

Practical considerations and skills: scientific knowledge as technical knowledge. MIT's Economic Department and the demarcation of mainstream economics territory

Camila Orozco Espinel¹.

* This version is incomplete and not finalized. Please do not quote or circulate without my permission*

MIT students developed a style that was either wonderfully pragmatic or disgustingly lacking in rigor, depending on your tastes: models derived from microfoundations were always the goal, but when observed experience was clearly at odds with what the models predicted, you'd just impose realistic behavior and leave its ultimate explanation as a project for the future.

Paul Krugman
28 February 2015
The New York Times

Introduction

Scholars of the 20th century history of economics have shown that in the United States during the years that immediately preceded and followed World War II, economics experienced great transformation². We now know that the years between 1930 and 1960 constitute a critical juncture in the emergence of what thenceforth became *mainstream economics*. The multiple trajectories, trainings and interests of postwar economists as well as the specific conditions of institutionalization of knowledge at different locations are now increasingly acknowledged and widely recognized³. Nevertheless, when explaining both the constitution of a new *mainstream* and the subsequent transformation of the discipline, specificities and distances remain a classificatory rather than explanatory principle. The purpose of this paper is twofold: First, present a frame to incorporate heterogeneity as an essential feature of

¹ PhD. Candidate, EHESS, camilaorozco@gmail.com

² For instance Roy Weintraub (2014) presents the 1940s as a “major break between and older and a newer economics” (3).

³ On the Economics Department of the University of Chicago see: Van Horn, Mirowski and Stapleford (2011) and Emmett (2010). On the Economics Department of MIT see: Weintraub (2014). On the Cowles Commission see: Düppe and Weintraub (2014), Boumans (2014), Mirowski (2002a; 2012; 2002b), Bjerkholt (2014). On the Department of Economics of Columbia University see: Camic and Xie (1994).

mainstream economics; Second, explore the *mainstream*'s heterogeneity by zooming in on one of the main locations of its emergence: the Economics Department of the Massachusetts Institute of Technology.

Rather than a logically unified system of theories or a group bound by a consistent set of principles and methods, we present *mainstream economics* as a heterogeneous territory in a multi-dimensional space structured by intellectual and social hierarchies. To incorporate heterogeneity and analyze its role, this paper focuses on the process of construction of the boundaries of this territory. Our analysis relies on the hypothesis that an inelible relationship exists between the position and dispositions of those who participated in the demarcation process, on the one hand, and the boundaries they drew, on the other hand. Due to the cognitive authority of science in American political culture, its different representations are a key entry point to issues of demarcation of academic territories. We focus on MIT's Economics Department to explore how—in the context of an engineering school—a representation of science as *technical* was mobilizing in demarcating the territory of *mainstream economics*.

The frame

Disciplines—relatively autonomous units of production and reproduction of a specific body of knowledge—are characterized by the articulation of three dimensions: research, teaching and professional activity (Heilbron 2004). Embedded in historically changing structures of power, within academia, but also beyond its boundaries, both their frontiers and the specific body of knowledge produced and reproduced, are neither natural outcomes of an “efficient” division of cognitive tasks and professional responsibilities, nor drawn once and for all. In these almost permanent demarcation processes of *areas of academic territory*, different academic groups guard external boundaries and police internal ones. Bourdieu's theory of action is useful to establish a connection between specific academic groups and their particular guarding and policing activities; that is to say, their respective functions in the demarcation process.

Endowed with different forms of capital, each group is defined by their relative positions in a particular and relatively autonomous *field* of economics. An intelligible

relationship between each group's functions in the demarcation processes can be established through its position in the *field* and their specific dispositions—that is to say, the specific ways in which they used their capital. To put it in or, in Bourdieu's terms, their *habitus*. This is, nevertheless, a complex dynamic and not a mere causality: political, economical and social mutations have an impact -are refracted- on the *field*. Bourdieu's action theory is thus also a *contextualization* tool.

Arguments regarding science are key elements during the contested process of disciplinary demarcation because:

- 1) Science possesses an uncontestable cognitive authority in modern societies — “Science is the next to being *the* source of cognitive authority: anyone who would like to be widely believed and trusted as an interpreted of nature need a license from the scientific community” (Barnes & Edge, 1982. In Gieryn 1995, 405); and
- 2) Since modern science is commonly depicted as organized in a wide range of disciplines, scientificity arguments are indeed mobilized regularly in debates between and within disciplines; in some cases they are even the only admissible argument.
- 3) Likewise, scientificity arguments are crucial while drawing disciplinary frontiers in the political arena. Yet, in US-American political culture, arguments regarding science play a structuring role in disentangling disciplinary legitimacy from moral, cultural and political bias.

However, far from being monolithic, science is ambivalent. At precise moments in time and within the boundaries of a single discipline, science can take empirical or theoretical forms, be pure or applied. More generally, there is a fundamental tension in science between basic and applied research, and between the empirical and theoretical aspects of inquiry.

Thomas Gieryn (Gieryn 1999; Gieryn 1983; Gieryn 1995) has shown with remarkable clarity, that science is a space that “acquires its authority from and through episodic negotiations of its flexible and contextually contingent borders and territory”(Gieryn 1995, 405). Different representations of science are mobilized to erect separate disciplinary boundaries in response to *different* challenges and obstacles to scientists'

pursuit of authority and recourses. Rather than an anomaly or a lack of consistence, the ambiguity of disciplinary boundaries results from the simultaneous pursuit of specific (but interconnected) disciplinary goals, each one requiring boundaries to be built in different fronts and ways.

