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Tax Credits and Small Firm R&D Spending†

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In 2004, Canada changed the eligibility rules for its Scientific Research and Experimental Development (SRED) tax credit, which provides tax incentives for R&D conducted by small private firms. Difference-in-difference estimates show a 17 percent increase in total R&D among eligible firms. The impact was larger for firms that took the tax credits as refunds because they had no current tax liability. Contract R&D expenditures were more elastic than the R&D wage bill. The response was also greater for firms that invested in R&D capital before the policy change. (*JEL* D22, G32, H25, L25, O32, O38)

Economists have long suspected that private incentives for research and development (R&D) are too low, since knowledge spillovers cause research spending to resemble investment in a public good. Tax subsidies are a market-oriented approach to this problem. It is often unclear, however, whether fiscal incentives for R&D produce a meaningful private response. This is particularly true for small firms that lack sophisticated tax planning capabilities, have little or no tax liability, and might balk at the fixed costs of starting a new line of research. We use a change in eligibility rules for R&D tax credits under Canada's Scientific Research and Experimental Development (SRED) tax incentive program to gain insight into the impact of fiscal incentives on R&D spending by small private firms.[1]

In 2004, Canadian-controlled private corporations (CCPCs) with prior-year taxable income between C$200,000 and C$500,000 became eligible for a fully refundable 35 percent R&D tax credit on a larger amount of qualifying R&D expenditures. We show that firms eligible to benefit from the more generous policy spent more on R&D following the change compared to firms with the same taxable income before the change. Specifically, eligible firms increased their R&D spending by an average

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[1] While the program is commonly referred to as SR&ED in Canada, we conserve ampersands by adopting the acronym SRED throughout this paper.

of 17 percent. Much of the response comes from firms with zero-tax liability. These zero-tax liability firms would not necessarily benefit from a nonrefundable credit, but can convert (some) refundable SRED credits to cash. We examine the components of R&D spending and find a smaller effect for R&D wages than for contract R&D expenditures. Finally, we show that firms increase their R&D spending by a larger amount if they recently made R&D-related capital expenditures.

Our findings make three contributions to the literature on R&D tax incentives. First, we focus on small private firms: the average firm in our estimation sample has annual revenues of C$1.2 million. While large firms account for the bulk of private R&D spending, several authors have argued that small firms have a comparative advantage in product innovation or exploratory research (Cohen and Klepper 1996, Akcigit and Kerr 2010). Our estimates suggest that small private firms are quite responsive to R&D tax incentives, consistent with the findings of other recent studies (Dechezleprêtre et al. 2016).

Second, because SRED credits are fully refundable for most of the firms in our sample, our findings are relevant to debates over the design of the US R&D tax credit. Before 2016, the US federal R&D tax credit was nonrefundable, so small firms that did not owe taxes could only benefit from carry-forwards. The law was changed in December 2015, allowing firms with gross receipts less than C$5 million to deduct up to C$250,000 of qualifying R&D expenditures from their payroll tax, making the R&D tax credit essentially refundable for small firms.[2] Roughly half of the firm-year observations in our data have no tax liability. We show that zero-tax liability firms are more responsive to the expansion of the refundable credit, presumably because these firms face a larger increase in the after-tax marginal cost of R&D once all of their credits are consumed.

Finally, our results highlight the potential importance of fixed costs in small firms' response to R&D tax incentives. We provide two pieces of evidence on this point. First, we show that contract R&D spending (a spending category we assume to have relatively low fixed costs) has a greater after-tax cost elasticity than the R&D wage bill. Second, we show that firms with recent R&D-related capital expenditures (one source of fixed costs) are more responsive to the more generous tax incentives.

In the remainder of the paper, we review prior research on R&D tax credits, describe the Canadian SRED program change and our empirical strategy in greater detail, present our empirical results, and speculate on the implications of our findings.

I. Related Literature

Hall and Van Reenen (2000) review the early literature on R&D tax incentives and identify two broad empirical strategies. One approach is to estimate a reduced-form R&D demand equation that includes a shift parameter to measure the impact of changes in the R&D tax credit. This strategy is used in several papers, including Swenson (1992) and Czarnitzki et al. (2011). A second approach is to regress R&D spending on the after-tax user cost of R&D to obtain a scale-free estimate of the cost

[2] The new law also made the US R&D tax credit permanent. Observers such as Tyson and Linden (2012) had long called for both changes.

elasticity of R&D spending.[3] This latter method is implemented by Hall (1993), Bloom et al. (2002), Lokshin and Mohnen (2012), Wilson (2009), and Rao (2016). Given the complexities of calculating the R&D user cost and the endogeneity introduced when user cost is a function of R&D spending, the reduced-form approach is often simpler. We estimate a reduced-form expenditure function and use the design of the credit to calculate an implied user-cost elasticity of R&D.

While early research on the impact of R&D tax incentives focused on the United States, some recent studies provide evidence from other countries, including Canada (Dagenais et al. 1997, Baghana and Mohnen 2009, Czarnitzki et al. 2011), Japan (Yohei 2011, Koga 2003), the Netherlands (Lokshin and Mohnen 2012), the United Kingdom (Dechezleprêtre et al. 2016, Guceri and Liu 2015), and China (Chen et al. 2017). The results of these studies are broadly consistent with those surveyed in Becker (2015) and with the conclusion in Hall and Van Reenen (2000, 467) that "A tax price elasticity of around unity is still a good ballpark figure, although there is a good deal of variation around this from different studies as one would expect."

Our study is one of a small number of papers on R&D tax credits to focus on small firms. Lokshin and Mohnen (2012) split their sample into large and small firms (above or below 200 employees) and find that small firms have a larger cost elasticity of R&D. Koga (2003) finds the opposite result—a larger cost elasticity for large firms—in a sample of Japanese manufacturing firms, though in that study size is based on capital rather than employees. More recently, Dechezleprêtre et al. (2016) estimate a user-cost elasticity of -2.6 for firms with assets in the vicinity of 86 million euros, the threshold for "small or medium" under a UK administrative rule. We do not provide an explicit comparison of the impact of tax credits on large and small firms, since our natural experiment only impacts those with taxable income between C$200,000 and C$500,000. Nevertheless, our estimates do suggest that the very small firms in our sample have a user-cost elasticity greater than one.

To our knowledge, no study has sought direct evidence of fixed costs in R&D investment. Many authors have noted that the within-firm variance in R&D expenditures is much lower than for capital goods and that one way to rationalize this observation is to assume some type of adjustment cost. There is some disagreement, however, over what these costs might be. For example, Lach and Schankerman (1989) argue that the bulk of R&D spending is labor costs, which should not impose substantial fixed costs, at least for large firms. On the other hand, Hall (1993) suggests that the long-term nature of research and the fact that much of a firm's knowledge capital is tied up in its R&D workforce make it difficult for even large firms to quickly adjust their R&D spending. A number of papers seek evidence of adjustment costs in the lag structure of R&D investments (e.g., Bloom et al. 2002). This is a difficult empirical exercise though, precisely because within-firm R&D expenditures are typically quite smooth over time (e.g., Hall et al. 1986). Unlike prior studies that identify adjustment costs by using a dynamic model (Hall 1993, Bernstein and Nadiri 1988), we compare different types of R&D spending—contracts versus wages—and utilize direct proxies for the firm-level cost of adding R&D resources.

