Nothing is Certain Except Death and Taxes: 
The Lack of Policy Uncertainty from Expiring “Temporary” Taxes

Andrew C. Chang*

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Abstract

What is the policy uncertainty surrounding expiring taxes? How uncertain are the approvals of routine extensions of temporary tax policies? To answer these questions, I use event studies to measure cumulative abnormal returns (CARs) for firms that claimed the U.S. research and development (R&D) tax credit from 1996-2015. In 1996, the U.S. R&D tax credit was statutorily temporary but was routinely extended ten times until 2015, when it was made permanent. I take the event dates as both when these ten extensions of the R&D tax credit were introduced into committee and when the extensions were signed by the U.S. president into law. On average, I find no statistically significant CARs on these dates, which suggests that the market anticipated these extensions to become law. My results support the fact that a routine extension of a temporary tax policy is not a generator of policy uncertainty and, therefore, that a routine extension of temporary tax policy is not a fiscal shock.

JEL Codes: E62; G12; G14; G17; H25; H39; K34; O31
Keywords: Cumulative Abnormal Returns; Excess Returns; Event Study; Fiscal Policy; R&D; Research and Development; Sunset Provision; Tax Extension; Temporary Tax; Uncertainty Shocks; User Cost of Capital

*Senior Economist, Division of Research and Statistics, Board of Governors of the Federal Reserve System. 20th St. NW and Constitution Ave., Washington DC 20551 USA. +1 (657) 464-3286. a.christopher.chang@gmail.com. https://sites.google.com/site/andrewchristopherchang/. ORCID: 0000-0002-9769-789X.

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1 Introduction

This paper analyzes whether routine extensions of statutorily temporary taxes are expected events.

Governments often design fiscal policies as statutorily temporary instead of permanent, perhaps to make the policies more appealing to voters or to increase the chance of forming a winning political coalition. But when these temporary policies expire the government often extends them. The state of Pennsylvania implemented an “emergency” corporate income tax in 1935 that was supposed to last two years — but the state of Pennsylvania extended the “emergency” tax at every single Pennsylvania legislative session until the tax was finally made permanent in 1957 — twenty two years after it was first passed.\(^1\) More recently, U.S. President George W. Bush’s 2001 tax cuts\(^2\) were set to expire after ten years so that they could pass with a simple majority, instead of a super majority, of senate votes. But when the time came for the tax cuts to expire, U.S. President Barack Obama extended the cuts through the end of 2012,\(^3\) and then in 2013 made most of them permanent.\(^4\)

But while statutorily temporary taxes are routinely extended, a necessary condition for determining their economic significance is estimating whether the taxes were expected to be extended. Expectations matter. Statutorily temporary taxes, such as many of the individual and pass-through taxes of the 2017 Tax Cuts and Jobs Act\(^5\), that everyone expects to be extended have economic effects equivalent to permanent taxes. In this case, statutory extensions of temporary taxes would not be fiscal shocks. Conversely, temporary taxes that are not expected to be extended have the economic effects of temporary taxes. And statutory extensions, which would be unexpected, are fiscal shocks.

I measure expectations of tax policy extensions using event studies and cumulative ab-

\(^1\)See Committe on Continuation of the Tax Study (1944); McKenna (1960); Commonwealth of Pennsylvania Department of Revenue (2007).


\(^3\)Public Law 111-312, the Tax Relief, Unemployment Insurance Reauthorization, and Job Creation Act of 2010.

\(^4\)Public Law 112-240, the American Taxpayer Relief Act of 2012.

\(^5\)Public Law 115-97.
normal returns (CARs) for firms that claimed the U.S. research and development (R&D) tax credit from 1996-2015. In 1996, the R&D tax credit was statutorily temporary but was routinely extended ten times until 2015, when it was made permanent. I measure CARs both when these ten extensions were introduced into committee and when they were signed by the U.S. president into law.

The key assumption linking CARs to expectations of tax policy extensions is that the market’s expectation about whether these extensions would become law should be reflected in the stock prices of firms that benefited from these extensions. If the market anticipated these tax credit extensions to become law, then the CARs for the firms that benefited from the extensions should be zero, as the passage of the bill was already integrated into firms’ stock prices before the extensions became law. Alternatively, if the market did not anticipate these extensions to become law, then the CARs should be positive, as the extended credit would have been an unexpected asset for these firms.

On average, I find no statistically significant CARs for firms that benefited from extensions of the R&D tax credit. This result suggests that the market anticipated these extensions to become law. My results support the fact that a routine extension of a temporary tax policy is not a generator of policy uncertainty and, therefore, that a routine extension of temporary tax policy is not a fiscal shock.

As far as I am aware of, this paper is the first to test whether routine extensions of statutorily temporary taxes are expected. My results contribute to at least four literatures.

The first literature is on measuring economic uncertainty.\textsuperscript{6} In particular, earlier versions of the Baker, Bloom, and Davis (2015, 2016) headline economic policy uncertainty index use the value of expiring taxes as an input, while later versions report the uncertainty of expiring taxes as a separate index (Economic Policy Uncertainty, 2017). My results suggest that, at least for expiring taxes that are routinely extended, that these policies should not be counted as a contributor to measured economic uncertainty.

