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Do Tax Incentives for Research Increase Firm Innovation? An RD Design for R&D, Patents and Spillovers

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Abstract
We present evidence of the positive causal impacts of research and development (R&D) tax incentives on a firm's own innovation and that of its technological neighbors (spillovers). Exploiting a change in the assets-based size thresholds that determine eligibility for R&D tax relief, we implement a Regression Discontinuity (RD) Design using administrative data. We find statistically and economically significant effects of tax relief on (quality-adjusted) patenting (and R&D) that persist up to seven years after the change. Moreover, we also find causal evidence of R&D spillovers on the innovation of technologically close peer firms. We can rule out elasticities of patenting with respect to the user cost of R&D of under 2 at the 5% level and show evidence that our large effects are likely because the treated group are more likely to be financially constrained.

Keywords: R&D, patents, tax, innovation, spillovers,Regression Discontinuity Design
JEL codes: O31; O32; H23; H25; H32

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1. Introduction

Innovation is recognized as the major source of growth in advanced economies (Romer, 1990, Aghion and Howitt, 1992). However, because of knowledge externalities, private returns on research and development (R&D) are generally thought to be much lower than their social returns, suggesting the need for some government subsidy.[1] Indeed, the majority of OECD countries have tax incentives for R&D and over the last two decades, these incentives have grown increasingly popular, even compared to direct R&D subsidies to firms.[2]

But do R&D tax incentives really increase innovation for eligible firms? And if so, does this innovation spill over to benefit other firms as the supporters of such R&D subsidies claim? In this paper, we identify the causal effects of R&D tax incentives by exploiting a policy reform that raised the size threshold under which UK-based firms could access the more generous tax regime for small- and medium-sized enterprises (SMEs). Importantly, the new SME size threshold introduced was unique to the UK R&D Tax Relief Scheme and did not overlap with access to other programs or taxes. This allows us to implement a Regression Discontinuity (RD) Design to assess the differences in innovation activity around the new SME threshold. We assemble a new database linking the universe of UK companies with their confidential tax returns (including R&D expenditures) from HMRC (the UK IRS), their patent filings in all major patent offices in the world, and their financial accounts. Our data are available for the periods before and after the R&D tax change, allowing us to analyze the causal impact of the tax credit up to seven years after (and six years before) the policy change. The long time horizon also allows us to implement alternative empirical models such as Difference-In-Difference and Differences-In-Discontinuity designs, which corroborate our main RD Design.

A key advantage of our firm-level patent dataset is that it enables us to assess the effect of tax incentives not only on R&D spending (an input) but also on innovation outputs.[3] A long-standing concern is that tax incentives could increase observed R&D without having much effect on innovation because firms relabel existing activities to take advantage of the tax relief (e.g., Chen et al., 2021) or only expand very low-quality R&D projects. We can directly examine the quality of innovations

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[1] Typical results find marginal social returns to R&D between 30% and 50% compared to private returns between from 7% to 15% (Hall, Mairesse, and Mohnen, 2010).

[2] In 2018, 80% of OECD countries had some type of additional R&D tax relief, whereas only 40% did in 2000 (OECD 2019). One reason for this shift is that subsidizing R&D through the tax system rather than direct grants reduces administrative burden and mitigates the risk of "picking losers" (e.g., choosing firms with low private and social returns due to political connections, as in Lach, Neeman, and Schankerman, 2017)

[3] There is a large literature on the effects of public R&D grants on firm and industry outcomes such as González, Jaumandreu, and Pazó (2005), Takalo, Tanayama, and Toivanen (2013), Einiö (2014), Goodridge et al. (2015), Jaffe and Le (2015), and Moretti, Steinwender, and Van Reenen (2019). The earlier literature is surveyed in David, Hall, and Toole (2000).

through various measures of patent value, such as future citations received and the number of countries in which a patent seeks protection.

The main economic rationale given for more generous tax treatment of R&D is that there are technological externalities, so that the social returns to R&D exceed the private returns. Almost uniquely, our design allows us to estimate the causal impact of tax policies on R&D spillovers, i.e., their effect on innovation activities of firms who were *technologically connected* to policy-affected firms. We find evidence that the R&D induced by the tax policy generated positive spillovers on innovations by technologically related firms, especially in smaller technology classes. Focusing on these smaller peer groups is exactly where we expect our design to have power to detect spillovers (see Angrist, 2014 and Dahl, Løcken, and Mogstad, 2014).

We find statistically and economically significant effects of the tax relief on eligible firms, increasing patenting by around 76% (0.05 over a pre-policy mean of 0.066). We can rule out elasticities of patenting with respect to R&D user costs of less than 2 at the 5% significance level.[4] Our relatively high elasticities are likely because the sub-population targeted in our design is composed of smaller firms than those typically studied in the literature. These firms are more likely to be financially constrained and therefore more responsive to R&D tax incentives. We confirm this intuition by showing the response was particularly strong for firms more likely to suffer from financial frictions.[5] Simple calculations suggest that between 2006 and 2011, the R&D Tax Relief Scheme induced about $2 of private R&D for every $1 of taxpayer money, and that aggregate UK business R&D (BERD) would have been about 14% lower in the absence of the policy.[6]

The paper is organized as follows. First, we offer a brief literature review; then Section 2 details the institutional setting and data. Section 3 explains the empirical design and results for the direct policy effect on innovation; and Section 4 does the same for the spillover effect on technologically connected firms. Section 5 then discusses the magnitudes and economic implications of the policy effects. Finally, Section 6 offers some concluding remarks. Online Appendices provide more institutional details (A), a deeper data description (B), robustness checks and extensions (C), and more econometric details (D-F).

[4] Similarly, we can rule out elasticities of R&D with respect to user costs of less than 1.4 at the 5% significance level. See surveys by Becker (2015), OECD (2013), or Hall and Van Reenen (2000) on R&D to user cost elasticities.
[5] Financial constraints are more likely to affect R&D than other forms of investment (Arrow, 1962). This is because (i) information asymmetries are greater, (ii) R&D is mainly researchers who cannot be pledged as collateral, and (iii) external lenders may appropriate ideas for themselves.
[6] See Akcigit, Hanley, and Stantcheva (2017) and Acemoglu et al. (2018) for rigorous discussion of optimal taxation and R&D policy in general equilibrium. Akcigit and Stantcheva (2020) survey the more general evidence on individual and corporate income tax on innovation.

Related literature. Most directly, our paper contributes to the small literature examining the causal effects of subsidies on innovation outputs using RD designs. One branch uses academic publications as a measure of innovation, using ratings given to grant applications as a way of generating exogenous variation around funding thresholds. Jacob and Lefgren (2010) and Azoulay et al. (2014) examine NIH grants and Ganguli (2017) looks at grants for Russian scientists. Howell (2017) and Howell et al. (2021) use the ranking of US SBIR proposals and finds significant effects of R&D grants on future venture capital funding, survival rate and patents, with stronger effects for firms likely to be more financially constrained. Bronzini and Iachini (2014), and Bronzini and Piselli (2016) study an R&D subsidy program in Northern Italy and find positive impacts on investment and patenting, particularly for smaller firms.[7] However, none of these papers examine tax incentives as we do.

A second literature does evaluate the effects of tax policies on R&D expenditures. Early evaluations conducted at the state or macro-economic level face the problem that policy changes often coincide with many unobserved factors that may influence R&D. Recent studies use firm-level data and more credible causal designs but tend to focus solely on the impact on R&D expenditures.[8] Like us, Rao (2016) uses administrative tax data and looks at the impact of US tax credits on R&D expenditure. She uses the changes in the Federal tax rules interacted with lagged firm characteristics to generate instrumental variables for the firm-specific user cost of R&D. Guceri (2018) and Guceri and Liu (2019) use a difference-in-differences strategy to examine the impact on R&D expenditure of a change in the UK R&D tax regime.[9] Bøler, Moxnes, and Ulltveit-Moe (2015) employ a similar strategy to investigate how the introduction of R&D tax credit in Norway affected profits, intermediate imports, and R&D. These papers find effects of tax incentives on R&D, but do not look at direct innovative outcomes as we do.[10] Chen et al. (2021) is perhaps the closest paper to ours. The authors examine the impact of tax changes in corporate tax regulations on R&D and other outcomes in a sample of Chinese firms using an RD Design. They find positive impacts, although about 30% of the

[7] Interestingly, most of the papers which have looked at this issue find larger program effects for smaller firms. In addition to Howell (2007), this is also found in Mahon and Zwick (2017) and Wallsten (2000) for the US, González et al. (2005) for Spain, Lach (2002) for Israel, Bronzini and Iachini (2014) for Italy, and Gorg and Strobl (2007) for Ireland. We will also see this heterogeneity in our later analysis.

[8] On more aggregate data, examples include Bloom, Griffith, and Van Reenen (2002), Wilson (2009), and Chang (2018). On the firm-level side, examples include Mulkay and Mairesse (2013) on France, Lokshin and Mohnen (2012) on the Netherlands, McKenzie and Sershun (2010) and Agrawal, Rosell, and Simcoe (2014) on Canada, and Parisi and Sembenelli (2003) on Italy.

[9] Although complementary to our paper, they look only at UK R&D and not at innovation outcomes or spillovers. Unlike them, we use an RD Design and do not condition on post-policy R&D performing firms.

[10] See also Czarnitki, Hanel, and Rosa (2011), Cappelen, Raknerud, and Rybalka (2012), and Bérubé and Mohnen (2009) who look at the effects of R&D tax credits on patents and/or new products. Mamuneas and Nadiri (1996) look at tax credits, R&D, and patents. These papers, however, have less of a clear causal design.

additional R&D was relabeling.

Third, our paper also contributes to the literature on the effects of R&D on innovation (see the Hall, Mairesse, and Mohnen, 2010 survey or Doraszelski and Jaumandreu, 2013, for example). We find that policy-induced R&D has positive causal effects on innovation, with elasticities that are underestimated in conventional OLS approaches. Although there is also a large literature on R&D spillovers (e.g., Bloom, Schankerman, and Van Reenen, 2013, Griliches, 1992, Jaffe, Trajtenberg and Henderson, 1993), we are, to our knowledge, the first to provide evidence for the existence of technology spillovers in an RD Design.

Finally, we connect to an emerging field, which looks at the role of both individual and corporate tax on individual inventors (rather than the firms that they work for). This literature also appears to be finding an important role for taxation on mobility, quantity, and quality of innovation. In particular, Akcigit et al. (2021) find major positive effects of individual and corporate income tax cuts on innovation using panel data on US states between 1940 and 2000.[11]

2. Institutional setting and data

2.1 The UK R&D Tax Relief Scheme

Full institutional details are in Online Appendix A, but we sketch the relevant details here. From the early 1980s the UK business R&D to GDP ratio fell, whereas it rose in most other OECD countries. In 2000, an R&D Tax Relief Scheme was introduced for small and medium enterprises (SMEs) and was extended to cover large companies in 2002 (but SMEs continued to enjoy more generous R&D tax relief). The policy cost the UK government £1.4 billion in 2013 alone (Fowkes, Sousa, and Duncan, 2015).

The tax relief is based on the total amount of R&D, i.e., it is volume-based rather than calculated as an increment over past spending like the US R&D tax credit. It works mostly through enhanced deduction of R&D from taxable income, thus reducing corporate tax liabilities.[12] At the time of its introduction, the scheme allowed SMEs to deduct an additional *enhancement rate* of 50% of qualifying R&D expenditure from taxable profits (on top of the 100% deduction that applies to any form of current expenditure). If an SME was not making profits, it could surrender enhanced losses in return for a payable tax credit. This feature is particularly beneficial to firms that are liquidity constrained

[11] A difference with our work is that some of their effects could come from geographical relocation within the country rather than an overall rise in aggregate innovation (although they do use a state boundary design to argue that not all the effects are from relocation). By contrast, our policy is nation-wide. For other work considering individual data on inventors and tax see Akcigit, Baslandze and Stantcheva (2016) and Moretti and Wilson (2017).

[12] Only current R&D expenditures, such as labor and materials, qualify for the scheme. However, since capital only accounts for about 10% of total R&D, this is less important.

and thus may not be making enough profits to benefit from enhanced tax deduction. We will present evidence in line with the idea that the large effects we observe were linked to the alleviation of such financial constraints. Large companies had a less generous enhancement rate of 25% of their R&D and could not claim the refundable tax credits in case of losses.

The policy used the definition of an SME recommended by the European Commission (EC) throughout most of the 2000s. This was based on total assets, sales, and employment. It also took into consideration company ownership structure and required that in order to change its SME status, a company must fall in the new category for two consecutive years. (See Appendix A.2 for further details on the SME definition.)

