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

Andrew C. Chang

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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

1 Introduction

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

Governments often design fiscal policies as 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 the 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 temporary tax policies are routinely extended, a necessary condition for determining their economic significance is estimating whether the policies were *expected* to be extended. Expectations matter. A statutorily temporary policy that everyone expects will be extended has economic effects equivalent to a permanent policy. In this case, a statutory extension of the temporary policy is not a fiscal shock. Conversely, only a temporary policy that is not expected to be extended has the economic effects of a temporary policy. And a statutory extension, which would be unexpected, is a fiscal shock.

I measure expectations of tax policy extensions using event studies and cumulative abnormal returns (CARs) for firms that claimed the U.S. research and development (R&D)

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

[2]Public Law 107-16, the Economic Growth and Tax Relief Reconciliation Act of 2001.

[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.

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.[5] 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.

The second literature is on the effects of uncertainty, or lack of uncertainty, on real

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[5]For example: Jurado, Ludvigson, and Ng (2015); Baker, Bloom, and Davis (2015, 2016); Husted, Rogers, and Sun (2016).

activity.[6] 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,[7] 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).[8]

The fourth literature is on predicting the effects of fiscal stimuli ***before the stimuli are in effect***.[9] 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 Council of Economic Advisers, and the Government Accountability Office. And

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[6]For example: Garfinkel and Glazer (1994) on the effect of elections on the timing of wage contracts, Bloom, Bond, and van Reenen (2007) on the relationship between uncertainty, investment, and demand shocks, Baker, Bloom, and Davis (2016); Gulen and Ion (2016) on how real activity correlates with the Baker, Bloom, and Davis (2015, 2016) economic policy uncertainty index, Kaufman (2011)'s case study on the effects of uncertainty from the US estate and gift tax, Pierce and Schott (2016) for the effects of granting China permanent favored tariff status, and Julio and Yook (2012); Canes-Wrone and Ponce de Leon (2014); Jens (2017) on the effects of electoral uncertainty on real activity. For a review see Bloom (2014).

[7]At 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).

[8]One 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 Pottelsberghe, 2003; Wilson, 2009; Chang, Forthcoming; Guceri and Liu, 2017). 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 (Forthcoming).

[9]For 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.

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.[10]

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 daily stock prices (Center for Research in Security Prices, 2016).[11]

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).[12]

I first restrict the sample to ordinary shares (Compustat-CRSP share codes 10 and 11) and firms with positive book and market equity.[13] I drop the Electric, Gas, and Sanitary

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[10]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.

[11]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.

[12]For additional details on the R&D tax credit, see Guenther (2015).

[13]I define book and market equity following Fama and French (1993). Book equity is the book value of

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 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).[14]

To calculate CARs, I take an agnostic view of what the "correct" specification is and instead average the statistical significance of CARs across many different specifications. The standard approach in economics and finance research, including many event studies that calculate CARs, is to have a "main specification" with a series of "robustness checks" where researchers are running single hypothesis tests on each specification.[15] But I instead average statistical significance of CARs across many specifications because I do not carry strong beliefs on what a "correct" specification is for determining CARs and also because I want to account formally for multiple hypothesis testing.[16]

Accordingly, for each event date I calculate fifteen different counterfactual cumulative returns to produce fifteen estimates of CARs per event date. For each CAR, I test whether the CAR is statistically significant or not.

I then conduct two multiple hypothesis tests that pool the individual statistical significance tests across specifications: (1) one test across all introduction date CARs and (2) a second test across all passage date CARs. I use Clopper and Pearson (1934) confidence intervals and take the individual statistical significance tests as inputs. The Clopper and Pearson (1934) confidence intervals assume that the outcome of whether individual CARs

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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).

[14]See the calculations in Appendix A for details.

[15]For example, Auerbach and Hassett (2005); Amromin, Harrison, and Sharpe (2008); Ray and Warusawitharana (2009); Kim (2015).

[16]Bayesians can think of this approach as a model-averaging method with an equal weighted prior, as in Morley and Piger (2012); Chang and Hanson (2016).

are statistically significant represent draws from an i.i.d. binomial. Overall this process tests whether, on average, a statistically significant CAR exists across the R&D tax credit introduction dates or passage dates.

