NBER WORKING PAPER SERIES

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Working Paper 24011
http://www.nber.org/papers/w24011

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
November 2017, Revised April 2018

Previously circulated as "Does Economic Insecurity Affect Employee Innovation?" We are grateful to Elizabeth Berger, Nick Bloom, Hans Hvide, Sabrina Howell, Hyunseob Kim, Josh Lerner, Hanno Lustig, William Mann, Ramana Nanda, Hoai-Luu Nguyen, Luigi Pistaferri as well as seminar participants at Boston College, London Business School Summer Symposium, Bergen Innovation Workshop, NBER SI Innovation, HULM Conference, Notre Dame, Stanford (GSB), Tuck Private Equity and Entrepreneurship Conference, University of British Columbia, University of Colorado Boulder, University of Texas Austin, and Western Finance Association for helpful comments and suggestions. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

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November 2017, Revised April 2018
JEL No. G01,O3,O31,O32

ABSTRACT

We investigate how the deterioration of household balance sheets affects worker productivity, and whether such effects mitigate or amplify economic downturns. To do so, we compare the output of innovative workers who experienced different declines in housing wealth, but who were employed at the same firm and lived in the same area at the onset of the 2008 crisis. We find that, following a negative wealth shock, innovative workers become less productive, and generate lower economic value for their firms. Consistent with a debt-related channel, the effects are more pronounced among those with little home equity before the crisis and those with fewer outside labor market opportunities.

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1 Introduction

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 deterioration of household balance sheets during the Great Recession due to the collapse of the housing market led to severe consequences, as consumers cut back significantly on their spending (Mian et al., 2013). This reduction in aggregate demand, in turn, led to a significant decrease in employment by non-tradable firms, further amplifying the severity of the crisis (Mian and Sufi, 2014). While a number of papers have examined these demand-side effects, it is possible that housing wealth shocks might not only affect the consumption patterns of households but might also impact their labor productivity, leading to additional supply-side channels through which the collapse of the housing market affected firms during the crisis.

In this paper, we investigate what impact, if any, the deterioration of household balance sheets has on worker productivity, and whether such effects mitigate or amplify the impact of economic downturns. To study this question, we examine the output of innovative workers employed within firms during the wake of the 2008 financial crisis.

We focus on the output of innovative workers, as innovation has long been recognized to be a critical driver of economic growth (Solow, 1957). In the context of the Great Recession, Hall (2015) shows that technological advancement slowed down significantly following the crisis, and suggests that this slowdown likely had long-lasting effects on the overall economy, even after consumer demand recovered. In addition to being important, innovative output is advantageous to study, as unlike most forms of worker output, measures of innovative output are available at the individual level. In particular, innovative output can be measured through patents, which credit individuals as inventors, even when the patent is assigned to a firm. Using patent-based measures, we can not only observe the quantity of a worker's innovative output but also characterize the quality and nature of this output in a very detailed manner. It is even possible to quantify this output in terms of its value to a worker's firm, as shown by Kogan et al. (2017). This allows us to explicitly link housing wealth shocks to the economic value workers generate for their firms.

Theory provides little guidance on whether household wealth shocks would either increase or decrease worker productivity. On the one hand, negative wealth shocks, and the resulting potential for financial distress, may lead individuals to work harder so as to increase job security or to make up for lost wealth (Rizzo and Zeckhauser, 2003; Goette et al., 2004; Disney et al., 2010; Cesarini et al., 2017). If these forces dominate, supply-side effects would help mitigate the impact of economic downturns. On the other hand, such shocks have also been shown to lead to declines in household spending, which may include spending on things that allow a worker to be more productive at work (Mian et al., 2013). For example, negative wealth shocks may cause workers to allocate more time and to home production as a substitute for purchasing market goods, leaving less time and energy for productivity in wage employment.[1] Workers may also cut back spending on health, education, and professional development. More generally, dealing with the consequences of negative wealth shocks may be both time consuming and stressful, making workers less productive (Deaton, 2012; Currie and Tekin, 2015; Dobbie and Song, 2015; Engelberg and Parsons, 2016). If these forces dominate, supply-side effects would amplify the impact of economic downturns. Ultimately, the question is an empirical one.

To study this question, we construct a novel dataset that links the output of innovative workers, as measured by their patents, with deed records. This allows us to examine the productivity response of individual workers who experienced major declines in housing wealth during the crisis.

It is important that our analysis is at the individual level rather than the firm level. Firms located in regions in which housing prices collapsed may produce less innovative output for reasons unrelated to the financial circumstances of their employees. For example, firms in crisis-affected areas may experience a decline in demand (Mian et al., 2013), or a tightening of financial constraints

[1]The idea that individuals substitute home production for market goods goes back to Becker (1965). Examples of goods where it may be possible to substitute home production for market purchase include child care, home/auto maintenance (housecleaning, lawn mowing, painting, repairs, and the like), food preparation, and financial services such as preparation of income tax returns. A large literature explores the link between home production and the business cycle (See, e.g., Benhabib et al., 1991; Greenwood and Hercowitz, 1991; Baxter and Jermann, 1999; Campbell and Ludvigson, 2001). More recently, Aguiar et al. (2013) show that time spent on home production increased during the Great Recession.

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.

By conducting the analysis at the individual level, we are able to compare 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. Since some firms may have multiple divisions scattered across different geographies, we apply an even stricter analysis, by comparing only employees who both work at the same firm and also live in the same metropolitan area, as defined by a census Core Based Statistical Area (CBSA).[2] 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 the output of innovative workers. We find that workers who experience a negative housing wealth shock produce fewer patents and patents of lower quality based on citations. Such workers 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 workers also produce narrower innovations, combining information from fewer disparate fields. These effects are strongest among those who suffer the largest housing price declines. Overall, the evidence suggests that, following a housing wealth shock, workers are less likely to successfully pursue innovative projects, particularly ones that are high impact, complex, or exploratory. These results are inconsistent with the hypothesis that negative wealth shocks may lead workers to become more productive.

To explore the robustness of these findings, we conduct even narrower comparisons within firms. For example, we compare the relative response of individuals who, in addition to working at the

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

same firm and living in the same area, specialized in the same technology at the onset of the crisis. Furthermore, we complement the patent data with data from LinkedIn, and compare workers within the same firm and metropolitan area, who are also similar in terms of other characteristics such as age, educational attainment, job title, house type, or neighborhood type. In all these cases, our key results remain remarkably stable. Therefore, our findings are unlikely to be explained by sorting of certain types of workers within a firm into more crisis-affected zip codes of a given metropolitan area.

