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Do Household Wealth Shocks Affect Productivity? Evidence from Innovative Workers During the Great Recession

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ABSTRACT

We investigate how the deterioration of household balance sheets affects worker productivity, and in turn economic downturns. Specifically, we compare the output of innovative workers who experienced differential declines in housing wealth during the financial crisis but were employed at the same firm and lived in the same metropolitan area. We find that, following a negative wealth shock, innovative workers become less productive and generate lower economic value for their firms. The reduction in innovative output is not driven by workers switching to less innovative firms or positions. These effects are more pronounced among workers at greater risk of financial distress.

OVER THE PAST SEVERAL DECADES, the annual proportion of households in the United States experiencing a severe economic loss has been steadily increasing, peaking with the recent financial crisis (Hacker et al. (2014)). Aggregate deterioration of household balance sheets has been shown to reduce consumption and in turn employment (Mian, Rao, and Sufi (2013), Mian and Sufi (2014)). While these demand-side effects have been well documented, in this paper we explore whether there is also a supply-side channel through which household wealth shocks affect the economy during significant

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downturns. In particular, we investigate whether negative household wealth shocks affect worker productivity.

Theory provides little guidance on whether household wealth shocks increase or decrease worker productivity. On the one hand, negative wealth shocks may make workers more productive by increasing their desire to maintain job security or to make up for lost wealth (Rizzo and Zeckhauser (2003), Goette, Huffman, and Fehr (2004), Disney, Gathergood, and Henley (2010), Cesarini et al. (2017)). If these forces dominate, supply-side effects would help mitigate the impact of economic downturns. On the other hand, negative wealth shocks may make workers less productive by increasing their psychological distress and decreasing the personal resources they have available to support their productivity in wage employment.[1,2] If these forces dominate, supply-side effects would amplify the impact of economic downturns. Ultimately, the effect of household wealth shocks on worker productivity is an empirical question.

We shed light on this question by examining the output of innovative workers during the wake of the 2008 financial crisis. To do so, we construct a novel data set that links the output of innovative workers with deed records that provide information on their housing transactions. This allows us to examine the productivity response of individuals who experienced major declines in housing wealth during the 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 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

[1] A growing literature documents that wealth losses and financial distress can negatively impact health, in part through increased stress, anxiety, and depression (e.g., Deaton (2012), McInerney, Mellor, and Nicholas (2013), Currie and Tekin (2015), Dobbie and Song (2015), Yilmazer, Babiarz, and Liu (2015), Boen and Yang (2016), Engelberg and Parsons (2016)). Moreover, clinical studies and surveys show that such mental states are associated with cognitive impairment and performance difficulties (e.g., Schaufeli and Enzmann (1998), Maslach, Schaufeli, and Leiter (2001), Stewart et al. (2003), Linden et al. (2005)). A key contribution of our paper is to measure directly the workplace productivity effects of household wealth shocks.

[2] Dealing with the consequences of negative wealth shocks may require substantial time and energy (e.g., by triggering cumbersome debt collection, bankruptcy, or foreclosure processes). Workers with less wealth may also allocate more time and energy to home production as a substitute for purchasing market goods (see, for example, Benhabib, Rogerson, and Wright (1991), Greenwood and Hercowitz (1991), Baxter and Jermann (1999), Campbell and Ludvigson (2001), Aguiar, Hurst, and Karabarbounis (2013)). Under both scenarios, less time and energy would be available for productivity in wage employment. In addition, workers may directly cut back spending on health, education, professional development, and other areas that are important for productivity.

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 that workers generate for their firms.

It is important that our analysis is conducted 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, Rao, and Sufi (2013)), or a tightening of financial constraints stemming from the decline in the value of their real estate collateral (Chaney, Sraer, and Thesmar (2012)). It is also possible that firms located in crisis-affected areas simply tend to be ones that had worse innovative opportunities during the crisis period.

By conducting the analysis at the individual level, we are able to compare employees who work at the *same firm*—and are therefore similarly affected by firm-level changes in demand, borrowing capacity, or innovative opportunities—but are exposed to different house price shocks. Since some firms may have multiple divisions scattered across different geographies, we apply an even more demanding analysis by comparing only those employees who both work at the same firm and live in the same metropolitan area, as defined by a census Core Based Statistical Area (CBSA).[3] Despite the fact that we compare workers 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 on 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 projects that are high impact, complex, or exploratory in nature. These results are inconsistent with the view that negative wealth shocks lead workers to become more productive.

To explore the robustness of these findings, we conduct even narrower within-firm comparisons. For example, we compare the relative response of individuals who, in addition to working at the same firm and living in the same metropolitan area, specialize in the same technology at the onset of the crisis.

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

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 of these cases, our key results continue to hold. 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, an inventor who experienced an average house price decline produced 8.4% less valuable output as a result and an inventor whose house price declined one standard deviation more than the mean produced 15.2% less valuable output.

One potential concern with the above analysis is that innovative workers who experienced severe wealth shocks may have been more likely to move to less innovative firms or less innovative positions within their firm. In that case, their innovative output might have declined but their overall productivity, which we cannot observe, may have stayed the same or even increased. Using employment history data from LinkedIn, we show that workers who experienced larger wealth losses were not more likely to change firms or positions within firms. Moreover, our results continue to hold when we restrict our analysis to individuals who held the same position with the same firm throughout the crisis.

A related 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 are able to shift work from individuals who suffer house price declines to those who do not. In our view, such perfect substitutability is unlikely given the high degree of specialized knowledge and expertise required for innovative work (Hall and Lerner (2010)). Nevertheless, to address this question empirically, we conduct an analysis at the firm level, in which we compare firms located in the same CBSA but with different employee-related exposure to the collapse of the housing market based on differences in where firms' employees live. For many of our measures of innovative output, we continue to find similar results at the firm level. Of course, this firm-level analysis suffers from more potential endogeneity concerns than the individual-level analysis. Nonetheless, these results are at least suggestive that our well-identified individual-level results aggregate to the firm level.

This paper is among 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 potential mechanisms.

Negative wealth shocks may make workers less productive by increasing their levels of psychological distress. Indeed, a growing literature documents a link between wealth losses and psychological conditions related to stress, anxiety, and depression (e.g., Deaton (2012), McInerney, Mellor, and Nicholas (2013), Currie and Tekin (2015), Dobbie and Song (2015), Yilmazer, Babiarz, and Liu (2015), Boen and Yang (2016), Engelberg and Parsons (2016)). These conditions have also been shown to be associated with decreased productivity (Schaufeli and Enzmann (1998), Maslach, Schaufeli, and Leiter (2001), Stewart et al. (2003), Kim and Garman (2004), Linden et al. (2005)).

Alternatively, negative wealth shocks may decrease the personal resources workers have available to support productivity in wage employment. For example, negative wealth shocks may lead workers to cut back spending on health, education, professional development, and other areas that are important for their productivity. Wealth losses may also lead workers to divert time and energy away from work. This could be because dealing with wealth losses consumes time directly (e.g., by triggering cumbersome debt collection, bankruptcy, or foreclosure processes) or because wealth losses lead workers to allocate more time to "home production" as a substitute for purchasing market goods (see, for example, Benhabib, Rogerson, and Wright (1991), Greenwood and Hercowitz (1991), Baxter and Jermann (1999), Campbell and Ludvigson (2001), Aguiar, Hurst, and Karabarbounis (2013)).

Wealth shocks may have asymmetric effects under either of these mechanisms. For example, wealth losses may have more of an effect on psychological distress than similar wealth gains due to loss aversion (Kahneman and Tversky (1984)). Similarly, wealth losses may have more of an effect on personal resource constraints than similar wealth gains due to nonlinearities in workers' production functions with respect to personal resources.[4] Motivated by these observations, we repeat our analysis during the housing boom period that preceded the crisis. In this case, we find no relation between house price increases and innovative output during the boom. While by no means definitive, the apparent asymmetry in the effects of wealth shocks on productivity is consistent with our proposed mechanisms.

Wealth losses are likely even more psychologically distressing and resource-draining if accompanied by a significant increase in the risk of financial distress. Therefore, under any of the mechanisms discussed above, one might expect the effects of negative wealth shocks to be even greater if accompanied by increased financial distress risk. The literature on housing-related financial distress shows that it is usually triggered by the combination of two conditions: negative home equity and a prolonged unemployment spell (Foote, Gerardi, and Willen (2008) and Foote et al. (2010)). Consistent with the idea that

[4] In particular, beyond a certain level, additional personal resources may no longer be helpful for productivity in wage employment. Therefore, above that level, increases in personal resources would not increase productivity, but decreases in personal resources might decrease productivity.

financial distress exacerbates the effects of negative wealth shocks, we find stronger declines in innovative output among workers at greater risk along either of these dimensions (i.e., those with less accumulated home equity prior to the crisis and those with fewer outside labor market opportunities based on their field of expertise). However, importantly, we do not interpret these results to mean that financial distress risk is required for wealth losses to affect productivity.

Of course, the innovative workers who we study are likely wealthier than the average worker. This raises the question of whether these workers would have experienced large enough wealth losses during the crisis to really trigger significant psychological distress, resource constraints, or risk of financial distress. To address this question, we employ additional data sources to perform various back-of-the-envelope calculations. Using data from Bell et al. (2016) and the Survey of Consumer Finances, we estimate that a worker in our sample who experienced a mean house price decline lost approximately 17.5% of her total wealth as a result. A worker who experienced a house price decline one standard deviation more than the mean lost approximately 31.5% of her total wealth.[5] Using additional data from CoreLogic, we estimate that 13% of the workers in our sample had a loan-to-value (LTV) ratio in excess of 90% during the crisis, compared to less than 4.8% prior to the crisis. Finally, using data from Adelino, Schoar, and Severino (2016), we find that approximately 9.8% of mortgages in the zip codes in which the workers in our sample live were delinquent or in foreclosure during the crisis, compared to only 1.3% prior to the crisis. Overall, these results suggest that it is plausible that patent inventors lost enough wealth during the crisis that their productivity may have been affected via the mechanisms discussed above.

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, Sraer, and Thesmar (2012) show that negative real estate shocks decrease collateral value and reduce the investment of public firms. Adelino, Schoar, and Severino (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 show that house price movements also affect worker productivity within firms.

Another line of research explores the relationship between household leverage and labor supply (e.g., Bernstein (2015), Mulligan (2008, 2009, 2010), Herkenhoff and Ohanian (2011), and Donaldson, Piacentino, and Thakor (2019)). 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

[5] These losses are substantial. We estimate that it would take the typical inventor suffering the mean house price decline approximately seven years to recover. An inventor suffering a house price decline one standard deviation greater than the mean would require approximately 13 years to recover.

individuals who are already employed and the impact of changes in housing wealth on worker productivity within firms.

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). This literature highlights a "top-down" view, whereby firms' profit-driven objectives determine innovation policy that 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, for example, Harhoff (1999), Aghion et al. (2005), Lerner, Sorensen, and Strömberg (2011), Manso (2011), Aghion, Van Reenen, and Zingales (2013), Ferreira, Manso, and Silva (2014), Seru (2014), Bernstein (2015)). In contrast, our findings also highlight the possibility that firm innovation may follow a "bottom-up" process, whereby innovative workers are not merely interchangeable parts, but rather play an important role in producing innovation.

Finally, in a paper that is closely related and complementary paper to ours, Maturana and Nickerson (2020) show that teacher bankruptcy is associated with lower student performance on standardized tests. Our paper differs in that, rather than studying productivity around bankruptcies, we study productivity around plausibly exogenous wealth shocks that come from changes in house prices. We also study a different measure of productivity—innovative output rather than student performance. Finally, in our setting we can observe the productivity of individual workers, while Maturana and Nickerson (2020) observe student performance at the campus-grade level.

The rest of the paper proceeds as follows. Section I describes the data. Section II details our empirical strategy. Section III presents our results. Section IV discusses potential mechanisms. Section V concludes.

