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Anthony Lepinteur
University of Luxembourg and IZA

Andrew E. Clark
CNRS and University of Luxembourg and IZA

Conchita D'Ambrosio
University of Luxembourg

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IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

Unsettled: Job Insecurity Reduces Home-Ownership*

We here evaluate the link between job insecurity and one of the most-important decisions that individuals take: homeownership. The 1999 rise in the French Delalande tax on firms that laid off older workers produced an unexpected exogenous rise in job insecurity for younger workers. A difference-in-differences analysis of panel data from the European Community Household Panel shows that this greater job insecurity significantly reduced the probability of becoming a homeowner. This drop seems more attributable to individual preferences rather than greater capital constraints, consistent with individuals reducing their exposure to long-term financial commitments in more-uncertain environments.

JEL Classification: I38, J18, R21

Keywords: homeownership, job insecurity, employment protection, difference-in-differences

Corresponding author:
Anthony Lepinteur
University of Luxembourg
2 Av. de l'Universite
4365 Esch-sur-Alzette
Luxembourg

E-mail: Anthony.lepinteur@uni.lu

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1. Introduction

Economic insecurity has become a central subject in both academic and political discourse, with the recent commission led by Blanchard and Tirole (2021) identifying it as one of the major future economic challenges, alongside economic inequality and climate change. Economic insecurity is multifaceted, as reflected in the wide variety of measures that have been proposed for its objective measurement (for example, Rhode *et al.*, 2022; Gallo *et al.*, 2023). We here focus on one particular dimension of insecurity, that in the labour market and more specifically the job insecurity that comes from the probability of job loss.

A number of indicators of insecurity on the labour market have been proposed, such as past unemployment or income movements, and the objective probability of job loss (see Section 2 of Clark, 2024). Arguably, what is important for the analysis of individual insecurity is on the contrary the fear of job loss (and its consequences): this is subjective and forward-looking in nature. Subjective measures of this fear have been shown to be correlated in the expected way with labour-market dynamics regarding hiring and layoffs (Böckerman *et al.*, 2011), and institutional differences in employment protection (Clark and Postel-Vinay, 2013; Lepinteur, 2024).

In terms of its consequences, job insecurity has been shown to affect a variety of aspects of individuals' lives outside of the strict confines of the labour market: health (Caroli and Godard, 2016; Lepinteur, 2021), fertility (Comolli and Vignoli, 2021; Da Paola *et al.*, 2021; Clark and Lepinteur, 2022), marriage decisions (Clark *et al.*, 2023a), and savings (Clark *et al.*, 2023b).

In this article, we add to the above literature by examining how job insecurity affects one of the major decisions in individuals' lives: that to become a homeowner. Homeownership has numerous benefits, such as wealth accumulation, insurance against labour-income risk, and positive spillovers on child development (Syz, 2008; Spilerman and Wolff, 2012; Angelini *et*

al., 2013; Laferrère *et al.*, 2017).[1] In theory, if housing prices are uncorrelated or even positively correlated with the risk of job loss, homeownership could play an insurance role and reduce individuals' overall exposure to risk. However, in many cases, and especially in Europe, homeownership involves substantial mortgages and long-term financial commitments. It may also be considered to be risky due to the illiquid nature of the asset that is purchased. Greater job insecurity, synonymous with higher labour-income risk, may then reduce the probability of homeownership via two mechanisms: preference-driven decisions by workers who consider that real-estate investments have now become too risky, and greater barriers to (homeownership) entry for those who are capital-constrained and need to borrow.

The change in job security that we analyse here results from a rise in a French layoff tax, the Delalande tax, that was implemented in 1999. Although this aimed to provide additional labour-market protection for workers aged over 50, it mechanically had a perverse effect on younger workers, who became relatively cheaper to fire. We exploit the firm-size discontinuity in the application of the 1999 tax increase to carry out a difference-in-differences analysis of the effect of job insecurity on homeownership.

We are also able to investigate the politically-important question of the mechanism which is at stake: preferences or capital constraints. Although we do not have a direct measure of preferences, we can consider proxy variables indicating whether respondents' capital constraints have changed. The decline in homeownership following greater insecurity is not moderated by the individual's potential need for borrowing (as measured, for example, by the recent receipt of an inheritance): as such preferences are a more-likely candidate.

[1] From a normative perspective, homeownership may not always be positive. At the national level, promoting homeownership may stifle labour-market mobility and potentially produce higher unemployment (Oswald, 1997). At the individual level, Clark and Diaz-Serrano (2023) show that the observed rise in subjective well-being following the transition into homeownership may well conflate the impact of homeownership itself with that of geographical relocation. Holding the latter constant, the direct effect of homeownership on well-being is much reduced.

We contribute to three key strands of literature: the unintended consequences of employment protection legislation, a field that has predominantly focused on labour-market outcomes (Kugler and Pica, 2008; Schivardi and Torrini, 2008; Messe and Rouland, 2014; Cingano *et al*., 2016; Bratti *et al*., 2021); the impact on greater job insecurity on individual decision-making (Caroli and Godard, 2016; Lepinteur, 2021; Comolli and Vignoli, 2021; Da Paola *et al.*, 2021; Clark and Lepinteur, 2022; Clark *et al.*, 2023a; Clark *et al.*, 2023b); and providing a causal analysis of the link between labour-income risk and homeownership (such as Haurin, 1991; Robst *et al*., 1999; Diaz-Serrano, 2005; Camilli, 2020).

The remainder of the paper is organised as follows. Section 2 describes the Delalande tax and how it changed over time, and provides an overview of homeownership in France in the late 1990s. Section 3 presents the data, the identification strategy and the estimation sample. Section 4 then discusses the main results, robustness checks, and the analysis of the potential mechanisms. Last, Section 5 concludes.

2. Institutional context

a. The 1998 rise in the Delalande Tax

The Delalande tax was initially introduced into French law in 1987, with the aim of restoring the financial equilibrium of the unemployment-insurance system by taxing layoffs among older workers. It applied to private-sector workers with permanent contracts. Despite various changes over the years, the central experience-rating principle of the tax remained the same: firms that laid off workers above a certain age had to pay the Delalande tax to the unemployment-insurance system. This tax was proportional to the gross wage of the laid-off worker. The initial tax between 1987 and 1992 was three months of this gross wage, and applied to laid-off workers aged 55 and above.

