The unintended consequences of R&D tax credits: exploitation, markups, and technological entrenchment

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Abstract:
Research and development (R&D) tax credit policies often aim to increase investment in research and experimentation, with the hope that firms invent new technologies that generate positive spillovers. Since most policy designs require that companies make profits to access a tax credit, they could also shift firms' investments towards less risky refinement and exploitation of previously patented technologies. We use California's 1987 tax credit change and strengthen identification using same firm but different state inventor location to illustrate such a shift within treated firms at their California locations. The shift towards refinement is stronger for firms operating in uncertain markets. Firms take advantage of tax credits by deepening invention in areas of their previous patenting activity, and this refinement precedes increased valuation and markups. Tax credits have strategic and competitive consequences, increasing treated firms' market valuation, as well as negative spillovers, e.g., competitors' valuation and future patenting success wanes. The results hold across a broad array of measures, robustness checks, and tax credit changes in other states.

Keywords: Innovation, Exploration, Exploitation, Markups, R&D tax credits, Patents

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1. Introduction

Research and development (R&D) tax credits typically aim to increase corporate R&D spending, arguably because firms cannot fully appropriate the returns to such spending and hence underinvest relative to the socially desirable level (Arrow, 1962; OECD, 2014; Becker, 2015; Bloom, Van Reenen, Williams, 2019). Much work has established that this intent usually succeeds. While early empirical studies provided relatively pessimistic estimations of the impact of tax credits on spending (Altschuler, 1988), more recent research has fairly implied elasticities around unity or higher (Hall, 1993; Hall and Van Reenen, 2000; Bloom et al., 2002; Dechezleprête et al., 2019). Hence, there is a consensus that a dollar in lost tax revenue probably results in a dollar or more of increased R&D spending (Bloom, van Reenen, and Williams, 2019).

The original question raised by Hall (1993, see also Chen et al., 2019) remains pertinent, however, "…of whether this R&D spending truly reflects increased spending of the sort envisioned by Congress (research and experimentation in the laboratory or technological sense), or merely a relabeling of related expenses as research, and an increase in such expenses as new-product-related market research, etc." Policy makers often hope that firms engage in basic research and enter new technological areas because such efforts generate greater positive externalities, typically in the form of knowledge spillovers (Akcigit and Kerr, 2018; Akcigit, Hanley, Serrano-Velarde, 2020). Yet, despite the consensus that R&D tax credits increase R&D and patenting, it remains to be established how credits (perhaps unintentionally) influence innovative search strategies (i.e., what gets invented and patented, in particular, research vs. applications) and how such strategies influence performance and competition.

When firms choose their innovation strategy in the presence of a tax credit, theory suggests they should be more likely to choose exploitation of previously successful technologies, because tax credits become less attractive in the absence of success and profits (Hall, 2019). This effect should be stronger for firms facing greater profit uncertainty.[1] We find support for these predictions using the 1987 introduction of Californian tax credits as a quasi-natural experiment, most convincingly *within* firms whose inventors locate both inside and outside of the state.

This paper develops a simple model (detailed in the appendix) which motivates how tax credits become less attractive without profits, inducing a shift away from riskier exploration and towards

[1]The mechanism does not apply to some tax credit schemes that offer refundable credits for loss-making firms.

exploitation of a firm's current technological expertise. While we confirm prior results that credits increase R&D and total patenting, we also find significant shifts in the proportion of patents that rely on technologies previously known to the firm (as opposed to patents in technologies that are new to the firm). Further, consistent with our proposed theoretical mechanism, this shift is greater for firms operating in uncertain markets, in industries with a higher risk of obtaining non-positive profits and in R&D intensive industries. The lower fraction of patents in new technology classes induces a number of strategic and competitive consequences; for firms affected by the California tax credit, it precedes an increase in average markups (estimated using the methodology proposed by De Loecker, Eeckhout and Unger, 2020) and an increase in market valuation (Kogan et al., 2017; Fitzgerald et al., 2020), and for the treated firms' competitors, it precedes a decrease in market valuation (Kurakina, 2021) and an increase in blocked patent applications (Lueck, Balsmeier and Fleming, 2020). The results hold across a broad array of robustness checks and other tax credits besides the 1987 California change. They are robust towards using a control group constructed using coarsened exact matching, limiting the sample to firms with inventors inside and outside of California or considering only firms outside of the IT sector. We also obtain qualitatively similar results for estimations that do not rely solely on patent measures, for example, the ratio of patents to scientific publications (Arora, Belonzon and Sheer, 2020).

The rest of this paper is organized as follows. In section 2, we discuss the related literature and our proposed mechanism of how R&D tax credits affect innovative search. Section 3 details the construction of our data set and our identification strategy. Results of our empirical analyses are presented in section 4. In section 5, we discuss the implications of our findings for economic theory and policy, section 6 concludes.

2. Why and how R&D tax credits might impact value creation and capture

The theoretical motivation for R&D tax credits emerged from Arrow (1962); firms cannot bear the entire risk of invention, are unlikely to succeed in appropriating all the benefits, and this remains especially true for basic research and knowledge production, which is often seen as the most valuable investment to society. Arguments that emphasize basic research rest on the assumption that it generates the most valuable positive externalities to other inventors, mainly through knowledge spillovers and diffusion (Nelson, 1959; Griliches, 1992; Hall, 1996; Mohnen, 1996; Hall and Wosinka, 1999). Policy makers hope that lower costs of R&D induce firms to conduct basic research, search for novel technologies, and discover breakthroughs – some of which will leak and be taken up by other firms, including competitors. The expected spillovers justify the subsidies.

R&D tax credits influence a range of decisions by private firms (Akcigit and Stancheva, 2020) and many have commented that firms will use credits to maximize the private returns to their R&D investment (Hall, 1993; Hall and Van Reenen, 2000; Bloom, Van Reenen, Williams, 2019). Thus, tax credits might not induce the desired investments in or outputs of fundamental knowledge, novelty, and desired spillovers, even though exploration of new technologies – "external effort" (Akcigit and Kerr, 2018) or "horizontal innovation" (Pless, 2019) – has been argued to provide approximately 80% of economic growth (Akcigit and Kerr, 2018).

Realistically, we would only expect fundamental research in new technologies if firms cannot generate higher returns per R&D dollar by exploiting extant technologies. In contrast to other mechanisms intended to generate positive externalities, for example, scientific funding programs, R&D tax credits are explicitly intended to avoid discriminating between types of inventions, in part to keep costs of administration low.[2] In the absence of refundable credits that allow loss-makers to benefit, firms need to generate a profit to take advantage of the tax credit. This may be difficult, particularly for those firms operating in a highly uncertain environment (Agrawal, Rosell and Simcoe, 2020). The interaction of tax credits and the ability to take advantage of such credits could shift R&D investment in ways that subsequently shift a firm's technological portfolio and market power.

Evidence on the effect of tax credits

Accurately assessing the impacts and foregone opportunities of tax credits presents many challenges (Hall, 1993; Akcigit and Stancheva, 2020); partly as a result, much empirical work compares incremental R&D investment to lost taxes. Hall and Van Reenen's (2000) survey reports that credits typically demonstrate an elasticity around or greater than one with respect to R&D, with some time lag, presumably due to firms' adjustment costs, e.g., on-boarding of technical professionals (Bloom et al., 2002 estimate similar long-run elasticities in OECD economies);

[2] Some R&D tax credit designs were meant to avoid funding of incremental inventions by limiting the tax credit to R&D increases beyond prior threshold levels, i.e., only big increases of R&D would be subsidized. However, there was no rule saying that increases of R&D expenditures actually need to be directed towards radical inventions.

subsequent work uses a variety of methods and samples to estimate elasticities ranging from 1.3 to 2.6 (Bronzini and Iachini, 2014; Dechezleprêtre et al., 2019; Guceri and Liu, 2019). Chen et al. (2019) estimates 1.3, for real R&D investment, as opposed to relabeling, for Chinese firms. Pless (2019) shows that tax credits and subsidies are complements for smaller and substitutes for larger UK firms, and that small firms invest in product, rather than process, innovations. Agrawal, Rosell and Simcoe (2020) demonstrate an increase in R&D by small Canadian firms.

Other work estimates the impact of credits on innovation outcomes, with ambiguous results. Czarnitzki, Hanel, and Rosa (2011) report increased product innovation amongst Canadian manufacturers, though Cappelen, Raknerud, and Rybalka (2012) find only increased process innovation for Norwegian firms. Bérubé and Mohnen (2009) report positive effects on R&D spending outside the U.S. but weaker impact on patenting. Dechezleprêtre et al. (2019) identify private value creation (as measured by patents) and positive externalities such as spillovers (as measured by future prior art citations) in small U.K. firms.

Recent work has begun to explicitly investigate the possibility of negative externalities of tax credits. Wilson (2009) illustrates a redistribution of R&D spending from unaffected to tax credit regions, such that the net effect for the entire U.S. economy might be zero. Structural models consider application and process costs including the shadow costs of public funds (Takalo, Tanayama, Toivanen, 2013, 2014 and 2017), windfall gains (González, Jamandreu and Pazó, 2005), or negative externalities on entry (Acemoglu et al., 2018). A few studies consider the interplay of tax credits with the patent system and highlight how firms might use tax credits for strategic purposes, for example, to raise rivals' costs (Salop and Scheffman, 1983; Shleifer and Vishny, 1989) or steal business from competitors (Bloom et al., 2013).

How do R&D tax credits affect innovative search?

Assume that firms can allocate their R&D budget across projects that exploit known areas of competency or explore areas that are new to the firm. Exploitation focuses on improving existing products and technologies and yields a rather low but positive expected return on investment with relatively low risk. In contrast, exploration has a higher variance of returns. If successful, exploration can yield a higher return than exploitation but also implies a higher risk of failure – in which case the firm does not make any profit from innovating.

R&D tax credits can usually only be fully used in the current time period if profits net of R&D are larger or equal to R&D expenditures. This implies that tax credits provide incentives to engage in activities with a lower risk of making zero or negative profits, for example, exploitation strategies. The same argument holds for basic (science-based) vs. applied research and for radical vs. incremental innovation. To inform these intuitions, we incorporate tax credits into a classic model of innovative search (March, 1991; Manso, 2011) in Appendix C.

It has to be noted that in the absence of profits, tax credits can be carried forward up to 20 years to offset taxes on future profits. However, as long as firms discount future payoffs, and in particular, if the commercial viability of the firm remains less clear, R&D tax credits still provide incentives to focus on exploitation rather than exploration. This effect will be amplified in the plausible case that exploration strategies have a longer time lag between investment and return than exploitation strategies. This model assumes that the firm operates in a jurisdiction that lacks refundable tax credits (which is the case for our empirical context, i.e., the state of California).

Some firms might generate stable positive profits independent of their innovation activities. The mechanism described above is therefore more relevant for firms whose profits rely heavily on innovation or who are faced by higher market uncertainty. In the empirical analysis, we show that the effects of R&D tax credits are indeed concentrated among firms with high profit uncertainty, measured by the standard deviation of profits divided by the absolute amount of profits, in pre-tax periods. We also estimate larger effects in industries where the probability of generating non-positive profits is higher and when investment in R&D is large relative to firms' revenue.

3. Data and methodological remarks

Patents and firm-level data

The main part of our empirical analysis is focused on all public US based firms that filed at least one patent between 1977 through 2006 (analogous data on smaller and private firms do not exist, thus limited generalizability of the current study). Using this sample as a baseline, we supplement it with patent data from PATSTAT (e.g., Balsmeier et al., 2018; Rassenfosse, Dernis, and Boedtstock, 2014), stock market data (Kogan et al., 2017), and accounting data from Compustat. Measures are aggregated at the firm level based on the application year of a patent. Due to the need for patent-based measures, the main sample comprises only firms that applied for at least one patent over the whole sample period. Our identification strategy, which relies on changes in state tax law, further limits the sample to firms with U.S. state headquarter location information. In order to limit selection in and out of the main sample we require firms to be observed at least twice. Table 1 and Appendices provide summary statistics and variable definitions.

Table 1 – Summary statistics (1977–1997, California plus control states, firms with patents)

Variable	N	Mean	Median	Sd	Min	Max
Eff. R&D Tax Credit %	22257	0.73	0	2.2	0	10.03
R&D	22257	0.25	0	1.86	0	56.72
Patents	22257	11.09	0	65.49	0	3638
Patents Known Tech	22257	10.01	0	64.09	0	3618
Patents New Tech	22257	1.08	0	2.68	0	53
Fraction Known Tech	22257	0.25	0	0.39	0	1
Markup	21759	1.33	1.28	0.34	1	8.01
Stock Market Value	22257	174.32	0	1882.1	0	126274.2
SM Value New Tech	22257	12.59	0	94.39	0	5138.08
SM Value Known Tech	22257	161.73	0	1831.19	0	124154.3
Fraction Value Known Tech	22257	0.24	0	0.39	0	1
Blocked EPO Patents	22257	3.54	0	24.08	0	1530
Strategic Patents	22257	0.32	0	2.25	0	68

Notes: This table reports summary statistics of the variables used in the study. Eff. R&D Tax Credit is the effective R&D tax credit that firms could maximally receive as calculated by Wilson (2009). The nominal rate was 8% since 1987. Patents is the total number of eventually granted patents applied for in a given year. Patents Known Tech is the number of patents that are filed in a 3-digit technology classes where the given firm has filed within the last 5 years in that class. Patents New Tech is the number of patents that are filed in a 3-digit technology classes where the given firm has not filed within the last 5 years in that class (note that this variable measures new to the firm technologies and not necessarily new to the world technologies). Stock Market Value is the total private value of patents applied in year t, measured as the sum of all market reactions to publications of these patents (data from Kogan et al., 2017). SM Value New Tech is the total private value of patents filed in a 3-digit technology classes where the given firm has not filed within the last 5 years in that class, measured as the sum of all market reactions to publications of these patents. SM Value Known Tech is the total private value of patents filed in a 3-digit technology class where the given firm has filed within the last 5 years in that class, measured as the sum of all market reactions to publications of these patents. % Value New Tech is the proportion of the latter two variables in percent. Blocked EPO Patents is the total number of blocked patent applications at the European Patent Office (EPO), defined as patent applications that were eventually denied by the EPO and referred to at least one US patent of the focal firm applied in year t, which was classified as potentially blocking (X or Y citations in the EPO examiner search report). Strategic Patents is the total number of patents that fall into the top 10% of the stock market value reactions in a given year but not into the top 10% of future citations.