\textsuperscript{6}For example: Jurado, Ludvigson, and Ng (2015); Baker, Bloom, and Davis (2015, 2016); Husted, Rogers, and Sun (2016).
The second literature is on the effects of uncertainty, or lack of uncertainty, on real activity. Because I find that the extension of expiring taxes are, on average, anticipated, my results support the position of treating these statutorily temporary policies as if they were permanent when estimating their effects on real activity.

The third literature is on estimating the effects of fiscal stimuli after the stimuli are in effect. In this literature, the assumptions on how to model extensions of fiscal policies vary, but my results support the modeling strategy of treating routine extensions as if the extensions are not fiscal shocks, as in Romer and Romer (2010).

The fourth literature is on predicting the effects of fiscal stimuli before the stimuli are in effect. Outside of the academic literature, modeling the effects of fiscal stimuli before they are implemented is also of paramount importance to several policymaking agencies including: the Board of Governors of the Federal Reserve System, the Congressional Budget Office, the Congressional Budget Office, and the Congressional Budget Office.

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8At one end of the spectrum, extensions are ignored when constructing fiscal shocks, which implicitly assumes that statutorily temporary policies have economic effects that are equivalent to statutorily permanent policies. This end of the spectrum occupied by, among others, Romer and Romer (2010)’s narrative method of constructing exogenous fiscal shocks (papers that use the Romer and Romer (2010) fiscal policy series will also implicitly treat statutorily temporary policies as economically equivalent to statutorily permanent ones, for example Mertens and Ravn 2012). At the other end of the spectrum are papers that make the critical assumption that statutorily temporary policies have the economic effects of temporary policies, as in House and Shapiro (2008). And in the middle of the spectrum are papers that either do not model whether fiscal policies are statutorily temporary or permanent, as in Blanchard and Perotti (2002), or papers where the statutory status of the fiscal policy only affects the interpretation of results, not effect sizes, as in Porcelli and Trezzi (2014); Zwick and Mahon (2017).

9One branch of this literature is concerned with investigating the effects of innovation-enhancing policies, of which fiscal policies are one tool to enhance innovation (Arrow, 1962; Bloom, Griffith, and Van Reenen, 2002; Guellec and van Pottelsbergh, 2003; Wilson, 2009; Chang, 2018; Guceri and Liu, Forthcoming). In this literature, the assumption that statutorily temporary policies are economically equivalent to permanent ones is also used by several studies on the effects of R&D tax credits on R&D expenditures, as in Bloom, Griffith, and Van Reenen (2002); Wilson (2009); Chang (2018).

10For example, Pinto and Tevlin (2014), who use an accelerator-style model with the Hall and Jorgenson (1967) user cost of capital to forecast U.S. investment. But the standard form of the Hall and Jorgenson (1967) user cost of capital assumes that temporary tax policies will be extended, and also omits expectations of future fiscal policies. See also Yang (2005, 2007); Mertens and Ravn (2011); Leeper, Walker, and Yang (2013) on modeling macroeconomic effects of future tax changes.
Office, the Council of Economic Advisers, and the Government Accountability Office. And at the 2016 meeting of G20 countries, a central theme was improving certainty over future taxes in order to support growth (Gurría, 2016). My results suggest that, at least in the context of a particular policy that has been extended multiple times, fears over tax policy uncertainty may be overstated.\footnote{Before the U.S. R&D tax credit was made permanent, policy briefs by Guenther (2015) and Rao (2015) discussed the effects of uncertainty over whether the R&D tax credit would be extended, but conducted no analysis of expectations of whether the credit would be extended.}

2 Data and Method for Computing Cumulative Abnormal Returns

For each extension of the R&D tax credit from 1996 to 2015, I use three main data sources to perform my event studies:

1. Compustat-CRSP merged database for firm financials and trading-day stock prices (Center for Research in Security Prices, 2016).\footnote{I downloaded Compustat-CRSP on August 4th, 2016, so most firms have data through the end of 2015. Different data versions can lead to different estimates, even when using the same time period (Faust, Rogers, and Wright, 2003; Koenig, Dolmas, and Piger, 2003; Chang and Li, 2018), so I fix the version of data.}

2. Legislative histories and roll call votes from the United States Congress (2015), which track each bill’s progression towards enactment. From these histories I take the event dates as the ten dates that extensions were introduced into committee (introduction dates) and the ten dates that extensions were subsequently signed by the U.S. president into law (passage dates), for a total of twenty events, shown in Figure 1. I choose the introduction and passage dates as my event dates because they are the first and last major observable legislative actions on a bill.

3. Legislative text from the U.S. Government Publishing Office (2015).\footnote{For additional details on the R&D tax credit, see Guenther (2015).}

I first restrict the sample to ordinary shares (Compustat-CRSP share codes 10 and 11)
and firms with positive book and market equity.\textsuperscript{14} I drop the Electric, Gas, and Sanitary Services industry (SIC 49), drop financial firms (SIC 60, 61, 62, 67), and drop the Public Administration industry (SIC 91 to 97). I drop firms if they are missing market equity, book equity, or both tic and gvkey.

