlighting regularities whose axiomatic or mathematical proof is still the subject of contemporary research.

• **The *classical* calculus of probabilities**

Throughout these developments, from the mid-18th century to the early decades of the 20th century, counting the sexes at birth offered an exemplary empirical trial. This topic was discussed in three ways, and it is useful to distinguish between them. Firstly, many and varied numerical observations have been available since the 17th century, for reasons that have not always been scientific in nature (parish registers; tax censuses). Secondly, academic thinking about the issue continued to develop without necessarily making use of the calculus of probabilities (medicine, moral sciences). Lastly, the schema of two mutually exclusive possibilities lent itself to *binomial* mathematical calculations.

Since the 17th century, it has been known that the numbers of combinations in such a case may be obtained by developing a binomial of the type $(a+b)^n$. Supposing that there are only two possible cases (a given sex being identified – whether boy or girl does not matter – and its alternative imagined respectively as girl or boy, on the understanding that for this reasoning, it is enough to view one sex as identified on one hand and all other cases on the other hand), and that the first has a probability s and the second a probability s', we have by construction $s+s'=1$ (1 being taken for the measure of certainty). For N cases, this certainty remains 1 and is written $1=1^N=(s+s')^N$, so that:

$$1=(s+s')^N=s^N+\cdots+\binom{N}{n}s^n s'^{(N-n)}+\cdots+s'^N$$

Here $\binom{N}{n}$ is the number of ways of taking n elements (without worrying about their order) in a set that includes N. The preceding formula amounts to the same thing as saying that this binomial can be broken down according to all possible combinations of the case in question and of its alternative. These combinations number 2^N.

The development of the binomial expresses the fact that *insofar as this is a matter of counting, anything can happen*: n cases of "boys" (from now on we shall use this category in the conventional way) and $(N-n)$ cases that are not identified as "boys", for all the values of n taken from 1 to N. From a strictly combinatorial point of view, case n will occur $\binom{N}{n}$ times and its probability is therefore $\binom{N}{n}/2^N$. Here we may speak of the *classical calculus of probabilities*, characterized by this ratio of $\binom{N}{n}$ favourable cases to 2^N possible cases. This was the reasoning used in the 17th and 18th centuries.

• **The *analytical* calculus of probabilities**

Various pieces of work in the final decades of the 18th century (Hald, 1990; Todhunter, 1865) prepared the ground for this, and it was introduced by the young Laplace; it is characterized by a fairly *similar* construction of probability, but this time on the level of the differential element that then has to be integrated according to the calculus operation. Laplace and his contemporaries devoted themselves to it at the cost of laborious development and new reflections on the theory of functions (hence the adjective *analytical*). From the time of the tensions between Laplace and Condorcet, this analogy has been the object of discussions, still bringing mathematics and philosophy together today (Brian, 1994a).

At this point, a historical sociological issue is not without importance. Although Laplace's calculus, its developments and its revisions led mathematicians out of some theological ruts, mastery of all these called for a particular mathematical competence, unfortunately most often routinized to established calculation techniques. The result was that its fundamentals were not so familiar to 19th- and 20th-century statisticians and philosophers, even though some of them had come into contact, in one way or another, with the bases of differential and integral calculus. Authors who knew how to control both the calculus and its philosophical analysis were rare – a fact that made the works of Condorcet (1783–1787), Laplace (1814), Cournot (1843), Venn (1888), Bertrand (1889), Borel (1924) and Fréchet and Halbwachs (1924) all the more valuable.

Let us consider that one case (let us say, the birth of a "boy", for example) has a probability p. Let q be the probability of the alternative to this case: $q = (1-p)$. Next, possible events must be envisaged, always according to the principle that *insofar as this is a matter of counting, anything can happen*, which is to say that all the proportions observed *a posteriori* x

and $(1-x)$ are possible, with x varying from 0 to 1 (Todhunter, 1865, e.g., §896–897, §1025, §1031).

