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Environmental policies and productivity growth – a critical review of empirical findings

by
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The economic effects of environmental policies are of central interest to policymakers. The traditional approach sees environmental policies as a burden on economic activity, at least in the short to medium term, as they raise costs without increasing output and restrict the set of production technologies and outputs. At the same time, the Porter Hypothesis claims that well-designed environmental policies can provide a "free lunch" – encouraging innovation, bringing about gains in profitability and productivity that can outweigh the costs of the policy. This paper reviews the empirical evidence on the link between environmental policy stringency and productivity growth, and the various channels through which such effects can take place. The results are ambiguous, in particular as many of the studies are fragile and context-specific, impeding the generalisation of conclusions. Practical problems related to data, measurement and estimation strategies are discussed, leading to suggestions as to how they can be addressed in future research. These include: improving the measurement of environmental policy stringency; investigating effects of different types of instruments and details of instrument design; exploiting cross-country variation; and the complementary use of different levels of aggregation.

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1. Introduction

The principal goal of environmental policies is to improve environmental outcomes, driven by the pursuit of objectives of broader wellbeing and ensuring sustainable growth. Environmental policies aim to achieve their objective by increasing the opportunity costs of pollution and environmental damage, curbing polluting behaviour, supporting investment and inducing innovation in less environmentally harmful technologies and so forth. However, they are likely to affect purely economic outcomes as well, particularly in the shorter term; these effects are of interest to policy makers when choosing to take action to improve environmental performance and selecting the relevant policy instruments.

A priori, the direction of the effect of environmental policies on macroeconomic variables such as GDP, productivity, innovation, employment, investment or trade, is far from clear. Traditionally, more stringent environmental policies have often been viewed as burdensome to economic performance – for instance by posing additional costs on producers without increasing output levels. Hence, they were considered detrimental to the profitability of the firm, productivity growth and GDP. For example, a number of studies attempted to attribute a significant part of the 1970s productivity slowdown in the United States to the increasing role of environmental policies (see Christainsen and Haveman, 1981, for a review). On the other hand, in the early 1990s, Michael Porter suggested that well-designed environmental policies might actually enhance productivity and increase innovation, yielding direct economic benefits next to the environmental benefits (Porter, 1991; Porter and van der Linde, 1995).

Empirical evidence on the effects of environmental policies on economic variables is rather weak. Many studies have been undertaken in the context of international trade, but empirical evidence on the effect on productivity is often context-specific and inconclusive. Early studies which were undertaken with the focus of explaining the contribution of environmental policy to the 1970s productivity slowdown in the United States, indeed found a negative effect on productivity growth, but more recent studies of the same phenomenon show either no effect at all or even a positive effect on productivity. Many of the attempts, in particular the earlier studies, do not address simultaneity issues, potentially attributing effects of other factors to environmental policies. Similarly, the majority of studies do not allow distinguishing among effects of different design aspects of environmental policies.

Several aspects of environmental policies matter for economic outcomes and are considered important elements of well-designed policies – in particular stringency, flexibility, predictability and competition friendliness. The objective of this paper is to review the existing empirical literature on the link between the stringency of environmental policies on the one side, and productivity and innovation, on the other. The paper also reviews the scarce literature on the effects of policy flexibility, and gives an overview of the empirical evidence on environmental policies' effects on the capital stock, investment, and trade flows. The guiding questions of this analysis are: What are the empirical findings regarding the relationship between environmental policies on the one side and productivity and innovation on the other side? Do the findings differ at the firm/industry/macro level? What may explain the differences found? Which challenges need to be addressed in future research?

Section 2 of this paper discusses the background intuition for the economic effects of environmental policies, the nature of the expected effects and potential channels through which they may work. Section 3 reviews the results of different empirical studies. Section 4 discusses challenges for empirical analysis, including an overview of the different measures of environmental policies used in the literature. Section 5 concludes with a summary of the main outstanding issues regarding investigation of such effects and provides some insight for future research.

2. Background on the channels through which environmental policies may affect economic outcomes

In a sense, the natural environment is an input into practically all economic activities. Environmental policies that guarantee the sustainable use of environmental assets can be seen as ensuring a certain level of aggregate productivity growth over the long term. While this is clearly not the only motivation for environmental policies, a counterfactual, disruptive development could mean lower overall productivity growth at some point, though potentially to the benefit of higher temporary growth in the shorter term.

Leaving the sustainability argument aside, long-term effects of an increase in the stringency of environmental policies on productivity growth, if present, are likely to hinge on the nature of the policy tool in question. Fairly static or one-off policy changes are likely to permanently affect productivity levels, but only temporarily affect productivity growth rates. Long-term changes in productivity growth could occur if, for instance, environmental policies provide permanent incentives to innovate more. This could be the case for more dynamic types of policies, increasing in stringency, such as emission caps, increasing environmental tax rates or performance standards with preannounced paths. At the same time, to the degree environmental policies decrease competitive pressures, for instance by raising entry barriers or directly discriminating between incumbents and new firms, they may pose a drag on productivity growth or innovation (see Aghion et al., 2005; Aghion et al., 2004; Bourlès et al., 2010; for discussions on entry barriers and competition). In the absence of more dynamic incentives, there are still various views on the direction and magnitude of short-term and medium-term effects, as discussed later in this section. While much of the short to medium term discussion applies primarily to traditionally-measured multifactor productivity (MFP) growth, many of the arguments can also be made in the context of productivity measures that are adjusted for environmental inputs.

2.1. The conventional argument – diverting resources from productive allocations

Traditional views of environmental policies tend to see them as a cost or burden to economic activity, at least in the short to medium term. Leaving aside the long-term sustainability argument, compliance with environmental policies generally forces firms to devote some part of inputs to pollution prevention and abatement, which are not traditionally considered as value added, or to curb production (Jaffe et al., 1995; Ambec et al., 2013). The effects can be both direct – with firms' costs rising directly due to pollution abatement – and indirect, such as through increases in input prices in the industries affected by the regulation (Barbera and McConnell, 1990).

More specifically, policies such as environmental taxes, tradable emission permits, or water and wastewater treatment charges impose an additional production-related cost for something that would be free otherwise, hence can induce firms to devote resources to reduce costly output. A similar argument can be developed for performance standards. Technology-based standards and restrictions or bans on certain substances can limit the range of production technologies available and otherwise profitable production output. In principle, and abstracting from market imperfections for the moment, if productivity could be increased by curbing environmental externalities, firms would have done so already, even in the absence of environmental policy. Hence the effect of environmental policies on productivity should be negative or, at best, neutral.

The effects of environmental policies on productivity might, however, be more complex as indirect effects work through a variety of channels. In the short term, environmental policies may actually improve productivity in some specific activities – for instance industries using water as an input may benefit from the fact that water becomes cleaner, via reduced inputs devoted to water purification (Jaffe et al., 1995). Similarly, workers may become more productive if the adverse effects of air pollution on their health are curbed (Ostro, 1983, or more recently Graff Zivin and Neidel, 2012). Similarly, to the degree that some environmental policies provide budgetary revenues, the use of these may have effects on aggregate productivity – for instance if these are used to reduce distortionary taxation or reduce relative labour costs – as often argued in light of the double-dividend of pigouvian-style taxes.

Indirect effects may come in the form of effects on the survival rate of firms. If, by imposing additional costs, environmental protection eliminates less efficient firms from the market, aggregate productivity is likely to rise. At the same time, additional (entry) costs, or vintage differentiated regulations can discourage entry and exit, reducing competition in the market, shielding potentially inefficient incumbents and obsolete capital stock, thereby leading to lower productivity levels and growth (Mohr and Saha, 2008; OECD, 2006; Heyes, 2014). Some further empirical evidence suggests that the plant birth rate is negatively influenced by environmental regulations (Gray, 1997).

Finally, environmental policies may also foster the creation of industries or activities that would otherwise not exist or not benefit from economies of scale. The effects on productivity, while unlikely to be large on aggregate, are complex and uncertain *a priori*. For example, regulations that imply certain monitoring requirements may directly decrease the productivity of the sector concerned but may also boost both monitoring services and the production of relevant equipment – providing a first-mover advantage, if other countries eventually adopt similar laws. Municipal waste and wastewater treatment laws can induce new demand for such services and foster the growth of companies providing dedicated services. To the extent that such environmental policies involve public subsidies, aggregate effects need to take into account that these funds are taxed away from productive activities.

The extent to which different effects of environmental policies can be observed will depend on the level of aggregation. The plant and firm level allow observing the effects on changes in production processes and technologies, the diversification or changes in activity and products and the related potential costs and synergies. Industry level analysis will pool some of these effects together with those of firm entry and exit, and changes in firm size. It will also depend on the actual level of industry aggregation. At the country level, changes in the industrial structure both as a result of policies and as result of changes in environmental quality are also likely to be present – but on the other hand, can be lost in the process of aggregation or cumbersome to identify. All the listed direct effects can be further complicated by spillover effects across firms, industries and countries as well as trade (Sauvage, 2014).

