

# GROUNDING IN METHODOLOGY, CERTIFIED BY JOURNALS: THE RISE AND EVOLUTION OF A MAINSTREAM IN ECONOMICS

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# Grounded in Methodology, Certified by Journals: The Rise and Evolution of a Mainstream in Economics

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## Abstract

In this paper, we introduce a new understanding of the mainstream notion in economics. Its distinct character is based on a set of methodological standards deemed compulsory in the theoretical or empirical practice of the discipline. We contend that a theoretical mainstream arose around the 1980s, when the prevailing methodological standards in microeconomics and game theory – mathematical language, equilibrium discipline, and ‘explicit micro-foundations’ – came to be adopted in theoretical papers across a wide range of fields and specializations. We further argue that the 1990 period witnessed the surge of a distinct empirical mainstream and the emergence of a joint mainstream, the result of the rise of experimental economics and a renewal of applied economics centered on the notion of causal inference. An examination of the contents of the articles published in top journals in selected years from 1970 to 2018 confirms our contention.

**Keywords:** mainstream, neoclassical approach, experimental economics, causal inference, methodology

**JEL codes:** A 10, B 20. B 41, C 9

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## 1. INTRODUCTION

Our aim in this article is to assess the existence of a mainstream in economics. In its common understanding, the notion of mainstream refers to prestige or power.<sup>1</sup> It is mainly found under the pen of ‘heterodox’ economists. The latter understand it derogatively, associating it with the neoclassical approach and lamenting the lack of pluralism it generates. By contrast, those who supposedly belong to the mainstream scarcely mention the word. Nonetheless, they implicitly admit the reality behind it by taking for granted the existence of a single shared view about what constitutes admissible research lines in economics.

The originality of our paper is that we understand the mainstream notion in a new way by giving it a methodological grounding. In our view, belonging to the mainstream is a matter of respecting well-defined methodological standards. In this acceptance, the notion encompasses more than a mere statistical preponderance. Moreover, this new understanding sheds new light on the development and present state of our discipline.

Two taxonomies lie at the heart of our analysis. The first, which we call the mainstream/non-mainstream divide, separates the papers abiding by the standards from those which do not. The second, which is instrumental to the study of the former, is the PT/T&M/PM taxonomy, where PT stands for ‘pure theory’, T&M for ‘theory-cum-measurement’ and PM for ‘pure measurement’.<sup>2</sup> This taxonomy assesses the kind of contribution to economic knowledge made by a given paper. That of PT papers is exclusively theoretical. T&M papers make both a theoretical and an empirical contribution. PM papers provide empirical evidence without making a theoretical contribution. Either they collect (or create) and analyze factual evidence, or they empirically test theoretical propositions. PM papers differ from T&M papers by not making a proper theoretical contribution.

The bulk of the paper consists in a historical/methodological study of the evolution of economics from the post-WWII period to the present, centered on bringing out the rise of a mainstream in economics, its composition, and transformation. We locate the ascent of a methodologically grounded mainstream in the 1970–1990-time span, with the emergence of a ‘theoretical mainstream’ pertaining exclusively to the PT + T&M cluster. We explain that its coming into existence is the result of an endogenous change that occurred more or less simultaneously in different sub-disciplines of economics. Thereby, a set of methodological choices that were already compulsory in microeconomics and game theory – the equilibrium discipline, mathematical reasoning, and explicit micro-foundations – came to prevail in a broad

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<sup>1</sup> E.g., “Mainstream consists of the ideas that are held by those individuals who are dominant in the leading academic institutions, organizations and journals at any given time, especially the leading graduate research institutions” (Colander, Holt and Rosser 2004: 490). Cherrier (2016) discusses how the notion of mainstream came to be used in economics.

<sup>2</sup> Our terminology is borrowed from T. Koopmans’s 1949 article, entitled “Measurement without theory” in which he criticized A. Burns and W. Mitchell’s 1946 *Measuring Business Cycles* book. Unlike Koopmans, we do not disparage this type of work. In his study of the transformation of U.S. economics between 1920 and 1960, Backhouse (1998) uses a similar taxonomy.

range of other fields. A further development in the mainstream configuration of economics occurred in the last decade of the past century with the ascent of methodological standards for a mainstream related to the PM cluster and associated with the emergence of laboratory experiments, randomized controlled trials, and natural experiment papers – the famous ‘empirical turn’. We call it the ‘measurement mainstream.’ A third step occurred at the turn of the century, the emergence of a joint mainstream pertaining to T&M papers abiding both by the theoretical and the measurement mainstream criteria.

The creation of a mainstream came along with a broader sociological transformation: ‘certification’ – that is, assessing existing practices or experiences in terms of their quality. Nowadays, certification permeates economics, as the multitudinous rankings of journals (and the invention of the ‘top journal’ label), departments, and people attest to. Once a consensus became established as to the required standards, economic journals have started to act as certification device. Being mainstream becomes a necessary condition for publication in top journals.<sup>3</sup>

This brings us to the empirical contribution of our paper, which consists in an examination of the content of articles published in top journals between 1970 and 2018. Among its several results, two stand out. The first is that the share of papers published in the four top journals fulfilling the criteria for a methodological mainstream amounted to approximately 90% in 2010 compared to 40% in 1970. The second pertains to the empirical turn which occurred around the millennium. We find that the share of PM papers rose from 11% in 1970 to 35% in 2018. Despite this impressive increase, the share of papers making a theoretical contribution (either PT or T&M papers) still represents 65% of total publications in 2018. Thus, the importance of the empirical turn must not be overrated. What, however, has plummeted is the share of PT papers. It fell from 65% in 1970 to 24% in 2018.

A study like ours, covering a half-century time span, faces a choice between two research orientations. In the first, the focus is on actors, institutions, and power relations, in the second, on methodological choices and their changes over time. These two projects are complementary, yet they can hardly be taken up in a single paper. Though we have opted for the second, the present work may still prove useful for researchers taking the first.<sup>4</sup>

Section 2 provides a brief literature review. Since the existence of a theoretical mainstream in economics and the predominance of neoclassical economics are connected issues, it is necessary to reconstruct the main constituents of neoclassical economics. This is done in Section 3. In Section 4, we provide an overview of the state of economics in the two

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<sup>3</sup> Thereby, the methodological and sociological aspects of the mainstream phenomenon come to be pieced together. Fourcade, Ollion, and Algan characterize economics as displaying a “tight management of the discipline from the top down,” “more consensus and more control than in sociology and political science, and more homogeneous standards of evaluation” (2015: 91, 96). This last characterization is tantamount to declaring that economics is a field wherein a mainstream is present.

<sup>4</sup> On the first line, see Fourcade (2010).

post-World War II decades, arguing that at the time the conditions for a methodological mainstream were not reunited. In Sections 5, we study the 1970-1990 period, which evinced the emergence of a theoretical mainstream. In Section 6, we study the ascent of the measurement mainstream starting in the mid-1990s and evoke the possibility of a joint mainstream. In Section 7, we expound the results of the empirical investigation we carried out to check the validity of the historical/theoretical analysis made in Sections 4, 5 and, 6.

## 2. THE LITERATURE

The existence of a mainstream in economics has been a recurrent theme of discussion among historians of economics. A pioneering event was the 1998 History of Political Economy Conference on the topic, “From Interwar Pluralism to Postwar Neoclassicism”, which took place at Duke University (Morgan and Rutherford 1998). Most of the papers presented there expressed a feeling of nostalgia for the ‘good old times’ of pluralism, when institutionalism evolved on a par with neoclassicism – a state of affairs which, they claim, disappeared in the 1960s.

Other historians of economics took the opposite viewpoint. In his Presidential Address to the History of Economics Society 1999 Conference, D. Colander proclaimed the death of neoclassical economics (Colander 2000). His claim was further developed in a joint 2004 paper with R. Holt and B. Rosser, “The Changing Face of Mainstream Economics.” In the same vein, several authors, in particular J. Davis, came to claim that a new ‘pluralistic mainstream’ was emerging, a conglomerate of new non-neoclassical research lines (Davis 2006, 2008). A few years later, R. Backhouse and B. Cherrier (2014, 2017a, 2017b) narrowed down the claim of a change in mainstream. According to them, applied economics has become the new up-and-coming research line.<sup>5</sup> In 2016, eighteen years after the From Interwar Pluralism HOPE Conference, they organized a new HOPE Conference, ‘Becoming Applied: The Transformation of Economics after 1970’, on the subject.

In the last decade, several quantitative studies (Angrist, Azoulay, Ellison, Hill, and Feng Lu (2017), Biddle and Hamermesh (2017), Brice and Montesinos-Yufa (2019), Card and DellaVigna (2013), Hamermesh (2013), Kelly and Bruestle (2011), and Panhans and Singleton (2017) have confirmed Backhouse and Cherrier’s view. Indeed, the last five decades have testified to a significant decrease in PT papers and a concomitant increase in empirical papers. The authors of all the papers mentioned above take a pragmatic standpoint that consists in building a cursory typology and measuring the share of each type in the total number of articles published in top journals. Our paper improves on these contributions in two ways: firstly, by providing a historical/methodological study of this transformation, and secondly, by using a more fine-grained typology in our empirical examination.

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<sup>5</sup> In their words, “Economists no longer view economic theory as standing above applied work in the (same) way as they had by the end of the 1960s.” (Backhouse and Cherrier 2014: 21)

### 3. NEOCLASSICAL ECONOMICS. ITS RISE AND CHARACTERIZATION

The neoclassical approach is the outcome of the ‘marginalist revolution’ associated to the names of A. Marshall, C. Menger, and L. Walras.<sup>6</sup> Their aim was to supplant the classical approach in its Ricardian variant and to replace it with a new one, which they deemed more scientific. At stake in such a project was replacing existing basic methodological choices with new ones.

Neoclassical economics depicts the economy as composed of separate individuals entering exchange relations guided by their self-interest. Its distinct take is the adoption of the subjective theory of value. According to the latter, the determination of relative prices and equilibrium quantities is built on the twin notions of marginal utility and marginal productivity. The underlying intuition is the ‘principle of substitution,’ the idea that households’ optimizing behavior requires them to keep substituting the quantity of any pair of goods they plan to consume up to the point where the marginal rate of substitution between the two goods (which, under a suitable mathematical representation of their preferences, corresponds to the ratio of their marginal utilities) and their relative prices are equal. *Mutatis mutandis*, the same principle is applied to the production process. The above has to do with individual equilibrium. Equilibrium in a given market (partial equilibrium analysis) or equilibrium in the entire economy (general equilibrium analysis) is a state where prices are such that all agents’ optimizing plans have been made compatible.

To come to grips with an as gigantic an object of analysis as ‘the neoclassical approach,’ we build on A. Leijonhufvud’s suggestion to regard the development of economic theory as a decision-tree (Leijonhufvud 1994). In this view, constructing theory amounts to making decisions about basic methodological nodes. They can be compared to forks or bifurcations in a road. Choosing one rather than another puts the theory on different tracks. First, there are basic choices to be made. Once a given branch has grown, the choices become more specific – that is second-, third level, etc. choices are to be made. The longer the sequence, the sturdier the branch, i.e., the research line.

The decision-tree approach can be used for differentiating the neoclassical approach from the classical and the institutionalists ones. Table 1 summarizes these differences in basic methodological choices. For lack of space, we do not comment them.