I then classify each firm-fiscal year using realized Compustat-CRSP financials into one of three bins: (1) firm-fiscal years that did R&D and received a R&D tax credit (RDCredit), (2) firm-fiscal years that did R&D but did not receive a R&D tax credit (RDnoCredit), (3) firm-fiscal years that did no R&D and, thus, did not receive a R&D credit (noRDnoCredit).\textsuperscript{15}

Figure 2 plots the expected increase in market equity for the portfolio of RDCredit firms, by bill passage year, if the extensions were unanticipated by the market. The figure assumes that R&D tax credit extensions are valued at the market to book ratio twenty trading days before passage times the book value of the extension for the current fiscal year, while future fiscal years are discounted by 15\% per year with proration for partial fiscal years. Figure 2 shows that the average expected increase, under unanticipated extensions, across all bills is about eighty basis points. The third extension, passed in 1999, also causes a larger than average expected increase in market equity.

The main outcome variable is the cumulative abnormal return (CAR) of the portfolio of RDCredit firms, which is the difference between $creturn_{t}^{RDCredit}$, the value-weighted cumulative return of the portfolio of RDCredit firms calculated using simple returns in the cross-section then differenced-natural logarithmic returns in the time-series,\textsuperscript{16} and the cumulative return of either the RDnoCredit firms, $creturn_{t}^{RDnoCredit}$, or the noRDnoCredit firms, $creturn_{t}^{noRDnoCredit}$. In this setup, I assume that the correlation between the cumulative returns of the RDCredit portfolio and the respective counterfactual cumulative return

\textsuperscript{14}I define book and market equity following Fama and French (1993). Book equity is the book value of stockholders’ equity (seq) plus balance-sheet deferred taxes and investment credits (txditc) minus the book value of preferred stock (pstk) using redemption (pstkrv) or, when redemption is also missing, liquidation (pstkl) for the book value of preferred stock when the book value of preferred stock is missing. Market equity is price (prc) times shares outstanding (shrout).

\textsuperscript{15}See the calculations in Appendix A for details.

\textsuperscript{16}See MacKinlay (1997); Perttunen (2015).
portfolio in the absence of an event is one ($\beta = 1$). This assumption is equivalent assuming that the portfolios for RDnoCredit or noRDnoCredit firms provide good counterfactual cumulative returns for the portfolio of RDCredit firms, as in a difference-in-differences setup.

The $\beta = 1$ assumption is motivated by the fact that estimating a model for $\beta$ will always lead to a larger standard error than assuming $\beta = 1$. This point is emphasized by Sercu, Vandebroek, and Vinaimont (2007), who find that reducing estimation error in $\beta$ always comes at the expense of higher standard errors. Assuming $\beta = 1$ might be biased but it also has the large benefit of having zero standard error.

Under the $\beta = 1$ counterfactual and letting $t = 0$ denote an event date, I calculate the CARs as the difference between $creturn_t^{RDCredit}$ either $creturn_t^{RDnoCredit}$ or $creturn_t^{noRDnoCredit}$ over three separate event windows: $t = [-5, 5]$, $t = [-3, 3]$, and $t = [-1, 1]$. Because the RDCredit portfolio tends to have a higher return than the other two portfolios, I also apply a bias correction using the difference in daily returns between $creturn_t^{RDCredit}$ and the respective counterfactual over $t = [-270, -21]$, scaled to the event window size. I use a t-test for statistical significance by taking the sample standard deviation of the difference between the daily returns of the RDCredit portfolio and the respective counterfactual portfolio over $t = [-270, -21]$ and scale this estimate of the standard deviation of daily returns to the size of the respective event window. This procedure creates an event date-event window-counterfactual portfolio specific estimate of the standard deviation of CARs.

3 Cumulative Abnormal Return Results

Figure 3 displays a scatter chart for CARs in excess of the expected increase in market

\[17\] Ahern (2009) also finds that generating abnormal returns by subtracting the raw return of a matched portfolio, where matching is done on an equal-weighted portfolio of ten control stocks matched by size and prior returns, also performs best for generating counterfactual returns.

\[18\] There is a large emphasis on unbiased estimates in the economics and finance literature, but in practice there is almost always a bias vs. precision tradeoff. And the standard root-mean squared error loss function accounts for this tradeoff.

\[19\] In practice this bias correction is small. The RDCredit portfolio tends to have a higher monthly return of between 8 to 24 basis points.
equity under unanticipated extensions, by bill introduction date. Each year has six estimates of CARs as there are three event windows and two counterfactual portfolios for each introduction date. Blue circles indicate that the CAR estimate is not statistically greater than zero at the 5% level or lower, whereas red squares indicate that the estimate is significant. Therefore, estimates that are above the zero line indicate that the CAR was larger than expected under an unanticipated extension. Figure 3 shows that there is, on average, no statistically significant effect of the R&D tax credit extensions on CARs.

Figure 4 displays an analogous scatter chart for bill passage dates. Figure 4 shows that, for the 6th extension passed in October 2008 during the middle of the Great Recession, there was a significant increase in CARs. That said, Figure 4 does not show a systematic pattern of positive CARs for the other bills passed outside of the Great Recession, suggesting that, on average, the market anticipated these extensions to become law.

Figure 5 is an alternative, pooled visualization of the returns on bill introduction dates. The figure plots the cumulative return of the RDCredit portfolio against a counterfactual cumulative return under unanticipated extensions. Therefore, estimates that are above the black 45 degree line indicate that the cumulative return for the RDCredit portfolio was larger than the expected increase in returns under an unanticipated extension. Again, blue circles indicate that the CAR is not statistically greater than zero at the 5% level or lower, whereas red squares indicate that the estimate is significant. Figure 5 shows that most of the returns are clustered around or just below the origin, indicating little movement in returns on bill introduction dates.