In our empirical analysis, we consider four different samples: 1) patenting firms "treated" by the California tax credit change, relative to all patenting firms that were not affected by a tax credit introduction, 2) patenting firms "treated" by the California tax credit change, relative to matched firms that were not affected by an introduction, 3) inventors of patenting firms "treated" by the California tax credit change, relative to the same firm's inventors at branches outside of California that were not affected by a tax credit (enabled by availability of city and state of residence of inventors), and 4) U.S. patenting firms "treated" by a series of changing state-level tax credit laws. The results remain robust to excluding firms active in SIC codes 357 'Computer and Office

Equipment', 366 'Communications Equipment', and 367 'Electronic Components' (Appendix A, Table A6).

While much of the empirical literature on innovation relies on patent data, it is worth acknowledging that studies using patent data implicitly assume that innovation output results in patents and knowledge is not protected via trade secrets. Some bias is possible as well if riskier projects do not result in any patents at all. However, since we are mainly interested in the *type* of innovation induced by R&D tax credits, it is most important in our context that tax credits do not change the incentives to protect knowledge via trade secrets relative to patenting.

R&D tax credits

Between 1980 and 2006, 32 U.S. states introduced R&D tax credits. Because aggregated analyses of law changes in a diff-in-diff type of setup may create severe biases (Goodman-Bacon, 2018; Chaisemartin and D'Haultfoeuille, 2019), we focus on the case of California before broadening the analysis to other states. California firms provide a particular interesting case; first, because they constitute the largest part of patenting activity within the country, private R&D played a crucial role in their widely acknowledged extraordinary growth, and tax credits have been underappreciated as a potentially important fuel of that growth. Second, although there are similar treated firms in different states and time, they are often not comparable in terms of how the credit interacts with other tax laws, which expenses actually qualify for the credit and which firms fulfill the eligibility criteria (Hall and Wosinska, 1999). Due to these differences across states, estimating an average treatment effect using R&D tax credits across all states and time periods remains problematic (see Lerner and Seru, 2017, for examples of unrevealed heterogeneous impacts of state law changes on innovation in the recent literature).[3] Third, the composition of the treated as well as untreated firms changes greatly over time, such that it is difficult to define a common control group that suits each state's R&D tax credit introduction. Fourth, the California R&D tax credit was one of the first significant provisions and not of temporary nature, while especially in later years, firms outside California may have anticipated further changes in R&D tax credit provisions such that the estimated effects might be confounded (Rao, 2016). These arguments

[3] We also estimate heterogeneous impacts of R&D tax credit introductions across states and time, as further discussed below.

notwithstanding, we illustrate qualitatively similar, though quantitatively weaker, effects in the Appendix (Table A2) for the effects of subsequent tax credits in states other than California.

The nominal R&D tax credit introduced by California in 1987 was 8%, before it was raised to 11% in 1997. To identify the impact of California's policy change, we restrict the sample to 10 years before and 10 years after the tax credit introduction and take all firms situated in states that had not introduced any R&D tax credit in the sampling period as the control group. Thus, we also avoid the confounding effect of California's introduction of an alternative incremental R&D tax credit in 1997. Consistent with our modeling strategies, we remove firms that were only active before or only after the tax credit introduction to limit potential influences from self-selection into or out of the sample.

The effective R&D tax credit rate differs sometimes from the nominal rate because of the interplay of R&D tax credits with other investment and federal taxes.[4] In the main part of our study, we focus on the effective rate because it is the source of exogenous variation that drives the actual R&D costs of the affected firms. Earlier literature often took the user costs of R&D as their main explanatory variable that takes alternative investment opportunities and interest rates into account, as introduced by Hall and Jorgenson (1967). While this should reflect actual R&D costs more accurately, it incorporates calculations that are predictable by firms, such as interest rates, thereby creating endogeneity concerns (Agrawal, Rosell, and Simcoe, 2020; Bloom et al., 2002). However, by focusing on the effective rates and the effect of the exact timing of the R&D tax credit introduction, we base our identification only on variation that is actually caused by the policy change (not interest rates or other extant corporate taxes). This reduces potential endogeneity biases and allows us to more accurately relate our findings to the actual introduction of R&D tax credits. For easier comparisons with the literature, we also provide estimations based on the user costs of R&D calculated according to Hall and Jorgenson (1967) in the Appendices.[5] For brevity, the body of the paper only presents the estimated coefficients for the effective R&D tax credit rate.

Empirical models

We estimate variations of the following specification using OLS:

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[4] The details are explained in Wilson (2009), or specifically for California in Hall and Wosinska (1999). The results are qualitatively the same if we take the nominal rate instead of the effective rate as shown in the Appendix, Part A.
[5] We also find similar results based on alternative R&D tax credit rate calculations that exploit the initial distribution of inventors across states within the same firm (Bloom, Van Reenen, Schankerman, 2013; Babina and Howell, 2018).

$$Y_{it} = \beta \cdot R\&D\, Tax\, Rate_{it-3} + \delta_t + f_i + \varepsilon_{it} \quad (1)$$

where Y_{it} stands for our various dependent variables observed for firm i at time t, $R\&D\ Tax\ Rate$ is the effective R&D tax credit rate three years before Y_{it} is observed, δ_t denotes a full set of year fixed effects to control for varying macroeconomic conditions, f_i controls for time-invariant unobserved firm and state characteristics that may confound our identification of β, and ε_{it} is the error term.[6]

To explore potential adjustments of firms to R&D tax credit provision over time (Hall, 1993), and test for pre-treatment trends, we alternatively estimate a more flexible version where we allow the effect of the R&D tax credit introduction to vary over time. Instead of the R&D tax credit rate in (1) we include dummy variables for each of the 5 years before and 10 years after the policy change and leave the rest of the specification unchanged. The coefficients of $\beta_{-5,\dots,-1}$ serve as a placebo test on whether firms may have expected changes in R&D tax credits or systemically differ from firms situated in non-affected states before the treatment.

$$Y_{it} = \sum_{\tau=-5}^{10} \beta_\tau \cdot t_{i\tau} + \delta_t + f_i + \varepsilon_{it} \quad (2)$$

Matching methodology

In order to address concerns that differences between California firms and firms in the control group might confound our estimations, we also estimate models based on a sample of California and other firms that are comparable in observable firm characteristics and industry composition.

Before matching, California firms are on average significantly more R&D intensive, younger and smaller compared to the average firm outside of California. California firms are also overrepresented in the manufacturing sector and underrepresented in the transportation and construction sector. If R&D tax credits are more (or less) effective for firms with those characteristics or in certain sectors, our baseline estimates might not be representative for the average effects of R&D tax credits on those outcomes.

[6] As we discuss below in more detail, our results are robust towards alternative specifications resembling a classic DiD setup where the treatment indicator is a dummy instead of the effective tax rate, IV estimations where R&D user costs are used as an instrument, additional firm level controls such as R&D expenditures, firm age and total assets, and alternative lags of the tax credit rate at t-2 or t-4 (while Akcigit and Stancheva (2020) indicate a three year lag is appropriate, see Appendix A, Table A3 to A9, for robustness checks).

To balance the sample with regard to the aforementioned characteristics, we apply Coarsened Exact Matching (CEM). CEM has the advantage over classic matching procedures, like propensity score matching, to balance across the entire distribution of observables, which improves causal inference (for details and comparison see Iacus, King, and Porro, 2012, 2017; and King and Nielsen, 2017). The matching procedure identifies for each California firm the most similar firm in the control group. To maximize similarity at time of treatment we match on firms' average R&D intensity, age, and size in 1987 and 1986 plus firms' industry affiliation. The CEM algorithm identified matches for 405 out of 419 California firms. We find no statistically significant differences in terms of all matching characteristics after matching (see Appendix, Table A10).

Firm-state level analysis

We additionally address concerns about potential biases from unobserved differences in affected and control group firms by exploiting different locations of R&D labs within firms, i.e. we take the patenting activity of inventors that work in other US states than California (and hence not affected by the introduction of R&D tax credit over the whole sampling period) as the control group for inventors of the same company working inside California (front page patent data which include the inventor's home town and state enable this). Within firm changes are to be expected because in principle only R&D expenses occurred in California qualify for the California tax credit (Ibele, 2003).[7] For patent-based measures[8], we thus estimate a variant of equation (1) at the firm-lab location-year level:

$$Y_{ist} = \beta \cdot R\&D\ Tax\ Rate_{ist-3} + f_{it} + \sigma_s + \varepsilon_{ist} \qquad (3)$$

where Y_{ist} stands for dependent variables observed for firm i's labs in state s at time t, $R\&D\ Tax\ Rate_{ist-3}$ is the effective R&D tax credit rate three years before Y_{ist} is observed, f_{it} denotes firm-year fixed effects which absorb all firm-specific shocks such as changes in productivity or organization, and σ_s captures unobserved location specific differences that may confound our identification of β, and ε_{ist} is the error term.

This approach is arguably the least likely to be confounded as it solves the problem of using firms outside California as a control group. It may underestimate the corporate-level effect,

[7] Since California firms may still temporarily send inventors hired in California to help inventors at other labs outside of California, the rule is not as sharp as it appears at first sight.

[8] Unfortunately, firms' R&D expenses cannot be broken out by lab location but the spatial distribution is likely to be highly correlated with the spatial distribution of inventors.

however, since inventors in the control group might still be similarly but less directly affected as their California-based counterparts. The results based on the within firm sample will thus likely indicate a lower bound of the estimates but may also be interpreted as a within-firm shift of innovation activity. Since the location information come from the inventors on the original patent publications by the USPTO and do not rely on firm addresses, it should also alleviate concerns with respect to potentially inconsistent headquarter location data in Compustat (Atanassov and Lu, 2019) or concerns with respect to reassigning and relabeling of research activities (Chen et al., 2019).

The results presented below draw on the sample of firms that have a lab inside of California and at least one outside of California, where no R&D tax credit was introduced during the sampling period. Noteworthy, all results are robust to excluding firms with headquarters inside of California and considering only firms with headquarters outside of California.

4. Results

R&D expenses and patents

Our main hypothesis proposes that firms that can claim R&D tax credits will shift their innovation strategies towards exploitation, and that this effect will be greater for firms operating in more uncertain environments. Before testing these predictions, we confirm prior results in the tax credit literature. Since increase in R&D may need some time to be reflected in patent applications we regress our outcome variables on the effective R&D tax credit rate in t-3 (see corresponding graphs of a more flexible model). Dependent variables are all taken as log (Y +1). Table 2 shows results for the effective tax credit rate and a) R&D expenditure and b) total number of patents applied for in year t, with and without matching.

The results in columns (c) and (d) of Table 2 indicate that one percentage point increase in R&D tax credits leads on average to an increase of R&D expenditures (for the matched sample) of about 3.5% and an increase in patenting of 3.7%, three years after its introduction. The estimated effect is larger than in most previous studies, which is due to our focus on patenting firms (which is necessary for distinguishing exploration and exploitation strategies).

Table 2 – The impact of R&D tax credits on R&D and patenting

	a	b	c	d	e
	R&D	Patents	R&D	Patents	Patents
	Original sample		After matching		Firm-state level
R&D tax credit rate$_{t-3}$	5.366***	4.539***	3.427***	3.656***	5.020***
	(0.524)	(0.344)	(0.582)	(0.474)	(0.662)
N	22257	22257	12590	12590	30825
Year FE	yes	Yes	yes	yes	yes
Firm FE	yes	Yes	yes	yes	yes
Lab location FE	no	No	no	no	yes
R^2	0.892	0.809	0.882	0.783	0.2389

Notes: All dependent variables are measured in logarithmic form. R&D tax credit is the effective rate as calculated by Wilson (2009). OLS regressions, heteroscedasticity-robust standard errors are clustered at the state level. ***, **, * indicate statistical significance at the 1%, 5%, 10% level, respectively.

Figure 1 illustrates the yearly trends of the full model (2), revealing how R&D expenditures responded to the introduction of tax credits across different years. There are no significant pre-tax credit differences between affected and non-affected firms and, consistent with prior studies, we find an increasing impact over time.

