DUAL CREDIT MARKETS:
INCOME RISK, HOUSEHOLD DEBT, AND CONSUMPTION

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ABSTRACT

Many young employees work on a temporary basis, which entails significantly greater income risk than "permanent" work, even for jobs in the same occupation and at a similar wage. We find that this income uncertainty leads lenders to ration credit to temporary workers, precisely at the stage of life when permanent workers rely on mortgages to invest in housing and loans to smooth consumption and purchase durable goods. Labor laws that improve job security for permanent workers create a dual credit market alongside the dual labor market, making it harder for young adults to establish financial independence and new families.

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1. Introduction

More than 27 million Europeans work on a temporary basis under short, fixed-term contracts. About 24 million Americans work on a similar basis as independent contractors (Abraham et al., 2024). In both economies, temporary jobs are filled disproportionately by the young, while "permanent" jobs of indefinite duration are filled by older workers, creating a so-called dual labor market (Saint-Paul, 1997). Employing workers on temporary contracts preserves operational flexibility for employers that face frictions in firing permanent workers. Providing flexibility to firms, however, entails uncertainty for temporary workers, who are the first to lose their jobs in business downturns.

In this paper, we show that dual labor markets beget dual credit markets. By exposing workers to job insecurity, temporary work severely limits workers' ability to borrow and consume. These effects are particularly acute for young adults, for whom temporary work can make it difficult to establish financial independence. Borrowing, which figures prominently in lifecycle consumption models, is critical for young adults who expect long-term income growth but have little savings. Such workers must borrow against future income to finance consumption, purchase durable goods, and invest in housing. However, giving credit to temporary workers is risky for lenders, not only because temporary workers are more likely to default,[1] but also because their defaults cluster at the worst possible time: recessions. The choice for lenders is to price and absorb such non-diversifiable risk or to control it by limiting lending (Musto and Souleles, 2006). The latter approach is common, especially when adverse selection prevents price adjustments from clearing the market (Jaffee and Russell, 1976; Stiglitz and Weiss, 1981). Lenders therefore routinely underwrite credit based on "W-2 income" in the US and labor contract type in Europe, demoting temporary workers to a lower rung of the credit market.

We study the consequences of temporary work for household finances and consumption using household survey data from Europe and the United States. These detailed survey data afford several advantages in our analysis. They allow us to compare temporary workers with permanent workers of similar demographics, educational attainment, and job characteristics, such as occupation, industry, and income. Panel data, available for three European countries, reveal how a given worker's borrowing and consumption change upon moving from temporary to permanent

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[1] Gerardi et al. (2018), Hsu, Matsa and Melzer (2018), and Keys (2018) show job loss to be a material factor in loan default and bankruptcy.

work. Lastly, the detailed financial variables include not only indebtedness, an equilibrium outcome, but also credit applications and denials, allowing us to distinguish credit supply from credit demand and to examine credit rationing.

We find that lenders ration credit to temporary workers, who consequently borrow less and reduce their purchases of housing and durables. Temporary workers are over 4 percentage points more likely than permanent workers to have a credit application denied, an increase of one-third relative to the sample mean. Temporary workers are also 3 percentage points more likely to withhold applications because they anticipate rejection. Consistent with credit rationing caused by adverse selection, the increases in loan denials and discouraged applications are largest among temporary workers in occupations with more private information on job loss risk. An important consequence of the lower credit supply to temporary workers is that they are less able to afford housing and vehicle purchases, which are typically financed with debt. Temporary workers are 30% less likely to purchase a home and their vehicles are worth 30% less.

Turning to the question of *why* temporary workers differ in their borrowing and consumption, we analyze the central economic difference between temporary and permanent work: job stability. A subsample of HFCS respondents estimate their likelihood of job loss over the coming year. From those responses we compute the average risk of job loss by occupation and contract type. This measure correlates strongly with measures of unemployment, such as receipt of unemployment benefits. The risk of job loss varies meaningfully among permanent workers in different occupations, and the risk among temporary employees is roughly twice that of permanent workers. The latter difference holds not only unconditionally but also within occupation groupings. Using the variation across occupations and contract type, we show that greater job loss risk is associated with reduced access to credit and durables consumption. The magnitudes of these effects are large enough to explain nearly the entire gap in borrowing and consumption between temporary and permanent workers, suggesting that lenders reject temporary workers because of their income uncertainty.

These empirical relations face an identification concern: if labor market sorting leads lower ability workers with less promising futures into riskier jobs, they might receive less credit and reduce consumption even if income risk has no causal impact. We explore the relevance of labor market sorting through two methods. First, we use panel data, available for three European countries, to identify the effects of temporary work from the changes in credit access and

consumption that workers experience when they transition from temporary to permanent work or vice versa. This analysis includes worker fixed effects to account for persistent differences in ability that might correlate with contract status. The results confirm our baseline finding that temporary contracts reduce workers' credit access. After individuals transition to permanent work, they are also more likely to purchase homes and vehicles. Second, we analyze the variation in job loss risk created by labor market regulations that protect permanent workers, such as mandated severance payments. In countries with stronger employment protections, permanent workers experience fewer layoffs, particularly in ex-ante riskier occupations. This compression of job loss risk across occupations provides identifying variation. We show that as employment protections reduce job loss risk between high- and low-risk occupations, so too do they narrow the credit and consumption differences between those occupations. These results, which are conditional on both occupation and country fixed effects, are consistent with the regulation-induced rise in job security leading to increases in credit supply, borrowing, and consumption. We conclude that temporary work and the income uncertainty it entails indeed reduce credit access and spending on durables.

Despite these reductions in credit and consumption, temporary jobs may nevertheless be attractive to workers who value flexibility (Chen, Chevalier, Rossi and Oehlsen, 2019) or supplemental income during a spell between permanent jobs (Koustas, 2018; Fos, Hamdi, Kalda and Nickerson, forthcoming; Jackson, 2022). Most workers on temporary contracts cannot find permanent work; but for others, who choose to work on a temporary basis, the benefits apparently outweigh the costs. The welfare implications of our findings depend on whether the negative credit consequences are concentrated among workers choosing to work on a temporary basis versus those who are forced to do so. While we cannot distinguish these workers in the European data, we can in U.S. data. For several cohorts, the Contingent Work Supplement of the Current Population Survey (CPS) identifies workers who work on a temporary basis and whether they would prefer a permanent contract. We show that temporary workers in the US are less likely to own a home, just like in Europe, and that this effect is driven entirely by workers who would prefer to work on a permanent basis.

Our findings highlight disadvantages of temporary work by comparing temporary jobs to permanent ones. Although comparing temporary workers to the long-term unemployed could make temporary contracts look more favorable, existing research suggests they are not the relevant comparison group. Typical policy interventions, such as strengthening protections for permanent

workers or relaxing constraints on temporary hiring, shift the composition of temporary and permanent workers rather than changing total employment. For example, a recent study of an Italian reform by Daruich et al. (2023) shows that allowing firms to use more temporary contracts reduced the likelihood of workers converting from temporary into permanent jobs without changing overall hiring. Analyses of employment protection reforms in France (Blanchard and Landier 2002) and Spain (Garcia-Perez et al. 2019) reach similar findings.

Our analysis points to an unintended cost of strict employment protections, which aim to reduce workers' income risk. Strict protections succeed in reducing the job loss risk of permanent workers, but perversely raise income risk for individuals who end up in temporary employment. The resulting credit market duality is not zero sum, as the older, permanent employees who benefit from protections need less credit than do the younger temporary workers who experience increased credit denials. Temporary workers' limited access to credit is particularly costly because these workers tend to be young and in need of loans to purchase homes, establish new families, and smooth their lifetime consumption. Temporary work is so widespread that the consequences also appear meaningful in the aggregate. Comparing across European countries, we find that where temporary contracts are more prevalent, young adults are less likely to own a home, live on their own, get married, or have children.

Our findings point to important determinants of consumer credit access that have not received much attention in the literature: employment contract status and employment protection regulation. Other key determinants of credit access include bankruptcy, foreclosure, and collateral repossession laws (Pence, 2006; Livshits, MacGee and Tertilt, 2007; Assuncao, Benmelech and Silva, 2014), social insurance generosity (Hsu, Matsa and Melzer, 2014; Ru and Schoar, 2016; Bornstein and Indarte, 2022; Braxton, Herkenhoff and Phillips, forthcoming), information asymmetry (Adams, Einav and Levin, 2009) and pricing restrictions (Rigbi, 2013; Agarwal et al., 2015; Han, Keys and Li, 2018; Nelson, 2018; Cuesta and Sepulveda, 2021).

This study also contributes to the labor and finance literature studying how a firm's earnings uncertainty affects its workers. Firms provide their workers partial insurance against idiosyncratic shocks (Guiso, Pistaferri and Schivardi, 2005), particularly when public insurance is lacking (Ellul, Pagano and Schivardi, 2018). Firms also reduce financial risk when employment protections push operational risk onto firms and away from employees (Simintzi, Vig and Volpin, 2015; Serfling, 2016). Workers nevertheless face residual income uncertainty, particularly through

changes in variable pay, and respond to firm-level uncertainty by reducing their purchases of durables and housing (Di Maggio et al., 2022). Our work highlights the relevance of labor contracts, employment protection laws, and credit markets in mediating the pass-through of uncertainty from firms to workers. In particular, we document novel links between temporary workers' employment risk, credit access, and consumption.