We focus on the major change to the scheme that commenced from August 2008 (Table A1). The SME assets threshold was increased from €43m to €86m, the sales threshold from €50m to €100m, and employment threshold from 249 to 499.[13] Because of these changes, a substantial number of firms that were only eligible for the large company rate according to the old definition then became eligible for the SME rate. In addition to the change in SME definition, the UK government also increased the enhancement rate for both SMEs and large companies in the same year. This increase was from 50% to 75% for SMEs and from 25% to 30% for large companies. This change induced a reduction in the tax-adjusted user cost of R&D for the newly eligible SMEs, from 0.19 down to 0.15, whereas the R&D user cost of firms that remained large companies was basically unchanged (Table A2).

We examine the impact of this sharp jump from 2008 onwards in tax-adjusted user cost of R&D at the new SME thresholds. There are several advantages of employing this reform instead of the earlier changes. First, unlike the previous thresholds based on the EC's definition, which were extensively used in many other support programs targeting SMEs, the thresholds introduced in 2008 were specific to the R&D Tax Relief Scheme. This allows us to recover the effects of the R&D Tax Relief Scheme without confounding them with the impact of other policies. Second, identifying the policy impacts around newly introduced thresholds mitigates concerns that tax planning may lead to endogenous bunching of firms around the thresholds. Indeed, we show that pre-2008, there was no bunching around these thresholds and predetermined covariates were all balanced at the cutoffs. This is important, as although the new policy's effective date of August 1st, 2008 was only announced less than a month earlier on July 16th, 2008, aspects of the policy were laid out in the Finance Act 2007, and so firms could have responded in advance. Information frictions, adjustment costs, and policy

[13] The other criteria laid down in the EC's 2003 recommendation (e.g., two-year rule) were maintained in the new provision in the Finance Act 2007. This act, however, did not appoint a date on which new ceilings became effective.

uncertainty mean that this adjustment was likely to be sluggish, especially for the SMEs we study.[14] We thus focus on the 2007 values of firm financial variables, as they matter for the firm's SME status in 2009 by the two-year rule, but were unlikely affected by tax-planning incentives.

Among the three determinants of SME status, we focus on total assets to avoid the issue of selective missing values among sales and employment. We will discuss this data issue in detail in subsection 4.1 and show that results remain qualitatively similar when sales and employment are taken into consideration in alternative specifications in Appendix C.6.

Finally, note that the policy does not require a firm to show any patenting activity, in either filing for R&D tax relief by the firm or auditing by the tax authority of how the R&D is spent. This is important as patents (counts and quality-weighted) will be our key measure of innovative output. Although there is no administrative pressure to increase innovation output, we will show that the policy has been successful in generating more innovation.

2.2 Data sources for R&D and patents

Online Appendix B details our three main data sources: (i) PATSTAT dataset, (ii) HMRC Corporate Tax returns (CT600) and its extension, the Research and Development Tax Credits (RDTC) dataset, and (iii) Bureau Van Dijk's FAME dataset. We give an overview here.

Patent data from PATSTAT. PATSTAT is the largest available international patent database, which covers close to the population of all worldwide patents since the 1900s. It brings together nearly 70 million patent documents from over 60 patent offices, including all of the major offices such as the European Patent Office (EPO), the United States Patent and Trademark office (USPTO), the Japan Patent Office (JPO), and also the UK Intellectual Property Office. To assign patents to UK-based companies we use the matching algorithm between PATSTAT and FAME implemented by Bureau Van Dijk and available from the ORBIS database. Over our sample period, 94% of patents filed in the UK and 96% of patents filed at the EPO have been successfully associated with their owning company. We consider all patents filed by UK companies up to 2015. Our dataset contains comprehensive information from the patent record, including application date, citations, and technology class.

Importantly, PATSTAT includes information on patent families, each of which is a set of patents protecting the same invention across several jurisdictions. This information allows us to identify all patent applications filed worldwide by UK companies, while avoiding double-counting inventions

[14] Sluggish adjustment to policy announcements is consistent with many papers in the public finance literature (e.g., Kleven and Waseem, 2013).

sought to be protected in multiple jurisdictions. We thus use the number of patent families, irrespective of where the patents are filed, as our baseline measure of innovation. Each patent family is assigned to its earliest application year, which tracks R&D much more closely than publication or granted dates.

Although patents have their limitations (see Hall et al., 2013), numerous studies have demonstrated a strong link between patenting and firm performance.[15] To tackle the problem of highly heterogeneous patent values, we use various measures of patent quality, including weighing patents by the number of countries where IP protection is sought (e.g., US and Japan) or the number of future citations.[16]

R&D data from CT600 and RDTC. CT600 is an administrative panel dataset provided by the HMRC Datalab, which consists of tax assessments made from the returns for all UK companies liable for corporation tax. The dataset covers financial years 2000 to 2011 and contains all information provided by firms in their annual corporate tax returns. We are specifically interested in the RDTC sub-dataset, which contains all information related to the R&D Tax Relief Scheme, including the amount of qualifying R&D expenditure for each firm-year and the scheme under which it made the claim (SME vs. Large Company Scheme). Firms made 53,000 claims over 2000-11 for a total of £5.8 billion in R&D tax relief with about 80% of the claims were under the SME Scheme.

We observe R&D when firms claim R&D tax relief. All firms performing R&D are in principle eligible for tax relief, which as we have discussed are generous. Further, all firms must submit tax returns each year and claiming tax relief is a simple part of this process. Hence, we believe we have reasonably comprehensive coverage of a firm's qualifying R&D spending.[17] Ideally, we would cross check at the firm level with R&D data from other sources, but UK accounting regulations (like the US regulation of privately listed firms) do not insist on small companies' reporting R&D. Statistics provided by HMRC indicate that qualifying R&D expenditure amounts to 70% of total business R&D (BERD).[18] In addition, the data contain information on the SME status of firms that claimed R&D tax relief. However, this information is not available for non-R&D-performing firms.

Financial data from FAME. Employment and total assets are not included in CT600 because

[15] E.g., see Hall, Jaffe, and Trajtenberg (2005) on US firms, or Blundell, Griffith, and Van Reenen (1999) on UK firms.
[16] Variations of these quality measures have been used by Lanjouw et al. (1998), Harhoff et al. (2003), and Hall et al. (2005), among others.
[17] That is, given the ease of the process, selection into claiming R&D tax relief (conditional on having performed R&D) is unlikely a first order concern.
[18] There are various reasons for this difference, e.g., BERD includes R&D spending on capital investment whereas qualifying R&D does not (only current expenses are eligible for tax relief). It is also the case that HMRC defines R&D more narrowly for tax purposes than BERD, which is based on the Frascati definition.

they are not required on corporate tax forms. Furthermore, only tax-accounting sales is reported in CT600, while the SME definition is based on financial-accounting sales as reported in company accounts.[19] Consequently, we turn to a second dataset, FAME, which contains all UK company accounts since about the mid-1980's. In addition to total assets, sales, and employment,[20] FAME also provides firms' industry, location, capital investment, other expenditures, profits, remuneration, and other financial information through to 2013, although coverage quality differs greatly across variables (depending on reporting requirement). As both CT600 and FAME cover the universe of UK firms, we obtain an excellent match rate of 95% between the two datasets (see Appendix B.4 for details).

While all firms are required to report their total assets in company accounts, reporting of sales and employment is mandatory only for larger firms. In our FAME data, over 2006-11, only 15% of firms reported sales and only 5% reported employment (see Panel B of Table B1). By comparison, 97% reported assets. Even in our baseline sample of relatively larger firms around the SME assets threshold of €86m (details in subsection 3.2), sales and employment are still only available for 67% and 55% of firms respectively. Thus, to avoid the problem of selection due to missing values, we focus on using the SME assets threshold for the bulk of the analyses in this paper.

3. Direct policy effect on innovation

3.1 Empirical strategies

We employ three complementary empirical designs to assess the direct effect of firms' assets-based eligibility for the SME Scheme on innovation output, as measured by patents. These include: (i) a baseline Regression Discontinuity Design ("RDD"), (ii) a Difference-in-Differences Design ("Diff-in-Diff"), and (iii) a Difference-in-Discontinuities Design ("Diff-in-Disc").

Regression Discontinuity Design. We first consider a baseline RD equation of the form:

$$PAT_i^{Post} = \alpha + \beta^{RDD} E_i^{2007} + f(z_i^{2007}) + \mu PAT_i^{Pre} + \varepsilon_i, \tag{1}$$

where PAT_i^{Post} is firm i's post-policy average annual patent application count, E_i^{2007} is a binary indicator of whether the firm's total assets in 2007 is at or below €86m, $f(z_i^{2007})$ are polynomials of the running variable (2007 assets) separately for each side of the threshold, PAT_i^{Pre} is the firm's pre-policy average annual patent application count, and ε_i is an error term. In an RD Design, the

[19] Tax-accounting sales turnover is calculated using the cash-based method, which focuses on actual cash receipts rather than their related sale transactions. Financial-accounting turnover is calculated using the accrual method, which records sale revenues when they are earned, regardless of whether cash from sales has been collected.

[20] Financial variables are reported in sterling while the SME thresholds are set in euros, so we convert assets and sales using the same conversion rules used by HMRC for this purpose (see Appendix B.5 for details).

identifying assumption requires that the distributions of all predetermined variables be smooth around the cutoff, which is testable on observables (details in subsection 3.2). This identification condition is guaranteed when firms cannot precisely manipulate the running variable around the threshold (Lee, 2008, Lee and Lemieux, 2010).[21] Under this assumption, E_i^{2007} is as good as randomly assigned at the threshold.

As described in Section 2, total assets is one of the criteria used to determine firm i's SME status and the only one for which there is close to complete data coverage.[22] Because of the two-year rule, a firm's SME status in 2009 was partly based on its financial information in 2007. Furthermore, using assets in 2007 as our primary running variable mitigates the concern that there might have been endogenous sorting of firms across the SME threshold. Indeed, Figure 1 shows that firms' 2007 assets distribution was continuous around the 2008 new SME threshold of €86m. The corresponding McCrary test yields a discontinuity estimate (log difference in density height at the SME threshold) of -0.026 with a standard error of 0.088, which is not statistically different from zero. Similar McCrary tests indicate that firms' 2007 sales and employment distributions were also smooth at the respective thresholds. On the other hand, there appears to be some small, but also insignificant, evidence of bunching below the SME thresholds in later years (see Appendix C.5 and Figure A1).

As 2007 assets do not determine post-policy SME status perfectly, equation (1) represents the reduced-form of a fuzzy RD Design in which E_i^{2007} is the instrument for firm i's actual eligibility for the more generous SME Scheme (SME_i^{Post}). We cannot directly implement this fuzzy RD Design, as SME_i^{Post} is not observed for the vast majority of firms that do not perform any R&D. Instead, the coefficient β^{RDD} captures the reduced-form effect of being below the assets threshold, and therefore more likely eligible for the SME Scheme, on a firm's patents at this threshold. It presents a lower bound for the treatment effect of moving a company from the Large Company Scheme to the SME Scheme. In subsection 5.2, we describe our strategy to derive this treatment effect from β^{RDD} and available information on the SME status of R&D performing firms.

We estimate equation (1) for various post-policy windows up to 2015 when our data ends. The

[21] Lee and Lemieux (2010)'s "local randomization result", i.e., $\lim_{z_i \to 86-} \mathbb{E}[U_i|E_i = 1] = \lim_{z_i \to 86+} \mathbb{E}[U_i|E_i = 0]$ for any observable or unobservable characteristic U_i of firm i, holds under the sufficient condition that there are some (possibly very small) perturbations so that firms do not have full control of their running variable (assets size). That is, even when firms could manipulate their assets, the RD Design identification condition remains valid as long as the manipulation could not be precise.

[22] Using only one threshold for identification in a multiple threshold policy design does not violate the RD Design identifying assumption, although it may reduce the efficiency of the estimates. We also experiment with using employment and sales to determine SME status (see Table A13 and Appendix C.6 for a detailed discussion). Although we obtained some improvements in precision (and similar quantitative results) using the employment criterion (much less so for the sales criterion), we also lost sample size due to non-reporting. Hence, we keep to assets as our baseline criterion.

"new SMEs," i.e., those becoming SMEs thanks to the new definition, were allowed SME tax relief rates only on R&D performed after August 2008. Hence, to the extent that firms could plan (or misreport) the timing of R&D, such companies would have an incentive to *reduce* 2008 R&D expenditures before August and increase them afterwards. Given these complexities and the potential lag between R&D inputs and outputs, we focus on 2009 and afterwards as full "policy-on" years. Relatedly, we use 2006-08 as our baseline pre-policy period.

As is standard in RD Designs, we control for separate polynomials of the running variable on both sides of the cutoff and follow Gelman and Imbens' (2018) advice to use first order polynomials when higher order coefficients are not statistically significant.[23] In addition, the inclusion of the lagged dependent variable control PAT_i^{Pre}, although not required in a basic RD Design, helps improve precision. Finally, we also estimate analogous regressions for R&D inputs using comparable data inside the HMRC Datalab.

