NET WORK LISER

Discussion Paper Series

IZA DP No. 19043

October 2026

Saving and Job-Loss Risk: Evidence from Italy

Andrew E. Clark
Paris School of Economics - CNRS
and IZA@LISER

Conchita D'Ambrosio
University of Luxembourg

Anthony Lepinteur
University of Luxembourg
and IZA@LISER

The IZA Discussion Paper Series (ISSN: 2365-9793) ("Series") is the primary platform for disseminating research produced within the framework of the IZA@LISER Network, an unincorporated international network of labour economists coordinated by the Luxembourg Institute of Socio-Economic Research (LISER). The Series is operated by LISER, a Luxembourg public establishment (établissement public) registered with the Luxembourg Business Registers under number J57, with its registered office at 11, Porte des Sciences, 4366 Esch-sur-Alzette, Grand Duchy of Luxembourg.

Any opinions expressed in this Series are solely those of the author(s). LISER accepts no responsibility or liability for the content of the contributions published herein. LISER adheres to the European Code of Conduct for Research Integrity. Contributions published in this Series present preliminary work intended to foster academic debate. They may be revised, are not definitive, and should be cited accordingly. Copyright remains with the author(s) unless otherwise indicated.

IZA DP No. 19043 October 2026

Saving and Job-Loss Risk: Evidence from Italy*

Abstract

Job insecurity produces uncertainty for workers, and likely affects their subsequent behaviour. We here ask whether the risk of job loss affects savings decisions. We analyse individual-level data from the Italian Survey on Household Income and Wealth, and consider the 2012 Fornero reform that reduced firing costs in large firms in Italy as a natural experiment. Greater job insecurity increases savings. In line with standard models of precautionary savings, this savings reaction to future job-loss risk is larger for workers who are more risk-averse.

JEL classification

D14, D91, J63

Keywords

job insecurity, precautionary saving, risk aversion, Italy

Corresponding author

Anthony Lepinteur
anthony.lepinteur@uni.lu

* We are grateful to Barry Watson and participants at the IARIW and Welfare and Policy conferences, and seminars at Bocconi, Leeds, the London School of Economics, the University of Luxembourg, the Paris School of Economics and Sapienza (Rome) for useful comments. We acknowledge financial support from EUR grant ANR-17-EURE-0001.

1. Introduction

In an era of increasing volatility understanding the behavioural consequences of labour-market risk has become increasingly important. Many theoretical models predict that workers faced with a higher probability of job loss will increase their precautionary savings (Leland, 1968; Sandmo, 1970; Drèze and Modigliani, 1972; Kimball, 1990, 1992; Caballero, 1991; Deaton, 1991; Dynan, 1993; Lusardi, 1998; Banks *et al.*, 2001; Carroll *et al.*, 2003; Lugilde *et al.*, 2019). These savings act as a safety net and ensure a certain level of consumption if adverse future outcomes are realised. Precautionary savings have attracted significant attention in the literature, reflecting their implications for household well-being, consumption patterns, and broader macroeconomic dynamics (Guiso *et al*., 1992, 1996; Michelacci and Ruffo, 2015; Bekaert and Engstrom, 2017; Guerrieri and Lorenzoni, 2017; Fagereng *et al.*, 2018; Bianchi and Mendoza, 2018; Bayer *et al.*, 2019; Benhabib *et al.*, 2019; Farhi and Werning, 2019; Boar, 2021).

Despite this extensive theoretical and policy interest, there is relatively little convincing empirical evidence on how savings actually do move in response to labour-market risk. One reason for this lack is that many of the popular general household surveys do not collect sufficiently-detailed information on consumption and/or savings. Perhaps even more fundamentally, labour-market risk is also difficult to measure and is often endogenous: workers facing a higher probability of job loss may differ systematically from other workers in ways that also affect how they consume and save. Experimental research can generate exogenous variations in income or employment risk (for example, Ballinger *et al*., 2003; Carbone and Hey, 2004), but generally does so in specific controlled environments and relatively small financial stakes, raising questions about external validity in the context of the significant real-world consequences of job loss. The causal analysis

of the savings response to labour-market risk requires both a reliable measure of the former and plausibly exogenous variation in the latter.

We here provide some evidence for this causal effect,[1] taking advantage of the firm-size discontinuity in layoff costs that appeared as part of the 2012 Fornero reform of the Italian labour market. This discontinuity produced a sharp cut-off in the probability of future layoff (and so future labour-income risk). A difference-in-differences analysis of data from the Survey of Household Income and Wealth from the Banca d'Italia shows that the workers who were exposed to this greater job insecurity increased their equivalent annual household savings by roughly twelve percent of a standard deviation.

Individuals save for a variety of reasons, among which to have a family, to buy a house, and as a precaution when faced with risk (precautionary saving). We do not believe that the first two of these are at play, as existing research has shown that the greater job insecurity associated with future potential job loss actually reduces both fertility and the transition to homeownership (Clark and Lepinteur, 2022; Lepinteur *et al.*, 2026). We instead interpret the rise in saving to be precautionary in nature. Greater saving when faced with risk may come about through higher income or a change in the savings function. In the former, individuals can increase their hours of work,[2] move to higher-paid jobs, or more generally contribute to household-level decisions regarding the extensive and intensive labour supply of other household members. In the latter, they change the way in which they save for any given level of (individual or household) income. The higher savings that identified below seem to reflect this latter change in the savings function, as

[1] We use "job insecurity", "layoff probability", "employment risk", "future job loss" and "labour income risk" interchangeably throughout this article. By doing so, we associate the notion of risk with the forward-looking concept of "quantitative job insecurity" in Hellgren *et al.* (1999).

[2] There is no extensive margin response at the level of the individual, as the greater job insecurity only directly affects those who are already employed.

we find no evidence of longer individual work hours, changes in household labour supply, or more generally higher household income. The fall in consumption following greater job-loss risk is similar in size to that in anticipation of future unemployment in Hendren (2017). Last, we show that risk aversion and initial wealth, traditional mediators in the theory of precautionary savings, affect workers' savings responses to the reform: workers who are more risk averse and who have medium levels of initial wealth save more when faced with greater labour-market risk.

These findings are related to various strands of the existing literature. One of these is public policy regarding layoff costs. While existing work on the Fornero reforms has focused on their labour-market implications (Berton *et al.*, 2017; Bratti *et al.*, 2021), and other work has uncovered the consequences of employment protection on a variety of worker outcomes (Comolli and Vignoli, 2021; Lepinteur, 2021; Clark and Lepinteur, 2022; Nieto, 2022; Clark *et al.*, 2023; Pieroni *et al.*, 2023), none of these has identified the relationship between employment protection and savings. In general, any social-welfare evaluation of labour-market policies has to account for their implications over all life domains.

The results are also related to research on precautionary savings, and lie at the intersection of two strands within this extensive literature. The first has provided suggestive empirical evidence of precautionary savings by highlighting the positive correlation between job-loss probability (either objective or perceived) and savings (Benito, 2006; Carroll *et al.*, 2003; Ceritoglu, 2013; Lugilde *et al.*, 2018), while the second has appealed to exogenous variations in income risk (see Lugilde *et al.*, 2019, for a literature review). This latter work has focused on second-degree rises in risk, *i.e.* changes in income variance. Our paper combines these two approaches and provides, to the best of our knowledge, the first evaluation of the savings response to a first-degree increase in risk (*i.e.* a rise in job-loss probability) via a quasi-natural experiment from a policy reform.

The remainder of this paper is structured as follows. Section 2 provides a description of the institutional context of the Fornero reform, while Section 3 presents the data and empirical strategy. Section 4 then discusses the main results regarding savings and addresses the identification assumptions. Section 5 considers the mechanisms at play, and Section 6 concludes.

2. The 2012 Fornero Reform

Under the Italian Laws 604/1966 and 300/1970 employers in Italy are allowed to dismiss private-sector workers with permanent contracts if they can prove *just cause* (e.g., violent behaviour at the workplace) or *justified reason* (e.g., breach of contract or objective business motives such as redundancy following a fall in demand). Employees may challenge their dismissal in writing and, absent settlement, the dispute is adjudicated by the territorially-competent Ordinary Court acting as a Labour Court (*tribunale in funzione di giudice del lavoro*). Up to 2012 the penalties that were paid by employers when the dismissal was adjudicated to have been unfair or unlawful depended on the size of the firm. In firms with up to 15 employees (called 'small firms' from now on), the employer could choose whether to re-instate the worker, without reimbursing wages lost between dismissal and re-hiring, or pay a statutory indemnity, typically between 2.5 to 6 months of wages. In larger firms with over 15 employees, it was on the contrary the wrongfully-dismissed worker who made the choice between re-instatement together with the payment of all foregone wages (and social-security contributions) or an indemnity without re-instatement. The majority of workers chose the former.