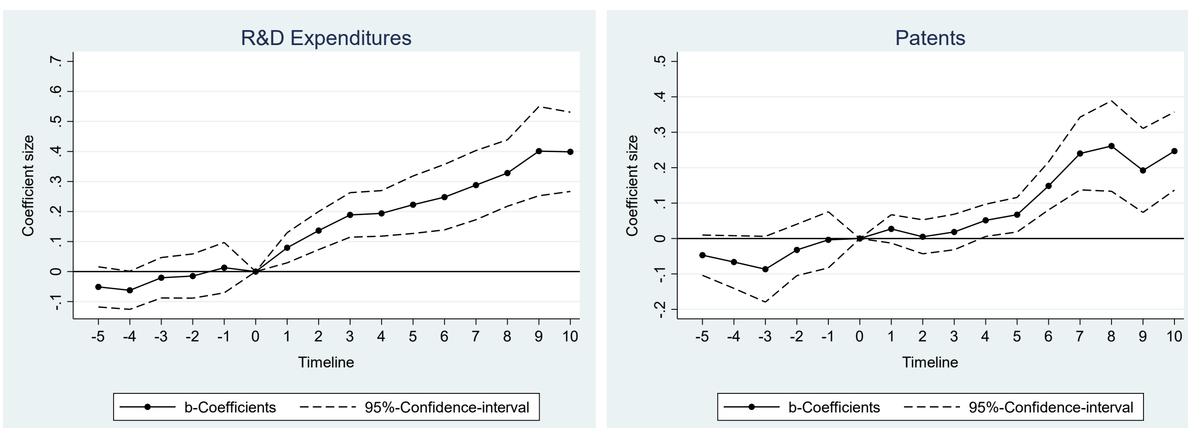

Figure 1: Yearly impact of the California tax credit of 1987.

Evidence on changes in innovative search

While tax credits appear to increase the total amount of patenting, it remains unclear whether firms shift towards more fundamental research envisaged by the Congress or whether they intensify development of their extant technologies. We empirically test heterogeneous effects in this respect by separately considering firm's patents filed in technological areas that are known to the firm as opposed to those patents that are filed in technological areas which are new to the firm. We draw on the original technological classification of each patent assigned by the USPTO. Technological areas of patenting are considered known to firm if the given firm filed at least one other patent in the 1 to 5 years prior to the observed patent in the same class.[9] Other patents are labeled as new to the firm if the firm did not patent in that technology class in years *t-1* to *t-5*.

Table 3 presents the results of separate regressions of patents in "known to the firm" technology classes (column a), new-to-the-firm technology classes (column b), and the ratio of patents in known-to-the-firm technology classes over patents in new-to-the-firm technologies (column c) for the original sample (Panel A), the matched sample (Panel B), and the firm-state sample (Panel C).[10]

All estimates indicate a strong positive response in patenting in technological areas known to the firm. Relative to other firms outside of California, we also observe a small increase in patenting in new to the firm technological areas (Panels A and B). This increase seems to be driven by labs outside of California, however, since the within-California results indicate a decrease in new to the firm patenting in California-based labs relative to the same firm's labs outside of California (Panel C). Regressions of the ratio of patenting in known to the firm technological areas (column c) all show a shift towards exploitation. Figure 2 illustrates the differential impact graphically.

These results support the argument that tax credits precede a shift in the types of patenting outcomes from new-to-the-firm technologies towards known to the firm technologies. Because tax credits increase the absolute amount of patenting, the absolute numbers of new technology patents also increase in two of the three samples. As predicted by theory, we consistently observe, however, a shift in innovative search towards exploitation, i.e. an increase in the proportion of patenting in known to the firm technologies over patenting in new to the firm technologies.

While our analysis in the main body focuses on this simple measure, an array of robustness checks in the Appendix support the result that R&D tax credits are associated with a stronger focus on exploitation, including evidence of (1) a higher proportion of patents per scientific publication (Arora, Belonzon, Sheer, 2020), (2) a declining reliance on basic research proxied by the fraction of backward citations to the non-patent (typically science) literature, (3) an increased reliance on

[9] Results are robust to considering the entire patent stock of each firm and alternative technological classifications at the CPC three-digit level.
[10] Reflecting the state-level analysis in Panel C, new-to-the-firm patents are defined as new-to-the-state-lab patents. Results look similar when we use new-to-the-entire-firm to classify exploration patents.

firms' own technologies as measured by increased fraction of self-citations, and (4) an increased preference for known technologies based on the distribution of the firm's patents in a given year, compared to the firm's extant patent portfolio, measured as the technology class-based overlap held by the same firm up to *t-1* and the patents applied in *t*, for five year windows (Fitzgerald et. al., 2020). We document these results in Table A11.

We also experimented with count data regressions for specifications using the number of patents as the dependent variables. Results documented in Table A12 in the Appendix show that we reach very similar conclusions based on models assuming a Poisson distribution.[11]

In order to test the proposed mechanism – that R&D tax credits shift innovative search to exploitation due to the risk of generating profits that remain too low to take full advantage of the tax credit – columns *d* and *e* of Table 3 show how the ratio of patents in known technology classes to patents in new technology classes responds to R&D tax credits, depending on profit uncertainty. We compute a measure of uncertainty as the standard deviation of profits divided by the absolute amount of average profits.[12] Standard deviations and average profits are measured over the period of 1977 to 1987, i.e., the years before the introduction of tax credit in California, to reduce endogeneity concerns. The baseline results indicate that one percentage point increase in the effective tax credit rate is associated with approximately 7% increase in the ratio of old to new technologies. Consistent with our proposed mechanism, this effect is significantly higher for firms that face levels of uncertainty above the median.

[11] A disadvantage of the Poisson regression models is that we either have to drop firm fixed effects or limit our sample to firms with a change in the number of patents over time. For this reason, we only use them as robustness check and prefer to work with linear models as our baseline specification. However, as Table A12 shows, count data regressions with and without firm fixed effects yield very similar results.

[12] Similar to Czarnitzki and Toole (2011) who employ a measure of uncertainty based on the standard deviation of sales per employee.

Table 3 – The impact of R&D tax credits on known vs. new to the firm technologies

Panel A: Original sample				**High uncertainty**	**Low uncertainty**
	a	b	c	d	e
	Known tech	New tech	Exploitation ratio	Exploitation ratio	Exploitation ratio
R&D tax credit $\text{rate}_{t\text{-}3}$	5.167***	1.544***	3.624***	4.170***	2.325***
	(0.355)	(0.221)	(0.280)	(0.255)	(0.347)
N	22,257	22,257	22,257	10,335	11,922
Year FE	yes	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes	yes
R^2	0.839	0.604	0.607	0.367	0.695
Panel B: Matched sample				**High uncertainty**	**Low uncertainty**
	a	b	c	d	e
	Known tech	New tech	Exploitation ratio	Exploitation ratio	Exploitation ratio
R&D tax credit $\text{rate}_{t\text{-}3}$	4.167***	1.341***	2.825***	3.608***	1.629**
	(0.466)	(0.263)	(0.391)	(0.356)	(0.616)
N	12,590	12,590	12,590	6,542	6,048
Year FE	yes	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes	yes
R^2	0.806	0.564	0.551	0.367	0.655
Panel C: Firm-state sample				**High uncertainty**	**Low uncertainty**
	a	b	c	d	e
	Known tech	New tech	Exploitation ratio	Exploitation ratio	Exploitation ratio
R&D tax credit $\text{rate}_{t\text{-}3}$	4.571***	-0.695***	5.267***	9.377***	3.604***
	(0.550)	(0.143)	(0.519)	(1.413)	(0.492)
N	25,125	25,125	25,125	5,345	19,780
Firm x Year FE	yes	yes	yes	yes	Yes
Lab location FE	yes	yes	yes	yes	Yes
R^2	0.323	0.320	0.328	0.405	0.317

Notes: All dependent variables are measured in logarithmic form. R&D tax credit is the effective rate as calculated by Wilson (2009). Patents in known technological areas is the number of patents that are filed in a 3-digit technology class where the given firm has filed beforehand in that class. Patents in new technological areas is the number of patents that are filed in a 3-digit technology class where the given firm has never filed beforehand in that class (note that this variable measures new to the firm technologies and not necessarily new to the world technologies). OLS regressions, heteroscedasticity-robust standard errors are clustered at the state level. ***, **, * indicate statistical significance at the 1%, 5%, 10% level, respectively.

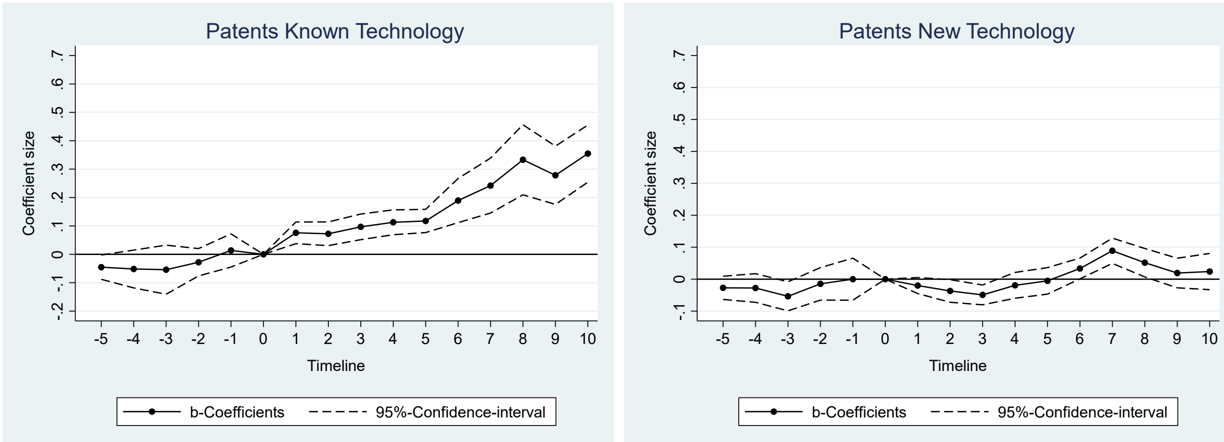

Figure 2: Yearly impact of the California tax credit on known vs. new to firm technologies

While the unconditional probability of non-positive pre-tax profits in a given firm year in our sample is about 9%, it is about 13.4% in the high-uncertainty subsample but only 3.4% in the subsample of firms that operate in industries with uncertainty below the median. To check the determinants of profit risk, we estimated Probit models in which we relate the probability of non-positive profits in a given firm-year to our high-uncertainty indicator, a dummy indicating firms with positive R&D expenditures and the *exploitation ratio*, controlling for time dummies. Results documented in Table 4 confirm that – conditional on R&D activity and the ratio of exploitation to exploration patents – the risk of no taxable profits is between 7 and 10 percentage points larger in industries with high uncertainty. Firms engaging in R&D face a risk of zero profits that is about 2.6 percentage points higher. Further, an increase in the exploitation ratio by one log point reduces the profit risk by about one percentage point. As an alternative measure of profits, we also experimented with earnings before interest and taxes (EBIT) which even yields a larger association between our variable of interests and zero profits (see columns c and d). Although the estimates do not necessarily have a causal interpretation, the results are consistent with our proposed mechanism. The risk of zero or negative profits depends on R&D activity and is higher in more uncertain environments; and firms seem to be able to reduce this risk by focusing on exploitation rather than exploration.

Table 4 – Determinants of non-positive profits

	a	b	c	d
	Full sample	California	Full sample	California
Profit measure	Pre-tax profits	Pre-tax profits	EBIT	EBIT
High uncertainty	0.102***	0.070***	0.172***	0.144***
	(0.001)	(0.004)	(0.002)	(0.005)
D(R&D>0)	0.026***	0.027***	0.136***	0.105***
	(0.001)	(0.003)	(0.002)	(0.006)
Exploitation ratio	-0.012***	-0.010***	-0.058***	-0.051***
	(0.001)	(0.877)	(0.002)	(0.003)
N	240,742	22,207	231,091	21,934
Year FE	yes	yes	yes	yes
Pseudo R^2	0.073	0.104	0.071	0.089

Notes: The dependent variable takes value one if profits are zero or negative. All models are Probit regressions. Table shows marginal effects. Heteroscedasticity-robust standard errors shown in parentheses. ***, **, * indicate statistical significance at the 1%, 5%, 10% level, respectively.

Table 5 illustrates the association between risk and exploitation with industry-level splits. In Panel A, we calculate a direct measure of profit risk, based on the share of firm-years with zero or negative profits at the industry-level, using only years before the introduction of the credit. Panel B shows results based on industry-level R&D intensity, again from the years before introduction. In industries with a high level of R&D intensity, innovation activity is likely to be an important determinant of firms' profits which is essential for our proposed mechanism. Table 5 shows that R&D tax credits induce exploitation mostly in industries with high profit risk and R&D intensity.[13]

[13] We measure profit risk and R&D intensity at the industry-level to reflect exogenous technological and market characteristics and to avoid that our sample split is affected by the success of firms' innovative activity.