[3] To our knowledge, the only papers to examine innovation-related outcome variables other than R&D spending are Czarnitzki et al. (2011) and Dechezleprêtre et al. (2016).

Finally, as noted in the introduction, the refundable nature of SRED credits makes our results relevant to recent US tax policy changes. Because most firms in our sample earn fully refundable credits, we cannot test whether the elasticity of R&D differs for credits earned as noncash carry-forwards versus cash equivalents. We do observe, however, that zero-tax liability firms are more responsive to the expansion of the refundable credit program. This finding complements the results in Zwick and Mahon (2014), which show that small financially constrained firms exhibit a greater response to accelerated depreciation benefits in their capital expenditures, and those of Himmelberg and Petersen (1994), which show that R&D investments are sensitive to cash flow for small firms in high-tech industries.

II. SRED Policy and Empirical Framework

A. *The SRED Tax Incentive Program*

The SRED program is a tax incentive provided by the federal government to encourage businesses of all sizes and sectors to conduct research and development in Canada. To qualify for SRED support, a firm's R&D expenditures must broadly satisfy two conditions. First, the work must be a "systematic investigation or search that is carried out in a field of science or technology by means of experiment or analysis." And second, this work must be undertaken to achieve a technological advancement or further scientific knowledge.[4]

SRED has three main features. First, all companies operating and carrying out R&D in Canada may deduct 100 percent of qualifying R&D expenditures (including capital expenditures) from their taxable income.[5] Second, SRED provides a two-tier tax credit. All firms are eligible to receive an investment tax credit on qualifying expenditures at the general rate of 20 percent.[6] Small and medium-sized CCPCs can receive an additional 15 percent tax credit, for a total credit rate of 35 percent, on R&D expenditures up to a threshold called the *expenditure limit*. Third, SRED credits earned at the 35 percent rate are fully refundable, while credits earned at the 20 percent rate are usually nonrefundable (but can be used to offset future tax liability).

Our empirical strategy exploits a change in the expenditure limit, which is a function of prior-year taxable income and prior-year taxable capital employed in Canada. (To simplify exposition, we focus only on how taxable income affects the expenditure limit, because taxable capital is only relevant for a handful of the firms in our estimation sample.) Before 2004, the expenditure limit was C$2 million for CCPCs with prior-year taxable income below C$200,000, with a phaseout for firms earning between C$200,000 and C$400,000. In 2004, as part of a broad package of tax reforms, the phaseout range was increased by C$100,000. This change in the expenditure limit formula was introduced in the Canadian Federal Government

[4] See https://www.canada.ca/en/revenue-agency/services/scientific-research-experimental-development-tax-incentive-program/eligibility-work-investment-tax-credits.html for more detail.

[5] Until 2014, qualifying expenditures included both current and capital expenditures used in the conduct of qualifying SRED activities. Since January 1, 2014, capital expenditures no longer qualify.

[6] As of January 1, 2014, the general credit rate is now 15 percent.

budget on February 18, 2003. Because budget details are typically kept secret until the document is presented, it is very unlikely that any firms knew about the planned expenditure limit reformulation before that date.

To illustrate the effects of expenditure limit reformulation, let $TY_{i(t-1)}$ denote prior-year taxable income, and define a variable Z_t that governs the location of the phaseout range. The expenditure limit for firm i in year t (EL_{it}) can then be written as

$$EL_{it} = \min\left\{\text{C\$2 million}, \max\left\{0, Z_t - 10TY_{i(t-1)}\right\}\right\}. \tag{1}$$

Figure 1 graphs this function before and after the 2004 policy change. The solid line represents the pre-2004 expenditure limit based on $Z_t = $ C$4 million. R&D expenditures below this line earned refundable tax credits at the rate of 35 percent, while additional expenditures above this threshold earned nonrefundable credits at 20 percent. The 2004 SRED policy change increased Z_t to C$5 million, shifting rightward the expenditure limit phaseout region, represented by the dashed line in Figure 1. If a firm's lagged taxable income was between C$200,000 and C$500,000, and if their R&D investment crossed the pre-2004 expenditure limit, then the policy change reduced their after-tax cost of R&D. More precisely, the *marginal* after-tax cost of R&D declined for firms located in the darkly shaded parallelogram, while the *average* after-tax cost of R&D declined for firms with total R&D spending in the lightly shaded area above the parallelogram. Note that Figure 1 is not drawn to scale: in practice, the expenditure limit phaseout is much steeper, with each C$1 increase in lagged taxable income producing a C$10 drop in the amount of R&D eligible for the refundable 35 percent credit rate.

Because the marginal cost of R&D increases discontinuously at the expenditure limit, we should expect an increase in the density of firm-year observations near that point, and online Appendix A shows that this does occur.[7] It is therefore important to recognize that our identification strategy does not rely on variation in tax rates produced by firms actually crossing the threshold. Instead, we exploit variation in the after-tax cost of R&D produced by the expenditure limit reformulation illustrated in Figure 1.

In addition to the expenditure limit, a firm's marginal credit rate depends on its tax liability. As noted above, SRED credits earned at the 35 percent rate are fully refundable.[8] Credits earned at the 20 percent rate reduce the marginal cost of R&D by C$0.20 as long as the firm has a remaining tax liability, since these credits can be used to fully offset taxes payable. If a firm does not owe taxes but does have the maximum expenditure limit (C$2 million during our sample period), it earns a

[7] The online Appendix also provides a simple model to explain how declining marginal returns to R&D can produce a discontinuous *increase* in the number of observations just above the discontinuity, which is what we actually find.

[8] Here we assume that the marginal SRED dollar represents a current (as opposed to a capital) expenditure. This is an important and sensible assumption. It is important because current expenditures earning the 35 percent credit rate are fully refundable, while only 40 percent of credits earned from capital expenditures are refundable. It is sensible to assume the additional dollar invested is a current expenditure because the vast majority of CCPC SRED expenditures are current expenditures.

