The Impact of Policy Uncertainty on Entrepreneurial Experimentation

Yang Fan* Lubomir Litov† Mishita Mehra‡ Mu-Jeung Yang§

February 2025

Using a Difference-in-Difference identification strategy, we find evidence that policy uncertainty has a negative impact on high-growth startups in terms of patent applications and VC funding. The mechanisms driving our overall findings can be understood from the perspective of a real options view of entrepreneurial experimentation and VC investment. Responses are more pronounced for startups that were unexpectedly denied new H-1B workers despite winning the H-1B visa lottery for new hires, which may signal unpredictability of high-skill immigration policy. Furthermore, for startups that did not apply for skilled immigrant labor, we do not find adverse effects of policy uncertainty on experimentation.

Keywords:

JEL Codes: L20; L26; L29; D21; D23

*Colby College

†Price College of Business, University of Oklahoma

‡Robins School of Business, University of Richmond

§Leeds School of Business, University of Colorado at Boulder

1. Introduction

A fundamental aspect of high-impact entrepreneurship is the importance of experimentation in the face of uncertainty about potentially market-disrupting innovations (Kerr, Nanda, and Rhodes-Kropf, 2014). From a finance perspective, key experiments such as entrepreneurial patent applications can be understood as irreversible investments (Gans, Stern, and Wu, 2019) providing informative signals about the expected present value and risk of further investments[1](Agrawal, Camuffo, Gambardella, Gans, Scott, and Stern, 2024; Kerr et al., 2014; Nanda and Rhodes-Kropf, 2016). The irreversible investment nature of experiments suggest that they can be analyzed as real options (Kerr et al., 2014; Nanda and Rhodes-Kropf, 2016), a popular framework in corporate finance to understand investment under uncertainty (Abel and Eberly, 1994; Dixit and Pindyck, 1994), including the effects of policy uncertainty (Baker, Bloom, and Davis, 2016; Gulen and Ion, 2016). In this context, many researchers have argued that the most effective way for governments to boost entrepreneurial experimentation is to provide favorable policy regimes or "table setting" conditions (Kerr et al., 2014; Lerner, 2009)[2]. However, causal evidence on how different policy regimes impact experimentation of high-impact entrepreneurs is rare.

To investigate this question, we analyze the first election of Donald J. Trump to the US Presidency in 2016 as a natural experiment for an unexpected policy regime shift. As we argue in more detail below, this led to strong shifts in terms of regulation, immigration policy as well as economic policy uncertainty, all of which are important ways in which policy regimes impact entrepreneurial experimentation. The analysis of the policy consequences

[1]Kerr et al. (2014) and Nanda and Rhodes-Kropf (2016) describe experiments as low-cost signals to assess the potential future profitability of opportunities before a full investment commitment. Filing a utility patent, costing around $6,000, primarily for attorney fees, offers such a low-cost way to externally evaluate the worth of a new product concept in terms value and novelty. If granted, a patent will increase the future profit and VC funding chances of a startup. The costs of patent applications are sunk and not recoverable, which makes patent applications an irreversible investment. Additionally, R&D costs in the innovation underlying the patent application can be much larger than the costs of the patent application itself, so much that a patent application is likely to become an irreversible investment, (Gans et al., 2019).

[2]For example, Stern (2006) writes that "a favorable environment for entrepreneurship and a high level of economic experimentation go hand in hand."

of the 2016 election offers several advantages. First, Trump's 2016 election victory was surprising to most observers, which rules out prior preparation of entrepreneurs or venture capitalists. Second, this regime shift was not in response to a ongoing recession, thereby avoiding the issue that the recession is the actual driver of lower entrepreneurial experimentation and VC investment (Gompers, Kovner, Lerner, and Scharfstein, 2008; Nanda and Rhodes-Kropf, 2013). Third, policy changes were not in anticipation of a recession either, which is the typical endogeneity problem in the analysis of other government policy changes, such as tax reform (Romer and Romer, 2010). And indeed there was no recession until the unanticipated and short-lived COVID-19 pandemic recession in the last year of the first Trump administration in 2020.

Theoretically, the impact of the first Trump presidency on startup experimentation is ambiguous due to several simultaneous policy shifts. Starting with one of his first executive orders[3], Trump pursued a massive program of deregulation, which some prominent VC investors, such as Ashu Garg argued would boosts startups across many industries (Garg, 2017). Such a prediction can be understood from the perspective of entrepreneurial experimentation as expressed by Kerr et al., (2014), who write that "Governments are more likely to facilitate effective entrepreneurship if they work to reduce the costs of experimentation in general. What we have in mind here is (...) a careful consideration of the broader regulatory framework, including labor laws and requirements with which new entrants need to comply (..)". More broadly, empirical work on entrepreneurial finance, going back at least to Klapper, Laeven, and Rajan (2006), has documented that deregulation often boosts entrepreneurship and startup outcomes.

Other policy shifts, suggest potentially negative effects of the policy regime during the first Trump administration on startup experimentation. Among these is the first Trump administration's anti-immigration policy which increased the effective costs of hiring skilled

[3]In the first week of his presidency, Trump signed executive order 13771, which was called "Reducing Regulation and Controlling Regulatory Costs" and which required of all regulatory agencies that "for every new regulation issued, at least two prior regulations be identified for elimination, and that the cost of planned regulations be prudently managed and controlled through a budgeting process"

immigrants in a startup ecosystem that strongly depends on global talent (Kerr, 2013).

Additionally, observers such as Nicholas Bloom (see Andrews, 2018) noted that Trump "changes his mind often and changes his advisors even faster. That makes him the most unpredictable president in recent times", which directly contributed to high levels of economic policy uncertainty during the first Trump administration. Economic policy uncertainty in turn may depress entrepreneurial experimentation from a real options perspective – similar to the negative impact of economic policy uncertainty on corporate investment (Baker et al., 2016; Gulen and Ion, 2016). In this context, the simultaneous presence of some policies potentially benefiting tech startups – such as deregulation and tax cuts – as well as some policies that may be costly for startups – such as restrictive high-skill immigration policies – suggest that policy uncertainty during the first Trump administration could be close to a pure uncertainty shock. As is well-recognized in the literature on uncertainty shocks, such as Bloom (2009), it is often difficult to separate times of higher economic uncertainty from bad news, which can be an empirical challenge because many theoretical predictions rely on uncertainty shocks as mean-preserving spreads. The natural experiment in this paper is more plausibly a persistent policy uncertainty shock that comes close to be such a mean-preserving spread.

Using a Difference-in-Difference (DiD) approach, we find that in the wake of the first Trump administration, US startups saw a disproportionate, systematic and sustained decline in patent applications, compared to Canadian startups. Furthermore, large US startups experienced a significant decline in average VC funding compared to their Canadian counterparts. These results are consistent with the a real-options view of entrepreneurial patent applications and VC investments, both of which are irreversible investments. The results also suggest that any benefits of deregulation for startups have been more than offset in the aggregate by costs imposed through high-skill immigration policy.

We then provide a deeper analysis of the potential mechanisms that might drive the overall effects, starting with immigration policy. The H-1B visa program is the main source of high-skill global talent for US startups and the institutional details of this visa program further facilitate identification. It is well-known that the allocation of H-1B visas in recent years has been randomized, due to the number of annual visa applications far exceeding the annual quota (Chen, Hshieh, and Zhang, 2021; Dimmock, Huang, and Weisbenner, 2022). A less appreciated fact is that winning the H-1B visa lottery still does not entitle an employer to hire an H-1B worker, since the executive branch as represented by the US Citizen and Immigration Service (USCIS), and importantly, appointed by the current presidential administration, has discretion to deny H-1B visas to winners of the H-1B lottery. We document a substantial increase in such discretionary denials to H-1B visa lottery winners during the Trump administration. We then measure variation in surprising H-1B visa denials on the start-up level, differentiating between denial of new hires and denial of continuations for current H-1B workers. Importantly, the denial of new hires likely implies only a small direct search cost, but it is a strong signal about the unpredictability of immigration policy, which in turn disproportionately increases the perceived uncertainty of high-skill immigration policy. We find that startups that were denied H-1B visa workers, despite winning the visa lottery exhibit particularly large declines in patent applications. Effects are similar for unexpected denials of new hires and denials of continuing H-1B workers, which suggest that perceived uncertainty and not the loss of firm-specific human capital is driving the effect.

In contrast to high-growth startups using H-1B visa workers, startups never applying for such workers prior to 2016, exhibit significant increases in patent applications. These results suggests that our main effects are neither driven by more patent applications in the control group of Canadian firms, nor that general economic policy uncertainty is driving our effects. Instead, economic policy uncertainty directly coming from uncertainty of the high-skill immigration policy during the first Trump administration is driving the decline in patent applications and VC funding.

Our study contributes to at least three distinct literatures. First, a fast-growing recent literature in finance and entrepreneurship on “Bayesian Entrepreneurship" (Agrawal et al., 2024), analyzes how initially over-optimistic entrepreneurs use experiments to learn, de-bias and persuade other important stakeholders such as VC investors. Indeed, Gans et al. (2019) argue that key experiments incur irreversible costs, much in the same way we model experimentation in our theory. However, this literature has mostly left unaddressed the question of how policy uncertainty empirically affects entrepreneurial experimentation.