To evaluate whether our estimated effects on patenting are economically meaningful, we use the methodology of Kogan et al. (2017) to assign a value to each patent that workers in our data produce. This measure is based on the stock market's reaction to the announcement of a particular patent grant. Using this measure, we find that workers who experience a negative housing wealth shock do indeed produce less value for their firm during the crisis than others working at the same firm who do not experience such a shock. Specifically, workers in the bottom quartile of house price changes produce 15% less economic value than workers in the top quartile.

A further question is whether the declines in individual-level output that we document translate into declines in firm-level output. For example, firm-level output might be unaffected if firms were able to simply shift work from individuals who had house price declines to those who did not. In our view, though, such perfect substitutability is unlikely given the high degree of specialized knowledge and expertise required for innovative work (Hall and Lerner, 2010). To tackle this question empirically, we conduct an analysis at the firm level, comparing firms located in the same CBSA but with different employee-related exposure to the collapse of the housing market, generated by differences in where firms' employees live. We find that firms whose employees live in more impacted zip codes have more substantial declines in innovative output than firms located within the same CBSA whose employees live in less impacted zip codes. As discussed earlier, such firm-level analysis suffers from more potential endogeneity concerns. However, these results are at least suggestive that our well-identified individual-level results do aggregate to the firm level.

As far as we are aware, this paper is the first to illustrate that household balance sheet shocks affect worker productivity. Understanding the existence and direction of this link is critical to understanding the full scope of the effects of such shocks on the economy. While determining the precise mechanism underlying these results is more difficult, we conclude by exploring this question. In particular, we investigate whether our results appear to be driven purely by declines in household wealth or whether debt and related financial distress issues appear to play an important role.

The literature on housing-related financial distress shows that financial distress is usually triggered by the combination of two conditions: negative home equity and a prolonged unemployment spell (Foote et al., 2008, 2010). Therefore, individuals who are more likely to become underwater or to become unemployed for an extended period of time are at greater risk. We find that the effect of negative housing shocks on worker productivity is stronger, precisely for those at greater risk along either of these dimensions. In particular, we find stronger declines in innovative productivity among workers with less accumulated home equity prior to the crisis and among workers with fewer outside labor market opportunities based on their field of expertise.[3]

There are at least two ways in which the risk of financial distress may affect productivity. First, Mian et al. (2013) show that highly levered individuals at risk of financial distress cut back on spending. This may include decreased spending on things that allow workers to be productive in wage employment, such as market goods that substitute for home production (Becker, 1965; Baxter and Jermann, 1999; Aguiar et al., 2013). Second, individuals at risk of financial distress may experience increased stress, anxiety, and distraction, which in turn may make them less productive (Deaton, 2012; Engelberg and Parsons, 2016; Currie and Tekin, 2015; Dobbie and Song, 2015).

As a final way of exploring the mechanism underlying our main results, we also examine whether positive wealth shocks increase productivity in the same way that negative wealth shocks decrease productivity. To examine the impact of positive wealth shocks, we repeat our analysis during the boom period leading up to the crisis, between 2002 and 2007. We find no statistically significant

[3]While the innovative workers in our sample are unlikely to be poor, they still face the possibility of financial distress. The expansion of credit in the run-up to the crisis was not solely confined to the poorest households (e.g., Mian and Sufi, 2016; Foote et al., 2016; Adelino et al., 2016). As a result, higher income households also became underwater during the crisis and were subject to the associated risk of financial distress. 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.

relation between house price changes and innovative output during the boom. While by no means definitive, this asymmetry is again consistent with an underlying debt-related mechanism, as negative shocks might trigger financial distress, while positive shocks lead to no change in financial distress.

This paper relates to several strands of the literature. A handful of recent papers have examined 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 worker productivity within firms.

Another line of research explores the relationship between household leverage and labor supply (as in Bernstein, 2015; Mulligan 2008, 2010, 2009; Herkenhoff and Ohanian, 2011; and Donaldson et al., 2015). In that literature, the focus is largely on how the decision of whether to be in the labor force is impacted by means-tested mortgage modification programs. These programs implicitly decrease work incentives, as those with higher income end up having higher mortgage payments. Our focus is on individuals who are already employed and the impact of household leverage on worker productivity within firms.

Finally, this paper also relates to a large literature on the determinants of firm innovation, originated by Schmookler (1962), Griliches (1957), Nelson (1959), and Arrow (1962). That literature highlights a "top-down" view, in which firms' profit-driven objectives determine innovation policy, which is then implemented by employees. Accordingly, most of the work in this area 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 firm innovation may follow a "bottom-up" process, wherein innovative workers are not merely interchangeable parts, but play an important role in producing innovative.

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 U.S. 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.[4]

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

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

inventor, we combine the patent data with CoreLogic, which 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 estimates for non-traded houses.[5]

Together, we construct an annual worker-level panel. In each year we observe a worker's innovative output along with the location of the worker'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 worker characteristics during years in which the worker has zero patents. For example, if a worker changes firms, we can only observe the change the next time the worker patents. In order to ensure that we are studying workers 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

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

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 workers, working at 31,327 firms.

Neither the USPTO data, nor the CoreLogic data, give us detailed demographic characteristics for the workers 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 a worker in our sample with the worker's LinkedIn profile, we first find a set of potential profile URLs by using Google to search LinkedIn for profiles containing the worker's name in conjunction with variations of the names of each firm the worker'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.[6] Using this procedure, we are able to find a LinkedIn profile for 72,681 (45%) of the workers in our sample.

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).[7] 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 a worker's innovative output is the number of citations the worker'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

[6] 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.

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

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 a worker'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 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.[8] 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).

[8]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).

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 worker'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 worker's firm has never patented in before.

In general, projects that result in patents that are highly cited, original/general, or exploratory are likely harder for workers 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 worker level, and the patent measures for a given worker are based on the worker'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 innovative worker 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 workers 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 worker 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.[9]

Panel D shows summary statistics for characteristics of the houses owned by workers 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 workers across the top 20 most populated fields in our sample. Workers 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 workers in our sample. Communications is in the second most common category with 10.21% of the workers. Other common fields include drugs, chemicals, and semi-conductor devices.

