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The Economics of Convention and Statistics: The Paradox of Origins

*Alain Desrosières**

Abstract: »Die *Économie des conventions und die Statistik: Das Paradox der Ursprünge*«. The line of heterodox economic thinking named “the economics of conventions” emerged in the 1980s in France. Four among its six founding fathers had a strong background in statistics and were working at INSEE (the French National Institute of Statistics and Economic Research). However, the numerous and fruitful researches in the line of this new paradigm have only slightly used the quantitative methods (above all econometrics) that are widely spread in mainstream economics as well as in other heterodox movements, e.g. the French school of regulation. In order to provide a rationale for this paradox, we are lead to set the development of the economics of conventions within a broader history of economics and social sciences. Indeed, from the 1980s onwards, social sciences have gone through a movement of bifurcation that brought about a deep change in the scientific and political status of quantification. Monitoring this movement leads to address the issue of the relationships between the search for theoretical reflexivity and the social demand for expertise addressed to economics.

Keywords: convention, economics of conventions, statistics, reflexivity, expertise, quantified knowledge, history of economics.

The field of research devoted to the economics of convention (*économie des conventions*) was launched in March 1989 when six collaborating scholars outlined a new and unorthodox approach to economics in a manifesto published in the *Revue économique*. Four of the authors – François Eymard-Duvernay, André Orléan, Robert Salais and Laurent Thévenot – were trained statisticians and had worked together at the *Institut National de la Statistique et des Études Économiques (INSEE)*, the official French statistics bureau, from the mid-1970s until the mid-1980s. The other two authors, Jean-Pierre Dupuy and Olivier Favereau, had no links to INSEE. Curiously, the quantitative methods (especially econometrics) which are highly esteemed in orthodox economics or in other heterodox streams (for example the “regulationist school” with Robert Boyer, a civil engineer, as one of the leading researchers) were rarely applied in the numerous and fertile research projects carried out under this new

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paradigm. How is this paradoxical phenomenon to be interpreted? By framing this moment within a longer history of economics and social sciences, we can see how the two fields parted ways. This separation took place in the early 1980s and led to a major change in the scientific and political status of quantitative techniques. This reveals the pre-eminent role played by INSEE in the history of the sciences from the 1960s to the 1980s, stemming from the particular prestige of the corps of engineers in France (and hence perhaps puzzling to scholars in other parts of the world).¹

When INSEE was created in 1946 it was meant – in contrast to similar institutes in other countries – to produce not only statistics *stricto sensu* but also to produce economic studies. Its professional staff – called “administrateurs de l’INSEE” – were trained at a prestigious “grande école”, the *École Nationale de la Statistique et de l’Administration Économique* (ENSAE) where they received high-level training in statistics and probability as well as in economics, econometrics and some sociology. Starting in the 1950s, the economist Edmond Malinvaud (director of INSEE from 1974 to 1987) imported the econometrics of the *American Cowles Commission* where he had worked in 1950 to France and subsequently taught the subject at ENSAE. The mathematical economics created by Walras and Pareto was not introduced at universities in France but at the *École des Mines* by an engineer, Maurice Allais whose teachings were followed by ENSAE students. At the same time, in the 1950s and 1960s, two new major tools, national accounting and survey-based socio-economic enquiries, had been developed by statistical institutes. This happened during a period of economic growth also characterised by great optimism regarding the powers, in particular the quantitative reach, of social sciences to accompany this progress.

Social sciences had become quantitative since the 1930s and the 1940s in various ways. At the turning point of the 1940s highly mathematical econometrics characterised by probability and inferential statistics were set up in the United States by a Norwegian, Trygve Haavelmo, and two Dutchmen, Jan Tinbergen and Tjalling Koopmans (Armatte 1995). Econometrics was first used in the context of Keynesian macroeconomic models. Later, quantifying empirical sociology was promoted in the United States by Paul Lazarsfeld and developed further in France after 1945 at INSEE and at the *Institut National d’Études Démographiques* (INED) rather than in university laboratories. Even history as practised by the “*École des Annales*” became quantitative (or “se-

¹ This article does not intend to reconstruct the genesis of the economics of convention in all its dimensions but to recall one of these dimensions that is sometimes overlooked, that is the role of certain statisticians-economists in this genesis. The important contribution of Olivier Favereau has other origins and is less studied here. His contribution is described in another article in this issue of the journal. I thank François Eymard-Duverny and Robert Salais for their notes on a first version of this text.

rial”), initiated by François Simiand, Ernest Labrousse as well as by François Furet (who changed direction after the branching of the 1980s).

