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DOES ECONOMIC INSECURITY AFFECT EMPLOYEE INNOVATION?

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ABSTRACT

Do household wealth shocks affect employee productivity? We examine this question through the lens of technological innovation, by comparing employees that worked at the same firm and lived in the same metropolitan area, but experienced different housing wealth declines during the 2008 crisis. Following a housing wealth shock, employees are less likely to successfully pursue innovative projects, particularly ones that are high impact, complex, or exploratory in nature. Consistent with employee concerns about financial distress, the effects are more pronounced among those who had little equity in their house before the crisis and among those with fewer outside labor market opportunities. Moreover, run-ups in housing prices before the crisis did not affect employee innovation. The results highlight a "bottom-up" view of innovation, in which individual employees influence the quantity and nature of innovation produced within firms.

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1 Introduction

How does employee productivity respond to large shocks to household wealth? Over the past several decades, the annual proportion of households in the U.S. experiencing a severe economic loss has been steadily increasing, peaking with the recent financial crisis (Hacker et al., 2014). The impact of household wealth shocks on consumption, savings, and retirement decisions has been carefully studied in the literature (e.g., Poterba et al., 1995; Dynan et al., 2004; Case et al., 2005; Campbell and Cocco, 2007; Goda et al., 2011; McFall, 2011; Mian et al., 2013). However, the effect of such shocks on employee behavior has remained largely unstudied. In this paper, we attempt to fill this gap, investigating whether economic insecurity affects employee productivity through the lens of technological innovation, a critical driver of economic growth (Solow, 1957).

Economists have long argued that the rate and direction of technological change should be understood as the outcome of firms' profit-driven investments in innovation (Schmookler, 1962; Griliches, 1957; Nelson, 1959; Arrow, 1962). Indeed, the literature on the determinants of innovation has focused almost entirely on market-level and firm-level factors such as competition, investment horizon, institutional ownership, and organizational structure.[1] This view, however, abstracts away from the individuals who actually produce innovative output within firms. Taking these individuals into account raises the possibility that the personal financial situations of employees might impact the innovative output they produce for their firms.

To study how economic insecurity impacts employee innovation, we focus on the 2008 financial crisis, which was a particularly severe and widespread shock that led to increased economic insecurity for many individuals. Specifically, we examine whether employees who experienced major declines in the value of their house during the crisis produced less innovative output for their firm as a result.

The effect that housing wealth shocks may have on employee output, and innovative output in particular, is theoretically ambiguous. First, it may be that employees are well-monitored and have little ability to change the amount of effort they exert. They may also have little control

[1] A few recent examples include Aghion et al. (2005, 2013); Bernstein (2015); Budish et al. (2015); Lerner et al. (2011); Seru (2014).

over which projects they work on, as projects may be determined by higher-level managers. Under these conditions, one might expect housing wealth shocks to have no effect on either the quantity or nature of their innovative output.

Absent these conditions, however, it is possible that negative housing wealth shocks, and associated financial distress, could impact employee productivity. On the one hand, such negative shocks may cause distraction or cognitive impairment, induced through, for example, stress and anxiety, making employees less productive (Deaton, 2012; Currie and Tekin, 2015).[2] On the other hand, these shocks may lead employees to become more productive so as to increase their job security and thereby ensure their ability to maintain mortgage payments (Kline et al., 2017).

Given these considerations, how housing wealth shocks affect employee productivity is ultimately an empirical question. The first challenge in answering this question is obtaining individual-level data on employee productivity. We address this by focusing on innovative output, as measured by patents. Patents credit individual employees as inventors, even when the patent is assigned to a firm. Therefore, we are able to observe innovative output at the individual level. We can not only observe the quantity and quality of an employee's innovative output, but can also characterize the nature of this output in a very detailed manner. Moreover, we complement the patent data with data from LinkedIn, which includes employee characteristics such as age, education, experience, tenure, and job title. Finally, we also link the patent data with deed records from CoreLogic, from which we observe detailed employee housing information such as location, date of purchase, square-footage, and number of bedrooms. These data allow us to link employee innovation to localized housing price shocks.

The second challenge is identifying the causal impact of housing shocks on employee innovation. Clearly, the location of an employee's house is not randomly assigned. For example, it may be that those who live in harder hit areas tend to work at firms that are themselves more affected by the crisis. In particular, firms in crisis-affected areas may experience a decline in local demand, or a

[2]If pursuing more innovative projects is riskier for employees due to a higher probability of failure, concerns about mortgage default may also lead employees to strategically pursue safer, and less innovative projects to enhance job security, ensuring the ability to maintain mortgage payments.

tightening of financial constraints stemming from the decline in the value of their real estate collateral (Chaney et al., 2012). It is also possible that firms located in crisis-affected areas simply tend to be ones that had worse innovative opportunities during this time period for reasons unrelated to the decline in local house prices. To address these issues, our analysis compares only employees working at the *same firm*—who are therefore similarly affected by firm-level changes in demand, borrowing capacity, or innovative opportunities—but who are exposed to different house price shocks.

Additional concerns, however, may arise within firms. Firms can have multiple divisions that are scattered geographically, and which may specialize in different technologies. Thus, it is possible that, even within the same firm, those who live in more crisis-affected areas may work in divisions with bigger changes in innovative opportunities. To address this concern, we further restrict our analysis to compare only employees who work at the same firm and also live in the same metropolitan area, as defined by a census Core Based Statistical Area (CBSA). For most firms, this implies that we are comparing employees working at the same local establishment.[3] Despite the fact that we compare employees living in the same metropolitan area, there remains substantial variation in the house price shocks that they experience, because we exploit house price shocks at the zip code level.

Using this empirical approach, we find that negative shocks to housing wealth during the crisis significantly affect employee innovation. We find that employees who experience a negative housing wealth shock produce fewer patents and patents of lower quality based on citations. Such employees are also less likely to patent in technologies that are new to their firm, and, more generally, their patents are less likely to draw upon information from outside their firm's existing knowledge base. Finally, these employees also produce narrower innovations, combining information from fewer disparate fields. These effects are strongest among those employees who suffer the largest housing price declines.

Overall, the evidence suggests that, following a housing wealth shock, employees are less likely to successfully pursue innovative projects, particularly ones that are high impact, complex, or

[3]This also implies that these employees reside within the same labor market, and thus likely face similar outside opportunities.

exploratory. The results are inconsistent with the null hypothesis under which employees cannot adjust the nature of their innovative projects, and also inconsistent with the hypothesis that housing wealth shocks lead employees to become more innovative.

We conduct additional tests to verify the robustness of the results, exploring even narrower comparison groups. For example, we compare the response of employees who, in addition to working at the same firm and living in the same CBSA, are also similar to one another in terms of age, educational attainment, technological specialty, or neighborhood/house type.[4] In all these cases, our key results remain remarkably stable. Therefore, the baseline results are unlikely to be explained by sorting of certain types of employees within a firm into more crisis-affected zip codes of a given metropolitan area. As further evidence against a sorting explanation, we also find that, even among employees living in the *same zip code*, productivity declines more for the ones we would expect based on the financial distress mechanism—for example, those with less accumulated home equity at the start of the crisis.

As far as we are aware, this paper is the first to link household balance sheets and employee productivity. Understanding this link is critical to understanding the full scope of the effects of wealth shocks on households. In addition, the finding that household wealth shocks can affect employee innovation has interesting implications for our understanding of how innovation is generated within firms.

A large literature on the determinants of firm innovation, originated by Schmookler (1962), Griliches (1957), Nelson (1959), and Arrow (1962), highlights a "top-down" view, in which firms' profit-driven objectives determine innovation policy, which is then implemented by employees. The literature, thus, has focused on firm-level and market-level factors to explain variation in innovation levels across firms (see, e.g., Harhoff, 1999; Aghion et al., 2005; Lerner et al., 2011; Manso, 2011; Aghion et al., 2013; Ferreira et al., 2014; Seru, 2014; and Bernstein, 2015). In contrast, our findings also highlight the possibility that innovation within firms may follow a "bottom-up" process, wherein

[4] To be more specific, we compare employees who choose to live in neighborhoods with similar characteristics such as income, ratio of urban and rural population, and family orientation, measured by average number of kids per family. We also compare employees that choose to live in a similar house, based on the square footage of the house.

innovative workers are not merely interchangeable parts, but play an important role in producing the innovative output of firms. This is likely to be particularly true since innovative workers are difficult to replace due to a high degree of firm-specific and project-specific knowledge (Hall et al., 1986; Lach and Schankerman, 1989; Hall and Lerner, 2010).[5]

We conclude by discussing the nature of the mechanism underlying our results. One possibility is that our results arise from pure wealth effects, which are unrelated to debt or financial distress per se. For example, following a decline in wealth, employees may become more risk-averse due to the shape of their utility functions. If pursuing innovative projects is risky for employees, since innovative projects are more likely to fail, those who have experienced a major wealth loss may reallocate their effort toward safer, less innovative projects. In this case, we would observe a decline in innovative output, consistent with our baseline results.

Under the pure wealth channel describe above, however, one might also expect wealth increases to lead to a corresponding decline in risk-aversion, and thus an increase in innovative output. To test whether this is the case, we repeat our analysis during the boom period leading up to the crisis, between 2002 and 2007. We find no statistically significant relation between house price changes and innovation during the boom. This suggests an asymmetry between the effects of housing wealth increases and decreases. Such an asymmetry is inconsistent with a pure wealth channel in which risk aversion increases with wealth losses and decreases with wealth gains.

Of course, such an argument relies on particular assumptions regarding the nature of employees' utility functions. For example, it is possible that, even in the absence of debt, significant wealth losses by themselves could induce stress and anxiety, leading to cognitive impairment, while wealth gains may do little to improve one's cognitive performance. We therefore examine directly the extent to which our results depend on the net equity position of employees. Inconsistent with the wealth effects hypothesis, we find that, in fact, employees with less accumulated home equity prior to the

[5]Consistent with the idea that innovative employees are hard to replace, past work has shown that innovative firms tend to smooth their R&D spending over time, in order to avoid having to lay them off (Hall et al., 1986; Lach and Schankerman, 1989; Hall and Lerner, 2010). Moreover, when employees of a given firm face correlated shocks, as in our setting, these issues are likely to be exacerbated, as it will be even more difficult to replace them all at once or to redistribute their work to less affected employees.

crisis had a substantially larger decline in their innovative output following the onset of the crisis. These findings are strongly suggestive that our baseline results are driven by employee concerns regarding housing-related financial distress, as those with low home equity prior to the crisis are at greater risk of becoming underwater on their mortgage and being forced into default (e.g., Foote et al., 2008, 2010).

Mortgage default is well known to involve a variety of substantial costs to households.[6] Moreover, simply being underwater carries significant financial distress costs, such as the inability to relocate in the event of job loss. Currie and Tekin (2015) and Deaton (2012) have argued that the large costs imposed by the financial crisis on households led to significant increases in stress and anxiety, which are known to negatively impact cognitive performance. Finally, the literature on housing-related financial distress emphasizes that the costs of being underwater are greatly exacerbated by unemployment or a lack of job security. This insight is often referred to in the literature as the "double trigger" model of housing-related financial distress. Consistent with these double trigger considerations, we also find that our results are significantly stronger among employees with fewer outside labor market opportunities.