As the legal definition of unfair dismissal was imprecise, there was considerable room for interpretation and delay in establishing unfair dismissal so that these layoff costs were meaningful

in both their incidence and their extent. In the Ministry of Justice statistics in Gianfreda and Vallanti (2017), unfair-dismissal cases between 2007 and 2010 lasted for an average of 2.3 years. Along the same lines, O'Higgins and Pica (2020) report an average figure for pre-reform out-of-court settlements for unfair dismissal in larger firms in the Milan area of around 24 months of salary. Up to 2012, expected firing costs were then far higher in larger firms, and as a result workers in these firms were more protected than those in smaller firms.

After the formation of the coalition government led by Mario Monti in 2011, the Fornero Reform (also known as the 'Fornero Law') was introduced in 2012. This was named after the then Minister of Labor, Employment, and Social Policies, Elsa Fornero, and involved significant labour-market reforms. One of these was to harmonise unfair-dismissal rules by firm size. While there was no change for small firms, reinstatement in larger firms was restricted to a narrower set of cases and was determined by the Labour Court rather than chosen by the worker. Where reinstatement was ordered, compensation for foregone wages was generally capped at 12 months; in other cases of unjustified dismissal, reinstatement was replaced by an indemnity of between 12 and 24 months of wages. Last, the legal definition of unfair dismissal was also clarified, reducing the uncertainty as to the outcome of any potential judgment.

The overall effect of the 2012 Fornero reform was to reduce layoff costs (and thus the protection afforded to workers) in large firms. This is reflected in the OECD index of the strictness of Employment Protection Legislation (https://stats.oecd.org/Index.aspx?DataSetCode=EPL_OV), which fell in 2012 in Italy for the first time in almost 20 years. At the micro level, Berton *et al.* (2017) use firm data to show that the reform did indeed increase separation and turnover rates in large firms.

The Fornero reform also affected other aspects of the labour market, and the details of the changes to hiring and safety-net measures can be found in Tiraboschi (2012). However, and crucial to our identification of the effects of employment protection and job insecurity, almost none of these other reforms were firm-size specific. The only exception is the firm-size specific change in the contribution to the *cassa integrazione*, which became mandatory for firms with over 15 employees. We will discuss the potential influence of this change in Section 4.2 below.

3. Data and Identification Strategy

3.1. The SHIW Survey

We will use data from the Survey on Household Income and Wealth (SHIW) carried out by the Banca d'Italia starting in 1965, with the SHIW microdata being available from 1977 onwards. The survey currently covers a nationally-representative sample of 8,000 Italian households per year (with about 20,000 individuals), and collects a wide range of information on economic and financial behaviour at both the individual and household levels. The SHIW was a repeated cross-section until 1989, when a randomly-selected sub-sample of about 4,000 previously-interviewed families was drawn to be part of the panel component of the study. From 1989 onwards, the data were collected biennially (with the exception of a three-year gap between 1995 and 1998 and a four-year gap between 2016 and 2020), and the latest available wave refers to 2022. The interviews are designed to be carried out in person.

One individual, generally the household member primarily responsible for the household budget, provides information on the household and its finances during their interview. All household members provide information on their demographic and labour-market characteristics (including

firm characteristics for the employed) during their interviews. This individual information is provided by a proxy household member if the individual in question is absent at the time of the interview.

The individual labour-market information given by the respondents (or their proxy) includes annual payroll income, net self-employment income, property income, pensions and transfers. The data producers use these variables to calculate the household's net disposable income. The head-of-household financial information includes the household's annual expenditure on durables (e.g., means of transport and furniture) and monthly expenditure on non-durables (e.g., rent, and food eaten inside and outside the home). These figures are used by the data producers to calculate annual household consumption. The household's annual savings are then given by the difference between these annual household disposable income and consumption figures.

Our empirical analysis uses the 2008, 2010, 2012 and 2014 waves of the SHIW. Individual-level information was collected for around 20,000 observations per wave (including children). Around one-third of observations come from the employed, with the analogous figures for homemakers, the unemployed and the retired being 11%, 7% and 28%. The other 20% of observations refer to children and students. Among those who report being employed, four out of five work in the private sector (and are thus potentially treated by the reform, according to the size of the firm). The sample starts in 2008 as this is the first wave in which information appears on the total number of employees in the firm at which the respondent works. This is recorded in seven categories: [0-4], [5-15], [16-19], [20-49], [50-99], [100-499] and 500+. This classification allows us to map workers cleanly into the legal regimes that applied before the Fornero reform: workers in firms

with up to 15 employees are in the control group (where the reform did not apply), while those in firms with more than 15 employees appear in the treated group.[3]

The sample stops in 2014 in order to avoid contamination from the subsequent Jobs Act, which introduced a new regime of decreased protection for workers with permanent contracts. Although the enabling Law for the Jobs Act was adopted in December 2014, the key dismissal provisions were implemented in 2015. These provisions again changed dismissal protection according to firm size, especially around the 15-employee threshold. The inclusion of post-2014 waves would therefore mix the effect of the 2012 Fornero reform with that from the reform in the later Jobs Act.

3.2. Identification Strategy

We identify the effect of the Fornero reform in the same way as in Berton *et al.* (2017), estimating the following two difference-in-differences regressions via OLS:

$$Y_{it} = \alpha_1 Treat_i + \lambda_t + \alpha_2 Treat_i * Post_{2012} + \alpha_3 X_{it} + \epsilon_{it} \quad (1)$$

$$Y_{it} = \beta_1 Treat_i + \lambda_t + \beta_2 Treat_i * \lambda_{2012} + \beta_3 Treat_i * \lambda_{2014} + \beta_4 X_{it} + \epsilon_{it}. \quad (2)$$

Here Y_{it} is equivalised annual household savings (and consumption in the robustness checks) at 2010 constant prices, with the equivalence scale being the square root of household size. All of the dependent variables are standardised to have a mean of zero and standard deviation of one. $Treat_i$ is a dummy variable for the worker being in a 'large' firm with more than 15 employees. The

[3] Firm size is in some cases reported by proxy respondents. We will consider the potential implications of this proxy information in the robustness checks. This turns out not to drive our conclusions.

vector λ_t denotes year fixed effects.[4] $Post_{2012}$ is a dummy variable for the observations coming from the 2012 or 2014 waves, and the λ_{2012} and λ_{2014} dummies split this up into the separate observations from 2012 and 2014 respectively. X_{it} is a set of standard individual characteristics (age, age squared, gender, education, marital status, homeownership status, and occupation and regional fixed effects).[5]

The coefficient of interest in Equation (1) is α_2, which we will argue captures the response of savings to the greater job insecurity induced by the Fornero reform. In Equation (2) this effect is allowed to change over time, via the β_2 and β_3 coefficients. As the Fornero reform was implemented in mid-2012, and consumption may be sticky, the immediate response (β_2) could be more muted than that over a longer time period (β_3).[6]

The estimation sample consists of permanent workers in the private sector. We in addition restrict the sample to workers who were hired before the announcement of the Fornero reform (in July 2012): this selection is important, as the reform arguably changed the type of workers that treated firms recruited post-reform so that their inclusion would confound the effect of the reform. The descriptive statistics for the 12,572 observations in the estimation sample are displayed in Table 1. Average equivalent annual household disposable income is 21,928 Euros, and around 27% of this amount is saved on average. This is slightly above the national gross domestic savings figure for Italy over the same period

[4] In the presence of year fixed effects, $Post_{2012}$ is absorbed by λ_t. Equation (1) therefore omits the separate $Post_{2012}$ term from the estimated specification, while the interaction $Treat_i * Post_{2012}$ captures the differential change for workers in firms above the 15-employee threshold after the reform.

[5] The influence of household size is already addressed to an extent by the use of equivalised savings (or consumption and income) as the dependent variable. Household size does not appear in the vector of control variables (although the estimated results do not change if it is included).

[6] Our baseline specification is at the individual level, with treatment defined according to the individual's employment situation and savings at the household level. Alternatively, exposure could be defined at the household level (such as any treated worker in the household, or the percentage treated). This will make a difference when more than one household members are employed. We will carry out these household-level regressions in the robustness checks.

(https://data.worldbank.org/indicator/NY.GDS.TOTL.ZS?locations=IT). This discrepancy likely reflects that sample individuals are employed and aged 18-60, and are predicted to save more from life-cycle theory. Average observation age is 42, and 60% are men. About half of the sample has at least Secondary education, and 62% are married. Just over half of the observations in Table 1 refer to treated individuals, and Appendix Figure A1 shows the distribution of the self-reported firm-size categories that we use to split the sample into the control (<=15 employees) and treated (>15 employees) groups. Figure 1 depicts the kernel densities of the key variables in the estimation sample: equivalent annual consumption, savings and income. While consumption is always positive, savings are negative for 5% of the observations.

