Governing by carrot and stick: A genealogy of the incentive
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In a review essay on recent popularizations of economic science the same imagery returns in a discussion of the instruments at the disposal of central government. It could use taxes as “the proverbial stick by deterring people from doing things that they otherwise would have done” whereas it could also provide a subsidy as “the proverbial carrot by inducing people to do certain things they otherwise would not have done” (Vromen 2009, 92).

Perhaps this is due to the current division of labor between those who practice and those who reflect on economics. On the one hand, postwar economists may be said to lack the historical interest and philosophical inclination to question the assumptions and trajectory of their own discipline. On the other hand, the history and methodology of economics has become a subdiscipline in its own right, with journals and conferences that engage only a small circle of specialists who have little exchange with or influence on practicing economists.

The narrative provided by Laffont and Martimort, is a good example of the kind of history found in scientific textbooks. The purpose and pitfalls of historical digressions in textbooks were nicely captured by Thomas Kuhn when he said that “the aim of such books is persuasive and pedagogic; a concept of science drawn from them is no more likely to fit the enterprise that produced them than an image of a national culture drawn from a tourist brochure or a language text” (1996, 1). When seen through this lens, two aspects of Laffont and Martimort’s narrative attract attention. First, they make a sharp distinction between ‘history’ and ‘present’. They placed their historical reflections in a preliminary chapter, whereas, in the remaining eight chapters, they summarized and systematized recent developments in the economics of incentives without further recourse to history. Second, they reconstructed the impact of incentives in economic thought based on the current importance of the subfield. Indeed, during the so-called incentive revolution of the seventies, only a small subset of the authors they discussed actually played a role in the incentive-focused articles published at that time.

With the ‘state’ as a new level of political reflection and action also came a series of normative questions about the legitimacy of its interventions. In light of the constitution of the state as an autonomous domain of action, contemporary political thinkers developed notions of ‘natural law’ and ‘social contract’ to determine when citizens still had to obey the authority of the state and when they were no longer bound by its laws (Foucault 2008, 6–10).
In his final studies and lectures of the 1980s, Foucault’s interest shifted again from power/ knowledge to questions about self-understanding and self-governance. Individuals have continuously sought to constitute themselves as a subject of knowledge and governance. The historical study of the ways in which humans were made subjects would therefore be incomplete without taking the technologies of the self into account (Foucault 1997; 1998, 460–461).

In *The Order of Things*, Foucault eliminated all traces of human agency from scientific inquiry and turned the production of knowledge into an anonymous process in which individual scientists were mere mediators between deep, discursive rules and surfaces of knowledge production. The subordination of traditional units of analysis, such as author, oeuvre and tradition, meant that the active role of scientists in shaping the object of inquiry was completely left out. Foucault’s reflections on the formation of objects in *The Archaeology of Knowledge* lay somewhat in between the archaeology of the human sciences and the genealogy of power and knowledge.

The discourse of psychopathology is Foucault’s specific example; but he chose it in order to facilitate his analysis and not because it is in any sense special (1972, 40).

Note that I extract these elements from the *Archaeology of knowledge* without the archaeological framework in which it is embedded. I do not pretend to have discovered the rules determining the production of statements about adequate inducements to action in the modern episteme. In this thesis I present a genealogy of the incentive, that is, a succession of related attempts to turn the incentivizable subject into an object of knowledge.

This was not the only time in Frederick Taylor’s writings on scientific management that labor relations were discussed in warlike terms (cf. Taylor 1998, 1, 11).

Towne soon discovered that the actual connection between the effort of an individual and his reward was weak; therefore, he abandoned gain-sharing in 1893 (Nelson 1995, 53).

When he published his paper on the premium plan, the plan was already in use in several factories in America. Halsey was the first, though, to demarcate and defend it vis-à-vis the other systems in vogue. Subsequently, the plan became popular in the United States and England (Drury 1922, 69–70).

