ABSTRACT

Title of Dissertation: ESSAYS IN INNOVATION AND ENTREPRENEURSHIP

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My dissertation focuses on the topics of innovation and entrepreneurship. In Chapter 1, I study how banks affect their borrowing firms' green innovation when they reduce credit to firms with high carbon emissions. Using banks' commitments to carbon neutrality as credit shocks to the borrowing firms, I first show that high-emission firms file fewer green patents following their relationship banks' commitments to carbon neutrality. At the same time, other borrowing firms that receive increased lending from these committed banks see an increase in green patent filings. Second, I present evidence suggesting that financial constraints and inventor mobility are important mechanisms driving these effects. Third, I find that the value of newly filed green patents by firms in high-emission industries declines post-commitment, whereas there appears to be no discernible impact on the value of green patents filed by other firms. Finally, I develop a novel measure that gauges a patent's relevance to mitigating climate change impact using text algorithms and show that banks' commitments lead to lower relevance of green patents filed by high-emission firms. Altogether, the paper highlights an unintended consequence of bank

divestment: a decrease in the production of high-quality green patents.

Chapter 2 is joint work with Sven Oskarsson and Rafael Ahlskog. This chapter investigates the effects of parental income volatility on individuals' entrepreneurial decisions in Sweden. Our results indicate that individuals who experience higher uninsurable parental income volatility during adolescence are more likely to become entrepreneurs. Specifically, a one-standard-deviation increase in parental income volatility is associated with an increase in the probability of becoming an entrepreneur by around 45% relative to the mean. Second, we find that firms started by individuals with higher parental income instability have a lower survival rate. Finally, we present evidence in line with higher risk tolerance being an important mechanism driving our findings. We do not find support for alternative mechanisms, including human capital accumulation and financial resources.

ESSAYS IN INNOVATION AND ENTREPRENEURSHIP

by

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Chapter 1: Bank Divestment and Green Innovation

1.1 Introduction

Since the inception of global fossil fuel divestment campaigns in 2011, financial institutions have faced mounting pressure to divest from companies with high carbon emissions. Given the heavy reliance of high-emission companies on debt financing,[1] the banking sector has become a focal point within these divestment campaigns. The underlying economic argument is that if banks stop financing high-emission firms, the high-emission firms would have to reduce their investment and production, thereby lowering their carbon emissions. Indeed, an increasing number of large banks across the globe have started to pull back from lending to high-emission firms over the past decade. This shift in lending practices is evident in Figure 1.1, which illustrates that loans received by public firms in the fossil fuel industries decreased from around 8% of all the loans received by public U.S. firms in 2010 to about 4% by the year 2022. However, the underlying economic argument does not account for the ability of firms to potentially substitute capital from other sources for bank lending. Moreover, even if firms cannot substitute for bank capital, it assumes investment by high-emission firms has the net effect of increasing emissions. However, firms in high-emission industries have historically been key innovators in the space of green technologies.[2] Green innovation is widely viewed as essential to reach the net zero target by 2050.[3] Thus, the consequences for investment and, ultimately, emissions from such policies

[1] Based on a report titled "Divestment – does it drive real change?" by Gan (2019), more than 90% of new funding for the mining, oil and gas, and utility and energy sectors come from debt and syndicated loans.

[2] See, e.g., Cohen et al. (2020).

[3] Based on a recent report authored by expert from the International Energy Agency (Bouckaert et al. (2021)), about half of the reductions in CO_2 emissions necessary to achieve the net zero emissions by 2050 will come from technologies that are not on the market yet.

remain unclear.

The objective of this paper is to understand how bank divestment from high-emission firms affects green innovation. Conceptually, the reduction of lending to high-emission companies can have either a positive or a negative effect on their investment in green innovation. On the one hand, reducing credit to high-emission firms could make the production of new green technologies more costly, especially if the affected high-emission firms have difficulty substituting credit from relationship banks with alternative sources of financing. On the other hand, high-emission firms may have an incentive to increase green innovation if the resulting green technology could lower their carbon emissions, thereby reducing the cost of capital from relationship banks. Moreover, divestment from high-emission firms can have spillover effects on the investment in green innovation by other firms that receive increased capital when supply-constrained banks reallocate credit away from high-emission firms or on innovation that builds upon the patents produced by high-emission firms.

Focusing on the patent filings of U.S. public firms with syndicated loans from 2011 to 2020, I provide evidence that high-emission firms file fewer green patents following their relationship banks' divestment. More specifically, firms with carbon emissions one standard deviation higher than the mean, on average, experience a decline in green patent filings of about 12.9% after their relationship banks divest, relative to firms with the same level of carbon emissions but without a divesting lender. This decline in green patent filings by high-emission firms is met with a corresponding increase in green patent filings by other borrowing firms which, on average, receive a greater influx of lending from the divesting banks. However, the decrease in the number of green patents filed by high-emission firms is accompanied by a decline in their average quality and relevance to mitigating climate change. In contrast, the quality of green patents put forth by other borrowing firms does not exhibit significant improvement in the aftermath of bank divestment.

I categorize a patent to be "green" or "non-green" following the guidelines created by the Organization for Economic Co-operation and Development (OECD). In particular, the OECD guidelines identify patent technological classes that are related to climate change adaptation, environmental management, and other environmental technologies and classify the patents associated with these technological classes as "green." The information on the technological classes of every patent is provided by the U.S. Patent and Trademark Office (USPTO). On average, firms in the ten industries with the highest scope-1 carbon emissions[4] file 50% more green patents in a year relative to public firms in other sectors. In addition, the green patents granted to these high-emission firms receive higher citations, are more original, and are of higher value based on stock market reactions.

To estimate the effect of bank divestment on green innovation, I leverage bank-level commitments to carbon neutrality under the Science Based Targets Initiative (SBTi). These commitments serve as credit shocks to firms that had prior lending relationships with these committed banks. Commencing in 2015, an increasing number of banks worldwide joined SBTi, which, in turn, provides explicit recommendations for reducing lending to fossil fuel and other high-emission companies. I start my empirical analysis by identifying the extent to which bank commitments affected the debt financing of firms that previously borrowed from them. Specifically, I estimate a triple-difference model, comparing syndicated loans received by firms with and without SBTi-committed relationship lenders before and after these lenders pledge their commitments to SBTi, for varying levels of pre-2015 carbon emissions. I control for both firm and lender fixed effects in the model to account for time-invariant characteristics that might affect debt financing, such as firms' profitability, and year fixed effects to account for macroeconomic shocks. In the most stringent specification, I control for firm-bank fixed effects – to ensure that the estimates are driven by changes in credit allocation within firm-bank pairs – and size quintile-year fixed effects to allow firms of different sizes to have different debt financing dynamics over time. Due to the staggered nature of the shocks, I adopt the stacked regression approach proposed by Baker et al. (2022) to account for potential bias stemming from heterogeneous effects of bank commitments. Consistent with the findings of Kacperczyk and Peydró (2021), who study a sample

[4] Scope-1 emissions are direct greenhouse gas emissions that come from sources controlled or owned by a firm. The ten brown industries with the highest average scope-1 emissions are independent power and renewable electricity producers, electric Utilities, airlines, multi-utilities, metals & mining, oil& gas & consumable fuels, air freight & logistics, construction materials, paper & forest products, and chemicals.

of global firms, I find that high-emissions firms with committed lenders experience a reduction in syndicated loans from their relationship banks after these banks commit to SBTi, compared to low-emission firms with committed lenders. However, if high-emission firms were able to seamlessly substitute debt financing from committed relationship banks with alternative lenders, the commitments would not have a substantial impact on their debt financing. To shed light on whether this is the case, I estimate the triple-difference model at the firm level and show that SBTi commitments have a statistically significant and economically meaningful negative effect on high-emission firms' debt financing. More specifically, firms with carbon emissions one standard deviation higher than the mean experience a decline in total debt of about 7%, on average, after their relationship banks commit, relative to firms with similar levels of carbon emissions but without a divesting lender. Therefore, I provide empirical support for the use of banks' commitments to SBTi as credit shocks to firms that previously borrowed from them.

Next, I estimate how bank divestment affects the borrowing firms' patent filings. Specifically, I employ the triple-difference strategy, described above, to compare the patent filings by firms with and without committed relationship banks, before and after the commitments, across different levels of carbon emissions. Additionally, I control for the average level of the patent filings at the industry-year level to account for industry trends of the innovation activity. I first demonstrate that the relative filings of both green and non-green patents by firms with higher and lower carbon emissions within the group of firms with committed lenders exhibit similar trends to those of firms with higher and lower carbon emissions within the group of firms without committed lenders before 2015. This, in turn, lends support to the validity of my empirical strategy. Second, I provide evidence that bank divestment from high-emission firms has a negative effect on green innovation. Specifically, firms with emissions one standard deviation higher than the mean experience a decline in green patent filings of about 12.9%, on average, after their relationship banks commit to SBTi, compared to firms with similar levels of carbon emissions but without a committed lender. Conversely, firms with low emissions experience a relative increase in green patent filings following their relationship banks' commitments. However, I do not find any statistically significant change in the number of non-green patent filings by either high- or low-emission firms. These effects suggest that, after the commitments, high-emission firms shift their focus away from green innovation, while low-emission firms prioritize green innovation. These heterogeneous effects of bank divestment on green versus non-green patents are consistent with the fact that green technologies may require higher initial investment[5] or they may require more contributions from more technological fields and may therefore be associated with more uncertainty.

One concern about my identification strategy is that my results might be driven by endogenous matching between firms and committed banks. I show that firms matched to banks that eventually commit have higher total assets and scope-1 emissions before 2015 but are otherwise similar in terms of other firm-level characteristics such as R&D expenditure and profitability. To alleviate the concern of potential selection bias, I perform a matched-sample test where I require the treated and control firms to have a similar distribution of total assets and emissions. I show that the estimated effects of bank divestment on patent filings are largely unaffected. One might also worry that my findings are driven by multinational corporations changing their patent filings due to governmental or social pressure in countries where the committed banks are headquartered rather than bank divestment itself. I estimate the effect of bank divestment on patent filings on a sample of non-multinational corporations and obtain similar results, thereby alleviating the concern.

Next, I focus on firms in the high-emission industries – which are most affected by the divestment shocks – to explore three non-mutually exclusive economic mechanisms that could drive my findings. First, it is possible that the decrease in green patenting among high-emission firms following bank divestment is driven by an increase in green patenting by control firms. For instance, firms operating within high-emission industries might be presented with more green technology opportunities, but only those without committed lenders can fully capitalize on these prospects. However, given that the trend of green patenting among firms in the high-emission in-

[5] *See, e.g.,* Allcott and Greenstone (2012), Fowlie et al. (2018), and Accetturo et al. (2022).

dustries without committed lenders has also been declining, this particular mechanism is unlikely to be the primary driver of my research findings.

Second, a possible explanation for my findings is a financial constraint channel. That is, high-emission firms that rely on relationship banks to finance green projects now have fewer resources to allocate to these projects, or those that do not finance green projects using bank loans directly now have to divert internal cash reserved for green projects to more essential operations. To examine the importance of this channel, I estimate the heterogeneous effects of bank commitments on green patenting across varying levels of access to alternative financing. Consistent with the financial constraint channel, I find that the negative effect of bank commitments on green patent filings is considerably weaker for high-emission firms with more internal cash flow, external public equity issues, and access to the public bond market.

Third, the negative effects of bank divestment on green patent filings could also be driven by high-emission firms losing talent central to developing green technologies. Exploring this channel is challenging because an inventor can only be linked to a firm in the years when she files a patent. In other words, in the event that an inventor switches employers, the exact timing of the switch is not observable to the econometrician. To shed light on this mechanism, I first define green inventors as those who file at least one green patent over my sample period. Then for a given firm i and year t, I calculate the number of departing green inventors as the number of green inventors that file a patent for the last time with firm i in year t. Next, I use a difference-in-differences model to compare the number of departing green inventors in high-emission firms with and without committed relationship banks before and after the commitments. I find that the high-emission firms with committed relationship banks are not more likely to have departing green inventors after the commitments. However, patents filed by a green inventor staying in a firm with a committed lender in the high-emission industries are 12.4 percentage points less likely to be green after the commitments, compared to those filed by green inventors who stay in the same high-emission firms without committed lenders. I also calculate the number of onboarding green inventors, defined as the number of green inventors who file a patent with firm i for the first time in year $t + 1$. I find that high-emission firms with committed lenders have about 24% fewer onboarding green inventors compared to those without committed lenders. In comparison, firms with committed lenders in industries other than the ten with the highest carbon emissions have 14% more onboarding green inventors compared to those without committed lenders. Altogether, although my findings do not strongly support the hypothesis of talent attrition as an important driver explaining the effects of bank divestment on green patent filings, they do suggest that the results might be partially explained by high-emission firms recruiting fewer green inventors and low-emission firms hiring more green inventors following the commitments.

Considering that the decline in green patent filings by high-emission firms is at least partially offset by an increase in green patent filings by low-emission firms, the last part of my empirical analysis delves into the impact of bank divestment on the characteristics of green patents. Textbook investment theory predicts that high-emission firms drop low-quality projects first when they face financial constraints. In this case, the average value of green patents produced by high-emission firms may increase after their relationship banks divest. However, it could be difficult even for the inventors to rank innovation projects based on their expected values due to the high levels of uncertainty associated with these projects. Consequently, firms might opt to allocate fewer resources to each green project following bank divestment, resulting in a decrease in the average quality. I first compare the private value, measured as the stock market value developed in Kogan et al. (2017) and number of claims in the patent applications,[6] of green patents filed by firms with and without committed relationship banks before and after the lenders' initial commitments. I find that the private value of green patents filed by firms in high-emission industries declined following the commitments, whereas the private value of green patents filed by other firms remained relatively stable. Second, I estimate the effects of bank divestment on the social value of green patents – which is measured through metrics including forward citation, generality, and originality – and find that the number of citations and generality of green patents filed by high-emission firms declined after their relationship banks committed to SBTi. Finally,

[6] Kline et al. (2019) show that patent claims have strong predictive power for private values.

recognizing that there is substantial variation in the relevance of green patents to addressing climate challenges, I develop a continuous measure of a green patent's relevance to climate change mitigation using a textual algorithm. Specifically, I start off by identifying a set of green patents that contain keywords strongly associated with climate change mitigation in their titles and summaries. Then, for each green patent, I calculate its textual similarity with all the green patents that are known to be related to addressing climate challenges and use the maximum value as its relevance to mitigating the impact of climate change. Using this measure, I find that the relevance of green patents to addressing climate challenges also declined for high-emission firms following the commitments.

Taken together, my paper shows that when banks reduce their lending to high-emission firms, they discourage these firms from investing in green innovation. Concurrently, other borrowing firms with lending relationships to these committed banks increase their green patent filings. However, the value of newly filed green patents filed by high-emission firms, which, on average, is considerably higher than that of other firms, declines following the commitments. In contrast, the value of newly filed patents filed by low-emission firms does not seem to exhibit a corresponding increase. Considering the significance of green innovation to the quest for effective climate solutions, and the adverse impact of bank divestment on green innovation, bank divestment from high-emission firms, contrary to its ostensible objective, could potentially hinder progress towards the net-zero target.

My paper contributes to several strands of the literature. First, it adds to the growing body of research on the role of finance in driving climate-related outcomes. Much of the existing research in this literature focuses on divestment in the equity market and carbon emissions.[7] A number of recent papers including Degryse et al. (2020), Kacperczyk and Peydró (2021), Accetturo et al. (2022), Degryse et al. (2023), Green and Vallee (2023), and Giannetti et al. (2023) also focus on bank finance. Kacperczyk and Peydró (2021) take advantage of the SBTi commitments and find

[7]Exploiting the Boardroom Accountability Project, Naaraayanan et al. (2021), for instance, finds a negative impact of environmental activist investing on GHG emissions and toxic releases. See also Bellon (2020), Oehmke and Opp (2020), Broccardo et al. (2020), Choi et al. (2021), Berk and van Binsbergen (2021), Hartzmark and Shue (2023), and Gryglewicz et al. (2023).

that, following their lenders' commitments, firms with higher scope-1 emission levels receive less total bank credit but make only small adjustments to their carbon emissions. Green and Vallee (2023) focus on the coal industry and show that bank exit policies can lead to improvement in carbon emissions. My paper focuses on a distinct and important outcome, innovation, and shows that bank commitments can have unintended consequences in reallocating tasks of green innovation from more productive to less productive innovators. Accetturo et al. (2022) also study bank credit and green investment. My paper is distinct from theirs in two important ways. First, the credit shock in my setting is tied to the greenness of the borrowing firms, therefore providing different incentives for investment in different types of innovation. Secondly, they focus on green investment identified from the financial statements of the firms, whereas I focus on green patents filed by the firms for which I can observe the (change in) characteristics. My result that low-emission firms increase their green patent filings following the relationship banks' commitments to carbon neutrality is consistent with the findings in their paper. Using the values and content of the patents, I go a step further to show that the increase in bank credit allocated to low-emission firms has not improved their green patents' value or relevance to mitigating climate change impact.

In addition, my paper is related to the burgeoning literature studying the determinants of green innovations. Acemoglu et al. (2016), for instance, show that carbon taxes and research subsidies can encourage green innovation through the lens of an endogenous growth model. Andriosopoulos et al. (2022) show that green patents have no significant impact on the granted company's value or environmental score, suggesting that investors care little about green innovation on average. Using a dynamic model of carbon emission management, Bustamante and Zucchi (2022) illustrate that carbon pricing could disincentivize firms to invest in green innovation, and the effect could be mitigated by innovation subsidies. Cohen et al. (2020) show that firms in high-emission industries produce more green patents than firms in other industries. Bolton et al. (2022) find that firms with high emissions engage more in R&D that reduce carbon emissions through improving production process efficiencies, relative to R&D aiming to substitute carbon dioxide emitting technologies for carbon dioxide-free technologies. I contribute to the literature by developing a novel measure of a green patent's relevance to mitigating climate change impact using machine learning techniques. I show that green patents filed by firms in high-emission industries are more likely to be of high relevance to addressing climate challenges. In addition, my paper shows that bank finance could be an important determinant of both the quantity and characteristics of green patents.

More broadly, my paper relates to the vast literature on bank financing and innovation (e.g., Benfratello et al. (2008), Amore et al. (2013), Cornaggia et al. (2015), Hombert and Matray (2017), and Babina et al. (2022)). In terms of the economic question at hand, my paper is closest to Nanda and Nicholas (2014), who leverage geographic variation in bank distress during the Great Depression and find a negative relationship between bank distress and both the quantity and quality of firm-level innovation. My paper extends the existing literature by studying the differential effects of negative credit shocks on different types of innovation within the same firms.

1.2 Institutional Setting and Data

1.2.1 Institutional details

Since the inception of global fossil fuel divestment campaigns in 2011, a growing number of companies, including financial institutions, have made commitments to reduce their carbon footprints. As part of one of the most influential initiatives to coordinate climate actions, banks have undertaken staggered commitments through the Science Based Targets initiative (SBTi) to reduce the carbon emissions within their lending portfolios (Kacperczyk and Peydró (2021)). The SBTi is a partnership between the Carbon Disclosing Project, the UN Global Compact, the World Resources Institute, and the World Wide Fund for Nature. Its core mission is to mobilize the private sector towards establishing science-based targets geared towards achieving net-zero emissions reductions.

Since 2015, an increasing number of banks worldwide have joined SBTi. Table 1.1 provides a comprehensive list of the banks that joined SBTi by the end of 2019, along with the respective years they pledged their initial commitments. The formal procedure of joining SBTi entails that participating banks first commit to align their lending targets with the net zero emissions goal. Subsequently, they are mandated to submit their plans for SBTi's evaluation and approval. Throughout this process, SBTi actively provides guidance, which includes explicit recommendations for phasing out financial support to thermal coal as well as other high-emission firms. Ultimately, SBTi serves as the arbiter, evaluating and validating the plans submitted by the banks.

1.2.2 Data

The main dataset used in the paper combines several sources of data. My main sample consists of all U.S. public firms which rely on syndicated loans between 2011 and 2020. For these firms, I first obtain information on their accounting characteristics – including total assets, R&D spending, profitability, cash flow, and external equity financing details – from Compustat. Following the methodologies outlined in Chava and Roberts (2008), the firms are then matched to their respective lenders in the Dealscan dataset, which provides comprehensive details concerning the syndicated loan amount, lenders, and loan initiation dates. Within this set of lenders associated with the borrowing firms, I additionally identify those that have made commitments through the SBTi, drawing from the official list of committed companies available on the SBTi's website.[8] To enrich the dataset further, I incorporate data on the scope-1 carbon emissions of the firms, sourced from Trucost Environment. Additionally, to measure the innovation production of the firms, in line with the existing literature on innovation, I use data on the patents filed by each firm within the dataset. This patent data comes from the U.S. Patent and Trademark Office (USPTO) compiled by PatentsView and is matched to Compustat using the crosswalk constructed by Wharton Research Data Services, which covers all the patents granted between 2011 and 2019.

[8] For further information about the SBTi, please refer to their website: https://sciencebasedtargets.org/. See also Bolton and Kacperczyk (2021).

I further extend this crosswalk to include all patent applications filed and/or granted after 2019 using fuzzy-string matching algorithms. Lastly, for the valuation of patents based on stock market reactions, I utilize a stock-market-based patent value metric developed in Kogan et al. (2017) (henceforth KPSS). This comprehensive amalgamation of data sources forms the cornerstone of my analysis.

I categorize patents in the USPTO data as either "green" or "non-green" following the guidelines established by the OECD.[9] The OECD guidelines identify patent technological classes that are related to climate change adaptation, environmental management, and other environmental technologies and classify the patents associated with these technological classes as "green." In a robustness test, I use an alternative definition of green patents introduced in Bolton et al. (2022), who define green technologies to be those that may substitute carbon dioxide emitting technologies for carbon dioxide-free technologies, and obtain similar results.

It is worth noting that, within the category of green patents in my sample, there exists significant variation in their relevance to addressing climate challenges. For instance, while both fall within the technological category of "emissions abatement from stationary sources," a patent such as "CO2 removal device that includes a CO2 capturing material which captures H2O and CO2 in gas" is inherently more relevant to mitigating climate change compared to a patent such as "cross-flow heat exchanger." Hence, to discern the relevance of these patents to climate change mitigation, I develop a novel measure that leverages textual data from the abstracts and summaries of green patents. Specifically, I first identify a benchmark set of green patents that contain keywords strongly indicative of their relevance to climate change mitigation in their titles and summaries. These keywords, detailed in Figure 1.2, include terms such as "carbon-neutral," "global decarbonization," and "solar energy" and are taken from Sautner et al. (2020). In the second step, I transform the unstructured text from each patent's abstract and summary into a numerical representation using the term frequency-inverse document frequency (tf-idf) natural language processing technique. Third, I compute the cosine similarity between each green patent

[9] See Haščič and Migotto (2015) for details.

and all those in the benchmark set, resulting in similarity scores ranging from zero to one. Finally, I use the highest similarity score attained by each green patent as its measure of relevance to mitigating the impact of climate change. This approach enables me to conduct a more nuanced evaluation of the green patents' contribution to climate change mitigation.