In this context, heterogeneity -within a singular academic territory- is functional. Defending science as a pure-abstract form of knowledge by stressing its capacity to go beyond the observation of facts, allows scientist to present their work as independent of context and time – science establishes universal truths in the form of *laws*, and not just context-dependent regularities. Conversely, highlighting the technical character of science could prove to be useful when claiming objectivity and neutrality while keeping accuracy and practical concerns in the first as priority.

Thus, rather than a classification criteria, *mainstream's* heterogeneity gives insight while approaching its great resilience and, moreover, the very existence of a recognized *mainstream* in the discipline. There is no need to present the different *forms of doing economics* as variations from an archetype. Certainly, a *core* of methods can be identified. Nevertheless, as a corollary of the multidimensional character of disciplines, a *logic* of reinforcement connects the different boundaries to each other. Yet, the underlying *logic* of action behind demarcations should not be interpreted as a result of any sort of rational calculation other than the representation of scientific knowledge as validated by consensus.

In the context of US-American university-based professionalism, the creation of an economics program at MIT was crucial to erect *mainstream* boundaries from—and also bridges to—the political powers. Within the context of an engineering school, MIT's Economics Department established a unique position in academic economics applied to policy. In the US-American environment, where expert knowledge is not a technocratic arm of the state itself, the representation of science as technical was crucial to reach this position. Concretely, the technical authority of economics was operationalized at MIT through the use of mathematical, yet simple, models aimed to understand a few aspects of a situation and applied to a wide range of issues. An economics department at an engineering school is a particularly insightful entry point to explore the tension between “scientific investigation of technical problems”

MIT's Economics Department

Compared to other academic groups of significant importance during the pre- and post- WWII demarcation process of *mainstream economics*, the actual creation of an economics program at MIT started late. In fact, it did not actually start until Paul Samuelson arrived at the Institute in 1941. Before then, the economists at MIT were a rather small group providing service teaching to science and engineering students⁴. Samuelson was crucial to fostering and structuring economics at MIT—as we will see—, nevertheless, his arrival was part of a process that goes far beyond the boundaries of both the Department and the Institute.

The Massachusetts Institute of Technology

The thirties were at Massachusetts Institute of Technology a turning point. The idea—driven by President Karl Compton and Provost Vannevar Bush—was to move the Institute away from the practical engineering training institution it had been from its creation in 1861. Before Compton's arrival, Richard Maclaurin's election as president in 1909 -and especially after his 1919's "Technology Plan"- MIT's idiosyncrasy as a place for specialized practical subjects and applications to real-world situations was reinforced and closely connected to industrial pursuits (Lécuyer 2010, 62). Significantly, during the 1920s, in exchange of industry patronage, research tasks were taken at corporates requests and the alumni records shared with corporate recruiters.

Compton, physicist member of the Academy of Science, and Bush, one of the first MIT's PhD in engineering, wanted to restructure the Institute turning it into a research university. In this process, science departments and research programs were created and emphasis on science was put in both, the undergraduate and graduate engineering curricula. Indeed, students' first two years were devoted to basic training in mathematics, physics, chemistry, English and history (the upper-class years offered the opportunity for specialization). Likewise, several renowned faculties were added to science and engineering departments alike. The specific target was what Compton

⁴ For details of the first years of MIT PhD program see Cherrier (2014) and Garcia Duarte (2014). See also Samuelson (2007).

called -in opposition to “mere technical education”- *technological education*, “education in the fundamental principles along with a training in their applications to important basic process and problems” (President Compton annual report for the academic year 1930-1931 in Lécuyer 2010, 71).

Patronage patterns deeply changed during Compton and Bush’s restructuring. While the Institute maintained the close ties with industrial corporations developed during Maclaurin’s presidency, other private benefactors, such as the Rockefeller Foundation, swiftly became important. For instance, Samuelson’s recruitment was part of a project on technological change -that included the creation, in 1937, of an Industrial Relations Section at MIT- funded by a grant of significant importance from the Rockefeller Foundation. Nevertheless, the government soon became MIT’s main source of income. Contracts were signed with federal agencies, such as the Tennessee Valley Authority, and, conspicuously, money started to arrive, in unprecedented amounts, from the military.

Nor MIT was the only university equipped with an Industrial Relations Section -seven other universities received the same Rockefeller grant (Samuelson 2007)-, neither the Institute was the only institution signing important contracts with the government. Nevertheless, as Samuelson said, at the beginning of the 1940s, when “government money came in heavily to the universities and enriched their research opportunities [...], MIT was at the frontier” (Samuelson 2007). MIT’s pre-war relations to the political powers are crucial here. For instance, both Compton and Bush participated on the National Defense Research Committee (NDRC), an organization created to coordinate, supervise, and conduct scientific research on the problems underlying the development, production, and use of mechanisms and devices of warfare. While Bush, after leaving MIT to direct Carnegie Institution, acted as chair of the newly created institution, Compton was the head of the division D, division dealing with instruments and controls – radars in particular. During wartime the links connecting MIT to the political powers were deeply reinforced.