For the first set of counterfactual returns, I estimate the market model in equation (1):

$$creturn_t^{RDCredit} = \alpha + \beta creturn_t^{Market} + \varepsilon_t \tag{1}$$

The dependent variable, $creturn_t^{RDCredit}$, is 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,[17] and $creturn_t^{Market}$ is the analogous return for the entire market covered by Compustat-CRSP. Letting $t = 0$ denote an event date, I use an estimation window of $t = [-270, -21]$ trading days. I then calculate the cumulative abnormal returns as the difference between the predictions of equation (1) and observed $creturn_t^{RDCredit}$ for three event windows: $t = [-20, 20]$, $t = [-5, 5]$, and $t = [-3, 3]$.[18] I require firms remain in the sample and be in the same bin from the beginning of the estimation window to the end of the largest event window, $t = [-270, 20]$.

For the second set of counterfactual returns, I estimate the market model as in equation (1) but instead use the cumulative return for the RDnoCredit firms on the right-hand side:

$$creturn_t^{RDCredit} = \alpha + \beta creturn_t^{RDnoCredit} + \varepsilon_t \tag{2}$$

For the third set of counterfactual returns, I estimate Fama and French (1992) three factor models using $creturn_t^{Market}$ and the same settings as the market model.

I test for individual statistical significance of the CARs from the market model and the Fama and French (1992) three factor model using the Kolari and Pynnönen (2010) modified Patell-statistics to account for cross-sectional correlation of returns.[19]

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[17]See MacKinlay (1997); Perttunen (2015).

[18]Because the last passage date is on December 18, 2015 as I have data through the end of 2015, for this passage date the [-20, 20] event window is truncated to the end of 2015.

[19]I implement the tests in Stata 14.2 SE (Linux 64-bit) with programs by Kaspereit (2015).

For the fourth set of counterfactual cumulative returns, I assume that the correlation between $creturn_t^{RDCredit}$ and $creturn_t^{RDnoCredit}$ in the absence of an event is one ($\beta = 1$). This assumption is equivalent assuming that the portfolio for RDnoCredit firms provides a good counterfactual cumulative return for the portfolio of RDCredit firms, as in a difference-in-differences setup where the counterfactual is $creturn_t^{RDnoCredit}$.

The counterfactual of assuming $\beta = 1$ is motivated by the fact that estimating a model for β 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 β always comes at the expense of higher standard errors.[20] Assuming $\beta = 1$ might be biased but it also has the large benefit of having zero standard error.[21]

Under the $\beta = 1$ counterfactual, I calculate the CARs as the difference between $creturn_t^{RDCredit}$ and $creturn_t^{RDnoCredit}$ for the same three event windows as the market and Fama and French (1992) models: $t = [-20, \ 20]$, $t = [-5, \ 5]$, and $t = [-3, \ 3]$. I use a t-test for statistical significance by taking the sample standard deviation of the difference between $creturn_t^{RDCredit}$ and $creturn_t^{RDnoCredit}$ over $t = [-270, \ -21]$.

For the fifth set of counterfactual cumulative returns, I assume that $\beta = 1$ between $creturn_t^{RDCredit}$ and $creturn_t^{noRDnoCredit}$, but otherwise the setup is the same as for $\beta = 1$ for $creturn_t^{RDCredit}$ and $creturn_t^{RDnoCredit}$.

To summarize, I start by calculating CARs using a total of five methods: the market model with the entire market on the right-hand side, the market model with only RDnoCredit firms on the right-hand side, Fama and French (1992) using the entire market, $\beta = 1$ between RDCredit and RDnoCredit firms, and $\beta = 1$ between RDCredit and noRDnoCredit firms. I then use these five models each over three event windows: $t = [-20, \ 20]$, $t = [-5, \ 5]$, and $t = [-3, \ 3]$ for ten extensions of the R&D tax credit, shown in Figure 1. And finally

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[20]Ahern (2009) also finds that generating abnormal returns by subtracting the raw return of 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.

[21]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.

I run two multiple hypothesis tests with Clopper and Pearson (1934) confidence intervals: one across all introduction date CARs and one across all passage date CARs.

3 Cumulative Abnormal Return Results

Figure 2 displays a bar chart for the percent of statistically significant CARs by bill introduction date, where statistical significance is at the 10% level. Figure 2 shows that, for most bill introduction dates, none of the methods of calculating CARs yield a statistically significant effect of the R&D tax credit extensions on CARs.

Because Figure 2 takes statistical significance at the 10% level, then we should expect that, in the absence of an effect of R&D tax credit extensions on CARs, that 10% of the test results should be significant by pure chance. But the number of statistically significant CARs is well below this 10% threshold. Therefore, I do not find evidence that, on the dates that R&D tax credit extensions are introduced into committee, that the portfolio of firms that would benefit from the extensions experience abnormal increases in their stock prices.