This paper relates to several strands of the literature. Beyond the innovation literature, mentioned above, this paper also relates to a recent literature that examines the impact of local house price movements on firm investment. Chaney et al. (2012) show that negative real estate shocks decrease collateral value and reduce the investment of public firms. Adelino et al. (2015) show that the collateral channel is particularly important for small businesses. Our channel is different. We control for the collateral channel at the firm level with firm fixed effects and instead illustrate that house price movements also affect employee incentives and innovative output within firms.

This paper also relates to a strand of the literature that explores the relationship between household leverage and labor supply (as in Bernstein, 2017; Mulligan 2008, 2010, 2009; Herkenhoff

[6] These include both direct financial costs from legal and moving fees as well as non-financial costs due to utility loss from relocating to a less desirable house, neighborhood, or school district. Mortgage default also may carry indirect costs stemming from reputational damage with potential creditors, employers, landlords, insurance companies, and others that screen based on credit reports. In addition, default may carry significant psychological costs due to associated stress and anxiety.

and Ohanian, 2011; and Donaldson et al., 2015). In that literature, the focus is largely on debt overhang and the decision of whether to work or not. Charles et al. (2015) studies the impact of the housing boom and bust on college enrollment and attainment. Conversely, our focus is on individuals who are already employed and the impact of household leverage on employee productivity within firms. Finally, this paper is related to a literature, both theoretical and empirical, which examines the provision of insurance to workers and how such insurance impacts incentives (see, e.g., Holmstrom and Milgrom, 1991; Guiso et al., 2005; Blundell et al., 2008; Kaplan, 2012; Lazear and Oyer, 2012; Berk and Walden, 2013).

The rest of the paper proceeds as follows. Section 2 describes the data. Section 3 details our empirical strategy. Section 4 presents our results. Section 5 investigates heterogeneity and Section 6 discusses potential channels. Section 7 concludes.

2 Data

2.1 Data Sources and Sample Selection

We obtain data on all US patents granted from 1976 through 2015 directly from the United States Patent and Trademark Office (USPTO). The USPTO data provide information on the date a patent was applied for and ultimately granted, the individual(s) credited as the patent's inventor(s), the firm to which the patent was originally assigned, and other patents cited as prior work. One challenge the data presents is that it lacks consistent identifiers for patent inventors and firms. In order to identify inventors and firms over time, we rely on two large-scale disambiguation efforts. The first is an inventor disambiguation provided by Balsmeier et al. (2015). Their algorithm combines inventor names, locations, co-authors, associated firms, and patent classifications to create an inventor identifier. While Balsmeier et al. (2015) also provide a firm identifier, they state that it is much less accurate and mainly created as a crude input for the inventor disambiguation. Therefore, for firm disambiguation, we instead rely on the NBER patent data project. The NBER firm identifier is based on a word frequency algorithm that ranks matches more highly if they share unusual words.

Because the NBER data end in 2006, we extend it forward based on code that they provide.[7]

The USPTO patent data contain the city and state of residence for patent inventors. Inventors also provide the USPTO with their full residential address on a signed oath as well as a patent application data sheet (ADS). Images of at least one of these forms are generally available starting in 2001 via the USPTO's Patent Application Information Retrieval (PAIR) portal. We download all of the relevant image files and apply optical character recognition (OCR) to make the text machine readable. Addresses are too irregular to extract consistently, however we are able to parse out zip codes coinciding the inventor's city of residence. To identify property owned by a patent inventor, we combine the patent data with CoreLogic. CoreLogic tracks housing transactions in the United States based on deed records as well as other sources. This makes it possible to construct the full ownership history of a given house. We match inventors to houses based on first name, last name, middle initial, city, zip code, and patent application date. This procedure yields a 52% unique match rate. The unmatched inventors either did not own a house, purchased a house before CoreLogic's coverage of their county, or were unmatchable due to name spelling irregularities (e.g., nicknames) on their patent application and/or deed. For matched inventors, we can observe detailed house characteristics.

Having matched inventors to houses, we next add in data on house price movements. Most house price indices aggregate at the city level due to the large volume of transactions needed to construct a constant-quality index. This allows for high-frequency measurement, but at the cost of smoothing the considerable variation that is present within a city. We are interested in comparing individuals who work at the same establishment of a firm, but who own houses in different local areas. Therefore, we use a zip code level price index constructed by Bogin et al. (2016), which overcomes the volume issue by reducing to an annual frequency. The index is based on the repeat-sales methodology and thus measures house price movements unrelated to changes in house quality. For robustness, we also use a similar index constructed by Zillow, which makes use of their proprietary house price

[7]https://sites.google.com/site/patentdataproject/

estimates for non-traded houses.[8]

Together, we construct an annual employee-level panel. In each year we observe an employee's innovative output along with the location of the employee's house and a price index associated with that location. It should be noted that one shortcoming of the data is that we are unable to observe certain employee characteristics during years in which the employee has zero patents. For example, if an employee changes firms we can only observe the change the next time the employee patents. In order to ensure that we are studying employees who are still active and that our information about them is not too stale, we limit our sample to individuals who, during the three years preceding the 2008 financial crisis, applied for at least one patent that was assigned to a firm. There are 321,837 such individuals in the USPTO data. Of these, we are able to identify a house in CoreLogic for 166,421 (52%). After requiring that other key variables are non-missing (e.g. zip code, house price index, etc.), we are left with a final sample of 162,011 employees, working at 23,075 firms.

Neither the USPTO data nor the CoreLogic data give us detailed demographic characteristics for the employees in our sample. Therefore, we augment these data with information from LinkedIn. Among other things, LinkedIn provides information on educational background, work history, and job titles, even in non-patenting years. In order to match an employee in our sample with the employee's LinkedIn profile, we first find a set of potential profile URLs by using Google to search LinkedIn for profiles containing the employee name's in conjunction with variations of the names of each firm the employee's patents have been assigned to, keeping only top ranked-results. We then visit those LinkedIn profile URLs and determine based on further information whether the profile appears to be a match.[9] Using this procedure, we are able to find a LinkedIn profile for 72,681 (45%) of the employees in our sample.

[8] http://www.zillow.com/research/data/

[9] A profile is considered a match if it was a top-ranked Google result and contains the name of the inventor and one of the inventor's firms. When an inventor name matches to multiple profiles based on different firms he worked at, firm names are prioritized as follows: (1) Multiple non-dictionary words, (2) a single non-dictionary word, (3) acronyms (e.g. IBM), (4) a single dictionary word. We only use data from public profiles, which we view as a non-logged-in user.

2.2 Key Variables

We use patent-based measures of an individual's innovative output that have been widely adopted over the past two decades (Jaffe and Trajtenberg, 2002; Lanjouw et al., 1998).[10] Our primary measure of the quantity of an individual's innovative output is the number of granted patents the individual applied for in a given period of time. Our primary measure of the quality of an inventor's innovative output is the number of citations the inventor's patents receive on a per patent basis. Patent citations are important in patent filings since they serve as "property markers" delineating the scope of the granted claims. Hall et al. (2005) illustrate that citations are a good measure of innovation quality and economic importance. Specifically, they find that an extra citation per patent boosts a firm's market value by 3%. Moreover, Kogan et al. (2012) show that the stock market reaction to patent approvals is a strong predictor of the number of future citations a patent receives. One challenge in measuring patent citations is that patents granted at the end of the sample period have less time to garner citations than those granted at the beginning. In addition, citation rates vary considerably over time and across technologies. To address both of these issues, we normalize each patent's citation count by the average citation count for all other patents granted in the same year and 3-digit technology class. We also construct a simple indicator variable equal to one if a patent was in the top 10% of patents from the same year and technology class in terms of citations received.

We further characterize the nature of an employee's innovative output by computing patent "Originality" and "Generality" scores. We define these variables following Trajtenberg et al. (1997). In particular:

$$Generality_i = 1 - \sum_{j}^{n_i} s_{ij}^2,$$

where s_{ij} denotes the percentage of citations received by patent i that belong to patent class j, out of n_i patent classes. Note, the sum is the Herfindahl concentration index. Thus, if a patent is cited by subsequent patents that belong to a wide range of fields, the measure will be high, whereas if

[10] Recent examples include Lerner et al. (2011); Aghion et al. (2013); Seru (2014).

most citations are concentrated in a few fields the measure will be low. A high generality score thus suggests that the patent had a widespread impact in that it influenced subsequent innovations in a variety of fields. "Originality" is defined the same way, except that it refers to citations made. Thus, if a patent cites previous patents that belong to a narrow set of technologies the originality score will be low, whereas citing patents in a wide range of fields would lead to a high score. These measures tend to be positively correlated with the number of citations made or received.[11] As before, we also normalize each patent's generality or originality by the mean generality or originality for all other patents granted in the same year and 3-digit technology class.

In addition, we attempt to measure the extent to which innovative output represents exploration versus exploitation from the perspective of the firm. Exploratory innovation requires new knowledge, whereas exploitative innovation builds upon a firm's existing knowledge (Manso, 2011). To operationalize this concept more directly, we follow Brav et al. (2016), and define a patent as "exploratory" if less than 20% of the patents it cites are not existing knowledge from the point of view of the inventor's firm. Existing knowledge is defined as all patents the firm was granted in the past five years as well as all patents the firm cited in the same time frame. We also follow Chen Lin et al. (2016) and define a simple "New class" indicator variable equal to one if a patent is in a technology class the inventor's firm has never patented in before.[12]

In general, projects that result in patents that are highly cited, original/general, or exploratory are likely harder for employees to execute. Therefore, one could think of all of the measures above as measures of project difficulty.

2.3 Summary Statistics

Panel A of Table 1 shows summary statistics for the various patent measures described in Section 2.2. Observations are at the employee level, and the patent measures for a given employee are based

[11]When there are more citations, there is a mechanical tendency to cover more patent classes. To correct for this tendency we apply a bias adjustment suggested by Hall et al. (2001).

[12]A large number of papers in the management literature define exploratory innovation in a similar fashion. In particular, risky, exploratory innovation is research which moves outside of a firm's knowledge base. Key papers in this literature include Jansen et al. (2006); Phelps (2010); Alexiev et al. (2010); Karamanos (2012).

on the employee's output during the five years following the onset of the crisis (2008–2012). A patent is associated with a year based on its application date, not the date it was ultimately granted. Panel B shows the correlation between the different measures of inventor productivity. In almost all cases the correlations between the different measures are fairly low, and this is not surprising given the different approaches taken to construct them. There are a few exceptions however. For example, as expected, a top patent is also a highly cited patent, and a top patent is also likely to be a very general one as well, that is, cited by a broad set of technologies. This confirms the intuition that highly cited patents are also broad patents, as measured by generality and originality, and also likely to be defined as exploratory patents, as discussed above.

Panel C shows summary statistics for characteristics of employees in our sample as of 2007. As one might expect, we find that patent inventors are highly educated with 97% holding a bachelor's degree, 30% holding a master's degree, and 28% holding a PhD. The average employee in our sample is approximately 41 years old, with 16 years of work experience, 6 years of which was at their pre-crisis (2007) firm. Approximately 48% held a senior position prior to the crisis.[13]

Panel D shows summary statistics for characteristics of the houses owned by employees in our sample as of 2007. The average house in our sample is nearly 30 years old, three thousand square feet in size, and had been purchased just less than 8 years ago. In terms of price movements, the average house was in a zip code where prices went up 22% from the end of 2004 to the end of 2007, and down 16% from the end of 2007 to the end of 2012. Panel E shows the distribution of employees across the top 20 most populated fields in our sample. Employees are assigned to a field using the modal NBER technology subcategory for the patents they applied for from 2005 through 2007. The most common field is computer hardware and software, representing 11.8% of the employees in our sample. Communications is in the second most common category with 10.21% of the employees. Other common fields include drugs, chemicals, and semi-conductor devices.