Having signed some four hundred contracts, MIT was the largest wartime research and development contractor of the US government (Kasier 2010). Conspicuously, Compton’s wrote in his 1945’s Annual Report: “MIT spent on its war contracts as

much money as it had spent on its normal activities during the previous 80 years of existence” (Douglas 2010, 95). Fourteen percent of the Institute’s total budget for the 1939-1940 academic year came from a contract to create a new laboratory to develop microwave radars. Headquartered at MIT, the Radiation Laboratory (Rad Lab), so named to disguise its real research objective, started operations in 1940 under the auspice of the Office of Scientific Research⁵. Nevertheless, this was but the first step in forging a new partnership with the military: all across MIT a new hybrid model of laboratory, where military problems were solved while a pedagogical enterprise was developed in parallel, took hold. Yet, while projects generated in the laboratories served as students’ thesis, MIT hosted special training courses for the military and government agencies (which included meteorology, aeronautical engineering, and chemical engineering) (Douglas 2010, 88).

Partnership with military agencies permanently transformed the Institute and continued after the end of World War II. Indeed, with the entrance of the United-States to the Korea War, MIT’s volume of the research conducted under contract with the government rapidly raised. The Institute operating budget leaped 36 percent during the first year of the new conflict and another 31 percent the following year – the fastest rates of growth since WWII (Kasier 2010, 105). During the 1950-1951 academic year, more than 96 percent of MIT researchers’ contracts came from the federal government (virtually all from the Department of Defense, the Atomic Energy Commission and the National Advisory Committee on Aeronautics). Yet, after the Korean conflict defense spending at MIT continued to climb exponentially well into the late 1960s. Adjusted for inflation, the volume of military sponsored research doubled every six years between 1948 and 1968 (Kasier 2010, 105). Throughout the 1950s and 1960s, sponsored research accounted for roughly 80 percent of MIT operation budget (Kasier 2010, 105). As Alvin Weinberg, physicist and director of the Oak Ridge National Laboratory (ORNL)⁶, noted in 1962:

⁵ The importance of the Rad Lab should no be underestimated. Twenty percent of the nation’s physicists had work on the Rad Lab’ only the Manhattan project employed more” (Douglas 2010, 95)

⁶ The ORNL is the largest science and energy national laboratory in the Department of Energy system. ORNL's scientific programs focus on materials, neutron science, energy, high-performance computing, systems biology and national security.

[...] it had become difficult to tell wheatear the Massachusetts Institute of Technology is a university with many government research laboratories appended to it or a cluster of government research laboratories with a very good educational institution attached (Kasier 2010, 109).

After the School of Humanities and Social Studies was founded in 1950, research on social sciences was incorporated into the Institute operations. For instance, the classified study known as “Project Troy”, focused on how to improve propaganda and physiological warfare techniques is a good example. “Rather than help to dilute the military’s influence on campus, the school settle quickly into the postwar patterns set by the School of Science and Engineering” (Kasier 2010, 114). One year after the School of Humanities and Social Studies was founded the Center for International Studies was added.

While the dangers of the fusion with the government and particularly with the military were discussed, they were seen as part of the Institute’s responsibilities. In his annual report for the academic year 1951-1952, MIT president Killian concluded, “an institute of technology has special resources which impose on it a responsibility in defense research different from many other kinds of educational institutions” (Kasier 2010, 108)

Science policy and MIT

Regarding science policy, well before the *Endless frontier*, MIT had an important role in the government. President Compton became, in 1933, chairman the Science Advisory Broad –the first presidential advisory broad appointed by Franklin D. Roosevelt. In this role, Compton advocated of a recovery program for science under the National Industrial Recovery Act. His proposal for a National Research Administration, called for expenditures of 100 million dollars for fellowships, contracts, and grants. While the proposal was not initially accepted, it was reanimated in Vannevar Bush’s proposal for a National Science Foundation after World War II (Rothenberg 2001, 133–134). Vannevar Bush, designed and built the new framework for science-government partnership during World War II, and help lay out the postwar path with his famous proposal, *Science: The Endless Frontier*. After the war, MIT’s

president, from 1894 to 1959, James Killian became the first chair of the president's Science Advisory Committee on its funding in the 1950s. (Kaiser 2010, 3)

The Institute close relations with the post-war powers is a key piece to understand the unique position of the MIT Economics Department in academic economics applied to policy. We now know that Paul Samuelson was one of the three actual authors of Vannevar Bush's report. In an interview in 2007, Samuelson, after discussing his participation in Bush's report, brings to the fore the importance of the Institute's close relations to power while, introducing his work as economic adviser for John F. Kennedy:

So MIT has been a great place to operate out of. It was in part because I was in MIT and Massachusetts that I became, for a time, the principal economic adviser for John F. Kennedy. Starting off when he was Senator and through the time that he was candidate for the primary and then the nominated thing, and then the president elect. Moses like I did not go in to the promised land though. But I could have if I had wanted to. And MIT was an important reason for that. (Samuelson 2007)

MIT and the transformation to the United States higher education system

In the wake of wartime projects, the government justified the skyrocketing expenditures in terms neither of gadgets delivered nor of instruments installed, but rather number of students trained. If with the return of war veterans MIT increased its enrolments significantly -by both, expanding access to already established programs and creating new (specially graduates) ones-, during the Cold War, the training of scientific and engineers became an urgent priority (Kasier 2010, 109–110). Indeed, during the 1940's and 1950s MIT increased its enrolments significantly.

The Act of 1944, known informally as the GI Bill brought about substantial transformation to the United Sates higher education system. And positively affected economics at the Institute.

Significantly, with the return of war veterans, economics graduate programs proliferated: Economics, a potential source of workplace credentials, was attractive to

GI's. MIT's PhD program in Industrial Economics was born in the midst of this influx of students. The growing emphasis on research purposes and graduate education created an opportunity in the postwar decades for economists at MIT to extend and to strengthen its credentials. The changes of the 1940s facilitated the development of the PhD program created in the 1940s whose focus showed an attempt to link the program with the core disciplines at MIT and to differentiate it from PhD programs in economics at the other research institutions. The development of the economics department also led to the establishment of a new undergraduate program of studies in economics and engineering, in 1946.