The July 1992 reform of the Delalande tax introduced significant changes to these rules, as listed in Table 1. These concerned both the age cut-off for the workers covered and the size of the firm that had to pay the tax. The tax amount now depended on firm size, with larger firms paying twice as much up to a maximum penalty of six months of gross wages, and the age threshold of workers dropping from 55 to 50. Further reforms were applied in January 1993 and January 1999. In the first of these, the tax became only a function of worker age and was independent of firm size. In January 1999, a tax increase was introduced, but only for larger firms (with 50 or more employees). This increase was publicly announced by the government in 1998. The Delalande tax was ultimately abolished in 2008.

Behaghel (2007) presents a theoretical analysis of the impact of these changes, concluding that a higher Delalande tax reduces the separation rate of covered workers but increases the separation rate of uncovered (younger) workers in the same firm. Georgieff and Lepinteur (2018) confirm that the 1999 rise in the Delalande tax did indeed affect younger workers (under the age of 50) in larger French firms, by increasing both their perceived job insecurity and their actual layoff risk as compared to workers of the same age in smaller firms (where the tax did not change).

b. Homeownership in France in the 1990s

Bosvieux (2005) notes that the majority of French households take out mortgages in order to buy their homes. From 1963 to 2002, the homeownership rate in France rose from 42% to 56%, with a slowdown in this growth in the early 1990s and then a second increase towards the end of the century. This second rise in homeownership is attributed to falling interest rates and the introduction of the 'prêt à taux zéro' (zero interest-rate loan) in 1995, which aimed to help lower-income households acquire property (Bosvieux, 2005). The longer time series for 25 to 44 year-olds in Bonnet *et al.* (2017) reveals similar trends.

At the same time as the homeownership rate was on the upswing, the characteristics of new homeowners changed: mortgages continue to be the primary route to homeownership, although the proportion of first-time buyers receiving financial support from their parents rose in the early 2000s (Bonnet *et al.*, 2017; Laferrère *et al.*, 2017). First-time buying also became more unequal, with a growing gradient between household income and the likelihood of becoming a homeowner from 1988 to 2006 (Fack, 2009).

Laferrère *et al.* (2017) argues that, as everywhere, the homeownership decision in France is affected by credit constraints (such as the size of the down payment, interest rates and the duration of the loan), income, price and rent expectations, as well as the individual's position in the life cycle. Fack (2009) calculates an indicator of the costs associated with housing: the net effort rate, which is the ratio of the amount spent on a dwelling (net of subsidies) to disposable income. From 1988 to 2006, the net effort rate of homeowners rose from 18.8% to 22.6%. The same figures for social-housing renters and for private-sector renters also rose from 14.6% to 17.8% and from 18.4% to 24%, respectively. As of 2006, private-sector renting was then more expensive than homeownership. However, in addition to the net effort rate, mobility costs are higher for homeowners than for tenants, as houses are a very illiquid asset.

3. Data, Empirical Strategy and Estimation Sample

a. European Community Household Panel

The data used in this paper comes from the European Community Household Panel (ECHP), a comprehensive longitudinal survey spanning 15 European countries, including France. This survey involved yearly interviews from 1994 to 2001 with nationally-representative samples of households and individuals. The French component of this panel covers around 15,000 individuals per wave, and interviews mainly took place in November and December.

The ECHP provides detailed information on the respondent's socio-economic situation, income, employment conditions, social relationships, and various other characteristics. Of particular significance here, respondents report their homeownership status at each survey wave. This allows us to observe the transition to homeownership, which is our primary outcome variable.

The 1999 increase in the Delalande tax only covered firms with 50 or more employees. We can identify the ECHP respondents who were affected via their reported number of employees in the firm in which they work, with response categories "None", "1 to 4", "5 to 19", "20 to 49", "50 to 99", "100 to 499", and "500 or more employees". The cut-off between the 4th and the 5th of these categories corresponds exactly to the firm-size cut-off in the 1999 reform.

Furthermore, the ECHP includes a series of questions on respondents' satisfaction with various aspects of their employment. To measure perceived job security, we consider the response to the following question:

"How satisfied are you with your present job in terms of job security?"

Responses to this question are on a 6-point scale, with 1 indicating "Not Satisfied" and 6 "Fully Satisfied". These kinds of perceptions of job security are robust predictors of individual choices, including future job quitting (Clark, 2001), reflect layoff and hiring rates (Böckerman *et al.*, 2011), and have been linked to labour-market characteristics such as permanent contracts, unemployment insurance benefits, and employment protection legislation (Clark and Postel-Vinay, 2009; Lepinteur, 2024).

b. Difference-in-Differences Models

We wish to establish the causal effect of job insecurity, following the Delalande tax reform, on workers' homeownership decisions. To do so, we appeal to the firm-size discontinuity that was introduced in January 1999 (see Table 1), and the unintended negative effect on the job security

of younger workers in larger firms. With there being no change in the tax for smaller firms, we can estimate a Difference-in-Differences (DiD) regression where workers under age 50 in larger firms are in the treatment group (with greater job insecurity) and workers of the same age in smaller firms are in the control group.

The DiD regression is as follows:

$$Y_{it} = \alpha_1 Treat_i * Post_t + \alpha_2 X_{it} + \mu_i + \lambda_t + \varepsilon_{it}, \quad (1)$$

where Y_{it} is first the subjective job security of worker *i* in year *t* and then a dummy for becoming a homeowner between the years *t-1* and *t*. $Treat_i$ is the treatment dummy, which is one for younger workers in large firms (50+ employees) and zero for those in smaller firms, and $Post_t$ is a dummy for observations after January 1999 (the date of the Delalande tax rise). Equation (1) includes year dummies, λ_t (which include the main effect of $Post_t$), and a vector of standard individual socio-demographic controls X_{it}. In specifications without the individual fixed effect, μ_i, these controls are gender, weekly working hours, monthly wages (in logs), the (lagged) number of children, and dummies for post-Secondary education, age (in 5-year bands), marriage, occupation and region; with the individual fixed effect, we drop the gender and education information as these do not vary (enough) over time.

The coefficient of interest in this equation is α_2, which reveals the effect of the 1999 reform on both job security and homeownership decisions. Equation (1) is estimated using OLS and standard errors are clustered at the individual level.

