Identifying the Heterogeneous Impact of Highly Anticipated Events: Evidence from the Tax Cuts and Jobs Act*

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ABSTRACT

We develop a method for estimating individual firm heterogeneity in the stock market impact of aggregate events, using data on both stock and options prices. Our method impounds the effects of event anticipation. We apply the method to the passage of the Tax Cuts and Jobs Act (TCJA), which exhibits both anticipation and heterogeneity. We estimate that the market anticipated the probability of passage to be 95% 30 days before the event. The full value impact of the TCJA is 12.36%, compared to 0.68% when market anticipation is ignored. Large, innovative firms with high growth prospects are the largest winners.
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I. Introduction

Since their introduction in Fama, Fisher, Jensen, and Roll (1969), event studies have been widely used in finance and economics. By measuring on a given day the difference between a company's actual and expected stock returns, one can assess the impact of specific events on the valuation of publicly traded companies. This useful and appealing simplicity rests on the assumption that the event is unanticipated. If not, the market's reaction at the event time can be misleading, as prices impound the impact of the event before the event's actual occurrence (Huberman and Schwert, 1985; Bhattacharya, Daouk, Jorgenson, and Kehr, 2000). Anticipation further complicates the identification of winners and losers from aggregate events. We use Monte Carlo methods to demonstrate that noise in individual firms' stock prices during the anticipatory period reshuffles the cross-section of observed price responses on the actual event day. This reshuffling can realistically be severe enough to misclassify the majority of firms as winners or losers.

To solve this problem, we introduce a new type of event-study methodology for estimating the heterogeneous impacts of an aggregate, anticipated event, and we apply it to the passage of the Tax Cuts and Jobs Act (TCJA), which exhibits both anticipation and heterogeneity. We estimate that the market anticipated the probability of passage to be as high as 95% 30 days before the event. We estimate that the impact of the TCJA on our sample of firms, inclusive of anticipatory effects, is 12.36%, which is much higher than the estimate of 0.68% from a traditional event study that ignores market anticipation. The impact of the TCJA is heterogeneous, with the biggest winners being large, innovative firms with high growth prospects. In contrast, we find a negligible effect on small firms that produce a low number of high-impact patents.

On an intuitive level, the method is straightforward. One can represent market anticipation econometrically as the probability that an event occurs. The distribution of the firm's return preceding an event is then the probability-weighted average of two latent distributions: one conditional on the event not occurring and the other conditional on the event occurring. Recovering the mean of the second distribution produces an estimate of the effect of the event that embodies anticipation. Data on stock returns alone are insufficient to identify the unobservable probability and latent distributions. However, additional data on options at different maturities offer identifying information by providing a sufficient number of estimating equations to identify all of the unknown parameters, namely the probability and the means and variances of the two distributions. These extra equations offer identification because option and stock prices depend on these parameters differently.

This basic insight, which is from Barraclough, Robinson, Smith, and Whaley (2013) and Borochin (2014), is useful for understanding the effects of events that involve individual firms or homogeneous industries. We extend this methodology to be useful when events have heterogeneous outcomes by providing a new solution to an identification issue that arises when an estimation is based on a weighted average of likelihoods, that is, a mixture model. This issue, called label switching (Stephens, 2000; Jasra, Holmes, and Stephens, 2005), arises because the overall likelihood does not change when the identities associated with one of the component likelihoods are swapped. In the context of an event study, this problem manifests itself because the pricing equations used for estimation do not change when the identity of a winner or loser in an event is swapped across the two (or more) unobserved state-contingent prices that are being averaged. Borochin (2014) provides additional identifying information in the form of a sign restriction on the average stock return of the industry affected by the event. While this type of restriction is sufficient for identification in the case of an event with a homogeneous outcome, it falls short in our setting of heterogeneous outcomes.

Instead, for each date preceding the event and for each firm, we determine whether our estimating equations fit better if the firm is assumed to be a winner or a loser. We then assign a firm winner status if more than half of these comparisons show that the model fits better in the winner case. Otherwise, we classify the firm as a loser. Given these data-driven restrictions on a given firm's winner status, we then have sufficient identifying information to use our data on stock and option returns to recover the effects of an aggregate event on individual firms. This new estimator represents a methodological contribution compared to existing firm-specific methods. Although we apply it to the TCJA, as long as the institutional setting allows the definition of a clear run-up period, the method can be applied to nearly any aggregate event.

The value of expected event probabilities in correctly assessing market reactions has been long recognized in works that improve event studies by including firm characteristics such as Malatesta and Thompson (1985), Brennan (1990), Eckbo, Maksimovic, and Williams (1990), Acharya (1993), Chaplinsky and Hansen (1993), Prabhala (1997), Song and Walkling (2000), Bhagat, Dong, Hirshleifer, and Noah (2005), and Cai, Song, and Walkling (2011). These characteristic-based studies are limited to the extent that the data on relevant characteristics might be unavailable. Moreover, any characteristics need to be identified ex-ante for each type of event, so a researcher is left with the burden of justifying these choices. Finally, firm characteristic data are not suitable for macroeconomic events, which are likely to be exogenous to the characteristics of any individual firm. Our work contributes to this work by using options price data to estimate anticipatory effects and therefore obviate the need to pick characteristics.

Related work by Snowberg, Wolfers, and Zitzewitz (2007), Wolfers and Zitzewitz (2009), and Snowberg, Wolfers, and Zitzewitz (2011) has also looked at anticipation effects in aggregate events. However, their work is limited because they borrow event probabilities from predictive markets. Thus their method, while innovative, is only applicable to those events that generate trades in prediction markets. In addition, public markets are deeper and more liquid than prediction markets, so the probabilities we estimate are likely more accurate.

A simple and commonly used empirical approach to estimate anticipation effects is to include a run-up window in the estimation. Because the first date of anticipation is unknown, the run-up window needs to be sufficiently long to cover this unknown first date. However, with a longer run-up window, other events falling in the run-up window can contaminate the estimation. To reduce this bias, researchers construct portfolios of firms during the event window, thus mitigating the impact of any idiosyncratic shocks (Jaffe, 1974; Mandelker, 1974; Fama, 1998). However, when the effects of the event are both heterogeneous and possibly anticipated, the portfolio approach fails to uncover the heterogeneity.

Our study follows a prior investigation of the potential future implications of a TCJA-like policy by Wagner, Zeckhauser, and Ziegler (2018), which relies on the unanticipated outcome of the 2016 presidential election to circumvent the challenge of anticipation in event studies. We believe the market's expectations about the effects of the TCJA are worth revisiting in data that fully incorporates the details of the proposed tax reform, as reliance on an election outcome for identification limits the external validity of any results to the details of the winning party's platform. Put differently, because we do not need to rely on an auxiliary event such as an election, our result is more broadly applicable to any aggregate event.

Finally, our paper builds on and complements several studies that relate the TCJA to a variety of firm outcomes. For example, Dyreng, Gaertner, Hoopes, and Vernon (2020) and Wagner, Zeckhauser, and Ziegler (2020) focus on tax rates, and Kalcheva, Plečnik, Tran, and Turkiela (2020) studies market reactions without explicitly accounting for investor anticipation. Other corporate policies that have been examined include stock repurchases (Bennett, Thakor, and Wang, 2019), leverage (Carrizosa, Gaertner, and Lynch, 2020), payouts (Hanlon, Hoopes, and Slemrod, 2019), executive compensation (Luna, Schuchard, and Stanley, 2019; De Simone, McClure, and Stomberg, 2020), IPO valuations (Edwards and Hutchens, 2020), and uses of repatriated cash (Atwood, Downes, Henley, and Mathis, 2020; Beyer, Downes, Mathis, and Rapley, 2019; Olson, 2019; Albertus, Glover, and Levine, 2022). We add to this work because our estimates are unbiased by anticipation, so we can provide detailed descriptive evidence regarding which firms win and which lose.

The paper is organized as follows. In Section II, we present some simple Monte Carlo results to illustrate the shortcomings of traditional event study methods in estimating anticipation effects and in ranking firms according to the benefits and costs they accrue from an event. In Section III, we propose an alternative method to estimate the full value effect of an event using information on both stock and option prices. In Sections IV and V, we describe our data and our estimates of the probability of the passage of the TCJA and its impact on firm value. In Section VI, we further study the effects of the passage of the TCJA on the state-contingent payoffs of our sample firms. Section VII concludes.

II. Anticipation Biases

A. Environment

In this section, we use Monte Carlo simulations of a simple model to illustrate the severity of the underlying problems associated with using traditional event study methods when an event is highly anticipated and actually occurs.[1] We consider an economy populated by M publicly traded firms, indexed by $i \in \{1, \ldots, M\}$. The stock price of firm i at time t is denoted by $P_{i,t}$. An event will either realize or fail to realize at time $T+1$. The successful realization of the event is a Bernoulli random variable, denoted by $\mathbb{I}$. If the event realizes, $\mathbb{I}$ takes a value of one and is zero otherwise. The probability of event realization is given by $q \in [0, 1]$. This probability q is common knowledge among market participants, but unknown to the econometrician.

If the event does not realize ($\mathbb{I} = 0$), the fair value of the stock of firm i at time $T+1$ is given by $x_i \epsilon_i$, where ϵ_i is an i.i.d. random variable with mean $\mu_\epsilon = 1$. x_i is a firm-specific scalar, which is common knowledge at time T. ϵ_i is a noise term that represents the fluctuation in the firm's value independent of the event. Therefore, $\mathbb{E}_T[x_i \epsilon_i | x_i] = x_i \mathbb{E}[\epsilon_i] = x_i$, so x_i represents the expected value of the stock of the firm at time T, conditional on the non-realization of the event.

