The effect of capital gains tax policy changes on long-term investments

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Abstract: Pressure from short-horizon investors can hurt investment in innovative, long-run value increasing projects. We explore the efficacy of a commonly proposed tax-based policy tool to mitigate this problem: imposition of greater taxes on short-term capital gains relative to long-term capital gains. Using a panel of 30 OECD countries and 21 capital gains tax shocks over 1991-2006, we find that rewarding longer-term ownership through lower capital gains taxes results in an increase in corporate innovation. The evidence should be of interest to lawmakers and regulators and also adds to our understanding of the real effects of taxation of investor trading activity.

JEL Classification: G38, H20, H24, O31
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1. Introduction

Investments in innovative technologies and products are important for long-term economic growth (Solow 1957; Romer 1990). Research-based and anecdotal evidence suggests that pressure from short-horizon investors impedes corporate innovation.[1] The argument is that pursuing innovation often requires extensive upfront investments in R&D, human capital, and other intangibles that are expected to payoff over the long-run but depress short-term profits. The long-run value created by such investments may not be apparent to short-horizon investors who are likely to devote their research efforts toward forecasting quarterly profits instead of trying to understand the long-term future prospects of a firm's R&D portfolio. As a result, corporate managers worry that short-horizon investors might perceive the poor short-term profitability of these investments as a sign of incompetent management or poor business prospects, hurting stock price performance. Anticipating such an outcome, managers who are sufficiently concerned about short-term stock price decline would forgo some long-term investments.

In this paper, we ask whether tax policy can mitigate this myopic underinvestment in innovation. Specifically, we evaluate the efficacy of a commonly proposed tax-based policy tool: imposition of greater taxes on short-term capital gains relative to long-term capital gains. The hypothesis is that a tax structure that rewards long-term ownership would motivate investors to hold stocks for longer periods. This, in turn, would alleviate the myopic pressures on managers stemming from a fear of stock price decline caused by short-term trading, thereby mitigating the under investment problem.[2] In support of this idea, several research studies find that the

[1]In a survey, Graham, Harvey, and Rajgopal (2005) find 78% of corporate managers admit to sacrificing economic value to achieve quarterly earnings targets and consider pressure from institutional investors as one of the main reasons for this myopic behavior. See, Bushee (1998), Fang, Tian, and Tice (2014), Bernstein (2015), Asker, Farre-Mensa, and Ljungqvist (2015), and Agarwal, Vashishtha, and Venkatachalam (2018) for archival evidence on how pressure from short-term oriented investors results in underinvestment in long-term projects. See Shleifer and Vishny (1990) for a formal model of how short-term oriented investors can generate corporate myopia.

[2] See Chemmanur and Ravid (1999) for an analytical demonstration of this idea.

incentive to save capital gains taxes indeed locks both individual investors and mutual funds into longer holding horizons (Reese, 1998; Huddart and Narayanan, 2002; Cici, 2012; Ivković, Poterba, and Weisbenner, 2005; Sialm and Starks, 2012). Furthermore, Dimmock, Gerken, Ivkovic, and Weisbenner (2018) find that this capital gains "lock-in" effect also influences investors' behavior in a way that suggests long-term engagement with investee firms.

Lawmakers, regulators, and politicians also tout the virtues of using differential capital gains taxes to mitigate corporate myopia despite the potential costs.[3] For example, Democratic presidential candidate Hillary Clinton proposed eliminating long-term capital gains taxes for some investments to promote start-ups and help struggling communities (Rosenfeld 2015, CNBC). Reduction in corporate myopia was explicitly offered as a rationale for the differential tax penalty for short-term capital gains in the 1997 tax law (Chemmanur and Ravid, 1999). The idea has also received support from the business community (e.g., the letter by Laurence Fink, CEO of BlackRock to the S&P 500 CEOs).

Despite the policy relevance and theoretical justification, little empirical evidence exists on whether capital gains tax policies mitigate the myopic underinvestment problem. The aim of this study is to fill this gap in the literature. To accomplish this task, we compile a comprehensive international dataset of capital gains tax rates for 30 OECD countries over a period 1991 to 2006. During this time period, we find 21 changes to the tax code for the capital gains tax structure, i.e., the difference in the tax rates on short-term vs long-term capital gains. Such a rich variation in the capital gains tax code, spanning a long time-period allows us to construct powerful tests to detect the effects of capital gains taxes on corporate innovation. Supporting our empirical

[3] Since capital gains taxation affects individuals' trading behavior (e.g., Reese, 1998), a differential capital gains tax can lead to efficiency losses because individuals' portfolio reallocation is distorted (Stiglitz, 1983; Constantinides, 1984; Auerbach, 1991). For example, capital gains taxes may prevent shares from being sold even if a sale would be beneficial for both, buyer and seller.

approach, we document that in contrast to other tax rates such as corporate taxes or consumption taxes (e.g., Vegh and Vuletin, 2015), changes in the difference between long-term and short-term capital gains taxes do not appear to be systematically correlated with economic conditions (GDP growth, inflation, or unemployment).

We use a generalized difference-in-differences (DiD) design in which we estimate how the change in innovation output of a country (first difference) varies around a change in the additional taxes charged on short-term relative to long-term capital gains (second difference). We measure the innovation output of a country at the industry level using successfully granted patents to a country-industry by the U.S. Patent Office (USPTO).[4] The industry classification we use refers to the "patent classes" defined by the USPTO based on a highly elaborate system to classify technological innovations.

We find that when countries increase the reward for longer-term ownership by charging additional taxes on short-term relative to long-term capital gains (henceforth, *TaxDiff*) firms in that country exhibit a significant increase in innovation output. The effects are economically large: a five percentage point decrease in taxes on long-term relative to short-term gains results in a 2-3% increase in the annual innovation output by the end of three years after the tax shocks. In support of the parallel trend assumption inherent in the DiD design, we find no evidence of innovation changes in any of the five years prior to the tax shocks.

One alternative explanation for our results is that capital gains taxes affect the supply of capital and the cost of equity (e.g., Becker, Jacob, and Jacob, 2013; Huizinga, Voget, and Wagner, 2018; Dimitrova and Eswar, 2018). To rule out this alternative explanation, we split the

[4] Prior research (for example, Griffith, Harrison, and Van Reenen (2006), Acharya and Subramanian (2009), and Hsu, Tian, and Xu (2014) uses USPTO patent data to assess foreign firms' innovation output. This approach is based on the assumption that because US is one of the largest consumers of technological innovation and most meaningful innovations that firms intend to patent would get covered by USPTO.

effect into changes in short-term or long-term capital gains taxes. We find that our results hold regardless of whether *TaxDiff* increases due to an increase in short-term capital gains tax or a decrease in long-term capital gains tax. The fact that innovation increase manifests even after short-term capital gains tax increases implies our findings cannot be explained by a potential decrease in cost of capital following capital gains tax cuts.

Another potential concern is that our results could be driven by other correlated shocks coinciding with capital gains tax code changes that also affect firms' ability and incentives to innovate. There are two main possibilities in this regard. First, the tax changes may coincide with increase in demand for technological consumption in the U.S. markets. We view this possibility as remote as it requires such demand shocks to coincide with 21 different tax shocks in our sample staggered over a period of 16 years. Nevertheless, we include industry-year fixed effects in the regressions that fully absorb any trends in innovation output that result from demand shocks at a given patent-class level. These specifications thus exploit within industry-year variation; that is, the effect of tax shocks is estimated by comparing the change in innovation output in a specific technology-class of a country experiencing a tax shock (treated patent class) to the contemporaneous change in innovation output in same technology-class in another country that does not exhibit a tax shock (counterfactual patent class). Our results are robust to this specification change.

The other possibility is that the capital gains tax changes may coincide with changes in local economic conditions that improve the local economy's ability to supply innovation for reasons unrelated to reduction in short-term pressures. Nevertheless, we take several steps to address this concern. First, we include extensive controls for the local economic conditions as well as corporate taxation to control for such shocks and the economic condition. Second, we conduct a placebo test using the innovation output of government organizations who are tax exempt and therefore should not be affected by capital gains tax shocks. We do not find evidence of increase in innovation output of government organizations around the tax shocks. To the extent innovation by government organizations is also affected by other confounding local economic shocks (e.g., greater availability of scientific talent in good times), this analysis further mitigates this concern. Third, we examine if our results are confounded by changes in other tax provisions that might be implemented as part of larger reform packages. Specifically, we additionally control for R&D tax incentives (to account for tax policy targeted at innovation) and personal income tax rates on wages (to account for labor costs for highly skilled employees) in supplemental tests. Our results are robust to controlling for these tax elements.

Finally, we find two cross-sectional patterns in the innovation increase that are not predicted by the explanation based on general improvements in innovation ability. Instead, these patterns are consistent with the results reflecting a decrease in short-term capital market pressures. First, we find that the innovation increase is greater in countries that: (i) are dominated by publicly owned firms (as compared to private firms characterized by illiquid, longer term ownership) and (ii) have high stock market turnover (as compared to stock markets with less active trading and thus less short-term trading). Second, we find that the innovation increase is greater for innovations that are likely to yield more long-term business opportunities and, thus, are more vulnerable to myopic pressures.

Our study contributes to the literature by providing evidence on how differential taxation of short-term and long-term capital gains can affect corporate investment in patentable innovation. This evidence should be of interest to lawmakers and regulators because they often recommend using capital gains taxation as a tool to combat corporate myopia by rewarding long-term investor ownership. More broadly, our paper informs the debate on whether taxes on trading activity can curb investor behavior that engenders negative social externalities. The idea of using such taxes has been proposed by many economists starting with Keynes (1936). For example, Stiglitz (1989) proposed using financial transaction taxes to discourage speculative short-term trading that could generate negative externalities on the real economy. Several studies have empirically explored how financial transaction taxes affect investor trading behavior and return volatility.[5] We add to this literature (i) by examining the effect of capital gains taxes as opposed to financial transaction taxes, and more important, (ii) by directly examining the effects of these taxes on the real economy beyond trading behavior.