Second, a related but separate literature in economics and finance analyzes entrepreneurial experimentation as a form of real option exercise (Kerr et al., 2014). Broadly, studies in this literature differ on whether they consider startup creation and VC funding as the real option (Manso, 2016; Nanda and Rhodes-Kropf, 2016; Howell, 2021; Gottlieb et al., 2022) or analyze experimentation within existing startups (Camuffo et al., 2019; Agarwal et al., 2021; Konings et al., 2022) for which experimentation can create real options such as patents (Bloom and Van Reenen, 2002). While much of this literature has focused on the value of the option to abandon investments at early stages in response to experimentation (Nanda and Rhodes-Kropf, 2016), we complement this work by emphasizing the implications of increased uncertainty on delaying experimentation itself. In other words, we model not only the VC investment as an irreversible investment, but also endogenize experiments as irreversible investments, which in turn implies the analysis of a compound real option.

Third, there is a large literature in corporate finance on the real options analysis of uncertainty shocks, (Baker et al., 2016; Bloom, 2009; Gulen and Ion, 2016; Kim and Kung, 2017; Sanford and Yang, 2022), which primarily focuses on delayed investments in response to higher uncertainty. The study from this literature closest to ours is Bhattacharya, Hsu, Tian, and Xu (2017), who estimate the effect of political uncertainty (close elections) on patent grants in a cross-country-cross-industry sample. Our results complement the analysis in Bhattacharya et al. (2017) by documenting the effects of policy uncertainty as opposed to political uncertainty. The effects of political uncertainty as in Bhattacharya et al. (2017) is concentrated around close elections, while policy uncertainty as in Baker et al. (2016) and Gulen and Ion (2016) exhibits persistent effects even after elections as election winners (re-)shape economic policy. Our specific policy focus in this paper is policy uncertainty in the area of high-skill immigration policy. Additionally, we focus on patent applications of existing US startups, instead of the general inventors in Bhattacharya et al. (2017), which include individuals as well as corporations.

2. Theory

We use a stylized two-period model of staged irreversible investments and experimentation, building on the staged VC investment model developed in Kerr et al. (2014) and especially Nanda and Rhodes-Kropf (2016). In this model, a VC's investment into a startup is irreversible investment, which implies that informative experiments can increase the option value of delaying the investments. Our key extension is to endogenize the choice of experimentation as an additional irreversible investment as in Gans et al. (2019), which may benefit from new information about uncertain economic policy. The presence of two irreversible investment problems will imply the structure of a compound option, which is challenging to analyze in a continuous time framework, so we stay within the two-period model of Nanda and Rhodes-Kropf (2016).

Entrepreneurs are considering to make an (VC-financed) irreversible investment in their venture. This irreversible investment can be through of as hiring employees or investing in firm-specific software or IP to to scale up the business. The irreversible costs of these investments will be given by C_I and are for simplicity fully irreversible[4]. We denote the NPV of benefits from this investment by B, which may be state-contingent. There are two types of random variables, that make the benefits of investment state-contingent. On the one hand, there is a risky aggregate state $S \in \{L, H\}$, which formalized policy uncertainty and for which $S = H$ is a high state (e.g. state in which advantages of de-regulation dominate) and $S = L$ is a low state (e.g. state in which other policies reduce demand). The probability

[4]For example, code to scale-up a specific website and allow for millions of users to access it simultaneously is almost worthless for other website and would therefore not be sellable, or "re-deployable" in the terminology of Kim and Kung (2017).

Experimental outcome E	Exogenous state S: $S = L$	Exogenous state S: $S = H$
$E = B$	\$0	\$0
$E = G$	B_L	B_H

Figure 1

Note: Figure 1 summarizes the interaction of policy uncertainty (or aggregate risk) with experimental uncertainty in the model.

of the high state is denoted by $p = P(S = H)$. On the other hand, entrepreneurs can run experiments, such as generating prototypes (Ries, 2011), RCTs to validate the product-market fit or applying for a patent. Let $E \in \{B, G\}$ denote the possible states for the product with $E = B$ signaling a bad product and $E = G$ a good product. The probability that the experiment is successful and signals a good product is given by $q = P(E = G)$.

For simplicity we assume that if the experiment indicates a bad product, there will be no benefit from the irreversible investment. At the same time, if the experiment is successful ($E = G$), then the benefits from the irreversible investment will still depend on the aggregate state. We summarize the interaction of policy uncertainty risk and experimental risk in Figure 1.

Like the costs of investment, the cost of experimentation are irreversible and we denote the amount of irreversible experimentation costs as C_E. The irreversibility of the experimentation costs come from at least two sources. On the one hand, direct fees for patent applications and prototypes are typically not refundable. On the other hand, development costs for the underlying intellectual property as well as associated intangible capital (Ewens, Peters, and Wang, 2024), are often costs of wages for inventors and R&D personnel, and are therefore also not refundable. Figure 2 summarizes the timing of the decisions in our

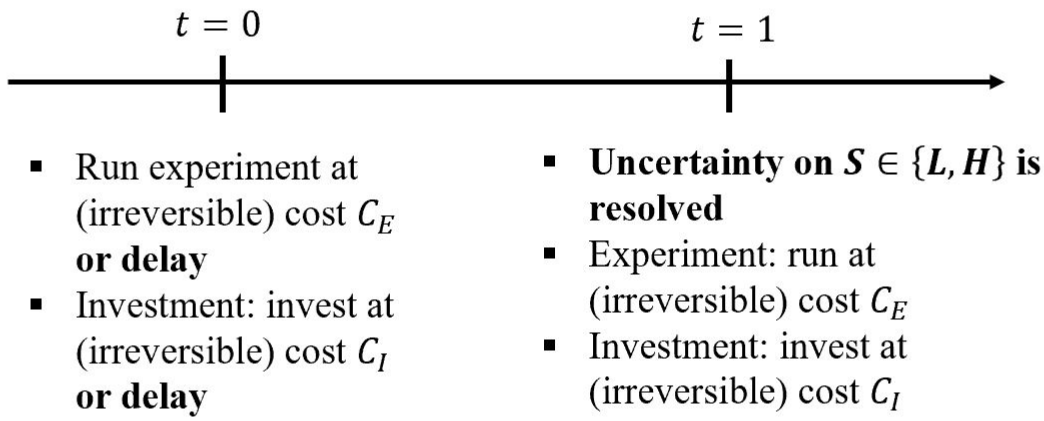


Figure 2

Note: Figure 2 summarizes the timing of the irreversible decisions made for experimentation and investment.

model. Broadly, both the investment and experimentation decisions could be made either before or after the realization of the aggregate policy uncertainty state S. This potential delay in both decisions adds an option value of delay to both decisions, since delay implies potentially better state-contingent decisions after the aggregate state has been realized. In other words, entrepreneurs are faced with a compound real option problem.

To highlighting the key theoretical prediction, we assume that

$$B_H > B_0 > C_I > B_L \tag{1}$$

This assumption makes sure that does not automatically generate into immediate exercise of options or no exercise of options, irrespective of aggregate policy uncertainty state S. Additionally this assumption simplifies the state-contingent decision at $t = 2$, as the startup will now only invest in the good state $S = H$ and not in the bad state $S = L$.

Our focus in this paper is on the optimal experimentation decision, for which the value function can be written as

$$V^e = \max\left\{V_1^e, V_2^e\right\} \tag{2}$$

with V_1^e as the value of immediate experimentation in $t = 1$, and V_2^e as the value of delaying experimentation as well as investment. Immediate experimentation will lead to value of the startup of

$$V_1^e = q \cdot \max\left\{B_0 - C_I, \left(\frac{1}{1+r}\right) \cdot p \cdot (B_H - C_I)\right\} - C_E \tag{3}$$

This value is the consequence of the expected benefit of experimentation only being realized with probability q, while the costs of experimentation C_E have to be paid irrespective of the outcome. Then, even if the experiment is successful, the entrepreneur will have to decide whether to immediately invest to scale up the startup. Under immediate investment, the value of the startup will be given by the NPV of the investment or $B_0 - C_I$. On the other hand, if the startup delays investments by one period, it is able to only invest in the high state of the world $S = H$, which happens with probability p, leading to the term $\left(\frac{1}{1+r}\right) \cdot p \cdot (B_H - C_I)$.

In contrast, the value of delayed experimentation, in $t = 2$ is given by

$$V_2^e = \left(\frac{1}{1+r}\right) \cdot p\left[q \cdot (B_H - C_I) - C_E\right] \tag{4}$$

Delayed experimentation contrasts with immediate experimentation in two key respects. First, due to the delay, the startup will only want to run the experiment, if the realized aggregate state of the world is high, i.e. $S = H$. Otherwise, since $B_L < C_I$, making the investment would destroy value. This lowers the expected costs of experimentation, since the experimental costs C_E only have to be run in the high aggregate state, which only happens with probability p. Second, realization of the high aggregate state still allows the entrepreneur to invest to scale up the startup, thereby generating an expected benefit of

$q \cdot (B_H - C_I)$ if the experiment succeeds.

In this model, the key shock is an uncertainty shock, defined as a mean preserving spread of the benefits of investment. Formally, this can be defined as an increase in B_H and a reduction in B_L, such that $p \cdot B_H + (1-p) \cdot B_L = B_0$.

Proposition 1 *In response to an uncertainty shock, delayed investments and delayed experimentation becomes more beneficial*[5].

Proposition 1 is the theoretical foundation for why one would expect policy uncertainty shocks to have a negative impact on entrepreneurial experimentation. The effect is akin to the option value of delayed investments, analyzed in the corporate finance literature (Gulen and Ion, 2016; Kim and Kung, 2017; Sanford and Yang, 2022), but here applied to the experimentation decision by startups. The key insight is that under irreversible investments – R&D, IP investments and specialized capital equipment such as customized computers and servers – any delay retains an option value since the startup can still decide to invest later, while any immediate investment eliminates this option value due to the costs of reselling the invested capital.