Figure 6 is the alternative visualization for bill passage dates. As with the bill introduction date chart, most returns for bill passage dates are clustered around or just below the origin. There are a few large, negative returns that arise from the October 2008 extension that indicate that, while the actual stock price of the RDCredit firm portfolio fell by a whopping double digit margin, the counterfactuals indicate that it should have fallen by even more.
4 Discussion of Possible Explanations for Insignificant CARs Other than Anticipation

4.1 Are My CAR Tests Appropriately Sized? Placebo Tests Say “Yes”.

One hypothesis for why I would find no statistically significant CARs is that the test that I am using for statistical significance is inappropriately sized. However, I verify that my tests have appropriate size using placebo tests.

To run placebo tests, I select 10 random placebo event dates from January 1, 1993 to December 31, 2015 and reestimate CARs using the same method that I use for the actual event dates, for a total of 60 placebo CARs. For these dates, any statistically significant CARs should be just attributable to noise in the estimation. To avoid confounding the effect of an actual event date with a placebo date, I require that the placebo dates be at least 40 trading days away from a bill introduction or passage date.\(^\text{20}\)

Figure 7 shows the results of the placebo tests. Blue circles indicate that the CAR estimate is not statistically greater than zero at the 5% level or lower, whereas red squares indicate that the estimate is significant. Most of the placebo estimates are insignificant and are also centered around zero, which suggests that my tests are appropriately sized.

4.2 Are R&D Tax Credit Extensions Valuable Enough to Cause CARs? High-R&D Firms Also Do Not Have CARs.

A second hypothesis for why I find no effect of R&D tax credit extensions on CARs could be that the R&D tax credits themselves are not useful for increasing the net present value of the firms that receive them. If R&D tax credits, which are a book asset, are not useful\(^\text{20}\)

\(^{20}\)I also find no statistically significant CARs, on average, on relevant committee hearing dates. See Appendix ?? for details.
for increasing net present value, then we would not see CARs.

To investigate this issue, I estimate CARs for the subset of firms that conducted the most R&D, which are the firms that benefited the most from R&D tax credit extensions and should also exhibit the largest CARs from unanticipated extensions. But I continue to find, on average, no statistically significant CARs for this subset of firms, which suggests that the extensions were anticipated.

To look for CARs of high-R&D credit firms, I follow my main event study procedure except that after forming the portfolios of all R&D credit claiming firms, for each bill I calculate the firm-fiscal year ratios of claimed R&D tax credit to book value of assets for the firm fiscal-year that contains each bill’s passage date. Then, for each bill, I use the firms with the highest 10% of this ratio to form the portfolios of high-R&D credit firms. These portfolios of high-R&D credit firms have a very large expected average CAR, over 8%, shown in Figure 8. This expected average CAR is about ten times as much as the expected average CAR for the portfolios of all R&D credit claiming firms.

Using these portfolios of high-R&D credit firms, I find, on average, a lack of statistically significant CARs, as shown for passage dates in Figure 9.

4.3 What About Other Event Dates? Committee Hearings as Event Dates.

Bill introduction and passage dates represent the first and last major legislative actions on a bill. Therefore, these dates are reasonable times when market expectations of new legislation could update, which would cause CARs.

Alternatively, market expectations for new legislation may update on congressional hearing dates. Congressional committees use hearings to gather information, but elected officials also use committee hearings to signal support for or against certain policies, which may cause

\[ \text{This statistic uses the same assumptions as Figure 2: R&D tax credit extensions are valued at the market to book ratio twenty trading days before passage times the book value of the extension for the current fiscal year, while future fiscal years are discounted by 15% per year with proration for partial fiscal years.} \]
markets to update expectations. However, on average I also find no statistically significant CARs on relevant committee hearing dates.

Unlike bill introduction and passage dates, which are clearly tied to a single bill, hearings are often held for general information gathering and not necessarily for a particular bill. Some bills have specific hearings associated with them, but other bills have no specific hearings. Therefore, I identify relevant committee hearing dates, which I take as hearings that discuss extensions of the R&D tax credit, using a two-stage approach.

First, using the “all actions” legislative histories on www.congress.gov, I identify all hearings where a R&D tax credit extension was referred to as “hearing held”. Second, I look for hearings on the topics of energy, the general economic outlook, innovation, and taxes in the following committees: the House Committees on Appropriations, Budget, Science, Space, and Technology (and its predecessors), Ways and Means, and the Senate committees on Appropriations, Budget, and Finance (U.S. Government Publishing Office, 2018).

Using the transcripts of these hearings, I look for oral arguments both for and against R&D tax credit extensions. Using this approach, I identify seven hearings where there were at least three oral arguments with regards to R&D tax credit extensions.22 With the same approach as for bill introduction and passage dates, on average I do not find any statistically significant CARs on these dates.


Yet another potential explanation for why I find, on average, no statistically significant CARs is that I am miscalculating the amount of R&D tax credit each firm can take from Compustat-CRSP and, therefore, I misclassify firms that are able to claim R&D tax credits.

Two issues arise when using Compustat-CRSP to identify firms that claimed R&D tax credit.

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credits: (1) Compustat-CRSP does not record the value of R&D tax credits claimed, so I estimate the amount of R&D tax credit each firm receives based on reported financials, and (2) the reported R&D in Compustat-CRSP may differ from the amount of R&D that firms use to claim R&D tax credits (Rao, 2016).