Our study is also related to prior studies that examine how increased capital gains taxes can affect investee firms by reducing investors' willingness to supply capital at reasonable prices. In the setting of cross-border takeovers, Huizinga et al. (2018) find that higher capital gains tax rates increase cost of equity. Using state-level changes in capital gains tax rates within the U.S., Dimitrova and Eswar (2018) find evidence consistent with higher capital gains tax rates hurting innovation output of private start-up firms by reducing the supply of Venture Capital (VC) funding. We examine a different explanation than the supply channel, i.e., how changing investors' holding horizon through capital gains taxes affect firm behavior. Thus, our study adds to the growing body of evidence (highlighted in footnote 1) that highlights the crucial role of investor horizon in affecting corporate investment for the long run.

[5] For evidence on the effect of financial transaction taxes, see, for example, Roll (1989), Umlauf (1993), Jones and Seguin (1997), Baltagi, Li, and Li (2006), Hau (2006), Liu and Zhu (2009), Pomeranets and Weaver (2018), and Colliard and Hoffmann (2017). For evidence on the effect of capital gains taxes on investor trading behavior, see, for example, Reese (1998), Blouin, Ready, and Shackelford (2003), Ivković, Poterba, and Weisbenner (2005), Dai, Maydew, Shackelford, and Zhang (2008), Seida and Wempe (2000), and Jacob (2018).

2. Arguments for how capital gains taxes alleviate myopic underinvestment in innovation

A significant body of theoretical and empirical work documents that short-horizon investors can make it difficult for corporate managers to undertake innovative investments with long gestations periods. The reason is that short-horizon investors are more likely to devote their costly information acquisition activities toward forecasting quarterly profits instead of learning about the long-term prospects of a firm's R&D portfolio. As a result, managers of firms with significant short-term ownership run the risk that the poor short-term profitability of long-term investments might be mistaken as a sign of poor business prospects by short-term traders, who might sell their shares, resulting in price decline. This can result in an equilibrium where managers who are sufficiently concerned about short-term price permanently forgo long-term investments (e.g., Shleifer and Vishny, 1990).

We expect capital gains taxation to affect corporate innovation by changing the above described capital market pressures associated with short-term ownership. To elaborate, suppose a country changes its capital gains tax scheme in a way that rewards long-term ownership (i.e., either by lowering (increasing) taxes on long-term (short-term) capital gains). This change in tax regime should ease managerial concerns about the threat of selling based on a short-term decrease in profits for at least two reasons. First, the change should reduce the extent of short-term trading because, all else equal, investors would want to hold the stocks longer to qualify for the favorable tax treatment. This prediction is supported by empirical evidence that the capital gains lock-in effect exists already a few months after stocks are purchased (e.g., Ivković, Poterba, and Weisbenner, 2005). Second, as the investors' return depends more on long-term value of investee firms because of longer holding periods, they should have greater motivation to expend research effort toward collecting information about the long-term prospects of investee firms’ R&D portfolio (e.g., Goldman and Slezak, 2003). The latter effect should further reduce managerial concern that short-horizon traders might mistake a short-term decline in quarterly profits as a sign of poor business prospects and sell their shares. Overall, such a change in capital gains taxation should increase managers’ willingness to undertake long-term investments by easing the capital market pressures that come with short-term investor base.

The above discussion highlights that our hypothesis rests on the premise that capital gains taxes actually influence investors’ holding horizon. While we are unable to directly test this assumption in our sample because of lack of sufficiently detailed international data on investor ownership, significant prior evidence exists on this matter using US data. For example, Ivković, Poterba, and Weisbenner (2005) show that lower long term capital gains taxes lock individual investors into longer holding horizons. Furthermore, studies (e.g., Huddart and Narayanan, 2002; Cici, 2012; Sialm and Starks, 2012) find that even the trading decisions of mutual funds – who pass on the capital gains tax burden to individual investors – are influenced by the capital gains tax lock-in effect on individual investors. This is consistent with the evidence from Bergstresser and Poterba (2002) who find that ignoring the tax incentives of their clients can be costly for mutual funds as it affects their fund flows. Most relevant to our study, Dimmock et al. (2018) find that the capital gains tax lock-in affects investor voting behavior in a way that reflects long-term engagement with the investee firm. Specifically, they find that instead of voting with their feet, locked in mutual funds are more likely to exercise governance through “voice” by voting against a management. For such behavior, they find that a critical holding period of one year – the U.S. cutoff for short-term versus long-term capital gains – is sufficient to induce long-term oriented voting behavior. Our study builds upon this prior evidence by examining whether capital gains tax lock-in also allows managers to more freely invest innovative projects with longer gestation periods.

3. Data and Sample

3.1 Measurement of capital gains tax changes

We collect data on capital gains taxation of individuals for OECD countries for the period 1990–2006 from all available issues of the PricewaterhouseCoopers Worldwide Individual Tax Summaries, Coopers and Lybrand International Tax Summaries, and the Ernst and Young Worldwide Personal Tax Guide. We augment this dataset with data from Jacob and Jacob (2013). Because there is no uniform tax rate on capital gains for all individuals, we need to make some simplifying assumptions to summarize capital gains taxation into a single measure. Whenever capital gains are taxed at a progressive tax rate, we assume that the investor is in the top income tax bracket. The rationale of this assumption is that ownership of shares is concentrated among higher income individuals (e.g., Piketty 2015; Saez and Zucman, 2016; Alstadsæter et al., 2017).[6]

Following prior literature, we assume that the marginal investor is subject to capital gains taxation as, e.g., investors such as mutual funds are treated as pass-through entities for tax purposes.[7] We also assume that the marginal investor is located in the same country as the firm following the "home bias" literature (Coval and Moskowitz 1999).[8] Prior literature suggests that

[6] We further assume that individuals hold a non-substantial shareholding. In a few countries such as Germany or Austria, capital gains taxation depends on the level of ownership (e.g., 1% of total equity in Germany). In case individuals own equity through a mutual fund, individuals would be taxed as non-substantial even if the mutual fund owns more than 1% of the firm (as long as the individual's share in the fund multiplied by the fund's share is below 1%). This as a reasonable assumption given the empirical evidence on capital gains realizations, for example, in Germany (Jacob 2013).
[7] For example, Becker et al. (2013) argue that outside the United States, investment funds are ultimately taxed as if the private investor held the stock directly (see, also, Jacob and Jacob 2013).
[8] The taxation of capital gains is typically residence based. That is, if an investor is located in country A, his or her realized capital gains will be taxed only in country A even if he or she sells stocks of firms located in another

assuming tax-sensitive investors in our setting is defendable. For example, Ferreira and Matos (2008) document that, in contrast to the U.S., the share of institutional ownership is below 20% (see their Table A.3) outside the U.S. In Europe and Asia, firms are predominantly owned by families or there is dispersed ownership, while charities, trusts, and governments, i.e., tax-exempt investors, own relatively small shares (Claessens, Djankov, and Lang, 2000; Faccio and Lang, 2002). Hence, the vast majority of investors are tax-sensitive during our sample period. For these reasons, personal income taxes can affect corporate decision-making.[9]

Important for our purposes, several countries such as Germany, the United States, or Spain tax capital gains at different rates depending on how long an investor holds a stock. In this case, the marginal tax rate on short-term gains (i.e., capital gains realized within the holding period) is typically larger than the tax rate on long-term gains (i.e., capital gains realized outside the holding period). The critical holding period often carries the name "speculation period" and is implemented as an incentive to hold shares for a certain amount of time. These holding periods range from three months (some years in the Czech Republic) to one year (e.g., Germany, the United States) to three years (e.g., Slovakia). The average and median holding period of our "treatment" sample countries that charge different taxes on short-term and long-term gains is about 1 year (result not tabled). Recall that Dimmock et al. (2018) document that in the U.S. setting a critical holding period of one year induces long-term oriented voting behavior. Since capital gains taxation causes a substantial capital gains lock-in effect already several months

country. This is the default case according to the OECD model tax treaty. Foreign investors will be taxed according to the capital gains tax rate in their respective country of residence.

[9] Empirical evidence suggest that personal tax rates on dividends and capital gains matter for (1) investments (Becker et al. 2013), (2) corporate payout (Jacob and Jacob 2013), and (3) capital structure (Faccio and Xu 2015). However, none of these papers not considers the difference between short-term and long-term capital gains taxes.

after a stock is purchased (Ivković, Poterba, and Weisbenner 2005), the median holding period of one year in our sample is sufficiently long to cause changes in corporate behavior.

In our empirical tests, we focus on the difference between short-term and long-term capital gains tax rates. We denote this variable *TaxDiff*. In case a country taxes short-term and long-term capital gains at the same rate, we set *TaxDiff* to 0% and the holding period to 0.

