Essays on Biased Innovation and Environmental Policy

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Abstract: The thesis comprises of three essays aimed at enhancing our understanding of the relationship of environmental policy, firms' innovation and efficiency. In the first essay, we examine the direction of innovation in the U.S. metal industry with data envelopment analysis and how is it affected by the change in the price of fossil fuels. We provide evidence that there was a environmentally-biased innovation and it was driven by the expectations of energy prices. In the second essay, we examine the effect of environmental policy on efficiency state-level manufacturing in the U.S. We use instrumental variables to control for simultaneous causality and find that stringent policy enhanced the efficiency of the manufacturing sectors. In the third essay, we examine the Climate Change Levy on firms operating in four manufacturing sectors in the U.K. Our results show that Climate Change Levy increased efficiency.

Contents

List of acronyms

CCA .... Climate Change Agreement

CCL .... Climate Change Levy

DEA .... Data Envelopment Analysis

DMU .... Decision Making Unit

EU ETS .... European Union Emission Trading Scheme

GDP .... Gross Domestic Product

MPI .... Malmquist Productivity Index

PH .... Porter hypothesis

SFA .... Stochastic Frontier Analysis

SIC .... Standard Industrial Classification

Chapter 1

Introduction

Environmental problems - especially the threat of climate change - loom large over the head of the world leaders. According to the most recent IPCC (2014) report, the impacts of climate change "are projected to slowdown economic growth, make poverty reduction more difficult, further erode food security" (p. 20). These are beside the costs of biodiversity loss and increased likelihood of extreme weather events (p. 13). On the other hand, governments are also keen to reignite economic growth, which has been modest in the decade since the Financial Crisis and the Great Recession of 2008. Most OECD countries still face higher unemployment and lower GDP than before the crisis (OECD, 2017a,b).

Competitiveness and environmental protection are often framed in a "jobs versus the environment" context (Morgenstern et al., 2002), which assumes that environmental regulations will add to firms' cost burdens, cause a productivity slowdown, and eventually lead to job destruction. Most recently, US President Donald Trump indicated the will for renegotiating the Paris Agreement, because "[t]he Paris accord will undermine [the U.S.] economy" and it "puts [the U.S.] at a permanent disadvantage" (Chakraborty, 2017).

Despite these recent developments, environmental awareness continues to rise (Gallup, 2017). Environmental issues took an increasingly prominent place in national and international politics in the last decade. Figure 1.1 shows the percentage of American respondents who agreed with two propositions by Gallup. The first, blue (top), line shows the ratio of people who agreed that environmental protection is more important than economic growth. Despite the ups and downs from one year to the next, there's a clear upward trend. While in 2008 only 40% of respondents thought that environmental protection should be given priority, it has risen to almost 60% in 2018. Similarly people, who think that the seriousness of global warming is underestimated (depicted by the green, bottom line) has risen from 30% to over 40% in the last decade. It thus seems that environmental protection is increasingly

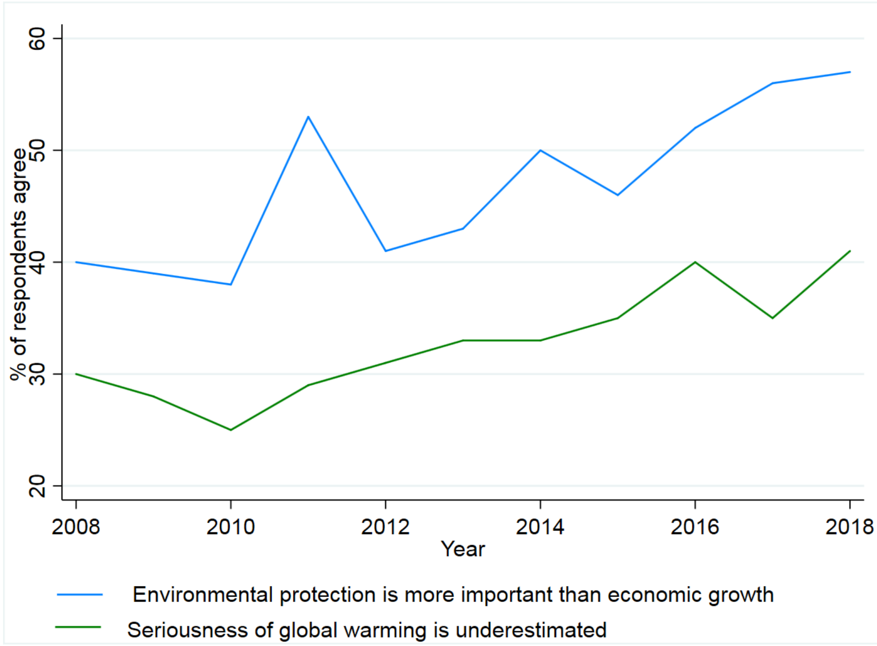


Figure 1.1: Importance of environmental issues in the U.S. (Source: Gallup, 2017)

important to voters. While environmental policies are designed to answer this need for environmental protection, political and economic consideration often lead to a focussing of attention towards their impacts on productivity. This tension between environmental policy and productivity often sparks heated debates about both trade-offs and possible, albeit unlikely, win-win scenarios.

Productivity is driven by innovation, but innovation also has an important role to play in addressing environmental problems. Indeed innovation is often thought of as a general shifter of well-being. For example, a UN report states we need innovation as a way out from the financial crisis (ITU, 2009). Innovation is a complex phenomenon, which rarely affects every factor of production in the same way. If it increases the marginal product of a factor over another factor, than it's *biased* innovation. For example there's a large literature in labour economics about the causes of the steep increase of the wages of the skilled workers in the 1970's and 1980's. According to the dominant explanation there was a large innovation, the computer, which was biased towards skilled workers; it increased their marginal product and hence their wages (Acemoglu, 2002; Violante, 2008).

Biased innovation has clear implications for environmental and energy economics as well. If innovation could be purposefully directed to be biased towards cleaner production, that would be a big step towards a solution of environmental problems. Hicks (1932) famously wrote that it's possible to control the bias of innovation: "a change in the relative prices of the factors of production is itself a spur to invention, and to invention of a particular kind fi?! directed to economizing the use of a factor which has become relatively expensive" (p. 124-125). The Hicksian *induced innovation hypothesis* generated a large interest from energy and environmental economists. In particular, a lot of papers asked whether there's an energy biased innovation if energy prices increase?

Newell et al. (1999) investigated this question by using product level data about home appliances. They found that innovation is responsive to price changes. Energy price increases induced 33-50% more energy-efficient products and changes in products that were on sale. Responsiveness increased when product labelling became obligatory, implying that there was also an informational problem between the consumers and producers. Popp (2002) studied a similar question: how energy prices motivate environmental innovation, using 1970-1994 U.S. patent data. He used patents and probability of citations (constructed from future citations) to proxy knowledge stock. He also confirmed the induced innovation hypothesis: energy prices significantly increase directed innovations. There's also evidence of path dependence: the current knowledge stock is a strong predictor of current patenting activity. Strong path dependence naturally leads to the question of lock-in: when path dependence is so strong that the market is "locked in" an inefficient equilibrium. Noailly and Smeets (2015) examine European firms' renewable energy innovation with patent data to investigate the possibility of innovation redirection (to change the bias). They estimate a zero inflated Poisson model and according their findings three factors influence redirection from non-green technology to green technology: energy prices increases, market size increases and knowledge stocks. Energy prices have the most effect, and they're perhaps the most easily manipulable and thus they're of policy interest.

Instead of looking at the energy prices some papers looked at the effect of a specific environmental policy on the biasedness of innovation. In general, these studies investigated either the effect of abatament costs, or one of the large policy experiments: the 1990 amendments of the Clean Air Act and the EU ETS. For example, Hamamoto (2006) examines how pollution abatement costs and expenditures induced overall R&D spending in Japan manufacturing sector between 1966 and 1976. He finds that increasing abatements costs increase R&D expenditure: a 1% increase in abatement costs increased R&D spending by 0.2%. Calel and Dechezleprêtre (2014) use a installation-level data to investigate the effect of the EU ETS on low carbon innovation. With firm-matching they find that firms which participated in the EU ETS patented about 10% more low-carbon technologies, when compared to non-regulated ones. Popp (2003) investigated the effect of regulation of the Clean Air Act amendments on innovative activity of coal power plants. His results show that the amendments changed the nature of the innovations. Before the amendments, most innovative activity was directed to lower the operation costs of scrubbers. Whereas after the amendments and the introduction of the cap-and-trade system, innovative activity was directed towards different solutions. Specifically after the amendments SO_2 removal efficiency increased by 1.58%.

Thus it seems that biased innovation could be directed at least for environmental purposes. But innovation is difficult to define and quantify. Though most of the time it's associated with new machines or better products, this is a relatively narrow view. Social and managerial innovations also improve the allocation of resources, thus increasing productivity and competitiveness. According to the Oxford Dictionary, 'to innovate' is to "make changes in something established, especially by introducing new methods, ideas, or products". Innovation describes all kind of changes in the way firm produces its outputs. This includes adoption of new technology (defined broadly) from the firm's point of view and the diffusion of a new method from society's point of view. In this sense innovation could be thought as a general process which improves the productivity of firms, be it technological or otherwise. For this and other reasons measuring innovation with patent counts could be misleading. Pakes & Grichilles (1980) provide an early critique of patent databases and despite important econometric advances patent counts are still considered to be unreliable; even the popular press raised concerns (Basulto, 2015).

First, the patenting system is almost by definition ad hoc (Pakes and Griliches, 1980), which makes international comparisons difficult. Even in a single country, firms can patent inventions outside their home country, taking advantage of the different rules. The ad hoc nature, also implies, that a patenting category has difficulty capturing the potential uses and effects of a patent, in other fields. It's also difficult to control for quality: some patents have no economic and technical value, whereas important innovations are not patented or patented only later.

Second, even if the patenting systems were perfectly harmonised with a firm theoretical grounding, they would still measure invention, but not innovation as a whole. Moreover the overwhelming majority of patents cover only product and process invention (managerial, market and organisational inventions are rarely patented).

Finally, the innovation policy literature (e.g. von Graevenitz et al., 2011; Blind et al., 2006, 2009) emphasises that patenting is a business decision and hence entails strategic considerations by the firm. Firms which have other means to protect their intellectual property (secrecy, copyright etc.) won't use the patent system. These imply that patents are an endogenous proxy for innovation.

Another popular measure for innovation is R&D expenditure (e.g. Jaffe and Palmer, 1997b). As with patents, it's unclear how it maps to innovation. Just as patents R&D expenditures usually don't cover all types of innovations: only product and process ones. The endogeneity problem arises here as well. Reporting R&D expenditures is a strategic decision. Some firms will report it accurately (especially if they're under regulatory scrutiny; Brouwer and Kleinknecht 1996); some firms won't report it, even if they collect it (hence a missing value doesn't necessarily mean zero R&D expenditure); some will overrepresent it to attract investment (Koh and Reeb, 2015).

These are the two most popular proxies. Some researchers use the responses from a survey conducted among the managers of the firms. To ease response burden it's usually a Likert scale type question, meaning a manager typically needs to value it's R&D activity on a scale of 1 to 5, where 1 is the lowest ("Strongly disagree"), 5 is the highest mark ("Strongly agree"). The answers to these types of questions are difficult to aggregate.

Another possible approach is to collect data from catalogues about product characteristics (e.g. Newell et al., 1999). This was criticised by Gallagher et al. (2006), because it ignores tacit nature of technical knowledge. Additionally it's difficult to distinguish between true innovations and imitations. Since it only measures product innovation, it's biased towards certain industries.

Jensen and Webster (2009) shows that the various innovations proxies are relatively weakly correlated. This implies that innovation is captured noisily by these variables. According to the (Jensen and Webster, 2009, p. 262): "[...] the different measures of innovation vary substantially at the firm level and, therefore, care must be taken when choosing amongst different innovation proxies", as the different variables can capture different phenomena.

When measuring innovation this thesis relies on production economics, in which innovation is defined as a shift in the production function (or isoquants) (e.g. Salter, 1960; Nelson and Winter, 1977). Using production economics (or more specifically productivity and efficiency analysis) to measure energy/environmental innovation is a recent practice. Fleishman et al. (2009) were the first to use two-stage data envelopment analysis in an energy/environmental setting. They find that the 1990 Clean Air Act revisions had a positive effect on energy efficiency (they omit prices from the estimation). Jaraite and Di Maria (2012) use country-level power-plant data to examine EU ETS, and comprehensively estimate the effect of energy prices as well. Estimates without shares show that coal has a moderate positive effect, oil and gas stronger negative on environmental efficiency. But the structure of the industry matters. CO2 price increases environmental efficiency, but it has no effect on economic productivity.

In the first chapter of the present dissertation, I contribute to this strand of literature by estimating and comparing both environmental and economic productivity. This enables a better understanding of the innovation pathways – and their bias – and to assess the impact of changes in energy prices on the bias. Economic productivity is measured by the productivity of inputs, whereas environmental productivity is the productivity of the pollutants. This reliance of productivity analysis avoids the shortfalls of other proxies discussed above. Unlike R&D expenditures or patents, they have clear relation to business conduct. They can also capture a wider of array of innovations; not only technological inventions, but also adoption and changes in managerial conduct. In the first chapter we examine the US metal industry and find significant environmentally biased innovation towards the price expectations.

An important and related question is whether this translates into different business conduct. Do environmental policies help firms' productivity or do they simply crowd out other investments, potentially drawing resources away from productivity enhancing innovations? Michel Porter famously conjectured that environmental policy could help firm performance (Porter, 1991; Porter and Van der Linde, 1995). Figure 1.2 depicts a schematic representation of the proposed relationship between environmental policy and firm performance. He believed that environmental policies induce innovations which improves environmental performance - a belief which is well-founded based on the empirical literature introduced above.[1] He thought that environmental innovations could translate to improvement of economic performance; an innovation spurred by a well-designed environmental regulation could often offset the associated costs. Porter proposed five mechanisms which could lead to this outcome.

First, a regulation might signal about resource inefficiencies. Firms are ill-informed or badly managed and miss out on the systematically profitable opportunities ("low hanging fruits"), which could be highlighted by an environmental regulation. Second, policies about reporting or information gathering could raise corporate awareness of environmental issues. Third, a regulation reduces uncertainty about the profitability of future environmental investments. If firms know that energy use will be taxed for the next years, it will more likely that they will invest in energy efficient machines or streamline the production process. Fourth, environmental regulation creates pressure for innovation and progress. Fifth, environmental policy levels the 'transitional playing field'. Firms can't gain advantage by avoiding environmental investment.

In other words, firms make suboptimal choices because of organizational or informational problems, or because of the presence of other market failures. Environmental policy could improve productivity and efficiency of firms. The Porter hypothesis has political implications, because if it's true then the competitiveness losses envisioned would be only true for badly designed policies.

The empirical literature is mixed on whether environmental policies could induce not only innovations, but efficiency and productivity improvement as well. Berman and Bui (2001) examine oil refineries in the Los Angeles Area with plant level data. They find that local environmental regulations increased abatement spending in these plants. The abatement investment, in turn, increased plant-level productivity. They conclude that the Porter hypothesis seems to be true, but abatement costs are likely to overstate the true cost of regulations.

Greenstone et al. (2012) research the effect of Clean Air Act Amendments on manufacturing

[1] Accordingly, one may place the literature introduced above within Porter's framework. In this context, the proposed link between environmental policy and innovation, i.e. the left side of Figure 1.2, is also referred to as the 'weak' Porter hypothesis.

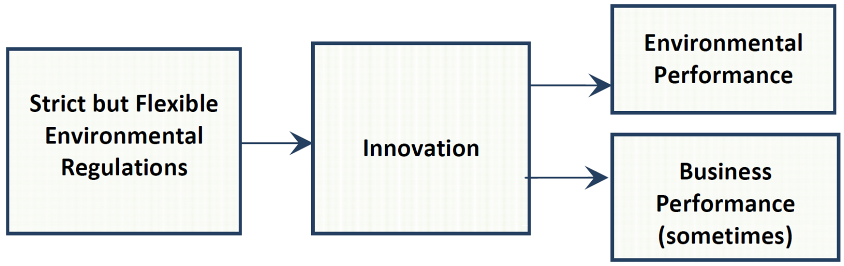


Figure 1.2: Schematic representation of the Porter hypothesis (adapted from Ambec et al., 2013).

plants' productivity. They proxy regulations with the attainment of plants and counties. Interestingly they find that strict regulations are associated with 2.6% TFP decline for all pollutants except for CO regulations where there's a positive effect on TFP. Broberg et al. (2013) use Swedish wood-, pulp-, metal- and chemical firm -level data in a dynamic stochastic frontier model. They use pollution prevention and pollution control investment as policy proxies and find no evidence of the PH. Specifically they find that environmental regulations significantly decrease firm efficiency.

The selected literature review shows that the evidence is mixed on the PH. In the second essay, I investigate how the results of these conflicting firm level studies would add up to an aggregate picture. In particular I examine the manufacturing sectors in the U.S. states data an stochastic inefficiency model (see next section). I aspire to provide a treatment for the endogeneity of policies, as environmental policies don't happen in a vacuum. While most theoretical models assume environmental policies to be exogenous (though there are exceptions, e.g. Barrett, 1994), in an empirical work we can't assume away the social and legal context of the policies. Industries lobby, consumers vote.

Similarly, in the third essay I investigate the empirical validity of the Porter hypothesis. But instead of looking at the manufacturing sector as a whole, I examine four selected sectors in United Kingdom. In recent years micro-databases are increasingly available, so I can look at the effect of environmental policies on plant level, but still examine the differences across sectors. Another feature which sets this essay apart from the first two is that it elicits the effect of a specific policy in the UK: the Climate Change Levy. This focus narrows the general setting of first and second essays, but it allows a more accurate elicitation of the effects in specific scenarios.

1.1 Innovation in a production economics framework

As mentioned in the previous part of the Introduction, the present work considers innovation to be a shift in the production process, regardless of the source.

The textbook models assume perfect competition, and hence no productive or allocative inefficiency of firms. The empirical literature in management research shows that this is assumption is often violated; there can be huge differences between firms even in narrowly defined industries (e.g. Chew et al., 1990). Clearly, this implies the untenability of the perfect competition model. As reactions to this untenability two separate, independent literatures emerged: one from industrial organisation (e.g. Olley and Pakes, 1996), and one from operations research (e.g. Aigner et al., 1977). The main differences between them are the assumptions they are willing to make. The industrial organisation productivity literature supports its identification strategy by making strong assumptions about the nature of competition and the mechanisms underlying it (Syverson, 2011a). Productivity analysis, spanned by the operations research literature, aspires to be more agnostic about the appropriate model of competition or the evolution of productivity differences. Despite the different approaches the estimates tend to be relatively similar (Van Biesebroeck, 2007). To be general, I will use the methods of productivity analysis in this dissertation. This also lowers the data demands, hence the effect can be reliably estimated.

Productivity analysis allows for inefficiencies arising from temporal bad governance or imperfect market structures. In production economics, production is defined as converting inputs (x) into outputs (y).

$$\text{Inputs} \longrightarrow \boxed{\text{Firm}} \longrightarrow \text{Outputs}$$

Innovation is assumed to be an improvement of production.

The way a firm can convert inputs into outputs depends on its technology; the technically feasible input-output combinations are described by its technology set:

$$T = \{x \text{ can produce } y\} \, .$$

One may distinguish two types of movement: movement of T and movement *within* T. Movement of T signals a change in possible achievable output for given quantity of input; technological change (in the broad sense of the word). The second movement (within T) shows the adoption or diffusion of a given technology as the firm is moving closer to the boundary of T - the production function, $f(x)$. There are multiple ways to interpret $f(x)$, depending on the approach taken it could be interpreted as a best practice of the firms

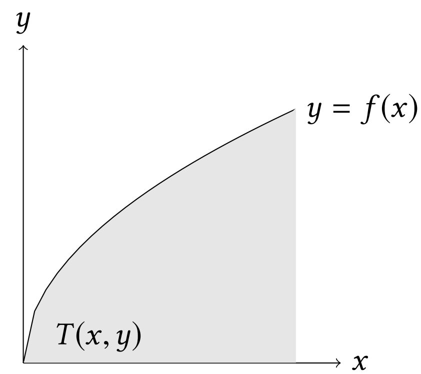


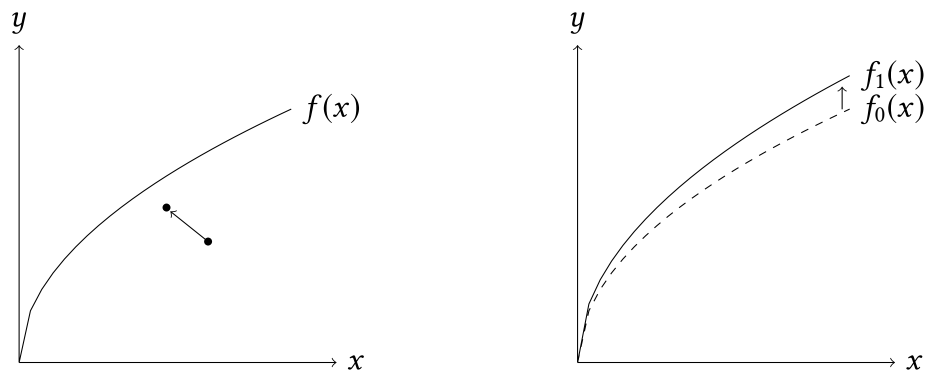
Figure 1.3: Technological set T and production function $f(x)$.

(Caves et al., 1982) or a first best, a theoretical best case scenario which won't materialise (Kumbhakar et al., 2013). Depending on the model it's not always possible to distinguish between the movements. But both are important: a new policy can induce new directions but if firms won't adapt these solutions the policy may not achieve its explicit goals. At the same time policies generally aim to induce inventions, e.g. IPCC (2014, p. 26) argues that countries need new technologies to mitigate the impacts of climate change.

Estimating the impacts of policies require to look at both outcomes. Figure 1.4a shows efficiency change in a simple one input, one output model. The firm, represented by the black dot, improves it's production by making *more* output, from *less* input. Note that $f(x)$ doesn't change, meaning there was no change in the overall technology only in the firm's conduct. While Fig. 1.4b looks at the intuition of a shift of $f(x)$, or technical change. The production function shifts out; it's not a change in production of a single firm but the change of the frontier achievable by all firms. This framework gives more flexibility and allows us to capture a wider array of innovations than the traditional proxies. In this dissertation I will look at how environmental policy affects the distance in T (distance from $f(x)$) and the position of T itself (or $f(x)$).

In general, there are two broad approaches of estimating T (or $f(x)$) and the corresponding efficiency of firms. First, data envelopment analysis is a completely non-parametric technique with great flexibility about the assumptions of the production process. It envelopes T and compares firms to the boundaries. This allows index-compositions and decompositions, but it also means that the method can't describe the production function ($f(x)$). Furthermore, being a deterministic method, the error term is assumed away.

The second method, stochastic frontier analysis is a parametric method, and instead of focusing on T, it aspires to describe $f(x)$ and compare the firm to that. It also allows for mismeasurement, and indeed its primary concern is to disentangle the unobserved error term



(a) Efficiency change.

(b) Technological change.

Figure 1.4: Possible effects of innovation.

from the unobserved inefficiency. While the potential questions which could be answered by these methods are similar, scholars typically use them for different purposes. Stochastic frontier analysis is used to answer questions about efficiency, whereas data envelopment analysis is used for productivity estimation and decompositions.

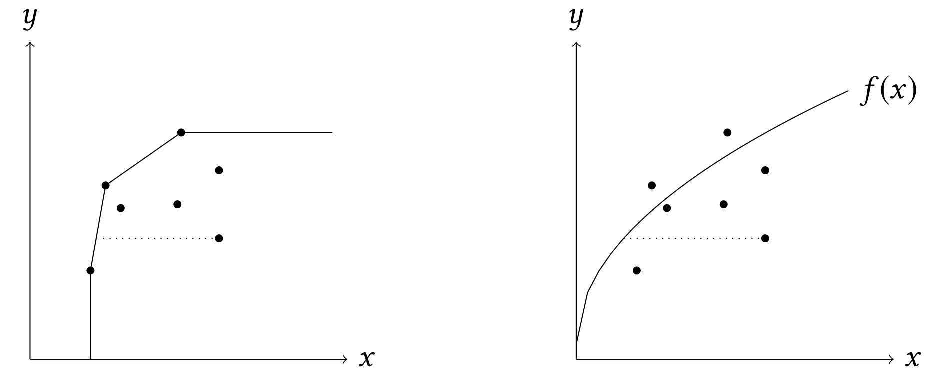
The dissertation employs both of these methods. Figure 1.5 provides a quick illustration and show the differences between the methods in an intuitive way.

The first panel of shows data envelopment analysis, where the frontier is based on the best practices and any shortfall is interpreted as inefficiency (shown by the dotted line for a firm). It's an agnostic method, it only assumes that what has been achieved (also their linear combinations) could've been achieved by other firms.

Stochastic frontier analysis, shown by the second panel is almost like a production function estimation, but with the added inefficiency. It's worth noting that the dotted line here includes the inefficiency *and* the error term. In this case we can easily interpret the coefficients of the inputs. Note that these graphs are highly stylized and both data envelopment and stochastic frontier includes vastly different families of models and could look completely different than the figures, depending on the assumptions.

1.2 Outline of the thesis

The thesis consists of three independent pieces of research and presented as such. As a result some repetition between the chapters is inevitable. The rest of the thesis is organised as follows. In Chapter 2, I investigate Hicks' (1932) induced innovation hypothesis focusing on energy consumption in the US metal sector. I use Data Envelopment Analysis to estimate the extent of 'green' (pollution reducing) and general (input reducing) innovation. During



(a) Data envelopment analysis. (b) Stochastic frontier analysis.

Figure 1.5: Intuition behind the productivity analysis methods.

the sample period I find evidence of environmentally biased innovation, driven by both technical change and efficiency change. In the second part of Chapter 2, I show that energy price expectations could explain the 'green' bias in the innovation and overall the induced innovation hypothesis is true.

The Chapter 3 looks at the effect of energy policy stringency on the efficiency of the US manufacturing sector. This chapter uses stochastic frontier analysis in a relatively short timeframe (5 years). The stringency is traced by an index number and the paper addresses the possible reverse causality issues by instrumental variables. The Porter hypothesis is strongly supported. Stringent policy enhances efficiency and hence competitiveness.

In the fourth chapter, I use UK firm level data to elicit the effect of the Climate Change Levy on efficiency in 4 selected sectors. We control for the non-random assignment of Climate Change Levy tax rates by using a sample selection model in the stochastic framework. While the inefficiency declines is large (more than 80%), this translates to 11-27% increase in output. The last chapter concludes.

Chapter 2

Energy Prices and Biased Innovation: New Evidence from the U.S. Metal Industry

Abstract:[1] This paper empirically investigates the induced innovation hypothesis of Hicks1932 examining the effects of changes in energy prices on environment-biased innovation in the US metal industry. Contrary to most of the existing literature, we focus on actual input and output data rather than invention proxies such as patent counts and R&D spending. Using Data Envelopment Analysis (DEA), we compute changes in both environmental and economic productivities and their components (technical and efficiency changes) and find strong evidence of environment-biased innovation. We also show that improvements in environmental productivity are associated with changes in energy prices over time. Overall, our results strongly support the induced innovation hypothesis.

2.1 Introduction

By providing transparent signals about the relative scarcity of goods, prices perform a fundamental role in the economy. As refers specifically to the prices of productive inputs, Hicks (1932) pointed out early on that

> "*A change in the relative prices of the factors of production is itself a spur to invention, and to invention of a particular kind – directed to economizing the use of the factor which has become relatively expensive.*" (*ibid.*, pp 124-125)

[1]This chapter is based on joint work with Dr. Corrado Di Maria.