In addition to assessing each patent's relevance to climate change mitigation, I construct several supplementary measures characterizing patent attributes using data from the USPTO. In particular, to gauge the extent to which a patent builds upon a firm's existing knowledge, I adopt the approach outlined in Brav et al. (2018). This involves constructing two binary variables, namely "explorative" and "exploitative." A patent is classified as "explorative" if at most 20% of the patents it cites originated from the firm itself or were cited by the firm within the five years preceding the patent's filing. Conversely, a patent is classified as "exploitative" if at least 80% of the patents it cites are linked to the firm's own inventions or citations within the same five-year window prior to filing. To measure the private value of a patent for the filing firm, I use the number of patent claims as a proxy, following the findings in Kline et al. (2019) that patent claims have strong predictive power for private values. As an alternative measure of the private value, I use the stock market value of the patent, as outlined in Kogan et al. (2017).[10] To gauge the social value of each patent, I utilize a set of metrics, including the number of forward citations, the originality index, and the generality index, as introduced in Hall et al. (2001).[11] The generality index, in this case, is defined as:

$$\text{Generality}_i = \frac{N}{N-1}(1 - \sum_{j \in J} s_{ij}^2)$$

where N is the total number of forward citations patent i receives, s_{ij} is the percentage of citations cited by patent i that belong to patent class j, and J is the set of patent classes associated with the citing patents.[12] The originality index measures the extent to which a patent builds on innovation

[10] It's worth noting that this stock market value is applicable to a subset of patents granted by 2020.

[11] It should be noted that Hall et al. (2001) also show that the generality and originality measures can be biased downwards when the number of citations is small. Thus, to address this potential bias, I implement the correction methods proposed in their work.

[12] When is patent application is associated with multiple technological classes, I use the first patent class that

in different technological fields and is defined in a similar way using backward citations.

1.2.3 Summary Statistics

The main sample for my analysis consists of 3,681 publicly traded innovative firms in the U.S., with 517 of them having data on their average scope-1 carbon emissions between 2013 and 2014. Throughout the paper, a firm is classified as "innovative" if it has filed at least one patent between 2011 and 2020. Table 1.3 presents descriptive statistics for the main sample. As demonstrated in panel A of the Table, firms in the ten industries with the highest emissions are, on average, older, larger, and have a higher ratio of green patents among all the patents they file. Panel B shows that green patents filed by firms in high-emission industries tend to have both higher private values and a greater number of forward citations compared to green patents filed by other firms. This pattern is consistent with the findings in Cohen et al. (2020), which suggest that high-emission firms have a comparative advantage in producing green innovation. Panel C of Table 1.3 demonstrates that green patents filed by high-emission firms are also more likely to be explorative and less likely to be exploitative, on average, when compared to non-green patents filed by these firms. In other words, green patents typically require more effort to explore beyond the technological areas central to the innovating firms. Figure 1.2 provides further insight into the distribution of the relevance of these green patents to climate change mitigation, by firms in the high-emission industries and other firms. The figure shows that, on average, green patents filed by firms in high-emission industries have a similar level of relevance to mitigating the impact of climate change as other firms, with a higher density on the right tail of the distribution.

1.3 Commitments to SBTi as Credit Shocks

The main objective of this paper is to assess the impact of banks on the green innovation of their borrowing firms when the banks reduce lending to firms with high carbon emissions. This is achieved by leveraging banks' commitments to carbon neutrality through the SBTi as credit

appears on the application.

shocks to the borrowing firms. To lend support to the validity of the research design, I begin by examining whether and how debt financing from relationship banks changes following their commitments to SBTi.

Lender-Firm-Year Level Analysis

I start off my empirical analysis by examining the relationship between debt financing and the SBTi commitments made by firms' relationship banks. This analysis focuses on firms for which I have data on their pre-commitment carbon emissions and employs the following triple-difference regression:

$$\begin{aligned} log(Debt)_{ijt} = \beta_1 Committed_{jt} + \beta_2 Committed_{jt} \times log(Emission_i) \\ + \beta_3 Post_t \times log(Emission_i) + Controls_{ijt} + \epsilon_{ijt} \end{aligned} \tag{1.1}$$

where the dependent variable, $log(Debt)_{ijt}$, represents the natural logarithm of the syndicated loan from lender j to firm i in year t. $Committed_{jt}$ is a binary variable that equals one if lender j has committed to SBTi by year t. Additionally, $log(Emission)_i$ corresponds to the natural logarithm of the average scope-1 carbon emission by firm i between 2013 and 2014. The coefficient β_2 is the key variable of interest, which captures the relationship between lenders' SBTi commitments and their lending activities towards borrowing firms, contingent upon the borrowing firms' pre-commitment carbon emissions. $Post_t$ is a dummy variable that equals one starting from the year 2015 and zero otherwise. It should be noted that, by controlling for the interaction of $Post_t$ and $log(Emission)_i$, I am allowing firms with different levels of carbon emissions to experience distinct trajectories of debt financing from 2015 onwards – with 2015 being the first year when lenders committed to SBTi and the year the Paris Agreement was signed. This adjustment acknowledges changes in the regulatory landscape and lenders' preferences, which may not align precisely with their SBTi commitments. $Controls_{ijt}$ includes firm-, lender-, and year-level fixed effects. Firm fixed effects absorbs firm-level pre-commitment carbon emissions and additional time-invariant firm characteristics influencing debt issuance, such as industry and location. Lender fixed effects account for lenders' time-invariant characteristics that are correlated with their credit supply. And year fixed effects are included to accommodate any macroeconomic shocks affecting debt financing. In the most stringent specification, I control for lender-firm fixed effects which allows banks that eventaully commit to have different lending preference from those that do not based on client firms' carbon footprint. In addition, this ensures that the estimated relationship between commitments and debt issuance is driven by comparing debt financing over time within the same lender-firm pairs. Furthermore, I include the interaction of firm size categories and year dummies, thereby allowing firms of different sizes to exhibit distinct dynamics in securing debt financing over time.

To sum up, equation 1.1 compares syndicated loan financing within firms before and after their relationship banks commit to SBTi, across firms matched to banks that eventually commit and to those that do not, and across firms with various levels of pre-2015 carbon emissions. It is labeled as a triple-difference specification to emphasize these three dimensions that are used to compare the firms in this analysis.

I also examine whether debt financing from committed lenders correlates with green patent filings among firms with high carbon emissions by estimating the following difference-in-differences regression analysis within the subset of firms in the highest quartile of pre-commitment carbon emissions:

$$log(Debt)_{ijt} = \beta_1 Committed_{jt} + \beta_2 Committed_{jt} \times \text{GreenPatent}_i + Controls_{ijt} + \epsilon_{ijt} \quad (1.2)$$

where GreenPatent_i is a binary variable that equals 1 if firm i ever files any green patent between 2011 and 2014.

Recent studies in the econometrics literature have highlighted potential bias in difference-in-differences analysis with staggered treatment timing in the presence of treatment effect heterogeneity (Goodman-Bacon (2021); Callaway and Sant'Anna (2021); Borusyak and Jaravel (2017); Sun and Abraham (2021)). Several factors could contribute to heterogeneity in the effects of SBTi commitments on debt financing. For instance, it's possible that banks joining SBTi earliest did so under greater pressure from their institutional investors. Consequently, high-emission firms previously borrowing from these early-joining banks might have experienced more substantial reductions in debt financing compared to other high-emission firms with relationship banks that joined SBTi later. To account for this potential bias, I adopt the stacked regression approach proposed by Baker et al. (2022) (hereafter BLW). Specifically, I create commitment-year-specific datasets by including both firms from each cohort h and all other "clean control" firms, i.e., those experiencing no changes in the commitment status of their relationship lenders within the estimation window. These cohort-specific datasets are then stacked to create the final dataset. Using this final dataset, I then proceed to estimate the model to calculate the average treatment effect of the commitments, including cohort fixed effects interacted with year and lender fixed effects.

The results of these regressions are presented in Table 1.4, with the independent variables standardized. Standard errors are double clustered at the lender and year levels. Columns 1-3 of the Table demonstrate that firms with higher pre-commitment carbon emissions tend to experience lower debt issuance following their lenders' commitments to SBTi, compared to firms with lower carbon emissions. Furthermore, among firms with high carbon emissions, those with committed lenders generally receive lower debt financing after their lenders' commitments, as opposed to high-emission firms without committed lenders. In particular, in my preferred specification in column 3, the estimates suggest that firms with pre-commitment emissions one standard deviation higher than the sample mean typically receive around 20% less debt financing after their relationship lenders commit, relative to firms with similar levels of carbon emissions but without committed lenders. Conversely, firms with carbon emissions lower than one standard deviation from the mean generally observe an increase in lending after their lenders' commitments. Estimates in columns 4 and 5, on the other hand, show that, among firms with high carbon emissions, there does not seem to be enough compelling evidence indicating that having filed green patents is linked to receiving different amounts of debt financing from committed lenders.

Overall, our findings in Table 1.4 indicate that banks tend to lower their lending to firms with high carbon-emission firms after they make pledges to be aligned with the net zero target.

They are also consistent with recent studies (Kacperczyk and Peydró (2021) and Green and Vallee (2023)) that focus on a sample of global firms with syndicated loans.

Firm-Year Level Analysis

A potential issue with the firm-lender level analysis is that my results might be driven by changes in the composition of borrowers and lenders over time. To address this concern, I proceed to estimate the effect of SBTi commitments on total debt across various levels of pre-commitment carbon emissions at the firm level using a triple-differences model, similar to the one presented in equation (1.1). To ensure that the estimated results will not be driven by changes in the composition of firms, I require firms to have non-missing total debt throughout the period of 2011 to 2021 to be included in this analysis. I adopt the stacked regression approach proposed by Baker et al. (2022) to account for potential bias stemming from heterogeneous effects of bank commitments. The specification is as follows:

$$\begin{aligned} log(Debt)_{it} = \beta_1 Committed_{it} + \beta_2 Committed_{it} \times log(Emission_i) \\ + \beta_3 Post_t \times log(Emission_i) + Controls_{it} + \epsilon_{it} \end{aligned} \tag{1.3}$$

where i and t denote firm i and year t, respectively. $Committed_{it}$ is a binary variable that equals one if firm i has at least one relationship lender that has committed to SBTi by year t. $Control_{it}$ includes firm and year fixed effects. For robustness, I use each firm's exposure to bank commitments, defined as the share of syndicated loan volume between 2009 and 2014 from committed relationship banks, as an alternative treatment variable.[13]

This exercise also allows me to study the equilibrium effect of lender commitment on the borrowing firms' level of debt. For example, even if committed lenders allocate less credit to or completely phase out from firms with high carbon emissions, if the affected firms could swiftly substitute lending from committed relationship banks from other banks or non-bank lenders, then we would see little effect on the total debt at the firm level. In other words, lenders' commitments to SBTi do not constitute credit shocks to the borrowing firms. A number of capital market

[13] The measure is similar to the shift-share instrument in Green and Vallee (2023).

frictions can prevent high-emissions firms from perfectly substituting debt financing after their relationship banks commit to SBTi and lower their lending. For example, the cost of capital from new lenders may be higher because relationship lenders offer better terms because of lower information asymmetry (see e.g., Boot and Thakor (1994), Bolton et al. (2016), Beck et al. (2018), Banerjee et al. (2021)); bank credit supply can be constrained (e.g., Paravisini (2008), Gilje et al. (2016), Drechsler et al. (2017)), and therefore, banks with an appetite for firms with high carbon emissions may not be able to take up the opportunities even if they face increasing demand from these firms. High-emission firms with committed lenders may also struggle to substitute lending from divesting lenders due to the preferences of other lenders. For example, other banks may risk devaluating existing positions held with incumbent clients in the same industries. In addition, if banks anticipate increasing scrutiny of carbon exposure of their lending activities, they may also become reluctant to finance firms negatively affected by the divestment even if they do not make the pledges to divest themselves. Nonetheless, compared to firms with high carbon emissions in other countries, firms in the U.S., which are the focus of this paper, may face less debt market frictions, especially after the U.S. withdrew from the Paris Agreement in 2017.

The main identification assumption underlying my triple-difference model is that the relative debt levels of firms with higher and lower carbon emissions among the group of firms with committed lenders trend in the same way as the relative debt of firms with higher and lower carbon emissions among the group of firms without committed lenders, in the absence of these commitments (Olden and Møen (2022)). This assumption may be violated if the banks committing to SBTi are mainly those that were already in the process of reducing their carbon exposure in lending activities. To assess the validity of this assumption, I plot the coefficients from a linear regression of the natural logarithm of total debt on the interactions between year dummies and the natural logarithm of carbon emissions in Figure 1.3. These regressions are conducted separately for firms with committed lenders and those without committed lenders. The results support the validity of the assumption and reveal that there is no statistically significant difference in total debt levels across different levels of carbon emissions prior to 2015 when the initial

SBTi commitments took place. Furthermore, the plot for firms with committed lenders suggests that the negative effect of commitment on the total debt of high-emission firms relative to other firms appears to persist over time. This observation suggests that the substitution of credit from committed relationship banks remains limited within the time frame considered in this paper.

Table 1.5 presents the point estimates from equation (1.3). Standard errors are double clustered at the firm and year levels.[14] Columns 1 and 2 show that high-emission firms experience a reduction in their total debt when their relationship lenders commit to SBTi, as compared to firms with similar carbon emissions but without committed lenders. To put it differently, high-emission firms that have committed relationship lenders experience a decline in total debt following these commitments, relative to firms with committed lenders but lower levels of emissions. Specifically, in my preferred specification given in column 2, where I also allow firms of different sizes to have different dynamics of debt issuance by controlling for size quartile-year fixed effects,[15] I find that firms with carbon emissions one standard deviation above the mean, on average, experience a decrease in total debt of approximately 6.9% after their relationship banks commit to SBTi, compared to firms with the same carbon emissions but without committed lenders. Similarly, as shown in columns 3 and 4 where the treatment variable is the shift-share instrument $exposure_it$ defined above, firms with higher-than-average carbon emissions experience a greater relative decline in total debt if a larger fraction of their debt comes from committed relationship banks. Specifically, the point estimate in column 4, which incorporates size quartile-year fixed effects, suggests that firms with carbon emissions one standard deviation higher than the mean observe a 5.9% reduction in total debt when 10% of their debt financing comes from committed relationship banks. Altogether, the effects in Table 1.5 are both economically significant and are consistent with the findings of Kacperczyk and Peydró (2021) and Green and Vallee (2023) in terms of magnitude.

[14]Standard errors in all subsequent analyses presented in the paper are clustered at both the firm and year levels.
[15]The size bin for each firm is determined based on its average total assets prior to 2015.

1.4 The Effect of Bank Commitments on Patent Filings

Given the evidence indicating that bank commitments result in decreased lending to high carbon-emission firms – that are not perfectly recovered by these affected firms – I turn to examining the impact of these commitments on borrowing firms' patent filings.

Conceptual Framework

There is a growing body of evidence suggesting that bank financing has a substantial impact on innovation,[16] even for young firms without much tangible collateral to pledge (Robb and Robinson (2014)). The consensus in the literature is that there is a positive correlation between access to bank financing and innovation. Importantly, firms do not necessarily need to issue debt directly to fund their innovation projects for bank finance to play a significant role. For instance, even if firms typically fund their innovation endeavors with internal cash flows, a reduction in bank lending can compel them to allocate more of their internal cash to essential operations, thereby reducing their capacity for innovation.

In the context of banks reducing lending to firms with high carbon emissions, its effect on green innovation is, however, ambiguous. On the one hand, the cost of financing innovation will increase if the affected firms cannot substitute bank finance with alternative sources of financing due to capital market frictions. In this case, the green innovation projects become less profitable, and the firms have less incentive to invest in green innovation. On the other hand, divestment also increases the marginal cost of production, irrespective of the decision to invest in green innovation. However, the magnitude of this cost increase may be mitigated if the firm engages in green innovation because such efforts could partially offset the rise in capital costs by improving the firm's carbon footprint. Therefore, in contrast to other forms of innovation, bank divestment could offer high-emission firms added incentives to pursue green innovation. Therefore, the ultimate effect of divestment on green innovation by firms with high carbon emissions depends on which of these two opposing forces dominates. In Appendix A.1, I formalize these arguments

[16] *See e.g.,* Nanda and Nicholas (2014), Mann (2018).

using a reduced form model that builds upon the vertical differentiation framework in Tirole (1988).

Firm-level Patent Filings

To empirically estimate the effects of bank commitments on patent filings, I employ a triple-difference Poisson regression model similar to equation (1.3) which compares patent filings within firms before and after their relationship banks commit to SBTi, across firms matched to banks that eventually commit and to those that do not, and across firms with various levels of carbon emissions:

$$\begin{aligned} PatentFiling_{it} = exp[&\beta_1 Committed_{it} + \beta_2 Committed_{it} \times log(Emission_i) \\ &+ \beta_3 Post_t \times log(Emission_i) + Controls_{it} + \epsilon_{it}] \end{aligned} \tag{1.4}$$

where $PatentFiling_{it}$ is the number of green (non-green) patents filed by firm i in year t, and the remaining variables are defined similarly to equation (1.3). In the model, I also use each firm's exposure to relationship banks' commitments defined in section 1.3 as an alternative treatment variable. $Controls_{it}$ includes firm and year fixed effects. Firm fixed effects absorbs pre-commitment average emissions, whether a firm is matched to a relationship bank that eventually commits, and other time-invariant firm characteristics that may influence patenting, such as the firm's industry, and year-fixed effects account for macro-level shocks affecting patent activity, such as changes in R&D tax credit policies and international agreements such as the Paris Agreement. I also control for the average patent filings at the SIC3-year level to take into account the fact that different industries may exhibit distinct patenting dynamics that are unrelated to bank divestment. Given the count-based nature of the dependent variables of interest, I choose to adopt a Poisson model in equation (1.4). An additional advantage of the Poisson model is that all firms with at least one positive value of the dependent variable during the sample period are included in the estimation, therefore fixing the composition of firms each year. Furthermore, to account for the potential bias caused by heterogeneous effect across firms with different treatment timing, I adopt the stacked regression approach proposed by Baker et al. (2022) when estimating the model.

Similar to the analysis for the effects of commitments on total debt, the identification assumption underlying the triple-difference model in equation (1.4) is that the relative patent filings of firms with higher carbon emissions and lower carbon emissions among the group of firms with committed lenders trend in the same way as the relative patent filings of firms with higher and lower carbon emissions among the group of firms without committed lenders, in the absence of commitments (Olden and Møen (2022)). Figures 1.4 and 1.5 provide evidence in support of the validity of this assumption. Specifically, these figures plot the coefficients derived from a Poisson regression of patent filings on the interactions between year dummies and the logarithm of carbon emissions for firms with and without committed lenders and show that there is no statistically significant difference in patent filings across various levels of carbon emissions prior to initial commitments.

Table 1.6 presents the point estimates derived from equation (1.4). Column 1 shows that high-emission firms tend to file fewer green patents when their relationship lenders commit to SBTi, in comparison to firms with similar carbon emissions levels but without committed lenders. Put differently, high-emission firms with committed relationship lenders experience a decrease in green patent filings following these commitments, relative to firms with lower emissions and committed lenders. To be more specific, firms with carbon emissions one standard deviation higher than the mean experience an average decline of about 12.9% in green patent filings after their relationship banks commit to SBTi, compared to firms with similar carbon emissions levels but without committed lenders. However, as shown in column 2, there is no statistically significant change in borrowing firms' non-green patent filings after the commitments. Similar to column 1, in column 3, we show that firms with higher-than-average carbon emissions experience a greater relative reduction in green patent filings if a larger fraction of their debt comes from committed relationship banks. In particular, the point estimates in column 3 imply that firms with carbon emissions one standard deviation higher than the mean experience a 12.6% decline in green patent filings if 10% of their debt financing comes from committed relationship banks.

Altogether, my findings suggest that the commitments made by relationship banks to achieve carbon neutrality have varying effects on different types of patents produced by borrowing high-emission firms. There are several potential factors contributing to the observed decrease in green innovation among high-emission firms facing divestment. For instance, green investments may require higher upfront expenditure compared to investments in non-green technologies.[17] Additionally, as indicated in panel C of Table 1.3, green innovation may demand more effort from high-emission firms to explore beyond their central technological areas, potentially introducing more uncertainty into the process.

Potential Selection Bias

One concern about my identification strategy is that firms with and without committed relationship banks may be different, and the estimated effects I find may be driven by characteristics in which they are different rather than relationship bank commitments. For example, banks might be more likely to commit to carbon neutrality when their borrowing high-emission firms are less profitable or invest less in R&D, and lower profitability and R&D expenditure could lead to lower green patent filings following the commitments even if the committed banks do not cut their lending. To address this concern, I start off by estimating how banks' likelihood to commit to SBTi depends on their borrowing firms' characteristics using the following linear regression:

$$Commit_j = \text{Characteristics}_j + \epsilon_j, \tag{1.5}$$

where $Commit_j$ is a binary variable that equals one if bank j eventually commits to SBTi and zero otherwise, and Characteristics_j denotes the average characteristics of firms borrowing from bank j between 2011 and 2014. As shown in Table 1.7, firms that borrow from committed banks tend to be larger in terms of total assets. However, after controlling for total assets, I find no statistically significant differences in various firm characteristics that could be correlated with green innovation between firms with committed relationship banks and those without committed lenders. These characteristics include total employment, profitability, R&D expenditure, and

[17] *See e.g.,* Allcott and Greenstone (2012), Fowlie et al. (2018), and Accetturo et al. (2022).

green patent filings. However, there is an exception observed in scope-1 carbon emissions. Firms with committed relationship banks exhibit, on average, higher scope-1 emissions. This finding aligns with the proposition that banks with substantial carbon exposure in their lending activities may experience heightened pressure from investors and governments, prompting them to commit to carbon neutrality. To alleviate the concern that my results might be driven by endogenous matching between committed banks and firms with larger sizes and higher emissions, I then employ a matching procedure that pairs each firm with committed lenders to a firm without committed lenders that have the natural logarithm of total assets and scope-1 carbon emissions at most 1% larger or smaller. This construction results in a matched sample where treated and control firms exhibit a comparable distribution of total assets. Subsequently, I estimate equation (1.4) using this matched sample. Table 1.8 presents the results. Overall, the estimated effects of bank divestment on patent filings remain largely unchanged.

