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

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Abstract: Pressure from short-horizon investors can hurt corporate investment in innovative projects that deliver value over long-run. 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 staggered over 1991 to 2006, we find that rewarding longer-term ownership through lowering of 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.

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Keywords: Capital gains taxes; real effects; myopia; investment; short termism; innovation

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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 research & development (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

[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), 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.

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 incentive to save capital gains taxes indeed locks investors into longer holding horizons (Reese, 1998; Huddart and Narayanan, 2002; Cici, 2012; Siam and Starks, 2012). Furthermore, Dimmock, Gerken, Ivkovic, and Weisbenner (2018) find that this capital gains "lock-in" effect also influences investors' voting behavior in a way that is suggestive of long-term thinking.

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. For example, in a letter to S&P 500 CEOs, Laurence Fink (BlackRock CEO) laments the problem of corporate myopia and proposes the following:

> "For tax purposes, the U.S. currently defines a long-term investment as one held for one year. Since when was one year considered a long-term investment? A more effective structure would be to grant long-term treatment only after three years, and then to decrease the tax rate for each year of ownership beyond that, potentially dropping to zero after 10 years. This would create a profound incentive for more long-term holdings and could be designed to be revenue neutral."

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

[3] Since capital gains taxation affects individuals' trading behavior (e.g., Reese, 1998; Ivkovic et al., 2005), 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 that shares are sold even if a sale would be beneficial for both, buyer and seller. As a result, the share may not be owned by the person who can best manage or monitor the firm, resulting in productive losses to the economy (Stiglitz, 1983).

Despite the policy relevance and theoretical justification, we are not aware of any empirical evidence that sheds light 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.

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 through lowering of long term capital gains taxes 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

[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, most meaningful innovations that firms intend to patent would get covered by USPTO.

DiD design, we find no evidence of innovation changes in any of the five years prior to the tax shocks.

A 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 shocks 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.

Second, the tax changes may coincide with local productivity shocks that improve the local economy's ability to supply innovation for reasons unrelated to reduction in short-term pressures (e.g., increased availability of high quality scientific talent). We include extensive controls for the local economic conditions to control for such shocks. Furthermore, in a placebo test, we do not find evidence of increase in innovation output of government organizations around the tax shocks. These organizations and their owners are tax-exempt and, thus, are not affected by the tax shocks we examine. However, they are subject to the same economic conditions and other potential concurrent changes in fiscal policy. To the extent innovation by government organizations also benefits from such local productivity shocks or concurrent policy changes, this analysis further mitigates this concern.

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, but 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, using two alternative approaches comparing either explorative versus exploitative innovations based on whether patents are highly or rarely cited, we find that the tax-induced 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 by providing some of the first 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 be used to curb investor behavior that can generate 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. Finally, our study adds to the growing body of evidence that highlights the crucial role of investor horizon in affecting corporate investment for the long-run.

2. Data and Sample

2.1 Measurement of capital gains tax shocks

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]

[5] For evidence on the effect of financial transaction taxes, see, for example, Roll (1989), Roll (1989), Umlauf (1993), Jones and Seguin (1997), Baltagi, Li, and Li (2006), Hau (2006), Liu and Zhu (2009), Pomeranets and Weaver (2012), 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)

[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).

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 (median) holding period of our sample countries that charge different taxes on short-term and long-term gains is 0.8 (1.0) years. 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. The average holding period after setting it to zero for countries with no differential capital gains taxation is 0.2 years.

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.[7]

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 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.[8]

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

[7] Our results are insensitive to the inclusion of these two countries in our empirical analyses.
[8] 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.

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. Table 2 shows that the mean value of corporate tax rate for our main sample is 32.7%.

2.2 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.[9] 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).

[9] 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).

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, 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.[10] 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. We obtain the patent assignment to corporations 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

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

(*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 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 2. On average, each country-patent class receives 5.3 successful patent applications and 65.1 citations every year.

2.3 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 2). 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.

3. 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 (the 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 the Δ symbol. 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.[11]

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

[11] 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).

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.

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 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.[12] We consider the holding period only as a control variable because fixing the

[12] 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

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 obtain our 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 show that our results are also robust to clustering at the country level or the country-industry level.