The Economics Department Project

A deliberated project to build an Economics Department oriented in a rather innovative and singular way started and settled throughout the next three decades under the new circumstances of the American higher education system and with the (financial and institutional) support of the Institute –but also enabled and reinforced by the peculiar demands of an Engineering school. While Samuelson's ideas structured the project and his presence and reputation propelled it, this was a team effort where Ralph Freeman (head of the department) and Rupert Maclaurin (professor Economics at MIT since 1936 and son of Richard Maclaurin, MIT president from 1909 to 1920) were active putting the group together. The well-endowed Institute, seemingly rising anew, was a fitting environment for such a project, as Paul Samuelson point it out comparing the late-1940s MIT and Harvard:

The great MIT department could never have been created at Harvard in the way that it was here. Because at Harvard every tub must end on its own bottom. You must already have the gift endowment to create a new professorship. If ten members of the Harvard tenured full professor faculty took an airplane to go to the annual economics convention and it crashed. At Harvard in those days, under the Groustein formula, they could have replaced one of them in one year. Another one two or three years later, another one so forth. This was to get equal opportunity. At MIT there was really no constitutional limits. Our saying always was how do we get out of our own way and do it....So that was how we were able to build up. (Samuelson 2007)

Engineering School

Two important features of the context of an engineering school should receive particular attention when explaining the specific shape that the Department's project took: the mathematical background of MIT's students and the applied character of their (engineer) training⁷.

Math back ground

As Samuelson (2007) pointed out, if students “were allergic to anything mathematical, [MIT was not] the place that they would be”. Samuelson knew this before he arrived. Roger Backhouse's (2014) account of Samuelson's *move* from Harvard to MIT gives significant archival evidence in this regard. For instance, in 1940, when Edwin Bidwell Wilson, Samuelson's mentor at Harvard, reassured him in his decision to accept MIT's offer, he stressed the students' mathematical and scientific training. Indeed, because it was an engineering school, MIT students' were all required to have two years of mathematics, physics and chemistry, with many of them having studied applied mechanics and thermodynamics. If Samuelson was “too mathematical” for Harvard (as for most economics departments at the early 1940s), MIT was a perfect fit.

Applied character of the engineering school

Wilson also insisted on the Institute's orientation to practical problems as one of the factors that could have a positive influence on Samuelson's work. Comparing Harvard and MIT, it was clear for Wilson that “economics at the Tech *because it is the Tech* will be keep closer to practical applied problems” (in Backhouse 2014, 67). At the *Tech* Samuelson could “have the chance to broaden out [...] on certain type of applications” (67).

⁷ As stressed in the articles of the most exhaustive and complete account available today of MIT's Economics Department (E. Roy Weintraub 2014), the engineering context of the Institute was decisive. See specially Weintraub (2014, 11), Cherrier (2014, 18–21), Backhouse (2014, 65–75)

A concern about the *right* mixture of basic science and engineering applications structured the Institute from its very foundation. Indeed, the combination of basic science and engineering practice played a pivotal role in the Institute's agenda⁸. This *cleavage* was institutionalized, from the 19th Century onwards, through laboratory instruction, where theory and practice were brought together. The idea was to introduce students to theoretical principles first, and, then expose them to practical problems and real world situations. MIT's seal (figure 1) and its motto *Mens et Manus* (Mind and Hands) (Smith 2010, 31) synthesize rather explicitly the niche William Barton Rogers, MIT's founder, was aiming to protect. This niche that proved useful while resisting the numerous attempts to merge MIT with Harvard during the late 19th and early 20th century⁹.

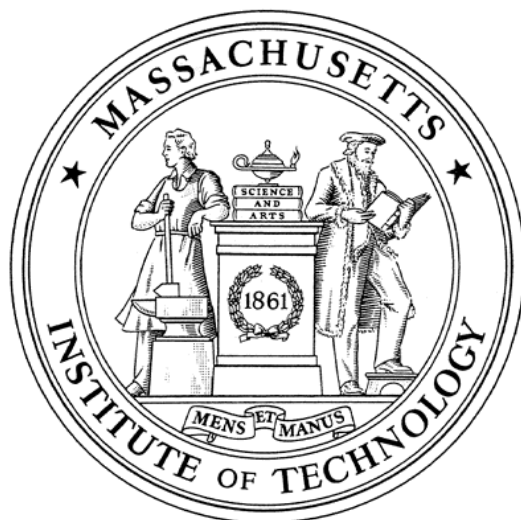


Figure 1.
MIT's seal.

Samuelson and Foundations

In 1947, the same year Samuelson was awarded the first J.B. Clark Medal, he published what came to be a watershed work: *Foundations of Economic Analysis*

⁸ French engineering practices were at this point of significant influence to the formation of MIT's educational program -just as they had with the establishment of West Point a half century early (Smith 2010, 24).

⁹ For an account -from the MIT's point of view- of the relations Harvard-MIT see Sinclair (2010).

(1947). While the book promoted Samuelson's already awarded reputation, his presence redounded on the Department, raising its visibility. *Foundations* put forward the guidelines of the *way of doing* economics associated with MIT. It relies on modeling microeconomic behavior as maximization under constraints and macroeconomic behavior as the interaction of aggregate demand and aggregate supply. Imperfect competition was part of the accurate analysis targeted and comparative statics the method to apprehend changes.

