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Abstract

This study estimates an aggregate green knowledge production function (GKPF) for 19 OECD countries from 1981 to 2012, using panel-data econometric methods to address spatial spillovers and unobserved heterogeneity. Both Cobb-Douglas and translog functional forms are evaluated with multiple estimators, including standard fixed and random effects models, pooled and mean group common correlated effects (CCE) estimators, and random-trend models to account for shared upward trends among variables. The regression analysis examines the relationship between green patenting and key determinants such as R&D expenditure, human capital, and environmental policy indicators. The results consistently show a robust positive effect of domestic R&D, whereas the impacts of other factors exhibit greater variability. Methodologically, the findings highlight the sensitivity of coefficient estimates to unobserved heterogeneity and the choice of functional form.

Keywords: Green innovation, knowledge production function, panel data, spatial spillovers

1 Introduction

The link between investment in research and the development of new technologies has long been seen as central to fostering economic expansion. Yet, empirically estimating knowledge production functions (KPFs) remains challenging—particularly at an aggregate level—due to the elusive nature of knowledge outputs and difficulties in accounting for cross-border knowledge spillovers. This paper tackles these empirical hurdles by estimating a Green Knowledge Production Function (GKPF), leveraging macro panel data from 19 OECD countries over the 1981–2012 period. It explores how green patenting activity responds to domestic R&D expenditure, levels of human capital, their foreign counterparts as proxies for spillovers, and a binary threshold variable capturing the stringency of national environmental regulations.

This study is driven by both analytical and policy-related motivations. On one hand, the shift to a low-carbon and resource-efficient global economy relies heavily on the widespread emergence and adoption of green technologies (UNIDO, 2018), as long-term environmental outcomes hinge on the degree to which policy can steer innovation in pollution-reducing technologies (Kneese and Schultze 1975; Weyant 1993; Clarke and Weyant 2002). Over the last thirty years, this shift has been molded by both technological advancements—such as digitalization, automation, and the Internet of Things—and major environmental agreements, including the US Clean Air Act, the 1992 Rio Earth Summit, the 2015 Paris Agreement (COP21), and the 2019 European Green Deal. Nevertheless, it remains unclear to what extent environmental policies contribute to green innovation when accounting for inputs like R&D and human capital.

Theoretically, this paper extends the foundational work of Griliches (1990) by applying the KPF framework—originally developed for firm- or sector-level data—to a macroeconomic setting. This approach accommodates national-level variables such as environmental policy stringency and enables a longer-term analysis of green innovation. As such, the paper aims to connect the micro-level emphasis of traditional innovation studies (e.g., Mairesse and Mohnen, 2010; Charlot et al., 2015) with the broader scope of macroeconomic environmental policy research. While micro-level studies provide insight into localized innovation drivers, they often omit the larger institutional and regulatory contexts shaping technological transitions at the national scale.

One key theoretical innovation of this study lies in its distinct treatment of inter-firm/region versus inter-country spillovers. While the former are implicitly absorbed by spatial units like regions or states, cross-national spillovers require explicit modeling. Given that knowledge *also* transcends national boundaries, a spatial distance-decay function is employed to weight spillover terms and minimize the risk of endogeneity caused by hidden economic, institutional, or cultural linkages.

Despite its practical relevance, macro-level modeling of innovation introduces notable econometric difficulties. Aggregated data may mask variation in key regressors, heighten multicollinearity due to macro trends, and complicate the identification of causal relationships. Furthermore, macro panel datasets often exhibit unobserved heterogeneity and cross-sectional dependence, both of which require careful methodological treatment.

Cross-country interdependence in KPF specifications à la Coe & Helpman is likely, and failing to address this may lead to flawed inference or inconsistent estimates. Cross-sectional dependence of residuals can stem from a finite set of observable or latent common factors that exert varying impacts across nations. These may include global technological shifts, domestic policy interventions to raise productivity, or oil price shocks affecting production costs. Such heterogeneity may arise from country-specific technological constraints. To account for this, the analysis adopts the Common Correlated Effects (CCE) framework by Pesaran (2006), which has been validated under a range of data-generating processes (Chudik et al., 2011; Pesaran and Tosetti, 2011; Kapetanios et al., 2011). Alternatively, spatial panel models can be used to represent spatial correlation in errors; however, CCE estimators have also proven reliable in such settings. Specifically, Pesaran and Tosetti (2011) show that CCE estimators yield consistent slope coefficients and standard errors in models where the error term includes both multifactor structures and spatial dependence. Furthermore, while Pesaran

(2006) introduces CCE with a finite number of strong factors and no weak ones, Chudik et al. (2011) demonstrate that the method remains valid even under infinite-factor settings that combine strong and (semi-)weak factors.

Accordingly, the empirical work moves beyond conventional panel-data estimators (fixed and random effects) and applies both pooled and mean group CCE estimators, alongside random-trend models, to account for the shared upward trend across variables. This diversity of estimation strategies helps test how sensitive the elasticity estimates are to alternative assumptions about unobserved heterogeneity.

The main empirical results are threefold. First, gross domestic R&D spending consistently predicts green patent output across specifications, with elasticity estimates ranging from 0.4 to 0.9. This supports theoretical expectations and reinforces the applicability of the KPF at macro levels. Second, the effects of human capital and foreign spillovers are less stable, showing significant variation in magnitude and sign across models. This inconsistency invites interpretive caution—potentially pointing to underlying economic dynamics (e.g., crowding out, diminishing returns) or model misalignment. Third, the environmental policy proxy is statistically insignificant in all settings, raising questions about its adequacy in capturing policy effects or suggesting limited direct policy impact on green patenting.

These more ambiguous findings require careful epistemological interpretation. Aggregating knowledge production at the national level entails assumptions about uniformity, monotonic relationships, and scale returns—assumptions that may not reflect reality. As noted by Fisher (2005), aggregate production functions often function more as useful approximations than as structural truths. In this context, the GKPF serves as a simplified but policy-relevant framework for exploring systemic innovation patterns, not a definitive causal structure. Thus, the study positions itself within the realm of empirical validation rather than theoretical innovation, aiming to test existing hypotheses in a novel macro setting.

Beyond methodological contributions, the findings carry implications for policy. The strong role of R&D reinforces the case for continuous investment in research. Meanwhile, the weaker and inconsistent effects of human capital and environmental policy point to the need for more precise, context-sensitive instruments to align technological change with sustainability targets. Future research should delve deeper into the interplay between policy mixes, innovation networks, and industrial structures to enhance understanding of green knowledge generation and dissemination.

In closing, this paper contributes to the expanding field of green innovation by presenting a macro-level, evidence-based analysis of what drives green knowledge production. It offers cautious support for the KPF at the aggregate level, while highlighting limitations in measuring spillovers and policy impacts. As the urgency of climate change intensifies, the insights offered here can inform both academic inquiry and policy design aimed at steering economies toward sustainable, innovation-led pathways.

2 The Knowledge Production Function - A Review of the Literature

2.1 Introduction: Boundaries of the Literature Considered

The conceptual foundation of the Knowledge Production Function (KPF) traces back to Griliches' (1979) seminal work, which sought to standardize the analysis of relationships between knowledge inputs and economic or environmental efficiency outputs. This framework inherits fundamental measurement challenges from its constituent variables - knowledge being inherently unobservable and requiring proxy measures (typically R&D expenditures, patents, or innovation surveys), while efficiency admits multiple competing definitions ranging from narrow financial metrics to broad social welfare concepts.

This dual ambiguity creates what Griliches termed "conceptual and semantic" difficulties in KPF specification. The literature has consequently evolved as an intricate web where variables alternate roles across studies - what serves as a knowledge proxy in one analysis becomes the efficiency measure in another. This reflexivity stems from the field's fundamental insight that knowledge production processes exhibit network characteristics, where each relationship informs our understanding of others in the system.

Our analysis focuses particularly on what we term the "input-KPF" segment ($P = f(R\&D)$), where patent counts (P) are modeled as outputs of R&D investments (R). This specification gains theoretical justification from Griliches' (1990) conceptual framework, which positions patents as costly, quality-filtered proxies for knowledge production. The patenting process itself embodies an implicit quality threshold, reflecting researchers' ex-ante assessments of an innovation's potential utility and market value. As Griliches, Nordhaus and Scherer (1989) note, the expected value of a patent incorporates both its probability of grant and the anticipated economic value of the protected rights, net of disclosure costs. This makes patents natural output measures despite their well-documented imperfections.

2.2 Early Literature on Existence and Shape of the Input-KPF

The empirical literature on knowledge production initially sought to establish two fundamental relationships: the existence of a positive input-KPF, and the shape of this relationship across different organizational contexts. Early longitudinal work by Schmookler (1966) demonstrated strong correlations between R&D inputs and patent outputs over extended periods (1870-1950), while Scherer's (1965) cross-sectional analysis of 500 large U.S. manufacturing firms confirmed these patterns using R&D employment as the input measure. These findings adopted a four-year lag structure reflecting contemporary patent approval timelines, an approach later validated by Pakes and Griliches (1980) though with some debate about optimal lag length (Hall, Griliches and Hausman 1984).

These empirical regularities stood in tension with Schumpeter's (1942) influential hypothesis regarding large firms' superior innovative capacity. Subsequent work systematically challenged this view, with Comanor (1965), Mansfield (1968a), and Schmookler (1972) all finding evidence that R&D efficiency (patents per R&D dollar) declined with firm size. The relationship between firm size and R&D intensity (R&D as percentage of revenue) proved more nuanced, with Loeb and Lin (1977) and Bound et al. (1982) documenting an inverted-U pattern that contradicted simple linear formulations. Only Horowitz (1962) and Hamberg (1966) found evidence supporting higher R&D intensity among large firms.

These empirical discrepancies prompted theoretical refinements, most notably Arrow's (1962) spillover hypothesis. The recognition that knowledge possesses public good characteristics - being non-rivalrous and often non-excludable - suggested that small firms might free-ride on the R&D investments of larger counterparts. This insight fundamentally altered the methodological trajectory of KPF research, motivating a shift toward higher levels of aggregation (regions, states, or nations) better suited to capture spatial spillover effects. As we discuss in Section 2.6, these considerations also inform our methodological choice to measure R&D using cumulative stocks rather than annual flows.

2.3 Spatial vs. Aspatial Knowledge Spillovers

The reliability of state-level KPF estimates depends critically on the spatial characteristics of knowledge spillovers. The core analytical challenge can be framed simply: when two firms co-located in a city benefit from mutual knowledge spillovers, aggregate city-level R&D and patent measures will capture these externalities. However, if firms primarily access knowledge through non-geographic networks (e.g., international research collaborations), spatial aggregation may introduce measurement error by associating outputs with the wrong inputs.

The literature distinguishes between spatial spillovers (geographically bounded) and network spillovers (potentially global in scope). Early network studies (DeBresson and Amesse 1991) often conflated these concepts, as pre-digital communication technologies made networks geographically constrained by necessity. Modern work (Singh 2005; Bercovitz and Feldman 2011) recognizes that while spatial and network spillovers may complement each other (Breschi and Lissoni 2009), they are theoretically distinct - with network ties becoming increasingly decoupled from geography as communication technologies advance.

Several robust empirical regularities emerge from this literature. Feldman and Florida (1994) provide compelling evidence for the spatial clustering of innovative activity, while Jaffe and Trajtenberg

(1999) demonstrate how national borders attenuate knowledge flows independent of geographic distance. Singh and Marx (2013) find political boundaries have independent effects on knowledge diffusion beyond pure geographic proximity. These findings collectively suggest that while spillovers may transcend municipal boundaries, national borders remain meaningful barriers to knowledge flows.

The literature also identifies important boundary conditions for spillover effects. Cohen and Levinthal's (1990) absorptive capacity framework establishes that firms (and by extension, regions) require minimum R&D thresholds to effectively utilize external knowledge. This helps mitigate concerns about distortionary international spillovers in state-level analyses, as Musolesi, Golinelli and Mazzanti (2025) confirm specifically for green innovation contexts. Similarly, while Bosetti et al. (2008) raise the possibility of crowding-out effects from international spillovers, these appear most relevant for developing economies (Li, Gaston and Alkemade 2020) rather than our OECD sample.

The empirical literature on spillover measurement has evolved considerably since Jaffe, Trajtenberg, and Henderson's (1993) pioneering work using patent citations. While subsequent studies (Thompson 2006; Agrawal, Kapur, and McHale 2008) largely confirmed the geographic localization of knowledge flows, methodological critiques have emerged. Thompson and Fox-Kean (2005) and Alcácer and Gittelman (2006) demonstrated how technical aspects of patent citation practices may exaggerate apparent spatial clustering, suggesting cautious interpretation of these results.

These measurement challenges notwithstanding, the weight of evidence supports three key conclusions relevant to our state-level analysis. First, innovative activity exhibits pronounced spatial clustering that cannot be fully explained by co-location of production (Audretsch and Feldman 1996; Carlino et al. 2012). Second, national borders significantly constrain knowledge flows beyond what geographic distance alone would predict (Jaffe and Trajtenberg 1999). Third, the network characteristics of modern knowledge spillovers, while increasingly global, still show strong national anchoring (Branstetter 2001).

The implications for KPF estimation are twofold. At the state level, we can reasonably expect to capture most spatially-mediated spillovers while minimizing distortions from international knowledge flows. This addresses Feldman and Avnimelech's (2011) concern about states being "too broad" as observational units - while states may not be ideal for studying spillover mechanisms per se, they provide adequate containment for estimating aggregate production relationships. The remaining measurement error likely biases our estimates downward rather than introducing spurious correlations, as uncaptured international spillovers would manifest as unexplained patent output rather than false attribution to domestic R&D.