Thus – the theory goes – the dynamics of input prices matter greatly for the type of technology developed by inventors and innovators. The natural implication of this is that purposeful modifications of input prices – e.g. via taxes and subsidies – could be used to direct innovation in the direction most favoured by the policy maker.

While these ideas hold general appeal for economists, they have encountered particular favour among economists investigating issues linked to environmental sustainability and climate change, due to the obvious relevance of policy-driven price changes in this context and the opportunities offered by 'green' innovation (e.g. Acemoglu et al., 2012). As current perceptions on climate – and more broadly environmental – policy relies heavily on the idea that technological change reduces the expected costs of future policy interventions, it is especially important to empirically assess the induced innovation hypothesis in this context.[2]

A large empirical literature has sought to investigate these theoretical predictions, and much work has been devoted, for example, to the impact of changes in energy prices on innovation (Newell et al., 1999; Popp, 2002; Aghion et al., 2016). Much of this literature, however, links the concept of innovation to the idea of available technology and, accordingly, proxies technical change with patenting activity. While this is in part explained by issues of data availability, the use of patent counts as metrics for innovation is problematic. It is apparent that patents filed and granted should be more appropriately considered as an output indicator of R&D (or invention) activity, rather than of wider innovative success, as inventions might not be widely deployed (Gallagher et al., 2006). Additionally, international comparisons in patents may be flawed because the quality of patents varies substantially across countries, as does the propensity to patent. Similarly, different industries have different patenting strategies adding to the noisiness of the data.[3] It is also important to keep in mind that while the availability of the technology is a necessary condition for innovation, it is far from sufficient. If the aim is to assess the impact of changes in input prices on the technology actually employed by firms, accounting for the deployment and the diffusion of the technology is critical (Linn, 2008).

In this paper, we try to address some of these shortcomings and revisit the induced (directed) innovation hypothesis using actual input and output data within a Data Envelopment Analysis (DEA) framework. We see three main advantages in this strategy, that underpin

[2]Despite a growing literature to the contrary – e.g. Bauman et al. (2008); Perino and Requate (2012); Di Maria and Smulders (2017) – the perception that current policy efforts *induce* the development of technology that facilitate future ones is still firmly radicated as emphasized, for example, by the recent contributions of Fried (2018) and Liu and Yamagami (2018).

[3]Since our efforts below are conceptually – if not methodologically – related to this strand of the innovation literature, it is appropriate to at least mention that the main results from the literature which uses patenting activity to proxy for innovation indicate a modest positive link between prices and 'green' patent counts (e.g. Popp, 2002; Ley et al., 2016; Aghion et al., 2016).

our contribution to the literature. On the one hand, we focus on actual firms' behaviour rather than on what is potentially feasible, i.e. we describe what happens as opposed to what could happen. Formally, our focus is on the realized best practice frontier rather than on changes in the production possibilities set. This is obviously important from the policy stand point as what matters is whether the policy actually produces desirable change, not just the possibility of it. On the other hand, we resort to data envelopment analysis (henceforth DEA) for our empirical investigation. Being a non-parametric method, DEA allows us to estimate the efficient frontiers over time and the associated changes in productivity without the need for the behavioural assumptions that underpin alternative methodologies. As our focus is on realized efficiency improvements and actual productivity changes, we can be agnostic as to the underlying model of competition, for example. Third, our chosen methodology further allows us to decompose changes in productivity over time in its two components of technical change, which shift the efficient frontier outwards, and efficiency change, which measures the degree to which individual decision making units approach or fall further away from the efficient frontier. This decomposition provides indication as to whether we are facing technological innovation implemented only by the industry forerunners, or whether there is a more generalized process of diffusion of new methods of production across the whole industry.

In what follows, we use industry-level data on U.S. metal manufacturing over the period 1990-2008 to estimate changes in economic and environmental productivity over time. By comparing and contrasting the economic and environmental measures, we are able to assess the existence of biased technical change, i.e. technical change that increases the productivity of certain inputs over that of the others (Acemoglu, 2002). Exploiting the features of our chosen measure of productivity change, the Malmquist productivity index, furthermore, we calculate indexes of technical change and efficiency change, thus disentangling shifts in the frontier from movements of our observations relative to the frontier. In the second stage of our analysis, we employ an econometric approach based on the seminal contribution of Simar and Wilson (2007) to gauge the impact that input prices have on productivity change and its components. Our results show a significant bias in the evolution of the relative productivity of productive inputs, with a sizeable increase in the productivity of polluting ones relative to others. We also find significant positive correlations between increases in energy prices and productivity gains.

While we are not the first to adopt such a methodology, using DEA to measure energy innovation is a relatively recent practice in the economic literature. Fleishman et al. (2009) were – to the best of our knowledge – the first to use a two-stage data envelopment analysis in an energy economics setting. Analyzing a panel dataset of power plants in the U.S., they find that the 1990 Clean Air Act Amendments had a positive effect on energy efficiency. Contrary to our contribution, however, they do not look directly at the role of energy prices nor do they discuss biased innovation. Within their discussion on the effectiveness of the EU climate policy action, Voltes-Dorta et al. (2013) examine the automobile market in Spain between 2004 and 2010 with a DEA-Malmquist approach. They find that higher fuel prices are correlated with efficiency improvement for gasoline cars, but seem to imply a counterintuitive drop in the efficiency of diesel cars. While methodologically related to ours, their analysis focuses on the short-run impact of emissions limits on new vehicle purchases and thus pertains more to consumers' choices rather than to biased technical change. Finally, Jaraite and Di Maria (2012) use a EU-wide dataset on fossil-fuel-based public power plant sectors over the twelve-year period (1996-2007) that spans the ratification of the Kyoto Protocol and the first three years of the European Union's CO_2 Emissions Trading Scheme (EU ETS). The authors investigate the link between environmental policy stringency and environmental efficiency, with a focus on the impact of allowance prices and the level of permit allocation on efficiency rather than on biased innovation. They conclude that system design matters very much in the EU ETS as emissions prices exhibit a strong positive correlation with efficiency change, whereas generous allocations have an opposite effect. The carbon price is found not to have any statistically significant effect on productivity change, however.

The rest of the paper is organized as follows, in Section 2.2 we first present the methodology that we will use to estimate the productivity of the decision making units over time and to decompose the productivity index into his two components of technical change and efficiency change (Section 2.2.1). Next – in Section 2.2.2 – we detail the empirical strategy that we will use to identify the drivers for each of the indexes, focussing on the challenges posed by the nature of the dependent variables calculated using DEA. The following section, Section 4.4, discusses the data and describes their sources and characteristics. Section 4.5 contains the description of the results of our empirical efforts and discusses the economic intuition behind them. Finally, Section 2.5 summarizes and concludes.

2.2 Measuring and explaining productivity changes

In this paper, we investigate to what extent changes in energy prices drive energy-saving innovation. Our goals are to account for both *bona fide* innovation and adoption of existing technologies within a unified framework, to identify the efficiency gains connected with changes in technology, and to provide evidence as to how these changes correlate with energy prices. Since our interest is motivated by sustainability concerns as refers to the environmental footprint of economic activity, it is important to keep our focus at the aggregate level, while ensuring sufficient variability in the data for our statistical analysis to be meaningful. To this end, we use data aggregated at the 4-digit SIC industry level, so that the units of our analysis are industrial sub-sectors rather than firms.[4]

In what follows, we resort to Data Envelopment Analysis (DEA) (Charnes et al., 1978) to identify the efficient frontier at each point in time and to quantify the relative distance of 'Decision Making Units' (DMUs) from the efficient frontier. Subsequently, we use econometric techniques to gauge to what extent the changes we observe in the indexes correlate with changes in the price of inputs, foremost among which is the price of energy. The use of DEA further allows us to disentangle shifts in the frontier – which are most closely related to innovation proper – from changes in DMUs' efficiency over time, an aspect more germane to catching-up and the adoption of established technology.

2.2.1 Data envelopment analysis and productivity measurements

DEA is a non-parametric linear programming method, which has the dual advantage of not requiring either behavioural (e.g. profit maximisation vs. cost minimisation) or structural (e.g. perfect competition vs. oligopoly) assumptions. It also provides a flexible framework within which efficiency gains may accrue from either output expansions or input contractions. By examining factor-specific efficiency and productivity, moreover, we will be able to gauge whether DMUs systematically improve the productivity of one factor more rapidly than that of another, providing evidence of biased technical change. Below, we compute both a standard measure of 'economic' productivity and a specialized measure of 'environmental' productivity, which flexibly accounts for efficiency gains in the use of polluting inputs. We would conclude that innovation is biased towards polluting inputs, if the latter measure grows consistently faster than the former.

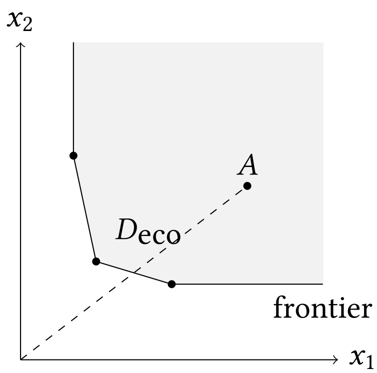
Having identified the relevant DMUs (of which there are N, say) and their input and output vectors, in order to assess them by DEA we need to construct the production possibility set (PPS) within which each DMU operates. The PPS contains all the correspondences of input and output vectors that are technically feasible, at least in principle. Once the PPS is known, the position of each DMU within it provides information on its relative performance, for example in terms of its (relative) efficiency or profitability. We can define the PPS as:

$$T = \{(\mathbf{x}, \mathbf{q}, \mathbf{z}) \,|(\mathbf{x}, \mathbf{z}) \text{ can produce } \mathbf{q}\}, \tag{2.1}$$

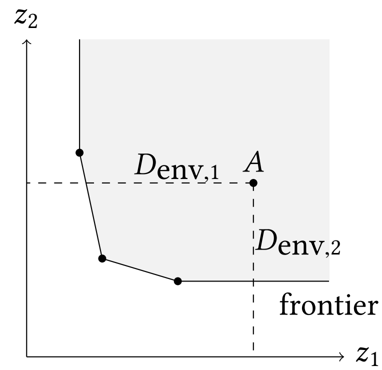
where $\mathbf{x}$ denotes the $K \times 1$ vector of inputs, $\mathbf{z}$ is the $S \times 1$ vector of pollutants, and $\mathbf{q}$ is the $M \times 1$ vector of outputs.

Since we are mostly interested in the energy-use and the polluting emissions of the different

[4] Prusa (2012); Akgobek and Yakut (2014); Kong and Tongzon (2006) are examples of the DEA applied to sector-level data.



(a) A radial measure.



(b) A non-radial measure (Russell measure).

Figure 2.1: Radial and non-radial measures in DEA.

DMUs here, we can alternatively recast our description of the technology using an input-orientation and define the input-pollution set,

$$L(\mathbf{q}) = \{(\mathbf{x}, \mathbf{z}) \,|(\mathbf{x}, \mathbf{z}, \mathbf{q}) \in T\}, \tag{2.2}$$

which emphasizes the DMUs' decisions about their inputs over those regarding their outputs.

To gauge the efficiency of each DMU relative to the technological (best-practice) frontier, we can use the concept of (radial) 'distance function'. An economic distance function measures the largest possible contraction (ρ) of inputs given the current input and output vectors, i.e. Shepard (1953):

$$\mathrm{D}_{ECO}(\mathbf{x}, \mathbf{q}) = \sup\{\rho : \mathbf{x}/\rho \in L(\mathbf{q})\}. \tag{2.3}$$

The radial characterisation of (2.3) implicitly assumes that input shares remain stable at different levels of efficiency. Figure 2.1a illustrate this for the single output, two inputs, two pollutants setting (1×2×2). In this input-space representation, the shaded area represents the input set, $L(\mathbf{q})$, whereas the solid line is the technology frontier – an isoquant drawn for an arbitrary level of production $q = q_0$. It is evident that DMU A uses more inputs than are required for the production of $q = q_0$ – i.e. it's not on the frontier. D_{ECO} then assesses A's efficiency by measuring the distance from A to the frontier along the ray through the origin: the input ratio is constant along the dashed line.

While this is likely to be a fairly realistic assumption with respect to traditional inputs into production, especially at an aggregated scale, it is rather more implausible when measuring environmental performance. Abatement technologies exists, for example, that allow the removal of only one type of pollutant, either due to technical limitations or as the consequence of policy forcing. The example of de-sulfurization units in power-plants comes to mind, where 'scrubbers' abate most of the SO_2 emissions from flue gases doing very little to remove other pollutants (e.g. CO_2 or NO_x). Firms deploying such technologies would achieve efficiency improvements and change the ratios of pollutant to other inputs and different pollutants simultaneously. With this in mind, one may define an environmental distance function in a similar way to (2.3), but allowing for the fact that the DMU may in fact contract polluting inputs not proportionally (Färe and Lovell, 1978):

$$\mathbf{D}_{ENV}(\mathbf{z}, \mathbf{q}) = \sup\{\boldsymbol{\rho} : \boldsymbol{\rho}^{-1}\mathbf{z} \in L(\mathbf{q})\}. \tag{2.4}$$

In (2.4), $\boldsymbol{\rho}$ is an $S \times S$ diagonal matrix composed of the $\rho_1, \ldots, \rho_S$ contractions corresponding to each of the $j = 1, \ldots, S$ pollutants. As mentioned above, this characterisation allows pollution contraction to be asymmetric. Figure 2.1b suggests that firm A just wants to get rid of pollutants, without any consideration of the possible cross-effects. As illustrated in Figure 2.1b, in this case there will be one efficiency estimates for each of the two pollutants , i.e.:

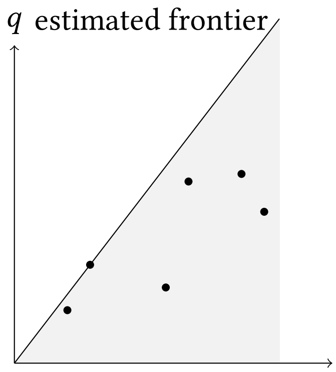
$$\mathbf{D}_{ENV} = \begin{pmatrix} D_{ENV,1} & 0 \\ 0 & D_{ENV,2} \end{pmatrix}. \tag{2.5}$$

Having specified the PPS and the appropriate efficiency measures, we can then use linear programming techniques to envelope the boundary of the input-pollution set for all output, using data on inputs, outputs and pollution for each of the DMUs. To make this explicit, we may replace the input-pollution set in (2.2) with its empirical counterpart:

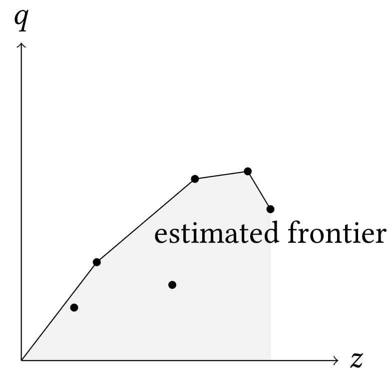
$$L(\mathbf{q_i}) = \left\{ (\mathbf{x}, \mathbf{z}) \;\middle|\; \begin{array}{l} \mathbf{Q}\boldsymbol{\lambda}_i \geq \mathbf{q}_i \\ \mathbf{X}\boldsymbol{\lambda}_i \leq \mathbf{x}_i \\ \mathbf{Z}\boldsymbol{\lambda}_i \leq \mathbf{z}_i \end{array} \right\} \quad \forall i = 1, \ldots, N. \tag{2.6}$$

In the expression above, $\mathbf{Q}$ is the $M \times N$ matrix of output data for all DMU and, similarly $\mathbf{X}$ is the $K \times N$ matrix of input data and $\mathbf{Z}$ is the $S \times N$ matrix of pollutant data. The vector of outputs for each DMU is $\mathbf{q}_i$, while $\mathbf{x}_i$ and $\mathbf{z}_i$ are the input and pollutant vectors of DMU i, respectively. The vector $\boldsymbol{\lambda}_i$ is a $N \times 1$ DMU-specific vector of parameters that envelope the boundary of the input set. Assumptions about $\boldsymbol{\lambda}_i$ represent assumptions about the returns to scale of the modelled technology. If one only assumes that the elements of $\boldsymbol{\lambda}_i$ are greater than 0, the frontier enveloped will exhibit constant returns to scale. If one assumes that the sum of its elements add up to 1, the frontier will exhibit variable returns to scale. Looking back at Figure 2.1, we can see that the nodes on the frontier represent other DMU with different, efficient input/pollution use, and that they shape the boundary of the input-pollution set.

As can be clearly seen from (2.6), pollution is treated similarly to an input. This is intuitively



(a) Strong disposability.



(b) Weak disposability.

Figure 2.2: Disposability assumptions in DEA

clear as the less of the polluting input is used in production, the better. One aspect that is less obvious is that pollutants differ from standard inputs in that it is generally costly to eliminate them. To reflect this, in what follows we add the assumption of weak-disposability rather than assuming as we had implicitly done so far that pollutants are strongly disposable, that is that it would be possible to eliminate their use at no cost, all else equal (Pittman, 1983; Färe et al., 1989).

Figure 2.2 illustrates how the different disposability assumption affect the PPS in the simplest case of one output and one polluting input. In this output-input representation, the shaded area once again represents the PPS, the dots indicate observed DMUs with their peculiar input-output combinations, and the solid line is the efficient frontier. When we assume strong disposability, having more of the input is always better: if there's too much input, it's always possible to dispose of it at no cost. Under weak disposability of the pollutants, however, more pollution hurts production, and might even entail negative marginal product.

In order to incorporate the weak disposability assumption in DEA, it is necessary to add a scalar $\delta \in [0,1]$, to the constraints on pollutants and change the inequality to equality (Fried et al., 2008). In this case, the input-pollution can be rewritten as:

$$L(\mathbf{q}) = \left\{ (\mathbf{x}, \mathbf{z}) \left| \begin{array}{l} \mathbf{Q}\boldsymbol{\lambda}_i \geq \mathbf{q}_i \\ \mathbf{X}\boldsymbol{\lambda}_i \leq \mathbf{x}_i \\ \mathbf{Z}\boldsymbol{\lambda}_i = \delta \mathbf{z}_i \end{array} \right. , \delta \in [0,1] \right\} \quad \forall i = 1, \ldots, N$$

To estimate the distance functions in (2.3) and (2.4), one needs to define appropriate linear programmes. Keeping in mind that, as discussed above, the economic distance function (2.3) assumes that the input shares are stable over time, it suffices to estimate just one efficiency parameter (θ_i) for each DMU, which contracts all inputs simultaneously. Conversely, since abatement technologies may have differential effects on different pollutants, it is necessary to estimate for each DMU and pollutant a separate efficiency parameter (ϕ_{ij}). Formally, the (envelopment) program for the economic distance function may be written as follows:

$$\begin{aligned} \mathrm{D}_{ECO} = \min_{\theta_i, \boldsymbol{\lambda}_i, \delta} \ & \theta_i \\ s.t.\ \ & \mathbf{Q}\boldsymbol{\lambda}_i \geq \mathbf{q}_i; \\ & \mathbf{X}\boldsymbol{\lambda}_i \geq \theta_i \mathbf{x}_i; \\ & \mathbf{Z}\boldsymbol{\lambda}_i = \delta \theta_i \mathbf{z}_i; \\ & \boldsymbol{\lambda}_i \geq 0;\ \delta \in [0,1]; \\ & i = 1, \ldots, N. \end{aligned} \tag{2.7}$$

Correspondingly, the environmental distance function can be computed as the solution to:

$$\begin{aligned} \mathrm{D}_{ENV} = \min_{\phi_{ij}, \boldsymbol{\lambda}_i, \delta} \ & \sum_{j=1}^{S} \phi_{ij} \\ s.t.\ \ & \mathbf{Q}\boldsymbol{\lambda}_i \geq \mathbf{q}_i; \\ & \mathbf{X}\boldsymbol{\lambda}_i \geq \mathbf{x}_i; \\ & \mathbf{Z}\boldsymbol{\lambda}_i = \delta \boldsymbol{\phi}_i \mathbf{z}_i; \\ & \boldsymbol{\lambda}_i \geq 0;\ \delta \in [0,1]; \\ & i = 1, \ldots, N;\ j = 1, 2, \ldots, S. \end{aligned} \tag{2.8}$$

In the last program, $\boldsymbol{\phi}_i$ is the $S \times S$ matrix with all the possible contractions, ϕ_{ij}, as diagonal elements. The characterisation of efficiency in the *ENV* measure is called a Russell measure (Färe and Lovell, 1978; Russell, 1985). From the simple non-negativity constraints imposed on the $\boldsymbol{\lambda}$'s, it is apparent that we are assuming constant returns to scale, due to the aggregate nature of our DMUs. While we believe that this is the correct assumption given the sectoral data[5], an additional benefit that arises from this choice is that in what follows we are going to compute Malmquist productivity indices and Grifell-Tatjé and Lovell (1995) have shown that such productivity estimates might be systematically biased by assuming non-constant returns to scale.

Obviously, individual DEA programs (just like a distance function) are only able to measure efficiency at a point in time. Using panel data, it is instead possible to measure relative

[5]Walheer (2018) tests the reliability of constant returns to scale with sectoral data in an econometric estimation and finds that "in general, this assumption is rather acceptable". For a detailed discussion about the feasibility of this assumption in DEA framework see Coelli and Rao (2005), who conclude that "when dealing with aggregate data [...] the use of a CRS technology is the only sensible option" (p. 122).

efficiency over time. In so doing, however, one conflates in the efficiency estimates both movements of DMU relative to the efficient frontier, which are typically linked with technology adoption, and movements of the frontier itself which are more specifically related to genuine technological advances. Since one of the goals of this paper is to discriminate, broadly speaking, between innovation and adoption, it is important to find ways to disentangle the two processes. In what follows, we try to identify these two contributions to efficiency changes using the Malmquist productivity index (MPI), first introduced by Malmquist (1953), which has been widely used in the literature.[6] The MPI is especially popular because it is easy to calculate, interpret and decompose.[7] The economic MPI for a given DMU over two periods of time (1,2) is defined as:

$$\mathscr{M}_{ECO}(\mathbf{x}_1, \mathbf{q}_1, \mathbf{x}_2, \mathbf{q}_2) = \left[\frac{\mathrm{D}_{ECO,1}(\mathbf{x}_1, \mathbf{q}_1)}{\mathrm{D}_{ECO,1}(\mathbf{x}_2, \mathbf{q}_2)} \times \frac{\mathrm{D}_{ECO,2}(\mathbf{x}_1, \mathbf{q}_1)}{\mathrm{D}_{ECO,2}(\mathbf{x}_2, \mathbf{q}_2)}\right]^{1/2}. \tag{2.9}$$

One may interpret it as a change in the input-output composition, between the two periods. Since there are two frontiers (for the two periods) and as such two measured distances for each DMU and little reason to favour one over another, one takes the geometric mean. This measures relative change in productivity and is 1 if there's no productivity change, larger than 1 if the productivity increased, and less than 1 if there is evidence of productivity regress.

Furthermore, one can decompose the estimated $\hat{\mathscr{M}}_{ECO}$ to emphasize the contributions of changes in efficiency and technological change by rewriting (2.9) as (e.g., Fried et al., 2008, Chap. 1):

$$\mathscr{M}_{ECO} = \underbrace{\frac{\mathrm{D}_{ECO,2}(\mathbf{x}_2, \mathbf{q}_2)}{\mathrm{D}_{ECO,1}(\mathbf{x}_1, \mathbf{q}_1)}}_{\text{Efficiency change}} \underbrace{\left[\frac{\mathrm{D}_{ECO,1}(\mathbf{x}_2, \mathbf{q}_2)}{\mathrm{D}_{ECO,2}(\mathbf{x}_2, \mathbf{q}_2)} \times \frac{\mathrm{D}_{ECO,1}(\mathbf{x}_1, \mathbf{q}_1)}{\mathrm{D}_{ECO,2}(\mathbf{x}_1, \mathbf{q}_1)}\right]^{1/2}}_{\text{Technical change}}. \tag{2.10}$$

Intuitively, the efficiency change component measures changes in the distance between each DMU and the relevant efficient frontier, which accounts for the fact that the frontier itself has shifted. The technical change component, transparently, captures changes in productivity due to the movement of the frontier itself. As $\mathscr{M}$ is the product of these two terms, the intuitive interpretation of these components is the same as $\mathscr{M}$: a number lower than 1 implies regress, above 1 shows progress, while exactly 1 means that the component didn't over the periods.

The MPI and its components are easily calculated for economic productivity change, thanks to the radial nature of the underlying distance measure. They can also be computed in a straightforward fashion for each of the individual pollutants (e.g. Zhou et al., 2010a).

[6]See Caves et al. (1982); Färe et al. (1994) for seminal contributions in this respect.
[7]For other commonly used indices and indicators see Fried et al. (2008, Chap. 5).

Unfortunately one cannot calculate the MPI for multiple pollutants, since the Russell measure is non-radial. Given that the individual components – the ϕ_{ij} – of the Russell measure are radial, however, one can circumvent this problem by calculating pollutant-specific MPIs, call them $\mathscr{M}_j$, and subsequently take their arithmetic mean to compose the environmental MPI as follows:[8]

$$\mathscr{M}_{ENV} = \frac{1}{S}\sum_{j=1}^{S}\mathscr{M}_j, \tag{2.11}$$

which can then be decomposed in the same way as the economic index in (2.10). To the best of the author's knowledge, this is the first where the productivity of multiple pollutants are measured in a DEA framework. Zhou et al. (2010b) provided a similar measure but only to measure carbon-productivity.

2.2.2 Explaining changes in productivity

Having introduced the methodology that allows us to compute the level of productivity and to decompose its changes over time into technical and efficiency change, we now turn our attention to the empirical methodology that we employ below to analyze the factors that drive such changes, within the framework of the induced innovation hypothesis. In the second stage of our analysis, we estimate equations of the following general form:

$$Y_{m,it} = \beta_0 + \boldsymbol{\gamma}\Delta\log(\mathbf{p})_{i,t+k} + \boldsymbol{\delta X}_{it} + u_i + \epsilon_{it}, \qquad \text{with } m = \{ENV, ECO\}. \tag{2.12}$$

Equation (2.12) shows that each of the measures of productivity detailed in the previous section – i.e. the MPIs and their components – here generically indicated by Y_m, where the index m is used to separate the *ECO* from *ENV* measures, will be regressed on a vector of input price changes, $\Delta\log(\mathbf{p})$, and additional control variables, $\boldsymbol{X}$, accounting for the non-observable heterogeneity across DMUs thanks to the panel structure of our data. The $\mathbf{p}$ vector includes the price of energy, the interest rate and the wage rate, whereas among the control variables we include measures of economic activity, such as value added. The terms u_i and ϵ_{it} are the DMU fixed effects and the error term, respectively.

In equation (2.12), the log change in prices is indexed with a subscript $t + k$, to emphasize that efficiency measures are likely to react (or have reacted) to past, current and future (expected) prices. In the innovation literature, it is common place to use either current spot prices or weighted averages of current and past prices. Intuitively, however, innovation would not be generally spurred by current prices, but rather by firms' expectations of future

[8] Taking the arithmetic mean makes more sense than the geometric mean in this case, because it retains the aggregate nature of the Russell measure, see (2.8).

costs. Arguably, firms currently facing high prices but expecting them to be low in the future will bide their time rather than investing in costly innovations. One possible way to estimate price expectations for fossil energy would be to follow Hamilton and Wu (2013) and Baumeister and Kilian (2014), both of whom use fossil fuel future contracts as a starting point. Unfortunately, natural gas and coal futures are relatively new products to the market and there's no data available for them before 1998 (Thomson Reuters, 2016). We thus assume that firms (DMUs) have rational expectations and use lead prices, instead. The use of current and lead prices, immediately raises the issue of potential endogeneity due to simultaneous causality as not only may future prices influence current behaviour, but current behaviour may in turn influence future prices. Fortunately, EIA (2006a) and EIA (2006b) suggest that the metal industry consumes only a marginal amount of energy compared to the total US consumption – less than 2%. The comparable figure for electricity is less than 2.5%, those for natural gas and coal are both around 3%. Based on this, we conclude that it is unlikely that the current behaviour of metal sectors would significantly influence current and future prices. In what follows, we estimate equation (2.12) using $k = 2$ as our preferred estimation, but we also discuss the results obtained with $k = -1$ and 0.