From the OECD countries with patent data and capital gains tax data, we exclude Australia and Luxembourg. We exclude Australia because the effective capital gains tax rate cannot be determined until 1999. During this period, Australia allowed for an indexation for inflation. Since the indexation for inflation affects the tax rate as a function of the holding period as well as the actual share price appreciation, we are unable to calculate a precise capital gains tax rate. We exclude Luxembourg following several other studies on cross-border capital flows (e.g., Amiram and Frank 2016). Luxembourg is characterized by a very high ratio of capital inflows and outflows relative to its GDP (see, for example, the Coordinated Portfolio Investment Survey data by the IMF). To ensure that our results are not driven by data from a country known as a hub for investments, we drop Luxembourg.[10]

Table 1 presents a comprehensive set of summary statistics of the tax shocks. Panel A shows the distribution of tax shocks across years, Panel B provides additional information on the size of the tax shocks, while Panel C provides distribution of tax shocks by country. Our sample contains 21 tax shocks that are reasonably staggered over time and there is at least one tax shock in ten of the 16 years in our sample. There is even distribution among shocks that increase or decrease *TaxDiff* (10 increases and 11 decreases). Changes in *TaxDiff* are experienced by seven of the 30 non-US OECD countries in our sample. Among countries that changed their relative

[10] Our results are insensitive to the inclusion of these two countries (see Section 5.6).

short-term capital gains tax rates, Austria had the least number of shocks (one), while Spain and Turkey experienced a change in the tax differential rate four times during the sample period.[11]

The size of the shocks is economically meaningful: the mean size of the tax increases ranges from one percentage point (Spain) to 50 percentage points (Austria), and the mean size of the tax decreases varies from -0.8 percentage point (Germany) to 50 percentage points (Austria). The majority of the tax shocks (13 out of 21) represent at least a five percentage points change in the tax differential between short-term and long-term capital gains.

In addition to collecting data on capital gains taxes, we also collect data on corporate tax rates. We obtain this data from Jacob and Jacob (2013) as well as Jacob et al. (2018). We add missing years from all available issues of the PricewaterhouseCoopers Worldwide Corporate Tax Summaries, Coopers and Lybrand International Tax Summaries, and the Ernst and Young Worldwide Corporate Tax Guide. We use the corporate tax rate that is applicable in the top tax bracket. In case of local differences in corporate tax rates as, for example, in Italy or Germany, we apply the average corporate tax rate across regions. In the United States, we use the federal level corporate tax rate.

3.2 When does the difference between short-term and long-term capital gains tax rates change?

One common concern about using tax changes for identification purposes is that these changes are endogenous. For example, prior literature documents that corporate tax changes or changes in sales and value added taxes are correlated with economic conditions (e.g., Vegh and Vuletin 2015, Jacob et al. 2018). To test whether changes in the difference between short-term and long-term capital gains tax rates (Δ*TaxDiff*) systematically correlate with economic

[11] For reasons explained in Section 3, our main analysis excludes the U.S., which experienced six changes in the capital gains tax differential in 1991, 1993, 1997, 2001, 2002, and 2003. In additional analyses, we show that our results are robust when we include US in the sample.

conditions, we use Δ*TaxDiff* as dependent variable and first differences of GDP growth, inflation, and unemployment as independent variables. We further include year fixed effects. The results (reported in Panel A, Table 2) suggest that Δ*TaxDiff* is not correlated with economic conditions. Hence, countries do not appear to systematically adjust the difference between short-term and long-term capital gains tax rates in response to economic conditions.

As second concern is that changes in capital gains tax rates typically do not appear in isolation. Instead, these changes may be part of larger reform packages. As we show in Panel B, Table 2, the difference between short-term and long-term capital gains tax rates (*TaxDiff*) is positively correlated with the corporate tax rate. Thus, in our baseline analysis, we control for the corporate tax rate for this reason. To control for other tax policy tools, we collect information on the level of the personal income tax rate on wages (*Income Tax*) assuming the top income tax bracket. Since the capital gains tax is part of the income tax, changes in capital gains taxation might be the result of changes to the marginal income tax rate on wages. We also collect information on *R&D Tax Incentives* from multiple sources (OECD 1996, Warda 2001, Ernst and Spengel 2011)[12] because governments use R&D tax incentives to foster innovation (Bloom, Griffith, and van Reenen 2002). Specifically, we use B-Index developed by Warda (2001), which is a summary measure of various R&D tax incentives. For both the income tax as well as R&D tax incentive variables, we do not find a significant correlation at the country level (Panel B, Table 2). There are two reasons why we do not include *R&D Tax Incentives* and *Income Tax* in our main analysis. First, we do not have full coverage on these variables for all country-years.

[12] Since we do not have a full time series of this index, we are bound to backfill information in the early 1990s. Also, the data are not available for all countries resulting in slightly fewer observations in this test.

Second, as we show in Section 5.6, controlling for these tax rules for the available countries does not change our results.

3.3 Measurement of Innovation

We use patent output to measure innovation activity instead of R&D expenditures for two reasons. First, patent activity reflects the combined output of several hard-to-evaluate tangible and intangible inputs that together produce innovations. For example, in addition to R&D expenditures, successful innovation also involves investments in intangible human capital, managerial and employee effort, and creativity; the long-run value of the latter investments is likely to be particularly difficult to understand for short horizon investors who may not find it worthwhile to expend the necessary research effort. As an example of the latter, consider the 15% time off policy for 3M (or 20% rule for Google), under which 3M allows its employees to take 15% of their time off regular work and devote it to for pursuing any innovative idea of their interest.[13] For short-horizon investors, it may not be obvious whether such a policy constitutes slack reflecting private benefit extraction or an investment in future growth, which, in turn, can create pressures on corporate managers to cut down on such expenditures. Second, firm-level R&D data are frequently missing (e.g., 50% missing rate in Compustat North America) and research shows that firms with missing R&D data can exhibit significant innovation activity (Koh, Reeb, Sojli, and Tham, 2016).

We measure the innovation output of a country using patents granted to firms in that country by the United States Patent and Trademark Office (USPTO). Several prior research (e.g., Griffith, Harrison, and Van Reenen, 2006; Acharya and Subramanian, 2009; Hsu, Tian, and Xu,

[13] In their 2004 IPO letter, Google founders note that this policy allows their employees "..to be more creative and innovative." Google time-off policy has been credited with the innovations such as Gmail and Google news (Guynn, 2015, USA Today). Similarly, 3M's Post-it notes and masking tapes are known to be a result of their time-off policy (Kretkowski, 1998).

2014) use the U.S. based patent measure to capture innovation output of foreign firms. This approach is based on the assumption that because U.S. represents the largest market for technological consumption, most meaningful innovations that foreign firms intend to patent would be covered by the USPTO.[14] We rely on the dataset compiled by PatentsView, an initiative supported by the USPTO to obtain information on these patents. We obtain all utility patents granted by the USPTO that were applied in years prior to and including 2011. Patents can be assigned to individuals, corporations, and government organizations. Because we are interested in firms' response to changes in capital gains tax rate, we focus our main analysis on patents assigned to corporations and use government patents in a placebo analysis. Patent assignment to corporations is based on USPTO's assignee classification (assignee code 2 or 3).

We measure the innovation output of each country at the industry-level by aggregating all patents that a country receives in a specific industry. By industry classification, we refer to the patent classes as defined by the USPTO. USPTO has developed an elaborate system for classifying innovations into more than 400 patent classes. The assignment of a patent into a specific technology class is done with great care to permit future searches of innovations into a technological area (Kortum and Lerner, 1999).

A simple patent count does not distinguish breakthrough inventions from less significant discoveries. Therefore, following prior work, we use the citation-weighted patent count (*Citations*) in a patent class of a year as our main measure for innovation output. This measure captures both the quantity and quality of innovation. To better reflect the actual timing of innovation, we use a patent's application year as opposed to its grant year (Griliches, Pakes, and Hall, 1987). Patent activity is set to zero for a country-industry-year if it is not included in the

[14] U.S. patents laws require anyone claiming rights for inventions to file patents in the U.S.

patent database. A country-industry pair is dropped from our sample if it never receives any successful patent applications. Because patent and citation counts are highly skewed, our dependent variable is calculated as the logarithm of one plus the innovation measure.

Both patent and citation counts are subject to truncation bias towards the end of the coverage years in the patent dataset. Because the PatentsView dataset we use includes utility patents granted until 2016 and we use patents applied prior to (and including) 2011, the truncation bias issue should be less severe in our sample. The number of patents is biased because on average it takes the USPTO two years to grant a patent after application. To adjust for this bias, we follow Hall, Jaffe, and Trajtenberg (2001, 2005) and scale each patent with weights estimated from the empirical application-grant lag distribution. Number of citations is also truncated because patents granted in later years in the dataset have fewer years to collect citations. We correct for the truncation bias in number of citations by using the weighting factor from Hall, Jaffe, and Trajtenberg (2001), who estimate the citation lag distribution. Specifically, the adjustment factor used in the NBER patents dataset (which ends at 2006) for patents granted in year t is applied to patents granted in t+10 in PatentsView, since year t in the NBER dataset and year t+10 in PatentsView has the same number of years remaining till the end of the datasets. Furthermore, to the extent both treatment and control countries are similarly affected by truncation biases, our DiD design further addresses this concern. Descriptive statistics of our innovation measures are reported in Table 3. On average, each country-patent class receives 5.3 successful patent applications and 65.1 citations every year.

3.4 Measurement of other variables

In our main specifications, we control for macroeconomic variables including GDP growth, inflation rate, and unemployment rate. Data on these variables are obtained from the World

Development Indicators (WDI) database of the World Bank. For our sample countries, the average GDP growth rate is 3.3%, average inflation rate is 6.3%, and the average unemployment rate is 8.1% from 1991 to 2006 (Table 3). Table 3 also shows that the mean value of corporate tax rate for our main sample is 32.7%. In the cross-sectional tests, we exploit differences in the importance of publicly listed firms and the concentration of firms. The importance of publicly listed firms is measured by the market capitalization as a percentage of GDP and is available in the WDI database. From the WDI database, we also obtain information on aggregate turnover ratio of domestic shares as a percentage of GDP.