I. Data

A. 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 a patent's application date, the date when the patent was 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 that the data present is that they lack consistent identifiers for patent inventors and firms. To identify inventors and firms over time, we rely on two large-scale disambiguation efforts. The first is the inventor disambiguation provided by Balsmeier et al. (2015). Their algorithm combines inventor names, locations, coauthors, 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 the data forward based on code that they provide.[6]

The USPTO patent data contain the city and state of residence for patent inventors as of the patent application date. Inventors also provide the USPTO with their full residential address on a signed oath as well as on 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 the zip code coinciding with an inventor's city of residence. To identify the property owned by a patent 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. CoreLogic's coverage start dates vary by state, with high-quality coverage beginning in the late 1980s for some states, such as California, Massachusetts, and Illinois, and in the early to mid-1990s for other states.[7] We can construct the ownership history of a given house during this coverage period.[8] We use these ownership histories to match an inventor on a patent to a house they owned on the patent application date based on first name, last name, middle initial, city, and zip code. This procedure yields a 52% unique match rate. The unmatched inventors did not own a house, purchased a house before CoreLogic's coverage of their county, were unmatchable due to name spelling irregularities (e.g., nicknames) on their patent application and/or deed, or had a common enough name that they matched to multiple houses in their zip code of residence. 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 firm, but who own houses in different areas within a CBSA. We therefore use the zip code–level price index constructed by Bogin, Doerner, and Larson (2019), 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 price estimates for nontraded houses.[9]

[6] See https://sites.google.com/site/patentdataproject.

[7] CoreLogic has transactions dating back farther, but the coverage is more sparse.

[8] For each transaction, we can observe both the buyer(s) and the seller(s). We therefore know the owner(s) of the house following the first transaction recorded in CoreLogic based on the buyer(s) associated with that transaction. We also know the owner(s) of the house prior to the first transaction recorded in CoreLogic based on the seller(s) associated with that transaction.

[9] See http://www.zillow.com/research/data/.

Together, we construct an annual worker-level panel. In each year we observe a worker's innovative output along with the worker's firm (based on their most recent patent), the worker's house location (based on their most recent patent), and a price index associated with that location. We note 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. 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 applied for at least one patent that was assigned to a firm between 2005 and 2007. There are 321,837 such individuals in the USPTO data. Of these, we are able to identify a house in CoreLogic for 166,421 individuals (52%). After requiring that other key variables be nonmissing (e.g., zip code, house price index), we are left with a final sample of 162,011 workers at 31,327 firms.

Neither the USPTO data nor the CoreLogic data give us detailed demographic characteristics for the workers in our sample. We therefore augment these data with information from LinkedIn. Among other things, LinkedIn provides information on educational background, work history, and job titles, even in nonpatenting years. 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.[10] Using this procedure, we are able to find a LinkedIn profile for 72,681 (45%) of the workers in our sample.

B. 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, Pakes, and Putnam (1998)).[11] Our primary measure of the quantity of an individual's innovative output over a given period is the log of one plus the number of granted patents the individual applied for during that period.

We measure the quality of a worker's innovative output as the log of one plus the average number of (normalized) citations received by the worker's patents from the period of interest (based again on application dates). For a

[10] A profile is considered a match if it is 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: (i) multiple nondictionary words, (ii) a single nondictionary word, (iii) acronyms (e.g., IBM), and (iv) a single dictionary word. We only use data from public profiles, which we view as a nonlogged-in user.

[11] Recent examples include Lerner, Sorensen, and Strömberg (2011), Aghion, Van Reenen, and Zingales (2013), Seru (2014), Bernstein, Giroud, and Townsend (2016).

worker with zero patents during the period, we define her average citations to be zero.[12] Patent citations are important in patent filings since they serve as "property markers" delineating the scope of the granted claims. Hall, Jaffe, and Trajtenberg (2005) show 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. (2017) show that the stock market reaction to patent approvals is a strong predictor of the number of future citations that 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 three-digit technology class. As an alternative measure of the quality of a worker's innovative output, we also use the log of one plus the number of a worker's patents that ended up in the top 10% of all patents from the same year and technology class in terms of citations.

We measure the generality of a worker's innovative output as the log of one plus the average (normalized) generality score of the worker's patents from the period of interest. For a worker with zero patents during the period, we define her average generality score to be zero. The generality score of a patent is defined following Trajtenberg, Henderson, and Jaffe (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 that the sum is the Herfindahl concentration index. Accordingly, if a patent is cited by subsequent patents that belong to a wide range of fields, the measure will be high, whereas if it is cited by patents in a narrow range of fields, the measure will be low. A high generality score thus suggests that the patent had a widespread impact, as it influenced subsequent innovations in a variety of fields. Generality tends to be positively correlated with the number of citations a patent receives. Therefore, we again normalize each patent' s generality score by the mean generality score for all other patents granted in the same year and three-digit technology class. We measure the originality of a worker's innovative output analogously, except based on backward citations (i.e., citations made) rather than forward citations (i.e., citations received).

[12] Thus, a worker with zero patents during the period is considered to have produced innovative output of equal quality as a worker with patents that received zero citations. An alternative measure of the quality of a worker's innovative output over a given period would be the log of one plus the total citations the worker's patents received over that period (see, for example, Jaravel, Petkova, and Bell (2018)). We find qualitatively similar results using this alternative measure (see Internet Appendix Table IA.I; the Internet Appendix may be found in the online version of this article).

We measure how exploratory a worker's innovative output was as the log of one plus the number of "exploratory patents" the worker applied for. Exploratory innovation requires new knowledge, whereas exploitative innovation builds on a firm's existing knowledge (Manso (2011)). To operationalize this concept more directly, we follow Brav et al. (2018) and define a patent as exploratory if less than 20% of the patents it cites are not existing knowledge from the perspective of the worker's firm.[13] We also follow Lin, Liu, and Manso (2020) and define a simple "New class" indicator that is equal to one if a worker applies for a patent 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. One could therefore think of all of the measures above as measures of project difficulty that capture various aspects of employee innovative productivity.

C. Summary Statistics

Panel A of Table I presents summary statistics for the various patent measures described in Section I.B. 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 assigned to 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. 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. Similarly, a top patent is likely to be a very general one as well, that is, cited by a broad set of technologies. These results are consistent with the intuition that highly cited patents are also broad patents, as measured by generality and originality, and are likely to be defined as exploratory patents, as discussed above.

Panel C reports 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, six years of which was at their precrisis (2007) firm. Approximately 48% of inventors held a senior position prior to the crisis.[14]

Panel D reports summary statistics for characteristics of the houses owned by workers in our sample as of 2007. The average house in our sample is nearly

[13] 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.

[14] We define a worker as having 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, and VP.

Table I

Summary Statistics

Panel A of this table reports summary statistics for patent measures used in the analysis. The patent variables are measured over the years 2008–2012. *Number of Patents* is the number of eventually granted patents a worker applied for 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* is 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* is the number of a worker's patents that are exploratory in the sense that less than 20% of the patents that they cite are existing knowledge from the perspective 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 but with respect to backward citations rather than forward citations. Panel B reports the correlation among the patent measures from Panel A. Panel C reports 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). *Age* is defined as 2007 minus the year the worker first obtained a degree plus 22. *Work Experience* is equal to 2007 minus the start year of the worker's first work position. *Tenure at Firm* is equal to 2007 minus the start year of the worker's 2007 work position. *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 or VP). Panel D reports summary statistics for house characteristics of workers in our sample as of 2007. *Years Owned House* is the number of years the worker had owned the house as of 2007, *Square Footage* is the size of the worker's house, *Age of House*is the age of the house in years as of 2007, and *%ΔHouse 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, *%ΔHouse 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 E reports the distribution of workers across technological fields. Workers are categorized using their modal NBER technology subcategory for patents applied for over the period 2005–2007.

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				

(*Continued*)

Table I—*Continued*

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

	Cites	Top	Gen	Orig	New	Explore
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

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

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 & Software	8,081	4.99
Surgery & 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

(*Continued*)

Table I—*Continued*

Panel E: Distribution of Workers Across Fields (2007)

NBER subcategory	Freq	Percent
Materials Processing & Handling	3,251	2.01
Motors, Engines & Parts	3,173	1.96
Electrical & Electronics (miscellaneous)	2,976	1.84
Resins	2,813	1.74
Nuclear, X-rays	2,497	1.54
Organic Compounds	2,253	1.39

30 years old, is 3,000 square feet in size, and had been purchased just less than eight years ago. In terms of price movements, the average worker in our sample experienced a house price decline of 16% from 2007 to 2012. Moreover, this masks significant heterogeneity. A worker whose house price declined one standard deviation more than the mean experienced a decline of 29%. 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 semiconductor devices.

To get a sense of which metropolitan areas drive the variation responsible for our results, Table II lists the 20 CBSAs in which the most workers in our sample are located. Consistent with what one might expect, the five CBSAs with the most patent inventors are San Jose, San Francisco, New York, Seattle, and Boston. However, it is worth noting that the patent inventors we study are fairly geographically dispersed, as 72% of them live outside of these five major innovation hubs.

In column (2), we show the mean percentage house price change that workers in our sample experienced within each CBSA. Across most CBSAs, patent inventors experienced significant declines in the price of their house. On average, patent inventors in San Jose, San Francisco, New York, Seattle, and Boston experienced house price declines of 11.3%, 17%, 18.5%, 23.2%, and 7.7%, respectively. In column (3), we show the standard deviation of the percentage house price changes that workers in our sample experienced within each CBSA. We find that there was also significant variation in the magnitude of house price declines that patent inventors experienced within these metropolitan areas. For example, one standard deviation below the mean, patent inventors in San Jose, San Francisco, New York, Seattle, and Boston experienced house price declines of 24%, 28%, 25.7%, 29%, and 15.7%, respectively. This within-CBSA dispersion in house price changes is important for our identification strategy, which we discuss next.

Table II
Key Regions

This table lists the 20 CBSAs in which the most workers in our sample are located. Column (1) reports the percent of the sample in the CBSA. Column (2) reports the mean percentage house price change that workers in our sample experienced within the CBSA. Column (3) reports the standard deviation of the percentage house price changes that workers in our sample experienced within the CBSA.

Core Based Statistical Area	% Workers (1)	Mean %Δ House Price (2)	St. Dev. %Δ House Price (3)
San Jose-Sunnyvale-Santa Clara, CA	7.49	−11.3	12.7
San Francisco-Oakland-Fremont, CA	5.35	−17.0	11.0
New York-Northern New Jersey-Long Island, NY-NJ-PA	5.28	−18.5	7.20
Seattle-Tacoma-Bellevue, WA	5.05	−23.2	6.08
Boston-Cambridge-Quincy, MA-NH	4.60	−7.72	7.98
Chicago-Joliet-Naperville, IL-IN-WI	3.61	−27.5	7.62
Los Angeles-Long Beach-Anaheim, CA	3.40	−23.4	9.07
San Diego-Carlsbad-San Marcos, CA	2.78	−19.3	8.78
Detroit-Warren-Livonia, MI	2.75	−28.4	7.16
Philadelphia-Camden-Wilmington, PA-NJ-DE-MD	2.68	−14.8	5.02
Minneapolis-St. Paul-Bloomington, MN-WI	2.57	−24.4	5.73
Austin-Round Rock-San Marcos, TX	2.36	0.98	4.02
Dallas-Fort Worth-Arlington, TX	2.24	−2.32	3.25
Houston-Sugar Land-Baytown, TX	2.24	1.21	5.92
Portland-Vancouver-Hillsboro, OR-WA	1.83	−23.4	5.56
Washington-Arlington-Alexandria, DC-VA-MD-WV	1.80	−15.3	11.2
Atlanta-Sandy Springs-Marietta, GA	1.64	−25.9	8.34
Raleigh-Cary, NC	1.50	−7.74	2.13
Phoenix-Mesa-Glendale, AZ	1.39	−41.7	6.31
Cincinnati-Middletown, OH-KY-IN	1.19	−11.5	4.12

II. Empirical Strategy

Our primary interest is in how changes in house prices associated with the 2008 financial crisis affect workers' innovative output. 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 preperiod is defined as 2005 to 2007 and the postperiod is defined as 2008 to 2012. The variable $y_{i,post}$ represents the various patent-based measures of innovative output discussed in Section I.B, including the total number of patents produced by individual i, the number of citations per patent. The variable $\triangle\%HP_{z,post}$ represents the percent change in the house price index during the postperiod 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 the case that those individuals 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, Sraer, and Thesmar (2012)). Finally, it is also possible that the firms located in crisis-affected areas simply tend to be ones that had worse innovative opportunities during this 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 regression specifications. 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, the divisions of a given firm that are in harder-hit regions may tend to be the ones that are affected by changes in local demand or in innovative opportunities. To address this issue, we refine our regression specification further by including firm $\times$ CBSA fixed effects.[15] If firms in our sample have only one "establishment" (i.e., business location) in the area surrounding a given city, these fixed effects will be equivalent to establishment fixed effects.[16] Note that with firm $\times$ 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 zip codes.