In Appendix A.2, I additionally address a related concern that my results might be driven by multinational corporations (MNCs) that operate in countries where the committed banks are headquartered. Indeed, a large share of high-emission firms in my sample are multinational corporations. If the high-emission MNCs receive similar pressure as banks headquartered in those countries, which might induce banks to commit to SBTi, the decrease in their green patent filings could be due to governmental or social pressures rather than relationship bank divestment.[18] I examine the relevance of this concern by estimating equation (1.4) on a sample of non-MNCs and obtain similar results.

Additional Analyses

I conduct a number of additional tests in Appendix A.2 to gain a more detailed understanding of the effects of bank commitments on high-emission firms' investment in green innovation. First, I estimate these effects separately at the intensive and extensive margins. I find statistically significant evidence that the effects of commitments on green innovation occur at the intensive

[18]Consistent with this scenario, Bustamante and Zucchi (2022) show that carbon taxes could induce high-emission firms to tilt towards transient green investment from green innovation using a dynamic model of carbon emission management.

margin. That is, high-emission firms that were actively producing green patents experienced a reduction in green patent filings after their relationship banks committed to carbon neutrality through SBTi. The estimated effect is also negative for the extensive margin, suggesting that some high-emission firms, which had previously filed green patents, might cease green patent filings altogether following their relationship banks' divestment. However, the estimated coefficients are insignificant at conventional levels.

I also examine the robustness of my findings to an alternative definition of green patents. While the definition of green patent by OECD has been the standard measure used in the literature, a recent paper by Bolton et al. (2022) (BKW) proposed an alternative measure, which uses a more stringent definition of green patents and includes only "technologies that may substitute carbon dioxide emitting technologies for carbon dioxide-free technologies" and separates them from "brown efficiency technologies." For instance, the green patent definition by BKW excludes technologies associated with energy efficient lighting and emissions abatement from stationary sources which the OECD definition includes, while including charging of electric vehicles, measurement of electricity consumption, and nuclear engineering, which are excluded by the OECD definition. In Figure 1.6, I show that there is a positive correlation between the BKW-defined green patenting at the firm level and the OECD-defined green patenting. In Appendix A.2, I re-estimate equation (1.4) using the BKW definition of green patents. This analysis reveals that the effects of bank divestment on green patent filings remain consistent when employing the BKW definition of green patents.

High-emission Industries

One limitation of the analysis from equation (1.4) is that many firms do not disclose their carbon footprints during the period I focus on in this paper. As argued in Matsumura et al. (2014), conditional on voluntarily disclosing carbon emissions, firm value is negatively associated with carbon emissions. Therefore, firms with high carbon emissions may have strong incentives not to disclose their carbon footprint absent regulatory requirements. Indeed, as shown in Figure 1.7, a large fraction of firms in the fossil fuel industries do not report their scope-1 emissions in my data before 2015. In particular, there are high-emission firms that invested heavily in green innovation and had committed relationship banks among the firms that did not report their carbon emissions, such as Enterprise Products. To address this limitation, I estimate the average effect of bank commitment on the patent filings of firms in the ten industries that have the highest scope-1 carbon emissions: independent power and renewable electricity producers, electric utilities, airlines, multi-utilities, metals & mining, oil& gas & consumable fuels, air freight & logistics, construction materials, paper & forest products, and chemicals, which are likely the target of bank divestment. The specification is as follows:

$$PatentFiling_{it} = exp[\beta \text{Committed}_{it} + \text{Controls}_{it} + \epsilon_{it}], \tag{1.6}$$

where $PatentFiling_{it}$ is the number of patents filed by firm i in year t before or after the commitment year. Commit_i denotes a binary variable equal to 1 if firm i is connected a lender that eventually joins SBTi. $Controls_{it}$ includes year and firm fixed effects. In addition, I control for lagged firm size and R&D expenditures to mitigate the potential selection bias, and industry trend of patent filings. To account for the potential bias caused by the heterogeneous effects of banks' divestment on patent filings, I use the BLW stacked regression approach. The identification assumptions are that firms do not anticipate and react to relationship banks' commitments before the commitments take place, and absent the commitments, the patent filings by firms with committed lenders trend the same way as the patent filings by those without committed lenders. To lend support to the identification assumptions, I estimate the event-study version of equation (1.6), i.e., the binary treatment variable *Committed* is replaced with a collection of variables $\sum_{t=-5}^{3} \text{Treated}_i \times \delta_t$, where Treated is dummy variable that equals one if the firm has a relationship bank that joins SBTi eventually and zero otherwise, and δ_t is a dummy variable that equals to one t years after (or before if t is negative) the commitment year and zero otherwise. As shown in Figure 1.8 below, there is no significant difference between the trend of either green or non-green patents filed by firms in high-emission industries with committed relationship banks and those filed by firms in high-emission industries without committed relationship banks before the treatment. This lends support to the validity of my identification strategy.

I report estimated coefficients from the formal estimation of equation (1.6) in Table 1.9. The point estimate in column 1 suggests that firms in high-emission industries reduce the number of green patent filings by about 39% after their relationship bank joins SBTi and subsequently decreases its lending to the borrowing firms in high-emission industries. However, as shown in column 3, such a pattern is not present for other types of patents. Overall, while the estimates in Table 1.9 are qualitatively consistent with the findings in Table 1.6, the magnitude of the effects of the commitments on green patent filings is larger. This suggests that the treatment effects may be stronger for some firms in high-emission industries that did not disclose their carbon emissions. To provide a comparison, in columns 2 and 4 of Table 1.9, I estimate the average effect of bank commitments on patent filings for firms in industries other than the ten with the highest carbon emissions. Consistent with the estimates in Table 1.6, I show that, when firms outside the high-emission industries receive more credit from the committed lenders, they prioritize green innovation over non-green innovation. Therefore, my findings suggest that the decrease in green innovation in high-emission sectors is at least partially offset by the increase in green innovation in other sectors.

1.5 Mechanisms

So far, I have shown evidence suggesting that decreasing lending to high-emission firms by relationship banks leads to less green patenting by the firms relative to firms without committed relationship banks. The observed effects could be potentially driven by several non-mutually exclusive channels. In this section, I aim to empirically assess the significance of these channels to better understand the underlying mechanisms behind my findings.

First, the estimated effects I document in the previous section could be driven by an increase in green patent filings by the control firms instead of a decrease in green patent filings by the treated firms. An economic mechanism consistent with this possibility could be that coinciding with the beginning of the banks' commitment waves, firms in the high-emission sectors face increasing return to investment in green technology due to the change in business environment associated with the Paris Agreement. In this case, firms in the high-emission industries whose relationship banks did not commit to carbon neutrality and reduce their lending were able to seize the business opportunity and increase their green patent filings. On the other hand, the high-emission firms with committed banks were not able to invest in green technologies due to their relationship banks' divestment. To shed light on the relevance of this mechanism, I plot the dynamics of green patent filings separately for firms in the ten industries with the highest carbon emissions with and without committed lenders in Figure 1.9. As shown in the figure, both the treated and control firms in the high-emission industries were on a downward trajectory in terms of green patent filings, with the trend being steeper for those with committed lenders after the commitments. To this end, the Figure does not seem to support the notion that increasing business opportunities in high-emission industries were a significant driving force behind my findings.

Dependence on the Relationship Banks' Finance

Given the nature of the bank divestment, the financial constraint channel is a probable explanation behind the findings documented in Section 1.4. That is, if firms in the high-emission industries are not able to substitute other financing for the credit from the divesting relationship banks, they would have less capital to allocate to green projects. Importantly, even if these firms typically rely on internal cash instead of debt financing for financing innovation projects, bank divestment may force them to divert cash from green projects to core operations. If the financial constraint channel is indeed a major mechanism driving my results, we should expect the treatment effects to be weaker for high-emission firms with better access to alternative sources of capital and less dependent on the relationship banks' finance. To test this hypothesis, I examine three different sources of alternative financing: internal cash flow, external equity financing, and access to the public bond markets. More specifically, I estimate equation (1.6), additionally including the interaction term of bank commitment and a binary variable that equals one if a firm is defined to have a "high" level of internal cash flow, external equity financing, or access to the public bond markets, on innovative firms in the ten industries with the highest carbon emissions. A firm is defined to have high cash flow if its average cash flow from 2011 to 2014 is in the top 30% among all the innovative high-emission firms; a firm is defined to have high external equity financing if its external equity issuance from 2011 to 2014 is in the top 30% among all the innovative firms in the high-emission industries; and a firm is defined to have access to the public bond market if it had bond ratings from 2011 to 2014. To mitigate the potential concern that cash flow or external equity issuance is driven by having greater business opportunities or profitability that could also affect patent filings, I additionally control for the interactions of bank commitments and a firm's average Tobin's Q between 2011 and 2014.

Table 1.10 presents the estimates of the heterogenous effects across firms with varying degrees of reliance on relationship banks' financing. Column 1 suggests that the adverse impact of bank commitment on green innovation is driven by firms with lower cash flow. Specifically, firms ranking in the top 30% percentile of cash flow experience only an 8% reduction in green patent filings compared to those without committed lenders. This is in contrast to the nearly 70% relative decline in green patent filings experienced by other high-emission firms. Analogously, columns 2 and 3 demonstrate that the negative effect of bank divestment on green patent filings by high-emission firms is driven by those firms that have raised less capital from the external equity market and those without bond ratings. These findings collectively suggest that the decline in the level of green innovation among firms in high-emission industries is less pronounced for those that are less dependent on financing from their relationship banks.

Inventor Reallocation

Finally, the decline in green patenting could also be attributed to borrowing firms having fewer inventors who are pivotal in generating green innovations. For instance, in the labor and finance literature, it has been shown that firms tend to reduce their workforce when facing deteriorating financial health.[19] Additionally, talented employees, who consider compensation and job risk in maximizing their utility, may seek employment elsewhere when their current employers' financial well-being declines,[20] even when the firms are not on the verge of bankruptcy (Gortmaker et al. (2022)). Moreover, Brown and Matsa (2016) demonstrate that financial distress can lead to a reduction in both the quantity and quality of job applications. In the context of bank divestment and green innovation, if skilled employees who play crucial roles in developing green technologies depart from their firms following the divestment by relationship banks, or if divested firms encounter difficulties in attracting talent for green projects, it would naturally result in a decrease in green patent filings. Importantly, the impact on green innovation, in this case, could be more enduring compared to cases in which the effects solely stem from high-emission firms temporarily reallocating their capital away from green projects towards core operations.

An important challenge to studying the inventor mobility channel arises from the fact that an inventor can only be linked to a firm during the years she files a patent. In other words, the precise timing of an inventor switching employers remains unobservable to econometricians. A common approach to address this challenge in the literature is to re-define each observation in the analysis to be a pair of subsequent patent filings by a specific inventor, using the midpoint between the two filing years as the estimated year of the job transition (Hombert and Matray (2017)). However, this approach might bias my estimates since the switching time is measured with errors. Hence, to shed light on the inventor mobility channel, I adopt a different approach. In particular, I first define green inventors as those who filed at least one green patent over my sample period. Then for a given firm i in year t, I calculate the number of its departing green inventors as the number of green inventors who filed their last patent with firm i in year t. In other words, these inventors were employed by firm i in year t and never filed another patent with the same firm thereafter.[21] Then I use the same stacked Poisson difference-in-differences

[19] *See e.g.,* Berton et al. (2018), Caggese et al. (2019), and Benmelech et al. (2021).

[20] *See e.g.,* Baghai et al. (2019) and Babina (2020).

[21] The measure includes green inventors who stay with the same employers but never file another patent. In an unreported robustness test, I include only those who file a patent with a different firm after year t and obtain similar results.

specification, given in equation (1.6), to estimate the effect of bank commitments on the number of offboarding green inventors, separately for firms in the ten high-emission industries and other firms. Additionally, for each firm i in year t, I calculate the number of its onboarding green inventors – defined as those who file a patent with firm i for the first time in year $t+1$ – and estimate the effects of bank divestment on the number of onboarding green inventors using the same Poisson difference-in-differences model. Finally, I examine the effect of bank divestment on project assignments to green inventors who remained employed by the same high-emission firms throughout my sample period. To do this, I estimate a linear difference-in-differences regression model using a binary variable that equals one if a patent filed by a specific green inventor is green and zero otherwise as the dependent variable. This analysis focuses on the sample of all patents filed by green inventors who remained with the same high-emission firms.

Table 1.11 presents the estimates. As shown in columns 1 and 2, the estimated coefficients in front of $Committed$ are negative for firms in high-emission industries and positive for firms in other industries, respectively, and are insignificant at the 10% level. They suggest that green inventors working in high-emission industries do not seem more likely to leave their employers following relationship banks' commitments, and those working in other industries do not seem more likely to be retained following the commitments. Column 5, however, suggests that patents filed by green inventors who remained with the same firm throughout the sample period are less likely to be green after the relationship banks' commitments. Specifically, patents filed by green inventors who stayed with a firm having a committed lender in the high-emission industries were 12.4 percentage points less likely to be green after the commitments compared to those filed by green inventors who stayed with similar high-emission firms without committed lenders. This implies that while green inventors do not show an increased likelihood of leaving their employers after the bank commitments, they may be assigned to fewer green projects. Columns 3 and 4 of the Table, on the other hand, show statistically and economically significant effects of bank commitments on the number of onboarding green inventors. In particular, column 3 shows a 24.1% reduction in the number of onboarding green inventors for firms in the high-emission industries, while column 4 demonstrates a 14.4% increase for firms in other industries. Taken together, these findings suggest that bank divestment may lead to a reduction in the number of green inventors for firms in high-emission industries and an increase in the number of green inventors for firms in other industries. This suggests that inventor mobility could be a significant mechanism behind the observed effects of bank divestment on green patent filings.

1.6 Effect on the Characteristics of Green Patents

Given that the decline in investment in green innovation by the polluting firms is at least partially offset by the increase in investment by the firms with lower carbon emissions, it is important to understand whether and how bank divestment affects the characteristics of green innovation being produced. For example, do firms with higher carbon emissions start producing lower-quality patents that generate less spillover to other innovations after their relationship banks commit to SBTi and divest from them? Do the firms with lower carbon emissions and committed lenders, which receive more credit from the lenders after the commitments, produce green patents that are truly relevant to achieving the net-zero target? When it comes to the relationship between access to credit and the quality of innovation, there could be several different economic mechanisms in place that lead to opposite predictions in the context of bank divestment from firms with high carbon emissions. First, if high-emission firms drop the low-quality green projects first when they start receiving lower bank credit, consistent with a cleansing mechanism (see e.g., Caballero and Hammour (1991), Osotimehin and Pappadà (2017)), we might see an increase in the average quality of green innovation after their lenders commit to SBTi and divest from them. On the other hand, even the inventors may find it challenging to systematically prioritizing innovation projects due to their inherent substantial uncertainty. As a result, firms may choose to allocate diminished resources to individual green projects following bank divestment, potentially leading to a decline in the overall project quality. At the same time, by reducing credit to firms with high carbon emissions, which concentrate in a number of industries, lenders may become less diversified in terms of their loan portfolio and hesitate to finance riskier projects such as green innovation, leading to a decline in the novelty of the green patents filed by firms with committed lenders. To identify the impact of bank commitments on the characteristics of green patents filed by borrowing firms, in this section, I estimate the same stacked difference-in-differences regression as equation (1.6) at the patent level with various patent characteristics as the dependent variable, separately for firms in the ten high-emission industries and other industries.

1.6.1 Private Value of Green Patents

I first examine the impact of reduced bank lending on the private value of new green patents, which is more reflective of the firms' incentives. To gauge this private value, I employ two distinct metrics. First, building upon the insights from Kline et al. (2019), which highlight the number of patent claims as a strong predictor of a patent's significance to the firm, I adopt the number of claims in a patent's application as the initial measure of private value. Second, I leverage the patent value based on stock market reactions, as proposed in Kogan et al. (2017) (the KPSS value), a metric that is available for patents that have been granted by 2020. Armed with these two metrics, I then proceed to estimate the effect of reduced bank lending on the private value of new green patents.

The results of my analysis are presented in Table 1.12. Specifically, columns 1 and 2 provide estimates for the KPSS values of patents, while columns 3 and 4 offer estimates for the number of patent claims. Column 1 demonstrates that the effect of bank commitments on the KPSS value of green patents is both negative and statistically significant for green patents granted to high-emission firms. To be precise, the KPSS value of green patents granted to high-emission firms with committed lenders tends to be 17% lower after the bank commitments compared to high-emission firms without committed lenders. Similarly, column 3 shows that firms in high-emission industries with committed lenders have around 20% fewer patent claims in their green patent applications following bank divestment, compared to their counterparts without committed lenders in high-emission industries. In contrast, columns 2 and 4 suggest that there is no statistically significant effect of bank commitments on the private value of green patents produced by firms not operating within high-emission industries. In summary, the findings presented in the table suggest that the private value of green patents submitted by firms within high-emission industries tends to decrease after bank commitments, whereas the private value of green patents within lower-emission industries does not appear to be affected.

1.6.2 Social Value of Green Patents

Next, I turn my attention to examining the impact of bank commitments on the social value of granted green patents, as assessed through parameters including the generality, originality, and the number of forward citations. To account for potential variations in citation dynamics among different technological classes of patents (Lerner and Seru (2022)), I additionally control for patents' technological class trends in these estimations.

The results are displayed in Table 1.13. Columns 1-3 of the Table provide estimates for firms within the ten high-emission industries, while columns 4-6 shed light on firms operating in other industries. The findings reveal a negative and statistically significant effect of bank commitments on both the generality and forward citations of green patents granted to firms within high-emission industries. In particular, I find that bank commitments are associated with a reduction in green patents' generality index by approximately 0.08 percentage points, corresponding to roughly 40% of the sample mean, along with a 60% decrease in forward citations. Conversely, there are no statistically significant effects observed on the social value of green patents filed by firms in other industries. Altogether, the findings suggest that the social value of green patents tends to decrease for firms within high-emission industries after bank commitments and does not seem to change for firms within lower-emission industries.

1.6.3 Relevance of Green Patents to Mitigating Climate Change Impact

Finally, I investigate the relevance of green patents to mitigating the impact of climate change. To accomplish this, I employ the same stacked difference-in-differences regression framework mentioned above, using the cosine similarity-based relevance measure outlined in section 2.3 as the dependent variable. Additionally, to account for the possibility of shifts in the language used within patent titles and summaries, I control for the trend in relevance scores at the industry level.

Column 1 of Table 1.14 highlights that the average relevance of green patents to mitigating the impact of climate change submitted by firms operating within high-emission industries declined by around 0.03 percentage points after their lenders joined SBTi. This reduction corresponds to approximately 10% of the sample average. This finding implies that as firms in high-emission industries curtail their investments in green innovation, the green projects they do undertake may have diminished relevance to mitigating the impact of climate change. In contrast, there is no observed change in the relevance of green patents filed by other firms following bank commitments.

In summary, the findings in this section collectively suggest that bank divestment leads to a decrease in the value of green patents produced by firms in high-emission industries. On the other hand, the new green projects undertaken by firms in other industries, after receiving increased credit from banks, do not seem to compensate for the forgone high-value green projects launched by firms in high-emission industries.

1.7 Conclusion

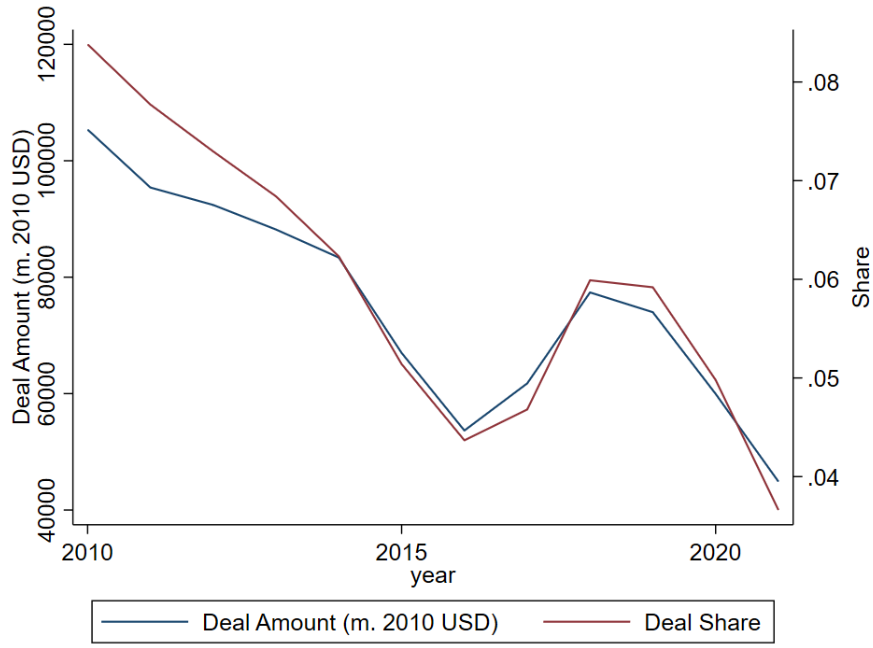
Can banks act as catalysts or deterrents to green innovation, i.e., innovation aimed at mitigating the impact of climate change, by limiting credit access to high carbon-emission firms? The answer to this question gains increasing relevance as we anticipate the emergence of more climate-related banking regulations in the near future.[22] In light of these developments, understanding how bank divestment affects firms' green innovation activities is crucial for devising an effective policy response to climate change and its consequences. While, in theory, bank di-

[22]For instance, Congresswoman Ayanna Pressley, alongside Congresswoman Rashida Tlaib and Senator Ed Markey, has proposed the Fossil Free Finance Act, which mandates that the Federal Reserve require large banks to cease financing projects and activities associated with increased greenhouse gas emissions. Indiana's new House bill 1224, targeting banks that divest from fossil fuels, is also currently making headway through the Statehouse.

vestment from high-emission firms might create additional incentives for them to invest in green innovation, the empirical evidence presented in this paper suggests that the adverse impact of increased capital costs tends to overshadow these potential benefits. Specifically, leveraging cross-sectional variation in banks' commitments to carbon neutrality as credit shocks to the borrowing firms, I find that bank divestment leads to a reduction in the number of green patents filed by high-emission firms that previously borrowed from these banks. At the same time, facilitated by the credit reallocation channel, I show that this decline in green patents from high-emission firms is partially offset by an uptick in green patent filings by other borrowing firms. However, both the relevance of these patents to mitigating the impact of climate change and their overall values show a decline among high-emission firms, and there is no apparent catch-up by low-emission firms. In summary, these findings suggest that bank divestment from high-emission industries has the unintended consequence of decreasing the production of high-quality green innovation by reshuffling the landscape of green patent producers.