4. Research design

We estimate the effect of capital gains tax shocks on innovation using a generalized difference-in differences design in which we compare how the change in innovation around the tax shocks (first difference) varies with changes in the difference between taxes charged on short- and long-term capital gains (second difference). We expect the change in innovation to be larger for countries that exhibit a greater increase in the relative tax difference between short- and long-term capital gains. We use the following regression specification to implement this DiD approach:

$$\Delta Ln(1 + Citations)_{i,c,t+k} = \beta \Delta TaxDiff_{c,t} + \Gamma \Delta X_{c,t} + \alpha_{i,t+k} + \epsilon_{i,c,t+k}, \quad (1)$$

where *i*, *c*, *t* index the industry (i.e., patent class), country, and year, respectively; *k* represents the number of years after the tax shock. We estimate equation (1) in first differences as denoted by Δ. As explained in the previous section, *TaxDiff* measures the difference between taxes charged on short-term and long-term capital gains. That is, a positive change in the relative tax difference implies a greater reward for longer-term stock ownership. The variable *Citations* is our main measure of innovation output. Finally, *X* represents a vector of time-varying country-level control variables and $\alpha_{i,t+k}$ represent industry-year interactive fixed effects.

We define equation (1) in first differences. Since both the dependent and the *TaxDiff* variable are measured in first-differences, the coefficient β carries a DiD interpretation. If increasing the reward for longer-term ownership (i.e., increasing *TaxDiff*) encourages investments in innovation, we would expect β to be positive. We use a first-difference version of the DiD specification as it allows us to accommodate multiple tax shocks for a country; this is not possible in a standard levels-on-levels specification that accommodates a single shock with clear pre- and post-shock periods.[15]

First-differencing further eliminates the effect of any time-invariant country and industry level factors that affect innovation. An important strength of our research design is that we are also able to fully control for the effect of any time-varying industry factors by including industry-year interactive fixed effect ($\alpha_{i,t+k}$). This allows us to address the potential concern that the tax shocks we study systematically coincide with increase in demand for technological innovation in the U.S. markets. Note that by including these fixed effects, we narrow down the counterfactuals to similar patent classes (or industries) and estimate the effect of capital gains taxation within a given industry-year. That is, we compare the innovation output of a treatment country in a specific industry to the contemporaneous change in innovation output in the same industry of a country that does not experience a tax shock. Because we are comparing changes in innovation supply to the U.S. markets in the same industry over the same time period, differences in industry-level demand shocks between treatment and control countries cannot explain our results.

[15] Such an approach is common in prior research. See, for example, Heider and Ljungqvist (2015), Mukherjee, Singh, and Zaldkos (2017), and Ljungqvist, Zhang, and Zuo (2017).

Our research design also addresses the concern that changes in tax codes coincide with other local economic shocks that alter firms' ability to supply innovation for reasons unrelated to decrease in short-term pressures. We include a variety of country-level variables ($X_{c,t}$) to control for such forces. First, we include GDP growth, inflation rate, and unemployment rate to capture the effect of local economic conditions. Next, we include additional fixed effects that fully control for the effect of common confounding shocks faced by countries with similar income levels. We adopt World Bank's classification of countries based on income level and include the year-income group pair fixed effects to control for within year-income group effects. To the extent that OECD countries within an income group primarily face similar economic shocks, inclusion of these fixed effects helps address this concern. In untabulated tests, we also use year-industry-income group level fixed effects and find similar results.

Finally, our specifications include controls for two tax-related variables. We control for corporate income taxes that can affect corporate investment by changing the profitability of such investments (e.g., Auerbach, 1983; Djankov et al., 2010; Patel et al., 2017; Giroud and Rauh, 2018). We also control for changes in the required holding period to qualify for long-term capital gains tax benefit. The effect of changes in holding period on corporate innovation is theoretically ambiguous and we therefore do not offer any predictions on the effect of this variable on innovation.[16] We consider the holding period only as a control variable because fixing the

[16] To see why the effect is theoretically ambiguous, consider the following example. Suppose a country requires a holding period of 1 year to qualify for tax benefit of 10% on long-term capital gains (i.e., *TaxDiff*=10%). Motivated by this tax benefit, suppose some investors hold a stock for one year instead of their unconstrained holding period of three months in the absence of tax benefits. Now suppose the country increases the holding period to three years from one year to get the same 10% tax benefit on capital gains. The effect of this increase in holding period on these investors' holding horizon depends on their perceived costs of holding the stock for additional two years. Some investors may conclude that it is not worth waiting for two additional years to get the same reward of 10%. These investors may revert back to their unconstrained horizon of three months in the absence of tax benefits. On the other hand, some investors may find that the 10% benefit is large enough to make them wait additional two years, leading to an increase in horizons.

holding period allows us to compare tax regimes with different levels of *TaxDiff* on a more apples-to-apples basis.

Two additional research design choices deserve discussion. First, what is the appropriate level of clustering for computing standard errors? Because we are modelling innovation output at industry level supplied to a single country (i.e., U.S.), we are primarily concerned about correlations in error-terms between multiple observations for the same industry. For example, innovation supply from Germany and Austria in industry A could be cross-sectionally correlated because they face the same demand shocks from industry A in U.S. (or the same global demand shock). There could also be serial correlation in error terms if there are time trends in technological consumption in specific industries. We therefore report standard errors by clustering at the industry level, which adjusts for arbitrary forms of correlations within the same industry. In robustness tests reported later, we document that our results are also robust to clustering at the country level or the country-industry level.

The second choice concerns the inclusion of U.S. in our sample, which also experienced several tax changes. Because we study the supply of innovation to U.S markets, we prefer to exclude U.S. from our main analyses. Tax policy changes are typically driven by local political and economic conditions and, therefore, exclusion of U.S. mitigates the concerns that our results may be driven by these confounding factors that can also affect innovation in U.S. In robustness tests, we show that we obtain similar results when we include U.S. in the sample.

5. Results

5.1 Main Results

Table 4 presents the main results on the effects of capital gains tax shocks on corporate innovation. We begin by examining the effect of tax shocks on innovation output three years after the tax shocks by estimating equation (1) using the difference in citation weighted patent counts three years after the tax shock $Ln(1+Citations)_{t+3}$ and one year before the tax shock $Ln(1+Citations)_{t-1}$ and as the dependent variable, where t denotes the year of the shock. We focus on the third year after the tax shock because innovation output is expected to change gradually and prior work suggests that a three-year window is sufficiently long to detect innovation changes.[17] Nevertheless, in Section 4.2, we explore the detailed year-by-year timing of innovation changes to document support for the parallel trends assumption and to explore the effects over longer windows.

Column (1) presents the estimates from a relatively parsimonious model that only controls for the required holding period to qualify for long-term capital gains, corporate income tax rates, and year fixed effects. We find that the coefficient on $\Delta TaxDiff$, which captures the effect of capital gains tax rate changes on innovation changes, is positive and significant at 1% level (coefficient estimate = 0.004). This result suggests that rewarding longer-term ownership through lower capital gains taxes is associated with an increase in corporate innovation output. The magnitude of the effect is economically meaningful, i.e., a decrease of five percentage points in taxes on long-term relative to short-term gains is associated with a nearly 2.1% increase in annual innovation rates 3 years after the tax shock.[18]

In Column (2), we expand the specification to include controls for local macroeconomic conditions (GDP growth, Inflation, and Unemployment) and find no change in the estimated effect of capital gains tax shocks (Coefficient = 0.004; p-value<0.01). That we do not observe a change in the magnitude of the coefficient suggests that other local economic shocks that might improve firms' ability to supply innovation (for reasons unrelated to short-term pressures) are

[17] See, for example, Aghion et al. (2013), Acharya et al. (2013), Fang et al. (2014), and Mukherjee et al. (2017).
[18] The effect is calculated as exp(coefficient*5)-1.

not systematically coinciding with the capital gains tax shocks. This inference is further reinforced based on the estimates reported in Column (3), where the coefficient estimate remains comparable even after including year-income group interactive fixed effects based on the World Bank's classification of countries into different income groups (Coefficient =0.006; p-value<0.01). These fixed effects flexibly absorb any economic shocks in countries with similar income levels. To the extent that OECD countries within an income group primarily face similar economic shocks, this result further mitigates concerns about the confounding effects of local economic shocks as we narrow down the counterfactuals to countries with similar income levels.

Finally, in Column (4), we further augment our model with industry-year fixed effects, which absorb any secular trends in innovation output at the industry (i.e., patent class) level; for example, trends resulting from increased demand for technological consumption in specific industries. The coefficient estimate on Δ*TaxDiff* continues to exhibit similar magnitude in the presence of these fixed effects (Coefficient = 0.006; p-value<0.01), suggesting that our findings are unlikely to be explained by any demand shocks in a specific industry in the U.S. that might systematically coincide with capital gains tax shocks in foreign countries. The coefficient estimate suggests that a decrease of five percentage points in taxes on long-term relative to short-term gains is associated with a nearly 3% increase in annual innovation rates.

5.2 Timing of innovation changes

In this section, we explore the detailed timing of the innovation changes around the tax shocks. The objective is to assess the speed and persistence of innovation changes following the tax shocks and to assess the validity of the parallel trends assumption that underlies our DiD design. We do so by modelling year-over-year changes in innovation output using equation (1) for different periods around the tax shocks. Table 5, Panel A presents the estimates from regressions that model innovation changes in each of the 5 years prior to the tax shocks and Panel B presents regressions that model annual innovation changes for up to 5 years after the tax shocks. We also visually illustrate the findings from this analysis by plotting the coefficient estimates on Δ*TaxDiff* from these models in Figure 1.