Figure 3 displays an analogous bar chart for the percentage of statistically significant CARs by bill passage date. As statistical significance is still at the 10% level, we should still expect that 10% of test results should be significant by pure chance. On passage dates I find that 14% of estimated CARs are statistically significant, with most of the statistically significant CARs coming from the R&D tax credit extension signed during the 2008 great recession. However, this 14% of statistically significant CARs is not statistically different from the 10% of significant results that we should expect by pure chance. The 90% confidence interval around the 14% estimate, calculated using a Clopper and Pearson (1934) confidence interval, is from [9.6%, 19.5%].[22] Therefore, on the dates that R&D tax credit extensions were signed by the U.S. president into law, I do not find evidence that the stock prices of firms that benefited from the extensions increased because of the extensions, which suggests

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[22]A more stringent confidence interval, such as 95% interval, would be even wider and still encompass the 10% mark.

that the market, on average, anticipated the extensions becoming law.[23]

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, which is an average across several models, is inappropriately sized. However, I verify that my tests have appropriate size using placebo tests.

To run placebo tests, I select 20 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 300 estimated placebo CARs.[24] 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 event window around the placebo dates do not overlap with the event windows around either a bill introduction or passage date. Therefore, the placebo dates are at least 40 trading days away from a bill introduction or passage date. I find that, for the placebo tests, 8% of dates are statistically significant at the 10% level, with a 90% confidence interval of [5.3%, 10.7%], suggesting that the CAR tests are appropriately sized.[25,26]

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[23]Excluding the two events that passed both chambers of congress with 2/3 majority, events 3 and 9, still yields no statistically significant CARs on average.

[24]Twenty placebo events times five models times three event window sizes equals 300 placebo CARs.

[25]Similarly, I find that 4% of placebo dates are significant at the 5% level, with a 95% confidence interval of [2.1%, 6.9%].

[26]I also find no statistically significant CARs, on average, on relevant committee hearing dates. See Appendix B for details.

4.2 Are R&D Tax Credit Extensions Valuable Enough to Cause CARs? Yes, Particularly for High-R&D Firms.

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 for increasing net present value, then we would not see CARs. I take two approaches to investigate this potential issue.

First, I estimate the expected changes to market equity for R&D performing firms from R&D tax credit extensions, assuming that extensions were unanticipated. I show that extensions should cause non-trivial increases in the CARs for R&D performing firms.[27]

Second, 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.[28]

4.3 Am I Measuring R&D Tax Credits in Compustat-CRSP Accurately? My Calculations Match Aggregate Tax Data.

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 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

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[27]See Appendix C for details.
[28]See Appendix D for details.

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 5 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 uncontroversial tax credit that has broad support from both major U.S. political parties.[29] An analysis

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[29]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.

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.[30]

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.

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For example, on budget deficit concerns, on December 3, 2014 Congressman Hoyer 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 said "This [bill] is a step away not towards fiscal responsibility." (Sessions, 2015). For concerns that the proposed extensions were too short, Senator Wyden, 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).

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

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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 (Forthcoming).

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

(Forthcoming). I also treat the estimated QREs as in-house, non-basic research following Wilson (2009); Chang (Forthcoming) 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, Forthcoming).

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.[31]

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.[32] 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 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

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[31]CFR § 1.41-3 (a).

[32]26 U.S.C. § 41 (c) (3) (B).

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.[33]

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[34] with a maximum of 16 percent[35] and a minimum of 50% of QREs.[36] 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.[37,38] 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.[39] 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 [1,1.5)/[1.5,2)/2+ 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[40], 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.

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[33]26 U.S.C. § 41 (c) (3) (A).
[34]26 U.S.C. § 41 (c) (3) (D).
[35]26 U.S.C. § 41 (c) (3) (C).
[36]26 U.S.C. § 41 (c) (2)
[37]26 U.S.C. § 41 (f) (4).
[38]CFR § 1.41-3 (d).
[39]26 U.S.C. § 41 (c) (4) (B).
[40]PL 106-170, Section 502 (c) (3).

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.[41] 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 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

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[41]PL 109-432 Section 104 (c) (2).

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 a 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.

B   Appendix: Committee Hearings as Event Dates

The main results of the paper use bill introduction and passage dates as event dates for computing CARs. The 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 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.[42] With the same approach as for bill introduction and passage dates, on average I do not find any statistically significant CARs on these dates.