Table 5 – The impact of R&D tax credits on known vs. new to the firm technologies: sample splits according to industry-level profit risk and R&D intensity

Panel A: Industry-level profit risk

	a	b	c	d	e	f
	Exploitation ratio	Exploitation ratio	Exploitation ratio	Exploitation ratio	Exploitation ratio	Exploitation ratio
Profit risk	high	low	high	low	high	low
Sample	Original	Original	Matched	Matched	Firm-state	Firm-state
R&D tax credit rate$_{t-3}$	4.050***	1.436***	3.206***	1.193*	5.754***	2.675***
	(0.345)	(0.372)	(0.578)	(0.577)	(0.886)	(0.641)
N	11,121	11,136	7,234	5,356	11152	13973
Year FE	yes	yes	yes	yes	no	no
Firm FE	yes	yes	yes	yes	no	no
Firm-year FE	no	no	no	no	yes	yes
State FE	no	no	no	no	yes	yes
R^2	0.557	0.621	0.483	0.602	0.366	0.308

Panel B: Industry-level R&D intensity

R&D intensity	high	low	high	low	high	low
Sample	Original	Original	Matched	Matched	Firm-state	Firm-state
R&D tax credit rate$_{t-3}$	3.491***	1.777***	2.960***	1.095*	5.940***	1.667**
	(0.445)	(0.423)	(0.600)	(0.439)	(0.895)	(0.806)
N	11,039	11,218	7,620	4,970	13008	12117
Year FE	yes	yes	yes	yes	no	no
Firm FE	yes	yes	yes	yes	no	no
Firm-year FE	no	no	no	no	yes	yes
State FE	no	no	no	no	yes	yes
R^2	0.557	0.621	0.483	0.602	0.358	0.302

Notes: All dependent variables are measured in logarithmic form. All models are OLS regressions. Heteroscedasticity-robust standard errors are clustered at the state level and shown in parentheses. ***, **, * indicate statistical significance at the 1%, 5%, 10% level, respectively.

Implications of tax credits and a shift in innovation: increasing markups

Even though R&D tax credits shift the proportion of innovation away from new areas, they still increase the absolute number of patents in new areas, and thus arguably meet policy goals of increased investment in fundamental research. Here, we investigate probably unintended side effects, however, and illustrate how credits lead to technological entrenchment that in turn leads to increased markups by already established incumbents.

To investigate whether this is the case, we analyze the effect of R&D tax credits on markups, defined as prices over marginal costs. As common in the literature, we do not have direct information about firms' prices and marginal costs. We therefore structurally estimate markups from production functions following De Loecker and Warzynski (2012) and De Loecker, Eckhout and Unger (2020).[14] An advantage of the production-based approach is that it can be estimated using standard balance sheet data which is available across a broad set of firms, industries and time periods, which is essential for our application. A further advantage of this approach is that we do not have to make strong assumptions about demand to derive markups.

Our starting point is an industry-specific production function (F(.)) where output (Q, measured as deflated sales) of firm i in industry j and time t is a function of variable production factors (V, measured as cost of goods sold), capital (K) and total factor productivity (Ω_{it}):

$$Q_{it} = F_j(V_{it}, K_{it})\Omega_{it}$$

Assuming firms minimize costs and take the production function as given, the first order condition yields an expression for a firm's markup (μ_{it}), defined as the ratio of price to marginal costs:

$$\mu_{it} = \frac{\partial Q_{it}}{\partial V_{it}} \frac{V_{it}}{Q_{it}} \frac{P_{it} Q_{it}}{P_{it}^{V} V_{it}} = \frac{\theta_{it}}{\alpha_{it}}$$

The revenue share (α) is observed in balance sheet data, the output elasticity of variable inputs (θ) can be estimated from a production function.

We experiment with alternative functional forms of *F(.).* In our baseline specification, we rely on a Cobb-Douglas production function:

$$q_{it} = \beta_v v_{it} + \beta_k k_{it} + \omega_{it} + u_{it}$$

where the lower-case letters denote logs, ω_{it} is log total factor productivity and u_{it} captures measurement error in output and β_v and β_k denote elasticities to be estimated. This yields a constant elasticity across firms within industries: $\theta_{it} = \beta_v$. Although restrictive, the Cobb Douglas production function has the advantage that any bias in elasticities (which could for instance stem from using sales as a proxy for output or measurement error in firms' capital stocks), and thus any bias in markups, is constant across firms *within* industries and time periods. For the Cobb Douglas specification, all variation in markups within industries over time stems from variation in the revenue share of variable inputs. Since we are interested in relative variation in markups within firms across time rather than a cross-sectional comparison of firms, the remaining bias is of less importance in our application.

[14] De Loecker and Scott (2016) and De Loecker, Eckhout and Unger (2020) provide evidence that markups from this approach are similar to those from a demand approach, commonly employed in the industrial organization literature, for selected industries where prices and quantities are available.

We also estimate translog production functions which allow elasticities to vary with input use and therefore across firms and time periods. We use a version of the translog production function proposed by De Loecker, Eckhout and Unger (2020):

$$q_{it} = \beta_v v_{it} + \beta_k k_{it} + \beta_{2v} v_{it}^2 + \beta_{2k} k_{it}^2 + \omega_{it} + u_{it}$$

For estimation, we use the two-step estimation method proposed by Ackerberg, Caves, and Frazer (2015). Demand for variable inputs is assumed to depend on an invertible function which depends on capital, R&D, and total factor productivity (TFP). This allows specifying a first stage equation which controls for productivity using a nonparametric function in R&D (rd_{it}), capital and variable inputs: $q_{it} = \phi(v_{it}, k_{it}, rd_{it}) + u_{it}$. This stage does not identify any coefficients from the production function but allows us to net out measurement error u_{it}. We approximate $\phi(.)$ using a fourth order polynomial in *v, k* and *rd*.

Following Dorazelski and Jaumandreu (2013), we allow the law of motion for the productivity process to depend on R&D: $\omega_{it} = g(\omega_{i,t-1}, rd_{it})$+$\varsigma_{it}$, where we approximate the unknown function *g(.)* by a fourth order polynomial. The law of motion yields the following moment condition for variable inputs: E$[\varsigma_{it}(\theta_{it})v_{i,t-1}]$ =0.[15]

A potential concern with the estimated elasticities is that they might be affected by unobserved price variation across firms (see, for instance, Bond et al., 2020). To address this problem, we follow two alternative approaches as a robustness check. First, De Loecker, Eckhout and Unger (2020) show that the output price bias can be addressed by controlling for the determinants of markups which are captured by market shares and time in the first stage. Second, we use an alternative approach developed by Forlani et al. (2016) which allows estimating elasticities from a function that explicitly relates sales to input factors and is thus not affected by price bias. A drawback of their approach is that they have to impose additional assumptions on the demand side and – in the absence of price data – can only identify markups up to scale (or precisely under constant returns to scale). Due to these additional assumptions, we do not use their method as our baseline equation but employ it only as a robustness check.

[15] Note that this specification allows R&D to impact productivity but it does not assume that a relationship exists as the estimation could yield coefficient values of zero for variables in *g(.)* that are a function of R&D.

Table 6 – R&D tax credits and markups

	a	b
	original sample	after matching
R&D tax credit rate$_{t-3}$	0.594***	0.503***
	(0.099)	(0.170)
N	21759	12237
Firm FE	yes	yes
Year FE	yes	yes

Notes: The dependent variable is the markup, defined as the ratio of price to marginal costs, estimated following De Loecker, Eckhout and Unger (2020). R&D tax credit is the effective rate as calculated by Wilson (2009). OLS regressions, heteroscedasticity-robust standard errors are clustered at the state level. ***, **, * indicate statistical significance at the 1%, 5%, 10% level, respectively.

Table 6 shows results from regressions relating markups to tax credits. Estimates for the original and the matched sample are depicted in columns *a* and *b*, respectively. The results indicate that an increase in tax credits by 10 percentage points is associated with an increase in prices relative to marginal costs between 5% and 6%. Table B1 in Appendix B shows that the effects of tax credits on markups are robust towards using different markup measures derived from alternative specifications of production functions.[16] First, we allow for time-varying production function coefficients in the Cobb-Douglas. Second, we derive markups from a translog production function in which elasticities vary with input use and are therefore firm-year specific. Third, we control for selling and general administrative expenses (SGA) as a measure of fixed costs in the production function and market share as a determinant of pricing heterogeneity within industries. Fourth, we implement an alternative estimator of production functions following Forlani et al. (2016). Finally, we follow De Loecker and Warzynski (2012) and analyze to which extent markups can be explained by increasing productivity. Since previous research has shown that R&D tax credits are associated with productivity growth (e.g., Bloom, Schankerman and van Reenen, 2013), it is possible that markups simply rise because of lower marginal costs and incomplete pass-through of costs to consumer prices.[17] For this purpose, we control for a third-order polynomial in measured (revenue-based) total factor productivity (TFP) to account for firm-specific changes in efficiency

[16] We mostly follow De Loecker, Eckhout and Unger (2020), who discuss the various methods in detail, in the choice of these alternative production functions.
[17] It has to be noted, however, that previous estimates of productivity in the tax credit literature are revenue-based measures of productivity which are potentially affected by prices. It is therefore possible that at least part of the productivity effects attributed to tax credits are due to increases in prices.

over time. The idea is that conditional on productivity, variation in markups stems from changes in prices rather than changes in marginal costs.[18] All these robustness checks confirm the positive effect of R&D tax credits on markups.

If the increase in markups in response to R&D tax credits is driven by changes in innovative search strategies, we should see that markup increases are concentrated among firms with a relatively high profit variation, i.e., among firms whose incentives for exploitation strategy are most affected by tax credits. For this purpose, we estimated heterogeneous effects for firms whose average profit uncertainties are below and above the median respectively. Again, we measure product market uncertainty as the standard deviation of profits divided by the absolute amount of average profits. Standard deviations and average profits are measured over the period 1977 to 1987, i.e., the years before the introduction of tax credit in California, to reduce endogeneity concerns. Results depicted in Table 7 indicate that markup increases are indeed driven by firms with high uncertainty in the years before the R&D tax credit. As Table B2 in Appendix B shows, our results are robust towards an industry-level measure of uncertainty which is simply computed as the average value over all firms within a 4-digit SIC industry.

Panel B shows the results of a sample split according to an industry-level measure of profit risk, based on the share of firms with zero or negative profits before the introduction of tax credits. The results show that the effects of tax credits on markups are concentrated in industries with high profit risk which is consistent with our uncertainty sample split and our proposed mechanism.

As a plausibility check, we also analyzed whether the effects of tax credits are driven by R&D activity. For this purpose, we split the sample according to whether a firm was engaged in R&D at any time during the sample period. Results in Table 8, Panel A, show that the effect of tax credits on markups is indeed solely driven by firms that perform R&D, both in the original and in the matched sample.

[18] Note that our measure of revenue TFP might partly capture variation in output prices as well. Hence, the estimated price increase should be regarded as a lower bound since TFP partially captures pricing heterogeneity within industries and might thus eliminate some of the variation in markups that stems from market power.

Table 7 –Tax credits and markups: heterogeneity by uncertainty

	a	B	c	D
	Original sample		Matched sample	
Panel A: Sample split based on market uncertainty				
Subsample:	High uncertainty	Low uncertainty	High uncertainty	Low uncertainty
R&D tax credit rate$_{t-3}$	1.100***	0.088	1.020***	-0.054
	(0.218)	(0.115)	(0.353)	(0.243)
N	10172	11587	6424	5813
Year FE	yes	Yes	yes	Yes
Firm FE	yes	Yes	yes	Yes
R^2	0.622	0.784	0.608	0.736
Panel B: Sample split based on profit risk				
Subsample:	High risk	Low risk	High risk	Low risk
R&D tax credit rate$_{t-3}$	0.919***	-0.138	0.869**	-0.182
	(0.195)	(0.112)	(0.310)	(0.169)
N	10717	11035	6943	5291
Firm FE	yes	Yes	yes	Yes
Year FE	yes	Yes	yes	Yes
R^2	0.673	0.769	0.633	0.754

Notes: The dependent variable is the markup, defined as the ratio of price to marginal costs, estimated following De Loecker, Eckhout and Unger (2020). R&D tax credit is the effective rate as calculated by Wilson (2009). The high (low) uncertainty subsample consists of firms whose standard deviation of profits divided by the absolute amount of average profits in the years before 1987 are above (below) the sample median. OLS regressions, heteroscedasticity-robust standard errors are clustered at the state level. ***, **, * indicate statistical significance at the 1%, 5%, 10% level, respectively.

Table 8 –Tax credits and markups: heterogeneity by R&D activity

	a	b	c	d
	Original sample		Matched sample	
Panel A: Firms with and without R&D activity				
Subsample:	R&D	no R&D	R&D	no R&D
R&D tax credit rate$_{t-3}$	0.713***	-0.070	0.615***	-0.082
	(0.129)	(0.069)	(0.211)	(0.217)
N	15932	5827	10026	2211
Year FE	Yes	Yes	yes	yes
Firm FE	Yes	Yes	yes	yes
R^2	0.664	0.814	0.635	0.844

Panel B: Sample split by industry-level R&D intensity

Subsample:	High R&D	low R&D	High R&D	low R&D
R&D tax credit rate$_{t-3}$	0.770***	0.188***	0.560*	0.221*
	(0.214)	(0.057)	(0.308)	(0.114)
N	10673	11079	7319	4915
Year FE	yes	Yes	yes	yes
Firm FE	yes	Yes	yes	yes
R^2	0.639	0.802	0.616	0.817

Notes: The dependent variable is the markup, defined as the ratio of price to marginal costs, estimated following De Loecker, Eckhout and Unger (2020). R&D tax credit is the effective rate as calculated by Wilson (2009). The high (low) R&D subsample consists of firms whose industry-level R&D in the years before 1987 are above (below) the sample median. OLS regressions, heteroscedasticity-robust standard errors are clustered at the state level. ***, **, * indicate statistical significance at the 1%, 5%, 10% level, respectively.

A potential concern is that firms engaging in R&D might differ in their managerial ability and other characteristics from remaining firms. Therefore, Panel B shows results of a sample split according to industry-level R&D intensity. The estimates indicate that the effects of tax credits on markups are more pronounced in industries with an R&D intensity above the median.