Difference-in-Differences Design. In addition to the baseline RD Design, we also consider a Diff-in-Diff equation using a panel of firms covering both pre- and post-policy periods:

$$PAT_{it} = \beta^{DiD}(E_i^{2007} \times \mathbb{1}_{\{t>2008\}}) + \theta_i + \tau_t + \varepsilon_{it}, \tag{2}$$

where PAT_{it} is firm i's patent application count in year t, $\mathbb{1}_{\{t>2008\}}$ is a binary indicator of whether year t is in the post-policy period, and θ_i and τ_t are firm and year fixed effects. The Diff-in-Diff Design compares the *difference* in patenting by firms below and above the SME threshold *after* the policy change to the analogous difference *before* the policy change. The identifying condition requires that firms below and above the threshold have similar trends in potential outcomes. This assumption may not hold as the policy change happened during the Global Financial Crisis: such recessions are likely to have larger negative effects on smaller firms, leading to underestimation of the positive causal impact of the policy. On the other hand, if the more stringent identifying assumptions of the Diff-in-Diff Design hold,[24] it allows us to study the policy effect across a wider range of the firm size distribution, not just those around the SME threshold as in the RD Design.

Difference-in-Discontinuities Design. Given the above considerations, we combine the features of the RD Design and the Diff-in-Diff Design in the following Diff-in-Disc equation:

$$PAT_{it} = \beta^{Disc}\left(E_i^{2007} \times \mathbb{1}_{\{t>2008\}}\right) + f_{\mathbb{1}_{\{t>2008\}}}(z_i^{2007}) + \theta_i + \tau_t + \varepsilon_{it}. \tag{3}$$

[23] We show in robustness checks that including higher second or third order polynomials produce qualitatively similar results, and that higher order coefficients are not statistically different from zero (Table A4).

[24] The RD Design assumption of imprecise manipulation of total assets at the SME threshold implies that firms just below the threshold are similar to firms just above the threshold across all dimensions, both observable and unobservable, including how they are affected by the Global Financial Crisis (Lee and Lemieux, 2010).

This design compares the *discontinuity* in patenting by firms below and above the SME threshold *after* the policy change to the analogous discontinuity *before* the policy change. Under the RD Design's identifying assumption, the Diff-in-Disc Design is also identified and, unlike the Diff-in-Diff Design, it is robust to differential trends in unobservables. As with our baseline RD Design, the Diff-in-Disc Design also exploits time-series variation in addition to cross-sectional variation. This enables us to identify the effect of assets-based eligibility for the SME Scheme even in the unlikely case that there is a confounding factor around the same threshold, provided that the effect of the confounding factor does not vary over time.

Relationship between models. Appendix C.1 discusses in detail the relationship between the models presented above and shows that the RD Design with lagged dependent variable control in equation (1) is the most general specification of the three. The Diff-in-Disc Design in equation (3) is a restricted version of equation (1) with $\mu = 1$.[25] The Diff-in-Diff Design in equation (2) is a restricted version of equation (3) with $f_{\mathbb{1}_{\{t>2008\}}}(z_i^{2007}) = 0$. This motivates our choice of equation (1) as the baseline specification throughout this paper.

3.2 Baseline sample

Our baseline RD Design sample contains 5,744 firms with total assets in 2007 between €61m and €111m, based on a €25m bandwidth around the SME assets threshold of €86m, with 3,485 and 2,259 firms below and above the threshold respectively (Table 1).[26] Our choice of baseline bandwidth is guided by results from Calonico, Catteneo, and Titunik's (2014) optimal bandwidth approach.[27] We show that our findings are robust to alternative bandwidths and kernel weights across all three empirical designs. Patents and R&D are winsorized at 2.5% of non-zero values to mitigate the leverage of outliers,[28] and all nominal variables are converted to 2007 prices. The first two columns of Table 1 shows that the numbers of R&D performing firms and patenting firms below the threshold rose after the policy, although the total number of performers is low. We choose to employ the full population

[25] That is, the Diff-in-Disc Design is equivalent to the following model:

$$PAT_i^{Post} - PAT_i^{Pre} = \alpha + \beta E_i^{2007} + f(z_i^{2007}) + \varepsilon_i.$$

[26] The equivalent in-lab sample for R&D analyses contains 5,888 firms, with 3,651 firms below the threshold and 2,327 firms above the threshold. The small difference is due to small variation across different vintages of the FAME dataset available inside and outside of the HMRC Datalab. See Appendix B.4 for details.

[27] The Calonico, Catteneo, and Titunik's (2014) optimal bandwidth for using patents as the outcome variable is €31m and for using R&D is €20m (see Tables A4 and A5). Our baseline bandwidth choice of €25m is in between these two, as we want to have a consistent baseline sample for both outcomes.

[28] This is equivalent to winsorizing the R&D of the top 5 to 6 R&D spenders and the number of patents of the top 2 to 4 patenters in the baseline sample each year. We also show robustness to using different winsorization windows, and to excluding outliers instead of winsorizing outcome variables (Tables A4 and A5).

of firms around the threshold as we do not want to ignore any extensive margins of adjustment for firms starting (or stopping) R&D and/or patenting as a result of the policy.[29] For similar reasons, firms that exited after 2008 are kept in the sample to avoid selection bias (as firm survival is also a potential outcome) and exiters are coded as having zero R&D and patents.

During the pre-policy period, column (7) of Table 1 shows that firms below the threshold filed on average 0.10 (= 0.62 - 0.71) less patents per annum than those above the assets threshold. This gap *reversed* after the policy, so that those below the threshold had a patent advantage of 0.016 (= 0.060 - 0.044), over the group above the threshold – see column (8). This was a change of 0.026 or 39% (0.026/0.066) of pre-policy annual average patents. Similar calculations suggest an increase of £11,109 (£32,758 - £21,649) in annual R&D expenditure for firms benefitting from the policy change. This is consistent with our hypothesis that the 2008 policy induced more patents and R&D. However, these calculations, which are versions of the Diff-in-Diff estimator discussed in subsection 3.1, are likely to understate the causal impact due to the confounding effect of the Great recession, which had a stronger effect on smaller companies.

Balance of predetermined covariates. Table 2 reports the balance of predetermined covariates conditional on the running variable to examine the RD Design assumption of their smooth distributions around the threshold. The estimates show that indeed, at the threshold, there is no statistically significant difference between firms below and above the threshold across a range of observable characteristics in 2006 and 2007 (e.g., sales, employment, capital, R&D, and patents, as well as value added, investments, profit margins, and productivity). However, while the pre-policy discontinuities in R&D and patents in 2007 are not statistically different from zero (columns (8) and (10)), their magnitudes are not trivial. This further motivates our choice to control for pre-policy patents (or R&D) in our baseline RD Design (equation (1)) and employ complementary Diff-in-Diff and Diff-in-Disc Designs (equations (2) and (3)), to ensure that our findings are not spuriously driven by pre-policy differences between the two groups of firms.

3.3 Evidence of direct effect on innovation

Table 3 presents evidence of the direct impact of the policy on innovation output. Panel A examines firms in the baseline sample using different empirical strategies and Panel B further looks at a

[29] Given that our variations come from a small subset of firms, one concern is that using the much larger full-population baseline sample could create artificial statistical power. However, we show in Table A8 that conditioning on more relevant subsets of firms (e.g., pre-policy R&D performers or patenters) yields qualitatively similar results with comparable statistical significance. Note that the shares of R&D performers and patenters among the universe of UK firms during 2009-11 are 0.9% and 0.4%, much lower than the corresponding shares in our baseline sample.

wider sample as well as using R&D spending as a dependent variable.

Policy effect on patents. Columns (1) to (3) in Panel A show the baseline RD Design (equation (1)) across various post-policy windows 2009-11 (in column (1)), 2009-13 (in column (2)) and 2009-15 in column (3)). Firms just below the threshold had significantly more patents than firms just above the threshold with RD estimates between 0.05 and 0.06. Indeed, Panel B of Figure 2 shows that there was a sizable discontinuity in firm's average patents over 2009-13 that corresponds to the estimate of 0.052 in column (2).[30] By contrast, this discontinuity did not exist in the 2006-08 pre-policy period (Panel A of Figure 2). Given that our instrument E_i^{2007} does not perfectly predict a firm's SME status, these reduced-form coefficients present a lower bound for the effect that the policy had on innovation. Even then, they amount to a substantial increase over the pre-policy mean of 0.066 (see column (1) of Table 1), suggesting that the policy impact was large in magnitude.

We next implement the Diff-in-Disc Design in equation (3). Column (4) of Panel A in Table 3 considers the baseline 2006-13 period and column (5) extends it to 2002-15. Both columns yield significant coefficients of 0.045 and 0.042, very close to those in the first three columns. Panel A of Figure 3 complements these estimates by plotting the year-by-year discontinuities (the coefficients on E_i^{2007}) over the entire 2002 to 2015 period, clearly showing that the discontinuity appeared after the policy change but not before then. In column (6), we implement a generalized version of equation (1), the Dynamic Diff-in-Disc Design (see Appendix C.1 for details),[31] which again generates a significant coefficient of 0.052. Finally, columns (7) and (8) present Diff-in-Diff estimates (equation (2)) from the baseline (2006-13) and extended (2002-15) periods and Panel B of Figure 3 plots the corresponding event study coefficients. While these Diff-in-Diff estimates are also positive and significant, they are only 0.026, about half the size of those in the other columns. This is likely to be because the 2008-09 Great Recession had a larger effect on small firms than large firms, breaking the parallel trends assumption underlying the Diff-in-Diff estimator. To investigate if this is really the case, we allow for a break for how firm size affects firm's innovation activities in the final column. This increases the Diff-in-Diff estimate to 0.047, very close to the earlier columns.[32]

[30] We will focus on the 5 years from 2009 to 2013 as our baseline post-policy period in column (2) for subsequent analyses. All results are qualitatively similar if we instead use 2009-11 or 2009-15 as the post-policy period.

[31] Specifically, we estimate the following equation that nests equation (1):

$$PAT_{it} = \beta^{DDisc}\left(E_i^{2007} \times \mathbb{1}_{\{t>2008\}}\right) + f(z_i^{2007}) \times \mathbb{1}_{\{t>2008\}} + \mu_t PAT_{i,t}^{Pre} + \theta_i + \tau_t + \varepsilon_{it},$$

where $PAT_{i,t}^{Pre}$ is firm i's average patent count over 2006-08 when t is in the post-policy period of 2009-13 and the firm's average patent count over 2002-05 when t is in the pre-policy period of 2006-08.

[32] Specifically, we add control for $z_i^{2007} \times \mathbb{1}_{\{t>2008\}}$ to equation (2). While this model is more general than the basic Diff-in-Diff Design, it remains a restricted version of the Diff-in-Disc Design in equation (3) with $f(z_i^{2007}) = \pi z_i^{2007}$.

Policy effect on R&D. Column (1) of Panel B in Table 3 estimates equation (1), but uses R&D spending as the dependent variable instead of patents. As discussed in subsection 3.2, because data on R&D come from confidential administrative source, the sample size is slightly different than that in Panel A and we can only estimate through 2011. The significant and positive RD estimate indicates that over 2009-11, firms just below the SME threshold increased their R&D by £63,387 per year more than firms above the threshold. This is sizable compared to a pre-policy average of £73,977 (column (1) of Table 1).

In column (2) of Panel B in Table 3, we estimate an "innovation production function":

$$PAT_i^{Post} = \alpha + \gamma^{IV} R_i^{Post} + f(z_i^{2007}) + \varepsilon_i, \tag{4}$$

where R_i^{Post} is annual average R&D over 2009-11 and E_i^{2007} is the instrument for R_i^{Post}. Column (3) additionally controls for PAT_i^{Pre} as in equation (1). Under the exclusion restriction that the discontinuity-induced exogenous fluctuations in E_i^{2007} did not affect patenting through any channel other than via R&D, the IV estimate (γ^{IV}) captures the causal marginal effect of policy-induced R&D on patents.[33] We argue in Appendix C.3, that this exclusion restriction is likely to hold in our setting, but it is clearly an additional assumption. The IV coefficients are positive, significant, and large.[34] Column (3) indicates that one additional patent costs on average \$3 million (= 1/0.434 using a \$/£ exchange rate of 1.33) in additional R&D, broadly in line with existing estimates for R&D costs per patent of \$1 to \$5 million.[35] At the pre-policy means of R&D and patents of £0.074m and 0.066 respectively, this implies an elasticity of patents with respect to R&D of about 0.5 (= (0.434/0.066)/(1/0.074)).