The young economic statisticians trained at ENSAE in the 1960s and early 1970s were subject to contrary influences. On the one hand the optimism concerning quantification and scientification accompanied the last days of a society of economic growth with low unemployment.² On the other hand, this young generation was also shaped by the highly political protests in the aftermath of the Algerian War (terminated in 1962) and in May 1968. Marxism was still very influential and in 1965 a “communist students union” (itself split into numerous sub-groups) was very active within the student body and especially at ENSAE.

The first “regulationist” economists (such as Michel Aglietta who had also been trained at ENSAE and was a member of INSEE for some time) were influenced by this “counter-training” of Marxist inspiration. Furthermore, the young sociologist Pierre Bourdieu – having met some INSEE statisticians in Algeria during the last years of the war – collaborated with these regulationists until the 1980s. Between 1963 and 1966 Bourdieu taught at ENSAE and started to reflect on the practice of statistics, planting some of the first seeds for the development of the economics of convention.

This process of reflection is seen in three phases. First, in the 1970s, a historical perspective applied to statistical categories and standardised surveys drew attention to the interrelated influences (often misperceived by a strictly positivistic and scientific view that was still predominant) of statistical tools and social issues, the former being applied to and providing arguments for the latter. A little bit later, in the early 1980s, the emphasis put on the processes of social categorisation and statistical codification constituted a major prelude to the economics of convention. In 1984 INSEE research projects on the tools of management, the rules and conventions structuring the labour market (negotiations, qualification, relations in terms of salary) ultimately formed the matrix on which the economics of convention would grow (Salais and Thévenot 1984). But here the focus was not on thinking about quantification as such. For each of these three moments we perceive a critical mood (implicit or explicit) with reference to the conception of social sciences that prevailed in the preceding period.

² The book by François Fourquet (1980) *Les comptes de la puissance. Histoire de la comptabilité nationale et du Plan*, briskly reveals the optimistic humour of this epoch, which ended in the early 1970s.

A Historical Perspective on Categories and Attention to the Procedures of Codification

The first step of reflexive withdrawal from statistical tools was suggested by Bourdieu and resulted from researching the nomenclature used by official statisticians which was at that time seen as an outcome of history. A pioneering work had been accomplished in 1971 by Bernard Guibert, Jean Laganier and Michel Volle on the history of the nomenclature for the branches of industry used in surveys and censuses in France since the 18th century (Guibert *et al.* 1971). They showed that the distinctions between the branches had changed continuously, taking first basic materials, then production techniques and finally the use of products as criteria. This highlighted the fact that the statistical tool which had heretofore been perceived as “neutral and objective” was linked to a context of political and social customs, and opened the path for a series of other historical research projects, pertaining to industrial surveys (Volle 1982), socio-occupational nomenclatures (Desrosières 1977, Desrosières 1998),³ unemployment (Salais, et al. 1986), surveys about social mobility (Thévenot 1990), among others. In 1976, INSEE organised a conference at Vaucresson with historians favouring “serial history” on the topic “*For a History of Statistics*”.

An odd exchange ensued: the historians asked mainly for sources and “long time series”, whereas the statisticians pushed them to historicise these very sources by asking them questions other than those leading to the statistical analysis of time series and of data tables. The classical and venerable “critique of sources” (*critique des sources*), the basis of all historians’ professional practice, had prepared them for this change of perspective. However, the aim was no longer the same: the production of statistics was to be regarded as a social practice that was interesting in its own right, rather than constituting merely a “source” entailing irritating “ruptures of time series continuity”. The conference resulted in the publication of two volumes titled “*Pour une histoire de la statistique*” (Affichard 1977, 1987), summarising the research of historians and statisticians. In the following period, the early 1980s, the pre-eminence of quantitative methods in history diminished.⁴ This reorientation was one element of the “critical turning point” (*tournant critique*) proclaimed in 1988 by the Journal *Annales* that contributed to the bifurcation mentioned above.

Detailed studies of the procedures of categorisation and codification were carried out at INSEE in the early 1980s, in the context of a revision of the

³ *The politics of large numbers* (Desrosières 1998) was also translated into German (Desrosières 2005a) [Editors’ note].