We will below discuss the parallel-trend assumption that is required for the causal interpretation of DiD regression coefficients. This is important in the current institutional context, as Georgieff and Lepinteur (2018) underline that there are anticipation effects with the subjective job security of treated workers starting to fall after the announcement but before the

implementation of the reform. As such, we will also estimate an equation where treatment is interacted with all of the separate years in the data:

$$Y_{it} = \sum_{t=1995}^{2001} \beta_t \, Treat_i * \lambda_t + \alpha_2 X_{it} + \mu_i + \lambda_t + \varepsilon_{it}. \quad (2)$$

Equations (1) and (2) are identical in all other respects. The set of estimated β_t coefficients will reveal whether the parallel-trend assumption holds. In particular, β_{1998} refers to the anticipation effects that occurred after the reform's announcement but before its implementation.

c. Estimation Sample

Our estimation sample covers all French adult respondents in the ECHP who are employed in the private sector, with permanent contracts, and with valid data on the sociodemographic variables, job-related characteristics, and perceived job security. To address concerns regarding self-selection into the treatment, we first only retain individuals who were hired before the reform's announcement (in 1998); second, we also drop individuals who stayed with the same employer but whose (reported) firm size switched between small and large over time.[2] These selection criteria yield a final estimation sample of 10,007 observations on 2,210 unique individuals from 1995 to 2001. We exclude the first 1994 ECHP wave as we would require 1993 data to know whether there had been a homeownership transition between 1993 and 1994.

Table 2 lists the descriptive statistics for this sample, which is almost equally split into treated and control observations. Regarding the two dependent variables, average job-security satisfaction (on a one-to-six scale) is just over four, and around 4% of observations report a transition into homeownership. Figure 1 depicts the distribution of subjective job security. Over two-thirds of responses are 4 or 5 on the 1-6 scale; this negative skewness is common in satisfaction measures.

[2] This selection drops 1,697 observations (on 499 individuals). We show in the robustness checks that including these observations, and so allowing the assignment to be time-variant, does not affect the results.

4. Job Insecurity and the Transition to Homeownership

a. Main Results

Table 3 shows how reported job security changed as relative employment protection fell for the treated group. The simple specification in column (1) is an OLS regression model with only the DiD variables and the wave fixed-effects (*i.e.* Equation (1) without the X_{it} or μ_i terms). Columns (2) and (3) then add the time-invariant and time-varying controls in turn.[3] Last, column (4) also includes individual fixed-effects (and corresponds to the full version of Equation (1)). Job security is standardised to have a mean of zero and a standard deviation of one in these regressions.

The 1999 higher layoff tax for older workers in larger firms consistently and significantly led to lower job security for younger workers in these firms, as compared to their counterparts in smaller firms where the layoff tax did not change. This fall in subjective job security is equivalent to around 15% of a standard deviation. That the inclusion of controls has only little effect on the estimated coefficients is consistent with the treatment being orthogonal to the control variables.

Table 4 then evaluates the effect of the reform on the transition to homeownership. The estimation sample is identical to that in Table 3, and the columns correspond to the same specifications. All of the first four coefficients are negative and statistically significant at conventional levels. Equally, as in Table 3, the inclusion of the sociodemographic controls has statistically little impact on the estimated effect of the reform. The point estimates range from -0.015 to -0.020: this is an economically large effect, as it represents around one-half to two-thirds of the mean probability of transitioning into homeownership pre-reform in the treatment

[3] Clark and Lepinteur (2022) and Clark et al. (2023*a*) show respectively that fertility and marriage were affected by the reform. As such, a regression that controls for these will potentially be mediated. In Appendix Table A1, children are not significantly correlated with entry into homeownership (conditional on all of the other controls), but marriage is. A specification without marriage and children produces an estimated coefficient on the treatment variable in a home-ownership regression that turns out to be very similar to that in column (3) of Table 3.

sample. Appendix Table A1 lists the coefficients on the various control variables, which can be used as a second benchmark for the treatment effect. In Column (4) of this table, the greater job insecurity from the layoff tax has an effect that is around 40% of the difference in the probability to transition to homeownership between respondents aged under 25 and those aged 31 to 40.[4]

Higher layoff taxes for older workers then not only reduced the job security reported by younger workers in the same firms but also substantially diminished their transition into homeownership. However, it could be argued that this reduced homeownership reflects something other than job insecurity that is linked to the reform. As a check, we add subjective job security to the regression specification in the final column of Table 4. If lower homeownership is driven by greater job insecurity, we would expect controlling for the latter to render the treatment effect insignificant. This is indeed the case: holding job security constant, the reform no longer has any effect on new homeownership.[5]

b. Identification Assumptions

One requirement for estimated DiD coefficients to be causal is that there is a common trend in the dependent variables (here subjective job security and becoming a homeowner) in the treatment and control groups pre-reform. Appendix Figure A1 plots the values of the two dependent variables over time in the two groups. As in Georgieff and Lepinteur (2008), the trends in subjective job security in the top panel of the figure are reasonably parallel prior to the announcement of the reform in 1998, with the trends diverging thereafter.

[4] To see whether this drop in new homeownership is different across groups of workers we interact the treatment with dummies for female, above median age, post-secondary education, at least one child, and married. The data on the number of children and marriage is pre-reform in order to cancel out the effect of endogenous changes in these variables post-reform. The results in Appendix Table A2 reveal no significant differences across these groups of workers.

[5] Appendix Table A3 replicates the baseline regressions in Tables 3 and 4 via Two-Stage Least Squares, where subjective job security is instrumented by the interaction $Treat_i * Post_t$.