This approach has several advantages. First, the workers we compare are likely to be similar, as they operate in the same labor market and face 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 are likely to be subject to the same division-level innovation shocks. Following the

[15] CBSAs are comprised of Metropolitan Statistical Areas (MSA) and Micropolitan Statistical Areas (μSAs). Essentially, CBSAs 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 reside in an MSA or μSA.; 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.

[16] In Table VI, we restrict the analysis to firms that we verify to have only a single establishment within a CBSA based on the Dun and Bradstreet DMI establishment database.

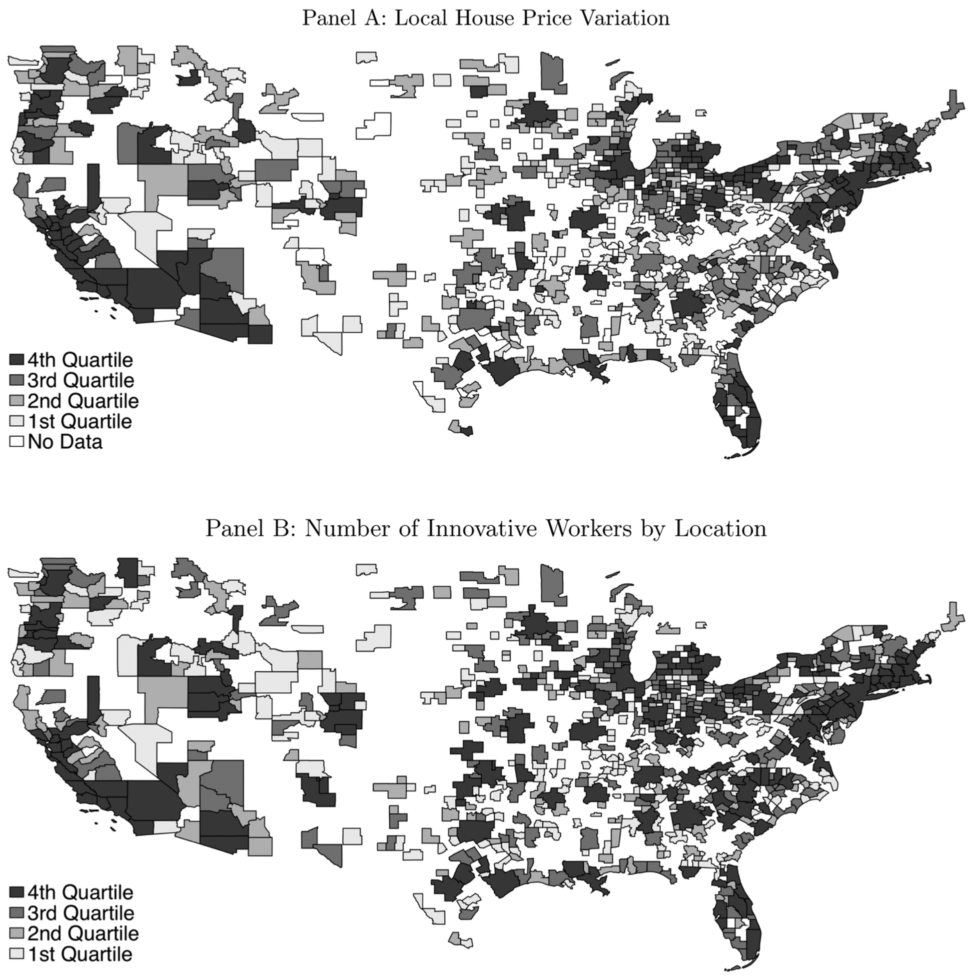

Figure 1. House price variation and innovative worker location. Panel A of this figure shows quartiles of zip-code–level price variance by CBSA. Panel B shows quartiles of the number of innovative workers by CBSA, based on residence.

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 × CBSA fixed effects. Note that with firm × CBSA fixed effects, we 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. As discussed above, Table II shows that within the top 20 CBSAs, there is significant dispersion in the house price changes that innovative workers experienced. Figure 1 illustrates this point further. Panel A shows the dispersion of house price changes in different metropolitan areas, with darker areas representing CBSAs with higher dispersion. Panel B shows that the workers in our sample tend to live in these high-price-change dispersion areas.

Even under the specification in equation (2), however, one may worry that firms may have multiple research establishments within a metropolitan area. To the extent that such research establishments focus on different technologies, we can further refine our specification to address such concerns. In robustness tests, we show that all of our results hold with firm × CBSA × 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 thus is 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 select into neighborhoods that are differentially exposed to the crisis. Such selection 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. For example, we narrow our comparison groups even further by including additional three-way fixed effects that address potential selection concerns. For instance, to address the concern that younger workers might tend to systematically live in the city center, while older workers tend to live in the suburbs, we include firm × CBSA × age cohort fixed effects. Similarly, to address the concern that higher-wage earners sort into richer neighborhoods, we include firm × CBSA × zip code income fixed effects. In Section III.B we provide more details on these specifications and discuss a variety of other such robustness tests. Overall, the results turn out to be quite robust to controlling for observable characteristics in a variety of ways.

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, namely, negative home equity and a prolonged unemployment spell (Foote, Gerardi, and Willen (2008), Foote et al. (2010)). It follows that individuals who are more likely to become underwater or to become unemployed for an extended period of time are at greater risk. Extending 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 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 control for differences across 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 $Characteristic = 0$. Accordingly, we can control for unobservable differences across 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.

III. Results

A. Main Findings

We begin in Table III, Panel A, by estimating variants of equation (2). Standard errors are double-clustered by firm and zip code.[17] One is added to all logged variables. In columns (1) and (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 precrisis period as a control to capture changes in productivity relative to the precrisis baseline. In addition, we also include firm $\times$ CBSA fixed effects, which means that we identify off of variation from workers who are employed by the same firm and own a house in the same metropolitan area but live in different zip codes. Comparing such workers further helps 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 result indicates that a larger decline in local house prices where a worker lives is strongly associated with lower patenting output. In terms of magnitudes, the estimated coefficient implies that a 1% decline in the value of a worker's house leads to a 0.218% decline in the number of patents the worker produces. This suggests that a worker who experienced an average house price decline during the crisis (i.e., a 16% house price decline) produced 3.5% fewer patents as a result. These magnitudes are economically meaningful given the importance of innovation to the economy. For example,

[17] In Internet Appendix Table IA.II, we show that our results are similar if we instead double-cluster standard errors by firm and CBSA.

Table III

Baseline Results

This table estimates the effect of changes in zip-code-level house prices on various measures of innovative output. In Panel A, the dependent variables are the quantity and quality of innovative output. We measure the quantity of an individual's innovative output over a given period as the log of one plus the number of granted patents the individual applied for during that period. We measure the quality of a worker's innovative output as the log of one plus the average number of (normalized) citations received by the worker's patents from the period of interest (based again on application dates). For a worker with zero patents during the period, we define her average citations to be zero. We normalize each patent' s citation count by the average citation count for all other patents granted in the same year and three-digit technology class. The preperiod is defined as 2005–2007. The post-period is defined as 2008–2012. In Panel B, the dependent variables are originality and generality of innovative output. We measure the generality of a worker's innovative output as the log of one plus the average (normalized) generality score of the worker's patents over the period of interest. For a worker with zero patents during the period, we define her average generality score to be zero. The generality score of a patent is defined following Trajtenberg, Henderson, and Jaffe (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. We again normalize each patent' s generality score by the mean generality score for all other patents granted in the same year and three-digit technology class. We measure the originality of a worker's innovative output analogously, except based on backward citations (i.e., citations made) rather than forward citations (i.e., citations received). In Panel C, the dependent variables are related to the exploratory nature of innovative output. We capture how exploratory a worker's innovative output was using the log of one plus the number of "exploratory patents" the worker applied for. Exploratory innovation requires new knowledge, whereas exploitative innovation builds on a firm's existing knowledge (Manso (2011)). To operationalize this concept more directly, we follow Brav et al. (2018) and define a patent as exploratory if fewer than 20% of the patents it cites are not existing knowledge from the perspective of the worker's firm. We also follow Lin, Liu, and Manso (2020) and construct a simple "new class indicator" variable that is equal to one if a worker applies for a patent in a technology class the worker's firm has never patented in before. The sample consists of U.S. patent inventors who are research-active as of the onset of the crisis in 2008 (i.e., who were credited on at least one assigned patent in the preperiod). 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.

Panel A: Quantity and Quality of Innovation

	Log(Number 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

(*Continued*)

Table III—*Continued*

Panel B: Originality and Generality

	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

Panel C: Exploratory Nature

	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 × 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

Hall (2015) shows that technological advancement slowed 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, we find substantial variation in the house price declines experienced by the workers in our sample during the crisis. Our estimates suggest that a worker who experienced a house price decline one standard deviation greater than the mean (i.e., a 29% house price decline) produced 6.3% fewer patents. It is worth noting that these declines in innovation are not a consequence of a change in firm policy, but rather are an outcome of idiosyncratic shocks to employees. In Section III.D, we revisit the interpretation of the magnitudes when we discuss the implications for firm value.

In column (2), we also include the change in house prices that a worker's zip code experienced leading up to the crisis as an additional control. Our main coefficient of interest changes little when controlling for house price appreciation during the run-up to the crisis. In fact, we find that precrisis price changes have no statistically significant relation to patenting during the postcrisis 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 hence to select on ex ante. In Section III.B, we also show that our estimates remain similar after controlling exhaustively for additional worker and house characteristics, which further cuts against a selection story.

In columns (3) and (4) of Table III, Panel A, we examine the effect of house price declines on patent quality as captured by average 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. The estimated coefficient implies that a 1% decline in the value of a worker's house leads to a 0.172% decline in the citations the worker's patents receive. This result suggests that a worker who experienced an average house price decline during the crisis produced patents that received 2.75% fewer citations as a result. A worker who experienced a house price decline one standard deviation greater than the mean produced patents that received 4.98% fewer citations. In columns (5) and (6) we find 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. Thus, declines in housing wealth appear to lead to a decrease in both the quantity and the quality of a worker's output.

In Table III, Panel B, 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 I.B, a high generality score indicates that a patent influenced subsequent innovations in a variety of fields, and a high originality score indicates that a 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, on average, in the postcrisis period. The coefficients imply that a worker who experienced an average house price decline during the crisis produced patents that were 1.47% less general and 2.50% less original as a result. A worker who experienced a house price decline one standard deviation greater than the mean produced patents that were 2.67% less general and 4.52% less original.

Finally, in Table III, Panel C, we investigate whether the patents of workers who experienced larger house price declines during the crisis became less exploratory in the sense that they relied more heavily on the existing knowledge of their firm. In columns (1) and (2), we find that larger house price declines are associated with a reduction in the tendency to patent in a new technology class. The coefficients imply that a worker who experienced an average house price decline during the crisis was 0.78% less likely to patent in a technology class new to the worker's firm. A worker who experienced a house price decline one standard deviation greater than the mean was 1.42% less likely. In columns (3) and (4) we examine whether those living in harder-hit zip codes produce fewer exploratory patents. As discussed in Section I.B, we classify a patent as exploratory if less than 20% of the patent's citations are to other patents granted to their firm or cited by their firm in recent years. The coefficients imply that a worker who experienced an average house price decline produced 3.00% fewer exploratory patents. A worker who experienced a house price decline one standard deviation greater than the mean produced 5.45% fewer exploratory patents. Since all of these 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.

