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## On the Modelling Method in Adam Smith's Economic Thought

O wykorzystaniu modeli w myśli ekonomicznej  
Adama Smitha

### Keywords:

classical economics, Adam Smith,  
history of models and modelling  
in economics, laws of economics

### JEL classification codes:

B11, B12, B31, B41

### Article history:

submitted: April 20, 2022

revised: July 29, 2022

accepted: January 4, 2023

### Słowa kluczowe:

ekonomia klasyczna, Adam Smith,  
historia modeli i modelowania  
w ekonomii, prawa ekonomiczne

### Kody klasyfikacji JEL:

B11, B12, B31, B41

### Historia artykułu:

nadesłany: 20 kwietnia 2022 r.

poprawiony: 29 lipca 2022 r.

zaakceptowany: 4 stycznia 2023 r.

### Abstract

Much has been written about Adam Smith's economics, but one unanswered question is whether his method of inquiry is a modelling approach. It is therefore interesting to investigate to what extent Smith can be described as an economic modeller. Such an investigation is presented in this paper. By studying elements of modelling methods developed by those who influenced Smith, as well as his own ways of doing economics, together with his general insights on how science, including economics, should be practiced, we show that his method of inquiry is hugely based on modelling empirical phenomena.

### Streszczenie

Wiele napisano o ekonomii Adama Smitha, jednakże to, czy jego metoda badawcza może być określona jako wyjaśnianie poprzez modelowanie, jest w dużej mierze nadal niezbadane. Interesujące jest więc przeanalizowanie tego, w jakim stopniu A. Smith może być uznany za budującego modele. Kwestia ta podejmowana jest w niniejszym tekście. Analizując elementy metody wyjaśniania poprzez modelowanie obecne u tych, którzy wywarli wpływ na Smitha, ale też badając jego własną metodę badawczą, wraz z odniesieniem się do jego generalnej refleksji nad tym, jak nauka, w tym ekonomia, powinna być uprawiana, pokazujemy, że jego metoda badawcza w dużej mierze opiera się na modelowaniu zjawisk empirycznych.

## Introduction

Economics is a modelling science. Therefore, to study its history, it is important to focus on the history of models that have been used in economics. When we search for the first models, we often refer to Quesnay's *Tableau Économique*, Hume's thought experiments, and Ricardian studies on rent and comparative advantage. But since it was Adam Smith who is rightly called a founding father of modern economics, it is definitively worth asking to what extent his way of doing economics can be described as one based on models. It seems that those who reflect on the history of economic thought claim that his method was not founded on clear-cut models but rather on more informal ways of studying markets. And hence, for instance, **Morgan [2012: 378; emphasis added]** states the following: "Adam Smith's *Wealth of Nations* of 1776 covered the whole territory of what then constituted the art and science of political economy in **an expansive verbal treatment** [...], simultaneously illustrating and supporting those [economic] laws by **the evidence of common experience and history**. Modern economics is qualitatively very different. It has become [...] dependent on small mathematical or diagrammatic models [...]".

On the other hand, we can find arguments in the literature claiming that although in Smith's writings we do not have models as such, we do have a lot of insights that can be viewed as precursors of models developed later by Smith's followers. For instance, **Samuelson [1977: 42]** states that Smith's economic analysis is "a valid and valuable anticipation of general equilibrium modelling", and that his "vision of transient growth from invention and capital accumulation [...] is *isomorphic* with the model of Ricardo, Malthus, and Marx" [**Samuelson, 1977: 42**, italics in original]<sup>1</sup>. In that sense, Samuelson argues that one can find a rich reservoir of interesting theoretical claims in Smith's works: "Too often theorists contrast Adam Smith to his disfavour with his brilliant predecessor, David Hume, and brilliant successor, David Ricardo" [**Samuelson, 1977: 42**]. But Samuelson did not claim that Smith was a model builder. In his 1977 paper, Samuelson developed his own formal model of economic exchange which translates Smith's claims into mathematical jargon. Therefore, Samuelson claims only that "with careful reading, we do infer in the *Wealth of Nations* a complete and valuable theoretical model" [**Samuelson, 1977: 42**] rather than just proclaim that we do have a particular theoretical model in Smith's 1776 opus magnum.

A different reading of Smith is offered by **Elmslie [2018: 210]**, who puts an emphasis on Smith's trade theory and asserts that, although the *Wealth of Nations* does not contain a formal analysis of relationships between the volume of trade and geographical distance, one can find, on careful reading, a pure model of international trade based on the idea of trade gravity, so "Smith laid out the basic model". Similarly, **Schliesser [2017]**, when analysing Smith's distinction between natural and market prices, claims that Smith uses theoretical idealisations (or, in other words, theoretical models) to generate predictions. In the same vein, **Weingast [2018: 4]** states that philosophy "in Smith's terms, involved building models that help us make sense of the world around us", but since Smith uses the terms "philosophy" and "science" almost synonymously then we have solid grounds, at least according to Weingast, to claim that Smith's economics involves model building<sup>2</sup>. Nevertheless, what is lacking in the literature on (possible) models in Smith's works, is a clear methodological approach. In other words, it would be interesting to look at Smith's arguments, presented especially in his *Wealth of Nations* through the lens of contemporary philosophy of economics, and especially philosophical studies on economic models. We do this here, and therefore the goal of this paper is to show that Smith can be legitimately treated as someone who explains the economic realm by constructing its models. Our argument is therefore predominately methodological. Also, taking into account the enormous scope of Smith's writings, we do not focus here on *all* of Smith's *possible* models, but rather we offer a general look into his method and ask whether it is a modelling approach. Only later we expand our analysis to include two case studies, namely

<sup>1</sup> Samuelson clearly subscribes himself to a long tradition of viewing Smith's writings as a Walrasian general equilibrium theory *in nucleo* [**Peil, 1999**].

<sup>2</sup> On the interchangeable use of these terms in Smith's writings, see, e.g., **Herzog [2013]**.

Smith's well-known pin factory example and his investigation of absolute advantage in international trade. Finally, we show that Smith's arguments taken from his general reflections on science, including economics, support our understanding of his method (cf. [Paganelli \[2017\]](#)).