4. The Effect of Job-Loss Risk on Savings

4.1. Labour-Market Risk Increases Savings

Table 2 lists the main results (the full list of estimated coefficients is in Appendix Table A1) from the estimation of Equations (1) and (2). In column (1), exposure to the Fornero reform significantly increases equivalent annual savings. Splitting the treatment up into its effect in 2012 (the year of the reform) and 2014 in column (2) shows that this rise in savings from job insecurity appears only in 2014. As noted above, this may reflect stickiness in consumption or 2012 being half-treated as the Fornero reform was only introduced in July 2012. There are no control variables in columns (1) and (2): their introduction in column (3) does not meaningfully change the estimated coefficients.[7]

[7] The drop in the 2014 point estimate between columns (2) and (3) is not significant.

The Fornero reform, which reduced employment protection for workers in firms with more than 15 employees, then increased the equivalent savings of the treated workers by around 12% of a standard deviation relative to workers in smaller firms. This corresponds to a rise of 826 equivalised Euros per year (or 69 equivalent Euros per month). This effect is slightly larger than those associated with marital status and education in column (4) of Table A1, and is equivalent to that of being a homeowner or occupying a managerial position. For a worker living in an average-sized household (3.2 in our estimation sample), the estimated annual rise in savings in 2014 in column (3) of Table 2 is 1,281 Euros (=0.119*6607.3*$\sqrt{3.2}$).

To give a sense of the size of this response, we can translate the higher savings into an equivalent change in perceived job-loss risk using a simple two-period precautionary-saving model. In the estimation sample, the annual earnings of treated workers account for around 64% of total household income (26,235/40,988). As equivalisation scales both quantities by the same factor, the same earnings share holds when expressed in equivalised terms.

With pre-reform equivalised household income of 24,058 Euros and an unemployment replacement rate of 52%,[8] job loss would reduce equivalised annual household income by 7,390 Euros (=(1−0.52)*0.64*24,058) to 16,668 Euros. We apply a CRRA utility function, $u(c) = c^{(1-\gamma)}/(1-\gamma)$, with $\gamma = 2$ (a common value in the literature: see e.g. Kimball and Weil, 2009),[9] and consider the additional 826 Euros of saving induced by the reform as precautionary. Optimal savings are derived from a Euler equation setting the marginal utility cost of reducing current consumption by one additional Euro equal to the expected marginal utility benefit of that Euro in

[8] https://data-explorer.oecd.org/vis?tm=replacement%20rate&pg=0&snb=11&vw=tb&df[ds]=dsDisseminateFinalDMZ&df[id]=DSD_TAXBEN_NRR%40DF_NRR&df[ag]=OECD.ELS.JAI&df[vs]=1.0&dq=...C_C2..AW100..M12.YES.YES%2BNO.A&pd=,&to[TIME_PERIOD]=false.

[9] Italy appears roughly in the middle of the World ranking of risk preferences in Falk *et al.* (2018).

the following period. Normalising the product of the discount factor and the interest factor to one, the perceived job-loss probability p that corresponds to the observed higher saving is given by:

$$(24{,}058 - 826)^{-2} = (1 - p)(24{,}058 + 826)^{-2} + p(16{,}668 + 826)^{-2}.$$

Solving this equation produces p = 0.144. In other words, the estimated saving response corresponds to a perceived job-loss probability of 14.4%.[10]

4.2. Identification Assumptions

The estimated coefficients in Table 2 are only causal under certain conditions. We first check the parallel-trend assumption in two ways. Figure 2 plots average equivalised savings and consumption over time separately for the treatment and control groups: these moved in a reasonably-parallel way prior to the 2012 Fornero reform (the date of which is indicated by the dashed vertical line). We then take an event-study approach to look for pre-reform differences between the treatment and control groups (using 2008 as the reference year), holding the set of control variables constant. The results in Figure 3 show that neither of the 2010-point estimates are statistically significant. This can be interpreted as a placebo test, showing that there was no difference in savings or consumption trends between the groups prior to the Fornero reform.

We also check that the sample composition did not change over time. Even though we have a number of control variables, and restrict the estimation sample to workers who were hired before the reform's announcement (to avoid endogenous selection from post-reform hiring), the treatment and control groups can still differ in terms of job separations starting in 2012. Appendix Table A2

[10] This calculation assumes that there was no initial risk of job loss at baseline. We do not actually observe the extent of the pre-reform perceived job-loss risk in Italy, but can instead consider a baseline figure of 10% (this corresponds to the perceived annual probability of job loss in the Australian HILDA dataset). In this case, the implied post-reform risk rises by 17.1 percentage points to a figure of 27.1%, rather than the 14.4% with zero baseline risk. Appendix Figure A2 illustrates how the implied post-reform job-loss risk that is consistent with the observed rise in savings varies by the baseline probability of job loss.

thus compares the differences in observable characteristics between the treatment and control groups before and after the Fornero reform. Workers in treated firms (>15 employees) are positively selected: they have higher education and earnings, and are more likely to be homeowners and managers. They are also a little older and are more likely to be men. These differences between groups are not important in a difference-in-differences regression as long as they remain constant over time. The last column of Table A2 shows that this is the case: nine of the ten gaps in observable characteristics between groups are statistically identical pre- and post-reform.[11] The only exception is age: the treated workers are older both pre- and post-reform, but the age difference is a little lower post-reform. As this difference is only small (around eight months), and is insignificant after correcting for common family-wise errors (such as the Bonferroni correction), we overall conclude that there is little change in sample composition.

The parallel-trend assumption is a necessary but not sufficient condition for identification. The estimates in Table 2 could also be confounded by events that *(i)* occurred between 2012 and 2014 and *(ii)* differed systematically for the treatment and control groups in the analysis. As assignment to treatment is based on firm size, the most-plausible confounding events are probably linked to labour-market reforms. From 2008 to 2014 there were around 50 amendments and labour-market reforms of varying importance in Italy. Of these, only three depended on firm size (as did the 2012 Fornero reform), but none involved the same firm-size threshold and time pattern. They also covered aspects of the labour market that do not appear to be directly related to precautionary savings (the working conditions of apprentices and the conditions for Sunday working).

[11] We do not show the results for the sixteen region dummies here (these figures are available upon request), but these also indicate no change in the control-treatment gap pre- and post-Fornero.

Along the same lines, the Fornero reform included a number of elements other than employment protection and it is potentially these that explain the estimated coefficients. This is *a priori* unlikely as almost all of these applied to all workers without any differentiation by firm size. The one exception is firms' contributions to the *cassa integrazione* (CI), an Italian Social Security programme that provides financial support to employers during periods of economic difficulty, in order to discourage layoffs, while also providing financial support to the employees who do lose their jobs. All employees of firms that contributed to the CI are covered by this support programme, and one element of the Fornero reform was to make contributions compulsory for firms with more than 15 employees. This expansion of CI coverage probably does not explain the higher savings in Table 2. First, as one of the goals of the CI is to provide employees a buffer against labour-income risk it acts a substitute for private savings; as such the estimates in Table 2 may be lower bounds of the true effect of job insecurity on savings. Second, Checchi and Leonardi (2015) conclude that in practice only the transportation and banking sectors had started to contribute to the CI by the end of 2014. The first three columns of Appendix Table A3 therefore reproduce the analysis in Table 2 excluding these 'CI compliant' sectors: the results are not materially different.

Last, we address the potential confounding effect of the tax bonus that was implemented in Italy in 2014 as a response to the severe economic downturn. While this was not differentiated by firm size, it did depend on earnings and it is well-known that earnings and firm size are correlated. The bonus represented an extra 80 Euros per month for employees earning up to 24 000 Euros per annum; the bonus then fell with income to reach zero for earnings over 26 000 Euros. The last three columns of Appendix Table A3 reproduce the estimates in Table 2 using only workers who earned over 26 000 Euros (who therefore did not receive the bonus): the results again change only little.

4.3. Robustness Checks

We discuss below a battery of robustness checks, focusing on the estimated 2014 treatment effect in column (3) of Table 2. The results appear in Table 3, column (1) of which reproduces the baseline estimate for comparison purposes.

We first consider the construction of equivalent savings, replacing the square root of household size equivalence scale by the OECD equivalence scale. The treatment effects in column (2) of Table 3 are a little smaller, although not significantly different from the baseline estimates. In a similar vein, not equivalising savings at all in column (3) yields qualitatively-similar estimates.