The overall rather negative perception of the effects of environmental policies on productivity growth stems partly from the nature of traditionally-measured MFP growth, which does not take into account environmental effects. In a simplified setting, it can be shown that environmental policies will negatively affect traditionally-measured MFP by definition (Box 1), even when they have no effect on broader productivity. Such ideas have led to various attempts at calculating adjusted MFP, or so-called "green" MFP, (see for instance Repetto et al., 1997; or Brandt et al., 2014) which would take into account the fact that pollution, or more broadly environmental services, can be seen as either an omitted output (i.e. side-product) or an omitted input. This concept also lies behind the distance function approach taken in several macro-level studies and discussed in the section below (Box 2).

2.2. The Porter Hypothesis – a free lunch?

If policies actually increase innovation they can enhance productivity growth. The general idea behind induced innovation goes back to Hicks (1932), and hinges upon the notion that increasing a factor's cost should spur innovation to economise on the use of this factor.[1] In the 1990s, Michael Porter argued that indirect effects of well-designed environmental regulatory instruments might induce firms to innovate, which in turn might increase productivity and hence profitability – potentially outweighing the increase in abatement costs (Box 3). In this vein, environmental regulation which would set dynamic, long-lasting incentives to innovate more could increase both the level and the growth rate of productivity. These indirect effects might come through changes in the composition of inputs or through changes in the whole production process as the new regulation might shift the production function through stimulating the invention and adoption of new technologies. *A priori*, it is however unclear whether these indirect effects are negative or positive, or whether they are large enough to outweigh the drag of the direct effect. As Barbera and McConnell (1990) point out, more traditional inputs such as labour and capital might be needed as complements to the abatement capital and hence lead to a negative indirect effect. On the other hand, old capital might be replaced with new investment, thereby increasing efficiency and leading to a positive direct effect.

Porter's arguments have met significant scepticism, though they had an important influence in environmental economics. Critics generally focus on the free-lunch argument, that is, that if there were productive opportunities available, they would have already been exploited by the firm. It is also unclear why policy-makers would be better aware than firms of the existence of these *win-win* areas to firms. Defendants tend to make the argument that because of market failures (e.g. information asymmetries), firms may be unwilling or unable to take a certain risk or action with net expected gains (Porter and van der Linde, 1995). Notably, Porter's arguments tend to rely on "*well-designed*" regulation. This

Box 1. **Effects of environmental policies on measured MFP growth**

Environmentally-adjusted measures of productivity growth are not aimed at answering the question about productivity effects of environmental policies *per se*. These productivity measures are rather developed to improve the measurement of productivity in the first place and can then be used to conduct analyses of the impact of environmental policies. Even assuming away any effects of environmental policies on actual MFP growth, traditional measures of MFP growth are likely to show changes due to the omission of environmental services in the production function. The intuition behind this can be demonstrated in a simple Cobb-Douglas production function with constant returns to scale and three inputs, capital (K), labour (L) and environment (E):*

$$Y = AK^{\alpha}L^{\beta}E^{1-\alpha-\beta} \quad [1]$$

taking logarithms and differentiating with respect to time:

$$\hat{A} = \hat{Y} - \alpha\hat{K} - \beta\hat{L} - (1-\alpha-\beta)\hat{E} \quad [2]$$

where $\hat{X}$ refers to the growth rate of X (i.e. $\frac{dlnX}{dt}$).

The Solow residual is the actual or "environmentally adjusted" MFP growth, in the sense that it captures the difference between the increase in outputs and the increase in capital, labour and environmental inputs used to produce them. At the same time, if the role of environmental inputs is ignored in the basic production function:

$$Y = B * K^{a}L^{1-a} \quad [3]$$

the "measured" growth in MFP is equal to:

$$\hat{B} = \hat{Y} - a\hat{K} - (1-a)\hat{L} \quad [4]$$

Assuming that the relative cost shares of labour and capital are not affected by the choice of measurement approach i.e. $\frac{a}{1-a} = \frac{\alpha}{\beta}$, equation [2] can be substituted into [4], which after rearranging gives:

$$\hat{B} = \hat{A} + (1-\alpha-\beta)\left[\hat{E} - (\frac{1}{\alpha+\beta})\left(\alpha\hat{L} + \beta\hat{K}\right)\right] \quad [5]$$

In which case, "measured" MFP growth $\left(\hat{B}\right)$ is equal to "actual" MFP growth $\left(\hat{A}\right)$ plus a bias. As the share of environmental services gets close to zero, this bias tends to disappear as $\left(\alpha+\beta\right)$ approaches 1.

Assuming a new or a more strict environmental policy affects the (implicit or explicit) price of environmental inputs without affecting the prices of labour or capital, it will reduce the use of environmental inputs $\left(\hat{E}\downarrow\right)$ and increase the use of labour $\left(\hat{L}\uparrow\right)$ and/or capital $\left(\hat{K}\uparrow\right)$. This means the bias will go down in levels. In a more complicated setting this could affect cost shares, but for simplicity this effect is ignored here.

If actual MFP growth $\left(\hat{A}\right)$ is unaffected (as assumed initially), the lower bias implies that the growth of measured MFP $\left(\hat{B}\right)$ will inevitably decrease. The intuition behind this measurement phenomenon can be found in Repetto et al. (1997): if the environmental policy change results in the substitution of part of a previously unobserved input (E, which in this case is treated as part of the residual, B) with an observed input (K or L, for instance through the installation of pollution abatement equipment or hiring of monitoring personnel), leading to lower observed productivity. As the bias depends on the environmental policy regime in place, it might change with a change in environmental policy. One-shot policies and long-term incentive-based policies may have different effects on the over-time prevalence of such a change in the bias.

* Environmental inputs can include any type of environmental services, such as natural resources, sink functions for pollution or land use.

makes inference from empirical estimations increasingly difficult because it can often be argued that the insignificant or negative effects of a *specific* environmental regulation on productivity come from its poor design, not from a negative effect *per se*.[2]

Box 2. **Measuring productivity to account for environmental services**

Environmental services, such as the use of natural inputs and sink services provided by the environment are ignored in traditional measures of productivity. In response, an increasing number of studies attempt to incorporate some of these services. Two methodologies dominate the literature – a distance function approach and an adjusted growth accounting framework.

In the distance function framework (including data envelopment analysis, DEA; and stochastic frontier analysis, SFA) quantity data on inputs, traditional outputs and "bad" outputs are used to determine the technological production frontier, describing all possible efficient input/output combinations. For each possible input-output combination, the distance function then measures the possible efficiency gain of moving from an inefficient point on to the frontier. Studies that evaluate the effects of environmental regulation on productivity often model three scenarios by varying the assumption about the bad output: free disposability of bad outputs; a constant level of bad outputs while good outputs increase; and, a reduction of bad outputs at the same time as good output increases.

The second approach, based on a growth accounting framework, adjusts traditional productivity growth by the weighted difference of bad output and input growth. This requires assumptions on explicit shadow prices of pollutants (for an application see Brandt et al., 2014).

In practical applications, a key problem may be linked to the choice of environmental "bads" and their implicit weights, which depend on the underlying shadow prices. Studies tend to differ vastly in this respect, hindering comparability and generality of conclusions. Among bad output measures used are CO_2 emissions (Wu and Wang, 2008), a combination of air and water pollutants (Yoruk and Zaim, 2005), air, water and toxic pollutant releases (Boyd and McClelland, 1999). Inevitably, such adjustments need to limit the bad outputs to a handful of environmental issues, meaning they may also hide an increase in other, non-measured environmental bads. Shadow prices of pollutants are rarely available and need to be estimated separately or proxied with observed prices, for example resulting from policies such as environmental taxes. Both approaches are fragile to the underlying assumptions. The distance function framework allows calculating implicit shadow prices of pollutants, which are the basis for the productivity adjustment. As most of the studies do not attempt to calculate these prices, it is often not possible to assess whether they take reasonable values or compare them to social costs of pollutants, such as those estimated from life and health valuations, or those derived from surveys of revealed preferences – the willingness to pay for reducing certain environmental externalities or willingness to accept a certain compensation for these.

Source: Brandt et al. (2014).

2.3. Trade, foreign direct investment and competitiveness

A large literature directly links environmental policies to competitiveness and trade outcomes (see Ambec et al., 2013; Wagner, 2003; Jaffe et al. 1995, for reviews). This literature is closely linked to the theory of pollution havens – pollution-heavy firms and investment relocating to environmentally lax countries. On the other hand, a number of

Box 3. **Various versions of the Porter Hypothesis and some of the theoretical underpinnings**

In the early 1990s, the debate around the economic impacts of environmental regulation gained new momentum through an article written by M.E. Porter. He claimed that properly designed environmental regulation can trigger innovation which in turn can decrease, and even offset the costs of pollution abatement and enhance competitiveness (Porter, 1991; Porter and van der Linde, 1995). Initially, the Porter Hypothesis was suggested without any theoretical explanation of the factors at work or any comprehensive empirical evidence aside from a few case studies which were collected by the authors.