2.4 Aggregation Challenges in KPF Estimation

Moving from firm-level to state-level analysis introduces well-documented aggregation challenges that warrant careful consideration. Fisher's (2005) critique of aggregate production functions highlights how combining heterogeneous inputs and outputs can obscure micro-level relationships. While knowledge production functions face somewhat milder versions of these problems than their physical counterparts, several issues remain salient.

First, the "output" homogeneity assumption becomes strained when aggregating across fundamentally different innovation types (e.g., pharmaceutical patents versus software patents). Griliches (1979) noted this challenge, observing that "the meaning of patent counts becomes more ambiguous the more heterogeneous the sample." Our focus on green patents provides some mitigation by restricting the domain of analysis, though heterogeneity within environmental technologies persists.

Second, multicollinearity tends to increase with aggregation, as Griliches (1979) observed: "There is much more variability in the R&D histories of particular firms than in the R&D histories of the corresponding industries." We follow Griliches' "strategy of moderation" in addressing this - using cumulative R&D stocks rather than annual flows and maintaining relatively parsimonious specifications.

Third, the constant-returns assumption implicit in many aggregate analyses becomes more problematic at larger scales. While Charlot, Crescenzi and Musolesi (2015) note the lack of clear theoretical guidance on functional form, we test both Cobb-Douglas and translog specifications to assess robustness to this concern.

2.5 Green Innovation and the GKPF

The application of KPF frameworks to environmental innovation introduces unique theoretical considerations and empirical challenges. The foundational insight comes from Jaffe, Newell, and Stavins (2005), who identified the dual market failures shaping green innovation: pollution generates negative externalities (leading to excessive emissions), while knowledge generates positive externalities (leading to insufficient innovation). This creates the distinctive dynamic where "the invisible hand allows too much pollution while producing too little innovation" (Popp, Newell, and Jaffe 2010).

Environmental policies attempt to correct these twin failures, but their innovation impacts are notoriously difficult to isolate empirically. The primary challenge comes from concurrent fluctuations in energy prices, which Newell, Jaffe, and Stavins (1999) show can independently induce innovation. Their analysis of appliance efficiency trends demonstrates how periods of declining real energy prices (1960s) corresponded with technological regress in energy efficiency, while the 1970s oil shocks drove dramatic improvements. Similar patterns appear in automotive innovation (Atkinson and Halvorsen 1984; Greene 1990), where fuel price changes often outweigh regulatory impacts.

The empirical literature on policy-induced green innovation has developed several strategies to address this confounding. Brunnermeier and Cohen (2003) use pollution abatement expenditures as their policy measure, finding significant effects on environmental patenting in U.S. manufacturing. Popp (2006) exploits cross-country variation, showing inventors respond primarily to domestic rather than foreign environmental regulations. More recent work by Costantini, Crespi, and Palma (2017) emphasizes policy mix effects, finding complementary between technology-push and demand-pull instruments.

Three particularly relevant findings emerge from this literature for our analysis. First, policy impacts appear highly technology-specific, with Johnstone, Haščič, and Popp (2010) demonstrating how renewable energy innovation responds differently to feed-in tariffs versus renewable certificates. Second, the innovation effects of environmental policies often follow non-linear patterns, as Song, Wang, and Zhang (2020) show with their U-shaped relationship in Chinese data. Third, the quality of environmental patents may respond differently than quantity to policy signals - a dimension we address through citation-weighting in our empirical approach.

2.6 Synthesis and Methodological Implications

The literature review yields several key insights that inform our empirical strategy. First, the state level represents a reasonable compromise between capturing spatial spillovers and minimizing aggregation bias, particularly when analyzing environmental innovation where policy instruments are often nationally implemented. Second, the input-KPF relationship appears robust enough to persist despite measurement challenges, though careful attention to functional form and lag structure remains essential. Third, environmental policies likely induce green innovation, but their effects must be disentangled from concurrent energy price movements.

These considerations lead us to adopt several methodological safeguards. We employ both Cobb-Douglas and translog functional forms to test robustness to specification choices. We measure R&D using cumulative stocks rather than annual flows to better capture knowledge persistence. We incorporate energy prices directly in our models to isolate policy effects. And we use multiple estimators (FE, RE, CCE) to assess sensitivity to unobserved heterogeneity and cross-sectional dependence. The resulting empirical approach aims to balance the insights from six decades of KPF research while adapting them to the specific context of green innovation.

3 Econometric Analysis of the Green Knowledge Production Function

3.1 Dataset, Dependent and Independent Variables

3.1.1 Dataset

The dataset is a balanced panel of 589 observations, with 19 OCED countries for 31 years (1981-2012). The variables considered, and described in the following sections, are the number of green patents (section 3.1.2), Gross domestic expenditure on R&D (or GERD), measured as a cumulative stock via the perpetual-inventory method (considered in section 3.1.3), , foreign spillovers, whether of HC or GERD, (ut supra), Human Capital (ut supra), and a binary environmental-policy variable (ut supra).

3.1.2 Dependent Variable

Regarding the dependent variable, we obtain data on green patents (GP) from the OECD Stat databases. Specifically, we focus on patents classified under the OECD's "selected environment-related technologies" category (IPC: ENV_TECH) that were granted by the USPTO (United States Patent & Trademark Office). The count of patents is aggregated by country based on the inventor(s)' country (or countries) of residence. If a patent is filed by an entity from country i but submitted in country j, it is still attributed to country i. Additionally, the dataset includes information on patent families, filing years, and the geographic location of inventors.

To avoid double-counting inventions across different regions, we apply fractional counting. This means that if a patent family involves multiple inventors from different locations, the patent's contribution is distributed proportionally across the relevant countries. Furthermore, the use of patent families helps account for the 'quality' of patents at the macroeconomic level, as they reflect broader innovation efforts rather than isolated filings.

3.1.3 Independent Variables

3.1.3.1 R&D Capital Stock Formula

The procedure used by Coe and Helpman 1995 is used to account for the cumulative knowledge-creation. The formula behind this R&D capital stock approaches its measurement via the perpetual-inventory, resulting in:

$$\text{R\&D\_Stock}_t = (1-\delta)\cdot \text{R\&D\_Stock}_{t-1} + \text{GERD}_t$$

where $\text{R\&D\_Stock}_t$ is the R&D capital stock at time t , δ is the depreciation rate and GERD_t is the gross domestic expenditure on R&D in year t.

This "cumulative" perspective is not a panacea; rather, it creates a new cost in suppressing another. The cost supressed lies in the problem of the "gestation" period of knowledge-creation: that is, the time needed to produce a patent, whether in research or in bureaucratic patenting[1]. If, instead of regressing our dependent variable on the expenditure of a given year, knowledge is measured as a stock, then the precise lag from investment to patent becomes less determinant in disturbing the corroboration of the presumed production function.[2]

[1] A secondary but not negligible cost is also the probable collinearity of R&D expenditures from year to year (Griliches 1979)

[2] As it is never the one-year expenditure that is singularly hypothesized to produce a speculated t-years after number of patents, but rather the cumulative higher or lower expenditures throughout the years increasing or decreasing future patents. A more familiar and, hopefully, clearer application of this perspective is given by many studies where income

But attaching an everlasting importance to past knowledge/expenditure in creating yet another patent, thirty years later, is unrealistic: that is, unless we model some sort of depreciation of such stock of knowledge. While variation in the rate of depreciation (unknown and possibly varying among units and time) can have serious consequences on the net rate of return to R&D (Griliches 2001), it has proven irrelevant to the estimation of the elasticity of the same input (Coe and Helpman 1995; Hall and Mairesse, 1995; Coe, Helpman and Hoffmaister, 2009).

3.1.3.2 Spillovers

We follow Ertur and Musolesi (2017) in focusing on geographic proximity as the channel through which international R&D spillovers occur. The idea is to use a variable that could have been measured "before and after the markets", in order to prevent problems of reverse causality, often affecting traditional spillover channels such as trade (as in Coe e Helpman), FDI, or patent citations. A country's participation in these channels may itself depend on its productivity or technological capacity, potentially biasing empirical estimates. In contrast, distance does not suffer from such endogeneity concerns. It can also proxy for other unobservable but relevant similarities—such as cultural, institutional, or linguistic affinities—that may facilitate the diffusion of knowledge. The measure, adopted from a spatial econometric approach, uses exponential decay functions based on bilateral distances between countries' capitals:

$$\mathrm{WRD}_{it} = \sum_{j \neq i} \exp(-d_{ij})\mathrm{RD}_{jt}, \quad \mathrm{WHK}_{it} = \sum_{j \neq i} \exp(-d_{ij})\mathrm{HK}_{jt}$$

where d_{ij} is the spherical distance between country i and country j. This method allows the model to capture how the intensity of knowledge spillovers *diminishes* with geographic distance.

3.1.3.3 Human Capital

The levels of human capital are extracted from the 9.0 version of the Penn World Table (Feenstra, Inklaar and Timmer, 2015), where such variable is proxied/constructed on average years of schooling, following Barro and Lee 2013. Notice that, although it is again stocks of, and thus cumulative measure for, HC, they are not discounted or depreciated as the R&D stocks considered above.

3.1.3.4 Environmental Binary Policy

Following Musolesi, Golinelli and Mazzanti 2025, the environmental policy variable, denoted as EP_{it}, is a binary indicator constructed to capture the presence of regulatory action in the air pollution domain. A binary format is chosen due to challenges in constructing consistent continuous measures across different countries and policy types, and to allow for a longer time span than alternatives like

is regressed on years of education; here, equally, it is not the 20th year of education alone that is assumed to increase income, but the higher cumulative number of years of education increased by studying yet another year.

EPSI, which excludes the 1980s. The indicator is based on OECD data and focuses specifically on air pollution regulations, given their historical depth and foundational role in environmental governance since early legislation like the U.S. Clean Air Act of the 1970s.

The original policy measure, EP_{base}, captures the number of policy categories—among deposit-refund schemes, fees, environmentally related tax rates, tradable permits, voluntary approaches, and subsidies—in which a country has implemented at least one policy. EP_{base} ranges from 0 to 6, depending on how many categories are active in a given year.The binary variable EP_{it} is then defined as follows:

$$EP_{it} = \begin{cases} 1 & \text{if } 2 \leq EP_{\text{base}} \leq 6 \\ 0 & \text{otherwise} \end{cases}$$

This threshold is intended to reflect a substantive policy effort, where complementarities and joint effects across categories may become economically meaningful. Moreover, from a statistical standpoint, this binary simplification helps avoid issues related to sparse data across multiple categories, ensuring a more balanced distribution of observations.

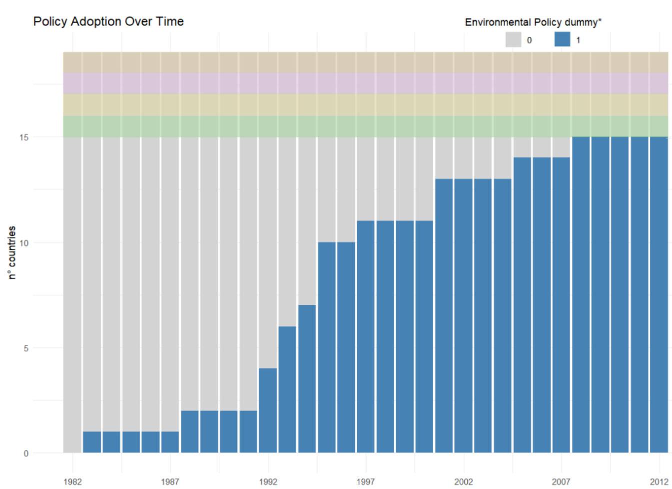


Figure 1: Policy adoption over time by country

According to this binary classification, 55% of the observations are never treated. As conveyed by the graph, there are four countries (the coloured rows) whose binary value remains constantly null: Finland, Greece, Japan, and Portugal. These countries will challenge the application of both the fixed-effects and mean-group estimators.

3.2 Descriptive Statistics

Below, the sum of all green patents (1982–2012) produced by the 19 countries in the dataset is shown, both in absolute terms (left) and per capita (right). Notably, the countries that rank lowest in absolute terms (e.g., Portugal, Greece) tend also to rank lowest per capita. This similarity suggests — with caution — that their weak green patent performance is unlikely due to a strategic focus on non-green R&D . Instead, it may reflect a broader lack of investment in R&D overall.

Table 1: Country Green Patent Statistics

Country	Total Green patents 1982-2012	Country	Patents per 100 people (%)
United States	142 616.9157	Germany	0.145 %
Germany	118 321.9118	Korea	0.136 %
Japan	95 220.7832	Sweden	0.075 %
Korea	66 921.8354	Japan	0.075 %
France	29 825.2360	Austria	0.073 %
United Kingdom	17 298.8683	Finland	0.071 %
Canada	10 577.8189	Denmark	0.070 %
Italy	8283.0386	United States	0.050 %
Netherlands	7475.6409	France	0.048 %
Sweden	6721.5579	Netherlands	0.046 %
Spain	6116.0269	Canada	0.033 %
Austria	5948.9888	United Kingdom	0.028 %
Australia	3951.3965	Belgium	0.025 %
Denmark	3845.3662	Australia	0.020 %
Finland	3713.9659	Ireland	0.017 %
Belgium	2715.0300	Italy	0.014 %
Ireland	728.6303	Spain	0.014 %
Greece	573.4387	Greece	0.005 %
Portugal	469.8069	Portugal	0.004 %

In the absence of data on either non-green patents or green-specific R&D expenditures, it is not possible to confirm or reject hypotheses of technological specialization. This limitation poses a challenge for evaluating the relative efficiency of countries based on their green patent output per unit of total R&D investment. For instance, consider the graph in the next page, plotting Green patents on the Y-axis and R&D expenditure on the X-axis for four top producers of green patents: the U.S., a country with fewer green patents per dollar may not be less efficient in innovation overall , but may instead be allocating its R&D resources toward non-green technological domains.