[13]We define an employee to have a senior position if the employee's title contains any of the following words: CEO, chair, chairman, chief, CTO, director, executive, head, management, manager, partner, president, principal, VP.

3 Empirical Strategy

Our primary interest is in how changes in house prices associated with the 2008 financial crisis affect the innovative output of employees. Because the 2008 crisis is a one-time event that affects all individuals in our sample simultaneously, we rely on cross-sectional variation in which we compare innovative output across employees living in zip codes that experienced differential house price shocks. To fix ideas, we begin by considering the following estimating equation:

$$y_{i,post} = \beta \triangle \% HP_{z,post} + \delta y_{i,pre} + \epsilon, \tag{1}$$

where i indexes individuals, and z indexes zip codes. The pre-period is defined as 2005–2007 and the post period is defined as 2008–2012. The variable $y_{i,post}$ represents the various patent-based measures of innovative output discussed in Section 2.2, including the total number of patents produced by individual i, the number of citations per patent, etc. The variable $\triangle \% HP_{z,post}$ represents the percent change in the house price index during the post period for zip code z in which individual i owned a house as of 2007.

Equation 1 poses several potential concerns, as the location of an employee's house is not randomly assigned. For example, it may be that those who live in harder hit areas tend to work at firms that are more affected by the crisis. One might naturally expect that to be the case as firms in crisis-affected areas are likely to experience a decline in local demand. It should be noted, however, that the innovative firms we study generally serve a national or global market. Another reason local house prices could affect firm innovation is that a decline in local house prices may reduce borrowing capacity for firms that rely on real estate collateral (Chaney et al., 2012). Finally, it is also possible that firms located in crisis-affected areas simply tend to be ones that had worse innovative opportunities during this time period for reasons unrelated to the decline in local house prices. To address these various issues, we begin by including firm fixed effects in all of our estimations. With the inclusion of firm fixed effects, we are identifying off of individuals that worked at the same firm but lived in areas with differential house price declines during the crisis. Such individuals are arguably similarly affected by firm level changes in demand, borrowing capacity, or innovative opportunities.

However, it remains possible that firms have divisions in multiple regions. In this case, divisions of the same firm that are in harder hit regions may tend to be the ones that are affected by changes in local demand or changes in innovative opportunities. To address this issue, we refine our specification even further by including firm by core based statistical area (CBSA) fixed effects.[14] Assuming that the firms in our sample have only one establishment in the area surrounding a given city, these fixed effects will be equivalent to establishment fixed effects. Note that, with firm by CBSA fixed effects, we are identifying off of employees who worked at the same firm and owned a house in the same general area, but who experienced differential price declines in their respective zip codes.

This approach provides several advantages. First, the employees we compare are likely to be similar, as they operate in the same labor market, and are facing similar employment opportunities outside of their firm. These employees are also likely to be similar given that they chose to live in the same general area. Finally, since they likely work in the same establishment of the same firm, they will likely be subject to the same division-level innovation shocks. Following the discussion above, in our baseline analysis we estimate equations of the form:

$$y_{i,post} = \beta \triangle \% HP_{z,post} + \delta y_{i,pre} + \eta_{f,c} + \epsilon, \tag{2}$$

where the key change relative to Equation 1 above is the addition of $\eta_{f,c}$, which represents firm by CBSA fixed effects. Note that with firm by CBSA fixed effects, we will only have power to estimate the key coefficient, β, if there is sufficient variation in house price shocks experienced by employees in the same firm and CBSA. We estimate that roughly 50% of the zip code level price variation during the crisis occurred within CBSA. Figure 1 provides evidence that such variation is indeed

[14]CBSAs are comprised of Metropolitan Statistical Areas (MSA) and Micropolitan Statistical Areas (μSAs). Essentially they are counties surrounding urban clusters both large (>50,000) and small (10,000–50,000). Not every county in the United States is located within a CBSA, as CBSAs do not include rural areas situated far from a significant urban cluster. Most of the individuals in our sample do reside in a Metropolitan or Micropolitan Statistical Area, however for those who do not, we define their local area simply by county. Thus, for rural individuals, our CBSA fixed effects are effectively county fixed effects.

present in the data. Panel A shows the distribution of housing price dispersion across different metropolitan areas. Darker areas represent CBSAs with higher price dispersion. Moreover, Panel B shows that the inventors in our sample also tend to live in such metropolitan areas with high housing price dispersion.

Even under this specification, however, one may worry that firms may have multiple establishments within a metropolitan area, perhaps focusing on different technologies. While this is unlikely to be the case, we can provide a further refinement to our specification. In robustness tests, we show that all our results hold with firm by CBSA by technology class fixed effects. By including these fixed effects, we essentially compare the innovative output of two employees who work at the same firm, reside in the same CBSA, and patent in the same technologies, but who experience different house price shocks during the crisis. The technology classes are based on the USPTO classification scheme. This classification scheme is comprised of approximately 400 different categories, and is thus very detailed. For example, just within the "Data Processing" area, there are different classes that capture "Artificial Intelligence," "Vehicles and Navigation," "Generic Control Systems," and "Database and File Management."

Still, it remains possible that even within the same firm and CBSA, different types of employees sort into neighborhoods that are differentially exposed to the crisis. Such sorting could bias our results to the extent that those individuals selecting into neighborhoods that were hardest hit by the crisis, were also those who decreased (or increased) their innovative output during the crisis for reasons unrelated to their house price decline. To address these concerns, we run a battery of robustness tests controlling for additional fixed effects which address potential selection stories. These additional fixed effects reflect both employee characteristics as well as zip code level neighborhood characteristics. As an example, to address the concern that younger workers tend to systematically live in the city center, while older workers live in the suburbs, we include firm by CBSA by age cohort fixed effects. To address the concern that more productive, higher-wage earners sort into richer neighborhoods, we include firm by CBSA by zip code income fixed effects. Section 4.3 provides greater detail on these specifications and discusses a variety of other such robustness tests. Our results remain unchanged with the inclusion of these controls.

Finally, to further address the concern that our results are driven by sorting of different types of workers into different zip codes within a CBSA, we take advantage of the fact that, within a zip code, the effect of the same house price shock on innovative output is likely to be larger for some subgroups relative to others. For example, house price shocks may be more important for those who bought their house during the boom. These employees might be more concerned about losing their job when hit with a negative house price shock because they would have accumulated less home equity prior. Similarly, house price shocks may be more important for employees who face a thin outside labor market based on their field of expertise. These employees may be more concerned about losing their job when hit with a negative house price shock because finding a new job would be more difficult. Motivated by these observations, we estimate variants of Equation 2:

$$y_{i,post} = \beta \triangle \% HP_{z,post} \times Characteristic_i + \gamma Characteristic_i + \delta y_{i,pre} + \eta_f + \eta_z + \epsilon, \tag{3}$$

where $Characteristic$ is an employee level characteristic such as an indicator for whether the employee bought their house during the boom, or an indicator for whether the employee specialized in a technology that is not widely-used. This specification allows us to test for heterogeneity in the effect of house price shocks. An important additional benefit of this specification is that it also allows us to include zip code fixed effects, η_z, which controls for differences among employees who choose to live in different zip codes. While the main effect of $\triangle\% HP$ is subsumed by the zip code fixed effects, we can estimate the coefficient β on the interaction term. In this case, β represents the differential effect of house price shocks for those with $Characteristic = 1$ relative to those with $Characteristic = 0$. Essentially, we can control for unobservable differences among employees who choose to live in different zip codes because two employees who live in the same zip code should respond differently to the same house price shock.

4 Results

4.1 Main Findings

We begin in Table 2 by estimating variants of Equation 2. Standard errors are double clustered by firm and zip code. In columns 1–2, we first examine the effect of changes in local house prices on the number of patents an employee produces. We include the number of patents produced in the pre-crisis period as a control, to capture changes in productivity relative to the pre-crisis baseline. In addition, we also include firm by CBSA fixed effects, meaning that we identify off of variation from employees who work at the same firm and own a house in the same area, but live in different zip codes. Comparing such employees further helps to minimize selection concerns, as these individuals are likely to be similar. In column 1 we estimate a positive coefficient that is statistically significant at the 1% level. This indicates that a greater decline in local house prices where an employee lives is strongly associated with lower patenting output. In column 2 we also include as an additional control the change in house prices that an employee's zip code experienced leading up to the crisis. Our main coefficient of interest changes little when controlling for house price appreciation during the run up to the crisis, and in fact we find that pre-crisis price changes have no statistically significant relation to patenting during the post-crisis period. Therefore, our results do not seem to be driven by selection of certain types of employees into more "bubbly" areas within a CBSA. The differences we find only coincide with ex-post price movements, which were presumably hard to predict and thus to select on ex-ante. As will be shown in Section 4.3, we also find that our estimates remain similar after controlling for additional employee and house characteristics, which further cuts against a selection story.

In columns 3–4 of Table 2 we examine the effect of house price declines on patent quality as captured by citations per patent. We again estimate a positive coefficient on the change in local house prices in an employee's zip code, significant at the 1% level. Thus, not only do house price declines lead to a reduction in the quantity of patents produced, the quality of those patents also appears to be lower. Finally, in columns 5–6 we find very similar results when patent quality is instead measured simply as the number of patents produced that are in the top 10% in terms of citations relative to other patents granted in the same year and technology class.

To explore how the effects change with the intensity of the house price declines, we separate our house price change variable into ten decile indicator variables and repeat the analysis, letting the top decile (highest percentage change) be the omitted category. The results are presented in Figure 2. We see that the results are strongest in the hardest hit areas and that the effect monotonically declines, for the most part, as the size of the housing price decline decreases. Figure 2 also demonstrates that the effects are economically as well as statistically significant. For example, employees in the bottom quartile of house price changes are approximately 6% less productive in terms of patent output than employees in the top quartile, while employees in the bottom decile are approximately 9% less productive. Similarly, those in the bottom quartile or decile are also less productive in terms of citations, experiencing a 5% or 7% larger decline, respectively.

In Table 3 we begin to investigate the nature of innovations produced by employees living in areas differentially affected by the crisis, focusing first on generality and originality. As discussed in Section 2.2, a high generality score indicates that the patent influenced subsequent innovations in a variety of fields; a high originality score indicates that the patent made use of prior knowledge from a wide variety of fields. We find that employees in zip codes with larger price declines also produce less general and less original patents in the post-crisis period.

Finally, in Table 4 we further investigate whether the patents of employees that experience larger house price declines during the crisis become less exploratory in the sense that they rely more heavily on the existing knowledge of their firm. In columns 1–2 we find that larger house price declines are associated with a reduction in the tendency to patent in a technology class that is new to an employee's firm. As discussed in Section 2.2, we define a patent to be exploratory if less than 20% percent of the patent's citations are to other patents granted to their firm or cited by their firm in recent years. Consistent with the idea that employees pursue less exploration when they experience a negative shock to their outside wealth, we find in columns 3–4 that those living in harder hit zip codes produce fewer exploratory patents. Since all of the results are *within firm*, they cannot be driven simply by a change in firm policy away from exploration during the crisis for firms located in harder hit regions.