C   Appendix: Expected Size of CARs Due to R&D Tax Credit Extensions

Because extensions of R&D tax credits are additions to firm book equity, assessing the expected effect of the extensions on firm market equity requires some assumptions. Figure 4 plots the expected size of the CARs due to the extensions under three sets of assumptions.

The middle-sized estimates of the expected CARs in Figure 4, the middle blue line with square markers, assume that the market values the unexpected book value asset proportional to the market to book ratio, which treats the R&D tax credit as increasing the net present value of the firm as if it were an average book asset.[43] For multi-year extensions, the middle-sized estimates assume that the current year's R&D tax credit is valued at the market to book ratio, while future years are heavily discounted by 30% per year. Under this set of

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[42]These dates are: March 17, 2007 in the House Committee on Science, Space, and Technology, June 23, 1999, September 26, 2000, June 2, 2011, April 26, 2012, and July 19, 2012 in the House Ways and Means Committee, and March 17, 2015 in the Senate Finance Committee.

[43]For each extension I compute the market to book ratios twenty trading days before passage as to not confound the effect of the extensions on market equity.

assumptions, the middle-sized estimates are around seventy basis points for each extension.[44]

You may think that the middle-sized estimates make assumptions that overstate the expected CARs. The bottom black line in Figure 4 plots a much more conservative series of lower-end estimates of CARs. The lower-end estimates assume that the market values R&D tax credit extensions as dollar-for-dollar additions to market equity and, furthermore, that the market only cares that the current year R&D tax credit was extended so that, for multi-year extensions, future years are not valued at all. Therefore, the lower-end estimates both treat unexpected R&D tax credit extensions as helicopter money that firms stuff into mattresses and proceed to forget about, instead of using R&D tax credits to invest and grow themselves as with their other book assets, and also ignore potential for future helicopter money. The lower-end estimates average about thirteen basis points.

You may instead think that the middle-sized estimates are too conservative. The top black line in Figure 4 plots expected CARs under the assumption that the market values the tax credit by 50% more than the average book value asset and that future years discounted by 15% per year, instead of the 30% per year in the middle-sized estimates. The higher-end estimates average about one hundred and twenty basis points.

D Appendix: Estimated CARs for High-R&D Credit Firms

The main results of this paper use the portfolios of firms that claimed the R&D credit as the portfolios that should have experienced CARs if the R&D credit extensions were unanticipated. These portfolios use the returns of all firms that claimed the R&D credit, regardless of how much R&D credit a particular firm claimed.

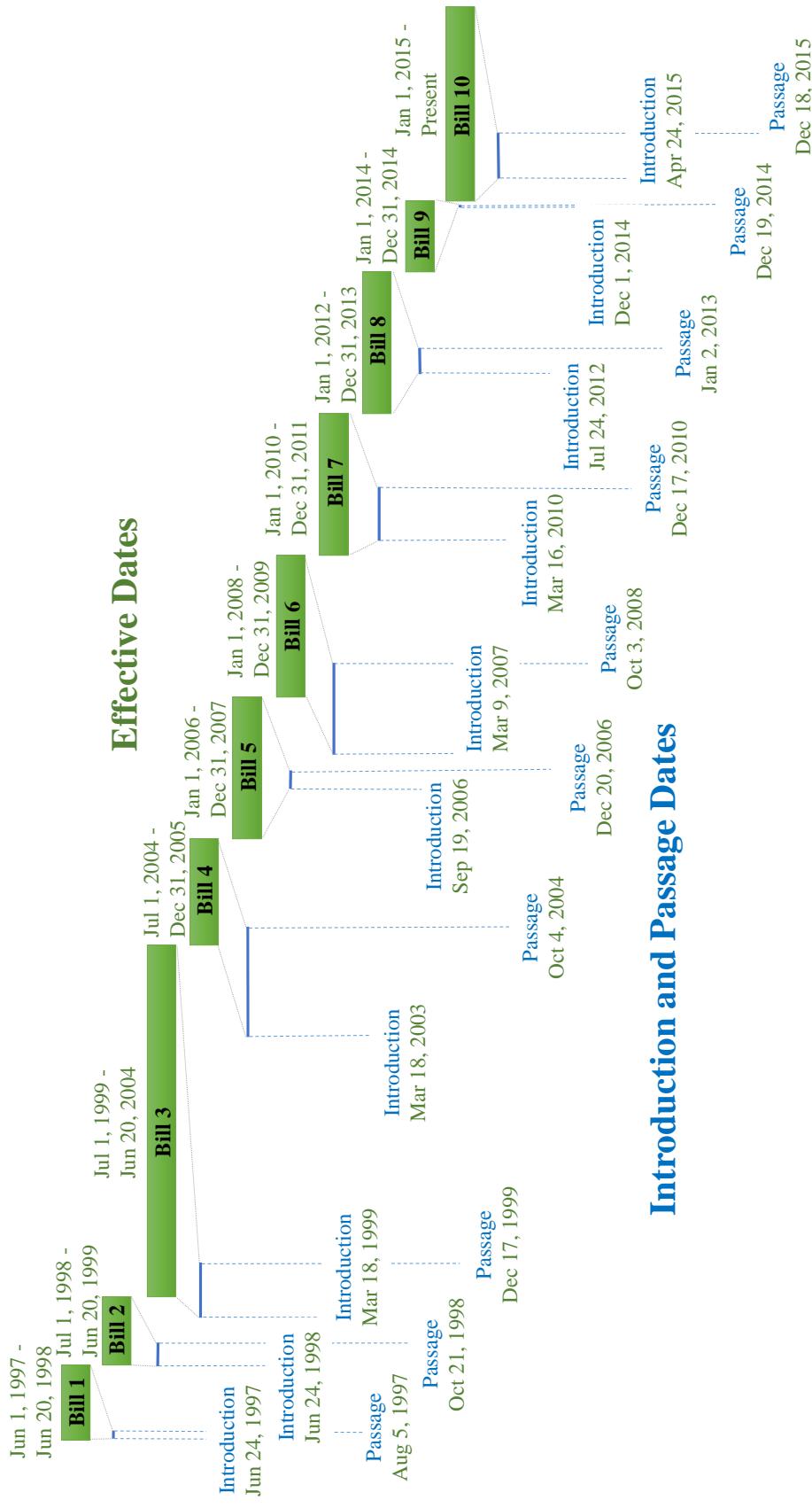I also estimate CARs using portfolios of high-R&D credit claiming firms, which should be