3 Empirical Strategy

Our primary interest is in how changes in house prices associated with the 2008 financial crisis affect the innovative output of workers. 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 workers 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

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

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 a worker'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 who 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.[10] 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 workers who worked at the same firm and owned a house in the same general area, but who experienced differential price declines in their respective

[10]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.

zip codes.

This approach provides several advantages. First, the workers 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 workers 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 workers 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 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 workers 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 workers 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 workers 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 worker 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 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 may be larger for some subgroups relative to others. For example, the literature on housing-related financial distress shows that distress is usually triggered by the combination of two conditions: negative home equity and a prolonged unemployment spell (Foote et al., 2008, 2010). Therefore, individuals who are more likely to become underwater or to become unemployed for an extended period of time are at greater risk. Following this reasoning, the same house price shock may be more important for those who bought their house during the boom and thus accumulated less home equity prior to the crisis. Similarly, the same shock may be more important for workers who face a thin outside labor market based on their field of expertise. 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 a worker level characteristic such as an indicator for whether the worker bought their house during the boom, or an indicator for whether the worker 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 workers 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 $Characteristic = 0$. Thus, we can control for unobservable differences among workers who choose to live in different zip codes by examining whether workers who live in the same zip code 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. One is added to all logged variables that include zeros. In columns (1)–(2), we first examine the effect of changes in local house prices on the number of patents a worker 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 workers who work at the same firm and own a house in the same area, but live in different zip codes. Comparing such workers 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 a worker lives is strongly associated with lower patenting output. In column (2) we also include as an additional control the change in house prices that a worker'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 workers 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 worker 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 a worker'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, but 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, in Figure 2 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. For context, mean percentage changes in house price by decile are shown in Appendix Figure A.1. Figure 2 shows 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. This figure also demonstrates that the effects are economically as well as statistically significant. For example, workers in the bottom quartile of house price changes are approximately 6% less productive in terms of patent output than workers in the top quartile, while workers 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 workers 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 workers 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 workers 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 a worker'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 workers 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, workers become less productive, particularly with respect to projects that are high impact, complex, or exploratory in nature. Moreover, workers who are most severely affected by the housing shock adjust their innovative projects most strongly. These results are inconsistent with the hypothesis that negative wealth shocks may lead workers to become more productive.

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 workers 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 workers 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 workers 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 workers who remain at the same firm and are not due to transitions to unemployment or to less innovative firms.[11]

4.3 Selection Concerns

As discussed in Section 3, concerns about selection issues motivate our empirical design. Specifically, potential unobservable differences between firms, and across geographic regions, lead us to include firm by CBSA fixed effects in the baseline specifications. With these fixed effects, we are effectively comparing individuals who work at the same firm and reside in the same metropolitan area. In this

[11]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.

section, we explore the possibility that different types of workers within the same firm select into different types of neighborhoods within the same CBSA. Importantly, even if workers do select into neighborhoods based on their characteristics, this would not necessarily explain our results. It would also have to be the case that, among those with similar productivity in the pre-crisis period, the types who select into neighborhoods that declined also had lower productivity during the crisis for reasons unrelated to house prices. Nonetheless, we attempt to address such concerns by conducting even narrow comparisons among workers within firms.

4.3.1 Technology

One potential concern is that we might be comparing individuals who work at the same firm and live in the same CBSA, but who do not work in the same division of the firm. If, for some reason, 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 worker technology class fixed effects in our regressions. The results are shown in Row 2 of Table 6, Panel A. We define a worker's technology class to be the modal 3-digit class of the worker's patents in the pre-crisis period (2005–2007). This specification is very conservative in that it only identifies off of variation from individuals who 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.

4.3.2 Neighborhood Characteristics

Next, we compare workers who not only work at the same firm and live in the same CBSA, but who also live in neighborhoods with similar characteristics. The advantage of narrowing the comparison group in this way is that workers who live in similar neighborhoods are also more likely to be similar along other dimensions we cannot observe. The first neighborhood characteristic we consider is average income in a worker's zip code, based on the 2000 census. In Row 3 of Table 6, Panel A, we sort workers into zip code income quartiles within each CBSA, and then include firm by CBSA by zip code income quartile fixed effects in our regressions. These regressions compare two individuals who work at the same firm, live in the same CBSA, and live in zip codes within that CBSA with similar mean income levels. The next neighborhood characteristic we consider is the average number of children per household in the worker's zip code, again based on the 2000 census. In Row 4, we include firm by CBSA by zip code family size quartile fixed effects. The census also categorizes zip codes in terms of how urban they are. In Row 5, we include firm by CBSA by zip code urban measure quartile fixed effects.

Finally, we also make use of the fact that we observe the square footage of a worker's home from CoreLogic. In Row 6, we include firm by CBSA by square-footage quartile fixed effects. In all cases, we find similar results, even in these narrower comparison groups. These results help to address potential concerns that workers who differed in terms of income or family size may have selected into harder hit zip codes and may also have been less productive during the crisis for unrelated reasons.

4.3.3 Worker Characteristics

Next, we directly explore various worker characteristics that may be associated with selection into different zip codes within a CBSA. First, for each worker, we calculate experience as the number of years, as of 2007, since the worker's first patent. In Row 7 of Table 6, Panel A, we include firm by CBSA by experience quartile fixed effects in our regressions.

Our patent data does not provide information regarding other worker characteristics such as age, education, or job title. We therefore use the data from LinkedIn described earlier to compare workers who are similar along these dimensions. While we only have LinkedIn data for approximately half our sample, our baseline results remain similar in this subsample, a shown in Row 1 of Table 6, Panel B. In Row 2 of Table 6, Panel B, we in include firm by CBSA by age quartiles fixed effects in our regressions. In Row 3 we include firm by CBSA by education fixed effects. In Row 4 we include firm by CBSA by senior position fixed effects.