⁴ The historian François Furet – formerly an active partisan of quantitative history – later described this turning point with a joke in the context of research studies on the libraries of the bourgeoisie in the 18th century: “Before, we counted the books: Now, we read them”.

nomenclature for socioprofessional categories (*catégories socioprofessionnelles* or *CSP*). The nomenclature had been constructed by Jean Porte at the beginning of the 1950s and had been used for the population census surveys ever since (Desrosières and Thévenot 1988). Following the lead of taxonomic studies in other disciplines, two major methods were found to be at work in Porte's categorisation: the method of "criteria" based on general logical principles and the method "by typicity" based on the successive resemblances of "typical cases" (Rosch and Lloyd 1978). Luc Boltanski, a sociologist who had previously worked closely with Bourdieu, and Laurent Thévenot, an INSEE statistician, produced a series of empirical studies on how social actors perform operations of social taxonomy in everyday life (Boltanski and Thévenot 1983). The CSP nomenclature, which was used not only by INSEE but also by many other social scientists and by private social and market research institutes, turned out to be heterogeneous, juxtaposing very different criteria and methods of coding and classifying in an apparently disordered way. This critique was formulated especially by Marxist theoreticians and by neoclassical economists who wanted one basic underlying criterion for social and economic classification.

This diversity and plurality irritated theoreticians and led to more general research on the *multiplicity* of conventions of equivalence serving as a foundation for procedures of categorisation as well as on the potential *hesitations* of actors when classifying persons and of the necessary *judgements* made. Thévenot (1983) described this phase in an article entitled "The economy of social codification" (*L'économie du codage social*), which foreshadowed the 1986 article on the investment in forms. This interrogation also referred to earlier works by Boltanski on *Les cadres* (1982) and to the "judgements of normality" within letters of denunciation received by the daily newspaper *Le Monde* (Boltanski *et al.* 1984). The systematic analyses of this multiplicity, of these hesitations and of these judgements led to the extensive construction of the "economics of worth" (*économie de la grandeur*) (Boltanski and Thévenot 2006) which constitutes one of the referential texts of the economics of convention.⁵ This approach of the economics of convention originated partly from research in the field of sociology, following works which had been originally inspired by Bourdieu. Although this line of research was conceived as distinct from and even opposed to the works of Bourdieu, his work was clearly well known and very much present in the minds of these authors.⁶

⁵ The book by Boltanski and Thévenot had already been published in France in 1991 as *De la justification* (Boltanski and Thévenot 1991).

⁶ Whereas at the same period, Michel Foucault or *a fortiori* Louis Althusser, two very famous authors in France and abroad in the 1970s and 1980s, were largely unknown to them.

1984: The First Steps of the Economics of Convention

Two rather different movements in French economics appeared at the turn of the 1980s: first, the regulation school, spurred by the pioneering works of Michel Aglietta, Robert Boyer, Alain Lipietz and Bruno Théret, and thereafter the economics of convention discussed here. Of the ten authors mentioned above who are the originators of these two strands, nine come from French engineering schools rather than from universities. Aglietta (X and ENSAE),⁷ Boyer (X and Ponts), Dupuy (X and Mines), Eymard-Duvernay (University and ENSAE), Lipietz (X and Ponts), André Orléan (X and ENSAE), Salais (X and ENSAE), Théret (Ecole Centrale), Thévenot (X and ENSAE). Only Olivier Favereau has a purely university background as a lecturer in economics. These researchers have high-level training in mathematics and statistics. The regulationists among them developed a macroeconomic and macrohistorical theory inspired partly by Marxist and Keynesian notions of the regime of accumulation and of salary relationships. Since the 1970s, the regulationist school has broadened its experience in macroeconomic modelling (Boyer 2004).

The conventionalists in turn started to question the basic principles of standard neo-classical microeconomic theory by adding an “interpretative” dimension to the usual notion of convention, in part derived from analyses of hesitation over statistical coding. The regulationists and the conventionalists have the same adversaries – that is the proponents of standard and unhistorical neo-classical theory. Still, their criticisms are rooted in different though complementary foundations. They know each other well, they often meet and invite each other to their respective conferences. In particular, Robert Boyer participated in the conference on the tools of labour management “*Les outils de gestion du travail*” organised by François Eymard-Duvernay, Robert Salais and Laurent Thévenot at INSEE in November 1984. This constituted a first step towards the economics of convention (although this expression was not yet in use). The proceedings of this conference were published by Salais and Thévenot in 1986 in an INSEE publication entitled: *Le travail. Marchés, règles, conventions* (abbreviated as TMRC). Almost twenty years later, in December 2003, another conference brought together economists of the two different approaches (Eymard-Duvernay 2006a, 2006b).