The theoretical explanations for the possible existence of a Porter effect were only developed at a later stage. Generally speaking, the Porter Hypothesis assumes that there are profit opportunities for firms which are not fully used until the firms are pushed to do so by the implementation of a new environmental policy. A comprehensive overview of the development of the different theoretical arguments to provide a foundation for the Porter Hypothesis can be found in Ambec et al. (2013) and Wagner (2003). Behavioural arguments are based on the idea that managers may be risk averse, myopic or rationally bounded and hence may not be able to realise all profitable investment opportunities. Environmental regulation might then require certain investments which turn out to be profitable. Another approach hinges on the presence of market failures, such as imperfect competition (due to first-mover advantage or barriers to entry) or high entry costs, asymmetric information (where "green" products are not correctly valued by consumers), R&D spillover effects (as innovation has a public good character and leads to underinvestment), co-ordination failure and organisational failure (where managers are able to lie about the true abatement costs in order to secure extra personal profits).

In order to find feasible empirical testing approaches, the Porter Hypothesis was divided into different sub-aspects which are nowadays known as the weak, strong and narrow version of the Porter Hypothesis (Jaffe and Palmer, 1997, were the first to differentiate the various aspects):

- The *weak version* of the Porter Hypothesis implies that environmental regulation will lead to an increase in environmental innovation. As Jaffe and Palmer (1997) describe, the firms subject to the new environmental regulation face an additional environmental constraint next to their financial ones. As firms are assumed to maximise profits, they will search for the most cost effective way to comply with the new regulation. The Porter Hypothesis suggests that firms will do so by innovating to reduce compliance costs, but does not necessarily imply more innovation in total.
- The *strong version* of the Porter Hypothesis claims that cost savings from the improved production processes are sufficiently large to increase competitiveness. It rejects the assumption of perfect markets with profit maximising firms and assumes instead that firms are not operating fully efficiently by leaving some profit opportunities unused. Environmental policies might hence induce the firm to rethink their production process. This might lead to extra profits which can in some cases be even larger than the cost of compliance. Various versions of this hypothesis have been tested, including the effects on more innovation and on actual company performance.
- Jaffe and Palmer (1997) describe also the *narrow version* of the Porter Hypothesis – *certain types* of environmental regulation, those which are designed to target the outcome rather than the design of the production processes, are more likely to increase innovation and improve company performance. In practice, this has been approached by looking at how the use of more flexible or market-based instruments affects environmental and total innovation, respectively.*

* Following De Serres et al. (2011) this review uses the term "market-based instruments" for environmental policy instruments aimed at addressing market failures mainly through price signals. This can include taxes, charges and fees, tradable permits, and subsidies for reducing pollution. Non-market approaches include direct environmental regulations, standards ("command-and-control" instruments), and voluntary approaches.

scholars emphasise the first-mover advantages and accompanying gains in the technological frontier. By exploiting and affecting competitive advantages, international trade can also mitigate potential adverse effects of environmental policies on productivity outcomes, meaning desired environmental outcomes can be achieved at lower economic cost. These perspectives are in principle compatible with Porter's ideas – specialisation and relocation may be an efficient response and does not preclude improvements in productivity, both at home and abroad. Productivity is a key driver of competitiveness, but given that competitiveness is driven by several other key aspects, in particular costs and location, and is a relative rather than an absolute concept (Krugman, 1994), a detailed review goes beyond the scope of this paper.[3]

Research in this area indicates that environmental policies are unlikely to be major determinants of a country's competitiveness, trade or FDI patterns (see Jaffe et al., 1995; Brännlund and Lundgren, 2009, Ambec et al., 2013, for reviews). This is probably because environmental costs are generally a small fraction of total firms' costs, which would also suggest relatively small effects on productivity growth. Competitiveness effects may also depend on country characteristics such as the level of development and income, trade openness, industrial structure and environmental, labour and other endowments, making the comparison of results of studies across different countries and periods difficult. Notably, the competitiveness literature has usually concentrated on the stringency of policies, rarely attempting to discriminate between different types of environmental policy tools.

3. Empirical evidence

In this review empirical research on the economic effects of environmental policies is classified according to the macroeconomic measure analysed and the level of aggregation of the study. Details of the studies cited regarding scope, methodology and results can be found in Appendix A, Table A.1.

3.1. Productivity

The majority of studies on the productivity effect of environmental policies are conducted at the firm- and industry-level, with only a few papers adopting a macroeconomic view. Common approaches include cost-function estimates, growth accounting and efficiency measures adjusted for environmental outputs (Box 2).

3.1.1. Firm- and plant-level studies using traditional productivity measures

Plant-level studies tend to compare traditionally measured productivity growth among regulated and non-regulated plants, with estimated effects being overall rather negative but not very robust. Effects of environmental regulation on MFP growth are found to be negative (Gollop and Roberts, 1983; Smith and Sims, 1985) or positive but insignificant (Berman and Bui, 2001a, in this case on productivity levels). None of these studies are able to control for potentially different characteristics of the two groups of plants, or to make a convincing case for ignoring such differences, which is a flaw in these types of fixed-effect or difference-in-difference approaches. Studies that do attempt to control for plant-level characteristics, including self-selection of firms into countries with more lax environmental regulation, tend to find many of them as significant determinants of productivity (for example, Becker, 2011; in the case of labour productivity).

Even within one industry (pulp and paper in this case) effects of environmental regulation on plant productivity can depend strongly on plant characteristics. Gray and Shadbegian (2003) show that integrated mills, subject to stricter environmental regulation due to the integrated pulping process, show a significant reduction of productivity due to increases in abatement costs, whereas productivity of non-integrated mills does not show any substantial reduction. Similarly, Becker (2011) finds no effect of environmental regulation on labour productivity levels in a broad sample of plants. Reducing the sample to plants which experienced a statistically-significant change in compliance costs over the years (only one-tenth of the sample), the study finds a negative effect of compliance costs on labour productivity.

At the same time, the effects of pollutant-specific regulations on productivity levels can be very different, depending on the pollutant. Greenstone et al. (2012) find a persistent negative effect of total environmental regulations, while ozone regulations and particulates emission regulations are estimated to have a negative effect on productivity levels, sulphur dioxide emission have no significant effect and carbon monoxide regulations even foster productivity. The paper does not discuss the reasons for such differences in outcomes, which may come from a number of areas, for instance be due to different prevention and abatement technologies readily available in case of each of the various pollutants.

Aside from methodological issues the conclusions from the bulk of studies suffer from the lack of generality. They evaluate very specific regulations, focus on firms in specific industries (electric power; brewing; pulp and paper; and manufacturing respectively) in different countries (United States and Canada) or areas (coastal California), different time periods and horizons. Only Greenstone et al. (2012) account for plant closure while Becker (2011) and, to a lesser extent Gray and Shadbegian (2003), explicitly control for some firm characteristics that may drive productivity. None of the studies attempts to control for spill overs across firms.

3.1.2. Firm- and plant-level studies using alternative measures of productivity

A number of more recent studies take into account bad outputs in the production process – and hence in the calculation of productivity – with the help of data envelopment analysis or stochastic frontier analysis (DEA and SFA respectively, Box 2). Interestingly, even in this case, the results are far from uniform. Negative effects on adjusted productivity are found by Broberg et al. (2012) for the heavily regulated Swedish pulp and paper industry, with no effect on the manufacturing sector. The specification for the pulp and paper industry however, seems questionable due to an overall poor fit of the model – as opposed to the pooled data sample which includes several industries; none of the traditional factors of the production function are found significant. Fleishman et al. (2009) find a multitude of different effects of the presence (but no effects of stringency) of air pollutant regulations for US power plants – positive, insignificant and negative – depending on types of plants and types of regulation. Positive effects of more stringent policies on adjusted technical efficiency are found in the Dutch horticulture sector (Van der Vlist et al., 2007) and companies under the EU ETS (Jaraite and Di Maria, 2012). Both studies examine fairly narrow definitions of stringency and do not investigate medium- to long term effects. Managi et al. (2005), who allow for time-varying effects, find negative short-term effects of regulation on productivity levels in US offshore oil and gas fields, which disappear over time, eventually becoming positive and outweighing the initial drag on productivity.

3.1.3. Industry-level studies

The empirical evidence produced by industry-level studies is even more ambiguous. Early studies tend to find a negative effect, while more recent ones suggest a positive link between environmental regulation and productivity. These older studies tend to suffer from serious identification problems. One of the more widely cited studies that attempts to explain the US productivity slowdown in the 1970s with environmental regulation finds strong significant results, which disappear upon the inclusion of controls or elimination of outliers (Gray, 1987). Other studies that find negative effects on productivity growth either are unable to control for industry characteristics (Barbera and McConnell, 1990) or do so, but remain questionable due to a small sample (Dufour et al., 1998).

As in the case of plant-level studies, the industry studies analyse rather specific cases – countries, a handful of very specific industries (five in Barbera and McConnell, 1990; Hamamoto, 2006), and specific environmental laws. While Gray (1987) and Yang et al. (2012) analyse a set of several hundred industries, in other cases the cross-section dimension is more limited. The somewhat longer time series available allows some studies to attempt to look at the time dimension of effects (Hamamoto, 2006; Lanoie et al., 2008). The results potentially reflect the bulky nature of up-front expenditures in reaction to changes of environmental laws – finding the negative contemporaneous effect outweighed by the subsequent positive effects of regulation on MFP growth (Lanoie et al., 2008). On the contrary, Hamamoto (2006) finds significant positive effects of command-and-control regulations in Japan – on R&D spending and in a second step on productivity growth – which tend to decline over time.