Though labor and macro economists have written extensively on the rise of temporary work and its impact on job transitions and aggregate employment (Boeri, 2011), we are the first to characterize the dual credit market caused by a labor market comprised of temporary and permanent workers. Herkenhoff (2019), Herkenhoff, Phillips and Cohen-Cole (2021, forthcoming), and Buchak (2024) also consider interactions between household borrowing and labor markets, emphasizing primarily the effects of credit access on job search, unemployment dynamics, and self-employment. Our analysis focuses on the converse relationship: how features of the labor market affect credit access.

The remainder of the paper is organized as follows. Section 2 describes the data. Section 3 provides institutional background and stylized facts on temporary work. Sections 4 and 5 compare the credit and consumption of temporary and permanent workers in Europe and examine how job transitions, job loss risk, employment protection laws, and credit rationing shape these outcomes. Section 6 disaggregates the effect of temporary work on consumption in the United States depending on whether the worker prefers temporary work or is forced into it. Section 7 explores the broader societal consequences of temporary work limiting young adults' access to credit. We conclude in section 8.

2. Data

To study the credit and consumption consequences of fixed-term contracts, we analyze data from multiple household surveys in Europe and the United States. For Europe, we examine cross-sectional data from the European Household Finance and Consumption Survey (HFCS) and panel data from three country-specific surveys. The HFCS integrates data from statistical agencies of over twenty European countries to produce consistent measures of household finances, consumption, and employment. The three waves of the survey span 2009 to 2018, providing repeated cross-sectional data on more than 150,000 workers.

The HFCS includes a rich set of borrowing and employment outcomes. Table 1, Panel A, presents summary statistics for the main HFCS variables used in our analysis. We measure whether

respondents have applied for credit in the last three years and whether they decided not to apply in anticipation of rejection. For households that applied for credit, we observe if a credit application was rejected and whether the household ultimately obtained a loan. We also observe the origination and interest rate of households' mortgages, as well as their total debt obligations, which we measure normalized by income for households with debt. In addition to credit outcomes, we also use the HFCS to measure households' purchase of homes and cars, including the current value of these assets.

Italy, the Netherlands, and Spain also gather panel data, tracking households' employment, finances, and consumption over time through repeated surveys of the same households. The Bank of Italy's Survey on Household Income and Wealth (SHIW) provides biannual panel data on 4,000 households per wave between 2000 and 2020. The De Nederlandsche Bank's Household Survey (DNBHS), administered by Centerdata at Tilburg University in the Netherlands, provides annual panel data on 2,000 households per wave between 1993 and 2021 in the Netherlands. Finally, the Bank of Spain's Survey of Household Finances (EFF) provides triannual panel data on over 2,000 households per wave between 2002 and 2017. The data available from each of these surveys do not align perfectly with the HFCS or with each other. For example, we observe credit applications for Spain and the Netherlands, but not for Italy. The rejection measure is thus conditional on applying for credit in Spain and the Netherlands, but unconditional in Italy. Only the Dutch survey elicits a forward-looking self-assessment of credit worthiness that is not in the other surveys. The surveys also differ in their interview frequency. Appendix A explains how we harmonize the survey information and define the key variables in each country in our analysis. Table 1, Panel B summarizes the panel data.

While much of our analysis examines dual labor markets in Europe, we also analyze temporary work in the United States, where we can differentiate between workers who seek out temporary work and those who are forced into it. While some individuals choose temporary work because it fits into their school schedule or because it allows them to fulfill family obligations, others work on a temporary basis because they cannot find permanent employment. The Contingent Work Supplement of the US Bureau of Labor Statistic's Current Population Survey (CPS), which is available in 1995, 1997, 1999, 2001, 2005, and 2017, asks temporary workers explicitly whether they would prefer permanent employment. We classify temporary work as being either forced or voluntary based on this response and pair these data with information on workers'

homeownership and demographic characteristics from the CPS monthly survey.[2] As indicated by the summary statistics in Table 1, Panel C, two-thirds of temporary workers would prefer permanent employment.

The HFCS and the CPS samples are representative of the European and American populations, respectively. In both datasets, the average age is about 43 years, men and women are equally represented, and about 70% of respondents have ever been married. Homeownership rates are about ten percentage points higher in the US than in Europe. The surveys making up the three-country panel oversample higher income households, resulting in a sample that skews slightly towards older men.

3. Temporary Work: Institutional Background and Stylized Facts

Many labor markets are composed of two tiers of jobs. The first tier typically represents regular jobs that come with fringe benefits and substantial job security, while the second tier represents various forms of irregular employment that have lower job security and limited benefits. In European labor markets, the first tier consists of workers with permanent employment contracts who have strong employment protection, while the second tier includes workers who have temporary contracts and far fewer legal protections. Despite workers having weaker formal employment protections, the U.S. labor market also has a de facto lower tier of workers, who have less job security and absorb most short-term economic fluctuations (Ahn, Hobijn, and Sahin 2023).

In theory, temporary contracts can have mixed effects on worker welfare. They have the potential to provide a stepping-stone to permanent employment (Booth et al. 2002) but may impede human capital development (Kaiser, 2024) and lead to a "dead-end" of temporary contracts intertwined with spells of non-employment (Autor and Houseman, 2010; Gagliarducci, 2005). Similarly, temporary work eases some workers' transitions out of unemployment (Berton and Garibaldi, 2010), but leaves them at elevated risk of unemployment later (Güell and Petrongolo, 2007).

Recent evidence suggests that temporary contracts hurt workers' career prospects on net and over the long term. For example, Daruich et al. (2023) use employer-employee linked microdata to examine the labor market effects of "partial" employment protection reforms

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[2] The CPS does not measure meaningful credit outcomes in samples that overlap with the Contingent Work Supplement.

allowing greater use of temporary contracts in Italy. They find that temporary contracts substitute for permanent contracts rather than unemployment. Allowing firms to use more temporary contracts reduced workers' likelihood converting from temporary into permanent jobs by almost 32% (60% for workers aged 25 or less). Instead of raising total employment, the reform increased the number of workers bouncing from one temporary job to another. Analyses of partial reforms in France (Blanchard and Landier 2002) and Spain (Garcia-Perez et al. 2019) reach similar findings.

Temporary contracts are used throughout the European economy. Figure 1 plots the prevalence of temporary contracts across roughly 50 occupations. Most occupations have over 10% temporary workers and the median occupation has 14%. Table 2, Panel A, lists occupations with the largest and smallest shares of temporary work. Temporary work is most prevalent among agricultural, mining and food services workers, and least prevalent among managers and professionals working in information technology or engineering.

The prevalence of temporary contracts varies across countries. The map in Figure 2 shows variation in the share of workers with a temporary employment contract across countries in Europe. This share varies between 3% and 26%. These cross-country differences result from variation in employment protection regulations, taxes on permanent contracts, the sectoral structure of the economy, cultural norms, and other factors.

There is a steep age gradient in the prevalence of temporary contracts. Figure 3 plots the prevalence of temporary contracts by age. While only 9% of workers aged 35–65 work in temporary jobs, the share doubles for those aged 25–35 and quadruples for those below 25. Both economic and legal factors lead to temporary work being concentrated among young adults. As a stepping stone to regular, permanent employment, temporary contracts are a natural fit for workers who have recently entered the labor market. Regulations can also limit how many consecutive temporary contracts a worker can have with a given firm before the firm is obliged to offer them a permanent contract.

The most salient difference between temporary and permanent employment contracts is job security. The HFCS asks a subset of individuals to self-assess the likelihood of losing their job in the next 12 months. This measure is akin to the subjective assessments of income uncertainty examined by Guiso et al. (1992) and Guiso et al. (1996). A comparison of self-assessed job loss risk with unemployment benefit receipt, which is presented in Figure 4, suggests that the measure

is reliable: There is a strong occupation-contract type-level correlation between respondents' average subjective assessments of job loss and unemployment benefits receipt in the last year (correlation coefficient of 0.83).[3] Table 2, Panel B lists the occupations with the greatest and least average job loss risk for workers with permanent contracts. Workers with temporary contracts report, on average, a 40% likelihood of job loss. Permanent workers, by contrast, report a 16% likelihood of job loss, on average. As shown in Figure 5, the elevated job loss risk of temporary versus permanent workers is present across all occupations.

4. Employment Contracts, Credit Access, and Consumption

4.1. Credit Access and Usage among Temporary Workers

To understand the consequences of temporary jobs for credit access and consumption, we contrast the outcomes of workers that hold different types of employment contracts while accounting for differences explained by occupation, income, education, age, and other characteristics. In the baseline specification

$$Y_{it} = \beta \cdot Temporary_i + \gamma \cdot X_{it} + \varepsilon_{it}, \tag{1}$$

we regress a credit or consumption outcome Y for individual i in year t on *Temporary*, an indicator for whether they are working on a temporary contract, and a rich set of controls X. These controls include sex and immigrant indicators as well as fixed effects for the worker's country of residence (20 values), occupation (53 values), industry (23 values), age (68 values), and education level (5 categories), and the year of the observation (10 values). To control for income non-parametrically, X also includes 100 indicator variables for whether the individual's income falls within each of the 100 percentiles of their country's income distribution.

The estimation results, reported in Table 3, show that temporary workers receive less credit. Holding a temporary employment contract is associated with 2.3 percentage points lower credit receipt over the prior three years (column 1), which is nearly a 10% decline relative to the average share receiving credit. Temporary workers who do receive credit obtain less debt relative to their income. The average borrower has a debt-balance-to-income ratio of 1.48 and for temporary workers the debt-balance-to-income ratio is lower by 0.2, or 13% (column 2). This

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[3] In this analysis, each observation represents a distinct occupation and contract type (i.e., temporary or permanent). In each cell, the subjective assessments are higher than the unemployment benefits receipt, likely because not everyone who loses their job takes up unemployment benefits and because some respondents' assessments are too pessimistic.

shortfall in indebtedness includes a sizeable difference in mortgage borrowing, with a 1.3 percentage point lower likelihood of mortgage receipt (column 3). This reduction is 20% relative to the sample mean.