If the event realizes, it has a heterogeneous impact on firms $i \in \{1, \ldots, M\}$. The

[1] Naturally, the same issues arise when a barely anticipated event fails to realize, for example, a vote that is highly anticipated to fail and that fails.

effect of the event on firm i is a random variable denoted by s_i, which is drawn from a distribution F with mean $\mu_s > 0$. The firm-specific effect, s_i, is known to the market participants even before the realization of the event, but it is unknown to the econometrician. The fair value of the stock of firm i at time $T+1$ conditional on the realization of the event ($\mathbb{I} = 1$) is then given by $s_i x_i \epsilon_i$. The expected value of the stock of the firm at time T conditional on the realization of the event is given as $\mathbb{E}_T[s_i x_i \epsilon_i | s_i, x_i] = s_i x_i \mathbb{E}[\epsilon_i] = s_i x_i$. Therefore, s_i represents the ratio of the expected value of the stock at time T conditional on realization divided by the same expectation, but conditional on non-realization. Thus, the net firm-specific impact of the event is given by $s_i - 1$.

B. Prices and the Traditional Event Study Estimators

Given the described environment, we can derive the implied stock prices. The stock price of firm i at time $T+1$ contingent on the realization of the event is:

$$P_{i,T+1} = \begin{cases} s_i x_i \epsilon_i & \text{if } \mathbb{I} = 1 \\ x_i \epsilon_i, & \text{if } \mathbb{I} = 0. \end{cases} \tag{1}$$

At time T, s_i and x_i are known to the market participants, but the noise term ϵ_i and the realization of the event, $\mathbb{I}$, are not. Therefore, the stock price of firm i at time T is calculated as:

$$\begin{aligned} P_{i,T} &= \mathbb{E}_T[\mathbb{I}(s_i x_i \epsilon_i) + (1-\mathbb{I})(x_i \epsilon_i) | s_i, x_i] \\ &= q s_i x_i \mathbb{E}[\epsilon_i] + (1-q) x_i \mathbb{E}[\epsilon_i] \\ &= (q s_i + (1-q)) x_i. \end{aligned} \tag{2}$$

Consider the case in which the event realizes. Suppose the econometrician is interested in the aggregate effect of the event, which requires both the estimation of μ_s and the impact of the event on different firms, that is, the estimation of s_i for each firm i. The traditional event study estimator for the net firm-specific impact of the event, $s_i - 1$, is then the change in the stock price of the firm between $T + 1$ and T. Given $\mathbb{I} = 1$, this estimate is calculated as:

$$\begin{aligned}\frac{P_{i,T+1} - P_{i,T}}{P_{i,T}} &= \frac{s_i x_i \epsilon_i - (qs_i + (1-q))x_i}{(qs_i + (1-q))x_i} \\ &= \frac{s_i \epsilon_i - (qs_i + (1-q))}{(qs_i + (1-q))}. \end{aligned} \tag{3}$$

If the event is completely unanticipated, the traditional event study estimator is reasonable because plugging in $q = 0$ yields:

$$\frac{P_{i,T+1} - P_{i,T}}{P_{i,T}} = s_i \epsilon_i - 1. \tag{4}$$

However, when the event is not completely unanticipated ($q > 0$), the traditional estimator captures only the unanticipated impact instead of the true impact, $s_i - 1$. Nonetheless, if one is interested solely in the relative impact on the firms (e.g. figuring out the winners and the losers), the estimate given by (3) is positively correlated with the true impact $s_i - 1$. If the correlation is high, this estimate might serve as a reasonable proxy. We examine this conjecture below.

A similar situation arises for estimating the net aggregate impact of the event $\mu_s - 1$. The traditional event study methodology consists of taking the average of the individual estimates of $s_i - 1$, that is:

$$\frac{1}{M}\sum_{i=1}^{M} \frac{P_{i,T+1} - P_{i,T}}{P_{i,T}} = \frac{1}{M}\sum_{i=1}^{M} \frac{s_i \epsilon_i - (qs_i + (1-q))}{(qs_i + (1-q))}. \tag{5}$$

Again, if the event is completely unanticipated ($q = 0$), this is a reasonable estimator because:

$$\mathbb{E}\left[\frac{1}{M}\sum_{i=1}^{M}\frac{P_{i,T+1}-P_{i,T}}{P_{i,T}}\right] = \frac{1}{M}\sum_{i=1}^{M}\left(\mathbb{E}[s_i]-1\right) \xrightarrow{p} \mu_s - 1. \tag{6}$$

However, for positive values of q, this estimator only captures the unanticipated fraction of the aggregate impact of the event. Because q is unknown to the econometrician, a true low impact (low μ_s) or high anticipation (high q) are observationally equivalent, with both producing a low estimated effect.

C. Traditional Event Study Estimator Performance

While it is clear that anticipation makes it impossible to recover the true overall price impact of an aggregate event, the magnitude of this problem is not obvious. Even less obvious is the quantitative impact of anticipation on the ability of a traditional event study to identify the relative winners and losers from an aggregate event.

To examine these questions, we conduct Monte Carlo simulations based on the following distributional assumptions. We assume that the idiosyncratic i.i.d. noise variable, ϵ_i, is drawn from a lognormal distribution, lognormal$(\sigma_\epsilon/2, \sigma_\epsilon)$. Next, we assume that the firm-specific s_i are drawn from a normal distribution $\mathbb{N}(\mu_s, \sigma_s)$. We do not need to make any assumptions regarding the distribution of x_i, as it is just a scaling constant.

For our baseline illustration, we choose the following parameter values: $\mu_s = 1.1236, \sigma_s = 0.0952, \sigma_\epsilon = 0.04$, and $M = 100$. To ensure that our Monte Carlos are empirically relevant, these values are from our empirical investigation of the TCJA below in Section V. First, μ_s and σ_s are from our estimates of the distribution of s_i.

Second, σ_ϵ is from the average daily stock price volatility in our sample of firms. Third, we set $M = 100$ because our sample consists of 100 firms. We conduct $T = 10,000$ simulations for each parameter set corresponding to a different value of the event anticipation q, and we use bootstrapping to obtain 10% upper and lower confidence bounds.

First, we assess the ability of the traditional event study estimator to recover the relative impacts of an aggregate event on individual firms. As noted above, even if the estimator does not capture the level of the individual true impacts $s_i - 1, \forall i$, it is possible that it might perform reasonably well in capturing the relative ranking of the firms. To evaluate performance among this dimension, we compute the cross-sectional correlation between $(P_{i,T+1} - P_{i,T})/P_{i,T}$ and $s_i - 1$. Figure 1 presents the mean correlation we obtain across $T = 10,000$ simulations as a function of the event anticipation q, which ranges from 1% to 99%. When anticipation is very low at $q = 1\%$, the correlation is quite high at 0.89, and the traditional event study estimator performs well. However, as the anticipation of the realized event increases, the reliability declines steeply. If $q = 80\%$, the correlation falls to 0.44, and if $q = 96\%$, the correlation is only 0.2, with a 10% lower bound of 0.087. We conclude that for highly anticipated events, the traditional event study estimator cannot assess the relative impact of the event across firms.

Next, we assess the ability of the traditional event study estimator to recover the aggregate impact of the event. Figure 2 displays the true aggregate impact of the event, along with the mean aggregate impact that the traditional estimator delivers, both of which we compute across $T = 10,000$ simulations as a function of the event anticipation q. Not surprisingly, the estimated impact is biased downwards, and the bias is increasing in q, as the increase in anticipation is mistakenly attributed to a lower true impact $\mu_s - 1$. At $q = 96\%$, the mean aggregate impact is tiny at 0.29%, which is 42 times smaller than the true impact of 12.4%. This result reinforces the idea that downward bias can be severe in the case of highly anticipated events.

D. Robustness of the Performance Results

Next, in Figure 3, we show how the performance of the traditional event study estimators is related to our parameter choices. We first investigate the effects of changing the net aggregate impact of the event $\mu_s - 1$ to either 50% or 150% of its baseline value. The top-left subfigure repeats the exercise in Figure 1. We find that the mean correlation is only slightly higher when the net aggregate impact $\mu_s - 1$ is lower. The top-right subfigure repeats the exercise in Figure 2. Naturally, changing the net aggregate impact $\mu_s - 1$ scales its estimate up and down by the same amount. However, the relative downward bias compared to its true value as a function of the event anticipation q remains the same. Therefore, the magnitude of the true aggregate impact of the event is irrelevant for the downward bias from ignoring event anticipation.

Next, we investigate the effects of changing the standard deviation parameters σ_s and σ_ϵ to either 50% or 150% of their baseline values. A higher value of σ_s implies more heterogeneity in the firm-specific impact, s_i, across firms. A higher value of σ_ϵ implies more volatile firm stock prices, independent of the event. The bottom-left and bottom-right subfigures repeat the exercise in Figure 1 for different values of σ_s and σ_ϵ, respectively. We find that the mean correlation is higher when σ_s is higher and σ_ϵ is lower. Intuitively, when the firm-specific effect has a higher variance, it is easier to distinguish the winners from losers, even in the presence of random noise from ϵ_t. Likewise, when the random noise has a lower variance, even small differences in s_i can more reliably be estimated, as less noise implies less reshuffling of the cross-sectional distribution of firms. Finally, even when the traditional event study estimator performs the best (high σ_s and low σ_ϵ), the cross-sectional correlation between the true and estimated event study effects remains average low at high values of event anticipation ($q > 90\%$).