First, Figure 1 and Table 5, Panel A show that the coefficient on Δ*TaxDiff* is economically and statistically indistinguishable from zero for each regression prior to the tax shock. The maximum absolute value of the coefficient estimate over these years carries an economically small magnitude of 0.001. These results provide support for the parallel trends assumption by showing that the treatment and control countries do not exhibit any meaningful differences in trends in innovation output prior to the tax shocks.

In Panel B, we find that innovation output exhibits a statistically significant increase starting two years after the tax shock. As expected, the increase in the innovation output is gradual and economically small in the years t=0 and t=1 because firms would not be able to drastically adjust their innovation output quickly. The innovation output exhibits a steep increase in the second and third year and the increases gradually taper down to an economically small coefficient estimate by year 4. Since we define our model in first differences, the coefficient captures incremental effects. In other words, the insignificant coefficients in t=4 and t=5 imply that the effect on innovation remains constant and does not reverse.

5.3 Ruling out capital supply as an alternative explanation

There is one alternative explanation for our results. Prior literature shows that lower capital gains taxes can increase the supply of capital and reduce the cost of equity (e.g., Becker, Jacob, and Jacob, 2013, Huizinga, Voget, and Wagner, 2018; Dimitrova and Eswar, 2018). Since a higher TaxDiff can be due to lower long-term capital gains taxation, our results might be explained by increased access to equity.

To rule out this alternative explanation, we split the effect of *TaxDiff* into changes in short-term (*Short CG Tax*) or long-term capital gains taxes (*Long CG Tax*). Our prediction is that increases (decreases) in the short-term (long-term) capital gains tax rate increase innovation activity. If the evidence is consistent with the supply side explanation, we should observe no innovation increases or possibly innovation decreases with increases in short-term capital gain taxes. Results reported in Table 6 indicate thatinnovation increases regardless of whether *TaxDiff* increases due to an increase in short-term capital gains tax or a decrease in long-term capital gains tax. The fact that innovation increases after short-term capital gains tax increases implies our findings cannot be explained by the abovementioned supply of capital explanation. Moreover, the magnitude on changes in short-term versus changes in long-term capital gains tax suggest that short-term capital gains taxes appear to have a stronger economic effect on innovation. This further speaks against the alternative explanation.

5.4 Additional tests in support of the short-termism story

Our previous analyses support our interpretation that a larger difference between short-term and long-term capital gains tax rates fosters innovation changes stemming from reduced short-term capital market pressure. However, there are still potential concerns about omitted variables correlated with tax changes as well as some research design choices such as the exclusion of the U.S. While we try to mitigate these concerns by limiting the counterfactuals to the same industry in other countries and to countries with similar levels of income, there might still be the concern that non-tax policies change at the same time. For this reason, we run a set of additional tests that collectively are designed to provide comfort that our interpretation is robust and to rule out additional alternative explanations.

5.4.1 Inclusion of the U.S.

First, we explore the robustness of our results to inclusion of the U.S. in the sample, which was not considered in the main analysis for reasons explained in Section 4. The inclusion of U.S. allows to us consider six additional capital gains tax shocks in the sample. Table 7, Panel A presents our main results after including the U.S. It is evident that inclusion of the U.S. results in virtually no change in either the statistical or economic significance of the effect of capital gains tax shocks. For example, the coefficient estimate on $\Delta TaxDiff$ from the full model after including the U.S. is 0.005 (p-value<0.01), which is similar to the coefficient we report in Table 4.

5.4.2 Placebo tests based on government patents

Second, unlike the corporate sector, government organizations are not vulnerable to the myopic pressures from investors. Furthermore, government organizations are tax-exempt. Hence, we would not expect their patenting activity to be affected by capital gains tax shocks. An added advantage of this placebo test is that it controls for innovation changes resulting from other contemporaneous shocks unrelated to the effects we are examining. To the extent, government organizations, like the corporate sector, benefit from local shocks and other policy instruments that improve the economy's ability to supply innovation, this analysis can help assess if our main results are driven by such local shocks or other non-tax innovation-related policies. An example of such a shock could be greater availability of high quality scientific talent, perhaps resulting from changes in labor market policies or high economic growth.

For this analysis, we use the citation-weighted patent counts of patents filed by foreign government organizations with the USPTO as the dependent variable. We identify government patents using the USPTO assignee code, which equals either six or seven for such patents. Table 7, Panel B presents the results of estimating equation (1) using the change in governmental innovation output three years following the year of the shock as dependent variable. We find that the coefficient on Δ*TaxDiff* is statistically and economically insignificant across all specifications. A potential concern is that the insignificant results for this analysis might be an artefact of the lower sample size (nearly 15,000 observations compared to the approximately 125,000 observations for the main analysis). We believe this is unlikely because low statistical power may decrease significance levels but cannot explain the low economic magnitudes of the coefficient estimates. Overall, this analysis mitigates concerns about the confounding effect of local economic shocks or other policies that might improve firms' ability to supply innovation.

5.4.3 Controlling for other tax policies

In the third step, we control for other tax policy tools, namely *Income Tax* and *R&D Tax Incentives*. To this end, we augment our baseline estimation equation and subsequently include *Income Tax* and *R&D Tax Incentives* in Columns (1) and (2), respectively of Table 7, Panel C to test the sensitivity of our results to the inclusion of other tax policy tools capturing broader tax changes. Finally, in Column (2), we jointly control for all tax variables. Importantly, irrespective of the choice of tax controls (individual tax elements or all items jointly), the coefficients on *ΔTaxDiff* are significant and remain similar across columns. Hence, it appears as if our results on the role of differential taxation of short-term and long-term capital gains cannot be explained by other tax policy changes such as R&D tax incentives.

5.5 Cross-sectional tests to support of the short-termism story

To further address concerns about omitted correlated variables and to tie the results closer the theory that capital gains taxes can alleviate myopic pressures, we run several cross-sectional tests that collectively are designed to provide comfort that our interpretation is robust and to rule out alternative explanations.

5.5.1 Variation in the intensity of myopic pressures

In this subsection, we examine whether the increase in innovation following an increase in the differential capital gains tax rate is more pronounced in countries that are exposed more severely to myopic pressures. Since the main channel behind our finding is that the differential capital gains taxation alleviates the short-term pressure on managers, we expect that in countries in which myopic pressure is more likely to exist, capital gains tax shocks have a stronger positive effect on innovation.

We follow two complementary approaches to measure the presence of myopic pressures. In the first approach, we assess the prevalence of myopic pressures based on the extent to which a country's economy is dominated by publicly listed versus private corporations. We expect myopic capital market pressures to be less of a problem in countries dominated by private firms because of the illiquid and longer-term nature of share ownership in these firms. Consistent with this argument, Bernstein (2015) and Asker, Farre-Mensa, and Ljungqvist (2015) show that compared to private firms, public firms are less willing to undertake long-term oriented investments. We assess the extent to which a country's economy is dominated by publicly listed firms (compared to privately owned firms) using total market capitalization of listed firms scaled by GDP obtained from the World Bank WDI database (*Public Ownership*). We standardize *Public Ownership* to have a mean of zero and a standard deviation of one to simplify the interpretation. We then interact *Public Ownership* with the Δ*TaxDiff* and include the main effect of *Public Ownership*. Consistent with our economic story, Table 8, Column (1) shows that the interaction term between Δ*TaxDiff* and *Public Ownership* is positive and significant (Coefficient

= 0.005; p-value<0.01). The estimates imply that the innovation increase is greater in economies dominated by publicly owned firms. In economic terms, the results suggest that a one standard deviation increase in *Public Ownership* increases the effect of differential capital gains taxes on innovation by about 53% (= 0.005 / 0.009).

Second, we test the idea that in countries in which myopic pressure is more likely to exist, capital gains tax shocks have a stronger positive effect on innovation using the aggregate turnover ratio of domestic shares from World Bank WDI database. To the extent greater share turnover is indicative of the prevalence of more short-term trading, we would expect the innovation increase to be greater in countries characterized by high share turnover prior to the tax shocks. We test this prediction by interacting share turnover (*Turnover*) with Δ*TaxDiff*. Again, we standardize *Turnover* to have a mean of zero and a standard deviation of one to simplify the interpretation. As expected, the estimates in Table 8, Column (2) indicate that the estimate of innovation increase is about 40% greater if turnover increases by one standard deviation from the mean (Coefficient on Δ*TaxDiff* x *Turnover*= 0.0023 compared to Coefficient on Δ*TaxDiff* = 0.0058).

One can also interpret our results from a different point of view, namely the myopic distortion created by *Public Ownership* and *Turnover*. The main coefficients on *Public Ownership* and *Turnover*, respectively are negative and statistically significant. These coefficients are consistent with both measures capturing myopic pressures that are detrimental to innovation. An increase in the tax rate differential between short and long-term capital gains can (partly) undo this distortion: A one percentage point increase in Δ*TaxDiff* reduces the magnitude of the negative coefficient of *Public Ownership* (*Turnover*) from –0.026 (–0.010) by 19.2% (20.0%) to –0.021 (–0.008).

5.5.2 Variation in the nature of innovation

Our economic story suggests that the effect of capital gains tax shocks should be greater for more radical innovations whose business potential may take a long time to realize. Such innovations would be more vulnerable to myopic pressures and would be expected to benefit the most from capital gains tax shocks. We test this idea using two alternative approaches.