We can also estimate the effect of the Fornero reform on the logarithms of equivalent savings (instead of their level), which addresses potential non-linearities. Given that savings can be negative in our estimation sample, we instead apply an Inverse Hyperbolic Sine transformation. The estimated coefficient in column (4) remains positive and significant.

In Figure 1, the distributions of savings and consumption have long tails. To ensure that our results are not influenced by potential outliers (which might explain why the treatment effect in column (4) is a little lower), we trim the top and bottom 1% of the savings distribution. The results, in column (5), are unaffected (the same conclusion holds when the trimming covers the top and bottom 5%).

As firm size is self-reported, it is potentially prone to measurement error: this can lead to workers being mis-assigned to the treatment or control group. We address measurement error by re-estimating the main regressions excluding the workers in the firm-size categories just below and just above the firm-size discontinuity (5-15 and 16-19). The treatment effects in column (6) are very similar to those in the baseline.

We next consider two features of the way in which the SHIW data are collected. Appendix Table A4 reports the results (with column (1) again showing the baseline estimate). First, information on firm size is not always provided directly by the worker concerned and may instead come from a proxy interview (this applies to 40% of observations). In column (2), we introduce an interaction term for firm size being proxy reported. This interaction is statistically insignificant, while the estimated main treatment effect remains positive and significant. The proxy reporting of firm size does not then drive our results. Second, the baseline specification is at the individual level, even though households may contain multiple workers with different treatment statuses. We therefore collapse the data to the household level and define treatment alternatively as either the share of working household members who are treated or a dummy variable for at least one household member being treated. Columns (3) and (4) show that both approaches yield positive and statistically-significant reform effects on household savings, which turn out to be very similar in size to the baseline estimate.

4.4. Evidence for Precautionary Savings

Following the results of the comprehensive set of tests in Sections 4.2 and 4.3 above, we conclude that rise in job-loss risk for workers in firms with more than 15 employees due to the Fornero reform led them to save more. The intuition is that precautionary savings rise for those who are faced with greater risk, but we have not provided any formal evidence for this reading. This Sub-section attempts to rule out a number of competing interpretations and presents some results that are in line with a precautionary-savings interpretation.

4.4.1. Ruling Out Competing Interpretations

Higher savings could reflect an increased savings rate at given income, or equally higher income with the same savings rate. In the latter case, household income may rise in response to job

insecurity via greater hours of work at the intensive margin or another family member starting a new job at the extensive margin. Appendix Table A5 shows the estimated 2014 coefficients from the specifications in columns (2) and (3) of Table 2, with the dependent variables being in turn the individual's own weekly hours of work and labour income, and then the household number of income earners and number of employment income earners. All of the estimated coefficients are insignificant, consistent with job insecurity producing a change in savings behaviour at a given income rather than an unchanged savings rate applied to higher household income.

Household income may change for any number of reasons (other than the number of earners), and the first two columns of Appendix Table A6 then estimate the effect of the reform on household equivalised income, again reproducing columns (2) and (3) of Table 2. There is no significant treatment effect on household income, so that controlling for income in the savings regression should not affect our main result. This is what columns (3) and (4) of Appendix Table A6 show. Last, with no change in household income, greater savings should be mirrored by in lower consumption. This is what we see in the last two columns of Appendix Table A6.[12] The change in workers' savings when faced with job insecurity then comes from a change in the savings function, rather than a change in income.

Hendren (2017) provides a benchmark figure for the size of this consumption response. In PSID data there is a 2.7% drop in food expenditure over the one to two years prior to unemployment relative to workers who remain employed. This estimate is only a little lower when controlling for

[12] The relationship between job insecurity and consumption may depend on the nature of the latter. This is addressed in Appendix Table A7, where total equivalent annual consumption is decomposed into non-durables (food expenditure, and other) and durables (means of transport expenditure, and other). As is the case for total consumption, all four estimated coefficients are negative. However, the only insignificant coefficient (and the smallest) is that for spending on food. The pattern of estimated coefficients is consistent with more-essential expenditure being less responsive to job insecurity.

household income. In more-recent biennial PSID data this fall is 3.6% measured between four and two years before unemployment. Our estimates in columns (5) and (6) of Appendix Table A6 correspond to a drop in total consumption equivalent to 3.3% of the mean without controlling for income controls and 4.2% with them. Although the expenditure measures, country and time period all differ, these consumption responses are similar in size.[13]

4.4.2. The Moderators of Precautionary Savings

We suggest that higher savings are a precautionary response to labour-income risk. If so, we expect the savings response to be moderated by a number of key variables. We here consider two of these: risk aversion and the initial level of wealth. It is standard that precautionary savings rise with risk aversion, while the effect of initial wealth on precautionary savings depends on the functional form of the utility function. Under Constant Absolute Risk Aversion, wealth and precautionary savings are independent of each other. However, under CRRA utility, precautionary savings fall with initial wealth ('initial' here referring to the level of wealth during the first period of the standard consumer problem).

It is often difficult empirically to isolate the effects of moderators due to practical limitations as empirical measures of risk aversion and initial wealth are not always readily available. Fortunately, the SHIW survey includes a question on risk aversion: *"In managing your financial investments, would you say you have a preference for investments that offer..."*, with response options of "*very high returns, but with a high risk of losing part of the capital*", "*a good return, but also a fair degree of protection for the invested capital*", "*a fair return, with a good degree of protection for*

[13] At the end of Section 4.1, we calibrated the consumption response in Italian data as corresponding to a rise in subjective job-loss risk from 0 to 14%, or 10 to 27%. While there are no such job-loss figures in the PSID, in HILDA data this is around 10% for those who will not lose their jobs next year and 26% for those who do. Assuming these subjective probabilities can be applied to the US, the drop in consumption in Hendren (2017) corresponds to a rise in subjective job-loss risk of 16%, again similar to the figure that we calibrate.

the invested capital" and "*a safe asset with small returns*". Around half of the individuals in the estimation sample chose "*a safe asset with small returns*", which is our measure of high risk aversion.[14]

The results of separate estimations by risk aversion appear in columns (1) and (2) of Table 46.[15] Consistent with precautionary savings, savings rose more following the Fornero reform (at a 10% level of statistical significance) for workers who are more risk-averse.

The analysis of initial wealth is less immediate. We apply a Principal Component Analysis (PCA) to two wealth measures in the SHIW data. The first is the value of real assets. These are particularly significant in the Italian context, as they are often passed down through generations and are arguably positively correlated with initial wealth. The second is the educational attainment of the respondents' father, which reflects the socioeconomic conditions experienced by respondents during their childhood. The first component of this PCA is used as the proxy measure of initial wealth, and we split respondents into 'low initial wealth' (the bottom two deciles), 'high initial wealth' (the top two deciles), and 'medium initial wealth' (everyone else).

The last three columns of Table 4 reveal the relationship between layoff risk and savings by wealth group. The relationship is non-linear, with medium-wealth individuals being the only ones to exhibit a significant rise in savings. The estimated coefficient for high-wealth individuals is smaller in column (5), as predicted by a CRRA utility function.[16] Low-wealth individuals do not increase savings as labour-market risk rises. As in Carroll *et al.* (2003) in the US, this may reflect

[14] To check whether the variable that we use to split the sample is itself affected by the reform, we re-run the analysis with the risk-aversion dummy as the dependent variable. There is no evidence that this is the case: the estimated change is 1.2 percentage points and is statistically insignificant (p-value=0.568).
[15] Using interaction terms rather than a sample split yields similar results.
[16] Notably, the point estimates in columns (4) and (5) are not significantly different at conventional levels. This may reflect the inherent measurement errors in the PCA-based calculation of initial wealth. Were the direct observation of initial wealth to be feasible, we would expect a larger gap between the point estimates.

that those with low wealth have lower household incomes and are constrained in their ability to increase their savings.[17]

5. Conclusions

We appeal to an Italian labour-market reform to establish the effect of job insecurity on savings. Workers whose employment protection was weakened increased their savings even though their household income did not change, with greater savings being accompanied by lower consumption, particularly of non-essential goods.

The labour-market reform that changed employment protection thus spilled over to financial behaviour. At the aggregate level, labour-market reforms can have knock on effect on savings and consumption. At a more-disaggregated level, not all workers saved more. In particular there was no savings response by those towards the bottom of the wealth distribution, which has implications for inequality in financial security.

Our findings also relate to the longstanding concern that households may save too little to protect themselves against future income shocks. One interpretation of insufficient saving is that households are not sufficiently sophisticated to calculate risk, cost and benefits. While we cannot say whether the current level of household saving is too low (or indeed too high), our results regarding an exogenous rise in labour-market risk underline that households behave rationally in the sense that as risk rises they save more.