In all specifications, the estimated effects are similar to the baseline results. Again, these results help to address potential concerns that workers who differed in terms of experience, age, education, or job seniority, may have selected into harder hit zip codes and may also have been less productive during the crisis for unrelated reasons. While we do not argue that workers select into neighborhoods randomly, the above analysis suggests that such sorting is unlikely to explain our 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 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 workers 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

It is also possible that our results are driven by individuals 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 individuals working at firms with less than 1000 innovative workers, at firms with less than 100 innovative workers, at firms with less than 50 innovative workers, at firms with less than 30 innovative workers, and at firms with less than 10 innovative workers. In all of these subsamples, we continue to find similar results.

4.5 Innovative Output and Firm Value

Our evidence thus far illustrates that workers who experience significant housing price declines are less likely to successfully pursue innovative projects, particularly ones that are high impact, complex, or exploratory in nature. We have shown these effects using a variety of detailed patent-based measures. However, a natural question is whether such effects translate into an impact on firm value. Kogan et al. (2017) illustrate that the median patent value to firms is substantial ($3.2 million in 1982 dollars) and its economic value is strongly correlated with its scientific value, as measured by patent citations. Given that we find that workers affected by the collapse of the housing market produce fewer patents, and also patents that are less cited, it is plausible that they create less value for their firms as well.

To explore this question more directly, we use a measure of patent value developed by Kogan et al. (2017). This measure is based on the stock market reaction to the announcement of a patent grant. While the Kogan et al. (2017) measure has been made publicly available for patents granted through 2010, it does not cover all the patents underlying our outcome variables (i.e., patents applied for through 2012 and eventually granted). We therefore extend the Kogan et al. (2017) measure through the end our sample period using the same methodology.[12]

In Table 7 we repeat our baseline analysis using the Kogan et al. (2017) measure. Specifically, the outcome variable that we examine is the log of the total value of the patents a worker applied for in the post-crisis period. Of course, because we only observe a stock price reaction for publicly traded firms, we must restrict the sample to employees of such firms for this analysis.[13] As can be seen in columns (1) and (2), we find similar results for patent value. Workers who experience a significant decline in housing wealth produce less value for their firms compared to peers in the same firm and metropolitan area. Moreover, for a given decline in housing wealth, the percentage decline

[12] We verify that in the overlapping sample, we are able to replicate the Kogan et al. (2017) measure closely. Regressing our version on the original yields a coefficient of 0.99.

[13] Appendix Table A.7 shows that our baseline results remain similar in this subsample.

in the value of a worker's output is larger than the percentage decline in our other measures. Thus, if anything, our baseline results appear to understate the value implications of house price declines. In columns (3) and (4) we repeat the analysis, but now include Firm by CBSA by Technology class fixed effects. The results remain similar even when comparing workers within the same firm that specialized in the same technology at the onset of the crisis. Finally, Figure 3 shows graphically that the effect of house price declines on the value of a worker's output is also monotonic, as with our other measures, and economically meaningful. For example, workers in the bottom quartile of house price changes produce output worth approximately 15% less than workers in the top quartile.

4.6 Aggregation to the Firm Level

Given that workers who suffer declines in the value of their house also produce less innovative output, a further question is whether such declines in worker-level output translate into declines in firm-level innovative output. It is possible that firm-level output would be unaffected if firms were able to simply shift work from individuals who had house price declines to individuals who did not. On the other hand, it may be difficult to effectively shift work among employees, particularly due to the high degree of specialized knowledge and expertise required for the type of innovative work that we study (Hall and Lerner, 2010).

Ideally, we would like to explore whether our baseline results aggregate to the firm level. Unfortunately, there is a trade-off between aggregation and identification in this setting. That is, firm-level analysis suffers from more severe endogeneity issues than worker-level analysis that compares productivity within firms. As discussed earlier, firms located in more crisis-affected areas may produce less innovative output for reasons unrelated to the house price declines of their employees. For example, such firms may suffer greater declines in demand, borrowing capacity, or innovative opportunities.

Nonetheless, to the extent possible, we attempt to shed light on whether our baseline results are likely to aggregate. To do so, we measure a firm's employee-related exposure to the crisis as the average house price decline of a firm's pre-crisis innovative employees, based on where those employees owned houses in 2007. From the patent data, we can observe the location of firms separately from that of their employees. We therefore compare firms located in the same CBSA, but with different employee-related exposure to the crisis.

Specifically, we estimate firm-level regressions analogous to Equation 1, with CBSA fixed effects based on firm locations. The outcome variables in this analysis represent the innovative output of the entire firm, regardless of the worker credited with the output. In particular, these outcome variables include the output of all innovative workers in a firm, including those who were not matched to a house prior to the crisis, as well as those who joined the firm after the crisis. Thus, if firms' existing (or new) employees are able to compensate for those suffering from house price declines, we would not expect to find an effect at the firm level. However, as shown in Appendix Table A.8, we do continue to find similar results at the firm level.

Of course, one could argue that, even among firms located within the same CBSA, those with employees residing in more crisis-affected zip codes may be different in other ways that account for their lower innovative output during the crisis. Nonetheless, we view these results as quite suggestive that our baseline results do likely aggregate to the firm level.

5 Heterogeneity

In this section, we explore whether the effects we have documented vary with how much home equity workers had accumulated prior to the onset of the crisis. We also explore whether the effects vary with workers' 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 worker 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 declines on worker output, 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 worker entered the crisis with. We proxy for this by exploiting the timing of when workers bought their houses. Workers 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.[14]

We estimate Equation 3, this time interacting house price shocks with an indicator equal to one if the worker bought their house prior to 2004. 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 workers who choose to live in different zip codes by taking advantage of the fact that two workers who live in the same zip code may respond differently to the same house price shock, due to having different accumulated home equity.[15] Table 8 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 workers who bought their house earlier, and thus were likely to have accumulated more equity.

[14] 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 innovative output. Therefore, we prefer to simply proxy for home equity with the timing of the purchase.

[15] 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. However, we are able to include separate firm fixed effects and zip code fixed effects.