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[44]The third extension, which was passed in 1999, is worth more because this extension was for four years instead of the one to two years that the remaining extensions cover. See Figure 1.

portfolios that would experience a particularly large CAR upon an unanticipated extension of the R&D credit. However, for these portfolios of high-R&D credit firms I still find limited evidence of CARs, suggesting that the market anticipated the R&D tax credit extensions to become law.

To look for CARs of high-R&D credit firms, I follow my main event study procedure, described in section 2, except that after forming the portfolios of all R&D credit claiming firms, for each bill I calculate the firm-level ratios of claimed R&D tax credit to book value of assets for each firm in the R&D credit portfolios 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, 7.9%, under the assumptions 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 30% per year (the middle assumptions in Appendix C). 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 under the same assumptions.
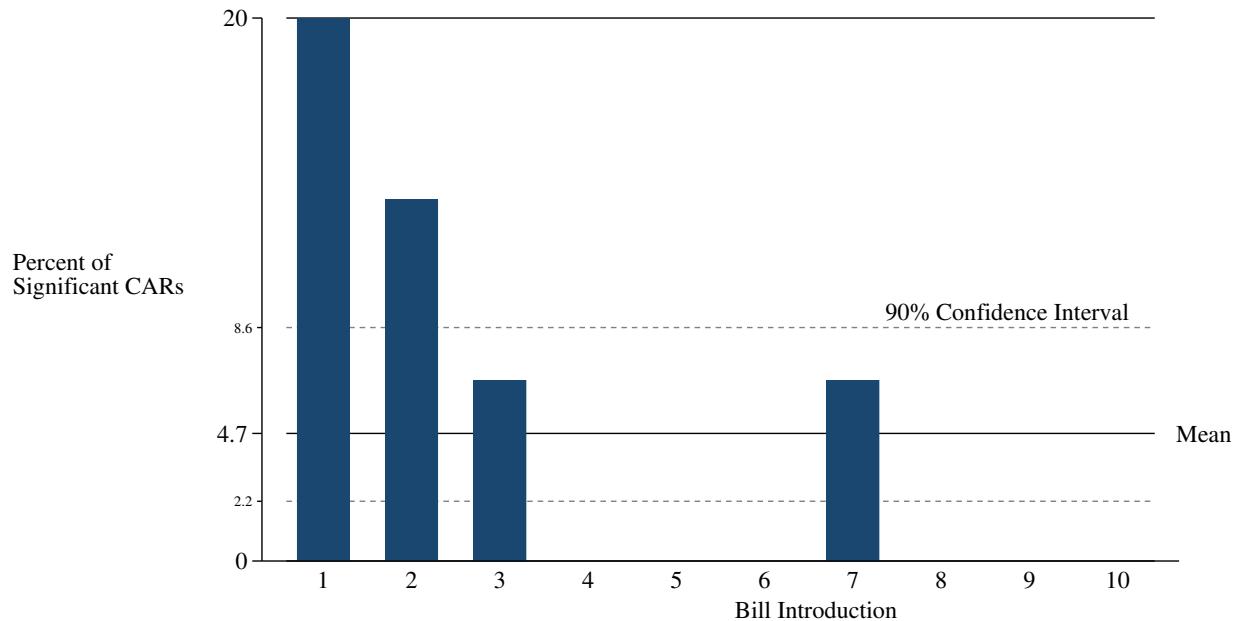
Using these portfolios of high-R&D credit firms, I find, on average, a lack of statistically significant CARs, just as I find when estimating CARs for the portfolios of all R&D credit claiming firms. On bill introduction dates, with the high-R&D credit firm portfolios I find 4% of my models generate a statistically significant CAR at the 10% level, vs. 4.7% with the portfolios of all R&D credit firms. On bill passage dates, the high-R&D credit firm portfolios yield 16% of models that generate a statistically significant CAR at the 10% level, vs. 14% with the portfolios of all R&D credit firms.