III. Methodology

In this section, we develop an estimator that produces estimates of the individual effects of an anticipated aggregate event, as well as an unbiased estimate of the average effect. We obtain our alternative estimate essentially by comparing a firm's value after an event to an estimated counterfactual that embodies anticipation. We identify the counterfactual by building off of prior work that uses options data to identify counterfactual firm values in the setting of M&A (Barraclough et al., 2013; Borochin, 2014) and industry sector regulation (Borochin and Golec, 2016).

We start with the model in Subramanian (2004) by defining the price process, dS, for an asset exposed to an upcoming binary event. This process follows one of two possible geometric Brownian motions at the event date with risk-neutral probabilities q and $1-q$:

$$\frac{1}{r}dS = \begin{cases} S_u + \sigma_u S_u dz & \text{if event occurs with probability } q \\ S_d + \sigma_d S_d dz & \text{otherwise.} \end{cases} \tag{7}$$

Here, dz is a standard Brownian motion increment, and r is the instantaneous risk-free rate. Equation (7) implies that the instantaneous expected returns in both states equal r under the risk-neutral measure. We assume that $S_u \neq S_d$ and $\sigma_u \neq \sigma_d$ without loss of generality. Next, we assume that σ_u and σ_d are investor expectations of the true state-contingent volatility of the underlying asset and that σ_u and σ_d do not vary across option moneyness. In this case, we can express the values of the stock and its options as functions of five unknown parameters: the risk-neutral probability of the event q, the state-contingent values S_u and S_d, and the volatilities σ_u and σ_d. Suppose there exist J options on the asset, with unique strike prices K_j and a common remaining time to maturity τ that ends after the event. In this case, for any firm i, on any day prior to the resolution of the binary event, we can characterize the prices of the $J+1$ assets as follows:

$$\begin{aligned}
S_{i,t} =& \mathbb{E}_t(q) \cdot \mathbb{E}_t(S_{i,u}) + (1 - \mathbb{E}_t(q)) \cdot \mathbb{E}_t(S_{i,d}) \\
c_{i,1,t} =& \mathbb{E}_t(q) \cdot C(E_t(S_{i,u}), \mathbb{E}_t(\sigma_{i,u}), K_1, \tau) + (1 - \mathbb{E}_t(q)) \cdot C(\mathbb{E}_t(S_{i,d}), E_t(\sigma_{i,d}), K_1, \tau) \\
&\vdots \\
c_{I,J,t} =& \mathbb{E}_t(q) \cdot C(E_t(S_{i,u}), \mathbb{E}_t(\sigma_{i,u}), K_J, \tau) + (1 - \mathbb{E}_t(q)) \cdot C(\mathbb{E}_t(S_{i,d}), E_t(\sigma_{i,d}), K_J, \tau).
\end{aligned} \tag{8}$$

Here, $\mathbb{E}_t$ represents investors' expectations at time t, $c_{i,j,t}$ is the price of a call with strike price K_j, $C(\cdot)$ is a function that gives the theoretical (Black-Scholes or binomial) price of the option. Note that $C(\cdot)$ is defined in terms of the event-contingent stock price.

The pricing equations in (8) for the $J+1$ assets, contain five parameters to estimate: the time-t expectations of q, S_u, S_d, σ_u, and σ_d. Therefore, these equations provide identifying restrictions on these parameters, as long as $J+1 \geq 5$. The optimal choice of J trades off an additional signal from overidentification against additional noise from the use of less-liquid and therefore less-accurate option prices.

As noted in the introduction, a further and well-known problem with identifying the five parameters of interest is called the label switching problem (Stephens, 2000; Jasra et al., 2005), which arises because the states (winner versus loser) that the unobserved $S_{i,u}$ and $S_{i,d}$ variables represent can be exchanged arbitrarily, without changing the solution to the pricing equations in (8). This problem clearly prevents the identification of the parameters that describe these states. A simple solution is to restrict $S_{i,u} > S_{i,d}$, which is a credible assumption in those applications in which one state undoubtedly leads to a higher value for a firm than another (Barraclough et al., 2013; Borochin, 2014; Borochin and Golec, 2016). For example, the passage of the Affordable Care Act was expected to benefit hospital administrators and insurers by increasing coverage rates (Borochin and Golec, 2016).

However, this identifying restriction cannot be applied to an event like the passage of the TCJA, which was not expected to be uniformly beneficial. For example, the low rate for repatriation of foreign cash holdings only affected a subset of firms.

We solve the label switching problem using a more general and data-driven approach that can identify the ranking of the firm-level state-contingent payoffs, $S_{i,u}$ and $S_{i,d}$, when there are heterogeneous expected outcomes from an event. First, for each firm and time before the event, we perform two estimations: one under the assumption that $S_{i,u} \geq S_{i,d}$ and another under the assumption that $S_{i,u} < S_{i,d}$. In both cases, we use our data on stock and option returns to pick q and the other model variables $\theta = \{S_{i,u}, S_{i,d}, \sigma_{i,u}, \sigma_{i,d}\}$ to minimize the equal-weighted difference between normalized observed security prices and their values implied by (8).

We express this minimization succinctly as follows:

$$V_{i,t,\text{winner}}(q,\theta) = \left|1 - \hat{P}_{i,t}(q,\theta_i)/P_{i,t}\right| \quad \text{if } S_{i,u} \geq S_{i,d} \tag{9}$$

$$V_{i,t,\text{loser}}(q,\theta) = \left|1 - \hat{P}_{i,t}(q,\theta_i)/P_{i,t}\right| \quad \text{if } S_{i,u} < S_{i,d} \tag{10}$$

Here, $P_{i,t}$ is the vector of prices on the left-hand side of the equations in (8), $\hat{P}_{i,t}(q,\theta)$ is the vector of implied prices on the right-hand side of these equations, and $|\cdot|$ is a vector norm that minimizes the sum of squares.

In our particular application, we classify the firm as a winner if the restriction $S_{i,u} \geq S_{i,d}$ results in a better fit in more than half of the minimizations over the 30-day period from November 10 to December 2, 2017, during which the TCJA was discussed in Congress. Otherwise, the firm is a loser. In other words, we let the data tell us which identifying restriction is more appropriate for each firm, allowing us to identify $S_{i,u}$ and $S_{i,d}$ regardless of whether the firm is expected to gain or lose from the passage of the TCJA.

Next, we impose one of the two identifying conditions, $S_{i,u} \geq S_{i,d}$ or $S_{i,u} < S_{i,d}$, for each firm. We then define the estimator by:

$$\left(\hat{q}_t, \{\hat{\theta}_{i,t}\}_{I=1}^{M}\right) = \underset{q,\{\theta_i\}_{i=1}^{M}}{\arg\min} \left\{ \sum_{i=1}^{M} W_{i,t}(q,\theta_i) \right\}, \tag{11}$$

where

$$W_{i,t}(q,\theta_i) = \begin{cases} V_{i,t,\text{winner}}(q,\theta), \text{or} \\ \\ V_{i,t,\text{loser}}(q,\theta) \end{cases} \tag{12}$$

Conditional on the identifying restrictions for $S_{i,u}$ and $S_{i,d}$, this estimator is a minimum distance estimator and thus inherits the consistency and asymptotic unbiasedness properties from this class of estimators.

Of particular interest is the estimate of the quantity $S_{i,u}/S_{i,d} - 1$, which is the event return for an individual return that impounds anticipation. Intuitively $S_{i,u}$ is the stock price after the occurrence of the event, and $S_{i,d}$ is the unobservable, counterfactual price that would have occurred in the absence of the event.

The feature of this estimator that makes it tractable is the assumption of a single common event probability q_t, which is common for all firms and which we estimate for each time period t. The estimation of the firm-specific parameters $\{\theta_{i,t}\}_{i=1}^{M}$ depends on the results from other firms only through the common event probability q. Using this property, we develop an efficient algorithm in which we first divide the range $q \in [0,1]$ into a discrete grid with Q points. Then, for each firm i, on each date t, and for every $q \in Q$, we estimate the $\theta_{q,i,t}$ that minimizes $W_{i,t}(q, \theta_i)$ given q. Then q_t can be calculated as:

$$\hat{q}_t = \arg\min_q \left\{ \sum_{i=1}^{M} W_{i,t}(q, \theta_{q,i,t}) \right\} \tag{13}$$

The estimates of the firm-specific parameters, $\{\hat{\theta}_{i,t}\}_{I=1}^{M}$, are those estimates that correspond to $\hat{q}_t$.

Without this simplification, the estimation would require us to solve for the values of $4M + 1$ parameters for each day with JM moments to match. Because we have $M = 100$ firms in our sample, this minimization would be difficult to perform. In contrast, our discretization procedure implies that we solve $Q \times M \times 2 = 20,000$ simple problems of fitting 4 parameters, where "2" comes from our data-driven identification condition procedure, and where Q contains 100 points. Once we compute the individual results, the final step in (13) is simple.

On the one hand, the assumption of homogeneity in q precludes potential differences in subjective beliefs regarding the event for possibly different sets of investors for different firms. On the other hand, assuming a single q also allows us to obtain precise estimates.