The second choice concerns 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.

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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.

4. Results

4.1 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 ($Ln(1 + Citations)_{t-1}$) 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.[13] 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.[14]

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

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 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 $\Delta TaxDiff$ 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.

4.2 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 $\Delta TaxDiff$ from these models in Figure 1.

First, Figure 1 and Table 5, Panel A show that the coefficient on $\Delta TaxDiff$ 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 first two 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 t=4.

4.3 Additional tests in support of the short-termism story

Our main analyses in Sections 4.1 and 4.2 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 alternative explanations or omitted variables correlated with tax changes. 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 placebo tests as well as several cross-sectional tests that collectively are designed to provide comfort that our interpretation is robust and to rule out alternative explanations.

4.3.1 Placebo tests based on government patents

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 6 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. Figure 2 plots the coefficient estimates on $\Delta TaxDiff$ for annual innovation changes for each of the 10 years surrounding the year of the tax shock. We find that the coefficient on $\Delta TaxDiff$ is statistically and economically insignificant across all specifications. For example, in the full model in Column (3) of Table 6, the coefficient (0.001) is economically small and statistically insignificant. 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. Furthermore, as observed in Figure 2, there is no discernable pattern in the coefficient estimates, which fluctuate randomly during the 10-year window surrounding the tax shocks. Overall, this analysis mitigates concerns about the confounding effect of local economic shocks or other policies that might improve firms' ability to supply innovation.

4.3.2 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 $\Delta TaxDiff$ and include the main effect of *Public Ownership*. Consistent with our economic story, Table 7, Column (1) shows that the interaction term between $\Delta TaxDiff$ 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 $\Delta TaxDiff$. 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 7, 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 $\Delta TaxDiff$ × *Turnover* = 0.0023 compared to Coefficient on $\Delta TaxDiff$ = 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 $\Delta TaxDiff$ reduces the magnitude of the negative coefficient of *Public Ownership* (*Turnover*) from –0.027 (–0.010) by 21.5% (22.0%) to –0.021 (–0.008).

4.3.3 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.[15] Exploitative innovations build upon the firms' existing body of technological knowhow and typically are about incremental improvements and refinements in existing

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

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 8 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 = 0.022, p-value < 0.01) than it is for exploitative innovations (coefficient = 0.008, 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 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.

4.4 Robustness tests

In this section, we explore the robustness of our main results to some key research deign choices. 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 3. The inclusion of U.S. allows to us consider six additional capital gains tax shocks in the sample. Table 10, 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.

For reasons explained in Section 2.1, our main sample also excludes Australia and Luxembourg. Table 10, Panel B 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 C, 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 D presents the result after clustering at country level and Panel E 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.

5. 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 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 can generate 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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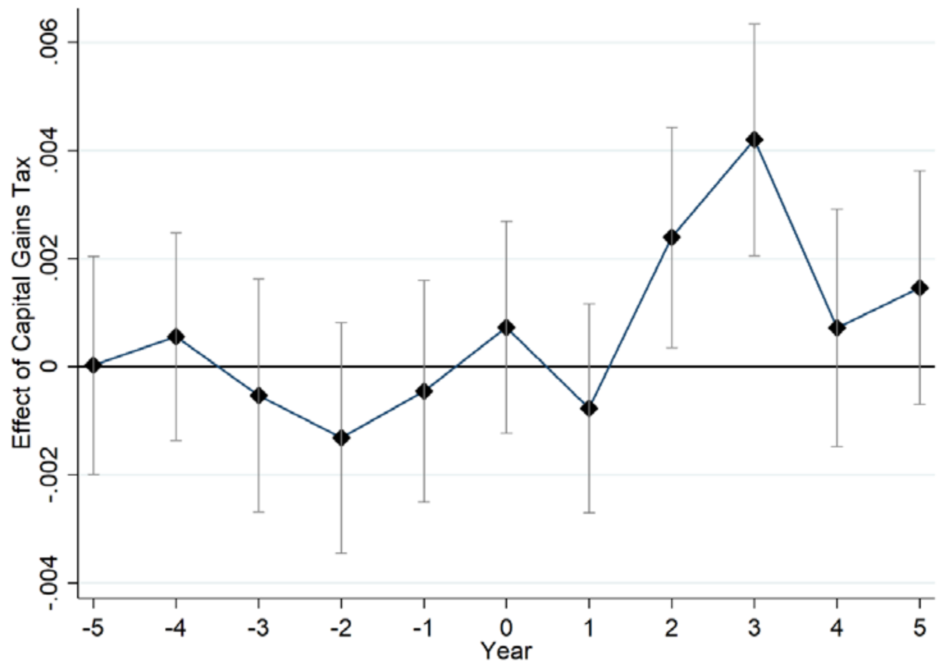
Appendix A: Variable definitions