The theory is also useful to derive additional implications to strengthen identification:

Proposition 2 *In response to an uncertainty shock, delayed investments and delayed experimentation becomes disproportionately more beneficial for firms with higher (sunk) costs of investments*[6].

Proposition 2 will allow us to strengthen identification and analyze heterogeneous treat-

[5]Proof: As can be directly seen in equation (4), an uncertainty shock will directly increase the value of delaying experimentation, since the startup will only invest if the aggregate state is high and not otherwise. In other words, delay of experimentation offers a call option value which will increase if the underlying has more extreme outcomes, since losses are limited by the option not to invest. At the same time, equation (3) shows any mean mean-preserving spread will leave B_0 constant by definition, while raising B_H even in the case of immediate experimentation. In other words, even if the startup would immediately experiment, an uncertainty shock will increase the value of delaying investment.

[6]Proof: Higher irreversible investment costs C_I make investment delay more likely according to value function (3), since the NPV of immediate investment declines by $-q$, while the NPV of delayed investment only declines by $-\left(\frac{1}{1+r}\right) \cdot p \cdot q$. Given that investment costs C_I are large enough that investments are delayed in (3), experimentation will also be delayed, since the net benefit from delayed experimentation is $V_2^e - V_1^e = \left\{1 - \left(\frac{1}{1+r}\right) \cdot p\right\} \cdot C_E > 0$.

ment effects, because it makes a prediction about which startups in industries with higher irreversible investment costs, disproportionately respond to policy uncertainty shocks.

3. Empirical Methodology

At least two issues are challenging for empirical work on the causal effects of policy uncertainty on outcomes such as investment (Baker et al., 2016) and innovation (Bhattacharya et al., 2017). First, even externally policy changes can be anticipated and result in sample selection, which can affect the responsiveness of treated firms to aggregate shocks. In our context, startups planning to aggressively experiment might also be more likely to anticipate policy uncertainty and move abroad, thereby leaving a selected sample of cautiously investing startups remaining in the US. As a result, US startups might seem to experiment less during a period of policy uncertainty, but this lower experimentation might only reflect sample selection.

Second, even if anticipation effects are not an issue, a recurring challenge in the literature on policy uncertainty (Baker et al., 2016) and economic uncertainty (Bloom, 2009) is that times of high uncertainty are usually also times of bad news (Sanford and Yang, 2022). For example, the US government might be more likely to revise the tax code and cut taxes, either in response to a recession, or in anticipation of one (Romer and Romer, 2010). As a result, low experimentation and VC investments might be driven by the fact that startups experiment or invest less because of low demand during recessions (Gompers et al., 2008; Nanda and Rhodes-Kropf, 2013).

Our empirical methodology seeks to address both of these endogeneity concerns to establish credible causal effects of policy uncertainty on entrepreneurial experimentation and VC investments. Specifically, we use a Difference-in-Difference (DiD) estimation approach with US startups as the treatment group and Canadian startups as the control group and the event of the unexpected election of Donald Trump to the US Presidency in 2016. Consistent with our theoretical approach, our hypothesis is that US startups were disproportionately affected by the policy uncertainty during the first Trump administration, compared to Canadian startups. Formally, our econometric specification can be written as

$$y_{i,t} = \beta \cdot (D_i \times D_t) + \delta_i + \delta_t + \epsilon_i \tag{5}$$

where $y_{i,t}$ are outcomes for startup i at time t, D_i is a dummy which is one for all US startups and zero for Canadian startups, D_t is an event dummy that is zero before 2017 and one after and δ_i, δ_t capture a full set of firm and time fixed effects. For our main outcome, $y_{i,t}$ will be measured by the number of patent applications for startup i in year t – a variable that is zero for most startups but will be positive yet discrete for some startups. In other words, patent applications are a good example of count data, for which we will use Poisson Maximum Likelihood models with fixed effects and firm-level clustered standard errors.

The first election of Donald Trump in 2016 is a particularly attractive natural experiment since it directly addresses the two endogeneity concerns discussed. On the one hand, this event allows us to rule out anticipation effects, since the election of Trump in 2016 was seen as very unlikely. Indeed political prediction and betting markets as well as prominent election forecasters in the media consistently gave Trump chances below 30% to win the election[7]. Additionally, if startups anticipated Trump becoming elected, there should be differential trends in startup experimentation before the election – an empirically testable hypothesis in a DiD approach.

On the other hand, the first election of Donald Trump on 2016 also helps us rule out concerns about our natural experiment confounding the effects of policy uncertainty with the effects of bad news. US economic growth had been strong before 2016 and unemployment historically low for years. At the same time, the Trump administration proactively passed growth-enhancing economic policies, often argued to help startup growth and innovation,

[7]For example, state prices on PredictIt were $.2 for the event of Donald Trump being election on the evening before the actual election while 538.com predicted a 28% chance of Trump winning while the NY Times predicted a chance of 15%.

such as aggressive de-regulation (Klapper et al., 2006) and cuts in income and corporate tax rates (Djankov, Ganser, McLiesh, Ramalho, and Shleifer, 2010; Romer and Romer, 2010). In this context, contemporary discussions in the media and during the legislative process do not mention the risks of a recession but broadly focus on long-run growth opportunities, which suggests that instead of bad news, aggregate macro policy provided good news. This directly addresses potential issues of mistaking the effects of policy uncertainty with the effects of bad news – either because on an ongoing recession or because of an expected one. The fact that the US macroeconomic environment provided good news for technology entrepreneurs is also helpful in further isolating the causal mechanism driving our results. Specifically, if a uncertainty of a particular policy dimension is driving our results, then it should affect only the startups at risk of this particular policy, while all other startups should exhibit more investment and experimentation – consistent with good macroeconomic news.

Our baseline DiD approach is useful to address these endogeneity concerns, but may itself raise the concern that results might be driven not by US policy uncertainty affecting US startups but instead this uncertainty affecting Canadian startups. To address this issue, we build on our theoretical model and especially Proposition 2, according to which startups in industries with more irreversible capital investment costs should see a more pronounced effect of policy uncertainty on startup experimentation. These predictions can be tested without considering Canadian startups as the control group but only focusing on US startups in industries with more or less irreversible investments. Finding effects in this approach will therefore provide more evidence for our basic theory without the need to rely on the contrast of Canadian and US startups.

4. Data

4.1 Data Construction

The sample used in this study is the result of merging data from three different sources: startup firms from CrunchBase, patent data from the USPTO, and H-1B petitions from the United States Citizenship and Immigration Services (USCIS). This section describes the steps undertaken to merge these three data sources to obtain our final sample. We begin with a sample of US- and Canada-based startup firms obtained from the CrunchBase organizations database. This database includes information such as the firm's name, founding date, location (state, region, city, postal code, and address), current status, a short description of the firm's business, funding details, employment data, CrunchBase-specific industry category group and category list classifications, the firm's URL, and CrunchBase's unique alphanumeric identifier for the firm (org_uuid). We limit our sample to startups, defined as firms with non-missing founding dates occurring on or after 2000. The Data Appendix outlines additional details on cleaning the CrunchBase dataset.

To construct the set of H-1B petitioning firms, we use publicly available data on H-1B petitions from 2009 to 2021, obtained from the United States Citizenship and Immigration Services (USCIS) portal on the H-1B Employer Hub. This data includes the number of H-1B petitions (new and continuing) that were approved and denied, firm names, the state and ZIP codes of the filing firms, the last four digits of the firms' Tax IDs, and the firms' self-reported NAICS industry codes. Non-profit firms like universities, colleges, churches, etc who are not subject to caps in H-1B visas are dropped from the sample.

We match our sample of CrunchBase firms to all firms that apply for H-1B visas between 2009-2021. Since no unique firm identifiers exist in the H-1B data and the employer's name may vary across petitions (either in same year or across years) due to self-reporting, we first standardized employer names and then, where applicable, aggregated petitions made by the same employer in the same year. In the Data Appendix, we describe our process of standardizing the H-1B firm names.

To match H-1B employer firm names with CrunchBase startup names, we use a probabilistic record-linking technique with Stata's 'matchit', which returns all possible matching pairs along with a Levenshtein Distance-based similarity score. To ensure accurate firm matches, we verify the matches using state and ZIP code records from both the H-1B employer firms and the CrunchBase startup firms. Our final sample includes: (i) perfectly harmonized firm-name matches (similarity score = 1) with matching state data, and (ii) manually checked imperfect name matches (with a high probabilistic match score) combined with matching state and ZIP code data. For H-1B firms matched with multiple CrunchBase firms, we retained the matches with the same ZIP codes and non-missing CrunchBase information. The final CrunchBase-H-1B matched sample consists of 48,448 firm matches, of which 25,570 are confirmed startups based on founding dates on or after 2000.

Next, we turn to matching the patenting activity of our firms. We consider both patent grants and patent applications and discuss both matching procedures below. We obtain data on granted patents from the granted patent applications dataset from USPTO's PatentsView Bulk download database tables. This provides us with the application date (filing date) and unique granted patent identifier (patent_id) that will enable us to subsequently merge in the granted patent's assignee name, location, and patent classifications that we describe below.