Although Taylor and his pupils prided themselves for the harmony they brought to industry, the actual response to scientific management by foremen, workers and labor unions was not exactly peaceful. The strike at Watertown Arsenal, a committee of the House of Representatives investigating the broader effects of scientific management and the resistance of several labor unions and federations attest to the fact that time study and wage incentive plans were far from uncontested (Nelson 1995, 60–61; Aitken 1985, 135). On the basis of these practical experiences "some of Taylor’s disciples acknowledged that scientific management was arousing the class conflict it had been designed to avoid" (Waring 1991, 13).

See Wilson (2003) for an overview of the development and use of Gantt charts in the twentieth century.

Besides drawing the lines accurately, there were other concerns that had to be considered in the creation of Man Record Charts. It was important, for instance, to minimize the distortions that could occur when the charts were reproduced and compared. Therefore, the charts had to be created on paper of a specific type (16 lb. bond, no water marks) and size, with a specific typeface (Gill Sans), and in black ink. These specifications were necessary for the blueprinting or photographing of charts that enabled the further circulation of facts throughout the factory (Clark 1942, 8–12).

Contrary to the widespread idea of workers who fiercely resisted scientific management, which they did on occasion (cf. note 10 above), the main opposition to the consulting engineers often came from managers. Gantt himself, for instance, had difficulties with the managers at both Sayles Bleachery and Joseph Bancroft & Sons, while other consulting engineers experienced similar difficulties in trying to transform other industrial firms (Nelson 1995, 76).

At the same time, wage incentives also became popular among executives because they were "consistent with the American business ethos of competition and the profit motive” (Aitken, 1985, 37). To say that they were popular, however, is not the same as saying that the engineers transformed industrial practice single handedly. The management systems of the consulting engineers were often only partially implemented as they had to adapt their ideas to their clients’ wishes (cf. Nelson 1995, 53).

In a sense, Taylor and Gantt had already moved...
away from the wage issue as the predominant management problem. Yet when it came to incentives, their management systems still relied heavily on the inducement that came from the promise of increased wages.

18 For the sake of completeness, I note that there were American economists who held positive views regarding the effects of scientific management. Thorstein Veblen, for one, expected a rise in efficiency when it was introduced more widely; whereas Irving Fisher lauded Taylor for bridging the gap between scientific theory and industrial practice (Caldari 2007, 58).

19 Although the field of economic science was far from homogeneous at the time, there were strong commonalities between British and American economists when it came to the problematization of current wage incentive systems. I discuss them here in order to set the stage for subsequent developments and not for thorough exploration of interwar pluralism in economics. As such, I will discuss neoclassical economists and institutional economists together.

20 On the basis of these reflections, Pigou even went so far as to say that the state should sometimes impede “any threatened excess in the growth of giant businesses, whether these are publicly or privately owned” (1921, 183–184).

21 Chicago economist Paul Douglas gave Clark’s search for new incentives its most concrete shape. Douglas analyzed a great many autobiographies of high-profile scientists and businessmen to understand the reasons or motives behind their quests for success. As it turned out, some of the most famous scientists of the nineteenth century showed a profound indifference to the monetary reward for their activities. Whereas the businessmen reviewed by Douglas were not primarily driven by money, but by the “joy from the competition itself”, the “zest of business” and the “the power of creation” (1924, 182).

22 Marshall postponed a full investigation of the wage problem until the second volume of *Industry and Trade*, which was ultimately never published. His remarks on wages in the first and only volume are mainly concerned with the controversy over and complexity of standard wages (Marshall 1932, 381). Pigou acknowledged that many labor sectors required a direct relationship between pay received and output delivered, but added certain requirements to the way that relationship was established. Methods of remuneration were not that important when interest in the activity itself drove people to work hard; but financial inducements were needed when it came to the performance of routine tasks. In many industries, however, the perceived unfairness of the rates made the introduction of piece wages a failure. To avoid friction and resentment among the laboring classes and make such incentives succeed, management had to actively seek the consent of the workers. One option was to give laborers a voice in the determination of the piece rates. Especially in countries such as England, where labor unions were strong, collective bargaining over wages could help to convince workers that the rates were fair (Pigou 1921, 436–445).