Table B3 in Appendix B shows that our markup results are robust towards using alternative measures of tax credit exposure. Specifically, our conclusions are very similar if we relate markups to the effective R&D tax credit rate in t-2 or t-4 (instead of t-3) or if we replace the effective rate by the nominal rate or a measure of R&D user costs, which is inversely related to R&D tax credits. As documented Table B4 in Appendix B our results for markups and R&D tax credits hold not only for the California experiment but also in the full sample which includes all years and states (though results are weaker, which may reflect smaller credits or competition between states). Within the full sample, the results seem to be entirely driven by firms that patent and by firms that engage in R&D, which is consistent with R&D tax credits affecting markups via innovation strategies.

Having established how tax credits influence search strategy, and that a shift towards exploitation strategies precedes increased markups, we now investigate the valuation, market, and competitive implications of R&D tax credits. There is no rule that requires firms to share the benefits such as the new knowledge generated through tax credit money with other firms. Instead firms have all the incentives to limit spillovers and exclude others from using their inventions – for example, by patenting defensively and strategically, with the intent to block others from following in their technological wake. Here we show a suite of outcomes consistent with this possibility, including an increase in stock market valuation of the treated firms and a decrease in stock market valuation of the closest competing firm, defensive and strategic patenting by treated firms, and decreased entry into new markets by treated firms.

An increase in treated firm stock valuation

Firms which took advantage of the tax credit experienced increased stock market valuations in subsequent years. Table 9 estimates a variety of models based on stock market reaction to patent issuance (using patent values estimated by Kogan et al., 2017). Most of the value increase correlates with patenting in known technologies.

We offer approximations of the value of these credits to California firms based on the following assumptions. Given the average portfolio value of $165.55 million US dollar (in dollar values of 1982), these assumptions imply an absolute return of $7.97 million in 2015 US dollars per one percentage point increase in R&D tax credit per firm, which implies $109.2 million per California firm, and $37.7 billion in total for all California firms in the sample. The amount may be underestimated as we only take publicly listed and patenting firms into account that were active before and after the tax introduction. This estimate ignores other potential positive impacts from additional firms that might have moved to California because of the R&D tax credit being in place, or positive spillovers to other firms.

Table 9 – Impact of the California tax credit of 1987 on financial patent value as measured by stock market impact – exploitative vs. explorative patents

Panel A: Original sample				
	a	b	c	d
	Stock market value of patent portfolio	Value of patents in known classes	Value of patents in new classes	Fraction of value coming from known technological areas
R&D tax credit rate$_{t-3}$	6.406***	6.772***	2.871***	1.168***
	(0.511)	(0.485)	(0.327)	(0.188)
N	22,257	22,257	22,257	8,963
Year FE	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes
R^2	0.100	0.030	0.064	0.045

Panel B: Matched sample				
	a	b	c	d
	Stock market value of patent portfolio	Value of patents in known classes	Value of patents in new classes	Fraction of value coming from known technological areas
R&D tax credit rate$_{t-3}$	5.243***	5.606***	2.469***	0.789**
	(0.827)	(0.757)	(0.515)	(0.327)
N	12,590	12,590	12,590	5,276
Year FE	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes
R^2	0.824	0.823	0.706	0.397

Notes: All dependent variables but in (d) are measured in logarithmic form. All models are OLS regressions. R&D tax credit is the effective rate as calculated by Wilson (2009). Value Known is the total private value of patents filed in a 3-digit technology class where the given firm has filed beforehand in that class, measured as the sum of all market reactions to publications of these patents. Value New is the total private value of patents filed in a 3-digit technology classes where the given firm has not filed beforehand in that class, measured as the sum of all market reactions to publications of these patents. Fraction Value Known is the proportion of the latter two variables in percent. Heteroscedasticity-robust standard errors are clustered at the state level and shown in parentheses. ***, **, * indicate statistical significance at the 1%, 5%, 10% level, respectively.

A decrease in closest competitors' valuation

Knowledge spillovers to other firms provide perhaps the most central argument to justify R&D tax credits. Spillovers, however, remain notoriously hard to measure. One common approach is to count future cites from other firm's patents (Jaffe, Trajtenberg, and Henderson, 1993). The main downside of this approach is that future cites are not only capturing positive knowledge spillovers but also potential business stealing effects, especially when they come from competing firms' patents (Bloom et al., 2013). Further, potentially competing firms that do not enter technological areas of the focal firm or leave that area in response to the tax credit remain unobserved. Those firms would typically enter the group of non-citing firms and thus fall into the group of unaffected firms. Negative externalities are therefore hard to detect and easy to miss with future cites, creating potentially upward bias in assessing positive externalities of tax credits. We address this issue in two ways by a) looking at competing firms' stock valuations and b) at future patent applications that are blocked by the treated firms' patents.

To measure competing firms' valuations, we extend the approach of Kogan et al. (2017). Instead of measuring the private value of a patent via stock market reactions of the focal firm *i*, we measure – with a very similar technique – the reaction of the closest competing firm *j*'s stock price to the publication of a given patent by i. Allowing stock market reactions to be negative allows capture of positive as well as negative externalities of patents. These negative externalities could, for instance, arise from business stealing or from blocking competitors from entering technological areas of the focal firm.

To avoid attenuation bias, we focus on the closest competitor instead of considering all potential competitors. This is also where negative spillovers are most likely to occur (Bloom, van Reenen, and Williams, 2019). We identify the closest technological competitor to a given firm within the same industry (three-digit SIC) by following Bloom et al. (2013) and Jaffe (1989) in calculating for each competing and focal firm the pair-wise technological proximity based on the distribution of patents across technology classes per firm. In particular, we employ the following variant of Jaffe's (1989) technological proximity measure to estimate similarity in technological space of firm i's patents and its competing firm j's patents, using patent counts per USPTO three-digit technology classes k:

$$Technological\ Proximit_{\ i,j} = \frac{\sum_{k=1}^{K} f_{i,k} f_{j,k}}{\left(\sum_{k=1}^{K} f_{i,k}^2\right)^{\frac{1}{2}} \left(\sum_{k=1}^{K} f_{j,k}^2\right)^{\frac{1}{2}}} \tag{4}$$

where $f_{i,k}$ is the fraction of patents granted to firm i that are in technology class k such that the vector $f_{i,k}$ locates the firm's patenting activity in K-dimensional technology space. $Technological\ Proximit_{i,j}$ is basically the cosine angle between both vectors and will be zero for a given firm-year when there is no overlap between this firm's patent technology classes with competing firm's technology classes. $Technological\ Proximit_{i,j}$ will equal one when the distribution of firm i's patents is identical to patents accumulated by firm j. Bloom et al. (2013)

study and discuss alternative measures of technological similarity in detail but find little differences in their results.

For each firm we define the one firm with the highest technological proximity within the same industry as the closest competitor. Using a similar method to Kogan et al. (2017), we run an event study, but measure the reaction of the closest competitor's stock price around the date of each patent granted to the focal firm *i* based on data from CRSP. Analogous to Kogan et al. (2017), the estimate of the economic value of a patent of firm *i* to the competing firm j (ξ_{ij}) is constructed as the product of the market capitalization of firm j (M_j), measured at $t = -1$, where $t = 0$ is the date of the announcement of firm *i*'s patent grant, and an estimate of the stock return of firm *j* (the competitor) related to *i*'s patent issue ($\mathrm{E}[v_{ij}|R_j]$). We further adjust this measure by the number of patents granted to firm i (N_i) on day *t* and the unconditional probability of success of a patent application $\bar{\pi}$ (56% according to Caley, Hedge, and Marco, 2014). Analogous to Kogan et al. (2017), the economic value of a patent to its closest competitor is defined as:

$$\xi_{ij} = (1 - \bar{\pi})^{-1} \frac{1}{N_i} \mathrm{E}[v_{ij}|R_j] M_j.$$

The patent related cumulative expected stock return of the competing firm $\mathrm{E}[v_{ij}|R_j]$ is calculated using the three-day event window (0, +2) around the date of firm *i's* patent announcement assuming the normal distribution of the value of the patent v_{ij}. We deviate from Kogan et al. (2017) only by not truncating at zero, in order to allow negative reactions. Due to potential blockings and business stealing effects, it is less plausible that the value of a focal firm's patent for competitors is a strictly positive random variable.

To analyze the value externalities of California's tax credit introduction, we calculate for each firm *i's* patent portfolio, assembled in year *t,* the total value reaction by its closest competitor *j,* and use this as the dependent variable. Table 10 shows the corresponding results in column *a*. The average potentially masks significant heterogeneity though. If patents are mainly used to exploit extant technologies and to shield the focal firm from additional competition, then firms that are close in technology space should be more negatively affected. Positive spillovers on the other hand might be more likely to occur when the competing firm is in the same industry but further away in technology space, such that technological entrenchment of firm *i* is less likely to interfere with technological developments of firm *j*. We thus split the sample at the median of technological proximity. Table 10, column *b* shows the corresponding results for competitors above the median of technological proximity and column *c* the corresponding results for competitors below or equal to the median of technological proximity. The results in Table 10 confirm that the negative association between tax credits and stock market valuation of competitors is indeed driven by firms with high technological proximity.

Table 10 – Impact of the California tax credit of 1987 on closest competitor's stock market value

Panel A: Original sample			
	a	b	c
	Value reaction of closest competitor	Value reaction of closest competitor > median Tech Proximity	Value reaction of closest competitor <= median Tech Proximity
R&D tax credit rate$_{t-3}$	-2.405**	-6.053*	3.530*
	(1.114)	(2.990)	(1.731)
N	7,539	3,699	3,766
Year FE	yes	yes	yes
Firm FE	yes	yes	yes
R^2	0.112	0.132	0.092
Panel B: Matched sample			
	a	b	c
	Value reaction of closest competitor	Value reaction of closest competitor > median Tech Proximity	Value reaction of closest competitor <= median Tech Proximity
R&D tax credit rate$_{t-3}$	-3.775***	-6.668***	1.840
	(1.196)	(1.392)	(1.879)
N	4,351	2,146	2,159
Year FE	yes	yes	yes
Firm FE	yes	yes	yes
R^2	0.121	0.132	0.120

Notes: All models are OLS regressions. R&D tax credit is the effective rate as calculated by Wilson (2009). Competing firms' value reactions are windsorized at the 1% and 99% level to restrict the influence of outliers on the estimates. Heteroscedasticity-robust standard errors are clustered at the state level and shown in parentheses. ***, **, * indicate statistical significance at the 1%, 5%, 10% level, respectively.

An increase in blocking and strategic patents

Here we investigate mechanisms that may have contributed to the increase in firm valuation, for firms that were able to take the tax credit. Akcigit, Baslandze, and Lotti (2018) illustrate how political lobbying can stifle innovation; here we illustrate a consistent but different mechanism through strategic patenting and the blocking of competitor's future innovation streams. We modeled the number of future inventions that are "blocked" by treated firms' patents (for a detailed exposition of the data and measure, please see Lueck et al., 2020). The idea is loosely opposite that of a prior art citation – rather than indicating a positive knowledge spillover, a blocking citation prevents a future patent from issuing, because the prior and blocking patent invalidates the novelty claim of the future patent. We also modeled patents which are economically valuable but do not appear to generate knowledge spillovers.

We define a blocked patent as a denied EPO patent application that refers to a given US patent as a X or Y-type of prior art (X applies when a single document invalidates the novelty of the application, Y applies when a combination of documents invalidates the application). Further, we define a strategic patent as falling into the top 50% of the stock market value reaction in a given year but receiving no future prior art citations (thus indicating financial value to the firm, but low knowledge spillovers, at least by the conventional measure of future prior art citations; see Jaffe, Trajtenberg, and Henderson, 1993; Kurakina, 2021). Tax credits appear to discourage subsequent innovation. Table 11, column *a* illustrates a positive and significant correlation between tax credits and future blockings. Table 11, column *b* shows that tax credits are also associated with a higher number of strategic patents. Analogous to the increase in markups as a measure of market power in product markets, these results can be interpreted as a measure of increased market power in technology markets.

A decrease in new market entry

Consistent with an increased focus on known technological areas, firms which were able to take advantage of tax credits were also less likely to subsequently enter new markets. Hall (1993) raised the possibility that tax credits could unintentionally lead to a favoring of new product development over fundamental research; ironically, this appears not to have happened, and that refinement in both technologies and markets occurred instead. The lack of new market entry is also consistent with a shift away from "external innovation" (Ackigit and Kerr, 2018).

To establish this empirically, we use the Compustat Historical Segment files, which measure each firm's sales generated across industries at the SIC 3-digit level. Based on these data we calculated the amount of sales generated in industries, where the same firm had not generated any sales beforehand, and the number of industries entered, measured as the number of distinct industries, where the focal firm had not generated any sales beforehand. We use information in *t+3*, i.e., six years after tax credit introduction, to allow for a sufficient time lag between investment into new product development, patenting of new technologies, and the actual introduction of new products to the market. Table 12 illustrates how R&D tax credits do not appear to encourage firms to enter new markets, as indicated by the number of new industries entered and sales generated in those industries.