The bottom panel of the figure plots the trends in the probability of transitioning into homeownership for the treatment and control groups. These trends (and the actual level itself) are similar pre-treatment. This similarity persists up to 1999, which is the first year after implementation, so that it might have taken a little time for workers to change their behaviour. The ECHP unfortunately stopped in 2001, and we do not know whether the transition rates between the treatment and control groups eventually converged again in the long run. Given that the rate of homeownership among treated workers one year prior to the reform was higher than that in the control group (at 59%, versus 52%), it may be that the potential for entry into homeownership post-reform is smaller for the treated workers. However, the equal transition rates in the treated and control groups prior to the reform suggests that this type of selection bias may not unduly bias our treatment effects.[6]

The conditional differences in job security and homeownership by year before and after the reform, from the estimation of Equation (2), are plotted in Figure 2. The difference between the treated and control workers in 1995 is the reference here. In the top panel we continue to see a drop in job security starting in 1998 with the announcement of the reform. In the bottom panel, the drop in the transition towards homeownership is statistically significant from 2000 onwards. For both outcomes, none of the differences prior to the reform's announcement are statistically significant, providing supporting evidence for the parallel-trends condition.

c. Robustness Checks

The parallel-trend assumption is a necessary condition for identification, but is not sufficient in terms of interpretation. Even if column (5) of Table 4 demonstrated that the decline in the transition towards homeownership can be attributed to the increase in perceived job insecurity,

[6] If we consider that those who are "treated" by the Delalande reform are less likely to react as much as would some other less-constrained group, then we will estimate a lower bound of the effect of insecurity on homeownership.

it can be argued that all of these changes were driven by some other shock that occurred at the end of the 1990s and affected workers differently on the basis of their firm size. Perhaps the most-plausible candidate in this respect is the 2000 French mandatory reduction in the working week from 39 to 35 hours. This new workweek was first applied in firms with over 20 employees in 2000, and then extended to smaller firms in 2002 (Lepinteur, 2019). Appendix Table A4 shows that there were no changes in the gaps in sociodemographic characteristics between the control and treatment groups after the implementation of the 1999 Delalande tax increase: workers in treated firms were more likely to be men, more-educated, older, and married before the reform, and continued to be so afterwards. However, there is a significant change in this respect for working time: workers in treated firms initially worked longer hours but the hours gap reversed after the 1999 Delalande tax increase. This may well reflect the influence of the 35-hour workweek that was introduced in 2000.[7]

Although we hold hours of work (and the sociodemographic characteristics) constant in our regressions, we cannot rule out the possibility that other changes driven by the shorter workweek (that we do not observe) lie behind our results. To check, we re-estimate Equation (1) excluding workers in firms with fewer than 20 employees; in this new estimation sample all workers were thus equally treated by the workweek reform. The estimated change in worker and job characteristics in this new restricted sample appear in Appendix Table A5: workers in both the treatment (firms with 50+ workers) and control (firms with 20-49 workers) groups experienced a similar drop in working hours. The treated and control groups are also more similar in terms of their sociodemographic characteristics in this restricted sample. The estimated reform effect on homeownership for this sample appears in the first column of Appendix Table A6: this is similar in size to the baseline effect but somewhat less-precisely

[7] The French government announced that this shorter workweek should not affect monthly earnings (so that the hourly wage rose). We find no change in the gap in monthly earnings in Appendix Table A4.

estimated (probably reflecting that the sample is 36% smaller). The French 35-hour week then does not seem to lie behind the homeownership results.

Self-selection into the treatment was addressed above by dropping observations on workers who switched between small and large firms over time. Including these switchers in Column (2) of Appendix Table A6 yields a treatment effect that is again similar to that in the baseline. Self-selection into treatment therefore does not seem to have been a major problem in the case of this reform. We also considered attrition in the estimation sample. While individuals in the treatment group (younger workers in large firms) are always more likely to attrit than individuals in the control group (younger workers in smaller firms), this attrition gap did not change in the post-reform period (1998 onwards).

Firm size is key in the analysis, as it determines allocation to treatment. However, firm size in ECHP is self-reported by respondents and may as such be measured with error. These errors will affect the estimation if they misallocate the worker into the treatment (control) group whereas they are actually in the control (treatment) group. As we do not have information on the actual firm size, we proceed by excluding workers who report being in firms in the category just under the treatment threshold (20 to 49 employees) or that just over it (50 to 99 employees). It is here that misallocation into treatment group via measurement error is the most likely. The results in Column (3) of Appendix Table A6 confirm a negative and significant effect of job insecurity on homeownership (and the larger standard error again reflects the smaller estimation sample).

Last, the results are not sensitive to the choice of the estimation technique. The main results come from linear regressions, but the results from a conditional FE logit regression are qualitatively similar in Column (4) of Appendix Table A6 (the figures in this column are the log of the odds ratios).

d. Preferences or capital constraints?

Greater job insecurity then seems to reduce homeownership. This may reflect two not necessarily mutually-exclusive channels. The first is individual exposure to risk. As becoming a homeowner often involves a long-term mortgage, the prospect of lower income following job loss makes these commitments less attractive. Second, those who are capital-constrained may find borrowing more difficult as their risk of job loss rises. The barrier may be extensive, if banks refuse to lend money, or intensive if they will only do so at a higher interest rate.

It is hard to separate these channels empirically. However, if capital constraints were the only factor at play, there should be a negative gradient between the effect of the 1999 Delalande reform on becoming a homeowner and the capital that workers have available. As in the literature on access to capital and self-employment (for example, Blanchflower and Oswald, 1998, and Jensen *et al*., 2022), capital constraints are irrelevant for those who do not need to borrow.

The ECHP questionnaire does not include explicit information on the respondent's capital. Nevertheless, there is a series of questions that may serve as proxies for (the lack of) capital constraints: the capacity to save at the end of the month, whether the respondent has a partner with a stable job (e.g., in the public sector or a small private-sector firm unaffected by the rise in the Delalande tax), whether household income is above the median annual income in our estimation sample, and whether the respondent recently received a gift, inheritance or lottery windfall (these last three sources appear in one single question in the ECHP). We interact the treatment effect with each of these variables in turn in Table 5. None of these interactions attracts a significant estimated coefficient (which we would expect to be positive were capital constraints to be at play). This non-result perhaps reflects the 'zero interest-rate loan' policy introduced in France in 1995, which eased access to mortgages and alleviated capital constraints.

If capital constraints are relatively unimportant, we can potentially attribute reduced transitions into homeownership to individual preferences. The last column of Table 5 provides some suggestive evidence along these lines. ECHP respondents are asked about their general perception of the current economic situation, replying that it is a "favourable", "not favourable but also not unfavourable" or "unfavourable" time for large purchases. The questionnaire does not state what constitutes a "large purchase", but it is reasonable to assume that house-buying falls into this category. In the last column of Table 5 the treatment effect is interacted with a dummy for those who consider it a "favourable time for large purchases". This interaction attracts a positive estimated coefficient (with a p-value of 0.13) of a size that entirely offsets the treatment effect. Individual preferences may then lie behind our results.[8]

5. Conclusion

Does employment protection affect the housing market? The analysis of the French 1999 Delalande tax reform reveals that greater job insecurity significantly reduced transitions into homeownership.