Beyond these principles, *Foundations* framed the very essence of what economics was about at MIT, establishing the scope and the approach to define and grasp *economic problems*. A distinctive form of doing economics emerged, one that Samuelson labeled *the neoclassical synthesis* in the third edition of *Economics* in 1955.

Driven by the transformation of the American higher education system and postwar restructure of science funding, MIT's way of doing economics is more than the result of an intellectual project starting from the ideas presented in *Foundations*: It is rather the result of the intertwining of a set of skills based on explicit principles, whose continuity was assured by its institutionalization in a PhD program in the context of an engineering school which was closely-connected (even inside) to the post-war sites of power.

Although Samuelson's and Solow's approaches to modeling were different, in the fifties MIT economics began to acquire its identity in the fifties, associated not only with Samuelson, and incredibly with Solow, but also with Kindleberger and Evsey Domar. The artifact that came to be known as Solow's growth model, gave insight into the *way of doing* economics associated with MIT.

Solow and models

At the end of the 1950's Robert Solow published three influential papers. One on steady state growth (1955-1956) and two on the technical change in aggregate production functions (1956; 1957). The objective was to provide an accurate

explanation of a “real world” situation by grasping its “essential” features in the simplest way possible so that, on the one hand, causality could be identified and, on the other hand, facts (data) could be interpreted. At this point in the mid-fifties, the European economies were beginning to grow rapidly and the United States did not experience the post-war recession which had been expected. The questions “in the air”, as Solow (2007) reminisced, were: “How to account for, how to provide an explanation of the growth paths of a modern economy, and why some [economies] grow faster than others? What Solow considered to be the legitimate existing literature -Roy Harrod (1939) and Evsey Domar (1946) – was lacking, he thought, the key feature of the problem: the exceptional character of crisis, “Harrod and Domar stories had the implication that growth path was unstable”. If they “couldn't have the thing right”, this was because, in Solow’s opinion, “The Depression of the 1930s [...] was so important precisely because it was such an exception. It was not the routine thing”. The idea was thus to grasp in a simplified form of a model this characteristic, the fact that in “the economies we know” there are deviations, occasionally bursts of rapid growth, occasionally bursts of slow growth or even a slight decline. But there's a tendency to get back. So there's some stability to that growth path.”

Solow, dismissing Harrod and Domar’s fixed-proportion assumption, and stressing the possibility of substitution between capital for labor, proposed what he considered to be a “better model”, a model that additionally to overcome Harrod and Domar “unfactual characteristic”, “would actually work”—that is to say “could be use to interpret facts, data”. “It turned out to be a good thing”, he stated later. After empirically implementing his model, Solow “discovered something that never entered [his] mind”: the main driving force for growth had not been the increase in population and the accumulation of capital goods, as “everybody else” before, had “taken for granted”—“if you looked at the data with all of the theory you could bring to bear, you could not make that story hold water”. Solow called this *main force for growth* technical progress.

Yet, Solow came out with the “collapsed production functions”, a major component of his model, while simplifying a dynamic input-output system in order to teach it to his students. Beyond its pedagogical role, the model was also an instrument of *measurement* of technological change as well a simplified world of the economy

where causal relations could be visualized (Halsmayer 2014). Swiftly *Solow's growth model* became the epitome of the MIT approach and reinforced the self-aware sense of identity of MIT economists, a rather endogamous group.

Indeed, a self-replication chain, that mostly relied in an adviser-advisee relationships, where a few key advisers mostly trained at MIT as well replicated a particular *way of doing* economics (Svorenčík 2014). Robert Solow (2007) described the Department as “one of these small, happy, high morale, everybody's in this together organizations”. Likewise, after teaching a visit course in the 1950s Hollis Chenery, then at Harvard, described the MIT's Economic Department as “the happiest economics department”. This is the ground where the overall consistency of MIT's *way of doing* economics is rooted.

Technical as craft

Internal hierarchies passed unnoticed for the most part¹⁰, and students were encouraged to collaborate with each other. Significantly, MIT's Economics Department was a very student-focused one. This becomes evident in the institutionalized *open door policy* that was, as well as in the importance that teaching had at the Department. There was a “sort of evolved the principle”: research time could not be bought off with research funds –“If you were a member of the faculty of the MIT economics department, you taught” (Solow, 2007). Students were taught in a set of principles; nevertheless, the consistency of MIT's particular *way of doing* economics was rather grown from a set of skills.

¹⁰ The fact that hierarchies passed unnoticed is certainly the result of the horizontality of the relations at the MIT Economics Department –what is more evident when we compare it with other Departments –the Economics Department of the University of Chicago for instance, infamous for its vertical and explicit hierarchical organization (see Emmett (2010)). Nevertheless, hierarchies existed, as was it evident for the black students of the Department (Darity and Kreger 2014). So they were between faculty. Samuelson description of the building up process of the department sheds light: “what subsequently came to happen was that this is the way it was done [...] me advising Ralph Freeman, I think we ought to try to get that guy, and we ought to avoid that guy. And I think this one is hurting. He's had some new babies and his salary ought to be increased. And the reason that could happen was that I was earning more, getting a higher salary than any of the rest of them. So there was no competition”(Samuelson 2007).