Although we declared above that our method is a methodological one, this paper should be also treated as a study in the history of economic thought. So, to fully appreciate Smith's methods, we need to put them into historical context, and in particular take into account the relevant models, explanations, and general views on the character of human knowledge that defined his era (cf. [Aydinonat and Käksal \[2019\]](#))<sup>3</sup>. So, for instance, we need to refer to physiocratism, which "with all its imperfections, is perhaps the nearest approximation to the truth that has yet been published upon the subject of political economy" [[Smith, 1993/1776: 157](#)]. We must also mention Hume's science of man, and in particular his analysis of the economic realm. Smith wrote of Hume: "I have always considered [...], both in his lifetime and since his death, as approaching as nearly to the idea of a perfectly wise and virtuous man, as perhaps the nature of human frailty will permit" (Smith's letter from Nov. 9, 1776, to W. Strahan; in: [Rasmussen \[2017: 222\]](#)).

Having said the above, we start by referring to physiocratic models and Hume's thought experiments. Next, we focus on Smith in general and try to disentangle his economic method, and later we put an emphasis on his pin-factory analysis as well as his investigation of absolute advantage in international trade. Then we offer remarks on Smith's metaanalysis of his method, with a particular emphasis on his "imaginary machines" and systematic analysis. Finally, we present our concluding remarks.

### Economic models and thought experiments before Smith

In general, one can understand a given model as a representation of something that is beyond this very model. So, in the case of economics, its models are representations of the economic realm and its parts. We understand representations here in the most general sense, namely as both actual and possible pictures of their targets. Therefore, such representations can be diagrams, numerical objects, material analogues, networks of metaphors, and so on. In the case of contemporary economics, however, the history of models starts with the *Tableau Économique*, which "can be regarded as the great-grandfather of models in many different economic traditions even while its own content and meaning remain somewhat mysterious" [[Morgan, 2012: 3](#)]<sup>4</sup>. As is well known, it can be treated as a numerical object, and precisely a kind of cross between a table and a matrix. It refers to three "parts" of the economy, namely farmers, manufacturers, and landowners. In particular, *Tableau* offers a kind of a picture of various relations between the three "parts". However, in order to construct such a model, its author should first have a very basic understanding of the ways the world works, and only later can the construction of a given model start. As Schumpeter argues:

"Our **individual must first recognize the phenomena on which he is going to work** and he must recognize them as being somehow connected with one another and distinct from others. This recognition is a cognitive act" [[Schumpeter, 2006/1954: 42–43](#); emphasis added].

It was also the case with Quesnay, who was inspired by a hydraulic machine developed by de Servière (according to [Charles \[2003\]](#)) and by an in-depth description of the economy by [Cantillon](#) in his *Essay [1755]*, not to mention Cantillon's own in-depth observations of the late 1750s economy<sup>5</sup>. Since, in fact, it was Cantillon

<sup>3</sup> Although one can interpret the proposed paper's goal as being within the framework of the so-called hermeneutical model of interpreting Smith, it is not entirely so since what we aim for here is to recontextualise Smith's works by treating his texts as being not autonomous to how Smith understands the fit between his theoretical claims and the way the world works.

<sup>4</sup> As is always the case in the history of ideas, tracking the origin of a given idea or method is not an easy undertaking. However, in the case of economics, there is a general consensus that Quesnay's *Tableau* was indeed the very first *modern* economic model (cf., e.g., [Blaug \[1990: 25\]](#); and [Schumpeter \[2006/1954: 229–233\]](#)).

<sup>5</sup> Quesnay definitely went beyond the mere "first recognition of the phenomena" and his *Tableau* proves that he also took a second step in model building, which, according to Schumpeter, is an analytical approach, namely researchers' first impressions of phenomena must be put into precise terms and labels (Quesnay's *Tableau*).

who “was the first to draw a *tableau économique*” [Schumpeter, 2006/1954: 214], let us look at the crucial elements of his *Essay*, and hence we can cite the following phrases (numbers added):

- (1) “There are none but the prince and the property owners who live independent; all other classes and inhabitants are hired or are entrepreneurs”.
- (2) “The farmers generally have two-thirds of the product of the land: one for their expenses and the maintenance of their assistants, the other for the profit of their enterprise. On these two-thirds, the farmer generally provides, directly or indirectly, subsistence for all those who live in the country [...]”.
- (3) “The owner usually has one-third of the product of his land, and on this third, he maintains all the artisans and others, whom he employs in the city as well [...]. It is generally assumed that one-half of the inhabitants of a state subsist and reside in cities, and the other half live in the country”.
- (4) “But at present, I am only considering a state with regard to its production and with its own industry, in order to avoid cluttering my argument with accidental circumstances”.
- (5) “The land belongs to the owners but would be useless to them if it were not cultivated. The more labor is expended on it, other things being equal [*toutes autres choses étant égales* in the French original], the more it produces; and the more its products are refined, other things being equal, the more value they have as goods” [Cantillon, 2010/1755: 70–71].

In these passages from Cantillon's *Essay*, we find many phrases and “analytical tools” that are somehow similar to a modelling approach. So, in (1) we have a typical idealisation since Cantillon does not take into account all the specific aspects of how property owners make their living. Next, in (2) and (3) we have a similar method used but one supplemented with a kind of distortion since it is claimed that farmers receive two-thirds of the product. Therefore, the idealisation is enriched with a tractability assumption (cf. Alexandrova [2006]). Now, in (4) a reference to “accidental circumstances” is used in order to name all the factors that are claimed to be unimportant for explaining a given fact. Thus, Cantillon, at least to some extent, anticipates Mill's distinction between minor and greater causes of phenomena [Mill, 1843]. And finally, in (5), we have a *ceteris paribus* clause in place to emphasise the fact that Cantillon's analysis can be claimed to be true only in very specific circumstances.

As we have already mentioned, Cantillon's ideas, including his “modelling method”, hugely impacted Quesnay's way of analysing economic processes and thus influenced Smith. And, more generally, since Cantillon's *Essay* can be seen as “the cradle of political economy” (as claimed by Jevons [1881: 342]), his way of doing economics formed, at least to an extent, a general intellectual landscape in which Smith developed his ideas. This view is supported by Smith himself, who directly refers to Cantillon in chapter eight of his 1776 opus magnum. There he writes the following: “Mr Cantillon seems, upon this account, to suppose that the lowest species of common labourers must everywhere earn at least double their own maintenance [...]. There are certain circumstances, however, which sometimes give the labourers an advantage, and enable them to raise their wages considerably above this rate” [Smith, 1993/1776: 33]. So Smith is conscious that Cantillon's ideas are only perfectly valid “upon this account”, or, in other words, in very specific conditions and beyond them Cantillon's observations do not fully hold. What should be noted here is that in Smith's 1776 book direct references to his predecessors are rare, so his mention of Cantillon is significant. Later we will show that Smith himself restricts his claims as being perfectly valid only in what he calls “imaginary machines”, a clear sign of being influenced by Cantillon's approach.