First, the disambiguated assignee dataset matched using patent_id provides information on the assignee(s) of the patent- the organizations and individuals with an ownership interest in the patent's legal rights. However, because the USPTO treats each patent application as a singular event, tracking the same organization (or individual) across multiple patent applications can become problematic if the organization's name is recorded differently each time, leading to different assignees being recorded. To address this, the USPTO applies a disambiguation algorithm that tracks the patenting activity of the same assignee over time. This algorithm unifies assignee names such as 'International Business Machines' and 'IBM Corp,' assigning the patenting activity of both recorded assignees to the same entity.

Next, the disambiguated patent assignee dataset is merged with several other PatentsView data files, including the granted patent's Cooperative Patent Classification (CPC) using patent_id and location data associated with the disambiguated assignee using location_id. However, since patents may be filed by the same assignee in different locations, there is no unique crosswalk between patent assignees and locations. As a result, we retain all unique assignee-state combinations, which will be used later in our matching algorithm with the CrunchBase-H1B set of firms.

We limit the sample of assignees to US and Canadian corporations and include only patents with filing dates after 2010. This aligns with the patenting activity of Crunchbase startup firms during our pre- and post-policy years of 2013-2019. To match the disambiguated assignee organization names to the CrunchBase-H1B set of firms, we use the same name standardization algorithm that was previously applied to CrunchBase and H1 employer firms. Since the goal is to match patent assignees with our sample of harmonized CrunchBase and H1 employer firms, we only match harmonized assignee names with previously harmonized CrunchBase names. The Data Appendix gives additional details on the matching process between patent grants and our H1-CrunchBase matched dataset.

Finally, to match the patent application of startup firms, we use the USPTO's Bulk Download Data Tables for pre-granted published patents. The Data Appendix provides additional details on our methodology for constructing and merging our sub-sample of patent applications. Our final CrunchBase-granted patent-H1B matched sample includes 10,238 unique CrunchBase firm ids.

4.2 Summary Statistics

Table 1 describes the summary statistics of key variables in our final matched sample obtained from CrunchBase, USCIS, and USPTO data.

Table 1. Summary Statistics

Variable	Mean	SD	Min	Max	N
All Firms					
Patent Applications	1.197	4.413	0	356	53,448
Patent Applications (Granted)	1.124	3.892	0	166	53,448
Citation-Weighted Patent Apps	39.809	257.363	0	16,620	53,448
Process Patents	0.967	3.619	0	166	53,448
Total Funding (millions USD$)	3.941	24.878	0	1,500	37,596
VC Funding (millions USD$)	1.899	17.059	0	1,500	37,596
Series A Funding (millions USD$)	0.312	2.751	0	100	37,596
New H1B Denials (%)	0.036	0.840	0	102	16,662
Continuing H1B Denials (%)	0.034	0.321	0	15	16,662
Acquired	0.015	0.123	0	1	37,596
IPO	0.008	0.086	0	1	37,596
Closed	0.002	0.042	0	1	37,596
USA Firms					
Patent Applications	1.220	4.503	0	356	50,112
Total Funding (millions USD$)	4.071	25.481	0	1,500	35,310
VC Funding (millions USD$)	1.974	17.548	0	1,500	35,310
Series A Funding (millions USD$)	0.323	2.818	0	100	35,310
Canadian Firms					
Patent Applications	0.851	2.692	0	65	3,336
Total Funding (millions USD$)	1.933	12.063	0	375.402	2,286
VC Funding (millions USD$)	0.743	5.288	0	120	2,286
Series A Funding (millions USD$)	0.143	1.343	0	25	2,286

5. Main Results

5.1 Parallel Trends

The key testable condition for any DiD identification strategy to be valid, is common trends among treated and control groups. Our main outcome variable of interest is entrepreneurial experimentation, as measured by the number of patent applications for each startup.

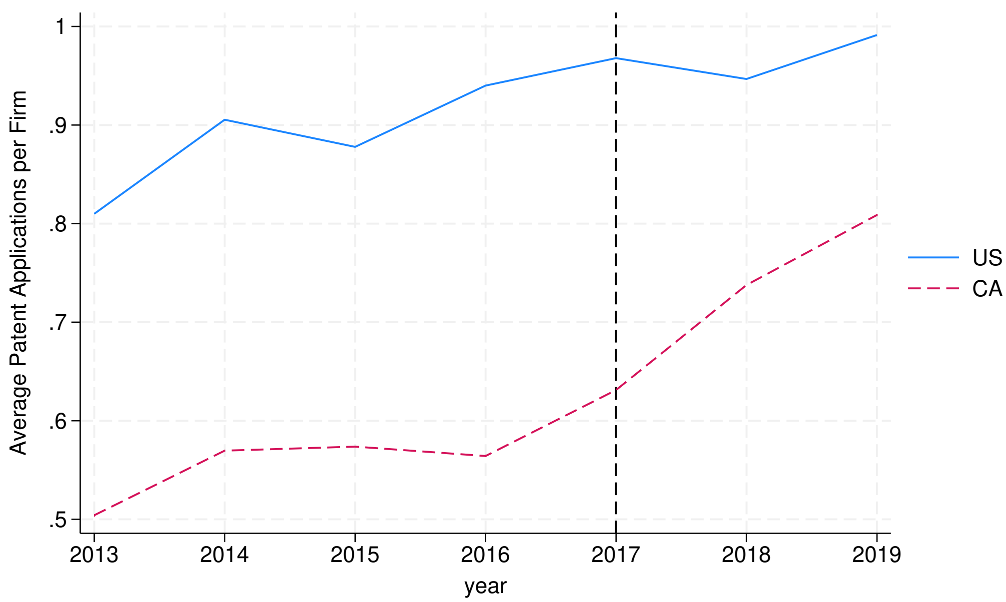


Figure 3. Pre-Trends in patent Applications: U.S. and Canada

Figure shows graphical evidence for the absence of differential pre-trends, before the first Trump administration took office in 2017. A F-Test for the presence of pre-trends cannot reject the hypothesis of parallel trends with a F-statistic of 0.64 (p-value: 0.42).

These results are reassuring, that the election of Trump to the presidency was indeed not anticipated and did not result in divergent pre-trends that could undermine identification.

5.2 Experimentation and VC Funding

Table 2 reports our baseline results. The first column shows that policy uncertainty caused a large and marginally insignificant decline in experimentation by high tech startups. This effect is qualitatively consistent with the main prediction of our theoretical model that policy uncertainty reduces entrepreneurial experimentation and might indicate that policy uncertainty effects dominated the potentially countervailing stimulative effects from de-regulation and tax cuts.

At the same time, the marginal insignificance of our results indicates the need to better understand the underlying driving forces, since the large statistical uncertainty might partly be the result of different effects on different groups of startups. One promising observable difference is firm size, as measured by employees: large startups are likely to have committed to a specific business model, so that any further investment will exhibit customization to the underlying business model. For example, hiring specialized programmers and training them on firm-specific IT systems is one example of an irreversible investment in firm-specific human capital. Commissioning a firm-internal project management and KPI software is another. In each of these cases, large firms are likely to make investments that are more firm-specific and irreversible, while small startups are more likely to use employees with generic skills as well as general-purpose technology, which is why they are often more nimble and flexible. If large startups are more likely to make irreversible investments, then our theory should apply more readily to large as opposed to small startups, and the effects of policy uncertainty should be stronger for large startups. This is indeed what we find in column (2) of Table 2, which documents that large startups – defined as having 100+ employees – respond to policy uncertainty much more negatively than the average startup. The effect of policy uncertainty on patent applications by large startups is 5x larger than the average startup in our sample. On the other hand, the effects are much weaker on patent applications for patents that are not only granted by end up having large impact, measured by patent citations. For these high-impact patents, entrepreneurs are likely to have high degrees of confidence ex ante or

Table 2. Baseline: Difference in Difference

	Experimentation			Funding for Large Startups		
	(1) Patent Applications	(2) Patent Applications Large Startups	(3) Patent Grants Cit. Weighted	(4) Funding (Total)	(5) Funding (VC)	(6) Funding (VC Series A)
PU Event	−0.227*	−1.205**	−0.013	−0.699***	−0.319**	−0.151**
× US-startup	(0.132)	(0.563)	(0.120)	(0.229)	(0.151)	(0.069)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	53448	4560	74578	56749	56749	56749

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Analysis sample is conditional on firms being founded in 2000 or later and time window spans 2013 to 2019 to avoid the COVID-19 Pandemic. PU Event is a dummy that is one after 2017 and zero before. US-Startup is a dummy that is one if the startup is located in the US and zero if it is located in Canada. Large startups are defined as having 100 or more employees. (1), (2), and (3) are estimated using a Poisson FE model with standard errors clustered at the firm level. (4), (5), and. (6) are estimated using a standard FE model with standard errors clustered at the firm level.

narrow priors, such that the value of the patent is not much affected by policy uncertainty.

The irreversibility of investments in our model also suggest that policy uncertainty should directly reduce investments, just as in the context of policy uncertainty and corporate investments (Gulen and Ion, 2016; Kim and Kung, 2017; Sanford and Yang, 2022). For startups, major investments will be externally funded – often by VC. We therefore analyze the impact of policy uncertainty on external funding of large startups in columns (4) to (6) of Table 2. For this analysis if again is sensible to focus on large startups, both because they are more likely to have a high percentage of irreversible investments and because small startups are usually unlikely to receive significant VC or other external investments. The results in Table 2 show that policy uncertainty significantly reduces funding for large startups, consistent with the predictions of our model around equation (3).