Finally, q is not a physical probability. However, because the analysis focuses on a relatively narrow 30-day window, and because a risk-neutral probability can be expressed as the product of a physical probability and the pricing kernel or stochastic discount factor, our estimate of q is unlikely to embody a priced risk. Thus, we can claim that q can be interpreted as a true, rather than a risk-neutral, probability. If our approach were to be extended to long-run events that are more likely to be related to an equity risk premium reflected in the pricing kernel, the risk-neutral probability q would understate the physical probability p.

IV. Data and Sample Characteristics

As mentioned in Section III, we use a sample of 100 firms. To build this sample, we require that the firms have stock price data from CRSP, fundamentals data from Compustat, and at least six option contracts with non-zero open interest during the 30 trading days between November 10, 2017, the first trading day after the TCJA bill was introduced in the Senate, and December 22, 2017, when the final reconciled bill was signed into law. We narrow this set of firms down to the 100 with the most liquid options. This choice trades off the informativeness of option data against the representativeness of the sample relative to the universe of public firms. Table 1 lists these firms, their average daily call option volume during the fourth quarter of 2017, their market capitalization at the beginning of the quarter, and their SIC2 industry sectors.

Setting an appropriate cutoff for the number of firms used in the study is an important consideration, as option market depth falls sharply across firms. For example, the firm with the most liquid options market in our sample, Apple Inc., has an average daily call option volume of 261,788 contracts during our sample period. In comparison, the sample firm with the least liquid option data, Caesars Entertainment Corporation, has an average daily call volume of only 6,758 contracts. Including more firms risks the inclusion of firms with stale prices, as firms can have positive open interest simultaneously with zero volume. Although our sample is only a subset of the universe of optionable stocks, over the fourth quarter of 2017, our sample firms account for 27% to 42% of the total daily equity options volume in the OptionMetrics universe, as well as 29% to 50% of its total daily call volume. Finally, to ensure that the options we use in our estimation have nonzero trading volume, we set the number of call option targets to $J = 6$.

The 100 firms in our sample are large, with an average market capitalization of \$95.84 billion and an average book asset value of \$166.44 billion. During the same time period, the average firm in the Compustat universe has a market capitalization of \$5.72 billion and a book asset value of \$14.67 billion. Although on this dimension, our sample firms are not representative of the universe of publicly traded firms, they cover 31 distinct SIC2 industry sectors. Moreover, because these firms are large, their combined market capitalization is 22.45% of the Compustat universe.

We focus the analysis on call options, consistent with related work by Barraclough et al. (2013), Borochin (2014), and Borochin and Golec (2016). Although put options could also be used, their prices are more likely to be distorted because of short-sale constraints around special events such as the passage of the TCJA. Furthermore, the market for equity puts is less liquid than the market for equity calls. During the fourth quarter of 2017, as the TCJA was being debated, an average of 8,154,592 equity call option contracts were traded each day in the OptionMetrics universe, whereas the volume of traded put contracts was 20% lower at 6,577,204. Figure 4 presents the time series of option trading volume for the firms in our sample, with increases in option volume when the TCJA bill is introduced in the House of Representatives on November 9 and around the time of the Senate vote on the pre-conference version of the bill on December 2. We argue that these volume increases are driven primarily by call option trading, consistent with their primacy relative to put options in capturing investor beliefs about the TCJA passage event.

In addition to estimating the payoffs implied by the passage of the TCJA, $S_{i,u}$ and $S_{i,d}$, we consider their relationship with firm characteristics, the construction of which we describe in Appendix A. We focus in part on variables related to innovation, specifically R&D intensity, the number of patents granted and citations received, asset tangibility, the growth rates of sales, assets, and employment, and measures of originality and generality of patents based on Hall, Jaffe, and Trajtenberg (2001).[2] We also consider general aspects of firm performance: the firm's effective tax rate, the amount of indefinitely reinvested foreign earnings, net tax assets existing prior to TCJA passage, the ratio of cash to assets, the market-to-book ratio, the maturity of

[2] In Appendix A, we explain how we extend the methods in Hall et al. (2001) and Acemoglu, Akcigit, and Celik (2020) to deal with complications introduced by the new patent classification system adopted by the United States Patent and Trademark Office (USPTO).

assets, size, leverage, and several measures of profitability.

V. Anticipation and Effect of the TCJA

We use the methods from Section III to estimate the implied probability that the market attached to the passage of TCJA before its realization, its effects on individual firms, and the aggregate impact on firm value. Because our estimation method relies on a two-step procedure in which a binary classifier based on a percentile provides identifying information in the second step, calculating standard errors analytically is intractable. Therefore, we use bootstrapping to compute confidence intervals. We construct 1000 simulated samples for each day in the period, and for each simulation, we draw 100 firms with replacement. Using the bootstrap sample, we consider two confidence intervals. One is based on plus or minus 1.645 standard deviations of the bootstrap sample, thus producing a 90% confidence interval. The other is based on the 10^{th} and 90^{th} percentiles of the bootstrap sample.

The estimation results are in Figure 5. The horizontal axis corresponds to the number of trading days between the event (December 22, 2017) and the data date t. For both subfigures, the solid blue line depicts the estimated event probability q_t before the passage of TCJA, where only data from that particular day is used in its estimation. The solid red line is the fitted value of estimated event probability using smoothing spline interpolation, which aggregates information across days. The dashed blue lines in the top subfigure display the top and bottom 10^{th} percentiles of the bootstrap sample. The dashed blue lines in the bottom subfigure delineate the 90% confidence interval obtained using the bootstrap sample standard deviation.

The main finding in Figure 5 is that the market had already anticipated the probability of the passage of TCJA to be as high as 95% up to 30 trading days before it happened. Although the estimated probability fluctuates somewhat, it always remains in a tight band of $[0.93, 0.97]$. In sum, much of the impact of the event was already priced on the event day, consistent with the lackluster stock market reaction on the event day. This evidence of high anticipation reinforces the notion that traditional event study methodologies struggle to capture the whole impact of an anticipated event.

As such, we also calculate the aggregate impact of TCJA passage on the firms in our sample. The results are shown in Table 2. The first column contains the mean of the estimated firm-specific impacts, $S_{i,u}/S_{i,d} - 1$, across our sample, which reveals a 12.36% return. The remaining columns report several measures traditionally used in event studies, such as the mean holding period return on the event date (RET), and the mean cumulative abnormal returns on firm stock prices using windows of 3, 5, 7, and 10 trading days. These further measures of the impact of the event are substantially smaller than our estimate, ranging between 0.68% and 2.29%. This pattern is consistent with our high estimates of the value of q prior to the event date, as anticipation biases these traditional estimators towards zero.

Table 3 presents the correlation between our estimates of the net firm-specific impacts, $S_{i,u}/S_{i,d} - 1$, and the traditional firm-level estimates considered in Table 2. We find that the correlation between these measures and our estimated firm-level impact is uniformly positive, but quite low, ranging between 13.3% and 23.3%. As explained earlier, because the event is highly anticipated, traditional estimates only capture price movements in a short window, so the relative value effects for individual firms are swamped by the noise introduced into the prices by unrelated idiosyncratic fluctuations during run-up period that may be longer than the event window employed. This result is in line with our Monte Carlo simulations in Section II, which show a correlation of 20% between the true firm-level impact and the traditional event study estimators when event anticipation is as high as we estimate for the TCJA.

We close with a remark on the interpretation of the estimate of 12.36%. As described above, we have a selected sample of 100 firms with the most liquid call options. Although our sample has broad industry coverage, we cannot extrapolate this finding to the universe of publicly traded firms, much less to any private firm. At the same time, because the market capitalization of the firms in our sample is quite high at 22.45% of the Compustat universe, we have estimated a sizable fraction of the overall impact.

VI. Winners and Losers from the TCJA

In this section, we consider the cross-sectional relation between firm characteristics and our estimates of the impacts of the passage of the TCJA on individual stock prices. We consider the effects of the passage of the TCJA on the state-contingent payoffs of our sample firms by splitting the sample at the median of our estimated individual returns, $\widehat{S_{i,u}/S_{i,d}} - 1$. We then test the null hypotheses that each firm characteristic is the same across the two samples. While obviously not causal in nature, this analysis nonetheless offers insight into which types of firms benefited from the TCJA.

Table 4 presents the results from our sample-splitting exercise. The first column lists the characteristic. The second column contains the average differences for each characteristic, with a positive number indicating that the high-expected-payoff firms have a larger value for a specific characteristic. The third column contains the corresponding *t*-statistic.

First, we examine a set of variables tied to innovation, as the TCJA introduced expensing of investment in intellectual property and innovation (Auerbach, 2018). We see consistent evidence of a strong association between innovative activity and greater benefits from the passage of the TCJA. The high-expected-payoff firms have higher R&D intensity, patent and citation counts, as well as total originality and generality of patents. They also exhibit greater investment in intangible capital, as evidenced by a lower tangibility ratio. Interestingly, the TCJA winners have lower average originality and generality per patent. These differences are all significant at the 1% level. Some of these differences in market expectations are unsurprising, given the favorable tax treatment for innovative activity. However, our results provide more texture by showing that the TCJA appeared to favor an approach to innovation that favors quantity over quality in innovation.

Prior to the TCJA, firms with overseas operations were able to defer U.S. corporate taxes by reinvesting profits overseas. The move toward territorial taxation in the TCJA rendered this tax shelter obsolete. Comfortingly, in Table 4 we see that firms expected to benefit more from TCJA have fewer tax shelters in place, specifically indefinitely reinvested foreign earnings and net tax assets relative to firm size. Both of these differences are statistically significant at the 1% level. Many pre-TCJA overseas assets were held as cash, so a related result is that firms that benefited from TCJA passage have lower cash holdings, although the statistical significance is marginal.