Our take on the intellectual roots of Smith's method would be incomplete without referring to Hume, “who was one of the first to conduct thought experiments in political economy” [Schabas, 2008: 161], and who directly influenced Smith. In order to fully understand Smith's method, we should distinguish between thought experiments and models. So, and foremost, models are more decoupled from the empirical realm than thought experiments (cf. Hausman [1992: 79]). Therefore, and secondly, in models we have Aristotelian and Galilean idealisations, while in thought experiments only those of the former kind are present<sup>6</sup>. Thirdly,

<sup>6</sup> As Frigg [2006] explains, “Aristotelian idealization amounts to ‘stripping away’, in our imagination, all properties from a concrete object that we believe are not relevant to the problem at hand”, while “Galilean idealizations are ones that involve deliberate distortions:

thought experiments should refer to a reality that is at least potentially possible, while models can describe purely fictional targets. Therefore, thought experiments are often accompanied by some references to empirical facts, while models do not necessarily offer such insights. Hence Schabas [2008: 165] writes: “I do not find any instances of models in Hume [but we have thought experiments in his writings], nor thought experiments in Melon or Quesnay [but we have models in Quesnay] ... There is also in Hume a much greater propensity to provide empirical support for his theoretical claims than in Melon or Quesnay”.

Although Schabas is definitely right in highlighting the differences between models and thought experiments, there are many overlaps between them, in particular the fact that claims of both models and thought experiments are only partially valid in reference to various empirical domains. So, let us illustrate this point by citing Hume’s well-known thought experiment on money: “For suppose, that, by miracle, every man in Great Britain should have five pounds slipped into his pocket in one night; this would much more than double the whole money that is at present in the kingdom; yet there would not next day, nor for some time, be any more lenders, nor any variation in the interest [rate]” [Hume, 1882/1758: 323–324].

Referring to a “miracle” means that we deal here with an imaginary situation, a kind of thought experiment, and not with a precisely defined model. Moreover, focusing on Britons’ pockets only points to the fact that Hume isolates his analysis from other potential determinants of the money supply, or, in other words, reasoning with a *ceteris paribus* clause is present here. Hence his claim: “Whenever I speak of the level of money, I mean always its proportional level to the commodities, labour, industry, and skill” [Hume, 1882/1758: 335]. One can therefore paraphrase this sentence and instead of “always” use just the *ceteris paribus* clause. Thought experiments are relatively close to the empirical realm, as is the case with the Hume study presented here, and hence he observes the following: “[...] the same causes, which would correct these exorbitant inequalities, were they to happen miraculously, must prevent their happening in the common course of nature, and must forever, in all neighbouring nations, preserve money nearly proportionally to the art and industry of each nation” [Hume, 1882/1758: 333]. At the very beginning of this passage we have a clear reference to Hume’s thought experiment (something happening miraculously), but later he changes his perspective and points to an empirical situation where money fluctuates “nearly proportionally”. And when he goes further in referring to particular economic situations, Hume is even more humble in suggesting perfect proportionality between economic facts. For instance, in his text *On Public Credit*, he writes about “this situation, to which Great Britain is visibly tending” [Hume, 1882/1758: 369]. What in a given thought experiment always holds, since it happens miraculously, in a given empirical context can only support claims about some imperfect regularities, and hence references to tendencies. Therefore, Hume can be treated as someone who anticipates Mill’s definition of laws of economics to be “statements of tendencies only.”

Many historians of economic thought criticise Hume for inconsistencies in his views on money. They claim that sometimes Hume states that money is neutral (see, e.g., the above-cited passage on the miraculous rise in the money supply), while in other places he seems to allow money to impact production (cf. Duke [1979]; Samuelson [1980]). However, one should not so easily claim that there are so many inconsistencies in Hume’s views because a sentence that is perfectly true in a given thought experiment *degenerates* into a tendency claim while being referred to a particular empirical context. Schabas [2008: 161–162] comments on this issue in the following way: “My resolution of this problem [of alleged inconsistencies] consists in showing that the specie-flow mechanism was couched as a thought experiment and was thus meant to isolate certain hypothetical tendencies in contrast to the description of the growth account that was intended to be entirely realistic”. Therefore, expecting thought experiments’ claims to preserve the same degree of lawhood once they are used to describe empirical phenomena is just methodologically wrong.

While assessing the character of conclusions produced by thought experiments, we should refer to Hume’s reflections on the nature of knowledge. In *Treatise*, he writes: “Knowledge and probability are of such con-

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physicists build models consisting of point masses moving on frictionless planes; economists assume that agents are omniscient; biologists study isolated populations; and so on” (cf. Weisberg [2007]).

trary and disagreeing natures, that they cannot well run insensibly into each other, and that because they will not divide, but must be either entirely present, or entirely absent” [Hume, 2000/1740: 119]. So, we have “knowledge” in thought experiments and only “probabilities” while describing the empirical targets of thought experiments. Moreover, Hume’s “probabilities” should be understood in terms of beliefs only since the objective of scientific inquiry is to achieve “a degree of belief, which is sufficient for our purpose” [Hume, 2000/1740: 122], and “[...] belief [...] can never be entire” [Hume, 2000/1740: 122]<sup>7</sup>. So, taking the example of Hume’s quantity view on money, one can claim that in a particular thought experiment a rise in the money supply should always produce a proportional rise in prices, while the very same rise in money in a given empirical context can only be believed to impact prices, or, in other words, there is an imperfect proportionality between money and prices<sup>8</sup>.

Since the goal of this paper is to check to what extent Smith’s method can be understood as a modelling one, in what follows we first present some general remarks on Smith’s ways of investigating markets, and later we focus on his analysis of the pin factory and absolute advantage in trade. Smith’s own ideas concerning his methods, and in particular “imaginary machines” together with systematic analysis, will be presented later. We aim to check to what extent Hume’s way of distinguishing between miraculous situations and their empirical counterparts can be found in both Smith’s writings and his reflections on proper methods of economics.

### Some general remarks on Smith’s ways of studying markets

There is an enormous literature on Smith’s economics, including his methods of explaining markets<sup>9</sup>. Our goal is not to present it in its entirety here, but rather focus on those contributions that can shed some light on our question about the extent to which Smith’s method can be described as a modelling one. There are many scholars studying methodological issues in Smith’s writings, but few of them focus exclusively on applying rigorous philosophical apparatus to examine to what extent Smith can be called a model builder. For instance, let us take Tobin’s reflection on *Theoretical Issues in Macroeconomics*, where he claims that Smith’s invisible hand does not hold in real-world markets: “[Smith’s] theorems depend on a host of conditions, many of dubious realism. The modern version [of invisible hand] might be taken to refute, not to support, the applicability of invisible hand propositions to real-world economies” [Tobin, 1985: 104].