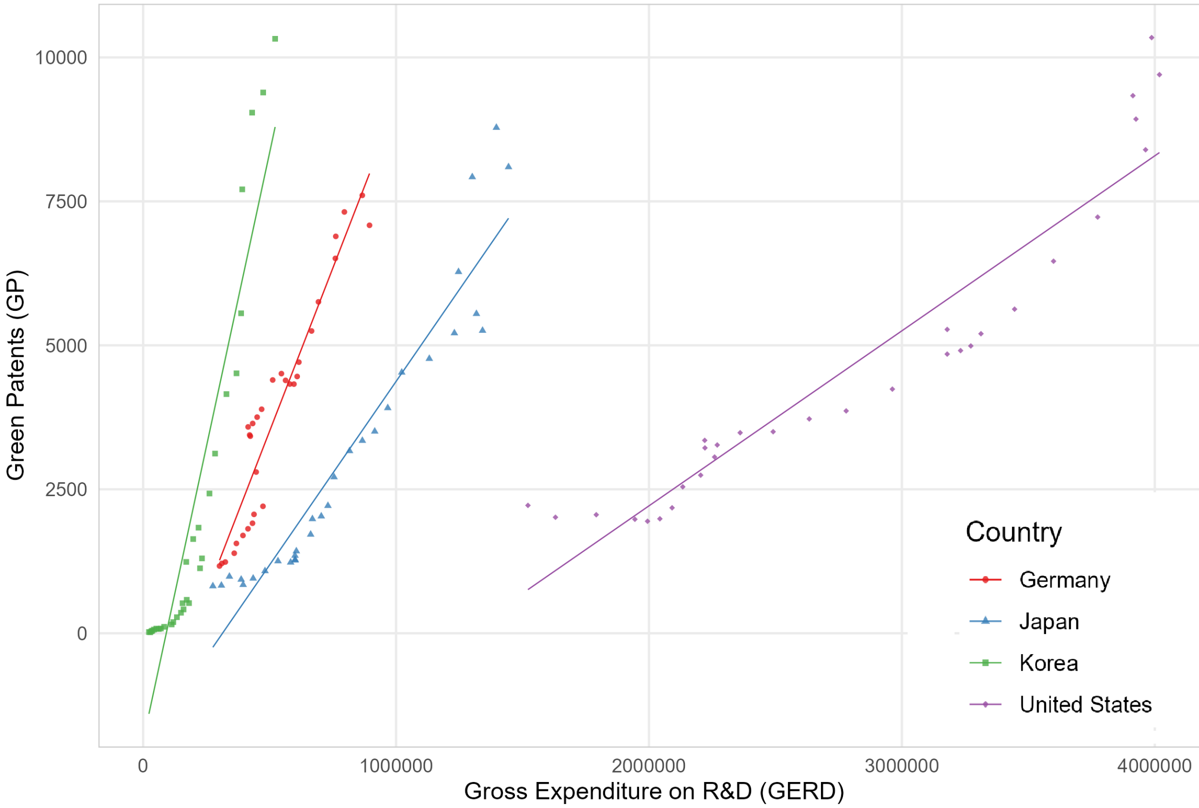


Figure 2: Green Patents vs R&D Expenditure (High-GP Countries)

3.2.1 Correlation Matrix

We might briefly consider the correlation matrix for the above variables. Not much could be guessed *a priori*, because while the interconnection among OECD states suggests that the R&D will be affected by macro-economic variables common to the highly-developed "economic area", the heterogeneity and path dependency for different clusters of countries might be stronger than the detrimental effects of, for instance, aggregate demand; furthermore, the hypothesis of a crowding-out effect of international spillovers implies that the given positive relation between R&D and Foreign R&D stemming from this latter reasoning might be opposed by the use of Foreign R&D as a substitute for the domestic one.

Table 2: Correlation Matrix

	R&D	HC	GP	Foreign R&D	Foreign HC
R&D	1.00	0.42	0.74	-0.04	0.41
HC	0.42	1.00	0.48	0.32	0.66
GP	0.74	0.48	1.00	0.03	0.44
Foreign R&D	-0.04	0.32	0.03	1.00	0.58
Foreign HC	0.41	0.66	0.44	0.58	1.00

No variables appears to be "alarmingly" correlated (or uncorrelated) with others; nor do we see nonsensical signs of the correlations. The highest correlation is given by green patents - domestic gross expenditure on R&D (0.74), followed by foreign HC and domestic HC (0.66), foreign HC (0.61), foreign Gross Expenditure on R&D and Foreign HC (0.58).

Finally, let us consider the elephant in the room, namely the plausible common (and positive) trend that might characterize the variables.

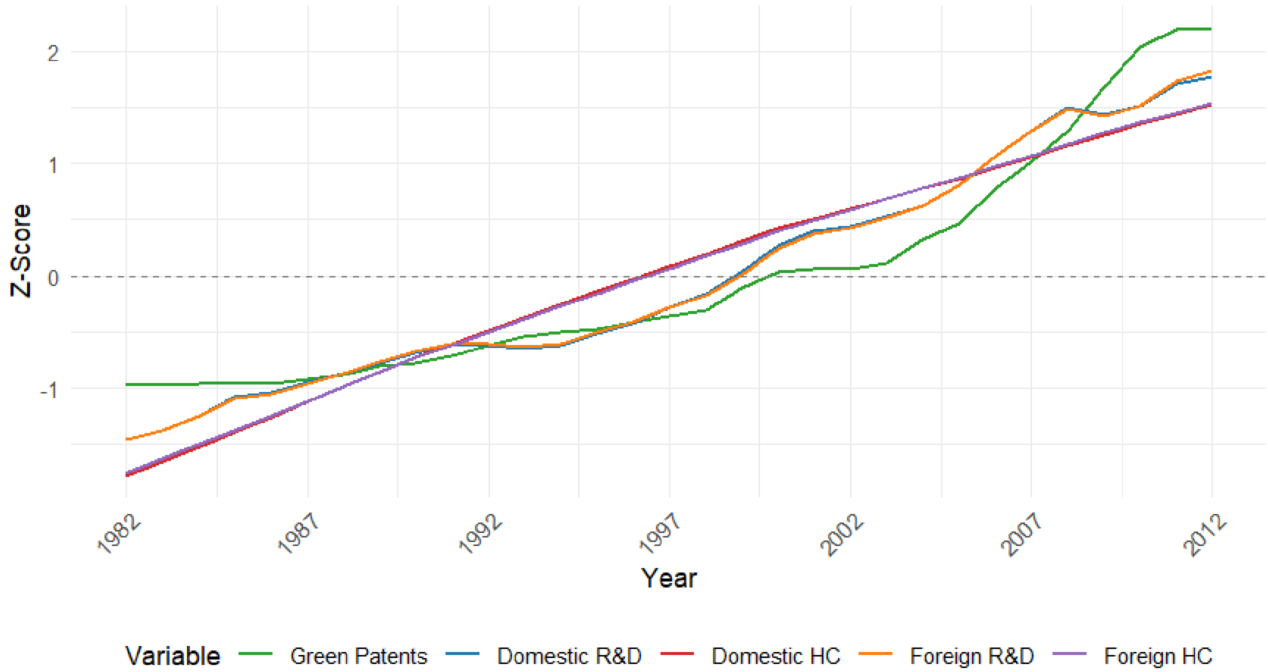


Figure 3: Standardized Yearly Sums of Innovation Inputs and Outputs

It should come as no surprise that there are lines overlapping: these are, given our proxy for foreign spillovers, the domestic and foreign pairs for R&D and HC. But there is indeed a trend, suggesting a degree of correlation that is simply given by the passage of time and the agglomerated "developing" of the OECD economies.

Given both the characteristics of panel-data, and the extreme interconnection of a OECD dataset, it would be useless to anticipate an analysis with the results without having considered the appropriate panel-data model and a plausible unobserved heterogeneity. The next section provides a quick summary of such models, before moving to their weighting, one after another,first with a Cobb-douglas specification and then with a Trans-log one.

3.3 Large panel data econometric modelling

Panel data econometrics provides powerful tools for analyzing datasets with both cross-sectional and time-series dimensions. The Common Correlated Effects Mean Group (CCEMG) estimator serves as the most comprehensive framework, accommodating unobserved common factors, unit-specific slopes, and cross-sectional dependence. Its general specification captures the complexity inherent in many economic datasets:

$$y_{it} = \alpha_i + \boldsymbol{\beta}_i' \mathbf{x}_{it} + \boldsymbol{\lambda}_i' \mathbf{f}_t + \varepsilon_{it} \tag{1}$$

Here, α_i represents unit-specific intercepts (fixed effects), $\boldsymbol{\beta}_i$ denotes unit-specific slope coefficients, and $\boldsymbol{\lambda}_i' \mathbf{f}_t$ captures unobserved common factors with heterogeneous factor loadings. This flexible formulation nests all standard panel data models as restricted special cases. The restrictions imposed by conventional models reveal their underlying assumptions about slope homogeneity and cross-sectional dependence.

Table 3: Nested Panel Estimators as Restrictions on CCEMG

Model	**Restrictions on CCEMG**
CCEP (Pooled Coefficients)	$\boldsymbol{\beta}_i = \boldsymbol{\beta} \quad \forall i$
Two-Way Fixed Effects	$\boldsymbol{\beta}_i = \boldsymbol{\beta}, \quad \boldsymbol{\lambda}_i = 1 \quad \forall i$
Fixed Effects (Within Estimator)	$\boldsymbol{\beta}_i = \boldsymbol{\beta}, \quad \boldsymbol{\lambda}_i' \mathbf{f}_t = 0, \quad \mathrm{Cov}(a_i, x_{it}) \neq 0$
Random Effects	$\boldsymbol{\beta}_i = \boldsymbol{\beta}, \quad \boldsymbol{\lambda}_i' \mathbf{f}_t = 0, \quad \mathrm{Cov}(a_i, x_{it}) = 0$
Random Trend Model	$\boldsymbol{\lambda}_i' \mathbf{f}_t = \delta_i t \quad$ (deterministic trend)
Pooled OLS	$\alpha_i = \alpha, \quad \boldsymbol{\beta}_i = \boldsymbol{\beta}, \quad \boldsymbol{\lambda}_i' \mathbf{f}_t = 0$

3.3.1 The Restrictive End of the Spectrum: Pooled OLS

The most constrained specification is Pooled Ordinary Least Squares (Pooled OLS), which imposes homogeneity in both intercepts and slopes while ignoring common factors:

$$y_{it} = \alpha + \boldsymbol{\beta}' \mathbf{x}_{it} + u_{it}. \tag{2}$$

where y_{it} is the observation of the dependent variable for the ith cross-sectional unit at time t for $i = 1, 2, \ldots, N; t = 1, 2, \ldots, T$; $\boldsymbol{\beta}$ is a k-dimensional vector of unknown parameters, $\mathbf{x_{it}}$ is a $k \times 1$ vector of regressors, and u_{it} is the error term.

Such a model can be estimated by the Pooled-OLS estimator:

$$\hat{\beta}_{pooled} = \frac{\sum_{i=1}^{N} \sum_{t=1}^{T} (\mathbf{x_{it}} - \bar{\mathbf{x}})(y_{it} - \bar{y})}{\sum_{i=1}^{N} \sum_{t=1}^{T} (\mathbf{x_{it}} - \bar{\mathbf{x}})(\mathbf{x_{it}} - \bar{\mathbf{x}})'}$$

where:

$$\bar{\mathbf{x}} = \frac{\sum_{i=1}^{N} \sum_{t=1}^{T} \mathbf{x_{it}}}{NT}, \quad \bar{y} = \frac{\sum_{i=1}^{N} \sum_{t=1}^{T} y_{it}}{NT}$$

For the Pooled-OLS to be consistent and unbiased, strict exogeneity $E(u_{it} | \mathbf{x_{it}}') = 0$, for all i, t and t', along with intercept-homogeneity ($\alpha_i = a$) is required. Inference further requires cross-sectional independence of the errors but withstands cross-sectionally heteroscedasticity and serial correlation (Pesaran, 2015a). In pooled cross-sections, a blind use of this estimator represents a smaller hazard, compared to panel-data, where it is conversely associated with bias from endogeneity. Intuitively, the pooled-ols pools (that is, does not differentiate between) two observations, from the same unit but at different time, and two observations from different units. The results are admissible in the case of pooled cross-section as the randomization before every new sample proper to the observation-generating process makes, intuitively, the just-considered distinction superfluous. Consider the graphs from Hsiao 2003, what the Pooled-OLS traces in the following figures is the straight (non-dotted) lines:

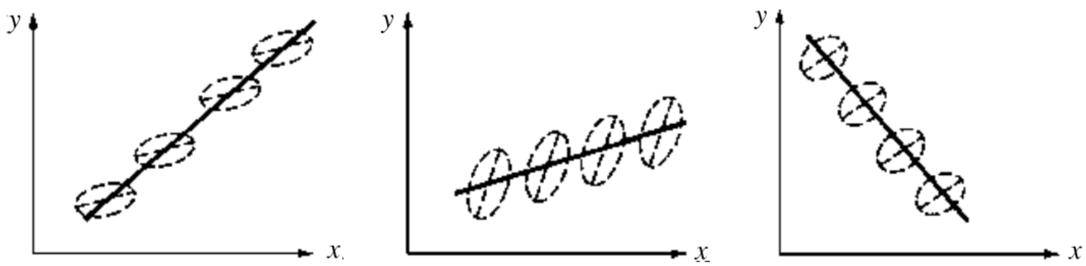


Figure 4: Hsiao 2003

The opposite signs of the pooled and unit-specific slopes must imply that the former is biased, and thus that strict exogeneity along with intercept-homogeneity are violated. Indeed, a violation of the latter implies a violation of the former when the heterogeneity in intercept is correlated with the regressors: as the failure to model heterogeneity implies that the error term now contains such heterogeneity, the latter becomes correlated with the regressors, thereby causing an endogeneity that is accordingly-named "unobserved heterogeneity bias".