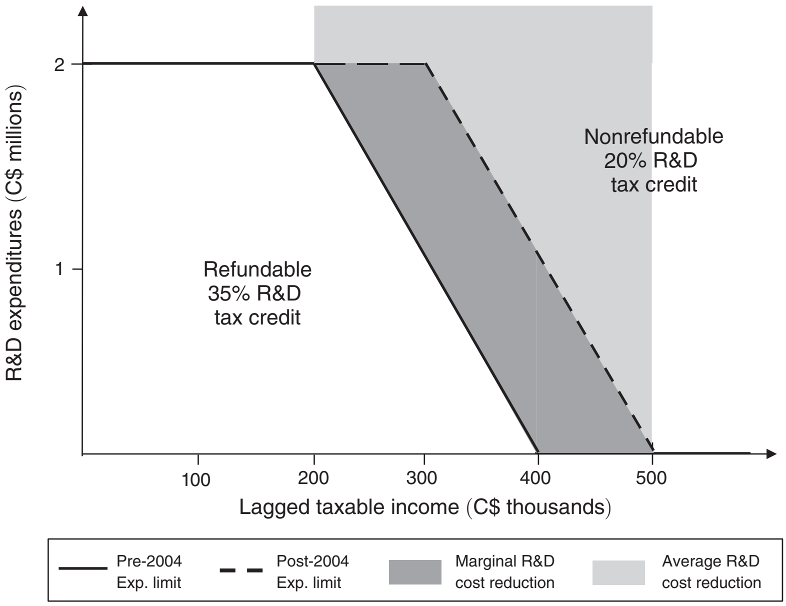

Figure 1. SRED Expenditure Limits before and after Program Change

fully refundable tax credit of 8 percent.[9] Thus, letting R denote R&D expenditures and *Tax* the total taxes owed after accounting for all other credits and deductions, a firm's marginal credit rate is

$$\rho(R, EL, Tax) = \begin{cases} 0.35 & \text{if } R \leq EL \\ 0.20 & \text{if } EL < R \text{ and } 0 < Tax \\ 0.08 & \text{if } EL < R,\ Tax \leq 0, \text{ and } EL = \text{C\$2,000,000} \\ 0.00 & \text{if } EL < R,\ Tax \leq 0, \text{ and } EL < \text{C\$2,000,000.} \end{cases}$$

This formula for the marginal credit rate shows that crossing the expenditure limit leads to a smaller increase in the after-tax cost of R&D for firms with tax liability, because the cash value of their credits will decline by C\$0.15 per dollar of R&D expenditure as opposed to the C\$0.27 or C\$0.35 drop experienced by a firm with zero-tax liability.

B. *Empirical Framework*

Our empirical analysis is based on an R&D demand equation similar to (3.1) in Hall and Van Reenen (2000), where we exploit the reformulation of the expenditure limit for identification. Specifically, we estimate the following reduced-form model of R&D spending:

$$(2) \qquad E\left[R_{it} \mid T_{it}, X_{it}\right] = \exp\left\{T_{it} \mathit{Post\text{-}Policy}_t \beta_1 + T_{it}\beta_2 + \gamma_i + \lambda_{jt} + X_{it}\theta\right\},$$

[9] In reality, credits and deductions are somewhat more valuable than we suggest here, since we do not account for the fact that firms may use them in other years.

where T_{it} is an indicator for *Eligible* firm-years with lagged taxable income between C\$200,000 and C\$500,000, *Post-Policy*$_t$ equals 1 for all years after 2003, γ_i are firm fixed effects, λ_{jt} are 3-digit NAICS-by-year effects, and X_{it} are time-varying firm-level controls.

Equation (2) is a difference-in-difference specification that compares firms inside versus outside of the phaseout region before and after the policy change, controlling for observed and unobserved firm-level heterogeneity.[10] The parameter β_2 measures the difference in R&D expenditures between eligible and ineligible firms before 2004. Because the model includes firm effects, β_2 is identified by firms that cross the eligibility threshold and have lagged taxable income above C\$200,000 during the pre-policy time period. Similarly, the average change in R&D expenditures for firms that cross the eligibility threshold during the post-policy period is $(\beta_1 + \beta_2)$. The parameter β_1 captures the pre- versus post-policy difference in the association between eligibility and R&D spending. We interpret β_1 as an intent-to-treat parameter that measures the average impact of raising the expenditure limit.

The key assumption for causal interpretation of β_1 is that β_2 is a valid estimate of the counterfactual relationship between eligibility (i.e., prior-year taxable income) and R&D expenditures in the absence of a policy change. Because we include industry-by-year fixed effects to control for aggregate time trends, the main threat to causal inference is an omitted variable that leads to an upward shift in β_2 around the same time as the policy change. To address this concern, we estimate a version of (2) that replaces *Post-Policy*$_t$ with a set of calendar-year dummies (leading to a separate β_1 parameter for each year of the panel) and show that crossing the eligibility threshold has no impact on R&D expenditures before the 2004 policy change.

The reduced-form parameter β_1 measures the impact of the policy change but not firms' sensitivity to the after-tax cost of R&D. We can use our estimates, however, to calculate an implied user-cost elasticity that is comparable to other published results. Suppose A^d is the present value of deductions and depreciation allowances and A^c is the value of tax credits, so the after-tax cost of a C\$1 R&D investment is $1 - A^d - A^c$. In Canada, $A^d = \tau$, the corporate tax rate, because R&D expenditures (including for capital) are fully deductible. We also have $A^c = \rho(1-\tau)$ because R&D tax credits are taxable income. The Hall-Jorgensen formula for the steady-state after-tax R&D user cost is then

$$U = (r+\delta)\frac{(1 - A^d - A^c)}{1-\tau} = (r+\delta)(1-\rho), \tag{3}$$

where r is the real return to the marginal R&D investment, and δ is the depreciation rate of the R&D capital stock.[11] Equation (3) shows that under SRED, the after-tax

[10] Because eligibility is a function of prior-year taxable income, (2) is not a standard difference-in-difference estimator. In particular, we never observe the average difference in outcomes for two firms with the same prior-year income but different SRED eligibility limits in a given year. Rather, our model compares the association between R&D and having prior-year taxable income in the relevant range before and after a change in SRED policy.

[11] Derivations and extensions of this formula can be found in many papers, including Jorgenson (1963), Hall and Van Reenen (2000), Bloom et al. (2002), and Rao (2016). In practice, because credits are taxed in the period after they are consumed, the exact formula for SRED is $U = (r+\delta)\{1 - \rho - r\rho\tau/((1-\tau)(1+r))\}$, where the third term reflects the benefits of deferring the tax on credits for one year. In our calculations, we assume that $r = 0.05$ and $\tau = 0.2$, though it makes no practical difference if we simply ignore the third term inside the braces.