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

Panel A: Patents assigned to corporations



Panel B: Patents assigned to government organizations

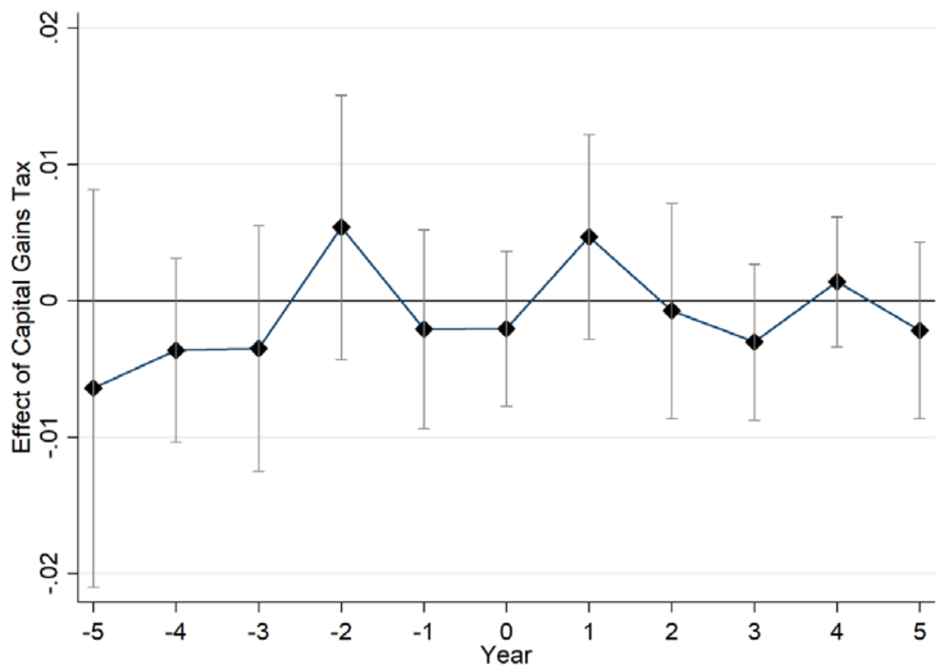


Table 1: Summary statistics: tax shocks
Panel A: Number of shocks

	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: Country-year level descriptive statistics

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

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. All other variables are defined in Appendix A.

Table 3: 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

This table reports sample descriptive statistics. All 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)
ΔHold Period	-0.008	-0.045	0.016	-0.024	0.019
	(-0.222)	(-1.450)	(0.455)	(-0.717)	(0.602)
ΔCorp Tax	0.004***	0.002	0.002	-0.002	0.001
	(2.820)	(1.037)	(1.345)	(-1.436)	(0.853)
ΔGDP Growth	0.006***	-0.001	0.000	-0.007***	0.001
	(2.677)	(-0.375)	(0.074)	(-3.174)	(0.321)
ΔInflation	0.000	0.008***	-0.004**	0.001	0.001
	(0.119)	(4.612)	(-2.268)	(0.736)	(0.670)
ΔUnemployment	0.006*	0.006*	0.000	0.001	-0.007**
	(1.931)	(1.935)	(0.048)	(0.155)	(-2.159)
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

This table reports OLS regression results on innovation. The dependent variable is the change in logarithm of (citation-weighted patent counts +1). From columns (1) to (5), we vary the point in time, we measure the independent variables from t-5 (Column (1)) to t-1 (Column (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. T-statistics are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Table 5 (Cont'd)