In a classic reference, Simar and Wilson (2007) point our that statistical inference from DEA estimates is not without pitfalls. They argue that since the productivity indices are serially correlated, standard inference approaches lose validity in this context. Intuitively, they explain, "the correlation arises in finite samples from the fact that perturbations of observations lying on the estimate frontier will in many, and perhaps all, cases cause changes in efficiencies estimated for other observations" (Simar and Wilson, 2007, p. 33). Depending on the data and the empirical application, such correlation may lead to either an over- or an underestimation of the productivity indexes and would similarly affect each of its components. The bootstrap procedure suggested by Simar and Wilson (2007) is aimed at solving this problem by repeatedly estimating the relation of interest over random subsamples and computing quantile based confidence intervals. If the number of iterations is sufficiently large, the boostrapped confidence interval can be used for inference without further corrections. Simar and Wilson (2007, p. 33) use 2,000 repetitions in their estimations of the confidence intervals, and we adopt the same approach in what follows.

2.3 Data

To estimate the *ECO* and *ENV* efficiency measures in (2.3) and (2.4), the MPIs in (2.9) and (2.11), as well as their constituents, see (2.10), we need data about input use ($\mathbf{X}$), output production ($\mathbf{Q}$) and polluting emissions ($\mathbf{Z}$). As mentioned in the introduction, in this paper we use a data-set that contains aggregate information on operations in the U.S. metal industry, and the associated polluting emissions spanning from 1990 to 2006 (with gaps; $T = 10$). Our choice is motivated by several considerations. First and foremost, the metal industry (and its subsectors) is a mature industry that has a relatively slow pace of product innovation in the short-run. This is important for us in the first instance because it makes it reasonable to estimate frontiers enveloping the DMUs and compare them over time. An industry in rapid transformation would not offer sufficient comparability over time. Moreover, given the nature of the industry, we would expect most innovation to be process-related, rather than product focussed, and we have reason to believe that such innovation reacts more to changes in energy prices than product innovation. Additionally, given our broader interest in environmental sustainability, it is crucial that the industry we investigate be both energy (and pollution) intensive, and economically significant.[9]

Our input and output data are taken from the NBER-CES Manufacturing Industry database.[10] The NBER-CES database contains yearly industry-level data on inputs of physical capital, labour, energy, and material costs, as well as output indicators such as value shipped from 1958 to 2012; as such, it is ideally suited to the application of DEA methods. All values are in current dollars, but the dataset also contains price deflators to allow the computation of value in real terms. As mentioned above, our DMUs are 4-digit Standard Industrial Classification (SIC) sub-industries.

The pollution data come, instead, from the U.S. Environmental Protection Agency's National Emissions Inventory (NEI).[11] The National Emissions Inventory (NEI) is "a comprehensive and detailed estimate of air emissions of criteria pollutants, criteria precursors, and hazardous air pollutants from air emissions sources", it is based primarily upon data provided by State, Local, and Tribal air agencies for sources in their jurisdictions and supplemented by data developed by the US EPA. The NEI contains source-level pollution data from 1990 to 2014, albeit with gaps as it is currently released every three years. This limits our data between 1990 and 2006 with $T = 10$. It tracks several pollutants, but the most detailed data refer to the so-called criteria pollutants: carbon monoxide (CO), nitrogen oxides (NO_x), sulfur dioxide (SO_2), particulate matter smaller than 10 and 2.5 μm (PM_{10} and $PM_{2.5}$, respectively), as well as volatile organic compounds (VOCs). The firms in the sample change every year so, in order to reduce biases, we average pollution by 4-digit SIC sub-industries.[12]

[9] The U.S. Geological Survey's Yearbooks contain a wealth of information on U.S. mineral industries. We refer the interested reader to their website for additional information: www.usgs.gov.

[10] See http://www.nber.org/nberces/ for details. Last accessed October 30, 2017.

[11] See https://www.epa.gov/air-emissions-inventories/national-emissions-inventory-nei for details. Last accessed October 27, 2017.

[12] The NEI classified firms according to SIC until 2008 and subsequently changed to the North American Industry Classification System (NAICS). To the best of our knowledge, there exists no direct concordance table between SIC and NAICS beyond the 2002 version. To make data comparable across vintages, then, a two step procedure is needed, in which first SIC codes are mapped into 2002 NAICS using the Bureau of Economic Analysis' tables, and subsequently they are converted into 2007 NAICS codes.

The top part of Table 2.1 presents the summary statistics for the variables used for the DEA estimations. The inputs are the real capital stock, the real energy and materials costs (all measured in millions of 1997 US$) as well as the production worker hours, in millions. Output is proxied by the real value shipped, also in millions of 1997 US$. The industries present sufficient variability for the purposes of our analysis but are relatively close in both size and input ratios, confirming that DEA is a reasonable methodology to apply in this context.

The remaining variables in the top panel of Table 2.1 are pollutants emitted in the course of production. As shown in Table 2.5, most sectors in our sample are linked to the steel industry, which we presently discuss in some more detail to explain the origin of the pollutants. Generally speaking, the general process necessary to produce steel from iron ore or scrap is relatively straightforward. Impurities such as nitrogen, silicon, phosphorus, sulfur and excess carbon need to be removed from the raw iron, whereas so-called ‘alloying elements’ such as manganese, nickel, chromium and vanadium are added to produce different grades of steel. Today, two main large-scale commercial methods co-exist for making steel: basic oxygen steel-making and electric arc furnace (EAF) steel-making. In basic oxygen steel-making carbon-rich molten pig iron is made into steel by blowing oxygen through it, which lowers the carbon content of the alloy and changes it into steel. This method initially requires the heating up and crushing of limestone in a sinter plant. Both coke and natural gas are used to generate heat, which makes sintering a pollution-intensive process entailing the emission of CO, VOC, PM_{10}, SO_2 and NO_x (EPA 2001, p. 28; Raczynsky and Watson 1999, p. 327). In the second step of the process the sintered limestone, iron ore and coke are fed into a blast furnace to produce pig iron, which is then poured into the basic oxygen furnace, often with scrap metal to obtain molten steel. This process is also a pollution-intensive and emits PMs, SO_2, CO and NO_x (Sell, 1992). Alternatively, the use of EAFs entails the of use scrap metal to make molten steel using electricity instead of natural gas and coke to heat the metal. EAFs directly emit negligible pollution (Strezov et al., 2013). According to the American Iron and Steel Institute around 60% percent of the plants use EAF and 40% uses basic oxygen steel-making methods (American Iron and Steel Institute, 2016). Although each plant typically only has one type of furnace, the data is compiled at the sub-industry level, so that substitution between the two methods is possible. In the last step, all firms purify the molten steel and make the castings. In the aluminium industry, firms first crystallise bauxite to aluminium oxide using natural gas. Then the aluminium oxide is heated up to 150°C and becomes aluminium in an electrolyte chamber. Natural gas and electricity, together make up more than 90% of the total energy consumption in the industry (EIA, 2006b). Aluminium smelting implies the emission of PM_{10}, $PM_{2.5}$, SO_2, CO, NO_x, as well as VOC.

The secondary metal industry is similarly uniform in terms of the general production process. Secondary metal firms buy primary metal plates or tubes manufactured in the primary industry and perform operations such as de-oxidation, vacuum degassing, alloy addition, inclusion removal, inclusion chemistry modification, de-sulphurisation and homogenisation to refine the metal, and produce different alloys and grades. Subsequently, the metal is worked with rollers, blankers, and welders to shape their final products. Table 2.7 compares the primary and secondary metal industry and shows that the two groups of sub-sectors are relatively different, the secondary industry being larger. The table also shows, however, that the overall emissions of pollutants are instead rather similar across the groups. To control for this we will include a secondary industry dummy variable that assumes the value of one for all DMUs in the secondary industry and zero otherwise. Our regressions will include CO, NO_x and VOC because of data availability.[13]

Table 2.1: Summary statistics

First stage: Data envelopment analysis					
Variable	**Mean**	**Std. Dev.**	**Min.**	**Max.**	**N**
Real capital stock (MUS$)	2119.56	1091.96	705.5	4259.1	81
Production worker hours (Mh)	72.81	38.8	17.1	136.1	81
Real energy costs (MUS$)	97.48	66.79	14.95	300.08	81
Real materials costs (MUS$)	3179.08	1861.52	509.03	8047.61	81
Real value shipped (MUS$)	5615.23	3161.63	1311.45	13142.61	81
CO (tons)	3.65	6.22	0.09	31.75	81
NO_x (tons)	6.22	6.29	0.45	27.43	81
SO_2 (tons)	97.27	400.82	0	1991.47	70
VOC (tons)	34.09	24.42	1.59	97.75	81
NH_3 (tons)	0.76	1.01	0.01	2.88	27
PM10 (tons)	6.26	6.45	0.02	21.17	35
PM2.5 (tons)	4.64	4.96	0.02	17.42	35
Second stage: panel estimation					
Energy price	1.26	0.14	1.05	1.72	81
Natural gas price (US$/ft^3)	4.27	1.68	2.93	8.56	81
Electricity price (US¢/kWh)	4.79	0.38	4.43	5.73	81
Coal price (US$/short ton)	26.55	3.8	23.92	36.8	81
Interest rate (%)	5.54	1.54	2.88	7.19	81
Average labour cost (,000s US$/employee)	26.23	4.39	16.95	38.4	81
Value added (MUS$)	3604.09	1977.95	1033.5	10036.8	81

Source: Author's calculations on EPA NEI and NBER CES Manufacturing Database data.
Notes: 1990-2006 (with $T = 10$) and the decision making units are the subsectors of the metal industry.

[13] As we can see from the means and standard deviations in Table 2.1, there are a number of outliers. Since DEA is outlier-sensitive, we use the blocked adaptive computationally efficient outlier nominators (BACON) method to identify and eliminate such observations (Billor et al., 2000; Weber, 2010).

The bottom part of Table 2.1 sums up the variables used in the second-stage, where we use input prices to explain the productivity indices computed in the first stage. The interest rate and the labour costs measures are used as proxies for the price of capital and labour, respectively. The labour cost variable is created by dividing aggregate (production-related) labour costs by the number of employees in the NBER CES database. The energy price is the energy deflator on each industry's expenditures on seven types of energy as provided in the NBER-CES database. Given the broad coverage and its (dis-)aggregation level, this variable provides a comprehensive, DMU-level assessment of the price of energy, which we use in our baseline regressions. To be able to gauge the impact of the price of individual fuel types, however, we additionally resort to the data provided by the Energy Information Administration on fuel prices faced by industrial consumers. We choose to use coal, natural gas and electricity prices, because they account for over 70% of the total energy consumption of metal firms.[14] The price of coal refers to bituminous coal, which is the most commonly used coal type in the metal industry. Energy prices tend to be highly correlated across fuel types and over time, which makes the statistical identification of their relationship with the outcome variable a very challenging proposition. In what follows, to facilitate our analysis, we focus on the growth rates of prices over time rather than their levels nor do we include lags and leads of prices within any given regression to limit the impact of serial correlation on our results. Finally, Table 2.5 also reports the number of observations by pollutant, for each SIC code. In what follow, we only consider SIC codes with at least 9 data points, resulting in a maximum of 81 observations in our analysis below.

2.4 Results

2.4.1 Data Envelopment Analysis

As discussed in Section 2.2.1, the MPI measures the changes in productivity over time experienced across different DMUs. For example the large improvement from 2001 to 2002, seven out of the the 9 sectors had a large improvement $\widetilde{\mathscr{M}}_{ENV}$, so one can't say that the results are driven by a single sector. $\widetilde{\mathscr{M}}_{ECO}$ moves slower than $\widetilde{\mathscr{M}}_{ENV}$ and they same to move together relatively well (there are large exceptions for example, SIC 3354 in 1998 had a productivity regress in ECO but a large productivity gain in ENV). To get a sense of the main trends, Figure 2.3a graphs the evolution of the two productivity indexes over time. The lines plot the chained average MPIs, computed by first averaging MPIs across DMUs in each period, and then multiplying them over subsequent years. For example, the cumulative

[14]The remaining 30% mostly comes from byproducts of the manufacturing process, for which no market price is available (EIA, 2002).

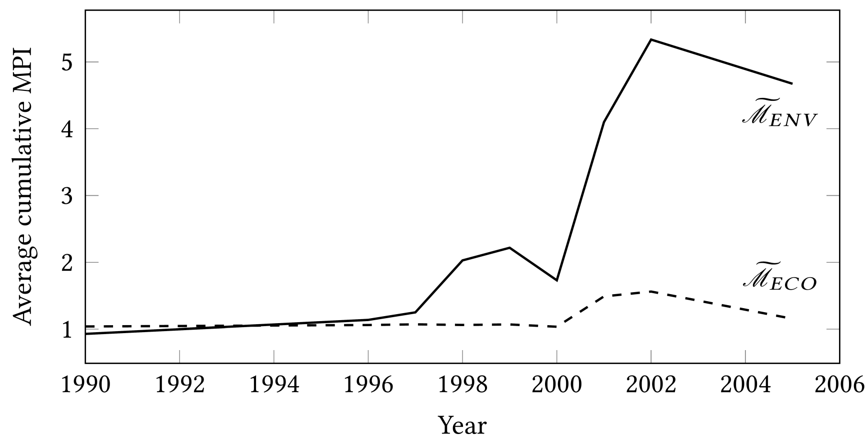
economic index from time t_0 until time T would be calculated as:

$$\widetilde{\mathscr{M}}_{ECO} = \prod_{t=t_0}^{T} \left(\frac{1}{N} \sum_{i=1}^{N} \mathscr{M}_{i,t} \right). \tag{2.13}$$

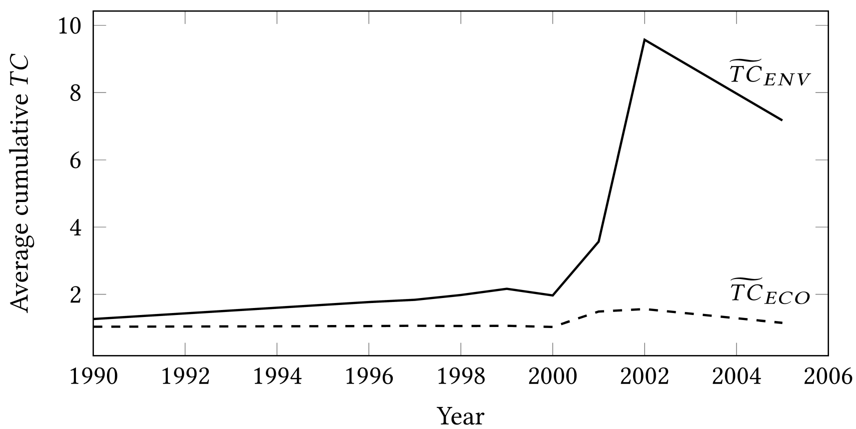
The environmental MPI is calculated in a similar fashion, using the aggregate $\mathscr{M}_{ENV}$ in (2.11). The advantage of estimating these cumulative indices is to get an intuition of the first stage and how do the individual productivity changes accumulate. This also allows us to plot against the evolution of energy prices, which helps the intuition of our results better than plotting the productivity changes against the energy price changes.

It is evident even from a cursory inspection of Figure 2.3a that environmental productivity grows much faster than economic productivity over the period spanned by our data. This suggests that the productivity of polluting inputs has, on average, grown faster than the productivity of 'standard' ones between 1990 and 2005, across the sub-industries we analyze. This provides the first suggestive evidence that technical change has been biased towards polluting inputs in this instance. Interestingly, the graphs show evidence of co-movement overall. Indeed, both graphs evolve quite closely until 1997; though we don't observe the years between 1990 and 1996, it is telling that the productivity increase between 1990 and 1996 is similar. In 1997 environmental productivity increases more rapidly until 1999 and then very markedly so over the subsequent two years, with a slight decline after 2002. The economic MPI is more sluggish until 2000. It then increases, albeit less dramatically than its environmental counterpart, exhibit a similar, if more muted, pattern of growth and subsequent decline over 2001-2005.

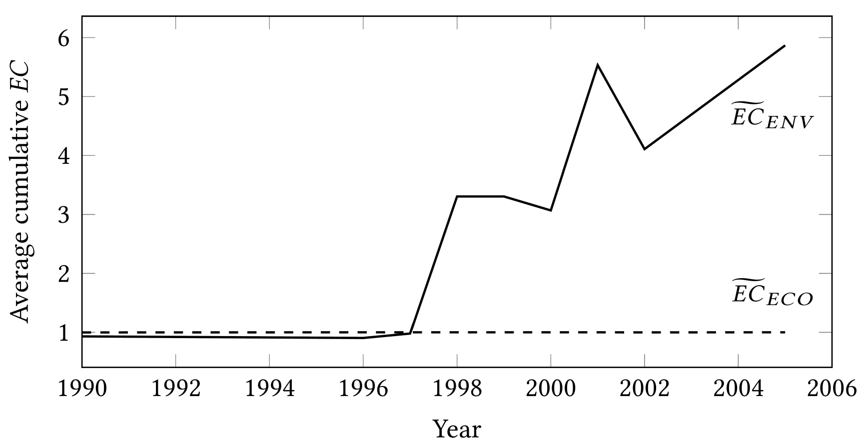
The possibility to decompose the MPIs into their efficiency and technological change constituents allows us to analyze in more depth the drivers of these aggregate patterns over time. Starting from Figure 2.3b, which describes the technological change component of the MPIs, it is striking how the environmental index has increased tenfold, peaking in 2002, whereas its economic counterpart shows a much more muted dynamics, while still providing evidence of some progress over time (at least until 2002). Figure 2.3c, which plots the evolution over time of the cumulative (chained) changes in efficiency, illustrates how there was virtually no change in the overall economic efficiency over the entire period of our study, implying that the relative position of the DMUs in relation to the economic frontier has not changed. In marked contrast, the DMUs under investigation show on average a rapid increase in environmental efficiency over the same period of time.



(a) Economic (dashed) and environmental (solid) cumulative MPIs (correlation: 0.83).



(b) Economic (dashed) and environmental (solid) cumulative technical change indexes (correlation: 0.73).



(c) Economic (dashed) and environmental (solid) cumulative efficiency change indexes (correlation: 0.34).

Figure 2.3: The Malmquist Productivity index and its components over time.

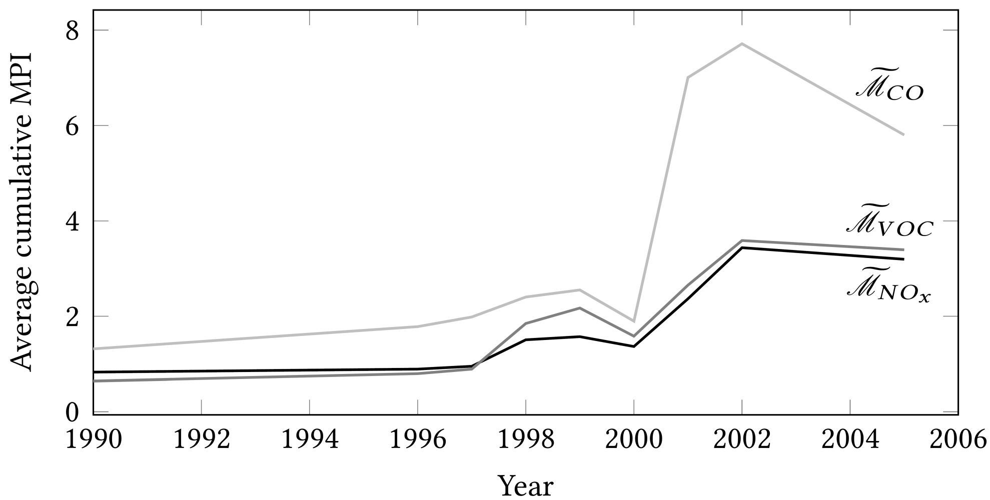


Figure 2.4: Cumulative technical change indexes for individual pollutants.

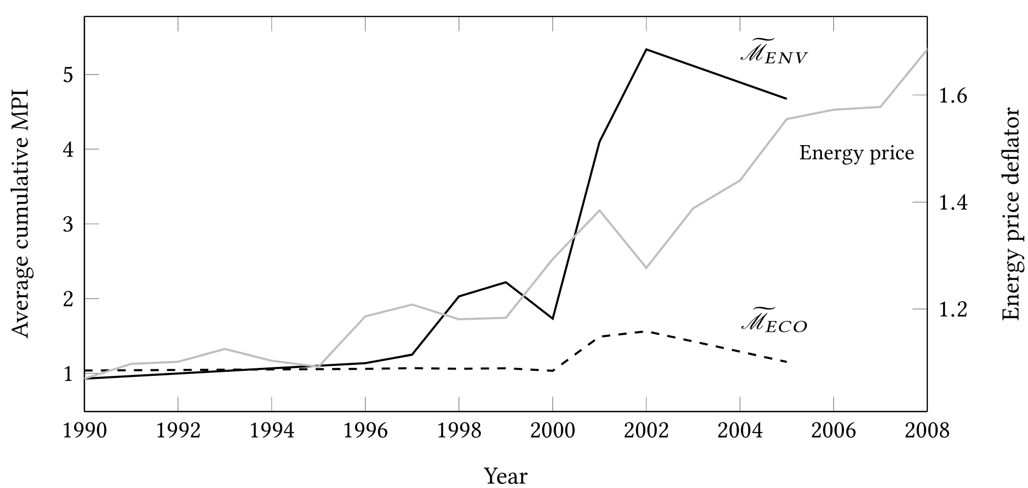


Figure 2.5: Productivity and the average cost of energy.

Taken in its entirety, this information leads us to conclude that the U.S. metal industry has witnessed a surprising degree of (biased) environmental innovation during the period of our study, and that such innovation has been the result of two types of changes. On the one hand, there has been a substantial amount of technological change that has improved the productivity of the front-runners in the industry. Indeed, the rapid growth in $\widetilde{TC}_{ENV}$ suggests significant shifts outward of the best-practice frontier in the industry. On the other hand, the parallel improvement captured by the time path of the $\widetilde{EC}_{ENV}$ component testifies of an industry in which even less efficient DMUs increasingly adopt best-practices from the technology leaders and approach the frontier. Our analysis so far, therefore, suggests that both *bona fide* innovation and the diffusion of existing technologies have played a role in the environmental improvements observed in the industry around the turn of the century.

As discussed in Section 4.4, due to data availability, the environmental index is constructed using information on the emissions of just three of the criteria pollutants: CO, NO_x, and VOCs. Looking at the evolution of each of the individual components of the MPI in (2.11) is nevertheless instructive in order to understand what drives the rapid increase of $\widetilde{\mathscr{M}}_{ENV}$. Figure 2.4 plots the evolution over time of the average MPI for each of the pollutants. The figure shows clearly that, while the productivity of each pollutant exhibits a qualitatively similar path, in that the different indicators tend to move together, the graph of $\widetilde{\mathscr{M}}_{CO}$ exhibits significantly faster productivity gains over the period of our analysis. Significantly, this improvement took place in the absence of any change in the rules regulating carbon emissions.

Table 2.8 reports the results of our year-on-year MPI estimation for each of the metal manufacturing sub-industries (4-digit SIC codes) in our data set. The top panel of the table refers to the economic MPI detailed in (2.9), whereas the bottom half contains the estimates for the environmental MPI, computed according to the expression in (2.11). The timing convention is important here and in what follows; in our results the value for $\widetilde{\mathscr{M}}_{ECO,t}$ indicates the productivity increase that occurs between time t and the following period in which observations are available. Thus, for example $\widetilde{\mathscr{M}}_{ECO,1999}$ refers to the productivity growth between the years 1999 and 2000, but $\widetilde{\mathscr{M}}_{ECO,2002}$ measures the change between 2002 and 2005. It's interesting to observe that there were generally little changes from 1990 to 1996 (under the column '1990'). Since we can't observe the years in between we assume that the change is more or less linear (i.e. there wasn't a productivity surge from 1990 to 1993 and productivity decline 1993 to 1996). There's heterogeneity across the sector responses which could be caused by a number of different factors (e.g. technological maturity, asymmetries in energy use or differences in vertical integration), but the main trends are more or less the same. Table 2.9 and 2.10 report the $\hat{EC}$ and $\hat{TC}$ components of the MPI. It seems that while the technological component moves more or less simultaneously across DMUs, the efficiency change component (especially improvement) is mainly driven by individual DMUs.

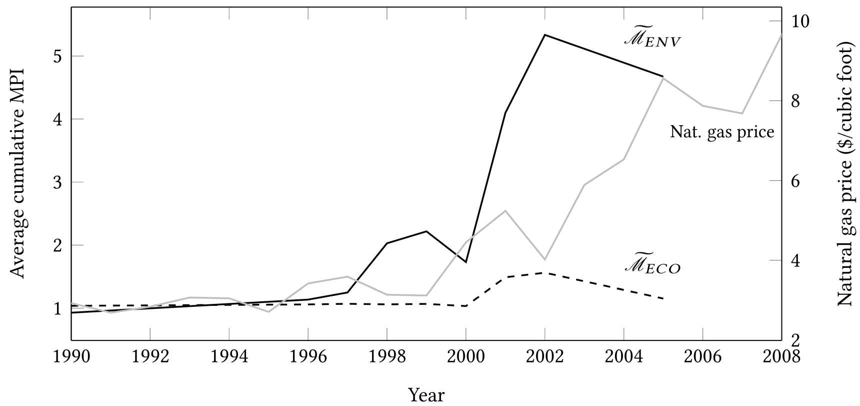
So it seems that given the technological frontier of the DMUS their distance stays the same. This could imply that either there's a constant lag between the frontier and the DMU or the DMU is simply always efficient. To investigate the issue Figure 2.7a and 2.7b illustrates the distribution of efficiency scores. It seems that economic terms the DMUs are very efficient; this implies that market encourages efficiency. In general the DMUs also efficient environmentally, but there's a much larger spread of the efficiency scores. This could also explain the large individual changes in efficiency. It seems that, in general, the production frontier shift improves productivity.

A proper analysis of the drivers of these productivity increases is very important since it is likely to provide crucial information to environmental regulators. Therefore, we devote the rest of the paper to this aspect.

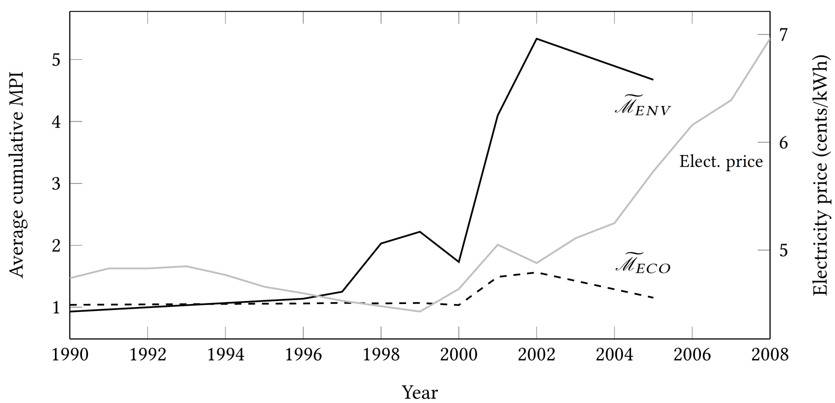
In line with the induced innovation hypothesis, we focus on the role played by input prices in driving the changes in the MPIs discussed above. Figure 2.5 illustrates the relationship between the aggregate MPIs and the (average) price of energy in the industry, as measured by the energy deflator provided in the NBER-CES dataset. Transparently, a close positive correlation emerges from the picture, whereby increases in the price of energy are linked to (mostly environmental) productivity improvements. Not surprisingly, a similar pattern emerges when plotting the MPIs against the price of natural gas, electricity and coal, as shown in Figures 2.6a-2.6c. While this conforms with our intuition, it is clear that a more precise statistical investigation is warranted to gauge to what extent the data really support the induced innovation hypothesis.