Table 11 – The impact of R&D tax credits on blockings and strategic patents

Panel A: Original sample		
	a	b
	Blocked EPO Patents	Strategic Patents
R&D tax credit rate$_{t-3}$	3.406***	0.541***
	(0.331)	(0.108)
N	19,942	22,257
Year FE	yes	yes
Firm FE	yes	yes
R^2	0.750	0.793
Panel B: Matched sample		
	a	b
	Blocked EPO Patents	Strategic Patents
R&D tax credit rate$_{t-3}$	2.798***	0.444***
	(0.382)	(0.150)
N	11,445	12,590
Year FE	yes	yes
Firm FE	yes	yes
R^2	0.685	0.819

Notes: All dependent variables are measured in logarithmic form. All models are OLS regressions. Blocked EPO Patents is the total number of blocked patent applications at the European Patent Office (EPO), defined as patent applications that were eventually denied by the EPO and referred to at least one US patent of the focal firm applied in year t, which was classified as potentially blocking (X or Y citations in the EPO examiner search report). Strategic Patents is the total number of patents that fall into the top 50% of the stock market value reactions in a given year but not into the top 50% of future citations. Heteroscedasticity-robust standard errors are clustered at the state level and shown in parentheses. ***, **, * indicate statistical significance at the 1%, 5%, 10% level, respectively.

Table 12 – The impact of R&D tax credits on sales in new to the firm markets

Panel A: Original sample		
	a	b
	Sales in new to the firm industries in t+3	New industries entered in t+3
R&D tax credit rate$_{t\text{-}3}$	-0.789*	-0.143**
	(0.439)	(0.063)
N	20,215	20,215
Year FE	yes	yes
Firm FE	yes	yes
R^2	0.148	0.134
Panel B: Matched sample		
	a	b
	Sales in new to the firm industries in t+3	New industries entered in t+3
R&D tax credit rate$_{t\text{-}3}$	-1.118**	-0.212***
	(0.527)	(0.073)
N	11,343	11,343
Year FE	yes	yes
Firm FE	yes	yes
R^2	0.149	0.142

Notes: All dependent variables are measured in logarithmic form. All models are OLS regressions. Sales New to the Firm are sales generated in SIC 3-digit industries where the given firm has never generated sales beforehand in that industry, measured in t+3. New Industries Entered is the total number of SIC 3-digit industries where the given firm has never generated sales beforehand in that industry, measured in t+3. Heteroscedasticity-robust standard errors are clustered at the state level and shown in parentheses. ***, **, * indicate statistical significance at the 1%, 5%, 10% level, respectively.

Consistent results in tax credit changes by other states, following California's change

Over the last decades, many states have followed California by introducing their own R&D tax credit schemes with varying effective rates. Despite the methodological problems discussed above, we re-estimated all models, including all states that introduced R&D tax credits over the period of 1977 to 2006. The results are shown in the Appendix A, Table A2. In the full sample we still find large private returns as measured by increased patenting and private value creation. The increased focus on known technologies, increased blockings and markups remain, though marginal effects are often lower.[19]

[19] The reasons for the differences in effects are not easy to disentangle. Time seems to play a role as we find lower effects in terms of significance and economic magnitude the more we move to the later years of the sample, which can be explained by a lower relative advantage of havening R&D tax credits when many technology intensive firms

The effects of a different R&D cost shock

The previous results raise the question to which extent the effects are specific to R&D tax credit or are also a feature of different innovation cost shocks. For instance, if marginal innovation projects are more likely to be characterized by exploitative innovation activities, we should see similar patterns for other exogenous events that induce patenting. For this purpose, we relate our main outcome variables to an innovation cost shock proposed by Aghion et al. (2019) which is based on time-varying state composition of Appropriation Committees. The idea is that legislators on Appropriation Committees often push for the approval of grants on R&D projects from the state which they represent. Following Aghion et al. (2019), we construct a variable which measures the number of senators on the Appropriation Committee for each state and year.[20]

Results in Table 13 show that this variable is positively and statistically insignificantly related to patents. An additional senator which represents a state is associated with a 4% increase in the expected number of patents per firm. However, it does not seem to affect the exploitation ratio nor markups significantly. These results are consistent with the hypothesis that our findings are a unique feature of R&D tax credits but not of other innovation policies.

Table 13 – The impact of state R&D committee members

Panel A: Original sample			
	a	b	C
	Patents	Exploitation ratio	Markups
R&D committee$_{t-3}$	0.040**	0.016	-0.003
	(0.019)	(0.020)	(0.008)
N	70489	70489	67465
Year FE	yes	yes	yes
Firm FE	yes	yes	yes
R^2	0.760	0.539	0.662

5. Discussion

Hall (1993), Hall and Van Reenen (2000), and Hall (2019) emphasize that when firms face lower costs of R&D they will maximize their private returns rather than the social benefits to their innovative efforts. Our evidence confirms this expectation. It further highlights that R&D tax

were already situated in states that had tax credits in place. Another explanation, as we discuss above, is measurement error due to interaction with other reforms which would bias our results in the full sample towards zero.

[20] See Aghion et al. (2019) for a detailed discussion of the institutional background and the exogeneity of this measure.

credits can encourage strategic use of the patent system. Increased blockings and increased markups point to possibly unintended consequences of R&D tax credit provision. The introduction of R&D tax credits in 28 U.S. states over the 1980s and 1990s may have contributed to the large increase of markups over the same period (De Loecker, Eeckhout and Unger, 2020). This finding points out that revenue-based productivity gains due to R&D subsidies found earlier (Einiö, 2014) might have been partially driven by increased market power, rather than innovation that enables new products or increased efficiency of the production process.

Negative externalities of tax credits, for example, blocking, strategic patenting, and negative impacts on competitors have been largely neglected in the broader literature on the impact of different kinds of R&D subsidies (see for example: Howell, 2017; Bøler, Moxnes and Ulltveit-Moe, 2015; Moretti, Steinwender and Van Reenen, 2016; Jaffe and Le, 2015; Azoulay et al., 2014; Lach, 2002; Branstetter and Sakakibara, 2002). This might have occurred due to missing data or because the classic theory (Arrow, 1962) did not consider the interplay between the unconditional provision of R&D subsidies and the patent system.[21]

This finding of negative externalities could warrant a reconsideration of Arrow's original theory. It is usually argued that firms underinvest in R&D because they are afraid of knowledge leakage to competitors, which reduces the appropriability of the returns to innovation. R&D tax credits appear – as intended – to ameliorate the problem of reduced R&D. Yet they appear to create both positive spillovers, for example, as measured by future prior art citations, as well as negative spillovers, for example, as measured by increased blocking patents. These results call for a reconsideration of the theory behind the combination of unconditional R&D tax credit provision with a patent system that intends to solve the same appropriability problem, as well as refinement of empirical estimations of positive and negative spillovers.

Others who have explicitly incorporated costs or negative externalities of R&D subsidies in their analyses considered costs stemming from the time and effort spent on the application processes, the shadow costs of public funds (Takalo, Tanayama, Toivanen, 2013, 2014 and 2017), windfall gains (González, Jamandreu and Pazó, 2005), or negative externalities on entry (Acemoglu et al., 2018). These studies do not consider, however, the interplay of tax credits with the patent system, which allows firms to use tax credits for strategic purposes, potentially just to raise rivals' costs (Salop and Scheffman, 1983; Shleifer and Vishny, 1989) or steal business from

[21] That patents can inhibit competition has been shown for instance by Cockburn and MacGarvie (2011).

competitors (Bloom et al., 2013).[22] Neglecting this interplay between tax credits and the patent system leads to potential overestimations of social benefits, at least in industries where patents are effective in both solving the appropriability problem and blocking competitors' research.

Policy makers have understood that tax credits are most valuable for firms that generate taxable income, and thus may unintentionally favor larger and older firms with more stable profit streams. In 2016, the US made 10% of qualifying R&D expenses deductible against payroll taxes for firms with maximal $5 million in revenue. This should effectively reduce incentives to prioritize exploitation over exploration. Akcigit, Hanley, Serrano-Velarde (2020) make a similar argument when they propose more targeted research subsidies that would favor basic rather than applied research. Richer datasets will provide fruitful avenues for research on this topic.

6. Conclusion

We proposed that R&D tax credits induce a shift towards refinement and exploitation, because exploration (though potentially offering a bigger upside) remains risky and less likely to generate a profit. While succeeding in reducing R&D costs, classic R&D tax credits may unintentionally make experimenting and exploration more costly relative to exploitation, because exploration increases the likelihood that R&D costs can either not be expensed – or expensed only later at a discounted rate. Since credits become less valuable without profits (Hall, 2019), innovation shifts towards refinement of known technologies, particularly for firms that face greater uncertainty.

We illustrated this empirically for California firms following a 1987 tax credit, most tellingly for firms with inventors residing both in and beyond California. Elaborating empirically on the strategic and industrial implications of this shift in search strategy, we also illustrated how firms generate large private returns as measured by stock market reactions (Kogan et al., 2017). This increase in private value comes mainly from an increased focus on the exploitation of the firm's existing technological trajectories; most of the increase in valuable patents is concentrated in technological areas known to the firm. We also find increased blockings of patent applications at the European Patent Office (Lueck et al., 2020), caused by patents previously filed by California firms following the introduction of tax credits. Furthermore, we observe a decrease in valuation

[22] If the patent system or intellectual property rights (IPR) are very effective, it can be shown that IPRs can be too strong from social planner's view (Acemoglu and Akcigit, 2012), and R&D tax incentives will rarely be helpful in such cases (Acemoglu et al., 2013).

for firms that are close technological competitors to the treated firm. Firms appear less likely to enter new markets, instead, a substantial part of the value creation stems from an increase in strategic patenting, that is, we observe a higher number of patents which are associated with large stock market returns but receive relatively few future citations. Results hold within firms (within and outside California), in a matched sample, and a broader sample which exploits the staggered introduction of R&D tax credits across states.

Despite the popularity of Schumpeter's idea that innovation causes creative destruction (1942), empirical work has more often focused on positive spillovers (usually future prior art citations, see Bloom et. al., 2013, for a discussion); here we use novel measures of "blocking patents," as well as the combination of competitive distance and the financial impact of competitors' patents, to investigate the negative externalities of innovation induced by tax credits. Many papers have evaluated credits with conventional measures such as spending, patents, and citations, however, there is a richer picture to be drawn of Schumpeter's idea of negative side effects.

Following Lerner and Seru (2017), this work sought to understand the impact and mechanisms of one tax credit in detail (Hall and Wosinska, 1999), before broadening the analysis and considering a number of later tax credit changes. We found that the benefits of tax credits for other states were qualitatively similar to California, although the effects were quantitatively smaller. It appears that California firms exploited a huge benefit in getting ahead of their out-of-state competitors. The 1987 tax credit may have played an under-appreciated role in Silicon Valley's rise to technological dominance. Surprisingly, that advantage came mainly from the exploitation of firms' prior trajectories, rather than from fundamentally new breakthroughs. If that is correct, then Silicon Valley's historical advantage becomes easier to understand – and more difficult to replicate, now that many regions have enacted tax credits.

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Appendices

Part A

Table A1 – R&D tax credit introductions 1978 to 2006

State	Year of Introduction	Nominal Rate	Effective Rate
Minnesota	1982	2.50%	2.50%
Indiana	1985	5.00%	5.00%
Iowa	1985	6.50%	6.50%
West Virginia	1986	10.00%	10.00%
Wisconsin	1986	5.00%	4.60%
California	1987	15.00%	13.70%
Kansas	1988	6.50%	0.40%
North Dakota	1988	4.00%	4.00%
Oregon	1989	5.00%	5.00%
Illinois	1990	6.50%	0.50%
Massachusetts	1991	10.00%	10.00%
Connecticut	1993	6.00%	6.00%
Arizona	1994	11.00%	11.00%
Missouri	1994	6.50%	0.50%
New Jersey	1994	10.00%	10.00%
Rhode Island	1994	16.90%	16.90%
Maine	1996	5.00%	0.40%
North Carolina	1996	5.00%	5.00%
Pennsylvania	1997	10.00%	0.90%
Georgia	1998	10.00%	10.00%
Montana	1999	5.00%	5.00%
Utah	1999	6.00%	6.00%
Delaware	2000	10.00%	0.90%
Hawaii	2000	20.00%	20.00%
Maryland	2000	10.00%	0.90%
Idaho	2001	5.00%	5.00%
South Carolina	2001	5.00%	5.00%
Texas	2001	5.00%	5.00%
Louisiana	2003	8.00%	8.00%
Vermont	2003	10.00%	0.90%
Ohio	2004	7.00%	0.50%
Nebraska	2006	3.00%	0.20%

Source: Wilson (2009), tax rates reflect the most recent rate.

Table A2 – The impact of R&D tax credits on R&D, patents, and exploitation vs. exploration (full sample patenting firms)

	a	b	c	d	e
	R&D	Patents	New tech	Known tech	Exploitation ratio
R&D tax credit rate$_{t-3}$	3.098***	1.855**	0.344*	2.409**	2.065**
	(0.902)	(0.877)	(0.177)	(1.106)	(0.981)
N	71,646	71,646	71,646	71,646	71,646
Firm FE	yes	Yes	yes	yes	yes
Year FE	yes	Yes	yes	yes	yes
R^2	0.877	0.761	0.534	0.791	0.559

Notes: All dependent variables are measured in logarithmic form. All models are OLS regressions. Heteroscedasticity-robust standard errors are clustered at the state level and shown in parentheses. ***, **, * indicate statistical significance at the 1%, 5%, 10% level, respectively.