Tobin’s opinion presented here rests on the assumption that Smith’s insights played an important role in leading economists to the general equilibrium framework, most notably Walrasian neoclassical theory. Such a stance is taken by Schumpeter, who claims the following: “[...] A. Smith, in fact points toward Say and, through the latter’s work, to Walras” [Schumpeter, 2006/1954: 183]. Moreover, Schumpeter himself seems to somehow subscribe to the tradition of claiming that truths valid in non-empirical conditions (in some theoretical worlds) do not hold while applied in the empirical realm. So we should not be surprised while reading him claiming: “I have spoken of prices ‘that would be paid in perfect equilibrium and pure competition’. This manner of speaking is not Walrasian: Walras, much like J. B. Clark, conceived these equilibrium prices to be, normally, the actual level around which prices oscillate in real life, which involves a claim which I do not wish to make” [Schumpeter, 2006/1954: 965].

So, without a doubt, we have a well-established tradition of seeing Smith as someone who constructed purely theoretical ideas with little resemblance to real markets. But representatives of this tradition do not claim Smith to be a model builder. Nevertheless, seeing Smith as someone who draws the distinction between *artificial* and *real* gives us a strong premise that he can be possibly treated as a model builder since one of the defining features of models is that they are pictures of something and that they are distinct from their tar-

<sup>7</sup> Most 18<sup>th</sup>-century philosophers, including Hume, can be called “belief philosophers” [Kuehn, 2006: 390]. They focused on subjective beliefs and their origins. This very term “belief philosophers” was originally coined and later popularised by K. Popper (e.g., [1972: 25]).

<sup>8</sup> Hume’s “degree of belief” shares many similarities with Lange’s ideas of “degrees of lawhood” and “grades of necessity” since “laws are set apart from accidents by the necessity they possess and from broadly logical truths by the necessity they lack” [Hume, 2009: 8].

<sup>9</sup> See, for instance, Peil [1999], Schliesser [2017], Paganelli [2020], and Berry et al. [2013].

gets. The same holds, but to a lesser extent, for thought experiments, so we see a clear parallel between Smith and Hume in this respect.

Another clue supporting our view that Smith can be described as a modeller can be found in his general view on the human realm, which he claims to be mechanistic [Aspromourgos, 2012; Skinner 1996]. For instance, in *Theory of Moral Sentiments*, he writes the following: “Human society, when we contemplate it in a certain abstract and philosophical light, appears like a great, an immense machine, whose regular and harmonious movements produce a thousand agreeable effects” [Smith, 2004/1759: 430]. First, in this sentence, we have a clear sign of his mechanistic worldview. Second, and even more importantly, we have the distinction between *artificial* and *real*. This is so because human society is like a machine only in abstract terms, and only movements of such highly idealised machines produce “agreeable effects”. In other words, as in Hume’s thought experiments, perfect proportionality or universal regularities take place in Smith’s imagined worlds and not in their real counterparts, but by studying these imagined worlds one can understand (at least partially) the ways the world works. Smith’s ideas of the natural and market prices are another aspect of his economic analysis that can be interpreted in terms of the distinction between “facts” that occur only in imaginary worlds (or, in other words, in idealised conditions) and facts of everyday life (cf. Peil [1999: 137])<sup>10</sup>. First, the market price can be interpreted as being influenced by short-run supply and demand, while the natural price is the price which is sufficient to pay for all factors of production [Schumpeter, 2006/1954: 183]. This is so because the natural price depends on the “particular nature of each employment [i.e., factor of production]” [Smith, 1993/1776: 21]. But since in real markets we have disturbing factors, it is hardly possible to fully notice the effects of the “particular nature of each employment”. So, in other words, nature manifests itself fully only in artificially created environments, such as thought experiments and models. Therefore, the natural and market prices diverge.

But let us dig more deeply into Smith’s references to the nature of various entities and prices<sup>11</sup>. The question to be asked is: Is it legitimate to claim that Smith’s references to the nature of entities can, at least to some extent, be understood in terms of Aristotelian powers and capacities? If this is so, then we can employ Cartwright’s view on models, and hence treat models as blueprints of nomological machines that offer us *artificial* environments where laws, including those of economics, can work uninterrupted<sup>12</sup>. Here it is crucial to properly understand one of Smith’s key concepts, namely that of the invisible hand, since the invisible hand can be treated as “the preserver and enforcer of that [natural] order” [Ahmad, 1990: 142]<sup>13</sup>. Also, as Khalil [2000: 49] claims, “the invisible hand is rather identical to what he calls on many occasions the ‘wisdom of nature’”<sup>14</sup>. Therefore, the *invisibility* of the invisible hand rests in Smith’s claim that “every object of nature [...] is supposed to act by the direction of some invisible and designing power” [Smith, 1980/1795: 49]. Also,

<sup>10</sup> As Schliesser [2017] claims, the distinction between these two kinds of prices is crucial to understand Smith’s methods.

<sup>11</sup> Smith’s references to nature in explaining the ways the world works definitely include his fundamental study on human nature, his “science of man” in which he follows and develops Hume’s insights (cf. Berry [2012]).

<sup>12</sup> Cartwright defines nomological machines in the following way: “It is a fixed (enough) arrangement of components, or factors, with stable (enough) capacities that in the right sort of stable (enough) environment will, with repeated operation, give rise to the kind of regular behaviour that we represent in our scientific laws” [50], and thus “laws of nature (in this necessary regular association sense of ‘law’) hold only *ceteris paribus* – they hold only relative to the successful repeated operation of a nomological machine” [Cartwright, 1999: 50].

<sup>13</sup> Giving the invisible hand such a role stems from the fact that Smith’s goal was to explain the ways the world works without a strict assumption that the world was constructed from the above by God. Rothschild [2001] even describes his universe as a fatherless world. Such a worldview was typical of the Scottish Enlightenment and in particular its project of building a science of man, namely “[a project] whose purpose [...] had been to release human beings from Christian bondage and to provide them with a model of themselves as historical agents whose understanding of themselves, their interests, and their happiness was shaped in the time-bound, historical world of common life” [Phillipson, 1989: 148].