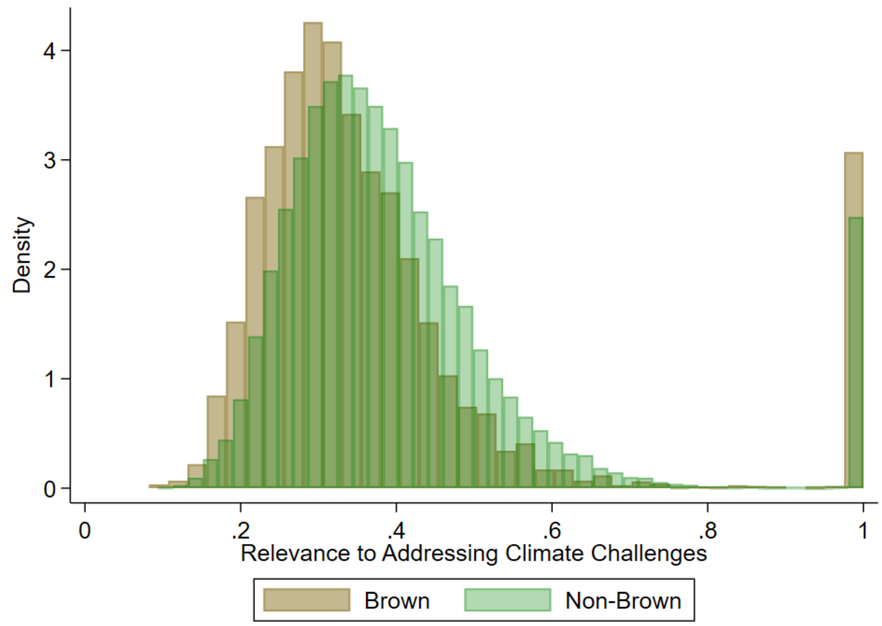
The results in this paper, combined with the findings in the literature that bank divestment from high-emission firms has not effectively reduced carbon emissions (Kacperczyk and Peydró (2021)), imply that bank divestment from high-emission firms may have a detrimental impact on achieving the net zero emissions target, at least in the short run. Given that the majority of energy consumption in the U.S. still relies heavily on fossil fuels, involving high-emission industries becomes pivotal in the transition toward a greener economy. Moreover, considering the consensus that only a small fraction of high-quality patents hold substantial value for their producing firms and possess the potential to drive significant spillover effects in subsequent innovation, it may be substantially more important to incentivize a small number of productive high-emission firms to invest in green innovation than simply expanding the pool of green innovation producers. Hence, for banks to play an important role in achieving net zero by promoting green innovations, screening borrowers based on their productivity in green innovation instead of excluding all firms in the high-emission industries could be a potentially more effective strategy.

Figure 1.1: Loans Received by U.S. Public Fossil Fuel Companies



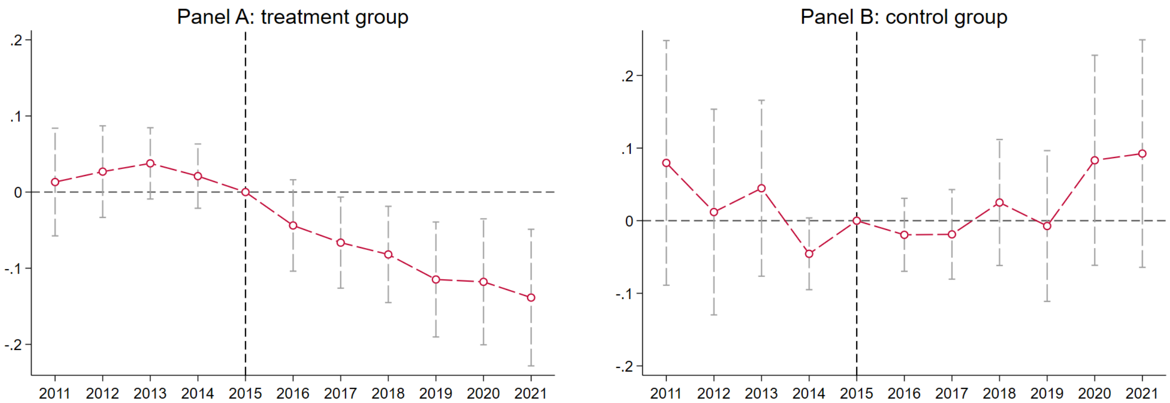
The Figure above plots the three-year moving average level (left axis and blue line) and ratio (right axis and red line) of syndicated loans received by the U.S. public firms in the fossil fuel industries between 2010 and 2022. The amount of loan issuance is normalized by the 2010 price.

Figure 1.2: Distribution of the Relevance of Green Patents to Mitigating the Impact of Climate Change



The Figure above plots the distribution of the relevance of green patents to mitigating the impact of climate change filed by the U.S. public firms in the ten industries with the highest carbon emissions (brown) and by those in the other industries (non-brown) between 2011 and 2020. The relevance score is defined in section 2.3 of the paper.

Figure 1.3: Dynamic Effects of Bank Commitments on Borrowing Firms' Debt



The figure plots the coefficients and their corresponding 95% confidence intervals obtained from estimating a stacked linear regression of the natural logarithm of total debt of a given firm from 2011 to 2021 on the interaction between year fixed effects and the natural logarithm of scope-1 carbon emission for firms with a relationship bank that eventually committed to carbon neutrality through SBTi (panel A), and for firms without committed relationship banks (panel B). The regressions control for firm- and size bin-year fixed effects. Standard errors are two-way clustered at the firm and year level.

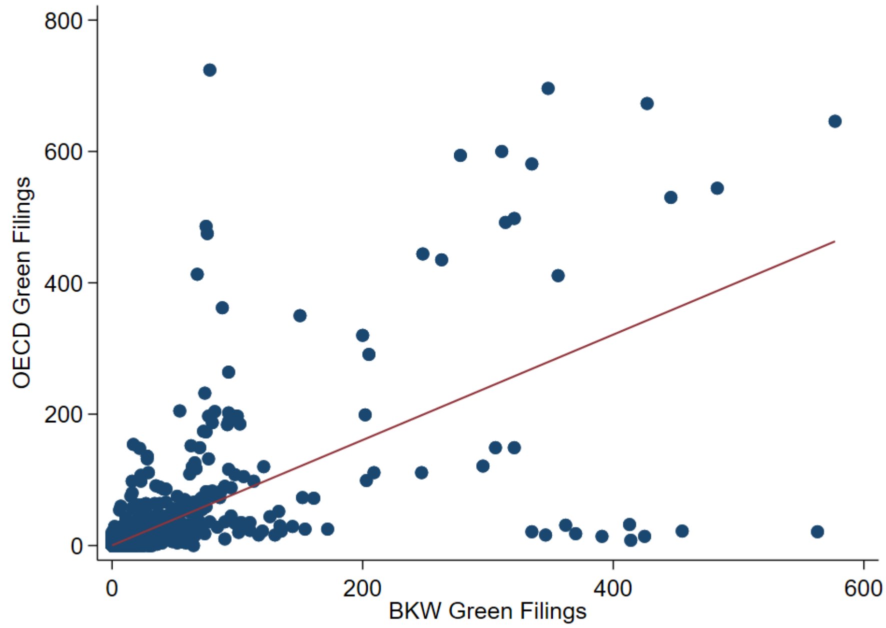
Figure 1.4: Dynamic Effects of Bank Commitments on Borrowing Firms' Green Patent Filings

The figure plots the coefficients and their corresponding 95% confidence intervals obtained from estimating a stacked Poisson regression of the number of green patents filed by a given firm from 2011 to 2021 on the interaction between year fixed effects and the natural logarithm of scope-1 carbon emission for firms with a relationship bank that eventually committed to carbon neutrality through SBTi (panel A), and for firms without committed relationship banks (panel B). The regressions control for industry trend of the dependent variable as well as firm- and year fixed effects. Standard errors are two-way clustered at the firm and year level.

Figure 1.5: Dynamic Effects of Bank Commitments on Borrowing Firms' Non-Green Patent Filings

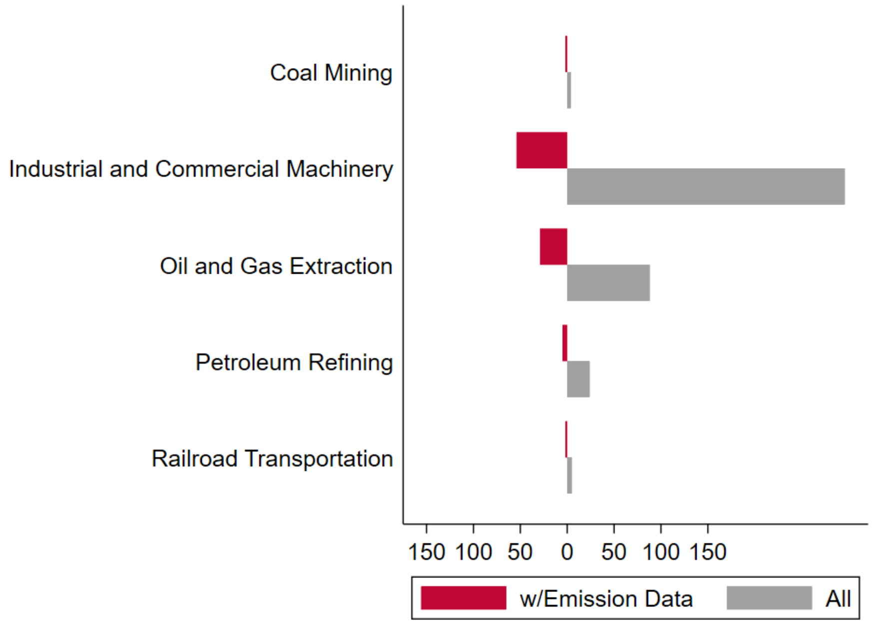
The figure plots the coefficients and their corresponding 95% confidence intervals obtained from estimating a stacked Poisson regression of the number of non-green patents filed by a given firm from 2011 to 2021 on the interaction between year fixed effects and the natural logarithm of scope-1 carbon emission for firms with a relationship bank that eventually committed to carbon neutrality through SBTi (panel A), and for firms without committed relationship banks (panel B). The regressions control for industry trend of the dependent variable as well as firm- and year fixed effects. Standard errors are two-way clustered at the firm and year level.

Figure 1.6: Correlation Between Green Patent Definitions Introduced by the OECD and BKW



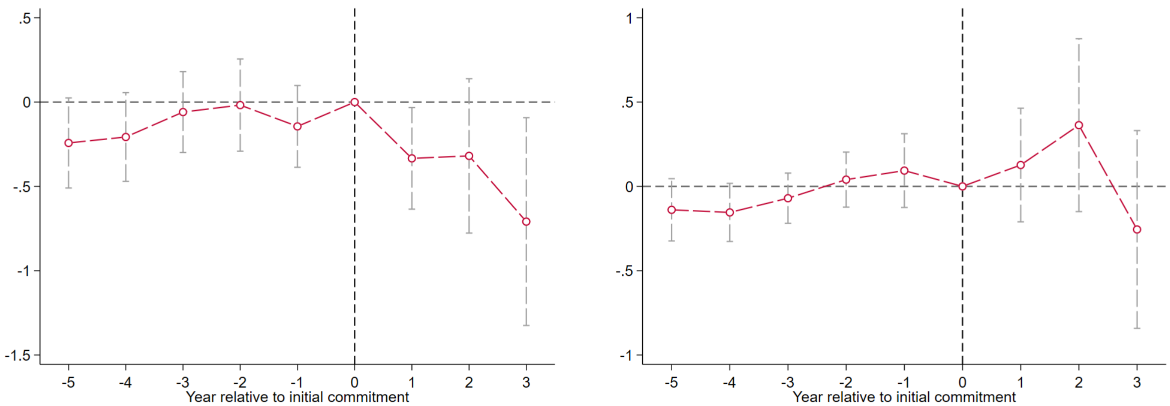
The figure above presents a scatter plot and a fitted line of green patent filings defined based on the OECD guidelines and on Bolton et al. (2022) (BKW) at the firm-year level.

Figure 1.7: Firm Coverage by Industry Based on the Availability of Carbon Emission Data



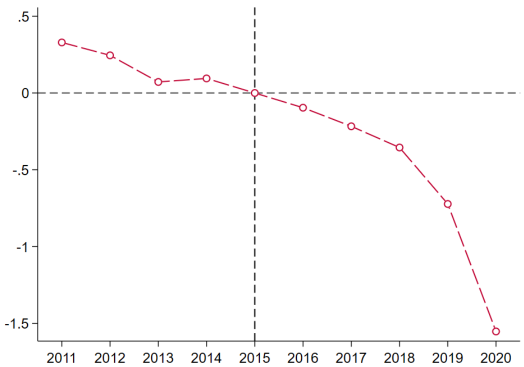
The figure plots the number of U.S. public innovative firms in my sample in each of the coal mining, industrial and commercial machinery, oil and gas extraction, petroleum refining, and railroad transportation industries (grey bars) and those that have data on scope-1 carbon emission between 2013 and 2014 in Trucost Environment.

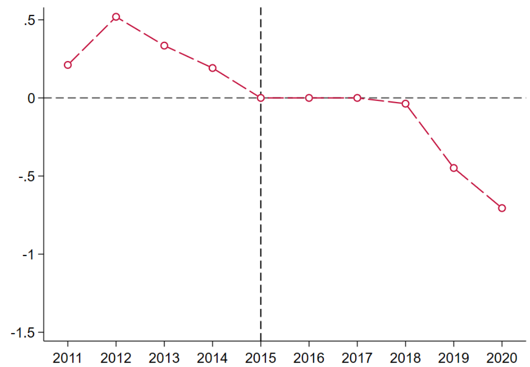
Figure 1.8: Dynamic Effects of Bank Commitments on Borrowing Firms' Innovation



The figure on the left plots the evolution of the number of green patents filed around the commitment year, and on the right, the number of non-green patents filed along with the 95% confidence interval for firms in the ten industries with the highest scope-1 emissions. The ten industries with the highest scope-1 emissions are: independent power and renewable electricity producers; electric utilities; airlines; multi-utilities; metals and mining; oil, gas, and consumable fuels; air freight and logistics; construction materials; paper and forest products; and chemicals. The coefficients and confidence intervals are obtained from estimating a stacked Poisson regression of the number of patents filed on the interactions of time from initial commitments of relationship bank, industry trend of the dependent variables, and firm and year fixed effects. The standard errors are two-way clustered at the firm and year level.

Figure 1.9: Trend of Green Patent Filings by Firms in High-Emission Industries





The figure plots the dynamics of the natural logarithm of the number of green patent filings by firms with committed lenders (left) and without committed lenders (right) in the ten industries with the highest scope-1 carbon emissions relative to the 2015 level. The ten industries with the highest scope-1 emissions are: independent power and renewable electricity producers; electric utilities; airlines; multi-utilities; metals and mining; oil, gas, and consumable fuels; air freight and logistics; construction materials; paper and forest products; and chemicals.

Table 1.1: List of Committed Lenders

Banks	Commitment year
AXA Group	2015
Bank Colombia	2015
Bank of Australia	2015
ING	2015
Westpac Banking Corp	2015
BNP Paribas	2016
Credit Agricole	2016
HSBC	2016
Metlife	2016
Societe Generale	2016
ABN AMRO	2018
BBVA	2018
Compass Bank	2018
LaSalle	2018
Raiffeisen Bank	2018
Standard Chartered Bank	2018
Swedbank	2018
YES Bank	2018

The table above lists lenders that committed to SBTi before 2020.

Table 1.2: List of Keywords Used to Identify Patents Aimed at Mitigating the Impact of Climate Change

air pollution	energy climate	renewable energy
air quality	energy conversion	sea level
air temperature	energy environment	sea water
biomass energy	environmental sustainability	snow ice
carbon dioxide	extreme weather	solar energy
carbon emission	flue gas	solar thermal
carbon energy	forest land	sustainable energy
carbon neutral	gas emission	water resource
carbon price	ghg emission	wave energy
carbon sink	global decarbonization	weather climate
carbon tax	global warm	wind energy
clean air	greenhouse gas	wind power
clean energy	heat power	wind resource
clean water	kyoto protocol	coastal region
climate change	natural hazard	new energy
electric vehicle	ozone layer	energy efficient

The table lists the keywords that I use to construct the benchmark set of green patents that are directly relevant to addressing climate change challenges. The keywords are obtained from Sautner et al. (2020).

Table 1.3: Summary Statistics

Panel A: Firm Level	All			High-Emi. Ind.			Other Ind.		
	count	mean	sd	count	mean	sd	count	mean	sd
N. of Patents Filed	19769	22.087	57.027	1968	12.946	40.377	17801	23.097	58.491
N. of Green Patents Filed	19769	1.036	3.585	1968	1.460	4.074	17801	0.990	3.523
Ever Committed	19769	0.383	0.486	1968	0.603	0.489	17801	0.359	0.480
Lead Ever Committed	19769	0.186	0.389	1968	0.380	0.486	17801	0.164	0.371
log(Asset)	19769	6.513	2.620	1968	8.165	2.613	17801	6.331	2.557
R&D	15489	0.180	0.317	956	0.054	0.162	14533	0.188	0.323
Average Q	16717	6.703	56.819	1517	5.352	52.354	15200	6.838	57.245
Average Cash Flow	19769	-0.291	1.310	1968	-0.199	1.443	17801	-0.301	1.294
Average Equity Financing	19769	0.149	0.310	1968	0.063	0.238	17801	0.159	0.315
Having Bond Rating	19769	0.232	0.42	1968	0.495	0.500	17801	0.208	0.406
log(Carbon Emission)	1346	5.820	2.725	332	8.997	1.652	1014	4.780	2.139
Onboarding Green Inventors	19769	0.605	2.388	1968	0.961	3.132	17801	0.577	2.316
Offboarding Green Inventors	19769	6.739	15.701	1968	9.538	21.469	17801	6.421	14.879

Panel B: Green Patents	All			High-Emission Industries			Other Industries		
	count	mean	sd	count	mean	sd	count	mean	sd
KPSS Value	23982	10.760	19.453	1095	40.138	47.970	22887	9.355	15.597
Forward Citations	34994	2.091	6.575	2606	2.616	7.294	32388	2.049	6.512
Generality Index	15092	0.340	0.390	1085	0.306	0.359	14007	0.342	0.394
Originality Index	33120	0.566	0.334	2430	0.601	0.319	30690	0.565	0.335
N. of Claims	42122	19.952	8.343	3643	20.918	11.287	38479	19.860	8.003

Panel C: High-emi. Industries	Non-green mean	Green mean	t-stat
Exploitative	.4602971	.3921402	7.657184
Explorative	.3321771	.3771558	-5.316305

The table above reports summary statistics at the firm level (panel A) and at the green patent level (panel B). High-emission industries are defined as the ten industries with the highest scope-1 emissions in my sample.

Table 1.4: Firm-Lender Level: Debt Financing

Dep. Var.	log(Debt)				
Sample	Full Sample			Top Quartile Emission	
	(1)	(2)	(3)	(4)	(5)
Committed	-0.0201	-0.113**	-0.109**	-0.252***	-0.293***
	(0.0363)	(0.0407)	(0.0408)	(0.0553)	(0.0464)
Committed x log(Emission)	-0.0872**	-0.0991***	-0.0923***		
	(0.0353)	(0.0181)	(0.0130)		
Post x log(Emission)	-0.0170	-0.0313	-0.0115		
	(0.0383)	(0.0318)	(0.0355)		
Committed x Green Patent					-0.0306
					(0.0894)
Committed x Patent					0.155
					(0.102)
Size Trend	No	No	Yes	Yes	Yes
Lender FE	Yes	Yes	No	No	No
Firm FE	Yes	Yes	No	No	No
Lender x Firm FE	No	No	Yes	Yes	Yes
Year FE	Yes	Yes	No	No	No
Observations	72362	39404	39404	10913	10913
Adjusted R^2	0.610	0.629	0.632	0.622	0.623

The table reports the effects of bank commitments on debt financing of a given bank from a relationship bank. Standard errors are two-way clustered at the firm and year level. *, **, and *** denote 10%, 5%, and 1% statistical significance, respectively.

Table 1.5: Firm-Level: Debt Financing

Dep. Var.	log(Debt)			
	(1)	(2)	(3)	(4)
Committed x log(Emission)	-0.0818**	-0.0690*		
	(0.0391)	(0.0338)		
Committed	0.0580	0.0828		
	(0.0548)	(0.0502)		
Exposure x log(Emission)			-0.691**	-0.588**
			(0.261)	(0.271)
Exposure			0.0264	0.239
			(0.428)	(0.408)
Post x log(Emission)	0.0373	0.0518	0.0247	0.0418
	(0.0389)	(0.0429)	(0.0316)	(0.0346)
Size Trend	No	Yes	No	Yes
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	9948	9948	9948	9948
Adjusted R^2	0.909	0.910	0.909	0.910

The table reports the effects of bank commitments on the level of natural logarithm of total debt of a given firm in a given year. Standard errors are two-way clustered at the firm and year level. *, **, and *** denote 10%, 5%, and 1% statistical significance, respectively.

Table 1.6: Effect on Patent Filings

Dep. Var.	Green Patent	Non-Green Patent	Green Patent	Non-Green Patent
	(1)	(2)	(3)	(4)
Committed	0.0971	0.00499		
	(0.0854)	(0.0963)		
Committed x log(Emission)	-0.129**	-0.0107		
	(0.0609)	(0.0519)		
Exposure			0.459	0.0290
			(0.638)	(0.613)
Exposure x log(Emission)			-1.263***	-0.00700
			(0.470)	(0.522)
Post x log(Emission)	0.0279	0.0535	0.0307	0.0501
	(0.0355)	(0.0480)	(0.0370)	(0.0452)
Industry Trend	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	5112	7631	5112	7631

The table reports the effects of bank commitments on the number of patent filings. Standard errors are two-way clustered at the firm and year level. *, **, and *** denote 10%, 5%, and 1% statistical significance, respectively.