As a sanity check, I compare the total amount of R&D tax credit that I estimate from Compustat-CRSP against estimates of the total R&D tax credit from the publicly available Statistics of Income (SOI) data (Internal Revenue Service, 2017). The SOI data are based on confidential firm tax records that record actual R&D tax credit claimed.

Figure 10 plots the reported SOI aggregate R&D tax credit claimed, the black line with circles, against my estimates from Compustat-CRSP, the blue line with squares. The two estimates track each other fairly closely (SOI data are not available after 2013). Therefore, I do not believe that mismeasurement of claimed R&D tax credits is a likely cause for finding no statistically significant CARs.

5 Conclusion

Before 2015 when the U.S. R&D tax credit was still statutorily temporary, anecdotal evidence from firms, policymakers, and the media suggested that there was uncertainty over whether the R&D tax credit would be extended and also that the uncertainty over a possible extension was diminishing the credit’s innovation-enhancing ability. For example, in a 2009 official letter to the Director for Tax Issues of Government Accountability Office, the Department of the Treasury’s Acting Assistant Secretary for Tax Policy stated directly that “Uncertainty about the future of the research tax credit diminishes the incentive effect.” (Mundaca, 2009). When this letter was written the R&D tax credit was about to expire but had already been extended multiple times previously. In another example, Malakoff (2014), writing for Science, noted that “impermanence [of the U.S. R&D tax credit] has created uncertainty for companies trying to make long-term investment decisions, critics say, and
potentially chilled spending on innovation.”

Why did non-market participants express public concern over whether the R&D tax credit would be extended when I find that the market anticipated these extensions? There are a few possibilities.

The first possibility is that the market assigned a very high, but not certain, chance that the R&D tax credit was going to be extended and that this expectation was shared by non-market participants. For example, suppose that the market priced in a 99.9% chance of an extension. Statistically, I would not be able to distinguish a 99.9% expected chance of an extension from a 100% expected chance of an extension by measuring CARs. But it also possible that the slightest bit of uncertainty from the 0.1% chance of no extension was enough to cause worry for policymakers, firms, or the media.

A second possibility is that policymakers, firms, or the media have different expectations for extensions than the market. It is possible that the market assigned a 100% chance of an extension, but a firm may have assigned only a 1% chance of an extension. I did not investigate non-market expectations, so I cannot rule out this possibility.

A third possibility is that the reports of concern were red herrings, cheap talk, or political posturing and did not reflect actual expectations of the probability of the credit being extended. An advantage of using market expectations of extensions to measure policy uncertainty is that the market has a strong incentive to form and act on accurate expectations. Non-market participants do not necessarily have these incentives.

In this paper I find that, on average, extensions of the U.S. R&D tax credit were anticipated by the market. I assert that this result implies that, at least for tax policies like the U.S. R&D tax credit that were extended multiple times, that extensions are expected. Therefore, extensions of these policies should not count toward measured economic uncertainty and that these extensions are not fiscal shocks. However, I believe that there are three main caveats to these assertions.

First, I only investigated extensions of the U.S. R&D tax credit, which is an uncontrover-
sial tax credit that has broad support from both major U.S. political parties. An analysis of extensions of other statutorily temporary policies may yield different conclusions about expectations.

Second, I measured expectations of tax policy extension as the market’s expectations due to the availability of daily stock price data and the fact that the market has a strong incentive to act on accurate expectations. But the market’s expectations may have differed from, say, firms or consumers. If there were a divergence between the market’s expectations and others, then it is possible that the statutorily temporary R&D tax credit had effects that resembled a temporary tax policy despite the market’s expectations of extensions, which would imply that the extensions could have been fiscal shocks.

Third, my finding that extensions of this credit were anticipated by the market is based on measuring CARs on major legislative event days. If the market’s expectations of an extension updated at other times, then I would not capture those CARs.

References


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23 This statement is based on floor debates from both chambers of congress. Members from both parties support the tax credit as a tool to create jobs and promote innovation. The opposition to particular extensions of the R&D tax credit, but not necessarily the concept of the R&D tax credit, was centered on either the effect of the credit on the federal budget deficit or because the proposed tax credit extensions were too short. For example, on budget deficit concerns, on December 3, 2014 Congressman Hoyer (D-MD) said “We ought to make the research and experimentation tax credit permanent, but we need to pay for it.” (Hoyer, 2014). Similarly, on December 18, 2015 Senator Sessions (R-AL) said “This [bill] is a step away not towards fiscal responsibility.” (Sessions, 2015). For concerns that the proposed extensions were too short, Senator Wyden (D-OR), on December 16, 2014, said “I hope that Senators will say, however they vote tonight, that the real lesson out of this is when you have an opportunity to provide certainty and predictability for the American economy, take it. Do not walk away from it. Unfortunately, because this bill is only 2 weeks long, that is what we are doing.” (Wyden, 2014).

24 See Kari, Karikallio, and Pirttilä (2008) for an example of how firms respond to future tax changes.


Clopper, Charles J., and Egon S. Pearson, “The Use of Confidence or Fiducial Limits Illustrated in the Case of the Binomial,” *Biometrika* 26:4 (1934), 404-413.