The TCJA proposed to lower the corporate tax rate from 35% to 21%, and Wagner et al. (2018) find stronger positive market reactions around the 2016 election for high-tax firms. Notably, around the passage of the TCJA itself, we find the opposite outcome, with the high-expected-payoff firms having a substantially lower effective tax rate, significant at the 1% level. At first puzzling, this result is consistent with the market having already incorporated the first-order benefits from the passage of the TCJA, leaving second-order effects like tax mitigation strategies to drive market reactions around the TCJA event itself.

Furthermore, we find that sorts on expected payoff correspond to other statistically significant firm characteristic differences. High-expected-payoff firms are larger in terms of total assets and have higher sales growth. These firms have lower leverage, consistent with the TCJA provisions on the limitation of interest payment deductions to 30% of firm EBIT after 2021. Next, the TCJA winners, are less profitable, as measured by return on assets. This last finding has not been previously documented in studies of the TCJA reform. Finally, it is important to note that our ex-ante results are predictive and obtained using price and fundamentals data at the time of the passage of the TCJA, whereas any ex-post studies of individual firm characteristics require a substantial window of time after the event to be possible and can only be applied in a retrospective, rather than a predictive, setting.

Because we have too many potential return predictors relative to our small sample of 100 observations, a standard regression analysis is infeasible, so we use a lasso regression of $\widehat{S_{i,u}/S_{i,d}} - 1$ on the set of variables in Table 4 to understand which ones are most important for predicting returns. The third column of Table 4 indicates which predictors matter. These results largely corroborate the qualitative conclusions from our simpler pairwise t-tests. Variables related to innovation and the corporate tax rate all matter, as do measures of growth, size, and profitability.

VII. Conclusion

In this paper, we develop a new method for conducting event studies around aggregate events. Our method overcomes two challenges that affect event studies: anticipation and heterogeneous impacts on individual firms. We address the first challenge by using both stock and option returns in a model analogous to a mixture model to identify the counterfactual announcement return in a world with both no event and no anticipation. By comparing the actual stock price reaction to this estimated counterfactual, we can impound the effects of anticipation in our estimate of the total effect of the event on stock prices. A useful and informative by-product of this method is an estimate of market expectations of the event probability.

While the use of option returns to provide extra information to identify anticipation effects is from Barraclough et al. (2013) and Borochin (2014), our work broadens the applicability of this method to a much broader set of events by addressing the second challenge related to heterogeneity. To this end, we develop a new data-driven method for identifying our estimating equations for individual firms, even when these firms might not be affected by the event in the same direction. The identifying restrictions used in prior work require monotonicity in the sign of the effect of the event on individual stock prices, while our method carries no such requirement. We use Monte Carlo studies to show that producing accurate estimates of individual abnormal returns is important, as traditional methods fail to recover even the relative magnitudes of abnormal returns.

Applying this methodology to the TCJA, we find that it was highly anticipated, with market participants expecting a 95% probability of passage. Moreover, we find that while traditional event study methods produce small estimates of the abnormal returns associated with the TCJA, our method produces a large effect of approximately 12%. We are also able to associate specific firm characteristics with the winners and losers from the TCJA in a predictive, rather than a retrospective fashion. We find that large innovative firms with many low-impact patents were the biggest winners.

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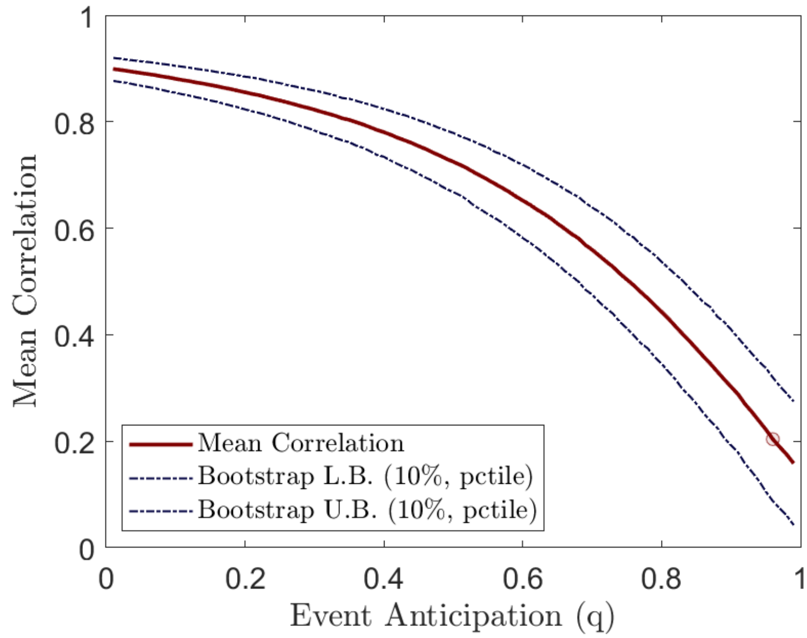
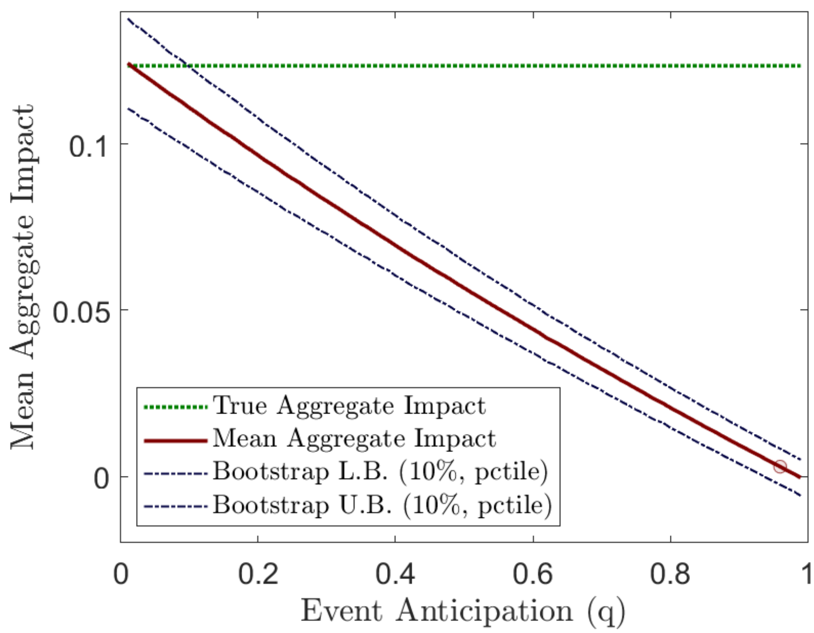
FIGURE 1: CROSS-SECTIONAL CORRELATION BETWEEN TRUE AND ESTIMATED PRICE RESPONSES



Note: Figure 1 presents the results from several 10,000-trial Monte Carlo simulations that assess the performance of a standard event study estimator of an anticipated aggregate event with heterogeneous impacts on individual firms. Each simulation corresponds to a different value of event anticipation, q, which ranges from 1% to 99%, and which is depicted on the x-axis. Each point on the line represents the cross-sectional correlation between the true and estimated effects of the event on the individual firms. Thus this correlation is depicted as a function of q. The dashed lines are boostrapped 10% upper and lower bounds.

FIGURE 2: TRUE AND ESTIMATED NET AGGREGATE EVENT IMPACT



Note: Figure 2 presents the results from several 10,000-trial Monte Carlo simulations that assess the performance of a standard event study estimator of an anticipated aggregate event with heterogeneous impacts on individual firms. Each simulation corresponds to a different value of event anticipation, q, which ranges from 1% to 99%, and which is depicted on the x-axis. Each point on the solid line represents the average estimate from a traditional event-study estimator of the true abnormal return from the event, which is depicted by the dotted line. The dash-dotted lines are bootstrapped 10% upper and lower bounds.