5.3 Granted Patents and Exit

One concern with our patent applications as measure of experimentation might be that it only captures low-quality applications that are not true experiments on whether a given innovation is novel enough to be granted a patent. We therefore redo our DiD analysis in the first two columns of Table 3, focusing only on patent applications that eventually are granted patents. The results confirm that even for these ex-post successful experiments, policy uncertainty has a significantly negative impact. This is especially true for patent applications that were granted within 3 years[8] as shown in column (2) of Table 3.

Despite the delay in experimentation and VC investment, exit outcomes of high tech startups do not seem to have been significantly impacted by the policy uncertainty event, as reported in columns (4), (5), (6) in Table 3. This is true for positive exit events such as the acquisition of a startup or an IPO, as well as for negative exit events such as bankruptcy.

[8]The truncation of time to grants of patents addresses a truncation issue in panel data on patents recently discussed by Lerner and Seru (2022).

Table 3. Granted Patents and Exit Outcomes

	Experimentation			Outcomes		
	(1) Patent Grants	(2) Patent Grants (3-Year Cap)	(3) Process Patents	(4) Acquired	(5) IPO	(6) Closed
PU Event	-0.168^{**}	-0.272^{***}	-0.131^{**}	-0.003	-0.002	-0.001
× US-Startup	(0.085)	(0.096)	(0.058)	(0.004)	(0.003)	(0.002)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	64122	54060	82992	56749	56749	56749

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes:Analysis sample is conditional on firms being founded in 2000 or later and time window spans 2013 to 2019 to avoid the COVID-19 Pandemic. PU Event is a dummy that is one after 2017 and zero before. US-Startup is a dummy that is one if the startup is located in the US and zero if it is located in Canada. (1), (2), and (3) are estimated using a Poisson FE model with standard errors clustered at the firm level. (4), (5), and. (6) are estimated using a standard FE model with standard errors clustered at the firm level.

6. Mechanisms

In this section we provide a deeper analysis of the underlying causal mechanisms that are driving the negative impact of policy uncertainty on entrepreneurial experimentation. On the one hand, in section 6.1 we closely follow the implications of our model and analyze whether more importance of irreversible investments magnifies the effect of policy uncertainty on experimentation. On the other hand, in section 6.2 we more deeply investigate uncertainty of a key policy area for high tech startups: high-skill immigration policy.

6.1 Irreversibility of Capital

Based on Proposition 2 of our model, we investigate in this section whether the degree of irreversibility of investments magnifies the responses to policy uncertainty. For this analysis, we use data on asset redeployability by Kim and Kung (2017), who use resale prices of capital equipment from the Bureau of Economic Analysis (BEA) to quantify the degree to which equipment can be reused within and across industries. Higher redeployability corresponds to lower discounts when reselling capital equipment and correspondingly lower degrees of irreversibility of investments. For example, a generic computer might be easily resold at a discount of 30% after six months. In contrast, commissioning a specialized computer with ASIC chips, that are specialized for Crypto mining may find little other uses and will therefore be sold at a discount of 70%. In this example the computer with the ASIC chip will be more irreversible as an investment than the generic computer.

Table 4. Policy Uncertainty and Industry-level Irreversibility

	Diff-in-Diff		Within-Country	
	(1)	(2)	(3)	(4)
	High Irreversibility	Low Irreversibility	US	CA
PU Event	-0.389**	-0.0675		
× US-Startup	(0.178)	(0.147)		
BBD-EPU-Index ×				
Irreversibility			-0.0297**	-0.244
			(0.0142)	(0.196)
Firm FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	No	No
Observations	11040	11766	28152	1668

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Analysis sample is conditional on firms being founded in 2000 or later and time window spans 2013 to 2019 to avoid the COVID-19 Pandemic. The dependent variable is patent grants. PU Event is a dummy that is one after 2017 and zero before. US-Startup is a dummy that is one if the startup is located in the US and zero if it is located in Canada. (1), (2), and (3) are estimated using a Poisson FE model with standard errors clustered at the firm level. (4), (5), and. (6) are estimated using a standard FE model with standard errors clustered at the firm level. BBD-EPU-Index is the Economic Policy Uncertainty Index of Baker et al. (2016), updated to 2019. Irreversibility of investment is based on Kim and Kung (2017). Models are estimated using a FE model with standard errors clustered at the firm level.

Table 4 begins our analysis of irreversibility patterns by classifying industries either in higher or lower irreversibility industries, using the industry-level measure of irreversibility of Kim and Kung (2017).[9]

Columns (1) and (2) use our DiD identification strategy to show that the negative impact of policy uncertainty of entrepreneurial experimentation[10] is driven by high irreversibility in-

[9] We classify Crunchbase startups into high- or low-irreversibility industries using a two-step process. First, we identify the patenting behavior of publicly traded firms and map each firm's SIC code to its corresponding patent CPC areas. Then, we use this SIC-to-CPC mapping to assign SIC codes to Crunchbase startups. Finally, startups with an irreversibility value above the median for a given year are assigned a high-irreversibility dummy.

[10] Entrepreneurial experimentation is based on the patent grants of our Crunchbase startups. Since we use

dustries. This makes sense in the context of Proposition 2 of our model: the retention of option value through delayed investment requires that early investments are irreversible, otherwise early investment would exhibit the same option value as delayed investment. Delayed investments in turn make delayed experimentation more likely, since startups can save on expected experimentation costs if they decide to invest later anyway. It is therefore natural to find that industries with low degrees of investment irreversibility exhibit only an insignificant reduction of entrepreneurial experimentation in response to policy uncertainty.

The data on irreversibility of investments also helps us to further address the concern that our DiD results may reflect the spillover effects of US policies to the control group of Canadian firms. Specifically, (Brinatti and Guo, 2024) have analyzed how Canadian employers have benefited from high-skill immigration in the wake of the first Trump administration taking office in 2017. The results in columns (1) and (2) of Table 4 begin to address the concern that our DiD results are mostly driven by higher experimentation of Canadian startups as opposed to lower experimentation of US startups. If the Canadian control group would drive the results, we would expect comparably negative results across high and low irreversibility industries. Instead, the negative effect of policy uncertainty on experimentation is only significant for high irreversibility industries and is quantitatively more than 5x stronger.

To further support the view that our main results are not driven by the control group of Canadian firms, we focus only on US startups in column (3) of Table 4. In this column, we also directly use the economic policy uncertainty index by Baker et al. (2016), named BBD-EPU-Index. This column shows that economic policy uncertainty leads to a significant decrease in entrepreneurial experimentation, mirroring the results of economic policy uncertainty reducing corporate investments in Gulen and Ion (2016) and Kim and Kung (2017).

We find similar qualitative effects, for Canadian startups in column (4) of Table 4, albeit not statistically significant and therefore statistically indistinguishable from a zero effect.

the patent grants of publicly-traded firms to create our CPC to SIC mapping, we also use the same patent grants for defining entrepreneurial experimentation.

This is consistent with the view that our DiD results are indeed not driven by a broadly stimulative impact of US policy uncertainty on Canadian startup experimentation.

6.2 High-Skill Immigration Policy Uncertainty

Beyond deregulation and tax reform, immigration policy was a key policy area of the first Trump administration. As is well-known, highly skilled immigrants are a key input for high tech entrepreneurship in the US (Glennon, 2024; Kerr, 2013). One of the critical gateways[11] through which skilled immigrants enter the US workforce and therefore the US startup ecosystem is the H-1B visa program (Chen et al., 2021; Dimmock et al., 2022). Employers apply for a H-1B work visa on behalf of their potential employees for highly specialized human capital and occupations, for which finding employees with US citizenship is very difficult. The number of available work visas through the H-1B visa program is capped at 85,000 per year[12] with demand for H-1B visas outstripping this quota for every year since at least 2014. For every year in which applications for H-1B by employers exceeds the quota of 85,000 the allocation of visas is conducted via a random lottery. Several recent empirical studies such as Chen et al. (2021) and Dimmock et al. (2022) have highlighted the positive causal impact of winning the H-1B visa lottery on startup innovation. See Glennon (2024) for an excellent recent survey on the literature on the impact of high-skill immigration on firm innovation.

However, less appreciated in the literature on the effects of the H-1B visa lottery on startup innovation, is the fact that USCIS has broad discretion to deny H-1B visas, even after an employer has won the H-1B visa lottery. Figure 4 shows the percentage of winners of the H-1B visa lottery that were denied an H-1B visa. It shows that discretionary denial rates of for winners of the H-1B lotteries were around 3-4% before the first Trump presidency,

[11] The other gateway is US academia, which attracts many students every year, but these students are required to devote their entire available time to their studies and are not allowed to work in the private sector.

[12] These include 65,000 per year without any requirement and an additional 20,000 work visas for qualified employees with a Masters degree from a US university.

but increased by a factor of 4x in 2017 and 2018 before declining again. We use this sudden policy shock in high-skill immigration policy as variation to estimate the causal effect of immigration policy uncertainty on entrepreneurial experimentation.