As illustrated in Panels (c) through (f) of Figure 2, the effect of housing prices on originality, generality, and exploration is again strongest in the hardest hit areas and is economically significant. Moreover, the effect monotonically declines, for the most part, as the size of the housing price decline decreases.

Overall, the results suggest that following a housing wealth shock, employees become less productive, particularly with respect to projects that are high impact, complex, or exploratory in nature. Moreover, employees who are most severely affected by the housing shock adjust their innovative projects most strongly. These findings are inconsistent with the null hypothesis that innovative output is unaffected by household wealth shocks, suggesting that firm- and market-level factors are not the only drivers of innovation. The findings are also inconsistent with the hypothesis that negative wealth shocks increase productivity.

4.2 Individuals Remaining at the Same Firm

One potential explanation is that the changes in innovative output that we document arise from periods of unemployment, or transitions to different firms. In fact, it might be the case that those who experience a negative house price shock move to less innovative firms. To explore whether our results are driven by individuals who separate from their pre-crisis employer, we repeat our baseline analysis among individuals who remain at the same firm. We identify employees as "stayers" if either (1) they are observed patenting at the same firm following the end our sample period, or (2) they were still employed at that firm following the end of our sample period according to their LinkedIn profile.

If our baseline results are driven by employees who separate from their pre-crisis employer and potentially sort into different types of new firms, we would expect to find no effect among stayers. However, in contrast to this view, we find that our main results hold for the employees that remained in the same firm in the post-crisis period as well. The results of this exercise are presented in Table

5. We find that stayers who experienced a decline in housing prices produce fewer patents as well as patents that are less cited, less original, less general, and less exploratory in nature. Thus, the changes in productivity occur for employees who remain at the same firm and are not due to transitions to unemployment or to less innovative firms.[15]

4.3 Selection Concerns

As discussed in Section 3, concerns about selection issues motivated our empirical design. Specifically, unobserved differences between firms, and across geographical areas lead us to include firm by CBSA fixed effects in the baseline specifications. With these fixed effects, we are effectively comparing employees who work at the same firm and reside in the same metropolitan area. In the next section, we explore more nuanced selection concerns that still may operate within a firm establishment. To address these concerns, we narrow the comparison group even further.

4.3.1 Technology

One potential concern is that we might be comparing employees that work at the same firm and live in the same CBSA, but who do not work in the same division of the firm. If those who live in more crisis-affected areas also tend to work in divisions experiencing greater declines in innovation for unrelated reasons, that would bias our estimates. To address this possibility, we include firm by CBSA by employee technology class fixed effects. The results are in Row 2 of Table 6, Panel A. We define an employee's technology class to be the modal 3-digit class of the employee's patents in the pre-crisis period (2005–2007). This specification is very conservative in that it only identifies off of variation from employees that work at the same firm, specialize in the same narrow technology class, and live in the same CBSA. Even under this very stringent specification, we estimate similar effects as before, which are presented in Row 1 for convenience.

[15] In a related test, we also find similar results when we condition on patenting at any firm after the onset of the crisis. That is, even among those who continued patenting, those who experienced a larger house price decline were less productive.

4.3.2 Neighborhood Characteristics

Next, we explore the impact of employee sorting into different types of neighborhoods within a CBSA (while working at the same firm). We begin by focusing on the average income level of the zip code in which an employee lives. This specification attempts to address the concern that employees with higher wages may sort into richer neighborhoods, and therefore may experience different housing price shocks while facing differential risks of job termination during the crisis. In Row 3 of Table 6, Panel A we sort employees into quartiles within each CBSA, based on the 2000 mean income level of the zip code in which they live. We then run our regressions with CBSA by firm by neighborhood income quartile fixed effects. These regressions compare two employees who work at the same firm, live in the same CBSA, and live in zip codes with similar mean income levels. The results are consistent with our baseline specification.

In Row 4 of Table 6, Panel A we sort employees into quartiles within a CBSA based on the number of children in their zip code, as reported by the 2000 census and run our regressions with firm by CBSA by zip code family size fixed effects. This specification is yet another check for the concern that employees with children, who likely sort into more family-oriented neighborhoods, were more concerned about job termination during the crisis while at the same time they may have experienced different housing price shocks. To further address this point, in Row 5 we sort employees into quartiles (within CBSA) based on the 2000 census measure of how urban their resident zip-code is and then include firm by CBSA by zip code urban measure quartile fixed effects. It seems likely that single employees put less of a premium on space and are thus more likely to live in the city center than employees with families. In both specifications, our estimates are very similar to the baseline results.

An additional attempt to explore potential selection issues related to employee wages and family is to include fixed effects based on the square-footage of the houses employees owned in 2007. It seems likely that employees with higher wages and those with children would, on average, live in larger houses. Therefore, in Row 5 of Table 6, Panel A we sort employees into quartiles based on the square-footage of their 2007 house (within CBSA), and run the regressions with CBSA by firm by square-footage quartile fixed effects. This specification compares two employees working at the same firm, living in the same CBSA, and living in houses of comparable size. Once again, the results are very similar to the baseline estimates.

4.3.3 Employee Characteristics

In this section we explore various employee characteristics that may correlate with employee sorting into different zip codes within a CBSA. One such selection story is that less experienced employees lived in zip codes which were disproportionately impacted by the housing crisis. It is plausible that less experienced employees may also have been more concerned about being terminated during the recession, which thus impacted their productivity. Alternatively, firms may have cut back on innovation during the recession and re-assigned the least experienced employees to less innovative projects. To address this possibility, for each employee we calculate experience as the number of years, as of 2007, since the employee's first patent and sort employees into experience quartiles. We then re-run our regressions with firm by CBSA by experience quartile fixed effects. This specification compares two employees of similar experience level, working at the same firm, and living in the same CBSA. We report the results in Row 7 of Table 6, Panel A. Our results are very similar to the baseline specification reported in row 1.

Similar to experience, it may be that younger employees, less educated employees, or employees in less senior positions were more worried about termination or were more likely to be re-assigned to less innovative roles within their firm. It is also plausible that younger employees tend to systematically live in different zip codes than older employees. For instance, younger employees may be more likely to live in the city center, while older workers tend to live in the suburbs. Similarly, employees in more senior positions likely have higher wages and may therefore tend to live in richer zip codes. Our patent data, however, does not provide information regarding age, education, or position. We therefore use the data from LinkedIn described earlier. This cuts our sample size approximately in half, but as Row 1 of Table 6, Panel B demonstrates, the results of our baseline specification using only the LinkedIn sample remain quite similar. Row 2 of Table 6, Panel B runs our regressions with firm by CBSA by age quartiles fixed effects. Row 3 of Table 6, Panel B runs them with firm by CBSA by education fixed effects. Row 4 of Table 6, Panel B shows the results with firm by CBSA by senior position fixed effects. In all specifications, the estimated effects are similar to the baseline results.

4.4 Robustness Tests

In this section, we perform a number of additional tests to further explore the robustness of the results.

Excluding 2008-2009 Patent Grants

One potential concern is that the patenting process takes time and our results may therefore reflect research initiated and/or completed prior to the start of the housing crisis. It should be noted that we base the timing of patents on their application date not their grant date. Thus, the time it takes to process a patent application should not affect our results. It is possible though that there is a lag between when a project is completed and when a patent associated with the project is applied for. However, it would not be in a firm's interest to delay applying for a patent associated with a completed project, as competitors may patent the same innovation during the period of delay. Another possibility is that some patents applied for after the onset of the crisis were associated with projects that were completed during that time period, but which were initiated earlier. However, to the extent that these projects were not completed prior to the onset of the crisis, they may still have been affected by wealth shocks to individuals leading the projects. Moreover, past work provides evidence that it generally takes less than a year for a project to result in a patent application (Hall et al., 1986).

Perhaps more importantly, even if there is measurement error in our measures of post-crisis innovative output, this would not explain our results, as such measurement error would likely be uncorrelated with house price movements of inventors within the same firm and CBSA. Nonetheless, we also re-run our main specification only including patents in the post-crisis period that were applied for after 2009. The results are reported in Appendix Table A.1. As can be seen, all of our results remain similar.

Shorter Time Horizons

We also verify that our baseline results are driven by the declines in housing prices during the crisis rather than by the subsequent recovery. We do this by changing the time horizon over which house prices changes are measured. In our baseline specification we define post-crisis house price movements based on changes from the end of 2007 to the end of 2012. In Appendix Table A.2 we instead define post-crisis house price movements based on changes from the end of 2007 to the end of 2010. In Appendix Table A.3 we only consider price movements through the end of 2008. In both cases we find similar results. Moreover, in Appendix Table A.4, we also find similar results when we examine the effect of price movements through the end of 2008 on patent applications after 2009.

Alternative Price Measures

Another possible concern is that our results may be sensitive to how we define zip code level house price changes. To address this, in addition to the house price index provided by Bogin et al. (2016) we also use a zip code level price index provided by Zillow. The results are reported in Appendix Table A.5. All of our results remain similar.

Firm Size

A final possibility is that our results are driven by inventors working at only a few large firms. To explore whether this is the case, in Appendix Table A.6, we split the sample based on firm size. In particular, we run our main specification for only inventors working at firms with less than 1000 inventors, at firms with less than 100 inventors, at firms with less than 50 inventors, at firms with less than 30 inventors, and at firms with less than 10 inventors. In all of these subsamples we continue to find similar results.

5 Heterogeneity

In this section, we explore whether the effects we have documented vary with how much home equity employees had accumulated prior to the onset of the crisis. We also explore whether the effects vary with employees' outside labor market opportunities. We have several motivations for this analysis. First, as discussed in Section 3, studying heterogeneity allows us to include zip code fixed effects, which provides yet another way to control for employee sorting within metropolitan areas and the resulting selection concerns. Second, studying heterogeneity allows us to document various ameliorating factors for the impact of housing price decreases on employee output and innovation in particular, which may be of interest to both market participants and policymakers. Finally, the heterogeneity analysis provides evidence on the channel through which our results operate, particularly given that both housing equity and employment opportunities are highlighted in the mortgage default literature as two prominent factors affecting household financial distress (e.g., Foote et al., 2008, 2010).

5.1 The Impact of Housing Equity

We begin by investigating whether the strength of our baseline results varies with the amount of housing equity the inventor entered the crisis with. We proxy for this by exploiting the timing of when employees bought their house. Employees who bought their house during the boom (just before the crisis) are more likely to have ended up with low or negative home equity after the crash since they had little time to accumulate equity and prices were likely to have been particularly inflated (while leverage was cheap). In contrast, those who bought earlier are more likely to have retained and accumulated significant equity.[16]

We estimate Equation 3 with zip code and firm fixed effects, this time interacting house price

[16] We do not observe home equity in the CoreLogic data. Moreover, even if we could observe home equity, it would partly reflect the down payment (and accelerated payment) decisions of home owners, which are endogenous. For example, loan-to-value at origination may be correlated with unobserved factors, such as risk aversion, which may also impact their innovative productivity. Therefore, we prefer to simply proxy for home equity with the timing of the purchase.

shocks with an indicator equal to one if the employee bought their house prior to 2004.[17] Table 7 shows that across almost all of our outcomes, we estimate a significant negative coefficient on the interaction term. This indicates that the effect of house price movements is indeed smaller for employees who bought their house earlier, and thus were likely to have accumulated more equity.