Formally, given a "true" data-generating process with

$$\alpha_i = a + \eta_i : \quad y_{it} = \alpha_i + \boldsymbol{\beta'} \boldsymbol{x_{it}} + \underbrace{(u_{it})}_{\epsilon_{it}},$$

but a mistakenly specified model:

$$y_{it} = a + \boldsymbol{\beta'} \boldsymbol{x_{it}} + \underbrace{(u_{it})}_{\epsilon_{it}+\eta_i}.$$

a (general) exogeneity violation $E[u_{it}|\mathbf{x_{it}}] \neq 0$ can be rewritten as $E[\epsilon_{it} + \eta_i|\mathbf{x_{it}}] \neq 0$. When $E[u_{it}|\mathbf{x_{it}}] \neq 0 \impliedby E[\eta_i|\mathbf{x_{it}}] \neq 0$ (as in the above figures), we have the unobserved heterogeneity bias.

3.3.2 One-way Fixed Effects

The fixed-effects estimator relaxes the intercept homogeneity restriction while maintaining slope homogeneity. Accordingly, controlling for the unobserved heterogeneity bias once intercepts are allowed to be heterogeneous passes through the (presumed) static nature of such heterogeneity, as done through the fixed-effect (aka within-transformation) estimator. This will present a cost: any estimate on a constant variable (sex, nationality and so on) will be impossible, as the following transformation cannot differentiate between a unit-specific, time-constant α_i and a unit-specific, time-constant x_i.

The fixed-effect estimator is an OLS one applied to the following transformed regression:

$$\hat{\beta}_{FE} = \hat{\beta}_{OLS}: \quad y_{it} - \overline{y}_i = \boldsymbol{\beta'}(\mathbf{x_{it}} - \overline{\mathbf{x}}_\mathbf{i}) + (u_{it} - \overline{u}_i),$$

where

$$\overline{y}_i = \frac{1}{T}\sum_{t=1}^{T} y_{it}, \quad \overline{\mathbf{x}}_\mathbf{i} = \frac{1}{T}\sum_{t=1}^{T} \mathbf{x_{it}}, \quad \overline{u}_i = \frac{1}{T}\sum_{t=1}^{T} u_{it}.$$

From the point of view of the unobserved heterogeneity bias, the main advantage is clearly the possibility of allowing for endogeneity coming from a very plausible correlation between unobserved heterogeneity and the regressors. This notwithstanding, it is important to understand that the fact that the fixed-effects estimator allows for conditional strict endogeneity does not imply that it is robust to unconditional strict exogeneity; in fact, it is not so, and other methods have better chances to produce unbiased estimators when, for instance, regressors cannot be assumed to be fully exogeneous[3]. Consider, respectively, an unconditional strict exogeneity and a conditional one:

$$\mathbb{E}\left[u_{it} \mid x_{i,1}, x_{i,2}, \ldots, x_{i,T}\right] = 0, \quad t = 1, \ldots, T, \quad i = 1, \ldots, N$$

$$\mathbb{E}\left[u_{it} \mid x_{i,1}, x_{i,2}, \ldots, x_{i,T}, \eta_i\right] = 0, \quad t = 1, \ldots, T, \quad i = 1, \ldots, N$$

The further specification in the latter logically implies a "restriction of the restriction" for which $\hat{\beta}_{FE}$ remains unbiased, namely: *for a given* unobserved heterogeneity η_i that *may or may be not* correlated with the regressors, we are assuming that the regressors are strictly exogenous. The correlation between the regressors and the errors that remains in our model must depend *only* on that part of the error term that is due to unobserved heterogeneity when the latter is unspecifiable; thus, we are "only" saying that the fixed-effects estimator is not biased by *that particular threat to strict exogeneity coming from* $\mathbb{E}(x_i\eta_i) \neq 0$.

Regarding the asymptotic properties of the fixed-effects estimator, the consistency of $\hat{\beta}_{FE}$ is valid for T fixed, $N \to \infty$, and for $N, T \to \infty$. Conversely, while $\hat{\alpha}_i$ can be computed as

$$\hat{\alpha}_i = \overline{y}_i - \boldsymbol{\hat{\beta}'_{FE}\overline{x}_i}$$

for such estimates to be consistent, N must be fixed as T goes to infinity (Arellano, 2003), as *"when N grows, the number of parameters α_i to be estimated becomes larger and larger"* (Balestra and Krishnakumar, 1996). Finally, it should be noted that insofar as $\mathbb{E}(x_i\eta_i) \neq 0$ and the errors u_{it} are independent with constant variance, the estimators of α and β in the model are in fact BLUE (Balestra and Krishnakumar, 1996).

[3]See Pesaran 2015c.

It is common in panel-data textbooks to introduce, after the fixed-effects estimator, the "random-effects" one, as the latter is more efficient when $\mathbb{E}(x_i\eta_i) = 0$. From an applied perspective, however, the importance of such an estimator remains unclear, as correlation between unobserved heterogeneity and the regressors is likely to be the norm in the field of economics. Instead, we consider that further specification of the fixed-effects that is able to withstand time-specific heterogeneity.

3.3.3 Two-way Fixed Effects

So far, the unobserved heterogeneity has been defined as time-invariant; but nothing precludes the two models just considered from modelling time-specific unit-invariant unobserved heterogeneity. The estimators will address the fixed or random effects "symmetrically", from the point of view of the cross-sectional dimension and into a temporal one. The modelling of both a unit-specific, time-constant unobserved heterogeneity and a unit-constant, time-specific one is called a two-way fixed-effects model:

$$y_{it} = \alpha_i + d_t + \boldsymbol{\beta' x_{it}} + u_{it},$$

where the d_t are the time-specific effects.

Two words of caution should be spent on this specification. The first one concerns a true-regression line congruent to the above equation, whose parameters of interest are estimated instead through the one-way fixed-effects estimator: the resulting estimates are biased and inconsistent (Pesaran, 2015a). Secondly, the fixed-effect estimator, in a two-ways specification, demeans through "cross-units" averages as well:

$$\hat{\beta}_{(2-way\,FE)} = \hat{\beta}_{OLS}: \quad y_{it} - \overline{y}_i - \overline{y}_t = \boldsymbol{\beta'}(\boldsymbol{x_{it}} - \boldsymbol{\overline{x}_i} - \boldsymbol{\overline{x}_t}) + (u_{it} - \overline{u}_i - \overline{u}_t),$$

with this second average given by:

$$\overline{y}_t = \frac{1}{N}\sum_{i=1}^{N} y_{it}.$$

Congruently, the price of the fixed-effect restraining the unobserved heterogeneity from biasing our estimates doubles: now it is not just any estimate of a time-constant variable that becomes impossible, but also estimates of unit-constant ones.

3.3.4 Mean Group

The Mean Group (MG) estimator represents a significant relaxation of assumptions by allowing fully heterogeneous slopes across units while still ignoring cross-sectional dependence. In a most extreme form, slope heterogeneity would be modelled with each slope being unique to both each observation and each time:

$$y_{it} = \boldsymbol{\beta'_{it}} x_{it} + u_{it}, \quad i = 1, 2, \ldots, N, \quad t = 1, 2, \ldots, T.$$

Beside the statistical implausibility of estimating a number of parameters equal to the number of observations, we should ask ourselves to what extent a model reflecting each aspect of the reality being modelled is effectively a model; that is, a simplification (Balestra, 1996). To overcome both the statistical and speculative irrelevance of the above model, the tendency is to focus on "*common features of interest*" (Pesaran, 2015b), whereby the response parameter is de-structured into two components:

$$\boldsymbol{\beta_i} = \boldsymbol{\beta} + \boldsymbol{\eta_i}, \quad i = 1, 2, \ldots, N,$$

where $\boldsymbol{\beta}$ is a $k \times 1$ vector of constants, and $\boldsymbol{\eta_i}$ is a $k \times 1$ vector of stationary random variables[4], and:

$$E(\boldsymbol{\eta_i}) = 0, \quad E(\boldsymbol{\eta_i x'_{it}}) = 0,$$

$$E(\boldsymbol{\eta_i \eta_j}) = \begin{cases} \Omega_\eta & \text{if } i = j, \\ 0 & \text{if } i \neq j. \end{cases}$$

A basic approach capable of addressing unit-specific slopes is the mean-group (MG) estimator proposed by Pesaran and Smith (1995), and defined as the simple average of the OLS estimators, $\hat{\beta}_i$; that is:

$$\hat{\beta}_{MG} = \frac{1}{N} \sum_{i=1}^{N} \hat{\beta}_i,$$

In general, the MG estimator is unbiased and consistent for $N, T \to \infty$, and for fixed T as well if strict exogeneity holds; nonetheless, it is not robust to cross-sectional dependence (an issue considered in the next section). Its efficiency depends on the variance of $\hat{\beta}_i$, which is in turn inversely related to the number of repeated t observations for the unit i of the panel dataset: this risks a lack of statistical significance for a majority of regressions with "limited" time-dimension (e.g., 40 years implies for $\hat{\beta}_i$ a regression akin to an OLS cross-sectional one with 40 units). More in general, if the slope-homogeneity assumption holds, the MG estimator is less efficient than both the pooled-OLS and the Fixed-effects estimator.

3.3.5 Common-Correlated Effects

Cross-sectional dependence (CSD) refers to the interdependence among cross-sectional units in econometric models. Unlike slope and intercept heterogeneity, CSD is not eliminated but rather interpreted through two perspectives: unobservable (and/or observed) common factors (considered in a multifactor error structure) and spatial effects. These perspectives differ mainly in their approach—whether they analyse shocks (e.g., oil crises, investor confidence) as directly influencing all units at different levels or spreading indirectly through inter-unit interactions.

[4]Notice that, in such definition, we are opting for assuming such heterogeneity to be time-invariant, as proposed by Swamy 1970; see Hsiao 1974 for a modelling of slope-heterogeneity varying among both the cross-sectional units and for the same cross-sectional unit at different times.

A second difference concerns the "strength" of the supposed cross-sectional dependence. The latter is conceptualized and measured according to Chudik et al. (2011), whereby strength indicates the number of cross-sectional units affected by the factor as $N \to \infty$:

$$\lim_{N \to \infty} N^{-b} \sum_{i=1}^{N} |\rho_i| = K < \infty,$$

where ρ_i is the factor loading for unit i, and b (ranging from $0 \leq b \leq 1$) indicates the strength of the factor: if $b = 0$, the factor affects only a fixed number of units; if $b < 1$, factors are "weak" as the number of affected units grows slower than N; this is the case with spatial effects. Conversely, unobserved effects are usually assumed to be strong, thus having $b = 1$: the subset of affected units grows at the same rate as N.

It is also important to understand how, in Chudik et al. (2011) framework, CSD is both multi-level, with strong and weak factors (thus unobserved effects and spatial ones) that can easily be concurrent at the same time; and non-cumulative: many weak cross-sectional factors do not add up to a strong one. Finally, notice that the presence of the former weak factors defines CSD as weak, while only the presence of at least one strong factor implies strong CSD. With this distinction in mind, the focus can be restricted to the approach used in the paper, namely the unobserved effects approach.

In an unobserved effects framework, errors u_{it} are de-structured into an idiosyncratic term, ϵ_{it}, and an $m \times 1$ vector of unobserved common factors, $\boldsymbol{\xi_t}$, with unit-specific factor loadings, $\boldsymbol{\rho_i}$:

$$u_{it} = \boldsymbol{\rho_i'} \boldsymbol{\xi_t} + \epsilon_{it}$$

The consequent data-generating process for y_{it} is thus:

$$y_{it} = \boldsymbol{\alpha_i'} \boldsymbol{d_t} + \boldsymbol{\beta_i'} \boldsymbol{x_{it}} + \underbrace{(u_{it})}_{\boldsymbol{\rho_i'}\boldsymbol{\xi_t} + \epsilon_{it}} \tag{3}$$

where $\boldsymbol{d_t}$ is an $l \times 1$ vector of observed common effects, $\boldsymbol{\alpha_i'}$ is the associated vector of parameters, and $\boldsymbol{x_{it}}$ is a $k \times 1$ vector of explanatory variables. The data-generating process allows for both slope homogeneity and heterogeneity as well, with the latter case defining $\boldsymbol{\beta_i'}$ with a random coefficient specification not dissimilar from Swamy's one above considered:

$$\boldsymbol{\beta_i} = \boldsymbol{\beta} + \boldsymbol{\eta_i}, \quad \boldsymbol{\eta_i} \sim i.i.d.(0, \Theta_\eta)$$