Differentiating between pollution abatement fees and pollution abatement capital expenditures can allow for dealing with some of the simultaneity problems of the Pollution Abatement Costs and Expenditures (PACE, see Box 4) environmental proxies. Capital expenditures are found to have no effect while abatement fees are found to stimulate R&D and higher productivity (Yang et al., 2012) , though the link between this (environmentally) induced R&D and MFP levels is found weaker than in terms of general R&D spending.

Adjusting productivity for "bad" outputs in a DEA framework does change the overall mixed conclusions – Domazlicky and Weber (2004) find no evidence of a link between environmental regulation and adjusted productivity growth in the US chemical sector. The only cross-country study, Alpay et al. (2002), similarly finds no effect on adjusted productivity growth on the US food manufacturing sector, but a positive one in Mexico.

3.1.4. Macroeconomic studies

Empirical evidence at the aggregate economy level is very limited, largely because of data and identification problems. The approach taken by all of the studies surveyed is to include bad outputs in a distance function framework and hence allow crediting the reduction of pollution. Evidence is fairly inconclusive – scenarios with constant bad outputs (as a result of environmental policies) find negative effects on adjusted MFP growth (Jeon and Sickles, 2004) or slightly positive effects in a different sample of countries (Wu and Wang, 2008).

Interestingly, even in cases where the ratification of the UNFCCC is used as a proxy for environmental policy stringency, the effects on adjusted MFP growth can be positive (Yörük and Zaim, 2005) or negative (Wu and Wang, 2008). Explanations may relate to different samples – Yörük and Zaim (2005) focusing on OECD economies, Wu and Wang (2008) on

Box 4. **Abatement expenditures as proxy for stringency of environmental regulation**

Firm and industry-level data on environmental protection expenditure, defined as expenditures on "purposeful activities aimed directly at the prevention, reduction and elimination of pollution or nuisances arising as a residual of production processes" (OECD, 2007), have been collected since the 1970s by national authorities and the OECD. The most known example is the PACE – a plant-level survey that asks questions on pollution abatement capital expenditures and operating costs associated with compliance to local, state, and federal regulations and voluntary or market-driven pollution abatement activities. The PACE survey was conducted by the US Census Bureau between 1973 and 1994 (annually, with the exception of 1987), and discontinued thereafter. The Environmental Protection Agency picked up the bill for a 1999 survey, which, however, had significant conceptual differences relative to the previous waves, complicating time-series analysis (Becker and Shadbegian, 2004). A new survey followed in 2005, and is for the moment the last one conducted. A similar, though not directly comparable concept governs the Joint OECD/Eurostat Questionnaire on Environmental Protection Expenditure and Revenues (EPER), which commenced in the late 1990s for EU countries, and several other exercises across countries such as in Canada or Korea.

Empirical studies that use environmental pollution abatement costs as a measure of environmental policy stringency rely primarily on the assumption that higher environment-related expenditure is induced by more strict environmental policy. In practice this concept faces a number of drawbacks, some of which are emphasised in an international context. *First* of all, the data are often not easily comparable across countries and over time. *Second*, it is difficult to classify expenditures – for example distinguishing what share of costs of a change in technology is driven by environmental policies and what share by profits. This relates to the issue of counterfactuals, that is identifying an expenditure scenario without environmental policies. The self-reporting nature of the exercise may exacerbate such issues, as different firm (and country) characteristics may influence responses. In fact, Broberg et al. (2012) claim that the pulp and paper industry generally categorises investments into closed-loop systems, which recycle and reuse wastewater, as environmental investment even though it is purely driven by profitability considerations. Over-reporting may also be a result of attempts by industries to gain a "green" image or to signal voluntary efforts in order to avoid hard policy intervention. On the other hand, Berman and Bui (2001a) claim that costs of abatement are often incompletely measured, such as for example a switch to another fuel where the extra cost of this switch is not reported. Arguments in both directions are also provided by Brunnermeier and Cohen (2003). Moreover, the share of such types of expenditure may also reflect regulations that are external to environmental policies – for example stricter safety regulations may improve safety (and hence lower the need for environmental pollution abatement). Finally, there is a sample selection issue: if environmental policies lead to changes in the industry structure, with firm entry and exit, these phenomena may not be easily captured with firm-level questionnaires.

Source: Becker and Shadbegian (2004); OECD (2007).

APEC economies – or differences in the set of bad outputs used to adjust MFP growth – Yörük and Zaim (2005) using an average of air and water pollutants, while Wu and Wang (2008) focus only on CO_2. More generally, in both cases the protocol ratification dummy seems rather prone to capturing a large number of other effects – an issue that would require further investigation.

3.2. Innovation as a driver of productivity growth

Innovation is a central aspect of productivity growth and has been studied in the context of effects of environmental policy stringency. The "weak" version of the Porter Hypothesis (Box 3) – more stringent environmental regulation will increase environmental innovation[4] – is basically a variation of Hick's (1932) argument and is fairly well supported by empirical evidence. On the contrary, there seems to be little evidence of firms actually innovating more overall:

- At the plant-level, stringency of policy tends to have a fairly strong effect on the decision to engage in environmental R&D (Johnstone and Labonne, 2006; Arimura et al., 2007; Lanoie et al., 2011; Yang et al., 2012).
- At the industry level, Jaffe and Palmer (1997) and Hamamoto (2006) find a positive effect of more stringent regulations on total R&D expenditures. However, Jaffe and Palmer (1997) test and fail to find any effect on actual patents. Essentially re-running the former study for environmental patents Brunnermeier and Cohen (2003) find a small but significantly positive effect. In a similar vein, Kneller and Manderson (2012), find that in UK manufacturing industries there is a positive relation between the stringency of environmental regulation and environmental R&D expenditure, but not total R&D expenditure.
- On a macro, cross-country level, some weak evidence of tighter environmental regulation on environmental innovation has been documented (Lanjouw and Mody, 1996; Popp, 2006; De Vries and Withagen, 2005).[5]

None of these studies looks into spillover effects across firms and industries – for instance the fact that more stringent environmental regulation may spur innovation in industries or firms not directly affected by the regulations, but result in improved "general-purpose" technologies (e.g. in information and communication technologies, electronics, chemicals or pollution abatement equipment and services). Furthermore, new evidence suggests that previous studies may have underestimated the effects on environmentally-friendly innovation by focusing only on domestic effects. Environmental policies can also promote foreign innovation (Dechezleprêtre and Glachant, 2014) and technology transfer (e.g. in the automobile sector, Dechezleprêtre et al., 2013).

Overall, the general tendency to find positive effects on innovation contrasts somewhat with the dimmer picture when looking at actual productivity outcomes, as in the preceding section. This may be linked to the inherent challenges of actually measuring innovation, often proxied with either an input based (R&D spending) or output based (patent counts) measure. Either type of measure is however imperfectly related to overall innovative activity and even less perfectly with resulting improvements in productivity. A further possible explanation could be linked to a tendency of firms to increase environmental R&D budgets at the expense of general R&D. Others could include the decreasing returns from the increased environmental innovation (as in Hamamoto, 2006; Yang et al., 2012). Finally, some of the effects could result from flaws of using self-reporting measures, such as PACE as proxy for environmental policy stringency. Purely technically, the time dimension may play a role – an increase in R&D capital or personnel, even if potentially increasing productivity in the future – can be expected to decrease measured productivity contemporaneously (Box 1). While there is little evidence of actual increases in R&D budgets, structural changes in the R&D direction may still imply lags in terms of productivity.

3.3. Effects on capital stock and investment

As environmental regulation often induces investment into pollution abatement capital (shown empirically by e.g. Gollop and Roberts, 1983; Berman and Bui, 2001a), theory suggests that this "abatement investment" might crowd out "non-environmental investment" into productive capital, potentially putting future productivity at risk. The empirical evidence on a possible crowding-out effect of investment is mixed. As Jaffe et al. (1995) conclude in their literature survey, there is no clear empirical evidence for a decrease in total investment due to necessary pollution abatement investment. Some evidence of crowding out has been observed (Rose, 1983; Gray and Shadbegian, 1998, 2003), while other studies found no effects (Kneller and Manderson, 2012). Still, the bulk of studies include at most a one year lag, failing to account for the fact that productivity-relevant effects on capital investment are likely to be of a longer-term nature.

Stricter environmental policies can also bring forward the obsolescence of existing capital. On the other hand, they may decrease entry (and exit), lengthen the life of polluting capital and actually delay investment (Heyes, 2014). There are only very few studies that analyse the effect of environmental regulation on the age of capital stock. The "modernising effect" of the capital stock (Xepapades and de Zeeuw, 1999) might be triggered by a downsizing of the capital stock or by investment into new machines, as confirmed by Hamamoto (2006) who finds a decrease in the average age of the capital stock in Japanese manufacturing in response to more stringent policies. On the other hand Nelson et al. (1993) find an increase in the age of capital stock, meaning a reduction in reinvestment in capital, in a sample of electric utilities in the United States. The different results may be linked to the market structure in the sectors examined and specific aspects of environmental policies, in particular if vintage differentiated laws are adopted – as for instance in the case of the New Source Review of the US Clean Air Act.