5.2 The Impact of Labor Market Outside Options

Next, we examine how our results depend on the outside labor market opportunities of inventors. To do this, we classify employees as specializing in widely-used technologies or narrowly-used technologies. Presumably, there is a thicker labor market for inventors specializing in widely-used technologies, making it easier for them to find another job if necessary. To test whether the effect of house prices varies with the popularity of an inventor's field of specialty, we classify technologies as popular or not based on the specialization of the inventor in the pre-crisis period. Specifically, we define an inventor's field of specialty based on the modal technology class of the inventor's patents in the three years leading up to the crisis. We classify a technology as popular if it is in the top quartile in terms of the total number of inventors in the population specializing in it over the same time period.[18]

We then estimate Equation 3, which interacts house price shocks with the popular technology indicator, which proxies for labor market thickness. As highlighted in Section 3, we are also able to include zip code fixed effects in this specification, which further helps to address selection concerns. Essentially, we can control for unobservable differences among inventors who choose to live in different zip codes by taking advantage of the fact that two inventors who live in the same zip code may respond differently to the same house price shock due to having different outside labor

[17]Note that this is a more demanding specification than the one used in previous results where we incorporate firm by CBSA fixed effects. We can control for zip code fixed effects in this specification because we estimate the interaction of housing price changes with home ownership duration. We cannot control for zip code fixed effects to estimate the direct effect of housing prices changes. The results in this section hold also when we simply control for firm by CBSA fixed effects. However, due to power limitations, we are not able to include firm by zip code fixed effects. We are able to add both firm fixed effects and zip code fixed effects.

[18]We find similar results if we classify a technology as popular based on the number of patents or number of firms working on this technology.

market opportunities.[19] Table 8 shows the results. Across almost all of our outcomes, we estimate a significant negative coefficient on the interaction term. This indicates that the effect of house price movements is indeed smaller for those employees, within the same firm, that are facing a greater set of outside labor market opportunities. We discuss the implications of both of these heterogeneity results in the following section.

6 Potential Channels

The primary contribution of this paper is to link household balance sheets and employee productivity. Understanding this link is critical to understanding the full scope of the effects of wealth shocks on households. Overall, the evidence suggests that following a housing wealth shock, employees are less likely to successfully pursue innovative projects, particularly ones that are high impact, complex, or exploratory in nature. These findings also highlight the possibility that innovation within firms may follow a "bottom-up" process, wherein innovative workers are not merely interchangeable parts, but play an important role in producing the innovative output of firms. This is likely to be particularly true when innovative workers are difficult to replace due to a high degree of firm-specific and project-specific knowledge (Hall et al., 1986; Lach and Schankerman, 1989; Hall and Lerner, 2010), and when shocks to these workers are correlated.

In this section, we discuss potential channels through which these effects may operate. In particular, we consider whether changes in employee behavior are driven by wealth losses, or concerns about financial distress that may arise in the case of mortgage default.

[19] As in the analysis of home ownership duration, it is worth noting that this specification is more demanding than the one used in the main results where we incorporate CBSA X Firm fixed effects. We can control for zip code fixed effects in this specification because we estimate the interaction of housing price changes with home ownership duration. We cannot control for zip code fixed effects to estimate the direct effect of housing price changes. The results in this section hold also when we simply control for firm by CBSA fixed effects. However, due to power limitations, we are not able to include firm by zip code fixed effects. We are able to add both firm fixed effects and zip code fixed effects.

6.1 Wealth Effects

One possibility is that our results arise from pure wealth effects, which are unrelated to debt or financial distress. For example, following a decline in wealth, employees may become more risk-averse if their utility function exhibits decreasing absolute risk aversion. If pursuing innovative projects is risky for employees, as innovative projects are more likely to fail, those who have experienced a major wealth loss may reallocate their effort toward safer, less innovative projects. In this case, we would observe a decline in innovative output, consistent with our baseline results.

To examine whether wealth effects drive our baseline results, we examine the impact of housing price increases on innovation during the housing boom which preceded the housing crisis. Under the wealth effects channel, one might expect our results to be symmetric during the housing boom. That is, employees who experienced an increase in housing wealth may have become more risk tolerant and thus more willing to pursue innovative projects.

To test this, we again estimate Equation 2, our baseline specification that includes firm by CBSA fixed effects. This time, we focus on a sample of employee homeowners that have at least a single patent in the years 1999-2001 and explore how subsequent house price increases, during the boom period of 2002-2007, affect employee innovation and risk taking. Table 9 shows that there is no effect of house price changes for any of our outcomes during the boom period, inconsistent with a symmetric wealth effect channel.

Of course it remains possible that our results are driven by an asymmetric wealth effect channel. For example, it is possible that, even in the absence of debt, significant wealth losses by themselves could induce stress and anxiety, leading to cognitive impairment, while wealth gains may do little to improve one's cognitive performance. However, as we discuss in Section 5.1, we also find that employees who bought their house earlier, before the boom, and who are therefore more likely to have had more equity at the onset of the crisis, were less sensitive to the shock, and experienced a smaller decline in innovative output. This is again inconsistent with the wealth effects channel since those who had more equity, if anything, experienced a larger wealth loss when house prices declined, than those who hit the zero equity bound. Therefore, based on the wealth effect channel, they should experience a larger decline in innovative output due to a greater increase in risk aversion, in contrast to our findings. Overall, the empirical findings do not appear consistent with the view that pure wealth effects can explain the decline in employees' innovative activity.

6.2 Effects of Financial Distress

An alternative possibility is that employees produce less innovative output following a decline in housing prices as a result of financial distress concerns. This could be true even if employees were risk neutral. There is substantial empirical evidence that mortgage default is costly for households. A family experiencing foreclosure likely has to incur significant moving costs due to the forced relocation. Children may be uprooted from their current school and could suffer educationally (Been et al., 2011). Credit scores are negatively and persistently impacted by a foreclosure, which can adversely affect future employment outcomes (Brevoort and Cooper, 2013). Finally, households may wish to avoid default to the extent that they view it as a significant personal failing or immoral, or to the extent that that there is a social stigma attached to defaulting on one's debt obligations. Using survey data, Guiso et al. (2009) find that, after relocation costs, the most important determinants of strategic default are moral and social considerations.

The literature on mortgage default emphasizes the "double-trigger" model as an important explanation for changes in default risk. Specifically, a borrower whose home equity becomes negative is likely to default if he or she also experiences a sufficiently severe income shock, typically in the form of unemployment (e.g., Foote et al., 2008, 2010). Once the borrower runs out of liquid financial resources, mortgage payments cannot be maintained and default is inevitable, as not even a quick sale can pay off the outstanding balance. Hence, this literature highlights the combination of negative equity and unemployment risks as potentially important factors that can increase mortgage default risk (Bhutta et al., 2017; Gerardi et al., 2013; Gyourko and Tracy, 2014; Niu and Ding, 2015).

In light of the potential costs of default, employees who have experienced a major house price decline may be distracted by stress and anxiety, for example, since they are more vulnerable to being forced into default (Currie and Tekin, 2015; Deaton, 2012). This distraction may reduce their ability to effectively produce innovative output. In addition, such employees may also actively try to increase their job security to mitigate the risk of a second default trigger, that is, unemployment. They may do this by pursuing safer projects. For example, employees may pursue projects that exploit existing knowledge and thus have a lower probability of failure, rather than pursue projects that explore new and uncertain technologies (Manso, 2011). In both cases, it is the prospect of costly default that leads to less innovation.

Of course, the inventors in our sample are likely not poor individuals. Most of the inventors in our sample are college educated and, in 2007, were working in technical fields offering a decent wage. Therefore, to the extent that the housing crisis was concentrated among the poorest households, the validity of the story outlined thus far would be weakened. However, empirical work (e.g., Mian and Sufi, 2016; Foote et al., 2016; Adelino et al., 2016) has shown that the expansion of credit in the run-up to the crisis was not solely confined to the poorest households. The mortgage debt of higher income households also significantly expanded between the years 2001–2007. As a result of this expansion in mortgage debt, higher income households also became underwater during the crisis and were subject to the associated delinquency risk. Indeed, while empirical work has shown that the level of foreclosures was highest among low income, subprime borrowers, the rate of foreclosures increased substantially for both low income and high income households during the crisis.[20]

Moreover, even if a household does not default, being underwater itself carries significant financial distress costs, especially in the event of lost income. First, underwater homeowners cannot take out a home equity loan to smooth consumption. Even more importantly, underwater homeowners may be unable to sell, since the proceeds would be insufficient to cover the mortgage balance, a phenomenon generally known as lock-in. This means that in the event of job loss, it could be difficult to re-locate to take on another job. Also, in the event that another source of income could not

[20]Foote et al. (2016) and Adelino et al. (2016) argue that, in percentage terms, the increase in the rate of foreclosures was actually highest among high income households.

be found, since the household would not have the option of selling, the household may have to cut back significantly on consumption or dip into savings to avoid a costly default. In the context of our setting, being underwater itself likely carries its own mental stress issues as a result of all the related costs, which could impair cognitive ability and negatively impact job performance. These concerns may also increase inventors' desire for job security, leading them to pursue safer, less innovative projects, which may lack the potential to result in a high impact patent (or a patent of any kind), but also may be less likely to result in a failure and consequent job loss.

Our empirical findings are consistent with the financial distress channel. As discussed in Section 5.1, we find that employees who are likely to have had more equity in their house at the onset of the crisis, who were therefore less likely to have had the negative equity trigger, experienced a smaller decline in innovative output. Moreover, as discussed in Section 5.2, we find that employees with more outside labor market opportunities, who therefore were less likely to have had the unemployment trigger, also experienced a smaller decline in innovative output. Together, these results are consistent with the double-trigger financial distress model, which emphasizes that the impacts of a housing crisis are particularly severe for individuals who experience both negative equity and job loss (e.g., Foote et al., 2008, 2010). Consistently, we find that employees with higher equity in their house, and greater labor market opportunities seem to experience a lower sensitivity to housing wealth shocks, arguably due to a lower default risk. This is also consistent with the lack of effect of housing prices on innovation during the run-up in the pre-crisis period. These results may be of particular interest to policymakers tasked with developing appropriate housing related macroprudential policy.

7 Conclusion

In this paper, we investigate whether household level shocks impact employee output in firms through the lens of technological innovation. The household level shocks that we focus on are changes in housing wealth experienced by employees during the financial crisis. Throughout the paper, we compare employees that worked at the same firm and lived in the same metropolitan area, but experienced different housing wealth declines during the crisis. Using this empirical strategy, we find that employees who experience a negative shock to housing wealth are less likely to successfully pursue innovative projects, particularly ones that are high impact, complex, or exploratory in nature.

These findings are most consistent with the hypothesis that negative housing wealth shocks lead to decreased innovative output due to heightened concerns among employees about the possibility of mortgage default. Consistent with this hypothesis we find that the effects are less pronounced among employees that are at a lower risk of facing mortgage default. That is, we find that housing wealth shocks particularly affect the productivity of employees with fewer outside labor market opportunities, and of employees who had little equity in their house before the crisis. These results may also be of interest to policymakers concerned with macroprudential policy related to the housing market, such as the appropriate level of loan-to-value requirements.