23 The lack of a technical program was not the only problem facing the economics profession in the interwar period. In the business community, their ideas may have been seen as a bit too radical (Gillespie 1983, 33–34). In contrast, the American institutional economists did have a more direct bearing on government planning at the national level (cf. Balisciano 1998, 157–160)

24 The sparse references to scientific literature in *The Functions of the Executive* are to Vilfredo Pareto’s treatise on sociology, to the Hawthorne experiments and its researchers and to Commons’s writings on labor economics (Barnard 1968, 202–205; Hoopes 2002, 1019).

25 The research on incentives and adjustment at the Harvard Business School was not only important for its industrial relevance; it also left its mark on (organizational) sociology and anthropology after the Second World War. A very preliminary survey of anthropological and sociological work quickly reveals a rich tapestry of studies on workers, motivation, industrial organization and leadership that followed the Hawthorne studies. Anthropologist William Lloyd Warner—supervisor of the Bank Wiring Test at Hawthorne—was the principal researcher of the famous Yankee City Series. This ethnographic research by Warner and Low on workers in a shoe factory between 1930 and 1935 resulted in *The Social System of the Modern Factory* (1947). William Foote Whyte—author of a classic in urban sociology, *Street Corner Society* (1943)—was another social scientist involved in the Mayo’s research group. Whyte published his own ethnographic research in *Money and Motivation: An Analysis of Incentives in Industry* (1955). George Homans, thirdly, was a member of the Pareto Circle at Harvard, which also included Elton Mayo, Chester Barnard, Talcott Parsons and Robert Merton (Cot 2011). Homans (1941) wrote a report
on worker fatigue for the National Research Council. Moreover, he visited and talked with managers, union officials, and workers in Detroit about the causes of frequent wildcat strikes (Scott and Homans 1947). In The Human Group, Homans (1965 [1950]) reanalyzed Hawthorne data that came from the bank wiring room and synthesized the available 'small group research'. Later, he collaborated with Roethlisberger for a study on The Motivation, Productivity and Satisfaction of Workers (1958). Robert Merton's 1947 article, “The Machine, the Worker and the Engineer” was also informed by the Hawthorne research, which he then incorporated into his theory of manifest and latent functions (1949). Finally, Alvin W. Gouldner (1950), edited a large number of contributions from members of the research network; he published these in a thick volume on leadership in modern society. As this brief list shows, many of the most well-known figures of the American social sciences profession were influenced by the Hawthorne research on human relations in industry.

26 In pursuing the genealogy of the incentive, I follow the official accounts of the experiments as presented by Mayo and coworkers. The emphasis on the sudden ‘discovery’ of the human factor in experiments that were meant to investigate the mechanical relationship between light and productivity was part of their strategy to constitute a new management style and not necessarily a good representation of actual experimental practice, as Gillespie has argued (1993, 47, 65).

27 Both the choice of method and the justification for the inquiry reveal the influence of Mayo’s human relations school (cf. Hall and Locke 1938, 82; Gardner 1940, 618).

28 The vulnerability of a technique that depends on the willingness of those interviewed was acknowledged by Florence, who wrote in a favorable review of Incentives and Contentment that “if the policy should become popular among employers with a different background, however, the interview might degenerate into an inquisition, and incentives would then be administered at the cost of contentment” (Florence 1939, 132).

29 Dickson stayed at Hawthorne for the rest of his career, but Warner continued with his anthropological study of American society. Together with a fellow anthropologist, and paid out of Mayo’s Rockefeller grant, he studied the small town of Newburyport in the early 1930s. This ethnographic research led to the famous Yankee City Series. In one of its volumes, Warner and Low set out to explain an unexpected strike in a shoe factory. While conducting the interviews and observations, they also found that the solidarity between workers of a certain department or team was related to their ethnic background. If the members of a working group had the same background, they could form a bloc more easily and this in turn gave them a better bargaining position vis-à-vis management. The wages of ethnically homogeneous groups were therefore significantly higher than the average wage of the factory worker. Again, the wage question was related to the social relations in the group (Warner and Low 1947, 92–98).