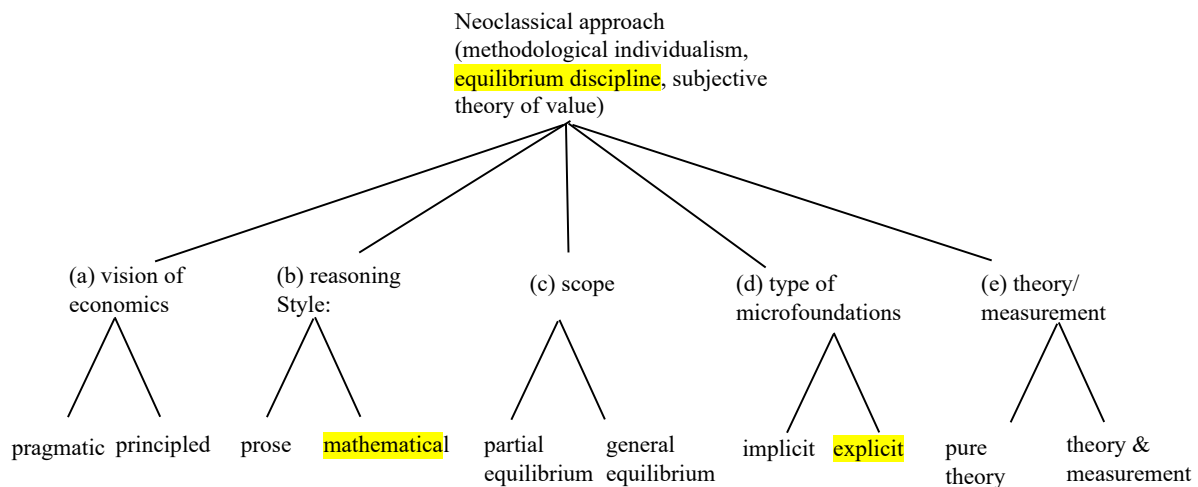
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<sup>6</sup> The marginal revolution in Great Britain originated in the works by Jevons. However, since Marshall’s *Principles of Economics* became the Magna Carta for economics for decades, we will refer to him rather than to Jevons.

Table 1. Differentiating between the classical, institutional, and neoclassical approaches

		Classical approach (Ricardo/Marx)	Institutional approach	Neoclassical approach
<b>Nodes</b>	<b>Bifurcations</b>			
<i>Vision of society:</i>	sociologically structured	✓	✓	
	atomistic			✓
<i>Equilibrium perspective:</i>	present	✓		✓
	absent		✓	
<i>Value theory:</i>	labor theory of value	✓		
	subjective theory of value			✓
<i>Reasoning style:</i>	prose	✓	✓	✓
	mathematical			✓
<i>Institutional perspective:</i>	present	✓	✓	
	absent			✓

For our purpose, what matters most is to bring out the bifurcations that open up once the decision to adopt the neoclassical approach has been made – that is, the methodological nodes its founders more or less unwittingly faced when starting their theoretical construct. They are displayed in Figure 1.

Figure 1. The basic methodological nodes of the neoclassical approach<sup>7</sup>

(a) Vision about how the construction of economics. The choice here is between a pragmatic approach to economics aiming at addressing concrete issues and an abstract one geared towards matters of principles, with the use of artificial model economies. In the first case, theoretical propositions pertain to the real world, in the second, to the fictitious model economy. External consistency is the overarching aim of the first line, internal consistency that of the second. Marshall took the first bifurcation, Walras and Menger the second.

(b) Mathematical versus non-mathematical reasoning. Walras (and Jevons) regarded the use of the mathematical language as compelling. Menger was fully against it. Marshall's standpoint

<sup>7</sup> More below on the yellow-colored slots.

was ambiguous. He found mathematics useful to test the consistency of one's ideas yet believed that its use had to be confined to the appendixes of theoretical works.

(c) Scope of the analysis. This bifurcation separates partial equilibrium (the study of isolated markets and bilateral exchanges) and general equilibrium analysis (the study of multilateral exchanges in the economy as a whole). Marshall opted for the first, Walras and Menger for the second.

(d) Type of microfoundations. All neoclassical economists agree with the principle that the study of the market economy must have individual decision-making as its starting point. Moreover, the general assumption about individual decision – drawn from A. Smith's considerations in *The Wealth of Nations* – is that economic agents behave in a self-interested way. However, when it comes to studying the functioning of markets, Marshall found it reasonable to skip the formal derivation of the market demand and supply functions from households' choices, and to start directly by analyzing aggregate functions. We call this standpoint 'implicit micro-foundations.' For their part, Menger and Walras were of the view that this individual decision-making stage could not be set aside. We capture this choice by stating that they took the 'explicit micro-foundations' bifurcation.

(e) Measurement. As far as the relation between theory and measurement is concerned, two bifurcations presented themselves to neoclassical economists: engaging in purely theoretical contributions or in contributions mixing theory and measurement. Menger and Walras confined themselves to pure theory. Marshall was certainly the most empirically inclined of the three. Though his approach to empirics was a far cry from further developments, we nonetheless ticked him in the theory + measurement slot.

Table 2 summarizes Marshall's, Menger's, and Walras's respective basic methodological choices.

Table 2 The differences in basic methodological choices between Marshall, Menger, and Walras

		<b>Marshall</b>	<b>Menger</b>	<b>Walras</b>
<i>Vision of economics</i>	pragmatic	✓		
	principled		✓	✓
<i>Reasoning style</i>	prose	✓	✓	
	mathematical	✓		✓
<i>Scope</i>	partial equilibrium	✓		
	general equilibrium		✓	✓
<i>Micro-foundations</i>	implicit	✓		
	explicit		✓	✓
<i>Theory/measurement</i>	pure theory		✓	✓
	theory + measurement	✓		



What is striking in this taxonomic exercise is that Marshall, Menger, and Walras differed in the basic methodological bifurcations they decided to take. We regard this as meaning that, though clearly separate from the classical and institutional approaches, the early neoclassical approach was broadly delineated and methodologically diverse. Put differently, a methodologically grounded mainstream was absent.

#### 4. ECONOMICS DURING THE FIRST TWO POST-WWII DECADES

In the first chapter of *The Wealth of Nations*, Adam Smith wrote that it was just a question of time for division of labor to make its way into the sphere of knowledge.<sup>8</sup> The history of economics in the second part of the twentieth century testifies to the soundness of Smith's prediction.

##### *Ramifications*

Figures 2 and 3 illustrate the evolution of the economics discipline from the first quarter of the twentieth century to its middle. Whereas its internal configuration was simple at the dawn of the century, by 1970 it had become intricate.

Figure 2. The configuration of economics at the turn of the 20<sup>th</sup> century

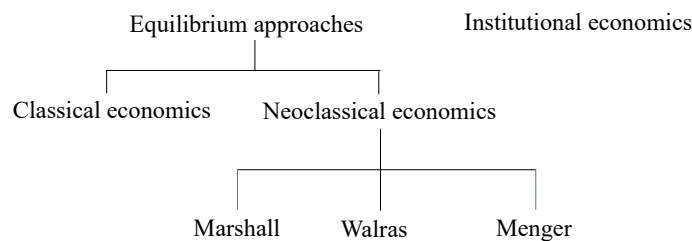
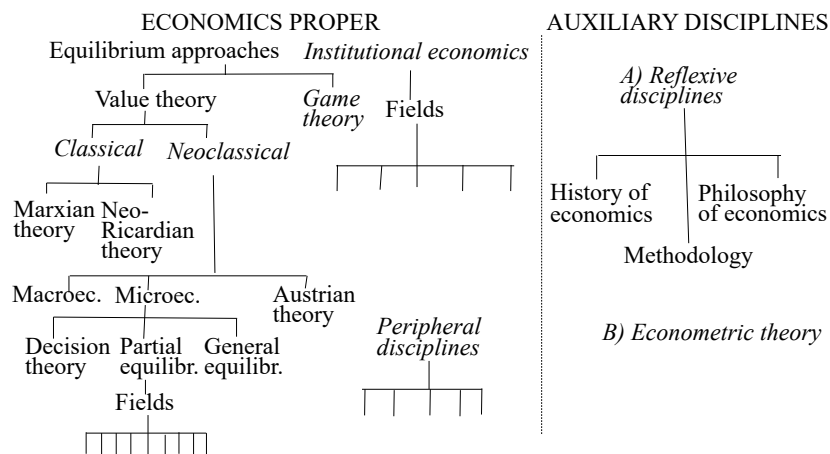


Figure 3. The configuration of economics in 1970



<sup>8</sup> "In the progress of society, philosophy or speculation becomes, like every other employment, the principal or sole trade and occupation of a particular class of citizens. Like very employment too, it is subdivided into a great number of different branches, each of which affords occupation to a particular class of philosophers; and this subdivision in philosophy as well as in every other business, improves dexterity, and saves time. Each individual becomes more in his own peculiar branch, more work is done on the whole, and the quantity of science is considerably increased by it" (Adam Smith, *An Inquiry into the Nature and Causes of the Wealth of Nations*, 1976, Oxford University Press: 21-22).

To help the reader understand what lies behind Figure 3, we invite her to suppose that on some occasion in 1970, such as an annual conference of the AEA, participants were interviewed about the type of economics they embraced. We surmise that their answers would have fallen in one and only one of the seven categories that are displayed in italics in Figure 3, classical economics, neoclassical economics, game theory, institutional economics, peripheral disciplines (e.g., law and economics, economics systems, and economic history), econometrics, and reflexive disciplines. If asked to be more specific, the economists interviewed would probably have mentioned the field in which they worked, or their specialization. By the latter, we mean a research community whose members share the same narrow object of study, the same vision of how to approach it, and the same toolbox.<sup>9</sup> They can either be sub-communities within a given field – e.g., microeconomists specialized in decision theory or in general equilibrium analysis – or communities centered on a specialization spanning different fields – e.g., the economics of information or the quasi-experimental economics community.

The 1960s were witness to two novelties of note: the emergence of game theory and econometrics. Game theory arose in 1944 with J. von Neumann and O. Morgenstern’s book, *Theory of Games and Economic Behavior*, and was reshaped by J. Nash through seminal articles published in the beginning of the 1950s. The period we are interested in this section were the years of ‘classical game theory,’ to use L. Samuelson’s label in a 2016 *Journal of Economic Perspectives* article, “Game Theory in Economics and Beyond” (Samuelson 2016).

Within economics, game theory occupied a rather isolated niche in the 1960s and 1970s. It was pursued by people who were known specifically as game theorists and who did almost nothing but game theory, while other economists had little idea what game theory was. Game theory was taught only in occasional specialty courses. Nonetheless, game theory was surrounded by a buzz of anticipation and excitement, especially moving into the 1980s and early 1990 (Samuelson 2016: 107).