Wider samples. Columns (4) to (9) in Panel B use a larger sample of firms based on a €35m (instead of €25m) bandwidth around the SME assets threshold of €86m. Column (4) employs the RD Design; columns (5) and (6) the Diff-in-Disc and generalized Diff-in-Disc Designs; columns (7) and (8) the Diff-in-Diff and augmented Diff-in-Diff Designs; and column (9) the R&D equation similar to column (1). All coefficients are comparable in magnitude and significance to those in the baseline sample with a narrower bandwidth of Panel A. We further extend this exercise to a range of alternative bandwidths that runs from €20m to €40m and show in Figure 4 that our estimates remain robust. This suggests that the positive effects of the R&D Tax Relief Scheme on innovation are not driven by sample choice (Figure 4's Panels A and B) and not restricted to just firms around the threshold but

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[33] Note that unlike β^{RDD}, this IV estimate is not subject to the "fuzziness" of our RD Design.

[34] Despite the weak adjusted first-stage F-statistics of around 6, the Anderson-Rubin weak-instrument-robust inference tests indicate that both IV estimates are statistically different from zero even in the possible case of weak IV.

[35] See Hall and Ziedonis (2001), Arora, Ceccagnoli, and Cohen (2008), Gurmu and Pérez-Sebastián (2008), and Dernis et al. (2015).

quite pervasive (Panel C).

Robustness checks. We discuss many further robustness checks in Online Appendix C**.** For example, we run a series of placebo tests at all possible integer thresholds between €71m and €101m using equation (1) and show that the estimated discontinuity in post-policy patents peaks at €86m and is not statistically different from zero almost anywhere else (Figure A3). The same also holds for R&D (Panel B of Figure A4). That is, the jumps in patents and R&D exist at the true SME threshold, as a result of the 2008 policy change. There are some positive effects on placebo thresholds close to the real threshold, but these are driven by contamination from the real policy effect at the threshold. Additionally, Tables A4 and A5 show that our results are also robust to a wide range of alternative specifications, as discussed in detail in Appendix C.4.[36]

Patent quality. As patents vary widely in quality, one important concern is that the additional patents induced by the policy could be of close to zero value. Table 4 considers different ways to account for patent quality (see Appendix B.1 for details). Column (1) reproduces our baseline patent count result using the baseline RD Design. Column (2) considers only "part-triadic" patents, i.e., those filed in one of the world's major patent offices: the European Patent Office (EPO), the US Patent and Trademark Office (USPTO), or the Japan Patent Office (JPO). Since filing at the EPO, USPTO, and JPO is more expensive than just at the local UK office, these patents are likely of higher value. Not only did the policy have a comparable effect on these high value patents, but its proportional effect (the RD coefficient divided by the pre-policy mean of the dependent variable, reported in the final row) on high value patents was if anything, larger than that in column (1), 1.6 vs. 1.3. In a similar vein, column (3) weights patents by the number of jurisdictions in which the invention is patented ("family-size"), yielding a proportional effect of 1.9. Column (4) uses granted patents instead of all applications and similarly reports a positive and statistically significant estimate. Columns (5) to (7) count the number of patents in the top-quality quartile with respect to their technology class-by-filing year cohorts using three different patent quality measures:[37] (i) scope, defined as the patent's distinct technology class count, (ii) originality index, which measures the technological diversity of the

[36] These robustness tests include (i) employing alternative kernel weights, (ii) imposing no sample bandwidth restriction, (iii) considering only firms below the employment threshold, (iv) using different winsorization or trimming rules, (v) adding higher polynomial controls or industry/location fixed effects, (vi) employing other estimation models (including count data models such as Poisson and Negative Binomial, and Calonico, Catteneo, and Titunik's (2014) robust bias-corrected optimal bandwidth RD Design), (vii) using different pre-policy years to construct the lagged dependent variable control, and (viii) considering 2008 as a post-policy period year.

[37] We focus on quality-weighted patent counts instead of average citations per patents, as the latter is not defined for most non-patenting firms. Furthermore, we do not expect the policy to increase average patent quality, but quality-adjusted patent counts (i.e., the policy did induce meaningful patents/innovations of some value).

patent's backward citations, and (iii) the patent's forward citation count. The results again indicate that the policy was also effective in inducing top-quality patents. Finally, column (8) focuses on non-ICT patents and shows that the results are not solely driven by more ICT patents (which are sometimes deemed of lower quality).

Summary and discussion. We find that the R&D tax policy had sizeable impacts on patents and R&D, and this is robust to a range of empirical strategies and estimation samples. Our results imply that after the policy change, firms below the new SME threshold filed 0.04 to 0.05 more patents per year compared with firms above the threshold. Furthermore, we find no evidence that these additional policy-induced patents were of lower quality. One reason for these large effects is that our sample is on SMEs and such firms are more likely to be financially constrained and have higher-return R&D projects which they could not have undertaken without the policy. We also present direct evidence supporting this financial constraint hypothesis in subsection 5.2.

The impact on patents occurs quite quickly after the policy change and lasts until at least 2015. One explanation for the relatively fast effects is that patent *applications* are often timed quite closely with R&D.[38] A second possibility is that the patents filed soon after the policy change (in say 2009-10) do not reflect new knowledge but rather patenting on existing (marginal) knowledge that the firm now has more cash to take out patent protection. This cannot explain the increased patenting in the later years shown in Figure 3 that documents impacts on patents filed up to seven years after the policy change. Nor is it consistent with the absence of a decline in patent quality in Table 4, which would occur if only minor inventions where now patented. Hence, the most likely explanation is that the policy induced more R&D which led to greater innovation and ultimately growth. A key issue for the growth effects is whether the benefits accrue to only the firms who received subsidies or whether these benefits spilled over to more firms. We now turn to this question.

4. Spillovers on technologically connected firms

The main economic rationale given for more generous tax treatment of R&D is that there are technological externalities, so the social returns to R&D exceed the private returns. Our design also allows us to estimate the causal impact of tax policies on R&D spillovers, i.e., innovation activities of firms that are *technologically connected* to policy-affected firms, through employing similar RD

[38] The literature starting with Hall, Griliches and Hausman (1986) consistently finds the strongest link between contemporaneous R&D expenditure and patenting when exploring the firm-level lag structure (see also Gurmu and Pérez-Sebastián, 2008, Wang et al, 1998, Guo and Trivedi, 2002). Wang and Hagedoorn (2014) argue that firms typically apply for some patents very early on in a longer R&D process, as the "first-to-file" rule provides a strong incentive for them to do so. This is then followed by further R&D spending and subsequent patents that provide improvements and further refinements on the initial patent.

and Diff-in-Disc Designs with connected firms' patents as the outcome variable of interest.[39] To our knowledge, this paper is the first to provide Regression Discontinuity estimates of technology spillovers.

4.1 Spillover estimation framework

Spillover structural equation. We start from a general system of spillover equations (see Carneiro et al, 2020 and Manski, 1993, for similar set-ups) in which each firm's innovation output (patents) depends on (i) its own R&D, (ii) all connected firms' R&D, and (iii) all connected firms' innovation outputs. Appendix D.1 shows that given this structure, an increase in firm i's R&D can affect a connected firm j's patenting via both a *direct* spillover from firm i's R&D, and an *indirect* spillover from firm i's patenting (which increases with firm i's R&D). The *net* effect of these two spillover channels can be recovered from the IV specification:

$$PAT_j^{Post} = \alpha + \xi^{IV} R_i^{Post} + f(z_i^{2007}) + g\left(z_j^{2007}\right) + \varepsilon_{ij}. \quad (5)$$

In equation (5) each observation is a dyad of technologically connected firms i-j, and firm i's post-policy R&D R_i^{Post} is instrumented with its below-assets-threshold indicator E_i^{2007} as in equation (4).[40] The exclusion restriction requires that E_i^{2007} only affects PAT_j^{Post} through spillovers from R_i^{Post} . This assumption can be decomposed into two elements: (i) E_i^{2007} should only affect firm j's innovation activities (and thus, PAT_j^{Post}) via firm i's innovation activities; and (ii) E_i^{2007} should only affect firm i's innovation activities (including R_i^{Post} and PAT_i^{Post}) via R_i^{Post}. Element (ii) is the exclusion restriction for estimation of equation (4) as discussed in Appendix C.3. Moreover, since E_i^{2007} is as good as random in the RD Design, Appendix D.2 shows that under mild sufficiency conditions, it is also conditionally uncorrelated with connected firm j's characteristics, including the firm's eligibility for the SME Scheme. This implies that the first element (i) of the exclusion restriction is also satisfied. Equation (5) then produces consistent estimates of the magnitude of R_i^{Post}'s *net* spillovers on PAT_j^{Post}.

Spillover RD equation. To examine the policy spillover effect, we focus on the following spillover RD equation that is the reduced-form of the structural equation (5), with the addition of a control for firm j's pre-policy patents as in equation (1):

$$PAT_j^{Post} = \alpha + \delta^{RDD} E_i^{2007} + f(z_i^{2007}) + g\left(z_j^{2007}\right) + \mu PAT_j^{Pre} + \varepsilon_{ij}. \quad (6)$$

[39] See Dahl, Løcken, and Mogstad (2014) for a similar methodological approach in a different context.
[40] $f(z_i^{2007})$ and $g(z_j^{2007})$ are polynomials of firms i and j's total assets in 2007.

Under the RD Design assumption that E_i^{2007} is conditionally uncorrelated with connected firm j's characteristics as discussed above, this equation identifies the spillovers on firm j's innovation output from being connected to a firm i that was below the SME threshold (relative to being connected to a firm that was above the threshold). That is, equation (6) relies on weaker assumption than equation (5), as it does not additionally require that E_i^{2007} affect firm i's innovation activities only via R_i^{Post}. Similar to β^{RD} in equation (1), δ^{RDD} is a lower bound for the spillovers that the SME Scheme had on firms connected to its recipients.

Spillover Diff-in-Disc equation. In addition, we also consider a complementary spillover Diff-in-Disc Design as follows:

$$PAT_{jt}^{Post} = \delta^{Disc} E_i^{2007} \times \mathbb{1}_{\{t>2008\}} + f_{\mathbb{1}_{\{t>2008\}}}(z_i^{2007}) + \theta_j + \tau_t + \varepsilon_{ij}. \tag{7}$$

This equation, which is analogous to the Diff-in-Disc Design in equation (3), compares the discontinuity in technology spillovers from firms below and above the SME threshold *after* the policy change to the corresponding discontinuity *before* the policy change.

4.2 Dyadic sample for spillover estimation

We consider two firms to be *technologically connected* if (i) most of their patents are in the same three-digit IPC technology class and (ii) the Jaffe (1986) technological proximity between them is above 0.75 (the median among all firm pairs sharing the same primary technology class).[41] The first criterion allows us to allocate each dyad to a single technology class, whose size N matters to the strength of the spillovers. However, as two firms sharing the same primary technology class could still have very different patent portfolios, we refine the definition of technological connectedness with the second criterion. Relaxing either criterion, or imposing further restrictions, does not materially affect our qualitative findings (see Appendices D.3 and D.5 for details).

Our baseline spillover estimation sample consists of all firm i and j dyads ($i \neq j$) such that firm i is within our baseline sample of firms with total assets in 2007 between €61m and €111, and firm j is technologically connected to firm i. Firms i and j are drawn from the universe of UK patenting firms over 2000-08 for which we can construct these measures. Similar to our earlier analyses of direct policy effects, we measure PAT_j^{Post} as firm j's average patents over 2009-13, PAT_j^{Pre} as firm j's average patents over 2006-08, and R_i^{Post} as firm i's average R&D over 2009-11. To examine the robustness of our findings, we also extend the sample to include firm i's that are further away from

[41] The Jaffe technological proximity equals 1 if firms i and j have identical patent technology class distribution and 0 if the firms patent in entirely different technology classes (see Appendix D.3 for details).

the SME threshold.

4.3 Evidence of spillovers in smaller technology classes

Spillovers by technology class size. Column (1) of Table 5 estimates equation (6) using the *full* dyadic sample of technologically connected firms, which yields an insignificant spillover coefficient δ^{RDD}. However, we expect spillovers to be measurable only in small-enough technology classes, where a single firm has better chances of influencing the field's technological frontier and thereby other firms' innovations, as formally shown in Appendix D.1.[42] To test this, in Figure 5, we semi-parametrically estimate δ^{RDD} as a function of the dyad's technology class's size percentile (details in Appendix D.4), which generates a downward sloping curve with significant positive effects only in the small technology classes (and small insignificantly negative ones in the large classes). We then generalize column (1) by including an interaction between E_i^{2007} and the size percentile (normalized to range from 0 to 1) of the dyad's technology class. The spillover effect is now positive and significant in the smallest technology classes, whereas the negative and statistically significant interaction term implies that this effect is close to zero in the largest ones.