[17] Some supporting evidence comes from the heterogeneity analysis in Appendix Table A8, based on gender, age, partnership status, parenthood and disposable equivalent annual household income. Workers without partners, with children, and with lower incomes do not increase their savings when treated. It may well be that they want to increase precautionary savings but cannot do so due to financial constraints.

The greater saving, and lower consumption, that results from labour-market risk may have macroeconomic consequences. Holding current income constant, an increase in the expected probability of job loss raises precautionary saving and reduces aggregate demand. If sufficiently widespread, pessimistic expectations about employment may therefore become partly self-fulfilling: greater fear of job loss reduces household spending and so firms' labour demand

Future research can build on these results by extending them to other settings in which individuals are exposed to financial risk (such as potential future health expenditures or insecurity on the housing market), or where individual savings are more or less important for financial security due to government safety nets. Over the longer run, the implications of precautionary savings for wealth accumulation, investment decisions, and economic stability would shed light on the broader economic consequences of labour-market reforms affecting employment protection.

References

Ballinger, T. P., Palumbo, M. G., and Wilcox, N. T. (2003). "Precautionary saving and social learning across generations: an experiment." *Economic Journal, 113,* 920-947.

Banks, J., Blundell, R. and Brugiavini, A. (2001). "Risk pooling, precautionary saving and consumption growth." *Review of Economic Studies, 68,* 757-779.

Bayer, C., Lütticke, R., Pham-Dao, L., and Tjaden, V. (2019). "Precautionary savings, illiquid assets, and the aggregate consequences of shocks to household income risk." *Econometrica, 87,* 255-290.

Bekaert, G., and Engstrom, E. (2017). "Asset return dynamics under habits and bad environment–good environment fundamentals." *Journal of Political Economy, 125,* 713-760.

Benhabib, J., Bisin, A., and Luo, M. (2019). "Wealth distribution and social mobility in the US: A quantitative approach." *American Economic Review, 109,* 1623-47.

Benito, A. (2006). "Does job insecurity affect household consumption?" *Oxford Economic Papers, 58,* 157-181.

Berton, F., Devicienti, F., and Grubanov-Boskovic, S. (2017). "Employment protection legislation and mismatch: Evidence from a reform.", *IZA Discussion Papers Series,* No. 10904.

Bianchi, J., and Mendoza, E. G. (2018). "Optimal time-consistent macroprudential policy." *Journal of Political Economy, 126,* 588-634.

Boar, C. (2021). "Dynastic precautionary savings." *Review of Economic Studies, 88,* 2735-2765.

Bratti, M., Conti, M., and Sulis, G. (2021). "Employment protection and firm-provided training in dual labour markets." *Labour Economics, 69,* 101972.

Caballero, R. J. (1991). "On the sign of the investment-uncertainty relationship." *American Economic Review, 81,* 279-288.

Carbone, E., and Hey, J. D. (2004). "The effect of unemployment on consumption: an experimental analysis." *Economic Journal, 114,* 660-683.

Carroll, C., Dynan, K. and Krane, S. (2003). "Unemployment risk and precautionary wealth: Evidence from households' balance sheets." *Review of Economics and Statistics, 85,* 586-604.

Ceritoglu, E. (2013). "The impact of labour income risk on household saving decisions in Turkey." *Review of Economics of the Household, 11,* 109-129.

Checchi, D., and Leonardi, M. (2015). "Labour market measures in Italy 2008–13: The crisis and beyond." *International Labour Organization, Research Department*.

Clark, A. E., and Lepinteur, A. (2022). "A natural experiment on job insecurity and fertility in France." *Review of Economics and Statistics, 104,* 386-398.

Clark, A. E., D'Ambrosio, C., and Lepinteur, A. (2023). "Marriage as insurance: Job protection and job insecurity in France." *Review of Economics of the Household, 21*, 1157–1190.

Comolli, C. L., and Vignoli, D. (2021). "Spreading uncertainty, shrinking birth rates: A natural experiment for Italy." *European Sociological Review, 37*, 555-570.

Drèze, J. H., and Modigliani, F. (1975). "Consumption decisions under uncertainty." In *Stochastic Optimization Models in Finance,* Academic Press, 459-486.

Dynan, K. E. (1993). "How prudent are consumers?" *Journal of Political Economy, 101,* 1104-1113.

Fagereng, A., Guiso, L., and Pistaferri, L. (2018). "Portfolio choices, firm shocks, and uninsurable wage risk." *Review of Economic Studies, 85,* 437-474.

Falk, A., Becker, A., Dohmen, T., Enke, B., Huffman, D., and Sunde, U. (2018). "Global evidence on economic preferences". *Quarterly Journal of Economics, 133,* 1645-1692.

Farhi, E., and Werning, I. (2019). "Monetary policy, bounded rationality, and incomplete markets." *American Economic Review, 109,* 3887-3928.

Gianfreda, G., and Vallanti, G. (2017). "Institutions' and firms' adjustments: Measuring the impact of courts' delays on job flows and productivity." *Journal of Law and Economics, 60,* 135-172.

Guerrieri, V., and Lorenzoni, G. (2017). "Credit crises, precautionary savings, and the liquidity trap." *Quarterly Journal of Economics, 132,* 1427-1467.

Guiso, L., Jappelli, T. and Terlizzese, D. (1992) “Earnings uncertainty and precautionary saving.” *Journal of Monetary Economics, 30*, 307-337.

Guiso, L., Jappelli, T. and Terlizzese, D. (1996). “Income risk, borrowing constraints, and portfolio choice.” *American Economic Review, 86,* 158–172.

Hellgren, J., Sverke, M., and Isaksson, K. (1999). “A two-dimensional approach to job insecurity: Consequences for employee attitudes and well-being.” *European Journal of Work and Organizational Psychology*, *8*, 179-195.

Hendren, N. (2017). “Knowledge of future job loss and implications for unemployment insurance.” *American Economic Review, 107*, 1778–1823.

Kimball, M. (1990). “Precautionary saving in the small and in the large.” *Econometrica*, *58*, 53-73.

Kimball, M. (1992). “Precautionary Motives for Holding Assets.” in *The New Palgrave Dictionary of Money and Finance,* Ed. by Peter Newman, Murray Milgate and John Eatwell. London: Macmillan Press Ltd.

Kimball, M., and Weil, P. (2009). “Precautionary saving and consumption smoothing across time and possibilities.” *Journal of Money, Credit and Banking, 41*, 245-284.

Leland, H. E. (1968). “Saving and uncertainty: The precautionary demand for saving.” *Quarterly Journal of Economics, 82*, 465-473.

Lepinteur, A. (2021). “The asymmetric experience of gains and losses in job security on health.” *Health Economics, 30*, 2217-2229.

Lepinteur, A., Clark, A. E., and D’Ambrosio, C. (2026). “Unsettled: job insecurity reduces homeownership.” *Scandinavian Journal of Economics*, forthcoming.

Lugilde, A., Bande, R. and Riveiro, D. (2018). “Precautionary saving in Spain during the great recession: Evidence from a panel of uncertainty indicators.” *Review of Economics of the Household, 16,* 1151-1179.

Lugilde, A., Bande, R., and Riveiro, D. (2019). “Precautionary saving: A review of the empirical literature.” *Journal of Economic Surveys, 33,* 481-515.

Lusardi, A. (1998). “On the importance of the precautionary saving motive.” *American Economic Review, 88,* 449-453.

Michelacci, C., and Ruffo, H. (2015). “Optimal life cycle unemployment insurance.” *American Economic Review, 105,* 816-859.

Nieto, A. (2022). “Can subsidies to permanent employment change fertility decisions?” *Labour Economics, 78,* 102219.

O’Higgins, N., and Pica, G. (2020). “Complementarities between labour market institutions and their causal impact on youth labour market outcomes.” *BE Journal of Economic Analysis & Policy, 20,* 20180165.