In view of the taxonomic nature of our paper, we need to come to grips with an issue that is scarcely addressed, the relationship between game theory and neoclassical economics. That is, must game theory be housed in neoclassical economics or *vice-versa*? Samuelson rightly asserts that “the answer is not obvious” (2016: 107). However, unlike him, we cannot sit on the fence. After giving it much thought, we have opted to regard the two approaches as separate. A first justification for this standpoint relates to the range of applicability of game theory and neoclassical economics. The former covers a broader domain than the latter, as it hardly limits its investigations to economic issues. A second justification is Myerson’s point

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<sup>9</sup> As noticed by Cedrini and Fontena (2018), insufficient attention is usually paid to specialization in discussions of the development of economics. An exception is the 1991 *Economic Journal* Symposium on The Next Hundred Years of economics, as the following two excerpts illustrate: “Economists will be an increasingly heterogeneous assortment of scholars” (Pencavel 1991: 85). “As progress is made into understanding the various branches and processes of economics, more detailed knowledge and expertise is required. This involves investment on the part of the individual in certain analytical techniques, necessitating his specialization to that sub-area” (Turnovsky 1991: 143). The interlinkage between fields and specializations is an ever-moving reality, hence as documented by Cherrier (2017) in her study of JEL codes, the difficulty of fixing it in a stable way.

that game theory is “a more general analytical framework for doing rational-choice analysis without the traditional market structures of goods and prices” (1999: 3).<sup>10</sup>

Let us now turn to econometrics. Empirical economics, broadly understood, started in the first decades of the twentieth century with the creation of new research institutions dedicated to the statistical study of business fluctuations. The Kiel Institute for the World Economy in Germany, founded in 1914, and the National Bureau of Economic Research in the United States, founded in 1920, are two such examples. S. Kuznets’s name comes to mind for his statistical work on the secular evolution in production and prices, and the long-run changes in the U.S. GDP (1930, 1951). The rise of econometrics proper is usually attributed to J. Tinbergen’s two-volume *Statistical Testing of Business Cycle Theories* (1939) – a statistical investigation of business fluctuations in the U.S. for the post-1918 period, written under the auspices of the League of Nations. The Klein-Goldberger model, created at the Cowles Commission, gave structural econometrics its momentum (Klein and Goldberger 1955), paving the way for several generations of structural econometric models. Soon, the econometrician community split into two distinct specializations, econometric theorists, on the one hand, and applied economists (the term commonly used for people who do applied econometrics), on the other – in Christ’s terms, the “rigorous and the applied realms” (Christ 1966: 9). Those in the rigorous realm are more statisticians than economists, and their main interest resides in methodological problems. By contrast, those in the applied realm regard themselves as economists. To them, econometrics is ancillary to economics, its function being to check the empirical validity of economic models or propositions. The relationship between econometrics and the PT/T&M/PM taxonomy can be clarified as follows. First, we regard econometric theory as an auxiliary discipline and hence off the taxonomy. Second, applied economics papers can belong either to the T&M or the PM cluster according to whether they make a theoretical contribution. Finally, not all PM papers comprise an econometric component; this is the case of data-gathering papers.

#### *The absence of a methodologically grounded mainstream*

In the 1960s, the neoclassical approach was already statistically predominant in the most renowned journals of the time. Nonetheless we claim that this predominance was not tantamount to the existence of a methodologically grounded mainstream. Two reasons explain this: the internal diversity displayed by the neoclassical approach in these times and a selective lack of exclusion of non-neoclassical works from top journals. Table 3 displays the first of these reasons by outlining how several fields or specializations in economics – industrial

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<sup>10</sup> Behind Myerson’s remark looms a deeper contrast pertaining to the definition of economics. Game theory stands on the side of L. Robbins’s definition, according to which economics studies how people make choices (Robbins 1935). Neoclassical economics rests on the Smith-Marshall-Walras definition according to which economics has for object value theory, a query about the efficiency of an economic system ruled by market forces.

organization, macroeconomics, decision-making theory, general equilibrium, and Austrian theory – fared with respect to the methodological nodes presented in Table 2.

Table 3. The methodological diversity of the neoclassical approach in the 1960s

		I.O.	Macro.	Decision theory	General equilibr.	Austrian theory
1. Vision of economics:	pragmatic principled	✓	✓	✓	✓	✓
2. Reasoning Style:	prose mathematical	✓	✓	✓	✓	✓
3. Scope:	individual equilibrium partial equilibrium general equilibrium	✓	✓	✓	✓	✓
4. Micro-foundations:	implicit explicit	✓	✓	✓	✓	✓
5. Theory/measure:	pure theory theory & measurement	✓	✓	✓	✓	✓

Table 3 conveys the view that neoclassical economics was hardly monolithic in the 1960s; it rather displayed internal diversity. Furthermore, this diversity within the neoclassical approach had an impact on the relationship between neoclassical and non-neoclassical economists. For example, institutional economists who were skeptical of the virtues of mathematics could feel a methodological acquaintance with Austrian economists, who did not resort to mathematical reasoning, and with macroeconomists who used a modicum of mathematics. Classical economists of the Sraffian type could league with general equilibrium theorists because of their common use of high-brow mathematical methods. It can be argued that this state of affairs percolated into publications. Take the case of economists like P. Garegnani, L. Pasinetti, S. Weintraub, or P. Davidson, who would now be considered ‘heterodox.’ In the 1960s and the 1970s, they published in the most renowned journals. While they kept writing abundantly in the following decades, their articles ceased to be published in the most prestigious journals.

The same conclusion as to an absence of a methodologically grounded mainstream must be made about applied economics. At the time, PM papers were just starting, and no precise methodological rules governing them prevailed (more on this below).

## 5. THE 1970-1990 PERIOD: THE RISE OF A THEORETICAL MAINSTREAM

According to our definition, the existence of a mainstream is conditioned on the existence of basic compulsory methodological bifurcations acting as barriers to entry and adopted for some supposedly good reason. In this section, we show that a mainstream arose in economics between the 1970s and the 1990s. It resulted from the ability of some economists in other fields than microeconomics or game theory to persuade their colleagues that it would be a good thing if the three methodological bifurcations that already prevailed in microeconomics and game theory became a *sine qua non* in their disciplines too – the equilibrium discipline,

mathematical reasoning, and explicit micro-foundations, the yellow-colored slots in Figure 1. No more basic bifurcations could be conceived of – altogether nothing is said about the other basic methodological bifurcations. We call this the ‘theoretical mainstream’ because the presence of a theoretical input is a necessary condition for belonging to it. PM papers are excluded from it by definition.

This transformation generated a split between ‘narrowly delineated’ and ‘broadly delineated’ neoclassical works. It relates to two of the above-listed decisional nodes, the reasoning style and the type of micro-foundations bifurcation chosen. Belonging to the narrowly delineated neoclassical requires using the mathematical language and adopting the explicit micro-foundations bifurcation. Papers which fail to make these choices – i.e., which belong to the broadly delineated definition of neoclassical economics – are excluded from the mainstream.

To make our case, we proceed in three steps. First, we document the changes in methodological practices that occurred in macroeconomics, industrial organization, labor and development economics. Second, we argue that a theoretical mainstream covering the whole economics profession arose from the extension of these transformations to other fields and specializations. Finally, we ponder the reasons why no mainstream emerged for PM papers in the period under study.

#### *Four case studies*

##### Macroeconomics

Between the mid-1970s and the mid-1980s, macroeconomics underwent a radical transformation. Different new theoretical streams emerged – non-Walrasian macroeconomics, associated with names of J. Drèze and J-P. Benassy, search-theoretical models *à la* P. Diamond, and new classical macroeconomics, initiated by R. Lucas. While they differed in several respects, they shared the same aim of anchoring macroeconomics onto microeconomics. In terms of our category, they wanted to shift from implicit to explicit microfoundations. Eventually, the line opened by Lucas and stabilized by F. Kydland and E. Prescott eclipsed the others. As argued by Manuelli and Sargent in their review of Lucas’s *Models of Business Cycles*, one of Lucas’s main contributions was methodological. It consisted in establishing “particular sets of rules and techniques to model aggregative economic observations” (Manuelli and Sargent 1988: 523).<sup>11</sup> These rules, Manuelli and Sargent pointed out, acted as standard-setters, discriminating between up-to-the-standard and substandard practices. Prominent among these standards were a general equilibrium perspective, dynamic analysis, the rational expectations assumption, explicit micro-foundations, market clearing, stochastic shocks, and a new equilibrium concept. Two additional traits of the Lucas/ Prescott approach to macroeconomics must be mentioned. The first is the unification of the two components of macroeconomics, business cycles and growth, around a single model, the Ramsey/Solow

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<sup>11</sup> On this, see Hoover (1998) and De Vroey (2016).

model. The second concerns the theory/measurement methodological node. It manifests both continuity and discontinuity: continuity because new classical macroeconomics followed Keynesian macroeconomics, both being emblematic examples of the ‘theory & measurement’ bifurcation; discontinuity because real business cycle (RBC) economists replaced econometric testing with calibration.

### Industrial organization

A transformation like what happened in macroeconomics took place in industrial organization. In the 1950s and 1960s, its core was the structure-behavior-performance framework. That is, the market structure in a given branch (concentration, vertical integration, product differentiation, and number of firms) is supposed to determine the behavior of the firms belonging to the branch, impinging in a second stage on observable market performances. For example, it was argued that the observable differences in profit rates across sectors resulted from the degree of competition. Barriers to entry, increasing returns, capital requirements, and product differentiation were considered central factors of collusive behavior. At the time, industrial organization had Cournot’s theory of oligopoly pricing as its only theoretical underpinning, but it was scarcely referred to. First and foremost, the discipline was descriptive and empirical. Most of the emphasis was on establishing correlations between market structure and performance. The second element of the triptych, behavior, received little attention. Techniques were rudimentary and data scarce. In the beginning, empirical work consisted in elementary statistical analysis of limited cross-industry data. Market structures were evaluated subjectively. Later, large cross-section samples of industry-level data became available. Nonetheless, identification problems abounded due to the simultaneous nature of the models used.

New style industrial organization arose at the turn of the 1980s under the combination of several factors.<sup>12</sup> A first one was the realization that the way in which issues were traditionally posited was wanting because it assumed that the sectoral structure of the market was given rather than determined endogenously. A second was the realization that behavior needed to be placed at the center of the analysis. A third was the adoption of new equilibrium concepts, such as the subgame perfect equilibrium concept proposed by R. Selten and J. Harsanyi, geared towards tackling multi-stage games.

Oligopoly theory took pride of place in the new paradigm. The latter also addressed research and development, the regulation of natural monopolies, contract theory, and banking theory. Moreover, the analysis zeroed in on agents’ interactions within a branch, bringing the topic of asymmetric information to the forefront, as well as the problems of adverse selection, moral hazard, and verifiability that it triggered. J. Tirole’s 1988 *Theory of Industrial Organization* provided a unified framework for the new industrial organization theory. In the

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<sup>12</sup> As noticed by Samuelson (2016: 118), a simple way for realizing the extent of the change that took place is to compare the Econometric Society World Congress lectures on industrial organization given by Weiss in 1969 (Weiss 1971), Schmalensee in 1980 (Schmalensee 1982), and Roberts in 1985 (Roberts 1987).

first years of the new approach, most papers belonged to the PT bifurcation. Later, at the turn of the twenty-first century, a return to the T&M line occurred with the rise of what T. Bresnahan termed the “new empirical industrial organization” (Bresnahan 1989).

### Labor economics

The institutional approach dominated labor economics until World War II (Boyer and Smith 2001). The interwar period was witness to two attempts at bringing labor economics under the neoclassical umbrella by J. Hicks and P. Douglas, who both authored a book with the same title, *Theory of Wages*. Their impact was limited, probably due to the Great Depression, and at the end of the Second World War, the institutionalist approach still prevailed. Yet from then on, its dominance gradually decreased. A first blow was the ‘full cost’ controversy in which neoclassicals took the upper hand over institutionalists.<sup>13</sup> A further one was M. Friedman’s 1953 “The Methodology of Positive Economics” essay. Herein, Friedman argued against the need for realistic assumptions and in favor of simple ‘as if’ models, two standpoints running counter to the premises of the institutional approach.<sup>14</sup>

In the post-World War II period, further attempts at bringing labor economics under the umbrella of neoclassical economics saw the light of day with more success than the earlier ones. Under the lead of H. G. Lewis, the Department of Economics of the University of Chicago played a central role in this endeavor. Unlike his labor economist colleagues from other universities, Lewis was adamant that labor economics needed to be anchored in the neoclassical approach. In a programmatic paper (Lewis 1957), he wrote, “Our approach is orthodox: mainly the theory of the demand for leisure viewed as a consumption good”, which suggests explicit micro-foundations. All in all, however, Lewis was less interested in theory than this quote suggests; his take was rather exploring factual evidence with neoclassical tools. Many of his students followed suit, to the effect that Chicago economics gradually drifted toward PM.