One founding idea of the economics of convention comprises the plurality of logical approaches within an enterprise which cannot be reduced to a single object. As early as 1982, Eymard-Duvernay described this plurality in a pioneering study about the watch industry, titled *Cohérence de la branche et diversité des entreprises* (Bony and Eymard-Duvernay 1982). A statistical analysis of the structures of employment and of the balance sheets of enterprises in the watch industry, along with field studies, showed the diversity of the forms

⁷ In the jargon of French students “X” denotes the Polytechnic School (École Polytechnique).

of entrepreneurship and of the quality of their products. This led to uncertainty regarding the very notion of “industrial branch”. This idea which was foreign to traditional business economics was to become one of the founding concepts of the economics of convention. The university researcher Olivier Favereau set up a link to the works of the sociologist Harrison White, the first connection between Favereau and the statistical researchers at INSEE.

Re-reading TMRC more than 25 years after the conference on “*Les outils de gestion du travail*” highlights the context of the emergence of the economics of convention, and the meeting of statisticians and economists who focused on labour and employment (Olivier Favereau). All of these researchers had begun to reflect critically on the tools provided to them during their education. For some of them these tools consisted of quantification as criterion of factuality and of reflection of a reality disconnected from the way of their recording, and for the others the application of standard neo-classical theory to labour. Although the second strand formed the origin of important studies within the forthcoming history of the economics of convention, this was less true for the study of the social usage of quantification (combining under this term statistics, business accounting and national accounting). Several texts in the TMRC collection implicitly criticise statistics, especially on the basis of the pioneering text of Eymard-Duvernay and Thévenot on “the investment of form: usage for manpower” (“*les investissements de forme: leurs usages pour la main d’oeuvre*”) published as a Note by the Employment department of INSEE in August 1983, and also on the basis of the article about the investment in forms published by Thévenot in 1984.

This text studies the statistical tools used by the INSEE in the analyses of work and employment and emphasises the expensive investment of setting up and codifying the categories that are at once cognitive and social, invented, negotiated and used for the management of manpower (*la gestion de la main d’oeuvre*). This codification has direct consequences on the sources (such as surveys and administrative indexes) and on the categories used by the statistical analyses of work and employment. The original intention of this INSEE note had been to bring together the economic notion of investment (especially since the examination of Taylorian firm reorganisations) and the notion of *forme* which originated from another tradition, i.e. the philosophy of knowledge. One clue to Thévenot’s intention is to be found in his former work on the nomenclature of socioprofessional categories used by statisticians, sociologists, demographers and (less frequently) by economists. However, this 1983 text was already well-known and applied by participants of the 1984 conference on the tools of labour management. Some examples based on texts published in 1986 in TMRC will follow.

Circularity of Conventions and Statistical Formatting

Bénédicte Reynaud showed that negotiations of collective agreements induce a certain type of labour codification by selecting some relevant objects at the cost of others, varying from branch to branch. Thus the statistical surveys describing the income relations drawing on variables and divisions deriving from these collective agreements “rediscover” the management models by which the former were organised beforehand. Within the same perspective, Pierre Rivard observed that using socially constructed codifications leads to elaborating an explanatory scheme which will be auto-validated by the collected observations. This scheme risks to be strongly partial since the underlying data derive from constitutive models of the codifications. Furthermore, Joëlle Affichard analysed the negotiations of authentication (in French *homologation*) of diplomas for technological education from private or public schools outside of the national education system (*Ministère de l'Education Nationale*). She showed that the quantification of the “diploma level” (*niveau de diplôme*) which is frequently used in the sociology of education and training depends on complex social processes of acceptance of equivalence between diplomas following very different courses of training.