2.4.2 Second stage estimation

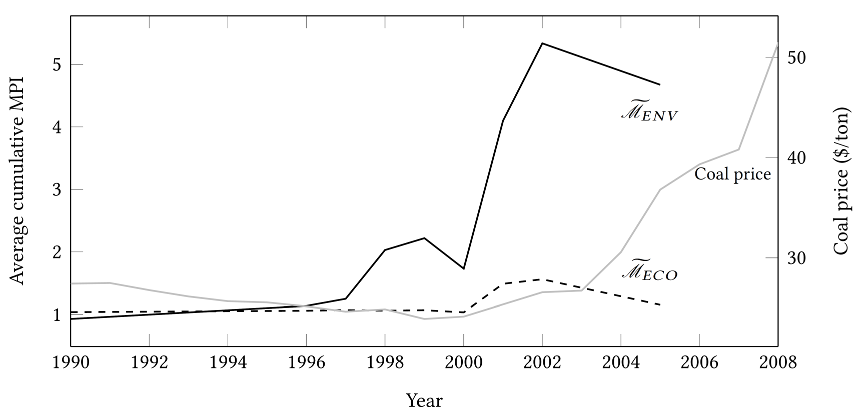
To investigate the relationship between input prices and productivity, we estimate equation (2.12) using the method proposed by Simar and Wilson (2007) and discussed in Section 2.2.2. To exploit the panel variation in the data, our preferred strategy is to estimate the equation using the 4-digit industry specific energy price deflators provided with the NBER-CES data. We run three sets of regressions using the energy price index at time t, $t + 1$, and $t + 2$, which, given the timing convention we adopted for the MPIs, allows us to capture gauge to what extent productivity responds to lagged, contemporaneous and future changes in prices, respectively. One potential concern with this strategy, obviously, is the potential endogeneity between the price of energy and the change in productivity: as prices increase and affect productivity, the demand for energy by metal industries change as well, feeding back into prices. Since the metal industry is responsible for only a small fraction of the overall amount



(a) Natural gas price.



(b) Electricity price.



(c) Coal price.

Figure 2.6: Productivity and energy prices.

Table 2.2: Explaining Productivity Changes: $\widetilde{\mathscr{M}}$, $\widetilde{TC}$ and $\widetilde{EC}$ vs lagged energy prices

	$\widetilde{\mathscr{M}}_{ECO}$	$\widetilde{TC}_{ECO}$	$\widetilde{\mathscr{M}}_{ENV}$	$\widetilde{TC}_{ENV}$	$\widetilde{EC}_{ENV}$
Δ log(Energy deflator)$_t$	1.30	1.30	0.96	1.47	-5.83
	(0.87)	(0.94)	(1.78)	(1.43)	(8.05)
Δ log(Interest rate)$_t$	-0.58***	-0.58***	-0.96*	-1.80**	1.02
	(0.18)	(0.19)	(0.52)	(0.73)	(1.34)
Δ log(Wage)$_t$	1.08	1.08	-1.54	4.32	-8.70
	(1.02)	(1.04)	(2.62)	(3.42)	(9.59)
log(Value added)$_t$	0.01	0.01	0.19	0.14	0.45
	(0.06)	(0.06)	(0.20)	(0.29)	(0.38)
Year gap dummy	-0.04	-0.04	-0.15	0.53	-0.63
	(0.09)	(0.09)	(0.19)	(0.49)	(0.63)
Secondary metal industry (dummy)	-0.06	-0.06	-0.33**	-0.92	-1.09
	(0.11)	(0.12)	(0.15)	(0.58)	(0.79)
Constant	0.88*	0.88**	-0.01	0.47	-0.72
	(0.47)	(0.45)	(1.57)	(2.32)	(2.96)
Observations	81	81	81	81	81
R^2	0.23	0.23	0.11	0.23	0.07
χ^2	27.93	27.50	39.94	21.10	7.74

Notes: *, **, *** indicate 10%, 5% and 1% statistical significance, respectively. Time period for all regressions is 1990-2006 (with 1991, 1992, 1993, 1994, 1995, 2003 and 2004 missing). Thus $t+k$ refers to the end year $+k$ years. For more information about the timing convention see section 2.4.1. Robust standard errors in parentheses. The significance levels are computed by applying Simar and Wilson's 2007 algorithm A with 2000 repetitions.

of energy consumed every year in the United States (Only between 2 and 3%, according to EIA, 2006a,b), however, it is unlikely that the behaviour of our DMUs has had a significant impact on prices.

Tables 2.2-2.4 present the results of this first estimations. In each table, each column refers to a different dependent variable, as indicated. The first three refer to the aggregate economic MPI and its components, whereas the last three refer to their ENV counterparts. As discussed previously, the prices of the polluting inputs constitute the main variable of interest here, whereas the other variables are used to control for the price of other factors, namely the interest rate for the cost of capital, and the average wage rate as a proxy of the cost of labour. The value added control is included to account for changes in the level of economic activity, and thus of aggregate demand. We also include a dummy variable that identifies observations for which the gap to the following available observation is larger than one year and another dummy that allows us to isolate primary metal industries from secondary ones.

Overall, the results suggest both the existence of a complex dynamic process of adjustment

Table 2.3: Explaining Productivity Changes: $\widetilde{\mathscr{M}}$, $\widetilde{TC}$ and $\widetilde{EC}$ vs energy price expectations

	$\widetilde{\mathscr{M}}_{ECO}$	$\widetilde{TC}_{ECO}$	$\widetilde{\mathscr{M}}_{ENV}$	$\widetilde{TC}_{ENV}$	$\widetilde{EC}_{ENV}$
Δ log(Energy deflator)$_{t+2}$	0.99**	1.00***	5.30**	4.21**	5.36
	(0.46)	(0.39)	(2.45)	(1.82)	(4.14)
Δ log(Interest rate)$_{t+2}$	-0.33	-0.34	0.02	-0.64	1.80
	(0.25)	(0.24)	(0.55)	(0.47)	(1.73)
Δ log(Wage)$_{t+2}$	0.93	0.93	2.36*	3.97	0.58
	(0.90)	(0.79)	(1.36)	(2.61)	(4.78)
log(Value added)$_{t+2}$	-0.02	-0.02	0.10	0.08	0.43
	(0.10)	(0.09)	(0.19)	(0.18)	(0.47)
Year gap dummy	-0.10	-0.09	-0.24*	0.47	-0.48
	(0.08)	(0.07)	(0.13)	(0.49)	(0.33)
Secondary metal industry (dummy)	-0.03	-0.03	-0.30*	-0.87	-1.19
	(0.13)	(0.13)	(0.16)	(0.55)	(0.81)
Constant	1.15	1.15*	0.55	0.89	-0.79
	(0.71)	(0.69)	(1.47)	(1.48)	(3.47)
Observations	81	81	81	81	81
R^2	0.13	0.14	0.14	0.15	0.07
χ^2	19.12	23.07	22.40	19.59	8.03

Notes: *, **, *** indicate 10%, 5% and 1% statistical significance, respectively. Time period for all regressions is 1990-2006 (with 1991, 1992, 1993, 1994, 1995, 2003 and 2004 missing). Thus $t + k$ refers to the end year $+k$ years. For more information about the timing convention see section 2.4.1. Robust standard errors in parentheses. The significance levels are computed by applying Simar and Wilson's 2007 algorithm A with 2000 repetitions.

to changes in prices over time, and a significant role played by price expectations. Starting with the role of current prices in Table 2.4, what emerges is a picture whereby economic productivity is negatively affected by increases in costs in the short run. The first column of Table 2.4 indeed suggests that as the price of energy and the cost of capital increase, the MPI decreases. The fact that the whole effect on $\widetilde{\mathscr{M}}_{ECO}$ come from changes in the TC component further suggests an overall pressure on the industry rather than significant changes in the relative performance of DMUs. The statistically significant and negative coefficient of energy prices in the environmental MPI equation might be due to a relative shift towards cheaper fuels, mostly coal, as a consequence of energy cost pressure. Since coal is also associated with the largest carbon-intensity, this seems like a plausible explanation.

Table 2.2, which reports the results of the estimation of equation (2.12) using lagged prices, shows, however, that these effects are short-lived. Indeed while the precision of the estimates leaves a lot to be desired, the positive coefficients in the first row suggest that once DMUs have time to adjust, price increases are associated with productivity gains. These results also chime with what we consider to be our main results in this part of the paper, namely the estimates in Table 2.3. We interpret the sizeable and statistically significant coefficients in the first row of Table 2.3 as indicating that an expectation that the price of energy is likely to increase leads to production adjustments, that in turn imply a more efficient use of the energy inputs. These changes result in an increase in the economic productivity driven by a shift in the best practice frontier, and are associated with similar, and much larger increases on the environmental side, especially in the primary metal industry. The fact that the metal industry is a mature sector and that product innovation occurs only at a limited pace suggests that the environmental improvements identified above are linked to process innovation. This is most likely connected to the choice of fuel, e.g. black vs brown coal vs gas for heat generation, and to a progressive shift away from blast furnaces and towards EAFs.

This latter aspect can be better gauged by moving away from a single price for energy and instead focusing on individual prices for the alternative energy sources available to the industry. Including prices for different energy sources allows to emphasize the substitution possibilities available to firms within sectors and across the industry. The available data, unfortunately, suffer from the obvious limitation of not having a panel structure, so that the same price is imputed for all DMUs. Data limitations notwithstanding, the results reported in Table 2.11 and 2.12 offer interesting insights. In both regressions, we include the (growth rates of) the price of coal, natural gas and electricity. The first two sources are mostly substitutable in terms of process heat auto-production, whereas electricity is only indirectly substitutable, as it allows metal production based on EAFs rather than on blast furnaces.

Tables 2.11 and 2.12 tell quite similar stories: an increase in the price of either coal or natural gas tends to make EAFs relative more competitive and shifts production away from pollution

Table 2.4: Explaining Productivity Changes: $\widetilde{\mathscr{M}}$, $\widetilde{TC}$ and $\widetilde{EC}$ vs current energy prices

	$\widetilde{\mathscr{M}}_{ECO}$	$\widetilde{TC}_{ECO}$	$\widetilde{\mathscr{M}}_{ENV}$	$\widetilde{TC}_{ENV}$	$\widetilde{EC}_{ENV}$
Δ log(Energy deflator)$_{t+1}$	-1.46**	-1.46**	-5.28**	-1.33	-5.19
	(0.58)	(0.62)	(2.39)	(1.57)	(3.34)
Δ log(Interest rate)$_{t+1}$	-0.41***	-0.41***	-0.68**	-1.32**	0.06
	(0.10)	(0.11)	(0.30)	(0.58)	(0.43)
Δ log(Wage)$_{t+1}$	0.33	0.32	3.25	-0.20	3.52
	(0.94)	(0.98)	(3.37)	(1.70)	(3.35)
log(Value added)$_{t+1}$	0.01	0.01	0.20	0.13	0.52
	(0.08)	(0.08)	(0.20)	(0.31)	(0.46)
Year gap dummy	-0.06	-0.05	-0.11	0.51	-0.31
	(0.06)	(0.07)	(0.15)	(0.47)	(0.35)
Secondary metal industry (dummy)	-0.03	-0.03	-0.30	-0.87	-1.18
	(0.11)	(0.12)	(0.20)	(0.57)	(0.84)
Constant	1.01*	1.01*	-0.18	0.70	-1.83
	(0.57)	(0.55)	(1.58)	(2.56)	(3.47)
Observations	81	81	81	81	81
R^2	0.19	0.19	0.19	0.16	0.06
χ^2	21.07	20.58	17.31	40.77	6.73

Notes: *, **, *** indicate 10%, 5% and 1% statistical significance, respectively. Time period for all regressions is 1990-2006 (with 1991, 1992, 1993, 1994, 1995, 2003 and 2004 missing). Thus $t+k$ refers to the end year $+k$ years. For more information about the timing convention see section 2.4.1. Robust standard errors in parentheses. The significance levels are computed by applying Simar and Wilson's 2007 algorithm A with 2000 repetitions.

intensive blast furnaces, thus increasing environmental productivity, especially in the sense of shifting the best practice frontier upwards, as can be seen from the $\widetilde{TC}_{ENV}$ column in both tables. Symmetrically, an increase in the relative cost of electricity favours more polluting processes and reduces environmental productivity across DMUs. Interestingly, this process also affects economic outcomes, in particular when changes are related to current rather than expected prices.

Our results overall comply with our intuition and point to three main conclusions. First, we find evidence of induced innovation as our regressions indicate that DMUs respond to changes in expected prices by planning ahead, and improving their productivity, both in terms of economic and environmental performance. The environmental improvements most refer to changes in the TC component of the MPIs, further suggesting that indeed we find ourselves in the presence of *bona fide* innovation. This result is interesting and seemingly robust. Indeed, Table 2.13 shows that, even excluding the years for which we do not have consecutive observations and which could bias our results, we observe a similar pattern to the one that emerges from the discussion above. While not surprising from the econometric point of view – indeed the dummy we introduced to control for the gap in the data was virtually never statistically significant– this result is still comforting in terms of the robustness of our results. Similarly, we re-estimate our regression in Table 2.3 separately for primary and secondary sectors, to control for differences in their core activities. Tables 2.14 and 2.15 present the results of these estimations and show that broadly speaking the results are robust to this additional split, despite the lack of statistical significance in the primary producers regressions, possibly a consequence of the very limited number of observations.

The second lesson that we can draw is that, when we regress MPIs on current price changes, i.e. when we look at price changes that happen during the period of the productivity measurement, adjustment is more challenging, the economic productivity suffers and the environmental performance of the DMUs worsens. These impacts tend to fade over time, however, as evidenced by the fact that lagged prices do not exhibit any significant correlation with the productivity of DMUs across the industries in our sample.

Finally, when looking at changes in the individual prices of different energy sources, we find, not surprisingly, that the environmental performance of the DMUs crucially depend on the relative price of fossil fuels vs the price of electricity. As fossil fuels prices increase, producers making use of EAFs and those who rely on electricity directly become more competitive gaining market share, so that output increases and emissions (in the industry) decrease. This is not *per se* an indication of induced innovation, but provides evidence that the purposeful modification of relative prices – via policy interventions, for example – may significantly affect the environmental productivity of manufacturing sectors.

2.5 Conclusions

In this paper we set out to test Hicks's (1932) induced innovation hypothesis; that increases in the price of productive inputs would lead innovators to develope new ways to economize on their use. The existing literature on the issue relies on self-reported R&D expenditures or, more commonly, on patents counts as proxies for innovation efforts. In our view, however, (induced) innovation is a much more complex process than the mere development of new types of blueprints or patents. It is rather a function of how (existing or new) technologies are applied in practice via changes in processes, management practices and novel applications of tacit knowledge. It follows that the sole availability of the technology represents at best a necessary condition, but it is clearly far from sufficient.

In our analysis we have used methods from productivity analysis that exploit available data on the actual behaviour of DMUs – such as revenue and cost data – to estimate changes in efficiency and productivity over time and across industries. In the second stage of our work we further investigated the link between changes in productivity, efficiency and technology and the evolution of input prices over time. We see this methodology as been more appropriate to the question at hand than either of the alternatives mentioned before and, thus, more suited to investigate whether changes in energy prices do in fact lead to efficiency gains.

The results from our DEA analysis of the U.S. metal industry provide convincing evidence that productivity changes in the industry were indeed significantly biased towards polluting inputs between 1990 and 2008. Furthermore, the MPI decomposition emphasizes that both *bona fide* innovation and technology diffusion contributed to this pattern.

In the second part of our work, we correlate these changes in productivity with the evolution over time of the relevant energy prices. We find that DMUs do indeed respond to expected changes in energy prices improving both their economic and environmental performance. Furthermore, the environmental improvements we identify almost invariably emerge as the result of changes in the technical change component of the MPIs, suggesting that DMUs consistently develop and apply technologies that push the environmental frontier outwards. These results are in line with the findings of Popp (2002) and Ley et al. (2016), for example. While these papers refer to patenting activity, however, our methodology uses actual costs and revenue data. In this respect, our analysis uncovers evidence that innovation efforts do have real effects. Whether these are causally correlated to the increasing availability of patents, however, is not something our analysis can address. The discussion of these aspects is left for future research.

The results of our analysis in this paper indicate that the induced innovation hypothesis is strongly supported by the data. We find evidence that both the improvement of current best practices and the adoption of existing ones by relatively less productive DMUs are sensitive to changes in relative prices. Our results may thus be seen as a strong endorsement of policy interventions that pursue the purposeful modification of relative prices, e.g. via market-based instruments. Indeed, our work suggests that such interventions could be quite successful in bringing about market conditions that are conducive to a more efficient use of polluting inputs.

Appendix to Chapter 2

Table 2.5: Industry list by SIC code and number of observations by pollutant

Name	SIC code	CO	NOx	VOC	PM10	PM2.5	SO2	NH3
Steel Wiredrawing and Steel Nails and Spikes	3315	10	10	10	8	7	9	3
Steel Pipe and Tubes	3317	7	7	10	8	7	7	6
Gray and Ductile Iron Foundries	3321	10	10	10	8	7	10	6
Steel Investment Foundries	3324	10	10	10	8	7	10	3
Steel Foundries, Not Elsewhere Classified	3325	10	10	10	8	7	10	6
Secondary Smelting and Refining of Nonferrous Metals	3341	10	10	10	8	7	10	7
Rolling, Drawing, and Extruding of Copper	3351	9	7	10	8	7	6	6
Aluminium Sheet, Plate, and Foil	3353	9	10	10	8	7	9	8
Aluminium Extruded Products	3354	10	10	10	8	7	7	6
Metal cans	3411	10	10	10	8	7	10	6
Metal barrels, drums and pails	3412	10	7	10	6	5	7	3
Hand and edge tools, not elsewhere classified	3423	6	9	10	6	5	6	3
Hardware, not elsewhere classified	3429	7	7	10	6	5	7	6
Plumbing fixture fittings and transformation	3432	10	10	10	8	7	10	3
Heating equipment, except electric	3433	9	9	10	8	7	9	3
Fabricated structural metal	3441	10	10	10	8	7	10	3
Metal doors, sash and trim	3442	10	10	10	8	7	6	3
Fabricated plate work	3443	7	7	10	8	7	7	7
Sheet metalwork	3444	7	7	10	6	5	7	6
Architectural metal work	3446	9	9	10	8	7	6	3
Prefabricated metal buildings	3448	10	10	10	7	6	9	3
Miscellaneous metal work	3449	10	7	10	5	4	6	3
Metal stampings, not elsewhere classified	3469	7	10	10	8	7	7	6
Plating and polishing	3471	7	7	10	6	5	7	6
Metal coating and allied services	3479	10	10	10	8	7	10	6
Ammunition, exc. small arms, n. e. c.	3483	6	9	9	5	4	6	6
Valve and pipe fittings, not elsewhere classified	3494	9	9	10	7	6	6	6
Misc. fabricated wire products	3496	10	10	10	8	7	7	3
Fabricated metal products, not elsewhere classified	3499	10	10	10	8	7	10	6

Source: EPA NEI, NBER CES Manufacturing database and U.S. Census Bureau's Subject Series General Summary (Appendix C).

Table 2.6: Variable mean and standard errors by SIC codes.

SIC code	Real value shipped	Real capital	Real material costs	Real energy costs	Production hours	Δln(Energy deflator)	Δln(Wage)	ln(Value added)
3324	1293.19	660.47	427.07	49.24	25.42	0.03	0.04	6.26
	(399.84)	(172.61)	(183.19)	(27.89)	(6.57)	(0.07)	(0.05)	(1.10)
3354	3575.07	2320.87	2566.70	115.22	42.52	0.03	0.04	6.79
	(868.46)	(297.38)	(610.07)	(32.28)	(7.27)	(0.08)	(0.04)	(0.86)
3432	1945.93	630.36	981.17	19.99	24.29	0.04	0.04	6.55
	(419.30)	(210.38)	(317.09)	(4.55)	(4.07)	(0.07)	(0.05)	(1.00)
3441	9814.35	3231.41	5737.60	96.26	135.65	0.04	0.04	8.03
	(2233.40)	(922.58)	(1500.98)	(22.86)	(16.71)	(0.07)	(0.03)	(0.84)
3442	5742.14	1856.60	3276.24	59.29	99.74	0.03	0.04	7.57
	(1345.35)	(662.99)	(960.13)	(14.19)	(10.02)	(0.07)	(0.03)	(0.87)
3448	2409.47	679.93	1583.95	22.99	28.66	0.04	0.04	6.57
	(952.89)	(322.67)	(699.29)	(7.65)	(8.85)	(0.08)	(0.07)	(1.14)
3479	4329.21	1569.51	2032.80	129.01	61.98	0.03	0.04	7.07
	(3112.83)	(1104.45)	(1408.77)	(93.45)	(20.26)	(0.08)	(0.05)	(1.30)
3496	2754.77	1371.57	1409.67	42.18	55.00	0.03	0.04	6.95
	(738.56)	(382.40)	(371.96)	(13.65)	(7.53)	(0.07)	(0.04)	(0.80)
3499	5884.68	2610.93	2733.35	93.52	97.62	0.04	0.04	7.62
	(2158.63)	(1036.20)	(1071.76)	(41.91)	(24.29)	(0.07)	(0.04)	(1.05)

Source: NBER CES Manufacturing database

Table 2.7: Primary and secondary metal industry: comparison of key indicators

	All	**Primary**	**Secondary**	**Difference**
Real value shipped (MUS$)	5615.23	3110.26	6330.94	-6.14
	(3161.63)	(1448.51)	(3159.18)	(0.00)
Production worker hours (Mh)	72.81	41.85	81.65	-7.33
	(38.80)	(9.15)	(39.52)	(0.00)
Real materials costs (MUS$)	3179.08	1882.07	3549.65	-4.35
	(1861.52)	(1294.64)	(1839.51)	(0.00)
Real energy costs (MUS$)	97.48	107.12	94.73	0.90
	(66.79)	(43.45)	(72.14)	(0.37)
CO (tons)	3.65	3.69	3.64	0.05
	(6.22)	(2.83)	(6.90)	(0.96)
VOC (tons)	34.09	39.61	32.52	0.85
	(24.42)	(33.57)	(21.19)	(0.41)
NO_x (tons)	6.22	9.92	5.16	2.20
	(6.29)	(8.81)	(4.96)	(0.04)
SO_2 (tons)	97.27	392.11	16.86	1.79
	(400.82)	(810.34)	(65.60)	(0.09)
NH_3 (tons)	0.76	1.14	0.62	1.04
	(1.01)	(1.21)	(0.93)	(0.33)
PM_{10} (tons)	6.26	9.58	5.27	1.43
	(6.45)	(7.93)	(5.76)	(0.19)
$PM_{2.5}$ (tons)	4.64	7.73	3.73	1.60
	(4.96)	(6.70)	(4.03)	(0.14)

Source: Author's calculations on EPA NEI and NBER CES Manufacturing Database data.

Table 2.8: Productivity Estimates

$\hat{\mathscr{M}}_{ECO,it}$									
SIC code	1990	1996	1997	1998	1999	2000	2001	2002	2005
3324	0.787	0.986	1.043	2.073	1.014	0.978	1.338	1.593	0.686
3354	0.808	1.029	0.981	0.604	0.948	1.019	1.575	0.974	0.583
3432	1.072	0.911	1.043	0.589	0.992	0.982	1.911	0.551	0.292
3441	1.691	1.098	0.995	0.657	1.025	0.971	1.245	1.197	0.454
3442	1.046	1.097	0.979	1.798	1.046	1.003	1.060	0.889	1.182
3448	1.589	0.982	1.012	0.417	0.986	0.902	2.015	1.029	1.136
3479	1.119	0.993	1.033	0.884	1.034	0.968	1.171	1.031	1.065
3496	0.345	1.032	0.981	1.305	1.011	0.960	0.982	0.893	0.602
3499	0.885	1.064	1.016	0.599	1.004	0.927	1.663	1.281	0.647
$\hat{\mathscr{M}}_{ENV,it}$									
SIC code	1990	1996	1997	1998	1999	2000	2001	2002	2005
3324	0.498	1.022	1.093	2.182	0.957	0.899	1.089	2.995	0.705
3354	0.565	1.330	1.141	5.024	0.902	1.007	2.324	0.954	0.817
3432	1.207	0.899	1.182	0.373	1.830	0.646	1.515	0.956	0.544
3441	1.795	1.739	0.918	0.737	0.903	0.696	1.486	1.257	0.751
3442	0.698	1.418	1.287	0.937	1.072	1.021	0.908	0.851	1.161
3448	1.535	0.981	1.032	1.035	0.978	0.751	4.182	0.363	1.204
3479	1.066	1.214	1.097	0.455	1.142	0.744	2.557	1.812	1.251
3496	0.277	1.364	1.112	2.569	1.023	0.871	0.712	0.818	0.599
3499	0.719	1.042	1.036	1.301	1.033	0.388	6.539	1.712	0.851

Notes: The table reports the results of the year-on-year Malmquist productivity index estimation for each of the 4-digit SIC codes for metal manufacturing. In each column we indicate the productivity increase that occurs between the year indicated and the following period for which observation are available, thus the 1990 column refers to productivity increases between 1990 and 1996, for example.

Table 2.9: Efficiency change estimates

				$\hat{EC}_{ECO,it}$					
SIC code	1990	1996	1997	1998	1999	2000	2001	2002	2005
3324	1	1	1	1	1	1	1	1	1
3354	1	1	1	1	1	1	1	1	1
3432	1	1	1	1	1	1	1	1	1
3441	1	1	1	1	1	1	1	1	1
3442	0.997	1.003	1	1	1	1	1	1	1
3448	1	1	1	1	1	1	1	1	1
3479	1	1	1	1	1	1	1	1	1
3496	0.981	1.019	1	1	1	1	1	0.987	1.013
3499	1	1	1	1	1	1	1	1	1
				$\hat{EC}_{ENV,it}$					
SIC code	1990	1996	1997	1998	1999	2000	2001	2002	2005
3324	1	1	1	1	1	1	1	1	1
3354	0.351	0.734	1.034	21.818	1	1	1	0.121	0.869
3432	1	1	1	1	1	1	1	1	1
3441	1.559	1	1	1	1	1	1	1	1
3442	0.739	0.576	1.712	1.583	1	1	0.636	0.788	2.237
3448	1	1	1	1	1	1	1	0.277	3.944
3479	1	1	1	1	1	1	1	1	1
3496	0.723	1.435	1	1	1	1	1	0.496	0.81
3499	1	1	1	1	1	0.355	8.591	1	1

Notes: The table reports the results of the year-on-year efficiency change index estimation for each of the 4-digit SIC codes for metal manufacturing. In each column we indicate the productivity increase that occurs between the year indicated and the following period for which observation are available, thus the 1990 column refers to productivity increases between 1990 and 1996, for example.