In the 1970s, still in Chicago, a different turn took place due to G. Becker and G. Stigler. Becker’s contributions were manifold. Human capital theory was a first one (J. Mincer was a co-inventor of the notion). Thereby, themes like education and other forms of skill improvement emerged as new important topics in labor economics. For his part, Stigler insisted on the view that information is a valuable, costly-to-acquire resource. Applying it to the labor market resulted in the rise of equilibrium search models of the labor market by J. McCall, D. Mortensen, R. Gronau, and P. Diamond. A few other developments –some of which originated

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<sup>13</sup> In a 1946 paper, R. Lester described the results of a questionnaire on firms’ reactions to relative price shocks. He claimed that firms did not react like marginalist theory predicted, thereby shedding doubt on the foundations of neoclassical economics. Machlup counter-attacked by arguing that the neoclassical model could easily be reconciled with Lester’s data. For an in-depth analysis, see. P. Mongin (1992).

<sup>14</sup> Friedman’s point can be summarized as follows. Economists know that in reality firms do not maximize profits or agents’ utility. However, for the sake of tractability, it is commended to assume that they behave ‘as if’ this was the case. This standpoint is justified on the grounds that the validity of a model depends on whether its ‘predictions’ are empirically verified rather than on the realism of its hypotheses. By contrast, institutionalists hardly bothered with predictions. To them, the lack of realism of assumptions was a sufficient condition for discarding them. For a wider discussion of Friedman’s methodological standpoint, see Forder (2019, Ch. 10).

in the works of economists, like C. Azariadis, C. Shapiro, and J. Stiglitz, who intended to rebuke Lucas's dismissal of the Keynesian concept of involuntary unemployment – must also be mentioned, contract theory and efficiency wage theory.

The general picture emerging from this brief overview is that in labor economics the transformation that occurred resulted in the coexistence of several research lines. The first consisted of works striving to make labor economics part of the neoclassical approach in its PT variant – that is, the human capital, household economics, contract theory, and search specializations. All of them adopted mathematical reasoning, and explicit micro-foundations on top of the equilibrium discipline and were thus part of the theoretical mainstream. A second line was composed of T&M papers which are neoclassical yet adopt the implicit micro-foundations bifurcation. Last but not least, the institutional tradition remained present.

### Development economics

Development theory started in the 1940s in a rather scattered way with pioneering figures, such as R. Nurske, P. Rosenstein-Rodan, R. Prebisch, G. Myrdal, H. Leibenstein, H. Chenery, and A. Hirschman, each pursuing their own way of addressing the underdevelopment issue. All these works were characterized by their discursive non-mathematical style. Likewise, these economists shared the firm conviction that neoclassical economic theory was of little help for the study of development. The grounds for their view were that its central assumptions were on a collision course with the basic characteristics of underdeveloped economies.

Nonetheless, something akin to what happened in labor economics also took place in development economics, namely some economists from other horizons decided to apply their microeconomic toolbox to development issues (moral hazard, asymmetric information, etc.). A landmark example is sharecropping. In his seminal article “Incentives and Risk Sharing in Sharecropping” (1974), Stiglitz proposed to conceptualize it as an equilibrium contract. Stiglitz's approach amounted to making development studies part of a new specialization, information economics and contract theory.

However, here as in labor economics, a full take-over hardly occurred – actually, in development economics, the ascendancy of neoclassical principles was even weaker than in labor economics. Nor were there grounds for the type of peaceful coexistence between distinct sets of standards that existed in labor economics. Instead, in the 1970s, development economics remained a divided community. The first issue of the *Journal of Development Economics*, dated September 1973, is a testimony to this situation. In their foreword, its two editors, A. Manne and T.N. Srinivasan, expressed their aim for the journal to reflect the diverse approaches to development. For its inaugural issue, they initially had the idea of publishing short articles by each member of the editorial board outlining their views of the state of the profession. The lack of consensus they discovered in the drafts they received led them to abandon this project, and to publish only one of the papers, Irma Adelman's. The latter expressed her view that development economics had failed. To remedy this failure, her first recommendation was to



“return to the grand economics of Marx and Schumpeter, but use the empirical, analytical, and mathematical techniques available today” (Adelman 2003: 4).

*An economy-wide theoretical mainstream*

The changes observed in the four fields studied above consisted in adopting the standards that prevailed in microeconomics and game theory since their inception. We further surmise that a similar evolution took place in other fields such as public economics, political economics, international economics, economic geography, environmental economics, etc. Moreover, as noted earlier, scientific communities must be identified not only in terms of fields but also of specializations. To the extent that some specializations abide by the mainstream standards, their presence in fields whose other components are non-mainstream increases the total weight of mainstream economics. Hence our contention that an economy-wide theoretical mainstream has come into existence.

This methodological transformation was bound to percolate into publication. This is where certification enters the picture. Several steps are involved. First, we take it that the definition of standards occurs within a field or specialization. Their transformation requires the adherence of a majority of the economists belonging to this field or specialization. Second, it may be presumed that sooner or later the modifications of the standards become reflected in the composition of the editorial boards of field journals. A similar process must take place on those of general-audience journals since their members are typically selected for their prominence in their field or specialization. Accordingly, at the end of the day, once a theoretical mainstream has emerged, PT and T&M papers that fail to abide by its standards will be excluded from publications in the most renowned journals.

*The lack of a measurement mainstream*

In the 1970-1990 period, econometrics was alive and well. The question coming to mind is whether an exclusively empirical mainstream took off in parallel to the theoretical mainstream. Our answer is ‘No.’ Two reasons explain this. The first is that theoretical econometricians, supposedly the standard-setters, hardly saw eye to eye about the direction econometrics was to take.<sup>15</sup> The second is that they realized that their standards for good applied econometric practice were too demanding to be abided by. In Christ’s words:

The theorems of the empirical realm are imported into the applied realm and used, but they do not carry the same force here, because here we are interested in whether a statement corresponds to observation about the real world, not simply whether it follows logically from a given set of premises. When conclusions of theorems from the rigorous realm are carried into the applied realm, they become like guaranteed products that have been used counter

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<sup>15</sup> D. Hendry and G. Mizon (1978) defended what they called “general to specific” modeling, a kind of disequilibrium approach to econometrics. For his part, Leamer advocated Bayesian econometrics (Leamer 1978). In reference to macroeconomics, C. Sims took the opposite standpoint that econometrics should be atheoretical. He criticized structural econometric models for comprising too much a priori theory and defended instead the vector auto-regression (VAR) line. Slightly later, A. Spanos highlighted the role of the observed data in specifying the statistical model, introducing the notion of “data generating process” (Spanos 1986).

to the manufacturer's instructions so that the guarantee no longer holds. The instructions, of course, in this case says 'not guaranteed unless used in a situation where the premises are known to be correct (Christ 1966: 10).<sup>16</sup>

At some point, however, tolerance gave way to 'enough is enough' outcries. One example is an article by Leamer entitled "Let's Take the Con Out of Econometrics" (Leamer 1983). Taking the heated issue of the deterrence effect of capital punishment on crime as an example, he argued that a large number of applied economics papers were so embroiled in identification problems that they deserved low credibility. To him, they were "based on 'whimsical' assumptions arbitrarily made by the model builders" (Leamer 1983: 37). Leamer may well have been right in this complaint. However, as will be seen in the next section, the turn that took place was hardly the one he called for.

## 6. THE 1990-2010 PERIOD: THE EMPIRICAL TURN

In this section, we describe the transformations that occurred in the sphere of PM papers in the 1990-2010 period, namely the rise of a second mainstream, grounded on different standards, the measurement mainstream associated with the rise of new empirical specializations: laboratory experiments, randomized controlled trials (henceforth RCT), and 'natural experiments'.<sup>17</sup> They often come under the 'experimental economics' label. Guala states that empirical economics consists of partly independent research lines (Guala 2008). For our part, we would go one step further and regard them as autonomous specializations evolving in parallel. All three have a reason for claiming the experimental label, but these reasons are different. Thus, experimental economics is just 'big tent' notion, and we prefer not to use it. Table 4 displays the most significant similarities and differences between the three new specializations.<sup>18</sup>

Table 4. Similarities and differences within the experimental economics specialization.

	<b>Data creation</b>	<b>Controlled environment</b>	<b>Purpose</b>
A. <i>'Laboratory' experiments:</i>	√	full	discriminating theoretical propositions
B. <i>Field experiments:</i>	√	partial	providing causal explanations
C. <i>Natural experiments:</i>	—	proxied	providing causal explanations

The purpose of laboratory experiments in economics is to assess the validity of theoretical propositions made about individual behavior or economic interactions at the market, interactive or individual. As well expressed by Roth, their overarching advantage is that the level of control is almost total, the result of the artificial environment in which they take place.

<sup>16</sup> Leamer had a more humorous way of making the point: "We comfortably divide ourselves into a celibate priesthood of statistical theorists, on the one hand, and a legion of inveterate sinner-data analysts, on the other. The priests are empowered to draw up lists of sins and are revered for the special talents they display. Sinners are not expected to avoid sins; they need only confess their errors" (Leamer 1978: vi).

<sup>17</sup> In what follows, we take for granted that readers are familiar with the three new specializations. In Appendix 1, we provide a short description of them.

<sup>18</sup> For a more detailed study of the differences between the new empirical streams, see Harrison and List (2004), Heukelom (20001) and Svorenčik (2016).

When I speak of 'laboratory' experiments, I am not speaking of the location where experiments are conducted.... Rather I am speaking of experiments in which the economic environment is very fully under the control of the experimenter, who also has relatively unimpeded access to the experimental subjects (Roth 1988: 974).

In this context, the integrity of the data is guaranteed unlike what is the case with collected data. Thereby, to the extent that experiments are well designed, there are no identification problems. The drawback is that laboratory experiments have a limited purview. Most issues economists are interested in cannot be handled by them. Hence the RCT and natural experiments strategies resorting to the treatment/control group device. The range of issues the RCT method can address is wide; its limit is that RCT experiments are expensive. Natural experiments are second-best to RCT.<sup>19</sup> Instead of creating data as RCTs do, natural experiments use existing ones, exploiting “situations where the forces of nature or government policy have conspired to produce an environment somewhat akin to a randomized experiment” (Angrist and Krueger 2001: 73).<sup>20</sup> Taking advantage of the surge in databases on all possible subjects, the control/treatment group framework is thereby proxied. As will be seen in the following section, RCTs and natural experiments form the bulk of the empirical turn. They also have in common to be part of the broader phenomenon spanning several disciplines, the ‘causal revolution.’<sup>21</sup> Henceforth, we will group them in our subsequent discussion under the ‘causal inference’ name.

Before the ‘new’ empirical specializations arose, PM studies were by and large of two types: data-gathering and traditional applied economics. Since we regard natural experiments as ‘new’ applied economics, we call traditional applied economics ‘old-style’ applied economics.<sup>22</sup>

Combining the new and previously existing PM specializations, we get the complete picture of the present-day PM cluster. It is displayed in Figure 4.

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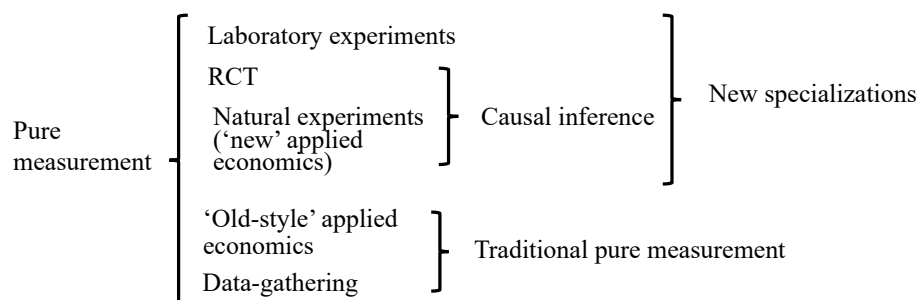
<sup>19</sup> “The gold standard for drawing inferences about the effect of a policy is a randomized controlled experiment. However, in many cases, experiments remain difficult or impossible to implement, for financial, political, or ethical reasons, or because the population of interest is too small” (Athley and Imbens (2017: 3).