In Figure 2, we explore these effects less parametrically by replacing our continuous house price change variable with house price change decile indicators. The top decile (highest percentage change) is the omitted category.[18] As can be seen, across all of our measures of innovative output, productivity declines the most among those who experience the largest declines in house prices, with the relationship between productivity and house price changes being almost monotonic.

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. These results are inconsistent with the view that negative wealth shocks may lead workers to become more productive.

B. Selection Concerns

As discussed in Section II, concerns about selection issues motivate our empirical design. Specifically, potential unobservable differences both between firms and across geographic regions lead us to include firm $\times$ CBSA fixed effects in our baseline specification. Given these fixed effects, we are effectively comparing individuals who work at the same firm and reside in the same metropolitan area. In this section we explore the possibility that different types of workers within the same firm select into different types of neighborhoods

[18] For context, Internet Appendix Figure IA.1 shows the mean percentage house price change within each decile.

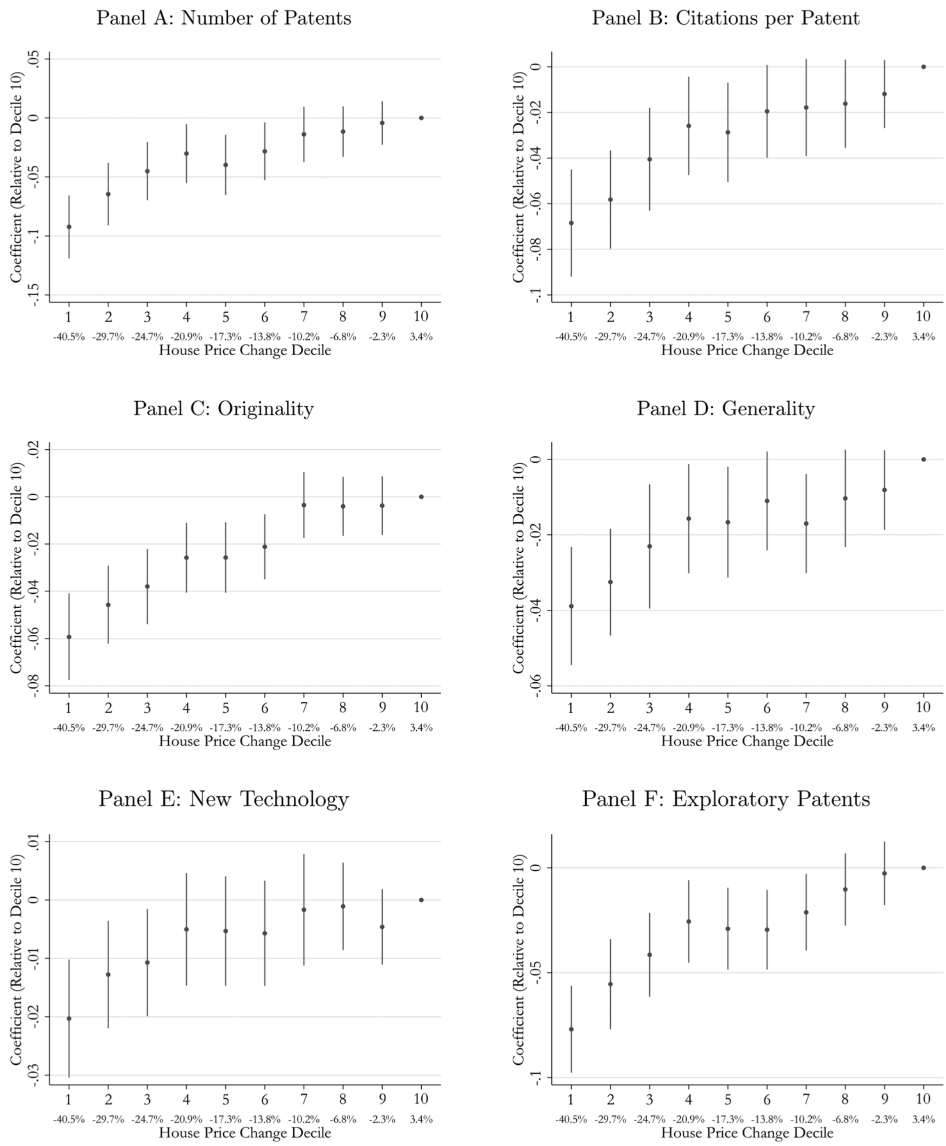

Figure 2. Treatment intensity. This figure repeats the analysis of Table III, but separates the variable *%ΔHouse Price* into 10 decile dummy variables and plots the estimates. The specification includes firm × CBSA fixed effects, and the graphs show estimates of the nine house price change deciles relative to the omitted category, the 10th decile (highest percentage change). The mean percentage house price change within a decile is reported below the decile label. Confidence intervals are at the 5% level.

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 precrisis period, the types who selected into neighborhoods that observed a decline in house prices also had lower productivity during the crisis for reasons unrelated to house prices. We attempt to address such concerns by conducting even narrower within-firm comparisons among workers.

B.1. Three-Way Fixed Effects

We first attempt to address endogeneity concerns using three-way fixed effect specifications (e.g., firm $\times$ CBSA $\times$ worker age quartile fixed effects). In these regressions, we compare workers who are employed at the same firm, live in the same CBSA, and are also similar along one other dimension. These specifications are demanding in the sense that identification comes from comparing groups that are quite narrow.

Table IV reports the estimated coefficient on house price changes using a variety of three-way fixed effect specifications. Overall, the estimated coefficient remains similar with the inclusion of these three-way fixed effects. In some cases, the magnitudes of the point estimates are smaller than the baseline estimates, but the differences are almost never statistically significant. In Internet Appendix Table IA.III, we formally test whether the coefficients that we estimate in Table IV are significantly different from the baseline estimates shown in the first row of each panel.[19] Specifically, Table IA.III shows p-values for the null hypothesis that the coefficient on house price changes is the same in both specifications. We fail to reject this null hypothesis at conventional levels of significance in all but one case.

In sum, while we acknowledge that unobserved worker heterogeneity could account for part of our baseline results, our robustness tests suggest that such heterogeneity is unlikely to be the primary driver. Below we discuss the results in Table IV in more detail.

Technology: One potential concern is that firms may have multiple research establishments (i.e., locations in which research takes place) throughout a metropolitan area. If innovative output declines at establishments in harder-hit areas for reasons unrelated to declines in employee housing wealth, our estimates would be biased. While we think it is plausible that firms tend to have only one research establishment in a given CBSA, it is certainly possible that this is not the case. However, to the extent that firms do have multiple research establishments in a CBSA, we also think it is plausible that

[19] We use the seemingly unrelated regression (SUR) methodology to estimate each three-way fixed effect specification jointly with its corresponding baseline specification, continuing to cluster by firm and zip code. This yields the same point estimates, but gives us a single variance/covariance matrix of the sandwich/robust type. Using this variance/covariance matrix, we then compute p-values for the null hypothesis that our key coefficient of interest is the same in both specifications.

Table IV

Three-Way Fixed Effects

This table repeats the analysis of Table III but allows the firm ×CBSA fixed effects to interact with various other 2007 characteristics. For brevity, only the main coefficient on %Δ *House Price Post* is shown, but the other controls remain the same. *Tech Class* is the modal three-digit technology class of the worker's patents in the pre-period. The variables *Neighborhood Income Q, Square Footage Q, Urban Neighborhood Q, Family Neighborhood Q*, and *Neighborhood Price Level 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 22. *Education* represents the worker's highest degree, and *Senior Position* is an indicator equal to one if the worker's position title includes managerial keywords, both are defined in Table I. Panel A specifications use the full sample, while Panel B specifications use only those 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	Num (1)	Cites (2)	Top (3)	Gen (4)	Orig (5)	New (6)	Explore (7)
	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 × Neighborhood Price Level Q. FE	0.224***	0.181***	0.136***	0.109***	0.147***	0.0438***	0.200***
	(0.0379)	(0.0322)	(0.0257)	(0.0208)	(0.0239)	(0.0141)	(0.0291)
(7) 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)
(8) 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)

(*Continued*)

Table IV—*Continued*

	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)

different establishments specialize in different areas of research. Therefore, to help address concerns about multiple establishments, we include firm × CBSA × worker technology class fixed effects in our regressions. The results are reported in row (2) of Table IV, Panel A. We define a worker's technology class to be the modal three-digit class of the worker's patents in the precrisis period (2005–2007). This specification is conservative in that it only identifies variation across 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) to facilitate comparison.

Neighborhood Characteristics: Next, we compare workers who not only work at the same firm and live in the same CBSA, but 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 that we cannot observe. The first neighborhood characteristic we consider is median income in a worker's zip code, based on the 2000 census. In row (3) of Table IV, Panel A, we sort workers into zip code income quartiles within each CBSA and then include firm × CBSA × 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 median income levels. The next neighborhood characteristic we consider is the percentage of a zip code's population that consists of children (i.e., individuals under the age of 18), again based on the 2000 census. In row (4), we include firm × CBSA × zip code child percentage quartile fixed effects. The census also categorizes zip codes in terms of their urban population percentage. In row (5), we include firm × CBSA × zip code urban percentage quartile fixed effects.

We also use the 2007 level of house prices in a worker's zip code as a proxy for the quality of the worker's neighborhood. In row (6), we include firm × CBSA × precrisis house price quartile fixed effects. Similarly, we make use of the fact that we observe the square footage of a worker's home from CoreLogic. In row (7), we include firm × CBSA × house square footage quartile fixed effects.

In all cases, despite these narrower comparison groups, we continue to estimate similar effects of house price changes on innovative output. These results help address concerns that workers who differed in terms of income or family composition may have selected into harder-hit zip codes and may have also been less productive during the crisis for unrelated reasons.

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 (8) of Table IV, Panel A, we include firm × CBSA × experience quartile fixed effects in our regressions.

Our patent data do not provide information regarding other worker characteristics such as age, education, or job title. We therefore use data from LinkedIn as 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, as shown in row (1) of Table IV, Panel B. In row (2) of Table IV, Panel B, we include firm $\times$ CBSA $\times$ age quartiles fixed effects in our regressions. In row (3) we include firm $\times$ CBSA $\times$ education fixed effects. In row (4) we include firm $\times$ CBSA $\times$ senior position fixed effects.[20]

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

B.2. Additional Functional Forms

The regression specifications estimated in Table IV use three-way fixed effects to compare employees who work at the same firm, live in the same CBSA, and are also similar along one other dimension. These specifications are demanding in the sense that identification comes from comparing groups that are quite narrow. However, this functional form allows us to control for only one additional characteristic at a time. In this subsection we explore the robustness of our results to controlling for multiple characteristics simultaneously.

First, we repeat the analysis of Table IV but include controls for characteristics that are not in the three-way fixed effects. These controls are both continuous and categorical. They consist of worker technology class fixed effects, log median zip code income, zip code child percentage, zip code urban percentage, log zip code house price level in 2007, log house square footage, years worker patenting experience, worker age, worker senior position indicator, and worker education fixed effects. As can be seen in Internet Appendix Table IA.IV, the results remain similar even after including all of these controls on top of the three-way fixed effects. One exception is the estimated effect of house price changes on the probability that an inventor patents in a new technology class, which becomes statistically insignificant, as is the case in some specifications in Table IV.[21]

In Table V we estimate a more traditional "kitchen-sink" specification in which we supplement our baseline two-way fixed effects specification with

[20] We calculate age based on the number of years since the worker's first degree (as of 2007) plus 22. Education fixed effects are based on the worker's highest degree. Finally, 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, and VP).

[21] The lack of statistical significance with this variable may be due in part to the fact that patenting in a technology class that is new to one's firm is a binary outcome and somewhat uncommon. When estimating such demanding specifications, we may simply no longer have sufficient variation to estimate the effect on this indicator variable.