R&D user cost depends upon the credit rate but not deductions or the corporate tax rate, because the latter two quantities are equal.[12]

The user-cost elasticity of R&D is $d\ln(R)/d\ln(U)$. For the numerator of this expression, we can substitute $d\ln(E[R])/dT_{it} = \beta_1$. The denominator can be found by substituting ρ into equation (3). For zero-tax liability firms that would have crossed the original expenditure limit threshold, $d\ln(U)/dT_{it} = -dU/U = -0.35/0.65$, while firms with tax liability have $d\ln(U)/dT_{it} = -0.15/0.65$. Thus, if s percent of eligible firms are in the shaded regions of Figure 1, and p percent of those firms have current tax liability, the implied user-cost elasticity is

$$\frac{d\ln(E[R])}{d\ln(E[U])} = \frac{0.65 \times \beta_1/s}{[0.20 \times p - 0.35]}. \tag{4}$$

Intuitively, β_1/s is the average treatment effect for treated firms, which is divided by the percentage change in R&D user costs to obtain an elasticity. Because s is derived from a counterfactual policy—applying the pre-2004 expenditure limit to post-2004 tax and R&D spending levels—we can compute implied elasticities even though very few firms actually cross the expenditure limit.

Finally, consider how this framework can be used to examine the role of fixed costs in small firms' responsiveness to R&D tax credits. Conceptually, fixed costs imply a discontinuous jump in the R&D supply curve. One source of fixed costs is specialized machinery and equipment. We expect firms that have recently made investments in R&D-related capital to have a larger supply of "bench-ready" projects. Having already incurred the sunk costs of capacity building, they should be more responsive to a change in the after-tax cost of R&D. To test this hypothesis, we estimate a triple-diffs version of equation (2) that interacts T_{it}*Post-Policy*$_t$ with an indicator for pre-2004 R&D capital investment.

Small firms also may view hiring new scientists or engineers as a fixed cost. If R&D capital accumulates within employees, hiring is based on the expectation that these knowledge workers will be retained over the long term. Tax credits can mitigate the cost of hiring but not by enough if potential future research projects are improbable and thus cause high expected rates of worker turnover. One alternative to hiring a new researcher is to outsource R&D projects to a contractor. Firms that face significant fixed costs of hiring but have a supply of one-off projects with an expected return near their hurdle rate may respond to a decrease in the after-tax cost of R&D by increasing their contract R&D spending. Thus, we can learn about the importance of fixed costs by comparing estimates of β_1 when the outcome variable is R&D wages to estimates when the outcome is contract R&D expenditures.

C. *Data and Measures*

Our data come from the tax records of the Canada Revenue Agency (CRA) for all firms claiming SRED credits during the 2000 to 2007 sample period. Our estimation sample includes all firms that operated as CCPCs throughout the sample

[12] Of course, deductions remain valuable to firms that would otherwise have to capitalize their R&D expenditures, because capitalization leads to $A^d < \tau$, which implies a higher after-tax user cost.

period and claimed R&D tax credits at least once between 2000 and 2003. We also limit the sample to firms that operated in only one province throughout the sample period to ensure that our analysis is not complicated by having to consider how firms active in multiple jurisdictions might geographically reallocate their R&D activity in response to differences in provincial R&D support.[13] This yields an unbalanced panel of 7,239 firms and 48,638 firm-year observations. Fifty percent of these firms are in service industries, 29 percent in manufacturing industries, and the remaining 21 percent are in other sectors (primarily agriculture).

Table 1 provides summary statistics for our estimation sample. Total annual SRED-eligible R&D expenditures averaged C$82,887 per year, which implies that aggregate annual R&D spending for the firms in our estimation sample was roughly C$600 million.[14] Sixty-six percent of a representative firm's annual expenditures (or C$55,217) reflect wages paid to R&D personnel. Seventeen percent of R&D expenditures (or C$14,077) were spent on contract research. Contract research reflects expenditures on the same type of activities that would qualify for SRED benefits if undertaken in-house. Tax credits on the value of those contracts are generally allocated to the client. Expenditures on R&D capital were the smallest component of R&D spending, accounting for only C$3,022 or about 3.6 percent of overall expenditures. Conditional on claiming R&D capital, however, the average expenditure was about C$27,000. The remaining 13 percent of total R&D spending is highly correlated with R&D wages, and we interpret this residual spending as overhead.[15]

Our main explanatory variables are a pair of dummies for eligibility before and after the policy change. The dummy variable *Eligible* (T_{it}) equals one in any year when a firm's prior-year taxable income falls between C$200,000 and C$500,000—the range of taxable income over which the expenditure limit increased as a result of the change in SRED (see Figure 1). We also create a variable $\textit{Post-Policy}_t$ that equals one in any year after the SRED eligibility limits were changed. The center panel in Table 1 shows that 7.3 percent of all observations are eligible and of those, 4.8 percent are treated (eligible after 2004). By far, the main reason why firms are not eligible is that their taxable income was less than C$200,000.

The average credit rate in our sample is 34.5 percent, indicating that almost all firms receive the fully refundable 35 percent credit. More than half of the firm-year observations have no tax liability. The zero-tax liability share drops to around 16 percent, however, if we condition on eligibility $(0.012/0.073 \approx 16.4\%)$.

Finally, the bottom panel in Table 1 provides summary statistics for several additional controls. Roughly 24 percent of the firms in our sample made *Pre-policy R&D Capital* investments. We use this variable as a proxy for fixed cost reduction. Our models also include revenues, assets, and liabilities as time-varying controls. The

[13] We also exclude any firm that is associated at any time during our sample period with any other firm. Under the SRED program, associated firms must share a common expenditure limit and must divide room under this limit. To simplify analysis, firms in such sets are not included in the sample.

[14] Thus, if SRED produced a 10–15 percent increase in aggregate R&D for firms in our sample, it would amount to incremental spending of C$60 to C$90 million. We do not view this amount as likely to merit investigation of general equilibrium effects or crowding out in the market for R&D labor.

[15] A two-way fixed effects regression of R&D wages on "other" R&D expenditures produces a coefficient of 0.16 with $t = 10.71$.

Table 1—Summary Statistics

Variable	Mean	SD	Min	Max
R&D indicator	0.590	0.492	0.0	1.0
Total R&D	82,887	216,352	0.0	>6.5M
R&D wages	55,217	147,591	0.0	>3.5M
R&D contracts	14,077	63,350	0.0	>2.5M
R&D capital	3,022	27,868	0.0	>2.0M
Non-R&D investment	78,420	368,447	0.0	>35M
Tax variables				
Eligible	0.073	0.260	0.0	1.0
Eligible × post-policy	0.048	0.214	0.0	1.0
Marginal credit rate	0.345	0.010	0.0	0.35
Zero-tax liability	0.568	0.495	0.0	1.0
Eligible × zero-tax	0.012	0.141	0.0	1.0
Control variables				
Pre-policy R&D capital	0.238	0.426	0.0	1.0
Total revenues[a]	1.166	3.822	<0.0	>200M
Total assets[a]	1.155	2.805	<0.0	>150M
Total liabilities[a]	0.769	1.630	0.0	>50M

Notes: All statistics are based on an unbalanced panel of $N = 48{,}638$ firm-year observations. Disclosure rules prevent reporting max and min for all variables.