<sup>20</sup> One century earlier, in his 1891 book, *The Scope and Method of Political Economy*, J. N. Keynes had it all realized: “It is accordingly to experiment that recourse is usually had for the application of the method of difference, which is the only completely adequate method of reasoning from specific experience. The essence of the method of difference is the comparison of two instances, which resemble one another in all material respects, except that in one a certain cause is present, while in the other is absent. The effects of that cause are thus manifest.” J. N. Keynes (1891: 170).

<sup>21</sup> See J. Pearl (2018) and J. Woodward (2003).

<sup>22</sup> One of the tools employed in causal inferences as understood here (i.e., adopting the control/treatment group device) is instrumental variables. It was already in use earlier on serving the purpose of estimating systems of simultaneous equations and counteracting bias from measurement errors. The novelty is that instrumental variables came to be used for a new purpose, overcoming omitted variables problems in the estimation of narrowly defined causal relations. See Angrist and Krueger (2001).

Figure 4. The subdivisions of the present-day PM cluster



Laboratory experiments started in the 1960s, quasi-experiments and behavioral economics in the 1980s, RCT in the 1990s. However, the blossoming of these new research streams occurred only around the turn of the new century. Several intertwined reasons hover behind it. Part of the explanation lies in the broader context. A first factor is the tremendous progress in numerical methods and simulations on the one hand, and the huge increase in available, fine-grained databases on the other.<sup>23</sup> A second factor of change — and of no minor importance — was the disenchantment of new generations of economists with respect to high-brow theory. They came to hold the view that more attention had to be paid to addressing pressing social and economic issues – like poverty, education, or discrimination – in a way that was transparent and credible for policymakers.<sup>24</sup> Another factor, is that the possibility of overcoming the long-held view that economics is a non-experimental discipline came to be abandoned could not but be music for economists’ ears. They have always wanted their discipline to resemble the hard sciences. Hence, they met this possibility with enthusiasm. Experimentation gave a new breath of life to the idea of falsifiability, which before had been deemed an unattainable ideal.<sup>25</sup>

#### *The rise of an empirical mainstream*

The task ahead is to examine whether the rise of these new specializations can be regarded as having prompted the rise of a second form of mainstream, the measurement mainstream. To begin with, it must be observed that assessing the existence of a measurement mainstream is more complicated than that of a theoretical mainstream. A first reason is that the

<sup>23</sup> See Backhouse and Cherrier (2017b) and Panhans and Singleton (2017).

<sup>24</sup> See Banerjee, A. and E. Duflo (2019). For historians of economics, this move can be interpreted as the end of grand abstract theorizing *à la* Walras and a return to Marshall’s pragmatism. “The need for such guidance [in the practical conduct of social life] was never so urgent as now; a later generation may have more abundant leisure than we for research that throw light on obscure points in abstract speculation, or in the history of past times, but do not afford immediate aid in present difficulties” (Marshall 1920: 42-3).

<sup>25</sup> The emergence of the new empirical streams and their gaining the status of empirical mainstream came at a surprisingly fast speed, as will be documented in Section 7. It took them about a decade to become the ‘new game in town’. The number of awards bestowed to its practitioners is an apt indicator. The award of the Nobel Prize in Economics, to D. Kahneman and V. Smith in 2002 was followed in the next decade by four other ones (Roth in 2012, R. Shiller in 2013, R. Thaler in 2017 and Banerjee, Duflo and M. Kremer in 2021). Another indicator is the numerous B. Clark medals bestowed on new empirical streams researchers. A detailed history of the twists and turns through which this coming to prominence is still to be written. For preliminary accounts, see F. Heukelom (2012, 2014), Thaler (2015) and A. Svorenčik (2020, 2021).

new empirical economics specializations cannot be subsumed under a single set of good practice standards. A second reason is that the meaning of the expression ‘standards for a good practice’ differs according to whether it relates to theoretical papers or to PM ones. In the first case, it designates precise methodological bifurcations, the adoption of which is deemed compulsory. It is a binary choice. When it comes to the new empirical specializations, standards consist of respecting identification procedures. The task of assessing the fulfillment of standards for a measurement mainstream is then less straightforward since it is a matter of degree rather than a binary choice. Despite these differences, we nonetheless argue that the transformations that took place amount to create a standard/substandard methodological cleavage. We start with the causal inference approach as used in natural experiments and RCT papers.

### The causal inference specialization

The formation of the theoretical mainstream started with a dissatisfaction with the existing practice. We contend that the same process happened for the applied economics specialization. Its result was the upcoming of a new way of doing applied economics. In his “Taking the con out of econometrics” article mentioned above, Leamer disparaged applied economics through the lens of theoretical econometrics. Causal inference economists had something quite different in mind. Their grievance was that theoretical econometrics exerted a negative impact on applied economics. The latter had to emancipate itself from theoretical econometricians’ dictates. The following three quotes drawn from Angrist’s contribution to a panel entitled “How to do empirical economics”? (Kramarz 2006) illustrate this. Its gist is that the econometrics of the time was too theory-driven to be of any use for addressing urgent policy-loaded real-world issues.

Q. *Why do you start an empirical project?*

ANGRIST: I usually start a research project because I am interested in a causal relation. I put causal questions at the top of my agenda because the answers to these questions can be used directly for predicting economic outcomes and for policy analysis.

Q. *What is the role of econometrics?*

ANGRIST: I like clever new econometric ideas as much as the next guy, maybe more. But econometrics for its own sake should not be confused with what I call real empirical work, which is question-driven. Most causal questions are better addressed using regression or Two-Stage Least Squares than fancier methods. This is because the case for causality is always so hard to make. Use of simple tools focuses your attention on core identification and measurement problems instead of second-order considerations like how to handle limited dependent variables.

Q. *What do you think of other approaches? For instance, natural experiments versus structural identification is seen as a strong divide by many.*

ANGRIST: Here is the litmus test in my view: applied structural empirical papers — even the most celebrated — rarely seem to be remembered because of their findings. Structural work seems to be mostly about methods. The big structural hits are often said to be making progress or showing how to do something, usually something econometrically difficult like estimation of a dynamic multinomial model of something. ... It is art for art’s sake. ... As far as substance goes, it does not seem to be meant to be taken seriously (Kramarz 2006: 181, 182, 192).

A further step was taken by Angrist and his co-author J-S. Pischke in a 2010 *Journal of Economic Perspectives* article in which they boasted about the occurrence of a revolution in applied economics. Its title – “The Credibility Revolution in Empirical Economics: How Better Research Design is Taking the Con out of Econometrics” – indicated that the causal inference research line had fixed the evils of earlier applied economics denounced by Leamer, though

hardly in the way he advocated.<sup>26</sup> As remarked by Panhans and Singleton, “Angrist and Pischke’s article was less an argument than it was a victory lap” (2017: 28).

Angrist and Pischke’s critique of econometrics as used in traditional applied economics can be found in their “Undergraduate Econometrics Instruction: Through Our Classes, Darkly” (2017) article. Their main aim in writing this article was to make the point that the teaching of econometrics had failed to keep abreast of the developments that had occurred in ‘new’ applied economics. Their paper also comprises a section more directly related to our query of confronting old-style and new applied economics. To this end, Angrist and Pischke compare two papers pursuing a similar object, one published in 1977 by A. Summers and B. Wolfe – “Do Schools Make a Difference?” – the other by C. Dale and A. Krueger and published in 2002 – “Estimating the Payoff to Attending a More Selective College: An Application of Selection on Observables and Unobservables.” Angrist and Pischke depict old-style applied economics as aiming at answering one ‘big question’ – such as Summers and Wolfe’s – by examining the relative impact of a series of explanatory factors. In the Dale-Krueger model, the query takes the form of a narrow question: does having attended a high- rather than a low-tuition university generate more income twenty years after graduation? Both the Summers-Wolfe and the Dale-Krueger models are based on a single linear equation. They differ, however, as regards the right side of the equation. In the Summer-Wolfe model, the right-side variables are all on equal footing, each of them considered a potential causal factor. This makes for vague causality conclusions and loose identification. By contrast, in Dale and Krueger’s article, the analysis bears on whether a single potential *explanans* can explain a single narrow *explanandum*.

Angrist and Pischke present the transformation that took place as an improvement in research design. Whilst this terminology may seem vague, what Angrist and Pischke mean by it is clear: the analysis must be causal and bear on the explanatory strength of a single, well-isolated, causal factor and the debunking of confounding factors. A more robust identification is thereby made possible. It calls for using rules and instruments. A crucial rule is taking stock of the selection bias and omitted variables to guarantee that the regression-estimated effect of the economic variable of interest can indeed receive a causal interpretation. Any absence of counterfactual examination is a sufficient reason to declare papers substandard. As for the techniques implemented for this end, some have long been used, but have been perfected, such as instrumental variables. Others are new like difference in differences or regression discontinuities. Angrist and Pischke, and their ilk, do not use words like ‘standards’ or ‘mainstream.’ Still, this means that old-style applied economics papers are substandard, whilst new ones abide by the standards. More subjectivity is present when it comes to assessing the seriousness of the identification procedure. However, no ambiguity exists about what it consists of.

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<sup>26</sup> It is small wonder that their article triggered strong critical responses from theoretical econometricians, among whom Leamer and Sims, published in the same *Journal of Economic Perspectives* issue.

### The laboratory experiment specialization

Here, the terms of the problem are different, because the existence of a mainstream hardly amounts to replacing an old practice with a new one. Moreover, if the existence of a measurement mainstream hinges on robust identification, then, almost by definition, laboratory experiments meet the standard, a result of the full control made possible by the artificial environment. Only if its protocol was blatantly deficient would an experiment be substandard. At stake is what is called ‘internal validity’ – that is, as nicely put by F. Guala (2005), ‘inferences within the experiment’ as opposed to ‘inferences from the experiment’ or ‘external validity,’ the recurrence of the experiment’s results. Referring for example to N. Jacquemet and O. L’Haridon’s *Experimental Economics. Method and Applications* (2018), the only possible identification flaws they bring out are ‘noises’ and ambiguities about the incentives for the subjects of the experiment and the information provided to them.<sup>27</sup> The main limit of laboratory experiments concerns their external validity. However, this is an issue different from assessing the standard for a good laboratory experiment practice.<sup>28</sup>

### *The emergence of a joint mainstream*

At their rise, the new specializations have evolved exclusively within the PM sphere. This raised no problems for applied economics as it corresponds to the way its practitioners envisaged their work from the start. Not so, however, for the RCT specialization. Its initiators and practitioners are part of the development economics community. Inspired by a perception that the field faced a stalemate, they decided to gear it towards the experimental approach. However, as seen above, a significant fraction of development economists had evolved toward adopting theoretical mainstream standards. It is then small wonder that the spread of RCT research in the field triggered strong critical reactions from these theory-minded development economists. One of its main spokespersons was the highly respected A. Deaton. In a 2010 *Journal of Economic Literature* article, he leveled fierce criticism of RCT development research, arguing that any knowledge that leaves aside the mechanism at work, which it is the task of theory to elicit, is wanting.<sup>29</sup> One canny response was Banerjee and Duflo’s:

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<sup>27</sup> The remedy to the latter problem is to depart from psychological experiments by using performance-based compensation and making it a rule to never use deception.