Figure 1: R&D Tax Credit Extensions Timeline

Effective Dates

Bill 1: Jun 1, 1997 - Jun 20, 1998
Bill 2: Jul 1, 1998 - Jun 20, 1999
Bill 3: Jul 1, 1999 - Jun 20, 2004
Bill 4: Jul 1, 2004 - Dec 31, 2005
Bill 5: Jan 1, 2006 - Dec 31, 2007
Bill 6: Jan 1, 2008 - Dec 31, 2009
Bill 7: Jan 1, 2010 - Dec 31, 2011
Bill 8: Jan 1, 2012 - Dec 31, 2013
Bill 9: Jan 1, 2014 - Dec 31, 2014
Bill 10: Jan 1, 2015 - Present

Introduction and Passage Dates

Bill 1: Introduction Jun 24, 1997; Passage Aug 5, 1997
Bill 2: Introduction Jun 24, 1998; Passage Oct 21, 1998
Bill 3: Introduction Mar 18, 1999; Passage Dec 17, 1999
Bill 4: Introduction Mar 18, 2003; Passage Oct 4, 2004
Bill 5: Introduction Sep 19, 2006; Passage Dec 20, 2006
Bill 6: Introduction Mar 9, 2007; Passage Oct 3, 2008
Bill 7: Introduction Mar 16, 2010; Passage Dec 17, 2010
Bill 8: Introduction Jul 24, 2012; Passage Jan 2, 2013
Bill 9: Introduction Dec 1, 2014; Passage Dec 19, 2014
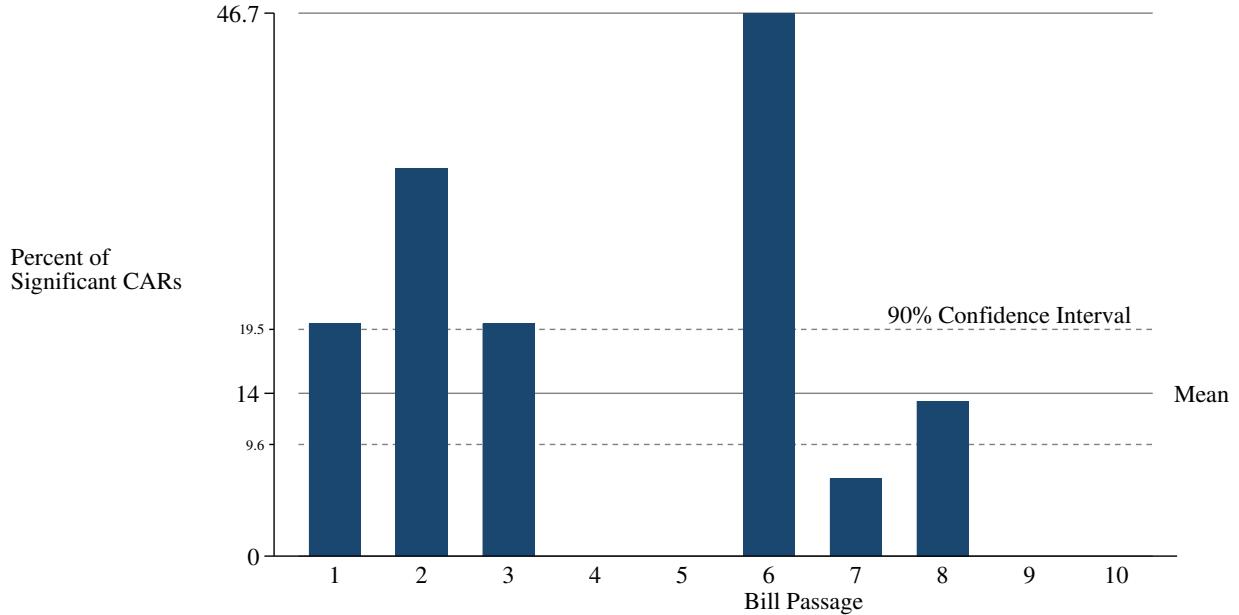Bill 10: Introduction Apr 24, 2015; Passage Dec 18, 2015