5.2 The Impact of Outside Labor Market Opportunities

Next, we examine how our results depend on workers' outside labor market opportunities. To do this, we classify workers as specializing in widely-used technologies or narrowly-used technologies. Presumably, there is a thicker labor market for innovative workers specializing in widely-used technologies, making it easier for them to find another job if necessary. Specifically, we define a worker's field of specialty based on the modal technology class of the worker's patents in the three years leading up to the crisis. We classify a technology class as popular if it is in the top quartile in terms of the total number of innovative workers in the population specializing in that technology over the same time period.[16]

We then estimate Equation 3, which interacts house price shocks with the popular technology indicator, which proxies for labor market thickness. Again, we are also able to include zip code fixed effects in this specification, which further helps to address selection concerns. Table 9 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 workers who 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 Discussion of Potential Channels

The primary contribution of this paper is to show a link between household balance sheet shocks and worker productivity. Understanding the existence and direction of this link is critical to understanding the full scope of the effects of such shocks on firms and the economy as a whole. While determining the precise channel through which the effects that we document operate is more difficult, we conclude with a discussion of potential channels that appear to fit with our findings.

It is possible that the declines in productivity that we observe are driven purely by declines in household wealth. Alternatively, debt and associated financial distress issues may play an important

[16]We find similar results if we classify a technology class as popular based on the number of patents in that technology class or the number of firms with any patents in that technology class.

role in driving the results. Our heterogeneity results appear more consistent with the presence of a debt-related channel. The literature on housing-related financial distress shows that distress is usually triggered by two conditions in combination: negative home equity and a prolonged unemployment spell (Foote et al., 2008, 2010). Therefore, individuals who are more likely to become underwater or to become unemployed for an extended period of time are at greater risk. Our heterogeneity results show that the effect of negative housing shocks on worker productivity is stronger precisely for those at greater risk of financial distress along either of these dimensions.

While the innovative workers in our sample are unlikely to be poor, they still face the possibility of financial distress. The expansion of credit in the run-up to the crisis was not solely confined to the poorest households (e.g., Mian and Sufi, 2016; Foote et al., 2016; Adelino et al., 2016). The mortgage debt of high-income households also significantly expanded between the years 2001–2007. As a result, high-income households also became underwater during the crisis and were subject to the associated risk of financial distress. 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. 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.

There are at least two ways in which the risk of financial distress may affect productivity. First, highly levered individuals at risk of financial distress may cut back on spending, as shown by Mian et al. (2013). This may include decreased spending on things that are important for productivity in wage employment, such as market goods that substitute for home production. In addition, beyond home production, workers may also cut back spending on other inputs into their productivity, such as health, education, and professional development. The idea that individuals substitute home production for market goods goes back to Becker (1965).[17] A large literature explores the link between home production and the business cycle (See, e.g., Benhabib et al., 1991; Greenwood and

[17] Examples of goods where it may be possible to substitute home production for market purchase include child care, home/auto maintenance (housecleaning, lawn mowing, painting, repairs, and the like), food preparation, and financial services such as preparation of income tax returns.

Hercowitz, 1991; Baxter and Jermann, 1999; Campbell and Ludvigson, 2001). More recently, Aguiar et al. (2013) show that time spent on home production increased during the Great Recession.

Moreover, 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 there is a social stigma attached to defaulting on one's debt obligations.[18]

Given such costs, a second possible channel is that individuals at risk of financial distress may experience increased stress, anxiety, and distraction, which in turn may make them less productive. Indeed, Currie and Tekin (2015) find that the risk of housing-related financial distress during the crisis is associated with declines in health. Dobbie and Song (2015) also finds that, in general, financial distress has negative health consequences. Using daily survey data from Gallup, Deaton (2012) finds that Americans reported sharp increases in worry and stress, and sharp decreases in their life evaluation and positive affect during the financial crisis. Engelberg and Parsons (2016) show that there is a strong inverse link between daily stock returns and hospital admissions, particularly for psychological conditions such as anxiety, panic disorder, and major depression. Given the above evidence, it is plausible that the risk of financial distress may directly lead workers to become less productive, especially on new, highly exploratory projects which are cognitively intensive. This is exactly what we find.

As a final way of exploring the mechanism underlying our main results, we also examine whether positive wealth shocks increase productivity in the same way that negative wealth shocks decrease productivity. To examine the impact of positive wealth shocks, we repeat our analysis during the boom period leading up to the crisis, between 2002 and 2007. As shown in Table 10, we find no

[18]Using survey data, Guiso et al. (2009) find that, after relocation costs, the most important determinants of strategic default are moral and social considerations.

statistically significant relation between house price changes and innovative output during the boom. While by no means definitive, this asymmetry is again consistent with an underlying debt-related mechanism, as negative shocks might trigger financial distress, while positive shocks lead to no change in financial distress.

7 Conclusion

In this paper, we investigate whether household level shocks impact worker output in firms through the lens of technological innovation. The household level shocks that we focus on are changes in housing wealth experienced by innovative workers during the financial crisis. Throughout the paper, we compare individuals who 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 workers 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. Using the methodology of Kogan et al. (2017), we show that these declines in innovative output translate into lower economic value for the firm.

Our findings are most consistent with the hypothesis that negative housing wealth shocks lead to decreased innovative output due to heightened concerns among workers about the possibility of mortgage default. Consistent with this hypothesis we find that the effects are less pronounced among workers that are at a lower risk of facing mortgage default. That is, we find that housing wealth shocks particularly affect the productivity of workers who had little equity in their house before the crisis and workers with fewer outside labor market opportunities. These results may also be of interest to policymakers concerned with macro-prudential 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 workers also have a significant impact on the types of innovative projects a firm successfully pursues, highlighting the role of lower ranked workers in influencing firm innovation.

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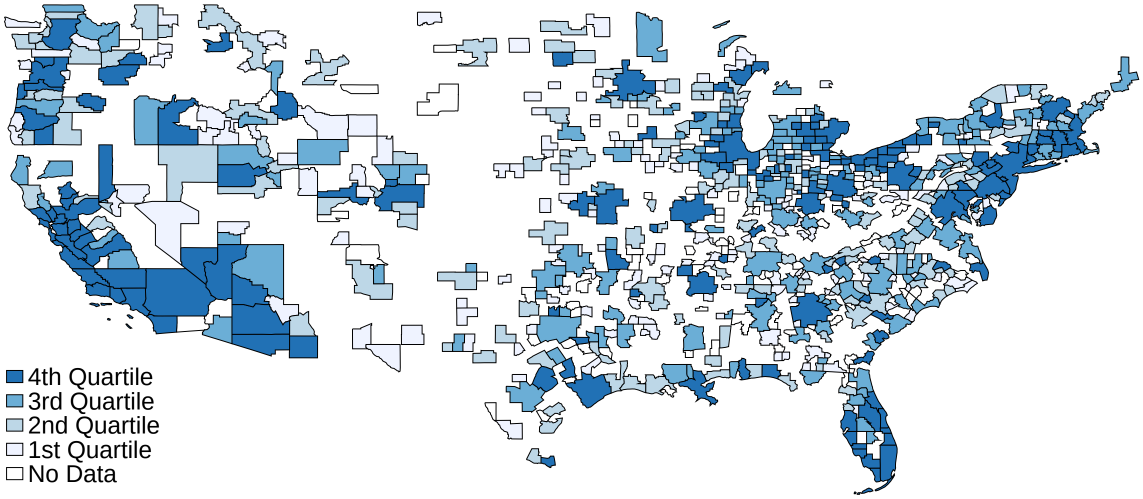
Figure 1
House Price Variation and Innovative Worker Location