Table 1.7: Predicting Commitments

Dep. Var.	Eventually Committed						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
log(asset)	0.0156***	0.0168***	0.0161***	0.0185***	0.0198***	0.0257***	0.0113
	(0.00340)	(0.00515)	(0.00348)	(0.00536)	(0.00651)	(0.00849)	(0.0169)
log(employment)		-0.000533					0.00468
		(0.00453)					(0.0144)
ROA			-0.00167				-0.00264
			(0.00245)				(0.0168)
R&D				-0.00809			0.199
				(0.179)			(0.443)
N. Green Patents					0.00156		0.000134
					(0.00209)		(0.00254)
log(emissions)						0.149***	0.0163***
						(0.00487)	(0.00565)
Observations	1412	1394	1412	973	854	721	647

The table above reports estimates from a linear regression at the lender-year level. All dependent variables are an indicator variable equal to one if a lender will ever commit to carbon neutrality through SBTi. Independent variables are the average characteristics of brown firms that borrow from each lender between 2011 and 2014. Standard errors are clustered at the lender level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Table 1.8: Matched Sample: Effect on Patent Filings

Dep. Var.	Green Patent	Non-Green Patent	Green Patent	Non-Green Patent
	(1)	(2)	(3)	(4)
Committed	0.0852*	-0.0324		
	(0.0514)	(0.106)		
Committed x log(Emission)	-0.0975***	-0.00626		
	(0.0355)	(0.0683)		
Exposure			0.299	-0.373
			(0.663)	(0.677)
Exposure x log(Emission)			-1.073**	-0.0746
			(0.518)	(0.602)
Post x log(Emission)	-0.0129	0.0693	0.00993	0.0717
	(0.0212)	(0.0685)	(0.0456)	(0.0568)
Industry Trend	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	4465	6643	4455	6643

The table above reports the effect of bank commitment on the number of patents filed across levels of scope-1 emissions on the matched sample. Standard errors are two-way clustered at the firm and year level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Table 1.9: High-emission Firms vs. Other Industries: Effect on Patent Filings

Dep. Var.	Green Patents Filed		Non-Green Patents Filed	
Sample	Firms in High-Emi. Industries (1)	Other Firms (2)	Firms in High-Emi. Industries (3)	Other Firms (4)
Committed	-0.388***	0.247***	-0.0545	0.0489
	(0.150)	(0.0679)	(0.0897)	(0.0437)
log(Asset)	0.104	0.104**	0.0178	0.134***
	(0.141)	(0.0423)	(0.0789)	(0.0439)
R&D	-0.112	0.122	0.179	0.137*
	(0.280)	(0.121)	(0.283)	(0.0713)
Industry Trend	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	1436	11803	2085	35942

The table above reports the effect of bank commitment on the number of patents filed by firms in the ten industries with the highest scope-1 emissions and by firms in other industries. Standard errors are two-way clustered at the firm and year level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Table 1.10: Heterogeneous Effect: Alternative Financing

Dep. Var.	Green Patent		
	(1)	(2)	(3)
Committed	-0.697***	-0.684***	-0.526*
	(0.246)	(0.169)	(0.314)
Committed x High Cash Flow	0.618**		
	(0.299)		
Committed x High Ex. Equity Financing		0.724*	
		(0.412)	
Committed x High Bond Rating			0.327**
			(0.143)
Committed x Q	-0.232	-0.847*	-0.314
	(0.233)	(0.464)	(0.318)
log(Asset)	0.112	0.162	0.117
	(0.134)	(0.149)	(0.203)
R&D	-0.106	-0.0635	-0.097
	(0.244)	(0.274)	(0.454)
Industry Trend	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Observations	1171	1171	1171

The table above reports heterogeneous effects of bank commitments on the number of patents filed based on access to alternative financing. In column 1, the main independent variable is the interaction of bank commitment and a dummy variable that equals 1 if a firm's average cash flow from 2011 to 2014 is at the top 30% percentile and 0 otherwise. The main independent variable in column 2 is the interaction of bank commitment and a dummy variable that equals 1 if a firm's net external equity financing from 2011 to 2014 is at the top 30% percentile and 0 otherwise. The main independent variable in column 3 is the interaction of bank commitment and a dummy variable that equals 1 if a firm has bond rating from 2011 to 2014 and 0 otherwise. Standard errors are clustered at the firm level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Table 1.11: Inventor Mobility

Dep. Var.	Number of Offboarding Green Inventors		Number of Onboarding Green Inventors		Green Patent Dummy
Sample	Firms in High-emi. Ind. (1)	Other Firms (2)	Firms in High-emi. Ind. (3)	Other Firms (4)	Incumbent Green Inventors Patents in High-emi. Ind. (5)
Committed	-0.133	0.126	-0.241*	0.144**	-0.124**
	(0.125)	(0.0778)	(0.136)	(0.0599)	(0.0588)
log(Asset)	0.278	0.628***	0.946***	0.625***	0.203**
	(0.313)	(0.184)	(0.321)	(0.165)	(0.0927)
R&D	0.297***	0.141*	-0.151	0.00946	2.61***
	(0.0527)	(0.0778)	(0.288)	(0.0722)	(0.850)
Industry Trend	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Observations	869	8276	1444	18185	16045

The table reports the effects of bank commitments on green inventor mobility. Standard errors are two-way clustered at the firm and year level.
*, **, and *** denote 10%, 5%, and 1% statistical significance, respectively.

Table 1.12: Private Value of Green Patents

Dep. Var.	log(KPSS Value)		log(N. of Patent Claims)	
	High-Emission Industries (1)	Other Industries (2)	High-Emission Industries (3)	Other Industries (4)
Committed	-0.166*	-0.0473	-0.199***	0.0176
	(0.0937)	(0.0544)	(0.0574)	(0.0243)
log(Asset)	0.472*	-0.170	-0.0103	0.0204
	(0.235)	(0.114)	(0.0873)	(0.0178)
R&D	2.536	-0.0677	0.256**	-0.00427
	(1.868)	(0.304)	(0.0915)	(0.00437)
Industry Trend	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	2265	32639	7025	55335

The table above reports the effect of bank commitment on the number of patent claims and the KPSS values. Standard errors are two-way clustered at the firm and year level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Table 1.13: Social Value of Green Patents

	High-emission Industries			Other Industries		
Dep. Var.	Generality	Originality	log(Citation)	Generality	Originality	log(Citation)
	(1)	(2)	(3)	(4)	(5)	(6)
Committed	-0.0803*	0.00113	-0.607***	0.0141	0.0138	-0.00459
	(0.0364)	(0.0361)	(0.204)	(0.0145)	(0.00777)	(0.0667)
log(Asset)	-0.284	0.0853**	-0.168	-0.0152	-0.0000716	0.0543
	(0.285)	(0.0362)	(0.249)	(0.0137)	(0.00868)	(0.0425)
R&D	-1.337	0.0776	-4.003	-0.0476	0.0234	-0.193
	(1.476)	(0.0639)	(2.535)	(0.0539)	(0.0300)	(0.201)
Technological Class Trend	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2222	4805	5092	21798	46783	46036

The table reports the effects of bank commitments on the generality index, the originality index, and the number of forward citations. Standard errors are two-way clustered at the firm and year level. *, **, and *** denote 10%, 5%, and 1% statistical significance, respectively.

Table 1.14: Effect on Green Innovation's Relevance to Climate Change Impact

	(1) High-emission Industries	(2) Other Industries
Committed	-0.0337**	-0.0119
	(0.0109)	(0.0150)
log(Asset)	-0.0372*	-0.00333
	(0.0170)	(0.00910)
R&D	-0.0213	0.0000285
	(0.0490)	(0.00165)
Controls	Yes	Yes
Industry Trend	Yes	Yes
Firm FE	Yes	Yes
Year FE	Yes	Yes
Observations	7054	55836

The table above reports the effect of bank commitment on green patents' relevance to mitigating the impact of climate change. Standard errors are two-way clustered at the firm and year level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Chapter 2: Parental Income Volatility and Entrepreneurship: Evidence from Sweden

2.1 Introduction

The past few decades have seen increasing household income volatility in developed countries such as the U.S. (Dynan et al. (2012); Moffitt and Gottschalk (2012); Heathcote et al. (2010); Western et al. (2012); Carr et al. (2020)), Sweden (Gustavsson (2004)), and Germany (Fuchs-Schündeln et al. (2010)). The increase in economic instability has drawn growing attention from both policymakers (Congressional Budget Office (2007)) and researchers in various disciplines. Though economists have mostly focused on the trend and causes of household income volatility, there has been increasing concern about the intergenerational consequences of increasing household income volatility (Hill et al. (2013); Western et al. (2012), Western et al. (2016)).[1] The instability of family consumption stemming from heightened parental income volatility in early life could affect one's risk preferences and other behavioral traits. This, in turn, may have long-term effects on an individual's decision on whether to start a new business. At the same time, the heightened risk of negative shocks due to higher parental income volatility may cause individuals to exit formal education at earlier stages, extending the window for starting a business, but with a potential cost of reduced accumulation of human capital.

In this paper, we utilize a unique Swedish administrative dataset that includes detailed

[1]A few recent papers in sociology and other disciplines have made contributions to understanding the intergenerational effects of household income volatility: Hardy (2014) shows that higher family income volatility during one's childhood is associated with higher high school dropout rate using data from the Panel Study of Income Dynamics (PSID); Cheng et al. (2020) find a negative relationship between parental income volatility and children's psychological well-being using U.S. and Swedish data.

information on individuals born between 1980 and 1994 to study the effects of parental income volatility during adolescence on entrepreneurship. We find that individuals who experience higher parental income volatility during adolescence are more likely to start businesses: a one-standard-deviation increase in parental income volatility is associated with an increase in the probability of becoming an entrepreneur[2] of 45% from the mean. However, conditional on becoming entrepreneurs, businesses started by individuals with higher parental income volatility have lower survival rates.

We measure parental income volatility by calculating the variance of residual log disposable income growth[3] for parents when an individual is between the ages of 13 and 19. This period is particularly significant, as adolescence is a formative time that can shape an individual's future traits and behaviors.[4] Residual log disposable income is estimated from a linear regression of the natural logarithm of parental disposable income on a comprehensive set of individual-level variables, including predictable characteristics such as age and household size and observable household decisions such as parental leave. Separating these predictable household characteristics and decisions from our measure of income volatility is important. It is well-established that households smooth consumption in response to anticipated life-cycle events (e.g., Carroll and Samwick (1997), Blundell et al. (2008)). Consequently, income fluctuations tied to foreseen events may not lead to fluctuations in consumption, breaking the most obvious link to children's preferences and beliefs. Therefore, compared to other measures of income volatility, such as the variance of parental income flows or the standard deviation of year-to-year percentage changes in parental income flows, the variance of residual log disposable income growth more effectively captures the parental income volatility that may affect children's future choices.

However, identifying the causal effects of parental income volatility on their children's

[2]Throughout this article, we focus on entrepreneurs defined as owners of any type of business unless specifically noted otherwise. As we show in one of our robustness tests, using a narrower definition of incorporated business owner produces consistent results.

[3]The measure is widely used in the macroeconomics literature (e.g., Heaton and Lucas (2000); Palia et al. (2014)). The detailed definition is given in Section 2.2.

[4]For instance, adolescence is a vital period when individuals' behavioral traits, such as risk preferences, become stable and converge to those of adults (Levin et al. (2007b); Levin et al. (2007a); Paulsen et al. (2012)).

entrepreneurial decisions still faces two key challenges. First, our measure of parental income volatility may not fully isolate unexpected fluctuations in income from the effects of endogenous choices, such as purposeful smoothing of consumption. Cunha et al. (2005), Low et al. (2010), and Guvenen and Smith (2014) make similar arguments in the context of measuring earnings risk. Since the measured parental income volatility can be either higher or lower than the actual consumption instability the individuals experience when they are young, the measurement problem can bias the estimated effect of parental income volatility downward, similar to a classical measurement error. Second, the estimated relationship between parental income instability and children's entrepreneurship may be biased due to omitted variables, such as endogenous matching between fathers and their industries, which are correlated with both parental income volatility and children's entrepreneurial decisions.

To establish the causal relationship between parental income volatility and entrepreneurship, we adopt an identification strategy similar to that developed by Fagereng et al. (2018). Specifically, we employ an instrumental variable approach whereby we instrument for every individual's parental income volatility using the variance of average wage shocks to the father's employer during the individual's ages of 13 to 19. We construct shocks to a firm's average wage by estimating the residuals from a linear regression of the natural logarithm of the firm's average wage on a set of firm-level controls. This strategy utilizes the idea that unexpected fluctuations in an employer's average wage generate income volatility for the father that is plausibly exogenous to parental choices that may be correlated with both income volatility and entrepreneurship. We exclude individuals whose fathers' employers have fewer than three employees to reduce the potential for a mechanical relationship between the firm's average wage volatility and an employee's income volatility.[5] We also include a rich set of controls. For instance, we control for industry fixed effects to account for endogenous matching between the fathers and their industries, and the fathers' years of schooling as a proxy for cognitive ability to account for the potential correlation between the fathers' ability with both their income volatility and their children's entrepreneur-

[5] In robustness checks, we show that the results are robust to excluding individuals whose fathers are owners of the firms and robust to using a Jackknife instrument.

ship. We discuss additional details in Section 2.2.

We obtain a strong first-stage of our IV estimation with an F-statistics of 28.2, and our second-stage estimates show that individuals who experience higher parental income volatility between the ages of 13 and 19 are more likely to become entrepreneurs: a one-standard-deviation increase in parental income volatility increases the probability of starting a business by 2.3 percentage points, i.e., around 45% of the sample mean of 5%. In comparison, our OLS estimates suggest that a one-standard-deviation increase in parental income volatility is associated with 0.00114 percentage points higher likelihood of becoming an entrepreneur. The gap in magnitudes between our IV and OLS estimates is consistent with the aforementioned conceptual measurement error in our income volatility measure, which biases the OLS estimates downward. That is, the instrument corrects for remaining endogenous variation in income volatility as intended. An alternative explanation is that the OLS estimates are contaminated by omitted variables, particularly the unobserved risk tolerance of the parents. However, since the parents' risk tolerance is likely to be positively correlated with their income volatility and the individuals' likelihood of becoming entrepreneurs, the OLS estimates should be biased upward instead of downward. Another possible explanation is that the effect of parental income volatility on entrepreneurial entry is stronger, i.e., the treatment effect is larger for individuals whose fathers' incomes are more affected by their employers' wage shock. We use a quasi-control function approach to examine the extent to which the gap between our IV and OLS estimates might be driven by the heterogeneous effect of parental income volatility. More specifically, we estimate the heterogeneous effects of parental income volatility across individuals with different magnitudes of estimated residuals from the first stage of our IV estimation. We find that even for individuals whose parental income volatility can be best predicted by our instrument, their OLS-estimated effect of parental income volatility on entrepreneurial entry remains significantly smaller than our IV point estimate. Therefore, heterogeneity is unlikely the main reason behind the difference between our IV and OLS estimates.

Using the same instrumental variable strategy, we further show that firms started by individuals with higher parental income volatility tend to have a lower survival rate. More specifically, a one-standard-deviation increase in parental income volatility is associated with a decline in the two- and three-year survival rates by 6.6 and 7.2 percentage points, respectively.

We then explore three mutually non-exclusive channels that may account for the effects of parental income volatility on entrepreneurship. First, experiencing higher income volatility during adolescence may increase an individual's likelihood of becoming an entrepreneur by fostering a greater tolerance for risk.[6] As suggested by theoretical frameworks such as Knight (1921), Kanbur (1979), and Kihlstrom and Laffont (1979) and empirical analyses such as Hvide and Panos (2014), this could, in turn, increase the likelihood of becoming an entrepreneur. In addition, ventures led by entrepreneurs with higher risk tolerance may have lower survival rates since they are more willing to accept riskier projects. Second, higher income instability may induce individuals to leave school earlier to work and help with household consumption.[7] Fewer years of schooling could give individuals more time to start a business, thus aligning with the positive effect of parental income volatility on the entrepreneurial entry that we have found. Moreover, it may also explain the negative effects of parental income volatility on business survival rates, as there would be less human capital accumulation from schools. Finally, higher parental income volatility may prompt households to save and accumulate wealth in order to hedge against future negative income shocks, potentially providing individuals with the financial resources needed to finance their new businesses and increasing their likelihood of becoming entrepreneurs.

Our findings provide suggestive evidence in favor of the risk-preference channel. For example, we show that entrepreneurs who experience a higher level of parental income volatility tend to start their businesses at a younger age. Furthermore, we find that entrepreneurs with higher parental income volatility are more likely to venture into riskier industries, as indicated by the industry's survival rate before the business' inception, and this effect is not explained by whether their parents worked in the same industry when they were young.

[6]This argument is consistent in spirit with the findings documented in Bernile et al. (2017) that CEOs who experience natural disasters without extremely negative consequences lead companies that behave more aggressively.

[7]This hypothesis is consistent with the evidence in Hardy (2014), showing a positive correlation between parental income volatility and high school dropout rates.

We conduct a number of tests to shed light on the relevance of schooling and financial resources as mechanisms driving our findings. For instance, we examine how the estimated effect of parental income volatility on business survival is affected when we additionally control for years of schooling fixed effects. If the negative effect of parental income volatility on business survival is driven by individuals with higher parental income volatility leaving school earlier, we would expect the estimated effect of parental income volatility to become smaller when we include schooling fixed effects. However, the point estimates remain largely unaffected. Using a small sample of individuals for whom we observe household wealth, we show that adding household wealth as a control in a linear regression of entrepreneurial entry on parental income volatility has little effect on the point estimate of the effect of parental income volatility. Overall, we do not find empirical evidence supporting schooling and financial resources being important mechanisms.

Taken together, our paper shows that the stability of parental income during one's adolescence has a long-term impact on one's entrepreneurial entry decision. In addition, parental income volatility affects the survival of the new firms that the entrepreneurs create. While this paper does not have direct causal evidence to confirm or reject a particular channel, our empirical evidence suggests that the risk tolerance developed from higher parental income volatility is a plausible mechanism underlying our findings, whereas alternative mechanisms such as human capital, and financial resources are less supported.

Our paper contributes to several strands of literature. First of all, it speaks to the literature on the factors that influence entrepreneurial entry decisions, particularly in the context of family backgrounds. Lindquist et al. (2015), for instance, use administrative Swedish data to show that children of entrepreneurial parents are more likely to become entrepreneurs. Hvide and Oyer (2017) show that male entrepreneurs are likely to start a firm in the same or similar industry as their fathers' industry of employment. Using U.S. survey data, Hurst and Lusardi (2004) find a positive correlation between family wealth and becoming a business owner among the extremely rich. We add to this literature by showing that family income stability during one's early life is an important determinant of entrepreneurial entry and subsequent performance. Because our estimates are driven by the wage volatility of the fathers' employers that is uninsurable to the parents, it allows us to isolate the effects of parental income volatility from other family background factors that have been shown to be important for entrepreneurship in the literature.

Our paper also relates to the strand of the literature that explores how risk tolerance and insurance affect entrepreneurship. For instance, Kerr et al. (2019) documents that entrepreneurs exhibit greater risk tolerance compared to non-founder CEOs/leaders and inventor employees. Using Norwegian administrative data, Hvide and Panos (2014) find that risk tolerance is a crucial positive determinant of starting a new business, in line with the theoretical predictions in Knight (1921), Kanbur (1979), and Kihlstrom and Laffont (1979). Hombert et al. (2020) find that the rate of firm creation in France experienced a significant boost following a reform of the country's unemployment benefits program, which included provisions to support individuals who start businesses while unemployed. We complement this literature by providing suggestive evidence that parental income volatility could play an important role in shaping risk preferences and, therefore, influence entrepreneurial decisions.

More broadly, our paper is related to the vast literature studying the impact of early life events and environment on economic and financial decisions later in one's life. Studies in this literature have primarily focused on level shocks or mean levels across two time points.[8] Our paper extends this literature by introducing parental income fluctuation as a novel determinant of adulthood economic behaviors.

Finally, our paper contributes to the emerging literature on the intergenerational consequences of income volatility. The literature has mainly looked at outcomes familiar in the fields of sociology and public health, such as schooling and psychological well-being.[9] We focus on entrepreneurship which is frequently studied in the economics and finance literature, and investigate the potential mechanisms behind our findings. Our paper is also the first, to the best of our knowledge, to causally estimate the intergenerational effect of parental income volatility by

[8] See, e.g., Malmendier et al. (2011), Bernile et al. (2017), and Sikhova (2023).

[9] See, e.g., Hardy (2014) and Cheng et al. (2020).

correcting for the measurement error in income volatility. This is crucial since the significant discrepancy between our IV and OLS estimates implies that the results of prior studies, which did not adequately address the measurement errors in parental income volatility, may have been substantially biased.

The rest of the paper is structured as follows: Section 2.2 discusses the construction of parental income volatility and our empirical strategy. Section 2.3 lays out the main data sources. Section 2.4 analyzes entrepreneurial entry, and Section 2.5 analyzes the survival of new businesses. Section 2.6 discusses the potential mechanisms. Section 2.7 concludes.

2.2 Empirical Strategy

The goal of our study is to estimate the causal effects of parental income volatility during adolescence on individuals' entry into entrepreneurship, as well as the subsequent performance of their businesses.

2.2.1 Measuring Volatility

To measure parental income volatility, we calculate the variance of unexplained parental disposable income growth between an individual's ages of 13 and 19. This method is similar to those used in prior studies, such as Heaton and Lucas (2000), Palia et al. (2014), and Fagereng et al. (2018). To determine unexplained parental earnings, we first estimate:

$$lny_{it} = \alpha + \boldsymbol{Z}'_{it}\gamma + \mu_{it} \tag{2.1}$$

where i and t denote an individual and the calendar year, respectively. y is the annual disposable income of an individual's parents. $\boldsymbol{Z}$ denotes a vector of controls. Household consumption is likely a crucial channel through which parental income volatility during adolescence can affect an individual. However, since households tend to smooth their consumption in response to anticipated events (e.g., Carroll and Samwick (1997), Blundell et al. (2008)), parental income volatility resulting from anticipated events may not affect household consumption, and thus may not be observable to individuals, making it less likely to have an effect on them. For instance, parents who anticipate taking time off to care for a newborn may begin saving money well in advance, allowing them to compensate for the decrease in wage income with savings and avoid reducing their consumption. To address this issue, we include in the vector $\boldsymbol{Z}$ a rich set of controls, including each parent's age, the square of their ages, parents' years of education, the number of children, sizes of the parents' employers, whether the parent has taken sick or parental leave, whether the parent has been unemployed, and fixed effects for household type, birth country, the parents' birth years, calendar year, and the municipality of residence. μ is the residual. Then parental income volatility for individual i, $Volatility_i^P$, is measured as:

$$Volatility_i^P = Var\Big(\triangle \left(lny_{it} - \boldsymbol{Z}_{it}'\hat{\gamma} - \hat{\alpha} \right) \Big) \tag{2.2}$$

In comparison to other measures of income volatility, such as the variance of parental income flows and the standard deviation of year-to-year percentage changes in parental income flows, the variance of residual log disposable income is a superior measure. This is because it more accurately captures parental income volatility that corresponds to fluctuations in consumption and thus is more likely to impact individuals.