Table A3 – The impact of R&D tax credits on R&D, patents, and exploitation vs. exploration (tax credits measured in *t-2, t-4*)

	a	b	c	d	e
	R&D	Patents	New tech	Known tech	Exploitation ratio
Effective tax rate in t-2					
R&D tax credit rate$_{t-2}$	5.280***	4.131***	1.431***	4.744***	3.313***
	(0.535)	(0.340)	(0.208)	(0.370)	(0.278)
N	22,257	22,257	22,257	22,257	22,257
Firm FE	yes	Yes	yes	yes	yes
Year FE	yes	Yes	yes	yes	yes
R^2	0.892	0.809	0.604	0.839	0.607
Effective tax rate in t-4					
R&D tax credit rate$_{t-4}$	5.524***	4.777***	1.624***	5.496***	3.872***
	(0.501)	(0.345)	(0.209)	(0.345)	(0.264)
N	22,257	22,257	22,257	22,257	22,257
Firm FE	yes	Yes	yes	yes	yes
Year FE	yes	Yes	yes	yes	yes
R^2	0.892	0.809	0.604	0.839	0.607

Notes: All dependent variables are measured in logarithmic form. All models are OLS regressions. The table shows estimations of the models introduced above for the California experiment but with nominal R&D tax credit rate instead of the effective R&D effective credit. Heteroscedasticity-robust standard errors are clustered at the state level and shown in parentheses. ***, **, * indicate statistical significance at the 1%, 5%, 10% level, respectively.

Table A4 – The impact of R&D tax credits on R&D, patents, and exploitation vs. exploration (California sample, nominal tax credit rate)

	a	b	c	d	e
	R&D	Patents	New tech	Known tech	Exploitation ratio
R&D tax credit rate$_{t-3}$	4.395***	2.972***	0.826***	3.697***	2.871***
	(0.475)	(0.258)	(0.159)	(0.289)	(0.213)
N	22,257	22,257	22,257	22,257	22,257
Firm FE	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes
R^2	0.892	0.808	0.604	0.838	0.607

Notes: All dependent variables are measured in logarithmic form. All models are OLS regressions. The tabke shows estimations of the models introduced above for the California experiment but with nominal R&D tax credit rate instead of the effective R&D effective credit. Heteroscedasticity-robust standard errors are clustered at the state level and shown in parentheses. ***, **, * indicate statistical significance at the 1%, 5%, 10% level, respectively.

Table A5 – The impact of R&D tax credits on R&D, patents, and exploitation vs. exploration (California sample, R&D user costs)

	a	b	c	d	e
	R&D	Patents	New tech	Known tech	Exploitation ratio
R&D user costs$_{t-3}$	-4.862***	-3.500***	-1.335***	-4.066***	-2.730***
	(0.874)	(0.590)	(0.155)	(0.723)	(0.709)
N	22,257	22,257	22,257	22,257	22,257
Firm FE	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes
R^2	0.892	0.809	0.604	0.838	0.607

Notes: All dependent variables are measured in logarithmic form. All models are OLS regressions. The table shows estimations of the models introduced above for the California experiment but with R&D user costs instead of the effective R&D effective credit as defined in Wilson (2009) and often previously used in the literature instead of the effective R&D effective credit. Note that R&D tax credits lowered the costs of R&D such that a negative sign implies a positive effect of R&D tax credit introductions and vice versa. Heteroscedasticity-robust standard errors are clustered at the state level and shown in parentheses. ***, **, * indicate statistical significance at the 1%, 5%, 10% level, respectively.

Table A6 – The impact of R&D tax credits on R&D, patents, and exploitation vs. exploration (California sample, effective rate, excluding IT sector: SIC codes 357 'Computer and Office Equipment', 366 'Communications Equipment', and 367 'Electronic Components')

	a	b	c	d	e
	R&D	Patents	New tech	Known tech	Exploitation ratio
R&D tax credit rate$_{t-3}$	4.284***	3.091***	0.730***	3.630***	2.900***
	(0.656)	(0.449)	(0.259)	(0.486)	(0.346)
N	19,070	19,070	19,070	19,070	19,070
Firm FE	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes
R^2	0.895	0.824	0.617	0.859	0.628

Notes: All dependent variables are measured in logarithmic form. All models are OLS regressions. Heteroscedasticity-robust standard errors are clustered at the state level and shown in parentheses. ***, **, * indicate statistical significance at the 1%, 5%, 10% level, respectively.

Table A7 – The impact of R&D tax credits on R&D, patents, and exploitation vs. exploration (California sample, classic Diff-in-Diff, dummy instead of tax credit rate for being treated)

	a	b	c	d	e
	R&D	Patents	New tech	Known tech	Exploitation ratio
Dummy (R&D tax credit rate > 0) $_{t-3}$	0.352***	0.238***	0.066***	0.296***	0.230***
	(0.038)	(0.021)	(0.013)	(0.023)	(0.017)
N	22,257	22,257	22,257	22,257	22,257
Firm FE	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes
R^2	0.892	0.808	0.604	0.838	0.607

Notes: All dependent variables are measured in logarithmic form. All models are OLS regressions. Heteroscedasticity-robust standard errors are clustered at the state level and shown in parentheses. ***, **, * indicate statistical significance at the 1%, 5%, 10% level, respectively.

Table A8 – The impact of R&D tax credits on R&D, patents, and exploitation vs. exploration (California sample, IV approach, using R&D user costs as the instrument for R&D expenses)

	a	b	c	d
	Patents	New tech	Known tech	Exploitation ratio
Log (R&D), est.	0.720***	0.275***	0.836***	0.562***
	(0.086)	(0.054)	(0.088)	(0.087)
N	22,257	22,257	22,257	22,257
First stage F	30.94	30.94	30.94	30.94
Firm FE	Yes	yes	yes	yes
Year FE	Yes	yes	yes	yes

Notes: All dependent variables are measured in logarithmic form. All models are IV regressions where log(R&D) is instrumented with the R&D user costs in t-3. F values are Kleibergen-Paap Wald F statistics of the first stage. Heteroscedasticity-robust standard errors are clustered at the state level and shown in parentheses. ***, **, * indicate statistical significance at the 1%, 5%, 10% level, respectively.

Table A9 – The impact of R&D tax credits on R&D, patents, and exploitation vs. exploration (California sample, effective rate, adding controls)

	a	b	c	d	e
	R&D	Patents	New tech	Known tech	Exploitation ratio
Log(age)	0.096	0.269***	0.128***	0.214***	0.086**
	(0.099)	(0.058)	(0.035)	(0.064)	(0.036)
Capital exp/assets	0.237***	0.201**	0.198***	0.084	-0.114
	(0.076)	(0.078)	(0.056)	(0.083)	(0.073)
ROA	0.008	0.019***	0.008***	0.012*	0.004
	(0.008)	(0.005)	(0.002)	(0.006)	(0.006)
Leverage	-0.009	-0.004	-0.002	-0.004	-0.002
	(0.006)	(0.006)	(0.004)	(0.004)	(0.003)
R&D tax credit rate$_{t-3}$	5.146***	4.081***	1.342***	4.775***	3.433***
	(0.469)	(0.316)	(0.208)	(0.316)	(0.273)
N	21,897	21,897	21,897	21,897	21,897
Firm FE	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes
R^2	0.895	0.813	0.608	0.842	0.610

Notes: All dependent variables are measured in logarithmic form. All models are OLS regressions. Heteroscedasticity-robust standard errors are clustered at the state level and shown in parentheses. ***, **, * indicate statistical significance at the 1%, 5%, 10% level, respectively.

A10 – Original vs. matched sample

The following Table A10 shows the difference in firms' key characteristics, age, total asset and R&D intensity as measured by R&D expenses over total assets for the matched sample before and after CEM matching as explained in the text.

Table A10 - Results of t-tests on mean differences in variables between California firms and control group firms, original vs. matched sample

	Original sample	**Matched sample**
Variable	**t-values for H0: mean(California)-mean(control)=0**	**t-values for H0: mean(California)-mean(control)=0**
R&D intensity Ø (1986+1987)	-2.918***	-0.062
log(age) Ø (1986+1987)	4.463***	0.036
log(total assets) Ø (1986+1987)	4.286***	-0.177

Notes: This table shows the differences in firms' key characteristics, age, total asset and R&D intensity as measured by R&D expenses over total assets in mean values of 1986 and 1987 before and after CEM matching as explained in the main body of the text. *N* = 810. ***, **, * indicate statistical significance at the 1%, 5%, 10% level, respectively.

A11 – Alternative measures of innovative search

In order to distinguish firms in any given year based on their relative focus on exploitation of existing known technologies versus exploration of newer technologies (otherwise referred to as a firm's level of *exploitation search focus*), we use four alternative empirical measures that draw on three distinct sources of information.

Our first measure is the ratio of patents to scientific papers, that is, the number of patents that a company receives, relative to the number of science papers it publishes (Arora, Belonzon and Sheer, 2020). Second, we measure the reliance of patenting on basic research by the fraction of citations to the scientific (non-patent) literature made by the firm's patents (patents cite other patents, and they also cite non-patent literature, and the latter is typically science publications, see Fleming and Sorenson, 2004). Our third measure is the increased reliance on firms' own technologies as measured by increased fraction of self-citations, i.e., cites to firms' own previously filed patents out of all cites to prior art. Fourth, we use the *Internal Search Proximity* score of Fitzgerald et al. (2020), which examines the degree of overlap between patents granted to the firm in year t and the existing patent portfolio held by the same firm up to year $t-1$. It is a variant of the Jaffe (1989) technological proximity measure to estimate the "closeness" in technological space of firm i's new patents in year t (patent flow f) and its pre-existing patent stock g at year $t-1$ using patent counts in different USPTO three-digit technology classes k:

$$Internal\ Search\ Proximity_{i,t} = \frac{\sum_{k=1}^{K} f_{i,k,t} g_{i,k,t-1}}{\left(\sum_{k=1}^{K} f_{i,k,t}^2\right)^{\frac{1}{2}} \left(\sum_{k=1}^{K} g_{i,k,t-1}^2\right)^{\frac{1}{2}}} \quad (1)$$

where $f_{i,k,t}$ is the fraction of patents applied by firm i in year t that are in technology class k such that the vector $f_{i,t} = \left(f_{i,1,t} \dots f_{i,K,t}\right)$ locates the firm's year t patenting activity in K-dimensional technology space and $g_{i,k,t-1}$ is the fraction of all patents applied for by firm i up to (and including) year $t-1$ that are in technology class k such that vector $g_{i,t-1} = \left(g_{i,1,t-1} \dots g_{i,K,t-1}\right)$ locates the firm's patent stock in K-dimensional technology space.[23] *Internal Search Proximity* will be zero for a given firm year when there is no overlap in a firm's innovative output in year t with the firm's patent stock at time $t-1$, while *Internal Search Proximity* will equal one when the technology class distribution of firm i's patents granted this year is identical to that of patents accumulated in

[23] When computing *Internal Search Proximity* measures for each firm, we only use patents initially granted to the firm itself (since these patents are internally generated based on the firm's R&D activities). In robustness tests, we also include patents acquired by the firm in our calculations and find qualitatively similar results.

previous years. Therefore, firms are classified as being relatively more focused on exploitation /(exploration) when they have high/(low) value of *Internal Search Proximity*.

Table A11 – The impact of R&D tax credits on patents per scientific article, fraction of cites to the scientific literature, fraction of self-citations, and search proximity

	a	b	c	d
	Patents per scientific article	Fraction cites to scientific literature	Fraction self-citations	Internal Search Proximity
R&D tax credit rate$_{t-3}$	1.832***	-0.187***	0.086**	1.176***
	(0.414)	(0.023)	(0.038)	(0.159)
N	10233	9,376	9,376	7,049
Year FE	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes
R^2	0.652	0.580	0.385	0.498

Notes: All dependent variables are measured in logarithmic form. All models are OLS regressions. Heteroscedasticity-robust standard errors are clustered at the state level and shown in parentheses. ***, **, * indicate statistical significance at the 1%, 5%, 10% level, respectively.

Table A12 – Count data models

Panel A: Original Sample

	a	b	c	d	e	f
	Patents	New tech	Known tech	Patents	New tech	Known tech
R&D tax credit rate$_{t-3}$	12.935***	14.683***	4.668***	13.205***	14.847***	4.261***
	(1.619)	(1.682)	(0.658)	(3.722)	(4.027)	(1.526)
N	22257	22257	22257	20640	13442	20585
Firm FE	no	no	no	yes	yes	yes
State FE	yes	yes	yes	no	no	no
Year FE	yes	yes	yes	yes	yes	yes

Panel B: Matched Sample

	a	b	c	d	e	f
	Patents	New tech	Known tech	Patents	New tech	Known tech
R&D tax credit rate$_{t-3}$	11.630***	13.289***	3.506***	11.761***	13.336***	2.948
	(1.927)	(2.100)	(1.080)	(3.494)	(3.866)	(1.853)
N	12590	12590	12590	11850	7924	11828
Firm FE	no	no	no	yes	yes	yes
State FE	yes	yes	yes	no	no	no
Year FE	yes	yes	yes	yes	yes	yes

Notes: All models are Poisson regressions. Heteroscedasticity-robust standard errors are clustered at the state level and shown in parentheses. ***, **, * indicate statistical significance at the 1%, 5%, 10% level, respectively.