The following expression then gives a basis for measurement. Once it is understood that the probability of an arrangement of events independent of one another is the product of the probabilities of its components, the formula conveys that, from x to $x+dx$, the share of probability to be taken into account is the product of x in proportion to p and of $(1-x)$ in proportion to q.

$$\int_0^1 x^p(1-x)^q \, dx$$

The calculus of probabilities, whether classical or analytical, characteristically starts from the fact that even though a thing cannot be taken as certain, one can still think very rigorously about its uncertainty. The calculus is then based on taking into account the whole spectrum of possibles, weighted by a measurement of each possibility (Condorcet, 1783; on this topic, Brian, 1994a). In both cases, the measurement of an event is obtained by comparing it with the arithmetical sum of all the possibles (classical calculus) or with the integral on all the possibles (analytical calculus), which is not exactly the same thing. The fact that integral calculus was decisive from Laplace onwards was because, compared to simple addition, it allowed processes that were more subtle, less ambiguous and convenient to approximations (which were almost always indispensable).

3. Sex ratio and probability: notations

In order to clarify this thinking, nowadays it is necessary to distinguish between several different things and then attribute different notations to them. These notations are used below, and again in the development of Chapter 6.

- **Notation *N***. – The total number of observed cases, taking both sexes together. M will be the number of boys; F, that of girls; i, the index of any case (taken from 1 to N).
- **Notation *s***. – The probability of one sex or the other at a particular moment of ontogenesis. For a case under consideration at the point of conception or even at birth, this is – for example – the *abstract* probability that the case is one of a boy. Laplace, in his texts on the proportion of the sexes at birth, saw this as a physical cause which he called the "*greater facility*" of male births.

- **Notation** γ. – The binomial *random variable* without which it would not nowadays be possible to reason mathematically on the possibility that a child is one sex or another. This variable conventionally takes the value 1 if it is a matter of one sex, 0 if the alternative. In the following, we shall associate the value 1 with the male sex. For a birth i, we let this random variable be γ_i. The parameter s is characteristic of this binomial if s is the probability that it takes the value 1. We may write this as: Proba$(\gamma_i = 1) = s$. Then the mathematical expectation of this random variable γ_i is also equal to s. This is written as: $\mathrm{E}(\gamma_i) = s$.
- **Notation** S. – The *observed secondary sex ratio* – that is, the observed frequency of the male sex among live births. $S = M/N$.
- **Notation** Σ. – The *random variable* that empirical measurement of this frequency S achieves for a set of N cases. It is made up of N elementary random variables of type γ thus: $\Sigma = (\gamma_1 + \cdots + \gamma_N)/N$. Its value, always random, lies between 0 and 1.
- **Notation** Γ. – The *number of boys per hundred girls*, the indicator used since the 17th century. $\Gamma = 100\,M/F$.
- **Notation** Π. – The probability that S is within a certain interval between two given values. The frequency S is, from the point of view of the calculus of probabilities, the realization of a random variable of type Σ. Therefore the observed frequency, the sex ratio S, and any value s can be compared by asking what the probability is that S and that value are sufficiently distinct from each other. This is the question of *estimating* Σ using S, with Σ understood as random and S as observed. Since Jakob Bernoulli (1713), the notions attached to the definitions of s, S, N and Π have been linked by a class of proofs identified (from the 19th century) by the expressions *law of large numbers* or *central limit theorem*. These allow a *confidence interval* to be drawn around S, characterized by a certain amplitude at a given level of probability Π. Applying the calculus of probabilities then consists of rigorously establishing a sentence of the kind: "the probability that the absolute difference between S and the mathematical expectation of Σ is greater than a given quantity is less than Π".

4. Probabilistic estimation of the secondary sex ratio

If the hypothesis is that, for the N cases observed, distribution of the random variable γ is constant and uniform – $\forall\, i$, Proba $(\gamma_i = 1) = s$ – and that these laws γ_i are independent of one another, how is the link between the

frequency of observations S and the probability s to be measured? Central limit theorem-type results lead to the conclusion that S is a good estimate of s; that is, that the law of probability of the frequency S rapidly approaches – from the point when N exceeds about ten – a law whose formula can be established. This is the "second law of Laplace" (also known as Laplace-Gauss distribution, or *normal distribution*). Its mathematical expectation (the parameter of its centre) is precisely s, and its variance (that of its dispersal around this centre) is σ, such that $\sigma^2 = s(1-s)/N$. This density is written as:

$$\frac{1}{\sigma\sqrt{2\pi}} e^{-\frac{(x-s)^2}{2\sigma^2}}$$

It is not necessary to take into account the fact that the variance itself depends on s. This is because the value of s is always between 0 and 1, and therefore s(1–s) is smaller than 1/4. Therefore:

$$\sigma^2 < \frac{1}{4N} \quad \text{and} \quad \sigma < \frac{1}{2\sqrt{N}}$$

The density formula (which is already an approximation) and this last upper bound allow two series of values α and Π, which obey this relationship, to be linked:

$$\text{Proba}\left(|s-S| \geq \frac{\alpha}{2\sqrt{N}}\right) \leq \Pi$$

For a long time, tables were used to link α and Π. Nowadays, it is easy to do this using spreadsheet software. It is convenient, although completely arbitrary, to limit ourselves to the pair $\alpha = 2$ and $\Pi = 4.55\%$, and although it is a misuse of language, it is acceptable to take $\alpha = 2$ and $\Pi = 5\%$: we can then speak of a *95% confidence level*.

$$\text{Proba}\left(|s-S| \geq \frac{1}{\sqrt{N}}\right) \leq 5\%$$

This formula is very commonly used. It explains why Fourier (1821) considered it sufficient, from a practical point of view, to confine himself to the fact that uncertainty about the proportion of male births was in the order of $1/\sqrt{N}$. Although a reduction in uncertainty of frequencies according to the number of cases considered had been recorded from the work of Jakob Bernoulli (1713) onwards, it was only following Poisson (1830) that the mathematical concepts involved were clarified.

The preceding formula links the number of observed cases N and the amplitude of the probable difference between the probability s and the observed frequency S. One difficulty in understanding this method arises from the fact that there are *three* things here that all come under the heading "probabilities": first of all, the presupposed probability s, then the frequency S observed *a posteriori*, and finally the probability of observing a difference between the two. This is the price to be paid for thinking rigorously in the face of an intrinsically uncertain phenomenon.

Thus, in order to judge a deviation of 1% on a proportion of boys at birth, with only a 5% chance of being mistaken, at least 10,000 observations are necessary. A deviation of 0.1% would call for a total number in the order of a million cases. Unfortunately, it is clear that many studies published nowadays commonly – and this despite Poisson's very explicit recommendations (1830) – offer estimates of proportions of births that are highly uncertain because numbers of cases are too low. The empirical strategy followed in Chapter 6 of this book is to deal with very large total numbers (often hundreds of millions), in order to reduce the random variation of the phenomenon and to establish the shape of its fluctuations.

5. Weakness of the estimator of number of boys per hundred girls

The behaviour of the indicator $\Gamma = 100\,M/F$ is not so satisfactory from the point of view of the calculus of probabilities. This is because, unlike $S = M/N$ where the numerator is random and the denominator is known, this time both terms of the ratio are random. There are several possible ways of showing that the dispersal of the random variable realized by Γ will be wider than that of Σ.

Having constructed, for example, a confidence interval for S, we have a maximum amplitude α for that interval and its probability Π. Therefore, the interval can be expressed in its equivalent for Γ.

$$B_1 = \frac{(S-\alpha)}{(1-S+\alpha)} \leq \frac{\Gamma}{100} \leq \frac{(S+\alpha)}{(1-S-\alpha)} = B_2$$

The semi-interval $\frac{1}{2}(B_2 - B_1)$, similar to α, is then:

$$\frac{1}{2}\left[\frac{(S+\alpha)}{(1-S-\alpha)} - \frac{(S-\alpha)}{(1-S+\alpha)}\right] \geq \frac{\alpha}{(1-S)^2} \approx 4\alpha$$

Thus, at the same 95% level, in order to take a decision on a deviation in the order of 0.1% (0.1 of a point respectively), calculation of the proportion of boys among births ($S = M/N$) calls for roughly a million cases, while that of the ratio of boys to girls ($\Gamma = 100\,M/F$) requires 16 times more. From this it is clear that, of the two traditional indicators, the only one through which we can hope to assess variations satisfactorily from the point of view of the calculus of probabilities is the proportion S, and so the number of boys per hundred girls Γ should be abandoned. That is why, in this book, we have systematically used the index that corresponds to the most precise probabilistic estimator: the proportion of one sex out of the total of known cases.

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