The idiosyncratic errors, ϵ_{it}, are assumed to be independently distributed over $(\boldsymbol{d_t}, \boldsymbol{x_{it}})$, whereas the unobserved factors, $\boldsymbol{\xi_t}$, can be correlated with $(\boldsymbol{d_t}, \boldsymbol{x_{it}})$, through the use of explanatory variables as linear functions of the observed and unobserved common factors (thus respectively $\boldsymbol{d_t}$ and $\boldsymbol{\xi_t}$):

$$\boldsymbol{x_{it}} = \boldsymbol{A}_i'\boldsymbol{d_t} + \boldsymbol{\Gamma}_i'\boldsymbol{\xi_t} + v_{it}$$

where $\boldsymbol{A_i}$ and $\boldsymbol{\Gamma_i}$ are $l \times k$ and $m \times k$ factor loading matrices, and $v_{it} = (v_{i1t}, v_{i2t}, \ldots, v_{ikt})'$ are the specific components of $\boldsymbol{x_{it}}$. Substituting such $\boldsymbol{x_{it}}$ in (5), results in:

$$\underset{(K+1)\times 1}{\mathbf{z}_{\mathrm{it}}} = \begin{pmatrix} y_{it} \\ \mathbf{x}_{it} \end{pmatrix} = \underset{(K+1)\times l}{\mathbf{B}_{\mathrm{i}}'} \quad \underset{l\times 1}{\mathbf{d}_{\mathrm{t}}} + \underset{(K+1)\times m}{\mathbf{C}_{\mathrm{i}}'} \quad \underset{m\times 1}{\boldsymbol{\xi}_{\mathrm{t}}} + \underset{(K+1)\times 1}{\mathbf{u}_{\mathrm{it}}} \tag{4}$$

where

$$\boldsymbol{u_{it}} = \begin{pmatrix} \mathbf{1} & \boldsymbol{\beta}_i' \\ \mathbf{0} & \boldsymbol{I_k} \end{pmatrix} \begin{pmatrix} \epsilon_{it} \\ \boldsymbol{v_{it}} \end{pmatrix} = \begin{pmatrix} \epsilon_{it} + \boldsymbol{\beta}_i'\boldsymbol{v_{it}} \\ \boldsymbol{v_{it}} \end{pmatrix}$$

$$\boldsymbol{B}_i = \begin{pmatrix} \boldsymbol{\alpha}_i & \boldsymbol{A}_i \end{pmatrix} \begin{pmatrix} \mathbf{1} & \mathbf{0} \\ \boldsymbol{\beta_i} & \boldsymbol{I_k} \end{pmatrix}$$

$$\boldsymbol{C}_i = \begin{pmatrix} \boldsymbol{\rho}_i & \boldsymbol{\Gamma}_i \end{pmatrix} \begin{pmatrix} \mathbf{1} & \mathbf{0} \\ \boldsymbol{\beta_i} & \boldsymbol{I_k} \end{pmatrix}$$

On the basis of the above equation, cross-sectional averages of the dependent variable and the individual-specific regressors, coupled with the observed effects $\boldsymbol{d_t}$, can be used as proxies for the unobserved factors, following the approach proposed by Pesaran (2006) and denoted as the Common Correlated Effects (CCE) approach, where the regressors are augmented with:

$$[\boldsymbol{d}_t' \, \bar{\boldsymbol{z}}_{\boldsymbol{wt}}'] \,,$$

where the latter are the cross-sectional averages of the above-defined $\boldsymbol{z_{it}}$:

$$\bar{\boldsymbol{z}}_{\boldsymbol{wt}}' = \sum_{j=1}^{N} w_j \boldsymbol{z_{jt}},$$

with w_j being weights equal to $1/N$.

Once the original regression is augmented through such cross-sectional averages, the mean group estimator and the pooled one can be used, depending on the presumed amount of heterogeneity in the slopes, resulting in, respectively, the CCEMG and the CCEP. The most important benefit of this approach, being tailored to large-N large-T data, is the consistency of the estimators regardless of whether the multi-factor error structure is concurrent to spatial (weak) cross-sectional dependence or not. This is especially important because there are theoretical reasons to predict the co-existence of both forms of cross-sectional dependence.

Having considered the CCEMG in the beginning of this section, we consider the CCEP, which relaxes the assumption of no cross-sectional dependence while maintaining slope homogeneity:

$$y_{it} = \alpha_i + \boldsymbol{\beta}'\mathbf{x}_{it} + \boldsymbol{\lambda}_i'\mathbf{f}_t + \varepsilon_{it} \tag{5}$$

Unlike its CCEMG counterpart, it maintains the assumption of slope homogeneity ($\boldsymbol{\beta}_i = \boldsymbol{\beta}$ for all units). The estimator remains consistent even in the presence of strong cross-sectional dependence, making it particularly valuable for empirical applications where common shocks affect all cross-sectional units. As the pooled version of the CCEMG approach, it provides a middle ground between the restrictive pooled estimators and the fully flexible CCEMG specification.

3.3.6 Conclusions

This section has introduced the primary benefits and issues in the econometrics of panel data. Panel data track multiple units over time, thus enabling researchers to favour between individual-specific traits or time-varying regressors as the more plausible factor behind a temporal change in the dependent variable. However, failing to account for heterogeneity has been shown to cause biased estimates, as exemplified by our reference to the pooled-OLS estimator.

This pooled-OLS, which “pools” by treating all observations alike, has been used through-out the section as a usually-inadequate estimator for panel-data. We have, consequently, proposed several estimators capable of facing the different threats posed by heterogeneity in this regard; that these specific threats to (and thus the specific cures for) the unbiasedness and consistency of the estimators depend on the way in which heterogeneity relates to the regressors is the main insight from what has been hitherto considered.

Heterogeneity in intercepts has been addressed by the fixed-effect estimator; likewise, the mean-group estimator has been proposed to address slope-heterogeneity. We followed Pesaran 2016 in ordering the two accordingly and in concluding with cross-sectional dependence, thus also setting a trend of increasing theoretical difficulty.

Last but not least, it should be highlighted that such ordering does not reflect the plausibility of the threat, that is: it would be a hazard to say that heterogeneity of intercepts is more common than heterogeneity in slopes or that the latter is in turn more probable than a bias from cross-sectional dependence. An apparent popularity of the fixed-effects estimator relatively to, say, the CCEMP probably depends on the "older age" of the former, or on its conceptual straightforwardness relatively to the latter; or, more optimistically, on the awareness of the researcher on the forms of heterogeneity deemed plausible by earlier theory *in regards to a given object of analysis*: the latter remaining the primary aspect in determining an optimal choice among the methods described in this section.

Table 4: Panel Data Estimators: Assumptions and Their Relationship to CCEMG

CCEMG estimator: $y_{it} = \alpha_i + \boldsymbol{\beta}_i' \mathbf{x}_{it} + \boldsymbol{\lambda}_i' \mathbf{f}_t + \varepsilon_{it}$

Model and CCEMG Restriction	Key Assumptions and Features
CCEP	
Restriction: $\boldsymbol{\beta}_i = \boldsymbol{\beta} \quad \forall i$	- Slope homogeneity across units - Accounts for cross-sectional dependence - Pooled counterpart to CCEMG
Mean Group (MG)	
Restriction: $\boldsymbol{\lambda}_i' \mathbf{f}_t = 0$	- Fully heterogeneous slopes - Separate regressions for each unit - No pooling of parameters
Two-Way Fixed Effects	
Restriction: $\boldsymbol{\beta}_i = \boldsymbol{\beta}, \quad \boldsymbol{\lambda}_i = 1 \quad \forall i$	- Unit-specific intercepts - Homogeneous slopes - Time fixed effects (common factor absorbed)
Fixed Effects (Within Estimator)	
Restriction: $\boldsymbol{\beta}_i = \boldsymbol{\beta}, \quad \boldsymbol{\lambda}_i' \mathbf{f}_t = 0$	- Unit-specific intercepts - Slope homogeneity - Correlated effects: $\mathrm{Cov}(a_i, x_{it}) \neq 0$
Random Effects (RE)	
Restriction: $\boldsymbol{\beta}_i = \boldsymbol{\beta}, \quad \boldsymbol{\lambda}_i' \mathbf{f}_t = 0$	- Unit-specific effects modeled as random - Slope homogeneity - Uncorrelated effects: $\mathrm{Cov}(a_i, x_{it}) = 0$ - More efficient than FE if assumption holds
Random Trend Model	
Restriction: $\boldsymbol{\lambda}_i' \mathbf{f}_t = \delta_i t$	- Allows for unit-specific deterministic trends - Slopes possibly heterogeneous
Pooled OLS	
Restriction: $\alpha_i = \alpha, \quad \boldsymbol{\beta}_i = \boldsymbol{\beta}, \quad \boldsymbol{\lambda}_i' \mathbf{f}_t = 0$	- Homogeneous intercepts and slopes across units - Ignores unobserved heterogeneity

3.3.7 Econometric Analysis - Cobb-Douglas

We start with a cobb-douglas functional form, in light of its simplifying assumptions below-considered:

$$\log(gp_{it}) = \beta_1 \log(R\&D_{it}) + \beta_2 \log(WRD_{it}) + \beta_3 \log(H_{it}) + \beta_4 \log(WHK_{it}) + EP_{it} + e_{it} \quad (6)$$

While this model has its obvious benefits in terms of interpretation of the results, it should be noticed that it implies strong assumptions. First, the additive specification forces a *ceteris paribus* perspective, whereby each coefficient is estimated keeping the other variables fixed (contrast this with a multiplicative specification when the coefficient of one variable depends on the value assumed by the variable the former one is being multiplied with). Secondly, we have considered in the second section of the literature review how the hypothesis of diminishing returns is "*quite sensitive to functional form, weighting schemes and the particular point at which the elasticity is evaluated*" (Griliches 1990, 303): here the log-log specification linearizes whatever non-linearity the data-generating process is likely to have, and this in a context in which we are being warned about the high probability of a functional form bias.

3.3.7.1 Pooled-OLS

The pooled-OLS estimates an ordinary least squares regression on the above formula ; allowing no filtering of observations of one state from another. With thus the 589 "pooled" observations of the dataset, the estimated coefficients are as likely to be significant as, given the inability to account for unobserved heterogeneity, they are to be biased.

All the coefficients but the policy-dummy are statistically significant. Except for the almost null negative coefficient of foreign R&D expenditure, the positive signs are also coherent to the KPF. The binary policy is irrelevant in magnitude and statistically insignificant. As previously said, nonetheless, these estimates are very likely to be biased. We turn next to the fixed-effects estimator, favoring thus a technique that sits on the other extreme in terms of a relatively lower efficiency but a higher robustness to unobserved heterogeneity and thus, it is hoped, a higher chance of unbiasedness.

Table 5: Pooled OLS Regression Results

Variable	**Coefficient**
Dom. GERD	0.9588*** (0.0000)
For. GERD	-0.1982*** (0.0001)
Dom. HC	2.9952*** (0.0000)
For. HC	2.1032** (0.0041)
Policy Dummy	0.0385 (0.5701)

3.3.7.2 Fixed-effects Estimators (One-way and Two-way)

Applying the fixed-effects model to the basic specification results in:

$$\log(gp_{it}) = \alpha_i + \beta_1 \log(R\&D_{it}) + \beta_2 \log(WRD_{it}) + \beta_3 \log(H_{it}) + \beta_4 \log(WHK_{it}) + EP_{it} + e_{it} \quad (7)$$

In this analysis, α_i represent country-specific and time-constant heterogeneity. In the current dataset, however, applying the within-transformation (demeaning through time-averages) is more challenging, as four countries do not show variation in the dummy-variable; consequently, any variable that is constant within units over time is dropped because it is perfectly collinear with the unit-fixed effects. Estimates of the dummy variable therefore lacks inference from the observations from these 4 countries times the 31 years considered, essentially regressing (for the dummy variable) on a dataset with now 56% of treated observations and 44% of untreated.

Table 6: Fixed Effects Regression Results Comparison

Variable	**One-Way FE Coefficient**	**Two-Way FE Coefficient**
Dom. GERD	0.7764*** (0.0000)	0.7333*** (0.0000)
For. GERD	0.4698*** (0.0001)	0.5028*** (0.0001)
Dom. HC	8.0442*** (0.0000)	7.8342*** (0.0000)
For. HC	-3.1873*** (0.0009)	-4.3230** (0.0087)
Policy Dummy	0.0366 (0.4493)	0.0172 (0.7366)

The fixed-effects estimates diverge substantially from Pooled OLS, particularly for foreign GERD, shifiting from negative to positive significance. Domestic HC's coefficient increases markedly, while foreign HC becomes negative. The policy dummy's continued insignificance persists. In the second column, incorporating time fixed effects yields coefficients similar to the one-way, with minor attenuation in domestic GERD (-5.5%) and amplification of foreign HC's negative effect (+35%). The policy dummy's coefficient further diminishes and remains insignificant, showing no influence of temporal effects.

3.3.7.3 Comparison of Pooled OLS and Fixed Effects Results

Table 7: Regression Results Comparison

Variable	Pooled OLS	One-Way FE	Two-Way FE
Dom. GERD	0.9588***	0.7764***	0.7333***
	(0.0000)	(0.0000)	(0.0000)
For. GERD	-0.1982***	0.4698***	0.5028***
	(0.0001)	(0.0001)	(0.0001)
Dom. HC	2.9952***	8.0442***	7.8342***
	(0.0000)	(0.0000)	(0.0000)
For. HC	2.1032**	-3.1873***	-4.3230**
	(0.0041)	(0.0009)	(0.0087)
Policy Dummy	0.0385	0.0366	0.0172
	(0.5701)	(0.4493)	(0.7366)

The juxtaposition reveals persistent significance for both domestic GERD and Domestic HC; differently, magnitudes vary considerably for Foreign GERD (sign reversal between Pooled OLS and FE models) and an inverse trend for Foreign HC's (shift from positive in Pooled, to negative in the two FE). The policy dummy is consistently insignificant. It is interesting to notice that,, from an economic perspective, these could be reasonable results: domestic inputs matter, while the spillovers variation in positive-negative contribution are simply the result of differences among countries that prevent stability in the coefficient signs. Clearly, the stress would move from assessing the importance of these foreign equivalents to controlling for them in estimating the relevance of their domestic counterparts: i.e., a downgrading of spillovers from basic inputs to disturbances being controlled for in estimating the fewer primary inputs, namely domestic GERD and domestic HC.