Spillovers in small technology classes. Guided by Figure 5, we split the full sample of all connected firm dyads by their technology class size at 200 firms, which corresponds to the 70th percentile, up to which there are positive spillover effects. The policy spillover coefficient is positive and significant in small technology classes in column (4), and insignificant (with a small negative coefficient) in column (3) in large technology classes. Focusing on the small classes, column (5) implements the Diff-in-Disc Design (equation (7)) for the 2006-13 period, which delivers a coefficient almost identical to that in column (4). Panel A of Figure 6 plots the annual spillover coefficients and shows that in the pre-policy period, there was no difference in patenting trends between firm j's connected to firm i's below the threshold and those connected to firms above the threshold. In column (6), we estimate the spillover structural equation (equation (5)) using E_i^{2007} as the instrument for firm i's R&D. Consistent with the earlier reduced-form results, the coefficient of R&D spillovers on technologically connected firms is also positive and statistically significant. In terms of magnitude, it is about 40% (= 0.222/0.563) of the effect of policy-induced own R&D on own patents (see column (2)

[42] For the same reason, Angrist (2014) recommends and Dahl, Løcken, and Mogstad (2014) implements looking at groups with small numbers of peers when examining spillover effects.

in Panel B of Table 3).[43] These results, which are robust to a range of robustness tests,[44] indicate the presence of positive spillovers from the R&D policy in small-enough technology classes.

Direct versus indirect spillovers. As discussed in subsection 4.1, the IV estimate of column (6) captures the *net* spillovers of firm i's R&D on connected firm j's patents, which on its own is an important policy-relevant parameter. Furthermore, Appendix D.1 shows that for a given value of the effect of PAT_i on PAT_j (denoted π), it is possible to back out the *direct* effects of R_i and R_j on PAT_j (denoted ψ and κ respectively) from the IV estimates of the *net* R&D spillover effect (ξ^{IV} in equation (5)) and *net* own R&D effect (γ^{IV} in equation (4)). With $\hat{\xi}$ = 0.222 and $\hat{\gamma}$ = 0.563, Figure A5 shows that the threshold $\bar{\pi}$ under which the effect of firm i's R&D on the patents of firm j (ψ) is positive, is strongly increasing in the size of the technology class (denoted N, the number of firms in the technology class). At $N = 109$ (the average value of N in small technology class sample), $\bar{\pi} = 0.98$, implying that ψ is positive for any reasonable value of π (see Figure A6). That is, it is most likely that R&D also has positive *direct* (not just net) impact on connected firms' innovations.

Wider samples. We also show that these R&D spillovers are not restricted to firms around the SME threshold. In columns (7) to (9) of Table 5, we replicate columns (2), (4), and (5) using an extended sample that includes firm i's with 2007 assets within €35m of the SME threshold and their technologically connected firm j's. As discussed above, all key results hold using this larger sample, albeit with somewhat smaller magnitudes. Similarly, further extending or narrowing firm i sample around the threshold does not qualitatively affect these results (see Panel B of Figure 6).[45]

5. Magnitude of effects and policy implications

5.1 Intensive versus extensive margins

To investigate whether our results are driven by the extensive margin, we estimate the baseline RD specification of equation (1) with binary indicators of whether the firm reports positive patents

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[43] Column (6)'s corresponding reduced-form and first-stage regressions are reported in columns (16) and (17) of Table A7. Back-of-the-envelope calculations combining the baseline reduced-form estimate in column (4) of Table 5 (which additionally controls for connected firm j's pre-policy patents) and the same first-stage estimate in column (17) of Table A7 suggest a smaller R&D spillover coefficient of 0.096 (= 0.085/0.884), which is about 22% of the similarly-estimated effect on own patents of 0.434 (see column (3) in Panel B of Table 3).

[44] See Table A7, Figure A7, and Appendix D.5. These tests include (i) employing alternative clustering schemes, (ii) controlling differently for $g(z_j^{2007})$, (iii) using alternative definitions of technological connectedness, (iv) employing count data models (Poisson and Negative Binomial) instead of OLS, and (v) considering alternative post-policy (as well as pre-policy) periods.

[45] Appendix D.6 also implements Bloom, Schankerman and Van Reenen's (2013) methodology to estimate both knowledge spillover and business stealing effects of rival R&D competition. We find that in our setting, business stealing effects were dominated by knowledge spillovers generated by policy-induced R&D.

(or positive R&D) as outcome variables. The coefficients, reported in Table A8, suggest some extensive margin effect on patents but not R&D. In a complementary approach, we split the baseline sample by firm's pre-policy patents and R&D or industry pre-policy patenting intensity, and find that firms and sectors already engaged in innovation activities had the strongest responses to the policy change (Table A9).[46] These results provide strong evidence that more generous R&D tax relief did not materially affect a firm's selection into performing R&D performance but worked mostly through the intensive margin. That is, the policy appears to mostly benefit firms that were already performing R&D and patents before the policy change, thereby improving these firms' chances of continuing to patent going forward. This is important for our elasticity calculations in the following subsection.

5.2 Magnitude of effects and tax-price elasticities

What is the implied elasticity of *innovation* with respect to the tax-adjusted user cost of R&D (e.g., Hall and Jorgenson, 1967, or Bloom, Griffith, and Van Reenen, 2002)? Given the large policy-induced patent increase in our setting, we focus on the following arc elasticity measure, which calculates the percentage difference relative to the midpoint instead of either end point:[47]

$$\eta_{PAT,\rho} = \frac{\%\text{ difference in } PAT}{\%\text{ difference in } \rho} = \frac{\frac{PAT_{SME} - PAT_{LCO}}{(PAT_{SME} + PAT_{LCO})/2}}{\frac{\rho_{SME} - \rho_{LCO}}{(\rho_{SME} + \rho_{LCO})/2}},$$

where ρ_{SME} and ρ_{LCO} are the firm's tax-adjusted user cost of R&D under the SME and the Large Company ("LCO") Schemes, and PAT_{SME} and PAT_{LCO} are the firm's corresponding patents.

Deriving percentage difference in patents, $\boldsymbol{PAT}$. As discussed in Section 3, to obtain estimates of the treatment effect of the SME Scheme on patents, i.e., $PAT_{SME} - PAT_{LCO}$, we need to scale β^{RDD} in equation (1) by how sharp E_i^{2007} is as an instrument for actual eligibility SME_i^{Post}. We estimate this "sharpness" λ using the following equation:

$$SME_i^{Post} = \alpha + \lambda E_i^{2007} + f(z_i^{2007}) + \varepsilon_i. \quad (8)$$

Equations (1) and (8) correspond to the reduced form and first stage equations in a fuzzy RD Design that identifies the effect of the more generous SME Scheme on a firm's patents at the SME assets

[46] We also split the baseline sample by the firm's pre-policy capital investments. The results indicate that policy effects on patents and R&D were larger among firms that had invested, suggesting that current R&D and past capital investments are more likely complements than substitutes. This is consistent with the idea that firms having previously made R&D capital investments have lower adjustment costs and therefore respond more to R&D tax incentives (Agrawal, Rosell, and Simcoe, 2020).

[47] Alternatively defining the elasticity as the log difference in patents over the log difference in the tax-adjusted user cost of R&D, i.e., $\eta = \frac{\ln(PAT_{SME}/PAT_{LCO})}{\ln(\rho_{SME}/\rho_{LCO})}$, yields quantitatively similar elasticity estimates (Table A15).

threshold, using E_i^{2007} as an instrument for SME_i^{Post}.

Our setting differs from standard fuzzy RD Designs in that SME_i^{Post} is missing for the firms with zero R&D. Therefore, we can only estimate equation (8) on the subsample of R&D performing firms.[48] Selection into this subsample by R&D performance raises the concern of whether the resulting $\hat{\lambda}$ is a consistent estimator of the true λ in the full baseline sample, which includes non-R&D performers. In Appendix E.2 we prove that a sufficient condition for $E(\hat{\lambda}) = \lambda$ is that the SME Scheme did not increase firm's likelihood of performing R&D, which holds in our setting as discussed in subsection 5.1. Then the composition of eligible and non-eligible firms below and above the threshold in the R&D-performer subsample would be the same as that in the full baseline sample. As a result, we are able to derive $\frac{\hat{\beta}^{RDD}}{\hat{\lambda}}$, in which $\hat{\beta}^{RDD}$ is estimated using the full baseline sample and $\hat{\lambda}$ the R&D-performer subsample, as consistent estimators of the causal effect of the SME Scheme on patents at the eligibility threshold.

Table 6 reports the results from estimating equation (8) using the subsample of R&D performing firms in each respective year. Columns (1) to (3) show that being under the new SME assets threshold in 2007 significantly increased the firm's chance of being eligible for the SME Scheme in the post-policy years, even though the instrument's sharpness decreases over time, as we would expect. Columns (4) to (6) aggregate a firm's SME status over different post-policy periods, which yield coefficients in the range of 0.25 to 0.46 that are all significant at the 1% level. Columns (7) to (9) report quantitatively similar estimates using the larger sample of firms with 2007 assets within €35m of the SME threshold. In what follows we will use the mid-range coefficient of 0.353 (column (5)) as the baseline estimate of λ.[49] Combined with $\hat{\beta}^{RDD}$ = 0.052 for patent outcome (column (2) in Panel A of Table 3), this implies a causal treatment effect (of the more generous SME Scheme) of 0.052/0.353 = 0.147 and a percentage difference in patents of 1.06.[50] Figure A8 shows the empirical distribution and confidence interval of this treatment effect estimate using a bootstrap procedure.

Deriving percentage difference in tax-adjusted costs of R&D, ρ. In Appendix E.3, we explain in detail how we calculate the tax-adjusted user cost ρ_f for $f \in \{SME, LCO\}$ based on the actual design of the R&D Tax Relief Scheme. The resulting average tax-adjusted user cost of R&D was 0.15

[48] Similarly, we cannot directly estimate the corresponding structural equation for the full baseline sample.

[49] A firm's SME status over a period is the maximum of its SME status in each of the year within the period. We also report elasticity estimates derived from alternative estimates of λ (using different post-policy periods) in Table A18.

[50] That is, $\frac{PAT_{SME}-PAT_{LCO}}{(PAT_{SME}+PAT_{LCO})/2} = \frac{0.147}{(0.147+0.066+0.066)/2} = 1.06$. As the tax-adjusted user cost of R&D for large companies remains unchanged over 2006-11 (Table A2), it seems reasonable to use the average patents over 2006-08 as a proxy for how much an average firm would patent if it remained a large company over 2009-11.

under the SME Scheme and 0.19 under the Large Company Scheme over 2009-11, which translates into a percentage difference in user cost of 0.27 (Table A2).

Deriving tax-price elasticity of patents, $\eta_{PAT,\rho}$. Putting the elements together we obtain a tax-price elasticity of patents of about 3.9 (= 1.06/0.27). Analogous calculations yield a comparable elasticity of R&D with respect to its user cost of 4.1 (see Appendix E.4 for details). These elasticity estimates are substantially higher than the typical values between one and two found in other studies. However, Acemoglu and Linn (2004) also find R&D elasticity estimates in the range of 4 with respect to market size and suggest that this should be the same as R&D elasticity with respected to its user cost. Similarly, Akcigit et al. (2021) find an elasticity of 3.5 using state level variation in income tax rules. Figure A9 further shows that based on the bootstrapped distribution of $\hat{\eta}_{R,\rho}$ and $\hat{\eta}_{PAT,\rho}$, a left-sided 5%-sized test rejects the hypothesis that patent tax-price elasticities were lower than 2, and that R&D tax-price elasticities were lower than 1.4. Note that there are considerable uncertainties, due to many assumptions that need to be made in calculating this elasticity.

The role of financial constraints. Our setting is different from those in previous studies on R&D tax credits, which have explicitly (by using publicly listed firms) or implicitly (by using aggregate data) focused on larger firms, as R&D is concentrated in such entities. Our sample, by contrast, is predominantly smaller firms around the €86m threshold. As mentioned in subsection 3.3, these firms were more likely to be financially constrained and thus more responsive to R&D tax incentives. Many recent empirical studies find greater responses of smaller firms to business support policies (for example, see the work and survey in Criscuolo et al., 2019).

In support of this idea, Table 7 shows that the policy effect was much larger for firms that are likely to be financially constrained. We construct an industry-level measure of financial constraints as the average cash holdings to capital ratio in each three-digit SIC industry among the population of UK firms (see Appendix B.5 for details). All else equal, we expect industries with higher cash-to-capital ratios to be less financially constrained. Throughout Table 7, we interact the right-hand-side variables with a binary indicator of whether the firm's industry is above or below median in this cash-to-capital measure (columns (1), (3), (5), and (6)) or with this measure itself (columns (2), (4), and (7)). Columns (1) and (3) show that the policy had positive and significant effect on patents only among firms in financially constrained sectors, consistent with the negative and significant interaction terms in columns (2) and (4). This also holds true for the policy effect on R&D (column (5)), and among firms further away from the SME eligibility threshold (columns (6) and (7)). We also calculate the Rajan and Zingales (1998) index of industry external-finance dependence and other industry-level measures of financial constraints, all of which yield qualitatively similar results (see Table A10).