30 These sociograms subsequently found their way into mainstream sociology and were used by sociologists who were interested in human behavior in small groups (cf. Homans 1950).

31 I use ‘modest’ here to indicate that industrial democracy meant giving workers a voice but only in the issues that had a direct influence on their working life; no management consultant would opt for a ‘radical’ proposal that would allow workers to have a say in company policy.

32 The verdict of the Program Committee of the Fourth World Congress of the Econometric Society that took place in 1980 corroborates the view that incentives quickly became a hot topic. According to the committee, the congress sought to cover “those areas in economic theory and econometrics where important research has come to light during the last five years” and the economics of incentives was one of those areas (Hildenbrand 1982). A later commentator equally confirmed that the “incentive-based literature blossomed from the late 1970s onwards” (Mookherjee 2008, 241).

33 Socialist writers developed their own perspectives on the motivation to work as an alternative to the self-interested behavior of entrepreneurs (cf. Grant 2012, 143–144). Under socialism every individual knew that he worked primarily for the sake of society and this knowledge would “provide him with the most powerful incentive to do his best” (Mises 1963, 677). Mises dismissed this image of what motivates the individual in a socialist society as entirely fictitious.

34 Robbins explicitly alludes to Taylor’s management system in order to exclude it immediately thereafter: “Motion study, for instance, may yield generalizations applicable to more than one occupation. But motion study has nothing to do with economics” (1949, 33).
The microeconomics of planning also had its roots in the work of economic experts during the Second World War: “Microeconomists dealt with all aspects of war mobilization, including resource allocation, price controls, incentives, and the conversion from one productive activity to another” (Goodwin 1998, 63–64).

36 The portrayal of Hurwicz as the one who broke with the vagueness and verbalism of the past is consonant with the well-known figure of the individual innovator in hagiographic accounts of science. In this case, however, it is problematic to ascribe changes in a discipline’s perspective to an individual scientist without acknowledging the work of other scientists in that field. As Weintraub (2002) has shown, the mathematization of economics was not the work of a single scientist who was unhappy with the language being used. Instead, it was due to a complex process that was bound up with several contingent features such as problems in prevailing econometric research, conflicts between different policy-oriented institutions, critiques from economists on measurement without theory and abilities to raise private and public funding for further research. Moreover, while Maskin and Myerson are right in saying that things change when mathematical formalization and analytical frameworks enter the picture, reduction of ambiguity and instant precision are probably not the most subtle (or neutral) terms in which to evaluate these changes. It suffices to note that, when economists working within a mathematical framework seek a synonym for the incentive, they still use the vague and ordinary language verb ‘to induce’. More to the point is the idea that mathematical formalization redirects scientific interest toward certain themes and questions at the cost of others, and it strengthens the displacement of the governmental problem discussed in the previous section.

37 In addition, Marschak had also noted the issue of incentives in his less obscure “Elements for a Theory of Teams”. In that article, he developed a simple model for a team of two members that had to choose to act or not act on the basis of the information they had about the situation. Yet in modeling such simple teams, the question as to the particular interests of the individual members was explicitly left out. By treating the team as a single person with a single goal in mind, Marschak sought to eliminate “the difficult problem of bargaining and of incentives” (1955, 128). Although he recognized the potential problems of the decentralized nature of information and action, he thus postponed its investigation.

38 Pareto efficiency was the predominant, normative criterion used to judge whether the outcome of the mechanism was optimal for societal welfare. An economic mechanism was Pareto optimal if it led to an allocation of resources such that none of the individuals could be made better off without harming the utility level of another.

39 Costly verification refers to situations where the principal wants to review how the agent performed at the end of the contract. Verifying the performance of the agent brings costs with it—hence the term. Costly verification receives the least attention in the literature and will not be discussed further here.

40 Gailmard (in press) noticed that the flexibility of principal-agent theory was also “a sort of limitation” because of the fact that “within its domain of application there does not seem to be any pattern of behavior that a principal-agent model cannot explain”. This perceived limitation, however, has not hindered the proliferation of principals and agents discerned in the public sector.