Panel B: Effects in years after the tax shock

	(1) t = 0	(2) t = 1	(3) t = 2	(4) t = 3	(5) t = 4	(6) 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)
ΔHold Period	-0.064**	-0.019	-0.099***	0.007	0.023	0.008
	(-2.025)	(-0.563)	(-2.865)	(0.203)	(0.661)	(0.205)
ΔCorp Tax	-0.001	0.000	0.003**	-0.007***	-0.002	0.002
	(-0.466)	(0.209)	(2.019)	(-4.390)	(-1.015)	(1.254)
ΔGDP Growth	-0.003	0.005**	0.001	0.000	-0.004**	0.003
	(-1.380)	(2.191)	(0.459)	(0.146)	(-2.004)	(1.145)
ΔInflation	0.000	-0.001	0.006***	-0.001	0.003*	-0.003*
	(0.042)	(-0.459)	(3.181)	(-0.739)	(1.863)	(-1.725)
ΔUnemployment	-0.000	0.002	0.001	0.001	-0.003	-0.006*
	(-0.104)	(0.744)	(0.440)	(0.292)	(-0.953)	(-1.799)
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). From columns (1) to (6), we vary the point in time, we measure 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: Placebo tests based on government patents

	(1) 3-year Window	(2) 3-year Window	(3) 3-year Window
ΔTaxDiff	-0.001	-0.001	0.001
	(-1.103)	(-1.075)	(0.222)
ΔHold Period	0.068	0.075*	0.004
	(1.530)	(1.675)	(0.018)
ΔCorp Tax	0.003	0.004	0.003
	(0.831)	(0.911)	(0.534)
ΔGDP Growth	-0.016***	-0.018***	-0.015**
	(-3.353)	(-3.243)	(-2.507)
ΔInflation	-0.000	0.004	0.006
	(-0.026)	(0.452)	(0.618)
ΔUnemployment	-0.021*	-0.017	-0.009
	(-1.715)	(-1.294)	(-0.609)
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

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. 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: Cross-sectional tests

	(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 8: 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)
ΔHold Period	-0.039**	-0.058***	-0.065***	-0.076***	-0.065***	-0.075***
	(-2.364)	(-4.456)	(-3.849)	(-5.592)	(-3.695)	(-5.186)
ΔCorp Tax	-0.001	-0.000	-0.002**	-0.001	-0.001**	-0.000
	(-1.322)	(-0.645)	(-2.559)	(-1.362)	(-2.038)	(-0.952)
ΔGDP Growth	0.004***	0.001*	0.004***	0.000	0.004***	0.000
	(5.884)	(1.787)	(5.132)	(0.452)	(5.393)	(0.739)
ΔInflation	0.001**	0.000	0.003***	0.000	0.003***	0.000
	(2.367)	(1.319)	(4.427)	(0.893)	(4.535)	(0.854)
ΔUnemployment	0.006***	0.004***	0.007***	0.005***	0.009***	0.006***
	(3.571)	(2.990)	(4.678)	(3.947)	(5.403)	(4.397)
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.

Table 9: 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)
ΔHold Period	-0.062***	-0.042***	-0.074***	-0.076***	-0.080***	-0.071***
	(-4.679)	(-2.690)	(-5.363)	(-4.784)	(-5.395)	(-4.299)
ΔCorp Tax	-0.000	-0.000	-0.001	-0.001**	-0.001	-0.001*
	(-0.493)	(-0.457)	(-1.176)	(-2.555)	(-1.074)	(-1.739)
ΔGDP Growth	0.002***	0.004***	0.001**	0.004***	0.001**	0.004***
	(2.817)	(6.951)	(2.240)	(5.465)	(2.162)	(6.099)
ΔInflation	-0.002***	0.004***	0.000	0.003***	0.000	0.004***
	(-5.452)	(8.543)	(0.326)	(6.093)	(0.090)	(6.367)
ΔUnemployment	0.000	0.010***	0.000	0.013***	0.001	0.015***
	(0.286)	(7.248)	(0.132)	(9.261)	(0.566)	(10.406)
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 U.S. in the sample