Finally, note that the policy change was introduced during the Global Financial Crisis when *all* firms were more likely to be credit constrained. Although this is not an identification threat to the RD Design, it may limit our results' external validity. However, we find that the effects of tax relief on R&D and patents were still strong as late as 2011 and 2015, well after the end of the credit crunch.

5.3 Cost effectiveness of the R&D Tax Relief Scheme

A full welfare analysis of the R&D policy is complex as one needs to consider general equilibrium effects through spillovers. We take one step in this direction by implementing a simple "value for money" calculation based on how much additional R&D is generated per taxpayer dollar (or £). We present details of the calculations in Online Appendix F. Our elasticity estimates imply that over 2006-11, the ratio of policy-induced R&D to taxpayer costs of the SME deductible scheme was 3.9, SME payable scheme was 2.9, and Large Company Scheme was 1.9 (Table A16).[51] During this period, annually, £302m (£660m) of Exchequer costs generated £992m (£1,258m) additional R&D in the SME Scheme (Large Company Scheme). This translates into an aggregate "value for money" ratio of about 1.3.

Figure 7 shows estimates of the counterfactual business R&D (BERD) to GDP ratio in the absence of the R&D Tax Relief Scheme. It is striking that since the early 1980's UK BERD became an increasingly small share of GDP, whereas it generally rose in other major economies. Our analysis suggests that this decline would have continued were it not for the introduction and extension of a more generous fiscal regime in the 2000's. Business R&D would have been 14% lower over the 2006-11 period.

5.4 R&D tax relief's effects on other aspects of firm performance

We examine if the R&D Tax Relief Scheme generated impacts on other aspects of firm performance through to 2013 (Table A12), using the RD Design similar to equation (1) but with (i) sales, (ii) employment, (iii) capital, and (iv) Total Factor Productivity (TFP) as the outcome variables.[52] Panel B reports sizable, robust, and growing lower-bound estimates of the impact of the SME Scheme on employment over 2009-13, consistent with a dynamic in which firms increased R&D, then innovated, and then grew larger. In Panel A, the estimates are less precise but exhibit similar pattern, suggesting that the SME Scheme also had some positive impact on sales. On the other hand, we find

[51] For the SMEs (under either deductible or payable scheme), we use the median elasticity estimate of 4.0 in our calculations. For the large companies, we use the lower-bound elasticity estimate of 1.4.
[52] We estimate the discontinuities in firm performance both before and after the policy change and therefore for consistency we do not control for firm's pre-policy performance in columns (1) to (8). Nevertheless, we report the "RD-in-Diff" estimates (equivalent to equation (3)'s Diff-in-Disc Design, see footnote 25 and Appendix C.1 for details) for firm performance outcomes in column (11).

little evidence of policy-induced increase in capital (Panel C). This may reflect contemporaneous substitution towards intangible capital (R&D) and away from tangible capital. In Panel D, we examine if more innovations translated into higher productivity by estimating the policy impact on TFP (details in Appendix B.5). Similar to Panel A, the resulting coefficients, although noisy, are substantially larger in the post-policy years, especially in comparison to the pre-policy ones of close to zero. Finally, we find no effect on firm's survival after the policy change.

These results should be interpreted with caution. As discussed above, there are many missing values for employment and sales as UK accounting regulations do not insist on these being reported for smaller and medium sized enterprises as in the US. Nevertheless, the results suggest that the policy positively affected other measures of size and productivity as well as innovation.

6. Conclusion

Fiscal incentives for R&D have become an increasingly popular policy of supporting innovation across the world. However, little is known about whether these costly tax breaks causally raise innovation for the firms receiving the subsidies, still less whether they generate spillovers on their technological neighbors. We address these issues by exploiting a change in the UK R&D Tax Relief Scheme in 2008, which raised the size threshold determining whether a firm was eligible for the more generous SME Scheme. This enables us to implement an RD Design to assess the impact of the policy on R&D and patenting. Using total assets in the pre-policy year of 2007 as the running variable, we show that there is no evidence of discontinuities around the new SME assets threshold prior to the policy change, which is unsurprising as this new threshold was used only by the R&D Tax Relief Scheme and no other programs targeting SMEs.

The policy generated economically and statistically significant increases in patenting and R&D. Furthermore, the tax relief also appears to stimulate positive technology spillovers. These results suggest that R&D tax policies are effective in increasing innovation, and not simply devices for relabeling existing spending or shifting innovation activities between firms. The implied elasticities of R&D and patents with respect to changes in R&D user cost are large, and we argue that this is probably because we focus on SMEs, which have been shown to be more likely to be financially constrained than those conventionally studied in the extant literature.

There are many caveats when moving from these results to policy. Although the results are optimistic about the efficacy of tax incentives, the large effects come from smaller firms and should not be generalized across the entire firm size distribution. This does imply that targeting R&D policy on financially constrained SMEs is worthwhile, although a first best policy would be to deal directly with the underlying credit market imperfections. Note that our estimates are based on the period during and after the global financial crisis when credit frictions might have been particularly acute. However, the fact that the impact is also large seven years after the crisis suggests that this caveat on external validity should not be overstated.

We have partially examined equilibrium effects by demonstrating that the R&D Tax Relief Scheme not only stimulated innovations by firms that directly benefited, but also generated positive spillovers on other firms. However, there may be other equilibrium effects that reduce innovation. For example, subsidies may be captured in the form of higher wages rather than higher volume of R&D, especially in the short-run. We believe that this is less likely a first order problem when there is large international mobility of inventors, as is the case in the UK (e.g., Akcigit, Baslandze, and Stantcheva, 2017, Moretti and Wilson, 2017). Furthermore, the policy's strong effect on patenting implies that the increase in R&D is driven by volume and not just wages. Nevertheless, investigating the magnitude of such equilibrium effects is an important area for future work.

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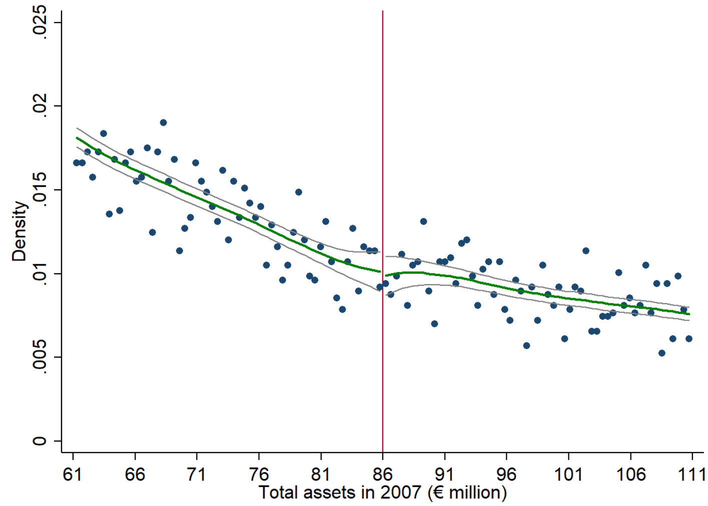
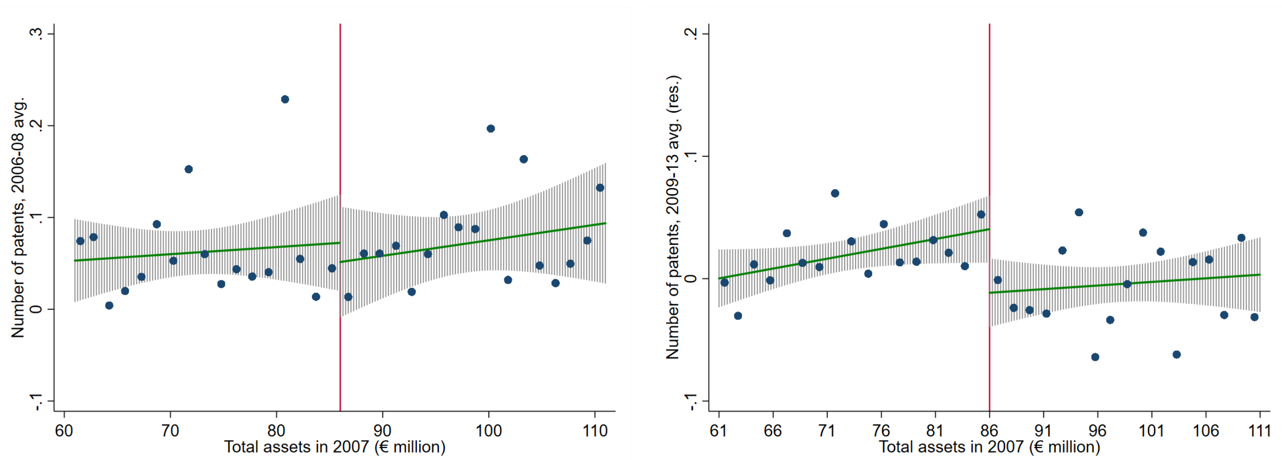
Figure 1: McCrary Test for No Manipulation at the SME Assets Threshold in 2007



Notes: This figure reports the McCrary test for discontinuity in distribution density of total assets in 2007 at the 2008 new SME assets threshold of €86m. Estimation sample includes firms with total assets in 2007 between €46m and €126m. The discontinuity estimate (log difference in density height at the SME threshold) (standard error) is -0.026 (0.088), not statistically different from zero.

Figure 2: Discontinuities in Patents at SME Threshold

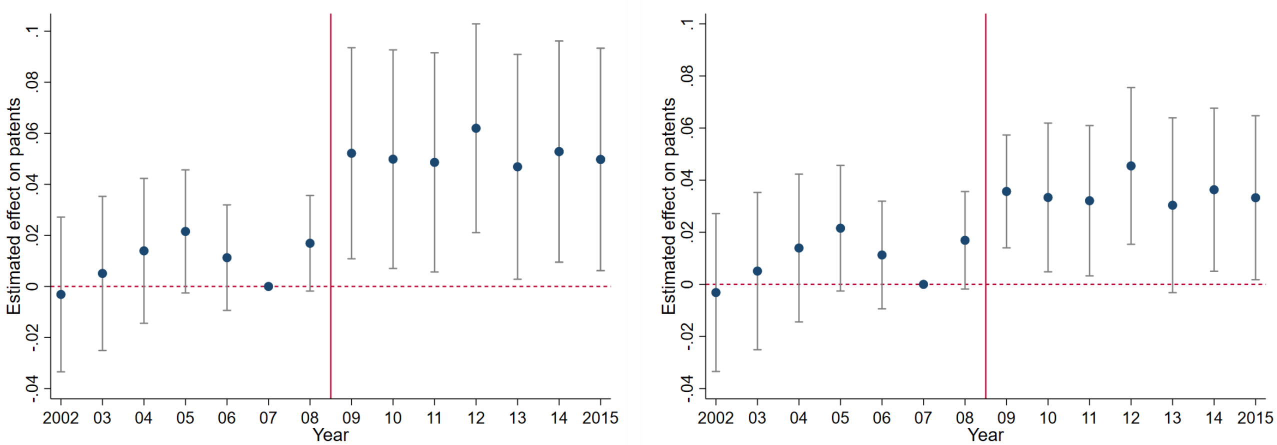


A. Before policy change

B. After policy change

Notes: This figure plots the discontinuity in firm's average patents at the SME assets threshold of €86m before and after the policy change. **Panel A:** The Y-axis variable is firm's average pre-policy patents over 2006-08. The discontinuity estimate (standard error) is 0.021 (0.032), not statistically significant, as reported in column (1) in Panel B of Table A3. **Panel B:** The Y-axis variable is firm's average post-policy patents over 2009-13 after partialling out its average pre-policy patents over 2006-08 using equation (1). The discontinuity estimate (standard error) is 0.052 (0.019), statistically significant at 1% level, as reported in column (2) in Panel A of Table 3. In both panels, each point represents a bin of 173 firms on average, over an assets range of €1.5m. The shaded areas indicate 95% confidence intervals of the fitted linear models shown on the plots.