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A Appendix: R&D Credit Calculation and Background

This section gives a brief overview of the U.S. R&D tax credit and discusses how I use
Compustat-CRSP to calculate R&D tax credits by firm. For additional details on the credit, see Guenther (2015); Chang (2018).

For calculating R&D tax credits, I use 50% of Compustat R&D (xrdq) as qualified research expenditures (QREs), which are research expenditures that qualify for the R&D tax credit, and revenue reported (revtq) as gross receipts. The Compustat-CRSP definition of R&D is more broad than what qualifies as QREs under the Internal Revenue Code, so I take the proportion of Compustat-CRSP R&D that are QREs as 50% from Wilson (2009); Chang (2018). I also treat the estimated QREs as in-house, non-basic research following Wilson (2009); Chang (2018) and drop observations missing a fiscal year identifier (fyearq).

Since its inception in 1980, effective in 1981, the U.S. R&D tax credit has been a credit for QREs over a base amount and, until 2015, was a statutorily temporary part of the tax code (see Figure 1). The credit has been extended and modified over its life and was allowed to lapse without a retroactive renewal once, applicable to QREs paid or incurred after June 30, 1995 and before July 1, 1996.

I focus my analysis on the extensions of the R&D tax credit after 1996 for two reasons: (1) the R&D tax credit was allowed to lapse once, for QREs paid or incurred after June 30, 1995 and before July 1, 1996. The willingness for congress to let the R&D tax credit lapse could have been taken as an indicator that the probability of R&D tax credit renewal in the future was lower, so renewals after this lapse could have been more likely to generate CARs, and (2) while the R&D tax credit has been changed slightly in addition to being extended repeatedly after 1996, before 1996 the credit underwent significant changes in addition to being extended, especially from PL 99-514 and PL 101-239, such that the bills that extended the credit before 1996 would be hard to characterize as routine extensions.

The calculation of the R&D tax credit’s base amount has changed over time, but the formulation was intended to represent the counterfactual amount of research the firm would do in the absence of the credit so that the credit would only apply to additional research caused by the credit. Public Law 97-34, effective in 1981, created the original R&D tax credit
for 25% of QREs over a three-year moving average of QREs (the base amount). Public Law 99-514 (also known as TRA86) reduced the credit rate to 20% but kept the three-year moving average base amount. Unfortunately, the three-year moving average base amount resulted in small and, often, negative marginal credit rates for R&D firms (Hall, 1993; Chang, 2018).

In 1989, PL 101-239 changed the base amount and provided for separate base amount calculations for startup firms and non-startup (established) firms. The law effectively eliminated the potential for negative marginal rates.

PL 101-239 classified a firm as a startup if the firm had fewer than three taxable years beginning after December 31, 1983 and before January 1, 1989 in which the firm had both gross receipts and QREs. By 1997 a firm was classified as a startup if either: (1) the first taxable year that the firm had both gross receipts and QREs in began after December 31st, 1983, or (2) the firm had fewer than three taxable years with gross receipts and QREs that began after December 31st, 1983 and before January 1st, 1989. The base amount for startups is a fixed-base percentage times the average annual gross receipts of the startup firm for the previous four taxable years. If the firm did not have gross receipts for the previous four taxable years, then the base amount is the fixed-base percentage times the average gross receipts for the years that it has been in existence.  

The fixed-base percentage varies for a startup firm’s first ten taxable years. The fixed-base percentage starts at 3 percent for the firm’s first five taxable years, and changes incrementally to eventually reaching, in the firm’s 11th taxable year, the aggregate of the firm’s QRE to gross receipts ratio for five of the six taxable years starting from the firm’s fifth taxable year to the firm’s tenth taxable year. The startup firm can choose which five of six taxable years to use as a fixed-base percentage, so I assume that startup firms choose the five years with the smallest QRE to gross receipts ratios, which would maximize their R&D credit.

For calculating startup values, I assume that firm age is equivalent to the difference between when the firm first appears in Compustat-CRSP and the current year. Because

\[^{25}\text{CFR § 1.41-3 (a).}\]
\[^{26}\text{26 U.S.C. § 41 (c) (3) (B).}\]
Compustat-CRSP only covers publicly traded firms, I do not observe firms that were in existence before being publicly traded. Therefore, firms could have been filing taxes as non-public firms and I would be underestimating their age.

For established firms, which are all non-startup firms, the credit after 1990 is 20 percent for QREs over an established firm base amount. The base amount for established firms is the product of the established firm fixed-base percentage and the average annual gross receipts for the 4 taxable years before the taxable year that the firm is trying to claim the credit for. The fixed-base percentage, for established firms, is the ratio of QREs to gross receipts for the firm’s fiscal years that begin after December 31st, 1983 and before January 1st, 1989.\textsuperscript{27}

In computing the fixed-base percentage for the base amount, for both startup and established firms, the fixed-base percentage is rounded to the 1/100th of 1 percent\textsuperscript{28} with a maximum of 16 percent\textsuperscript{29} and a minimum of 50% of QREs.\textsuperscript{30} I annualize taxable years of fewer than 12 months, which I take as two annual reports from Compustat-CRSP that appear less than one calendar year apart.\textsuperscript{31,32} I require firms have a non-negative base amount. Negative base amounts can occur because, in some years, gross receipts are negative.