Table V
Baseline Specification with Additional Controls

This table repeats the analysis of Table III, but includes additional control variables. *Controls* include both linear and quadratic variables for job tenure and age (based on LinkedIn), and years of patenting experience (based on the year of the worker's first patent application), the log of zip code house price levels in 2007, and additional zip code characteristics (log median income, child percentage, mean family size, urban percentage) and house characteristics (log square footage and total rooms). *Education FE*are educational degree fixed effects based on degrees listed on LinkedIn (i.e., indicator variables for BA, MA, MBA, JD, MD, PhD). *Seniority FE* and *Function FE* are based on individuals' job titles from LinkedIn. Specifically, we identify 26 common keywords relating to seniority (e.g., "senior" "manager,'" "group," "director," "principal," and "VP") and 32 common keywords related to functional roles (e.g., "product," "marketing," "engineer," "hardware," and "software"). We group variations of the same word together (e.g., "engineer" and "engineering"). We then define seniority levels and functional roles, respectively, based on each combination of these keywords that appear in job titles (e.g., "senior manager" or "hardware engineer"). This gives use 258 seniority categories and 426 functional role categories. *Technology Class FE* are fixed effects based on inventor modal NBER technology subcategory for patents applied for in the preperiod. Standard errors are clustered by firm and worker residential zip code. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

	Num (1)	Cites (2)	Top (3)	Gen (4)	Orig (5)	New (6)	Explore (7)
%Δ House Price Post	0.206**	0.230***	0.151**	0.130***	0.112**	−0.0101	0.176***
	(0.0865)	(0.0701)	(0.0638)	(0.0462)	(0.0474)	(0.0314)	(0.0552)
%Δ House Price Pre	−0.0691	−0.0942	0.00136	−0.0476	−0.0408	−0.0779*	−0.104
	(0.0943)	(0.0699)	(0.0645)	(0.0503)	(0.0560)	(0.0432)	(0.0770)
Pre-2008 Measure	0.746***	0.219***	0.416***	0.122***	0.159***	0.0779***	0.255***
	(0.0262)	(0.0140)	(0.0189)	(0.00841)	(0.0134)	(0.00876)	(0.0133)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Education FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Seniority FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Function FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Technology Class FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm × CBSA FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.525	0.373	0.435	0.328	0.332	0.380	0.367
Observations	44,258	44,258	44,258	44,258	44,258	44,258	44,258

more detailed controls. For example, we parse individuals' job titles from LinkedIn and use these data to define more detailed job seniority fixed effects and functional role fixed effects.[22] We also include new controls for mean zip code family size and total rooms in a worker's house, along with both linear and quadratic controls for job tenure (based on LinkedIn), age (based on LinkedIn), and years of patenting experience (based on the year of the worker's

[22] Specifically, we identify 26 common keywords related to seniority (e.g., "senior," "manager," "group," "director," "principal," and "VP") and 32 common keywords related to functional roles (e.g., "product," "marketing," "engineer," "hardware," and "software"). We group variations of the same word together (e.g., "engineer" and "engineering"). We then define seniority levels and functional roles, respectively, based on each combination of these keywords that appear in job titles (e.g., "senior manager" or "hardware engineer"). This gives use 258 seniority categories and 426 functional role categories.

first patent application). In addition to these new controls, we continue to control for other zip code characteristics (log median income, log house price level in 2007, child percentage, urban percentage), house characteristics (log square footage), and inventor characteristics (education fixed effects and technology class fixed effects). As shown in Table V, we continue to find similar results, although once again the estimated coefficient on the *New Class* indicator becomes statistically insignificant.

B.3. Single Establishment Subsample

It is possible that firms have multiple research establishments throughout a metropolitan area. This would bias our results if innovative output declines at establishments in harder-hit areas for reasons unrelated to declines in employee housing wealth. To the extent that firms do have multiple research establishments in a CBSA, it seems plausible that different establishments may specialize in different areas of research. Therefore, we attempted to address this concern earlier by estimating regressions with firm × CBSA × inventor technology class fixed effects.

To further address this concern, we examine whether our results continue to hold for the subsample of firms for which all establishments within a metropolitan area are in the same zip code. To do so, we rely on data from Dun and Bradstreet's DMI database. As far as we are aware, the DMI data are the most comprehensive establishment data available outside of the confidential census databases (such as the Longitudinal Business Database).[23] The DMI database contains information about business establishments for both public and private firms. Dun and Bradstreet identifies business establishments based on credit inquiries, Department of Motor Vehicle records, newspapers, commercial telephone directories ("yellow pages"), unemployment insurance records, and other public records. These data have been validated against the census (Barnatchez, Crane, and Decker (2017)).

We match the firms in our database to firms in the DMI database based on exact name matching (after some standardization). After matching the two databases, we restrict our sample to inventors living in CBSAs in which their firm has establishments in only a single zip code as of 2007.[24] This restriction implies that inventors living in CBSAs in which their firm has both research and nonresearch establishments (e.g., retail stores, warehouses) are excluded from the sample. Likewise, inventors living in CBSAs in which we are

[23] Another well-known establishment-level database is the National Establishment Time Series (NETS). The NETS provides panel data for establishments based on annual snapshots of the DMI database. For our analysis, a single snapshot from 2007 is all that we require. Therefore, we simply use one snapshot of the DMI database from 2007.

[24] When a firm has multiple addresses in a single zip code, they typically correspond to different buildings on the same "campus."

Table VI

Single Establishment Subsample

This table repeats the analysis of Table III, but limits the sample to inventors living in CBSAs in which their firm has establishments in only a single zip code as of 2007. Establishment data come from Dun and Bradstreet's DMI database. We match the firms in our database to firms in the DMI database based on exact name matching (after some standardization). 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.

	Num (1)	Cites (2)	Top (3)	Gen (4)	Orig (5)	New (6)	Explore (7)
%Δ House Price Post	0.209***	0.198***	0.159***	0.108***	0.167***	0.0819***	0.181***
	(0.0778)	(0.0592)	(0.0404)	(0.0394)	(0.0420)	(0.0308)	(0.0504)
%Δ House Price Pre	0.0822	0.0493	−0.00232	−0.0403	0.133**	−0.000780	0.0423
	(0.127)	(0.0910)	(0.0801)	(0.0584)	(0.0607)	(0.0516)	(0.0871)
Pre-2008 Measure	0.776***	0.235***	0.448***	0.136***	0.175***	0.0959***	0.245***
	(0.0144)	(0.0120)	(0.0172)	(0.00906)	(0.0157)	(0.00915)	(0.0130)
Firm × CBSA FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.281	0.053	0.181	0.025	0.008	0.013	0.063
Observations	28,807	28,807	28,807	28,807	28,807	28,807	28,807

unable to identify any establishments corresponding to their firm are excluded.[25] Table VI shows that the results continue to be similar in this highly restricted sample for which we are confident we are comparing inventors who work not only at the same firm but also at the same establishment.

C. Robustness Tests

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

Excluding 2008 to 2009 Patent Grants: One potential concern with our main analysis is that the patenting process takes time and hence our results may 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, and thus the time it takes to process a patent application should not affect our results. It is possible that there is a lag between when a project is completed and when a firm applies for a patent associated with the project. 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 period, but initiated earlier. However, to the extent that these projects were not completed prior to the onset of the crisis, they may

[25] It is likely that some of the cases for which we find no nearby establishments associated with an inventor's firm are due to our conservative matching procedure, as firms may appear in the two databases under slightly different names (e.g., "IBM" and "International Business Machines").

still have been affected by wealth shocks to individuals leading the projects. Moreover, prior research shows that it generally takes less than a year for a project to result in a patent application (Hall, Griliches, and Hausman (1986)).

Perhaps more importantly, even if there is measurement error in our measures of postcrisis 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 rerun our main specification only including patents in the postcrisis period that were applied for after 2009. The results are reported in Internet Appendix Table IA.V. As can be seen, all of our results continue to hold.

Shorter Time Horizons: We also verify that our main results are driven by the declines in housing prices during the crisis rather than by the subsequent recovery. To do so, we change the time horizon over which house price changes are measured. In our baseline specification, we define postcrisis house price movements based on changes from the end of 2007 to the end of 2012. In Internet Appendix Table IA.VI, we instead define postcrisis house price movements based on changes from the end of 2007 to the end of 2010. In Internet Appendix Table IA.VII, we only consider price movements through the end of 2008. In both cases, similar results obtain. Moreover, in Internet Appendix Table IA.VIII, we find similar results when we examine the effect of price movements through the end of 2008 on patent applications after 2009.

Alternative Price Measures: Our results may also be sensitive to how we define zip-code–level house price changes. To address this concern, in addition to the house price index provided by Bogin, Doerner, and Larson (2019), we use a zip-code–level house price index provided by Zillow. The results are reported in Internet Appendix Table IA.IX. All of our main results continue to go through.

Firm Size: It is also possible that our results are driven by individuals working at a few large firms. To explore whether this is the case, in Internet Appendix Table IA.X we split the sample based on firm size. In particular, we run our main specification separately for individuals working at firms with fewer than 1,000 innovative workers, 100 innovative workers, 50 innovative workers, 30 innovative workers, and 10 innovative workers. We consistently find similar results.

D. Innovative Output and Firm Value

Our evidence thus far indicates that workers who experience significant housing price declines are less likely to successfully pursue innovative projects, particularly those that are high impact, complex, or exploratory in nature. These effects obtain using a variety of detailed patent-based measures. However, a natural question that arises is whether these effects are economically meaningful. Kogan et al. (2017) show that the median patent value to firms is substantial ($3.2 million in 1982 dollars), and that its economic value is strongly correlated with its scientific value, as measured by patent citations.

Table VII

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 who 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 preperiod is defined as 2005–2007. The postperiod is defined as 2008–2012. The sample consists of U.S. patent inventors within publicly traded firms who are research-active as of the onset of the crisis in 2008 (i.e., who were credited on at least one assigned patent in the preperiod). All variables are as defined in Table I. 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

Given our findings that workers affected by the collapse of the housing market produce fewer patents, and patents that are less cited, it is plausible that they generate 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 of 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.[26]

In Table VII we repeat our baseline analysis using the Kogan et al. (2017) measure. More specifically, the outcome variable that we examine is the log of one plus the total value of the patents a worker applied for in the postcrisis period. This measure captures, in dollar terms, changes in both the total quantity and the average quality of a worker's patents. Of course, because we only observe stock price reactions for publicly traded firms, we restrict the sample to employees of such firms for this analysis. Internet Appendix Table IA.XI shows that our baseline results remain similar in this subsample. As can be seen in columns (1) and (2), we find strong, statistically significant effects of house

[26] 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.

price changes on total value. Workers who experience relatively larger declines in housing wealth produce less value for their firms compared to their peers in the same firm and metropolitan area. These effects are also economically meaningful. The estimated coefficient implies that a 1% decline in a worker's house price leads to a 0.525% decline in the value the worker produces. This suggests that a worker who experienced an average house price decline during the crisis (i.e., a 16% house price decline) produced 8.4% less value as a result. A worker who experienced a house price decline one standard deviation greater than the mean (i.e., a 29% house price decline) produced 15.2% less value.

Importantly, these percentage declines in the value of a worker's output due to housing price changes are larger than the percentage declines reported for our other measures in Section III.A. 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 × CBSA × 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. Figure 3 shows graphically that the effect of house price declines on the value of a worker's output is also roughly monotonic.