First, we use the distinction between explorative and exploitative innovations widely used in prior literature.[19] Exploitative innovations build upon the firms' existing body of technological knowhow and typically are about incremental improvements and refinements in existing technologies. In contrast, explorative innovations involve developing new technologies outside of firms' existing scope, are based on learning-by-experimentation (e.g., Henderson, 1993; Levinthal and March, 1993), and tend to result in radical technological advances. Explorative innovations therefore are more likely to result in path-breaking products whose business potential may take a long time to realize. Following recent work (e.g., Agarwal et al., 2018), we define exploitative patents as those that include at least one citation to a prior patent assigned to the same assignee (i.e., at least one self-citation). Intuitively, patents that exhibit self-citations are likely to be building on firms' prior knowhow. Conversely, explorative patents are those that do not exhibit any self-citations.

Table 9 Panel A presents the results from regressions that separately model the patent counts for explorative and exploitative patents. We find that across all specifications the increase in explorative patent output is economically much larger than the increase for exploitative patents. For example, estimates from the full models shown in Columns (5) and (6) imply that the effect of capital gains tax shocks is almost three times greater for explorative innovations (coefficient =

[19]See, for example, March (1991); Henderson (1993); Levinthal and March (1993); Sørensen and Stuart (2000); Chava et al. (2013); Agarwal et al. (2018).

0.0022, p-value < 0.01) than it is for exploitative innovations (coefficient = 0.0008, p-value = 0.07).

Second, we exploit the difference between highly cited and rarely cited patents. Similar to our arguments from above, highly cited innovations tend to relate radical technological advances, which are subject to greater myopic pressures. In contrast, rarely cited innovations are comparable to exploitative innovations subject to less myopic pressures. We define highly cited (rarely cited) innovations as those ranked top (bottom) 40% in truncation-adjusted citations. Table 9 Panel B presents the regression results from separately estimating the effects for highly cited innovations (Columns (1), (3), and (5)) and rarely cited innovations (Columns (2), (4), and (6)). Consistent with our previous results, we find that across all specifications the effect of *ΔTaxDiff* on highly cited innovation is economically much larger than the effect on rarely cited innovations. In fact, we find significant effects only for highly cited innovations (all p-values < 0.01) whereas the effect on rarely cited innovations is insignificant in all specifications.

5.6 Robustness tests

In this section, we explore the robustness of our main results to some key research deign choices. For reasons explained in Section 2.1, our main sample also excludes Australia and Luxembourg. Table 10, Panel A examines the robustness of our results to inclusion of these countries in the sample. Again, including these countries results in no meaningful change in the statistical and economic significance of our results. In the same vein, we address concerns that countries with relatively limited patenting activity in the U.S. drive our results. In Table 10, Panel B, we examine the sensitivity of our results to dropping of countries that file less than 100 patents with USPTO over our sample period. Again, our results are robust to the exclusion of these countries. Finally, we examine the sensitivity of our inferences to alternative clustering choices. Table 10, Panel C presents the result after clustering at country level and Panel D presents the results after clustering at the country-industry level. Our inferences are unchanged when we consider alternative clustering choices. However, we note that clustering at the country level bears some potential statistical issues because of the relatively low number of clusters.

6. Conclusions

Numerous studies document that pressure from short-horizon investors can make it difficult for corporations to undertake innovative investments with long gestation periods. In this study, we provide evidence on the efficacy of a commonly suggested policy tool to mitigate this problem: imposition of greater taxes on short-term capital gains relative to long-term capital gains. Using a panel of 30 OECD countries and 21 capital gains tax shocks staggered over 16 years, we find that rewarding longer-term ownership by relaxing capital gains taxes results in an increase in corporate innovation.

Our study provides some of the first evidence on how differential taxation of short-term and long-term capital gains at the *individual* level can affect *corporate* investment in patentable innovation. The evidence in this paper should be of interest to regulators and practitioners as it informs the broader economic debate on whether taxes can be used to curb trading activity that engenders negative social externalities. We add to the growing body of evidence that highlights the crucial role of investor horizon in affecting corporate investment for the long run.

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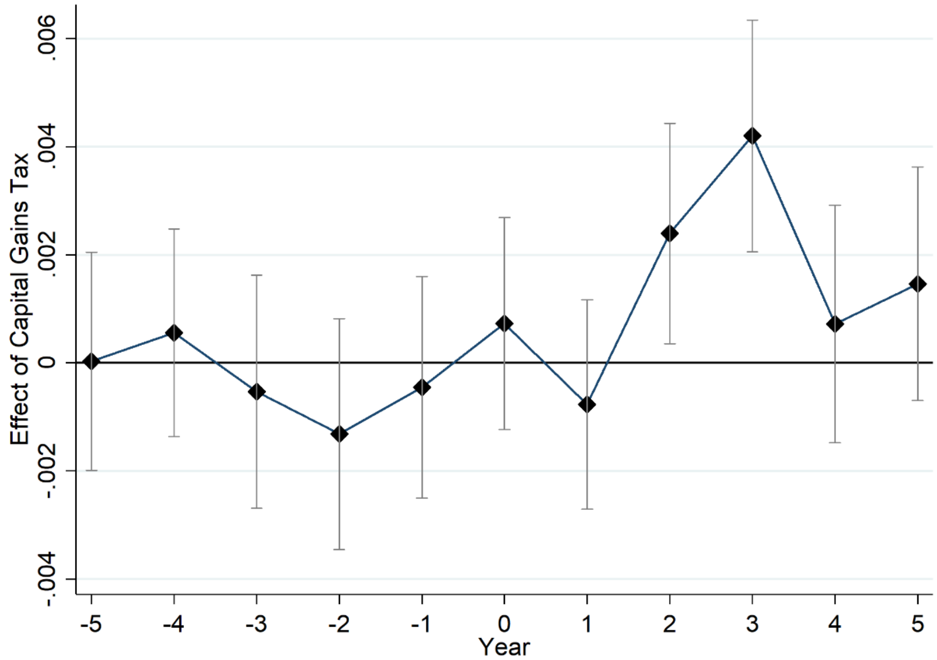
Appendix A: Variable definitions

Variable	Definition
Short CG Tax	Short-term capital gains tax rate.
Long CG Tax	Long-term capital gains tax rate.
TaxDiff	The difference between short-term and long-term capital gains tax rates.
Hold Period	Holding period required to qualify for the long-term capital gains tax benefit. If a country taxes short-term and long-term capital gains at the same rate, we set the holding period to zero.
Corp Tax	The corporate tax rate that is applicable in the top tax bracket. In case of local differences in corporate tax rates as, for example, in Italy or Germany, we apply the average corporate tax rate across regions. In the United States, we use the federal level corporate tax rate.
GDP Growth	GDP growth (annual %). Annual percentage growth rate of GDP at market prices based on constant local currency.
Inflation	Inflation, consumer prices (annual %).
Unemployment	Unemployment, total (% of total labor force) (national estimate).
Patents	Truncation-adjusted number of patents applied for and eventually granted. To adjust for the truncation bias, we follow Hall, Jaffe, and Trajtenberg (2001, 2005) and scale each patent using weight factors constructed using the empirical application-grant lag distribution of patents granted during the 10-year period from 1990 to 1999.
Citations	Truncation-adjusted number of citations. To adjust for the truncation bias, we multiply the unadjusted citation counts by the adjustment factor (hjtwt) in the NBER patents dataset. Specifically, the adjustment factor used in the NBER patents dataset for patents granted in year t is applied to patents granted in t+10 in PatentsView, since year t in the NBER dataset and year t+10 in PatentsView have the same number of years remaining till the end of their datasets. The weight factors are constructed using the methodology described in Hall, Jaffe, and Trajtenberg (2001), which estimates the shape of the citation lag distribution.
Public Ownership	Market capitalization of listed firms as a percentage of GDP.
Turnover	Aggregated turnover of domestic shares as percentage of GDP.

Figure 1: The effect of capital gains tax rates on managerial myopia



This figure plots coefficient estimates for Taxdiff (denoted "*Effect of Capital Gains Tax*") from OLS regression results on innovation. The dependent variable is the change in logarithm of (citation-weighted patent counts +1). We measure the independent variables from t-5 to t+5. Independent variables are defined in Appendix A. All columns include IMF-income-group-year fixed effects and patent-class-year fixed effects. Standard errors are clustered at the patent class level. 95% confidence bounds are indicated by the gray lines.

Table 1: Summary statistics: tax shocks

Panel A: Number of shocks

	All shocks			Big shocks only		
Year	Total	Positive	Negative	Total	Positive	Negative
1991	0	0	0	0	0	0
1992	0	0	0	0	0	0
1993	0	0	0	0	0	0
1994	0	0	0	0	0	0
1995	2	2	0	2	2	0
1996	1	0	1	0	0	0
1997	0	0	0	0	0	0
1998	0	0	0	0	0	0
1999	4	3	1	4	3	1
2000	2	1	1	1	0	1
2001	3	1	2	2	1	1
2002	2	2	0	1	1	0
2003	3	0	3	2	0	2
2004	2	1	1	0	0	0
2005	1	0	1	0	0	0
2006	1	0	1	1	0	1
Total	21	10	11	13	7	6

This panel reports the distribution of changes in relative long-term capital gains tax rates. Relative long-term capital gains tax rate is the difference between long-term capital gains tax rate and short-term capital gains tax rate. Big shocks are tax changes that are greater than or equal to five percentage points.