Finally, our results also shed light on the origins of innovation within firms. While much of the innovation literature emphasizes the importance of firm level factors along with the strategy set by top executives, the evidence presented here suggests that shocks to individual employees also has a significant impact on the types of innovative projects a firm successfully pursues, highlighting the role of lower ranked employees in influencing firm innovation.

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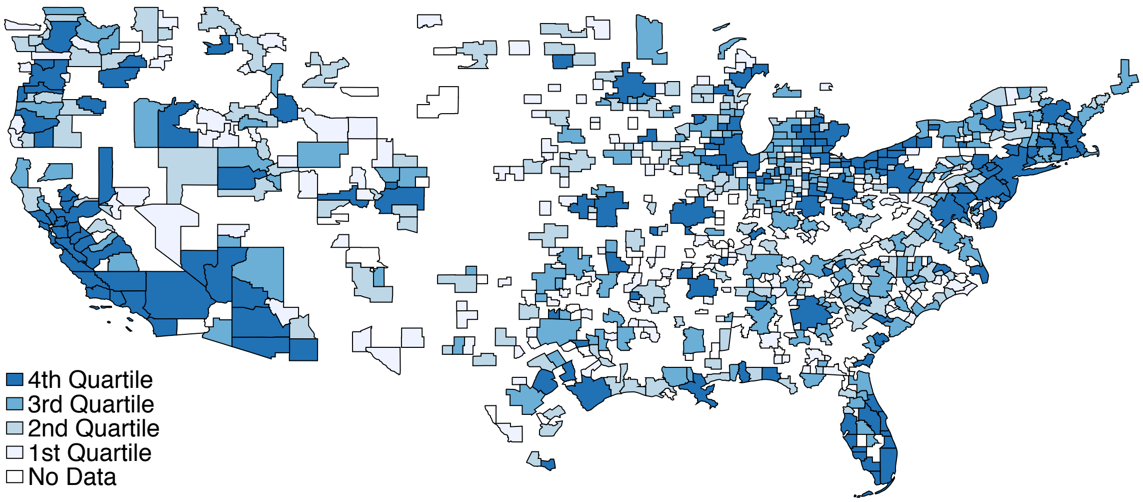
Figure 1
House Price Variation and Inventor Location

Panel (a) of this figure shows quartiles of zip code level price variance by CBSA. Panel (b) shows quartiles of the number of inventors by CBSA, based on residence.

(a) **Local House Price Variation**



(b) **Number of Inventors by Location**

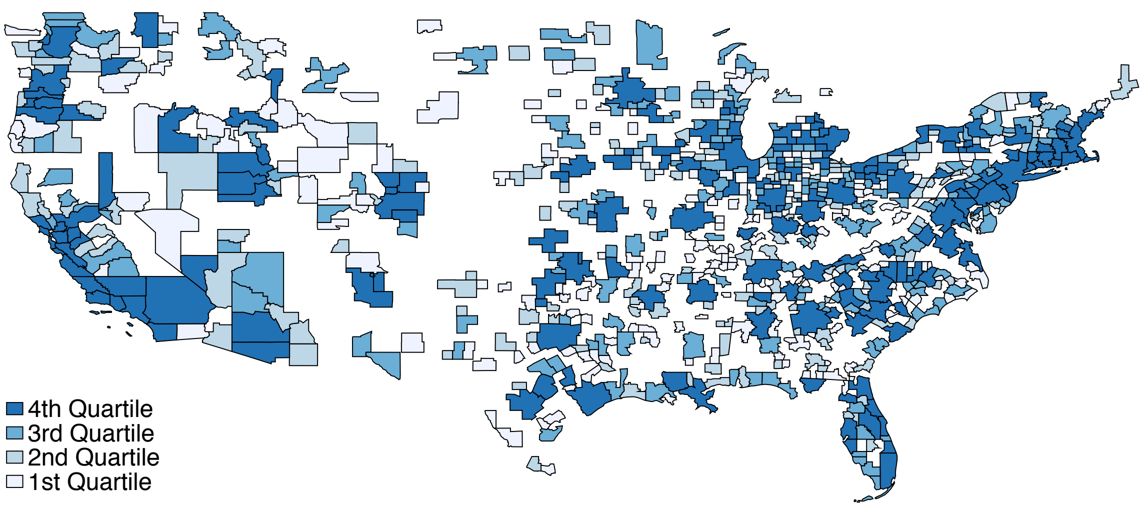
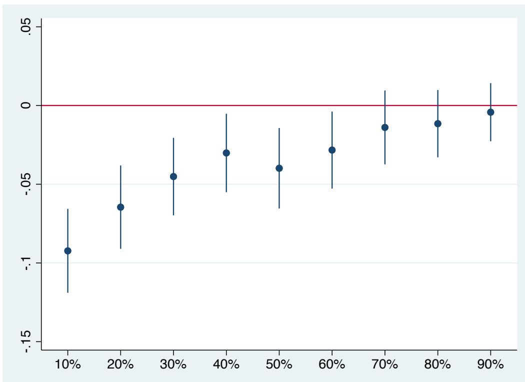


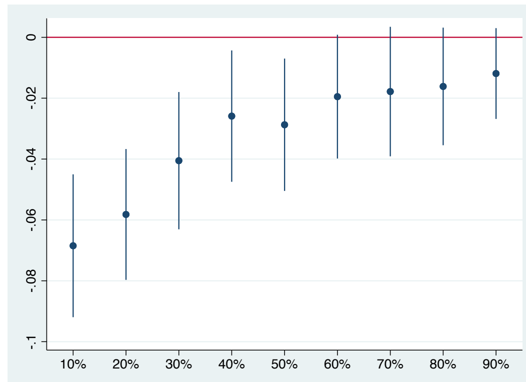
Figure 2
Treatment Intensity

This figure repeats the analysis of Tables 4-6, but separating the variable %Δ *House Price* to 10 decile dummy variables, and plots these estimates. The specification includes firm by CBSA fixed effects, and graphs report estimates of the 9 house price change deciles, relative to omitted category. The omitted category is the 10th decile (highest percentage change). Confidence intervals are at the 5% level.

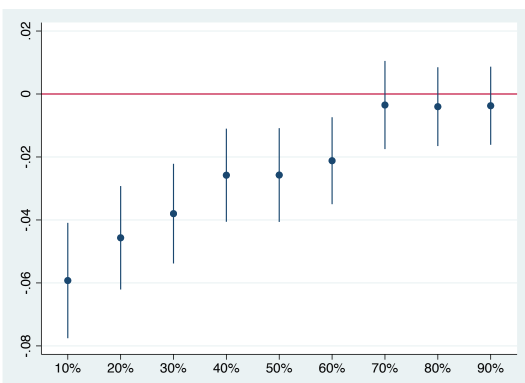
(a) **Number of Patents**



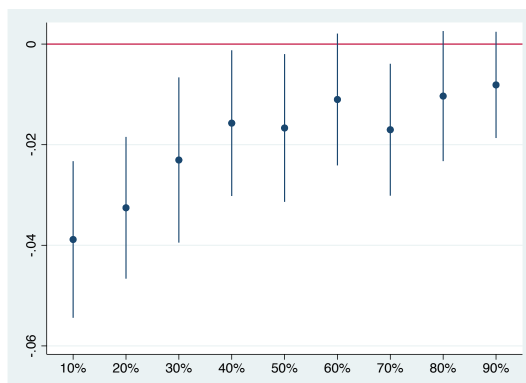
(b) **Citations per Patent**



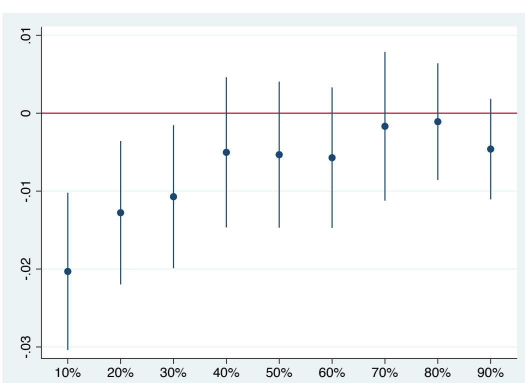
(c) **Originality**



(d) **Generality**



(e) **New Technology**



(f) **Exploratory Patents**

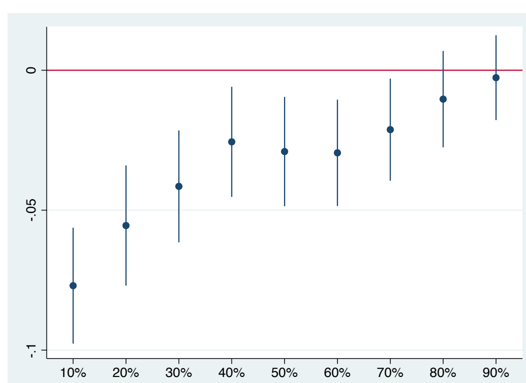


Table 1
Summary Statistics

Panel A of this table shows summary statistics for patent measures used in the analysis. The patent variables are measured over the years 2008–2012. *Number of Patents* is defined as the number of eventually granted patents applied for by an inventor during the period. *Normalized Citations Per Patent* is the total number of normalized citations received by an inventor's patents, divided by *Number of Patents*. A patent's normalized citations are its total citations received divided by the mean number of citations received by patents granted in the same year and technology class. *Number of Top Cited Patents* counts the number of an inventor's patents that were in the top 10% of all patents granted in the same year and technology class in terms of citations. *New Class Indicator* is an indicator variable equally to one if any of the inventor's patents were in a technology class the inventor's firm has never patented in before. *Number of Exploratory Patents* counts the number of an inventor's patents that are exploratory in the sense that less than 20% of the patents they cite are existing knowledge from the point of view of the inventor's firm. Existing knowledge is defined as all patents the firm was granted in the past five years as well as all patents the firm cited in the same time frame. *Normalized Generality Per Patent* is defined as the average normalized generality for an inventor's patents. Normalized generality scales generality by the mean value of generality for all patents granted in the same year and technology class. Generality is equal to one minus the Herfindahl-Hirschman Index (HHI) of forward citations across technology classes. *Normalized Originality Per Patent* is defined analogously to *Normalized Generality Per Patent* but with respect to backward citations rather than forward citations. Panel B shows the correlation among the patent measures from Panel A.