The remaining analysis within Table 3 explores whether temporary workers' reduced use of credit stems from reductions in their demand for credit (e.g., Fos, Hamdi, Kalda and Nickerson, forthcoming) or in lenders' supply of credit. Column 4 analyzes the relationship between temporary work and the price of credit. Temporary workers pay on average 10 basis points more in annual interest for their mortgages, which is roughly 3% higher than the average mortgage rate of 3.30%. The increase in price suggests that the lower quantity of credit reported in columns (1) to (3) results from reduced credit supply, not demand, as an inward shift in demand would result in declines in both the quantity and price of credit. The results reported in the remaining columns of Table 3 are consistent with this conclusion. Column (5) shows that temporary workers are 1.6 percentage points less likely to apply for credit, which represents a more than 5% decline relative to the average application rate of 29%. While fewer applications could in theory reflect reduced credit demand, column (6) shows that this reduction in applications is explained completely by workers who need credit but withhold applications because they anticipate being rejected. Temporary workers are 3.0 percentage points more likely to withhold credit applications. The fact that this decline exceeds temporary workers' overall shortfall in applications suggests that working on a temporary contract actually increases an individual's demand for credit, perhaps to help the worker absorb income shocks.

Banks' reluctance to lend to workers on temporary contracts is most directly evident in their evaluation of loan applications. Despite temporary workers withholding many loan applications, the applications they do submit are more likely to be rejected. Conditional on applying, temporary workers' rejection rate is 4.1 percentage points higher, which is an increase of nearly 35% from the 12.2% mean rejection rate (column 7). In a final analysis, reported in column (8), we combine the effects on applications being withheld and rejected. Looking at all workers, we code credit refusal as 1 if the worker has applied for credit and was rejected or if the worker has not applied anticipating rejection, and as 0 otherwise. This comprehensive measure of credit refusal is over 3 percentage points higher for temporary workers, which corresponds to a 40% decline vis-à-vis the sample average.

The substantial decline in the quantity of credit supplied to temporary workers stands out relative to the much smaller rise in their price of credit, implying that lenders are rationing credit to temporary workers. Though temporary workers pose greater credit risk due to their higher likelihood of job loss, lenders could adjust by approving those applicants for loans at higher interest rates. The fact that they instead reject riskier applicants hints at a deeper credit market failure. Because the rationing of credit is inefficient, it constitutes a welfare cost of the employment protections that create dual labor and credit markets.

4.2. Adverse Selection and Credit Rationing

We next explore an underlying reason for the credit market failure – temporary workers' private information about their risk of job loss. We use workers' subjective reports about their job security to examine whether lender's imperfect information about job loss risk causes them to ration credit, as in Stiglitz and Weiss (1981). Temporary contracts are likely to exacerbate the effects of workers' private information about their productivity and job prospects, creating adverse selection. Whereas productivity has a limited effect on the job security of permanent employees, who are protected by employment regulations, it can substantially influence the job loss risk of temporary workers. Because workers know more about their productivity than do lenders, this asymmetric information can lead to adverse selection, wherein high-productivity workers choose not to borrow at prices calibrated to the average risk of job loss and default. Their opting out lowers the average productivity among borrowers, raising the expected default risk and offered interest rates for the remaining borrowers. This adverse selection in the pool of borrowers gives lenders an incentive to ration credit rather than to clear the market by adjusting the interest rate.

To explore the role of information asymmetries in credit supply, we examine whether temporary workers' credit applications are treated differently when they work in occupations with greater dispersion in job loss risk across individuals. In occupations with greater dispersion in risk, borrower's private information is more likely to lead to adverse selection in borrowing. We measure this uncertainty at the occupation level by computing the standard deviation of workers' estimated risk of job loss within that occupation. Rather than measuring the average level of risk in each occupation, this measure represents the degree of dispersion in job loss risk across individuals. In the presence of adverse selection, we would expect temporary workers' credit

applications to be rejected at a higher rate when they work in high-dispersion occupations (Stiglitz and Weiss, 1981).

To evaluate this prediction, we estimate a modified version of eq. (1) that allows for the difference in credit outcomes between temporary and permanent workers to vary with the degree of dispersion in job loss risk among workers in occupation o:

$$Y_{iot} = \beta \cdot Temporary_i + \phi \cdot Temporary_i \times High\ Dispersion_o + \boldsymbol{\gamma} \cdot \boldsymbol{X}_{iot} + \varepsilon_{iot}, \quad (2)$$

where $High\ Dispersion$ is an indicator for above-median dispersion. The standard errors are adjusted for clustering at the occupation level. Table 4 reports the results.

We find that temporary workers' credit applications are indeed more likely to be rejected in occupations with greater dispersion in job loss risk. Whereas temporary workers are 2.6 percentage points more likely to be rejected for credit in low-dispersion occupations ($p < 0.01$), temporary workers in high-dispersion occupations face nearly twice as large of an increase in the rate of credit rejections (2.4 percentage points larger; $p < 0.05$; column 1). In high-dispersion occupations, temporary workers are likewise more likely to either withhold a credit application or be rejected ($p < 0.01$; column 2). Analysis of mortgage interest rates, meanwhile, shows a small (relative to the sample mean) and statistically insignificant differential increase in rates paid by high-dispersion-occupation temporary workers who receive credit. Together, these findings are consistent with temporary workers facing credit rationing, whereby lenders concerned about adverse selection reject many applications rather than using risk-based pricing. Lenders' actions have important welfare implications for dual credit markets, which we discuss below in our conclusions.

4.3. Effects on Durable Consumption and Housing Investments

With reduced access to credit, temporary workers might be unable to afford housing and durable goods investments, which are typically debt financed. We investigate this hypothesis by regressing measures of durable consumption on the indicator for temporary contract and controls as described in eq. (1). Table 5 shows the estimation results, which confirm that temporary workers consume significantly fewer durables than otherwise similar workers who are on permanent contracts. Temporary workers are 1.3 percentage points less likely to have bought a house in the last three years (column 1)—an almost 30% reduction from the sample mean of 4.5%. Furthermore, the homes they do buy are worth about 7% less (column 2). While temporary workers

are not significantly less likely to have purchased a car in the last 12 months (column 3), the cars that they buy are roughly 16% cheaper (columns 4). Temporary workers' stock of cars is also worth about a third less than those of permanent workers with the same income, age, and other characteristics (column 5).

In summary, our analysis of credit usage, applications, rejections, and interest rates supports the conclusion that temporary workers face significantly reduced credit supply and, as a result, reduce their consumption of durables. These findings are consistent with lenders responding to temporary workers' greater income uncertainty, a conjecture we explore in Section 5.

4.4. Employment Transitions

Our analysis of temporary work and household borrowing in Tables 3 and 5 begs the question: do workers in less secure jobs receive less credit because of their contract status or is there some unobserved characteristic that prevents them from obtaining both credit and stable work? We evaluate this issue using panel data from Italy, the Netherlands, and Spain.

We first use the panel data to study credit applications and denials in a variant of eq. (1) that is augmented to include worker fixed effects, ω:

$$Y_{it} = \beta \cdot Temporary_{it} + \boldsymbol{\gamma} \cdot \boldsymbol{Z}_{it} + \omega_i + \varepsilon_{it}. \tag{3}$$

The coefficient estimates for β, which are reported in Table 6, reveal how a given worker's credit outcomes differ when they are in a temporary job as compared to a permanent job.[4] All identifying variation is within worker, relying only on workers who experience transitions in contract status from temporary to permanent or vice versa. The worker fixed effects strengthen the identification by controlling for any unobserved differences in worker quality that are not already captured by the age, education, occupation, and income controls.

The regression estimates confirm that temporary work is associated with fewer credit applications and more denials. On average, workers apply for credit 1.9 percentage points less often during periods of temporary work than they do during periods of permanent work (column 1). Applications are also more likely to be rejected when workers apply for credit while in a temporary job. Denials are 0.7 percentage points greater during periods of temporary work as

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[4] Other than the worker fixed effects, the controls are similar to those in eq. (1). They include within-country income percentile and country-specific fixed effects for age, education, occupation, industry, and year. See Appendix A for details.

compared to periods of permanent work (column 2). Lastly, many workers expect their credit application to be rejected while in a temporary job. The share expecting a rejection is 1.9 percentage points higher during periods of temporary work than during periods of permanent work, a more than 10% increase relative to the sample average of 18% (column 3). The magnitude of the rise in expected rejections fully accounts for the decline in applications among workers in temporary jobs.

We also use the panel data to examine how workers adjust their consumption after transitioning from a temporary to a permanent contract. When analyzing consumption outcomes, we focus on transitions to permanent contracts because these transitions expand a worker's ability to borrow and consume. In contrast, those who transition to a temporary contract are not forced to relinquish those assets unless they default. We consider changes over a three-year horizon, which is the minimum lag common among the three surveys composing the sample. We estimate the following regression model:

$$Y_{it} = \beta \cdot New\ Permanent\ Contract_{it} + \gamma \cdot X_{it} + \varepsilon_{it}, \tag{4}$$

where the binary variable $New\ Permanent\ Contract_{it}$ takes a value of 1 if a worker experiences a transition from temporary to permanent contract between year t-3 and t. We examine two consumption outcomes that are observable in all three countries' surveys. The first outcome variable measures changes in home ownership: *New Homebuyer* is an indicator for an individual owning a home at t but not at t-3. The second outcome, *Bought a Car*, is an indicator for the survey recording an individual buying a car between t and t-3. See Appendix A for detailed definitions of these variables in each country.