Panel B: Size of shocks (percentage point change)

	All shocks						Big shocks only					
	Positive			Negative			Positive			Negative		
Year	Mean	Min	Max	Mean	Min	Max	Mean	Min	Max	Mean	Min	Max
1991												
1992												
1993												
1994												
1995	24.0	5.0	43.0				24.0	5.0	43.0			
1996				-3.0	-3.0	-3.0						
1997												
1998												
1999	40.7	30.0	50.0	-15.0	-15.0	-15.0	40.7	30.0	50.0	-15.0	-15.0	-15.0
2000	0.8	0.8	0.8	-8.0	-8.0	-8.0				-8.0	-8.0	-8.0
2001	5.0	5.0	5.0	-16.1	-28.2	-4.0	5.0	5.0	5.0	-28.2	-28.2	-28.2
2002	10.5	1.0	20.0				20.0	20.0	20.0			
2003				-20.7	-38.0	-4.0				-29.0	-38.0	-20.0
2004	3.0	3.0	3.0	-1.8	-1.8	-1.8						
2005				-1.6	-1.6	-1.6						
2006				-10.0	-10.0	-10.0				-10.0	-10.0	-10.0

This panel reports the descriptive statistics of percentage point changes in relative long-term capital gains tax rates. For example, a tax increase from 10% to 20% is a ten-percentage point increase. Relative long-term capital gains tax rate is the difference between long-term capital gains tax rate and short-term capital gains tax rate. Big shocks are tax changes that are greater than or equal to five percentage points.

Panel C: Tax shocks by country

	Number of shocks			Size of Positive Shocks			Size of Negative Shocks		
Country	Total	Positive	Negative	Mean	Min	Max	Mean	Min	Max
AUSTRIA	1	1	0	50.0	50.0	50.0			
BELGIUM	0	0	0						
CANADA	0	0	0						
CHILE	0	0	0						
CZECH REPUBLIC	3	1	2	43.0	43.0	43.0	-5.5	-8.0	-3.0
DENMARK	0	0	0						
ESTONIA	0	0	0						
FINLAND	0	0	0						
FRANCE	0	0	0						
GERMANY	4	1	3	0.8	0.8	0.8	-10.6	-28.2	-1.6
GREECE	0	0	0						
HUNGARY	0	0	0						
IRELAND	0	0	0						
ISRAEL	0	0	0						
ITALY	0	0	0						
JAPAN	0	0	0						
KOREA (SOUTH)	0	0	0						
LATVIA	0	0	0						
MEXICO	0	0	0						
NETHERLANDS	0	0	0						
NEW ZEALAND	0	0	0						
NORWAY	0	0	0						
POLAND	0	0	0						
PORTUGAL	2	1	1	20.0	20.0	20.0	-20.0	-20.0	-20.0
SLOVAKIA	3	1	2	42.0	42.0	42.0	-21.0	-38.0	-4.0
SPAIN	4	3	1	11.3	1.0	30.0	-4.0	-4.0	-4.0
SWEDEN	0	0	0						
SWITZERLAND	0	0	0						
TURKEY	4	2	2	5.0	5.0	5.0	-12.5	-15.0	-10.0
UNITED KINGDOM	0	0	0						

This panel reports the descriptive statistics of changes in relative long-term capital gains tax rates (*TaxDiff*) by country. We define a tax increase from 10% to 20% is a 100% increase and a ten-percentage point increase. *TaxDiff* is the difference between long-term capital gains tax rate and short-term capital gains tax rate. Big shocks are tax changes that are greater than or equal to five percentage points.

Table 2: Potential Endogeneity of Changes in Capital Gains Taxation

Panel A: ΔTaxDiff and Relation to other Economic Factors

	(1)	(2)	(3)
	ΔGDP Growth	*ΔInflation*	*ΔUnemployment*
Economic Factor	0.0372	0.0536	0.1305
	(0.1078)	0.0554	0.3994
Observations	450	Adj R^2	0.0224
Year Fixed Effects	Yes		

Panel B: Correlation of TaxDiff and Other Tax Policy Variables

	TaxDiff	Corporate Tax	Income Tax
Corporate Tax	0.2337***		
Income Tax	-0.0752	0.2874***	
R&D Tax Incentives	-0.0797	0.0591	-0.0591

Panel A of this table reports OLS regression results on changes in TaxDiff. We use the first difference of GDP Growth, Inflation, and Unemployment as control variables. We also include year fixed effects. Standard errors (clustered at the country level) are reported in parentheses. Panel B reports the correlation of different tax policy tools. TaxDiff and Corporate Tax are defined as above. Income Tax is the top marginal income tax rate on wages. R&D Tax Incentives denotes the B-Index according to Warda (2001) collected from OECD (1996), Warda (2001), and Ernst and Spengel (2011). Standard errors are in parenthesis. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Table 3: Descriptive statistics

Panel A: Country-year level

Variable	N	Mean	S.d.	Min	P5	P25	P50	P75	P95	Max
Short CG Tax	458	19.3	18.0	0.0	0.0	0.0	20.0	32.0	50.0	55.0
Long CG Tax	458	13.1	15.7	0.0	0.0	0.0	0.0	26.0	42.0	50.0
TaxDiff	458	6.2	14.9	0.0	0.0	0.0	0.0	0.0	50.0	55.0
Hold Period	458	0.2	0.4	0.0	0.0	0.0	0.0	0.0	1.0	3.0
Corp Tax	458	32.7	8.4	12.5	16.0	28.0	33.0	36.7	50.0	58.2
GDP Growth	456	3.3	2.8	-6.0	-0.9	1.7	3.2	4.7	8.4	11.9
Inflation	457	6.3	12.5	-0.9	0.5	1.7	2.6	4.9	23.1	106.3
Unemployment	458	8.1	4.0	1.8	2.9	4.7	7.7	10.4	16.0	24.2
Mean Patents	458	5.3	15.1	0.0	0.0	0.1	0.7	3.7	26.1	103.6
Mean Citations	458	65.1	184.8	0.0	0.0	0.9	9.0	53.5	200.5	1338.0

Panel B: Sample descriptive statistics

Variable	N	Mean	S.d.	Min	P5	P25	P50	P75	P95	Max
Short CG Tax	126,169	19.0	17.6	0.0	0.0	0.0	20.0	30.0	50.0	55.0
Long CG Tax	126,169	13.9	15.3	0.0	0.0	0.0	10.0	26.0	40.0	50.0
TaxDiff	126,169	5.0	14.0	0.0	0.0	0.0	0.0	0.0	50.0	55.0
Hold Period	126,169	0.1	0.3	0.0	0.0	0.0	0.0	0.0	1.0	3.0
Corp Tax	126,169	34.2	8.0	12.5	21.3	29.0	34.0	38.0	52.0	58.2
GDP Growth	125,940	2.9	2.4	-6.0	-0.9	1.5	2.8	4.1	7.1	11.9
Inflation	125,752	3.8	7.6	-0.9	0.3	1.4	2.2	3.4	10.6	106.3
Unemployment	126,169	7.6	3.9	1.8	2.7	4.5	7.2	9.9	15.0	24.2
Patents	126,169	7.9	43.4	0.0	0.0	0.0	0.0	3.0	28.0	1,964.00
Citations	126,169	96.9	608.7	0.0	0.0	0.0	0.0	24.9	330.3	31,829.00

Panel A of this table reports the country-year level descriptive statistics. The variable *mean_patents* (*mean_citations*) is the average truncation-adjusted patent counts (citation counts) across patent classes within a given country-year. Panel B presents statistics for the regression sample. All other variables are defined in Appendix A.

Table 4: Cumulative effects of relative capital gains tax rates on managerial myopia

	(1) 3-year Window	(2) 3-year Window	(3) 3-year Window	(4) 3-year Window
ΔTaxDiff	0.004***	0.004***	0.006***	0.006***
	(3.837)	(4.034)	(4.887)	(5.102)
ΔHold Period	-0.073**	-0.075**	-0.105***	-0.111***
	(-2.096)	(-2.151)	(-2.888)	(-2.880)
ΔCorp Tax	-0.002	-0.003*	-0.004**	-0.003**
	(-1.579)	(-1.840)	(-2.247)	(-2.005)
ΔGDP Growth		0.006***	0.007***	0.007***
		(3.112)	(3.085)	(3.060)
ΔInflation		-0.003**	0.003*	0.003*
		(-2.146)	(1.931)	(1.853)
ΔUnemployment		0.003	0.004	0.005
		(0.649)	(1.026)	(1.136)
Year FE	Yes	Yes	No	No
Year × income group FE	No	No	Yes	Yes
Year × patent class FE	No	No	No	Yes
Observations	126,169	124,664	124,664	124,550
R-squared	0.031	0.031	0.034	0.104

This table reports OLS regression results on innovation. The dependent variable is the change in logarithm of (citation-weighted patent counts +1). Independent variables are defined in Appendix A. Columns (1) and (2) include year fixed effects. In Columns (3) and (4), we include IMF-income-group-year fixed effects. Column (4) additionally includes patent-class-year fixed effects. Standard errors are clustered at the patent class level. T-statistics are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Table 5: Year-by-year effects of relative captains tax rates on managerial myopia

Panel A: Effects in years prior to the tax shock

	(1)	(2)	(3)	(4)	(5)
Timing of Independent Variables	t = -5	t = -4	t = -3	t = -2	t = -1
ΔTaxDiff	0.000	0.001	-0.001	-0.001	-0.000
	(0.025)	(0.567)	(-0.489)	(-1.209)	(-0.433)
Year × income group FE	Yes	Yes	Yes	Yes	Yes
Year × patent class FE	Yes	Yes	Yes	Yes	Yes
Observations	124,550	124,550	124,550	124,550	124,550
R-squared	0.056	0.057	0.058	0.059	0.059