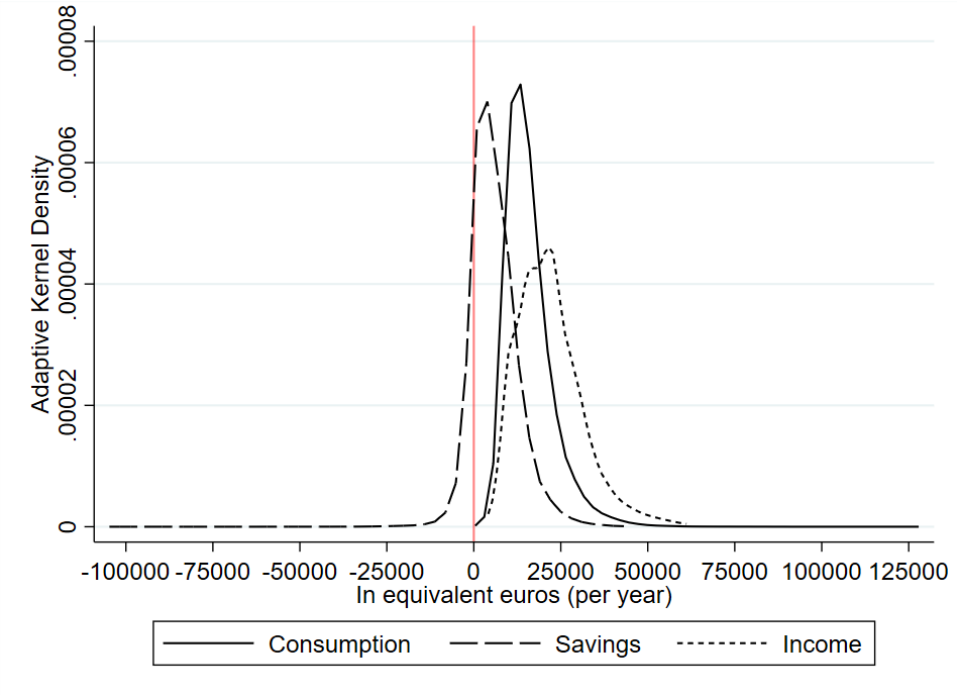
Pieroni, L., d’Agostino, G., Lanari, D., and Scarlato, M. (2023). “Temporary employment and fertility in Italy: The effect of two labor market reforms in the early 2000s.” *Economic Modelling, 124,* 106298.

Sandmo, A. (1970). “The effect of uncertainty on saving decisions.” *Review of Economic Studies, 37,* 353-360.

Tiraboschi, M. (2012). “Italian labour law after the so-called Monti-Fornero reform (law no. 92/2012).” *E-Journal of International and Comparative Labour Studies, 1*, 47-86.

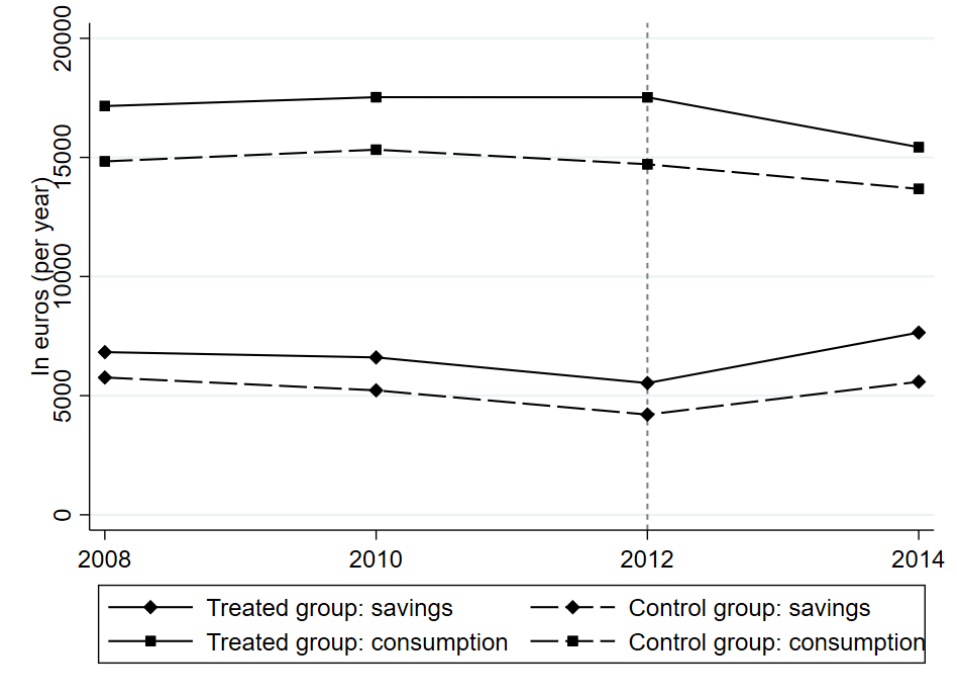
Figures and Tables

Figure 1: The Distributions of Consumption, Savings and Income in the Estimation Sample



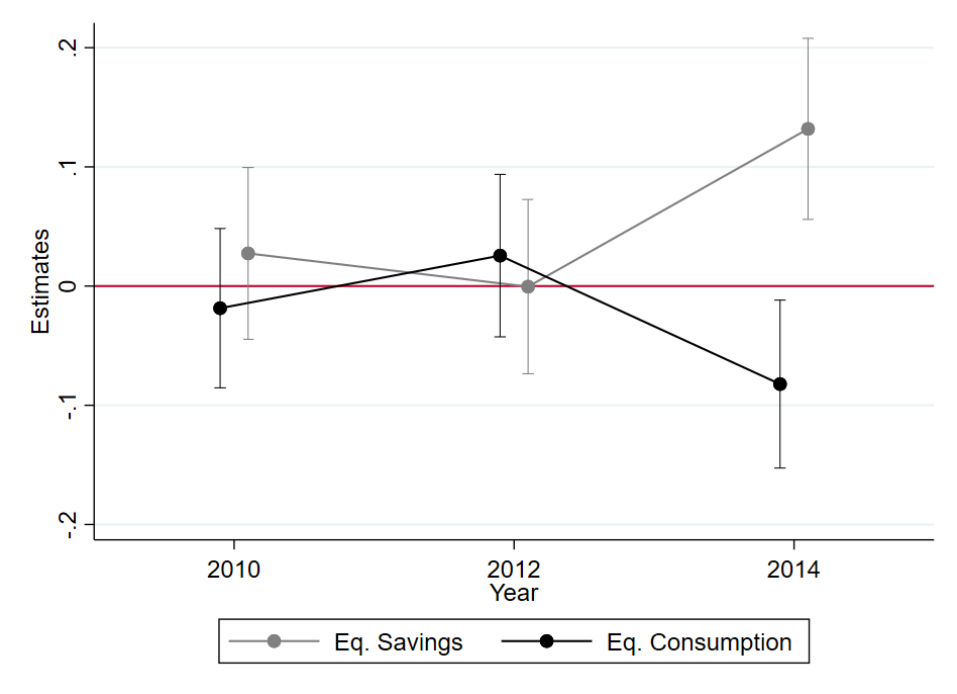
Notes: Data from 12,572 observations on permanent private-sector employees in the 2008, 2010, 2012 and 2014 SHIW. 572. All variables are expressed in annual Euros (at constant 2010 prices), and equivalised using the square root of household size.

Figure 2: Parallel Trends in Equivalent Annual Savings and Consumption – Non-parametric Approach



Notes: Data from 12,572 observations on permanent private-sector employees in the 2008, 2010, 2012 and 2014 SHIW. This figure 572plots average equivalent annual savings and consumption in the control and treatment groups over time. The short-dashed vertical line refers to the year in which the Fornero reform was introduced All variables are expressed in annual Euros (at constant 2010 prices), and equivalised using the square root of household size.

Figure 3: Parallel Trends in Equivalent Annual Savings and Consumption – Parametric Approach



Notes: Data from 12,572 observations on permanent private-sector employees in the 2008, 2010, 2012 and 2014 SHIW. 572Each dot is a point estimate in units of the standard deviation of the dependent variable for the difference between the treatment and control groups over time (using the 2008 difference as the reference) from a linear regression controlling for a dummy for workers in treated firms, year dummies, age, age-squared, gender, marital status, homeownership, and occupation, education and region dummies. Standard errors in parentheses are clustered at the individual level and the bars refer to 90% confidence intervals.

Table 1: Descriptive Statistics – Estimation Sample

	Mean	SD	Min	Max
Dependent Variables. Household Equivalised:				
Disposable income	21928	9534	5363	59916
Consumption	15909	7067	1200	127083
Savings	6019	6922	-103899	42302
Difference-in-Differences Variables:				
Treatment group	0.55		0	1
Post period	0.24		0	1
Socio-Demographic Characteristics:				
Age	42.14	10.06	18	60
Female	0.40		0	1
At least Secondary education	0.50		0	1
Married	0.62		0	1
Homeowner	0.69		0	1
Blue-collar worker (or similar)	0.57		0	1
Office worker (or similar)	0.36		0	1
Managers	0.07		0	1

Notes: Data from 12,572 observations on permanent private-sector employees in the 2008, 2010, 2012 and 2014 SHIW. All dependent variables are expressed in annual Euros (at constant 2010 prices) and equivalised using the square root of household size.