Table 2.10: Technological change estimates

				$\hat{TC}_{ECO,it}$					
SIC code	1990	1996	1997	1998	1999	2000	2001	2002	2005
3324	0.787	0.986	1.043	2.073	1.014	0.978	1.338	1.593	0.686
3354	0.807	1.029	0.981	0.604	0.948	1.019	1.575	0.974	0.583
3432	1.072	0.911	1.043	0.589	0.992	0.982	1.911	0.551	0.292
3441	1.691	1.098	0.995	0.657	1.025	0.971	1.245	1.197	0.454
3442	1.050	1.094	0.979	1.798	1.046	1.003	1.060	0.889	1.182
3448	1.589	0.982	1.012	0.417	0.986	0.902	2.015	1.029	1.136
3479	1.119	0.993	1.033	0.884	1.034	0.968	1.171	1.031	1.065
3496	0.352	1.012	0.981	1.305	1.011	0.960	0.982	0.904	0.594
3499	0.885	1.064	1.016	0.599	1.004	0.927	1.663	1.281	0.647
				$\hat{TC}_{ENV,it}$					
SIC code	1990	1996	1997	1998	1999	2000	2001	2002	2005
3324	0.498	1.022	1.093	2.182	0.957	0.899	1.089	2.995	0.705
3354	3.814	2.130	1.107	0.358	0.902	1.007	2.324	11.171	1.052
3432	1.207	0.899	1.182	0.373	1.830	0.646	1.515	0.956	0.544
3441	1.192	1.739	0.918	0.737	0.903	0.696	1.486	1.257	0.751
3442	0.934	2.593	0.767	0.690	1.072	1.021	1.515	1.128	0.525
3448	1.535	0.981	1.032	1.035	0.978	0.751	4.182	1.445	0.312
3479	1.066	1.214	1.097	0.455	1.142	0.744	2.557	1.812	1.251
3496	0.415	0.974	1.112	2.569	1.023	0.871	0.712	1.665	0.752
3499	0.719	1.042	1.036	1.301	1.033	1.543	0.952	1.712	0.851

Notes: The table reports the results of the year-on-year technical change index estimation for each of the 4-digit SIC codes for metal manufacturing. In each column we indicate the productivity increase that occurs between the year indicated and the following period for which observation are available, thus the 1990 column refers to productivity increases between 1990 and 1996, for example.

Table 2.11: Explaining Productivity Changes: $\widetilde{\mathcal{M}}$, $\widetilde{TC}$ and $\widetilde{EC}$ vs current multiple energy prices.

	$\widetilde{\mathcal{M}}_{ECO}$	$\widetilde{TC}_{ECO}$	$\widetilde{\mathcal{M}}_{ENV}$	$\widetilde{TC}_{ENV}$	$\widetilde{EC}_{ENV}$
Δ log(Coal price)$_{t+1}$	3.99	4.00	7.27	13.31**	-22.59
	(3.05)	(2.61)	(7.58)	(5.96)	(27.86)
Δ log(Natural gas price)$_{t+1}$	0.38	0.38*	0.59	3.25***	-3.73
	(0.27)	(0.22)	(0.76)	(1.24)	(3.09)
Δ log(Electricity price)$_{t+1}$	-5.37**	-5.37***	-12.53**	-22.82***	15.80
	(2.23)	(1.75)	(5.98)	(7.79)	(20.36)
Δ log(Interest rate)$_{t+1}$	-0.31***	-0.31***	-0.42*	-0.76**	-0.27
	(0.08)	(0.08)	(0.25)	(0.38)	(0.47)
Δ log(Wage)$_{t+1}$	-0.43	-0.44	1.92	-4.40*	7.50
	(0.90)	(0.80)	(3.61)	(2.61)	(8.21)
log(Value added)$_t$	-0.00	-0.00	0.21	0.07	0.77
	(0.10)	(0.09)	(0.28)	(0.35)	(0.90)
Year gap dummy	0.01	0.01	0.04	0.88	-0.44
	(0.06)	(0.07)	(0.13)	(0.58)	(0.38)
Secondary metal industry (dummy)	-0.03	-0.03	-0.34	-0.83	-1.38
	(0.13)	(0.13)	(0.24)	(0.56)	(1.22)
Constant	1.10	1.10*	-0.15	1.42	-3.77
	(0.75)	(0.61)	(2.13)	(2.77)	(6.70)
Observations	81	81	81	81	81
R^2	0.25	0.25	0.20	0.25	0.09
χ^2	80.05	88.82	88.77	136.19	11.75

Notes: *, **, *** indicate 10%, 5% and 1% statistical significance, respectively. Time period for all regressions is 1990-2006 (with 1991, 1992, 1993, 1994, 1995, 2003 and 2004 missing). Thus $t + k$ refers to the end year $+k$ years. For more information about the timing convention see section 2.4.1. Robust standard errors in parentheses. The significance levels are computed by applying Simar and Wilson's 2007 algorithm A with 2000 repetitions.

Table 2.12: Explaining Productivity Changes: $\widetilde{\mathscr{M}}$, $\widetilde{TC}$ and $\widetilde{EC}$ vs multiple energy prices expectations.

	$\widetilde{\mathscr{M}}_{ECO}$	$\widetilde{TC}_{ECO}$	$\widetilde{\mathscr{M}}_{ENV}$	$\widetilde{TC}_{ENV}$	$\widetilde{EC}_{ENV}$
Δ log(Coal price)$_{t+2}$	0.04	0.05	2.01	9.39*	1.41
	(1.00)	(1.03)	(2.39)	(5.38)	(3.70)
Δ log(Natural gas price)$_{t+2}$	0.81***	0.82***	2.78***	2.82**	3.71
	(0.21)	(0.20)	(1.07)	(1.26)	(2.46)
Δ log(Electricity price)$_{t+2}$	-3.01**	-3.04**	-7.43*	-14.09*	-8.31
	(1.29)	(1.39)	(4.33)	(8.32)	(9.41)
Δ log(Interest rate)$_{t+2}$	-0.40	-0.41	-0.04	-0.50	1.78
	(0.30)	(0.28)	(0.66)	(0.57)	(2.33)
Δ log(Wage)$_{t+2}$	0.12	0.11	-0.38	-1.44	-2.02
	(0.86)	(1.19)	(2.29)	(3.48)	(9.86)
log(Value added)$_{t+2}$	0.01	0.01	0.12	-0.06	0.46
	(0.09)	(0.08)	(0.19)	(0.37)	(0.63)
Year gap dummy	-0.08	-0.08	-0.28*	0.14	-0.46
	(0.08)	(0.08)	(0.15)	(0.33)	(0.44)
Secondary metal industry (dummy)	-0.04	-0.04	-0.28	-0.77	-1.18
	(0.14)	(0.12)	(0.18)	(0.58)	(0.98)
Constant	0.95	0.95	0.54	2.22	-1.00
	(0.68)	(0.62)	(1.41)	(3.02)	(4.86)
Observations	81	81	81	81	81
R^2	0.20	0.20	0.20	0.20	0.10
χ^2	35.93	37.27	41.01	56.56	11.39

Notes: *, **, *** indicate 10%, 5% and 1% statistical significance, respectively. Time period for all regressions is 1990-2006 (with 1991, 1992, 1993, 1994, 1995, 2003 and 2004 missing). Thus $t+k$ refers to the end year $+k$ years. For more information about the timing convention see section 2.4.1. Robust standard errors in parentheses. The significance levels are computed by applying Simar and Wilson's 2007 algorithm A with 2000 repetitions.

Table 2.13: Explaining Productivity Changes: $\widetilde{\mathscr{M}}$, $\widetilde{TC}$ and $\widetilde{EC}$ vs energy price expectations (1996-2001)

	$\widetilde{\mathscr{M}}_{ECO}$	$\widetilde{TC}_{ECO}$	$\widetilde{\mathscr{M}}_{ENV}$	$\widetilde{TC}_{ENV}$	$\widetilde{EC}_{ENV}$
Δ log(Energy deflator)$_{t+2}$	1.13***	1.13***	5.54**	2.25*	6.18
	(0.35)	(0.37)	(2.65)	(1.25)	(5.65)
Δ log(Interest rate)$_{t+2}$	-0.28	-0.28	-0.02	-0.13	1.95
	(0.22)	(0.24)	(0.74)	(0.38)	(2.60)
Δ log(Wage)$_{t+2}$	1.67**	1.64**	3.54	1.98	2.39
	(0.66)	(0.72)	(3.33)	(2.36)	(9.70)
log(Value added)$_{t+2}$	-0.09	-0.09	0.02	-0.21	0.84
	(0.15)	(0.05)	(0.34)	(0.18)	(1.04)
Secondary metal industry (dummy)	-0.03	-0.03	-0.34	0.06	-2.08
	(0.14)	(0.08)	(0.37)	(0.17)	(1.64)
Constant	1.66	1.65***	1.23	2.70**	-3.46
	(1.13)	(0.38)	(2.59)	(1.35)	(7.73)
Observations	54	54	54	54	54
R^2	0.12	0.12	0.12	0.06	0.09
χ^2	22.11	27.01	7.76	5.65	4.22

Notes: *, **, *** indicate 10%, 5% and 1% statistical significance, respectively. Time period for all regressions is 1990-2006 (with 1991, 1992, 1993, 1994, 1995, 2003 and 2004 missing). Thus $t + k$ refers to the end year $+k$ years. For more information about the timing convention see section 2.4.1. Robust standard errors in parentheses. The significance levels are computed by applying Simar and Wilson's 2007 algorithm A with 2000 repetitions.

Table 2.14: Explaining Productivity Changes: $\widetilde{\mathscr{M}}$, $\widetilde{TC}$ and $\widetilde{EC}$ vs energy price expectations (primary sector only)

	$\widetilde{\mathscr{M}}_{ECO}$	$\widetilde{TC}_{ECO}$	$\widetilde{\mathscr{M}}_{ENV}$	$\widetilde{TC}_{ENV}$
Δ log(Energy deflator)$_{t+2}$	2.89	2.89	12.87*	10.49
	(2.01)	(1.82)	(7.80)	(10.03)
Δ log(Interest rate)$_{t+2}$	-0.06	-0.06	1.73	-2.01**
	(0.40)	(0.36)	(2.11)	(0.90)
Δ log(Wage)$_{t+2}$	2.92***	2.92***	4.80	12.16
	(1.11)	(1.11)	(15.11)	(19.81)
log(Value added)$_{t+2}$	-0.37**	-0.37**	0.52	3.14*
	(0.16)	(0.15)	(0.89)	(1.65)
Year gap dummy	-0.26***	-0.26***	-0.44	2.88*
	(0.01)	(0.01)	(0.60)	(1.50)
Constant	3.79***	3.79***	-2.43	-23.71*
	(1.11)	(1.03)	(6.91)	(12.29)
Observations	18	18	18	18
R^2	0.37	0.37	0.35	0.36
χ^2	0.37	0.37	0.35	0.36

Notes: *, **, *** indicate 10%, 5% and 1% statistical significance, respectively. Time period for all regressions is 1990-2006 (with 1991, 1992, 1993, 1994, 1995, 2003 and 2004 missing). Thus $t+k$ refers to the end year $+k$ years. For more information about the timing convention see section 2.4.1. Robust standard errors in parentheses. The significance levels are computed by applying Simar and Wilson's 2007 algorithm A with 2000 repetitions.

Table 2.15: Explaining Productivity Changes: $\widetilde{\mathscr{M}}$, $\widetilde{TC}$ and $\widetilde{EC}$ vs energy price expectations (secondary sector only)

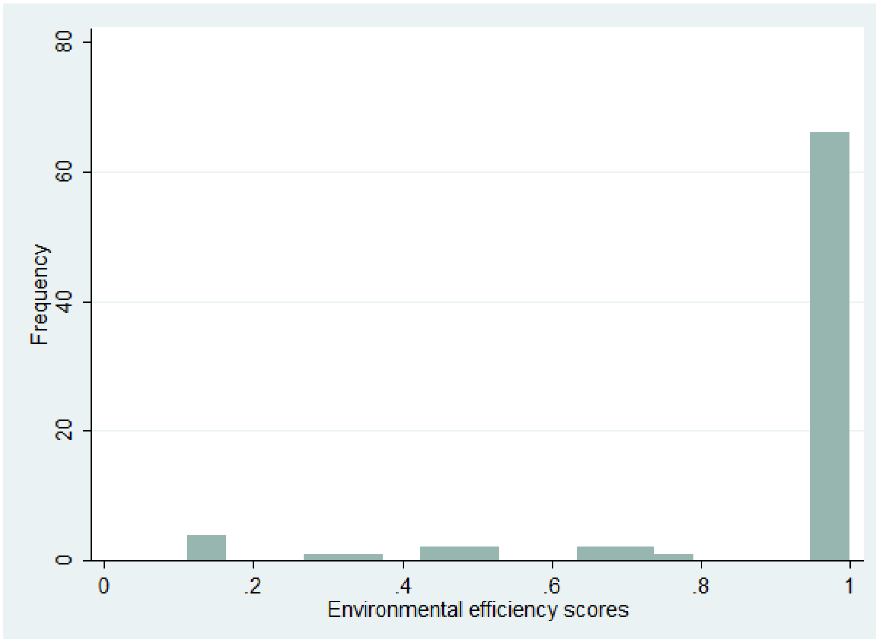
	$\widetilde{\mathscr{M}}_{ECO}$	$\widetilde{TC}_{ECO}$	$\widetilde{\mathscr{M}}_{ENV}$	$\widetilde{TC}_{ENV}$	$\widetilde{EC}_{ENV}$
Δ log(Energy deflator)$_{t+2}$	0.71**	0.72*	4.35**	2.59*	1.81
	(0.35)	(0.37)	(2.13)	(1.57)	(2.65)
Δ log(Interest rate)$_{t+2}$	-0.43	-0.44	-0.54	-0.30	-0.26
	(0.27)	(0.28)	(0.52)	(0.39)	(0.55)
Δ log(Wage)$_{t+2}$	0.20	0.19	0.99	2.71	-3.28
	(0.63)	(0.61)	(1.46)	(1.71)	(2.00)
log(Value added)$_{t+2}$	0.00	0.00	0.06	-0.04	0.16
	(0.11)	(0.10)	(0.17)	(0.12)	(0.16)
Year gap dummy	-0.07	-0.07	-0.18*	-0.08	-0.06
	(0.08)	(0.09)	(0.11)	(0.08)	(0.12)
Constant	0.94	0.94	0.54	1.33	-0.08
	(0.89)	(0.76)	(1.39)	(0.94)	(1.38)
Observations	63	63	63	63	63
R^2	0.12	0.12	0.13	0.13	0.04
χ^2	12.34	12.71	12.41	5.08	8.61

Notes: *, **, *** indicate 10%, 5% and 1% statistical significance, respectively. Time period for all regressions is 1990-2006 (with 1991, 1992, 1993, 1994, 1995, 2003 and 2004 missing). Thus $t + k$ refers to the end year $+k$ years. For more information about the timing convention see section 2.4.1. Robust standard errors in parentheses. The significance levels are computed by applying Simar and Wilson's 2007 algorithm A with 2000 repetitions.

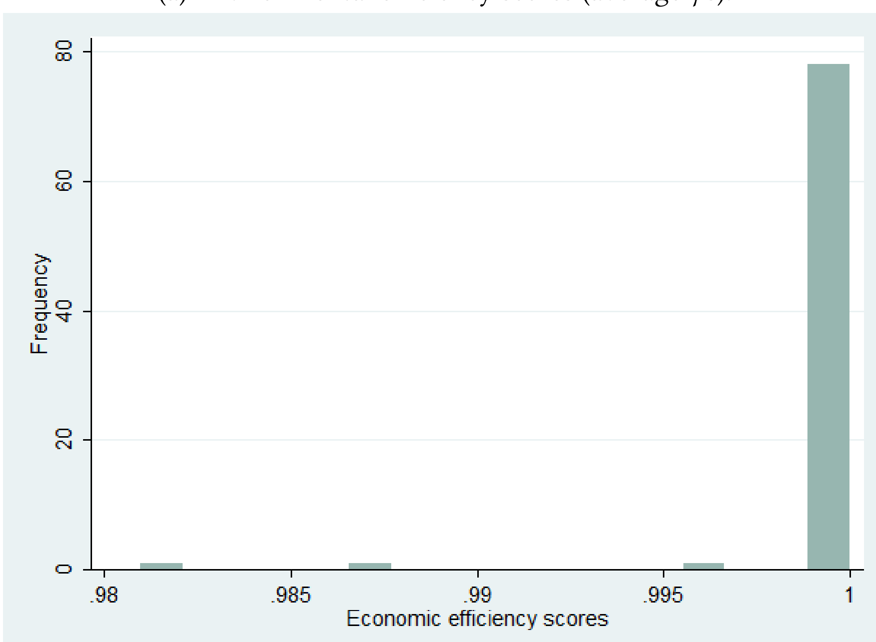
Table 2.16: Explaining Environmental Productivity Changes: $\widetilde{\mathscr{M}}$, $\widetilde{TC}$ and $\widetilde{EC}$ vs energy price expectations (weighing by emissions of pollutants)

	$\widetilde{\mathscr{M}}_{ENV}$	$\widetilde{TC}_{ENV}$	$\widetilde{EC}_{ENV}$
Δ log(Energy deflator)$_{t+2}$	5.95***	5.39***	2.95
	(2.06)	(1.65)	(2.67)
Δ log(Interest rate)$_{t+2}$	0.06	-0.51	1.05
	(0.50)	(0.49)	(1.09)
Δ log(Wage)$_{t+2}$	3.88**	5.57**	-1.75
	(1.73)	(2.72)	(3.56)
log(Value added)$_{t+2}$	0.06	0.04	0.23
	(0.17)	(0.16)	(0.20)
Year gap dummy	-0.16	0.38	-0.26
	(0.13)	(0.36)	(0.27)
SIC dummy	-0.36**	-0.71***	-0.59
	(0.18)	(0.24)	(0.46)
Constant	0.99	1.11	0.12
	(1.25)	(1.29)	(1.42)
Observations	81	81	81
R^2	0.16	0.21	0.06
χ^2	53.03	77.01	7.29

Notes: *, **, *** indicate 10%, 5% and 1% statistical significance, respectively. Time period for all regressions is 1990-2006 (with 1991, 1992, 1993, 1994, 1995, 2003 and 2004 missing). Thus $t + k$ refers to the end year $+k$ years. For more information about the timing convention see section 2.4.1. Robust standard errors in parentheses. The significance levels are computed by applying Simar and Wilson's 2007 algorithm A with 2000 repetitions.



(a) Environmental efficiency scores (average ϕs).



(b) Economic efficiency scores (θ).

Figure 2.7: Distribution of efficiency scores.

Chapter 3

Porter hypothesis in the U.S. manufacturing: An instrumented stochastic frontier model

Abstract: The present paper revisits the "strong" Porter hypothesis using a stochastic frontier model. Taking advantage of a new proxy for stringency and weather instruments it estimates the impact of stringent energy policy on the inefficiency of state-level manufacturing. The Porter hypothesis is strongly supported. This result is robust to time effects, controlling energy prices, state heterogeneity and corrections for endogeneity.

3.1 Introduction

"Strict environmental regulations do not inevitably hinder competitive advantage against rivals; indeed they often enhance it" suggested Michael Porter in 1991 (Porter, 1991, p. 108). This simple, but counter-intuitive idea had considerable political success, even though (or especially since) it went against the intellectual currents of the time (Jaffe et al., 1995, p. 133). In the following years several economists examined it both in theoretical and empirical models. Quarter of a century later there's still little consensus on what has come to be known as the Porter hypothesis (PH).

The Porter hypothesis posits that environmental regulation enhances competitive advantage through five mechanisms (Porter and Van der Linde, 1995): (1) it highlights resource inefficiencies, (2) it raises corporate awareness of environmental issues, (3) it reduces uncertainty about the profitability of future investments, (4) it creates pressure to innovate, (5) it levels the competition during an environment-friendly transition (i.e. opportunistic firms can't gain advantage by avoiding environmental investment). The mechanisms suggest an imperfect world where firms have slacks and production inefficiencies, as Porter and Van der Linde (1995) state ”[…] the world does not fit the Panglossian belief that firms always make optimal choices” (Porter and Van der Linde, 1995, p. 99); firms are sometimes inefficient. Methods of productivity analysis, with their focus on inefficiency, seem to be the perfect empirical tool to address the PH. Notwithstanding this clear complementarity, few studies to date have investigated the PH with efficiency analysis.

In this paper, we revisit the Porter hypothesis with a stochastic efficiency analysis using state-level data of manufacturing. Using a new proxy for stringency and an instrumental variable approach, we estimate the causal effect of strict policy on inefficiency. Our findings show a strong impact of stringent policy on inefficiency. The results show that 2% stricter policy (which is 1 point increase on the 0-50 scale policy score) decreases inefficiency by 1.4% (see Section 3.4). If we assume linearity in the response this translates to a -0.7 elasticity of inefficiency with respect to policy stringency.

Jaffe and Palmer (1997a) classified the literature on the PH into three strands: ”narrow”, ”weak”, and ”strong”; this classification has become accepted as a general guide to the literature (see e.g. Ambec et al., 2013). The ”narrow” strand compares regulations, testing Porter and Van der Linde’s (1995) claim, that flexible regulations enhance productivity more than prescriptive ones (e.g. Butraw, 2000; Driesen, 2005), or how do various regulations affect the costs of adjustment. Although the limitations of data bounds the scope of these studies, in general they found that more flexible policies are less costly than prescriptive ones, partially because they lead to more innovations. The ”weak” strand of literature examines how do regulations affect innovations (e.g. Brunneimer and Cohen, 2003; Popp, 2006). Most studies confirm the weak form of the PH; regulations induce innovations. Finally, the ”strong” PH literature asks how do regulations affect firms’ competitiveness, which may be proxied in different manners depending on the research objective. The present paper fits within the ”strong” branch, therefore we review the ”strong” literature. From here on ’Porter hypothesis’ refers to the strong version. For a recent literature review including the other strands see Ambec et al. (2013).

Most studies find negative or no effect of regulations - usually proxied by abatement costs - on competitiveness (e.g. Gray, 1987; Barbera and McConnell, 1990; Dufour et al., 1998; Alpay et al., 2002; Gray and Shadbegian, 2003; Managi et al., 2005; Böhringer et al., 2012; Broberg et al., 2013; Rubashkina et al., 2015); few studies find positive effect in this framework (Berman and Bui, 2001; Lanoie et al., 2008). Abatement costs (and related variables), however, are often said to be unreliable measures for regulation stringency. Berman and Bui (2001) write that ”abatement costs may severely overstate the true cost of environmental regulation”, because abatement technologies may increase productivity, so a firm may invest in abatement technologies without any added incentive from environmental regulation - making abatement costs partially endogenous (Berman and Bui, 2001, p. 509). Ambec et al. (2013) argue that large abatement costs may be caused by other factors than policy, such as older plants or energy prices. Besides the PH ”does not posit that higher abatement costs will lead to innovation” (Ambec et al., 2013, p. 14). Similarly Broberg et al. (2013) posit that ”environmental investment will [...] not correctly reflect regulation stringency”, because firms may improve their image by environmental investments (Broberg et al., 2013, p. 53). They also add that the survey question may lead to inconsistent data, as different firms categorise their environmental investment in different ways.

Studies with different stringency proxies paint a subtler picture. Lanoie et al. (2011) use a seven-country OECD survey to estimate the effects of perceived environmental stringency on business performance and find a positive correlation. Greenstone et al. (2012) find positive effect of CO regulation on plant level TFP, but negative effect of every other pollutant. They proxy regulations with the attainment of plants/counties. Earnhart and Rassier (2016) apply an index number to examine the effect of regulations on return to sales in the US. Their results show that loose monitoring makes the PH true. Albrizio et al. (2017a) use an index number to estimate the effect of stringent environmental policy on industries and firms. They find that strigent environmental policy enhances the productivity of productive firms, while it slows the less productive ones. Franco and Marin (2017) find that tax-intensity drives productivity both in up- and in down-stream electricity sectors. The stringency proxy seems important: estimations with abatement costs overwhelmingly contradict the PH, with other proxies they lend qualified support.

The present paper’s chosen proxy is a policy index. Brunel and Levinson (2016) compare proxies of environmental stringency and show that there’s no perfect measure; each has its merits and shortcomings. An index number could be more comprehensive than the others (appropriate for the PH), but its coefficient is difficult to interpret. While this is a just criticism, it is also true that the Porter hypothesis suggests an (economically significant) positive effect, but it’s silent about the exact magnitudes. To help the interpretation and intuition, the results are presented as ’elasticities’: how does 1-2% increase in stringency affect inefficiency?

Another aspect often overlooked in the literature is the possible endogeneity of the environmental policy (Brunel and Levinson, 2016). As we argue in Section 3.3, (the lack of) competitiveness could influence the strictness of environmental policy; to control for the endogeneity we use an instrumental variable estimation, with the frequency of thunderstorms and average wind speed as instruments (to the best of our knowledge Rubashkina et al. 2015, are the only ones to date, to explicitly address this issue).

The contribution of this paper is threefold: firsts, instead of abatement costs we use a state-level policy index to proxy environmental regulations. As we argued above a policy index is

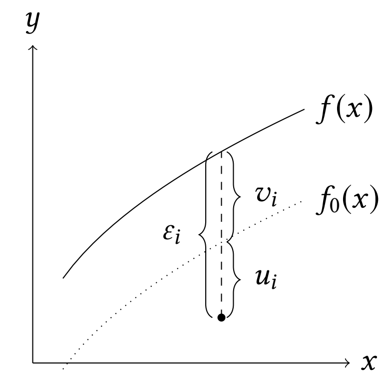


Figure 3.1: Composed error of stochastic frontier model.

more aligned with the spirit Porter hypothesis. Second, we follow Porter and Van der Linde (1995) and relax the assumption of efficient firms. Finally, we address the endogeneity of policies with an IV estimation.

The rest of the paper is structured as follows. The next section outlines the methodology. Section 3.3 introduces the data. Section 3.4 presents and discusses the results. The last section concludes.

3.2 Porter hypothesis in an inefficiency framework

As we said in the Introduction, efficiency analysis can be seen as the ideal empirical tool to address the Porter hypothesis, given its focus on firms' inefficiency. Fig. 3.1 displays a simple one input, one output stochastic frontier model.[1] The dot shows a firm, which operates inside the production possibility set, below the production function, $f(x)$. Part of the total shortfall (ε_i) is random error, v_i; the other part of it inefficiency, u_i. Thus, in this example, a direct comparison to $f(x)$ overestimates inefficiency; the proper comparison is with $f_0(x)$, the frontier available for the firm.

According to the Porter hypothesis an exogenous environmental policy might motivate the firm to close the gap between its production point and $f_0(x)$, thus eliminating inefficiency (u_i) by raising awareness, highlighting inefficiencies or inducing managerial innovations (Porter and Van der Linde, 1995, p. 99-100).

[1] Kumbhakar and Lovell (2000) and Greene (2008) provide introductions and detailed overviews of the field. For review of the recent literature see Parmeter and Kumbhakar (2014).

In a simple model with M inputs:

$$y_i = \underbrace{f(\mathbf{x}_i)}_{\text{Deterministic}} \quad \underbrace{-u_i + v_i}_{\text{Stochastic } (\varepsilon_i)} . \tag{3.1}$$

Where y_i is firm i's output, $\mathbf{x}_i$ is its $1 \times M$ input vector. The production function shows the deterministic part of the production, while the composed error term (ε_i) is the stochastic part (Aigner et al., 1977; Meeusen and van den Broeck, 1977). Inefficiency (u_i) is one-sided, because a firm can't produce more than the production function, only equal or less. The random error, v_i, on the other hand, is two-sided. Ideally one not only estimates inefficiency (u_i), but also the effect of environmental variables ($\mathbf{z}_i$) on inefficiency. The drawbacks of two-step approach (i.e. estimating u_i's, then regressing them on the environmental variables) are well-established (Schmidt, 2011). It's generally agreed that if environmental variables are omitted in the first step, the coefficient estimates will be biased - akin to the omitted variable bias in a linear regression model. This bias would be carried forward to the second step - both in the mean and the standard error of the inefficiency distribution (Wang and Schmidt, 2002; Greene, 2008). A single step estimation is preferred, but how do the environmental variables fit in the estimation differs from model to model.