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

(a) **Local House Price Variation**



(b) **Number of Innovative Workers by Location**

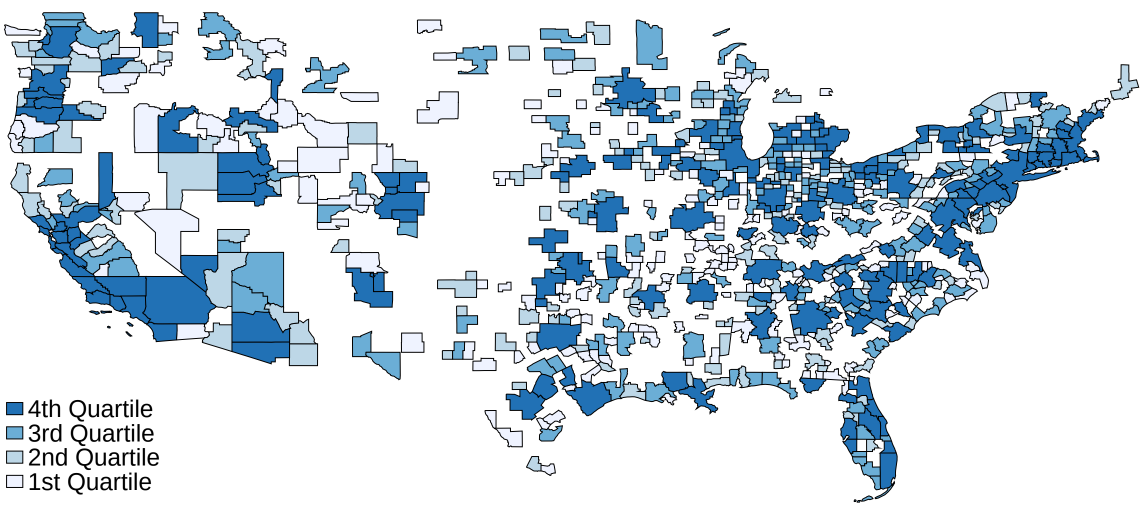
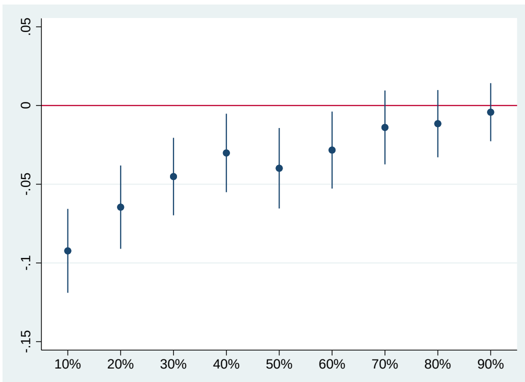


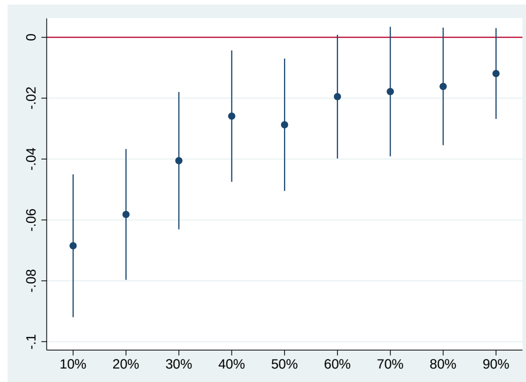
Figure 2
Treatment Intensity

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 the omitted category. The omitted category is the 10th decile (highest percentage change). Confidence intervals are at the 5% level.