FIGURE 3: SENSITIVITY OF MONTE CARLO RESULTS

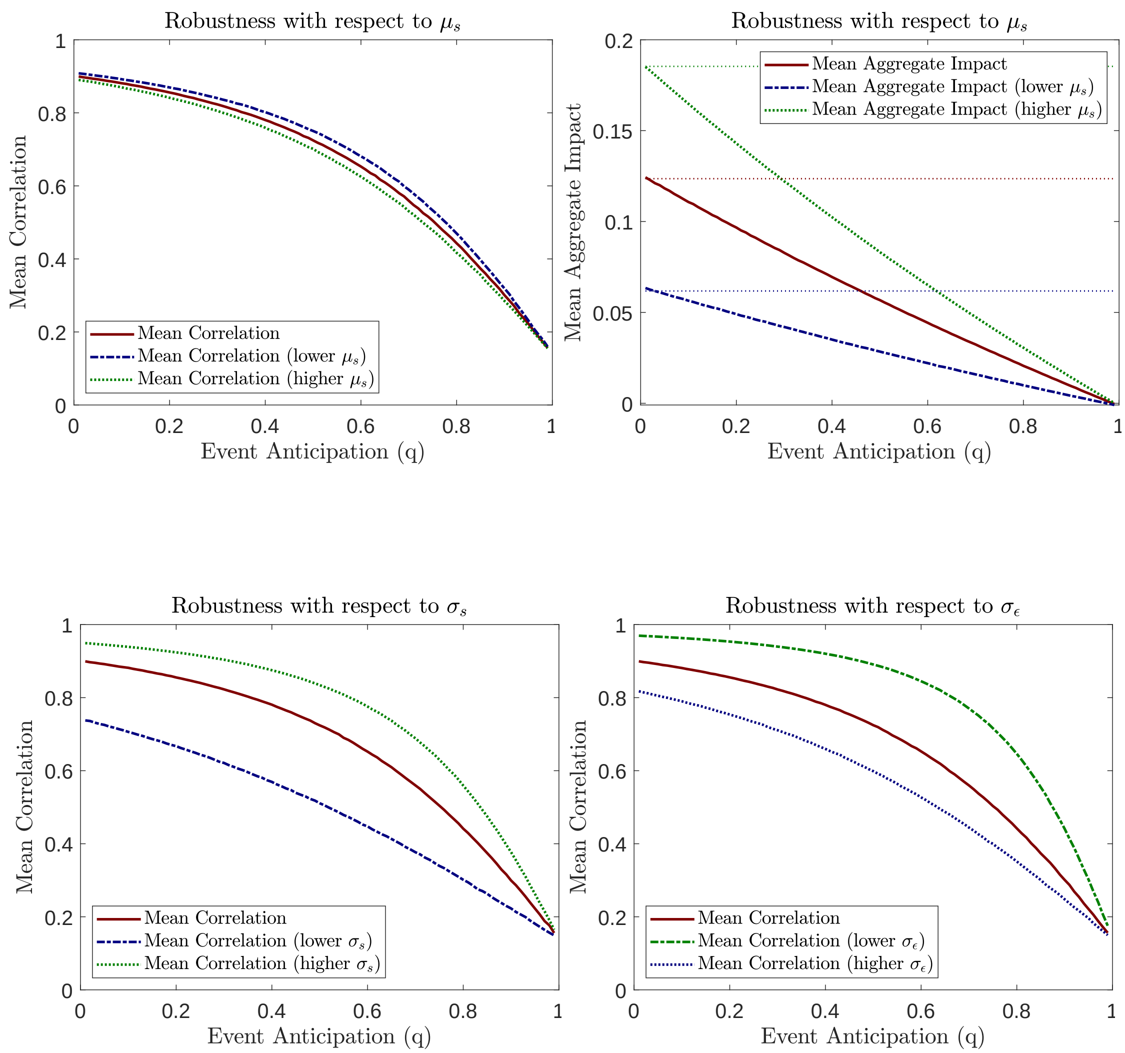
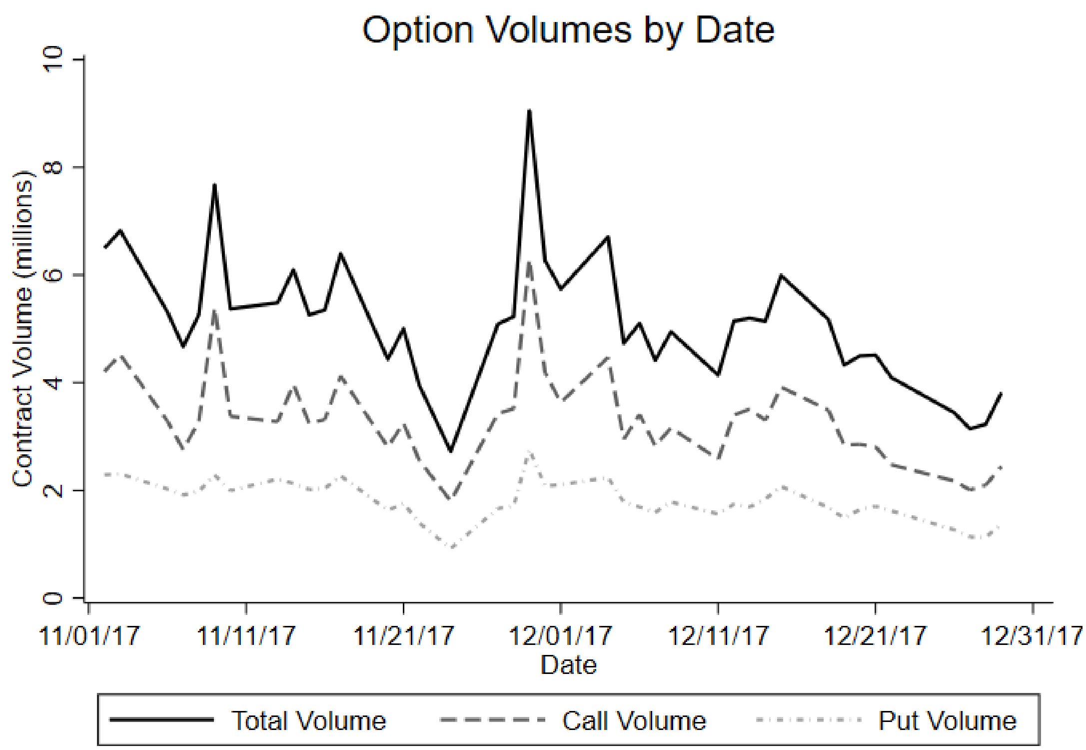


Figure 3 repeats the analysis in Figures 1 and 2 for different values of the parameters μ_s, σ_s, and σ_ϵ, which represent the mean of the event effect, the standard deviation of the event effect, and the standard deviation of stock price noise. The top-left subfigure repeats the exercise in Figure 1. The top-right subfigure repeats the exercise in Figure 2. The bottom-left and bottom-right subfigures repeat the exercise in Figure 1 for different values of σ_s and σ_ϵ, respectively.

FIGURE 4: DAILY OPTION VOLUME FOR SAMPLE FIRMS



This figure depicts the daily option volume for all options for all firms in our sample. The solid line represents total daily trading volume for all options for all firms in our sample. The dashed line represents the call option volume, while the dot-dashed line represents the put option volume. Option trade data is from the OptionMetrics database.

FIGURE 5: THE ESTIMATED PROBABILITY OF TCJA PASSAGE

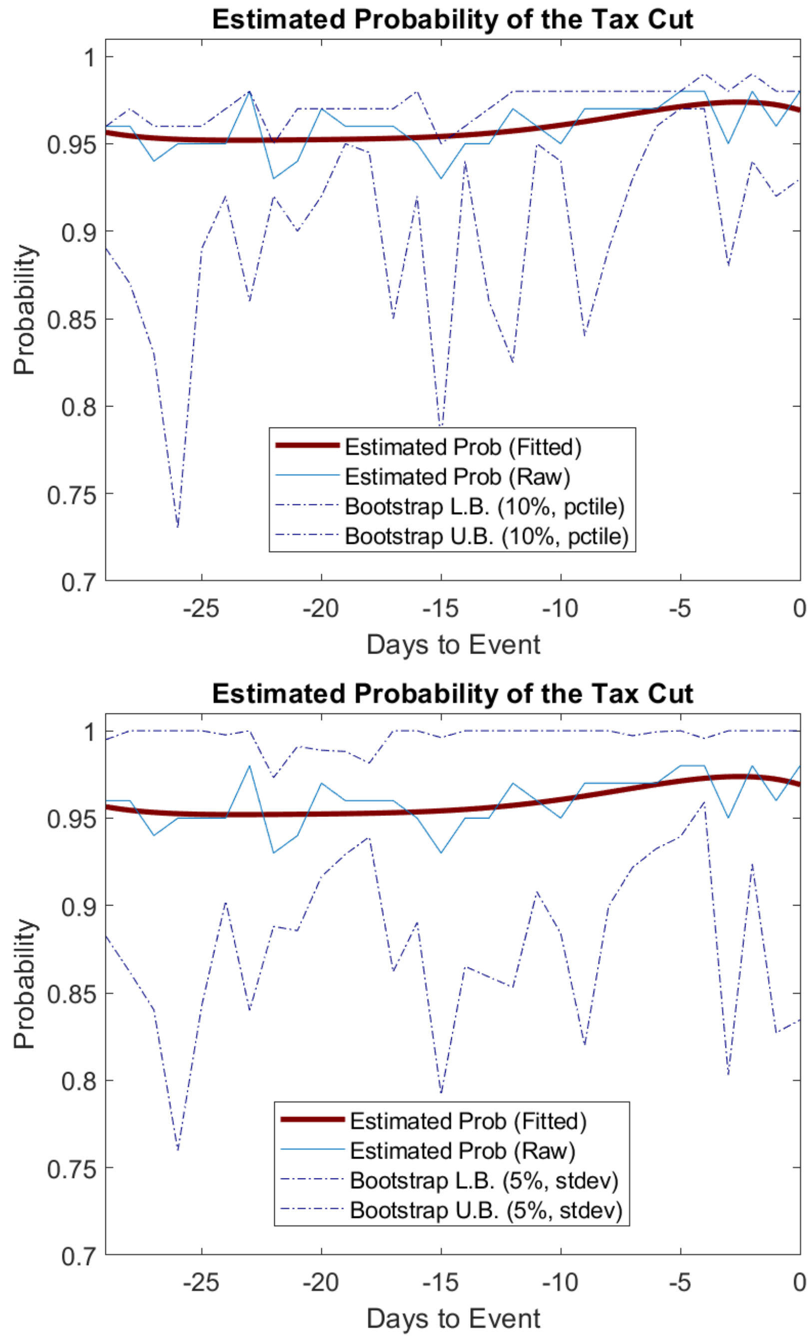


Figure 5 shows the estimated probability of the passage of the TCJA. The horizontal axis corresponds to the number of trading days between the event (December 22, 2017) and the data date, t. For both subfigures, the solid blue line depicts the estimated event probability q_t before the passage of TCJA, where only data from that particular day is used in its estimation. The solid red line is the fitted value of estimated event probability using smoothing spline interpolation, which aggregates information across days. The dashed blue lines in the top subfigure display the top and bottom 10^{th} percentiles using the bootstrap sample. The dashed blue lines in the bottom subfigure delineate the 90% confidence interval obtained using the bootstrap sample standard deviation.