<sup>28</sup> Additional problems arise with RCT studies. One procedure invented to address one of them – changing the conditions of an ongoing experiment – is the creation of a register of experiments in which project launchers must provide a detailed account of the steps they plan to take in the experiment before its start, and mention every change made after its start. To register a field, the following information must be provided: trial title; country; status; keywords; abstract; trial start date; date; interventions; experimental design; treatment clusters (if yes, number of clusters); and approval by an institutional review board. See the American Economic Association’s RCT Registry (<https://www.socialscienceregistry.org>).

<sup>29</sup> “For an RCT to produce ‘useful knowledge’ beyond its local context, it must illustrate some general tendency, some effect that is the result of mechanism that is likely to apply more broadly” (Deaton 2010: 448). “Learning about theory, or mechanisms, requires that the investigation be targeted toward that theory, toward why something works, not whether it works. Projects can rarely be replicated, though the mechanisms underlying success or failure will often be replicable and transportable. This means that, if the World Bank had indeed randomized all of its past projects, it is unlikely that the cumulated evidence would contain the key to economic development” (Deaton: 2010 442).

Economists are more like plumbers; we solve problems with a combination of intuitions grounded in science, some guesswork aided by experience, and a bunch of pure trial and error” (Banerjee and Duflo, 2019: 7).

More importantly, there is no reason of principle for a permanent confinement of RCT and, more generally, causal inference papers in the PM cluster. As it will be seen in the next section, papers abiding by the standards for a measurement mainstream *and* making a theoretical contribution have made their appearance in top journals.<sup>30</sup>

## 7. EMPIRICAL ASSESSMENT

Our aim in this section is to empirically assess the claims made in the historical and methodological sections. They can be summarized as follows: (a) during the first two decades after WWII, no methodological mainstream was present; (b) a theoretical mainstream, based on the fulfilment of three standards – equilibrium, mathematics, and explicit micro-foundations – emerged during the 1970-1990 period; (c) a measurement mainstream arose in the 1990-2010 period in the wake of the surge in experimental economics broadly understood; and (d) we adumbrated the possible emergence of a joint mainstream composed of papers abiding by the two sets of standards in present-day times.

Since the criteria defining the theoretical and the measurement mainstreams do not overlap, four mainstream configurations may arise: no mainstream, a theoretical mainstream alone, an empirical mainstream alone, and a joint mainstream. Accordingly, papers may fall into one or more of these configurations, depending on their being pure theory (PT), pure measurement (PM), or theory and measurement (T&M). Any PT paper either abides or does not abide by the theoretical mainstream standards. Likewise, any PM paper either abides or does not abide by the measurement mainstream standards. The case of T&M papers is more complicated as four possibilities exist: (a) a T&M paper can have its theoretical component abide by the standards for theoretical mainstream, whilst its empirical component fails to do so for the measurement mainstream standards; (b) conversely, a T&M paper can have its empirical component abide by the standards for measurement mainstream, whilst its theoretical component fails to do so for the theoretical mainstream; (c) a T&M paper can fail to abide by both sets of standards and therefore be non-mainstream on both scores; and (d) finally, a T&M paper can abide by both sets of standard, what we call ‘joint mainstream.’ These distinct configurations, as will be seen, impinge on our empirical analysis.

In view of the subtleties of our taxonomies – the need of separating T&M papers from both PT and PM papers and the need of establishing the presence of explicit microfoundations – we have opted to engage in a paper-by-paper content examination, as in Hammermesh (2013) or Biddle and Hammermesh (2017). This involves working with a reduced sample instead of using algorithms. At the end of the day, we find that the results we obtained are sufficiently interesting for justifying this decision. To minimize the risk of subjectivity, we proceeded as follows. After a first round of individual examination, it turned out that twelve percent of the

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<sup>30</sup> Coupling the empirical mainstream part of a paper with a non-mainstream theoretical part – e.g., institutional economics – is also a possible occurrence.



articles in the sample raised a classificatory problem. A second examination allowed us to limit the residual set of ambiguous papers to sixteen, representing less than 2% of the sample. Instead of eliminating them, we opted for classifying them at the best of our understanding after having checked whether eliminating them from the sample would change our results.

Our sample consists of the four top journals according to the REPEC ranking of journals in economics: the *Quarterly Journal of Economics (QJE)*, the *American Economic Review (AER)*, the *Journal of Political Economy (JPE)*, and *Econometrica (ECONa)*.<sup>31</sup> We exclude presidential addresses, communications, notes, short papers, papers and proceedings, and special issues. The sample is composed of 833 articles whose distribution is displayed in Table 5.

Table 5. The sample (number of articles)

	<b>AER</b>	<b>ECONa</b>	<b>JPE</b>	<b>QJE</b>	<b>Total per year</b>
<i>1970</i>	51	56	61	29	197
<i>1990</i>	54	52	54	38	198
<i>2010</i>	68	42	30	44	184
<i>2018</i>	103	50	61	40	254
<i>Total per journal</i>	276	200	206	151	833

Our main interest lies in papers that can be regarded as candidates for belonging to the mainstream. This implies discarding econometric theory papers and reflexive papers. Their elimination generates a restricted sample of 775 papers. Most of our examination will be concerned with this restricted sample.

Our study bears on the years 1970, 1990, and 2010. Such rather long spans of time are congruent with our purpose of bring out long-run developments. We started with studying two two-decade-long periods. The first is the 1970-1990 period, during which, we contend, a theoretical mainstream arose; the second, the 1990-2010 period which, we have claimed, testified to the rise of a measurement mainstream and possibly the emergence of a joint mainstream. To get a view of the present-day state of affairs, we undertook a further examination bearing on the year 2018.<sup>32</sup> In a first step, we study the evolution over time of the PT/T&M/PM taxonomy. In a second step, we come to grips with our core object of investigation, the mainstream/non-mainstream divide.

#### *The PT/T&M/PM taxonomy*

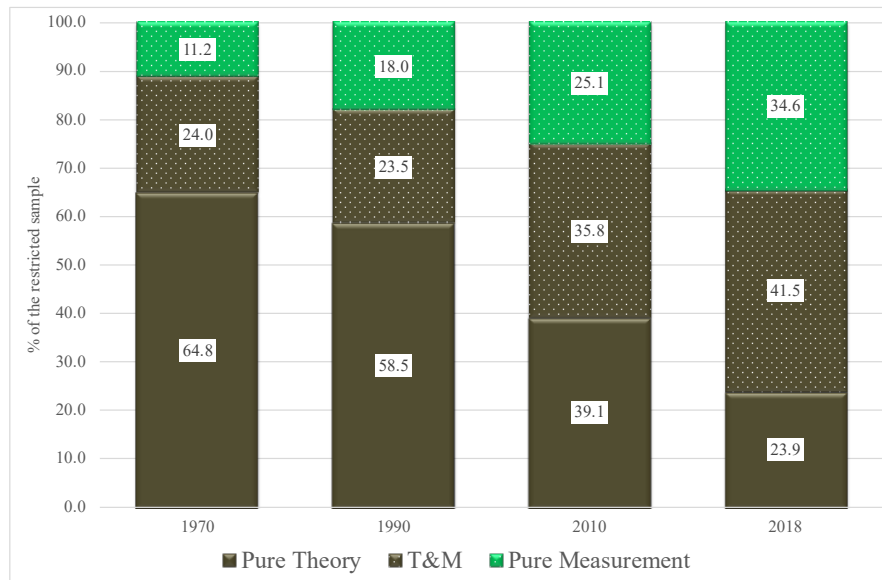
Figure 5 displays the share of PT, T&M, and PM articles in the restricted sample over the time span of our analysis, leaving aside their mainstream/non-mainstream characterization. Two of the patterns it describes are in accordance with Backhouse and Cherrier's claim and the findings of Hamermesh (2013), Angrist *et al.* (2017): the shrinking weight of PT papers (from 65% in 1970 to 39% in 2010) and the increased weight of PM papers (from 11% in 1970 to

<sup>31</sup> Aggregate ranking, *all years* (<https://ideas.repec.org/top/top.journals.all.html>; accessed August 6, 2019). Our analysis is limited to top journals on the grounds that they are trend-defining. Moreover, this choice allows our results to be compared with those of other studies.

<sup>32</sup> This date was the latest available when the bulk of our research was done.

25% in 2010). This pace of change was amplified over the subsequent eight-year period, PT papers falling from 39% to 24% and PM papers jumping from 25% to 35% in 2018.

Figure 5. The share of PT, T&M, and PM papers. Restricted sample



As stated, the originality of our approach is that it uses a three-pronged taxonomy, the additional category being T&M papers. Therefore, the PT/T&M/PM taxonomy can be grouped in two ways. The first consists in forming the PT+T&M cluster by bringing together the two types of papers making a theoretical contribution – the PT papers (the slots coloured in plain brown) and the T&M papers (the dotted-brown slots). The second consists in forming the T&M+PM cluster by bringing together the two types of papers making an empirical contribution: the T&M papers (the dotted-brown slots) and the PM papers (the dotted-green ones). Viewed against this background, the relative decrease in theoretical papers, though sizeable, is less striking than in Hamermesh’s and Angrist Azoulay, Ellison, Hill, and Feng Lu.’s results.

Let us delve deeper in this conclusion by comparing our results with those of Angrist *et al.*. Their paper uses a data set comprising 134,892 articles published in 80 journals between 1980 and 2015. They divide articles into ten fields, studying the evolution of their theoretical or empirical relative components by means of textual algorithms. Angrist *et al.*’s definitions are as follows:

Papers in fields other than econometrics are classified as theoretical or empirical. We aim to label papers as “empirical” if they use data to estimate economically meaningful parameters’ (2017: 293-4).

In Table 6, we compare the results from their analysis with ours.<sup>33</sup> It emerges from this comparison that our three-pronged taxonomy provides a more nuanced view of the present-day configuration of research in economics. According to Angrist *et al.* ‘theoretical papers’ make 36% of total publication. According to our taxonomy, theoretical papers (either PT or T&M papers) make 65%. This difference is hardly trivial – and we doubt that it can be linked to the difference in scope between the two studies.

Table 6. A comparison between Angrist’s and De Vroey and Pensieroso’s results

Angrist <i>et al.</i>			De Vroey and Pensieroso		
	1980	2015		1970	2018
<i>Theoretical</i>	59%	36%	<i>Pure theory</i>	64,8%	23,9%
			<i>Theory and measurement</i>	24%	41,5%
<i>Empirical</i>	32%	56%	<i>Pure measurement</i>	11,2%	34,6%
			<i>Econometrics</i>	7,1%	6,3%

In view of the huge increase in PM papers from 1970 to 2018, it is worth the while to describe its present-day internal composition. This is done Table 7. Its striking feature is the dominance of causal inference papers. By contrast, laboratory experiments papers represent a small part of it. As far as traditional PM (data-gathering and ‘old-style’ applied economics papers) are concerned, they still make up one-fifth of total PM publications.