<sup>14</sup> We are conscious that there is no consensus among historians of economic thought about the interpretation of Smith’s invisible hand (s). Nevertheless, most researchers agree that there are considerable differences between the meanings of Smith’s references to the invisible hand in *The Theory of Moral Sentiments*, *The Wealth of Nations*, and *History of Astronomy*. Since we are interested in Smith’s general reflections on how science, including economics, should be done, we are naturally inclined to focus on the invisible hand as it is understood in Smith’s *History of Astronomy*. Thus we refer to Khalil’s [2000] paper interpreting Smith’s invisible hand beyond the traditional references to the ideas of the unintended consequences of human action and Pareto optimality. However, we agree with Aydinonat [2008:

in other places in his *History of Astronomy*, he writes about “invisible power”, “invisible causes”, and “invisible chains”. And, therefore, nature makes things happen: “Fire burns, and water refreshes; heavy bodies descend, and lighter substances fly upwards, by the necessity of their own nature” [Smith, 1980/1795: 32]. However, just after saying the above, Smith writes about “the invisible hand of Jupiter” which is responsible for “more irregular events”. Therefore, we do not have here the invisible hand in general but rather the hand of Jupiter, and thus, as Macfie [1971] claims, the invisible hand is behind irregular events and not regular ones. In other words, “the invisible hand [of Jupiter] in this text is a capricious hand that stops and disturbs the regular order of cause and effect” [Khalil, 2000: 54]. “The invisible hand of Jupiter” was therefore used by Smith to show that things’ working according to “the necessity of their own nature” does not mean that, for instance, heavy bodies will always descend, since, for example, someone can stop them from descending. However, their nature is to always descend.

The above gives us strong arguments to treat Smith as someone who uses the distinction between the regular ways the world works, namely the ways that are in accordance with the nature of entities, and irregular movements of the world that are due to some disturbing factors. However, we do not claim that we can apply, for instance, Cartwright’s framework to fully account for Smith’s method without any reservations. Still, we can argue that Smith’s regular events are due to the nature of things (capacities in Cartwright’s vocabulary), and that these capacities always manifest themselves only when there are no disturbing factors (i.e., in model conditions)<sup>15</sup>. Moreover, Smith even claims that in analysing markets one can construct some imaginary worlds that represent these model conditions. Such worlds would be named by Cartwright as blueprints of nomological machines. Since Smith’s insights into the role of “imaginary machines” in studying markets are central to our arguments for treating him as someone who uses the modelling method, we will put a special emphasis on it later (Section 5).

But two points need to be made now. First, according to Smith, our ability to see ourselves at a distance from what we want to explore is central to our capability to understand the world around us, and this ability, as Paganelli [2017] claims, is synonymous with building a model of a given target we are interested in<sup>16</sup>. Second, Schliesser, while studying Smith’s methods, in particular his distinction between the natural and market prices, observes the following: “Smith offers (a) a “natural” model (based on certain assumptions of human nature, historical change, etc.) of what would be the case under ideal circumstances with (b) a list of factors (stipulated in advance) that will cause deviations from the idealization in order to (c) stimulate research on a part of his readers, both to (d) investigate the nature and extent of these causes, and if they do not turn out to be exhaustive, to what degree there are (e) new causes that need to be incorporated in the model, which, in extremis (f) may be revised. Smith is starting, then, an open-ended process in which one moves from theory to facts and back” [Schliesser, 2017: 309].

It would be better to say that in this open-ended process one moves from models, *producing* given theories, to facts modifying models (and hence theories) and back. We will come back to this issue later in Section 5. Now we would like to look at his well-known descriptions of the pin factory and absolute advantage in international trade. Can they be legitimately treated as models? We try to answer this question below.

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74] that “[...] the discussion of the invisible hand of Jupiter in *History of Astronomy* provides the methodological background for the use of the invisible hand in *The Wealth of Nations* and *The Theory of Moral Sentiments*”; we therefore reflect on Smith’s ways of doing science.

<sup>15</sup> Such a reading of Smith’s methodology, and especially the interplay between the propensities of things and the ways the world works, is also offered by Schliesser [2017: 308], who claims the following: “While propensities are themselves nomological, their persistent triggering and expression can lead to nomological outcomes such that the outcome could not be otherwise – presumably as necessary that all humans are mortal” (cf. idea of economic laws as normic laws presented in: Hardt [2017]).

<sup>16</sup> In Smith’s words: “I can form a just comparison between those great objects and the little objects around me, in no other way, than by transporting myself, at least in fancy, to a different station, from whence I can survey both at nearly equal distances, and thereby form some judgment of their real proportions” [Smith, 2004/1759: 176; emphasis added].

## Smith's studies on specialisation in production and absolute advantage in international trade as cases of modelling method?

Volumes have been written about the first few chapters of Smith's *Wealth of Nations* (see., e.g., [Peaucelle \[2006\]](#); [Groenewegen \[1977\]](#)). However, if one reads papers dealing with Smith's philosophy and economic methodology, there are surprisingly not so many that directly offer methodological appraisals of his pin factory and international trade examples. For instance, in his well-known *Methodology of Economics*, Blaug claims that books I and II of *Wealth of Nations*, which include these two examples, "make a liberal use of the method of comparative statics" [[Blaug, 1980: 52](#)]. [Skinner \[1974\]](#), on the other hand, asserts that Smith's method should be seen as one close to how Newton studied the empirical realm. So an in-depth analysis is needed of the two above-mentioned studies by Smith. Let us start with the pin factory.

We do not aim to offer a contextual analysis of Smith's pin factory case since there are many studies analysing, for instance, the sources of his example (see, e.g., [Peaucelle \[2012\]](#)). What we want to propose is just a methodological interpretation of the first few pages of *Wealth of Nations*, where the pin factory case is presented. We will simply apply some earlier insights to assess to what extent Smith's pin factory can be conceptualised as a model. Smith starts by clearly stating that "the productive powers of labour [...] seem to have been the effects of the division of labour" [[Smith, 1993/1776: 3](#)], and next he introduces the way in which he aims to prove such a claim, in particular "by considering in what manner it [i.e., division of labour] operates in some particular manufactures" [[Smith, 1993/1776: 3](#)]. His famous pin factory example follows. He starts by clearly stating that he offers just an example of the division of labour, i.e., "to take an example" (4), and that the pin factory case is one where the division of labour is relatively unconstrained. Next, in the very same paragraph, we find the following from Smith: "I have seen a small manufactory of this kind" [[Smith, 1993/1776: 3](#)], but also his knowledge of pin production came from French authors he read [[Peaucelle, Guthrie, 2011](#)]. But his one-page-long description of such an enterprise is definitely deprived of any details. Also, he somehow exaggerated the extent to which the division of labour impacts productivity. Therefore, we have both Aristotelian and Galilean idealisations.