3.4. Evaluating the effects of policy instrument flexibility

More flexible environmental policy instruments are, under certain conditions, seen as achieving environmental goals in combination with superior economic outcomes due to their static and dynamic efficiency properties (see De Serres et al., 2011 for a review). Hence, the so-called "narrow" version interpretation of the Porter Hypothesis, stipulates that more flexible, market-based instruments will stimulate more innovation than less flexible command-and-control policies (Box 3). Few studies allow for testing such a hypothesis – most focus on either a single policy change or on a measure of policies that does not allow distinction between different instruments.

As a result, only a handful of studies look at whether more flexible instruments cause more environmental innovation. Both Johnstone and Labonne (2006) and Lanoie et al. (2011) find marginal and rather fragile evidence for performance standards yielding more innovation, in the latter case with respect to the less flexible technological standards. At the same time, they find no effect of taxes with respect to other instruments. Johnstone et al. (2010b) find some evidence of perceived flexibility of environmental policies increasing patenting behaviour when applying a factor analysis to extract the variation in cross-country perceived flexibility, which tends to be highly correlated with stringency.

Arimura et al. (2007) do not find any different effects of voluntary approaches relative to command-and-control measures. Possible explanations for the above findings are the difficulty of assessing the actual flexibility of an instrument and omitted variables. The fact that different types of policy instruments are effective for stimulating innovation activity in different sectors might also lead to contradicting results of studies. In the renewable energy sector for example, broad policies, such as tradable energy certificates are more effective in more mature technologies, while more targeted subsidies, such as feed-in tariffs are more effective in case of more costly technologies (Johnstone et al., 2010a).

The only study that looks at overall innovation effects, Hamamoto (2006) claims that in Japan in the 1960s and 1970s command-and-control policies did trigger higher overall R&D activity but that a SO_X charge, which was introduced later, did not.

4. Challenges for empirical implementation

Neither the choice of the set-up nor the identification strategy of empirical analyses of the economic effects of environmental policies is straightforward. The choice of the level of aggregation determines which effects can and cannot be captured in the analysis. The choice of variables and datasets are equally challenging, in particular with respect to the timing of effects. Analyses of the effect on innovation face the problem of measuring the latter through proxies. A clear, basic challenge confronting all studies is the measurement of the policy variable – that is finding a suitable, measurable variable for the stringency of environmental regulation.

4.1. Measuring environmental policies – various approaches to the explanatory variable

Estimating the effect of environmental policies often requires a proxy variable for the stringency of the regulation. In a comprehensive review of available measures, Brunel and Levinson (2013) list multidimensionality and simultaneity among the main issues to be solved in such measurement. In particular, multidimensionality represents the challenge to summarise – in a meaningful and comparable way – the potentially available information on stringency across different pollutants (with different short and long-term effects, local and national or global in nature); different environmental domains or media; and a multitude of instruments (often industry, pollutant, location and vintage specific) in the context of countries with very different industrial structures and geographical characteristics. Simultaneity can be seen as a general problem – affecting both the measurement of policies itself and the actual empirical applications of the policy stringency variable. The latter is discussed in the section on identification below, but the former problem affects a number of measures used in the literature – particularly those based on outcomes of policies, such as spending or emissions, rather than actual policy characteristics. In this case simultaneity arises from the fact that measured environmental policy stringency cannot distinguish the effect of actual environmental policy stringency from that of other contributors to the measured outcome, such as non-environmental regulations; market imperfections; levels of income, skills and technology; capital intensity and pollution levels. While various attempts at measuring environmental policy stringency have been used in the past, none addresses the above considerations in a satisfactory manner.

There are several dominant approaches to measuring environmental policies in the empirical literature (Appendix A, Table A.1):

- One of the most popular is the use of survey-based firm or plant-level expenditures, which are interpreted as induced by environmental rules and hence intended to proxy for their stringency. The US Pollution Abatement Costs and Expenditures (PACE) and more generally environmental protection expenditures (Box 4) are widely used examples (Gray, 1987, Morgenstern et al., 2002; Gray and Shadbegian, 2003). This measure suffers from poor comparability across countries and time, problems of counterfactuals, or defining what part of expenditures are driven by environmental policies, the self-reporting nature of the surveys and sampling. The use of such proxies may pose practical problems in the case of attempts to assess the Porter Hypothesis – looking at the effects on environmental innovation may preclude using expenditures on innovative, pollution-reducing technologies as the explanatory variable – i.e. as proxy for environmental policies. Moreover, they are to some degree endogenous – higher compliance costs may come from older plants and technologies, rather than stricter policies (Ambec et al., 2013).
- As an alternative, "shadow prices" of pollution are proposed (reviewed in more detail by Brunel and Levinson, 2013). These stem from the idea that environmental policies put a price on pollution, hence the more stringent the policies, the higher the price faced by polluters, affecting their optimisation decisions. The main appeal of this approach is the fact that it focuses on the estimated "cost" of pollution faced by the firm, regardless of the instrument mix in place, circumventing the problem of multidimensionality. However, among the main problems are simultaneity (estimated shadow prices are also affected by other factors) and strong reliance on estimation assumptions (for example, regarding the functional form of the cost or production functions).
- A number of studies have used environmental or related performance data as a measure of policy stringency. Examples include energy intensity (Cole and Elliot, 2003; Van Beers and Van den Bergh, 1997; Harris et al., 2003), state compliance with environmental standards (McConnell and Schwab, 1990), and pollutant emission intensity (Smarzynska and Wei, 2004). Brunel and Levinson (2013) themselves follow this approach, proposing a measure of actual pollution intensity relative to what could be expected given the country's industrial structure. While interesting, particularly in cross-country empirical applications such measures tend to suffer from simultaneity and possible reverse causality as these measures might be influenced by e.g. differences in factor prices, technology and industrial structure more than by environmental regulation.
- Perceptions of the stringency of laws and their enforcement, as compiled by the World Economic Forum (WEF),[6] were used by Kalamova and Johnstone (2011) and Johnstone et al. (2010b). Similarly, Johnstone and Labonne (2006) and Lanoie et al. (2011), use survey based company level perceptions of the stringency of environmental regulations.[7] While potentially reflecting what matters – that is the stringency perceived by those who are affected – such measures have several downsides: they are context-specific (e.g. depending on the business cycle), linked to respondents subjectivity, problematic in international and inter-temporal comparability, and prone to the state of the economy (cyclicality) and sampling bias (for example, reflecting only the "incumbents" views).
- Some studies, in particular those based on difference-in-difference estimations, use a more event-based approach, based on the event of introduction or significant change in

a particular policy (Berman and Bui, 2001b; Van der Vlist et al., 2007; Curtis, 2012). While these approaches may better capture causal relationships in the case of very specific effects of individual policy changes, this will often be at the sacrifice of the generality of conclusions. Moreover, by measuring *de jure* aspects of environmental policies, they may omit implementation details and the enforcement of the environmental regulation.

- Others have experimented with broader policy proxies. Examples include the ratifications of international environmental treaties (counts, as in Smarzynska and Wei, 2004; or incidence, as in Yörük and Zaim, 2005; or Wu and Wang, 2008) or policy instruments in a given environmental domain (Jaraité and Di Maria, 2012). These measures apply only to a specific industry or pollutant dimension, several other approaches have attempted to summarise information in a number of "representative" industry and pollutant dimensions to create a more general index of environmental policy stringency (Berman and Bui, 2001a; Fleishman et al., 2009). These may again suffer from a selective approach, possibly omitting crucial areas, and in an international context often from aggregation issues when quantifying and compiling the stringency across very different instruments, pollutants and industrial structures. They also focus on the *de jure* rather than the *de facto* stringency of environmental policies.
- The spectrum of other proxies used in empirical studies is wide, including congressional pro-environment voting records of state representatives (Gray, 1997) or environment-related inspection frequency (Alpay et al., 2002; Testa et al., 2011; Brunnermeier and Cohen, 2003).

Overall, the choice of the most appropriate variables is likely to depend on data availability and the nature of the exercise – for example the type of pollution in question or the type of cross section units. When investigating the effects of several environmental domains or instruments, empirical analyses could either use a set of separate measures or a composite indicator, which summarises information across a number of the main dimensions of environmental policies. To the extent environmental policy characteristics can be approximated by a selected number of policy instruments, composite indicators can prove very powerful in empirical analysis. At the same time, a set of separate, non-aggregated measures may more directly address issues of trade-offs and complementarities across policies, instruments and design features, but may prove problematic due to multicollinearity, a loss of degrees of freedom, and, inevitably, the inability to cover all potentially relevant dimensions. In the context of international analysis, where comparable data is needed, the use of a composite index seems rather underexploited. This may be a consequence of poor data availability, and potential risks related to the choice of areas and quantification and aggregation of gathered information. Nonetheless, this omission is striking given that similar attempts have taken place in areas of significant complexity and multidimensionality, such as Product Market Regulation or Employment Protection Legislation (Nicoletti et al., 2000).