In addition to the regular R&D tax credit, formerly under 26 U.S.C. § 41 (c) (4) firms could instead claim the Alternative Incremental Research Credit (AIRC). The default action is to claim the regular credit, but should a firm choose the AIRC, then the AIRC applied to all taxable years after the first election to use the AIRC, unless the firm received approval from the Secretary of the Treasury.\textsuperscript{33} The AIRC corresponded to credit tiers for QREs as a percentage of gross receipts for the firm’s previous four taxable years and did not distinguish between startup and established firms. For fiscal years that begin after June 30, 1996 the AIRC was a three-tier credit of 1.65/2.2/2.75 percent of QREs for QREs between

\begin{itemize}
  \item \textsuperscript{27}26 U.S.C. § 41 (c) (3) (A).
  \item \textsuperscript{28}26 U.S.C. § 41 (c) (3) (D).
  \item \textsuperscript{29}26 U.S.C. § 41 (c) (3) (C).
  \item \textsuperscript{30}26 U.S.C. § 41 (c) (2)
  \item \textsuperscript{31}26 U.S.C. § 41 (f) (4).
  \item \textsuperscript{32}CFR § 1.41-3 (d).
  \item \textsuperscript{33}26 U.S.C. § 41 (c) (4) (B).
\end{itemize}
percent of the ratio of the firm’s QREs to average gross receipts for the previous four years. The AIRC credit rates increased to 2.65/3.2/3.75 by PL 106-170, Section 502, for amounts paid or incurred after June 30th, 1999\textsuperscript{34}, which I take as firm fiscal years that begin in July 1999 or later. The AIRC credit rates increased again to 3/4/5 percent by PL 109-432, Section 104 (a) (2) for taxable years ending after December 31, 2006.

For calculating whether a firm took the regular credit or the AIRC, I assume that if a firm’s AIRC in a given fiscal year would be $1 million more than the regular credit, then the firm switches to AIRC. I assume that waivers from the Secretary of the Treasury are not granted, so an AIRC election is binding.

PL 109-432 also introduced the Alternative Simplified Credit (ASC). Starting for taxable years that included January 1, 2007, firms that had not yet elected the AIRC could then either elect the AIRC or the ASC. Claiming the ASC, like claiming the AIRC, bound the firm to claiming the ASC for all subsequent taxable years, unless the firm obtained a waiver from the Secretary of the Treasury. For taxable years ending before January 1, 2009, the ASC was 12 percent of QREs over a three-year moving average of the firm’s QREs, or 6 percent if the firm had no QREs in any of the previous three taxable years.

During the transitional period of 2007 to 2009, firms could either be on the regular credit, the AIRC, or the ASC. And in the transitional period for fiscal years that included January 1, 2007, firms that had previously opted into the AIRC could switch to the ASC without consent from the Secretary of the Treasury.\textsuperscript{35} During this transitional period, I classify firms that were on the AIRC as switching to the ASC if the ASC would have given the firm more than $1 million in credit compared to the regular credit. Otherwise I leave the firm as on the AIRC. If the firm is on the regular credit, then I switch the firm to the ASC if the ASC gave the firm more than $1 million in extra credit. Therefore, for firm fiscal years that encompassed January 1, 2007, firms could be eligible for a combination of the regular credit and ASC, or the AIRC and ASC. Credits were prorated based on the number of days that

\textsuperscript{34}PL 106-170, Section 502 (c) (3).
\textsuperscript{35}PL 109-432 Section 104 (c) (2).
the firm fiscal years were pre-January 1, 2007 vs. post-January 1, 2007.

PL 110-343 Section 301 (1) (b) terminated the AIRC, restricting its election to taxable years that begin on or before December 31, 2008, and (1) (c) of the same law increased the ASC base rate from 12 to 14 percent for taxable years that end on or after January 1, 2009.

I calculate R&D tax credits using realized Compustat-CRSP data. As firm financial data are only available with a lag, this process assumes that market participants correctly anticipated, in expectation, how much each firm benefited from the extensions. As a robustness check, I also use four separate forecasting schemes, using real-time data, to forecast each firm’s benefit from each R&D tax credit extension and to classify each firm-fiscal year into:

1. firm-fiscal years that did R&D and received a R&D tax credit (RDCredit),
2. firm-fiscal years that did R&D but did not receive a R&D tax credit (RDnoCredit),
3. firm-fiscal years that did no R&D and, thus, did not receive a R&D credit (noRDnoCredit).

These four classification schemes assume that the calculation of R&D tax credit is what would have been observed in the most recently observed fiscal year (i.e., regular, AIRC, or ASC) and give similar results to using realized financials.

First, I assign a firm’s classification to be the same as the firm’s previous fiscal year, for which market participants had the firm’s financial data available.

Second, I use forecasts of QREs and gross receipts using moving averages of the annual Compustat files. For a firm’s fourth fiscal-year and later, I take a 3-year moving average. For a firm’s third fiscal year, I take a 2-year moving average. For a firm’s second fiscal year, I take the previous fiscal year’s values. For a firm’s first fiscal year, I linearly extrapolate annual QREs and gross receipts using the available real-time quarterly Compustat observations (i.e., a firm with one quarterly report would use that quarter’s data multiplied by four, a firm with two quarterly reports would use the sum of those reports times two, etc.), and treat missing quarterly values as zero.