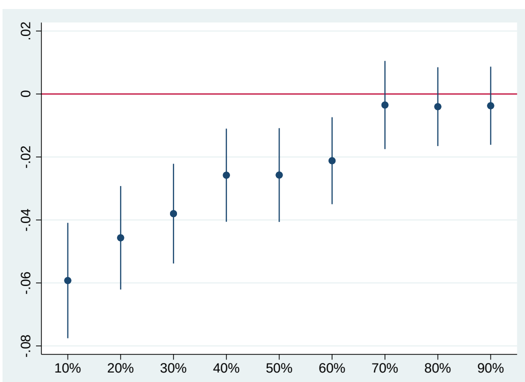
(a) **Number of Patents**



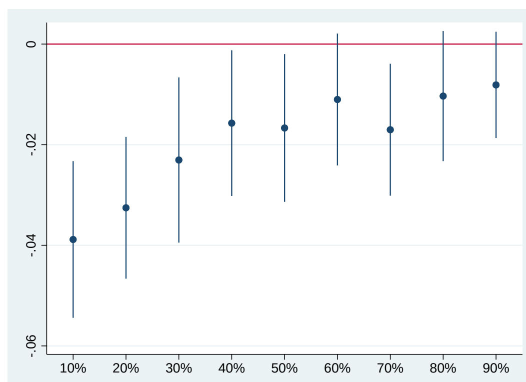
(b) **Citations per Patent**



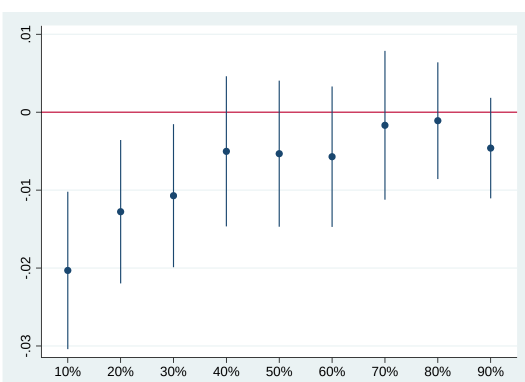
(c) **Originality**



(d) **Generality**



(e) **New Technology**



(f) **Exploratory Patents**

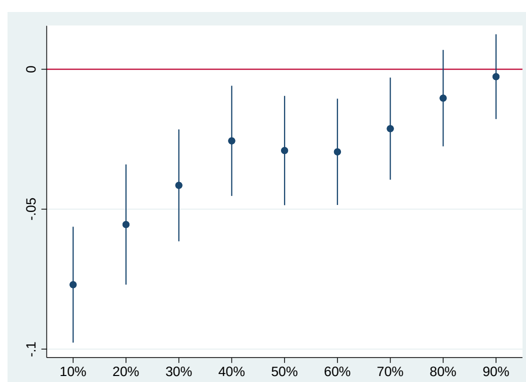


Figure 3
Value of Innovative Output

This figure repeats the analysis of Table 7, but separating the variable %Δ *House Price* to 10 decile dummy variables, and plots these estimates. The dependent variable is the log of firm value associated with all patents produced by a worker during the crisis. The value of each patent is estimated following the methodology of Kogan et al. (2017). This measure is based on the stock market's reaction to the announcement of a particular patent grant. The specification includes firm by CBSA fixed effects, and controls for the value of a worker's output in the pre-period. The graph reports estimates of the 9 house price change deciles, relative to the omitted category. The omitted category is the 10th decile (highest percentage change). Confidence intervals are at the 5% level.

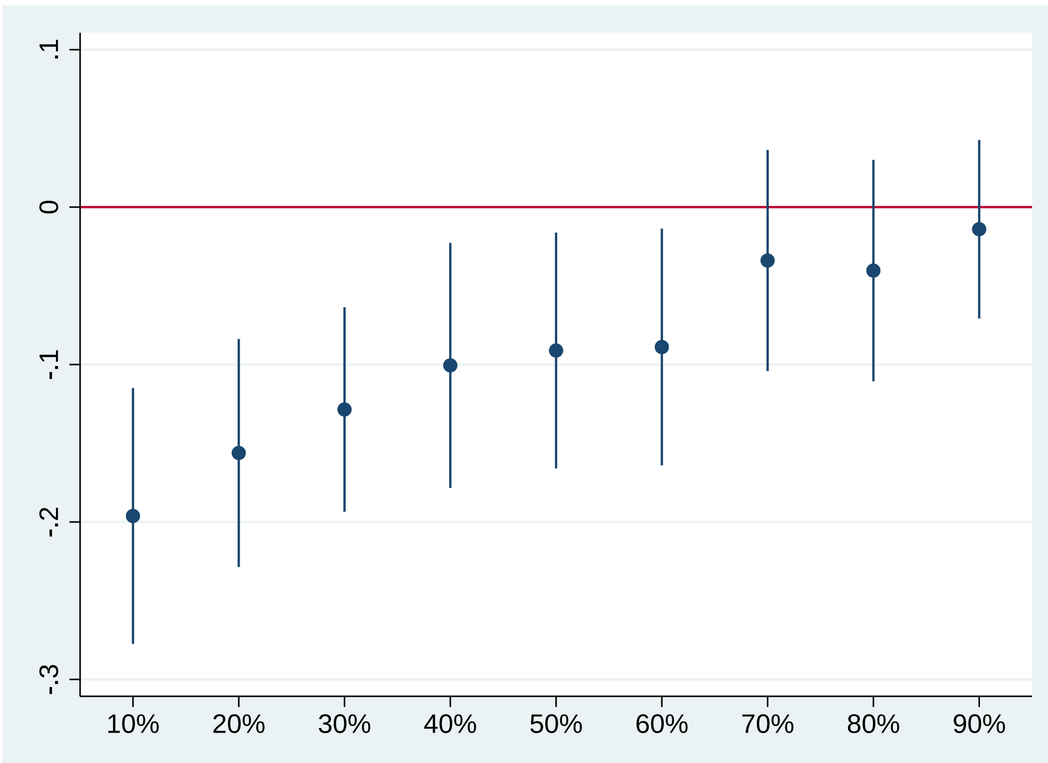


Table 1
Summary Statistics

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 a worker during the period. *Normalized Citations Per Patent* is the total number of normalized citations received by a worker'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 a worker'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 equal to one if any of the worker's patents were in a technology class the worker's firm has never patented in before. *Number of Exploratory Patents* counts the number of a worker'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 worker'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 a worker'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 workers in our sample as of 2007. The *Degree* variables are dummy variables equal to one if the worker holds the stated degree (workers can have multiple degrees). The variable *Age* is defined as 2007 minus the year the worker first obtained a degree plus twenty-two. The variable *Work Experience* is equal to 2007 minus the start year of the worker's first work position. The variable *Tenure at Firm* is equal to 2007 minus the start year of the worker's 2007 work position. The variable *Senior Position* is an indicator equal to one if the worker'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 workers in our sample as of 2007. The variable *Years Owned House* is the years the worker had owned the house as of 2007, *Square footage* is the size of the worker'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 worker'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 worker's house from the end of 2007 to the end of 2012.

Panel C: Worker 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: Worker 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 workers across fields. Workers are categorized using their modal NBER technology subcategory for patents applied from 2005–2007.