E. Innovative Productivity versus Occupational Choice

One potential concern is that innovative workers who experience severe wealth shocks may tend to move to less innovative occupations. This could take the form of workers moving to less innovative firms, less innovative positions within a given firm, or even less innovative assignments within a position and firm. For example, innovative workers may derive utility from doing research, but when faced with a negative wealth shock, they may choose to switch to better-paying managerial roles. In this case, their innovative output might decline, but their overall productivity, which we cannot observe, may stay the same or even increase.[27]

To address this concern, we again make use of the data we gathered from LinkedIn. These data allow us to observe job transitions across and within firms. In Table VIII, we examine whether workers who experienced larger house price declines were more likely to change jobs during the crisis, according to their LinkedIn profiles. In Panel A, we reestimate our baseline specification, changing the outcome variable to an indicator equal to one if a worker changed firms in the first one, two, three, four, or five years following the onset of the crisis in 2008. We do not find a statistically significant effect of house price declines on across-firm job changes at any of these horizons. In Panel B, we examine within-firm job changes. Specifically, we repeat the analysis of Panel A, after changing the outcome variable to an indicator equal to one if a worker changed positions within a firm according to the worker's LinkedIn profile. We do not find a statistically significant effect of house price declines

[27] A related concern is that innovative workers who experience negative wealth shocks may be more likely to become unemployed, which would mechanically decrease their productivity.

Table VIII

Job Changes

This table repeats the analysis of Table III, but explores employee mobility. In Panel A, we reestimate our baseline specification, changing the outcome variable to an indicator equal to one if a worker changed firms in the first one, two, three, four, or five years following the onset of the crisis in 2008, respectively. In Panel B, we examine within-firm job changes. Specifically, we repeat the analysis of Panel A, with the outcome variable now being an indicator equal to one if a worker changed positions within a firm according to the worker's LinkedIn profile. In Panel C, we pool across-firm and within-firm job changes by making the outcome variable an indicator variable equal to one if a worker changed either firms or positions within a firm. Standard errors are clustered by firm and worker residential zip code. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Panel A: Firm Change

	1 Year (1)	2 Years (2)	3 Years (3)	4 Years (4)	5 Years (5)
%Δ House Price Post	0.0289	0.0419	0.0496	0.0420	0.0415
	(0.0287)	(0.0342)	(0.0365)	(0.0386)	(0.0402)
%Δ House Price Pre	0.0106	0.0265	0.00540	0.0414	0.0476
	(0.0365)	(0.0439)	(0.0460)	(0.0511)	(0.0607)
Firm × CBSA FE	Yes	Yes	Yes	Yes	Yes
R^2	0.000	0.000	0.000	0.000	0.000
Observations	69,931	69,931	69,931	69,931	69,931

Panel B: Position Change Within Firm

	(1) 1 Year	(2) 2 Years	(3) 3 Years	(4) 4 Years	(5) 5 Years
%Δ House Price Post	−0.00410	−0.0113	−0.0105	−0.00401	−0.00865
	(0.0186)	(0.0258)	(0.0257)	(0.0258)	(0.0249)
%Δ House Price Pre	0.0343	0.0545	0.0408	−0.000123	0.0321
	(0.0342)	(0.0337)	(0.0415)	(0.0428)	(0.0447)
Firm × CBSA FE	Yes	Yes	Yes	Yes	Yes
R^2	0.000	0.000	0.000	0.000	0.000
Observations	69,931	69,931	69,931	69,931	69,931

Panel C: Firm Change or Position Change Within Firm

	1 Year (1)	2 Years (2)	3 Years (3)	4 Years (4)	5 Years (5)
%Δ House Price Post	0.0248	0.0306	0.0391	0.0379	0.0328
	(0.0304)	(0.0380)	(0.0347)	(0.0381)	(0.0383)
%Δ House Price Pre	0.0450	0.0811*	0.0462	0.0413	0.0798
	(0.0433)	(0.0476)	(0.0528)	(0.0496)	(0.0524)
Firm × CBSA FE	Yes	Yes	Yes	Yes	Yes
R^2	0.000	0.000	0.000	0.000	0.000
Observations	69,931	69,931	69,931	69,931	69,931

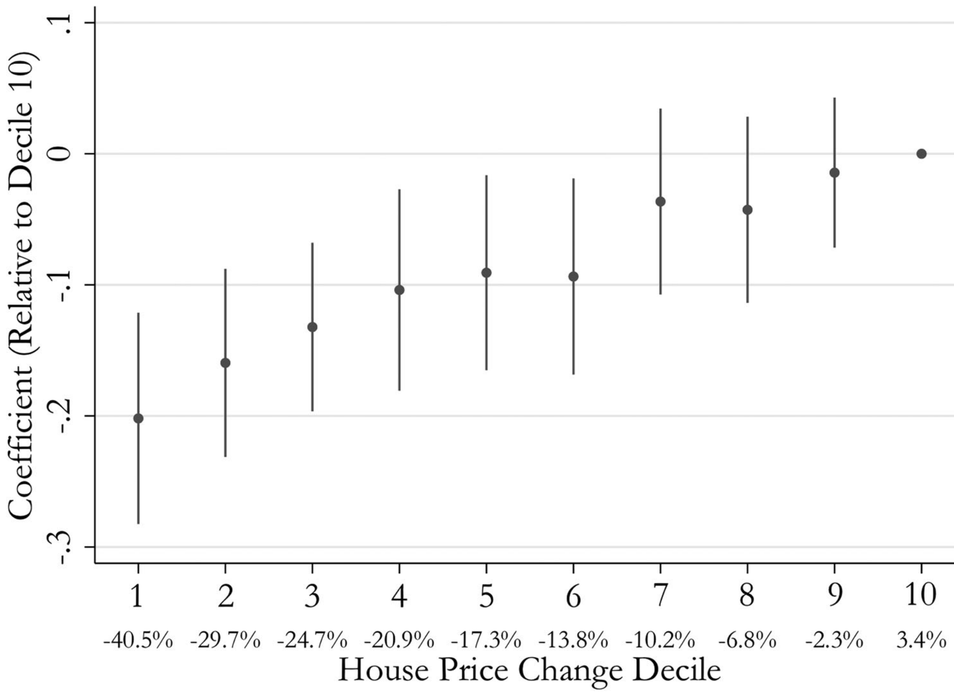

Figure 3. Value of innovative output. This figure repeats the analysis of Table VII, but separates the variable %Δ *House Price* into 10 decile dummy variables and plots the estimates. The dependent variable is the log of firm value associated with all patents produced by a worker during the crisis. 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 × CBSA fixed effects and controls for the value of a worker's output in the preperiod. The graph shows estimates of the nine house price change deciles relative to the omitted category, the 10^{th} decile (highest percentage change). The mean percentage house price change within a decile is reported below the decile label. Confidence intervals are at the 5% level.

on within-firm job changes. Finally, in Panel C we pool across-firm and within-firm job changes by using as the outcome variable an indicator variable that is equal to one if a worker changed either firms or positions within a firm. Using this pooled definition of a job change, we again do not find a statistically significant effect. These results suggest that patent inventors who experienced larger declines in house prices were not more likely to move to different (less innovative) firms or different (less innovative) positions within their firm.

Of course, workers may be able to request less innovative assignments without formally changing positions or firms. However, if house price declines led to such unobservable job changes, they would also likely have led to observable job changes. We do not find any evidence that house price declines led to observable job changes.

Table IX

Workers Who Remain in Same Firm and Position

Panel A of this table repeats the analysis of Table III, but limits the sample to workers who remain in the same pre-crisis (2007) position and firm throughout the post-period (2008–2012) according to their LinkedIn profile. 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.

	Num (1)	Cites (2)	Top (3)	Gen (4)	Orig (5)	New (6)	Explore (7)
%Δ House Price Post	0.321***	0.353***	0.219***	0.202***	0.279***	0.0601**	0.263***
	(0.0824)	(0.0614)	(0.0509)	(0.0442)	(0.0415)	(0.0258)	(0.0657)
%Δ House Price Pre	0.0209	−0.0285	−0.0636	−0.00697	−0.0356	−0.131***	−0.0305
	(0.118)	(0.0863)	(0.0742)	(0.0582)	(0.0752)	(0.0471)	(0.0905)
Pre-2008 Measure	0.824***	0.231***	0.463***	0.140***	0.201***	0.0893***	0.310***
	(0.0263)	(0.0150)	(0.0186)	(0.00800)	(0.0150)	(0.00959)	(0.0152)
Firm × CBSA FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.314	0.056	0.179	0.029	0.012	0.011	0.089
Observations	36,740	36,740	36,740	36,740	36,740	36,740	36,740

One potential weakness of the above analysis is that workers may not keep their LinkedIn profiles up to date. This could bias us toward finding that workers do not change jobs in response to negative housing wealth shocks when, in fact, they do. While we cannot rule out this possibility entirely, it is worth noting that several years elapsed between the end of our estimation window (2012) and when we extracted the data from LinkedIn (2016). In that time frame, it seems plausible that a significant portion of the workers in our sample would have updated their profiles to reflect any job changes that occurred during the estimation window. For across-firm job changes, individuals might find it undesirable to represent themselves online as being associated with a firm that is different from their current employer. With respect to within-firm job changes, evidence from Moallemi, Ramakrishnan, and Shyu (2017) also suggests that individuals report such changes quickly on their profiles—at least conditional upon ever eventually reporting them. Specifically, using internal data from LinkedIn, Moallemi, Ramakrishnan, and Shyu (2017) find that 75% (95%) of those who report a within-firm job change do so within one month (six months) of the position start date.

To further examine whether our results are driven by job changes or declines in worker productivity, in Table IX we also repeat our baseline analysis among those workers who remained in the same position with the same firm throughout the crisis, according to their LinkedIn profile. As can be seen, even within this subsample, we continue to find that those who experienced larger house price declines produced less innovative output. If our baseline results were driven by workers switching to less innovative jobs, we would expect to find no effect within this subsample. Instead, the estimated magnitudes are, if anything, somewhat larger within this subsample (compared to the baseline results for the LinkedIn sample shown in Table IV). This suggests that the decrease in productivity that we document is not due to formal job changes.

Finally, we repeat our baseline analysis among the more successful patent inventors who, based on their precrisis productivity, were less likely to transition to noninnovative jobs during the crisis. In particular, in Internet Appendix Table IA.XII we restrict the sample to the top 50% (Panel A) and top 25% (Panel B) of inventors in terms of average (normalized) citations, based on their patents in the precrisis period. Once again, even within these subsamples of workers who were less likely to have switched to noninnovative jobs, we continue to find that those who experienced larger house price declines produced less innovative output during the crisis. Therefore, overall, we do not find any evidence to suggest that our baseline results are driven by job switching.

F. 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 that arises 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 shift work from individuals who had house price declines to individuals who did not. However, it may be difficult to shift work among the employees we study due to the high degree of specialized knowledge and expertise required for the type of innovative work of interest (Hall, Griliches, and Hausman (1986), Bernstein and Nadiri (1989), Lach and Schankerman (1989), Himmelberg and Petersen (1994)).

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. In particular, 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 the firm's precrisis innovative employees, based on where those employees owned houses in 2007. From the patent data, we can observe firm locations separately from the locations 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 which workers are 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

Table X

Firm-Level Aggregation

This table estimates firm-level regressions analogous to equation (2), with CBSA fixed effects based on firm location. We observe the locations of firms separately from the locations of their employees. We measure a firm's employee-related exposure to the crisis as the average zip-code–level house price decline of a firm's precrisis innovative employees, based on where those employees owned houses in 2007. The dependent variables are defined differently in Panel A and Panel B. In Panel A the dependent variables at the firmlevel are: log(1+total number of patents), log(1+average normalized cites per patent), log(1+total number of top patents), log(1+average normalized generality per patent), log(1+average normalized originality per patent), new technology indicator, and log(1+total number of exploratory patents). In Panel B, the dependent variables are not per-patent averages but rather firm-level totals: log(1+total normalized citations), log(1+total normalized generality scores), and log(1+total normalized originality scores). The outcome variables in this analysis represent the total innovative output of the entire firm, regardless of the worker credited with the output and whether the worker owned a house. Standard errors are clustered by firm CBSA. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Panel A: Baseline Outcomes

	Num (1)	Cites (2)	Top (3)	Gen (4)	Orig (5)	New (6)	Explore (7)
%Δ House Price Post	0.250***	0.253***	0.0516**	0.279***	0.213***	0.0827***	0.0894**
	(0.0425)	(0.0492)	(0.0241)	(0.0429)	(0.0464)	(0.0281)	(0.0398)
Pre-2008 Measure	1.041***	0.764***	0.796***	0.679***	0.989***	0.441***	0.794***
	(0.00682)	(0.00815)	(0.00814)	(0.0139)	(0.00655)	(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

Panel B: Total Outcomes

	Log(Total Cites Post) (1)	Log(Total Generality Post) (2)	Log(Total Originality Post) (3)
%Δ House Price Post	0.253***	0.279***	0.213***
	(0.0492)	(0.0429)	(0.0464)
Pre-2008 Measure	0.764***	0.679***	0.989***
	(0.00815)	(0.0139)	(0.00655)
CBSA FE	Yes	Yes	Yes
R^2	0.491	0.474	0.566
Observations	31,327	31,327	31,327

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.