However, identifying the causal effects of parental income volatility on entrepreneurial decisions poses two major challenges. The first challenge concerns the potential for measurement error in our parental income volatility measure. While our measure of parental income volatility accounts for observable changes such as parental leave and unemployment, it may still be influenced by unobservable expected choices (see e.g., Cunha et al. (2005), Low et al. (2010), and Guvenen and Smith (2014)). For instance, a successful actor may choose to work on lower-quality films after accumulating enough wealth, but this decision may not affect their household consumption if they have a good consumption-smoothing plan. Hence, the difference between the measured parental income volatility and the actual instability experienced by individuals during adolescence due to their parents' income fluctuations may introduce estimation bias, similar to the classical measurement error that biases OLS estimates towards zero.

Secondly, parental decisions that lead to income volatility, even if they are unanticipated, may be correlated with parental characteristics that affect the likelihood of the individuals becoming entrepreneurs and the type of businesses they start. Therefore, the estimated effects of parental income volatility that we capture may reflect the effects of unobserved parental characteristics that indirectly affect the individual's entrepreneurship through parental income volatility. For instance, parents who have volatile income because of frequent gambling[10] may have higher risk tolerance which they pass on to their children through genetics, resulting in their children having higher risk tolerance and a higher propensity to become entrepreneurs (Dohmen et al. (2012)). In this case, the relationship between parental income volatility and entrepreneurship that we observe may, in fact, capture the relationship between inherited risk tolerance and entrepreneurship.

2.2.2 Instrumental Variable

To estimate the causal effects of parental income volatility on individuals' entrepreneurship, we employ an instrumental variable strategy, building on the approach developed by Fagereng et al. (2018). Specifically, we instrument for parental income volatility experienced by individuals between the ages of 13 and 19 using the variability in the average wage of the father's employer during the same period. The rationale behind this instrument is that the idiosyncratic variation in the average wage of the fathers' employers may cause fluctuations in the parental disposable income. To the extent that the instrument is exogenous to the parental income volatility, our instrumental variable strategy addresses both the potential measurement error and the omitted variables concerns. Furthermore, using the volatility of the employer's average wage as an instrument restricts the type of variation in parental income volatility that drives our results, allowing

[10] According to Binde (2014), 73% of the adult population in Sweden gambled in 2013, and 89% in 1998.

for an easier interpretation of the estimated effects. This is particularly advantageous compared to the ordinary least squares (OLS) approach where, even in cases with no measurement error or omitted variable bias, the interpretation of the estimated coefficient can be complicated due to the differing nature of the (random) events that lead to income volatility (e.g., gambling versus layoffs). To construct the instrument, similar to our measure of parental income volatility, we first estimate a linear regression of the logarithm of firm average wage W_{ijt}:

$$lnW_{ijt} = \gamma + \boldsymbol{D}'_{jt}\delta + \nu_{ijt} \tag{2.3}$$

where i, j, t denote an individual i, her father's employer j, and the calendar year t, respectively. W denotes the average wage income of the employees in firm j. $\boldsymbol{D}$ is a vector of controls that includes the number of employees and fixed effects for the employer's municipality, industry, and calendar year. ν is the residual. Then the average wage volatility of individual i's father's employer, $Volatility_i^E$, is calculated as:

$$Volatility_i^E = Var\Big(\triangle \left(W_{ijt} - \boldsymbol{D}'_{jt}\hat{\delta} - \hat{\gamma}\right)\Big) \tag{2.4}$$

To mitigate the potential for a mechanical relationship between the firm's average wage volatility and an employee's income volatility, we only include individuals whose fathers' employers have at least three employees when constructing the instrument. Furthermore, in Section 2.4.3, we perform a set of robustness checks by excluding individuals whose fathers are owners of the firms and by excluding the individuals' fathers when calculating the employers' average wage and show that our results remain virtually the same.

For the instrument to be valid, it should explain the individuals' entrepreneurship only through its effect on parental income volatility. There are two immediate threats to the exclusion restriction. First, our instrumental variable strategy does not require the fathers to stay with the same employers throughout their children's adolescence. Therefore, our results could potentially be driven by comparing individuals whose fathers work in multiple firms, potentially in the same industries, and those with fathers who stay with the same employers when the individuals are between 13 and 19 years old. Fathers who switch employers may have different unobserved characteristics that correlate with both the wage volatility of their employers and their children's entrepreneurship compared to those who do not switch. To address this concern, in Section 2.4.3 we perform a robustness check in which we restrict the analysis to individuals whose fathers remain with the same employer during their adolescence and find that our results remain largely unchanged.

The second potential threat arises because different industries may have varying wage volatility due to different responses to industry productivity shocks (Lagakos and Ordonez (2007)) and parents may sort into different industries based on their abilities and behavioral traits, which could in turn affect their children's entrepreneurship through genetics (Dohmen et al. (2012)). To address this concern, we directly control for each parent's years of education as a proxy for their cognitive abilities. To account for endogenous matching between the fathers and their industries, we also control for the fathers' industry fixed effects so that our results are not driven by comparing individuals whose fathers work in different industries. In a robustness check in Section 2.4.3, we directly control for the fathers' employer fixed effects to address a similar concern of endogenous matching between workers and employers and find that our results remain largely unchanged.[11]

One might also be concerned with the interpretation of our IV results because of downward wage rigidity. If the fathers of the individuals in our sample almost never experience negative real wage growth, our IV estimates should be interpreted as the effects of fluctuation of *positive* wage growth rather than the effects of income fluctuations that can be either negative or positive. We find that in our sample only less than 15% of the fathers never experience negative real wage growth when individuals are between 13 and 19. Furthermore, around 30% of the fathers

[11]Without any data restriction, one might prefer to control for family fixed effects to better address the omitted variable concern. In this case, the effects of parental income volatility would be identified by comparing siblings who were born in different years. In our case, this strategy would not provide enough statistical power. This is because most siblings are born only a few years apart, and thus there is little variation in their corresponding parental income volatility and the average wage volatility of the father's employer.

experience negative real wage growth in three or more years during the same period. This could be due, in part, to the relatively high inflation rate in Sweden during the 1990s, when a significant portion of the individuals in our sample were in their adolescence. Therefore, downward wage rigidity seems to be of limited concern to the interpretation of our results.

2.2.3 Specification

To estimate the effects of parental income volatility on individuals' entrepreneurial outcomes, we estimate the following linear probability model with parental income volatility being instrumented by the volatility of the average wage of the father's employer:

$$E_i = \beta_0 + \beta_1 \text{Volatility}_i + \boldsymbol{X}_i'\lambda + \epsilon_i, \tag{2.5}$$

where i denotes the individual. E is the outcome of interest, such as entrepreneurial entry defined as a binary variable that equals one if an individual i ever starts a business before 2014 and zero otherwise, and the survival rate of the new businesses started by the entrepreneurs in our sample. The key independent variable of interest is parental income volatility between an individual's ages of 13 and 19. We standardize parental income volatility for easier interpretation. $\boldsymbol{X}$ is a vector of controls. Specifically, across all regressions, we control for the logarithm of average parental income between the individuals' ages of 13 and 19 and the parents' years of schooling. We additionally include a number of factors that have been shown to have strong impacts on one's decision to become an entrepreneur in the entrepreneurship literature, including gender, birth order, parental entrepreneurship, and an individual's ability which we proxy using grades in the ninth grade. We also control for a rich set of fixed effects to further alleviate omitted variable concerns. We include birth country and the individual's cohort, and municipality fixed effects. Including the cohort fixed effects ensures that the estimates are driven by variations in parental income volatility among individuals born in the same year. This is important because the individuals born earlier have a much higher probability of becoming entrepreneurs compared to the ones born later in our sample.[12] In addition, cohort fixed effects control for common business cycles experienced by the individuals born in the same year, which might correlate with both the business opportunities for them to start a firm and their parental income volatility. Municipality fixed effects capture time-invariant determinants of starting a business in each area, such as geographic characteristics or the presence of a startup hub. We fix an individual's municipality to be the one she lives in at the age of 13. As discussed in Section 2.2.2, to account for the endogenous matching between the fathers and their industries that can contaminate our IV estimates, we include the father's sector fixed effects. We set the father's sector to be the one in which he works when the individual is 13 years old. To ensure that our results are not driven by the age benchmark, we exclude individuals whose fathers ever switched industries when they were between 13 and 19 years old in a robustness check. We also control for each parent's birth year fixed effects, which account for the life cycle effects on the parents' income and choice of employers when the individuals are in their adolescence. We cluster standard errors at the municipality level.

2.3 Data and Summary Statistics

Our paper combines several sources of Swedish administrative data. Information on an individual's birth year, municipality of residence, and country of birth comes from the Population and Housing Census data (FoB). The Longitudinal Integration Database for Health Insurance and Labor Market Studies (LISA) provides annual-level data on annual income, education attainment, professional status, employer, industry, employer size, and employer location from 1990 to 2014. Entrepreneurship classifications are based on individuals' primary work activity in November of each year. Using Swedish administrative data, Humphries (2019) shows that this classification of entrepreneurship aligns closely with an alternative classification based on an individual's primary source of labor market income during the year. Our baseline analysis defines entrepreneurs as individuals who ever start any type of business before 2014. We also

[12] An individual born in 1980 reaches the age of 34 at the end of our sample period, while an individual born in 1994 only reaches 20.

use an alternative definition of entrepreneurs – individuals who start an incorporated business as a robustness check. We use three-digit SNI industry codes provided by LISA to construct individuals' and their parents' industries. The linkage between each individual and her parents is provided by the Multi-Generation Register of Statistics Sweden. We complement the education data from LISA with data on grades at the end of ninth grade taken from the government authority for education (Skolverket). We construct parental income volatility using the annual income data of the parents. Since both the annual income data of the parents and the annual entrepreneurial status of the individual in our sample are available between 1990 and 2014, we focus on the Swedish individuals born between 1980 and 1994. We drop individuals who have at least one parent with missing income data when they are between 13 and 19. We also drop all the entrepreneurs who have zero employees when starting their businesses from the sample. To construct our instrument, we further limit the sample by dropping individuals whose fathers' employers have fewer than three employees. This leaves us with around 1 million individuals who make up our main sample.

Table 2.1 presents descriptive statistics for our sample of Swedish individuals born between 1980 and 1994. In Panel A, we provide statistics for the full sample, including those for whom we can construct parental income volatility and the instrument in Columns 1-3, as well as those who ever started a business before 2014 in Columns 4-6. Notably, we find that the entrepreneur sample exhibits higher levels of parental income volatility and average wage volatility of the fathers' employers compared to the full sample, indicating a positive correlation between entrepreneurial entry and both parental income volatility and its instrument in the data. The ratio of individuals who become entrepreneurs before 2014 is around 5% in the full sample, which is lower than the numbers reported in a few previous studies using Swedish administrative data. This is not unexpected, given that our sample's oldest cohort only reaches 34 years old by the end of our sample period, whereas other studies often focus on individuals at a higher age.[13] Furthermore, the entrepreneur sample is distinct from the full sample in several other ways, including an earlier birth

[13] See, e.g., Humphries (2019).

year, a higher likelihood of having parents who are entrepreneurs, better academic performance, and lower female representation.

In Panel B, we present summary statistics on the businesses established by the individuals in our entrepreneur sample. The survival rates of their businesses are around 57% and 49% two and three years after they are established, respectively. This is not surprising given that the average entrepreneur in our sample starts their business at the age of 25, and may lack the necessary experience and financial resources to run a successful business. We measure the size of each business by the number of its employees. Conditional on survival, an average business established by the entrepreneurs in our sample has around 4.5 and 5.1 employees two and three years after establishment, respectively. Our benchmark measure of an industry's riskiness is its ex-ante survival rate. This rate is determined by dividing the number of firms in the entrepreneur's industry that have at least one employee two years before she establishes her business by the number of firms that have at least one employee one year before her entry. As an alternative measure, we also compute the five-year average of our benchmark measure to account for the variation in the average business survival rate in an industry driven by the business cycle instead of riskiness. On average, the entrepreneurs' industries have an average ex-ante firm survival rate of around 85%.

2.4 Likelihood of Becoming Entrepreneurs

In this section, we discuss the effects of parental income volatility during adolescence on individuals' entrepreneurial entry.

2.4.1 Estimation Results

We report the estimates of Equation 2.5 in Table 2.2, where the dependent variable is a binary variable that takes the value of one if an individual becomes an entrepreneur by the end of 2014 and zero otherwise. Columns 1 and 4 present the OLS estimates. In Column 1, we find a statistically significant and positive correlation between parental income volatility and entrepreneurial entry within cohorts. In Column 4, we add a full set of controls as described in Section 2.2.3, and the estimated coefficient in front of parental income volatility remains statistically significant. In addition, consistent with the existing literature, we find that entrepreneurial entry is positively associated with having entrepreneurial parents, an individual's cognitive ability which we measure with grades, and negatively associated with being female.

Columns 2 and 5 of Table 2.2 present the first stage of our IV estimates, with the latter including the full set of controls. As anticipated, we find that individuals with fathers whose employers experience greater unexpected wage volatility tend to have higher parental income volatility, on average. Our results are supported by the Kleibergen-Paap F-statistics of 47.48 and 28.20 for Columns 2 and 5, respectively, both of which strongly reject the null hypothesis that our instruments are irrelevant in the first stage.

The results of the second-stage estimates are presented in Columns 3 and 6. In both cases, the coefficient in front of parental income volatility is positive and statistically significant at the 1% level.[14] Specifically, the point estimate from our preferred specification in Column 6 implies that a one standard deviation increase in parental income volatility increases the probability of becoming an entrepreneur by approximately 2.3 percentage points. This estimated effect is economically significant, as the average probability of becoming an entrepreneur is about 5% among the individuals in our sample. Thus, a one-standard-deviation increase in parental income volatility would lead to a 45% increase in the probability of becoming an entrepreneur relative to the sample mean.

2.4.2 Discrepancy between the IV and OLS Estimates

Upon comparing the results presented in Columns 1 and 3, as well as Columns 4 and 6, we observe that the IV estimates are considerably larger than their OLS counterparts. One potential explanation for the discrepancy is the presence of (conceptual) measurement error, as discussed

[14] Since both our key independent variable and its instrument are predicted variables, we also estimate the regression using cluster bootstrap and find that the significance of our key coefficient remains robust.

in Section 2.2.1. This view is supported by previous research on earnings risk, such as Cunha et al. (2005), Low et al. (2010), and Guvenen and Smith (2014), which suggest that a substantial portion of our measure of parental income volatility captures endogenous choices rather than unexpected income fluctuations. In the presence of measurement error associated with our parental income volatility measure, we would expect our IV estimates to be larger than the OLS estimates. This is because an individual's response to expected parental income fluctuations, including their decision to become an entrepreneur, is likely to be less sensitive than their response to uninsurable parental income fluctuations.

Another possible explanation for the gap between the IV and OLS estimates is that the instrument addresses omitted variables that bias the OLS estimates. While it is not possible to enumerate and evaluate all the possible omitted variables, a crucial potential variable of concern is the unobserved risk tolerance of the individuals' parents, which our instrument at least partially captures. However, omitting the parents' risk tolerance from the analysis may lead to an overestimation of the true effect rather than an underestimation, as risk tolerance is likely to be positively correlated with both parental income volatility and individuals' propensity to engage in entrepreneurship. For instance, parents with higher risk tolerance may be more willing to make decisions that result in higher income volatility, such as investing in high-risk financial assets or participating in gambling activities.

Additionally, heterogeneous effects of parental income volatility on entrepreneurial entry might also account for the divergence between our OLS and IV estimates. Compared to OLS, IV estimation assigns more weight to the individuals whose parental income volatility is more affected by the instrument. If these individuals – whose parental income volatility is more sensitive to the instrument – are more susceptible to the effects of parental income volatility on entrepreneurial entry, we would expect the IV estimated coefficient to be larger than the OLS estimated coefficient, even in the absence of measurement error or omitted variable.

There are strong reasons to believe that the effect of parental income volatility may be more pronounced for individuals whose parental income volatility is more influenced by the instrument.

For instance, the income volatility of lower-income parents is likely to be more affected by the average wage volatility of their employers, as higher-income parents may have other sources of income aside from wages. In addition, the effect of parental income volatility on entrepreneurial entry may be weaker for individuals whose parents have higher incomes, as they may be more likely to become entrepreneurs through other channels such as family wealth and connection with other entrepreneurs through family networks.[15] This hypothesis is supported by our data, as shown in Figure 2.1. Compared to individuals in the highest parental income quintile, those in the lowest parental income quintile have around 50% percent (or .03 percentage point) higher correlation between parental income volatility and the average wage volatility of the fathers' employers. Moreover, the correlation between parental income volatility and entrepreneurial entry is significantly higher for individuals in the lowest parental income quintile.

To estimate the extent to which heterogeneous effects contribute to the discrepancy between our IV and OLS estimates, we adopt a quasi-control function approach that is in a similar spirit to the one in Eisensee and Strömberg (2007). Specifically, we estimate:

$$E_i = \beta_1 \text{Volatility}_i + \beta_2 |\text{FS residual}| + \beta_3 \text{Volatility}_i \times |\text{FS residual}| + \boldsymbol{X}'_i \lambda + \xi_i \qquad (2.6)$$

where i denotes the individuals and the dependent variable E is a binary variable that equals one if an individual ever starts a business before 2014 and zero otherwise. $\boldsymbol{X}$ is the same vector of controls detailed in Section 2.2.3. *FS residual* is the predicted residual from the first-stage of our preferred IV specification. The absolute value of *FS residual* measures the extent to which each individual's parental income volatility is explained by the instrument. If the effect of parental income volatility on entrepreneurial entry is indeed stronger for individuals whose parental income volatility is more impacted by the wage volatility of the fathers' employers, we would expect a higher correlation between parental income volatility and entrepreneurial entry for those with *FS residual* values close to 0, which is captured by β_1. In other words, the magnitude of estimated

[15] Consistent with this hypothesis, Hvide and Panos (2014) show that the relationship between stock market participation and entrepreneurial entry is weaker for individuals with lower parental income.

β_1 should be greater than the OLS estimate in Column 4 of Table 2.2. Similarly, if our assumption of the heterogenous effects is valid, we would anticipate a negative β_3, given that we expect a weaker effect of parental income volatility for the individuals with larger estimated residuals from the first-stage.

Table 2.3 reports the estimates from Equation 2.6. As anticipated, the estimated β_1 of 0.0045 is significantly larger than the benchmark OLS estimate of 0.0011 reported in Table 2.2, and the estimated β_3 is negative and statistically significant. If we assume that the effect of parental income volatility is the strongest for individuals whose parental income volatility is most sensitive to the instrument, the difference between $\hat{\beta}_1$ estimated from Equation 2.6 and the OLS estimated coefficient from Equation 2.5 provides an upper bound of the extent to which our IV-OLS difference can be attributed to the heterogeneity in the effect of parental income volatility on entrepreneurial entry. Since the difference of 0.0034 accounts for only approximately 15% of the gap between our IV and OLS estimates in Table 2.2, the heterogeneous effect of parental income volatility on entrepreneurial entry may only partially account for the difference between our IV and OLS estimates. Hence, we interpret these results as supporting evidence that our IV-OLS gap is at least partially attributed to the conceptual measurement error in parental income volatility.

2.4.3 Robustness

We conduct a battery of additional tests to ensure the robustness of our findings. First, we show that our results for entrepreneurial entry are robust to alternative ways of constructing our instrument. Specifically, we re-construct the instrument using all employers of the fathers instead of requiring them to have at least three employees in Columns 1 and 2 of Table 2.4. In Columns 3 and 4 of Table 2.4, we construct a Jackknife instrument, i.e., we exclude all the individuals' fathers when computing the average wage of the employers. The instrument is statistically strong in both cases, with the corresponding first-stage F-statistics (Kelibergen-Paap) being 408.95 and 17.49, respectively. Furthermore, the point estimates of the effect of parental income volatility remain consistent with our benchmark IV specification.

We then explore the robustness of our IV results to different sample and specification restrictions. Following preceding studies such as Hvide and Panos (2014), we redefine entrepreneurs as incorporated business owners. As shown in Column 1 of Table 2.5, the estimated effect of parental income volatility then becomes 0.011, which is significantly smaller than the benchmark estimate of 0.023 reported in Table 2.2. However, given that the average probability of becoming an incorporated entrepreneur is about 1.7% among the individuals in our sample, the point estimate for the effect on incorporated entrepreneurial entry implies a similar estimated effect in terms of the percentage change relative to the sample mean.

In Section 2.2.2, we discussed the concern of a mechanical relationship between a firm's average wage volatility and an employee's income volatility when an employer has very few employees. A similar concern may arise when an individual's father is the owner of a business, and his wage represents a disproportionately high fraction of the total wages. To address this issue, we excluded all individuals with fathers who are entrepreneurs when they are between 13 and 19 years old from our sample and re-estimated our preferred IV specification. This exercise also sheds light on the extent to which our results are driven by comparison of individuals whose fathers have higher risk tolerance with those whose fathers have lower risk tolerance. If higher parental income volatility is driven by the fathers' higher risk tolerance, the entrepreneurs with higher parental income volatility may be more likely to start a business due to the inherited stronger risk tolerance from their parents through genetics rather than parental income volatility per se. In this case, when we exclude individuals whose fathers were entrepreneurs and thus are likely to be more risk-tolerant, we would expect the estimated effect of parental income volatility to become smaller in magnitude and statistically less significant. However, as shown in Column 2 of Table 2.5, the estimated effect of 1.8 percentage points remains similar to our benchmark IV estimate of 2.3 percentage points and is statistically significant at 5%.

In Column 3 of Table 2.5, we investigate whether our results are sensitive to how we measure fathers' industries. We limit our sample to individuals whose fathers did not change industries between the individuals' ages of 13 and 19. In Column 4, we explore whether our results are driven by comparison of individuals whose fathers work in multiple different firms and those whose fathers stay with the same employers. This is important because fathers who switch jobs may have different unobserved characteristics correlated with both the employer type, therefore the employers' wage volatility, and their children's propensity to become entrepreneurs compared to those who do not switch jobs. To address this, we limit our sample to individuals whose fathers did not change employers during the individuals' adolescence. Finally, in Column 5, we control for the fathers' firm fixed effects instead of sector fixed effects to account for the potentially endogenous matching between the fathers and their employers. Specifically, we control for the fixed effects of the fathers employers when the individuals are 13 years old and obtain an estimate of 1.7 percentage points. Overall, in Table 2.5, we show that our results are robust to these alternative sample restrictions and specifications.

2.5 Survival Rates of New Business

We now explore whether parental income volatility has any effect on the survival of businesses founded by the entrepreneurs born between 1980 and 1994. We estimate the IV regression specified in Equation 2.5, with a binary variable that equals 1 if a business survives and 0 otherwise as the dependent variable. Since the entrepreneurs in our sample reach at most the age of 34 by the end of our sample period, we focus on survival rates two or three years after a business is established.