The disclosure of this mirror effect between social practices, conventional codifications and statistical representations is characteristic of the conventionalist approach to statistical surveys in the 1980s. Even though it did not aim at disqualifying statistical surveys but rather at illuminating the limits of producing new knowledge, this disclosure weakened the naive empirical vision and perhaps even contributed to disillusionment with this approach. This illuminates the subsequent distancing of conventionalist economists from the systematic usage of quantification (which often ignored these issues) as practised by other heterodox movements, and *a fortiori* orthodox ones. The difficulty is central to the recurrent questions that arise from the interpretation of survey results and *a fortiori* of administrative sources, for example those referring to unemployment and to delinquency (Desrosières 2005b). This can lead to a sociology of quantification which concentrates (primarily but not exclusively) on the circulation of statistical action and representation. This kind of research focuses on the whole of operations of construction and usage related to statistic tools, no longer within intending to “reflect reality” but rather to produce and reinforce certain effects of reality, neglecting others, implying specific ends of action rather than the idea of knowledge independent of its application (Desrosières 1998, 2008a, 2008b). This idea is implied in the text on the investment in forms which resulted from an analysis of the costs and the consequences of the crystallisation of statistical categories.

This path of research will be taken up again, for example, much later by Robert Salais. Salais described the indicators of the “open method of coordination”, supported by the European Union in order to harmonise at the same time

the social policies of its Member States and the statistical tools used to evaluate the effects of these politics and in order to make them accessible to benchmarking (Salais 2004; Bruno 2008). He also analysed the perverse effects of the indicators aimed at managing public policies according to performance criteria (Salais 2010). Furthermore, a research project by Laurent Thévenot and Olivier Monso (2009) on the transformation of statistical surveys on professional and social mobility, updated an analogous research project undertaken thirty years earlier in the 1970s (Thévenot 1990). This leads to incommensurability between surveys carried out some decades ago, since underlying principles and public action orientations had changed within the period from 1960 to 2000. This constitutes an obstacle to the construction of long time series by historians and economists using *cliometry*, an application of economic methods towards long term history.

The notion of convention of equivalence (*convention d'équivalence*) is of special importance for the sociology of quantification, which has often been referred to in the works of Bruno Latour (1984) and in the economics of convention of the 1980s. This expression intends to combine the social term *convention* with the logical term *equivalence*. It is necessary to unify in order to agree on the things which are understood as equal. The equivalence is never given in advance. This idea represents a veritable break with the positivistic conception of quantitative social sciences by denaturalising the categories involved.⁸ This idea is important for the quantitative or “serial history” (*histoire sérielle*) which is grounded precisely in interpretations of long time series. The “equivalence” of the objects being referred to in the course of time is fundamentally conventional and can therefore be questioned again at any time.

Thus, a controversy emerged in 1991 upon the occasion of the publication of a book written by two INSEE statisticians, Olivier Marchand and Claude Thélot, who designed just such long time series: *Deux siècles de marché du travail. Population active et structure sociale, durée et productivité du travail* (Desrosières 1992). The terms activity and salary did not have the same meaning at the beginning of the 19th century and at the end of the 20th century. Historians, sociologists and statisticians (Eric Brian, Alain Desrosières, Bernard Lepetit, Olivier Marchand, Claude Thélot, Christian Topalov and Florence Weber) took positions in a dossier presented by the Revue *Genèses* (No. 9, October 1992, pages 90 to 119). The main argument of the two authors of the book evolved around the notions of “order of worth” (“ordre de grandeur”) and of “approximation”. This dispensed with thinking about the terms ‘comparability’, and ‘convention of equivalence’ as a procedure oriented towards a final aim or a historical interpretation and not towards a simple state of facts. The term ‘in-

⁸ A remark similar to this one but with another terminology refers to *commensuration* and is developed by the sociologists Wendy Espeland and Mitchell Stevens (1998) under the title “Commensuration as a Social Process”.

terpretation', which is central in the economics of convention, is absent in this construction of time series data.

In the 1986 TMRC publication François Eymard-Duvernay developed the term "qualification of products", a convention of equivalence especially important for economics. In fact, the theory of the general equilibrium and the standard theory derived therefrom postulate the existence of "products", goods and services of which the respective definition, the list and the nomenclature are thought of as preordained.