4.2. Time horizon matters but longer-term effects are difficult to capture

The time dimension of the economic effects of environmental policy may depend on the specific policy design and its announcement. On the one hand impacts may precede the date of introduction of the policy – firms can decide to make investments or change technologies upon announcement or even in anticipation of a new environmental rule. This argument however, relies strongly on the assumption that the policy change is credible and announced well in advance, for firms to take early action. Such a situation can arise due to international obligations – which require policy action by a certain date (frequent case in the EU) or due to a general perception of tightening of environmental rules, or a stable commitment to environmental policies – for instance Johnstone et al. (2010b) find a significant role of environmental policy predictability in triggering investment into renewable energy innovation. This might reflect the more general argument that in the presence of higher uncertainty of future laws firms may delay investments and adoption of new technologies (Shadbegian and Gray, 2005). On the other hand, impacts may take time – if policies trigger higher R&D investment, it may take years to actually bring about measurable improvements in technology and processes. In this vein, Jaffe and Palmer (1997) find a positive effect of pollution control expenditures on R&D expenditure in the manufacturing sector in the United States in 1975–1991, yet no significant effect on the number of successful patents.

By sheer nature, short-term and long-term effects may differ. If a new policy induces lump-sum investment, be it into capital (necessitated directly for pollution reduction or due to an accelerated scrapping rate as old "dirty" capital becomes obsolete), hiring of new personnel (to cope with environmental requirements or as a change of technology) or increased R&D expenditure, short-term inputs will increase without an equivalent increase in output. This would show up in curbed productivity growth. Thereafter, rebound effects or the benefits of new technologies may imply temporarily higher productivity growth – even assuming away the strong version of Porter's Hypothesis. While these effects may eventually fade out, with productivity growth returning back to trend, most empirical studies focusing on short-term or even contemporaneous effects will have a problem capturing the dynamics of such developments. Further complications may arise from the fact that less environmentally-efficient companies may go bankrupt as a result of regulation, while new companies may enter the market. Such effects are rarely taken into account in empirical research. Finally, trade and regulation avoidance may complicate the picture further, as for instance firms may tend to reallocate production and investment to plants subject to more lax environmental rules among US states (Gray and Shadbegian, 1998), or commence activity in States with less stringent legislation (Gray, 1997).

The focus on contemporaneous and short-term effects in empirical literature is driven by data availability and attempts to capture some longer-term effects have been fairly modest. Notable efforts, for example, Managi et al. (2005) include up to eight years of the lagged policy variable in their analysis of productivity growth in the offshore oil and gas industry in the United States. They find a negative contemporaneous effect. After the second year, the regulation's effect on productivity becomes positive. The accumulated effect outweighs the initial negative impact after the fourth year. The subsequent lags are also significantly positive. Lanoie et al. (2008) provide similar evidence for the Canadian manufacturing sector: the negative contemporaneous effect on productivity growth is outweighed by the accumulated effect after three years. Similarly, for the Taiwanese manufacturing sector Yang et al. (2012) find a small but significantly positive effect of the second year lag, but do not include further lags due to the short sample. Hamamoto (2006) tests different lag-structures in several models and shows that the specification of the six year lag fits better in terms of explained variability than for example a three-year lag structure. In a study of Swedish manufacturing industries, Broberg et al. (2012) do not find a significant longer-term effect. Only when looking explicitly at the pulp and paper industry, does a significant negative effect appear with a two year lag. Greenstone et al.

(2012) investigate the manufacturing sector in the United States and find a negative dynamic effect as well.

Much of the research discussed above takes a fairly static approach to environmental policies – one looking for a single, unique effect of a unique policy change. However, as discussed in De Serres et al. (2010), the nature of incentives provided by policies may be more dynamic. For instance, market-based instruments tend to provide more persistent incentives to innovate and to increase productivity than other policy instruments. These effects can be even stronger if policies have longer time horizons – for example when tax rates or caps on carbon emissions are (credibly) announced for years to come (such as in the case of the EU ETS), or when standards depending on best available technologies are to be revised periodically. Such characteristics of environmental policies are rarely straightforward to capture through standard measures of stringency and hence are usually ignored in empirical analysis.

4.3. Studies at different levels of aggregation may yield complementary insight

In the case of firm- or plant-level studies most standard data sets are fairly limited – they offer a short time series and often focus on firms in particular sectors and countries, hence limiting the generality of the investigation. Such datasets generally cover only incumbent firms over the sample period, leading to a disregard for entry and exit, which may reflect precisely the effects of environmental policies. In practice, very few studies actually attempt to adjust their estimates for entry and exit of firms using alternative data sources. Greenstone et al. (2012) show that the negative estimated effects of environmental policies on productivity are actually larger when adjusted for the fact that most inefficient firms drop out of the sample as a result of the environmental laws. For employment outcomes, Berman and Bui (2001b) find, however, no effect of taking account of dissuaded entry and induced exit. Finally, studies do not investigate network and spillover effects, such as those of tighter policies in a given sector which might spur innovation and productivity growth in other sectors.

Industry-level studies potentially provide longer time-series and are more suitable for dealing with entry and exit, but still usually ignore network and spillover effects. The level of aggregation of industries may matter – some effects may be missed at higher levels of aggregation, while lower levels will suffer from lack of data. Similarly, industry-level analysis will also miss the costs of labour substitution or movement from one plant to another, when total industry employment remains stable (Morgenstern et al., 2002). Furthermore, many studies tend to focus on a subset of industries only (e.g. Barbera and McConnell, 1990; Hamamoto, 2006; Yang et al., 2012).

The macroeconomic level approach potentially deals with some of the above issues by capturing the overall effects on productivity. They also offer more policy instrument variation than present in a single sector and often longer time series. These advantages come at a cost – measurement and comparison of policies may be more cumbersome – requiring to summarise multidimensional information, as discussed before. Moreover, the identification of effects of policy changes becomes difficult, and requires controlling for a wide range of other variables.

4.4. Identification of the true effect – a challenging task

Environmental policies are rarely introduced in isolation, making it difficult to assess their individual effects. Environmental policies can interact with each other and are often accompanied by mitigating measures, designed to soften perceived adverse impacts; by promoting environmental action in other countries; or even by border tax adjustments (OECD, 2010). The simultaneous implementation of related measures makes it difficult to properly identify the initial effect and to attribute an observed economic outcome to the specific policy under consideration. Moreover, many measures are temporary and eventually reversed, which due to time lags in their implementation and effects poses a further challenge for the identification of actual effects.

The comparability across studies suffers from the fact that different types of policy instruments as such can have different economic effects. The stringency of an appropriately designed performance-based standard for example might rely on the best available technologies, and will hence increase as more environmentally efficient technologies develop. On the other hand, the effective stringency of a fixed pollution cap may decrease over time for the very same reason. This makes it difficult to compare studies analysing different policy approaches.

One additional complication for empirical analysis is the potential reverse causality, that is, the extent to which the stringency of environmental policies is driven by productivity growth, causing practical problems for estimation such as biased estimates (Gray, 1987). As economic growth can spur demand for environmental quality (environmental Kuznets curve), stricter environmental laws might be a response of policy makers to this increasing demand. Similarly, environmental policies create new rent opportunities and as such can incentivise lobbying for more stringent laws. In an attempt to deal with issues of this type Managi et al. (2005) use a Granger causality test to investigate if environmental stringency is affected by productivity growth in the offshore oil and gas industry, and cannot reject non-causality. A recent study by Abdullah and Morley (2014) investigates this reverse causality problem on the macro-economic level by looking at environmental tax revenues and GDP, finding some weak evidence of a long-run causal effect from GDP to environmental tax revenues.

5. Conclusion

Empirical research on the productivity effects of environmental policies is largely inconclusive. Results are usually very context-specific and hence can only provide limited general policy conclusions – raising the question to what extent the results from a specific policy change, aimed at a particular pollutant and industry in a given country, can be generalised. The fairly broad support for the weak version of the Porter Hypothesis is not very surprising. However, the findings of an ambiguous effect of environmental policies on productivity, in line with the strong Porter Hypothesis, are rather unexpected, considering the earlier research aimed at explaining the US productivity slowdown with the tightening of environmental regulation.

Finding significant effects of environmental policy changes may be hard because environmental compliance costs are usually only a small share of total costs (Gray and Shadbegian, 2003). The size of the effects that are found in different studies reviewed is hardly comparable, mainly due to the crudeness of the environmental policy proxy variable.

Firm-level difference-in-difference studies are methodologically the most convincing approach. These studies allow clear identification of the economic effects of environmental policies, which helps to understand the forces at work on a microeconomic level. However, the very same argument implies that these studies suffer from lack of generality. Most studies also have problems dealing with a selection bias, dynamics (short samples), sample selection (entry/exit) or network effects/spillovers. Moreover, given that most studies only look at one type of policy instrument, they are also poorly suited to assess the potentially different effects of market-based and command-and-control policies. At the industry-level, generality is also dubious, and the bulk of older studies tend to suffer from the simultaneity problem. Spillovers across industries are left unaccounted for. While in practice only DEA and SFA studies control for environmental outcomes, the use of environmentally-adjusted productivity *per se* is asking a different question – no longer focusing on pure economic performance but implicitly weighting it against environmental performance (Boxes 1 and 2).