More than the result of a five steps recipe this particular *way of doing* economics, as narrow and specialized as the exercise looked like, the MIT approach was the result of a highly demarcating craft that could be mastered, with years of study and practice, in an apprenticeship fashion. Conspicuously, Paul Samuelson, in the 90th Nobel Jubilee, presented economics as an “inexact science [that] along with logic and mathematics, [...] still depends on the art of judgment” (PASP. Box 4. Folder: Nobel Jubilee). The importance of the adviser-advisee links, the open door policy and the focus on teaching is evident here: they contribute building the economist’s judgment. In other words, the institutionalization of these practices was a shield against indoctrination that for MIT economics differentiated its approach from those of other regions of *mainstream* territory. Solow’s, referee letter to Bernard Haley, editor of The American Economic Review, illustrated this point:

[...] the author simply brushes off any suggestion that the housing market may be too imperfect to admit simple conclusions from very simple theory. I am not an expert, and I am not asserting that the market for houses is too imperfect for the theory to apply. But the paper is written exactly as if we were talking about assigning apples of different quality to people of different income and tastes. And I know houses are different from apples. (The complete of external effects is especially surprising.) If the paper is ultimately to be published, Smith must argue the case that his conclusions may be applied with some confidence to Philadelphia. As it stands, it’s too doctrinaire (RSP. Box 1. Folder 1961)

Yet, the rules of MIT’s *way of doing* economics were precise enough that there was – they thought- little opportunity for waywardness, or at least protection from it. Thus, a stance of objectivity and neutrality was coupled with the not doctrinaire craft exercise institutionalized at the Department. Indeed, the *way of doing* economics streaming from *Foundations* and epitomized with Solow’s growth model, created quantitative technologies -“small tractable objects”- that situated their conclusion in an unbiased sphere.

As Theodor M. Porter showed (1997), the suspicions towards ideological underpinnings of knowledge coming from the social sciences, that characterized US-

American political culture, encouraged the use and reinforced the authority of these *quantitative technologies*.

In this context MIT models acted as concrete mechanisms of demarcation between academic territory and the political arena. While demarcation was accomplished, communication with political powers remained open, from the catalyzer questions to the conclusions. Conspicuously, in the final section of Solow's 1956 paper, the author emphasized that policy conclusions should not be *directly* drawn from his model. The idea was that stating from the "right strategic simplifications [...] generate a lot of understanding from focusing on a very small number of casual arrows" (Solow 1956, 92). Thus the authority to tell governments how to manage their affairs comes from the confidence in both existence of underlying rather simple causality relation between individual behavior and aggregate economic variables, and the possibility to access this form of causality through simplifications. Combined with the mastered of a set of skills protecting from indoctrination, demarcation and communication were accomplished.

If in the context of the Cowles Commission in the early 1950s "foundations" was link with a proof of the consistence of the theory, at MIT *Foundations* was related with *accurate* and *objective*. If at the Cowles Commission, in the process of seeking a deeper foundation for the theory, one "obvious problem was a proof of existence" (K. Arrow and Debreu 1954, 58), at MIT the existence of a solution was but a hypothesis of their models. If at Cowles a "broad approach to the analysis of existence" was to set before the theory "could be applied in many different directions" (K. J. Arrow 1992, 126), at MIT existence was a requirement that ought to be part of the design of the models already catalyzed by those *different directions*. If one of the *different direction* at Cowles was "the analysis of models which represented in one way or another imperfections in the competitive system" (K. J. Arrow 1992, 126), imperfect competition was constitutive of the *Foundations* of MIT. If "The requirement of proving an existence theorem in each case leads to the need for a rigorous spelling out of the assumptions" was central at Cowles, at MIT the *consistency* guarantee by the existence of a solution reinforced the stance of objectivity of their technologies of distance. Samuelson described *Economics*, his famous textbook, "as *accurate* and *objective* as the present-day science of economics enable one to be" (PASP, Box 11,

Folder American Institute for economic research). This accurate and objective knowledge was driven by the problem of the “economies we know” and thus from practical concerns.

At Cowles “the view that the competitive model is a reasonably accurate description of reality, at least for certain purposes, presupposes that the equations describing the model are consistent with each other” (K. Arrow and Debreu 1954, 60). So it was at MIT. In a letter to W. Leontief, after highlighting the importance of consistency, Solow rhetorically asks: “if we will not to guarantee the existence of an equilibrium solution, what confidence can we have in its other promises?” (in Halsmayer 2014, 237). Nevertheless, while in the context of the Cowles Commission “one *check on the empirical usefulness of the model* [was] the prescription of the conditions under which the equations of competitive equilibrium have a solution” (K. Arrow and Debreu 1954, 60), this was not a matter of prescription at MIT.

We see two different *habitus* in action. While for cowlesmen at the beginning of the 1950s, rigor preceded and drove application and practical concerns, practical concerns preceded and drove theory for MIT economists. If at Cowles a *mathematical habitus* led theory, at MIT an *engineer habitus* structured their *way of doing* economics. If economics was more closely associated with a pure and abstract representation of science at Cowles, economics was associated with a technical representation of science where applications and practical concerns drive theory at MIT. For Paul Krugman (2015), one of the most famous alumni of the MIT Economics Department, it was “obvious why this approach was better suited for producing future central bank governors, chief economists, and even pundits than an approach that elevated purity over realism”¹¹.

Pedro Garcia Duarte (2014) has identified the placement patterns of MIT’s PhD students during the period 1944-1959. His results show that, the majority students went to academic positions (38,5% in the 1940s and 66,7% in the 1950s), an important –though decreasing- percentage went to the private sector (30,8% in the

¹¹ A non-exhaustive list of MIT’s students that became central bank presidents includes: Mario Draghi (PhD MIT, 1977) European Central Bank; Stanley Fischer (PhD MIT, 1969; MIT professor, 1973-1979) FED; Ben Bernanke (PhD MIT, 1979) FED.