3.3.7.4 One-way and Two-way Fixed Effects Plus Random Trend

We estimate both one-way and two-way fixed effects models with unit-specific linear trends. The two-way specification includes year fixed effects (γ_t), while the one-way version omits them. The two-way fixed effects estimator with country-specific trends is:

$$y_{it} = \alpha_i + \gamma_t + \beta_1 X_{1it} + \cdots + \beta_K X_{Kit} + \delta_i t + \epsilon_{it} \tag{8}$$

where:

- y_{it}: Log of green patents (dependent variable)
- α_i: Country fixed effects (accounts for time-invariant heterogeneity)
- γ_t: Year fixed effects (controls for common temporal shocks)
- $X_{1it}, \ldots, X_{Kit}$: Regressors

- δ_i: Country-specific linear trends (random coefficients on t)
- ϵ_{it}: Idiosyncratic error (clustered at country level)

Table 8: Comparative Fixed Effects Regression Results

Variable	One-Way FE	One-Way FE + Trend	Two-Way FE	Two-Way FE + Trend
Dom. GERD	0.7764*** (0.0000)	0.4720*** (0.0001)	0.7333*** (0.0000)	0.4084*** (0.0006)
For. GERD	0.4698*** (0.0001)	-0.1454 (0.3826)	0.5028*** (0.0001)	0.0675 (0.7557)
Dom. HC	8.0442*** (0.0000)	-0.8789 (0.5803)	7.8342*** (0.0000)	-1.7049 (0.3302)
For. HC	-3.1873*** (0.0009)	-4.6163 (0.0739)	-4.3230** (0.0087)	-10.1022 (0.0741)
Policy Dummy	0.0366 (0.4493)	-0.0074 (0.8758)	0.0172 (0.7366)	-0.0005 (0.9916)

The comparative analysis of the four fixed effects specifications demonstrate substantial attenuation effects when accounting for temporal dynamics, particularly for domestic R&D expenditure. The coefficient for domestic GERD declines by 39% when moving from the basic one-way fixed effects specification to the trend-augmented version, and by 44% in the corresponding two-way models.

Domestic human capital, which appears strongly significant in basic specifications with a coefficient of 8.04, becomes statistically insignificant when including unit-specific trends. The author would suggest that this might depend on the countries considered (high-developed ones) and the time-series at hand: OECD countries, by 1980, would have already accumulated a high amount of Human capital, perhaps sufficient to sustain patents-specific R&D projects (i.e., expenditures). It is then being proposed that patents produced as a function of human capital would show, in general, decreasing returns to scale after a minimum threshold that our considered countries had already reached by the initial date of our dataset, namely 1980. This, to the author, seems reasonable. The problem lies in the values shown by (foreign) human capital, whose coefficients become increasingly negative across specifications. The magnitude grows from -3.18 in basic fixed effects to -10.10 in the full two-way trend model, suggesting that crowding-out effects strengthen over longer time horizons and that conventional fixed effects models may understate these displacement effects by as much as 68%.

3.3.7.5 Random-effects estimator and Hausman-Test

We can, however, infer on the relative unbiasedness of the fixed-effects estimator by considering the correlation between the hypothetically-relevant fixed-effects and the regressors. In order to do so, a random-effects estimate has to be compared to the fixed-effects one, in a Hausman-test. The null-hypothesis in the Hausman test is $\text{Cov}(\alpha_i, \mathbf{x}_{it}) = 0$; insofar as such correlation exists, one generally concludes the optimality of fixed-effects relatively to the random-effects one.

Table 9: Hausman Test Results

Test Statistic	Value
Chisq	23.062
p-value	0.0003***

Having rejected the null-hypothesis, the random-effects estimator is now known to be biased and inconsistent, therefore one might ask what is the use in considering its estimates. The idea is that these are now expected to be quite different from the fixed-effects estimates.

Table 10: Regression Results Comparison (Four Estimators)

Variable	**Pooled OLS**	**One-Way FE**	**Two-Way FE**	**Random Effects**
Dom. GERD	0.9588*** (0.0000)	0.7764*** (0.0000)	0.7333*** (0.0000)	0.8290*** (0.0000)
For. GERD	-0.1982*** (0.0001)	0.4698*** (0.0001)	0.5028*** (0.0001)	0.3032** (0.0034)
Dom. HC	2.9952*** (0.0000)	8.0442*** (0.0000)	7.8342*** (0.0000)	7.1862*** (0.0000)
For. HC	2.1032** (0.0041)	-3.1873*** (0.0009)	-4.3230** (0.0087)	-1.8194* (0.0447)
Policy Dummy	0.0385 (0.5701)	0.0366 (0.4493)	0.0172 (0.7366)	0.0275 (0.5699)

As we can see, we are in *terra incognita*. We have a pooled-ols that gives similar results to the two fixed-effect ones, in turn closer to the random-effects, *inspite of the Hausman-test suggesting the inconsistency of the latter*. Before problematizing the results, let us circumscribe the possible causes by considering slope-heterogeneity and cross-sectional dependence.

3.3.7.6 Mean Group

The first question that one is to address before considering the mean-group estimator is the presence or absence of slope-heterogeneity. This is because, as earlier considered, if the slope-homogeneity assumption holds, the MG estimator is less efficient than *both* the pooled-OLS and the Fixed-effects estimator. A second problem lies in its computation: in effect, the statistical insignificance of its estimates is almost "induced" by construction in a panel-set with a relatively low number of period observed (recall that the different β_i whose average is the mean-group final output will each be estimated from a regression having T-observations). The same argument relatively to the bias of dropping four countries with a constant value is to be repeated, this time however leading to the algebraic impossibility of computing an average elasticity in regards to the policy variable from a set containing these four *NA* values.

Table 11: Mean Group Regression Results

Variable	**Coefficient**
Dom. GERD	0.2185 (0.3100)
For. GERD	0.2143 (0.6052)
Dom. HC	17.9941* (0.0198)
For. HC	-6.8303 (0.5162)
Policy Dummy	- (-)

Surprisingly, given the tendency for this estimator to fail statistical significance in few-t panel-dataset, we have statistical significance for (domestic) Human capital: but its large coefficient (17.9941) appears anomalous. There is, strengthening skepticism, a reason a priori not to prefer the mean-group estimator. Hsiao considers how "*there is nothing in growth theory that would lead one to think that the marginal effect of a change in high school enrolment percentages on the per capita growth of the United States should be the same as the effect on a country in sub-Saharan Africa*" (Hsiao, 2003). In this paper, conversely, the units are OECD countries, with thus a more similar economic structure among the units, suggesting that the response variable is in fact the ouput of a function that is similarly shaped, and that the peculiarity of each countries could be best handled by considering it as affecting the dependent variable, rather than "through" the regressors, through the intercept, as done with the fixed-effects, or the errors, as we turn to consider with the Common Correlated Effects in the next section.

3.3.7.7 Common Correlated Effects

Indeed, the common correlated effects (CCE) proposed by Pesaran (2006) primarily addresss such heterogeneity in the context of cross-sectional dependence of the errors. As we have seen, the choice of one of the above estimators among the others have resulted in too dramatic a change to propose inference; yet to gain a hint of the amount of cross-sectional dependence, the elasticities of the CCE transformation below should be mentally compared with earlier results. If few T-observations implied lack of statistical significance for the MG and its CCE version, the CCEP results surprises in following this pattern. Controlling for cross-sectional dependence has effectively eroded statistical significance completely in CCEP and nearly completely in CCEMG. Only foreign HC under CCEMG attains significance, though its implausible magnitude (117) suggests, as in its non-cce domestic equivalent, model misspecification. These results indicate strong cross-sectional dependence confounding earlier inferences. In general, one could have expected, given the OECD dataset, a high-degree of CSD; but not to the point of a rejection in toto of the statistical significance for the input-KPF.

Table 12: CCEP and CCEMG

Variable	**CCEP**	**CCEMG**
Dom. GERD	-0.2019	-0.0263
	(0.6563)	(0.9165)
For. GERD	-0.3170	-0.1577
	(0.4899)	(0.9214)
Dom. HC	-0.7967	7.4746
	(0.8727)	(0.7361)
For. HC	-6.9873	117.6832*
	(0.9107)	(0.0473)
Policy Dummy	-0.0239	0.0229
	(0.7720)	(0.6049)

3.3.7.8 Cobb-Douglas Summary

The analysis presented in this section reveals the influence of R&D productivity estimates to the treatment of unobserved heterogeneity, with substantial difference in signs, let alone strength of coefficients on the basis of the differing assumptions on unobserved heterogeneity inherent to each estimator. Nonethelesss, the gap in our knowledge of the real functional form for a KPF questions how much the just-considered unobserved heterogeneity is responsible for the variation, prompting, as an alternative "culprit", the restrictions of the Cobb-Douglas considered in the introduction of this section.

Initially, Pooled OLS estimates yield economically intuitive signs for domestic inputs but a perplexing negative coefficient for foreign R&D expenditure. This initial specification, while computationally straightforward, ignores the panel structure of the data and assumes away any country-specific fixed effects. The consequence is severe omitted variable bias, as unobserved time-invariant factors evidently correlate with our inputs: it is worth in this context to repeat that these time-fixed unobserved effects are being considered from a specific moment on, namely 1980, thus at a time in which virtually all OECD countries already had an economic structure commonly associated with the label of "developed".

The dramatic reversal of foreign R&D's sign under fixed effects models (turning from significantly negative to robustly positive) underscores this point. This reversal suggests that countries with inherently lower innovative capacity may simultaneously import more foreign R&D and produce fewer patents, creating a spurious negative correlation that only disappears when country-specific intercepts are controlled. Yet one-way fixed effects not only reverses foreign R&D's coefficient but amplifies domestic human capital's elasticity to implausible magnitudes (8.04), while transforming foreign human capital into a significant negative predictor. The subsequent inclusion of time fixed effects in the two-way specification intensifies foreign human capital's effect by 35%.

Most critically, incorporating unit-specific linear trends precipitates a collapse in the statistical and economic significance of our key variables. Domestic R&D elasticity halves, domestic human capital becomes indistinguishable from zero, and foreign R&D loses significance entirely. This dramatic attenuation implies what the plotting of these core variables in the descriptive statistics section already hinted at: country-specific trajectories accounted for nearly half the apparent productivity of domestic inputs in simpler specifications.

Now, the Cobb-Douglas assumes constant elasticities and ignores interaction terms. It is quite unclear in this regard how shall it be able to corroborate, consistently, a function of pairs of two "very interacting" terms, namely gross expenditure on R&D and Human Capital, whether in the domestic or in the foreign/spillover form. If the progressive intensification of foreign human capital's negative effect (from -1.82 in random effects to -10.10 in two-way FE with trends) had happened in regards to Foreign R&D, the author would have considered a crowding-out effect; conversely, such trend from foreign human capital seems more plausibly described as a failure of the model to capture interaction between domestic and foreign knowledge stocks.

The Hausman test's rejection of random effects also constitutes a problematic dilemma: it formally validates concerns about unobserved heterogeneity bias (i.e., the latter correlated with the regressors), yet random effects estimates align more closely with fixed effects than pooled-OLS does.

The hope in controlling slope-, rather than intercept-heterogeneity, in the Mean Group and

CCEMG could have been earlier discarded by sticking to theory; at least in this sense, the results are coherent in producing statistically fragile results due to the relatively short time dimension (T=31). In this regard, the only (slightly) statistically-significant result of Domestic Human capital can be safely disregarded. Much more surprising, to the author, were the results from the Common Correlated Effects Pooled estimator, where the collapse of statistical significance once cross-sectional dependence has been controlled for casted stronger doubts on the validity of our specification.

Of all these results, two variables have shown consistent results among the estimators; one "positively" and one "negatively". Starting from the latter, there has been a persistent insignificance of the policy dummy across all specifications. Now, at least in this regard, neither the Cobb-Douglas nor problems of aggregation should be the primary culprits: much more probable is the difficulty in modelling what are inherently-qualitative factors (policies) in such a way that a quantitative analysis can be realistically approached. For this reason, in attempting to free the functional form from the restrictions of the Cobb-Douglas, we will drop the dummy-variable and focus, conversely, in preserving the domestic expenditure as the cardinal input that both theory and results (because this is the positive, constant result) have proposed.

The translog function is subsequently considered to account for interaction and non-linearities; the Cobb-Douglas elasticities are plotted as a straight line on the distribution of those elasticities that the translog is capable of describing by relinquishing the assumption of their constancy.

3.3.8 Econometric Analysis - Translog

First proposed by Christensen, Jorgenson and Lau, 1973, the translog is a flexible functional form used to allow for differing elasticities, with a benchmark form that includes quadratic terms of the input as well as interaction ones:

$$\begin{aligned}
\ln Y = \alpha_0 &+ \alpha_1 \ln X_1 + \alpha_2 \ln X_2 + \alpha_3 \ln X_3 + \alpha_4 \ln X_4 \\
&+ \frac{1}{2}\beta_{11}(\ln X_1)^2 + \frac{1}{2}\beta_{22}(\ln X_2)^2 + \frac{1}{2}\beta_{33}(\ln X_3)^2 + \frac{1}{2}\beta_{44}(\ln X_4)^2 \\
&+ \beta_{12} \ln X_1 \ln X_2 + \beta_{13} \ln X_1 \ln X_3 + \beta_{14} \ln X_1 \ln X_4 \\
&+ \beta_{23} \ln X_2 \ln X_3 + \beta_{24} \ln X_2 \ln X_4 \\
&+ \beta_{34} \ln X_3 \ln X_4
\end{aligned} \tag{9}$$

Where:

- Y = Number of green patents
- X_1 = Domestic GERD (Gross Expenditure on R&D)
- X_2 = Foreign GERD
- X_3 = Domestic Human Capital (HC)

- X_4 = Foreign Human Capital (HC)
- α_0 = Constant term
- α_i = First-order coefficients
- β_{ii} = Quadratic terms
- β_{ij} = Interaction terms (with $\beta_{ij} = \beta_{ji}$ by symmetry)

Then, the output elasticity of e.g. input 1 (Domestic GERD, X_1) is given by:

$$\epsilon_1 = \frac{\partial \ln Y}{\partial \ln X_1} = \alpha_1 + \beta_{11} \ln X_1 + \beta_{12} \ln X_2 + \beta_{13} \ln X_3 + \beta_{14} \ln X_4 \tag{10}$$

Where:

- α_1 is the first-order coefficient of $\ln X_1$,
- β_{11} captures the curvature effect of X_1 (how its own marginal productivity changes),
- β_{12} , β_{13} , β_{14} measure how the elasticity of X_1 (thus, of Domestic GERD) changes with Foreign GERD (X_2), Domestic HC (X_3), Foreign HC (X_4),
- Symmetry condition: $\beta_{1j} = \beta_{j1}$ for all j.