In terms of methodology, several general ideas for future research can be drawn:

- *First*, adding an international dimension can increase the variation both across policies and across outcomes, providing a richer sample. This could possibly reduce the need for a longer time series. So far, studies predominantly tend to look at national effects of national policies. With respect to many of the studies, aside perhaps from some of those conducted across US states, this additional degree of variation, both in policies and in outcomes, can allow for better identification of effects, hence easing some of the simultaneity issues. Such a comprehensive approach can also prove more suitable in comparing effects of market-based and command-and-control instruments, or other pertinent aspects of policy design, such as imposed administrative burdens, barriers to entry, exit and competition (Koźluk, 2014).
- *Second*, combining various levels of aggregation, such as macro, industry and firm-level, can yield complementary insights. Most studies look at one single level, while as discussed above, effects can differ significantly on each of the levels, due to considerations such as entry/exit, and spillover and network effects, and international trade. Looking at the three levels can help reconcile results that seem to be conflicting.
- *Third*, the utilisation of composite measures of environmental policies in cross-country comparisons can provide interesting insight on the effects of environmental policies, and their specific design characteristics – such as stringency and flexibility. The use of such composite indexes is rather scarce so far. This can partly be explained by the prevailing interest in national level studies, where effects of policies might better be analysed using more direct measures, such as discrete changes in specific policies. In the context of international comparisons, where different countries tend to use a multitude of different tools (Botta and Koźluk, 2014), an effort to construct such a composite index can provide a basis to analyse the effects of environmental regulations. Sub-indicators, by type of policy instrument or design characteristic, can also allow differentiating among the effects, for instance of market-based instruments versus command-and-control regulations. Practical challenges in constructing such composite indexes are surely also relevant, for example the complexity of multi-dimensional environmental policies and the data gathering process. Tackling these challenges, a composite index can provide policymakers with more insight on the choice among different policy approaches available.

Notes

1. Hicks (1932), p. 124: "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 economising the use of a factor that has become relatively expensive".
2. This often omitted point is also made by Romstad (1998).
3. An important point in this context is that many studies look at a fairly short-term perspective, where the link between productivity and competitiveness is not as evident.
4. The terms "environmental" innovation and "environmental" R&D, used commonly in the literature, are often defined fairly loosely. The approach taken in this review is to use them very broadly, in relation to innovative and R&D activity that focuses on reducing (some) environment-related effects, such as pollution or use of natural resources.
5. In De Vries and Withagen (2005) two out of the three measures of environmental regulation do not have any significant effect on environmental innovation. Their preferred model, which uses an instrumental variable approach, shows a significant and rather large positive effect on the number of environmental patents, they fail to provide any details on the validity of the instrumental variable approach.
6. The WEF "Executive Opinion Survey", conducted annually, asks respondents (business executives) a number of questions related to environmental policy design. The questions, coverage and sampling have varied across the years, but the most common questions were to assess the "stringency" (and "enforcement") of the overall environmental regulation in the country of operation, on a 1 to 7 scale. The survey was implemented by the WEF's partner institutes in over 150 economies. In most years, there were responses from between 8 000 and 15 000 firms (see WEF, 2013; for a description of the sampling strategy.) *http://www3.weforum.org/docs/WEF_GlobalCompetitivenessReport_2012-13.pdf*
7. The 2003 survey data were collected for the manufacturing sector in seven OECD countries and include information on environmental R&D expenditure, environmental and commercial performance and perceived stringency of environmental regulation.

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Appendix A

Table A.1. **Overview of empirical studies**

Author and year	Dep. variable	Independent variable	Sample	Methodology	Result
Productivity measures					
Gollop and Roberts (1983)	ΔTFP	Ratio of legal emission target to effective enforcement	American electric power industry, 1973-1979 (F)	• compare estimated productivity and growth contributions across regulated and non-regulated plants	• productivity growth is reduced for restricted plants, by 0.5 percentage points per year on average • evidence for increased costs due to sulphur dioxide emission restriction
Smith and Sims (1985)	ΔTFP	Payments for BOD and SS emissions	Canadian brewing industry, 1971-1980 (F)	• compare productivity growth of regulated and non-regulated firms	• negative impact of pollution charges on productivity growth
Berman and Bui (2001a)	TFP	Count variable of number of regulations in place	American oil refineries (Los Angeles basin), 1977-1992 (F)	• fixed effect estimation • compare regulated and non-regulated refineries	• positive, insignificant effect for regulated plants • no evidence for increase abatement operating costs due to regulation found
Gray and Shadbegian (2003)	TFP	PAOC	American pulp and paper mills, 1979-1990 (F)	• fixed effect and GMM estimation of regression of TFP and PAOC and directly of production function which includes PAOC	• negative effect on productivity levels is driven by integrated mills; effect for non-integrated mills is negligible
Greenstone et al. (2012)	TFP	Dummy variable of attainment/non-attainment of air pollution regulations	American manufacturing sector, 1972-1993 (F)	• include up to two lags • fixed effect estimation regressing TFP on environmental policy variable	• overall, negative effect on MFP • ozone regulations have strongest contemporaneous negative effect, PM and SO regulations strongest accumulated effect • negative accumulated overall effect is larger than contemporaneous effect
Becker (2011)	Labour productivity	Ratio of PACE to economic activity	American manufacturing industries,1980-1994 (F)	• fixed effect estimation of Cobb-Douglas function including compliance costs	• no significant effect on productivity • significant negative effect is found in sample consisting only of plants which experienced statistically meaningful changes in environmental compliance costs
Broberg et al. (2013)	Efficiency score derived from SFA	Environmental protection investments (distinguished into pollution prevention and pollution control)	Four Swedish manufacturing industries, 1999-2004 (F)	• efficiency measure using translog stochastic production frontier model • efficiency modelled as function of regulatory proxy • including up to two lags	• no support for Porter Hypothesis in overall manufacturing industry • negative effect in pulp and paper industry, mainly driven by negative effect of lagged variables
Van der Vlist et al. (2007)	Efficiency score derived from SFA	Dummies for relevant environmental policies	Dutch horticulture sector, 1991-1999 (F)	• estimation of stochastic production frontier including dummy variables of environmental policies	• positive effect on technical efficiency • firms under strict environmental policy regime are found to be more technically efficient than those under lax regime

Table A.1. **Overview of empirical studies** *(cont.)*

Author and year	Dep. variable	Independent variable	Sample	Methodology	Result
Jaraité and Di Maria (2012)	1/Δ efficiency score derived from DEA	Average annual spot price of CO_2 allowance Ratio of initial permit allocation to verified emissions	24 European fossil-fuel based public power generating sectors, 1996-2007 (F)	• efficiency measure using DEA incl. CO_2 and SO2 as bad outputs • fixed effect estimation	• adjusted efficiency score increases over time as bad output was reduced but laxity of policy (low CO_2 prices) partly offset the effect • positive effect on technical change, negative effect on technical efficiency
Fleishman et al. (2009)	Efficiency score derived from DEA	Dummies for SOX and NOX regulation	American gas and coal power plants, 1994-2004 (F)	• efficiency measure using DEA incl. bad outputs • Tobit estimation	• positive effect of SO2 regulations for coal and gas plants • negative effect of NOX regulation for gas plants • effect driven by presence not stringency of regulation • using adjusted or traditional efficiency scores does not alter the results
Managi et al. (2005)	Δ Efficiency score derived from DEA	Environmental compliance cost	American offshore oil and gas industry, 1968-1988 (F)	• efficiency measure using DEA incl. bad outputs • Almon distributed lag model and Granger causality tests • includes up to eight lags	• negative effect in the short term • positive effect in medium to long term • excluding bad outputs from efficiency score calculation shows no significant effect of environmental regulation • evidence that higher technological change of market outputs leads to more stringent environmental regulations
Boyd and McClelland (1999)	Efficiency score derived from DEA	Air and water pollutants, toxics	American integrated paper plants, 1988-1992 (F)	• compares efficiency score under the assumption of weak and free disposability of bad outputs	• positive effect of environmental regulation on efficiency
Gray (1987)	ΔTFP	PACE Worker and health regulation (number of inspections)	450 American manufacturing industries, 1958-1978 (I)	• uses growth accounting to calculate productivity • simple regression analysis	• negative effect when regulation measures are included separately without any covariates • no significant effect when both regulations plus other explanatory variables are included • sensitivity tests render PACE coefficient insignificant
Dufour et al. (1998)	ΔTFP	Investment in pollution-control equipment to total input costs worker and health regulation	Canadian manufacturing industries, 1985-1988 (I)	• uses growth accounting to calculate productivity • GLS estimation, controlling for economies of scale and business cycle fluctuations	• negative effect of environmental regulation • effect of worker health regulation depends on type of regulation (negative effect of protective reassignments, positive effect of mandatory prevention programmes and fines)
Barbera and McConnell (1990)	ΔTFP	Abatement capital	Five American manufacturing industries, 1960-1980 (I)	• estimating cost elasticity of abatement capital • differentiate direct and indirect effect: • direct effect comes through changes in costs and their effect on productivity • disentangling effect of abatement capital costs on ΔTFP yields indirect effect	• small indirect effect (positive/ negative) • negative direct effect (as long as abatement capital grows) • negative net effect (direct plus indirect)