There are two potential channels: capital constraints and individual preferences. We find no evidence of the former, but some suggestive evidence of the latter. This is significant from a policy perspective as it may reflect the insurance effect of the French 'zero interest-rate loan' policy that was in force at the time of the firing-tax reform. Without this lending policy it is likely that the fall in homeownership would have been even larger. It may also reflect the rise

[8] It is here implicitly assumed that all the respondents in Table 5 experienced an equal fall in subjective job security. It could on the contrary be the case that those who perceive the present economic situation as a "favourable time for large purchases" did not feel that their job had become less secure. Appendix Table A7 investigates this possibility by interacting the treatment dummy with the various "favourable time" categories. None of these attracts a significant estimated coefficient, so that the fall in job security seems to have been experienced equally across the treated workers.

in the percentage of new homeowners after 2000 who received parental support for the acquisition of their first property.

More generally, the results here underscore the broad social impact that employment-protection regulations, and probably labour-market policies in general, can have. Any cost-benefit analysis of these types of policies often focuses primarily on the impact in the confines of the labour market, which may omit considerable changes elsewhere in life. We have here considered how job insecurity affects homeownership, and future work can address other individual behaviours in order to produce a more-comprehensive understanding of the far-reaching impact of labour-market policy.

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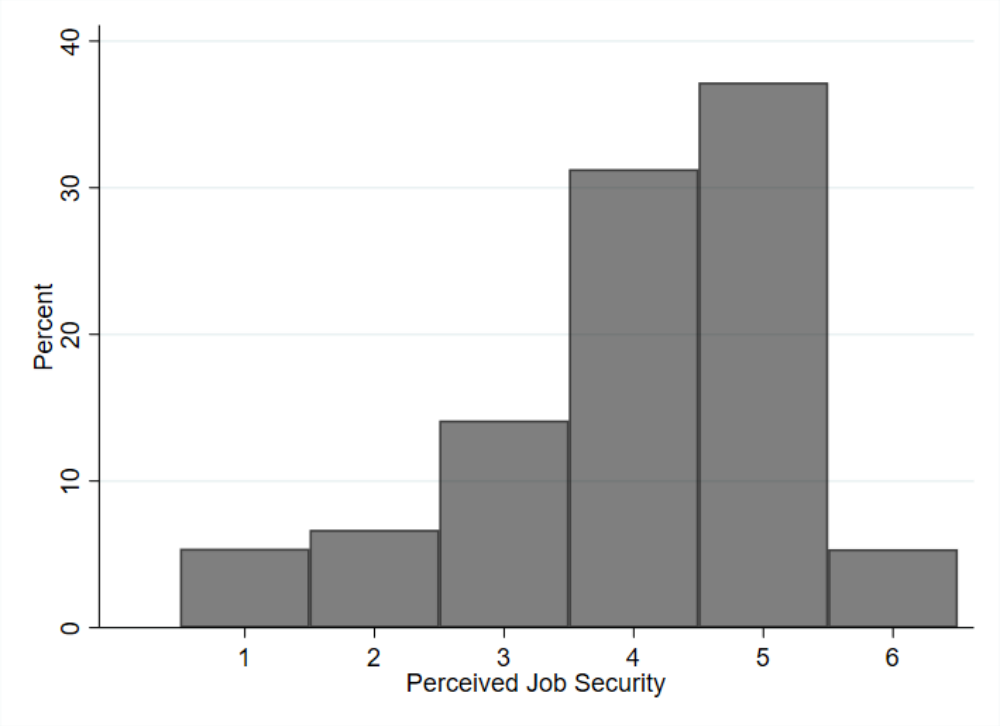
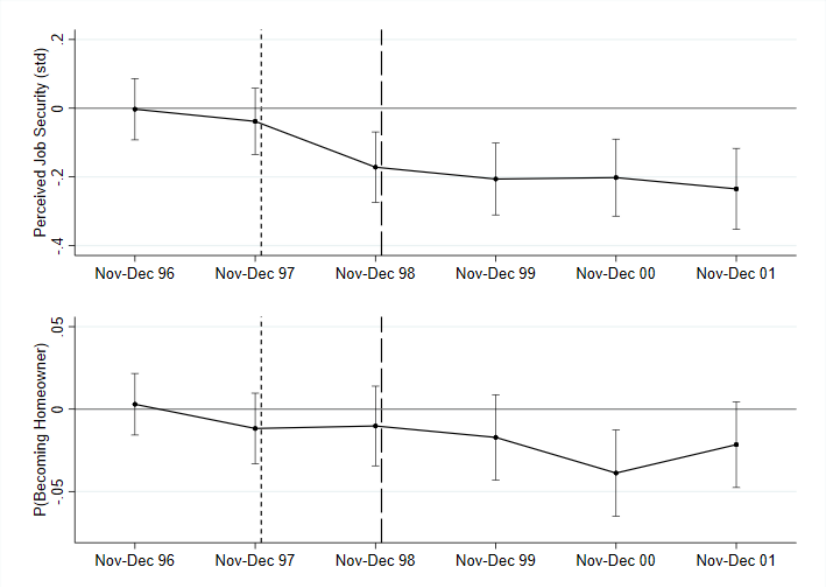
Figures and Tables:

Figure 1: The Distribution of Subjective Job Security



Note: These figures refer to the sample of private-sector workers aged 20-49 with permanent contracts in the French ECHP.

Figure 2: Differences between Young Workers in Large and Small Firms over Time – Panel Regression Results



Notes: These figures refer to the sample of private-sector workers aged 20-49 with permanent contracts in the French ECHP. The short-dashed vertical line indicates the date at which the rise in the Delalande tax was announced, while the long-dashed line indicates the date of its implementation. The points on the horizontal axis refer to the time of data collection of each ECHP wave (in November to December of each year). Each dot shows the effect of being in the treatment group in that year on the outcome in question. These numbers come from regression analyses that include individual and year fixed-effects, as well as age dummies (in five-year bands), the (lagged) number of children in the household, a dummy for being married, weekly working hours, the log of the monthly wage, and occupation and region dummies. The error bars represent the 90% confidence intervals.