Panel A: Patent Measures (2008-2012)

Variables	Obs	Mean	Std Dev
Log(Number of Patents)	162,011	0.64	0.80
Log(Normalized Citations Per Patent)	162,011	0.27	0.50
Log(Number of Top Cited Patents)	162,011	0.17	0.44
Log(Normalized Generality Per Patent)	162,011	0.15	0.33
Log(Normalized Originality Per Patent)	162,011	0.35	0.38
New Technology Indicator	162,011	0.09	0.28
Log(Number of Exploratory Patents)	162,011	0.23	0.47

Panel B: Patent Measure Correlation Matrix (2008-2012)

	Cites	Top	Gen	Orig	New	Explore
Log(Normalized Citations Per Patent)	1					
Log(Number of Top Cited Patents)	0.737	1				
Log(Normalized Generality Per Patent)	0.834	0.617	1			
Log(Normalized Originality Per Patent)	0.545	0.406	0.471	1		
New Technology Indicator	0.231	0.255	0.191	0.295	1	
Log(Number of Exploratory Patents)	0.317	0.448	0.257	0.425	0.410	1

Table 1
(Continued)

Panel C shows summary statistics for characteristics of employees in our sample as of 2007. The *Degree* variables are dummy variables equal to one if the employee holds the stated degree (employees can have multiple degrees). The variable *Age* is defined as 2007 minus the year the employee first obtained a degree plus twenty-two. The variable *Work Experience* is equal to 2007 minus the start year of the employee's first work position. The variable *Tenure at Firm* is equal to 2007 minus the start year of the employee's 2007 work position. The variable *Senior Position* is an indicator equal to one if the inventor's position title includes managerial keywords (CEO, chair, chairman, chief, CTO, director, executive, head, management, manager, partner, president, principal, VP). Panel D shows summary statistics for house characteristics of employees in our sample as of 2007. The variable *Years Owned House* is the years the employee had owned the house as of 2007, *Square footage* is the size of the employee's house as of 2007, *Age of House* is the age of the house in years as of 2007, %Δ*House Price Pre* is the percent change in house prices in the zip code of the inventor's house from the end of 2004 to the end of 2007, %Δ*House Price Post* is the percent change in house prices in the zip code of the inventor's house from the end of 2007 to the end of 2012.

Panel C: Employee Characteristics (2007)

Variables	Obs	Mean	Std Dev
BA Degree	58,750	0.97	0.17
MA Degree	58,750	0.30	0.46
PhD Degree	58,750	0.28	0.45
MBA Degree	58,750	0.09	0.29
JD Degree	58,750	0.01	0.09
MD Degree	58,750	0.01	0.09
Age	49,077	41.14	8.93
Work Experience	61,180	15.60	8.37
Tenure at Firm	57,892	6.47	6.86
Senior Position	69,930	0.48	0.50

Panel D: Employee House Characteristics (2007)

Variables	Obs	Mean	Std Dev
Years Owned House	157,194	7.66	5.91
Square Footage	107,074	2952.73	1919.70
Age of House	144,747	29.77	26.85
%Δ House Price Pre (2004-2007)	162,011	0.22	0.15
%Δ House Price Post (2007-2012)	162,011	-0.16	0.13

Table 1
(Continued)

Panel E shows the distribution of employees across fields. Employees are categorized using their modal NBER technology subcategory for patents applied from 2005–2007.

Panel E: Distribution of Employees Across Fields (2007)

NBER subcategory	Freq	Percent
Computer Hardware & Software	19,153	11.82
Communications	16,530	10.21
Drugs	13,445	8.30
Chemical (miscellaneous)	8,889	5.49
Electronic Business Methods and Software	8,081	4.99
Surgery and Medical Instruments	7,542	4.66
Semiconductor Devices	7,380	4.56
Information Storage	6,457	3.99
Power Systems	5,861	3.62
Measuring & Testing	5,424	3.35
Mechanical (miscellaneous)	4,696	2.90
Transportation	3,890	2.40
Electrical Devices	3,765	2.32
Computer Peripherals	3,419	2.11
Materials Processing and Handling	3,251	2.01
Motors, Engines and Parts	3,173	1.96
Electrical and Electronics (miscellaneous)	2,976	1.84
Resins	2,813	1.74
Nuclear, X-rays	2,497	1.54
Organic compounds	2,253	1.39

Table 2

Quantity and Quality of Innovation

This table estimates the effect of changes in zip code level house prices on the quantity and quality of innovative output for patent inventors who own a house. The pre-period is defined as 2005–2007. The post-period is defined as 2008–2012. The sample consists of US inventors within firms who are research-active as of onset of the crisis in 2008 (i.e., were credited on at least one assigned patent in the pre-period). All variables are as defined in Table 1. Standard errors appear in parentheses and are clustered by firm and inventor residential zip code. *,**, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

	Log(Number of Patents Post)		Log(Citations Per Patent Post)		Log(Top Cited Patents Post)	
	(1)	(2)	(3)	(4)	(5)	(6)
%Δ House Price Post	0.218***	0.219***	0.172***	0.172***	0.135***	0.135***
	(0.0317)	(0.0316)	(0.0240)	(0.0239)	(0.0190)	(0.0189)
%Δ House Price Pre		-0.0310		0.00866		0.00904
		(0.0523)		(0.0432)		(0.0343)
Pre-2008 Measure	0.789***	0.789***	0.212***	0.212***	0.416***	0.416***
	(0.0205)	(0.0205)	(0.00895)	(0.00896)	(0.0138)	(0.0138)
Firm × CBSA FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.290	0.290	0.048	0.048	0.157	0.157
Observations	162,011	162,011	162,011	162,011	162,011	162,011

Table 3
Originality and Generality

This table estimates the effect of changes in zip code level house prices on the originality and generality of innovative output for inventors that own a house. The pre-period is defined as 2005–2007. The post-period is defined as 2008–2012. The sample consists of US inventors within firms who are research-active as of onset of the crisis in 2008 (i.e., were credited on at least one assigned patent in the pre-period). All variables are as defined in Table 1. Standard errors appear in parentheses and are clustered by firm and inventor residential zip code. *,**, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

	Log(Generality Post)		Log(Originality Post)	
	(1)	(2)	(3)	(4)
%Δ House Price Post	0.0922***	0.0921***	0.156***	0.156***
	(0.0163)	(0.0163)	(0.0195)	(0.0194)
%Δ House Price Pre		0.00317		-0.00821
		(0.0277)		(0.0328)
Pre-2008 Measure	0.123***	0.123***	0.192***	0.192***
	(0.00479)	(0.00479)	(0.00754)	(0.00754)
Firm × CBSA FE	Yes	Yes	Yes	Yes
R^2	0.023	0.023	0.010	0.010
Observations	162,011	162,011	162,011	162,011

Table 4
Exploration

This table estimates the effect of changes in zip code level house prices on the exploratory nature of innovative output for inventors that own a house. The pre-period is defined as 2005–2007. The post-period is defined as 2008–2012. The sample consists of US inventors within firms who are research-active as of onset of the crisis in 2008 (i.e., were credited on at least one assigned patent in the pre-period). All variables are as defined in Table 1. Standard errors appear in parentheses and are clustered by firm and inventor residential zip code. *,**, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

	New Technology Indicator Post		Log(Exploratory Patents Post)	
	(1)	(2)	(3)	(4)
%Δ House Price Post	0.0486***	0.0489***	0.188***	0.188***
	(0.0118)	(0.0118)	(0.0237)	(0.0236)
%Δ House Price Pre		-0.0265		0.0309
		(0.0186)		(0.0399)
Pre-2008 Measure	0.0756***	0.0756***	0.277***	0.277***
	(0.00431)	(0.00431)	(0.0105)	(0.0105)
Firm $\times$ CBSA FE	Yes	Yes	Yes	Yes
R^2	0.008	0.008	0.077	0.077
Observations	162,011	162,011	162,011	162,011

Table 5
Inventors Remaining at Same Firm

This table repeats the analysis of Tables 2–3, limiting the sample to inventors who are observed patenting at their pre-crisis firm or who list themselves as still employed at their pre-crisis firm on LinkedIn after our estimation period ends in 2012. Standard errors appear in parentheses and are clustered by firm and inventor residential zip code. *,**, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

	(1) Num	(2) Cites	(3) Top	(4) Gen	(5) Orig	(6) New	(7) Explore
%Δ House Price Post	0.296***	0.246***	0.223***	0.131***	0.181***	0.0637***	0.240***
	(0.0562)	(0.0364)	(0.0338)	(0.0268)	(0.0269)	(0.0177)	(0.0401)
Pre-2008 Measure	0.757***	0.263***	0.465***	0.162***	0.257***	0.0986***	0.310***
	(0.0225)	(0.0120)	(0.0149)	(0.00727)	(0.0116)	(0.00664)	(0.0128)
Firm $\times$ CBSA FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.308	0.066	0.178	0.033	0.020	0.012	0.088
Observations	77,942	77,942	77,942	77,942	77,942	77,942	77,942

Table 6

Alternative Specifications

This table repeats the analysis of Tables 2–3 but allowing the firm by CBSA fixed effects to interact with various other 2007 characteristics. For brevity, only the main coefficient on *%Δ House Price Post* is shown, but other controls remain similar. *Tech Class* is the modal 3-digit technology class of the inventor's patents in the pre-period. The variables *Neighborhood Income Q., Square Footage Q., Urban Neighborhood Q.,* and *Family Neighborhood Q* are quartiles of the respective variables. *Patent Experience Q.* are quartiles based on the number of years since the inventor's first patent (as of 2007). *Age Q.* are quartiles based on the number of years since the inventor's first degree (as of 2007), plus twenty-two. *Education* represent the inventor's highest degree as defined in Panel A of Table 1. *Senior Position* is an indicator equal to one if the inventor's position title includes managerial keywords (CEO, chair, chairman, chief, CTO, director, executive, head, management, manager, partner, president, principal, VP). Panel A specifications use the full sample, while Panel B specifications use only inventors with available information on LinkedIn. Standard errors are clustered by firm and inventor residential zip code. *,**, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Fixed Effects Specification	(1) Num	(2) Cites	(3) Top	(4) Gen	(5) Orig	(6) New	(7) Explore
Panel A: Full Sample							
(1) Firm × CBSA FE	0.218***	0.172***	0.135***	0.0922***	0.156***	0.0486***	0.188***
	(0.0317)	(0.0240)	(0.0190)	(0.0163)	(0.0195)	(0.0118)	(0.0237)
(2) Firm × CBSA × Tech Class FE	0.181***	0.137***	0.127***	0.0941***	0.128***	0.0330**	0.168***
	(0.0366)	(0.0274)	(0.0234)	(0.0183)	(0.0203)	(0.0133)	(0.0287)
(3) Firm × CBSA × Neighborhood Income Q. FE	0.201***	0.143***	0.107***	0.0819***	0.147***	0.0464***	0.183***
	(0.0398)	(0.0332)	(0.0265)	(0.0220)	(0.0231)	(0.0152)	(0.0312)
(4) Firm × CBSA × Family Neighborhood Q. FE	0.198***	0.166***	0.127***	0.0870***	0.143***	0.0561***	0.184***
	(0.0401)	(0.0309)	(0.0239)	(0.0226)	(0.0239)	(0.0145)	(0.0320)
(5) Firm × CBSA × Urban Neighborhood Q. FE	0.232***	0.186***	0.142***	0.0936***	0.170***	0.0600***	0.199***
	(0.0334)	(0.0285)	(0.0212)	(0.0201)	(0.0224)	(0.0138)	(0.0264)
(6) Firm × CBSA × Square Footage Q. FE	0.193***	0.160***	0.124***	0.0876***	0.138***	0.0382***	0.162***
	(0.0334)	(0.0273)	(0.0212)	(0.0185)	(0.0201)	(0.0128)	(0.0269)
(7) Firm × CBSA × Experience Q. FE	0.191***	0.142***	0.111***	0.0741***	0.115***	0.0480***	0.151***
	(0.0325)	(0.0246)	(0.0201)	(0.0172)	(0.0176)	(0.0126)	(0.0256)
Panel B: LinkedIn Sample							
(1) Firm × CBSA FE	0.270***	0.238***	0.174***	0.135***	0.198***	0.0402**	0.233***
	(0.0498)	(0.0372)	(0.0317)	(0.0253)	(0.0278)	(0.0171)	(0.0359)
(2) Firm × CBSA × Age Q. FE	0.309***	0.241***	0.181***	0.146***	0.202***	0.0332	0.295***
	(0.0721)	(0.0563)	(0.0532)	(0.0361)	(0.0441)	(0.0255)	(0.0505)
(3) Firm × CBSA × Education FE	0.224***	0.186***	0.128***	0.118***	0.174***	0.00618	0.180***
	(0.0549)	(0.0415)	(0.0375)	(0.0290)	(0.0287)	(0.0186)	(0.0424)
(4) Firm × CBSA × Senior Position FE	0.284***	0.231***	0.176***	0.131***	0.199***	0.0335**	0.237***
	(0.0530)	(0.0412)	(0.0367)	(0.0285)	(0.0283)	(0.0169)	(0.0412)