1940s and 9,9% in the 1950s) and worked for the government immediately after graduation (7,7% in the 1940s and 7,7% in the 1950s)¹². In a country that has traditionally filled its top civil service positions with outsiders, economists are a professional community rooted in universities. Thus reliance on academic institutions, deeply embedded in the nature of American political institutions, framed the interaction process between policy and the *technologies of distance*. Marion Fourcade (2009) used the metaphor of “scientific professionalism” to illustrate this process. For Fourcade the intervention of economics in public arenas has been shaped not only by their own “scientific” capabilities but also by the particular expectations emanating from the institutions that request such expertise in the first place” (128).

During World War II economists gained positions that provided a strong argument for acknowledging formally their specific role in government, both as highly skilled technicians within the administrative structure and as aides to decision making. Indeed, during the conflict economist work reached beyond the improvement of arcane technologies to core functions of the state, so it did the work of scientist in general. As Porter (2009) pointed out, “rarely, if ever, has science been so effectively merged into the bureaucratic elite as during the 1940s” (305). While the influence of science and economics waned with time, MIT economists managed to keep close to power, yet confined to matters of technique. Its compromise with realism over rigor, their *engineer habitus*, allowed them to be well-off in this role.

While the quantitative technologies that the “small tractable objects” resulted from the institutionalization of these practices endowed MIT economists with a stance of objectivity and neutrality, the judgment developed through highly demarcating craft as well as the commitment with the “economies we know” and to explain of what is “out there” gave relevance to their work. Relevance had a rather narrow meaning at MIT: technical knowledge.

MIT’s way of doing economics is one of the pillars of the frame (the mold?) in which new generations of economist worldwide have been socialized. From this mold a highly organized but rather promiscuous, intellectual edifice took on a life of its own.

¹² Compared with the results of the Bowen Report (1953), MIT PhD students went more – during the 1940s and 1950s- to the private sector and the government.

It is not accidental that MIT's reinterpretation of the agenda of scientism, was led and succeeded in the context of an engineering school.

Bibliography

- Arrow, Kenneth, and Gerard Debreu. 1954. "Existence of an Equilibrium for a Competitive Economy." *Econometrica* 22 (3): 265–290.
- Arrow, Kenneth J. 1992. "Nobel Lecture." In *Prize Lectures in Economic Sciences 1969-1980*. Singapur: World Scientific Publishing Company.
- Backhouse, Roger E. 2014. "Paul A. Samuelson's Move to MIT." *History of Political Economy* 46 (Supplement 1) (December 15): 60–77.
- . 2014a. "MIT and the Other Cambridge." *History of Political Economy* 46 (Supplement 1) (December 15): 252–271.
- Bjerkholt, Olav. 2014. "Trygve Haavelmo At the Cowles Commission." *Econometric Theory* 31 (01): 1–84.
- Boianovsky, M., and K. D. Hoover. 2014. "In the Kingdom of Solovia: The Rise of Growth Economics at MIT, 1956-70." *History of Political Economy* 46 (Supplement 1) (December 15): 198–228.
- Boumans, Marcel. 2014. "Friedman and the Cowles Commission." *Milton Friedman: Contributions to Economics and Public Policy, Forthcoming*.
- Bourdieu, Pierre. 1975. "La Spécificité Du Champ Scientifique et Les Conditions Du Progrès de La Raison." *Sociologie et Sociétés* 7 (1): 91–118.
- . 1982. *Homo Academicus*. Les Editio. Paris.
- . 1994. *Raisons Pratiques : Sur La Théorie de L'action*. Paris: Editions du Seuil. <http://www.amazon.fr/Raisons-pratiques-Sur-th%C3%A9orie-laction/dp/275784170X>.
- Bourdieu, Pierre, and Loic J. D. Wacquant. 1992. *An Invitation to Reflexive Sociology, Bourdieu, Wacquant*. Chicago: Chicago University Press.
- Bowen, Howard R. 1953. "Graduate Education in Economics." *American Economic Review* 43 (4): ii–xv+1–223.
- Camic, Charles, and Yu Xie. 1994. "The Statistical Turn in American Social Science: Columbia University, 1890-1915." *American Journal of Sociology* 59 (5): 773–805.
- Cherrier, B. 2014. "Toward a History of Economics at MIT, 1940-72." *History of Political Economy* 46 (Supplement 1) (December 15): 15–44.
- Darity, W., and A. Kreeger. 2014. "The Desegregation of an Elite Economics Department's PhD Program: Black Americans at MIT." *History of Political Economy* 46 (Supplement 1) (December 15): 317–336.
- Domar, Evsey D. 1946. "Capital Expansion, Rate of Growth, and Employment." *Econometrica* 14 (2): 137–147.
- Douglas, Deborah. 2010. "MIT and the War." In *Becoming MIT. Moments of*