More specifically, the definition of a product is always problematic, uncertain and liable to discussion and negotiation even at the very centre of economic processes. A by-now classical theoretical formulation of the problem was framed by Akerlof (1970), and illustrated by the example of second-hand goods. Within this field, the Eymard-Duvernay text prepared the way for a great number of research projects conducted by Eymard-Duvernay himself and his research group. This subject is implicitly present in the technical debates between statisticians of international organisations on the revision and harmonisation of the nomenclatures of goods and services used in surveys and analyses, thus taking place without the participation of economists, conventionalists or others. This describes an area of research which has hardly been explored so far.

The theme of the uncertainty of quality extends beyond the economics of convention and into currencies and assets, goods, firms and labour (Eymard-Duvernay, Salais, and Thévenot). This theme induces more or less explicitly a critical view of statistics in order to supplement or even to replace this approach with methods of observation that allow more detailed analyses of inherent qualities. This end motivated Eymard-Duvernay and Thévenot to approach the *Centre d'Études de l'Emploi* (CEE). This centre where Pierre Boisard, Marie-Thérèse Letablier, Christian Bessy and Emmanuelle Marchal worked was renowned for its qualitative surveys and was to become one of the poles of the economics of convention, in between INSEE and the University.

At CEE, the research projects of Eymard-Duvernay, Emmanuelle Marchal and Christian Bessy, focused particularly on employee recruitment procedures and on the assessment of candidates' skills. These are excellent cases that show the uncertainties of the labour market, and the role of public and private intermediaries in reducing this uncertainty. Within this scope, Eymard-Duvernay distinguishes individual judgements (for example resulting from psychometrical tests) and "sociological" judgements of collective properties which imply an actuarial rationality and hence statistical tools. In this context he states "Official statistics make a new entity visible, a society which is neither the individual nor nature, by showing the existence of macro-social regularities. The socioeconomic nomenclatures permit this way of reasoning." (Eymard-Duvernay

and Marchal 2000, 427).⁹ Thus, actuarial mechanisms appear via statistical techniques as a means to connect the individual to a collective through the occurrence of a class of equivalence deriving from codifications of the official statistics. In the case of recruitment, the authors even compare the implementation of such an actuarial mechanism to those already existing for work accidents or unemployment. The actuarial statistical approach “firmly linked to the sociology of Durkheim” creates a collective entity. This intention paves the way for a sociology of quantification that focuses on its own effects on society and not simply on what it reflects of society.

André Orléan, who himself contributed to TMRC, indirectly questioned the frequent and usual version of probability applied by social sciences since Quetelet’s research on the average man (*l’homme moyen*). Corresponding to this *frequentist conception*, uncertainty can be quantified as form of a “risk” and can be calculated from the recurrence of analogous former events, in the same way as for example insurance companies assess risks. With reference to the famous distinctions made by Knight and Keynes between probabilistic risk and uncertainty, both impossible to quantify, he showed that most of the decisions taken by economic actors can be assigned to uncertainty which is not probabilisable. The conventions thus appear as tools of *coordination* of the projects and decisions of the actors and as instruments to reduce the uncertainty. For Orléan, money represents the object *par excellence* of the reign of convention whereas the neo-classical economists cannot interpret it in this way. The term of coordination will then become a central element of the economics of convention. However, the “style of statistical thinking”, to use the same words as Ian Hacking, was weakened by this distinction between risk and uncertainty. Indeed, the quantitative social sciences are based on ideas of statistical regularity of causes and effects which are observed and formatted in the same way that natural phenomena would be treated. The terms of convention, of coordination and of non-probabilistic uncertainty are unknown within this statistical and econometric methodology. There is yet another way of considering quantification; instead of a tool to describe reality and a tool of proof, quantification is understood as one system of conventions among others, that is a tool of coordination and therefore also a tool of government.

⁹ “La statistique administrative permet de rendre visible une entité nouvelle, la société, qui n’est ni l’individu ni la nature, en montrant l’existence de régularités macrosociales. Les nomenclatures socioéconomiques instrumentent cette façon de raisonner.” (Eymard-Duvernay and Marchal 2000, 247).