Figure 3: Pre- vs. Post-Policy Effects on Patents

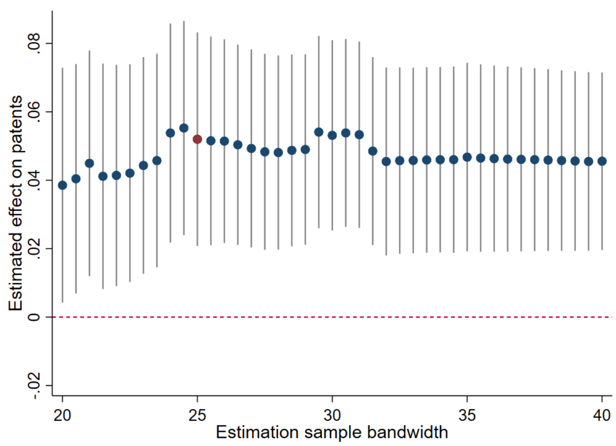


A. Diff-in-Disc Design

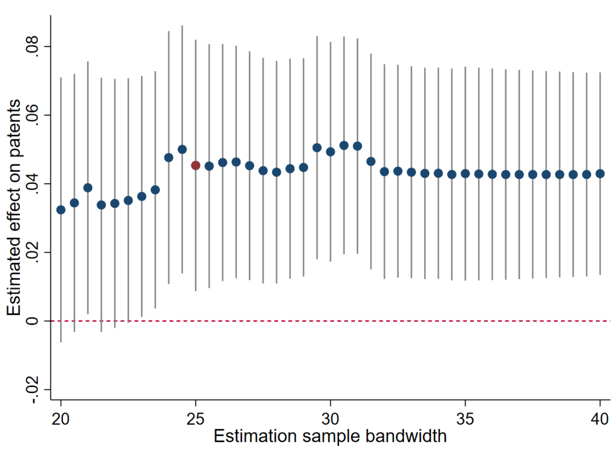
B. Diff-in-Diff Design

Notes: **Panel A** plots the annual discontinuity in patents at the SME assets threshold, relative to that discontinuity in 2007. These discontinuities are estimated using a year-specific coefficient on the Diff-in-Disc estimator in equation (3). **Panel B** plots the annual difference in average patents by firms below the SME assets threshold and firms above the threshold, relative to that difference in 2007. These differences are estimated using a year-specific coefficient on the Diff-in-Diff estimator in equation (2). In both panels, standard errors are clustered by firm. The grey lines indicate 90% confidence intervals of the estimates. (See Appendix C.2 for further details.)

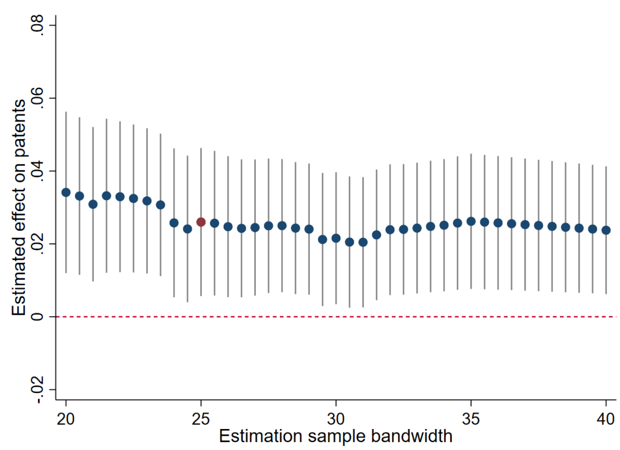
Figure 4: Policy Effect on Patents By Sample Bandwidth



A. RD Design



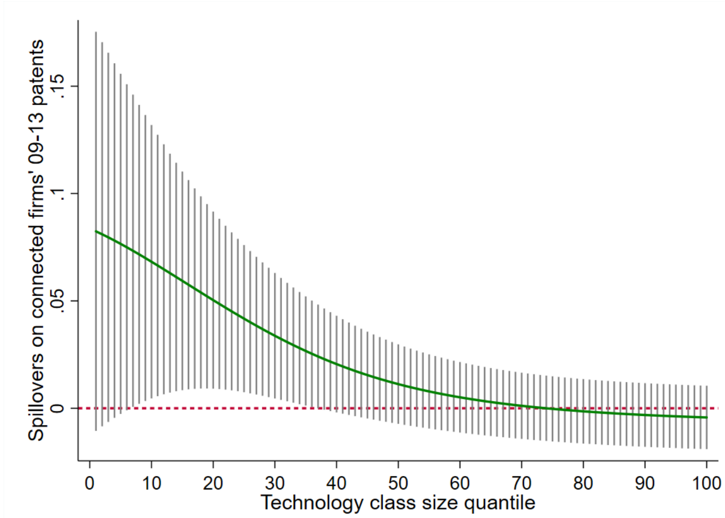
B. Diff-in-Disc Design



C. Diff-in-Diff Design

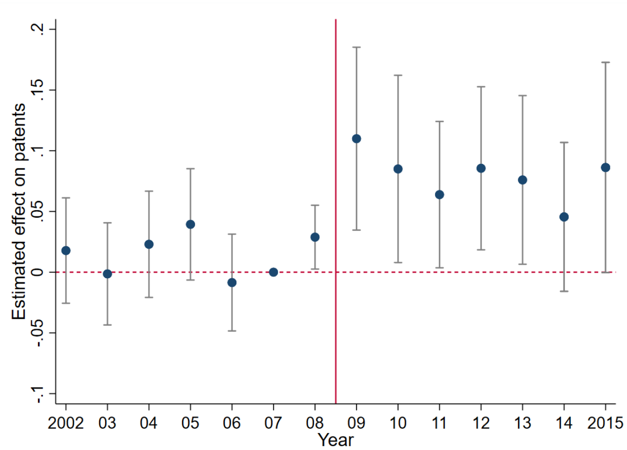
Notes: This figure plots the estimated policy effect on patents in various samples of firms around the SME assets threshold of €86m (the X-axis variable), ranging from 4,501 firms within a €20m bandwidth (i.e., firms with 2007 total assets between €66m and €106m) to 10,165 firms within a €40m of bandwidth (i.e., firms with 2007 total assets between €46m and €126m). **Panel A** plots the RD coefficients estimated using equation (1) over 2006-13. **Panel B** plots the Diff-in-Disc coefficients estimated using equation (3) over 2006-13. **Panel C** plots the Diff-in-Diff coefficients estimated using equation (2) over 2006-13. In both panels, for estimates of larger bandwidths, observations are weighted by their distance to the threshold. The red dots correspond to the baseline sample (i.e., firms within a €25m bandwidth of the threshold), as reported in columns (2), (4), and (6) in Panel A of Table 3. Standard errors are clustered by firm. The grey lines indicate 90% confidence intervals of the estimates.

Figure 5: Spillovers on Connected Firms by Technology Class Size

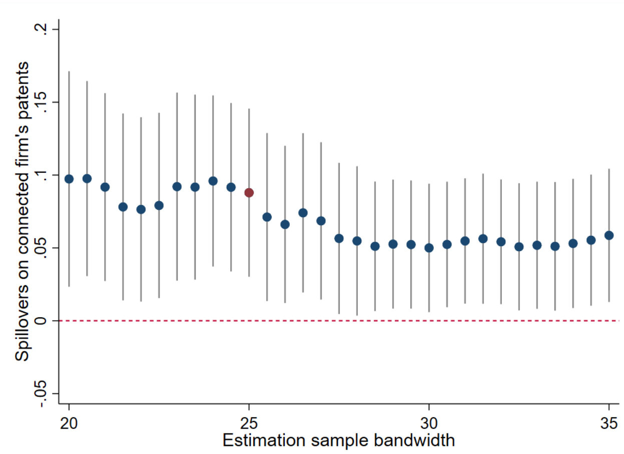


Notes: This figure plots the estimated spillover effect on technologically connected firms' patents as a function of the technology class size percentile (the X-axis variable). The semiparametric estimation is based on a generalized version of equation (6) as specified in equation (D5), using a Gaussian kernel of the variable on the X-axis and a bandwidth of 25% of the range (see Appendix D.4 for details). Standard errors are clustered by primary technology class. The grey lines indicate 90% confidence intervals of the spillover estimates.

Figure 6: Spillovers on Connected Firm in Small Technology Classes



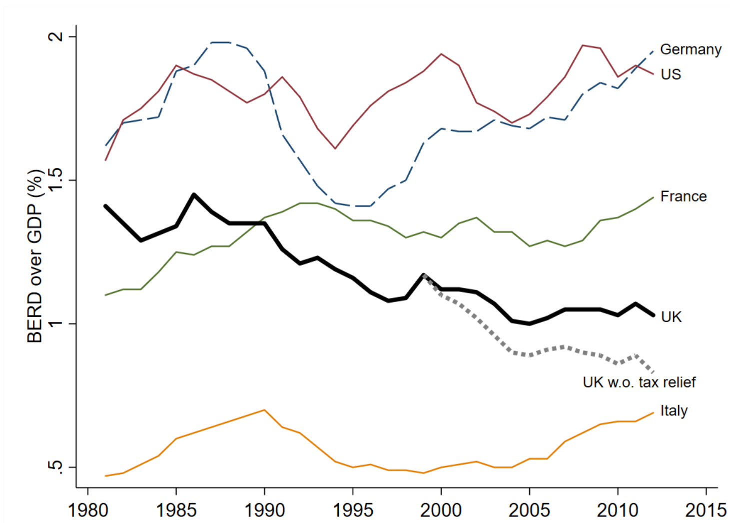
A. Pre- vs. post-policy spillover effects



B. Spillovers by sample bandwidth

Notes: This figure presents evidence of technology spillovers on patents of technologically connected firms in small technology classes (i.e., those with no more than 200 firms). **Panel A** plots the annual discontinuity in technologically connected firms' patents at the SME assets threshold, relative to that discontinuity in 2007. These discontinuities are estimated using a year-specific coefficient on the spillover Diff-in-Disc estimator in equation (7) (see Appendix C.2 for details). **Panel B** plots the estimated spillover effect on technologically connected firms' patents in various samples of spillover-generating firms around the SME assets threshold of €86m (the X-axis variable). These coefficients are estimated using equation (6) over 2006-13 (with weights for larger bandwidths). The red dot corresponds to the baseline sample (i.e., firms within a €25m bandwidth of the threshold), as reported in column (4) of Table 5. In both panels, standard errors are clustered by primary technology class. The grey lines indicate 90% confidence intervals of the estimates.

Figure 7: Evolution of Business Enterprise R&D (BERD) over GDP



Notes: The data is from OECD MSTI downloaded February 9th, 2016. The dotted line ("UK without tax relief") is the counterfactual R&D intensity in the UK that we estimate in the absence of the R&D Tax Relief Scheme (see subsection 5.3 and Appendix F.3 for details).

Table 1: Baseline Sample Descriptive Statistics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Sample:	**Full baseline sample**		**Firms below threshold**		**Firms above threshold**		**Difference**	
Period:	Pre-policy	Post-policy	Pre-policy	Post-policy	Pre-policy	Post-policy	Pre-policy	Post-policy
Number of firms in patent sample	5,744	5,744	3,485	3,485	2,259	2,259	1,226	1,226
Number of patenting firms	171	185	102	118	69	67	33	51
Mean annual patent count	0.066	0.054	0.062	0.060	0.071	0.044	-0.010	0.016
Number of firms in R&D sample	5,888	5,888	3,561	3,561	2,327	2,327	1,234	1,234
Number of R&D performing firms	259	329	160	210	99	119	61	91
Mean annual R&D expenditure (£)	73,977	88,825	61,030	80,269	93,788	101,917	-32,758	-21,649

Notes: Baseline sample includes firms with total assets in 2007 within €25m of the threshold (i.e., between €61m and €111m). Firms below SME assets threshold include those with total assets in 2007 between €61m and €86m. Firms above SME assets threshold include those with total assets in 2007 between €86m and €111m. Columns (7) and (8) report the respective differences between firms below and above the threshold in pre- and post-policy periods. Specifically, *column* (7) = *column* (3) − *column* (5) and *column* (8) = *column* (4) − *column* (6). Pre-policy period is 2006-08. Post-policy period for patents is 2009-13, and for R&D is 2009-11. Total assets are from FAME and are converted to € from £ using HMRC rules. Patent counts, reported for the baseline out-of-lab sample, come from PATSTAT. Qualifying R&D expenditure, reported for the in-lab sample, comes from CT600 panel dataset and are converted to 2007 prices.

Table 2: Balance of Predetermined Covariates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dependent variable:	**ln(Sales)**		**ln(Employment)**		**ln(Capital)**		**R&D exp.**		**Patent count**	
Year:	2006	2007	2006	2007	2006	2007	2006	2007	2006	2007
Below-threshold indicator	-0.124	0.086	0.117	0.157	0.023	-0.006	43.4	81.9	-0.011	0.028
	(0.162)	(0.161)	(0.135)	(0.131)	(0.112)	(0.103)	(50.6)	(59.2)	(0.035)	(0.035)
Number of firms	4,155	4,348	2,973	3,089	4,766	5,078	5,888	5,888	5,744	5,744

Notes: Differences in firms below and above the SME assets threshold in pre-policy years are estimated using the RD Design analogous to equation (1). The running variable is total assets in 2007 with a threshold of €86m. Baseline sample includes firms with total assets in 2007 within €25m of the threshold (i.e., between €61m and €111m). Controls include first order polynomials of the running variable separately for each side of the threshold. Columns (1) and (2) report pre-treatment covariate tests for sales (from CT600); columns (3) and (4) employment (from FAME); and columns (5) and (6) fixed assets (from FAME). Columns (7) and (8) report pre-treatment tests for R&D expenditure (from CT600) and columns (9) and (10) patent count (from PATSTAT). Robust standard errors are in brackets.