The effective bar for Bill #10 scaled to September 30th, 2017. Sources: United States Congress (2015); U.S. Government Publishing Office (2015).

Figure 2: On Average There Are No Statistically Significant Cumulative Abnormal Returns on Bill Introduction Dates
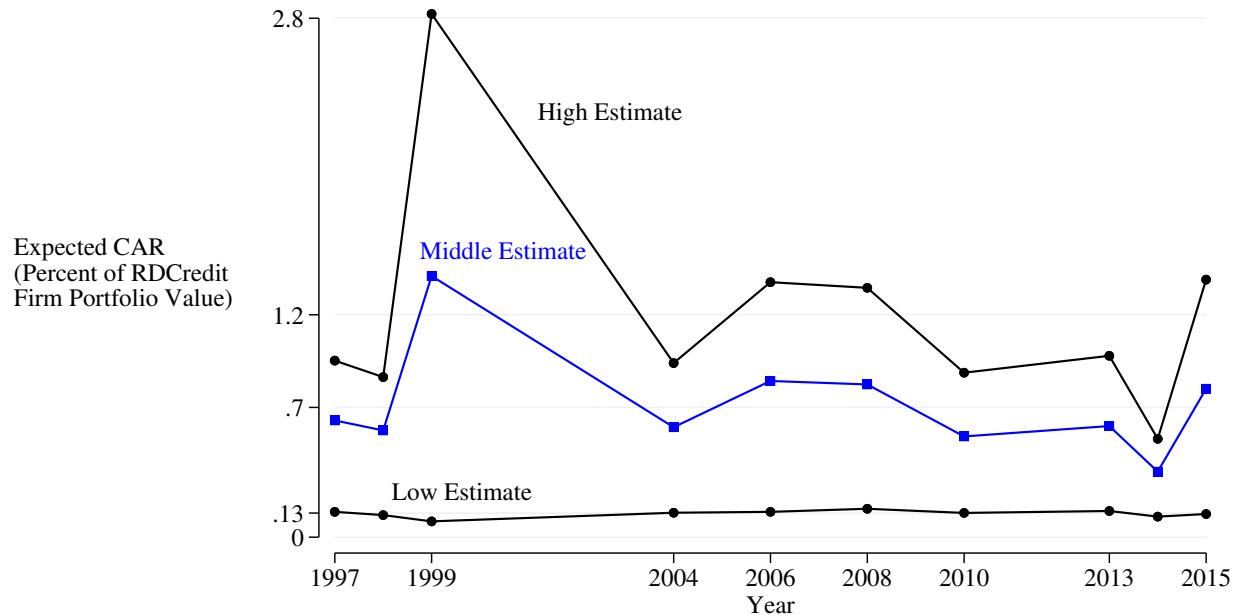

This figure plots the percent of statistically significant cumulative abnormal returns (CARs) at the 10% level by introduction date and the overall mean of significant CARs. Counterfactual returns calculated over three event window sizes using the following five models, for a total of fifteen CARs per event: two versions of the market model, the Fama and French (1992) three factor model, and two matched portfolios assuming equal correlation between the return the portfolio of firms that claimed the R&D tax credit and the matched portfolio. Dashed lines are the 90% Clopper and Pearson (1934) confidence intervals. Because I find only 4.7% of CARs on introduction dates are statistically significant at the 10% level, well below the 10% of CARs that should be significant by chance alone, my results imply that, on average, there is no effect of R&D tax credit extensions on CARs.