Full-translog (as in the above formula) are relatively fewer in econometric estimates, relative to their "truncated" equivalent (where specific elements of the former are chosen), mainly because it requires a large amount of observations in its "jump" in the parameters to be estimated (Pavelescu, 2011). Hence, for instance, Griliches' restrain in weighting-in the benefit of more flexibility for a KPF at the cost of unreliability of the estimates (Griliches' 1998a); the (full) translog, the same author says in another paper, becomes useful "*if we suspect a particular complementarity between physical and research capital*"(Griliches, 1979) : in our case, we *do* suspect complementarity between domestically-produced knowledge and spillovers, as well as the more straightforward one of R&D expenditure and Human Capital.

In the following distributions of elasticities, apart from the cobb-douglas means differently-coloured according to their statistical significance, a thin, grey line is plotted as well, dividing the positive elasticities from the negative ones (i.e., it marks the zero). This is done to pay attention to a property of production functions, namely monotonicity, that simply states that for increase in input, there cannot be a decrease in output. This requirement, while natural enough in "physical" (i.e. goods-related) production functions to the extent of deducing misspecification from its violation (Henningsen, 2015), is more at odds with the object of the function hereby considered, as well as the aggregated framework adopted: does the possibility of a *net* crowding-out effect from foreign spillovers sound as implausible as a negative return of capital in output produced?

3.3.8.1 Distribution of Elasticities

There is an obvious difficulty in inferring from the multitude of histograms (36), a difficulty resulting from the impossibility of clear-cut measures to collect and group into aggregate parameters. A possible way out would be an estimator-centered perspective, where each of the now-four inputs is presented with its elasticity-histogram from that estimator, in thus a row of four histograms. But there are obvious downsides in this approach. Consider, for instance, an evaluation of the Pooled-OLS :

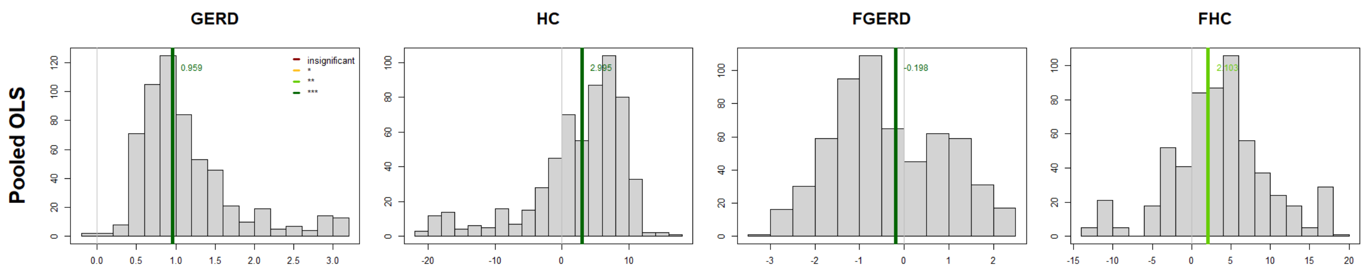


Figure 5: Pooled-OLS Translog Elasticities & Elasticity of Cobb-Douglas

We would say that, while the significant Cobb-Douglas coefficients portrayed fully-acceptable estimates (all positive, all statistically-significant), the huge dispersion in the elasticities of Human Capital or Foreign Human capital reveals the weakness of the estimator in front of unobserved heterogeneity. All good, except that if inferring means making "an educated guess", our guess is not very "educated", as the dispersion of elasticities in the foreign and domestic HC is something that characterises all estimators. Clear challenges to our task starts to emerge; forcing us to zoom-out from the estimator-specific elasticities in order to find patterns common, per input, among estimators.

Beneath is a table trying to circumscribe accordingly patterns of an input common to all estimators. The list of all statistics considered to draw such (arbitrary) inference is provided in table 8 at the end of the paper.

Table 13: Summary of Input Elasticity Properties Across Estimators

Input	Variance	Negative %	Skewness	Relative Stability
GERD	Low (except MG/CCEMG)	Low (mostly <6%)	Mild left-skew	Stable
FGERD	Moderate to Huge (MG/CCEMG)	High across all	Mild right-skew	Unstable
HC	Moderate to High	Very low	Consistently left-skewed	Moderately stable
FHC	Always high	Mixed (some high)	Mixed	Moderately unstable

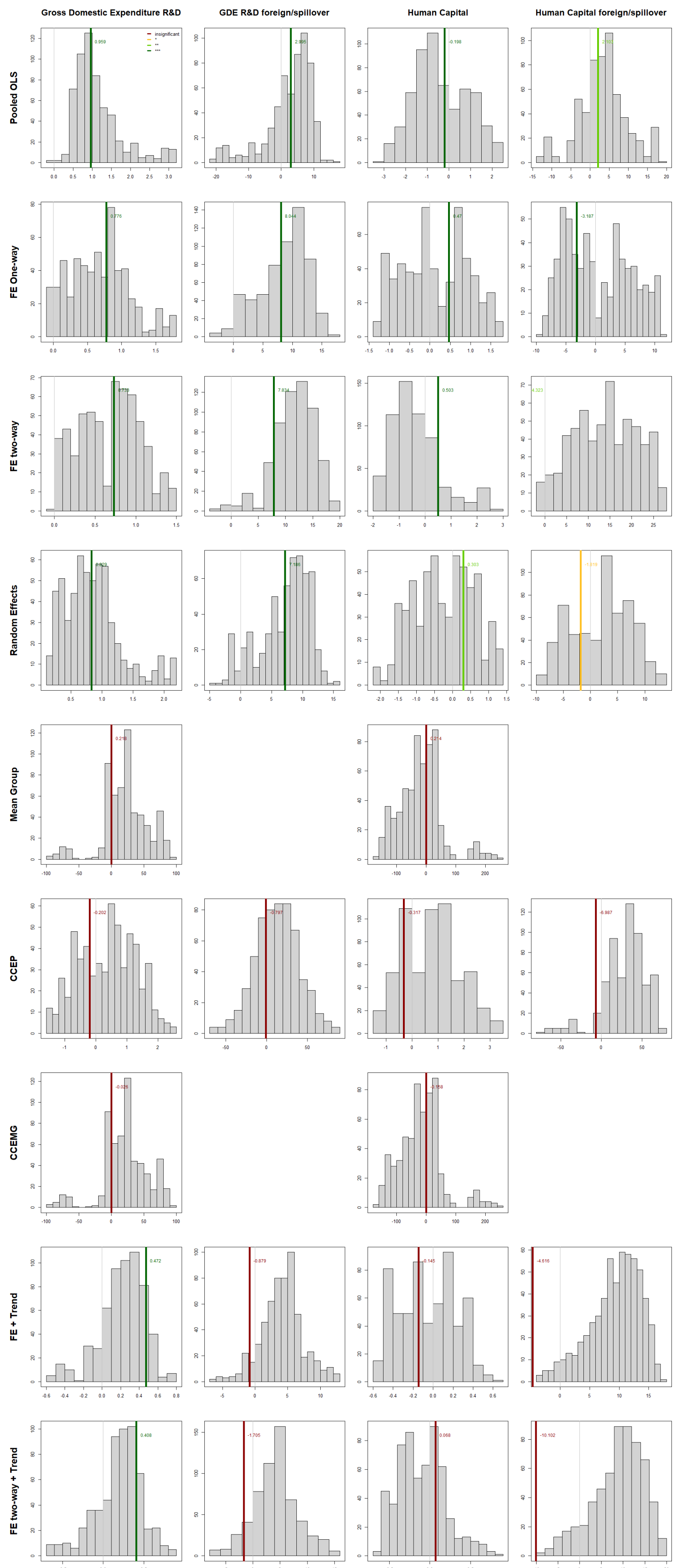
Across all estimators, GERD consistently shows positive average elasticities, with relatively low standard deviations in most models (generally below 1), suggesting stable contribution to Green patents. Its percentage of negative elasticity values is low across most estimators, often under 25%, though CCEP (36.5%) and the pooled OLS (34.0%) show some dispersion. Notably, the MG and CCEMG models report much higher means and standard deviations for GERD (over 21 for mean and above 34 for SD); but MG and CCEMG estimates are clearly off the mark for any input considered. Foreign-GERD/spillovers generally has negative or near-zero average elasticities across most models. It exhibits high variability, with standard deviations usually near or above 1 (and again, exceptionally high under MG/CCEMG with SDs of 72+). The percentage of negative elasticities is consistently high, typically ranging from 48% to over 70%, with the exception of CCEP where it is slightly lower (30.9%). This indicates a weaker and more uncertain contribution of foreign R&D funding to productivity outcomes, with a strong presence of negative effects (more of this in the conclusions).HC is characterized by highly positive and large elasticities, especially in fixed effects models like TWFE, FE, and RE, where the mean ranges from approximately 4 to 12. Its standard deviation is considerable, indicating variance in response, but still lower than that seen for FHC in some cases. Negative elasticities are rare here, with most estimators reporting single-digit percentages, except for pooled OLS (26%) and CCEP (31%), suggesting that HC has a mostly robust and positive impact on output. Foreign HC consistently exhibits strong positive elasticities, often larger than HC (especially in CCEP, where the mean exceeds 29). However, it also tends to have larger standard deviations, reflecting higher variability in how foreign human capital contributes to outcomes. The frequency of negative elasticities is more varied, ranging from around 2% (TWFE, FE + Trend) to over 50% (FE), and even 35% in RE, indicating more instability.

In relative terms, GERD has the lowest variability across models (except MG/CCEMG), whereas FGERD and FHC display much higher dispersion and inconsistency, both in standard deviation and in negative share. HC and FHC both typically show positive and large elasticities, but FHC is more volatile. With these "stylized facts" aside, let us see if specific inputs for specific estimators show contrasts between the cobb-douglas elasticity and the translog-distribution beneath (see the histograms "matrix" next page).

An intuitive and yet fascinating point is that given by the botton-half of the matrix: exactly where the cobb-douglas becomes insignificant (red lines), the distribution of elasticities estimated through the translog often (but not always) becomes centered very far away from the cobb-douglas mean. But indeed, not always. The CCEP foreign GERD, the (domestic) Human Capital of one-way and two-ways fixed effects (both with random trends), in all these histograms the statistically-insignificant cobb-douglas is more or less close to the center of the translog-distribution. What is happening? Quite easily, though it requires a look at the translog-regression results, the elasticities of such specifiation themselves are "statistically-insignificant" : that is, they are composed of terms (see the elasticity-computation section above) that proved statistically-insignificant themselves in the translog estimation.

Then there are those histograms where the cobb-douglas is clearly distanced from the center of a *clear* translog distribution of elasticities: for instance, Foreign HC of, again, random trends plus one-way and two-way FE. But also (quite peculiarly), the foreign human capital in the two-waays fixed-effects (with no random-trend) where a cobb-douglas elasticity significant at the 5% is plotted

Figure 6: Histograms of Translog and Cobb-Douglas elasticities



away from the translog-distribution; here the latter, differently from the first divergence of insignificant cobb-douglas and insignificant translog distribution, is computed from significant components as well. Hence, unically for this estimator/input pair, the co-existence of a statistically significant cobb-douglas mean and of a "statistically-significant" distribution, whose center is however very distant from the former mean.

Perhaps, however, the most interesting contrappositions are in the upper-half of the matrix, i.e. where the vast majority of the statistically-significant cobb-douglas are present. Here to its largest extent we can see how much lies beneath a statistically-significant elasticity from the cobb-douglas; we will, to make the point, refer to a most extreme case: Human capital and foreign human capital for the one-way FE estimator (without random trend). The Cobb-douglas means are extremely significant, but the distribution behind is far more heterogeneous than the coefficient and p-value would have betrayed. In the first (HC), the cobb-dogulas is at least halfway through "two" distributions, resulting in an average value that might not, in effect, distance itself too much from the translog; the startling element is the large frequency of very distant elasticities from such mean.

In the latter (foreign HC) histogram, the location of the cobb-dougla mean is, to say the least, counter-intuitive: a phenomenon made more damning by the sign-opposed halves characterizing these widely-distributed elasticities. One could have consumed paragraphs on a crowding-out effect of foreign HC, when infact around 40% of the elasticities of the translog suggests an inverse (e.g. positive) effect. Thus, it is not so much the translog-distribution beneath an already statistically-insignificant cobb-douglas that alarms, but, on the opposite, the latter' reassuring statistically-significant elasticities hiding those translog ones of opposite inference.

4 Conclusions and Reflections

This paper attempted a corroboration of the *input*-knowledge-production-function at the national level, confronting difficulties inherent to the nature of knowledge vis-à-vis physical output, as well as benefits and challenges of aggregation in the context of plausible spillovers. The most immediate conclusion of this paper is the corroboration of R&D as the engine of green innovation; a result that appears reliable in light of all the econometric methods applied to prevent spurious inference. As such, given the *a priori* uncertainty characterizing aggregated production functions, the "inter-estimator" positive elasticity of the most intuitive input in knowledge creation is a conforting result suggesting the plausibility, for KPFs, of units of analysis higher than the firm.

Although falling in the cautionary rather than prepositive side of scientific research, both the instability of the other inputs as we moved among the estimators, and the one of the translog elasticities vis-à-vis the Cobb-Douglas, are elements worth of consideration. From a more practical perspective, future researchers should consider these results as the clearest-possible warning on the importance of model-selection: that is, varying statistical significance of sign-varying coefficients should impress caution in limiting oneself to the fixed or random effects, inasmuch enormous variation in inference can be provided by different assumptions on the existence and behaviour of unobserved heterogeneity.