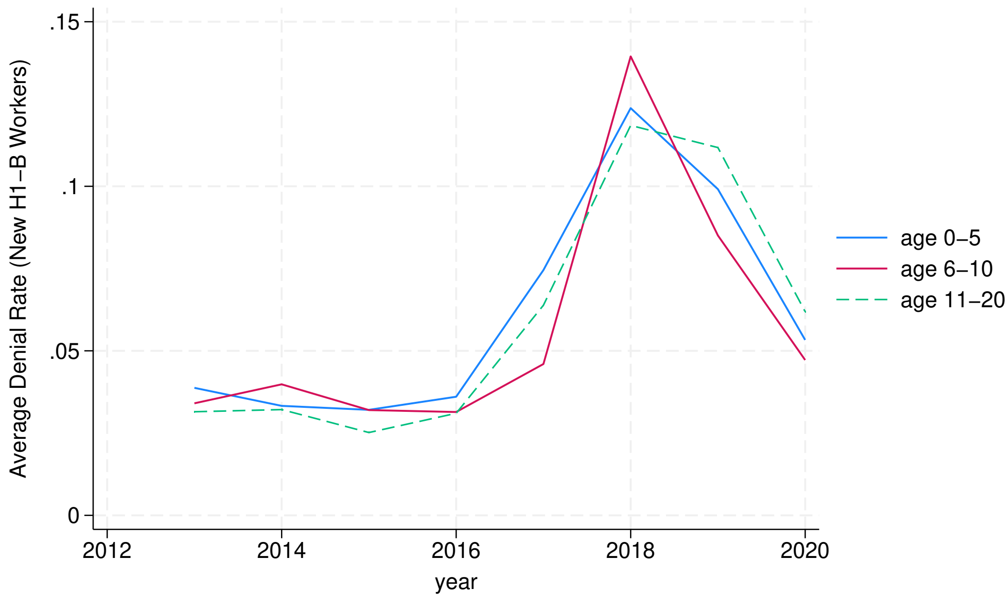


Figure 4. Average Denial Rate of new H-1B Workers by Firm Age

We begin our analysis of high-skill immigration policy by first exploring the importance of high-skill immigration for the impact of policy uncertainty on experimentation. Specifically, Table 5 uses our baseline DiD regression for three groups of startups, which differ in the degree to which they generally relied on H-1B worker before 2017, the first year of the first Trump presidency. "Never H-1B" firms did not apply for any H-1B visas during the three years of 2014-2016. "Always H-1B" are startups that applied for H-1B visas in every year during 2014-2016 and "Sometimes H-1B" are firms that applied during some years but not others. If high-skill immigration policy matters to understand the effects of policy uncertainty on experimentation, one should see stronger negative effects for startups "at risk" of being affected by immigration policy uncertainty, which we capture by their past usage of high-skilled immigrants. On the flipside, firms that never used the H-1B program should be either unaffected by policy uncertainty or they might benefit from the upside of the policy reforms in the deregulation and tax areas.

The results shown in Table 5 confirm that the negative impact of policy uncertainty on experimentation is completely driven by startups at risk of using high-skilled immigrants – either “sometimes" or “always". The impacts of policy uncertainty on patent applications and granted patents are not only statistically significant, but quantitatively larger for startups using H-1B workers in 2014-2016. For these startups policy uncertainty strongly reduced patent applications as well as the number of granted patents. In contrast, startups that never used any H-1B workers – “Never H-1B" firms – saw strong increases in both experimentation and number of granted patents as columns (2) and (5) show. These results are consistent with the view that the first Trump administration's deregulation and tax policies had a stimulative effect on experimentation and innovation on startups that never used skilled immigrants.

Table 5. Effect of H-1B Policy on Experimentation / Patent Grants

		Experimentation			Patent Grants		
	(1) Full Sample	(2) Never H-1B	(3) Sometimes H-1B	(4) Always H-1B	(5) Never H-1B	(6) Sometimes H-1B	(7) Always H-1B
PU Event	-0.227^{*}	0.337^{***}	-0.498^{***}	-0.970^{***}	0.339^{***}	-0.420^{***}	-0.760^{***}
× US-Startup	(0.132)	(0.048)	(0.143)	(0.220)	(0.042)	(0.092)	(0.140)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	53448	39960	13488	4422	48696	15426	5352

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Analysis sample is conditional on firms being founded in 2000 or later and time window spans 2013 to 2019 to avoid the COVID-19 Pandemic. PU Event is a dummy that is one after 2017 and zero before. US-Startup is a dummy that is one if the startup is located in the US and zero if it is located in Canada. Estimated using a Poisson FE model with standard errors clustered at the firm level. Never H-1B refers to startups that never apply for a new or continuing H-1B application in 2014-2016. Sometimes H-1B refers to startups that apply at least once for either a new or continuing H-1B applicion. Always H-1B refer to startups that apply to either new or continuing H-1B applications.

A key question for our analysis of policy uncertainty is whether the immigration policy effects are driven by changes in human capital inputs or policy uncertainty. Prior studies of the impact of winning the H-1B visa lottery on startup patenting, such as Chen et al. (2021) and Dimmock et al. (2022) have mostly focused on changes in human capital inputs: a winning startup adds a skilled employee, who works on innovations for the startup. However, a decline in experimentation in our context can not only be driven by the loss of a highly skilled employee. Instead, such a decline could be driven by uncertainty about the availability of future skilled immigrant hires due to policy uncertainty and whether the USCIS under the Trump administration will deny H-1B visas on a discretionary basis going forward. Fortunately, the H-1B visa application data directly measures whether a specific application is for a continuing H-1B visa worker or whether it is for a new H-1B employee. If the effects of discretionary denials of H-1B visa applications mainly reflects loss of skilled immigrant employees, we would expect the effects on experimentation to be significantly negative for denied continuing H-1B worker applications, while being insignificant and weaker for denials of new H-1B work visas.

Table 6. Effect of H-1B Policy on Experimentation

		Applicants for New H-1B			Applicants for Cont. H-1B		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Full Sample	Applicants	Approved	Denied	Applicants	Approved	Denied
PU Event	-0.227^{*}	-0.546^{***}	-0.446^{***}	-1.762^{**}	-0.558^{***}	-0.426^{***}	-1.316^{**}
$\times$ US-Startup	(0.132)	(0.185)	(0.161)	(0.714)	(0.161)	(0.145)	(0.545)
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	53448	5839	5497	102	6870	6445	136

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Analysis sample is conditional on firms being founded in 2000 or later and time window spans 2013 to 2019 to avoid the COVID-19 Pandemic. PU Event is a dummy that is one after 2017 and zero before. US-Startup is a dummy that is one if the startup is located in the US and zero if it is located in Canada. (2) and (5) include firms in our samples that had a non-missing H-1B denial rate between 2017-2019. (3) and (6) include firms that had a denial rate of 0 for their H-1B lottery wins, and (4) and (7) include firms that received a positive denial rate for their lottery wins. Analysis sample is conditional on firms being founded in 2000 or later. Estimated using a Poisson FE model with standard errors clustered at the firm level.

As columns (2) and (5) of Table 6 show, the negative impact of the first Trump presidency taking office is significantly negative, quantitatively large and comparable across applicants for either new or continuing H-1B visas. This similarity in effects is inconsistent with the view that it is the loss of a current skilled employee that is driving the negative effect of the first Trump term on experimentation. Furthermore, as columns (3) and (6) of Table 6 show, even startups that win the H-1B lottery and get their petition approved reduce their patent applications. In other words, these startups end up getting a new skilled immigrant worker but still decide to innovate less. If the loss of human capital would be driving our analysis of immigration policy, one would expect that startups with full approval of H-1B workers would suffer no decline in experimentation. Furthermore, those firms that are denied H-1B visa applications despite winning the lottery suffer the largest reductions in entrepreneurial experimentation. These effects are consistent with our theory that policy uncertainty – especially high-skilled immigration policy uncertainty – are driving the negative effects of general policy uncertainty on entrepreneurial experimentation.

7. Robustness

7.1 Synthetic Control Method

One concern of our DiD identification strategy may be that the average US startup in the Crunchbase data is more innovative and active in different industries than the average Canadian startup. While we agree with this statement, our identification approach allows for differences in levels of innovation and experimentation and DiD as an identification strategy is only threated if trends among treatment and control group are different. However, to highlight the robustness of our results, we follow the Synthetic Control Approach by Abadie, Diamond, and Hainmueller (2010), to allow for time-varying unobserved confounders. For this approach we match pre-trends of US startups with pre-trends of corresponding Canadian startups before 2017 and then track experimentation after 2017.

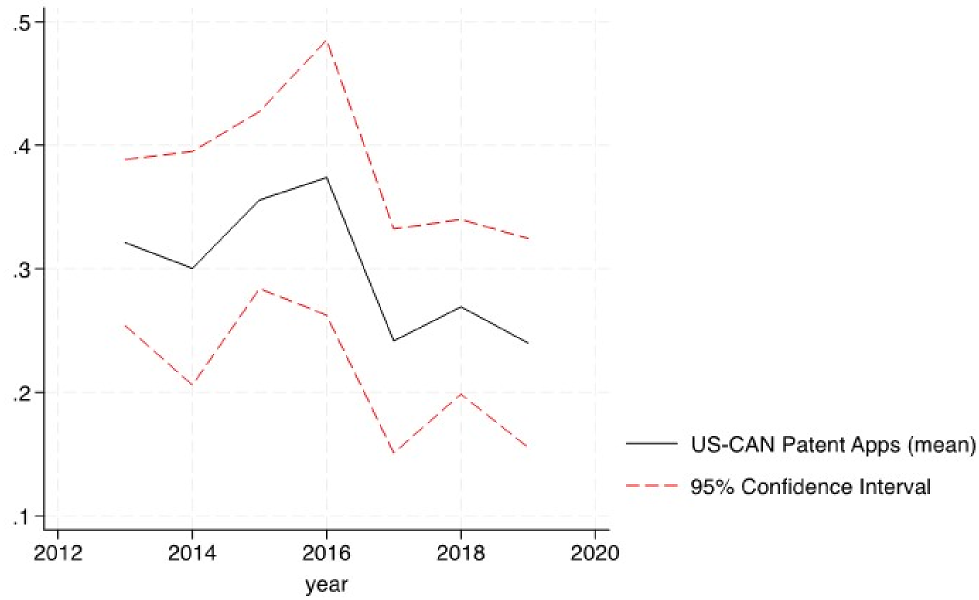


Figure 5. Text-matching crunchbase industries of US and CAN startups

Figure 5 documents that the negative impact of policy uncertainty on experimentation is robust to the use of this Synthetic Control Method.