[a]Millions of nominal Canadian dollars.

table shows that on average, firms in our sample had C$1.2 million in revenue and a similar amount of accounting assets.

III. Results

A. *The Impact of R&D Tax Credits*

Figure 2 provides some graphical intuition for our main result. To create the figure, we estimate a two-way fixed-effects model (i.e., a linear regression of total R&D on a full set of firm and year effects) and then use a local polynomial regression to plot the mean of the residuals from that regression against prior-year taxable income. Recall that the change in the SRED expenditure limit formula potentially lowers the after-tax cost of R&D for firms with prior-year taxable income between C$200,000 and C$500,000. So we expect to see an increase in the residual part of R&D expenditures for firms making more than C$200,000 in the post-policy period. This is exactly what we observe in Figure 2.[16]

We now turn to a regression that decomposes the residuals graphed in Figure 2. Table 2 presents estimates of the impact of expenditure limit reformulation on total R&D from Poisson-QML estimation of equation (2). A Poisson estimator handles the many cases where $R_{it} = 0$ more naturally than a log-log specification, and QML simply means that we use robust standard errors (clustered by firm) to correct for over-dispersion, leading to asymptotically correct confidence intervals. Estimates

[16]While it would be reassuring to observe a return to the same mean-zero baseline for firms above C$500,000, we do not have enough data to reliably estimate the mean residual on that portion of the support of the prior-year taxable income distribution.

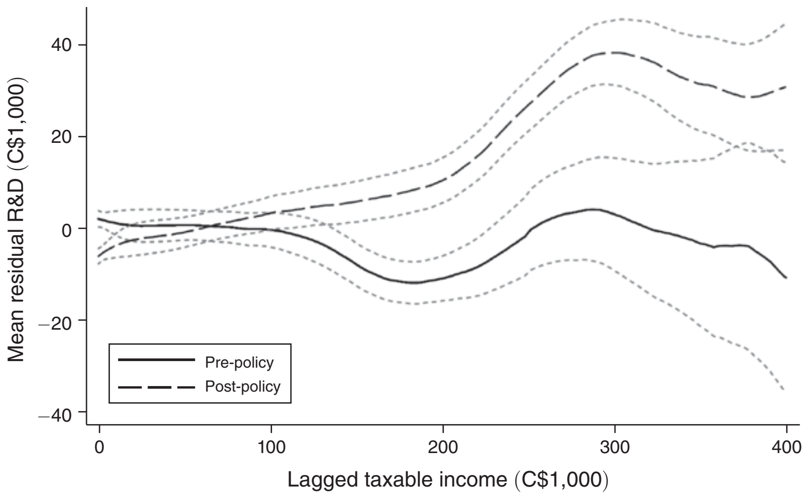

FIGURE 2. PRE- AND POST-POLICY R&D

Notes: Figure 2 plots fitted values and 95 percent confidence intervals from a local polynomial regression. The outcome variable is residual R&D obtained by estimating a two-way fixed effects model of total R&D spending that includes firm and year effects. The explanatory variable is prior-year taxable income, which determines the location of the expenditure limit (see Figure 1).

of β_1, the impact of the change in the expenditure limit, appear in the first row of the table.

Column 1 contains estimates from a parsimonious specification with only firm effects plus dummies for *Eligible*, *Post-policy*, and their interaction. The coefficient of 0.17 in the first row can be interpreted as an elasticity: crossing the eligibility threshold produces a 17 percent greater increase in R&D expenditures after the policy is in place than before. This effect is statistically significant at the 1 percent level. The coefficient on *Eligible* shows that firms above the threshold had greater R&D expenditures than firms below the threshold, even before the policy change. The coefficient on *Post-policy* shows that there was a secular trend toward more R&D expenditures over this period, even among firms that did not change eligibility status. Nevertheless, the *Eligible* $\times$ *Post-policy* interaction shows that in the post-policy time period, the average difference in total R&D expenditures between eligible and ineligible firms is about 50 percent larger than the average difference during the baseline period.

In column 2, we add 3-digit NAICS industry-by-year effects, which absorb the main effect of *Post-policy* but produce no change in our estimate of β_1. In column 3, we add the log of *Assets*, *Liabilities*, *and Revenue* as time-varying firm-level controls. Adding these size controls removes any correlation between eligibility and R&D expenditures during the pre-policy period. Even with these controls, however, we continue to find a highly significant ($p < 0.001$) increase in R&D expenditures for eligible firms following the 2004 expenditure limit reformulation.[17]

[17]Estimates from OLS regressions using log(*Total R&D*) as the outcome variable yield similar results but are sensitive to the treatment of observations with zero reported R&D expenditure (see Table B-6 in the online

Table 2—Impacts of the Change in SRED Eligibility Limits

	Specification: Poisson QML regression Unit of analysis: Firm-year					
Outcome variable	Total R&D (1)	Total R&D (2)	Total R&D (3)	R&D wages (4)	R&D contracts (5)	Non-R&D investment (6)
Eligible × post-policy	0.17	0.17	0.17	0.11	0.34	0.14
	(0.05)	(0.05)	(0.04)	(0.04)	(0.09)	(0.08)
Eligible	0.09	0.07	0.01	0.02	0.03	0.05
	(0.04)	(0.04)	(0.03)	(0.04)	(0.08)	(0.06)
Post-policy	0.11					
	(0.02)					
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year fixed effects	No	Yes	Yes	Yes	Yes	Yes
Controls	No	No	Yes	Yes	Yes	Yes
Observations	48,638	48,638	48,638	38,748	36,235	46,809
Number of firms	7,239	7,239	7,239	5,806	5,378	6,895
Mean of outcome	82,887	82,887	82,887	69,310	18,895	81,732
Psuedo-R^2	0.75	0.75	0.82	0.84	0.68	0.75
Implied user-cost elasticity[a]						
Lower bound	−0.71	−0.71	−0.71	−0.46	−1.42	
($s = 0.59, p = 0.43$)	(0.21)	(0.21)	(0.17)	(0.17)	(0.38)	
Upper bound	−4.57	−4.57	−4.57	−2.95	−9.13	
($s = 0.11, p = 0.65$)	(1.34)	(1.34)	(1.07)	(1.07)	(2.42)	

Notes: Robust standard errors (clustered by firm) are in parentheses. All models are estimated using an unbalanced panel of all available firm-years; changes in sample-size occur when firms with all-zero outcomes are dropped from the conditional fixed-effects specification. The mean value of the outcome variable is calculated for all firm-years used in the estimation.

[a] See text for discussion of user-cost elasticity calculations.