Columns 1 and 3 of Table 2.6 present the first-stage estimates for the IV regressions of the two- and three-year survival rates, respectively. The corresponding Kleibergen-Paap F-stats are 183.45 and 98.47, respectively, lending strong support to the relevance of the instrument in the first stages. Columns 2 and 4 report the second-stage estimates. The point estimates suggest that a one-standard-deviation increase in parental income volatility when an individual is between 13 and 19 years old is associated with a decline in the two-year survival rate of approximately 6.6 percentage points and a decline in the three-year survival rate of about 7.2

percentage points. These effects are economically significant. Given that the average two- and three-year survival rates of businesses started by the entrepreneurs in our sample are 57% and 49%, respectively, our point estimates correspond to a decrease in the two- and three-year survival rates by approximately 12% and 15%, respectively, compared to the mean. Overall, our results demonstrate that higher parental income volatility is linked to worse entrepreneurial performance, on average.

2.6 Mechanisms

We have argued that higher parental income volatility during adolescence raises the likelihood of individuals starting their own businesses. Furthermore, once these individuals become entrepreneurs, their performance tends to be weaker. The observed effects could potentially stem from various channels, which we explore in this section. To that end, we offer empirical evidence to assess the relevance of each of these channels.

2.6.1 Risk Tolerance

Individuals who experience hardship during their early life may develop a higher risk tolerance (e.g., Bernile et al. (2017); Yi et al. (2022)). This may be particularly relevant to entrepreneurial entry, as prior research has found that higher risk tolerance is associated with a greater likelihood of starting a business (Hvide and Panos (2014)). Therefore, it is possible that individuals who experience higher parental income volatility during adolescence may be more likely to start new businesses because they develop a stronger tolerance for risk. Furthermore, since higher risk tolerance has also been associated with lower business survival rates, it may also explain the negative effect of parental income volatility on business survival documented in Section 2.5.

To examine the relevance of this mechanism, we first estimate the effect of parental income volatility on the age at which entrepreneurs initiate their businesses, using the IV regression specified in Equation 2.5. Starting a business at a younger age is generally considered riskier, as younger entrepreneurs may lack the experience and knowledge needed to run a business successfully, and may have limited financial resources to invest and weather unexpected challenges.

As an alternative check for the risk tolerance mechanism, we then investigate the relationship between parental income volatility and the ex-ante riskiness of the industries in which the entrepreneurs start their businesses. We use two measures for the ex-ante riskiness of an industry. The first one is the average one-year firm survival rate in the industry, which is computed for the year before the entrepreneur starts the business. This statistic contains the most recent information available to the entrepreneurs before starting the businesses, and thus is likely to be taken into account in their risk assessment. However, one potential concern is that this measure may also reflect business cycle conditions, which can confound the inherent riskiness of the industry. To address this concern, we also calculate the five-year average survival rate of firms in the industry as an alternative measure of the ex-ante riskiness of an industry.

Table 2.7 reports the estimates. In Column 1, we find that individuals who experience higher parental income volatility during adolescence tend to start their businesses at a slightly younger age. Specifically, a one-standard-deviation increase in parental income volatility is associated with a 1% decrease in the average age of business initiation, which translates to approximately 3 months earlier than the average age of starting businesses. In Columns 2 and 3, we find that a one-standard-deviation increase in parental income volatility is associated with a 14.8 and 12.3 percentage points decrease in the one- and five-year average firm survival rates of the entrepreneurs' industries, respectively, indicating that entrepreneurs with higher parental income volatility tend to start businesses in riskier industries. Overall, the findings presented in Table 2.7 are consistent with the hypothesis that individuals who experience higher parental income volatility during adolescence may develop stronger risk tolerance, which, in turn, helps them sort into entrepreneurship.

2.6.2 Human Capital Accumulation

Another possible explanation for our findings, which show a positive association between parental income volatility and entrepreneurial entry, and a negative association with business survival, is that higher parental income volatility may lead to lower levels of human capital. Hardy (2014), for instance, finds a negative correlation between parental income volatility and individuals' level of education in the United States, and our own data shows a similar pattern. If higher parental income volatility leads to individuals leaving schools earlier, it could be associated with a lower accumulation of knowledge that is crucial for running a successful business. Indeed, this could potentially explain the negative correlation we observe between parental income volatility and the age at which entrepreneurs start their businesses. Moreover, individuals with higher parental income volatility may be more likely to start businesses in industries that are different from their parents', where income is expected to be more stable. This could result in them benefiting less from the "dinner table" human capital that their parents may have acquired through their own industry experience (Hvide and Oyer, 2017).

In order to distinguish between the risk tolerance and human capital interpretations of our findings in Columns 1-3 of Table 2.7, we include additional controls in Columns 4-6 of the Table. Specifically, we control for 1) years of schooling fixed effects to ensure that our results are not driven by differences in the levels of education, 2) a dummy variable indicating whether an entrepreneur's parents have worked in the industry where she starts her business to account for the industry-specific knowledge that may be obtained from parental experience, and 3) cognitive and non-cognitive abilities of individuals taken from their military records, which provide more comprehensive measures of human capital that could affect entrepreneurship. If our results supporting the risk tolerance channel were driven solely by differences in human capital across individuals with different levels of parental income volatility, we would expect the estimated effects of parental income volatility on the age at which entrepreneurs initiate their businesses and the ex-ante riskiness of their industries to weaken after introducing these additional controls. However, by comparing our estimates in Columns 4-6 to their respective counterparts in Columns 1-3, we find that the signs of the estimated coefficients for parental income volatility remain unchanged, and the magnitudes become even stronger.

We also re-estimate the effects of parental income volatility on business survival, controlling for the years of schooling fixed effects and a dummy variable indicating whether the entrepreneur's parents worked in the industry where the business was started in Columns 1 and 3 of Table 2.8. If the estimated effects of parental income volatility on business survival, we documented in Section 2.5, are mostly driven by comparing individuals with different levels of human capital accumulated from studying in schools or learning from parents who worked in the same industries, we would expect the estimated effect on survival to become weaker after adding these additional controls. However, our results show that the estimated effect of parental income volatility on business survival remains largely unchanged. We also estimate the effect of parental income volatility on the size of the business two and three years after it was founded, conditional on survival. If entrepreneurs who experience higher parental income volatility are of lower quality, we should find a negative estimated effect on the size of the business. As shown in Columns 2 and 4 of Table 2.8, however, we find a positive and statistically significant effect of parental income volatility on the firm size: a one-standard-deviation increase in parental income volatility corresponds to 12.3% and 14.2% more employees two and three years after a business is launched, respectively.

These results suggest that human capital accumulation may not be the main driver behind our findings.

2.6.3 Financial Resources

Finally, it is possible that parents with more volatile income are more motivated to save and accumulate wealth in order to hedge against potential income shocks. As a result, individuals with higher levels of parental income volatility during adolescence may have greater financial resources for starting a new business. To investigate this potential mechanism, we analyze a sample of Swedish individuals for whom we have access to household asset and debt data from the Wealth Registry of Statistics Sweden between 1999 and 2007. The focus on the 1999-2007 period is due to data availability: data on individuals' wealth was collected in Sweden starting from 1999 and continued only until 2007 due to the repeal of the wealth tax in Sweden. Given that our parental income volatility measure is constructed based on an individual's adolescent years, specifically between the ages of 13 and 19, we narrow our focus to household wealth data before an individual reaches the age of 20.[16] By doing so, we restrict the sample to consist of individuals born between 1980 and 1990. To address the possibility of reverse causality, i.e., the possibility that the association between parental income volatility and household wealth is driven by an individual running a (successful) business, we exclude all individuals who started businesses before the age of 21 from our analysis. These restrictions result in a final sample of 3,744 individuals. We measure household wealth using the total assets net of debt. Table 2.9 presents the summary statistics for this sample and demonstrates that the descriptive statistics for this sample are comparable to those presented in Table 2.1 for the full sample.

One limitation we have is that our sample for which we have wealth information consists of genotyped twins whose data is collected by the Swedish Twin Registry, and uses different individual and firm identifiers than our full sample data. Therefore, we are unable to match this sample with our full sample and construct the firm average wage volatility as an instrument. This is due to the fact that for each firm, we only observe its employees who are genotyped twins or their parents. To explore the relevance of wealth accumulation as a channel through which parental income volatility affects entrepreneurship, we thus examine how the correlation between parental income volatility and entrepreneurial entry changes after we control for household wealth. Specifically, we first regress the binary variable of ever starting a business after the age of 20 on parental income volatility:

$$Entrepreneur_i = \beta_0 + \beta_1 \text{Volatility}_i + \text{Cohort FE} + \epsilon_i, \tag{2.7}$$

[16] Since the individuals in our sample are young, their household wealth instead of their own wealth is a more appropriate measure of their financial resources for starting a business.

We then add household wealth as a control variable to Equation 2.7 and examine how controlling for household wealth affects the OLS estimated coefficient $\hat{\beta}_1$. Table 2.10 reports the estimates. In Column 2, wealth is measured as the average household wealth when an individual is between 17 and 19 years old. One potential concern regarding our results in Column 2 is that the average household wealth calculation may differ slightly among individuals born in different years, due to the limited availability of our wealth data.[17] To address this concern, we have taken a more stringent approach in Columns 3 and 4, by restricting the sample to individuals born between 1980 and 1988 and measuring household wealth when each individual is 19 years old. Although the coefficients presented in Table 2.10 may not be causally interpreted due to the identification challenges outlined in Section 2.2, and the small sample size that reduces statistical power, it is noteworthy that the magnitude of the coefficients associated with parental income Volatility is comparable to that reported in Column 1 of Table 2.2 for the main sample. Furthermore, after adding household wealth as a control variable, we observe only a small change in the estimated coefficient in front of parental income volatility when comparing Columns 1 and 2, and Columns 3 and 4. Hence, it seems improbable that wealth accumulation solely drives the relationship between parental income volatility and entrepreneurial entry. We leave a thorough examination of this channel to future research.

2.7 Conclusion

Does parental income volatility affect entrepreneurship? Results from this research are particularly relevant given the persistent emphasis on promoting entrepreneurship as a key policy priority.[18] Understanding the factors that drive individuals' entrepreneurial decisions and performance is crucial for encouraging entrepreneurship. We use a unique Swedish administrative dataset to investigate this question. To account for the potential conceptual measurement

[17] For example, individuals born in 1980 would have their household wealth observed only at age 19, while those born in 1990 would have their household wealth observed only at age 17.

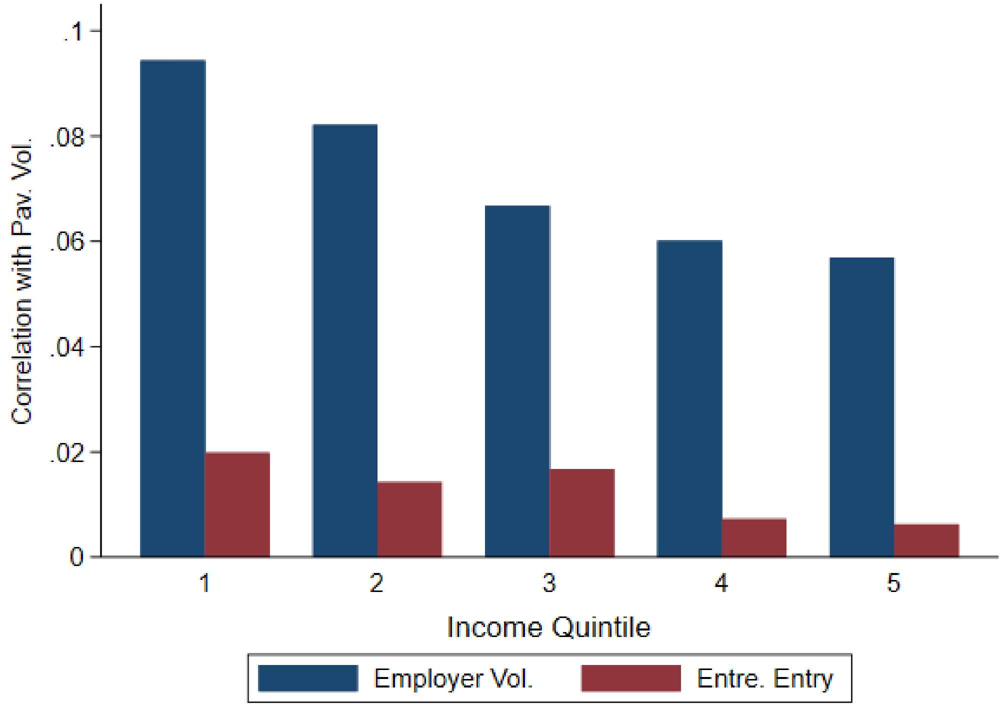
[18] For instance, Sweden spent about 1% of its GDP promoting small businesses and entrepreneurship in 2009 (Vikstrom (2011)).

error in parental income volatility and omitted variable bias to establish causality, we use the unexpected average wage volatility of the individuals' fathers' employers as an instrument and include a rich set of controls. We find a strong and positive effect of parental income volatility on entrepreneurial entry and a negative effect on subsequent business survival.

We also provide suggestive evidence that stronger risk tolerance developed as a result of higher parental income volatility during adolescence is likely to be an important channel through which higher parental income volatility leads to higher probability of starting a business and worse subsequent entrepreneurial performance. We do not find evidence for alternative mechanisms, including human capital accumulation and wealth accumulation. A more thorough investigation of the mechanisms through which parental income volatility affects entrepreneurship may require more direct measures of the individuals' risk preferences, such as survey and experimental data on risk preference, as well as more comprehensive wealth data that covers the full sample of individuals. We, therefore, leave it to future research.

Our findings shed light on a new externality from governmental programs that aim to stabilize income of families with children, such as the EITC and unemployment insurance: these programs could have long-term impacts on individuals' economic and financial decisions such as whether, when, and where to start new businesses, by affecting their risk preference. However, generalizing our results to other economies would require caution. As Bernile et al. (2017) argue, the effects of exposure to a particular macroeconomic or personal event on risk-taking and decision-making may not be unidirectional. If the population in an economy has a much more spread out distribution of family income volatility during early life, the relationship between this instability and individuals' entrepreneurial decisions and other behaviors can be very different.

Figure 2.1: Correlation with Parental Income Volatility by Income Group



The figure shows the correlation between parental income volatility and employer wage volatility (blue bars) and the correlation between parental income volatility and individuals' entrepreneurial entry (red bars) by income quintiles.

Table 2.1: Summary Statistics

Panel A	All			Entrepreneur Sample		
	count	mean	sd	count	mean	sd
Parental Income Volatility	1178366	0.053	0.107	58386	0.063	0.116
Firm Volatility	1081738	0.087	0.237	50988	0.128	0.309
Firm Volatility (size<3)	992595	0.022	0.039	44160	0.028	0.046
Firm Volatility (Jackknife)	1030438	0.045	0.108	47060	0.063	0.137
Entrepreneur	1178366	0.050	0.217	58386	1.000	0.000
Incorporated Entrepreneur	1178366	0.017	0.128	58386	0.337	0.473
Parental Income(/1000SEK)	1178366	396.782	109.411	58386	396.597	114.279
Birth Year	1178366	1987.468	4.203	58386	1984.857	3.596
Father Entre.	1178366	0.255	0.436	58386	0.391	0.488
Mother Entre.	1178366	0.131	0.337	58386	0.229	0.420
Grade	1144068	0.106	0.941	57503	0.195	0.899
Female	1178366	0.488	0.500	58386	0.348	0.476
Education	1139116	12.786	1.841	57331	12.858	1.856
Birth Order	1178366	1.606	0.772	58386	1.424	0.649
Father Education	1178366	11.642	2.371	58386	11.759	2.612
Mother Education	1175585	11.639	2.154	58319	11.819	2.326
Father SameEmployer	1178366	0.496	0.500	58386	0.492	0.500
Father SameSector	1178366	0.544	0.498	58386	0.539	0.498

Panel B	Entrepreneur Sample		
	count	mean	sd
Age Starting a Business	58386	24.780	3.989
2-Year Survival Rate	43938	0.574	0.495
3-Year Survival Rate	36966	0.489	0.500
2-Year Business Size	25208	4.484	18.916
2-Year Business Size	18078	5.126	22.215
Industry Ex-ante Survival Rate	58356	0.848	0.050
Industry Ex-ante Survival Rate (5-Y Average)	57852	0.848	0.045

The table reports the summary statistics of average individual-level characteristics (Panel A) and average business-level characteristics (Panel B).

Table 2.2: Effect of Parental Income Volatility on Entrepreneurial Entry

	OLS	IV		OLS	IV	
		First-Stage	Second-Stage		First-Stage	Second-Stage
Dependent variable:	Entrepreneur (1)	Par. Volatility (2)	Entrepreneur (3)	Entrepreneur (4)	Par. Volatility (5)	Entrepreneur (6)
Volatility	0.00432***		0.0402***	0.00114***		0.0225***
	(0.000242)		(0.00353)	(0.000370)		(0.00535)
Firm Volatility		0.695***			0.377***	
		(0.101)			(0.0710)	
log(income)				0.00183**	0.221***	-0.00289*
				(0.000625)	(0.0173)	(0.00157)
Female				-0.0250***	0.00752***	-0.0251***
				(0.00463)	(0.00199)	(0.00460)
Father Entre.				0.0279***	0.240***	0.0225***
				(0.00621)	(0.0163)	(0.00671)
Mother Entre.				0.0305***	0.343***	0.0230***
				(0.00643)	(0.0195)	(0.00718)
Grade				0.0158***	-0.00189	0.0159***
				(0.00244)	(0.00183)	(0.00242)
Birth Order				-0.00125***	0.0902***	-0.00325***
				(0.000389)	(0.0118)	(0.000707)
Education				-0.00843***	-0.00399**	-0.00832***
				(0.00118)	(0.00149)	(0.00119)
Father Educ.				0.000815***	-0.00408***	0.000905***
				(0.000213)	(0.000905)	(0.000203)
Mother Educ.				0.00156***	-0.0108***	0.00179***
				(0.000342)	(0.00150)	(0.000326)
Constant	0.0427***	0.352***		0.105***	-2.664***	
	(0.000820)	(0.00523)		(0.00890)	(0.201)	
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Foreign Born FE				Yes	Yes	Yes
Father Cohort FE				Yes	Yes	Yes
Mother Cohort FE				Yes	Yes	Yes
Municipality FE				Yes	Yes	Yes
Father Industry FE				Yes	Yes	Yes
Observations	979788	979788	979788	891145	887722	887722
Kleibergen-Paap F-stat		47.48			28.20	

The table reports the estimated effects of parental income volatility on entrepreneurial entry. The dependent variable *Entrepreneur* is a binary variable that equals one if an individual has ever become an entrepreneur. The dependent variable *Par. Volatility* is the parental income volatility when an individual is between 13 and 19, normalized by the sample standard deviation. Standard errors are clustered at the municipality level. *, **, and *** denote 10%, 5%, and 1% statistical significance, respectively.

Table 2.3: Effect of Parental Income Volatility on Entrepreneurial Entry: Heterogeneity

Dependent variable:	Entrepreneur (1)
Volatility	0.00451***
	(0.00125)
Volatility x abs(First-Stage Residual)	-0.00106***
	(0.000300)
abs(First-Stage Residual)	0.00227
	(0.00198)
log(income)	0.00240***
	(0.000466)
Female	-0.0213***
	(0.00418)
Father Entre.	0.0277***
	(0.00615)
Mother Entre.	0.0305***
	(0.00633)
Birth Order	-0.00313***
	(0.000493)
Education	-0.00348***
	(0.000658)
Father Educ.	0.00134***
	(0.000246)
Mother Educ.	0.00237***
	(0.000406)
Constant	0.0196***
	(0.00549)
Foreign Born FE	Yes
Cohort FE	Yes
Father Cohort FE	Yes
Mother Cohort FE	Yes
Municipality FE	Yes
Father Industry FE	Yes
Observations	905471

The table reports the estimated effects of parental income volatility on entrepreneurial entry. The dependent variable *Entrepreneur* is a binary variable that equals one if an individual has ever become an entrepreneur. Standard errors are clustered at the municipality level. *, **, and *** denote 10%, 5%, and 1% statistical significance, respectively.

Table 2.4: Robustness to Different Instrument Construction

Dependent variable:	Entrepreneur			
Instrument construction:	Incl. Employers of All Sizes		Jackknife	
	First-Stage (1)	Second-Stage (2)	First-Stage (3)	Second-Stage (4)
Volatility		0.0170***		0.0140**
		(0.00456)		(0.00696)
Firm Volatility	0.345***		0.0600***	
	(0.0171)		(0.00701)	
log(income)	0.245***	-0.00343*	0.254***	-0.00194
	(0.0142)	(0.00174)	(0.0142)	(0.00166)
Female	0.0105***	-0.0275***	0.0104***	-0.0259***
	(0.00253)	(0.00494)	(0.00236)	(0.00475)
Father Entre.	0.386***	0.0207**	0.409***	0.0223***
	(0.0158)	(0.00754)	(0.0157)	(0.00710)
Mother Entre.	0.354***	0.0257***	0.353***	0.0267***
	(0.0145)	(0.00761)	(0.0147)	(0.00716)
Birth Order	0.0938***	-0.00269***	0.0934***	-0.00253***
	(0.00998)	(0.000690)	(0.00979)	(0.000845)
Education	-0.00430**	-0.00907***	-0.00432**	-0.00868***
	(0.00158)	(0.00127)	(0.00153)	(0.00122)
Father Educ.	-0.00336***	0.000797***	-0.00420***	0.000827***
	(0.00105)	(0.000201)	(0.000999)	(0.000213)
Mother Educ.	-0.0126***	0.00185***	-0.0123***	0.00174***
	(0.00132)	(0.000282)	(0.00130)	(0.000339)
Grade	-0.00618**	0.0164***	-0.00551**	0.0161***
	(0.00226)	(0.00248)	(0.00219)	(0.00246)
Constant	-2.951***		-3.056***	
	(0.162)		(0.163)	
Foreign Born FE	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes
Father Cohort FE	Yes	Yes	Yes	Yes
Mother Cohort FE	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes
Father Industry FE	Yes	Yes	Yes	Yes
Observations	975560	975560	930352	930352
Kleibergen-Paap F-stat	408.95		17.49	

The table reports the effects of bank commitments on the number of patents filed by brown firms. Standard errors are clustered at the firm level. *, **, and *** denote 10%, 5%, and 1% statistical significance, respectively.