The regression estimates in Table 7 show that temporary workers are more likely to purchase homes and vehicles after obtaining work under a permanent contract. Those transitioning to permanent work become new homeowners at a rate 1.3 percentage points higher than do other workers, which corresponds to 46% of the sample average. Likewise, newly permanent workers are 1.9 percentage points more likely to buy a car than are other workers, a 16% increase relative to the sample average.

The Dutch survey is conducted every year, allowing us to investigate the timing of this homeownership response in more detail. After restricting the sample to Dutch households, we estimate a modified version of eq. (4) in which *New Permanent Contract* is replaced by annual indicators for the years before and after shifting from a temporary contract to a permanent contract

in event time. Figure 6 presents the coefficients on these indicators for analysis of *New Homebuyer*. The results show that the increase in homeownership after transitioning to permanent work does not reflect a preexisting trend. The probability of becoming a new homeowner increases and becomes statistically significant two years after transitioning to a permanent contract. This pattern is consistent with causality running from contract type to homeownership, not the other way around, and with the time that it takes to settle into a new job, find a house, and arrange financing. The year-3 point estimate suggests that those who did not already buy a home by year two are also more likely to become a homeowner in year three, although this estimate is not statistically significant, which may not be surprising as the sample restrictions required for this test limit its statistical power.

5. Job Loss Risk and Employment Protection

The most salient characteristic of temporary work is the lack of job security. As shown in Section 3, workers on temporary contracts face substantially greater unemployment risk than do permanent workers. Temporary contract holders' less secure jobs provide income that is more volatile and cyclical than that of otherwise similar workers with permanent contracts. To connect our analysis more directly with the parameters of lifecycle consumption models and to illustrate the importance of temporary workers' job insecurity for credit access and consumption, we use regression analysis to relate these outcomes directly to the likelihood of job loss. We then study the credit and consumption impacts of differences in job loss risk induced by employment protection laws.

5.1. Job Loss Risk

Using the HFCS data, we measure *Job Loss Risk* as the average likelihood of job loss reported by respondents in occupation o and contract type c (i.e., temporary or permanent). We measure risk at the occupation-contract level rather than the individual level for two reasons: (1) for credit underwriting, lenders can gauge risk related to observable characteristics like occupation and contract type, but not the worker's private information about job insecurity; and (2) only 30% of survey respondents report job loss risk, so by using a coarser measure, we avoid losing a large portion of the sample to missing data. We estimate a modified version of eq. (1) that substitutes job loss risk for the temporary contract indicator:

$$Y_{ioct} = \beta \cdot Job\ Loss\ Risk_{oc} + \boldsymbol{\gamma} \cdot \boldsymbol{X}_{ioct} + \varepsilon_{ioct}, \tag{5}$$

where X is the same set of controls used in eq. (1) above. The standard errors are adjusted for clustering at the occupation-contract type level.

The results in Table 8 provide an estimate of the impact of job loss risk on borrowing and consumption. We find that riskier jobs are associated with fewer applications for credit and more credit refusals (columns 1 and 2) and thus lower credit volumes (columns 3 and 4) and a higher interest rate (column 5). As a consequence, workers with riskier jobs are less likely to be recent homebuyers, and the values of their recently purchased homes and cars are lower. All of these effects are statistically significant at the 1% level.

The magnitudes of these effects of job loss risk are in line with our estimates for temporary contracts in Table 3 and 5. The likelihood of job loss is approximately 40% for temporary workers and 16% for permanent workers. Hence, if we multiply the job loss risk coefficients from Table 8 by the difference of 24 percentage points suggests, we can approximate what share of the temporary-permanent gap is likely explained by job loss risk. Doing so reveals that job loss risk explains close to 100% of the effect for all outcomes (estimates in last row of Table 8).

5.2. Employment protection

To provide further evidence that job loss risk, per se, causes declines in borrowing and consumption, we study differences in job loss risk driven by legal differences across countries. Differences in countries' labor protections include differences in the regulations governing firing rules, severance payments, and notice periods. The OECD's Employment Protection Index measures the degree to which these regulations protect permanent workers from either individual or collective dismissal. The index, which theoretically takes values from 0 to 5, varies from 1.59 for Hungary in all years to 4.42 for Portugal in 2009. Figure 7 shows the average level of the index in each country.

We analyze how increased job security affects credit access and the consumption of durables by estimating the differential impact of employment protection on workers with different baseline levels of job loss risk using the following specification:

$$Y_{iogt} = \beta \cdot Job\ Loss\ Risk_o \times Employment\ Protection_{gt} + \gamma \cdot X_{iogt} + \varepsilon_{iogt}, \qquad (6)$$

where an employment, credit, or consumption outcome of worker i in occupation o, country g, and year t is regressed on an interaction of the average occupation-level likelihood of job loss (across all countries) and the country-level strength of employment protection. The matrix X is the same

set of controls used in eq. (1), including both occupation and country fixed effects. The sample is limited to workers on permanent contracts, who are the relevant group covered by employment protection legislation. The standard errors are adjusted for clustering at the occupation level.

The first test, reported in Panel A of Table 9, examines whether employment protection affects individuals' self-reported job security differently depending on the *Job Loss Risk* of their occupation. Stricter employment protection increases job security for all permanent workers. However, this effect is likely more pronounced in occupations in which workers would be at greater risk of losing their job without these protections. Intuitively, strong national employment protection laws do not have much effect on the job security of teachers, because they are unlikely to lose their jobs in any case. But manufacturing workers' jobs are much more secure in countries with strict employment protection than in countries with weak employment protection. As a result, interacting occupation-level job loss risk with the country-level employment protection index allows us to capture the differential impact of employment protection on more affected workers, thereby identifying the effect of having greater job security. This estimation strategy is akin to that of Rajan and Zingales (1998), which identifies the effects of financial development on growth by comparing the performance of industries with varying reliance on external finance.

We find that employment protection does indeed reduce job loss risk more for riskier occupations. The estimated β coefficient of -0.238, reported Panel A of Table 9, indicates that for an occupation with job loss risk of 0.05, a 1-point rise in the employment protection index reduces job loss risk by 0.01, whereas for an occupation with job loss risk of 0.20 job loss risk would decline by 0.04.

The remainder of Table 9 presents analysis examining how these regulation-induced changes in job loss risk affect credit and consumption. These specifications can be interpreted as a difference-in-differences design in which the first difference is taken across countries and the second is taken across occupations. There are two key components to this identification strategy. First, the occupation fixed effects account for workers' selection into occupations, as long as the extent of selection does not correlate with the strength of employment protection. Second, the country fixed effects account for legal and institutional factors, such as credit market development, that affect credit access and may correlate with employment protection.

The results are presented in Table 9, Panel B. We find that greater job security increases credit applications and reduces credit denials (columns 2 and 3). Consistent with those changes,

job security leads to more credit on the intensive margin (column 4) and credit being cheaper (column 6). The coefficient estimate for having a new mortgage is positive but insignificant (column 5). The improved credit outcomes translate to significantly greater consumption of homes and cars (columns 7–9).

We conclude that job security indeed has a causal effect on workers' ability to access credit and finance their purchases of durable goods. Robustness tests reported in Table 10 lend further support to this conclusion. Those tests account for unemployment insurance and tax policy, which might affect low- and high-risk occupations differently, by controlling for those factors as well as their interaction with occupation-level job loss risk. Those specifications yield similar estimates for employment protection's effects on credit access and consumption.

6. Forced Versus Voluntary: Evidence from Temporary Work in the United States

Our analysis shows that lower job security prevents temporary workers from obtaining credit and limits their purchases of durables. One aspect of temporary work that begs further inquiry is the heterogeneity in why workers end up with temporary rather than permanent jobs. Some individuals voluntarily seek out temporary work because of benefits, such as schedule flexibility, that outweigh any costs of being less securely employed. Others find temporary jobs less attractive but nonetheless accept them because they cannot find permanent work. In the analysis that follows, we compare the effects of temporary work on home ownership for voluntary and involuntary temporary workers. By observing whether voluntary temporary workers choose to bear the costs of reduced credit access and delayed home ownership, we try to shed light on the severity of those consequences.

The European HFCS data lack information on temporary workers' alternatives and reasons for accepting a temporary contract. The US Census Bureau's Contingent Worker Supplement to the Current Population Survey (CPS), however, asks respondents whether they work on temporary basis and whether they would prefer to work on a permanent basis. We use these data to explore the relationships of homeownership with voluntary and forced temporary work in the United States. Unlike the HFCS, the CPS does not include information about credit outcomes or car purchases, so we study homeownership alone.

We analyze a sample of over 200,000 CPS respondents and study the relationship between homeownership and temporary work. The sample consists of household heads or their spouses

who respond to both the contingent work supplement and a monthly CPS survey. We estimate the following linear probability regression:

$$Homeowner_{it} = \beta Temporary_i + \gamma Z_{it} + \varepsilon_{it}, \tag{7}$$

where *Homeowner* is an indicator variable for person *i* owning their residence, *Temporary* is an indicator variable for whether he or she is working on temporary basis, and Z is a rich set of controls. These controls include a male indicator and fixed effects for the worker's state of residence (51 values), occupation (452 values), industry (234 values), age (75 values), race (21 values), and education level (5 categories), family income (20 values), and the year of the observation (6 values).