Columns 4 through 6 in Table 2 examine different outcome variables.[18] Column 4 shows an 11 percent increase for *R&D Wages*. Column 5 reports that *Contract R&D* expenditures increase by 34 percent. Because wages account for two-thirds of R&D spending, the dollar-denominated impact on wages and contracts is very similar (see Table 5). The scale-free coefficient on *Contract R&D* is twice that of *Total R&D*, however, and three times the size of the *R&D Wages* effect.[19]

Unfortunately, our data on the R&D wage bill do not distinguish between hiring additional employees (real effects) and paying higher R&D wages (crowding out). If starting a new project requires hiring a new R&D employee, however, we expect that fixed costs reduce the impact of a more favorable tax credit policy. Intuitively, these small firms face an integer constraint—new employees must be hired one at a time—and an incremental unit of R&D labor is not a negligible expenditure for

Appendix). Santos-Silva and Tenreyro (2006) explain how log-linear models can produce biased estimates, particularly in applications with many zeroes, and suggest using Poisson-QML as an alternative.

[18] Sample sizes change for different outcomes because our models contain a multiplicative fixed effect, and therefore all observations with all-zero outcomes are dropped. As a robustness check, we rerun all regressions with the outcome set to $\max\{1, R_{it}\}$ and obtain identical results.

[19] We also estimate the impacts for R&D capital and other R&D spending. Neither effect is statistically different from zero.

firms whose average R&D wage bill is C$55,217 (roughly the starting salary for a single engineer).[20]

Our discussions with managers and tax practitioners also suggest several ways that firm-level fixed costs might influence the decision to outsource R&D. First, if managers view both their research budget *and* the quantity of permanent R&D labor as fixed factors, contracting provides a way to exhaust the budget when tax incentives reduce the cost of internal R&D. Second, contract R&D may provide a relatively transparent (i.e., easy to document) form of R&D expenditure. Thus, even if a firm could allocate its current employees to a new research project, managers may favor contract R&D because they believe use of contracted R&D services facilitates the assessment of these expenditures for purposes of the tax credit.[21] Finally, contractors can pass any SRED-related tax savings to clients in at least two different ways: by allowing a client to claim the credits directly or by claiming the credit themselves and passing the savings to clients in the form of lower prices.

Table 2 column 6 considers whether our main results might come from relabeling of other types of investment as R&D. For example, Chen et al. (2017) suggest that roughly 30 percent of the measured response to a Chinese fiscal R&D incentive comes from relabeling. Column 6 examines changes in *Non-R&D Investment*. If the observed increase in *Total R&D* reflects relabeling of expenditures that firms would have made even in the absence of a SRED program change, we would expect a reduction in other types of investment. Instead, we find a statistically insignificant 14 percent increase in non-R&D capital expenditure for eligible firms in the post-policy period.[22]

Finally, the bottom panel of Table 2 reports implied user-cost elasticities for different combinations of s and p based on equation (4), along with standard errors calculated via the delta method. In the top row, we use the overall sample means. Specifically, we assume any firm with positive R&D faces a binding expenditure limit ($s = 0.59$) and that 57 percent of those firms have no tax liability ($p = 0.43$), which leads to an implied elasticity of -0.71 for *Total R&D*, -0.46 for *R&D Wages*, and -1.42 for *R&D Contracts*. We see these figures as a lower bound on the magnitude of the true user-cost elasticity, because the credit rate does not actually change for most firms (suggesting s should be smaller) and because eligible firms are more likely to pay tax (suggesting p should be larger).

Our second set of user-cost calculations assumes $s = 0.11$, which is the share of post-policy observations having R&D expenditures above the pre-policy expenditure limit (i.e., the share of eligible firms in the shaded parts of Figure 1). We also assume $p = 0.65$, which is the share of firms with current tax liability in this "treated" group. These assumptions imply a user-cost elasticity of -4.57 for *Total R&D*, -2.95 for *R&D Wages*, and -9.13 for *Contract R&D*. This is

[20]The website talentegg.ca reports starting salaries for Canadian engineers between C$57,000 and C$84,000, with a median of roughly C$65,000 in 2013 or about C$60,000 in 2008 dollars.

[21]We find supporting evidence for this story by examining related party (i.e., non-arm's-length) contract R&D expenditures and finding that they are a significant piece of the overall contract R&D effect.

[22]If we change the outcome to *Non-R&D Investment/Sales*, the coefficient changes sign and remains statistically insignificant, suggesting that the positive estimate in Table 2 column 6 is not driven by complementarities between R&D and non-R&D spending or a large income or scale effect.

a large response. At the same time, the 95 percent confidence interval for *Total R&D* covers other recent estimates for small and medium-sized firms (e.g., Dechezleprêtre et al. 2016), and the very large elasticity for *Contract R&D* reflects substitution from wages to contracts as well as a lower baseline spending level.

We view the calculations in the bottom row of Table 2 as an upper bound on the true user-cost elasticity, because our data show that R&D increases even among firms that do not cross the pre-2004 expenditure limit. In particular, if we discard all observations with R&D expenditures above the pre-2004 expenditure limit, our baseline empirical model still finds positive and statistically significant impacts of the policy change.[23] This suggests that the true value of s is greater than 11 percent. This is an interesting finding in its own right because it implies that firms respond not only to their marginal tax rates but also the amount of "spending room" they have before exhausting the fully refundable 35 percent SRED credits.

B. *Trends and Timing*

One concern with our difference-in-difference design is that firms may have anticipated the policy change and tried to manipulate their treatment status before its implementation. As a practical matter, it is unclear how firms that anticipate a policy change should respond. Zero-tax liability firms with R&D spending in the vicinity of the pre-policy expenditure limit may delay some R&D spending to take advantage of the shift. On the other hand, firms with taxable income that anticipate large R&D expenditures will typically want to accelerate their spending in order to create a current-year deduction and an increase in the next year's expenditure limit threshold. Thus, the bias could go in either direction. As a robustness test, we re-estimated our baseline model using a sample that discards all data from the years 2003 and 2004. For that sample, the coefficient on *Eligible* × *Post-policy* increases to 0.22 with a standard error of 0.06 (not significantly different from our baseline estimates).[24] We conclude that any bias from firms anticipating the SRED policy change was small and likely caused us to underestimate the impact of the policy.

A second concern with our difference-in-difference research design is that β_1 may be measuring a secular trend in β_2. In other words, the treatment effect may reflect a preexisting trend in the relationship between R&D expenditures and eligibility. This concern motivates the standard "parallel-trends" falsification test, which we implement via an event-study specification. Specifically, we estimate

$$E\left[R_{it} \,|\, T_{it}, X_{it}\right] \;=\; \exp\left\{T_{it}\beta_t + \gamma_i + \lambda_{jt} + X_{it}\theta\right\},$$

where the β_t are year-specific treatment effects that measure the difference in R&D spending for eligible versus noneligible firms in each year of the panel. The parallel-trends test is equivalent to a test of the null hypotheses that β_t are jointly

[23] We report these estimates in online Appendix Table B-3.

[24] Estimation results for a variety of samples that exclude observations from 2003, 2004, and 2005 are provided in Table B-5.