Table 7. The internal composition of the PM cluster in 2018. Restricted sample

	Laboratory experiments	Causal inference		Traditional PM
		RCT	Natural experiments	
<i>Percentage:</i>	6%	14%	62%	18%

A final interesting result about the PT/T&M/PM allocation concerns the distribution of its categories across the four journals of the sample. In particular, the share of theoretical papers (the PT+T&M cluster) varies across journals with the *Quarterly Journal of Economics* acting as an outlier. In 2008, this share totalizes 87% in *Econometrica*, 76,5%, in the *American Economics Review*, 71% in the *Journal of Political Economy*, and 36% in the *Quarterly Journal of Economics*.<sup>34</sup>

#### *The mainstream/non-mainstream divide*

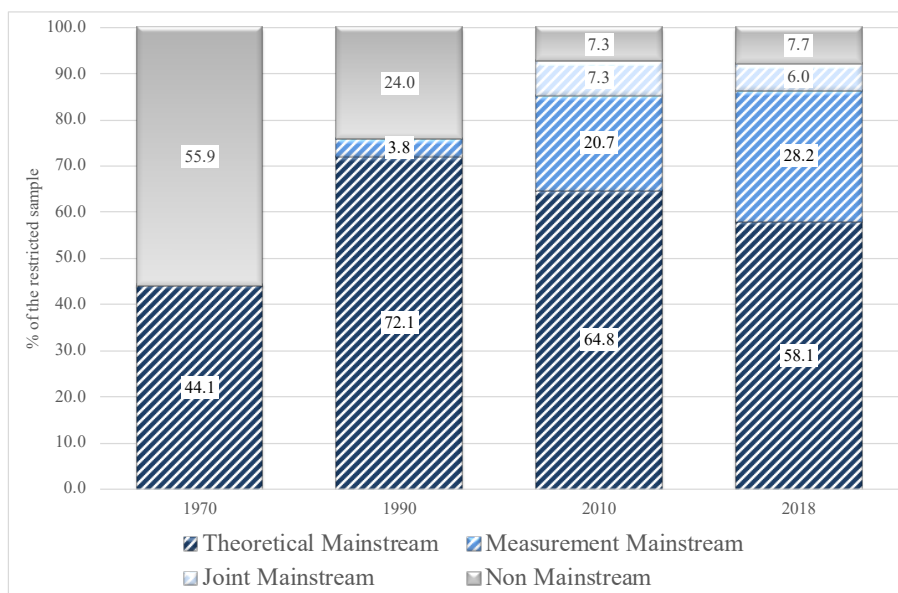
Figure 6 represents the mainstream configuration over the full period of analysis. The striped surfaces designate the share of mainstream papers in the restricted sample. The dark blue ones are the share of theoretical mainstream papers, the lighter blue ones the share of

<sup>33</sup> What they call ‘theoretical style’ (econometrics) must correspond to our PT (econometric theory) category. This makes any paper containing an empirical assessment (probably using econometrics) belong the ‘empirical style’ even if it also makes a theoretical contribution.

<sup>34</sup> For a more detailed description, see Table B-1 in Appendix B.

measurement mainstream papers, and the even lighter ones the share of joint-mainstream papers. The residual grey surface designates the share of non-mainstream papers.<sup>35</sup>

Figure 6. The configuration of mainstream economics, 1970-2018. Restricted sample.



We begin by commenting the 1970, 1990, and 2010 columns. As far as the theoretical mainstream is concerned, in 1970 the share of papers abiding by its standards amounted to 44%. Therefore, we need to qualify our assertion that no (methodological) mainstream existed in 1970. This assertion is true in the sense that 44% can be regarded too low a figure for establishing the existence of a mainstream and its associated exclusion effect. Nonetheless, it suggests that the mainstream formation process was on its way. Be that as it may, our contention about the formation of a theoretical mainstream in the 1980s (that is, within the 1970–1990-time span) is strongly confirmed. Indeed, from 1970 to 1990, the share of papers belonging to the theoretical mainstream jumps to 72%, an annual rate of increase of 2.5%. It shrinks to 65% in 2010, the concomitant result of the surge in the share of a measurement mainstream (from 4% to 21%), and of the emergence of a joint mainstream, the share of which jumped from zero to a 7% in the 1990-2010 period. Two features of this evolution must be underlined. First, in 2010 the share of the theoretical mainstream is still about three times bigger than that of the measurement mainstream. Second, adding up the three types of mainstreams in 2010 makes for a 93% prevalence.

Turning to the 2018 situation, Figure 6 indicates a strengthening of the pattern that starts in the 1990-2010 period. The share of papers belonging to the measurement mainstream now

<sup>35</sup> The papers composing the ‘non-mainstream’ category are: Ricardian economics, institutional economics, peripheral and upcoming disciplines, and broadly neoclassical papers (i.e., neoclassical papers taking the implicit micro-foundations bifurcation).

reaches 28%, whilst that of the theoretical mainstream shrinks from 65% to 58%. Joint-mainstream papers are quasi-stable at 6% of the restricted sample. Again, adding up the three types of mainstreams in 2018 makes for a 92% prevalence. The large bulk of the remaining 7% share is composed of traditional PM papers.

In Table 8, we recast the evolution of the share of mainstream papers according to their being PT, T&M or PM. It shows the rise of a methodologically grounded mainstream since 1970 and its strength by 2010. In 2010, 99% of PT papers in the sample were mainstream, a result that is worth stressing. The figure for PM papers is 82% and that for T&M ones 94% (by adding up (3) and (4)). In 2018, the figures are almost the same. Thus, a juggernaut mainstream is prevalent in present-day economics. Non-mainstream theoretical papers have almost no chance of being published in top journals.

Table 8. The share of mainstream papers per type of paper. Restricted sample

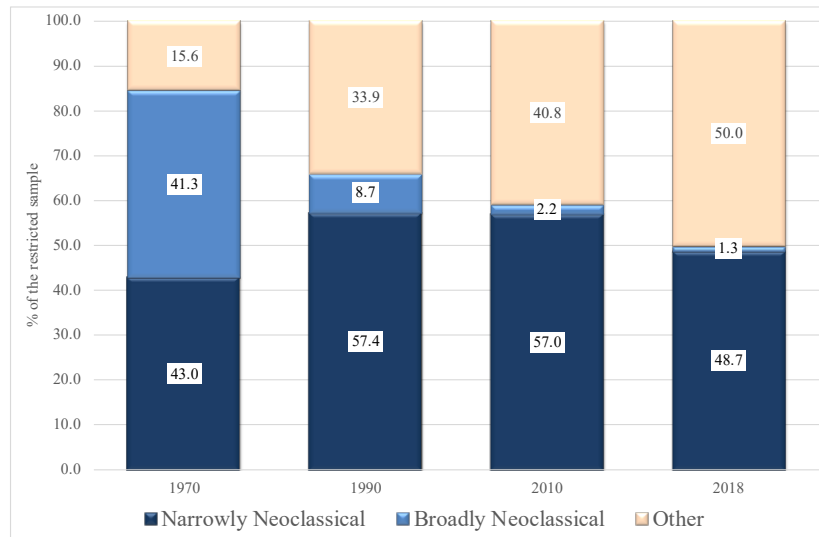
	(1) PT	(2) PM	(3) T&M	(4) T&M
<i>1970</i>	61.2	0.0	18.6	0.0
<i>1990</i>	95.3	21.2	69.8	0.0
<i>2010</i>	98.6	82.2	73.4	20.3
<i>2018</i>	100.0	81.5	82.5	14.4

Column (1) is the percentage of PT papers abiding by the standards for the theoretical mainstream in total PT papers. Column (2) is the percentage of PM papers abiding by the standards for the measurement mainstream in total PM papers. Column (3) is the percentage of T&M papers abiding by the standards for the theoretical mainstream in total T&M papers. Column (4) is the Percentage of T&M papers abiding by the standards for the joint mainstream in total T&M papers.

An additional point worth delving into is the share of neoclassical papers in the restricted sample. Figure 7 below illustrates. Two results come out. The first relates to the internal composition of neoclassical economics. In 1970, the share of broadly delineated neoclassical papers in total neoclassical papers published in the four journals amounts to 42% – an almost equal distribution with the narrowly delineated ones, qualifying for the theoretical mainstream. Adding up the two types of neoclassical papers gives a total of 84,3%. Were the mainstream notion understood as a mere statistical reality, the conclusion could be drawn that a mainstream prevailed in 1970. However, this conclusion does not hold if our definition is adopted, according to which the coming into existence of a mainstream depends on the prevalence of compulsory methodological standards. Be that as it may, broadly neoclassical papers were about to experience an arresting fall: in 2010, their share falls to 2.2% and reaches 1.3% in 2018. As for the share of narrowly neoclassical papers in total publications it rises to approximately 57% in 1990 and 2010 to subsequently fall to 49% in 2018. We interpret the tremendous change in the internal composition of neoclassical papers present in the sample from 1970 to 2010 as meaning that the rise of the theoretical mainstream was to a large extent a transformation taking place within neoclassical economics. It amounted to the exclusion of

the implicit microfoundations bifurcation from the set of admissible methodological choices. The second result again relates to the added share of the two types of neoclassical papers in top journals. From 1970 to 2018, it falls from 84.3% to 50%. In Section 2, we mentioned Colander’s 2000 paper, “The Death of Neoclassical Economics.” The ‘death’ pronouncement may have been an exaggeration, but nonetheless Colander put his finger on a striking decline.

Figure 7. The share of neoclassical papers in total publications. Restricted sample<sup>36</sup>



### *The full sample*

To close our examination, we return to the full sample, which includes econometric theory and reflexive papers. Unsurprisingly, only the former makes a difference. Table 9 displays the complete picture of the different types of papers published in the top journals. Econometric theory papers turn out to fare well in them. Their share in total publications is 7% in 1970, 6% in 1990, 3% in 2010, and 6% in 2018. Because of our taxonomic definitions, the numbers on the share of the mainstream are automatically lower than in the restricted sample.

<sup>36</sup> In this figure, game-theory papers are classified as ‘other’.

Table 9. The share of the different types of papers in the whole sample

Year		1970	1990	2010	2018
<i>Mainstream</i>	<i>Theoretical</i>	39.9	66.7	63.0	53.8
	<i>Measurement</i>	0.0	3.5	20.1	26.1
	<i>Joint</i>	0.0	0.0	7.1	5.5
	<i>SUB-TOTAL</i>	39.9	70.2	90.2	85.4
<i>Non-Mainstream</i>	<i>PM Traditional</i>	10.1	13.1	4.3	5.9
	<i>Broadly Neoclassical</i>	37.4	8.1	2.2	1.2
	<i>Non-Neoclassical</i>	3.0	1.0	0.5	0.0
	<i>Econometric Theory</i>	7.1	6.1	2.7	6.3
	<i>Auxiliary disciplines</i>	2.5	1.5	0.0	1.2
	<i>SUB-TOTAL</i>	60.1	29.8	9.8	14.6

## CONCLUDING REMARKS

The aim of our paper was to attempt to give a methodological foundation to the mainstream notion. We hope to have convinced the reader that such a task is not only feasible, both theoretically and empirically, but it also provides compelling results. Drawing from a historical/methodological analysis, we put forward three contentions: (a) a theoretical mainstream came to prevail in the 1980s; (b) the rise of a measurement mainstream followed at the turn of the century; (c) a joint mainstream saw the day of light around the same time. These conjectures were confirmed by our empirical analysis. Our taxonomic approach also allowed us to shed new light on the empirical turn of the economic literature in the XXI century. In particular, we were able to show that the quantitative decline in pure theory papers was partially compensated by an increase in T&M papers, to the extent that papers making a theoretical contribution still represented the majority of the scientific production in 2018.

Our paper has two main limitations. The first is our exclusive focus on the ‘top-4’ journals in our empirical analysis. The second is that, by focusing on methodology, our study leaves aside the sociological dimension of the working of the economics profession. Hence, our work can only provide a partial picture of what is happening in economics, and therefore one must be careful when drawing general conclusions from it. Take the case of institutional economics or peripheral and reflexive disciplines. Their absence from top journals may prompt the conclusion that they are unimportant. This is far from true. They are thriving specializations, supported by lively professional associations and a host of specialized journals.

Finally, the rise and evolution of a mainstream in economics cries out for a question, Is the existence of mainstream a good or a bad thing? As historians of economics, we want to steer clear of the fray. Hence, we content ourselves with making the following observation.