The paragraph following the one where he describes pin production is important from our perspective. Smith writes there: "In every other art and manufacture, the effects of the division of labour are similar to what they are in this very trifling one; though, in many of them, the labour can neither be so much subdivided, nor reduced to so great a simplicity of operation" [[Smith, 1993/1776: 5](#)]. So, nowhere are the effects of the division of labour the same as those described in his book. This is so because nowhere can work be divided so perfectly. Therefore, we have statements that are simply true in Smith's pin factory example (i.e., the division of labour increases productivity), but once they are referred to real life circumstances we have just *approximations*, i.e., "The division of labour, however, so far as it can be introduced, occasions, in every art, a proportionable increase of the productive powers of labour" [[Smith, 1993/1776: 5](#)]. Next, Smith discusses three well-known mechanisms that make the division of labour such a powerful factor in raising productivity.

Now, let us have a brief look at Smith's analysis of absolute advantage in international trade, which he presents in the chapter on "restraints upon the importation from foreign countries of such goods as can be produced at home". Here his study is not so focused as the one on specialisation in the pin factory case. Instead, there are many places in this chapter where he gives arguments in favour of free trade, including international exchange. So we have the well-known phrase with the invisible hand as well as many claims that it is not wise to produce at home things that can be cheaply bought on the market. There, for instance, Smith writes the following: "If a foreign country can supply us with a commodity cheaper than we ourselves can make it, better buy it of them" [[Smith, 1993/1776: 130](#)]; And similarly, "The natural advantages which one country has over another in producing particular commodities are sometimes so great that it is acknowledged by all the world to be in vain to struggle with them" [[Smith, 1993/1776: 131](#)]. What Smith proposes next is just an illustration of his insights regarding international trade, and hence his references to Scotland and the fact that although producing wine on Scottish soil is theoretically possible, it is nevertheless economically unreasonable. Therefore,

he even speculates on how much capital vine production in Scotland might require: “But if there would be a manifest absurdity in turning towards any employment thirty times more of the capital and industry of the country [i.e., Scotland] than would be necessary to purchase from foreign countries an equal quantity of the commodities wanted, there must be an absurdity, though not altogether so glaring, yet exactly of the same kind, in turning towards any such employment a thirtieth, or even a three-hundredth part more of either” [Smith, 1993/1776: 131]. So, as in the case of the pin factory, Smith offers some numbers that are hardly taken from the empirical realm, thus Galilean idealisations also are present. Also, while referring to Scotland and vine production, he claims that the principle of absolute advantage in international trade always holds: “As long as the one country has those advantages, and the other wants them, it will **always** be more advantageous for the latter rather to buy of the former than to make” [Smith, 1993/1776: 132; emphasis added]. Again, as in the case of the pin factory, we have statements that are entirely true in Smith's examples (hypothetical vine production in Scotland), but they change their status when referred to real world circumstances and when they take into account transportation costs. Hence Smith's remarks about “the short sea between Ireland and Great Britain” that eases trade in agricultural products. So, the workings of the principle of absolute advantage in trade are influenced by many factors that are not taken into account in Smith's original example.

Now, it seems that Smith's method (at least in the two cases described above) consists of three steps: first, stating a clear thesis (e.g., specialisation increases productivity), next drawing a simple story/example where it always holds (e.g., pin factory), and only later claiming that beyond these *fictional* worlds things are more complicated (e.g., the extent to which specialisation influences production depends on many factors). Interestingly, Smith himself claims the following regarding his method of enquiry: “If there be but one proposition necessary to be proved, there can be nothing more simple; the best method here undoubtedly is [the following]. First, to lay down the proposition, and afterwards advance the several arguments that tend to prove it; which may be summed up, or brought to conclude in the same terms as the proposition” [Smith, 1983/1763: 142]. Therefore, for what method is Smith in fact arguing in this passage? In light of our earlier discussion, Smith's opinion can be hardly understood as an argument for an inductive method. However, some historians of economic thought claim Smith used such a method. For instance, Henderson [2006: 85] states the following: “His approach, here, is inductive. He merely **illustrates it in the pin factory by spelling out the processes involved** and extends it, by informal analogy or by metaphorical extension, to a whole series of other simple examples” (emphasis added). Others, most notably J. Schumpeter, interpret Smith as someone favouring more abstract and theoretical methods, and hence Schumpeter claims the following: “A. Smith frequently made use of a pseudo-historical method of exposition: they liked to start, in explaining a social phenomenon such as property or money, from **an imaginary ‘early state’ of society**” [Schumpeter, 2006/1954: 106; emphasis added]. Smith's pin factory can definitely be treated as an instance of such an ‘imaginary state’.

Let us now dig more deeply into the two aforementioned works on Smith, starting with Henderson's opinion. His “spelling out the processes involved” may be interpreted as a claim that Smith isolates his pin factory example from unimportant factors while taking into account those crucial from his standpoint. Therefore, we have the procedure of isolation involved, which is definitely central to modelling. Next, “extending” original examples (Smith's pin factory story) metaphorically may be understood in two ways. First, it may be understood as just a claim that the models' conclusions are only partially valid while referred to extra model conditions. We offered such an interpretation earlier. Second, metaphorically extending a given illustration (Smith's pin factory) may also be understood as incorporating some other factors into a given model. What matters here is that metaphorising economic phenomena is in fact similar to modelling some empirical targets. And, thus, Black claims the following: “Use of theoretical models resembles the use of metaphors in requiring analogical transfer of a vocabulary. Metaphor and model-making reveal a new relationship; both are attempts to pour new content into old bottles” (Black [1962: 238–239], see also Hardt [2016]). Therefore, Henderson's reference to metaphors and analogies while describing Smith's method may be better interpreted as an argument for Smith-the-modeller rather than as proof that Smith's primary method of investigation is just induction.