Table A.1. **Overview of empirical studies** *(cont.)*

Author and year	Dep. variable	Independent variable	Sample	Methodology	Result
Hamamoto (2006)	TFP	Induced R&D (derived from pollution abatement capital cost effect on R&D spending)	Five Japanese manufacturing industries, 1966-1982 (I)	• elasticity of R&D expenditure w.r.t. lagged regulatory stringency is used to calculate induced R&D • extended (standard inputs plus R&D capital) Cobb-Douglas production function is used to examine effect on productivity growth • includes up to six lags	• negative effect of PACE on age of capital stock which in turn does not affect TFP growth rate • positive effect of PACE on R&D expenditure and induced R&D positively affects TFP growth • longest lag structure model performs best
Yang et al. (2012)	TFP	R&D induced by environmental regulation (calculated through abatement capital costs and pollution abatement fees)	Taiwanese manufacturing industries, 1997-2003 (I)	• use Levinsohn and Petrin (2003) productivity measure • fixed effect estimation • include one lag	• positive effect of induced R&D on TFP • effect of induced R&D is smaller than of scheduled R&D • positive effect of environmental regulation on R&D • positive direct effect of environmental regulation on TFP (larger for pollution control fees than for PACE)
Lanoie et al. (2008)	∆TFP	Investment in pollution-control equipment to total input costs	Canadian manufacturing industries (Quebec), 1985-1994 (I)	• up to three years of lagged regulatory variable • GLS estimation	• negative contemporaneous effect • positive effect of second and third lag (outweighs initial negative effect) • effect stronger in sectors which face more international competition
Domazlicky and Weber (2004)	∆ Efficiency score derived from DEA	Emission into air, water, land or underground	Six American chemical industries, 1988-1993 (I)	• compare efficiency score under the assumption of weak and free disposability of bad outputs	• positive effect on productivity growth • adjusted and traditional efficiency measures are significantly different from each other • no correlation of abatement costs and productivity growth found
Alpay et al. (2002)	∆TFP	• Pollution abatement costs (US) • Frequency of reported plant inspections (Mexico)	Mexican and US food sector, 1962-1994 (I)	• use elasticities to calculate contribution of environmental regulation to productivity growth • calculates TFP growth with and without abatement costs	• positive effect on productivity growth in Mexico, no significant effect in the United States • negative effect on profitability in Mexico, no significant in the United States • larger productivity growth rates in Mexico when environmental regulation is included in productivity calculation, the United States show slightly lower productivity growth rates
Jeon and Sickles (2004)	∆ Efficiency score derived from DEA	CO_2 emissions	17 OECD and 11 Asian economies, 1980-1995 (M)	• compares efficiency scores of three scenarios (free emission, no change of emission levels, partial reduction of emissions)	• adjusted TFP growth is lower than traditional for OECD countries whereas it is higher for ASEAN countries • productivity growth is lower in constant emission scenario then in free emissions scenario for OECD and ASEAN economies • productivity growth is higher in scenario of emission reduction in OECD and ASEAN economies

Table A.1. **Overview of empirical studies** *(cont.)*

Author and year	Dep. variable	Independent variable	Sample	Methodology	Result
Wu and Wang (2008)	Δ Efficiency score derived from DEA	CO_2 emissions	17 APEC economies, 1980-2004 (M)	• compares efficiency scores of three scenarios (free emission, no change of emission levels, partial reduction of emissions) • fixed effect regression of dummy marking years of UNFCCC ratification on productivity growth	• productivity growth slightly higher in scenario of no change and reduction of emission levels than in free emission scenario • negative effect of ratification of UNFCCC on productivity growth
Yörük and Zaim (2005)	Δ Efficiency score derived from DEA (CO_2, NOX and water pollutants)	UNFCCC protocol ratification	OECD economies, 1983-1998 (M)	• compares traditional with adjusted productivity index (emission reduction scenario) • fixed effect regression of dummy marking years of UNFCCC ratification on adjusted productivity growth	• adjusted productivity growth is significantly larger than traditional • effect of NOX and water pollutants is largest • significant positive effect of UNFCCC ratification on adjusted MFP growth (no effect on traditional MFP growth)
Innovation					
Johnstone and Labonne (2006)	Environmental R&D expenditure	Compiled environmental stringency indicator (survey data)	7 OECD manufacturing sectors, 2003 (F)	• survey data • probit estimation	• positive effect of environmental taxes on environmental R&D expenditure • negative effect of technology-based standards
Arimura et al. (2007)	Environmental R&D expenditure	Compiled environmental stringency indicator (survey data)	7 OECD manufacturing sectors, 2003 (F)	• survey data • Tobit and bivariate probit estimation	• positive effect of perceived policy stringency on environmental R&D expenditure • no support for stronger effect of flexible policy instruments
Lanoie et al. (2011)	Environmental R&D Environmental and business performance	Compiled environmental stringency indicator (survey data)	7 OECD manufacturing sectors, 2003 (F)	• uses survey data (Heckman sample selection procedure) • tests all versions of Porter Hypothesis in one common framework • probit, 2SLS, instrumental variable probit estimation	• support for weak and narrow version of Porter Hypothesis, no support for strong version • effect of environmental taxes is driven by stringency of taxes • stringency of performance standards has larger impact than technology based standards • direct effect on business performance is negative and larger than indirect positive effect
Jaffe and Palmer (1997)	Total R&D expenditure Number of successful patents	PACE	US manufacturing industries, 1975-1991 (I)	• pooled model and fixed effects estimation • including up to five lags	• positive effect on R&D expenditure of lagged PACE • no effect of PACE on number of successful patents
Brunnermeier and Cohen (2003)	Environmental patents	PACE Number of inspections	146 US manufacturing industries, 1983-1992 (I)	• different estimation models: fixed effect, Poisson count data model, negative binominal fixed and random effects	• positive effect of PACE on number of patents • insignificant results for number of inspections • environmental innovation is more likely to occur in industries which are exposed to international competition
Kneller and Manderson (2012)	R&D expenditure Total R&D activity	Environmental protection expenditures	UK manufacturing industries, 2000-2006 (I)	• GMM estimation • include up to two lags	• positive effect on environmental R&D and investment in environmental capital, no effect on overall R&D or total capital accumulation • environmental R&D may crowd out non-environmental R&D • no evidence of crowding out effect of environmental capital

Table A.1. **Overview of empirical studies** *(cont.)*

Author and year	Dep. variable	Independent variable	Sample	Methodology	Result
Lanjouw and Mody (1996)	Patent counts	PACE	US, Japanese and German economies, 1971-1988 (M)	• evaluate effect of pollution abatement capital expenditure on patent count with simple time-series correlation	• positive effect on patent count, but lagged by 1-2 years • evidence is found that foreign regulations also influence domestic patent count
Popp (2006)	Environmental patents	SOX and NOX regulations	US, Japanese and German economies, 1967-2003 (M)	• evaluates effect of domestic and foreign regulation on innovation with simple time-series correlation	• inventors respond to environmental regulation pressure in their own country but not to foreign environmental regulation
Johnstone et al. 2010a	Patent counts in renewable energy sectors	Renewable energy policy variables	25 OECD countries, 1978-2003 (M)	• panel estimated with a negative binomial model, • fixed effects are included, • 3 of 6 policy variables are modelled with dummies (introduced or not)	• renewable energy policies have a significant effect on related patents, • feed-in-tariffs have an additional positive effect on solar power patents, renewable energy certificates have a positive effect on wind energy patents.
Johnstone et al. 2010b	Environmental patent counts	Perceptions of environmental policy stringency, flexibility and predictability (WEF survey)	OECD countries, 2000-2007 (M)	• panel estimated with a negative binomial model, • due to high collinearity of the policy variables, orthogonal factors are extracted, • no fixed effects are included	• policy stringency, flexibility and stability have a positive coefficient (weak Porter Hypothesis).
De Vries and Withagen (2005)	Environmental patents	Dummy variable for regulations	14 OECD economies, 1970-2000 (M)	• instrumental variable approach • fixed effect estimation	• large positive effect on patent count
Kalamova and Johnstone (2011)	Bilateral FDI flows	Perceived environmental policy stringency index (WEF)	27 OECD countries + 99 host countries, 2001-2007 (M)	• panel estimation with controls for drivers of FDI	• positive effect of lax environmental stringency on FDI inflows • non-linear effect: FDI inflows decrease after a certain threshold of laxity is reached
Investment and capital stock					
Gray and Shadbegian (1998)	Investment	PACE	American paper mills, 1979-1990 (F)	• different regression specifications, including dummies for high pollution abatement investment years and a variable which divides total plant abatement investment over time	• plants with high abatement investment spend significantly less on productive capital – crowding out effect. For one dollar spent on environmental capital, investment into productive capital is reduced by 1.88. • effect is 0.99 when adjusted for within firm (across plant) allocation.
Nelson et al. (1993)	Age of capital stock	• Enforcement costs of environmental agency • Value of pollution control facilities	US electric utilities, 1969-1983 (F)	• 3SLS estimation	• positive effect on age of capital stock • increase in age of capital does not impact emission level

Note: Studies are listed according to the dependent variable of the analysis.
Abbreviations: DEA – Data Envelopment Analysis; FDI – Foreign Direct Investment; GLS – Generalised Least Squares; GMM – Generalised Methods of Moments; PACE – Pollution Abatement Costs and Expenditures; PAOC – Pollution Abatement Operating Costs; SFA – Stochastic Frontier Analysis; TFP – Total Factor Productivity; 3SLS – Three State Least Square.
In sample descriptions: (F) stands for firm/plant-level analysis, (I) for industry and (M) for macro/country-level analysis.

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