Third, I extrapolate a firm’s QREs and gross receipts using industry-level growth forecasts at the Standard Industrial Classification (SIC) 3 digit level (279 total industries). I use a
quarterly industry-level iterative forecast with an AR(12) model, seasonally adjusted with quarterly dummies, separately for growth in QREs and growth in gross receipts and require a minimum of 14 observations. For the first three years and one quarter, I replace the first 3 years and 1 quarter by SIC industry with moving averages, up to a three quarter moving average (2 quarters for fourth quarter, 1 quarter for third quarter). I then apply forecasted industry-level growth rates to firm-specific realized amounts, where the realized amount is the latest real-time quarterly report available. For firms with fiscal years that do not end on calendar quarter end dates, I assign a firm’s fiscal quarter to the calendar quarter during which the firm’s fiscal quarter ends. For firms without quarterly reports, I use an AR(3) on annual growth rates of QREs and gross receipts. The forecasts, for firms without quarterly reports, is the annual model’s forecast times the observed firm-specific realized amount from previous fiscal year. I restrict implied QREs to be non-negative.

Fourth, I use a firm-specific AR(3) regression on annual data separately for QREs and gross receipts, with quarterly data for the current fiscal year if those data were available in real time. I use my industry-level growth forecasts to inform firm-level values in the first three years that the firm is in Compustat.
Figure 1: R&D Tax Credit Extensions Timeline

This figure plots estimates of the effect of R&D tax credit extensions on the portfolio of firms that claimed the R&D tax credit by bill passage year, assuming that R&D tax credit extensions are valued at the market to book ratio twenty trading days before passage times the book value of the extension for the current fiscal year, while future fiscal years are discounted by 15% per year with proration for partial fiscal years. The mean effect across all extensions is eighty one basis points. Calculations using legislative text and Compustat-CRSP (U.S. Government Publishing Office, 2015; Center for Research in Security Prices, 2016).
This figure plots the cumulative abnormal returns (CARs) on introduction dates, normalized by the expected increase in market equity under an unanticipated extension, by year. Blue circles indicate that the CAR estimate is not statistically greater than zero at the 5% level or lower, whereas red squares indicate that the estimate is significant. My results imply that, on average, there is no effect of R&D tax credit extensions on CARs on bill introduction dates.
Figure 4: On Average There Are No Statistically Significant Cumulative Abnormal Returns on Bill Passage Dates

This figure plots the cumulative abnormal returns (CARs) for passage dates, normalized by the expected increase in market equity under an unanticipated extension, by year. Blue circles indicate that the CAR estimate is not statistically greater than zero at the 5% level or lower, whereas red squares indicate that the estimate is significant. The extension passed in 2008 generated statistically significant increases in CARs, though outside of 2008 there is little evidence that the extensions caused CARs on bill passage dates.
This figure plots the cumulative returns of the portfolio of R&D credit claiming firms, pooled across bill introduction dates, against the cumulative return of the counterfactual portfolio plus the expected increase in market equity of the R&D credit claiming portfolio under an unanticipated extension. Blue circles indicate that the CAR estimate is not statistically greater than zero at the 5% level or lower, whereas red squares indicate that the estimate is significant.
This figure plots the cumulative returns of the portfolio of R&D credit claiming firms, pooled across bill passage dates, against the cumulative return of the counterfactual portfolio plus the expected increase in market equity of the R&D credit claiming portfolio under an unanticipated extension. Blue circles indicate that the CAR estimate is not statistically greater than zero at the 5% level or lower, whereas red squares indicate that the estimate is significant.
This figure plots the cumulative abnormal returns (CARs) for 10 placebo dates. Blue circles indicate that the CAR estimate is not statistically greater than zero at the 5% level or lower, whereas red squares indicate that the estimate is significant. The placebo tests indicate that the tests are appropriately sized.
Figure 8: Expected Effect of R&D Tax Credit Extensions on Market Equity for High-R&D Credit Claiming Firms is Eight Percentage Points

This figure plots estimates of the effect of R&D tax credit extensions on the portfolio of high-R&D credit claiming firms by bill passage date, where high R&D credit claiming is the top decile of R&D credit claimed in the fiscal year of bill passage to book value of assets in the fiscal year prior to passage. This figure assumes that R&D tax credit extensions are valued at the market to book ratio times the book value of the extension for the current year, while future years are discounted by 15% per year. The mean effect across all extensions is about eight percentage points. Calculations using legislative text and Compustat-CRSP (U.S. Government Publishing Office, 2015; Center for Research in Security Prices, 2016).
Figure 9: On Average There Are No Statistically Significant Cumulative Abnormal Returns on For High-R&D Firms On Bill Passage Dates

This figure plots the cumulative abnormal returns (CARs) for passage dates for the portfolio of high-R&D firms, normalized by the expected increase in market equity under an unanticipated extension, by year. Blue circles indicate that the CAR estimate is not statistically greater than zero at the 5% level or lower, whereas red squares indicate that the estimate is significant. The second extension passed in 1998 generated statistically significant increases in CARs, though otherwise there is little evidence that the extensions caused CARs on bill passage dates.
This figure plots my estimated amount of R&D tax credit claimed from Compustat-CRSP (Center for Research in Security Prices, 2016) for firm-fiscal years that include each passage date using the model in Appendix A, the blue line with squares, and the publicly available Statistics of Income estimates for total R&D tax credit claimed, the black line with circles (Internal Revenue Service, 2017).