The estimation results, which we report in Table 11, show that temporary employment is negatively correlated with homeownership in the United States, similar to what we found in Europe. Homeownership rates are 4.4 percentage points lower among temporary workers. This result is similar quantitatively to the analogous relationship in Europe.[5]

The majority of those who work on a temporary basis would prefer permanent employment. Among CPS respondents with temporary work, 59% state they would prefer a permanent contract. The other 41% prefer to work on a temporary basis for various reasons, including health limitations or the need to combine work with school. Based on these responses, we separate temporary workers based on whether they chose temporary work or were forced into it, and we then relate homeownership with each of these variables. As demonstrated in column 2, the negative correlation of temporary work and homeownership stems fully from those who would prefer permanent work. The homeownership rate of permanent employees is similar to that of voluntary temporary workers, but is 7 percentage points higher than the homeownership rate of forced temporary workers.

The effect of forced temporary work on durable consumption has implications for the optimal design of labor market institutions. While some may choose to work on a temporary basis, these individuals do not seem to suffer reduced credit access. However, institutions that create dual

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[5] In the analysis in Section 4, we show that temporary workers are less likely to have recently purchased a home. When we regress a binary indicator of homeownership (irrespective of when the house was purchased) on temporary contract in the HFCS data, we obtain a similar coefficient of -5.6 percentage points.

labor markets shift some individuals who prefer permanent work to temporary contracts, thereby reducing their access to credit and durable consumption.

7. Demographic Implications of Dual Credit Markets

Finally, we explore the individual and aggregate demographic consequences of temporary work. By diminishing access to credit, temporary work can inhibit workers, most often young adults, from establishing the financial independence needed to support a family, including purchasing a home. In a standard lifecycle model, workers smooth their lifetime consumption by borrowing when they are young and repaying those debts as they age and their incomes rise. In this sense, temporary work is concentrated among workers who need credit the most.

Figure 8 depicts the lifecycle pattern in new mortgage origination of permanent workers, for whom job insecurity is low. Mortgage origination is concentrated among younger workers aged 25–35 years, the same age range for which temporary work is most prevalent (see Figure 3). As a result, temporary work meaningfully constrains home ownership among young adults. Temporary work's effect on home ownership is reflected not only in our individual-level analyses of Tables 5, 7, and 10, but also in country-level aggregates. Figure 9 plots the relative home ownership rates of young adults (versus older adults) against the share of temporary workers in European countries.[6] We find a strong negative relationship: a 10-percentage-point greater prevalence of temporary contracts is associated with a 0.06 lower relative homeownership rate ($p = 0.06$), an 11 percent difference relative to the sample mean.

The instability of temporary work might likewise affect young adults' decisions to establish their own household, get married, and have children. Similar to new mortgage borrowing, those transitions typically occur in the 25- to 35-year age range in which temporary work is also most common. Credit constraints are one reason to expect temporary work to have demographic impacts (Hacamo, 2020), but they are not the only reason, as income uncertainty itself might also inhibit or delay long-term commitments like marriage and childbearing. Recent studies by Asai and

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[6] To account for institutional and other factors that boost home ownership at the country level, we normalize young adults' home ownership rate by the home ownership rate of older adults of working age. The reported correlation controls for countries' GDP per capita, adjusted for purchasing power, from the International Monetary Fund October 2022 World Economic Outlook. Analysis that does not control for GDP reveals a similar pattern.

Koustas (2023) and Martinez-Jimenez and Castello (2023) find that that temporary work reduces family formation and fertility in the Japanese airline industry and 1980s Spain, respectively.

Regression analysis reported in Table 12 examines the effects of temporary work, job loss risk, and employment protection on young adults' decisions to establish their own household, get married, and have children. Panel A presents estimates of eq. (1) for these outcomes. We find that young temporary workers are 9 percentage points less like to live on their own (apart from their parents), 5 percentage points less likely to get married, and nearly 2 percentage points less likely to have children than are young permanent workers. Panel B reports results from analysis that examines individual young workers' transitions from temporary to permanent contracts using eq. (4). In the years immediately after the transition, young adults are more likely to get married and become a parent. In Panel C, we examine the effects of job loss risk using eq. (5). We find that young adults with riskier jobs are less likely to form their own household, get married, and have children. Finally, Panel D reports analysis of the effects of employment protection using eq. (6). These estimates show that strong employment protection lessens the negative impacts of job loss risk on marriage and birth rates. In all, these findings reveal that temporary work and the associated income uncertainty have meaningful demographic consequences for young adults.

These demographic effects on individual temporary workers aggregate to sizable differences at the country level. Figure 10 presents the cross-country conditional correlations of these three demographic outcomes and temporary employment contracts among young adults. The reported correlations control for countries' national income using data on purchasing power parity GDP per capita from the International Monetary Fund October 2022 World Economic Outlook.

The analysis reveals large differences across countries. As shown in Panel A of Figure 10, the prevalence of temporary contracts is negatively correlated with young adults forming their own household by living on their own as opposed to with their parents. An additional 10 percentage points in the prevalence of temporary contracts is associated with 5.7 percentage points fewer young adults living on their own ($p = 0.08$). Temporary contracts are also negatively correlated with family formation. An additional 10 percentage points in the prevalence of temporary contracts at the country level is associated with 3.7 percentage points fewer young adults getting married (Panel B, $p = 0.08$) and 4.9 fewer having children (Panel C, $p = 0.03$). In this way, dual labor markets appear to depress fertility across European nations.

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8. Conclusion

Using detailed survey data on employment and household finances, we study the consequences of income uncertainty for workers' access to credit, consumption of durable goods, and investments in housing. We show that a dual credit market has developed alongside the dual labor market of temporary and permanent employees. Workers with temporary jobs not only face greater income uncertainty than peers with open-ended employment, they also receive significantly less credit, preventing them from borrowing against future income to purchase durable goods and housing. These consequences affect a significant share of workers, especially younger workers who have little savings and rely on credit to establish their independence and start families. Accordingly, differences in the breadth of temporary work correlate with persistent cross-country differences in family formation and fertility.

Our findings highlight previously overlooked costs of labor market regulations that promote temporary work. Labor economists have long debated benefits and costs of policies that support a dual labor market system. On the one hand, temporary jobs provide much needed flexibility to firms and can be a stepping stone into a successful career for workers who otherwise would be unemployed (Holmlund and Storrie, 2002; Heinrich et al., 2005). On the other hand, temporary jobs are associated with higher job turnover and greater inequality (Abowd et al, 1999; Blanchard and Landier, 2002; Cazes and Laiglesia, 2015). Our findings show that these career- and firm-related effects are accompanied by unintended consequences on the credit access and consumption of temporary workers.

These unintended consequences on credit access have important welfare implications. While the dual labor market system shifts income uncertainty from permanent to temporary workers, doing so does not simply transfer surplus between them. Rather, temporary workers' increased income uncertainty exacerbates credit market imperfections that create inefficiency. Consistent with this, we find that temporary work is associated with tighter credit constraints in occupations with more private information about job loss risk and thus greater potential for adverse selection.

Temporary and permanent workers' differences in age make the welfare consequences of this credit rationing more severe. Because permanent workers are older, they are typically less reliant on credit. Income risk that impedes credit access is therefore less costly for them than for younger, credit-reliant temporary workers. As a result, shifting income uncertainty from

permanent to temporary workers reduces the overall credit provision in the economy rather than simply reallocating credit from temporary to permanent workers. The way labor contracts affect aggregate borrowing and consumption is akin to how housing wealth affects aggregate consumption. In that context, heterogeneous-agent New Keynesian models with credit constraints provide a theoretical framework in which the cross-sectional distribution of housing wealth matters for aggregate dynamics (see Kaplan and Violante, 2018, for a survey). Future research should use these tools to consider the importance of credit constraints when evaluating labor market protections from a macroeconomic perspective.

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Appendix A. Construction of the Three-Country Panel Data

The household panel data we study combines records from three surveys: the Italian Survey on Household Income and Wealth (SHIW), the Spanish Survey of Household Finances (EFF), and the Dutch De Nederlandsche Bank Household Survey (DNBHS). All three surveys are longitudinal, following the same households over time and collecting information on finances, consumption, and other variables. While the content and structure of these surveys are broadly similar, several differences influence how we construct and analyze these data.

We analyze SHIW data from the years 2000–2020. Although the survey started in 1977, it began measuring temporary contract status only in 2000. The survey is bi-annual, except for 2018 when it was not conducted. The survey covers around 8,000 households a year, with the sample composition changing over time due to the attrition and addition of respondents. In a typical year after 2000, about half of households were also observed in the previous wave of the survey. Households who complete multiple surveys appear in 5.1 waves, on average.

The EFF survey covers the years 2002–2017 and is conducted every three years. Each wave typically includes around 4,000 households, with about half also observed in the previous wave. Individual households are surveyed up to four times. Households that appear in multiple waves are observed in 3.0 waves, on average.

DNBHS is an annual survey of approximately 2,000 households each year, with about 80% of them also observed in the previous wave. The data cover the years 1993–2021, and households that appear in multiple waves are observed in 11.2 waves, on average.

All three surveys cover topics that overlap with the HFCS, but the questions in each country's survey sometimes differ from those in the HFCS. Some questions included in the HFCS are not present in the panel surveys, and vice versa. Additionally, the answer choices for variables such as education are standardized across countries in the HFCS but differ by country in the panel surveys.

The temporary contract indicator, our key independent variable, is available in all three datasets. In the EFF and the DNBHS, households are first asked if they applied for credit in the last year and, if they answer "yes," they are then asked if the application was rejected. In contrast, all SHIW households are asked whether they had a credit application rejected, but they are not asked if they applied for credit. As a result, our sample for the *Applied for Credit* outcome does not include Italy, and our sample for the *Application Rejected* outcome includes all respondents in Italy but only credit applicants in Spain and the Netherlands. Lastly, the *Expects to be Rejected* outcome is only available in the DNBHS. This outcome is a forward-looking, subjective assessment of creditworthiness that is not available in HFCS.