Table 7
House Ownership Duration

This table repeats the analysis of Tables 2–3, now allowing *%Δ House Price Post* to interact a *Purchased before 2004* indicator equal to one if the inventor's house was purchased prior to 2004. Standard errors are clustered by firm and inventor residential zip code. *,**, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

	(1) Num	(2) Cites	(3) Top	(4) Gen	(5) Orig	(6) New	(7) Explore
%Δ House Price Post ×	-0.0985***	-0.0636**	-0.0361*	-0.0341**	-0.0407**	0.00348	-0.00889
Purchase before 2004	(0.0370)	(0.0257)	(0.0191)	(0.0163)	(0.0200)	(0.0129)	(0.0265)
Pre-2008 Measure	0.790***	0.217***	0.413***	0.126***	0.193***	0.0763***	0.270***
	(0.0181)	(0.00818)	(0.0120)	(0.00445)	(0.00725)	(0.00377)	(0.00926)
Zip Code FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.473	0.314	0.370	0.279	0.264	0.328	0.277
Observations	162,011	162,011	162,011	162,011	162,011	162,011	162,011

Table 8
Labor Market

This table repeats the analysis of Tables 2–3, now allowing *%Δ House Price Post* to interact a *Popular Technology* indicator. To define the *Popular Technology* indicator, we classify inventors to a technology class based on the modal technology class they patented in during the three years before the crisis (2005–2007). An inventor is considered to specialize in a popular technology if the inventor's technology class is in the top quartile in terms of number of total inventors. Standard errors are clustered by firm and inventor residential zip code. *,**, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

	(1) Num	(2) Cites	(3) Top	(4) Gen	(5) Orig	(6) New	(7) Explore
%Δ House Price Post ×	-0.114**	-0.0862***	-0.0585**	-0.0509**	-0.0718***	0.0207	-0.0419
Popular Technology	(0.0514)	(0.0306)	(0.0245)	(0.0203)	(0.0271)	(0.0140)	(0.0291)
Pre-2008 Measure	0.789***	0.218***	0.413***	0.126***	0.191***	0.0760***	0.271***
	(0.0183)	(0.00819)	(0.0120)	(0.00447)	(0.00719)	(0.00378)	(0.00935)
Zip Code FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.470	0.313	0.369	0.279	0.263	0.328	0.275
Observations	162,011	162,011	162,011	162,011	162,011	162,011	162,011

Table 9

Housing Prices Effects in 2002

This table repeats the analysis of Tables 2–3, but estimates the effect of changes in zip code level house prices on innovative output for an earlier period. The pre-period is defined as 1999–2001. The post-period is defined as 2002–2006. The sample consists of US inventors within firms who are research-active as of onset of the crisis in 2008 (i.e., were credited on at least one assigned patent in the pre-period). All variables are as defined in Table 1. Standard errors appear in parentheses and are clustered by firm and inventor residential zip code. *,**, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

	(1) Num	(2) Cites	(3) Top	(4) Gen	(5) Orig	(6) New	(7) Explore
%Δ House Price Post	-0.0233	0.00971	-0.00680	-0.00349	-0.0153	-0.0130	-0.0228
	(0.0463)	(0.0261)	(0.0235)	(0.0219)	(0.0222)	(0.0140)	(0.0327)
Pre-2002 Measure	0.539***	0.154***	0.252***	0.118***	0.163***	0.0442***	0.178***
	(0.0247)	(0.00680)	(0.0113)	(0.00575)	(0.00808)	(0.00387)	(0.0102)
Firm $\times$ CBSA FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.105	0.028	0.061	0.009	0.005	0.003	0.029
Observations	161,887	161,887	161,887	161,887	161,887	161,887	161,887

Appendix

A Tables

Table A.1
Excluding 2008-2009 Patents

This table repeats the analysis of Tables 2–3, now excluding patents applied for in 2008-2009 from the outcome variables. Standard errors are clustered by firm and inventor residential zip code. *,**, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

	(1) Num	(2) Cites	(3) Top	(4) Gen	(5) Orig	(6) New	(7) Explore
%Δ House Price Post	0.149***	0.136***	0.0915***	0.0694***	0.128***	0.0317***	0.109***
	(0.0249)	(0.0215)	(0.0141)	(0.0135)	(0.0183)	(0.00874)	(0.0167)
Pre-2008 Measure	0.544***	0.161***	0.265***	0.0784***	0.143***	0.0380***	0.145***
	(0.0137)	(0.00769)	(0.0104)	(0.00452)	(0.00692)	(0.00305)	(0.00612)
Firm $\times$ CBSA FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.223	0.033	0.112	0.013	0.006	0.004	0.044
Observations	162,011	162,011	162,011	162,011	162,011	162,011	162,011

Table A.2
Three-Year House Price Changes

This table repeats the analysis of Tables 2–3, redefining %Δ *House Price* to represent the zip code level change in house prices from 2007 to 2010, rather than 2007 to 2012. Standard errors appear in parentheses and are clustered by firm and inventor residential zip code. *,**, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

	(1) Num	(2) Cites	(3) Top	(4) Gen	(5) Orig	(6) New	(7) Explore
%Δ House Price Post	0.264***	0.192***	0.154***	0.100***	0.177***	0.0489***	0.203***
	(0.0378)	(0.0282)	(0.0238)	(0.0194)	(0.0237)	(0.0147)	(0.0292)
Pre-2008 Measure	0.789***	0.212***	0.416***	0.123***	0.192***	0.0756***	0.277***
	(0.0204)	(0.00894)	(0.0138)	(0.00479)	(0.00755)	(0.00431)	(0.0105)
Firm $\times$ CBSA FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.290	0.048	0.157	0.023	0.010	0.008	0.077
Observations	161,971	161,971	161,971	161,971	161,971	161,971	161,971

Table A.3
One-Year House Price Changes

This table repeats the analysis of Tables 2–3, redefining %Δ *House Price* to represent the zip code level change in house prices from 2007 to 2008, rather than 2007 to 2012. Standard errors appear in parentheses and are clustered by firm and inventor residential zip code. *,**, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

	(1) Num	(2) Cites	(3) Top	(4) Gen	(5) Orig	(6) New	(7) Explore
%Δ House Price Post	0.292***	0.230***	0.200***	0.116***	0.213***	0.0700***	0.237***
	(0.0534)	(0.0379)	(0.0305)	(0.0257)	(0.0355)	(0.0210)	(0.0445)
Pre-2008 Measure	0.789***	0.212***	0.416***	0.123***	0.192***	0.0756***	0.277***
	(0.0205)	(0.00894)	(0.0138)	(0.00478)	(0.00754)	(0.00431)	(0.0105)
Firm × CBSA FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.290	0.048	0.157	0.023	0.010	0.008	0.077
Observations	161,989	161,989	161,989	161,989	161,989	161,989	161,989

Table A.4
One-Year House Price Changes and Excluding 2008-2009 Patents

This table repeats the analysis of Tables 2–3, redefining %Δ *House Price* to represent the zip code level change in house prices from 2007 to 2008, and also excluding patents applied for in 2008-2009 from the outcome variables. Standard errors appear in parentheses and are clustered by firm and inventor residential zip code. *,**, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

	(1) Num	(2) Cites	(3) Top	(4) Gen	(5) Orig	(6) New	(7) Explore
%Δ House Price Post	0.194***	0.179***	0.131***	0.0831***	0.171***	0.0446***	0.141***
	(0.0443)	(0.0337)	(0.0224)	(0.0210)	(0.0335)	(0.0158)	(0.0314)
Pre-2008 Measure	0.544***	0.161***	0.265***	0.0785***	0.143***	0.0381***	0.145***
	(0.0137)	(0.00769)	(0.0104)	(0.00452)	(0.00693)	(0.00305)	(0.00612)
Firm × CBSA FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.223	0.033	0.112	0.013	0.006	0.004	0.044
Observations	161,989	161,989	161,989	161,989	161,989	161,989	161,989

Table A.5

Alternative House Prices Measure (Zillow)

This table repeats the analysis of Tables 2–3, using an alternative zip code level price index from Zillow. Standard errors are clustered by firm and inventor residential zip code. *,**, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

	(1) Num	(2) Cites	(3) Top	(4) Gen	(5) Orig	(6) New	(7) Explore
%Δ House Price Post	0.188***	0.129***	0.105***	0.0665***	0.135***	0.0426***	0.162***
	(0.0283)	(0.0224)	(0.0166)	(0.0140)	(0.0155)	(0.00956)	(0.0185)
Pre-2008 Measure	0.787***	0.213***	0.418***	0.124***	0.193***	0.0773***	0.278***
	(0.0198)	(0.00893)	(0.0143)	(0.00477)	(0.00781)	(0.00439)	(0.0105)
Firm × CBSA FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.290	0.048	0.159	0.023	0.011	0.008	0.078
Observations	153,169	153,169	153,169	153,169	153,169	153,169	153,169

Table A.6
Firm Size

This table repeats the analysis of Tables 2–3, successively limiting the sample to firms with less than 1000, 100, 50, 30 and 10 inventors in the sample, respectively. Standard errors appear in parentheses and are clustered by firm and inventor residential zip code. *,**, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

	(1) Num	(2) Cites	(3) Top	(4) Gen	(5) Orig	(6) New	(7) Explore
Less than 1000 inventors (N=138564)							
%Δ House Price Post	0.206***	0.173***	0.142***	0.0886***	0.146***	0.0502***	0.179***
	(0.0357)	(0.0275)	(0.0210)	(0.0183)	(0.0221)	(0.0138)	(0.0271)
Less than 100 inventors (N=87355)							
%Δ House Price Post	0.161***	0.151***	0.122***	0.0833***	0.125***	0.0761***	0.156***
	(0.0477)	(0.0375)	(0.0284)	(0.0249)	(0.0286)	(0.0221)	(0.0329)
Less than 50 inventors (N=71843)							
%Δ House Price Post	0.117**	0.149***	0.123***	0.0733**	0.105***	0.0824***	0.150***
	(0.0556)	(0.0457)	(0.0352)	(0.0290)	(0.0325)	(0.0280)	(0.0400)
Less than 30 inventors (N=61366)							
%Δ House Price Post	0.0835	0.147***	0.115***	0.0833**	0.108***	0.0772**	0.119***
	(0.0604)	(0.0512)	(0.0393)	(0.0337)	(0.0364)	(0.0338)	(0.0443)
Less than 10 inventors (N=43944)							
%Δ House Price Post	0.121	0.145**	0.117**	0.104**	0.119***	0.116**	0.162***
	(0.0741)	(0.0630)	(0.0494)	(0.0418)	(0.0459)	(0.0506)	(0.0570)