Table 3—Event Study Specification

	Specification: Poisson QML Unit of analysis: Firm-year		
Outcome	Total R&D (1)	R&D wages (2)	R&D contracts (3)
Eligible × **1**[2000]	0.09	0.16	−0.07
	(0.09)	(0.09)	(0.25)
Eligible × **1**[2001]	−0.02	0.00	0.10
	(0.07)	(0.07)	(0.16)
Eligible × **1**[2002]	−0.07	−0.02	−0.07
	(0.05)	(0.05)	(0.10)
Eligible × **1**[2003]	0.04	0.01	0.12
	(0.04)	(0.04)	(0.12)
Eligible × **1**[2004]	0.07	0.07	0.14
	(0.03)	(0.03)	(0.10)
Eligible × **1**[2005]	0.19	0.15	0.39
	(0.04)	(0.04)	(0.12)
Eligible × **1**[2006]	0.23	0.16	0.49
	(0.04)	(0.04)	(0.13)
Eligible × **1**[2007]	0.19	0.11	0.45
	(0.04)	(0.04)	(0.13)
Firm fixed effects	Yes	Yes	Yes
Industry-year effects	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes
Pre-trends F-statistic	5.59	3.82	2.33
(p-value)	0.23	0.43	0.68
Observations	48,638	38,748	36,235
Total firms	7,239	5,806	5,378
Mean of outcome	82,887	69,310	18,895
Pseudo-R^2	0.82	0.84	0.68

Notes: Robust standard errors (clustered by firm) are in parentheses. Pre-trends F-statistic (p-value) is for null hypothesis that β_{2000} through β_{2003} are jointly equal to zero. All models are estimated using an unbalanced panel of all available firm-years; changes in sample size occur when firms with all-zero outcomes are dropped from the conditional fixed-effects specification.

zero for all $t \leq 2003$. Table 3 reports estimates of β_t for each of the three main outcomes: *Total R&D*, *R&D Wages*, and *Contract R&D*.

In the bottom row of the table, we report tests of the parallel-trends hypothesis. For all three outcomes, we find no statistically significant evidence of a pre-policy trend in β_1. This lends credibility to a causal interpretation of the reduced-form results in Table 2, since the main threat to our identification strategy is an upward trend in the slope of the lagged-earnings-to-R&D relationship over the entire sample period.

Table 3 also provides evidence on the timing of the policy impacts. In particular, the coefficients in 2003 and 2004 are generally small and statistically significant, while the coefficients for 2005 through 2007 are two or three times larger and statistically significant at conventional levels. The pattern of treatment effects, starting in 2004 and increasing through 2006, also suggests that firms face adjustment costs in their R&D spending. If that is the case, the true user-cost elasticity of R&D will be somewhat larger than our estimates in Table 2, because those coefficients are based on an average that includes the (smaller) short-term response.

C. *Treatment Heterogeneity: Tax Liability and Fixed Costs*

This subsection estimates a pair of triple-difference models that allow the impact of the SRED policy change to vary across firm-years. We use these models to examine treatment heterogeneity for zero-tax liability firms and firms that made pre-2004 R&D capital investments. Specifically, let the indicator variable D_{it} divide the estimation sample into two groups according to current-year tax liability or pre-policy R&D capital investments. We estimate the following regression,

$$(5)\quad E\left[R_{it}\,|\,T_{it}, D_{it}, X_{it}\right] = \exp\left\{D_{it}T_{it}\textit{Post-Policy}_t\beta_1 + D_{it}\textit{Post-Policy}_t\beta_2 + D_{it}T_{it}\beta_3 + T_{it}\textit{Post-Policy}_t\beta_4 + T_{it}\beta_5 + D_{it}\beta_6 + \gamma_i + \lambda_{jt} + X_{it}\theta\right\},$$

where all of the other variables are defined above. Note that this model contains a full set of two-way interactions and that the main effects of $\textit{Post-Policy}_t$ are subsumed in the industry-by-year fixed effects.

In the first three columns of Table 4, the indicator D_{it} is a dummy for firm-year observations with no current tax liability. The *Zero-tax* dummy variable identifies firms that cannot deduct their marginal dollar of R&D expenditure and who therefore face a steeper increase in the after-tax cost of R&D when crossing the expenditure limit threshold. Put differently, these firms benefit not only from a higher credit rate but also from the refundable nature of SRED credits, because they have exhausted the value of any tax deductions.

Estimates of the triple-difference coefficient, β_1, appear in the top row of Table 4. One way to interpret the results in columns 1 through 3 is to note that the coefficient on *Eligible* × *Policy* is the difference-in-difference estimate for firms that pay tax, whereas the difference-in-difference estimate for zero-tax liability firms is found by adding the coefficient on *Eligible* × *Policy* to the three-way interaction. These estimates indicate that the impact of expenditure limit reformulation was three to five times larger for zero-tax liability firms across all three outcomes, although β_1 is significant at the 5 percent level only for *Total R&D* spending.[25]

In columns 4 through 6, D_i corresponds to an indicator for firms that made R&D capital investments in the pre-policy period. Because this is a time-invariant explanatory variable, the main effect of D_i is absorbed by firm fixed effects. For these models, we exclude firms in the Professional, Scientific and Technical Services sector (NAICS 541) from the estimation sample because we expect fixed costs to be less

[25] The triple-differences coefficient for *Contract R&D* spending is significant at the 10 percent level, and the significance of β_1 for *Wages* depends upon the choice of specification (e.g., see Table B-4). In Table B-7, we also show that these results are robust to using an alternative definition of zero-tax liability based on taxes paid rather than current tax liability.

Table 4—Tax Liability and Fixed Costs

	Specification: Poisson QML regression Unit of analysis: Firm-year					
Sample	All firm-years			Non-NAICS 541 firm-years		
R&D outcome variable	Total (1)	Wages (2)	Contracts (3)	Total (4)	Wages (5)	Contracts (6)
Eligible × policy × zero-tax	0.16 (0.08)	0.11 (0.08)	0.36 (0.21)			
Policy × zero-tax	−0.24 (0.04)	−0.16 (0.04)	−0.31 (0.08)			
Eligible × zero-tax	−0.08 (0.07)	−0.07 (0.07)	−0.13 (0.16)			
Zero-tax liability	0.12 (0.03)	0.10 (0.03)	0.10 (0.07)			
Eligible × policy × capital				0.24 (0.11)	0.22 (0.10)	0.09 (0.23)
Policy × capital				−0.26 (0.05)	−0.18 (0.05)	−0.24 (0.11)
Eligible × capital				−0.14 (0.09)	−0.14 (0.08)	−0.06 (0.18)
Eligible × policy	0.04 (0.05)	0.02 (0.05)	0.11 (0.12)	−0.01 (0.05)	−0.07 (0.05)	0.32 (0.16)
Eligible	0.07 (0.04)	0.07 (0.04)	0.11 (0.10)	0.10 (0.04)	0.13 (0.05)	−0.01 (0.13)
Firm fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Industry-year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Additional controls	Yes	Yes	Yes	Yes	Yes	Yes
Psuedo-R^2	0.81	0.84	0.67	0.74	0.74	0.58
Observations	48,638	38,748	36,235	34,595	25,964	26,133
Total firms	7,239	5,806	5,378	5,051	3,837	3,793
Mean of outcome	82,887	69,310	18,895	66,176	57,108	13,393