Table 1: List of sample firms with market capitalization, call option volume, and industry group

Name	Average daily call volume	Market cap ($MM)	SIC2 group
APPLE INC	261788.30	790050.13	Electronic & Other Electric Equipment
BANK OF AMERICA CORP	227064.60	303681.16	Depository Institutions
MICRON TECHNOLOGY INC	151133.80	35550.64	Electronic & Other Electric Equipment
GENERAL ELECTRIC CO	134709.80	151475.97	Conglomerate
FACEBOOK INC	130835.90	512792.75	Business Services
ADVANCED MICRO DEVICES INC	9940.76	2071.92	Electronic & Other Electric Equipment
A T & T INC	95675.03	238700.84	Communications
NVIDIA CORP	71702.85	148954.80	Electronic & Other Electric Equipment
INTEL CORP	70861.27	216351.92	Electronic & Other Electric Equipment
TESLA INC	67707.01	52554.95	Transportation Equipment
MICROSOFT CORP	62581.75	531312.44	Business Services
NETFLIX INC	62356.93	83194.12	Motion Pictures
TWITTER INC	58536.18	17933.12	Business Services
CITIGROUP INC	52913.8	191226.92	Nondepository Institutions
QUALCOMM INC	51401.65	76412.16	Electronic & Other Electric Equipment
AMAZON.COM INC	50022.58	566023.50	Miscellaneous Retail
BAUSCH HEALTH COMPANIES INC	47908.05	7246.17	Chemical & Allied Products
GENERAL MOTORS CO	45226.69	57386.00	Transportation Equipment
JPMORGAN CHASE & CO	43622.73	366301.59	Depository Institutions
FREEPORT MCMORAN INC	43530.30	27454.08	Metal Mining
TEVA PHARMACEUTICALS	40405.23	19272.15	Chemical & Allied Products
WALMART INC	36441.19	314683.19	General Merchandise Stores
CISCO SYSTEMS INC	35655.68	156715.34	Industrial Machinery & Equipment
WELLS FARGO & CO	35181.57	296774.41	Depository Institutions
SQUARE INC	32631.31	13701.38	Business Services
C B S CORP	31366.14	22597.00	Communications
COMCAST CORP	29656.43	186012.55	Communications
MACYS INC	27328.83	7908.65	General Merchandise Stores
GILEAD SCIENCES INC	26517.76	93705.117	Chemical & Allied Products
UNITED STATES STEEL CORP NEW	25735.71	6166.03	Primary Metal Industries
VERIZON COMMUNICATIONS INC	25576.61	215926.67	Communications
TIME WARNER INC NEW	23795.57	71346.60	Communications
DISNEY WALT CO	21265.29	149530.69	Communications
APPLIED MATERIALS INC	21029.74	59815.80	Industrial Machinery & Equipment
ORACLE CORP	20978.34	186739.84	Business Services
UNION PACIFIC CORP	20808.68	104721.10	Railroad Transportation
CELGENE CORP	19270.86	79240.55	Chemical & Allied Products
CHESAPEAKE ENERGY CORP	19169.32	3589.7083	Oil & Gas Extraction
HOME DEPOT INC	18810.10	232642.2	Building Materials & Gardening Supplies
INTERNATIONAL BUSINESS MACHS CORP	18485.36	141480.7	Business Services
GOLDMAN SACHS GROUP INC	18471.11	99076.164	Security & Commodity Brokers
EXXON MOBIL CORP	18442.48	354549.97	Petroleum & Coal Products
PFIZER INC	17279.66	216559.38	Chemical & Allied Products
VISA INC	16858.28	239315.77	Depository Institutions
STARBUCKS CORP	16359.45	239315.77	Eating & Drinking Places
BARRICK GOLD CORP	16170.36	16880.37	Metal Mining
UNITED CONTINENTAL HOLDINGS INC	16082.52	19341.98	Transportation by Air
DELTA AIR LINES INC	15626.81	39603.09	Transportation by Air
BOEING CO	15018.13	174303.31	Transportation Equipment

Name	Average daily call volume	Market cap ($MM)	SIC2 group
NIKE INC	14807.01	114951.80	Rubber & Miscellaneous Plastics Products
CATERPILLAR INC	14547.30	94173.91	Industrial Machinery & Equipment
J C PENNEY CO	14527.58	1157.52	General Merchandise Stores
COCA COLA CO	14227.10	195402.92	Food & Kindred Products
MERCK & CO	14107.95	151738.36	Chemical & Allied Products
AMERICAN AIRLINES GROUP INC	14003.07	24740.68	Transportation by Air
ALPHABET INC	13827.38	731884.44	Business Services
CENTURYLINK INC	13193.02	17833.74	Communications
MARATHON OIL CORP	13126.21	14390.50	Oil & Gas Extraction
RITE AID CORP	12780.18	2102.62	Miscellaneous Retail
BLACKBERRY LTD	12553.10	6515.96	Electronic & Other Electric Equipment
SALESFORCE.COM INC	12152.13	83137.56	Business Services
ELECTRONIC ARTS INC	11902.96	37144.30	Business Services
CHIPOTLE MEXICAN GRILL INC	11861.91	8100.36	Eating & Drinking Places
WESTERN DIGITAL CORP	11809.77	26048.40	Industrial Machinery & Equipment
M G M RESORTS INTERNATIONAL	11653.05	18907.96	Amusement & Recreation Services
SCHLUMBERGER LTD	11391.93	93263.24	Oil & Gas Extraction
SPRINT CORP NEW	11371.16	19544.40	Communications
HALLIBURTON CO	11330.84	42663.51	Oil & Gas Extraction
JOHNSON & JOHNSON	11237.08	374802.41	Chemical & Allied Products
MORGAN STANLEY	11166.05	93820.92	Security & Commodity Brokers
KROGER COMPANY	11097.44	26413.20	Food Stores
WEATHERFORD INTL PLC	10890.79	4140.81	Oil & Gas Extraction
LOWE'S COS INC	10615.52	86925.90	Building Materials & Gardening Supplies
MCDONALD'S CORP	10481.8	136680.50	Eating & Drinking Places
ACTIVISION BLIZZARD INC	10467.48	47965.21	Business Services
BRISTOL-MYERS SQUIBB CO	10229.38	100070.20	Chemical & Allied Products
ABBVIE INC	10006.11	153975.00	Chemical & Allied Products
TRANSOCEAN LTD	9972.15	4178.41	Oil & Gas Extraction
EBAY INC	9672.068	38834.46	Business Services
CHEVRON	9414.29	238449.60	Petroleum & Coal Products
ALTRIA GROUP	9314.74	135768.90	Tobacco Products
OVERSTOCK.COM INC	9057.30	1757.06	Miscellaneous Retail
BRITISH PETROLEUM PLC	8959.34	138820.40	Petroleum & Coal Products
WYNN RESORTS LTD	8928.98	17365.78	Amusement & Recreation Services
UNITED PARCEL SERVICE INC	8841.19	102349.90	Trucking & Warehousing
BLACKSTONE GROUP L P	8826.52	20149.99	Security & Commodity Brokers
SEAGATE TECHNOLOGY PLC	8837.90	11307.25	Industrial Machinery & Equipment
WALGREENS BOOTS ALLIANCE INC	8528.18	83443.70	Miscellaneous Retail
MACERICH CO	8376.49	9260.49	Holding & Other Investment Offices
C S X CORP	8280.11	48950.70	Railroad Transportation
ANADARKO PETROLEUM CORP	8113.05	28472.11	Oil & Gas Extraction
ADOBE INC	8087.35	89149.31	Business Services
MONDELEZ INTERNATIONAL INC	7942.79	63692.22	Food & Kindred Products
ALLERGAN PLC	7777.47	54014.12	Chemical & Allied Products
KINDER MORGAN INC	7671.50	40063.18	Electric, Gas, & Sanitary Services
MASTERCARD INC	7288.50	159533.40	Depository Institutions
PEPSICO INC	7142.62	170286.40	Food & Kindred Products
SHOPIFY INC	6934.33	10087.68	Business Services
M B I A INC	6854.60	669.66	Insurance Carriers
CAESARS ENTERTAINMENT CORP	6758.14	8652.60	Amusement & Recreation Services

Firms are sorted based on their average call option volume during the fourth quarter of 2017. We also provide their size measured by market capitalization, and industry classification according to the SIC.