Panel B: Effects in years after the tax shock

	(1)	(2)	(3)	(4)	(5)	(6)
Timing of Independent Variables	t = 0	t = 1	t = 2	t = 3	t = 4	t = 5
ΔTaxDiff	0.001	-0.001	0.002**	0.004***	0.001	0.001
	(0.726)	(-0.778)	(2.300)	(3.841)	(0.642)	(1.326)
Year × income group FE	Yes	Yes	Yes	Yes	Yes	Yes
Year × patent class FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	124,550	124,550	124,550	124,550	124,550	124,550
R-squared	0.059	0.060	0.060	0.060	0.060	0.060

This table reports OLS regression results on innovation. The dependent variable is the change in logarithm of (citation-weighted patent counts +1). In Panel A, from columns (1) to (5), we measure the independent variables from t-5 (Column (1)) to t-1 (Column (5)). Panel B, columns (1) to (6) measures the independent variables from t (Column (1)) to t+5 (Column (6)). Independent variables are defined in Appendix A. All columns include IMF-income-group-year fixed effects and patent-class-year fixed effects. Standard errors are clustered at the patent class level. T-statistics are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Table 6: Splitting the Cumulative effects effect into long-term versus short-term capital gains tax rates

	(1)	(2)	(3)
ΔShort CG Tax	0.005***	0.006***	0.007***
	(4.418)	(5.217)	(5.490)
ΔLong CG Tax	-0.002**	-0.003***	-0.003***
	(-2.025)	(-2.636)	(-2.589)
Year FE	Yes	No	No
Year × income group FE	No	Yes	Yes
Year × patent class FE	No	No	Yes
Observations	124,664	124,664	124,550
R-squared	0.031	0.035	0.104

This table reports OLS regression results on innovation. We follow our baseline specification. The dependent variable is the change in logarithm of (citation-weighted patent counts +1). Instead of using the change in *TaxDiff* as main independent variable, we use the change in short-term capital gains tax rates (*ΔShort CG Tax*) and the change in long-term capital gains tax rates (*ΔLong CG Tax*) separately. Independent variables are defined in Appendix A. Column (1) includes year fixed effects. In Columns (2) and (3), we include IMF-income-group-year fixed effects. Column (3) additionally includes patent-class-year fixed effects. Standard errors are clustered at the patent class level. T-statistics are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Table 7: Additional tests on the cumulative effects

Panel A: Including U.S. in the sample

	(1)	(2)	(3)
ΔTaxDiff	0.003***	0.004***	0.005***
	(3.215)	(4.067)	(4.369)
Year FE	Yes	No	No
Year × income group FE	No	Yes	Yes
Year × patent class FE	No	No	Yes
Observations	131,608	131,608	131,432
R-squared	0.032	0.036	0.104

Panel B: Placebo tests based on government patents

	(1)	(2)	(3)
ΔTaxDiff	-0.001	-0.001	0.001
	(-1.103)	(-1.075)	(0.222)
Year FE	Yes	No	No
Year × income group FE	No	Yes	Yes
Year × patent class FE	No	No	Yes
Observations	16,729	16,729	15,553
R-squared	0.006	0.007	0.250

Panel C: Control for other Tax Variables

	(1)	(2)	(3)
ΔTaxDiff	0.004***	0.004***	0.004***
	(4.042)	(3.804)	(4.055)
ΔPersonal Income Tax	0.005**		0.006**
	(2.299)		(2.341)
ΔR&D Tax Incentives		-0.018	0.090
		(-0.109)	(0.526)
Year FE	Yes	No	No
Year × income group FE	No	Yes	Yes
Year × patent class FE	No	No	Yes
Observations	111,201	111,201	111,201
R-squared	0.111	0.111	0.111

This table reports OLS regression results on innovation. In Panel A, we replicate our main results and include the U.S., Panel B examines innovation of government owned entities. The dependent variable is the change in logarithm of (citation-weighted patent counts +1) in Panel B. Panel C follows our baseline model and add controls for other taxes. Independent variables are defined in Appendix A. Column (1) includes year fixed effects. In Columns (2) and (3), we include IMF-income-group-year fixed effects. Column (3) additionally includes patent-class-year fixed effects. Standard errors are clustered at the patent class level. T-statistics are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Table 8: Cross-sectional tests: Effect of public ownership and stock turnover

	(1) 3-year Window	(2) 3-year Window
ΔTaxdiff	0.009***	0.006***
	(5.949)	(4.797)
Public Ownership	-0.026***	
	(-7.449)	
ΔTaxdiff × Public Ownership	0.005***	
	(2.890)	
Turnover		-0.010**
		(-2.555)
ΔTaxDiff × Turnover		0.002***
		(2.849)
Controls	Yes	Yes
Year × income group FE	Yes	Yes
Year × patent class FE	Yes	Yes
Observations	112,990	113,443
R-squared	0.113	0.112

This table reports OLS regression results on innovation of government owned entities. The dependent variable is the change in logarithm of (citation-weighted patent counts +1). Independent variables are defined in Appendix A. All Columns include IMF-income-group-year fixed effects and patent-class-year fixed effects. Standard errors are clustered at the patent class level. T-statistics are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Table 9: Cross Sectional tests based on nature of innovation

Panel A: Explorative versus Exploitative innovation

	(1) Explorative 3-year Window	(2) Exploitative 3-year Window	(3) Explorative 3-year Window	(4) Exploitative 3-year Window	(5) Explorative 3-year Window	(6) Exploitative 3-year Window
ΔTaxDiff	0.001***	0.000	0.002***	0.001**	0.002***	0.001*
	(2.914)	(1.193)	(4.354)	(1.973)	(4.381)	(1.817)
Year FE	Yes	Yes	No	No	No	No
Year × income group FE	No	No	Yes	Yes	Yes	Yes
Year × patent class FE	No	No	No	No	Yes	Yes
Observations	124,664	124,664	124,664	124,664	124,550	124,550
R-squared	0.026	0.011	0.030	0.012	0.115	0.078

This table reports OLS regression results. The dependent variable is the change in logarithm of (patent counts +1). Explorative patents are the patents that do not exhibit any self-citations, and exploitative patents are those that have at least one self-citation. Columns (1) and (2) includes year fixed effects. In Columns (3) to (6), we include IMF-income-group-year fixed effects. Columns (5) and (6) additionally include patent-class-year fixed effects. Standard errors are clustered at the patent class level. T-statistics are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively. All variables are defined in Appendix A.

Panel B: Highly cited vs. Rarely cited innovation

	(1) High 3-year Window	(2) Low 3-year Window	(3) High 3-year Window	(4) Low 3-year Window	(5) High 3-year Window	(6) Low 3-year Window
ΔTaxDiff	0.003***	-0.000	0.003***	0.000	0.003***	0.000
	(6.342)	(-0.608)	(7.341)	(1.069)	(7.590)	(0.692)
Year FE	Yes	Yes	No	No	No	No
Year × income group FE	No	No	Yes	Yes	Yes	Yes
Year × patent class FE	No	No	No	No	Yes	Yes
Observations	124,664	124,664	124,664	124,664	124,550	124,550
R-squared	0.033	0.022	0.037	0.025	0.118	0.116

This table reports OLS regression results. The dependent variable is the change in logarithm of (patent counts +1). Highly cited (rarely cited) patents are the patents ranked top (bottom 40%) in truncation-adjusted citations. Columns (1) and (2) includes year fixed effects. In Columns (3) to (6), we include IMF-income-group-year fixed effects. Columns (5) and (6) additionally include patent-class-year fixed effects. Standard errors are clustered at the patent class level. T-statistics are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively. All variables are defined in Appendix A.

Table 10: Robustness tests

Panel A: Including AUS and LUX in the sample

	(1)	(2)	(3)
ΔTaxDiff	0.004***	0.005***	0.005***
	(3.624)	(4.438)	(4.640)
Year FE	Yes	No	No
Year × income group FE	No	Yes	Yes
Year × patent class FE	No	No	Yes
Observations	134,728	134,728	134,598
R-squared	0.030	0.033	0.098

Panel B: Dropping countries with less than 100 patents

	(1)	(2)	(3)
ΔTaxDiff	0.006***	0.007***	0.007***
	(4.423)	(4.573)	(4.825)
Year FE	Yes	No	No
Year × income group FE	No	Yes	Yes
Year × patent class FE	No	No	Yes
Observations	115,528	115,528	115,414
R-squared	0.034	0.036	0.112

Panel C: Standard errors clustered at country level

	(1)	(2)	(3)
ΔTaxDiff	0.004*	0.006***	0.006***
	(1.774)	(3.788)	(3.548)
Year FE	Yes	No	No
Year × income group FE	No	Yes	Yes
Year × patent class FE	No	No	Yes
Observations	124,664	124,664	124,550
R-squared	0.031	0.034	0.104

Panel D: Standard errors clustered at country-patent class level

	(1)	(2)	(3)
ΔTaxDiff	0.004***	0.006***	0.006***
	(4.111)	(5.011)	(5.134)
Year FE	Yes	No	No
Year × income group FE	No	Yes	Yes
Year × patent class FE	No	No	Yes
Observations	124,664	124,664	124,550
R-squared	0.031	0.034	0.104

This table reports OLS regression results. The dependent variable is the change in logarithm of (citation-weighted patent counts +1). Standard errors are clustered at the patent class level in Panels A and B, at the country level in Panel C, and at the country-patent class level in Panel D. In Panels A and B, we report results from alternative samples. Specifically, we drop countries with less than 100 patents (Panel A) and include Luxembourg and Australia in Panel B. Column (1) includes year fixed effects. In Columns (2) and (3), we include IMF-income-group-year fixed effects. Column (3) additionally includes patent-class-year fixed effects. T-statistics are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively. All variables are defined in Appendix A.