Notes: Robust standard errors (clustered by firm) are in parentheses. All models are estimated using an unbalanced panel of all available firm-years; changes in sample size occur when firms with all-zero outcomes are dropped from the conditional fixed-effects specification. The mean value of the outcome variable is calculated for all firm-years used in the estimation.

relevant for R&D service providers—with a steady flow of projects, there is less chance that specialized assets will sit idle.[26]

If capital expenditures represent a fixed cost that dampens small firms' response to a change in the after-tax cost of R&D, then firms with pre-policy R&D capital investment should have a larger response to the policy change, because those fixed costs have already been sunk. This is exactly what we see in columns 4 and 5 of Table 4, where firms that made ex ante R&D capital expenditures increase *Total R&D* spending by 25 percent more and *R&D Wages* by 24 percent more than other

[26] Examples of firm types in this industry are engineering and internet consulting companies as well as specialized software development companies. In a previous version of the paper, we estimated triple-difference models that included a three-way interaction for firms in NAICS 541 and found that they were more responsive to the SRED policy change on the intensive margin (*R&D Wages*), but not the extensive margin (*Contract R&D*), relative to firms in other industries. This is the same pattern we find for firms *outside* NAICS 541 with pre-2004 R&D capital investments.

eligible firms in the post-policy period.[27] The fact that we find no statistically significant change on the extensive margin (*R&D Contracts*) is also consistent with the fixed cost hypothesis. In particular, the results in Table 2 show that on average, contract spending is more responsive to the policy shift than wages. But once the necessary fixed costs are sunk, these results suggest that more of the incremental R&D spending shifts back inside the firm.

D. *Decomposition of Policy Impacts*

Before concluding, we use the estimates reported above to decompose the overall impact of the policy change into a series of dollar-denominated effects for firms with and without tax liability. The top panel in Table 5 shows the firm-level impacts of expenditure limit reformulation. To compute these figures, we multiplied the pre-2004 mean of each outcome variable by our difference-in-difference estimates of the policy impact. For the *All Firms* column, we use estimates from Table 2, columns 3 to 5. For the *Taxes Owed* and *Zero-Tax Liability* columns, we use estimates from Table 4, columns 1 and 2. The bottom panel in Table 5 shows the *aggregate* impact of the policy along each margin. These numbers are calculated by multiplying the figure in the top panel by the average annual post-policy observation count in each cell.

The results in the top panel of Table 5 show that in dollar terms, zero-tax liability firms increased their annual R&D spending by roughly an order of magnitude more than firms owing tax in the current year. This reflects a difference in baseline expenditures—firms without tax liability spend roughly 2.5 times more on R&D—and a larger impact of the SRED policy change. The top panel also shows that for each type of firm (i.e., with and without tax liability), spending on contracts increased by roughly the same amount as the R&D wage bill. The bottom panel of Table 5 shows that zero-tax liability firms account for more than half of the aggregate impact of the SRED expenditure limit reformulation. Overall, the calculations in Table 5 indicate that firms that face no tax liability, and have therefore exhausted any benefit from deductibility of R&D expenditures, are very responsive to the refundable SRED credits. This finding suggests that the recent change in the US tax policy described in the introduction may have a substantial impact on small-firm R&D expenditures.

IV. Conclusions

We exploit a change in eligibility rules for R&D investment tax credits under the Canadian SRED policy to estimate the impact of this program on small-firm R&D expenditures. Privately owned firms that became eligible to benefit from a refundable 35 percent R&D tax credit on a greater amount of qualified R&D expenditures increased their R&D spending by an average of 17 percent compared to before the program. This corresponds to an R&D user-cost elasticity between −0.7 and −4.57. Our findings contribute to a growing body of evidence suggesting that tax incentives

[27] Table B-4 shows that we obtain very similar results using a balanced panel.

Table 5—Marginal Effects at Sample Means

	All firms	Taxes owed	Zero-tax liability
Firm-level impact (C$ per year)			
Total R&D	27,419	4,991	64,597
	(8,065)	(6,239)	(23,384)
R&D wages	11,668	1,746	24,665
	(4,243)	(4,365)	(14,032)
R&D contracts	8,977	2,245	24,911
	(2,376)	(2,449)	(8,605)
Aggregate impact (C$Million per year)			
Total R&D	16.1	2.5	6.0
	(4.7)	(3.1)	(2.2)
R&D wages	6.8	0.9	2.3
	(2.5)	(2.2)	(1.3)
R&D contracts	5.3	1.1	2.3
	(1.4)	(1.2)	(0.8)

Notes: Calculations for All firms are based on estimates in Table 2, columns 3 to 5. Calculations for subsamples with and without tax liability are based on estimates in Table 4, columns 1 to 3. Standard errors are in parentheses.

can induce private R&D expenditures, even among small and young firms (e.g., Dechezleprêtre et al. 2016). While small firms account for a modest share of aggregate R&D, they may have a comparative advantage in specific types of innovation, and linking our findings to innovation outcomes is an important topic for future research.

This study also provides several pieces of evidence that fixed costs play an important role in how small firms respond to a change in the after-tax cost of R&D. First, we decompose R&D spending into wages and contracts and show that estimated user-cost elasticities are much larger for contract R&D expenditures. Second, we show that the response to the SRED policy change was larger among firms that recently made R&D-related capital investments and that for those firms, the response was stronger for the R&D wage bill than for contract expenditures. Many of the firms in our sample are small enough that they may not be able to fully utilize an additional scientist or engineer, and these findings suggest that such firms use external contract R&D to avoid the fixed costs of launching a new project internally but shift back to internal work after capacity is in place.

Beyond providing new evidence on fixed costs and the response of small firms to the R&D tax credit, our findings are useful for projecting the effects of recent US tax policy changes in this area. Prior studies of the US R&D tax credit have typically focused on larger firms, reflecting both data availability and the fact that smaller firms with no tax liability received limited benefits. In our sample of small Canadian firms, over half of the firm-year observations had no tax liability and would therefore only receive carry-forwards under US policy prior to 2016 as opposed to cash under SRED. We show that Canada's SRED program is particularly effective at stimulating R&D for these small zero-tax liability firms. Evaluating the impacts of recent changes in the US R&D tax credit is a promising topic for future research, particularly if it becomes possible to link policy-induced changes in R&D spending to innovation outcomes for a broader range of public and privately owned firms.

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