*** denotes statistical significance at 1% level, ** 5% level, * 10% level.

Table 3: Tax Policy Effects on Innovation

Panel A. Policy effects on patents using RD, Diff-in-Disc, and Diff-in-Diff specifications

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dependent variable:	**Patent count**								
Specification:	RD			Diff-in-Disc			Diff-in-Diff		
Period:	09-11 avg.	09-13 avg.	09-15 avg.	06-13	02-15	06-13	06-13	02-15	06-13
Below-threshold indicator	0.060***	0.052***	0.048***						
	(0.022)	(0.019)	(0.018)						
Below-threshold × Post-2008				0.045**	0.042*	0.052***	0.026**	0.026**	0.047**
				(0.022)	(0.024)	(0.019)	(0.012)	(0.012)	(0.022)
Augmentation						*Dynamic*			*With break*
Observations	5,744	5,744	5,744	45,952	80,416	45,952	45,952	80,416	45,952
Number of firms	5,744	5,744	5,744	5,744	5,744	5,744	5,744	5,744	5,744

Panel B. Policy effects on R&D (and robustness of Panel A's results on patents)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Sample bandwidth:	Firms with 2007 assets ∈ [€61m-€111m]			Firms with 2007 assets ∈ [€51m-€121m]					
Dependent variable:	**R&D exp.**	**Patent count**		**Patent count**					**R&D exp.**
Specification:	RD	RD IV		RD	Diff-in-Disc		Diff-in-Diff		RD
Period:	09-11 avg.	09-13 avg.	09-13 avg.	09-13 avg.	06-13	06-13	06-13	06-13	09-11 avg.
Below-threshold indicator	63.4**			0.047***					58.8*
	(32.1)			(0.017)					(33.5)
Below-threshold × Post-2008					0.043**	0.047***	0.026**	0.044**	
					(0.019)	(0.017)	(0.011)	(0.019)	
R&D expenditure, 09-11 avg.		0.563**	0.434*						
		(0.282)	(0.243)						
Anderson-Rubin test p-value		0.008	0.012						
Augmentation						*Dynamic*		*With break*	
Observations	5,888	5,888	5,888	8,577	68,616	68,616	68,616	68,616	8,818
Number of firms	5,888	5,888	5,888	8,577	8,577	8,577	8,577	8,577	8,818

Notes: RD Design is based on equation (1). Diff-in-Disc Design is based on equation (3). Diff-in-Diff Design is based on equation (2). RD IV Design is based on equation (4). Baseline sample includes firms with total assets in 2007 within €25m of the threshold (i.e., between €61m and €111m) unless noted otherwise. The running variable is total assets in 2007 with a threshold of €86m. Controls in RD and RD IV Designs include (i) first order polynomials of the running variable separately for each side of the threshold, and (ii) 2006-08 (pre-policy) average of the dependent variable. Controls in the Diff-in-Disc Design include (i) first order polynomials of the running variable separately for each side of the threshold and pre- and post-policy periods, and (ii) firm and year fixed effects. Controls in the Diff-in-Diff Design include firm and year fixed effects. Columns (6) in Panels A and B (indicated by *"Augmentation = Dynamic"*) employ the Dynamic Diff-in-Disc Design, which additionally includes a lagged dependent variable with differential effects in pre- and post-policy periods. Column (9) in Panel A and column (8) in Panel B (indicated by *"Augmentation = With break"*) additionally control for firm size separately for before and after the Global Financial Crisis. Standard errors are clustered by firm. The units for R&D expenditure as the dependent variable in columns (1) and (9) in Panel B are £ thousand. The units for R&D expenditure as the explanatory variable in columns (2) and (3) in Panel B are £ million. Both are in 2007 prices.
*** denotes statistical significance at 1% level, ** 5% level, * 10% level.

Table 4: Tax Policy Effects on Quality-Adjusted Patents

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent variable:	**Quality-weighted patent count (2009-13 average)**							
Quality measure:	Baseline	EPO, US, or Japan	Family-weighted	Granted patents	Scope top quartile	Originality top quartile	Citation top quartile	Non-ICT patents
Below-threshold indicator	0.052***	0.037*	0.104*	0.023***	0.035***	0.039***	0.021**	0.034***
	(0.019)	(0.022)	(0.054)	(0.009)	(0.012)	(0.014)	(0.010)	(0.012)
Dependent variable mean, 2006-08 average	*0.066*	*0.199*	*0.060*	*0.041*	*0.042*	*0.044*	*0.046*	*0.055*
Normalized effect size (estimate over mean)	1.26	1.62	1.92	1.81	1.19	1.14	2.25	1.63
Number of firms	5,744	5,744	5,744	5,744	5,744	5,744	5,744	5,744

Notes: RD estimates are based on equation (1). The running variable is total assets in 2007 with a threshold of €86m. Baseline sample includes firms with total assets in 2007 within €25m of the threshold (i.e., between €61m and €111m). Controls include (i) first order polynomials of the running variable separately for each side of the threshold, and (ii) 2006-08 (pre-policy) average of the dependent variable. Quality measures are baseline patent family count (column (1)); EPO, US, or Japan patents (column (2)); patent by family size count (i.e., patent by country count) (column (3)); granted patents (column (4)); patents in top patent scope quartile (column (5)); patent in top originality quartile (column (6)); patents in top citation quartile (column (7)); and non-ICT patents (column (8)). Patent quality quartiles are determined relative to the patent's technology class by application year cohort. Information and communication technology (ICT) patents include all patents classified into either patent field (4) Digital communication, (6) Computer technology, or (7) IT methods for management. Robust standard errors are in brackets.
*** denotes statistical significance at 1% level, ** 5% level, * 10% level.

Table 5: Spillovers of Tax Policies on the Innovation of Technologically Connected Firms

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dependent variable:	**Firm j's patent count (2009-13 avg. or annual over 2006-13)**								
Sample bandwidth:	Firm i's 2007 assets ∈ [€61m-€111m]						2007 assets ∈ [€51m-€121m]		
Specification:	RD				Diff-in-Disc	RD IV	RD		Diff-in-Disc
Technology class size:	All	All	Large	Small	Small	Small	All	Small	Small
Firm i's below-threshold indicator	-0.001	0.229***	-0.004	0.085**			0.137*	0.056**	
	(0.009)	(0.084)	(0.009)	(0.033)			(0.075)	(0.027)	
Firm i's below-threshold × Technology class size		-0.244***					-0.147*		
		(0.088)					(0.078)		
Firm i's below-threshold × Post-2008					0.082**				0.057*
					(0.037)				(0.029)
Firm i's R&D expenditure, 2009-11 average						0.222**			
						(0.110)			
Difference between columns' estimates			0.090**						
			(0.035)						
Anderson-Rubin test p-value						0.036			
Dependent variable mean, 2006-08 average	*0.349*	*0.349*	*0.352*	*0.286*	*0.286*	*0.291*	*0.342*	*0.287*	*0.287*
Observations	156,908	156,908	150,205	6,703	53,624	2,093	245,137	10,640	85,120
Number of firm i-j pairs	156,908	156,908	150,205	6,703	6,703	2,093	245,137	10,640	10,640
Number of connected firm j's	15,685	15,685	12,708	2,977	2,977	1,190	16,552	3,610	3,610
Number of treated firm i's	517	517	371	146	146	67	773	227	227
Number of 3-digit IPC classes	83	83	28	55	55	36	95	67	67

Notes: Spillover RD Design is based on equation (6). Spillover Diff-in-Disc Design is based on equation (7). Spillover RD IV Design is based on equation (5). Each observation is a pair of a treated firm i and a technologically connected firm j. The running variable is treated firm i's total assets in 2007 with a threshold of €86m. Controls in the spillover RD Design include (i) first order polynomials of the running variable separately for each side of the threshold, (ii) second order polynomial of connected firm j's total assets in 2007, and (iii) firm j's 2006-08 (pre-policy) average patent count. Controls in the spillover Diff-in-Disc Design include (i) first order polynomials of the running variable separately for each side of the threshold and pre- and post-policy periods, and (ii) firm j and year fixed effects. Column (6)'s spillover RD IV Design controls for (i) first order polynomials of the running variable separately for each side of the threshold, and (ii) second order polynomial of firm j's total assets in 2007, using similarly-constructed in-lab sample. Technology class size is the number of firms whose primary technology class is the said class, converted to percentile and normalized to be between 0 and 1. Small (large) technology class subsample includes firms whose primary technology classes have below (above) 200 firms, which corresponds to the 70^{th} percentile as guided by Figure 5. "Difference" is the test of whether the coefficients of interest are statistically different between columns (3) and (4). Standard errors in brackets are clustered by primary technology class. The units for R&D expenditure as the explanatory variable in column (9) are £ million in 2007 prices.
*** denotes statistical significance at 1% level, ** 5% level, * 10% level.

Table 6: Being Below the Assets Threshold as a Predictor for SME Eligibility

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dependent variable:	**Indicator: Has R&D claims under SME Scheme**								
Sample bandwidth:	Firms with 2007 assets ∈ [€61m-€111m]						2007 assets ∈ [€51m-€121m]		
Period:	2009	2010	2011	2008-09	2008-11	2009-11	2008-09	2008-11	2009-11
Below-threshold indicator	0.326***	0.301***	0.184*	0.464***	0.353***	0.248***	0.427***	0.345***	0.271***
	(0.085)	(0.089)	(0.100)	(0.087)	(0.090)	(0.093)	(0.079)	(0.082)	(0.085)
Number of firms	215	218	248	265	361	333	407	555	520

Notes: Sharpness of the below-assets-threshold indicator as predictor for firm's post-policy SME status is estimated using the RD Design in equation (8). The running variable is total assets in 2007 with a threshold of €86m. Baseline sample includes firms with total assets in 2007 within €25m of the threshold (i.e., between €61m and €111m) unless noted otherwise. Controls include first order polynomials of the running variable separately for each side of the threshold. The sample for a certain year (period) effectively includes firms in the baseline sample with R&D tax relief claims in that year (period). A firm's SME status over a period is the maximum of its SME status in each of the year within the period. Robust standard errors are in brackets.
*** denotes statistical significance at 1% level, ** 5% level, * 10% level.

Table 7: Larger Tax Policy Effects on Innovation for More Financially Constrained Firms

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Sample bandwidth:	Firms with 2007 assets ∈ [€61m-€111m]					2007 assets ∈ [€51m-€121m]	
Dependent variable:	**Patent count**				**R&D exp.**	**Patent count**	
Specification:	RD		Diff-in-Disc		RD	RD	Diff-in-Disc
Period:	09-13 avg.	09-13 avg.	06-13	06-13	06-11 avg.	09-13 avg.	06-13
Below-threshold × D: Low Cash/K	0.113***				129.5**	0.100***	
	(0.036)				(65.1)	(0.033)	
Below-threshold × D: High Cash/K	0.001				9.7	0.002	
	(0.018)				(15.1)	(0.015)	
Below-threshold × Cash/K measure		-0.027**					
		(0.012)					
Below-threshold × Post-2008 × D: Low Cash/K			0.096**				0.087**
			(0.041)				(0.037)
Below-threshold × Post-2008 × D: High Cash/K			0.004				0.008
			(0.026)				(0.020)
Below-threshold × Post-2008 × Cash/K measure				-0.020*			
				(0.012)			
Difference	0.112***		0.091*		119.7*	0.097***	0.080*
	(0.040)		(0.049)		(66.8)	(0.035)	(0.042)
Observations	5,285	5,285	42,280	42,280	4,504	7,907	63,256
Number of firms	5,285	5,285	5,285	5,285	4,504	7,907	7,907

Notes: RD Design is based on equation (1). Diff-in-Disc Design is based on equation (3). The running variable is total assets in 2007 with a threshold of €86m. Baseline sample includes firms with total assets in 2007 within €25m of the threshold (i.e., between €61m and €111m) unless noted otherwise. Controls in the RD Design include (i) first order polynomials of the running variable separately for each side of the threshold, and (ii) 2006-08 (pre-policy) average of patent count. Controls in the Diff-in-Disc Design include (i) first order polynomials of the running variable separately for each side of the threshold and pre- and post-policy period, and (ii) firm and year fixed effects. Cash/K is calculated as the three-digit SIC industry average of firms' cash and cash equivalents as the share of capital using UK firm data over 2000-05. Low (high) Cash/K subsample includes firms with below (above) median industry Cash/K measure. "Difference" is the test of whether the two coefficients of interest reported in the corresponding column are statistically different. Standard errors are clustered by firm. The units for R&D expenditure as the dependent variable in column (5) are £ thousand in 2007 prices.
*** denotes statistical significance at 1% level, ** 5% level, * 10% level.

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