Panel E: Distribution of Workers 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 patent 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 worker 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 patent 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 patent 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 worker 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 patent 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 patent 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 worker 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

Workers Remaining at Same Firm

This table repeats the analysis of Tables 2–3, limiting the sample to workers 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 × 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 worker'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 worker's first patent (as of 2007). *Age Q.* are quartiles based on the number of years since the worker's first degree (as of 2007), plus twenty-two. *Education* represent the worker's highest degree as defined in Panel A of Table 1. *Senior Position* is an indicator equal to one if the worker'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 workers with available information on LinkedIn. Standard errors are clustered by firm and worker 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
Value of Innovative Output

This table estimates the effect of changes in zip code level house prices on the value of workers' innovative output for workers that own a house. The value of each patent is estimated following the methodology of Kogan et al. (2017). This measure is based on the stock market's reaction to the announcement of a particular patent grant. The pre-period is defined as 2005–2007. The post-period is defined as 2008–2012. The sample consists of US patent inventors within publicly-traded 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 worker residential zip code. *,**, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

	Log(Value Post)			
	(1)	(2)	(3)	(4)
%Δ House Price Post	0.525***	0.525***	0.441***	0.439***
	(0.0843)	(0.0836)	(0.0921)	(0.0918)
%Δ House Price Pre		0.0000579		0.0819
		(0.163)		(0.166)
Pre-2008 Measure	0.678***	0.678***	0.703***	0.703***
	(0.0270)	(0.0270)	(0.0262)	(0.0262)
Firm × CBSA FE	Yes	Yes	No	No
Firm × CBSA × Tech Class FE	No	No	Yes	Yes
R^2	0.137	0.137	0.146	0.146
Observations	85,362	85,362	85,362	85,362

Table 8
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 worker's house was purchased prior to 2004. Standard errors are clustered by firm and worker 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 × Purchase before 2004	-0.0985***	-0.0636**	-0.0361*	-0.0341**	-0.0407**	0.00348	-0.00889
	(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 9

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 workers to a technology class based on the modal technology class they patented in during the three years before the crisis (2005–2007). A worker is considered to specialize in a popular technology if the worker's technology class is in the top quartile in terms of number of total workers. Standard errors are clustered by firm and worker 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 10
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 patent 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 worker 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 × 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 Supplemental Figures and Tables

Figure A.1
Distribution of House Price Changes
This figure shows the mean percentage change in house price by decile for workers in our sample.

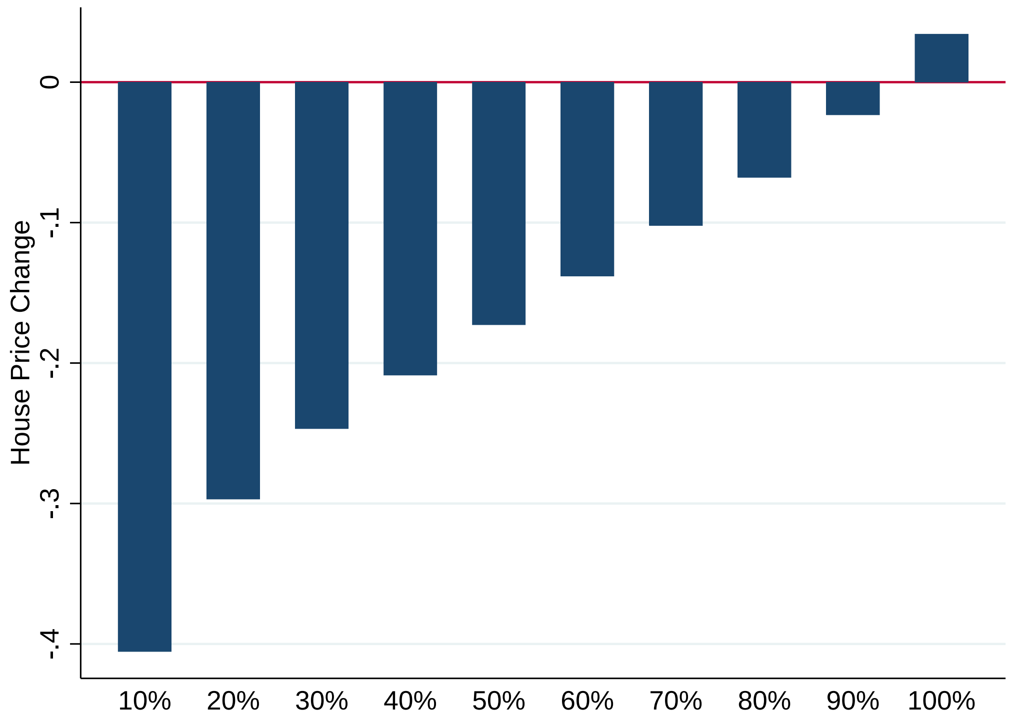


Table A.1
Excluding 2008-2009 Patents
This table repeats the analysis of Tables 2–3, now excluding patents applied for in 2008-2009 from the outcome variables. Standard errors are clustered by firm and worker residential zip code. *,**, and *** denote statistical significance at the 10%, 5%, and 1% level, 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 × 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 worker 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 × 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 worker 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 worker 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 worker 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 innovative workers in the sample, respectively. Standard errors appear in parentheses and are clustered by firm and worker 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)

Table A.7
Public Subsample

This table repeats the analysis of Tables 2–3, now limiting the sample to workers who worked at a public firm during the pre-period. Standard errors are clustered by firm and worker 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	0.224***	0.182***	0.137***	0.105***	0.172***	0.0343***	0.185***
	(0.0389)	(0.0295)	(0.0223)	(0.0208)	(0.0240)	(0.0121)	(0.0282)
Pre-2008 Measure	0.803***	0.210***	0.419***	0.122***	0.182***	0.0776***	0.289***
	(0.0270)	(0.0125)	(0.0185)	(0.00642)	(0.00934)	(0.00618)	(0.0130)
Firm × CBSA FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.308	0.048	0.163	0.022	0.009	0.008	0.086
Observations	85,362	85,362	85,362	85,362	85,362	85,362	85,362

Table A.8
Firm-Level Aggregation

This table estimates firm-level regressions analogous to Equation 1, with CBSA fixed effects based on firm locations. We observe the location of firms separately from that of their employees. We measure a firm's employee-related exposure to the crisis as the average house price decline of a firm's pre-crisis innovative employees, based on where those employees owned houses in 2007. The outcome variables in this analysis represent the innovative output of the entire firm, regardless of the worker credited with the output. Standard errors are clustered by firm CBSA. *,**, 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	0.250***	0.0256	0.0516**	0.0292	-0.0107	0.0827***	0.0894**
	(0.0425)	(0.0240)	(0.0241)	(0.0187)	(0.0219)	(0.0281)	(0.0398)
Pre-2008 Measure	1.041***	0.282***	0.796***	0.173***	0.282***	0.441***	0.794***
	(0.00682)	(0.0100)	(0.00814)	(0.00751)	(0.0107)	(0.0106)	(0.0126)
CBSA FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.589	0.148	0.519	0.102	0.072	0.192	0.460
Observations	31,327	31,327	31,327	31,327	31,327	31,327	31,327