Heterodox economists take it for granted that the existence of a mainstream is bad because it is tantamount to a lack of pluralism, a deplorable situation in their view. Among the many examples illustrating this standpoint we want to quote just one, firstly because it was written not so long after the emergence of a theoretical mainstream, and secondly, because of its iconic status – it appeared in the May 1992 issue of *The American Economic Review* yet in its paid section. Entitled, “A Plea for a Pluralistic and Rigorous Economics’, written by G. Hodgson, U. Mäki, and D. McCloskey and signed by 44 ‘leading’ economists, its message is as follows:

We the undersigned are concerned with the threat to economic science posed by intellectual monopoly. Economists today enforce a monopoly of method or core assumptions, often defended on no better ground than it constitutes the “mainstream.” Economists will advocate free competition but will not practice it in the marketplace of ideas! Consequently, we call for a new spirit of pluralism in economics, involving critical conversation and tolerant communication between different approaches. Such pluralism should not undermine the standards of rigor; an economics that requires itself to face all the arguments will be a more, not a less, rigorous science. We believe that the new pluralism should be reflected in the character of scientific debate, in the range of contributions in its journals, and in the training and hiring of economists.

Mainstream economists are of the opposite view. They do not see why pluralism should reign in science – and hence in economics, which in their view must be as close as possible to hard sciences – while it must do in the field of politics or human rights. To them, the existence of a mainstream indicates that the members of a scientific community have become unified around a set of common methodological standards. They deem this a sign of progress and hence good news, especially if no intangibility in the content of the mainstream follows on.

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## APPENDIX A: THE NEW EMPIRICAL SPECIALIZATIONS, A BIRD'S EYE VIEW

### Laboratory experiments: market experiments<sup>1</sup>

Market experiments started in the 1950s when a young professor at Purdue University, V. Smith, decided to carry out experiments with his students about the equilibrium price in an exchange economy, in the spirit of Chamberlin's early similar attempts. To his surprise – so he writes in his autobiographical Nobel Prize essay – it turned out that the equilibrium described in theory was reached rather quickly in his experiments. Smith's results were obtained under precise conditions. He assumed the existence of a double auction trade technology – that is, any buyer is free to announce a bid to buy and any seller is free to announce an offer to sell as soon as the market opens. Participants were well informed about the aim of the experiment, which comprised a monetary incentive to trade. Trading periods were short (five minutes). Experiments were repeated over successive time spans, thereby allowing participants to adjust their decisions. Indeed, iteration was key to obtaining an equilibrium outcome. Obviously, real-world markets function differently from this artificial market. It remains that Smith's experiments opened a new frontier in research. Further refinements of the initial model confirmed its results. The general conclusion Smith drew from a long chain of similar experiments was that they broadly supported the predictions of competitive price theory.

Another main leading character in experimental economics is C. Plott, who joined with Smith to boost the new program. Plott's background was general equilibrium, social choice and voting procedures. This led him to use experimentation for addressing public governance issues and solving voting issues within executive committees, such as, for example, the allocation of landing and departures slots in congested airports. At Caltech, Plott founded the Laboratory for Experimental Economics and Political Science (EEPS), a hothouse for young experimental economists. It served as a model for the labs that were created later. In 1986, the Economic Science Association was established regrouping the experimental economics community. In the following years the number of experimental laboratories increased tremendously.<sup>2</sup>

### Laboratory experiments: individual experiments

Behavioral economics arose in the 1990s. Its aim is to replace the traditional neoclassical *homo oeconomicus* assumption – according to which agents are rational optimizing decision-makers acting in a selfish way – by experimentally drawn more realistic assumptions. It all began with the work of two cognitive psychologists, D. Kahneman and the late A. Tversky who authored a landmark paper in *Econometrica* in 1979 in which they criticized the theory of

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<sup>1</sup> For a general introduction to laboratory experiments, see Guala (2005), Heukeom (2014), Svorenčík (2015), Svorenčík and Maas (2016), Svorenčík (2021).

<sup>2</sup> “By the early 1990s the landscape had changed dramatically. In 1992 almost forty groups were in operation, each with a laboratory. Currently [in 2015], there are at least 175 active laboratories worldwide” (Svorenčík 2015: 61). For a detailed history of the creation of this community, see Svorenčík and Maas (2016).

expected utility on the grounds of its multiple violations of real-world decision-making. R. Thaler was one of their first economist followers. He and a few others single-handedly engineered the rise of behavioral economics as a new thriving specialization striving “to highlight behaviors that are in conflict with the standard rational model” (Thaler 2015: 261). A host of violations of optimizing rationality obtained through experiments were brought out, especially in household and financial behavior.<sup>3</sup>

#### Laboratory experiments: strategic interactions

In the 1980s, game theory moved away from its ‘classical state’ by adopting an ‘instrumental’ perspective, to use Samuelson’s terminology.<sup>4</sup> A new challenge ensued: heroic assumptions foregone, how to choose between possible alternative assumptions about, e.g. bargainers’ information, preferences, or strategic options? The idea dawned on H. Saueremann, a Frankfurt University professor, that engaging in controlled experiments might be a good way of guiding these choices. R. Selten happened to participate in Saueremann’s team while working on his Ph.D. in mathematics. This experience nurtured his long-lasting conviction that game theory and experimentation needed to be intertwined in order to be socially useful. As he wrote in an autobiographical essay:

More and more I came to the conclusion that purely speculative approaches like that of our paper of 1962 are of limited value. The structure of boundedly rational economic behavior cannot be invented in the armchair, it must be explored experimentally. (Selten 1994)

Behavioral experiments have acted as a springboard for a transformation of game theory, to the effect that currently behavioral economics spans two sub-types of the PT/T&M/PM taxonomy. They can be either PM (when they deal with experiments, either individual or interactive ones) or PT (when they are game-theoretical). Behavioral game-theory papers depart from classical game theory – sometimes labeled call ‘analytical’ (Camerer 2003) – for the rationale evoked by Selten: the replacement of the traditional selfish assumption – “armchair inventions” – with more realistic ones, drawn from experiments and receiving the generic ‘bounded rationality’ label. This new sub-branch of game theory, which arose under the impulsion of G. Loewenstein and C. Camerer, and later under the stewardship of D. Laibson, M. Rabin, and E. Fehr, to mention just a few names, soon thrived. Two main routes were taken. The first consisted in reconstructing decision theory (e.g., Rabin 1993, and Köszegi and Rabin

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<sup>3</sup> To name but a few: the endowment effect, preference reversal, the law of large numbers, predictable errors, the lack of fungibility of money, the doer-planner contrast, status quo bias, prospective accounting, and limited self-control (see Thaler 2015).

<sup>4</sup> In Samuelson’s words: “In this view, the game is not a literal description of an interaction but is a model that one hopes is useful in studying that interaction. In the words of Aumann, ‘Game-theoretic solution concepts should be understood in terms of their applications and should be judged by the quantity and quality of their applications.’ [2000, p. 38; originally 1985]. The game is thus a deliberate approximation, designed to include important aspects of the interaction and exclude unimportant ones” (Samuelson 2016: 113).

2009), the second in using such results in strategic interaction modeling (e.g., Camerer 2003, and Camerer, Loewenstein, and Rabin 2004).

The two types of game theory, analytical and behavioral, now live more or less uneasily side by side.<sup>5</sup> For their part, as it suits challengers, the proponents of behavioral game theory have the conviction that their line will prevail:

The eventual goal is for game theorists to accept behavioral game theory as useful and necessary. When that time comes, the ideas in this book will be part of every standard game theory book and the term ‘behavioral’ can be shed. (Camerer 2003: 405).

### Natural experiments

The father figure of the natural experiment research line is O. Ashenfelter, whilst D. Card, the late A. Krueger, Angrist, and Pischke stabilized it.<sup>6</sup> In the 1970s, for a couple of years, Ashenfelter was the Director of the Office of Evaluation at the U.S. Department of Labor, whose mission is to evaluate programs financed by the government. The implementation of this mission faced many problems – data collecting, finding data with which to compare the outcome of the ‘treatment group,’ and presenting the outcome of the study as transparently and credibly as possible for policymakers. To solve them, new tools had to be devised and existing ones revised. The natural experiment specialization was the offshoot of this. Over the years, it became a thriving specialization, making its way into several fields, such as labor, health, and education economics.<sup>7</sup>

### Randomized controlled trials

The RCT research line has a long history in medicine and epidemiology (and a more succinct one in economics). It relies on splitting a sample between a treatment and a control group. At the beginning of this century, it started to be extensively used in development economics in association with a new vision of poverty and of the means to overcome it. The premise adopted was regarding the poor as decision-makers instead of viewing them as stuck in poverty and in need of outside help. A seminal RCT paper, authored by E. Miguel and M. Kremer (2004), addressed the issue of school absenteeism in Kenya. More precisely, it attempted to assess the impact of deworming on school attendance. The conclusion of their experiment was that deworming reduced school absenteeism by one quarter and was far cheaper than alternative ways of increasing school attendance; hence, the policy recommendation of fully subsidizing deworming. Soon, several laboratories using randomized trials came into existence, the pioneering one being the Abdul Latif Jameel Poverty Action Lab founded by A. Banerjee, E. Duflo, and S. Mullainathan.

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<sup>5</sup> Aumann (2019) is an attempt at reconciling them.

<sup>6</sup> Angrist, Card and Pischke got their PhD at Princeton under Ashenfelter’s supervision in 1983, 1987 and 1989 respectively. Krueger was a Harvard graduate.

<sup>7</sup> Card and Krueger (1994) is a landmark example.

## APPENDIX B: DECOMPOSITION BY JOURNAL

Table B-1. The share of PT, T&M and PM papers by journal, 1970-2018, restricted sample <sup>8</sup>

Journal	Year	Pure theory	Theory & measurement	Pure measurement
<i>AER</i>	1970	54.9	23.5	21.6
	1990	67.9	17.0	15.1
	2010	29.4	47.1	23.5
	2018	14.1	51.5	34.3
<i>QJE</i>	1970	71.4	17.9	10.7
	1990	44.7	42.1	13.2
	2010	25	34.1	40.9
	2018	17.9	17.9	64.1
<i>JPE</i>	1970	62.1	32.8	5.2
	1990	47.2	22.6	30.2
	2010	36.7	40.0	23.3
	2018	20.7	50.0	29.3
<i>ECONa</i>	1970	76.2	16.7	7.1
	1990	74.4	15.4	10.3
	2010	75.7	13.5	10.8
	2018	60.5	26.3	13.2

Between 1970 and 2018, the share of PT papers published in the *AER*, the *QJE* and the *JPE* plummeted – a 74%, 75% and 67% fall respectively. By contrast, in *ECONa* this share remained stable at a high level, approximatively 75%, until 2010. However, from 2010 to 2018, it lost 19%. Yet at that date its share was still high – 60,5 % compared to the level of 14 % for the *AER*, 18 % for the *QJE* (18 %) and 12 % for the *JPE*. As for the share of PM papers, it steadily increased in all four journals with the *QJE* taking the lead. Finally, as far as T&M papers are concerned, trends are less clear. We observe a strong jump between 1990 and 2010 for the *AER* and the *JPE* followed by a further increase in 2018. The evolution of the *QJE* is more erratic. It witnessed the same jump as the other two between 1990 and 2010, but in 2018 the share of T&M papers returned to its 1970 level. Looking at the respective shares of theoretical papers (the PT+T&M cluster) and PM ones, it turns out that in 2008 this share of theoretical papers totalizes 87% in *ECONa*, 76,5% in the *AER*, 71% in the *JPE*, and 36% in the *QJE*.<sup>9</sup>

<sup>8</sup> The restricted sample excludes econometric theory papers. From 1970 to 2018, they amounted to forty-seven articles. Forty-three of them were published in *ECONa*.

<sup>9</sup> On this, see Card and Card and Della Vigna (2013).



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