Before going to Schumpeter's point, we would like to be clear that modelling does not necessarily exclude the usage of some inductive leaps. Usually, the relation of similarity between a given model and its target cannot be formulated in mathematical or logical terms, depending instead on the subjective judgement of the modeller, and thus, giving the basis for inductive inferences [Mróz, Hardt, 2020: 5]. Any such inference requires additional hypotheses about the proprieties of extra-model reality, and such proprieties are not parts of models as such (therefore, subjectivity). Now, as Sugden [2000: 23] explains, "if we are to make inductive inferences from the world of a model to the real world, we must recognise some significant similarity between those two worlds", and also, "the greater the similarity between the model world and the real world, the more confidence we can have in inferences from the former to the latter" [2013: 241]. Coming back to Smith, what comes first in his writings is a particular simplified illustration (model) of a given empirical realm (e.g., his pin factory case), and only later an inference from such a model to a given empirical situation. Thus modelling enables the use of inductive leaps.

Now, let us focus on Smith's "imaginary 'early state' of society", as it is called by Schumpeter. So, what matters here is that Schumpeter claims that Smith starts from a given "imaginary" situation (for instance, the pin factory), and only later extends his analyses to include some empirical and non-imaginary situations. But what does "imaginary" mean here? Well, imagination is central to modelling since "a model is a work of fiction" [Cartwright, 1983: 153]. But we have two kinds of fiction in model building: whole-cloth fiction and worldly fiction [Levy, 2012]. The former stands for a totally hypothetical situation without any links to the real world, while the latter relates to descriptions of actual entities, albeit with creativity and imagination. In Smith's works, we definitely have worldly fiction since his "imaginary states" are always in relation to the actual world. Paraphrasing Mäki [2009], we may say that we can have realistic "imaginary states" with unrealistic assumptions. As was described earlier, in Smith's pin factory example, we have many unrealistic assumptions, but still his imaginary pin factory can be treated as a realistic model, since the "realisticness" of models depends on whether they do not ignore greater causes of phenomena they try to explain. Smith is definitely not someone who mistakes minor causes for greater ones in his simplified images of the economic realm.

What is also worth referring to here is Smith's self-reflection on his "imaginary states" or situations that emerge only in ideal circumstances, e.g., his pin factory example or his analysis of absolute advantage in international trade. Here Smith may be legitimately treated as someone who employs the so-called "as if" methodology. So, for instance, he studies the division of labour as if work could be perfectly divided, and he analyses international trade as if transaction costs did not exist. This means that the pin factory behaves *as if* it were influenced only by a limited set of significant factors (i.e., those mentioned by Smith), whereas it is actually influenced by a larger set of factors [Mäki, 1998: 27]. Smith describes this aspect of his method in the following way: "And even we, while we have been endeavouring to represent all philosophical systems as mere inventions of the imagination, to connect together the otherwise disjointed and discordant phaenomena of nature, have insensibly been drawn in, to make use of language expressing the connecting principles of this one, as if they were the real chains which nature makes use of to bind together her several operations" [Smith, 1980/1795: 105; emphasis added]. The use of the "as if" methodology is a clear sign that Smith builds imaginary worlds that serve only as models of target systems Smith tries to understand. So, here we see "Smith's commitment to a modest scientific realism", as Schliesser [2017: 278] nicely puts it, or, in other words, Smith's ideal of taking into account only greater causes of economic phenomena and ignoring minor ones, to use the well-known distinction by Mill. But doing it is just model building.

Before going any further let us recapitulate in brief the arguments that have been presented so far. First, it was shown that Smith was greatly influenced by scholars who put models (Quesnay) and thought experiments (Hume) at the centre of their research practice. Next, presenting some general remarks on Smith's ways of studying markets enabled us to claim that Smith's distinctions between *artificial* and *real*, and between natural and actual, legitimise the claim that Smith is in fact someone who is able to construct imaginary worlds that can be used to account for empirical phenomena. Third, more detailed studies of his imaginary worlds

followed, and in particular an emphasis was put on the pin factory example and his remarks on international trade. It enabled us to claim that Smith can be treated as someone using the modelling method. Since imagination is crucial to building models, and since models can rightly be called imaginary states, and also since Schumpeter, Paganelli, and Schliesser, among others, claim that such states are ubiquitous in Smith's writings, let us in what follows focus on Smith himself discussing his method as one using imaginary states.

### Smith's "imaginary machines" as models

Much has been said above about imaginary states in Smith's writings, but what about machines in his oeuvre? **Aspromourgos [2012]** nicely shows that Smith often refers to machines, including machine metaphors and analogies (see, also, Section 3). His worldview is definitely very mechanistic, but his theories are also of this kind. This is definitely not a surprise having in mind his strong admiration for I. Newton<sup>17</sup>. In *Theory of Moral Sentiments*, he writes the following: "We naturally run it together in our imagination with the order, the regular and harmonious movement, of the system or machine that produces it" [**Smith, 2004/1759**: 241]. By "naturally running it" Smith just means the process of understanding, which consists of imagination, abstraction, systemic thinking, and references to machines or mechanistic metaphors and analogies. Therefore, in his own reflection on how empirical phenomena should be investigated, he states the following: "Systems in many respects resemble machines. **A machine is a little system**, created to perform, as well as to connect together, in reality, those different movements and effects which the artist has occasion for. **A system is an imaginary machine** invented to connect together in the fancy those different movements and effects which are already in reality performed. The machines that are first invented to perform any particular movement are always the most complex, and succeeding artists generally discover that, with fewer wheels, with fewer principles of motion, than had originally been employed, the same effect may be more easily produced. The first systems, in the same manner, are always the most complex, and a particular connecting chain, or principle, is generally thought necessary to unite every two seemingly disjointed appearances: but it often happens, that one great connecting principle is afterwards found to be sufficient to bind together all the discordant phenomena that occur in a whole species of things" [**Smith, 1980/1795**: 66; emphasis added]. This passage from Smith nicely portrays his modelling method. So, first, a system treated as an imaginary machine is constructed. Being imaginary means here that we are faced with something abstract and theoretical. Second, such an imaginary machine *represents* processes that are present in the empirical realm. However, once such an image of the real world is constructed, the task of the researcher is to make an imaginary machine simpler. Therefore, one isolates the system in question and hence arrives at discovering "the great connecting principle" [**Hardt, 2017**: 18]. So again, we have a typical method employed in model building, namely one of isolation. Also, as **Aspromourgos [2012**: 481] underlines, Smith's imaginary systems and machines are just "intellectual systems". Therefore, Smith's words give us a strong argument in favour of our claim that in fact we have models in his writings and that he can be treated as a model builder<sup>18</sup>. Such a reading of Smith's method is also offered by **V. Smith [2019]**, who claims that "Smith sought to explain through systematic analysis disciplined by 'experiments'. In other words, he sought to explain using test cases". But these "experiments" and "test cases" can be understood just as models (cf. **Mäki [2005]**)<sup>19</sup>.