Table 1: The Delalande Layoff Tax over Time

		Worker's Age								
		50	51	52	53	54	55	56-57	58	59
July 1987-June 1992	All firm sizes						3	3	3	3
July 1992 - Dec. 1992	20 or more employees	1	1	2	2	4	5	6	6	6
	Under 20 employees	0.5	0.5	1	1	2	2.5	3	3	3
Jan 1993-Dec 1998	All firm sizes	1	1	2	2	4	5	6	6	6
January 1999-2008	50 or more employees	2	3	5	6	8	10	12	10	8
	Under 50 employees	1	1	2	2	4	5	6	6	6

Source: Legislative texts.

Notes: For each age group, the table shows the tax to be paid by the firm to the unemployment-insurance system if it lays off a worker of that age. The tax is a function of the worker's wages, and is stated in months of gross wage.

Table 2: Descriptive Statistics: Estimation Sample

	Mean	SD	Min	Max
Dependent variables:				
Subjective Job Security	4.04	1.22	1	6
Became Homeowner	0.04		0	1
DiD variables:				
Large Firms	0.47		0	1
Post 1998	0.34		0	1
Treat*Post	0.16		0	1
Time-invariant characteristics:				
Female	0.41		0	1
Post-Secondary Education	0.24		0	1
Time-varying characteristics:				
Age: 20-25	0.09		0	1
Age: 26-30	0.21		0	1
Age: 31-35	0.23		0	1
Age: 36-40	0.22		0	1
Age: 41-45	0.20		0	1
Age: 46-49	0.05		0	1
Married	0.58		0	1
Number of Children in the HH (lagged)	0.98	0.98	0	8
Region of Residence:				
Ile de France	0.17		0	1
Bassin Parisien	0.20		0	1
Nord - Pas-de-Calais	0.08		0	1
Est	0.12		0	1
Ouest	0.14		0	1
Sud-Ouest	0.09		0	1
Centre-Est	0.12		0	1
Méditerranée	0.08		0	1
Monthly Wage (nominal, in logs)	8.97	0.49	5.08	11.60
Weekly Working Hours	39.36	7.99	2	96

Note: These numbers refer to the sample of private-sector workers aged 20-49 with permanent contracts in the French ECHP.

Table 3: The 1999 Change in the Delalande Tax and Subjective Job Security - Pooled and Panel Results

	Subjective Job Security (std)			
	(1)	(2)	(3)	(4)
Treat*Post	-0.152***	-0.152***	-0.149***	-0.156***
	(0.042)	(0.042)	(0.041)	(0.039)
Time-invariant characteristics	.	✓	✓	.
Time-varying characteristics	.	.	✓	✓
Individual FE	.	.	.	✓
Observations	10007	10007	10007	10007

Notes: These are linear regressions. The sample here is private-sector workers aged 20-49 with permanent contracts in the French ECHP. Standard errors in parentheses are clustered at the individual level. All of the regressions include year fixed-effects. The time-invariant characteristics are gender and a dummy for post-Secondary education. The time-varying characteristic are age dummies (in five-year bands), the (lagged) number of children in the household, a dummy for being married, weekly working hours, the log of the monthly wage, and occupation and region dummies. *, ** and *** indicate significance at the 10%, 5% and 1% levels respectively.

Table 4: The 1999 Change in the Delalande Tax and the Probability of Becoming a Homeowner - Pooled and Panel Results

	P(Became Homeowner)				
	(1)	(2)	(3)	(4)	(5)
Treat*Post	-0.016*	-0.016*	-0.015*	-0.020**	0.009
	(0.009)	(0.008)	(0.008)	(0.009)	(0.030)
Time-invariant characteristics	.	✓	✓	.	.
Time-varying characteristics	.	.	✓	✓	✓
Individual FE	.	.	.	✓	✓
Job insecurity	.	.	.	.	✓
Observations	10007	10007	10007	10007	10007

Notes: These are linear regressions. The sample here is private-sector workers aged 20-49 with permanent contracts in the French ECHP. Standard errors in parentheses are clustered at the individual level. All of the regressions include year fixed-effects. The time-invariant characteristics are gender and a dummy for post-Secondary education. The time-varying characteristic are age dummies (in five-year bands), the (lagged) number of children in the household, a dummy for being married, weekly working hours, the log of the monthly wage, and occupation and region dummies. *, ** and *** indicate significance at the 10%, 5% and 1% levels respectively.

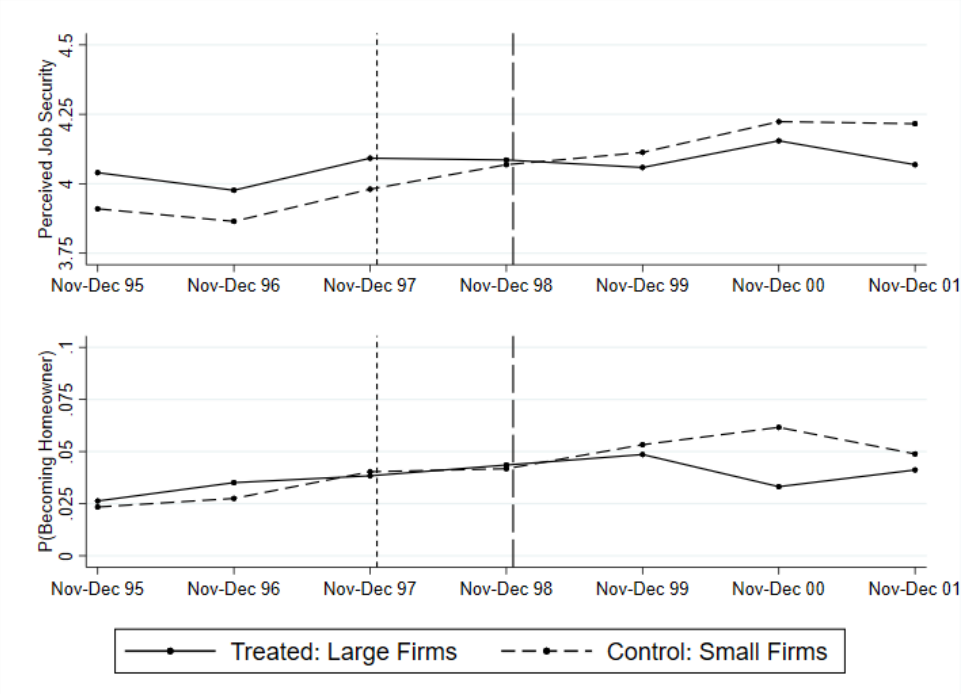
Table 5: The 1999 Change in the Delalande Tax and the Probability of Becoming a Homeowner – Preferences or Constraints? Panel Results