Reflexivity and Expertise: A Fork in the Road of Human Sciences

How is the relative eclipse of statistics in the work of conventionalists to be explained when this approach had been so present at the beginning? Statistics can interest social scientists in two highly different ways. On the one hand statistics can provide a tool of proof in order to underline and argue using quantitative facts; and on the other hand they can be a tool of coordination and of government. These two aspects can come into conflict, especially when the latter raises doubts about the relevance and the reliability of the observations used as tools of proof. The first viewpoint espouses classical epistemology in which quantification is the privileged approach in modern sciences and particularly in social sciences (“Science is always measurable” was a maxim engraved on the pediments of universities in the 19th century).¹⁰ In this instance conventionalist research ostensibly tended to weaken the power and the efficacy of the statistical argument, by showing the historical and conventional origin of nomenclatures, and the ambiguities of coding qualitative uncertainties, without other possible forms of quantification being proposed. Consequently this methodology was no longer taught or criticised by conventional economists. It was very nearly forgotten, except for some research by Salais and Thévenot.

Soon after his research on “the invention of unemployment” (*l’invention de chômage*) was published in 1986, Salais, with significant experience in econometrics, implemented a original technique of statistical analysis, i.e. the correspondence analysis of Jean-Paul Benzècri (Greenacre and Blasius 1994). This method became very popular in France (especially among Bourdieu’s sociology students), but was little known in English-speaking circles. Using this method Salais was able to show the link between the existence of offices for unemployed people and the fact that persons state themselves as being unemployed in surveys; thereby pointing to the mirror effect between institutions and statistical representations.

Another idea was advanced in order to explain *a posteriori* the relative absence of statistical usage among the conventionalists. According to this viewpoint, statistics are assimilated at the “macro level” (e.g. the State), whereas research in the economics of convention is located at the “micro level”. However, this research programme, emphasising analysis of the role of *information* in economic decisions (Favereau 2000) cannot ignore the role of business accounting, a magnificent set of conventions for the decisions of economic actors, starting with enterprises. Research on accounting could have a place within this program (Colasse 2000; Chiapello and Desrosières 2006).

¹⁰ “Il n’est de science que du mesurable.”

The second research perspective on quantification (statistics, but also business accounting and national accounting) focuses on its role as a tool of coordination of government. This perspective was mentioned by Michel Foucault in his lecture at the *Collège de France* in 1978. He distinguished on the one hand the attributes of sovereignty (*régner*) from the administration of daily affairs (*gouverner*). According to Foucault, statistics in their modern sense emerged in the context of what he termed governmentality (Foucault 2004; Dardot and Laval 2009). Foucault did not, however, deepen this conceptualisation of government by instruments (Lascoumes 2004). An analogous idea was put forward by the philosopher of law, Thomas Berns (2009), also inspired by Foucault, in an illuminating small book entitled: *Gouverner sans gouverner. Une archéologie politique de la statistique*. This book presents the ideas of Jean Bodin in *Les Six Livres de la République* (1596), where the distinction between reigning and governing already existed together with a project of *ensor* (that had existed in the Roman Republic) implemented to survey the citizens and to compile a *census*. According to a good phrase coined by Berns, the idea was to progress from “governing the real” to “governing from the real”.

Thus, a “reality” is established that is exterior to the Prince – the reality of statistics. This statistical reality, as Orléan pointed out, must generate trust in order to be operative, just as currency must generate trust in order to be accepted as payment, which is in both cases a well-established belief. Statistics and currency are *conventions* that draw their power from a subtle dialectic relationship between independence and sovereign guarantee (Aglietta and Orléan 2002). The requirement of independence of the institution of statistics can be compared to the requirement of independence of the Central Bank from political power. Both forms of independence are inscribed in the founding texts of the European Union. Nevertheless, the independence of the Central Bank has a higher status as it is included in the Treaty of Maastricht of 1992 whereas the independence of statistics is merely subject to a “code of good practice” adopted in 2005. The comparison can be pushed even further by reviewing the conditions of possibility for harmonising and unifying statistics and the currencies of the States of the Union. The institutionalisation of unified European Statistics can be compared to the establishment of the Euro. The cognitive and social efficiency of these two systems of belief depend on an unstable combination between exteriority and authority of the State. This is conceptualised *a fortiori* since it means building a system of national accounting which combines both legitimacies, that of the currency as additive unit of account and that of statistics which have direct effects as demonstrated in connection with the “criteria of Maastricht” referring to membership in the Union (public deficit and debts) and *a contrario* following the crisis of 2009 when the trust granted to Greek statistics was lost.