Table 2: The Effect of the Fornero Reform on Equivalent Annual Savings - OLS Results

	Savings (std)		
	(1)	(2)	(3)
Treatment (Post 2012)	0.085**		
	(0.035)		
Treatment (2012)		0.016	-0.013
		(0.040)	(0.037)
Treatment (2014)		0.123***	0.119**
		(0.044)	(0.039)
Observations	12572	12572	12572
Controls	No	No	Yes
Adjusted R^2	0.012	0.020	0.184

Notes: Data from permanent private-sector employees in the 2008, 2010, 2012 and 2014 SHIW. These are linear regressions. Standard errors in parentheses are clustered at the individual level. All regressions include a dummy for workers in treated firms and year dummies. The control variables are age, age-squared, gender, education, marital status, homeownership, and occupation and region dummies. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: The Effect of the Fornero Reform on Equivalent Annual Savings - Robustness Checks

	Equivalised Annual Savings (std)					
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment (2014)	0.119***	0.086**	0.134***	0.099**	0.112***	0.092*
	(0.039)	(0.040)	(0.039)	(0.040)	(0.039)	(0.052)
Observations	12572	12572	12572	12572	12326	8199
Adjusted R^2	0.184	0.183	0.202	0.116	0.171	0.188

Notes: Data from permanent private-sector employees in the 2008, 2010, 2012 and 2014 SHIW. These are linear regressions. Standard errors in parentheses are clustered at the individual level. All regressions include the following controls: a dummy for workers in treated firms, year dummies, age, age-squared, gender, education, marital status, homeownership, and occupation and region dummies. Column 1 reproduces the baseline results. Column 2 uses the OECD equivalence scale. Column 3 considers unequivalised savings and controls for household size. Column 4 applies an IHS transformation of annual equivalent savings (with an equivalence scale based on the square root of household size). Column 5 trims the top and bottom 1% of the savings distribution. Column 6 is a donut DiD that excludes the respondents in the firm-size categories just below and above the 15-employee threshold. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

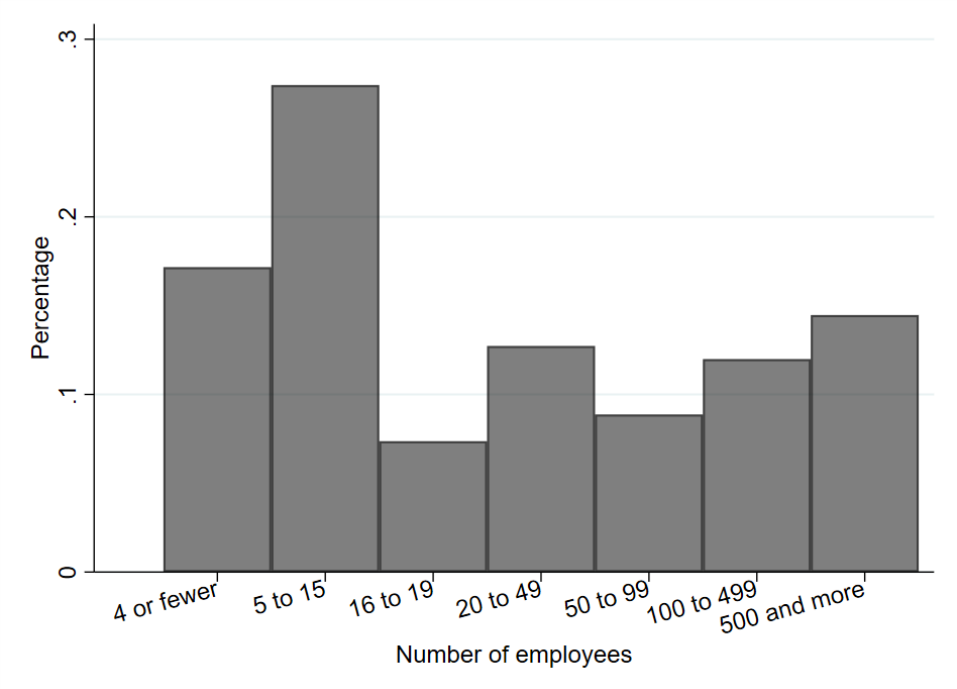
Table 4: The Effect of the Fornero Reform on Equivalent Annual Savings - Heterogeneity

	Equivalised Annual Savings (std)				
	Risk Aversion		Initial Wealth		
	Low	High	Low	Medium	High
	(1)	(2)	(3)	(4)	(5)
Treatment (2014)	0.065	0.175***	-0.002	0.140***	0.082
	(0.059)	(0.055)	(0.086)	(0.051)	(0.099)
Observations	6263	6309	2755	7309	2508
Adjusted R^2	0.180	0.183	0.225	0.168	0.136

Notes: Data from permanent private-sector employees in the 2008, 2010, 2012 and 2014 SHIW. These are linear regressions. Standard errors in parentheses are clustered at the individual level. All regressions include a dummy for workers in treated firms, year dummies, age, age-squared, gender, education, marital status, homeownership, and occupation and region dummies. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

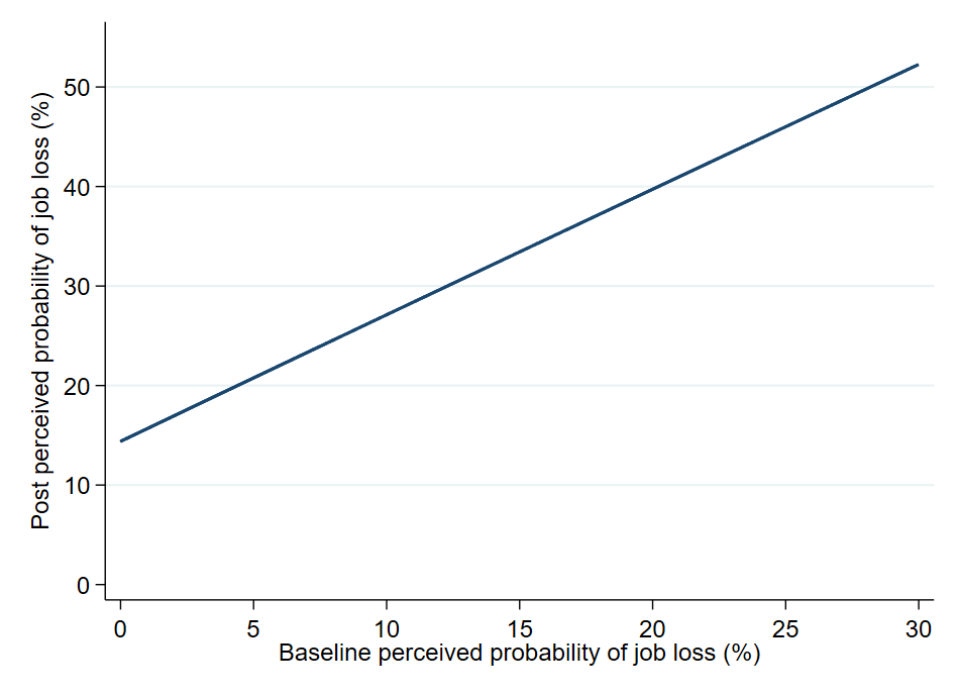
Appendix:

Figure A1: The Distribution of Self-Reported Firm Size



Note: Data from 12,572 observations on permanent private-sector employees in the 2008, 2010, 2012 and 2014 SHIW.

Figure A2: The Implied Increase in Perceived Job-Loss Risk Under Alternative Baseline Risks



Notes: The figure plots the increase in the perceived annual probability of job loss that is consistent with the estimated rise in equivalised annual savings figure of €826. The calculation is based on a two-period precautionary-saving model with CRRA utility ($\gamma = 2$), pre-reform equivalised household income of €24,058 and post-job-loss equivalised income of €16,668. The X-axis shows the assumed baseline perceived probability of job loss and the Y-axis the post-reform perceived risk that is consistent with the estimated saving response.