	P(Became Homeowner)				
	(1)	(2)	(3)	(4)	(5)
Treat*Post	-0.016	-0.024**	-0.013	-0.020**	-0.024**
	(0.012)	(0.012)	(0.011)	(0.009)	(0.010)
Interacted with:					
Capacity to save at the end of the month: Yes	-0.005				
	(0.017)				
Partner with a stable job		0.008			
		(0.015)			
Above median household income			-0.016		
			(0.015)		
Recently received a gift, inheritance or windfall gain				-0.025	
				(0.079)	
Favourable time for large purchases: Yes					0.028
					(0.022)
Observations	10007	10007	10007	10007	10007

Notes: These are linear regressions. The sample here is private-sector workers aged 20-49 with permanent contracts in the French ECHP. Standard errors in parentheses are clustered at the individual level. All of the regressions include year fixed-effects, age dummies (in five-year bands), the (lagged) number of children in the household, a dummy for being married, weekly working hours, the log of the monthly wage, and occupation and region dummies. *, ** and *** indicate significance at the 10%, 5% and 1% levels respectively.

Online Appendix:

Figure A1: Subjective Job Security by Treatment Group



Notes: These figures refer to the sample of private-sector workers aged 20-49 with permanent contracts in the French ECHP. The short-dashed vertical line indicates the date at which the rise in the Delalande tax was announced, while the long-dashed line indicates the date of its implementation. The points on the horizontal axis refer to the time of data collection of each ECHP wave (in November to December of each year).

Table A1: The 1999 Change in the Delalande Tax and the Probability of Becoming a Homeowner – Full Pooled and Panel Results

	P(Became Homeowner)				
	(1)	(2)	(3)	(4)	(5)
Treat*Post	-0.016*	-0.016*	-0.015*	-0.020**	0.009
	(0.009)	(0.008)	(0.008)	(0.009)	(0.030)
Treat	0.003	0.001	0.001		
	(0.004)	(0.004)	(0.004)		
Female		-0.002	0.004		
		(0.004)	(0.004)		
Post-Secondary Education		0.022***	0.002		
		(0.005)	(0.006)		
Age: 26-30			0.015*	0.022	0.022
			(0.008)	(0.014)	(0.014)
Age: 31-35			0.010	0.050**	0.050**
			(0.008)	(0.021)	(0.021)
Age: 36-40			-0.018**	0.042	0.042
			(0.008)	(0.027)	(0.027)
Age: 41-45			-0.042***	0.022	0.021
			(0.007)	(0.032)	(0.032)
Age: 46-49			-0.052***	0.020	0.019
			(0.009)	(0.038)	(0.038)
Married			0.014***	0.029**	0.029**
			(0.005)	(0.014)	(0.014)
Number of Children in the HH (lagged)			-0.000	0.002	0.002
			(0.002)	(0.005)	(0.005)
Monthly Wage (log)			0.009*	0.018	0.017
			(0.005)	(0.012)	(0.012)
Weekly Working Hours			0.000	-0.000	-0.000
			(0.000)	(0.000)	(0.000)
Occupation and region dummies	.	.	✓	✓	✓
Individual FE	.	.	.	✓	✓
Job insecurity	.	.	.	.	✓
Observations	10007	10007	10007	10007	10007

Notes: These are linear regressions. The sample here is private-sector workers aged 20-49 with permanent contracts in the French ECHP. Standard errors in parentheses are clustered at the individual level. All of the regressions include year fixed-effects. *, ** and *** indicate significance at the 10%, 5% and 1% levels respectively.

Table A2: The 1999 Change in the Delalande Tax and the Probability of Becoming a Homeowner - Heterogeneity

	P(Became Homeowner)				
	(1)	(2)	(3)	(4)	(5)
Treat*Post	-0.017	-0.032	-0.014	-0.011	-0.011
	(0.012)	(0.025)	(0.010)	(0.016)	(0.016)
Interacted with:					
Female	-0.008				
	(0.019)				
Above Median Age		0.009			
		(0.020)			
Post-Secondary Education			-0.032		
			(0.025)		
Presence of Children in HH				-0.013	
				(0.019)	
Married					-0.015
					(0.019)
Observations	10007	10007	10007	10007	10007

Notes: These are linear regressions. The sample here is private-sector workers aged 20-49 with permanent contracts in the French ECHP. Standard errors in parentheses are clustered at the individual level. All of the regressions include year fixed-effects, age dummies (in five-year bands), the (lagged) number of children in the household, a dummy for being married, weekly working hours, the log of the monthly wage, and occupation and region dummies. *, ** and *** indicate significance at the 10%, 5% and 1% levels respectively.

Table A3: The 1999 Change in the Delalande Tax and the Probability of Becoming a Homeowner - 2SLS Results

	First-stage: Subjective Job Security (std)			
	(1)	(2)	(3)	(4)
Treat*Post	-0.152***	-0.152***	-0.149***	-0.156***
	(0.042)	(0.042)	(0.041)	(0.039)
	Second-stage: P(Became Homeowner)			
	(1)	(2)	(3)	(4)
Subjective Job Security (std)	0.106*	0.106*	0.100	0.130*
	(0.063)	(0.063)	(0.063)	(0.068)
Time-invariant characteristics	.	✓	✓	.
Time-varying characteristics	.	.	✓	✓
Individual FE	.	.	.	✓
Observations	10007	10007	10007	10007
Kleibergen-Paap rk Wald F-statistics	13.361	13.333	13.050	15.660

Notes: The sample here is private-sector workers aged 20-49 with permanent contracts in the French ECHP. Standard errors in parentheses are clustered at the individual level. All of the regressions include year fixed-effects. The time-invariant characteristics are gender and a dummy for post-Secondary education. The time-varying characteristic are age dummies (in five-year bands), the (lagged) number of children in the household, a dummy for being married, weekly working hours, the log of the monthly wage, and occupation and region dummies. *, ** and *** indicate significance at the 10%, 5% and 1% levels respectively.