To construct the consumption outcomes *New Homebuyer* and *Bought a Car*, we use observations from multiple waves of each survey. By doing so, we capture car purchases and changes in homeownership that were reported in surveys conducted over the last three years, which is the minimum observable time horizon in the EFF. To match this horizon in the DNBHS, we use four waves of the survey covering years $t-3$, $t-2$, $t-1$, and t. However, given the SHIW's biannual frequency, matching a three-year horizon in the Italian data is not possible. Instead, we define our outcomes based on the last four years, using survey waves from years $t-4$, $t-2$, and t. (We find similar results if we define our outcomes based on the last two years instead.) In 2020, we only use waves $t-4$ and t because the 2018 wave was not conducted. In Italy and Spain, the surveys ask households explicitly if they have purchased a car in the preceding 12 months, and we define

Bought a Car to be equal to 1 if we observe a purchase in any of the surveys conducted during relevant time horizon. Because the Dutch survey does not ask explicitly about car purchases, we define *Bought a Car* in the Netherlands to be equal to 1 if in the respondent has a car in year t but not in year t-1 for any year t in the relevant horizon. For all countries we define house purchase in a similar way: *New Homebuyer* is an indicator for whether the household switches from not owning to owning a home between any of the survey waves in the relevant horizon. We measure the shift from temporary to permanent contracts similarly.

Several of the control variables in our panel analysis are defined differently in each country, reflecting differences in the countries' surveys. For example, education fixed effects have eight levels in Italy, twelve levels in Spain, and nine levels in the Netherlands. While some of these values correspond to similar levels of education (e.g., bachelor's degree), others represent types of education that are unique to a given country (e.g., vocational schools in the Netherlands). To fully account for these differences, we interact the education and country fixed effects to estimate a different set of education fixed effects in each country. Similarly, we control for country-specific fixed effects for occupation and industry that reflect the different occupational classifications and varying levels of detail in the three datasets. In the Netherlands, industry is not available in all waves; whenever it is missing, we control for fixed effects for the individual's pension fund, which tends to be industry specific. Lastly, we control for income by including the within-country income percentile of a given household. Consistent with how household income is defined in the various surveys, this percentile is defined based on household income net of taxes and transfers in Italy and gross of taxes and transfers in Spain and the Netherlands.

Figure 1. Prevalence of Temporary Contracts by Occupation
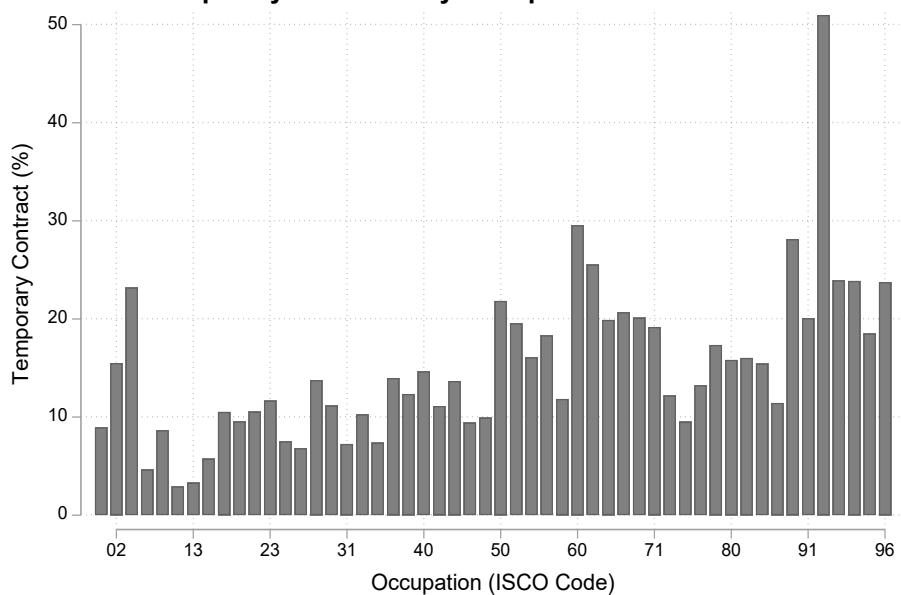
Notes: The bars show the percentage share of workers with temporary contracts by occupation, which are coded using the ISCO-08 classification. See Table 2 for the names of occupations with the highest and lowest shares of temporary contracts.

Figure 2. Prevalence of Temporary Contracts by Country
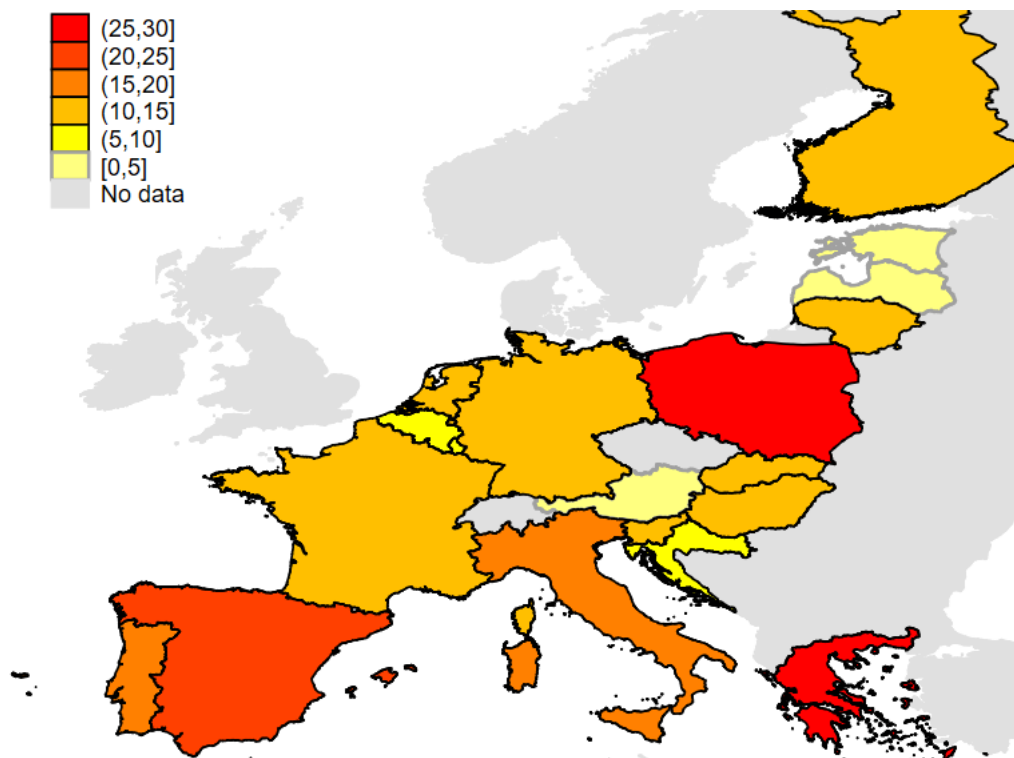
Notes: The map shows the percentage share of workers with temporary contracts by country.

Figure 3. Prevalence of Temporary Contracts by Age
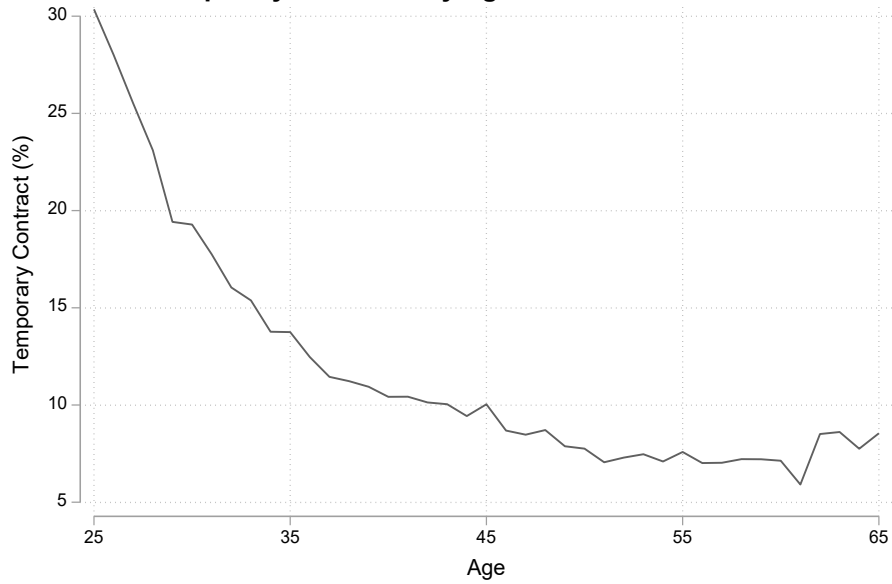
Notes: The line shows the percentage share of workers with a temporary contract by age.