	(1) 3-year Window	(2) 3-year Window	(3) 3-year Window
ΔTaxDiff	0.003***	0.004***	0.005***
	(3.215)	(4.067)	(4.369)
ΔHold Period	-0.048	-0.075**	-0.083**
	(-1.393)	(-2.082)	(-2.178)
ΔCorp Tax	-0.003**	-0.004**	-0.004**
	(-2.209)	(-2.465)	(-2.217)
ΔGDP Growth	0.006***	0.006***	0.006***
	(2.877)	(2.880)	(2.932)
ΔInflation	-0.004***	0.003	0.003
	(-2.865)	(1.477)	(1.480)
ΔUnemployment	0.003	0.004	0.004
	(0.688)	(1.076)	(1.118)
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: Including AUS and LUX in the sample

	(1) 3-year Window	(2) 3-year Window	(3) 3-year Window
ΔTaxDiff	0.004***	0.005***	0.005***
	(3.624)	(4.438)	(4.640)
ΔHold Period	-0.075**	-0.098***	-0.102***
	(-2.115)	(-2.670)	(-2.681)
ΔCorp Tax	-0.003*	-0.003**	-0.003*
	(-1.661)	(-2.069)	(-1.884)
ΔGDP Growth	0.006***	0.007***	0.007***
	(3.359)	(3.277)	(3.254)
ΔInflation	-0.003**	0.003*	0.003*
	(-2.003)	(1.729)	(1.744)
ΔUnemployment	0.006	0.008*	0.009**
	(1.459)	(1.945)	(2.156)
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 C: Dropping countries with less than 100 patents

	(1) 3-year Window	(2) 3-year Window	(3) 3-year Window
ΔTaxDiff	0.006***	0.007***	0.007***
	(4.423)	(4.573)	(4.825)
ΔHold Period	-0.113*	-0.109*	-0.121**
	(-1.897)	(-1.808)	(-2.010)
ΔCorp Tax	-0.002	-0.004**	-0.004**
	(-1.257)	(-2.201)	(-2.024)
ΔGDP Growth	0.005**	0.006**	0.006**
	(2.146)	(2.397)	(2.235)
ΔInflation	-0.004**	0.003	0.002
	(-2.032)	(0.859)	(0.683)
ΔUnemployment	-0.000	0.004	0.005
	(-0.013)	(0.794)	(0.981)
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 D: Standard errors clustered at country level

	(1) 3-year Window	(2) 3-year Window	(3) 3-year Window
ΔTaxDiff	0.004*	0.006***	0.006***
	(1.774)	(3.788)	(3.548)
ΔHold Period	-0.075	-0.105**	-0.111*
	(-1.020)	(-2.123)	(-2.026)
ΔCorp Tax	-0.003	-0.004	-0.003
	(-0.983)	(-1.389)	(-1.095)
ΔGDP Growth	0.006***	0.007**	0.007**
	(2.780)	(2.715)	(2.482)
ΔInflation	-0.003	0.003	0.003
	(-0.981)	(0.896)	(0.777)
ΔUnemployment	0.003	0.004	0.005
	(0.220)	(0.409)	(0.428)
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 E: Standard errors clustered at country-patent class level

	(1)	(2)	(3)
	3-year Window	3-year Window	3-year Window
ΔTaxDiff	0.004***	0.006***	0.006***
	(4.111)	(5.011)	(5.134)
ΔHold Period	-0.075**	-0.105***	-0.111***
	(-2.180)	(-2.934)	(-2.889)
ΔCorp Tax	-0.003*	-0.004**	-0.003**
	(-1.858)	(-2.267)	(-2.030)
ΔGDP Growth	0.006***	0.007***	0.007***
	(3.201)	(3.110)	(3.080)
ΔInflation	-0.003**	0.003*	0.003*
	(-2.142)	(1.919)	(1.821)
ΔUnemployment	0.003	0.004	0.005
	(0.580)	(0.927)	(1.039)
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, B, and C, at country level in Panel D, and at country-patent class level in Panel E. In Panels A, B and C, we report results from alternative samples. Specifically, we add U.S., drop countries with less than 100 patents, and include Luxembourg and Australia in the three panels, respectively. 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.