Table A1: The Effect of the 2012 Fornero Reform on Annual Savings: Full OLS Results

	Savings (std)		
	(1)	(2)	(3)
Treatment	0.085**		
	(0.035)		
Treatment (2012)		0.016	-0.013
		(0.040)	(0.037)
Treatment (2014)		0.123***	0.119***
		(0.043)	(0.039)
Age			-0.060***
			(0.007)
Age-squared/100			0.068***
			(0.009)
Female			0.018
			(0.020)
At least Secondary education			0.071***
			(0.022)
Married			-0.168***
			(0.021)
Homeowner			0.531***
			(0.019)
Blue-collar worker			-0.662***
			(0.052)
Office-workers			-0.390***
			(0.051)
Observations	12572	12572	12572
Adjusted R^2	0.012	0.020	0.184

Notes: Data from permanent private-sector employees in the 2008, 2010, 2012 and 2014 SHIW. These are linear regressions. Standard errors in parentheses are clustered at the individual level. All regressions include a dummy for workers in treated firms and year dummies. Column (3) also includes region dummies. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A2: Treatment-Control Differences in Variables: Pre- and Post-Reform

	Before the Fornero Reform			After the Fornero Reform			
	Treated	Control	Difference	Treated	Control	Difference	DiD
Age	42.81	39.94	2.87***	45.14	42.92	2.22***	-0.65
	[9.52]	[10.47]	(0.20)	[9.19]	[10.43]	(0.37)	(0.42)
Female	0.34	0.44	-0.10***	0.37	0.48	-0.11***	-0.01
	[0.48]	[0.50]	(0.01)	[0.48]	[0.50]	(0.02)	(0.02)
At least Secondary education	0.54	0.43	0.10***	0.58	0.47	0.11***	0.01
	[0.50]	[0.50]	(0.01)	[0.49]	[0.50]	(0.02)	(0.02)
Homeowner	0.73	0.62	0.11***	0.76	0.66	0.11***	-0.00
	[0.44]	[0.49]	(0.01)	[0.43]	[0.47]	(0.02)	(0.02)
Household size	3.22	3.21	0.01	3.20	3.16	0.04	0.03
	[1.17]	[1.24]	(0.02)	[1.18]	[1.30]	(0.05)	(0.05)
Married	0.67	0.56	0.11***	0.66	0.57	0.09***	-0.02
	[0.47]	[0.50]	(0.01)	[0.47]	[0.50]	(0.02)	(0.02)
Blue-collar workers	0.51	0.66	-0.16***	0.49	0.63	-0.14***	0.01
	[0.50]	[0.47]	(0.01)	[0.50]	[0.48]	(0.02)	(0.02)
Office workers	0.39	0.31	0.08***	0.41	0.34	0.07***	-0.02
	[0.49]	[0.46]	(0.01)	[0.49]	[0.47]	(0.02)	(0.02)
Managers	0.10	0.03	0.07***	0.10	0.03	0.07***	0.00
	[0.30]	[0.16]	(0.00)	[0.30]	[0.17]	(0.01)	(0.01)

Notes: These figures refer to our estimation sample of 12,572 observations. Standard deviations appear in square brackets and standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A3: The Effect of the Fornero Reform on Equivalent Annual Savings with Different Estimation Samples - OLS Results

	Savings (std)					
	Excluding the Transportation and Banking Sectors			Excluding Workers with Annual Earnings Below 26000 Euros		
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment (Post 2012)	0.102***			0.124**		
	(0.037)			(0.054)		
Treatment (2012)		0.016	-0.011		0.044	-0.004
		(0.042)	(0.039)		(0.063)	(0.061)
Treatment (2014)		0.151***	0.134***		0.168**	0.145**
		(0.045)	(0.041)		(0.068)	(0.063)
Observations	11049	11049	11049	5955	5955	5955
Controls	No	No	Yes	No	No	Yes
Adjusted R^2	0.008	0.018	0.178	0.003	0.013	0.126

Notes: Data from permanent private-sector employees in the 2008, 2010, 2012 and 2014 SHIW. These are linear regressions. Standard errors in parentheses are clustered at the individual level. All regressions include a dummy for workers in treated firms and year dummies. The control variables are age, age-squared, gender, education, marital status, homeownership, and occupation and region dummies. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A4: The Effect of the Fornero Reform on Equivalent Annual Savings when Accounting for the Specificity of the Data Collection – OLS Results

	Savings (std)			
	Sample: at the individual level		Sample: at the household level	
	(1)	(2)	(3)	(4)
Treatment (2014)	0.119***	0.085*		
	(0.039)	(0.049)		
Treatment (2014) * Firm size reported by another household member		0.069		
		(0.081)		
Share of treated workers in the household (2014)			0.108**	
			(0.045)	
At least one treated worker in the household (2014)				0.096**
				(0.044)
Observations	12572	12572	9961	9961
Adjusted R^2	0.184	0.195	0.195	0.196

Notes: Data from permanent private-sector employees in the 2008, 2010, 2012 and 2014 SHIW. These are linear regressions. Standard errors in parentheses are clustered at the individual level in columns 1 and 2, and robust standard errors are reported in columns 3 and 4. All regressions include year dummies. The control variables in columns 1 and 2 are age, age-squared, gender, education, marital status, homeownership, and occupation and region dummies. The control variables in columns 3 and 4 are the average age of workers in the household; the shares of household workers who are women, have at least secondary education, are married, or are blue-collar workers, office workers, or managers. These regressions also include homeownership and region dummies. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: The Effect of the 2012 Fornero Reform on Weekly Hours of Work, Individual Earnings, Number of Income Earners in the Household, and Employment Income Earners in the Household – OLS results

	Weekly Hours of Work		Individual Earnings		No. Income Earners		No. Employment Income Earners	
	(1)	(2)	(3)	(4)	(5)	(6)		
Treatment (2014)	0.054	0.048	0.053	0.048	0.038	0.014	0.051	0.033
	(0.045)	(0.042)	(0.042)	(0.037)	(0.045)	(0.042)	(0.046)	(0.042)
Observations	12572	12572	12572	12572	12572	12572	12572	12572
Controls	No	Yes	No	Yes	No	Yes	Yes	Yes
Adjusted R^2	0.005	0.132	0.045	0.269	0.002	0.148	0.001	0.161

Notes: Data from permanent private-sector employees in the 2008, 2010, 2012 and 2014 SHIW. These are linear regressions. All dependent variables are standardised. Standard errors in parentheses are clustered at the individual level. All regressions include a dummy for workers in treated firms, year dummies, and an interaction between treatment and 2012 (as in columns 2 and 3 of Table 2). The controls are age, age-squared, gender, education, marital status, homeownership, and occupation and region dummies. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: The Effect of the 2012 Fornero Reform on Equivalent Annual Household Income, Savings and Consumption – OLS results

	Annual household Income (std)		Savings (std)		Consumption (std)	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment (2014)	0.035	0.032	0.119***	0.096***	-0.074*	-0.094***
	(0.042)	(0.033)	(0.039)	(0.031)	(0.038)	(0.031)
Annual household income				0.714***		0.650***
				(0.013)		(0.012)
Observations	12572	12572	12572	12572	12572	12572
Controls	No	Yes	Yes	Yes	Yes	Yes
Adjusted R^2	0.041	0.420	0.184	0.479	0.255	0.500

Notes: Data from permanent private-sector employees in the 2008, 2010, 2012 and 2014 SHIW. These are linear regressions. Standard errors in parentheses are clustered at the individual level. All regressions include a dummy for workers in treated firms, year dummies, and an interaction between treatment and 2012 (as in columns 2 and 3 of Table 2). The controls are age, age-squared, gender, education, marital status, homeownership, and occupation and region dummies. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A7: The Effect of the Fornero Reform on Equivalent Annual Consumption by Type of Expenditure – OLS Results

	Non-Durable Expenditure (std)		Durable Expenditure (std)	
	Food	Other	Means of Transport	Other
	(1)	(2)	(3)	(4)
Treatment (2014)	0.000	-0.041	-0.084**	-0.103**
	(0.043)	(0.040)	(0.042)	(0.047)
Observations	12572	12572	12572	12572
Adjusted R^2	0.116	0.162	0.014	0.022

Notes: Data from permanent private-sector employees in the 2008, 2010, 2012 and 2014 SHIW. These are linear regressions. Standard errors in parentheses are clustered at the individual level. All regressions include a dummy for workers in treated firms, year dummies, an interaction between treatment and 2012 (as in columns 2 and 3 of Table 2), age, age-squared, gender, education, marital status, homeownership, and occupation and region dummies. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A8: The Effect of the Fornero Reform on Equivalised Annual Savings - Heterogeneity

	Equivalised Annual Savings (std)									
	Male	Female	Below Age 45	Age 45 and Above	Not Partnered	Partnered	No Children in HH	Children in HH	Below Median Income	Above Median Income
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treatment (2014)	0.099**	0.147**	0.128**	0.104*	-0.134	0.133***	0.021	0.176***	0.008	0.162**
	(0.049)	(0.065)	(0.057)	(0.055)	(0.122)	(0.042)	(0.062)	(0.050)	(0.055)	(0.067)
Observations	7600	4972	6448	6124	1137	11435	4668	7904	7004	5568
Adjusted R^2	0.216	0.151	0.225	0.160	0.237	0.189	0.163	0.200	0.116	0.075

Notes: Data from permanent private-sector employees in the 2008, 2010, 2012 and 2014 SHIW. These are linear regressions. Standard errors in parentheses are clustered at the individual level. All regressions include a dummy for workers in treated firms, year dummies, an interaction between treatment and 2012 (as in columns 2 and 3 of Table 2), age, age-squared, gender, education, marital status, homeownership, and occupation and region dummies. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.