Figure 4. Likelihood of Job Loss and Unemployment Insurance Receipt
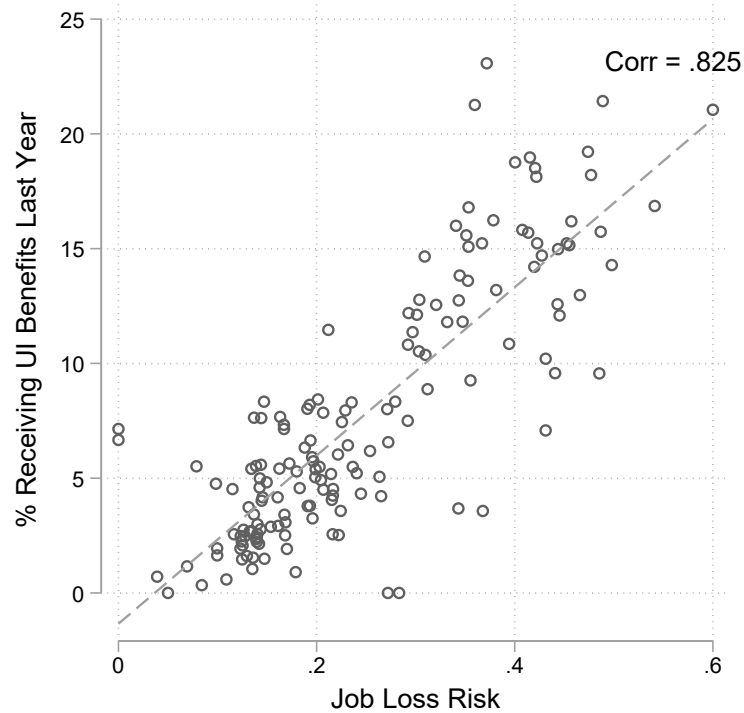
Notes: Each point represents a given occupation-contract-type's average (self-reported) likelihood of job loss (x-axis) and the percentage share of workers with that occupation-contract type that received unemployment (UI) benefits in the last year (y-axis).

Figure 5. Job Loss Risk by Occupation and Contract Type
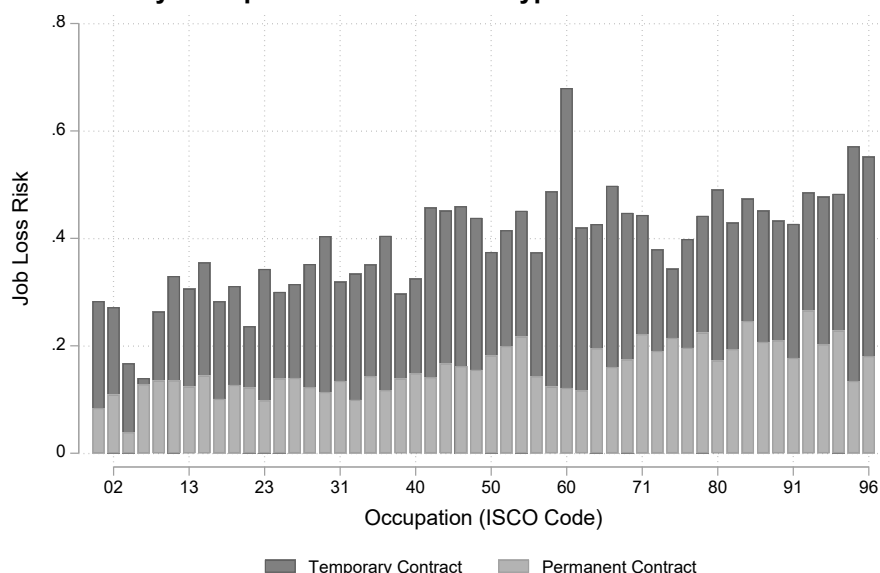
Notes: The bars show average workers' assessment of the likelihood of job loss (on a scale of 0 to 1) by occupation. Light bars show the average likelihood of job loss for workers with permanent contracts, while the dark bars show the additional likelihood of job loss for workers with temporary contracts. The average job loss risk for temporary workers is the combined height of the light and dark bars.

Figure 6. Transition to Permanent Contract and Becoming a Homeowner
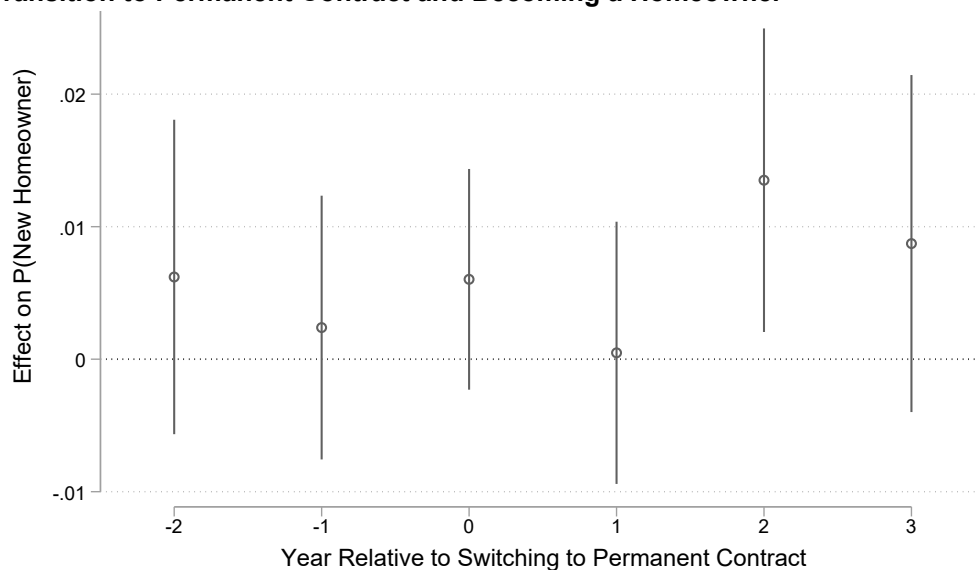
Note: The dots represent the coefficients from regressions of an indicator for becoming a new homeowner in year t on indicators for switching from temporary to permanent contract in years $t+2$, $t+1$, t, $t-1$, $t-2$, and $t-3$. The regressions include the same controls as in the analysis reported in Table 7: income percentile, age, education, occupation, industry, and year. The sample includes all person-year observations in the Netherlands' panel data for which all included lags and leads are available. The whiskers represent 95% confidence intervals.

Figure 7. Strength of Employment Protection by Country
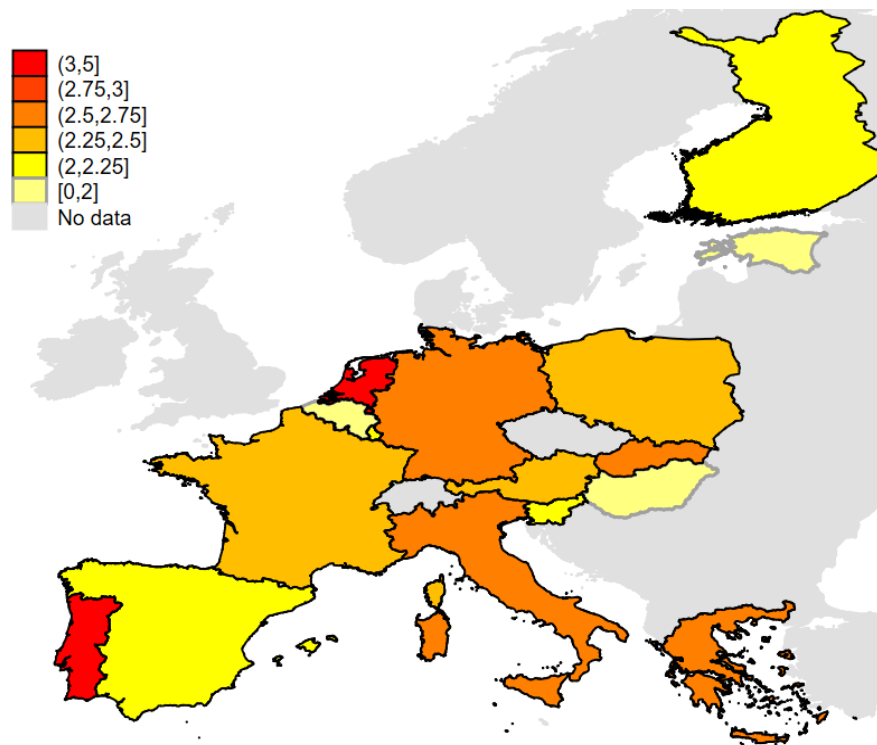
Notes: The map shows the average value of the OECD employment protection index in a given country in our sample.

Figure 8. Mortgage Origination Rate by Age
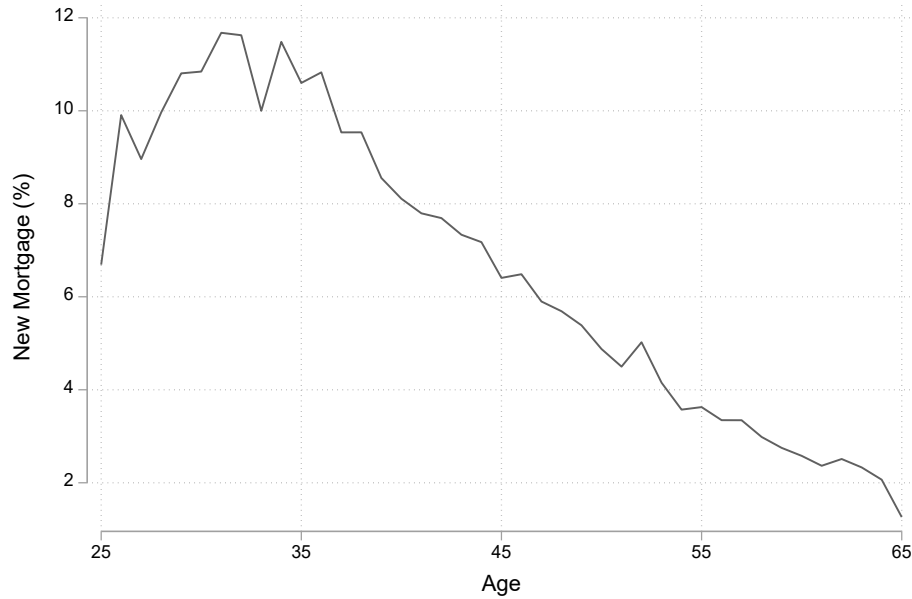
Notes: The line shows the percentage share of permanent workers who have originated or refinanced a mortgage in the last 3 years by age.