7.2 Partisanship

As discussed by Baker et al. (2016), political polarization may be considered one of the driving forces of increased political uncertainty in the last 20 years. The effects of political partisanship and polarization may provide a competing explanation for the effects of the first Trump presidency on entrepreneurial experimentation. Indeed, Engelberg, Guzman, Lu, and Mullins (2022) have shown that startup entry in the US, systematically reflects partisanship: elections of Democratic presidents stimulate startup entry in Democratic areas, while Republicans presidents winning induces more entry in Republican-voting areas. This mechanism may explain some of the patterns we find, if urban areas and states with large technology startup clusters tend to be Democratic politically, so that the surprising election of the Republican Trump administration is interpreted by Democratic entrepreneurs and VC investors as bad macro-economic news. As a result, these partisan Democratic entrepreneurs and VC investors may reduce experimentation and VC investments.

Table 7. Effect of Political Affiliation on Patent Applications

	(1) Democratic	(2) Republican	(3) Marginal States	(4) Inframarginal States
US × Policy	0.065	0.071	-0.037	0.081
	(0.060)	(0.072)	(0.080)	(0.057)
Firm FE	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes
Observations	36786	13326	8502	41610

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Analysis sample is conditional on firms being founded in 2000 or later and time window spans 2013 to 2019 to avoid the COVID-19 Pandemic. PU Event is a dummy that is one after 2017 and zero before. US-Startup is a dummy that is one if the startup is located in the US and zero if it is located in Canada. Democrat States are defined as US states that voted for the Democratic Presidential Candidate in 2016. Republican States are defined as US states that voted for the Republican Presidential Candidate in 2016. Marginal States are defined as states that went to either candidate in 2016 within a 5% margin. Inframarginal States voted for a candidate by more than a 5% margin. Standard errors clustered at the firm-level and are reported in parentheses.

To directly test the hypothesis that political partisanship is driving our results, we re-estimate our baseline DiD regressions, while focusing on startups in either Democratic or Republican states. If our effects are driven by partisanship, one would expect that startups in Democratic states exhibit a more negative impact of the first Trump presidency on experimentation. Columns (1) and (2) of Table 7 show that this is not the case. Startups in democratic states do not exhibit a more negative impact of the first Trump presidency on entrepreneurial experimentation than startups in Republican states.

Another possibility of how partisan ship may driven our results is that changes in the Trump administration's skilled immigration policy maybe be targeted in a politically favorable way. For example, USCIS may have been under the instruction to be more generous with H-1B approvals in US states that are more important for Trumps re-election efforts. In 2017-2018, these may be US states that Trump only narrowly won. Alternatively, the first Trump administration may have tried to target H-1B denials at infra-marginal states, where it may not have feared political consequences of stricter skilled immigration policy. Columns (3) and (4) of Table 7 show that conditioning either on marginal or infra-marginal states in the 2016 election do not show any significantly different results. Overall our results do not seem to reflect political partisanship, either as perceived by entrepreneurs and investors, nor by the first Trump administration.

8. Conclusion

This paper uses the surprising election of Donald Trump to the US Presidency in 2016 as a natural experiment to establish the causal effect of policy uncertainty on entrepreneurial experimentation and VC investments. It uses a real-options framework to link the literature on the investment effects of policy uncertainty as in Baker et al. (2016), Gulen and Ion (2016), and Kim and Kung (2017) to the recent literature on entrepreneurial experimentation (Kerr et al., 2014) and Bayesian entrepreneurship (Agrawal et al., 2024). Our main result is that policy uncertainty – especially in the area of high-skill immigration policy – had a significantly negative impact on entrepreneurial experimentation and patenting, as well as VC investments in large startups. These negative effects are driven by large and highly innovative startups that rely on high-skill immigrants. In contrast, US tech startups that are either small or that are not using immigrants for their high-skill employee needs saw either no significant impact of policy uncertainty or even experimented and innovated more.

References

Abadie, Alberto, Alexis Diamond, Jens Hainmueller. 2010. Synthetic control methods for comparative case studies: Estimating the effect of california's tobacco control program. *Journal of the American statistical Association*, **105**(490) 493–505.

Abel, Andrew B, Janice C Eberly. 1994. American economic association. *The American Economic Review*, **84**(5) 1369–1384.

Agrawal, Ajay, Arnaldo Camuffo, Alfonso Gambardella, Joshua Gans, Erin Scott, Scott Stern. 2024. Bayesian Entrepreneurship. *MIT Working Paper Series*.

Baker, Scott R, Nicholas Bloom, Steven J Davis. 2016. Measuring economic policy uncertainty. *The quarterly journal of economics*, **131**(4) 1593–1636.

Bhattacharya, Utpal, Po-Hsuan Hsu, Xuan Tian, Yan Xu. 2017. What affects innovation more: policy or policy uncertainty? *Journal of Financial and Quantitative Analysis*, **52**(5) 1869–1901.

Bloom, Nicholas. 2009. The impact of uncertainty shocks. *econometrica*, **77**(3) 623–685.

Brinatti, A., X. Guo. 2024. Third-country effects of us immigration policy. *Available at SSRN 4892498*.

Chen, Jun, Shenje Hshieh, Feng Zhang. 2021. The role of high-skilled foreign labor in startup performance: Evidence from two natural experiments. *Journal of Financial Economics*, **142**(1) 430–452.

Dimmock, Stephen G, Jiekun Huang, Scott J Weisbenner. 2022. Give me your tired, your poor, your high-skilled labor: H-1b lottery outcomes and entrepreneurial success. *Management Science*, **68**(9) 6950–6970.

Dixit, Avinash K, Robert S Pindyck. 1994. *Investment under uncertainty*.

Djankov, Simeon, Tim Ganser, Caralee McLiesh, Rita Ramalho, Andrei Shleifer. 2010. The effect of corporate taxes on investment and entrepreneurship. *American Economic Journal: Macroeconomics*, **2**(3) 31–64.

Engelberg, Joseph, Jorge Guzman, Runjing Lu, William Mullins. 2022. Partisan entrepreneurship. Tech. rep., National Bureau of Economic Research.

Ewens, Michael, Ryan H Peters, Sean Wang. 2024. Measuring intangible capital with market prices. *Management Science*.

Gans, Joshua S, Scott Stern, Jane Wu. 2019. Foundations of entrepreneurial strategy. *Strategic Management Journal*, **40**(5) 736–756.

Glennon, Britta. 2024. Skilled immigrants, firms, and the global geography of innovation. *Journal of Economic Perspectives*, **38**(1) 3–26.

Gompers, Paul, Anna Kovner, Josh Lerner, David Scharfstein. 2008. Venture capital investment cycles: The impact of public markets. *Journal of financial economics*, **87**(1) 1–23.

Gulen, Huseyin, Mihai Ion. 2016. Policy uncertainty and corporate investment. *The Review of financial studies*, **29**(3) 523–564.

Kerr, William R. 2013. Us high-skilled immigration, innovation, and entrepreneurship: Empirical approaches and evidence. Tech. rep., National Bureau of Economic Research.

Kerr, William R, Ramana Nanda, Matthew Rhodes-Kropf. 2014. Entrepreneurship as experimentation. *Journal of Economic Perspectives*, **28**(3) 25–48.

Kim, Hyunseob, Howard Kung. 2017. The asset redeployability channel: How uncertainty affects corporate investment. *The Review of Financial Studies*, **30**(1) 245–280.

Klapper, Leora, Luc Laeven, Raghuram Rajan. 2006. Entry regulation as a barrier to entrepreneurship. *Journal of financial economics*, **82**(3) 591–629.

Lerner, Josh. 2009. *Boulevard of broken dreams: why public efforts to boost entrepreneurship and venture capital have failed–and what to do about it*.

Lerner, Josh, Amit Seru. 2022. The use and misuse of patent data: Issues for finance and beyond. *The Review of Financial Studies*, **35**(6) 2667–2704.

Nanda, Ramana, Matthew Rhodes-Kropf. 2013. Investment cycles and startup innovation. *Journal of financial economics*, **110**(2) 403–418.

Nanda, Ramana, Matthew Rhodes-Kropf. 2016. Financing entrepreneurial experimentation. *Innovation Policy and the Economy*, **16**(1) 1–23.