It is then being argued that we are in that part of scientific corroboration where the relation is known, and the challenge lies in showing it. Simply said, nobody doubts that invention is a function of expenditure on research. This contrasts with pioneering research establishing new relationships, where negative findings are daunting; whereas here they help filtering the long list of methodological choices left to try in the fulfilment of our task. Indeed, the limitations of this paper are centred on those issues that *restrain our explanation of the failure* to corroborate the contribution of human capital and foreign spillovers, as well as the binary environmental variable. As we move, conversely, from R&D to these less-established factors, we approach the pioneer's dilemma: do unstable estimates reflect economic reality or econometric limitations? While the (relatively smaller) instability of human capital might be an intuitive sign of decreasing returns[5], the same results in the foreign inputs (that is, in our proxy for spillovers) might be either a proof of crowding-out effects or of a, broadly defined, mispecification.

The first, "economic" rationale would say that the instability of the estimates correctly reflects the instability of the relation in the "true-regression" function: different (and opposing) tendencies between scarce innovators and capable ones "balancing out", with thus spillovers from the latter crowding out research in the former — but, crucially for the agglomerate function, without "a" vice versa. The other, "wrong specification" alternative to an explanation for the instable result is fundamentally a label for the limits of our paper. These have been extensively considered, but often concluded with a crude admission of ignorance on the amount of "disturbance" that these limits might have entailed. A hierarchical list moves, arguably, from the ontology to the epistemology or, in our context, the econometrics.

Has a macroeconomic, agglomerated function any meaning? If the answer is that these are nothing but "*A Pervasive, but Unpersuasive, Fairytale*" (Fisher 2005), then a knowledge production function might be neither a cold case, nor an open one: simply not a case. But then, how much of a production-function does our knowledge-one resemble? It did share, with its traditional equivalent, a priori presumptions of monotonicity: our regressors were inputs, and the inputs were expected to be positively related with the output. Even in the case of "negative" spillovers, these have been explained—if they were to be explained at all—by "substitution" for (and an insufficient one, thus resulting in a *net* crowding-out effect of) a domestic R&D expenditure or human capital that was nonetheless supposed to be positively correlated with the output. But, in the trans-log section, the monotonicity was rejected in that large amount of the elasticity distribution that showed negative

[5] Secondary-school literacy, in its being more basic and less specific than a PhD, helps research in green technologies, while the PhD contribution is restricted by the subjects unrelated to the green technologies.

values. Not many other similarities with the goods-producing function are self-evident: constant returns to scale, fixed shares of inputs, these did not concern us once in the paper[6]. This does not mean that scepticism about the existence of an aggregate, physical production function does not cast doubt on our knowledge-equivalent: only that the unknown extent to which they do is a limit of this paper.

After ontology, there are the econometrics limits. The environmental binary variable was too easily implemented in a research that stood firm on continuous ones: the econometrics consequence being pushed aside by the author who, quite frankly, ignores the related econometrics that could have helped inferring over the constant statistical insignificance of this dummy. Multicollinearity is pervasive in aggregation, inasmuch as smaller units of analysis show more variation (Griliches 1975); and our variables, like many other ones at the macroeconomic level (GDP, trade, or, to a lesser extent, productivity), are always increasing since Second World War. But the random trend did not produce more theory-appropriate estimates than the two-way fixed effects (nor did the latter *and* the random trend). Cross-sectional dependence, a priori guessed as pervasive, was removed in the CCE application, but with it went statistical significance : again, a conclusion to be expected as the previously-considered "strategy of moderation" (see the concluding quote from Griliches 1975 in section 3.4) in what we should ask from our aggregated, and thus, multicollinearity-prone data is at odds with the large number of parameters behind any CCE estimate. But a daunting aspect is that a one-way or a two-way FE, these former two plus a random trend, and finally the CCEP are closely related estimators with increasing restriction on the allowed behaviour of unobserved heterogeneity. Such increasing constraints did not produce a recognizable pattern in the changing *coefficients* among the estimators. Finally, but this is arguably a weakness limited in its acclaimed presence simply by the unwillingness of researchers to establish it (i.e., a text-book definition of "the elephant in the room"), the translog shows cracks even behind the most respectable Cobb-Douglas estimate of elasticity.

Again, it is up to the reader to decide whether casting doubts is as productive an academic activity as solving them. To the author, in front of the paucity in insights concerning aggregated knowledge production functions, it did appear as such.

[6]Investigating the possibility of returns to scale, as done in the review of the literature, is quite different from assuming it in order to have a meaningful estimation, as in the case of the “traditional” aggregated production function.

5 Tables & Aggregated Results

Table 14: Cobb-Douglas Regression Results (Transposed Layout)

Estimator	Dom. GERD	Dom. HC	For. GERD	For. HC
Pooled OLS	0.959***	2.995***	-0.198***	2.103**
	(0.000)	(0.000)	(0.000)	(0.004)
FE One-way	0.776***	8.044***	0.470***	-3.187***
	(0.000)	(0.000)	(0.000)	(0.001)
Random Effects	0.829***	7.186***	0.303**	-1.819*
	(0.000)	(0.000)	(0.003)	(0.045)
Mean Group	0.219	17.994*	0.214	-6.830
	(0.310)	(0.020)	(0.605)	(0.516)
CCEP	-0.202	-0.797	-0.317	-6.987
	(0.656)	(0.873)	(0.490)	(0.911)
CCEMG	-0.026	7.475	-0.158	117.683*
	(0.916)	(0.736)	(0.921)	(0.047)
FE Two-way	0.733***	7.834***	0.503***	-4.323**
	(0.000)	(0.000)	(0.000)	(0.009)
FE + Trend	0.472***	-0.879	-0.145	-4.616
	(0.000)	(0.580)	(0.383)	(0.074)
FE Two-way + Trend	0.408***	-1.705	0.068	-10.102
	(0.001)	(0.330)	(0.756)	(0.074)

Notes: ***, **, * denote significance at 1%, 5%, and 10% levels. "Dom." = Domestic, "For." = Foreign.

Table 15: Elasticity Statistics by Estimator and Input (Translog Models)

Estimator	Input	Mean	Median	SD	Min	Max	Skewness	Neg. Count	Neg. %
FE	GERD	0.673	0.673	0.435	-0.077	1.774	0.366	30	5.093
	FGERD	0.132	0.026	0.788	-1.280	1.647	0.039	286	48.557
	HC	8.426	9.424	4.101	-3.819	17.481	-0.661	13	2.207
	FHC	0.430	-0.867	5.506	-9.242	11.052	0.234	313	53.141
TWFE	GERD	0.675	0.756	0.373	-0.009	1.455	-0.026	1	0.170
	FGERD	-0.374	-0.540	0.951	-1.894	2.518	1.155	420	71.307
	HC	11.703	12.292	3.878	-2.826	19.331	-0.816	8	1.358
	FHC	13.777	14.119	7.281	-1.675	26.818	-0.108	16	2.716
FE + Trend	GERD	0.218	0.250	0.243	-0.537	0.782	-0.814	89	15.110
	FGERD	-0.054	-0.063	0.283	-0.543	0.607	0.031	322	54.669
	HC	4.140	4.324	3.244	-6.365	12.594	-0.165	56	9.508
	FHC	9.312	10.168	4.299	-3.925	17.007	-0.707	22	3.735
TWFE + Trend	GERD	0.186	0.214	0.285	-0.686	0.885	-0.654	128	21.732
	FGERD	-0.098	-0.110	0.286	-0.639	0.818	0.512	364	61.800
	HC	4.190	4.245	4.087	-7.320	14.618	0.002	82	13.922
	FHC	8.849	9.693	5.815	-9.667	19.601	-0.663	57	9.677
RE	GERD	0.841	0.796	0.455	0.102	2.190	0.935	0	0.000
	FGERD	-0.243	-0.271	0.820	-2.130	1.389	-0.061	333	56.537
	HC	7.153	8.094	3.998	-4.071	15.939	-0.710	42	7.131
	FHC	2.099	3.054	5.383	-8.565	12.949	-0.186	209	35.484
MG	GERD	21.823	21.584	34.729	-94.613	99.768	-0.652	136	23.090
	FGERD	-19.355	-17.392	72.285	-179.312	240.183	0.742	357	60.611
	HC	–	–	–	–	–	–	0	–
	FHC	–	–	–	–	–	–	0	–
CCEP	GERD	0.369	0.467	0.920	-1.514	2.516	-0.041	215	36.503
	FGERD	0.746	0.812	1.068	-1.466	3.186	0.105	182	30.900
	HC	13.121	13.196	27.131	-66.992	83.697	-0.070	186	31.579
	FHC	29.629	36.404	25.774	-71.386	71.214	-1.208	51	8.659
CCEMG	GERD	21.823	21.584	34.729	-94.613	99.768	-0.652	136	23.090
	FGERD	-19.355	-17.392	72.285	-179.312	240.183	0.742	357	60.611
	HC	–	–	–	–	–	–	0	–
	FHC	–	–	–	–	–	–	0	–

Table 16: Translog Regression Results (Transposed for readability)

Variable	Pooled	FE	RE	MG	CCEP	CCEMG	FE(2)	FE Tr	FE(2) Tr
Domestic GERD	-6.51***	-4.14***	-4.77***	104.98	10.87	104.98	-2.77**	1.10	1.87
	(0.0000)	(0.0000)	(0.0000)	(0.4209)	(0.6594)	(0.4209)	(0.0013)	(0.4297)	(0.1854)
Domestic HC	95.57***	43.75***	44.38***	-1142.99	-338.97	-1142.99	25.49**	-17.69	-23.42
	(0.0000)	(0.0000)	(0.0000)	(0.7032)	(0.4350)	(0.7032)	(0.0049)	(0.2096)	(0.1432)
Foreign GERD	-7.86***	-7.51***	-6.36***	-257.24	12.48	-257.24	-10.75***	-2.08	0.27
	(0.0000)	(0.0000)	(0.0000)	(0.2231)	(0.7651)	(0.2231)	(0.0000)	(0.4328)	(0.9203)
Foreign HC	-12.72	31.02	15.63	1985.42	-57.23	1985.42	56.57**	-20.85	-34.68
	(0.4182)	(0.0576)	(0.2774)	(0.5937)	(0.9678)	(0.5937)	(0.0016)	(0.3879)	(0.1989)
Env. Policy Dummy	0.08	-0.03	-0.02	NA	0.02	NA	0.01	-0.00	0.02
	(0.1882)	(0.5000)	(0.7278)		(0.8637)		(0.7789)	(0.9800)	(0.7062)
(Dom GERD)²	0.08***	0.15***	0.13***	-1.47	-0.35	-1.47	0.14***	-0.07	-0.10
	(0.0002)	(0.0000)	(0.0000)	(0.5544)	(0.6992)	(0.5544)	(0.0000)	(0.1680)	(0.0862)
(Dom HC)²	15.35***	15.98***	14.78***	2425.26	28.57	2425.26	17.52***	6.06	6.14
	(0.0000)	(0.0000)	(0.0000)	(0.6081)	(0.8763)	(0.6081)	(0.0000)	(0.3642)	(0.3704)
(For GERD)²	-0.05	0.43*	0.15	3.17	-0.40	3.17	0.82***	0.16	0.16
	(0.6142)	(0.0128)	(0.2494)	(0.7148)	(0.8222)	(0.7148)	(0.0000)	(0.3712)	(0.4064)
(For HC)²	-41.23**	32.92*	16.75	-2071.97	-111.00	-2071.97	69.73***	29.20	51.87
	(0.0057)	(0.0327)	(0.1985)	(0.6942)	(0.8005)	(0.6942)	(0.0000)	(0.3850)	(0.1431)
Dom GERD × For GERD	0.93***	0.60***	0.69***	9.90	-0.55	9.90	0.46***	0.22	0.18
	(0.0000)	(0.0000)	(0.0000)	(0.3085)	(0.8725)	(0.3085)	(0.0000)	(0.0574)	(0.1783)
Dom GERD × For HC	-2.59***	-4.15***	-4.04***	-495.27*	-0.97	-495.27*	-4.21***	-2.50*	-3.09**
	(0.0004)	(0.0000)	(0.0000)	(0.0240)	(0.9812)	(0.0240)	(0.0000)	(0.0290)	(0.0070)
Dom GERD × Dom HC	-2.47***	-1.19*	-1.22**	348.58	4.95	348.58	-0.63	0.86	1.66
	(0.0000)	(0.0141)	(0.0066)	(0.0661)	(0.7880)	(0.0661)	(0.1825)	(0.3352)	(0.0629)
For GERD × For HC	8.79***	-4.40	-0.45	751.56*	6.83	751.56*	-10.99***	-2.14	-4.10
	(0.0004)	(0.1387)	(0.8557)	(0.0203)	(0.8998)	(0.0203)	(0.0003)	(0.5843)	(0.3216)
For GERD × Dom HC	-10.67***	-4.42***	-4.64***	-720.34*	-2.90	-720.34*	-2.34	-1.74	-1.47
	(0.0000)	(0.0003)	(0.0000)	(0.0102)	(0.9259)	(0.0102)	(0.0533)	(0.3155)	(0.4032)
Dom HC × For HC	28.19***	-2.70	0.75	NA	238.51	NA	-14.93	17.65	11.57
	(0.0005)	(0.8234)	(0.9390)		(0.6214)		(0.2308)	(0.4771)	(0.6458)

"Dom" stands for "Domestic", "For" from "Foreign". In order: Pooled = Pooled-OLS; FE = One-way Fixed effects; RE = Random Effects; MG = Mean Group; CCEP = Common Correlated Effects Pooled estimator; CCEMG = Common Correlated Effects Mean group; FE(2) = Two-way Fixed effects; FE Tr = One-way Fixed effects plus random trend; FE(2) Tr = Two-way Fixed effects plus random trend. As usual, ***, ** and * denote significance

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