In this paper we assume that *scaling property* is true (Wang, 2002). The scaling property states that

$$u_{it}(\mathbf{z}_{it}, \boldsymbol{\delta}) = h(\mathbf{z}_{it}, \boldsymbol{\delta})u^*,$$

where $h(.)$ is the scaling function, which scales the basic variable, u^*; $\boldsymbol{\delta}$ is a parameter vector to be estimated. The economic intuition is that firms have the same potential for efficiency, but they exploit that potential to different extents based on their environmental variables ($\mathbf{z}_{it}$) alvarez2006interpreting. We choose the exponential function as the scaling function, $h(\mathbf{z}_{it}, \boldsymbol{\delta}) = \exp(\mathbf{z}'_{it}\boldsymbol{\delta})$, because it's easy to interpret the marginal effects: $\partial \ln(u)/\partial z = \delta$, a unit increase in z increases inefficiency (u) by δ percentages. The scaling property is a distribution-free approach, so one needn't assume a specific distribution for u^* (Simar et al., 1994b).

For the sake of generality, we specify a translog production function[2]:

$$\ln y_{it} = \alpha + \Sigma_j^M \beta_j \ln(x_{j,it}) + \Sigma_j^M \Sigma_k^M \beta_{jk} \ln(x_{j,it}) \ln(x_{j,it}) - u_{it} + v_{it} \tag{3.2}$$

$$v_{it} \sim N(0, \sigma_v^2) \quad u_{it} = \exp(\mathbf{z}'_{it}\boldsymbol{\delta})u^*.$$

Where β's are parameters to be estimated and assumed to be symmetric $\beta_{jk} = \beta_{kj}$.[3]

[2]The F-test also indicated significant gains choosing translog over Cobb-Douglas functional form.

[3]Equation (3.2) could be simply estimated with non-linear least squares. Since u_{it} measures the possible output gap, given the inputs, it indicates the output-oriented inefficiency.

Using instrumental variables in a stochastic frontier setting is a new research topic, still in its infancy (Tran and Tsionas, 2015; Amsler et al., 2016). With the scaling property, however, instrumental variable estimation fits relatively easily into the stochastic framework. Amsler et al. (2016) have shown that the standard IV estimators will be consistent if u^* is assumed to be independent of both the instruments and the environmental variables $\mathbf{z}_{it}$; environmental variables can only help exploit the efficiency improvement possibilities, but themselves can't open up new ones:

$$\frac{\partial u^*}{\partial \text{Instruments}} = 0 \qquad \frac{\partial u^*}{\partial z_{it}} = 0.$$

We believe that since the instruments are weather phenomena the first assumption will likely to hold. The second assumption is more restrictive. In that case, we assume that since our framework is relatively short (5 years, see next section), it's unlikely that the policies could affect the potential and structure of inefficiency distribution across states under such a short timespan.

3.3 Data

Estimating (3.2) requires data about the the output (y), inputs (x's), and the environmental variables ($\mathbf{z}$). The cross-sectional units (i) are the U.S. states' manufacturing sectors; the time variable, t, goes from 2010 to 2014, annually. This short run examination implicitly excludes most technological innovations, the focus is on managerial practices and organisational improvements. As the environmental management literature has shown (e.g. Petts et al., 1999; Martin et al., 2012; Park et al., 2014), managerial practices and attitudes are important determinants of environmental-friendliness of firms.

Table 4.2 describes the summary statistics of the variables. We include three inputs: labour, capital, and materials. The labour input data (employment in manufacturing) comes from the Bureau of Labor Statistics' State and Metropolitan Area Employment database (SAE). Capital and material data come from the geographic area statistics of the Annual Survey of Manufacturers (ASM). State-level data is available from 2010 to 2014. The output variable (the manufacturing sector's contribution to the state's current GDP) comes from the Bureau of Economic Analysis' Regional Economic Accounts (REA).

The data is somewhat limited, because of the state-level aggregation, but the stochastic frontier methodology proved to be instructive in country-level estimations (e.g. see Koop et al., 1999; Greene, 2004).

The policy proxy comes from the American Council for an Energy-Efficient Economy. The

Table 3.1: Summary statistics.

Variable	Mean	Std. Dev.	Min.	Max.	N	Source	
Contr. to current GDP ($1,000)	38,603.565	46,698.249	230	255,634	255	REA	y_{it}
Employment (1000s)	224.459	229.226	1	1,269.6	255	SAE	$\mathbf{x}_{it}$
Capital expenditures ($1,000)	3,085,226.631	3,515,361.309	3,551	19,927,758	255	ASM	
Material costs ($1,000)	64,146,187.671	79,914,570.273	79,169	494,603,256	255	ASM	
Revised ACEEE score	9.164	4.735	0	20.875	357	ACEEE	$\mathbf{z}_{it}$
Energy price ($/million Btu)	13.805	6.148	6.41	54.17	459	EIA	
Avg. days with thunderstorm	1.168	0.788	0.038	4.186	252	QCLCD	Instruments
Avg. wind speed (km/h)	7.275	1.509	4.437	11.111	252	QCLCD	

The y_{it} and $\mathbf{x}_{it}$ are specific for the states' manufacturing sector.
ACEEE scores and days with thunderstorm are state-level. Energy prices are state-level industrial prices.
Years: 2010-2014 (5 years), 51 states.

American Council for Energy-Efficient Economy (ACEEE) is a nonprofit organisation which conducts analysis regularly about energy efficiency in the US economy. One of its projects is the 'State Energy Efficiency scorecard' published annually. The scorecards assign scores for the states between 0 and 50 based on the stringency of state-level energy policy. They include utility, transportation, building and combined heat-and-power policies, state initiatives and application standards; these are the main categories of evaluation. Most categories are divided into subcategories (see Table 3.6 in the Appendix for details). The states are evaluated in several years, which makes the ACEEE scores preferable over other state-level indices (e.g. 24/7 Wall Street 2010 ranking, Greenopia 2011 State Sustainability Index, Forbes 2007 America's Greenest states).[4] The most recent executive summary states that: "[a]s in the past, this year's report ranks states on their policy and program efforts, not only assessing performance but also documenting best practices and recognizing leadership", discussing the beneficial effects of "stringent [...] savings targets", "stringent building energy codes" and improved compliance on the individual rankings (ACEEE, 2016). This database has been used before in the energy policy and economics literature as a policy stringency proxy (e.g. Murray and Mills, 2011; Yan et al., 2015).

Fig. 3.2 shows the geographical variation in 2014. As one may see, there's a large variation across states, Massachusetts being the highest ranking state with 42 points out of the possible 50; North Dakota being the lowest with 4.

For illustrative purposes Fig. 3.3 shows the evolution of policy scores in all states. As we can see most variation comes from across states, though different states have different trends in their scores. We can also see large persistence of scores. In the estimations, however, we're only interested in the effect of the current one.

Energy prices come from the State Energy Data System of the Energy Information Administration. They are state-level industrial prices. Manufacturing, by its size, may influence

[4] Policy scores for consecutive years are available since 2008.

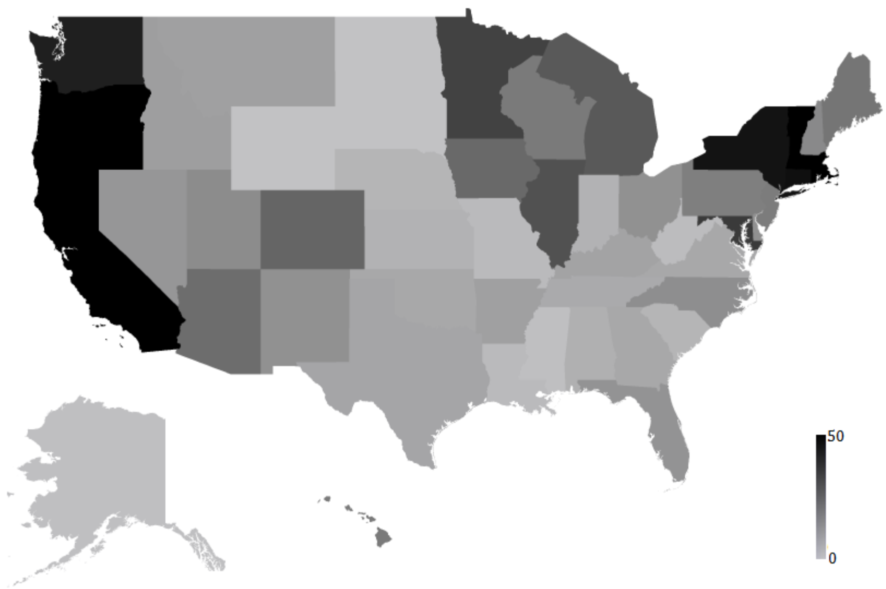


Figure 3.2: Geographical variation of the 2014 policy scores (Source: Gilleo et al., 2014).

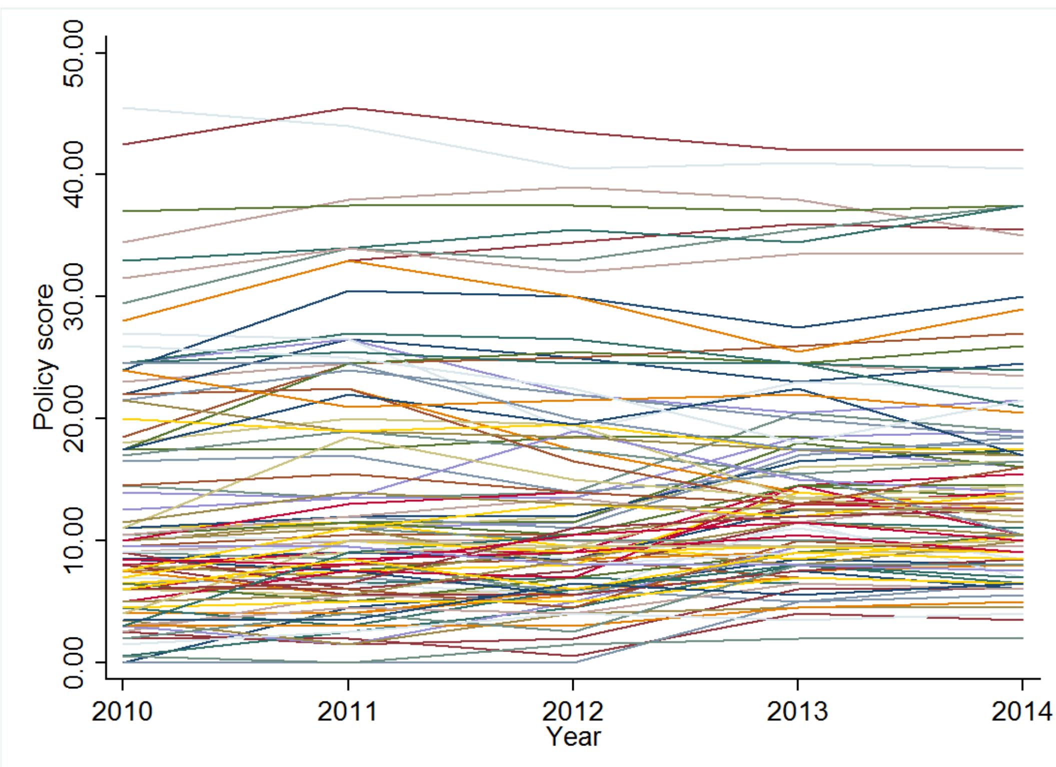


Figure 3.3: Time variation of policy scores by states.

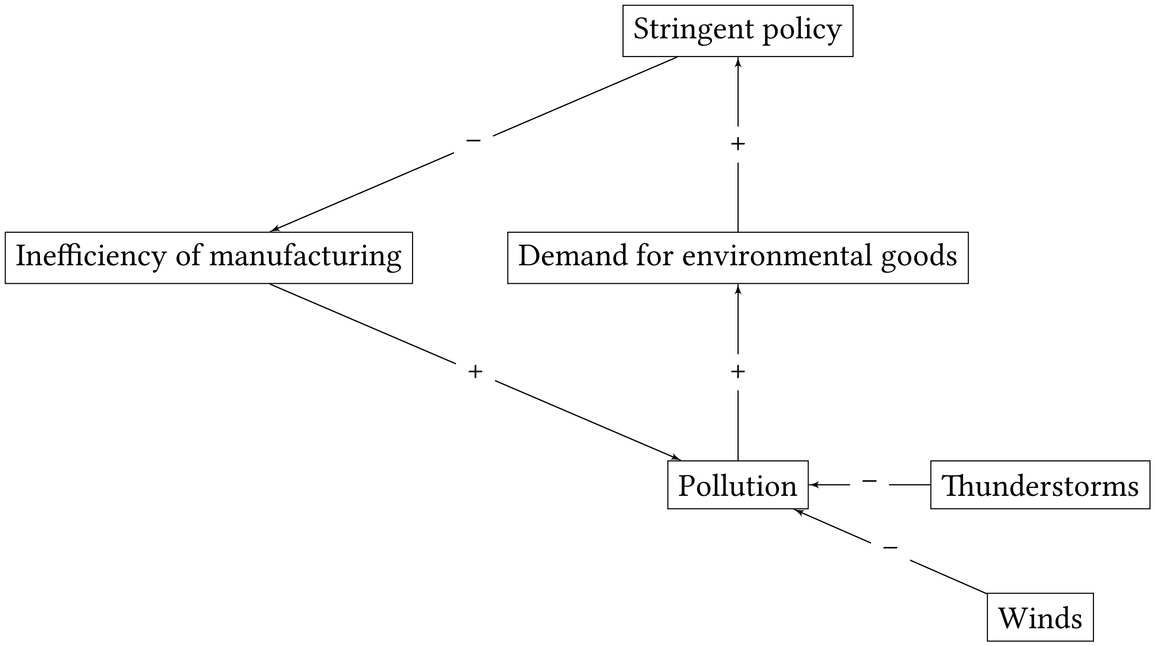


Figure 3.4: Links of causality and the reasoning behind the instruments.

energy prices leading to issues of reverse causality and endogeneity; to minimise the impact of potential endogeneity, as well as to take into account of any time lag in the response of firms to energy prices, we estimate the effects of lagged prices.

Unfortunately the causal links between manufacturing and environmental policy are likely to be two-way. Figure 3.4 sketches the potential links between them signs show the expected direction of the effects. Inefficiency of manufacturing is likely to increase pollution, as inefficient sectors use resource inefficiently which includes the polluting inputs. The increased pollution (be it air, ground, or water) faced by the electorate in the state increases the demand for environmental quality and goods (e.g. clean air or water). This increased demand leads to a stringent environmental policy. Stringent environmental policy then decreases inefficiency - according to Porter. If Figure 3.4 is accurate, then the potential endogeneity bias dampens the Porter effect.

To address the endogeneity of the policy scores, we will use an instrumental variable approach. Our chosen instruments are the mean number of thunderstorms and the average wind speed in the year, in the given state. They come from the Quality Controlled Local Climatological Data (QCLCD). We convert the daily, station-level data by averaging the observations from stations by states. These instruments fit in the long tradition of weather instruments in economics (e.g. Wolpin, 1982; Luechinger, 2009; Sarsons, 2015). As any weather phenomena, thunderstorms and wind speed are unlikely to be correlated with the economic variables of manufacturing.

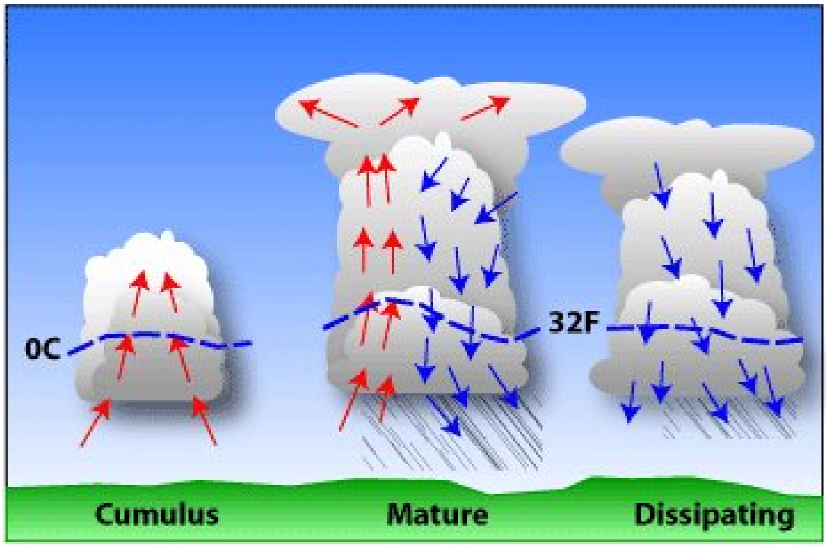


Figure 3.5: Thunderstorm's life-cycle. Source: Ward (2016).

Yet, they could affect the states environmental and energy policy. The wind speed is an accepted instrument by the literature; the wind blows away the pollution, affecting demand for environmental goods and thus the stringency of environmental policy (see e.g. Luechinger, 2009). Thunderstorms work similarly to winds. During a thunderstorm-formation, the clouds absorb all the air on the ground level, *including air pollution* (Dickerson et al., 1987). Fig. 3.5 provides a simple illustration of a thunderstorm's life cycle. During the formation ("cumulus" stage) the thunderstorm absorbs surface-level air in the updraught and during its mature stage it starts to pump the air into higher atmospheres.

Thunderstorms and wind clean the air exogenously. If a state encounters lots of thunderstorms and strong winds in a given year, its air automatically cleans itself, lowering the demand for environmental goods, leading to laxer policy.

3.4 Results

3.4.1 Estimates without instruments

Table 3.2 reports the results of estimations without instrumentation. The lower half shows the estimation of the deterministic part of the model, the production function; the upper half reports the variables in the scaling function, showing the effect on inefficiency. Unlike the coefficients of a Cobb-Douglas production function, the coefficients of the translog don't have a straightforward interpretation. Negative coefficients in the first and second order terms are common (Greene, 2003, Chap. 5). To help the interpretation we calculate the observation-specific elasticities of inputs (averages are reported in Table 3.2). The elasticity of input j at time t for state i's manufacturing sector in a translog production function is calculated as:

$$\eta_{it,j} = \beta_j + \Sigma\beta_{j,k} \ln x_{it,k} + 2\beta_{jj} \ln x_{it,j}.$$

The average elasticities of capital (around 0.21) and labour (around 0.83) roughly correspond to comparable estimates of the US manufacturing (e.g. Felipe and Adams, 2005; Abraham and White, 2006; Growiec et al., 2015). The average elasticity of materials is around 0.05, thus the implied returns to scale is around 1.08, again in line with comparable estimates (Levy, 1990). These estimates are remarkably stable across specifications (see column (i)-(v) of Table 3.2).

Above the production function estimates are the results for the scaling function. The first column shows the effect of the scores on inefficiency without controlling to energy prices or time effects. The score is significant and negative supporting the PH; it has a negative association with *in*efficiency. The coefficient shows that if there's (1/50=)2% increase in stringency, inefficiency decreases by 0.9%. Column (ii) shows the estimation controlling for energy prices. Energy price changes are insignificant and negative; stringency stays negative and significant, supporting the PH. Column (iii) adds time effects to the estimation; intuitively this means that the production function is allowed to shift up or down, across periods. The coefficient of the policy score stays significant and stable at -0.009, as are the input elasticities. This seems to support our assumption of stable production function (time effects are insignificant) and the focus on managerial practices. Column (iv) and (v) shows the estimation with lagged policy scores. The coefficients are statistically significant. The strong association between stringency and inefficiency is likely to show the importance of the choice of stringency proxy. In the next section we only rely on the causal effect of the current score, which according to Figure 3.4 would have a larger negative effect on inefficiency.

3.4.2 IV estimates

Table 3.4 reports the first stage results, and as expected the instruments are strong and they have a negative impact on policy scores (i.e. the more thunderstorms there are, the policy makers are less under pressure to implement stringent policies).

Table 3.3 reports the results of the IV estimations. The production function has similar coefficient to the ones in Table 3.2. The elasticity of capital stays roughly the same as before, whereas the elasticity of labour seems to decrease a bit, while elasticity of materials increases. Some of the inputs lose statistical significance, to be expected by the increased variance of

Table 3.2: Results of the naive estimations (without instrumentation).

	(i)	(ii)	(iii)	(iv)	(v)
Scaling function [Inefficiency]					
Policy score$_t$	-0.009***	-0.009***	-0.009***		
	(0.003)	(0.003)	(0.003)		
Policy score$_{t-1}$				-0.009***	
				(0.003)	
Policy score$_{t-2}$					-0.010***
					(0.003)
Δ Energy price$_{t-1}$		-0.003	-0.006	-0.005	-0.005
		(0.010)	(0.010)	(0.010)	(0.010)
Δ Energy price$_{t-2}$		-0.003	-0.007	-0.005	-0.004
		(0.010)	(0.011)	(0.011)	(0.011)
Δ Energy price$_{t-3}$		-0.006	-0.012	-0.010	-0.009
		(0.008)	(0.010)	(0.010)	(0.010)
ln(Value added$_t$) [Production frontier]					
ln(Capital$_t$)	-4.067***	-3.900***	-3.790***	-3.841***	-3.670***
	(1.217)	(1.251)	(1.271)	(1.272)	(1.268)
ln(Materials$_t$)	-5.537***	-5.741***	-5.870***	-5.851***	-5.833***
	(1.042)	(1.116)	(1.148)	(1.151)	(1.139)
ln(Employment$_t$)	10.123***	10.131***	10.112***	10.165***	9.951***
	(1.492)	(1.511)	(1.541)	(1.540)	(1.531)
$\left[\ln(\text{Capital}_t)\right]^2$	0.093	0.090	0.081	0.096	0.088
	(0.074)	(0.076)	(0.077)	(0.076)	(0.076)
$\left[\ln(\text{Employment}_t)\right]^2$	0.425***	0.425***	0.422***	0.426***	0.415***
	(0.076)	(0.077)	(0.079)	(0.079)	(0.078)
$[\ln(\text{Materials}_t)]^2$	0.147**	0.157**	0.159**	0.165**	0.165**
	(0.066)	(0.070)	(0.072)	(0.071)	(0.070)
ln(Capital$_t$)×ln(Employment$_t$)	-0.363***	-0.355***	-0.344***	-0.355***	-0.341***
	(0.121)	(0.123)	(0.125)	(0.125)	(0.124)
ln(Capital$_t$)×ln(Materials$_t$)	0.195*	0.188*	0.193*	0.176	0.174
	(0.110)	(0.113)	(0.115)	(0.114)	(0.113)
ln(Employment$_t$)×ln(Materials$_t$)	-0.475***	-0.482***	-0.488***	-0.484***	-0.477***
	(0.096)	(0.098)	(0.099)	(0.099)	(0.098)
Intercept	59.106***	59.673***	60.049***	60.160***	59.251***
	(7.509)	(7.715)	(7.873)	(7.879)	(7.824)
Avg. elasticity of capital	0.21	0.21	0.22	0.21	0.21
Avg. elasticity of labour	0.82	0.83	0.83	0.84	0.83
Avg. elasticity of materials	0.05	0.05	0.05	0.05	0.06
Time effects	No	No	Yes	Yes	Yes
Number of observations	255	255	255	255	255

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. $N = 51$, $T = 5$.

the IV estimator. The rows at the bottom show the standard IV tests. The Sargan-Hansen test for overidentifying restrictions shows that wind speed and thunderstorms are uncorrelated with the error term in the second stage, while the weak identification test shows that they have a strong effect on the energy policy of the state. These are true for all specifications.

Column (i) shows the estimation only with the policy stringency. The estimation strongly supports the Porter hypothesis: the coefficient is negative and significant on 5%. Its magnitude is larger; shows that 2% increase in stringency (a 1 point increase on the 0-50 point scale) leads to 1.4% decrease in inefficiency. Assuming a linear effect this translates to an elasticity of -0.7. The result seems to support the relations outlined in Figure 3.4: the endogeneity hinders the observation the Porter effect.

Column (ii) shows the addition of energy prices. Like before, energy prices are insignificant, but the score is significantly negative. Column (iii) is the preferred specification: it controls for time effects, energy prices and endogeneity. It shows a strong negative effect; as in column (i) the effect shows (1/50=)2% increase in stringency decreases inefficiency by 1.4%. If we assume that the policy scores are linear in stringency, and linear response by the sector, this would show a -0.7 elasticity of inefficiency with respect to stringency. The following columns show robustness checks.

The first robustness checks address the policy scores (appendix with details available on request). I compose alternative scores, which exclude all categories that aren't evaluated every year, and categories which weights change are reweighed with the 2014 weights; intuitively this suggests that ACEEE knows the stringency of categories evaluated in all years, but the inclusion of new categories and changing weights disrupt the evaluation. Column (iv) shows the estimation with this new score composition. The coefficient changes to -0.033; it still supports the PH. Under the conditions outlined in the previous paragraph, this would translate to -0.79 elasticity; close to the estimation with the unaltered scores.

Column (v) takes a step further. Not only new categories are excluded, but all other categories which weight change across time. This assumes that categories which are differently weighted are incomparable. Again, the significantly negative coefficient supports the PH. It implies a -0.76 elasticity; close to the elasticity of the preferred specification.

Finally, column (vi) shows the robustness in controlling for state-level heterogeneity, by Mundlak transformation (Mundlak, 1978). This is a standard way to control for heterogeneity in stochastic frontier studies (see Farsi et al., 2005). Mundlak transformation essentially controls for state-level heterogeneity by including cross sectional averages of the inputs.[5] Since

[5] In a stochastic frontier model this would be $\ln y_{it} = \alpha + \Sigma\beta_j \ln x_{it,j} + \Sigma\Sigma\beta_{jk} \ln x_{it,j} \ln x_{it,k} + a_i - u_{it} + v_{it}$, where a_i is the heterogeneity assumed to be $a_i = \Sigma \ln \bar{x}_{i,j} + \Sigma\Sigma\gamma_{jk} \ln \bar{x}_{i,j}\bar{x}_{i,k}$. Mundlak (1978) showed that βs can be interpreted as the 'between' estimator, while the γ's show the difference between the 'within' and 'between' estimator. For that reasons those coefficients don't have a straightforward interpretation. Therefore they're usually omitted from the discussions.

the timeframe is short most inputs lose statistical significance, but score is still significant (though only on 10%) and negative. The coefficient is smaller (in absolute terms), though the difference is insignificant. The implied elasticity is -0.5, again relatively close to the elasticity of the preferred specification.

3.5 Conclusion

The effect of endogeneity is quite large: about 35% of the effect is unobserved in the non-instrumented estimations. In the instrumented estimations not only support the PH strongly, but the implied elasticity is stable across specifications around -0.7 and -0.8. An increase in stringency decreases inefficiency by 0.7%.

How can this happen? Out of the possible mechanisms listed by Porter and Van der Linde (1995) the highlighting of resource inefficiency seems to be the most natural explanation in our case. Stringency might also raise corporate awareness, but that would be unlikely to show up in a stochastic frontier estimation, unless it also indicates inefficiencies.

With environmental policy sparking heated debates, decision makers make statements often implicitly assessing the validity of the Porter hypothesis. Is there a trade-off between economic performance and stringent environmental policy? Most research on PH to date proxied environmental policy with abatement costs and found little evidence for it. The present paper, on the other hand, contributes to the newly emerging literature using alternative proxies, and controls for the potential endogeneity of environmental policy. The estimation strongly supports the Porter hypothesis: policy stringency enhances competitive advantage. The result is robust to alternative score composition, the inclusion of time effects, energy prices, addressing heterogeneity and endogeneity. The effect is smaller in the non-instrumented estimations. This and the new proxy might explain the negative findings of previous investigations. It also shows that in conventional estimations, the negative effects of environmental policies are likely to exaggerated. This could serve as an important caveat for policy makers, when estimating the effect of a future environmental policy on competitive industries.

Table 3.3: Results of the IV estimations.