Romer, Christina D, David H Romer. 2010. The macroeconomic effects of tax changes:

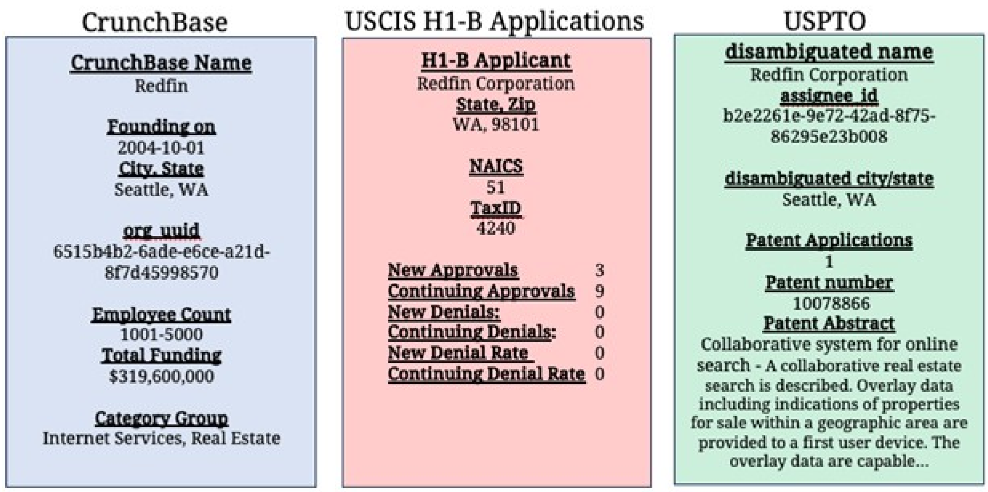
estimates based on a new measure of fiscal shocks. *American economic review*, **100**(3) 763–801.

Sanford, Anthony, Mu-Jeung Yang. 2022. Corporate investment and growth opportunities: The role of r&d-capital complementarity. *Journal of Corporate Finance*, **72** 102130.

Stern, Scott. 2006. Economic experiments: The role of entrepreneurship in economic prosperity. *Melbourne Review: A Journal of Business and Public Policy, The*, **2**(2) 53–56.

Data Appendix

This Appendix provides additional details on cleaning and merging the dataset the three datasets used in the study: startup firms from CrunchBase, patent data from the USPTO, and H-1B petitions from the United States Citizenship and Immigration Services (USCIS). See the figure below for an example of a startup in our sample.



H-1B firm-name strings are harmonized by correcting typos, removing punctuation, and standardizing commonly used prefixes and suffixes, similar to the modifications applied to the names of startup firms in CrunchBase. To further address duplicate issues (e.g., misspelled employer names or different variations of the same employer's name), we apply a probabilistic record-linkage technique using the Stata function 'strgroup'. This function compares two strings of characters (employer names) and assigns them to the same group identifier based on a user-defined threshold of similarity. We then use these assigned groups, along with the employer's self-reported NAICS, Tax ID, state, and ZIP code, to identify duplicate employers. Employers are defined as duplicates if 'strgroup' assigns them to the same group and at least two of the four variables—NAICS, ZIP code, 4-digit Tax ID, and state—are the same.

Table A1. Strgroug: Probabilistic grouping if same H1 Employer Firms

Firm Name	Strgroup (assigned)	State	Tax ID	NAICS
AMERICAN LIBERTY	12040	IL	4464	52
AMERICA LIBERTY	12040	IL	4464	52
ANCHOR FREE	14348	CA	6083	54
ANCHORFREE	14348	CA	6083	51
L C FOOD DISTRIBUTION	116221	CA	7067	42
LC FOOD DISTRIBUTION	116221	CA	7067	99
SERENA WHOLESALE	189905	GA	988	54
SERENA WHOLSALE	189905	GA	7805	99

Second, while ‘strgroup’ and our algorithm effectively identified and grouped duplicate firms, the algorithm performed best when the compared name strings were of similar length. For example, ‘QUALITY GLOBAL SERVICES’ and ‘QUALITY GLOBAL’ appeared as separate firm entries and were not grouped by ‘strgroup‘, even though they represent the same firm. To address this challenge, we implemented a two-step grouping process. In the first step, we grouped firms that shared the same first three string characters as well as the same 4-digit Tax ID, state, NAICS, and ZIP code. In the second step, for firms that remained ungrouped, we relaxed the requirement for matching first characters and only required that the employers had the same 4-digit Tax ID, state, NAICS, and ZIP code. We visually inspected the groups to ensure that the correct firms were grouped. This approach was useful for identifying duplicate employer names such as ‘SYMMETRY SOFTWARE RP’ and ‘TEKNON RP SYMMETRY SOFTWARE’ or ‘UNWIN SCHEBEN KORYNTA HUETL’ and ‘USKH’, among many others.

Table A2. **MATCHIT TABLE WITH SIMILARITY SCORES**

Original CrunchBase Name	CrunchBase Firm Name	H1 Employer Name	CrunchBase State	H1 State	Similarity Score	Match?
Scribd	SCRIBD	SCRIBD	CA	CA	1.0000	Yes
Brocade Communications Systems	BROCADE COMMUNICATIONS SYST	BROCADE COMMUNICATIONS SYST	CA	CA	1.0000	Yes
Sucden	SUCDEN	SUCDEN AMERICAS	FL	FL	.9973	Yes
NxtBio Technologies	NXTBIO TECH	ACE SCIENTIFIC NXTBIO TEC	CA	CA	.9954	Yes
Medtrx	MEDTRX	MEDTRX HEALTHCARE SOLNS	NJ	NJ	.9997	Yes
BRICKELL TECHNOLOGY	BRICKELL TECH	BRICKELL INVESTMENT REALTY	FL	FL	0.99	No

To match H-1B employer firm names with CrunchBase startup names, we use a second probabilistic record-linking technique with Stata's 'matchit'. Unlike 'strgroup', 'matchit' returns all possible matching pairs along with a Levenshtein Distance-based similarity score. To ensure accurate firm matches, we verify the matches using state and ZIP code records from both the H-1B employer firms and the CrunchBase startup firms. Our final sample includes: (i) perfectly harmonized firm-name matches (similarity score = 1) with matching state data, and (ii) manually checked imperfect name matches (with a high probabilistic match score) combined with matching state and ZIP code data. For H-1B firms matched with multiple CrunchBase firms, we retained the matches with the same ZIP codes and non-missing CrunchBase information. The final CrunchBase-H-1B matched sample consists of 48,448 firm matches, of which 25,570 are confirmed startups based on founding dates on or after 2000.

To match the disambiguated assignee organization names to the CrunchBase-H1B set of firms, we use the same name standardization algorithm that was previously applied to CrunchBase and H1 employer firms. Since the goal is to match patent assignees with our sample of harmonized CrunchBase and H1 employer firms, we only match harmonized assignee names with previously harmonized CrunchBase names. To ensure accurate matches across the datasets, we followed a multi-step process:

1. Exact Name Matches: We first retained all exact name matches between assignee firms and CrunchBase firms, provided they also had matching state locations. Once a CrunchBase firm is matched to an assignee (and its corresponding state), other assignee-state pairs are dropped, and the matched CrunchBase firm is excluded from subsequent matching stages.

2. Probabilistic Matching: For the remaining unmatched CrunchBase firms, we applied the probabilistic record-linkage 'matchit' function in Stata. We manually checked firm name pairs based on:

   - High Probabilistic Matching Scores (greater than 0.95) with matching states.
   - High Probabilistic Matching Scores (greater than 0.95) with non-matching states.

Table A3. Granted Patent Disambiguated Assignee Matching

Original Patent Assignee Name	Harmonized Assignee Name	Original CrunchBase Name	Harmonized CrunchBase Name	Match Type	Matchit Similarity Score
XCOM Labs, Inc.	XCOM LABS	XCOM Labs	XCOM LABS	Exact Name + Location (state)	-
OCZ TECHNOLOGY GROUP, INC.	OCZ TECH GRP	OCZ Technology	OCZ TECH	Probabilistic + Location (state)	.9308
Route4Me, Inc.	ROUTE4ME	Route4Me	ROUTE4ME	Exact Name + Location (state)	-
LUMEnergi, Inc.	LUMENERGI	Lumenergi	LUMENERGI	Exact Name + Location (state)	-
Advenchen Pharmaceuticals, LLC	ADVENCHEN PHARMAS	Advenchen Laboratories	ADVENCHEN LABORATORIES	Probabilistic + Location (state)	.9697
AKHAN Semiconductor, Inc.	AKHAN SEMICONDUCTOR	AKHAN Semiconductor	AKHAN SEMICONDUCTOR	Exact Name + Location (state)	-

Finally, to match the patent application of startup firms, we begin with USPTO's Bulk Download Data Tables for pre-granted published patents. Patent applicants can be either the eventual assignee(s) of the patent if the patent is granted but also can be the original inventors of the patent as well. However, a patent application is publicly disclosed (published) and open to public inspection generally 18-months after the application date. There are several incentives for recording an assignee for a published patent, particularly when the patent is held by an organization. For example Goldman and Schwartz (2016) argue that recording patent assignments is crucial for establishing a public record of ownership, especially in cases that involve disputes over patent rights later on. Additionally, patent portfolios may impact investor or market perception, which can be a significant factor in financing, valuation, and investment decisions, especially for startup firms. As a result, operating under the assumptions that startup firms are incentivized to disclose assignees when the patent applications become public information, we match the USPTO's pre-granted published applications to the pre-granted disambiguated assignee database using the pre-granted patent identifier pgpub_id. For the remaining unmatched pre-granted publications, we use the USPTO's Patent Assignment dataset, which is derived from the recording of patent transfers by parties with the USPTO. We include all assignment entries that convey an"Assignment of Assignor's Interest." Using these two matching methods, we are able to uncover 2.7 million patent application-to-patent assignee relationships, accounting for slightly less than 50% of the patent applications in the USPTO pre-granted patent database. This combined patent application "assignee" data is modified in the same way as the H-1B applicant firm names mentioned above. We apply 'strgroup' to group likely assignee under the same umbrella, capitalize and remove common firm prefixes, suffixes, and punctuations. We then use Stata's 'matchit' function to match the assignee names to the CrunchBase organization name. Our crunchbase-granted patent-H1B matched sample included 10,238 unique crunchbase firm ids.