- Decision*, edited by David Kaiser, 81–102. Cambridge, MA: MIT.
- Düppe, Till, and E. Roy Weintraub. 2014. *Finding Equilibrium: Arrow, Debreu, McKenzie and the Problem of Scientific Credit*. New Jersey: Princeton University Press.
- Emmett, Ross B. 2010. *The Elgar Companion to the Chicago School of Economics*. Northampton: Edward Elgar Publishing.
- Fourcade, Marion. 2009. *Economists and Societies: Discipline and Profession in the United States, Britain, and France, 1890s to 1990s*. Princeton, New Jersey: Princeton University Press.
- Garcia Duarte, P. 2014. “The Early Years of the MIT PhD Program in Industrial Economics.” *History of Political Economy* 46 (Supplement 1) (December 15): 81–108.
- Gieryn, Thomas F. 1983. “Boundary-Work and the Demarcation of Science from Non-Science: Strains and Interests in Professional Ideologies of Scientists.” *Review, American Sociological* 48 (6): 781–795.
- . 1995. “Boundaries of Science.” In *Handbook of Science and Technologies Studies*, edited by Sheila Jasanoff, Gerald E. Markle, James C. Petersen, and Trevor Pich, 393–443. Thousand Oaks: Sage Publications.
- . 1999. *Cultural Boundaries of Science: Credibility on the Line*. Chicago: Chicago University Press.
- Giraud, Y. 2014. “Negotiating the ‘Middle-of-the-Road’ Position: Paul Samuelson, MIT, and the Politics of Textbook Writing, 1945-55.” *History of Political Economy* 46 (Supplement 1) (December 15): 134–152.
- Halsmayer, V. 2014. “From Exploratory Modeling to Technical Expertise: Solow’s Growth Model as a Multipurpose Design.” *History of Political Economy* 46 (Supplement 1) (December 15): 229–251.
- Harrod, Roy F. 1939. “An Essay in Dynamic Theory.” *The Economic Journal* 49 (193): 14–33.
- Heilbron, Johan. 2004. “A Regime of Disciplines: Toward a Historical Sociology of Disciplinary Knowledge.” In *The Dialogical Turn: New Roles for Sociology in the Postdisciplinary*, edited by Chartes Camic & Hans Joas, 23–42. Lanham: Rowman & Littlefield.
- . 2006. *Naissance de La Sociologie*. Marseille: Éditions Agone.
- Heilbron, Johan, Remi Lenoir, and Gisèle Sapiro. 2004. *Pour Une Histoire Des Sciences Sociales: Hommage À Pierre Bourdieu*. Fayard.
- Kasier, David. 2010. “Elephant on the Chareles: Post Growing Pains.” In *Becoming MIT. Moments of Decision*, edited by David Kaiser, 103–122. Cambridge, MA: MIT.
- Krugman, Paul. 2015. “Empire of the Institute.” *The New York Times*.
<http://krugman.blogs.nytimes.com/2015/02/28/empire-of-the-institute>
- Lécuyer, Christophe. 2010. “Patrons and Plan.” In *Becoming MIT. Moments of Decision*, edited by David Kaiser. Cambridge, MA: MIT.
- Maas, H. 2014. “Making Things Technical: Samuelson at MIT.” *History of Political Economy* 46 (Supplement 1) (December 15): 272–294.

- Mirowski, Philip. 2002a. "Cowles Changes Allegiance: From Empiricism to Cognition as Intuitive Statistics." *Journal of the History of Economic Thought* 24 (2): 165–193.
- . 2002b. *Machine Dreams: Economics Becomes a Cyborg Science*. London: Cambridge University Press.
- . 2012. "The Unreasonable Efficacy of Mathematics in Modern Economics." In *Philosophy of Economics*, edited by Uskali Mäki, 159–197. San Diego: North Holland.
- Porter, Theodore M. 1997. *Trust in Numbers: The Pursuit of Objectivity in Science and Public Life. (eBook and Paperback)*. Princeton University Press.
- . 2009. "How Science Became Technical." *Isis* 100 (2): 292–309.
- Samuelson, Paul A. 1947. *Foundations of Economic Analysis*. Holiday House.
- . 2007. "Infinite Histor. Massachusetts Institute of Technology 150th Anniversary."
- Smith, Merritt Roe. 2010. "'God Speed the Institute': The Foundational Years, 1861-1894." In *Becoming MIT. Moments of Decision*, edited by David Kaiser, 15–36. Cambridge, MA: MIT.
- Solow, Robert M. 1955. "The Production of Function Capital the Theory." *Review of Economic Studies* 23 (2): 101–108.
- . 1956. "A Contribution to the Theory of Growth." *The Quarterly Journal of Economics* 70 (1): 65–94.
- . 1957. "Technical Change and the Aggregate Production Function." *The Review of Economic Statistics* 39 (3): 312–320.
- . 1997. How Did Economics Get That Way and What way Did it Get? In *American Academic Culture in Transformation*. Princeton University Press. New Jersey
- . 2007. "Infinite Histor. Massachusetts Institute of Technology 150th Anniversary."
- Svorenčik, Andrej. 2014. "MIT's Rise to Prominence: Outline of a Collective Biography." *History of Political Economy* 46 (Supplement 1) (December 15): 109–133.
- Teixeira, P. 2014. "Serving the Institute and the Discipline: The Changing Profile of Economics at MIT as Viewed from Textbooks." *History of Political Economy* 46 (Supplement 1) (December 15): 153–174.
- Thomas, W. 2014. "Decisions and Dynamics: Postwar Theoretical Problems and the MIT Style of Economics." *History of Political Economy* 46 (Supplement 1) (December 15): 295–314.
- Van Horn, Robert, Philip Mirowski, and Thomas Stapleford, ed. 2011. *Building Chicago Economics. New Perspectives History Americas Most Powerful Economics Program: History of Economic Thought and Methodology*. Cambridge: Cambridge University Press.
- Weintraub, E. R. 2014. "Introduction: Telling the Story of MIT Economics in the Postwar Period." *History of Political Economy* 46 (Supplement 1) (December 15): 1–12.

Weintraub, E. Roy, ed. 2014. *MIT and the Transformation of American Economics*.
Durham: Duke University Press.