TABLE 2: THE IMPACT OF THE PASSAGE OF THE TCJA

$\widehat{S_u/S_d} - 1$	RET	CAR[-3,0]	CAR[-5,0]	CAR[-7,0]	CAR[-10,0]
12.36%	0.68%	0.96%	0.89%	1.09%	2.29%
(0.96%)	(0.18%)	(0.37%)	(0.41%)	(0.46%)	(0.77%)

In this table, we report the estimated average impact of the passage of the TCJA on firm stock returns using both our model and traditional event study methods. $\widehat{S_u/S_d} - 1$ is our estimate of the event return that impounds anticipatory effects, where we calculate this quantity firm by firm and then average. S_u represents the stock price after the event, and S_d represents the counterfactual stock price in the absence of both the event and anticipation of the event. RET is the holding period return on the event date. CAR$[-x, 0]$ is the average of the cumulative abnormal returns of firm stock prices within the window of $[-x, 0]$, where $x = 3, 5, 7, 10$. The cumulative abnormal returns are computed based on Fama-French three-factor model, where abnormal returns are computed as the excess return on the stock minus the sum of its factor exposures times the factor returns. Standard errors reported in parentheses are computed using bootstrapping method.

TABLE 3: THE CORRELATION BETWEEN THE ESTIMATED AVERAGE IMPACT BASED ON OUR MODEL AND TRADITIONAL METHODS

$S_u/S_d - 1$	RET	CAR[-3,0]	CAR[-5,0]	CAR[-7,0]	CAR[-10,0]
1.000	0.135	0.183	0.133	0.197	0.233
	(0.113)	(0.094)	(0.107)	(0.110)	(0.124)

In this table, we report the correlation between our estimated impact of the passage of the TCJA, $S_u/S_d - 1$, and measures of stock-return gains from traditional event study methods. RET is the holding period return on the event date. CAR$[-x, 0]$ is the average of the cumulative abnormal returns of firm stock prices within the window of $[-x, 0]$, where $x = 3, 5, 7, 10$. The cumulative abnormal returns are computed based on the Fama-French three-factor model, where abnormal returns are computed as the excess return on the stock minus the sum of its factor exposures times the factor returns. Standard errors reported in parentheses are computed using a bootstrap.

Table 4: Testing differences in firm characteristics of relative winners and losers from the passage of the TCJA

Firm Attributes	Differences	t-statistics	Lasso predictors
R&D Intensity	0.021***	(4.05)	
Patent Count	0.498***	(5.37)	X
Total Citations	0.476***	(4.95)	X
Total Originality	0.345***	(4.53)	X
Total Generality	0.201***	(4.09)	X
Average Citations	-0.008	(-0.60)	X
Average Originality	-0.018***	(-3.41)	
Average Generality	-0.009**	(-2.16)	X
Tangibility	-0.029***	(-2.98)	X
Cash Effective Tax Rate	-1.616***	(-3.59)	X
Reinvested Foreign Earnings/Assets	-0.048***	(-5.14)	X
Net Tax Assets/Assets	-0.023***	(-4.50)	X
Cash/Asset	-0.006*	(-1.76)	
Sales Growth	0.040***	(6.44)	X
Asset Growth	0.022***	(3.39)	
Employment Growth	0.011**	(2.51)	
Sales Growth (DHS)	0.033***	(6.53)	
Asset Growth (DHS)	0.015***	(3.09)	
Employment Growth (DHS)	0.011***	(2.86)	
Market to Book Ratio	-0.088	(-1.64)	X
Size (log(assets))	0.232***	(3.56)	X
Leverage	-0.012*	(-1.95)	
Asset Maturity	-0.727***	(-4.13)	X
Profitability	-0.014***	(-3.95)	
Return on Assets (ROA)	-0.015***	(-4.97)	
Return on Equity (ROE)	-0.057***	(-3.48)	X
Advertising Expenses	-0.007***	(-5.54)	

The first two columns of Table 4 contain pairwise t-tests of the null hypothesis that a firm characteristic is the same in groups with high versus low expected gain, $S_{i,u}/S_{i,d}$, from the passage of the TCJA. Three, two, and one stars indicate significance at the 1%, 5%, and 10% level, respectively. The third column presents the results of a LASSO fit of $S_{i,u}/S_{i,d}$ on these characteristics, with the penalty parameter chosen by cross-validation to minimize mean squared prediction error.

Appendix A. Data Construction

Variable List

We summarize the construction of our variables below.

R&D Intensity = xrd/sales
The R&D to Sales ratio averaged over the preceding five years, 2012 to 2016

Patent Count
The number of successful patent applications by the firm averaged over the preceding five years, 2012 to 2016

Total Citations
The number of citations received by the firm's new patents averaged over the preceding five years, 2012 to 2016

Total Originality
The total dispersion of the patents cited by the firm's new patents across technology sectors following Hall et al. (2001) and averaged over the preceding five years, 2012 to 2016

Total Generality
The total dispersion of the patents citing a firm's new patents across technology sectors following Hall et al. (2001) averaged over the preceding five years, 2012 to 2016

Average Citations
The average citations per patent received by the firm's new patents averaged over the preceding five years, 2012 to 2016

Average Originality
The average originality per patent of the firm's new patents averaged over the preceding five years, 2012 to 2016

Average Generality
The average generality per patent of the firm's new patents averaged over the preceding five years, 2012 to 2016

Tangibility = ppent/at
The ratio of the firm's tangible to total assets averaged over the preceding five years, 2012 to 2016

Sales Growth = (sales - sales_{-1})/sales_{-1}
The year-on-year growth in sales averaged over the preceding five years, 2012 to 2016

Asset Growth = $(at - at_{-1})/at_{-1}$
The year-on-year growth in assets averaged over the preceding five years, 2012 to 2016

Employment Growth= (emp - emp_{-1})/ emp_{-1}
The year-on-year growth in the number of employees averaged over the preceding five years, 2012 to 2016

Cash Effective Tax Rate =100* (txpd/(pi-spi))
The ratio of the firm's tax to its net income averaged over the preceding five years, 2012 to 2016

Indefinitely Reinvested Foreign Earnings / Assets = X / at / 1000000,
averaged over the preceding five years, 2012 to 2016. X is from the Audit Analytics database and is foreign earnings that are to be indefinitely held outside the U.S.

Net Tax Assets / Assets = (X)/1000000/at,
averaged over the preceding five years, 2012 to 2016. X is from the Audit Analytics database and is the total amount of deferred tax assets, net of valuation allowance and deferred tax liabilities.

Cash / Assets = ch /at
The ratio of cash holdings to book assets averaged over the preceding five years, 2012 to 2016

Market-to-Book = (csho$\times$prcc_c+at-ceq)/at
The ratio of the firm's market value to book value averaged over the preceding five years, 2012 to 2016

Firm Size = log(at)
The log of firm book assets averaged over the preceding five years, 2012 to 2016

Leverage= (dltt+dlc)/at
The ratio of total debt to total assets averaged over the preceding five years, 2012 to 2016

Asset Maturity = act/at+ppent/at$\times$ppent/dp
The average maturity of short-term and long-term assets weighted by their proportion of total assets following Benmelech (2006) and averaged over the preceding five years, 2012 to 2016

Profitability = oibdp/at
The net income of the firm averaged over the preceding five years, 2012 to 2016

ROA = ni/at
The firm's return on assets averaged over the preceding five years, 2012 to 2016

ROE = ni / ceq
The firm's return on equity averaged over the preceding five years, 2012 to 2016

Advertising Expenses = xad /sales

The firm's advertising expenditure normalized by sales averaged over the preceding five years, 2012 to 2016

Treatment of Patent Data

On January 1, 2013, the USPTO moved from using the United States Patent Classification (USPC) system to the Cooperative Patent Classification (CPC) system, a jointly developed system with the European Patent Office (EPO). Unlike the older USPC system, under CPC, patents are not assigned a unique primary technology class, but a large distribution of multiple classes. This change requires the development of new methods to account for technology class citation bias and to generate measures like originality and generality that capture the dispersion of inbound and outgoing citations in the technology space.

USPTO assigns multiple 4-digit CPC technology classes to each patent, and unlike earlier patent classification schemes, none of them are indicated as the primary technology class. This means the location of a patent in the technology space can no longer be defined by a single technology class, which creates challenges in constructing innovation measures used in previous studies (Hall et al. (2001), Acemoglu et al. (2020)).

To overcome this challenge, we represent the location of a patent in the technology space as a vector rather than a single technology class. To do so, we focus on the first digit of the CPC code, which can take nine different values, and represents broad categories such as "Physics," "Electricity," or "Human Necessities." Next, for each patent, we calculate what percentage of the technology classes associated with the patent fall into the nine separate bins. For instance, a patent with two distinct technology classes in "Physics" and three technology classes in "Electricity" would be classified as 40% in "Physics" and 60% in "Electricity," and its technology class vector would have zeroes for all the remaining seven categories.

When we calculate the counterparts of the originality and generality metrics developed in Hall et al. (2001). Instead of calculating the shares of patents in a distinct technology class, we first sum up the 9-dimensional vectors for all the cited/citing patents, normalize it by the sum across all 9 dimensions, and only then calculate the square of each element. The rest of the construction is as in the original Hall et al. (2001) study.

To account for truncation and technology class biases in constructing patent citation measures, we again make use of the technology class vector of each patent. To remove these biases, we must make sure a patent's raw citation numbers are compared against patents that are applied for in the same year and that are technologically comparable. To do so, we first calculate the average number of citations for each year and the nine broad CPC categories indicated by the first digit of the CPC code. For all patents that are applied for in a given year and that are associated with one of the nine categories, we calculate the weighted average citations, where the weights correspond to the technology class share of the patent in its technology class vector. Once these are calculated for each category and year, we calculate a normalized citation count for each patent by dividing its raw citation count by the weighted average across the average citations for the nine categories in its application year, weighted by the patent's technology class vector. This procedure makes it possible to compare a patent's citations to other patents that are technologically similar and applied for in the same year.