	(i)	(ii)	(iii)	(iv)	(v)	(vi)
Scaling function [Inefficiency]						
Instrumented Policy score$_t$	-0.014**	-0.013**	-0.014**	-0.033**	-0.038*	-0.010*
	(0.006)	(0.006)	(0.006)	(0.017)	(0.017)	(0.006)
Δ Energy price$_{t-1}$		-0.005	-0.010	-0.011	-0.008	-0.006
		(0.017)	(0.017)	(0.020)	(0.027)	(0.014)
Δ Energy price$_{t-2}$		-0.006	-0.011	-0.014	-0.011	-0.008
		(0.012)	(0.014)	(0.016)	(0.026)	(0.011)
Δ Energy price$_{t-3}$		-0.005	-0.010	-0.014	-0.010	-0.004
		(0.012)	(0.015)	(0.017)	(0.020)	(0.013)
ln(Value added)$_t$ [Production frontier]						
ln(Capital$_t$)	-1.759	-1.596	-1.403	-1.449	-1.734	-0.882
	(2.670)	(2.954)	(3.053)	(2.783)	(2.746)	(3.072)
ln(Materials$_t$)	-5.900***	-6.163***	-6.315***	-6.576***	-6.297***	-6.109
	(1.634)	(1.768)	(1.840)	(1.701)	(1.794)	(4.001)
ln(Employment$_t$)	8.318***	8.404***	8.321***	8.768***	8.717***	6.341*
	(1.936)	(2.088)	(2.133)	(1.941)	(2.044)	(3.405)
$\left[\ln(\text{Capital}_t)\right]^2$	0.023	0.022	0.012	0.022	0.016	-0.056
	(0.119)	(0.128)	(0.138)	(0.133)	(0.150)	(0.172)
$\left[\ln(\text{Employment}_t)\right]^2$	0.345***	0.348***	0.342***	0.374***	0.367***	0.237
	(0.088)	(0.096)	(0.100)	(0.091)	(0.096)	(0.222)
$[\ln(\text{Materials}_t)]^2$	0.179	0.193	0.197	0.212*	0.189	0.165
	(0.119)	(0.126)	(0.127)	(0.118)	(0.123)	(0.178)
ln(Capital$_t$)×ln(Employment$_t$)	-0.218	-0.212	-0.194	-0.213	-0.229	-0.045
	(0.202)	(0.226)	(0.242)	(0.221)	(0.236)	(0.286)
ln(Capital$_t$)×ln(Materials$_t$)	0.138	0.128	0.129	0.119	0.150	0.156
	(0.217)	(0.212)	(0.212)	(0.208)	(0.225)	(0.237)
ln(Employment$_t$)×ln(Materials$_t$)	-0.450***	-0.461***	-0.467***	-0.492***	-0.474***	-0.420***
	(0.117)	(0.115)	(0.121)	(0.121)	(0.143)	(0.128)
Intercept	49.728***	50.675***	50.795***	52.481***	52.108***	42.702***
	(11.914)	(12.847)	(13.203)	(11.484)	(11.693)	(12.070)
Avg. elasticity of capital	0.23	0.22	0.20	0.21	0.21	-
Avg. elasticity of labour	0.75	0.76	0.80	0.78	0.78	-
Avg. elasticity of materials	0.11	0.10	0.09	0.09	0.09	-
Sargan-Hansen test for over-id.	0.73	0.66	0.66	0.97	1.15	1.64
	(0.39)	(0.42)	(0.42)	(0.32)	(0.28)	(0.20)
Weak identification test	58.84	61.35	62.33	39.04	37.54	53.77
	(0.01>)***	(0.01>)***	(0.01>)***	(0.01>)***	(0.01>)***	(0.01>)***
Time effects	No	No	Yes	Yes	Yes	Yes
Mundlak transformation	No	No	No	No	No	Yes
Number of observations	255	255	255	255	255	255

* $p < 0.1$,** $p < 0.05$, *** $p < 0.01$. $N = 51$ (states), $T = 5$ (years) . Instruments: avg. windspeed, avg. number of thunderstorms. Column (i)-(iv) are estimations with the unaltered scores, columns (iv)-(v) use alternative score compositions. Column (vi) uses the unaltered scores with Mundlak transformation.

Appendix to Chapter 3

Other results

Table 3.4: First stage estimation (Dep. var: Raw policy scores).

Excluded instruments	
Thunderstorms	-6.741 ***
	(0.670)
Avg. wind speed	-0.524 *
	(0.313)
Included instruments	
ln(Capital)	-152.411 ***
	(41.880)
ln(Employment)	161.607 ***
	(42.766)
$[\ln(\text{Capital})]^2$	9.590 ***
	(2.354)
$[\ln(\text{Employment})]^2$	8.643 ***
	(2.207)
ln(Capital)×ln(Employment)	-15.128 ***
	(3.872)
ln(Materials)	-217.529***
	(32.772)
$[\ln(\text{Materials})]^2$	1.102
	(1.288)
ln(Employment)×ln(Materials)	-0.811
	(1.707)
ln(Capital)×ln(Materials)	-2.964
	(3.478)
$\Delta\text{Energy price}_{t-1}$	-0.226
	(0.239)
$\Delta\text{Energy price}_{t-2}$	-0.736 ***
	(0.236)
$\Delta\text{Energy price}_{t-3}$	-0.299
	(0.203)
Intercept	780.964 ***
	(201.815)
Number of obs.	255

* $p < 0.1$,** $p < 0.05$, *** $p < 0.01$. $N = 51$ (states), $T = 5$ (years).

ACEEE scores

We start from the premise that the staff of ACEEE are experts: the scores as they are, reflect regulation stringency. A relaxation of this assumption would exclude all the categories which aren't evaluated in all years (denoted by dash [-] in Table 3.6) and categories which are evaluated and count toward a final score, but to an unknown extent are denoted with an asterisk [*]). Table 3.6 shows the scoring in detail for each year. The dash (-) denotes that the subcategory isn't measured, while the asterisk (*) denotes that it is assessed and counts toward the final score, with an unknown weight. To bridge the problem, I only include categories and subcategories which are scored in all years.

Furthermore one might argue that the changing weighs in the scoring also biases the results. To address this problem I reweigh all scores with 2014 weights. Table 3.7 reports the correlation between three compositions:

- raw scores,
- revised scores without any reweighing, and
- revised scores with reweighing.

The idea behind leaving the weights intact is that the criteria for stringent regulation may be changing across periods, and the changing weights maybe simply reflect this fact.

Table 3.5: Hausman test for endogeneity (for the preferred specification).

F-value	3.67
p-value	0.058

Table 3.6: Changes in ACEEE score weights and categories.

	2008	2009	2010	2011	2012	2013	2014
Utility and public benefits programs and policies	**20**	**20**	**20**	**20**	**20**	**20**	**20**
Spending/Budget for Electricity efficiency	5	5	5	5	5	5	5
Spending/Budget for Nat. Gas efficiency	3	3	3	3	3	3	2
Savings from Electricity	5	5	5	5	5	5	5
Savings from Natural gas	-	-	-	-	-	1	2
Opt-out policies	-	-	-	-	-	-	0
Energy Efficiency Resource Standards	4	4	4	4	4	3	3
Incentives and removal of disincentives	3	3	3	3	3	3	3
Transportation policies	**6**	**5**	**8**	**9**	**9**	**9**	**9**
GHG standards	2	*	*	2	2	2	1.5
Smart growth policies	1	-	-	-	-	-	-
Hybrid and electric vehicle incentives	1	-	-	-	-	-	-
Electric vehicle registrations	-	-	-	-	-	-	0.5
Integration of transportation and land use	-	-	-	2	2	2	1
Freight plans	-	-	-	-	-	-	1
Targets to reduce vehicle miles travelled	-	*	*	2	2	2	1
Change in vehicle miles travelled	-	-	-		-	-	1
Transit funding	1	*	*	1	1	1	1
Transit legislation	-	-	-	-	1	1	0.5
Complete streets policies	-	-	-	1	0.5	0.5	0.5
High-efficiency veh. incentives	1	*	*	1	0.5	0.5	0.5
Building energy codes	**8**	**7**	**7**	**7**	**7**	**7**	**7**
Stringency	5	5	5	5	5	5	5
Enforcement	3	2	2	2	2	2	2
Combined heat and power	**5**	**5**	**5**	**5**	**5**	**5**	**5**
Interconnection standard	*	*	*	*	1	1	1
Treatment under renewable portfolio standard (RPS)	-	-	-	-	-	-	0.5
Treatment under energy efficiency resource standard (EERS)	-	-	-	-	-	-	1
Treatment under EERS/RPS	*	*	*	*	1	1	-
Revenue streams	-	-	-	-	-	-	0.5
Net metering	-	-	*	*	0.5	0.5	-
Incentives and grants	*	*	*	*	1	1	0.5
Financing assistance	-	-	-	-	0.5	0.5	0.5
Standby rates	*	*	*	*	-	-	-
Additional policy support	-	-	-	-	0.5	0.5	0.5
Emissions treatment	*	*	*	*	0.5	0.5	0.5
State government initiatives	**-**	**7**	**7**	**7**	**7**	**7**	**7**
Financial incentives	3	3	3	3	3	2.5	2.5
Energy disclosure policies	-	-	-		-	1	1
Lead-by-example efforts in state facilities and fleets	2	2	2	2	2	2	2
Research and development	2	2	2	2	2	1.5	1.5
Appliance and equipment efficiency standards	**4**	**3**	**3**	**2**	**2**	**2**	**2**

Table 3.7: Cross-correlation table.

Variables	Raw scores	Revised scores with reweighing	Completely revised scores
Raw scores	1.000		
Revised scores with reweighing	0.847	1.000	
Completely revised scores	0.946	0.910	1.000

Chapter 4

Climate policy and the efficiency of firms: New Evidence from UK manufacturing

Abstract:[1]The present paper presents a new empirical investigation into the Porter hypothesis; it tests whether climate policy – in the form of the UK Climate Change Levy – enhances firms' efficiency. Taking advantage of the variation introduced by the design features of the Climate Change Levy, we use British firm-level data to investigate this question within a Stochastic Frontier Analysis framework. Our results show that the Climate Change Levy had a significant positive impact on firm's efficiency in four large manufacturing sectors.[2]

4.1 Introduction

While President Trump's description of the Paris Climate Agreement as "totally disastrous, job-killing, wealth-knocking out" at the Conservative Political Action Conference in February 2018 might have been characteristically antagonistic, there is little doubt that environmental policies are consistently linked in certain circles with increased costs for businesses and

[1]This chapter is based on joint work with Dr. Corrado Di Maria.

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growth slowdowns. In other words, the suggestion seems to be that there exists an inescapable trade-off between businesses' competitiveness and environmental protection.

A starkly contrasting view has been put forward by Michael Porter, who famously suggested that such a link need not be inevitable (Porter, 1991). In fact, Porter (1991) stated that "[s]trict environmental regulations do not inevitably hinder competitive advantage against rivals; indeed they often enhance it" (Porter, 1991, p. 108). Porter's simple, if counter-intuitive, idea that, when prodded by environmental regulations, firms would find creative ways to restructure their operations – gaining competitive advantages over their unregulated counterparts in the process – has come to be known as the *Porter Hypothesis* (henceforth, PH for short).

Porter and Van der Linde (1995) expand on Porter's original intuition suggesting several channels through which the PH might arise. Crucially, they argue that since "the world does not fit the Panglossian belief that firms always make optimal choices" (Porter and Van der Linde, 1995, p. 99), there are ample opportunities for firms to make efficiency gains under the push of environmental regulations.[3]

In this paper, we concentrate on this aspect of the PH and investigate whether or not businesses are able to pick the 'low-hanging fruit' that can be found in their organization, if any. To do this, we need to focus on detailed firm-level data and on the short run reactions of firms to the introduction of (sufficiently stringent) regulation. Porter and Van der Linde's view summarized above, moreover, grates with the neoclassical tradition that views firms operating under the strict discipline of the market as intrinsically efficient, as thus also requires a methodological deviation from the mainstream to address. In what follows, we cast our analysis of the consequences of environmental regulation within a Stochastic Frontier Analysis (SFA) framework that relaxes the assumption that firms operate on the efficient frontier, thus creating room for potential production inefficiencies (e.g. Kumbhakar and Lovell, 2000). This method openly allows for the the possibility that inefficient firms coexist on the market with much more efficient competitors and is, in our view, the ideal methodological complement to Porter and Van der Linde's world view.[4]

In what follows, we use the variation caused by the design features of the 2000 Climate Change Levy (CCL) package to identify the impact of the introduction of the levy on treated firms. We do this by using confidential British firm-level manufacturing data, made available via the UK Data Service, which contains detailed information on firm performance and

[3] Ambec et al. (2013) provide an interesting introduction to the Porter Hypothesis, along with an overview of the rich literature that discusses it.

[4] This framework is also consistent with the empirical evidence suggesting the existence of staggering that total factor productivity (TFP) differences across manufacturing firms. Syverson (2011b), for example, reports that the average TFP ratio between the 10th and the 90th percentile plants in U.S. four digits manufacturing plants is 1.92.

productive inputs, as well as energy use by fuel type. We are able to estimate the impact of the CCL on firms across four large manufacturing sectors: 'Food Products and Beverages' (SIC-15), 'Basic Metals' (SIC-27), 'Fabricated Metal Products' (SIC-28), and 'Machinery and Equipment' (SIC-29). We estimate the aggregate impact at the level of each industry, and then discuss differential impacts over time and across subsets of firms with different energy intensity, as a proxy for the intensity of the treatment. Our results show that the CCL had a significant negative impact on inefficiency across all of the industries, with more nuanced results when looking at the time profile and the energy-intensity split.

To the best of our knowledge, we are the first to address the PH in the context of climate change regulation using a SFA framework. In this sense, our paper contributes to the literature on the PH in at least two ways. First of all, we are the first to use British firm-level manufacturing data across a number of two-digit industries, and thus contribute both to painting the aggregate picture and to filling in the more granular, sectoral detail. Secondly, we showcase a methodology which not only matches the theoretical underpinnings of the PH, but it also allows us to estimate the impact of CCL on firms' inefficiency without having to formulate distributional assumptions. We find this a very appealing property of our methodology. More broadly, our work contributes to the vast literature on the impact of market-based environmental regulation on firms, and thus complements efforts by other by offering variation in both methodology, geographical coverage and sectoral breakdown.

Following Jaffe and Palmer (1997a), it has become customary in the PH literature to distinguish three variants of the PH: the narrow, the weak, and the strong variant. Each variant has a somewhat different flavour and focuses on different outcomes. The narrow strand, for example, focuses on the cost of adjusting to changes in regulatory stringency, e.g. Butraw (2000); Driesen (2005). The weak strand, instead, limits itself to examining the effect of regulations on the innovative activities undertaken by firms, usually with a focus on R&D expenditures and patenting (e.g. Brunneimer and Cohen, 2003; Popp, 2006). Finally, the strong strand of this literature tries to be more comprehensive and examines the regulations' effect on business competitiveness. In so far as we concentrate in estimating improvements in efficiency recorded across firms, we share our focus with the strong PH literature. While we are broadly agnostic at this stage about the mechanisms behind the efficiency gains, it is clear that some type of change in practices occurs to bring them about. In this sense, our paper is also related to the weak PH literature, albeit with a somewhat broader understanding of the meaning innovation than found elsewhere in the literature. In the interest of brevity, however, in what follows we mostly refer to the literature that directly maps regulatory stringency and firm's competitiveness.

In an early contribution, Barbera and McConnell (1990) find that abatement costs are associated with lower TFP growth in American pollution intensive industries during the period 1960-1980. Alpay et al. (2002) find negligible and negative effects on productivity growth in the U.S. food industry, but positive effects in the Mexican counterpart. They proxy regulation stringency using abatement costs in the U.S., but with inspection frequency in Mexico. Gray (1987) examines U.S. manufacturing, find that abatement costs are associated with lower productivity growth. (Gray and Shadbegian, 2003) find similar, negative effect on the U.S. paper mills. Berman and Bui (2001) find a strong positive effect of abatement costs on productivity in the oil refineries in the Los Angeles area. Staying in North America, Dufour et al. (1998) find negative effect of abatement costs on the TFP growth rate in Québec manufacturing. Böhringer et al. (2012) find that in German manufacturing firms, environmental investment tend to enhance productivity growth, but energy and environmental expenditure doesn't.

The studies above all use static estimations. The Porter hypothesis, however, suggests a dynamic framework, as the full effects of the regulatory tightening take time to display as both innovation and market adjustments take time. Several studies confirm that the inclusion of dynamic aspects has considerable impact from the empirical point of view: Lanoie et al. (2008), like Dufour et al. (1998) before them, examine Québec manufacturing using abatement costs as their stringency proxy. They also include lagged abatement costs in the estimation, however, and find a significant positive effect on TFP growth, reverting the previous conclusions.

Only a handful of other studies have brought dynamic estimations to bear on the PH question, with mixed results. Managi et al. (2005) apply data envelopment analysis to oil and gas production data in the Gulf of Mexico. They find negative effect of abatement costs on both productivity and technical change. Broberg et al. (2013) use Swedish manufacturing data in a dynamic stochastic frontier model. They use pollution prevention and pollution control investment as stringency proxies and find no evidence in support of the PH. Rubashkina et al. (2015) examine European firms with a fixed effect regression using abatement costs as a proxy for stringency and find no evidence in favour of the PH. The latter authors are – to our knowldege – the only ones to have attempted to correct for the endogeneity of policies in their work.

The studies above have all used abatement costs or closely related measures as their proxy for the stringency of environmental regulation, albeit usually with a caveat. Berman and Bui (2001), for example, write that "abatement costs may severely overstate the true cost of environmental regulation", because abatement technologies may increase productivity, so a firm could invest in abatement technologies even without the added incentive introduced by the environmental regulation – making abatement costs at least partially endogenous (Berman and Bui, 2001, p. 509). Ambec et al. (2013) argue that large abatement costs may be caused by a number of factors other than policy, for example by the vintage of the installed capacity, or changes in energy prices. Besides, they also point out that the PH "does not posit that higher abatement costs will lead to innovation" (Ambec et al., 2013, p. 14). Similarly, Broberg et al. (2013) argue that "environmental investment will [...] not correctly reflect regulation stringency", because firms may wish to improve their green credentials by environmental investments, if they thought this might help them increase their market appeal (Broberg et al., 2013, p. 53). They also add that the use of survey questions may lead to inconsistent data, as different firms categorise their environmental investment in different ways.

Dynamic studies using different proxies paint a more nuanced picture. Leeuwen and Mohnen (2013), for example, find no effect of energy price on TFP in the Netherlands. Taking an international perspective, Lanoie et al. (2011) use a seven-country OECD survey to estimate the effects of perceived environmental stringency on business performance. They find a positive correlation between the two. Greenstone et al. (2012) find positive effect of CO regulations on plant-level TFP, but negative effect from regulations of every other pollutant. Their approach is to proxy regulatory stringency by the non-attainment status of plants/counties. Earnhart and Rassier (2016) apply a special index to examine the effect of regulations on return to sales in the US. Their results show that, if monitoring is tight the PH does not hold. If it's not tight, the PH holds. Finally, Henderson and Millimet (2005) find that, at the state-level, the elasticity of output with respect to relative abatement costs is statistically indistinguishable from zero.

The rest of the paper is structured as follows, we first introduce the institutional context and discuss the key design features of the CCL package (Section 4.2). Section 4.3 then introduces the methodology and discusses the key econometric challenges. In Section 4.4 we discuss the data, followed by the results in Section 4.5. Finally, Section 4.6 summarizes the main insights and concludes with possible directions for further research.

4.2 The UK Climate Change Levy

The 2008 Climate Change Bill forces the UK to reduce its carbon emissions by 50% until 2050. Achieving this ambitious goal requires strong economic incentives, likely to be provided by stringent environmental policies. The Climate Change Levy considered as one of the flagship policies, one of the main tools for country-wide reduction of carbon emissions (HM Government, 2006) .

The CCL is a per unit energy tax on non-domestic energy consumption, which came into effect in 2001. Table 4.1 shows that there are different tax rates for different the fuels. Though the CCL was presented as a carbon tax (certain fuels were exempt because of their low carbon content), the third column of Table 4.1 shows that depending on the fuel, the implicit carbon tax varies widely. For this reason Martin et al. (2014) argues that it's misleading to think of the CCL as a carbon tax, but it should be considered as a tax reflecting different financial and political goals.

The UK government aimed to counteract the possible adverse effects on competitiveness, so it made possible to get a 80% discount of the CCL by negiotiating a Climate Change Agreement (CCA). In the Climate Change Agreements the sector associations and the government setup a sector-wide and facility specific targets for energy use or carbon emission reductions. To be eligible a plant needs to be in an energy intensive industry and carry out at least one "qualifying activity" (the sector-specific qualifying activities are listed in Part 1 of Schedule 1 of the Pollution Prevention and Control Regulations 2000).

Martin et al. (2014) argues that targets of the CCAs weren't binding. First, there was a large overcompliance with the CCA targets. By the end of the first compliance period in 2002 almost twice of the initial targets were achieved: 4.5 MtC instead of 2.2 MtC.

Second, if the sectors as a whole met its agreed target, the individual facilities didn't have to comply to get the reduced rate in the next compliance period. This also lowers the incentives of sticking to the CCA targets. Third, there was large degree of flexibility renegiotating the agreements. Sectors could choose their baseline given the targets by the government. CCA targets could be revised and adjusted to various 'relevant constraints'. Hence it seems unlikely that CCAs were stringent and binding agreements, thus in effect, the firms participating in CCAs could be viewed as the "non-treated" group. Since selected firms could opt out from the 'treated' to the 'non-treated' group, we could think of the treatment as partially endogenous. To solve this 'endogenous switching' problem we use Heckman's (1979) model, and since the estimation is done separately for 'treated' and the 'non-treated', we could think of this problem as sample selection (see next section).

There were few evaluations of the CCL package. Ekins and Etheridge (2006) use a computational model to estimate the impact of CCAs on the energy consumption of firms. They found decrease CCA firms and argue that this is because CCA raised corporate awareness and disseminated the relevant information. Barker et al. (2007) use the same computational model designed by Cambridge Econometrics and find a large, 9.1%, average decrease in the manufacturing sector's energy use in the UK. The whole energy consumption declines by 2.6% and there's a slight increase of economic competitiveness as a result. Martin et al. (2014) were the first to estimate the effect of the CCL package with microdata. They used an IV approach to look at the economic and environmental effects of the CCL. Their instrument was the firms status (listed/not listed) in the European Pollution Emission Registry, arguing a listed status indicates a larger exposure to international environmental policies and competition. They find that CCL significantly reduced energy intensity by around 20% and electricity consumption by 7%, but no effect on revenues, employment or plant exit.

Table 4.1: Summary of the Climate Change Levy as a taxation

	Unit tax (p/kWh)	Tax rate (%)	Implicit carbon tax (£/t)
Natural gas	0.15	16.5	30
Coal	0.15	6.1	16
Electricity	0.43	10.1	31
LPG	0.07	8.2	22

Source: Martin et al. (2014), Pearce (2006).

4.3 A stochastic frontier model with sample selection

As mentioned in the Introduction, thanks to its focus on allowing and explaining inefficiencies in production and business management, stochastic frontier methods can be seen as an ideal empirical counterpart for the theoretical views suggested by the PH literature. In this section, we put forward an empirical model that will allow us to test, in the remainder of the paper, the idea that the introduction of environmental regulation may increase firms' efficiency.

Given the specific design features of the UK CCL, in what follows we describe a Stochastic Frontier model with sample selection á la Wang and Schmidt (2002) and Alvarez et al. (2006), which allows us to capture the fact that we only observe 'treated' firms if they pay the full tax. Consider a panel of N firms indexed by $i = 1, \dots, N$, observed over $t = 1, \dots, T$ consecutive periods of time. Let y_{it} be the (log of the) outcome variable of interest – the log of turnover in our empirical investigation – and $\mathbf{x}_{it}$ a vector of factors (in logs) that determine the position of the frontier. Furthermore, let $\mathbf{z}_{it}$ be a vector of variables that affect the magnitude of technical inefficiency. Generally speaking, the $\mathbf{x}_{it}$ are productive inputs, whereas $\mathbf{z}_{it}$ may either be themselves inputs, or 'environmental variables' that measure the state of the environment the firm operates in. Moreover, since we assume that the $\mathbf{x}_{it}$ and the $\mathbf{z}_{it}$ are fixed, they cannot be functions of y_{it}.

Let $y^*_{it} \geq y_{it}$ be the unobserved frontier. The linear stochastic frontier model asserts that, conditional on $\mathbf{x}_{it}$ and $\mathbf{z}_{it}$, the observations on the frontier are distributed according to $N(\mathbf{x}'_{it}\boldsymbol{\beta}, \sigma^2_v)$. It follows that the frontier can be written as

$$y^*_{it} = \mathbf{x}'_{it}\boldsymbol{\beta} + v_{it}, \tag{4.1}$$

where $v_{it} \sim N(0, \sigma^2_v)$, and is independent of $\mathbf{x}_{it}$ and $\mathbf{z}_{it}$. The model further assumes that (conditional on $\mathbf{x}_{it}$, $\mathbf{z}_{it}$, and y^*_{it}) the actual output level y_{it} equals y^*_{it} minus a one-sided error whose distribution only depends on the $\mathbf{z}_{it}$. It follows that we can write the model as

$$y_{it} = \mathbf{x}'_{it}\boldsymbol{\beta} + v_{it} - u_{it}(\mathbf{z}_{it}, \boldsymbol{\delta}). \tag{4.2}$$

We require that u_{it} and v_{it} be independent of each other and of x_{it}, and, additionally, that v_{it} be independent of $\mathbf{x}_{it}$. Following Wang and Schmidt (2002), we say that the model exhibits the *scaling property*, if

$$u_{it}(\mathbf{z}_{it}, \boldsymbol{\delta}) = h(\mathbf{z}_{it}, \boldsymbol{\delta}) \cdot u^*_{it},$$

where $h(\mathbf{z}_{it}, \boldsymbol{\delta}) \geq 0$, and $u^*_{it} \geq 0$ has a distribution that does not depend on $\mathbf{z}_{it}$. In the language of Wang and Schmidt (2002), $h(\mathbf{z}_{it}, \delta)$ is called the *scaling function*, u^*_{it} is the *basic random variable*, and the distribution of the latter is called the *basic distribution*.[5]

To the best of our knowledge, the use of the scaling property in SF models was first suggested by Simar et al. (1994a), who also pointed to some of the appealing features of the method. Perhaps the most interesting one from our point of view is the fact that the shape of the distribution of the inefficiency term is the same for all firms; the scaling factor "essentially just stretches or shrinks the horizontal axis, so that the scale of the distribution of u changes, but its underlying shape does not", in the words of Wang and Schmidt (2002). Intuitively, this is similar to assuming that the basic random variable u^* captures things like the managers' natural skills, whose realization at the firm's level comes from a random draw from the (common) distribution of skills. How well these skills are subsequently put to good use and result in the efficient management of the firm depends on other variables, that might refer to the manager's education or experience, or on measures of the environment in which the firm operates, including the degree of competition or the stringency of the environmental regulation, for example.

Assuming the frontier to be Cobb-Douglas and the scaling function to take the simple, exponential form:

$$h(\mathbf{z}_{it}, \boldsymbol{\delta}) = \exp(\mathbf{z}'_{it}\boldsymbol{\delta}) \cdot u^*_{it},$$

in the rest of the paper, we estimate the following stochastic frontier model with exponential scaling:

$$y_{it} = \mathbf{x}'_{it}\boldsymbol{\beta} + \boldsymbol{\xi}_t + v_{it} - \exp(\mathbf{z}'_{it}\boldsymbol{\delta}) \cdot u^*_{it}. \tag{4.3}$$

In the expression above, the $\boldsymbol{\xi}$'s capture the influence of Hicks neutral technical change over time. $\boldsymbol{\beta}$ and $\boldsymbol{\delta}$ are the parameters to be estimated. Following Alvarez et al. (2006); Wang and Schmidt (2002) chose the exponential function as our scaling function.