Table A4: Differences in observable characteristics between the treatment and control groups before and after the reform's implementation

	Before 1999 rise in Delalande Tax			After 1999 rise in Delalande Tax			
	Treated	Control	Diff.	Treated	Control	Diff.	DiD
Female	0.39	0.44	-0.05***	0.38	0.42	-0.04**	0.01
	[0.52]	[0.67]	(0.01)	[0.70]	[0.96]	(0.02)	(0.02)
Post-Secondary Education	0.27	0.19	0.08***	0.28	0.20	0.08***	0.00
	[0.44]	[0.58]	(0.01)	[0.61]	[0.82]	(0.01)	(0.02)
Age	34.26	33.28	0.98***	37.37	36.67	0.70***	-0.28
	[6.90]	[8.99]	(0.16)	[9.42]	[12.83]	(0.23)	(0.28)
Married	0.58	0.54	0.04***	0.63	0.59	0.05***	0.01
	[0.52]	[0.67]	(0.01)	[0.71]	[0.96]	(0.02)	(0.02)
Number of Children in the HH	0.93	0.93	-0.00	1.06	1.07	-0.01	-0.01
	[1.03]	[1.34]	(0.02)	[1.40]	[1.91]	(0.03)	(0.04)
Monthly Wage (log)	9.04	8.81	0.23***	9.18	8.97	0.20***	-0.03
	[0.49]	[0.64]	(0.01)	[0.67]	[0.92]	(0.02)	(0.02)
Weekly Working Hours	40.00	39.43	0.57***	38.56	38.84	-0.28	-0.85**
	[8.37]	[10.90]	(0.20)	[11.43]	[15.56]	(0.27)	(0.34)

Notes: The sample here is private-sector workers aged 20-49 with permanent contracts in the French ECHP. Standard errors are in parentheses and standard deviations are in square brackets. *, ** and *** indicate significance at the 10%, 5% and 1% levels respectively.

Table A5: Differences in observable characteristics between the treatment and control groups before and after the reform's implementation – Workers in Firms with 20+ Employees

	Before 1999 rise in Delalande Tax			After 1999 rise in Delalande Tax			
	Treated	Control	Diff.	Treated	Control	Diff.	DiD
Female	0.39	0.41	-0.02	0.38	0.41	-0.03	-0.01
	[0.41]	[0.96]	(0.02)	[0.57]	[1.33]	(0.02)	(0.03)
Post-Secondary Education	0.27	0.20	0.07***	0.28	0.19	0.10***	0.03
	[0.37]	[0.85]	(0.02)	[0.50]	[1.19]	(0.02)	(0.03)
Age	34.26	33.73	0.53**	37.37	37.04	0.33	-0.20
	[5.50]	[12.69]	(0.23)	[7.52]	[17.68]	(0.32)	(0.39)
Married	0.58	0.56	0.03	0.63	0.59	0.04*	0.02
	[0.42]	[0.96]	(0.02)	[0.57]	[1.34]	(0.02)	(0.03)
Number of Children in the HH	0.93	0.95	-0.02	1.06	1.04	0.03	0.04
	[0.84]	[1.93]	(0.03)	[1.14]	[2.68]	(0.05)	(0.06)
Monthly Wage (log)	9.04	8.91	0.14***	9.18	9.03	0.15***	0.02
	[0.38]	[0.89]	(0.02)	[0.53]	[1.24]	(0.02)	(0.03)
Weekly Working Hours	40.00	40.05	-0.05	38.56	38.44	0.12	0.17
	[5.87]	[13.55]	(0.24)	[8.02]	[18.87]	(0.34)	(0.42)

Notes: The sample here is private-sector workers aged 20-49 with permanent contracts from companies with at least 20 employees in the French ECHP. Standard errors are in parentheses and standard deviations are in square brackets. *, ** and *** indicate significance at the 10%, 5% and 1% levels respectively.

Table A6: The 1999 Change in the Delalande Tax and the Probability of Becoming a Homeowner - Pooled and Panel Results

	P(Became Homeowner)			
	Excluding workers in firms with fewer than 20 employees	Relaxing assumption about self-selection	Donut DiD	Conditional Logit
	(1)	(2)	(3)	(4)
Treat*Post	-0.022*	-0.018**	-0.018	-0.553**
	(0.013)	(0.009)	(0.011)	(0.242)
Observations	6700	11780	7324	10007

Notes: These are linear regressions except in Column (4). The sample here is private-sector workers aged 20-49 with permanent contracts in the French ECHP. All of the regressions include year fixed-effects, age dummies (in five-year bands), the (lagged) number of children in the household, a dummy for being married, weekly working hours, the log of the monthly wage, and occupation and region dummies. The sample in column (1) excludes workers in firms with fewer than 20 employees, while those in columns (2) and (3) respectively include workers reporting a different treatment status over time due to a change in firm size and exclude workers from firms with 20 to 100 employees. Standard errors in parentheses are clustered at the individual level. The conditional FE logit coefficient in column (4) refers to the log of the odds ratio. *, ** and *** indicate significance at the 10%, 5% and 1% levels respectively.

Table A7: The 1999 Change in the Delalande Tax and Subjective Job Security – Preferences or Constraints? Panel Results

	Subjective Job Security (std)				
	(1)	(2)	(3)	(4)	(5)
Treat*Post	-0.149***	-0.144**	-0.167***	-0.157***	-0.140***
	(0.049)	(0.054)	(0.051)	(0.040)	(0.043)
Interacted with:					
Capacity to save at the end of the month: Yes	-0.007				
	(0.062)				
Partner with a stable job		-0.023			
		(0.064)			
Above median household income			0.025		
			(0.062)		
Recently received a gift, inheritance or windfall gain				0.079	
				(0.150)	
Favourable time for large purchases: Yes					0.002
					(0.068)
Observations	10007	10007	10007	10007	10007

Notes: These are linear regressions. The sample here is private-sector workers aged 20-49 with permanent contracts in the French ECHP. Standard errors in parentheses are clustered at the individual level. All of the regressions include year fixed-effects, the (lagged) number of children in the household, a dummy for being married, weekly working hours, the log of the monthly wage, and occupation and region dummies. *, ** and *** indicate significance at the 10%, 5% and 1% levels respectively.