The conventionalist research program places itself resolutely upstream of decisions and of economic action and focuses on the moments of hesitation,

interpretation and evaluation of situations which have not yet been qualified and identified as parts of a specified cognitive and pragmatic register. However, the trust required by the social uses of statistics requires, as for currency, that these doubts and questionings disappear and be forgotten. So, the period of the 1970s to the 1980s seems to have been the moment of a major division within human sciences. Formerly, an optimistic conception of measurement, derived from the natural sciences, appeared to be the basis of the scientific legitimacy of these sciences, both from the point of view of “pure” research and of “applied” research aimed at supporting decision-making and action. Then, after the so-called critical turning point, the hermeneutic turning or the linguistic turning, several reflexive research programmes took an interest in the array of cognitive tools mobilised by human sciences, not only in economics but also in history, in sociology, in anthropology. Thus, a rift appeared between, on the one hand, research of this kind examining the cognitive schemes according to a grand tradition of philosophy of knowledge and, on the other hand, applied research. This applied research takes these categories and these schemes, especially those of statistics, for granted; so that reflexivity seems to be antithetical to expertise. The economics of convention can thus be regarded as part of this larger movement by introducing a reflexive analysis of the corresponding knowledge tools in the economic sciences – a kind of reflexivity to which the economic sciences are rather unaccustomed.

This does not imply that the reflexive sciences have abandoned empirical research, but they accord less importance to the “quantitative” side, preferring what is often (and awkwardly) called “qualitative” work, that is monographs, direct observations, interviews, archival analyses, etc. This kind of empirical material is more suitable for interpretation according to distinct grammars, which also explains why it is preferred by the economics of convention at the expense of statistical surveys. In return, the expert social sciences, which are more oriented towards the tool of government, make broad use of these statistics as tool of proof. Economics as governmental science and as “engineering” (Armatte 2010) largely use the term *variable* which is expected to enable action (the aim) or by means of which action is possible (the means). The econometric techniques lead to the question of “the pure effect of a variable” or “the causal effect” in order to *evaluate* the effects which can be expected to result from this or that action. This can be done indirectly by the methods of regression or even directly by *randomised experiments* – widely used nowadays (Labrousse 2010). This notion of variable tends to change the historical specification of the social sciences, as it is transformed from the pure determination by facts towards a determination that depends on the position in society of the actor endowed with capabilities of judgement and interpretation.

Furthermore, the “re-opening of the black boxes” practised by the reflexive sciences may restrict the argumentative efficiency of statistics. This can be noticed particularly in regards to “metadata” (or “data about data”) that are

widely required under good methodology. However the statistical argument is especially efficient when it is “stark-naked”: too many metadata kill the data. Of course, this remark is not normative, but provides a path for empirical studies of the social uses of the statistical argument. Thus, by means of its conventions of equivalence and its codes, statistics (etymologically the science of the State) correspond to collective action, optimisation, decision-making, accounting, insurance, evaluation of risks, epidemiology, school management, court action, public law, offering a large spectrum of conventional research of quantification and its applications.

The history of statistics offers numerous cases showing that the transformations of the forms of government and their instruments are directly linked to the appearance (and disappearance) of statistical tools. For example, the 1929 crisis not only induced the “Keynesian revolution” but also the complex apparatus of *National Accounts*, a form of action on which so-called “Keynesian policies” are based (Vanoli 2002). The same crisis also radically changed the ways of thinking about agricultural policies and employment. This led to a system of survey-based *enquiries* which were also radically new. In particular, these surveys involved several modes of discovery, of identification and conventional qualification in regards to the surveyed entities (acreage, active people, unemployed people, etc.) that shaped today’s America (Didier 2009).

The current ecological crisis and the debates about “wealth” also introduce new entities to be identified and qualified (ecological imprint, carbon equivalent, biodiversity, happiness, etc.) and proposals of new quantification of the gross interior product (Gadrey and Jany-Catrice 2005; Cassiers and Thiry 2009). From a very different point of view, statistical computers allow the constitution and the use of complex data files on individuals and institutions. These data can be used to classify them according to their productive performance (ranking, or *benchmarking*), or to exploit them through techniques of *data-mining*, using *profiling* to reach customers of *marketing* or identify possible delinquents (Rouvroy and Berns 2010). All English words ending in *-ing*, derived from verbs, imply *actions on the world* by means of statistical tools which are both tools of the government in a wider sense, and tools of proof used by quantitative social sciences prior to the rift.

Thus a conventionalist programme of research on the procedures, the uses and the effects of quantification constitutes a very interesting undertaking.

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