Since in the above passage Smith treats "imaginary machines" as "little systems", it is worth commenting precisely on his method of systematic analysis. According to **Skinner [1996]**, Smith's "little systems" are just

<sup>17</sup> For instance, he claims the following: "[Newton] made the most happy, and, we may now say, the greatest and most admirable improvement that was ever made in philosophy" [**Smith, 1980/1795**: 98].

<sup>18</sup> Here our understanding of Smith's "imaginary machines" is slightly different from that offered by **Schliesser [2017**: 279], who treats them as scientific theories. As we stated earlier, Smith's "imaginary machines" are fictional worlds where given theoretical claims perfectly hold, or they can be treated also as *producers* of theories (cf. **Cartwright's [1999]** idea of models as nomological machines).

<sup>19</sup> But it does not mean that V. Smith treats A. Smith's "experiments", "test cases" or simply models as predecessors of models as they are understood in neoclassical economics; Smith's models are more sensitive to the complexities of human life than neoclassical models (see, e.g., **Smith and Wilson [2019]**).

“thought-systems” that are produced by researchers’ imagination. They can be understood simply as sets of coherently linked entities. Their role is to “introduce harmony and order into the mind’s conception of the movements of those bodies” (in: **Smith [1980/1795]**). So systems are just models since what matters primarily here is, first, their internal consistency, and only later the extent to which they correctly correspond to the empirical world. But systems are never perfect pictures of their targets (cf. **Schliesser [2017: 9]**). An important part of scientific practice is making such models more isomorphic to the world. So, from the perspective of this paper, it suffices to treat Smith’s “systems” as (nearly) synonymous with his “imaginary machines”.

Now, let us compare the above interpretation of Smith’s methods with that given by his well-known contemporary and commentator Marquis de Condorcet (cf. **Rothschild [2001]**). In one of his works, Condorcet states the following: “[...] the **truths of the theory are necessarily modified in practice**; there are really unavoidable inaccuracies, which cannot be avoided and we must not have a chimerical hope to prevent them; a large number of data on the needs, resources, time, and the expenditures necessarily neglected in theory should have an immediate effect on the problem under investigation” [**Condorcet, 1822/1794: 240**; emphasis added]. In claiming the above, Condorcet definitely thought about political economy, since shortly after stating what we have just cited he referred to public economics (*économie publique*). Also, in his opinion, economics, treated by him as a “new science”, was “developed by Stewart, Smith, and in particular French authors” [**Condorcet, 1822/1794: 199**]. So we can legitimately take Condorcet’s comments on scientific theories as referring to economics, primarily that of Smith.

What especially matters in the above passage from Condorcet is his distinction between “truths of the theory” and “[their] necessary modifications”. His reference to the truths of theories can be understood as a claim that sentences forming theories are perfectly true only in some *artificial* worlds, or, in other words, in models. For instance, saying that the division of labour increases production is entirely true in Smith’s pin factory example, where disturbing factors are absent, but if one refers to the real world then we can only believe that the division of labour will increase production. Coming back to Hume’s idea of the “degree of belief” introduced earlier in Section 2, the degree of belief can be entirely true, to use Hume’s term, only in models or thought experiments, including Smith’s imaginary machines, but not when such beliefs are referred to real world situations. Such a way of distinguishing between truths that are always valid in *artificial* circumstances (i.e., models, thought experiments, etc.) and truths modified in practice was typical not only of Smith, as we tried to prove it, but also of J. S. Mill (his distinction between “abstract truth” and “truth in the concrete”)<sup>20</sup> as well as, for instance, of A. Marshall (his understanding of economic laws as statements valid entirely only in “normal conditions”, and beyond such conditions laws of economics are only tendency laws)<sup>21</sup>; see **Hardt [2017]** for more arguments. Therefore, without a doubt, Smith’s remarks on “imaginary machines”, together with Condorcet’s comments on Smith, as well as a general trend in classical economics based on distinguishing between “imagined situations” and the empirical realm, offer strong support for our claim that Smith was a model builder.

## Conclusions

There is a widespread view among historians of economic thought that Smith’s method of inquiry puts an emphasis on reasoning with words rather than on reasoning with models (see, e.g., **Morgan [2012]**). However, as we have tried to show in this paper, Smith’s way of investigating markets exhibits many similarities with model building. We listed some of these similarities in the paragraph above, and thus Smith can in no way be considered to be less intensively using the modelling method than his predecessors (especially Quesnay, Hume, and Cantillon) and successors (including Ricardo and Mill) (cf. **Samuelson [1977]**, see also **Hardt [2017]**; and also similar arguments offered by **V. Smith [2019]**; **Paganelli [2017]**, and **Weingast [2018]**, among others). But

<sup>20</sup> See, e.g., **Mill [2008/1836]**.

<sup>21</sup> See, e.g., **Marshall [1920]**.

the question remains, why is such a view not dominant in studies on Smith's methods? Answering such a question would definitely require a separate study, but some points are worth making now. First, in Smith's writings we do not have clear-cut diagrams or tables (unlike, for instance, in Quesnay and Ricardo), which are usually seen as being synonymous with models. But the absence of such objects does not mean that we are not dealing with a modelling method, which is predominately a way of reasoning based on constructing some imaginary worlds that are used to account for empirical phenomena. Second, models can only be constructed from words without any use of algebra and mathematical formalism [Crombie, 1994]. This is so since modelling can be understood simply as giving form to economic intuitions, and these *forms* do not need to be put in mathematical or diagrammatic terms. Smith's pin factory is definitely such a *form*. Third, since many social sciences, including economics, have been greatly transformed in recent decades towards using modelling techniques, the philosophy of science has ceased to be "law-centred", with a greater focus placed on models and modelling. Still, this process is not fully reflected in the history of sciences, where writing about great theories of the past is still more popular than studying great models of the past. Nevertheless, since "[...] what have traditionally been interpreted as laws of nature thus turn to be merely statements describing the behaviour of theoretical models" [Giere, 1999: 523], then the focus of historians of economics should also evolve towards being more concentrated on models<sup>22</sup>. This does not mean that laws should disappear from our focus, but by treating laws as being *produced* or *illustrated* by models we should get a more complete insight into the history of our discipline. We hope that our study on the modelling method in Smith's economics is a step in this direction.

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<sup>22</sup> An even more radical stance than that of Giere is taken by Harré, who states the following: "Most philosophers now believe that the laws play no part in the genesis of natural regularities and natural tendencies that are displayed in them" [Harré, 2001: 213].

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