Part B: Robustness checks for markups

Table B1 – Alternative markup estimates

	a	b	c	d	e	f
	Time-varying coefficients	Translog production function	Controlling for SGA	Controlling for market share	Forlani et al.	Controlling for TFP, TFP^2, TFP^3
R&D tax credit rate$_{t-3}$	0.546***	0.745***	0.341***	0.425**	0.507*	0.207**
	(0.082)	(0.087)	(0.123)	(0.180)	(0.261)	(0.100)
N	21759	21733	19523	21833	19438	21759
Firm FE	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes

Notes: The dependent variable is the markup, defined as the ratio of price to marginal costs, estimated following De Loecker, Eckhout and Unger (2020). R&D tax credit is the effective rate as calculated by Wilson (2009). OLS regressions, heteroscedasticity-robust standard errors are clustered at the state level. ***, **, * indicate statistical significance at the 1%, 5%, 10% level, respectively.

Table B2 – Industry-level measure of uncertainty

	a	b	c	d
	Original sample		matched sample	
Subsample:	High uncertainty	Low uncertainty	High uncertainty	Low uncertainty
R&D tax credit rate$_{t-3}$	0.802***	0.129	0.653**	0.159
	(0.184)	(0.094)	(0.311)	(0.156)
N	10825	10934	6842	5395
Firm FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
R^2	0.008	0.011	0.039	0.030

Notes: The dependent variable is the markup, defined as the ratio of price to marginal costs, estimated following De Loecker, Eckhout and Unger (2020). R&D tax credit is the effective rate as calculated by Wilson (2009). High uncertainty (low uncertainty) subsample consists of firms whose standard deviation of profits relative to the absolute level of average profits areabove the median, based on observations before 1987. OLS regressions, heteroscedasticity-robust standard errors are clustered at the state level. ***, **, * indicate statistical significance at the 1%, 5%, 10% level, respectively.

Table B3 – Markups and alternative measures of tax credit exposure

	a	b	c	d
R&D tax credit rate$_{t-2}$	0.633***			
	(0.100)			
R&D tax credit rate$_{t-4}$		0.624***		
		(0.100)		
R&D tax nominal rate $_{t-3}$			0.508***	
			(0.086)	
R&D user cost $_{t-3}$				-0.565***
				(0.083)
N	21759	21759	21759	21759
Firm FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes

Notes: The dependent variable is the markup, defined as the ratio of price to marginal costs, estimated following De Loecker, Eckhout and Unger (2020). R&D tax credit is the effective rate as calculated by Wilson (2009). OLS regressions, heteroscedasticity-robust standard errors are clustered at the state level. ***, **, * indicate statistical significance at the 1%, 5%, 10% level, respectively.

Table B4 – Markups and tax credits in the full sample

	a	b	c	d	e
Sample	All states and years, all firms	Firms that patent	Firms that do not patent	Firms that engage in R&D	Firms that do not engage in R&D
R&D tax credit rate$_{t-3}$	0.334**	0.700***	0.098	0.619***	0.143
	(0.156)	(0.255)	(0.107)	(0.215)	(0.122)
N	203531	70294	133237	89518	114013
Firm FE	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes

Notes: The dependent variable is the markup, defined as the ratio of price to marginal costs, estimated following De Loecker, Eckhout and Unger (2020). R&D tax credit is the effective rate as calculated by Wilson (2009). OLS regressions, heteroscedasticity-robust standard errors are clustered at the state level. ***, **, * indicate statistical significance at the 1%, 5%, 10% level, respectively.

Part C: Theoretical Model

We assume a two-period model (t = 2) (Manso (2011) shows that the fundamental logic and results hold for longer periods and team production). In each period, a firm decides to invest in a research strategy $r \in [0,1]$. The research strategy, r, reflects the firm's balance between investing into a well-known research strategy (exploitation, T) and investing in a novel research strategy (exploration, R). $r = 0$ is equivalent to fully investing into a well-known research strategy (exploitation, T), with known probability p of success (S_T) and $(1-p)$ of failure (F_T), with $S_T > F_T$. $r = 1$ is equivalent to investing in a novel research strategy (exploration, R) with an unknown probability q of success (S_R) and $(1-q)$ of failure (F_R), such that $S_R > S_T > F_T > F_R$ (all outcomes are expressed as after-tax). For any choice of research strategy, r, the four possible outcomes occur with probabilities: $(1-\mathrm{r})p$ of S_T, $(1-r)(1-p)$ of F_T, rq of S_R, and $r(1-q)$ of F_R.

Additionally, in this model we assume that exploration generates not only higher return in case of success than exploitation, but at the same time has a higher volatility of returns, represented by the outcome in case of an exploration failure that is lower than in case of an exploitation failure.

Learning about q increases as the research strategy is balanced towards exploring (r increases). The expected probability of success when exploring is $E[q]$ in the first period and in the second period after realizing S_T or F_T. $E[q|S_R]$ after experiencing a S_R, and $E[q|F_R]$ after experiencing a F_R. From Bayes' rule follows: $E[q|F_R] < E[q] < E[q|S_R]$.

Exploration requires that the firm experiments. Hence, it is initially not as likely to succeed when it explores. Exploration might still be perceived as more beneficial than exploitation, because after the first period the firm updates its beliefs about the probability q of success with exploration, meaning that if the firm succeeds in finding something interesting in the first period, exploration is then perceived as better than exploitation. This is captured as follows:

$$E[q] < p < E[q|S_R]$$

Additionally, we assume the following relationship is true:

$$S_R E[q] + F_R(1 - E[q]) < S_T p + F_T(1-p)$$

A fully specified strategy for the firm is a decision in the first period r, and a set of contingent strategies that depend on the information set the firm has in the second period (r_2).

In the second period, the firm decides conditional on their beliefs.

With belief $E[q|S_R]$ the firm's payoff in period 2 is equal to:

$$\pi_{2,S_R} = (r_2(S_R E[q|S_R] + F_R(1 - E[q|S_R])) + (1 - r_2)[S_T p + F_T(1 - p)]$$

$$\frac{\partial \pi_{2,S_R}}{\partial r_2} = S_R E[q|S_R] + F_R(1 - E[q|S_R]) - (S_T p + F_T(1 - p))$$

$\frac{\partial \pi_{2,S_R}}{\partial r_2} > 0$, and thus having learned that exploration is more valuable, the firm optimally chooses to explore ($r_2 = 1$).

With belief $E[q|F_R]$ the firm's payoff in period 2 is equal to:

$$\pi_{2,F_R} = (r_2(S_R E[q|F_R] + F_R(1 - E[q|F_R])) + (1 - r_2)[S_T p + F_T(1 - p)]$$

$$\frac{\partial \pi_{2,F_R}}{\partial r_2} = S_R E[q|F_R] + F_R(1 - E[q|F_R]) - (S_T p + F_T(1 - p))$$

$\frac{\partial \pi_{2,S_R}}{\partial r_2} < 0$, hence using a similar logic, having learned that exploration is not valuable, the firm optimally exploits ($r_2 = 0$)

And lastly, with belief $E[q]$, the payoff is equal to:

$$\pi_2 = (r_2(S_R E[q] + F_R(1 - E[q])) + (1 - r_2)[S_T p + F_T(1 - p)]$$

$$\frac{\partial \pi_2}{\partial r_2} = S_R E[q] + F_R(1 - E[q]) - (S_T p + F_T(1 - p))$$

Absent learning motives, the firm optimally exploits: $\frac{\partial \pi_2}{\partial r_2} < 0$ ($r_2 = 0$).

Following Manso (2011), we assume risk-neutrality and a discount factor of δ. The first relevant action plan requires exploitation in both periods, as there is no chance to learn about something new if a firm sticks to its own knitting. The other relevant action plan, exploration, is to experiment in the first period and continue exploring only if success occurs, as we have just illustrated above. The firm's total payoff is thus the following ($r_1 = r$):

$$\begin{aligned}\pi = {} & S_T(1 - r)p + F_T(1 - r)(1 - p) + S_R r E[q] + F_R r(1 - E[q]) \\ & + \delta(rE[q](S_R E[q|S_R] + F_R(1 - E[q|S_R])) + r(1 - E[q])\big(S_T p + F_T(1 - p)\big) \\ & + [S_T(1 - r)p + F_T(1 - r)(1 - p)])\end{aligned}$$

Maximizing the payoff with respect to r:

$$\begin{aligned}\frac{\partial \pi}{\partial r} = {} & -S_T p - (1 - p)F_T + S_R E[q] + F_R(1 - E[q]) \\ & + \delta[E[q](([E[q|S_R]S_R + (1 - E[q|S_R])F_R) - (pS_T + (1 - p)F_T))]\end{aligned}$$

It follows that the total payoff from exploration is higher than the total payoff from exploitation $\left(\frac{\partial \pi}{\partial r} > 0\right)$ if:

$$E[q] \geq \frac{p(S_T - F_T) + (F_T - F_R)}{(S_R - F_R)(1 + \delta E[q|S_R]) - \delta[p(S_T - F_T) + (F_T - F_R)]}$$

Adding R&D tax credits

We now introduce R&D tax credits into the model. Assume that the firm's R&D expenditures are fixed and equal to RD, where $S_R > S_T > F_T > RD > F_R$, meaning that in case of failure during exploration the firm makes no profit.

Tax credits allow firms to deduct a fraction of their R&D expenses from their taxable income. Following the discussion in section 3, R&D tax credit can only be used in the firm's profit after accounting for R&D expenditures is larger or equal than R&D. Let the tax credit rate be denoted as c, thus the received tax credit will be cRD unless the firm fails in case during exploration (payoff F_R), in which case the R&D tax credit is equal to cF_R.

In the second period, the firm decides conditional on their beliefs (we assume that the decisions are the same as in the absence of R&D tax credit):

With belief $E[q|S_R]$ the payoff is:

$$\pi^*_{2,S_R} = (r_2((S_R + cRD)E[q|S_R] + (F_R + cF_R)(1 - E[q|S_R])) + (1 - r_2)[(S_T + cRD)p + (F_T + cRD)(1 - p)]$$

$$\frac{\partial \pi^*_{2,S_R}}{\partial r_2} = S_R E[q|S_R] + F_R(1 - E[q|S_R]) - (S_T p + F_T(1 - p)) + (cRD - F_R)(E[q|S_R] - 1) =$$

$$\frac{\partial \pi_{2,S_R}}{\partial r_2} + (cRD - F_R)(E[q|S_R] - 1) < \frac{\partial \pi_{2,S_R}}{\partial r_2}, \text{ as } (cRD - F_R)(E[q|S_R] - 1) < 0$$

As $\frac{\partial \pi_{2,S_R}}{\partial r_2} > 0$, we shall additionally assume that $\frac{\partial \pi^*_{2,S_R}}{\partial r_2} > 0$ too, thus having learned that exploration is valuable, the firm optimally explores ($r_2 = 1$).

With belief $E[q|F_R]$ the payoff is:

$$\pi^*_{2,F_R} = (r_2((S_R + cRD)E[q|F_R] + (F_R + cF_R)(1 - E[q|F_R])) + (1 - r_2)[(S_T + cRD)p + (F_T + cRD)(1 - p)]$$

$$\frac{\partial \pi^*_{2,F_R}}{\partial r_2} = \frac{\partial \pi_{2,F_R}}{\partial r_2} + (cRD - F_R)(E[q|S_R] - 1) < 0$$

$\frac{\partial \pi^*_{2,S_R}}{\partial r_2} < 0$, thus having learned that exploration is not valuable, the firm optimally exploits ($r_2 = 0$)

With belief $E[q]$, the payoff is:

$$\pi_2^* = (r_2((S_R + cRD)E[q] + (F_R + cF_R)(1 - E[q])) + (1 - r_2)[(S_T + cRD)p + (F_T + cRD)(1 - p)]$$

$$\frac{\partial \pi_2^*}{\partial r_2} = \frac{\partial \pi_2}{\partial r_2} + (cRD - F_R)(E[q|S_R] - 1) < 0$$

Absent learning motives, the firm optimally exploits: $\frac{\partial \pi_2^*}{\partial r_2} < 0$ $(r_2 = 0)$.

The new total payoff is then the following:

$$\begin{aligned}\pi^* = &(1 - r)p(S_T + cRD) + (1 - p)(1 - r)(F_T + cRD) + rE[q](S_R + cRD) \\ &+ r(1 - E[q])(F_R + cF_R) \\ &+ \delta\left[\left(p(1 - r)(S_T + cRD) + (1 - p)(1 - r)(F_T + cRD)\right)\right. \\ &+ (rE[q](E[q|S_R](S_R + cRD) + (1 - E[q|S_R])(F_R + cF_R)) \\ &\left.+ r(1 - E[q])(p(S_T + cRD) + (1 - p)(F_T + cRD)))\right],\end{aligned}$$

where $\frac{\partial \pi^*}{\partial r} = \frac{\partial \pi}{\partial r} - c(RD - F_R)(1 - E[q] + \delta E[q](1 - E[q|S_R]))$ or we could express it as:

$$\frac{\partial \pi^*}{\partial r} - \frac{\partial \pi}{\partial r} = -c(RD - F_R)\left(1 - E[q] + \delta E[q](1 - E[q|S_R])\right) < 0$$

Given that $c(RD - F_R) > 0$, and $1 - E[q] + \delta E[q](1 - E[q|S_R]) > 0$, as long as $E[q] \in (0,1)$, introduction of R&D tax credit will always make an option to explore look worse.

Thus, under the introduction of an R&D tax credit, firms are more likely to pursue exploitation search strategies as opposed to exploration search strategies. The intuition is that R&D tax credits make experimenting and exploration more costly relative to exploitation, because exploration increases the likelihood that R&D costs can either not be expensed – or expensed only later at a discounted rate. That exploration may yield higher returns than exploitation does not matter since the size of the monetary returns from governmental tax credits are constrained by profits.