As shown in the first column of Table X, Panel A, however, we continue to find similar results at the firm level with respect to the quantity of innovative output. That is, among firms within the same CBSA, those with higher employee-related exposure to the crisis produce fewer patents. Interestingly, in column (1), we estimate similar magnitudes at the firm level as we do at the worker level. In columns (6) and (7), we also find that firms with higher employee-related exposure to the crisis produce fewer patents in new technology classes and fewer exploratory patents. However, we do not find evidence that these firms produce patents with fewer average citations or lower average generality/originality.

What explains the discrepancy between the worker-level and firm-level results? First, it is important to note that the two sets of results rely on entirely different identification strategies. The worker-level results are much better identified than the firm-level results. Thus, there is no mechanical relationship between the two sets of results that would require that they line up with one another. For example, it could be the case that firms located near harder-hit areas became more productive during the crisis for reasons unrelated to housing prices. At the same time, declines in housing prices in these areas could lead workers living there to become less productive. Thus, a negative correlational relationship between house prices and productivity at the firm level could cancel a positive causal relationship at the worker level.

It is also worth noting that, although firms with higher employee-related exposure to the crisis do not appear to have produced patents with lower average citations, originality, and generality when we aggregate to the firm level, they do appear to have produced patents with lower total citations, generality, and originality. Specifically, in Table X, Panel B, we replace the average (per patent) measures of productivity with the corresponding total measures of productivity.[28] In column (1), the outcome variable is the log of one plus the total number of (normalized) citations the firm's postcrisis patents received. In columns (2) and (3), the outcome variables are the log of one plus the total (normalized) generality and originality scores of the firm's postcrisis patents, respectively. We find that employee-related exposure to the crisis is associated with lower total productivity along each of these dimensions. Overall, we view the results in Table X as being suggestive that our baseline results largely aggregate to the firm level.

IV. Discussion of Potential Mechanisms

A. Potential Mechanisms

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 balance sheet shocks on the economy. Determining the precise mechanism through which such effects operate is more difficult. In this section, we discuss likely potential candidates.

First, negative wealth shocks may make workers less productive by increasing their levels of psychological distress. Engelberg and Parsons (2016) show that drops in the stock market lead to increases in hospital admissions for

[28] The measures excluded from Panel B are already total measures in Panel A.

psychological conditions related to stress, anxiety, and depression. A growing literature in medicine/psychology also shows a link between wealth losses and such conditions (McInerney, Mellor, and Nicholas (2013), Boen and Yang (2016), Pool et al. (2018)). Most related to our paper, Yilmazer, Babiarz, and Liu (2015) find that drops in *housing wealth* are associated with increases in self-reported psychological distress. Psychological distress may, in turn, reduce cognitive performance and workplace productivity (Schaufeli and Enzmann (1998), Maslach, Schaufeli, and Leiter (2001), Stewart et al. (2003), Kim and Garman (2004), Linden et al. (2005)).[29]

Second, negative wealth shocks may decrease the personal resources workers have available to support their productivity in wage employment. For example, negative wealth shocks may lead workers to cut back spending on health, education, professional development, and other area that are important for their productivity. Wealth losses may also lead workers to divert time and energy away from their job. This could be because dealing with wealth losses is directly time-consuming (e.g., because losses trigger cumbersome debt collection, bankruptcy, or foreclosure processes) or because wealth losses lead workers to allocate more time to "home production" as a substitute for purchasing market goods.[30,31]

In addition, under any of the mechanisms proposed above, one might expect the effects of wealth losses to be greater if accompanied by increased financial distress risk.[32] This is because financial distress is associated with many additional costs for households on top of wealth losses. A family

[29] According to clinical evidence, stressed individuals complain about their inability to focus on their tasks at work (Schaufeli and Enzmann (1998), Maslach, Schaufeli, and Leiter (2001)). Linden et al. (2005) show that stress is associated with reduced ability to focus, significant cognitive deficits, and greater performance variability. Depressive episodes are well understood to have cognitive symptoms, including diminished ability to think and concentrate, as well as learning and memory impairments (Marin et al. (2011)). Stewart et al. (2003) use survey evidence from the Depressive Disorders Study, derived from the American Productivity Audit, to document productivity losses among depressed workers. Kim and Garman (2004) use a survey of white-collar workers to argue that stress due to personal finances is associated with impaired workplace performance, as evident in their tendency to waste work time and to be absent from work more frequently.

[30] The idea that individuals substitute home production for market goods goes back to Becker (1965). Examples of goods for which 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, for example, Benhabib, Rogerson, and Wright (1991), Greenwood and Hercowitz (1991), Baxter and Jermann (1999), Campbell and Ludvigson (2001), Aguiar, Hurst, and Karabarbounis (2013)).

[31] If the marginal hours that innovative workers spend at their jobs have higher value than the marginal hours that they spend on home production, it may not make sense for them to substitute home production for purchasing market goods. However, using data from the American Time Use Survey (ATUS), in Internet Appendix Figure IA.2, we do find that time spent on home production is sensitive to income even at the high end of the income distribution.

[32] Yilmazer, Babiarz, and Liu (2015) highlight this point, illustrating that negative housing wealth shocks are associated with increases in psychological distress, with such effects more pronounced among those households that, as a result of the shock, are at risk of losing their home. Pool et al. (2018) convey a similar message.

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 persistently negatively impacted by a foreclosure, which can adversely affect future employment outcomes (Brevoort and Cooper (2013)). Finally, default on debt obligations may be viewed as immoral or a significant personal failing (Guiso, Sapienza, and Zingales (2009)). Wealth losses may therefore lead to additional psychological distress when financial distress risk is high. Wealth losses may also lead to additional resource constraints if workers cut back spending more or have to divert more time away from their job when financial distress risk is high.

B. Plausibility of Potential Mechanisms

The innovative workers who we study are likely wealthier than the average worker. Therefore, even if house prices did decline in their zip codes, they may not have lost sufficient wealth from these price declines to plausibly trigger psychological distress, constraints on personal resources that support productivity, or financial distress. In this section, we discuss the plausibility of the potential mechanisms described above.

B.1. Did Innovative Workers Lose Significant Wealth?

To explore the magnitude of the wealth losses that inventors experienced, we conduct a simple back-of-the-envelope calculation. As discussed above, the mean house price decline in our sample was 16%, and a one standard deviation larger house price decline in our sample was 29%. These house price declines correspond to even larger housing wealth declines since the workers in our sample also had significant leverage. As we discuss in more detail in the next subsection, we examine how levered the workers in our sample were by imputing their LTV ratios using additional data from CoreLogic. We estimate that the average worker in our sample had an LTV ratio of 54% in 2007. For such a worker, a 16% house price decline corresponds to a 35% (=16%/46%) housing wealth decline. Similarly, a 29% house price decline corresponds to a 63% (=29%/46%) housing wealth decline.

Next, we combine data from Bell et al. (2019) and the Survey of Consumer Finances to estimate the decline in total wealth that such declines in housing wealth translate into. Specifically, we first use data from Bell et al. (2019) to get a sense of the income range for workers in our sample. We then use data from the Survey of Consumer Finances to examine the percentage of total wealth that workers in that income range have in their house.

Bell et al. (2019) provide aggregate Internal Revenue Service (IRS) data on the income distribution of patent inventors of different ages. Using these data, along with the age distribution of the workers in our sample (from LinkedIn), we impute the income distribution for our sample. Internet Appendix Table IA.XIII shows this imputed income distribution. We estimate that the median income among the workers in our sample was \$101K in 2007, while the 30$^{\text{th}}$ percentile of income was \$68K and the 70$^{\text{th}}$ percentile was \$154K. At the high end of the distribution, we estimate that the 90$^{\text{th}}$ percentile of income was \$345K and the 99$^{\text{th}}$ percentile was \$2.13M. Hence, the majority of patent inventors in our sample fall in the middle to upper middle class income ranges, although there are a few extremely wealthy inventors.

According to data from the Survey of Consumer Finances, the median homeowner in patent inventors' income range had approximately 50% of their total wealth in their house in 2007 (see Internet Appendix Figure IA.3).[33] This implies that a patent inventor who experienced an average house price decline would have lost approximately 17.5% (=50% × 35%) of her total wealth as a result, which we estimate would take approximately seven years to recover through saving future income.[34] An inventor whose house price declined by one standard deviation more than the mean would have lost approximately 31.5% (=50% × 63%) of her total wealth, which we estimate would take approximately 13 years to recover through saving future income. These numbers are consistent with the idea that innovative workers experienced significant enough wealth losses to induce psychological distress and constraints on personal resources that support productivity in wage employment.

B.2. Did Innovative Workers Experience Increased Risk of Financial Distress?

To examine whether innovative workers may have worried about financial distress during the crisis, we estimate their LTV ratios using additional data from CoreLogic. Piskorski, Seru, and Vig (2010) show that loans with higher LTV ratios face significantly higher risk of foreclosure. From the CoreLogic data, we can typically observe LTVs at mortgage origination (including refinancings) based on primary loans, but we cannot observe how these LTVs change over subsequent years as mortgage balances are paid down and the value of houses fluctuate. We therefore use an imputation procedure to estimate LTVs during the crisis based on the LTVs at origination that we observe.[35] With these imputed LTVs, we construct a distressed LTV indicator

[33] Using data from the American Community Survey, we estimate that households whose primary earner had income of approximately \$101K in 2007 had total household income of approximately \$130K. This corresponds to the ninth income decile in Internet Appendix Figure IA.3.

[34] To estimate how long it would take to recover lost wealth through future savings, we first use our imputed house prices to compute the dollar mean and standard deviation of wealth lost over the crisis, relative to 2007. We then assume annual household income of \$130K, a personal savings rate of 9.97%, and an annual return on savings of 4%. The 9.97% personal savings rate is based on the mean savings rate from the Consumer Expenditure Survey for households that earn approximately \$130K. Under these assumptions, we then compute how many years it would take to recover lost wealth.

[35] Specifically, we impute loan balances over time using a standard amortization schedule, and we impute home values over time by compounding initial sale prices forward using zip-code-level price changes. We assume a 30-year fixed-rate mortgage with an annual interest rate equal to 5.75%, the average interest rate in our sample over the relevant period.

variable that is equal to one if a worker has an LTV in excess of 90%, a cutoff commonly used in the literature.[36]

We find that approximately 13% of the workers in our sample had an LTV of greater than 90% by the end of our estimation period in 2012. This number represents a 2.7-fold increase from 2007, when only 4.8% of workers in our sample had a distressed LTV. Among workers in our sample who bought their house in the three-year run-up to the crisis (2005–2007), we find that 31.8% had a distressed LTV in 2012, up from 6.3% in 2007, approximately a fivefold increase. It is also worth noting that, since the LTVs that we impute are based on primary loans only and do not include piggyback loans, these LTVs likely understate true LTVs. This is especially the case given that the individuals in our sample are located in areas where the use of piggyback loans is likely relatively common. The fact that we cannot take these piggyback loans into account means that, if anything, we are likely underestimating the frequency of distressed LTVs.