Table 2.5: Robustness to Different Sample Restrictions and Firm Fixed Effects

Dependent variable:	Incorporated	Entrepreneur			
		excl. Father Entre.	excl. Ind. Switchers	excl. Firm Switchers	Firm FE
	(1)	(2)	(3)	(4)	(5)
Volatility	0.0108**	0.0181**	0.0290***	0.0314***	0.0166*
	(0.00521)	(0.00906)	(0.00964)	(0.0103)	(0.00951)
log(income)	0.0000607	-0.00208	-0.00514**	-0.00580**	-0.00200
	(0.00149)	(0.00208)	(0.00218)	(0.00251)	(0.00276)
Female	-0.0155***	-0.0222***	-0.0247***	-0.0238***	-0.0238***
	(0.00362)	(0.00421)	(0.00457)	(0.00462)	(0.000952)
Father Entre.	0.0190***		0.0278***	0.0279**	0.0186***
	(0.00614)		(0.00905)	(0.0100)	(0.00323)
Mother Entre.	0.0150**	0.0153***	0.0236***	0.0237**	0.0197***
	(0.00575)	(0.00437)	(0.00788)	(0.00838)	(0.00338)
Grade	0.00615***	0.0150***	0.0163***	0.0155***	0.0153***
	(0.00131)	(0.00233)	(0.00239)	(0.00243)	(0.000463)
Birth Order	-0.00228***	-0.00158*	-0.00308**	-0.00333**	-0.00262***
	(0.000487)	(0.000738)	(0.00117)	(0.00115)	(0.000816)
Education	-0.00356***	-0.00743***	-0.00862***	-0.00829***	-0.00792***
	(0.000544)	(0.00116)	(0.00121)	(0.00121)	(0.000245)
Father Educ.	-0.0000292	0.00116***	0.000877***	0.000909***	0.000990***
	(0.0000876)	(0.000223)	(0.000208)	(0.000304)	(0.000171)
Mother Educ.	0.000508***	0.00165***	0.00205***	0.00199***	0.00154***
	(0.000129)	(0.000334)	(0.000390)	(0.000327)	(0.000211)
Foreign Born FE	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes
Father Cohort FE	Yes	Yes	Yes	Yes	Yes
Mother Cohort FE	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes
Father Industry FE	Yes	Yes	Yes	Yes	
Father Firm FE					Yes
Observations	925670	783507	529087	476762	832289

The table shows the robustness to alternative sample restrictions and firm fixed effects. *Incorporated* is a dummy that equals one if an individual ever starts an incorporated business. *Entrepreneur* is a dummy that equals one if an individual is ever an entrepreneur. Columns 2-4 excludes all the individuals whose father are ever entrepreneurs, the individuals whose fathers ever switch industries, and individuals whose fathers ever switch employers, respectively, from the sample. Column 5 includes firm fixed effects. Standard errors are clustered at the municipality level. *, **, and *** denote 10%, 5%, and 1% statistical significance, respectively.

Table 2.6: Effect of Parental Income Volatility on the Type of Startups

Dependent variable:	2-Year Survival		3-Year Survival	
	First-Stage (1)	Second-Stage (2)	First-Stage (3)	Second-Stage (4)
Volatility		-0.0660**		-0.0722**
		(0.0276)		(0.0254)
Firm Volatility	1.914***		1.956***	
	(0.144)		(0.197)	
log(income)	0.289***	0.00266	0.290***	0.00524
	(0.0131)	(0.00921)	(0.0156)	(0.00744)
Female	-0.00295	-0.0130	-0.00613	-0.00252
	(0.0147)	(0.0102)	(0.0156)	(0.0111)
Father Entre.	0.265***	0.0136**	0.252***	0.00137
	(0.0202)	(0.00507)	(0.0189)	(0.00846)
Mother Entre.	0.342***	0.0439**	0.349***	0.0415**
	(0.0279)	(0.0173)	(0.0272)	(0.0191)
Father Educ.	-0.00429	-0.00989***	-0.00421	-0.00958***
	(0.00436)	(0.00118)	(0.00295)	(0.00125)
Mother Educ.	-0.0126***	-0.00389**	-0.0141***	-0.00352
	(0.00389)	(0.00162)	(0.00430)	(0.00212)
Grade	-0.0160	-0.0179***	-0.00874	-0.0123*
	(0.0113)	(0.00451)	(0.0113)	(0.00647)
Birth Order	0.0868***	0.0181**	0.0882***	0.0218**
	(0.0173)	(0.00624)	(0.0139)	(0.00852)
Constant	-3.544***		-3.560***	
	(0.185)		(0.211)	
Foreign Born FE	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes
Father Cohort FE	Yes	Yes	Yes	Yes
Mother Cohort FE	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes
Father Industry FE	Yes	Yes	Yes	Yes
Observations	23851	23851	19905	19691

The table shows the effect of parental income volatility during an entrepreneur's adolescence on the survival the new business. *Survival* is a dummy that equals one if the business started by the entrepreneur survives. Standard errors are clustered at the municipality level. *, **, and *** denote 10%, 5%, and 1% statistical significance, respectively.

Table 2.7: Mechanism: Risk Tolerance

Dependent variable:	log(StartAge) (1)	Ind. Surv. Rate (2)	5-Yr. Ind. Surv. Rate (3)	log(StartAge) (4)	Ind. Surv. Rate (5)	5-Yr. Ind. Surv. Rate (6)
Volatility	-0.0113*	-0.140**	-0.110**	-0.0263**	-0.274*	-0.210**
	(0.00650)	(0.0657)	(0.0510)	(0.0129)	(0.147)	(0.101)
log(income)	0.00172	0.00400**	0.00340**	0.00400	0.00601	0.00537**
	(0.00226)	(0.00178)	(0.00139)	(0.00450)	(0.00367)	(0.00245)
Female	0.00728***	0.00476***	0.00665***	0.0198*	0.00529	0.00488
	(0.00136)	(0.000877)	(0.000707)	(0.0112)	(0.00522)	(0.00451)
Father Entre.	-0.00368	0.00309**	0.00243*	-0.00546	0.00317	0.00401*
	(0.00298)	(0.00136)	(0.00126)	(0.00550)	(0.00240)	(0.00220)
Mother Entre.	-0.00434*	0.00359*	0.00232	-0.000449	0.00670	0.00403
	(0.00269)	(0.00202)	(0.00149)	(0.00528)	(0.00398)	(0.00279)
Grade	-0.00411***	-0.00780***	-0.00788***			
	(0.000804)	(0.000435)	(0.000332)			
Father Educ.	0.000728**	-0.00122***	-0.00122***	-0.0000808	-0.000488*	-0.000663***
	(0.000337)	(0.000195)	(0.000156)	(0.000626)	(0.000238)	(0.000204)
Mother Educ.	0.00134***	-0.000700***	-0.000725***	-0.000775	-0.000943**	-0.000794**
	(0.000356)	(0.000196)	(0.000156)	(0.000650)	(0.000363)	(0.000286)
Birth Order	0.00353**	0.000522	0.000529	0.0112***	0.000864	0.000834
	(0.00149)	(0.000714)	(0.000581)	(0.00291)	(0.00112)	(0.000905)
Non-cognitive Score				-0.00175**	0.00271***	0.00257***
				(0.000700)	(0.000252)	(0.000191)
Cognitive Score				-0.0000807	-0.00171***	-0.00206***
				(0.000783)	(0.000498)	(0.000410)
Foreign Born FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Father Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Mother Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
Father Sector FE	Yes	Yes	Yes	Yes	Yes	Yes
Same-Industry Dummy					Yes	Yes
Schooling FE				Yes	Yes	Yes
Observations	28448	23593	23556	9871	8166	8150

The table shows the effect of parental income volatility during an entrepreneur's adolescence on the age of starting a business and the ex-ante riskiness of the new business. *log(StartAge* is the log of age when an entrepreneur starts a business. *Ind. Surv. Rate* is the last-year average survival rate of firms in the industry where the entrepreneur starts the business. *5-Yr Ind. Surv. Rate* is the last-five-year average survival rate of firms in the industry where the entrepreneur starts the business. Standard errors are clustered at the municipality level. *, **, and *** denote 10%, 5%, and 1% statistical significance, respectively.

Table 2.8: Robustness to Same-Sector and Schooling Fixed Effects

	2-Year Outcomes		3-Year Outcomes	
Dependent variable:	Survival (1)	log(Emp) (2)	Survival (3)	log(Emp) (4)
Volatility	-0.0737** (0.0297)	0.123** (0.0605)	-0.0761** (0.0346)	0.142** (0.0589)
log(income)	0.0148* (0.00812)	-0.0274 (0.0226)	0.0148 (0.0101)	-0.0310 (0.0223)
Female	-0.0223** (0.00855)	-0.0526*** (0.00700)	-0.0110 (0.00892)	-0.0844*** (0.0102)
Father Entre.	0.0129 (0.00761)	-0.0466** (0.0211)	0.0000184 (0.00915)	-0.0590** (0.0254)
Mother Entre.	0.0422** (0.0190)	-0.0385 (0.0252)	0.0371 (0.0224)	-0.0419 (0.0248)
Father Educ.	-0.00468*** (0.00111)	0.00107 (0.00175)	-0.00447*** (0.00113)	-0.0000906 (0.00288)
Mother Educ.	0.000221 (0.00173)	-0.000735 (0.00173)	0.000415 (0.00228)	-0.000348 (0.00279)
Grade	0.0280*** (0.00507)	-0.00841 (0.00608)	0.0321*** (0.00641)	0.00208 (0.00706)
Birth Order	0.0157*** (0.00517)		0.0184** (0.00717)	
Initial Size		0.397*** (0.00671)		0.403*** (0.00742)
Foreign Born FE	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes
Father Cohort FE	Yes	Yes	Yes	Yes
Mother Cohort FE	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes
Father Sector FE	Yes	Yes	Yes	Yes
Same Sector FE	Yes	Yes	Yes	Yes
Schooling FE	Yes	Yes	Yes	Yes
Observations	23293	11493	19327	7731

The table shows the effect of parental income volatility during an entrepreneur's adolescence on the survival rates and size of the new business. *Survival* is a dummy that equals one if the business started by the entrepreneur survives. Standard errors are clustered at the municipality level. *, **, and *** denote 10%, 5%, and 1% statistical significance, respectively.

Table 2.9: Descriptive Statistics: Subsample with Wealth Information

	All		Entrepreneur	
	mean	sd	mean	sd
Parental Income Volatility	0.017	0.035	0.024	0.047
Entrepreneur	0.036	0.185	1.000	0.000
Parental Income(/1000SEK)	394.002	129.292	384.694	125.118
birthYear	1986.206	3.103	1984.662	3.017
Father Entre.	0.233	0.423	0.451	0.499
Mother Entre.	0.121	0.326	0.195	0.398
female	0.622	0.485	0.436	0.498
Av. Household Wealth At Age 17-20(/1000SEK)	2284.163	5352.619	2463.252	3363.691
Household Wealth At Age 20(/1000SEK)	2271.511	5522.259	2439.593	3357.140
Observations	3744		133	

The table reports the summary statistics of average individual-level characteristics of the sample of individuals for whom we have data on household wealth.

Table 2.10: Alternative Mechanism: Wealth Accumulation

Dependent variable:	Entrepreneur			
Sample:	Cohort80-90	Cohort80-90	Cohort80-88	Cohort80-88
	(1)	(2)	(3)	(4)
Volatility	0.00703**	0.00619*	0.00619	0.00551
	(0.00334)	(0.00341)	(0.00414)	(0.00420)
log(Household Wealth)		0.00341**		0.00336**
		(0.00166)		(0.00166)
Cohort FE	Yes	Yes	Yes	Yes
Observations	3421	3353	2736	2678

The table shows the effect of parental income volatility during an entrepreneur's adolescence on entrepreneurial entry. Standard errors are clustered at the municipality level. *, **, and *** denote 10%, 5%, and 1% statistical significance, respectively.

Appendix A: Supplements to Chapter 1

A.1 Conceptual Model

In this section, I present a reduced-form model built upon the vertical differentiation model in Tirole (1988) to highlight the interaction between bank divestment and the borrowing firm's investment in green innovation. There are two major economic mechanisms in place: (a) divestment leads to lower profits regardless of whether firms invest in green innovation. However, the extent of this drop may be smaller if the firm invests in green innovation because green innovation alleviates the increase in the cost of capital by improving the greenness of the firm; (b) divestment makes investment in green innovation more costly, thereby reducing benefits of green innovation. The ultimate effect of divestment on green innovation depends on which of these opposing forces dominates.

Model Setup. In my setting, there is a mass-one continuum of consumers with a heterogeneous taste for greenness θ of the consumption good. θ is uniformly distributed on $[0, 1]$. For a given level of greenness s and price p, a consumer can purchase one unit of the good, and her corresponding utility is:

$$U = \begin{cases} \theta s - p & \text{if buying the good} \\ 0 & \text{if not buying the good} \end{cases}$$

Assume there is one firm in the economy that lives for one period and produces the good. Then the demand for the good with price p and greenness s is:

$$D(p) = 1 - \frac{p}{s}$$

which is in a similar spirit to the inverse demand function in Bustamante and Zucchi (2022). Let's additionally assume that the firm must borrow some exogenous fraction $\gamma \in [0, 1]$ of its costs prior to production. The firm then repays these costs with interest at the end of the period. Moreover, let's assume that there is an exogenous probability $\delta \in [0, 1]$ that after borrowing these costs upfront, but prior to production, the firm's technology becomes obsolete. Additionally, let's assume that lenders set an ex-ante interest rate, r, to break even in expectation:

$$b(1 + w(\bar{s} - s)) = b(1 + r)(1 - \delta) \implies r = \frac{w(\bar{s} - s) + \delta}{1 - \delta}$$

where w is a parameter capturing the additional cost the lender faces when lending to the firm with lower greenness and $\bar{s}$ is a benchmark level of greenness that is exogenous to both the firm and the lender. The monopoly firm that does not innovate then chooses p to maximize:

$$\begin{aligned}
\pi_0 &= (1 - \delta)D(p)[p - (1 - \gamma)c_1 - \gamma c_1(1 + r)] \\
&= (1 - \delta)D(p)[p - (1 + \gamma r)c_1]
\end{aligned}$$

where c_1 is the constant marginal cost for production. The firm has the option to invest in green technology, thereby altering the level of the greenness of its good from s to $(s + G)$. The innovation comes at a cost of c_2 for each unit of the good that is manufactured, resulting in a total cost that is directly proportional to the overall production. This reflects the notion that increased production necessitates higher replacement expenses. If the firm chooses to innovate, the interest rate it faces becomes:

$$r_I = \frac{w(\bar{s} - s - G) + \delta}{1 - \delta}$$

and its profit function will be:

$$\pi_I = (1 - \frac{p}{s+G})[p - (1+\gamma r_I)(c_1 + c_2)]$$

Therefore, the firm will choose to invest in green technology if $\Delta\pi^* = \pi_I^* - \pi_0^* > 0$, where $\Delta\pi^*$ equals the sum of the following three components:

1. $(D_I^* - D_0^*)\frac{s-c_1}{2}$
2. $D_I^* \times \frac{G-c_2}{2}$
3. $-(D_I^*\frac{(1+\gamma r_I)(c_1+c_2)}{2} - D_0^*\frac{(1+\gamma r)c_1}{2})$

I model bank divestment as an increase in w. The first component of $\Delta\pi^*$ represents the loss in profits from production.[1] When w increases, production becomes less profitable, regardless of whether the firm chooses to invest in innovation, due to an increase in the cost of capital. However, by investing in green innovation, the firm mitigates the effect of w on the cost of capital by improving the greenness of its good. As a result, the decline in profitability resulting from production will be less significant if the firm chooses to invest in green innovation. In other words, Arrow's replacement effect[2] becomes weaker as bank divestment takes place.

The second component of $\Delta\pi^*$ captures the net gain from increased greenness, which decreases as w increases because the product demand goes down.[3]

The third component, $-(D_I^*\frac{(1+\gamma r_I)(c_1+c_2)}{2} - D_0^*\frac{(1+\gamma r)c_1}{2})$, captures the change in the financial cost the firm needs to pay to the bank. The effect of divestment on this term can be either positive or negative, but the magnitude of the impact will generally be small since it has opposing effects on the change in product demand and the change in per unit cost of the good. Overall, the model

[1] I assume that $\frac{c_1}{s} < \frac{c_2}{G}$. Specifically, this condition means that if green innovation can double the greenness of the product, the unit cost of innovation needs to be higher than the marginal cost of production. Under this condition, investment in innovation leads to a lower pre-divestment equilibrium demand for the good.

[2] As argued in Arrow et al. (1962), a firm earning more profit has a stronger interest in protecting the status quo and becomes less likely to invest in disruptive new technology.

[3] Since the product demand goes down when investing in innovation, under my assumption, investing in innovation will also make the firm strictly worse-off if $G - (1+\gamma r_I)c_2 < 0$. Therefore, I focus on the non-trivial case of $G - (1+\gamma r_I)c_2 > 0$.

highlights that the net effect of divestment on the firm's incentive to invest in green innovation can be either positive or negative, depending on whether Arrow's replacement effect dominates the shrinking benefit of green innovation and the change in the financial cost.

A.2 Additional Analyses

In this section, I perform three additional analyses robustness checks. First, to gain a more detailed understanding of the effects of bank divestment on patent filings, I estimate these effects separately at the intensive and extensive margins. For the intensive margin, I restrict the sample to firm-year observations where the number of green patent filings is positive. For the extensive margin, I estimate the linear version of equation (1.4) with the dependent variable being a binary variable equal to one when the firm files at least one green patent in a given year and zero otherwise. The estimates for both margins are presented in Table A.1. They suggest that the effects of commitments on green innovation occur at the intensive margin. That is, high-carbon-emission firms that were actively producing green patents experienced a reduction in green patent filings after their relationship banks committed to carbon neutrality through SBTi. For the extensive margin, the coefficients of interest are also negative in columns 3 and 4. The coefficients suggest that some high-emission firms, which had previously filed green patents, might cease green patent filings altogether following their relationship banks' divestment, or some firms with low carbon emissions suddenly begin investing in green innovation after receiving more credit from their relationship banks. However, given that the estimated coefficients are insignificant at conventional levels, I can not draw a conclusion regarding the effect of divestment on green patent filings at the extensive margin.

Table A.1: Effects of Bank Commitments on Borrowing Firms' Patent Filings at the Extensive and Intensive Margins

Dep. Var.	Green Patent Filings		$\mathbb{1}[GreenPatent > 0]$	
	(1)	(2)	(3)	(4)
Committed	0.0618 (0.0750)		0.0304 (0.0895)	
Committed x log(Emission)	-0.105** (0.0525)		-0.0509 (0.0562)	
Exposure		0.367 (0.578)		0.293 (0.519)
Exposure x log(Emission)		-1.214*** (0.416)		-0.374 (0.297)
Post x log(Emission)	0.0196 (0.0285)	0.0276 (0.0286)	0.0258 (0.0530)	0.0189 (0.0451)
Industry Trend	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	2959	2959	5112	5112

The table reports the effects of bank commitments on the green patent filings at the intensive and extensive margins. The dependent variable in column (1) is the number of green patents filed. The sample for column (1) includes all the brown firm-year observations where at least one green patent is filed. The dependent variable in column (2) is a dummy variable that equals 1 if a firm files at least one green patent in a given year and 0 otherwise. Standard errors are clustered at the firm level. *, **, and *** denote 10%, 5%, and 1% statistical significance, respectively.

Second, I re-estimate the staggered triple-difference Poisson model given in equation (1.4) using the definition of green patents developed in Bolton et al. (2022). Table A.2 presents the estimates and highlights that the estimated effects of commitments across levels of carbon emissions are similar to those in Table A.1, where green patents were classified following the OECD guidelines.

Table A.2: Effects of Bank Commitments on Borrowing Firms' Patent Filings Using an Alternative Definition of Green Patents

Dep. Var.	Green Patent	Non-Green Patent	Green Patent	Non-Green Patent
	(1)	(2)	(3)	(4)
Committed	0.150	0.0339		
	(0.117)	(0.100)		
Committed x log(Emission)	-0.116*	-0.0520		
	(0.0661)	(0.0610)		
Exposure			0.619	0.0132
			(0.729)	(0.668)
Exposure x log(Emission)			-0.733*	-0.130
			(0.455)	(0.578)
Post x log(Emission)	0.0517	0.0504	0.0442	0.0399
	(0.0536)	(0.0568)	(0.0544)	(0.0504)
Industry Trend	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	5,775	7,648	5,775	7,648

The table above reports the effect of bank commitment on the number of patent filings using the definition of green patents developed in Bolton et al. (2022). Standard errors are two-way clustered at the firm and year level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Finally, I examine the relevance of the concern that my findings might be driven by MNCs. That is, if high-emission MNCs receive similar pressure as banks headquartered in those countries, which might induce the banks to commit to SBTi, the decrease in their green patent filings could be due to governmental or social pressures rather than the relationship bank divestment. To address this concern, I estimate a tripe-difference Poisson regression similar to Equation 1.4:

$$\begin{aligned} PatentFiling_{it} = exp[\beta_1 Committed_{it} + \beta_2 Committed_{it} \times \mathbb{1}[\text{High-Emission Ind.}]_i \\ + \beta_3 Post_t * \times \mathbb{1}[\text{High-Emission Ind.}]_i + Controls_{it} + \epsilon_{it}] \end{aligned} \quad \text{(A.1)}$$

where $\mathbb{1}[\text{High-Emission Ind.}]_i$ denotes a binary variable that equals one if firm i is in one of the ten industries with the highest scope-1 carbon emissions.[4] Table A.3 shows that the estimated

[4] I replace carbon emission with the indicator variable of high-emission industry in the specification because very

effects remain similar, thereby alleviating the concern of selection bias.

Table A.3: Non-MNCs Sample: Effect on Patent Filings

Dep. Var.	Green Patent	Non-Green Patent	Green Patent	Non-Green Patent
	(1)	(2)	(3)	(4)
Committed	0.144	0.0785		
	(0.160)	(0.125)		
Committed x 𝟙[High-Emission Ind.]	-0.528***	0.616		
	(0.183)	(0.513)		
Exposure			1.450	-0.369
			(1.537)	(0.741)
Exposure x 𝟙[High-Emission Ind.]			-3.029**	-0.332
			(1.535)	(0.912)
Post x 𝟙[High-Emission Ind.]	0.212	-0.252*	0.367**	-0.366***
	(0.142)	(0.149)	(0.144)	(0.140)
Industry Trend	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	4711	9069	3115	10084

The table above reports the effect of bank commitment on the number of patents filed across levels of scope-1 emissions on the non-MNCs sample. Standard errors are two-way clustered at the firm and year level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

few non-MNCs firms report carbon emissions prior to 2015.

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