Given the discussion in Section 4.2 above, our empirical setting provides a fundamental econometric challenge: to estimate the impact of the CCL on the treated firms, we need to take into account that same plants – namely those who are eligible to enter into a CCA with the regulating agency – may decide to opt out of the treatment group and face a reduced tax rate. In other words, we face a non-random assignment to the treatment group. In this

[5] See wang2002one and alvarez2006interpreting for thorough discussions of the estimation issues arising from the model and possible interpretations.

situation, it is well known that attempting to estimate the effect of the treatment on the treated leads to biased estimates (Heckman, 1976, 1979).

Our econometric approach to correct for this potential bias follows Greene's (2010) and Lai's (2015) contributions, that extend the seminal work by heckman1976common, heckman1979sample to stochastic frontier models.

Greene (2010) shows that the standard Heckman's (1979) approach would not work in a SFA context. Heckman (1979) suggests that, to correct for the selection bias, one can try to control for the 'latent variables' that determine the selection decision. In practice, this entails estimating a two step procedure, whereby an auxiliary (probit) regression is estimated to explain the the probability that firms selected themselves into one group rather than the other. Using the estimated coefficients from the auxiliary regression, the fitted inverse Mills ratios are computed for each observation and may be included in an augmented regression model. Heckman (1979) shows that this procedure corrects the bias.

Greene (2010), however, has shown that this is unsuitable for SFA models, due to their non-linearity. Intuitively, since the inverse Mills ratio arises as a consequence of the linearity of the model, correcting with it in a non-linear model is inappropriate. Lai (2015) extends Greene (2010) to switching models and truncated normally distributed inefficiency. Lai (2015) further shows that a straightforward maximization could be unfeasible in empirical research, so he proposes an alternative two step procedure: in the first step, one estimates a probit model for the switching variable, similar to the one suggested by Heckman (1979); in the second stage, the *fitted values* of the regression are then included in an (augmented) frontier estimation. After again applying an appropriate correction to the estimation of the standard errors to account for the fitted nature of the additional regressors, the coefficients are unbiased and the (corrected) standard errors may be used for inference (e.g. Murphy and Topel, 1985).

4.4 Data

To estimate (4.2) one needs data about the inputs ($\mathbf{x}_{it}$), output (y_{it}) and the environmental variable ($\mathbf{z}_{it}$). The cross-sectional units are British firm in the production sector and t goes from 1998 to 2004.

The output is the annual turnover of firms, three inputs - employment, energy and materials, which - are from Annual Respondents Database (ARD). Capital stock comes from the Capital Stock Database, derived from the ARD. The ARD is a rich, firm-level data, which contains more than 10,000 firms, from 1973 to 2008.

The ARD is matched with the Quarterly Fuel Inquiry (QFI). The QFI is quarterly survey that contains firm-level data about the energy prices and energy consumption for several energy sources. It also has detailed information about the firms' Climate Change Levy payment (including whether or not they participate in a CCA). Since ARD and QFI contain the same sampling frame, it's relatively easy to match them following Martin (2006); Martin et al. (2014); Barnes and Martin (2002). Similarly to Martin et al. (2014), we annualise QFI by taking the yearly averages.

We restrict our sample to firms participating in four manufacturing industries: Food products (SIC 15), Basic metal (SIC 27), Fabricated metals (SIC 28), and Machinery and equipment (SIC 29). By focusing on individual industries we can credibly estimate a single production function, under which firms operate. This also helps the assumption of a common potential for efficiency.

Finally, to control for the endogenous switching bias in the Climate Change Levy (measured by taking part in Climate Change Agreements), we use the CO_2 intensity (calculated from the energy use) and energy intensity to determine the switching to reduced CCL rate, since for the discount a firm has to fulfill the binding targets of these. Notice that the energy intensity is a ratio (and not Btu/£); this is because the energy intensity target is given in terms of energy costs, not energy use.

Table 4.2: Summary statistics

Variable	Mean	Std. Dev.	N
log(Turnover)	10.1191	1.3355	2,341
log(Employment)	5.4617	0.9697	2,341
log(Real capital stock)	9.7860	1.3424	1,711
log(Energy)	16.0699	1.7906	2,341
Climate Change Levy (dummy)	0.7104	0.4536	2,341
Climate Change Agreement (dummy)	0.0748	0.2630	2,341
CO_2 intensity	4.8370	47.0345	2,341
Energy intensity	0.0002	0.0044	2,341

4.5 Results

Table 4.3 shows the effect of CCL for the four chosen sectors. The top part shows the effects on inefficiency, the bottom part shows the coefficients of the production function. In all the sectors CCL decreased inefficiency significantly, though there is variation in the size of the effect. The coefficients are large in terms of marginal effect on efficiency. For example in the Food sector (SIC 15) and the Basic metal sector (SIC 27) the effect $\exp(-2) - 1 = -86\%$ change in inefficiency. This large effect could be explained by strong competition (low inefficiency) in these sectors, so even a small improvement entails a large decrease in relative inefficiency. To see what is the actual "economic" effect of the policy, we could look at the effect on turnover:

$$\frac{\partial \ln(y)}{\partial CCL} = -\delta \exp(\delta CCL)E(u^*). \tag{4.4}$$

Unfortunately we can't estimate u^*, because of convolution problems associated with estimating two parameters for a single variable. We can however scale down the scaling function so that the product of the first two terms of (4.4) describe the effect of the CCL on turnover. Then the effect of CCL is around 27% increase in output for Food manufacturing (SIC 15) and the Basic metal industry (SIC 27), 11% increase for the Fabricated metal industry (SIC 28), and 15% increase in the Machinery and equipment (SIC 29). These are large effects, but they corresponds well with other, recent research on the effect of environmental policy. Both Calel and Dechezleprêtre (2018) and Pavan et al. (2018) examine the effect of EU ETS on manufacturing firms and find an effect of similar magnitude on the TFP, though the exact channels through which the improvements happen are unclear.

Table 4.3: Effects of CCL on inefficiency for firms (CCA=0)

	(1) SIC 15	(2) SIC 27	(3) SIC 28	(4) SIC 29
Inefficiency				
CCL	-1.984***	-2.014***	-3.523***	-3.009***
	(-5.21)	(-5.01)	(-10.68)	(-10.20)
Prod. func. [ln(Turnover)]				
ln(Capital)	0.579***	0.429***	0.409***	0.320***
	(10.19)	(7.46)	(9.44)	(9.72)
ln(Employment)	0.243***	0.145**	0.638***	0.670***
	(4.42)	(2.43)	(10.04)	(12.15)
ln(Energy)	0.136***	0.154***	-0.00112	0.0322
	(3.67)	(4.09)	(-0.04)	(1.38)
$\widehat{CCA}$	1.913**	9.053***	-0.924	155.9***
	(2.03)	(8.07)	(-0.23)	(5.23)
R^2	0.823	0.838	0.737	0.883
N	364	271	387	546
Time effects	Y	Y	Y	Y

z statistics in parentheses

* $p < 0.10$,** $p < 0.05$, *** $p < 0.01$

The coefficients of the inputs show that (unsurprisingly) the sectors have large differences in their technology, but despite these differences they're almost uniformly have approximately constant returns to scale. The coefficient of $\widehat{CCA}$ is significant in three out of four sectors which implies that the switching decision to the Climate Change Agreements entailed a strategic consideration and switching is endogenous. The large differences of the coefficients of $\widehat{CCA}$ across sectors is interesting, but it only carries information with respect to the same coefficient in a $CCA = 1$ estimation. The homogenous effect of joining a CCA is described by: $\beta_{\widehat{CCA},CCA=1} - \beta_{\widehat{CCA},CCA=0}$. Table 4.7 reports the estimations for the four sectors without any correction for endogeneity of the CCA decision. As we can see CCL has a negative sign as in the baseline estimations (Table 4.3), but it's insignificant in all sectors. Ignoring endogenous switching seems to make the estimates less precise and biases (attenuates) the effect of the CCL.

Tables 4.4 show the effect over time with post-policy year dummies interacted with the CCL dummy. The estimations all support that policy has a larger effect over time, which is in line with Porter's theory, that over time firm have time to adapt. This after-effect is largest in the Machinery and equipment industry (SIC 29) where the coefficient is -2.8 at the induction of policy until -3 in 2004, which implies about 7% of the effect happens in the years after the policy. For Basic metals (SIC 27) and Fabricated metals (SIC 28) the it is somewhat smaller with 4% and 5% of the effect appearing in later years, respectively. In Food manufacturing (SIC 15) the effect in 2004 is virtually the same as in 2001. These estimations also show that the effect of environmental policy on inefficiency is persistent.

We look at the heterogeneity of responses across firms by estimating the effect on split sample by energy intensity at the median. Table 4.5 shows the results of the estimations. The coefficients are larger for the below median energy intensity firms and smaller for the above median firms, but this translates to a lower marginal effect, because the second term of (4.4) increases more quickly than the first term does. In the Food industry (SIC 15) the below median firms have a very small average effect of 0.003%, compared to their above median counterparts which experience an 10% improvement. In the Basic metal industry (SIC 27) the divide between the below and above median energy intensity firms is also large with an effect of 4% on the below median firms and 22% on the above median firms. In the Fabricated metal industry the difference is less prounounced with 2% and 6% improvement for below and above median firms, respectively. This supports the "low hanging fruits" interpretation of our results: energy-intensive firms can improve by adopting the existing practices and technologies, if a policy motivates them to do so. Interestingly we find in the Machinery industry (SIC 29) the effect is larger in firms with smaller energy intensity: 10% for the below median firms and 5% for the above median firms. This means that the CCL has a larger negative effect on the inefficiency of firms which were already near the frontier. A possible way to explain this is that firms with less energy intensity adapt to the policy quicker. Efficient firms are more likely to take part in international trade, hence they have

Table 4.4: Effects of CCL over time on inefficiency for firms (CCA= 0).

	(1) SIC 15	(2) SIC 27	(3) SIC 28	(4) SIC 29
Inefficiency				
CCL×(Year=2001)	-1.779***	-1.973***	-3.396***	-2.840***
	(-5.53)	(-5.96)	(-10.95)	(-9.62)
CCL×(Year=2002)	-1.984***	-2.014***	-3.523***	-2.845***
	(-5.18)	(-6.03)	(-11.31)	(-10.06)
CCL×(Year=2003)	-1.791***	-2.008***	-3.465***	-2.929***
	(-5.66)	(-6.06)	(-11.17)	(-10.34)
CCL×(Year=2004)	-1.777***	-2.061***	-3.576***	-3.009***
	(-5.40)	(-6.15)	(-11.63)	(-10.69)
Prod. func. [ln(Turnover)]				
ln(Capital)	0.579***	0.429***	0.409***	0.320***
	(10.35)	(7.46)	(9.49)	(9.32)
ln(Employment)	0.243***	0.145**	0.638***	0.670***
	(4.50)	(2.28)	(9.92)	(11.96)
ln(Energy)	0.136***	0.154***	-0.00112	0.0322
	(3.76)	(4.04)	(-0.04)	(1.27)
$\widehat{CCA}$	1.913**	9.053***	-0.924	155.9***
	(2.00)	(8.01)	(-0.25)	(4.97)
R^2	0.823	0.838	0.737	0.883
N	364	271	387	546
Time effects	Y	Y	Y	Y

z statistics in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4.5: Effects of CCL on inefficiency for firms with different energy intensity (CCA= 0)

	SIC 15		SIC 27		SIC 28		SIC 29	
	Below median	Above median	Below median	Above median	Below median	Above median	Below median	Above median
Inefficiency								
CCL	-10.26***	-3.556***	-4.786***	-2.359***	-5.871***	-4.214***	-3.458***	-4.487***
	(-9.00)	(-8.65)	(-12.41)	(-5.72)	(-9.99)	(-11.51)	(-8.13)	(-13.36)
Prod. func. [ln(Turnover)]								
ln(Capital)	0.231***	0.540***	0.110**	0.314***	0.377***	0.321***	0.360***	0.195***
	(3.63)	(10.58)	(2.24)	(5.37)	(3.50)	(6.66)	(8.77)	(3.44)
ln(Employment)	0.206***	0.136***	-0.0324	0.136**	0.153	0.572***	0.552***	0.558***
	(3.39)	(3.07)	(-0.48)	(2.33)	(0.81)	(8.31)	(8.19)	(6.25)
ln(Energy)	0.145***	0.0487*	0.340***	0.223***	0.110***	0.0669*	0.110***	0.0669*
	(3.40)	(1.90)	(14.11)	(5.33)	(3.17)	(1.72)	(3.17)	(1.72)
$\hat{CCA}$	-18.90***	3.040***	5.429***	4.955***	15.47***	-0.672	-11.54***	14.46***
	(-7.19)	(3.92)	(9.27)	(6.28)	(2.58)	(-0.15)	(-4.13)	(4.72)
R^2	0.867	0.749	0.893	0.903	0.709	0.714	0.870	0.752
N	301	259	100	171	96	291	293	253
Time effects	Y	Y	Y	Y	Y	Y	Y	Y

z statistics in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

a more direct access to R&D and new technologies (Bernard et al., 2003). They also have access to global supply chains, so it's easier to substitute their (polluting) inputs. Albrizio et al. (2017b) find similar results: in their estimation environmental policy improves the performance of the top firms, but slows the growth of the laggard firms.

To test the robustness of our results we conduct several robustness checks. First, we relax the assumption of a Cobb-Douglas production function and generalise it to a translog production function. Though the main coefficient becomes insignificant, the direction is the same (negative) and statistically indistinguishable from our baseline results. Table 4.8 looks at what happens if instead of controlling for time effects (assuming that demand shocks are same within the 2-digit SIC code), we include 4-digit sector specific time effects. This implies that the demand shocks are more specific to sectors, for example in Food manufacturing (SIC 15), there's different demand shocks to the production of poultry meat (SIC 1511) and the manufacturing of margarine (SIC 1543). The results are robust to the inclusion of sector-specific time effects.

Finally, Table 4.9 reports the robustness of results to outliers. We exclude all the observations above the 99th and below the 1st percentile. The results change a bit, in Basic metals (SIC 27) and Fabricated metals (SIC 28) the point estimates are larger (in absolute value). In the Machinery (SIC 29) it's slightly lower, but these differences are insignificant when compared to the baseline estimates.

4.6 Conclusion

While there's a growing concern about the need for climate regulations, often the competitiveness concerns are highlighted in the political area as the main constraints of implementing stringent policies. The present study investigated the empirical relevance of these concerns. It found that environmental regulations actually improve competitiveness. The results support Michael Porter's hypothesis about the positive effect of environmental regulations on competitiveness. In the present, stochastic frontier, framework it was found that stringent environmental policy enhances efficiency. Specifically the Climate Change Levy in the United Kingdom increased output (given the inputs) of Food manufacturing and Basic metal manufacturing firms by approximately 27%, Fabricated metals by 11%, Machinery by 15%. These estimations strongly support the PH across sectors. The dynamic estimation provides a further support: in the long run the efficiency gains increase, as firms have more time to adapt. It's important to note that the results are robust to a variety of robustness checks and setting.

There could be several mechanisms at work here. A stochastic frontier setting naturally lends itself to the interpretation of low hanging fruits; managers systematically make suboptimal decisions, environmental policies could 'nudge' them towards the optimum. This 'nudge' could happen by simply reducing information asymmetries about best practices or highlighting input (energy) inefficiencies by increasing the marginal price of the input. The other mechanisms (e.g. new technologies, investment) of PH are unlikely to manifest immediately after the policy, though there's an indication that these mechanisms might also be at work, as the effect increases over time.

The findings of this study show that while climate change regulation do often impose costs on the economy, if well-designed they could help firms to be efficient. The Climate Change Levy package seemed to improve the efficiency in Food manufacturing, Machinery and equipment, Basic metal and Fabricated metal industries.

Appendix to Chapter 4

Table 4.6: Results of the first stage probit estimation of the selection model.

	(1) SIC 15	(2) SIC 27	(3) SIC 28	(4) SIC 29
CCA				
CO_2 intensity	0.00264**	0.00113	0.0700***	0.0300
	(2.44)	(0.15)	(3.48)	(0.15)
Energy intensity	-5687.9*	-37306.4*	-3932.4	-63258.2
	(-1.89)	(-1.92)	(-0.35)	(-0.49)
Constant	-0.937***	-0.897***	-2.278***	-2.456***
	(-14.59)	(-9.50)	(-14.85)	(-10.98)
N	572	424	613	732
Time effects	Y	Y	Y	Y

z statistics in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4.7: Effects of CCL on inefficiency for firms, without sample selection correction.

	(1) SIC 15	(2) SIC 27	(3) SIC 28	(4) SIC 29
Inefficiency				
CCL	-0.129	-0.0155	-0.144	-0.363
	(-1.51)	(-0.11)	(-0.68)	(-0.93)
Prod. func. [ln(Turnover)]				
ln(Capital)	0.583***	0.597***	0.413***	0.358***
	(12.55)	(9.81)	(9.77)	(10.26)
ln(Energy)	0.139***	0.165***	-0.00232	0.0276
	(4.51)	(3.74)	(-0.08)	(1.15)
ln(Employment)	0.272***	0.128*	0.649***	0.798***
	(5.81)	(1.95)	(10.35)	(16.41)
R^2	0.812	0.763	0.743	0.861
N	439	326	396	550
Time effects	Y	Y	Y	Y

z statistics in parentheses

* $p < 0.10$,** $p < 0.05$, *** $p < 0.01$

Table 4.8: Sectoral time effects (CCA= 0)

	(1) SIC 15	(2) SIC 27	(3) SIC 28	(4) SIC 29
Inefficiency				
CCL	-1.984***	-2.014***	-3.651***	-3.009***
	(-5.02)	(-5.09)	(-10.81)	(-10.19)
Prod. func. [ln(Turnover)]				
ln(Capital)	0.579***	0.429***	0.409***	0.320***
	(10.47)	(7.70)	(9.64)	(9.41)
ln(Employment)	0.243***	0.145**	0.638***	0.670***
	(4.41)	(2.48)	(10.18)	(11.98)
ln(Energy)	0.136***	0.154***	-0.00112	0.0322
	(3.63)	(4.07)	(-0.04)	(1.37)
$\widehat{CCA}$	1.913**	9.053***	-0.924	155.9***
	(2.01)	(8.26)	(-0.21)	(5.17)
R^2	0.823	0.838	0.737	0.883
N	364	271	387	546
Sector-specific time effects	Y	Y	Y	Y

z statistics in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4.9: 1% extreme values dropped (CCA= 0)

	(1)	(2)	(3)	(4)
	SIC 15	SIC 27	SIC 28	SIC 29
Inefficiency				
CCL	-1.984***	-2.567***	-3.790***	-2.981***
	(-4.97)	(-5.88)	(-11.44)	(-9.55)
Prod. func. [ln(Turnover)]				
ln(Capital)	0.579***	0.429***	0.404***	0.324***
	(10.43)	(7.34)	(9.57)	(9.79)
ln(Employment)	0.243***	0.144**	0.583***	0.707***
	(4.39)	(2.41)	(9.25)	(14.19)
ln(Energy)	0.136***	0.154***	0.0121	0.0298
	(3.64)	(3.85)	(0.39)	(1.28)
$\widehat{CCA}$	1.913**	9.058***	-1.083	126.8***
	(2.01)	(7.99)	(-0.27)	(5.43)
R^2	0.823	0.820	0.723	0.887
N	364	267	378	540
Time effects	Y	Y	Y	Y

z statistics in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4.10: Effects of CCL intensity on inefficiency for firms when the production function is Translog (CCA=0)

	SIC 15	SIC 27	SIC 28	SIC 29
Inefficiency				
CCL	-1.914	-3.386*	-8.366	-11.75
	(-0.89)	(-1.69)	(-0.00)	(-0.00)
Prod. func. [ln(Turnover)]				
ln(Capital)	2.377***	0.566	-0.00823	-1.036**
	(5.77)	(1.00)	(-0.02)	(-2.19)
ln(Employment)	-0.801*	1.443**	1.758***	1.975***
	(-1.72)	(2.02)	(2.69)	(2.74)
ln(Energy)	-0.564	-0.494	-0.845*	-0.786*
	(-1.53)	(-1.45)	(-1.71)	(-1.84)
ln(Capital)^2	-0.0476	0.0826***	0.0249	0.0847*
	(-1.25)	(2.86)	(0.66)	(1.89)
ln(Energy)^2	0.0418	0.0827***	0.0217	0.0293
	(2.22)	(5.14)	(0.74)	(1.58)
ln(Employment)^2	0.082**	-0.112	0.0434	0.0631
	(2.06)	(-1.53)	(0.49)	(0.72)
ln(Capital)×ln(Employment)	0.0406	0.185***	-0.107	-0.126
	(0.67)	(3.12)	(-1.23)	(-1.15)
ln(Capital)×ln(Energy)	-0.0593	-0.160***	0.0375	0.0261
	(-1.11)	(-3.14)	(0.64)	(0.64)
ln(Energy)×ln(Employment)	-0.0241	-0.112*	-0.0399	-0.0548
	(0.53)	(-1.84)	(-0.56)	(-0.94)
$\widehat{CCA}$	0.564	8.227***	-1.006	171.6***
	(0.53)	(7.45)	(-0.23)	(5.40)
R^2	0.855	0.859	0.756	0.891
N	364	271	387	546
Time effects	Y	Y	Y	Y

z statistics in parentheses

* $p < 0.10$,** $p < 0.05$, *** $p < 0.01$

Chapter 5

Conclusion

Environmental problems pose a major challenge for governments around the world. While the population demands more and more environmental protection, the perceived or real tensions between the stringency of environmental policies on one hand, and productivity and efficiency on the other hand make governments cautious when implementing these policies.

In this dissertation I aimed to tackle crucial questions such as: Do environmental policies induce innovation? Can they be used to change the direction of innovative activity? Do these innovations result into efficiency and productivity gains?

To capture innovation and competitiveness more fully, I used the methods of productivity and efficiency analysis. These methods enabled me to capture a wider variety of effects: not only technological innovation, but also innovation in management and corporate practices. Instead of proxying innovation by patent counts or R&D expenditures, I simply assumed that any change in productive activities may be viewed as an innovation (technological or otherwise). Similarly, I remained agnostic about the nature of competition and the relation between investment and productivity. Indeed, this dissertation could be viewed as an agnostic estimation of the impact of environmental policies on innovation and competitiveness.

I have largely cast my work within Michael Porter's (1991) framework, which is helpful in contextualizing many of these questions and in shaping possible policy responses: the increased biasedness of innovation as a reaction for environmental regulation may be explained by the 'weak' form of the Porter Hypothesis, whereas the possible channels through which innovation increases competitiveness may be described by the 'strong' Porter Hypothesis. For example, Porter and Van der Linde (1995) give the example of firms which participated voluntarily in the Environmental Protection Agency's 'Green Lights' program and within 2 years their initial investments more than paid off.

The first essay examined how energy prices are correlated with environmental and economic innovation, and found significant and robust associations with energy price expectations. With data envelopment analysis in this chapter, I was able to disentangle the efficiency and technical change. Overall, Hick's induced innovation hypothesis seems to hold: energy price expectations are in fact associated with environmentally biased technological change in the US metal industry. Future prices are better predictors than current prices, which conforms to economic intuition. It's not worth investing innovative activities if the prices increase only temporarily and won't hold in the future. This supports the results of the previous literature that energy prices induce energy specific innovations with a few qualifications: (1) the induced innovation hypothesis is true for inventions (i.e. for technical change), (2) that energy price expectations drive innovation rather than current energy prices and (3) not all energy prices have the same effect. Environmental innovation is found to be driven by natural gas and coal price expectations, but hindered by electricity price expectations.

The second essay investigated the other side of the question: do these induced, environmental innovations result into efficiency and productivity gains? Unlike the prior, firm-level literature the second essay looked at the outcomes at the (U.S.) state level. By using stochastic frontier analysis it used a tool fitting Porter's framework of inefficient firms. In effect, the chapter estimated a production function and examined deviation from it as efficiency shortfalls. The focus of the chapter was the relationship between the shortfalls and environmental policy. Using the stochastic framework the chapter looked at the effect of environmental policy on inefficiency, thereby testing the Porter hypothesis. To estimate the causal impact on inefficiency it used instrumental variable approach, taking advantage of the exogenous variation of thunderstorms and wind speed. The results showed that the Porter hypothesis is true; environmental regulation modestly, but significantly increases efficiency and reverse causality was found to attenuate the Porter effect. Inefficient sectors increase pollution, which increases the demand for a stringent environmental policy, so it's more difficult to observe the negative relationship between environmental policy and inefficiency. The chapter contradicts the early literature of Porter hypothesis, which used abatement costs as stringency proxies, and generally found that the PH didn't hold up to empirical scrutiny. It contributes to the newly emerging literature using different stringency proxies, which tend to find that PH is true, at least in some cases.

The third essay continued to investigate this topic, but looking at more sectors (at a more granular level) and basing the estimation on plant-level outcomes. The PH was tested by exploiting the variation introduced by the Climate Change Levy across UK plants. In this essay, I found that stringent environmental policy reduced inefficiency in all the four sectors examined (Food and beverages, Basic metal, Fabricated metal, and Machinery). The actual effect on turnover is between 11% and 27%. A large, significant effect.

The chapters do point to a few conclusions. First, inefficiencies seem to be large enough to be measurable (as defined by stochastic frontier analysis and data envelopment analysis). This implies that policy-makers and the literature should take Porter and van der Linde's (1995) claim seriously: "[i]n many cases emissions are a sign of inefficiency" (p. 105); and indeed "[t]he world doesn't fit the Panglossian belief that firms always make optimal choices" (p. 99).

The second overarching conclusion is that environmental policies do affect the economic activity of firms. The policies' effect is not always (or even most of the time) negative, however. In the dissertation, both the sectoral and firm-level estimations showed efficiency improvement as a result of environmental policy. This implies that, (at least) under certain conditions, the Porter hypothesis is true: flexible regulations enhance efficiency. Though the data limitations didn't enable a detailed investigation of the exact mechanisms involved, the stochastic frontier setting points mostly towards technology adoption, improving resource inefficiencies and possibly raising corporate awareness.

By using productivity analysis in the dissertation, I was able to capture a broader range of innovations. Innovation is often thought of in terms of a technological advancement in a product or a process. However the process of innovation must be seen as a much wider concept: productivity analysis captures any change in productive activity, be it from a more competent manager or a new software for the assembly line.

The variety of proxies for the stringency of environmental policies in the dissertation also showed that the results are likely to be independent of the specific variables. In the first essay, I proxied environmental policy stringency with energy prices, which can captures environmental policies, even though it also captures other factors (e.g. demand or supply shocks). In the second essay, I used an index number, which is more comprehensive and it only measures what it's intended to measure. Finally, in the third essay I evaluated a specific policy (the Climate Change Levy) in the United Kingdom. Evaluating a specific environmental policy improves the identification, hence the accuracy of the estimations and these estimations also confirmed the Porter hypothesis. Still the dissertation has limitations. All the essays examined the manufacturing sector, because manufacturing has clearly defined boundaries, inputs and outputs. However the share of manufacturing in total economic activity is declining; the service sector takes an ever larger slice in total economic activity. The conclusions found in the present dissertation may not be applicable in an economy where the majority of environmental regulations address the service sector.

The conclusions of the dissertation have bearing on current debates about environmental policies. It seems that the worries for costs of environmental policy in terms of competitiveness may be overstated. With badly designed policies, certain industries or under certain time horizon they might have empirical relevance, but the productive efficiency improving effects seem to be robust. This implies that there needn't be a trade-off between environmental protection and competitiveness. Or as Catherine Mann, the former chief economist of the OECD, put it: “what is good for the environment, can be good for growth, too” (Mann, 2017).

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