We also confirm that the variation in house price movements that we exploit in our baseline empirical specification is associated with statistically significant increases in the incidence of distressed LTVs. In Table XI, Panel A, we estimate our baseline empirical specification using the distressed LTV indicator as the outcome variable. Comparing individuals working at the same firm and living in the same CBSA, we find that a 1% decline in house prices leads to an increase of about 0.8% in the probability of having a distressed LTV. Given that the average decline in house prices during the crisis is equal to 16%, these estimates imply that the average worker in our sample experienced a 12.8% (=0.8% $\times$ 16%) increase in the probability of having a distressed LTV due to the crisis. A worker whose house price declined by one standard deviation more than the mean experienced a 23.2% (=0.8% $\times$ 29%) increase in the probability of having a distressed LTV.

To further examine whether it is plausible that innovative workers may have worried about financial distress, we examine how common mortgage delinquency and foreclosure were in the zip codes in which they lived. To do so, we use loan payment data from Lender Processing Services (LPS, formerly known as McDash). These are the same data used by Adelino, Schoar, and Severino (2016).[37] Following Adelino, Schoar, and Severino (2016), we study delinquency and foreclosure together, where a loan is classified as delinquent if payments are more than 90 days past due. Using the universe of loans originated since 2002, we compute the percent of active loans by zip code income decile that were delinquent or foreclosed as of the beginning of each year from 2007 to 2010, when the data end.[38] The results are reported in Table XI, Panel B. We find that delinquency/foreclosure rates increased during the crisis

[36] For example, Elul et al. (2010) and Gerardi et al. (2018) find that mortgage default risk increases significantly when households hold negative equity or near-negative equity positions.

[37] The underlying loans represent a 5% sample of loans in LPS originated after 2002.

[38] Following Adelino, Schoar, and Severino (2016), we measure mean household income within a zip code using data from the IRS and data from the Home Mortgage Disclosure Act (HMDA) in 2002. The former covers all households within a zip code, while the latter covers households

Table XI
Leverage and Delinquency/Foreclosure

Panel A estimates the effect of changes in zip-code-level house prices on workers' likelihood of experiencing a loan-to-value (LTV) ratio that is above 90%. An individual's LTV ratio on their house is defined as the balance of all loans secured by the house divided by the value of the house. The sample consists of U.S. patent inventors who are research-active as of the onset of the crisis in 2008 (i.e., were credited on at least one assigned patent in the preperiod). All variables are as defined in Table I. 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. Panel B reports mortgage delinquency/foreclosure rates by zip code income decile. Loan-level data come from Lender Processing Services (LPS, formerly known as McDash). The underlying loans represent a 5% sample of loans in LPS originated after 2002. A loan is defined as delinquent if payments are more then 90 days past due. Using the universe of loans originated since 2002, we compute the percent of active loans by zip code income decile that were delinquent or foreclosed as of the beginning of each year from 2007–2010, when the data end. We measure mean household income within a zip code using data from the Internal Revenue Service (IRS) and data from the Home Mortgage Disclosure Act (HMDA) in 2002. The former covers all households within a zip code, while the latter covers households applying for a mortgage. The zip code income deciles are formed based on IRS income. In the last row of the table, we examine delinquency/foreclosure rates in the specific zip codes in which the patent inventors in our sample are located, weighted by the number of inventors in each zip code.

Panel A: Incidence of Distressed LTV

	Crisis LTV > 90%	
	(1)	(2)
%Δ House Price Post	−0.803***	−0.800***
	(0.0411)	(0.0414)
%Δ House Price Pre		−0.148***
		(0.0462)
Firm × CBSA FE	Yes	Yes
R^2	0.027	0.027
Observations	102,691	102,691

Panel B: Delinquency/Foreclosure Rates

Zipcode			Delinquency/Foreclosure Rate (%)				
Income Decile	IRS Income	HMDA Income	2007	2008	2009	2010	2010/2007
Low	26,941.8	60,774.8	5.52	11.6	18.5	22.4	4.06
2	32,611.7	62,889.4	3.78	8.78	15.5	19.7	5.22
3	35,833.3	67,667.1	3.26	7.63	13.1	17.3	5.31
4	38,851.6	71,095.2	3.05	6.41	11.2	15.6	5.10
5	42,023.2	78,532.3	2.67	6.14	10.6	15.2	5.70
6	45,967.3	85,406.5	2.10	4.84	8.78	13.7	6.55
7	50,577.9	91,324.9	1.89	4.58	8.49	12.2	6.43
8	56,918.5	103,009.0	1.43	3.56	6.92	11.2	7.83
9	67,507.0	122,946.3	1.15	2.80	5.67	9.53	8.31
High	113,310.0	180,057.4	0.81	1.88	3.84	7.13	8.85
Inventor Zip Codes	72,717.3	122,702.5	1.28	2.89	5.68	9.77	7.61

across the income spectrum. Even in zip codes in the highest decile of the income distribution, approximately 7% of active loans in LPS were delinquent or foreclosed in 2010. For zip codes in the second-highest decile, the 2010 delinquency/foreclosure rate was 9.5%. While these rates are, of course, lower than those in zip codes at the bottom of the income distribution, they are still economically significant. Moreover, as shown in the last column, the growth in delinquency rates from 2007 to 2010 was actually largest in the top deciles, since delinquencies were very uncommon in these zip codes prior to the crisis. For example, delinquency rates for the highest income decile increased nearly ninefold from 2007 to 2010.

In the last row of the table, we examine delinquency/foreclosure rates in the specific zip codes in which the patent inventors in our sample are located, weighted by the number of inventors in each zip code. We find that 9.8% of active loans in the LPS data were delinquent or foreclosed in inventors' zip codes in 2010. This represents a dramatic increase of more than 7.5-fold from 2007. Overall, this evidence suggests that there was indeed a substantial increase in financial distress risk among those living in inventors' zip codes. It is also important to note that the increase in delinquency/foreclosure rates in these zip codes may have also affected the productivity of workers who did not ultimately experience delinquency or foreclosure, but believed that they were at greater risk of these types of events. In the next section, we discuss additional empirical tests that are consistent with these mechanisms.

C. Evidence Related to Potential Mechanisms

C.1. Asymmetric Effects

It is possible that wealth shocks have asymmetric effects on psychological distress. For example, due to loss aversion, wealth losses may increase psychological distress more than similar wealth gains decrease psychological distress (Kahneman and Tversky (1984)). Consistent with this idea, McInerney, Mellor, and Nicholas (2013) show that there are no improvements in subjective mental health for stockholders who gained wealth during an earlier stock market boom in the mid 2000s.[39] Fichera and Gathergood (2016) find that housing-related wealth increases do not lead to improved psychological health. Pool et al. (2018) also document asymmetric effects of wealth shocks and mental health.

It is also possible that wealth shocks have asymmetric effects on personal resource constraints. For example, due to nonlinearities in workers' production functions, wealth losses may increase personal resource constraints more than wealth gains decrease these constraints. In particular, beyond a certain

applying for a mortgage. Like Adelino, Schoar, and Severino (2016), we find that HMDA income is significantly higher than IRS income, most likely because home-buying households have higher earnings than non–home-buying households. The zip code income deciles are formed based on IRS income.

[39] This paper reinforces the earlier evidence of Smith (2004).

Table XII
Housing Prices Effects in 2002

This table repeats the analysis of Table III, but estimates the effect of changes in zip-code–level house prices on innovative output for an earlier period. The preperiod is defined as 1999–2001. The postperiod is defined as 2002–2006. All variables are as defined in Table I. 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.

	Num (1)	Cites (2)	Top (3)	Gen (4)	Orig (5)	New (6)	Explore (7)
%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

level, additional personal resources may no longer be helpful for productivity in wage employment. Therefore, above that level, increases in personal resources would not increase productivity, but decreases in personal resources might decrease productivity.

We explore whether wealth shocks have asymmetric effects in our setting by repeating our analysis during the housing boom period between 2002 and 2007. The analysis again includes CBSA × firm fixed effects, and we explore all seven dependent variables. As can be seen in Table XII, we find no statistically significant relation between house price increases and innovative output during the boom. While by no means definitive, these results are consistent with our proposed mechanisms.

C.2. Heterogeneity by Financial Distress Risk

As discussed above, financial distress may exacerbate the effects of negative wealth shocks. The literature on housing-related financial distress shows that it is usually triggered by the combination of two conditions, namely, negative home equity and a prolonged unemployment spell (Foote, Gerardi, and Willen (2008), Foote et al. (2010)). It follows that individuals who are more likely to become underwater or to become unemployed are at greater risk. In this section we examine whether the effects of negative wealth shocks on productivity are stronger for those at greater risk along either of these dimensions.

Focusing on the first condition, workers who entered the crisis with low home equity were at greater risk of becoming underwater. To investigate whether the strength of our baseline results varies with workers' home equity, we exploit the timing of when workers bought their houses. Workers who purchased 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 inflated at the time of

Table XIII

House Ownership Duration

This table repeats the analysis of Table III, but now allows %Δ *House Price Post* to interact with 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.

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

purchase (while leverage was cheap). In contrast, those who bought earlier are more likely to have retained and accumulated significant equity.[40] We capture likely accumulation of significant equity using an indicator equal to one if the worker bought their house prior to 2004. We then reestimate equation (3), this time interacting house price shocks with this indicator. As in Section II, we are able to include zip code fixed effects in this specification, which further helps address selection concerns. In effect, we control for unobservable differences across 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 have different responses to the same house price shock, due to having different accumulated home equity.[41] Table XIII shows that across almost all of our outcomes, we estimate a significant negative coefficient on the interaction term. This result indicates that the effect of house price movements is indeed smaller for workers who bought their house earlier and as a result were likely to have accumulated more equity. One potential concern with this analysis might be that workers who bought their house earlier also tend to be older, and the productivity of older workers may be less affected by house price movements for reasons unrelated to home equity. However, in the subsample for which we can

[40] Using home equity instead of timing of purchase would partly reflect homeowners' down payment (and accelerated payment) decisions, which are endogenous. For example, LTV 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.

[41] Note that this is a more demanding specification than the one used in previous results where we incorporate firm × 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 also hold when we simply control for firm × CBSA fixed effects. However, due to power limitations, we are not able to include firm × zip code fixed effects. That said, we are able to include separate firm fixed effects and zip code fixed effects.

Table XIV
Labor Market

This table repeats the analysis of Table III, but now allows %Δ *House Price Post* to interact with a *Popular Technology* indicator. To define the *Popular Technology* indicator, we classify workers into 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 as specializing in a widely used—or 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.

	Num (1)	Cites (2)	Top (3)	Gen (4)	Orig (5)	New (6)	Explore (7)
%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

observe worker age, we do not find evidence to suggest that it drives the results reported in Table XIII.

Turning to the second condition, workers with fewer outside labor market opportunities were at greater risk of becoming unemployed for an extended period during the crisis. To examine how our results depend on workers' outside labor market opportunities, we classify workers as specializing in widely used technologies or narrowly used technologies. Presumably, the labor market for innovative workers specializing in widely used technologies is thicker, making it easier for such workers to find another job if necessary. 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, and we classify a technology class as widely used—or 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 period.[42] We then reestimate equation (3), interacting house price shocks with the popular technology indicator, which proxies for labor market thickness. Again, we are able to include zip code fixed effects in this specification, which further helps address selection concerns. Table XIV reports 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 face more outside labor market opportunities.

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

Overall, the evidence in this section is consistent with the idea that financial distress risk exacerbates the effects of wealth losses on productivity. Importantly, however, we do not interpret this evidence to mean that financial distress risk is required for wealth losses to affect productivity.

V. 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 projects that are high impact, complex, or exploratory in nature. Using the methodology of Kogan et al. (2017), we also show that these declines in innovative output translate into lower economic value created for firms.

While it is difficult to determine the precise mechanism driving these results, they are consistent with previous literature documenting adverse psychological consequences of significant wealth losses. The results are also consistent with the idea that negative wealth shocks may decrease the personal resources that workers have available to support productivity in wage employment. We conclude by showing that our effects are more pronounced among those individuals who had little equity in their house at the onset of the crisis and those with fewer outside labor market opportunities. These results suggest that financial distress may be an exacerbating factor.

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 effect 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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Supporting Information

Additional Supporting Information may be found in the online version of this article at the publisher's website:

Appendix S1: Internet Appendix.
Replication Code.