Figure 3: On Average There Are No Statistically Significant Cumulative Abnormal Returns on Bill Passage Dates
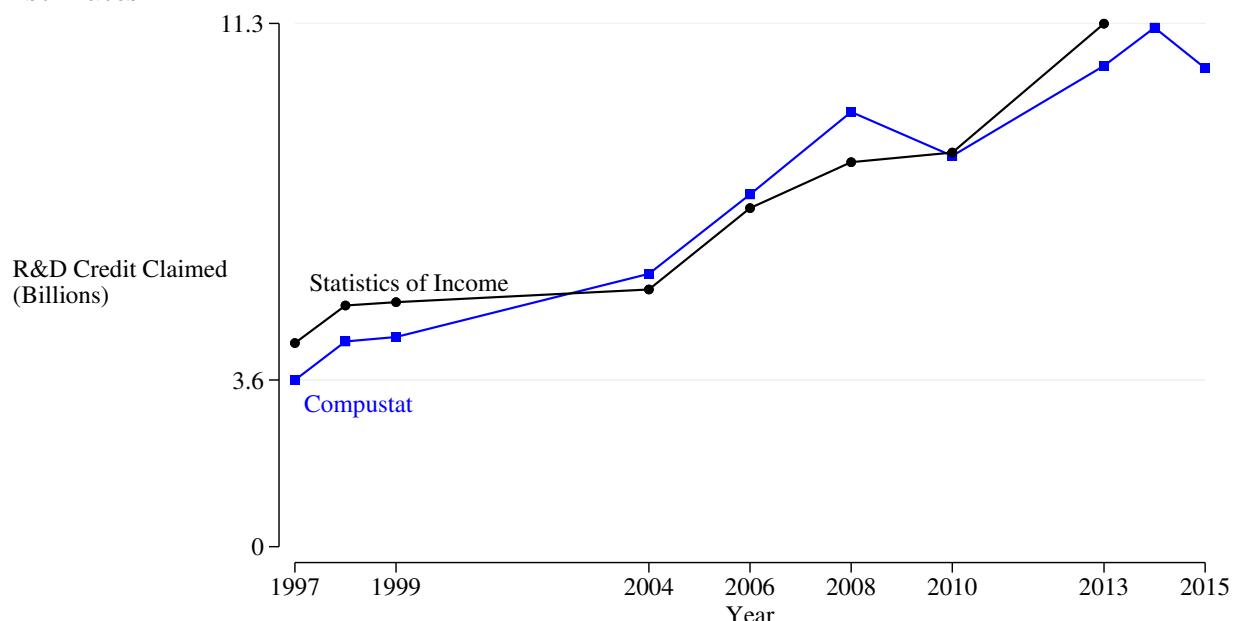

This figure plots the percent of statistically significant cumulative abnormal returns (CARs) at the 10% level by passage date and the overall mean of significant CARs. Counterfactual returns calculated over three event window sizes using the following five models, for a total of fifteen CARs per event: two versions of the market model, the Fama and French (1992) three factor model, and two matched portfolios assuming equal correlation between the return the portfolio of firms that claimed the R&D tax credit and the matched portfolio. Dashed lines are the 90% Clopper and Pearson (1934) confidence intervals. Because I find that 14% of CARs on introduction dates are statistically significant at the 10% level, and the 14% is not statistically different from the 10% of CARs that should be significant by chance alone, my results imply that, on average, there is no effect of R&D tax credit extensions on CARs.

Figure 4: Expected Effect of R&D Tax Credit Extensions on Market Equity Varies With Assumptions, but the Average Effect is Seventy Basis Points

This figure plots three estimates of the effect of R&D tax credit extensions on the portfolio of firms that claimed the R&D tax credit. The high estimates line, the top black line with circles, assumes that R&D tax credit extensions are valued at 150% of 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 middle estimates line, the middle blue line with squares, 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 30% per year. The low estimates line, the bottom black line with circles, assumes that R&D tax credit extensions are valued equal to their book value for the current year and future years are not valued at all. Market equity and book equity are calculated twenty trading days before passage for each R&D tax credit extension. The means for the high, medium, and low estimates are about one hundred and twenty, seventy, and thirteen basis points, respectively. Calculations using legislative text and Compustat-CRSP (U.S. Government Publishing Office, 2015; Center for Research in Security Prices, 2016).

Figure 5: Compustat Claimed R&D Tax Credit Estimates Are Close to Statistics of Income Estimates

This figure plots my estimated amount of R&D tax credit claimed from Compustat-CRSP (Center for Research in Security Prices, 2016) 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).