Figure 9. Temporary Work and Homeownership Among Young Adults
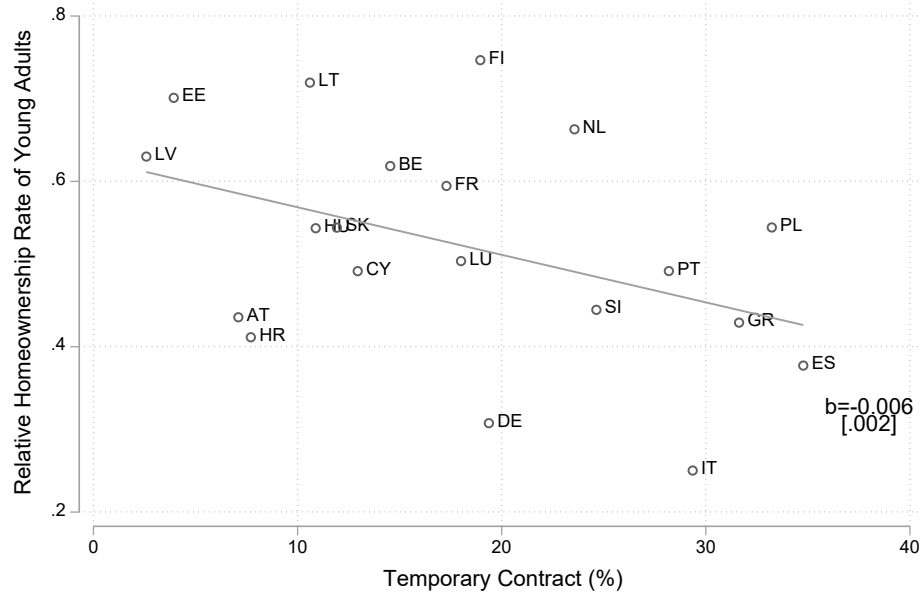
Notes: This figure presents the cross-country relationship between temporary work and homeownership among young adults, controlling for GDP per capita. Each point represents rescaled residuals from country-level regressions of the following variables on GDP per capita: (1) the average share of workers aged 25–35 who have temporary contracts; and (2) 100 times the ratio of the share of homeowners among workers aged 25–35 to the share of homeowners among workers aged 35–65. For each variable, the residuals are rescaled by adding the variable's sample average.

Figure 10. Temporary Work and Household and Family Formation

Panel A. Lives on Own

Panel B. Ever Married
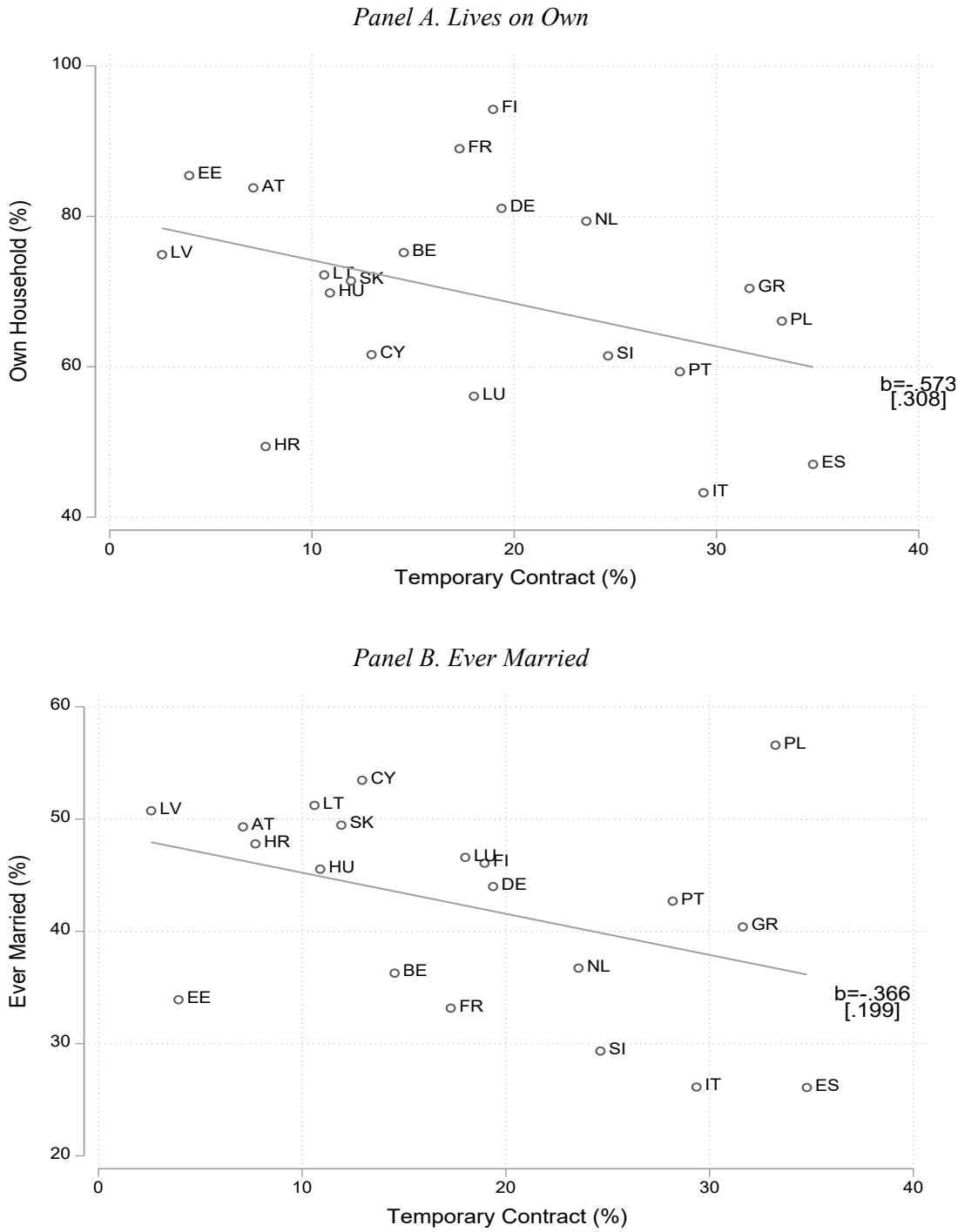
Figure 10. Temporary Work and Household and Family Formation (continued)

Panel C. Has Children
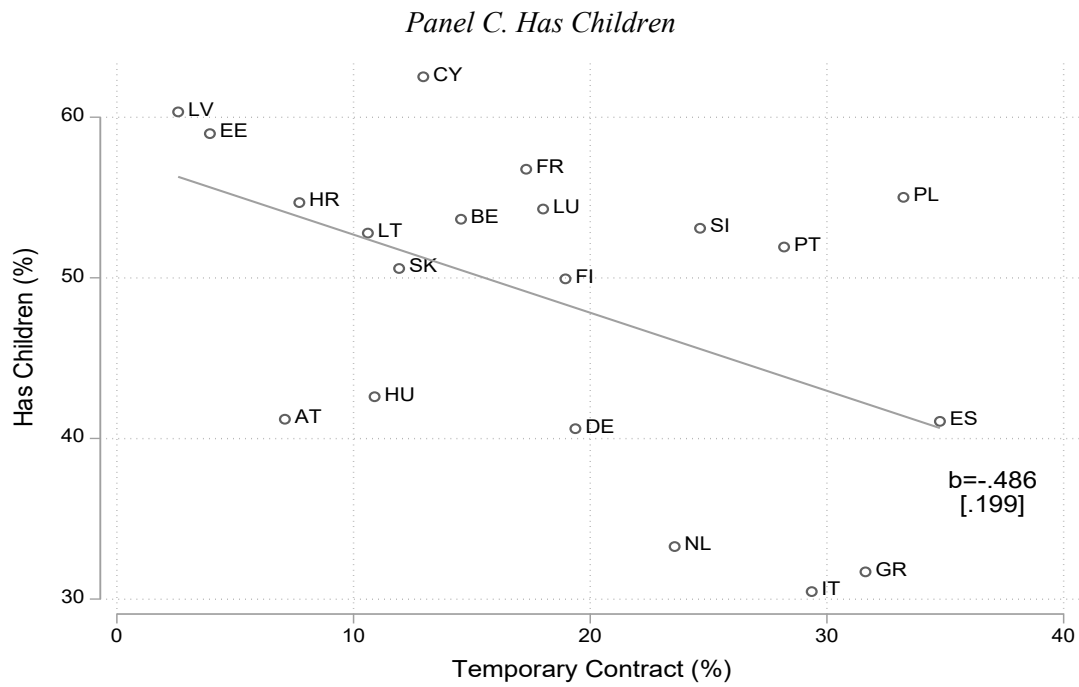
Notes: This figure presents cross-country relationships between temporary work and household and family formation among young adults, controlling for GDP per capita. In all three panels, the x-axis value represents rescaled residuals from country-level regressions of the average percentage share of workers aged 25–35 who have temporary contracts on GDP per capita. The y-axis value represents rescaled residuals from country-level regressions of the following variables on GDP per capita: percentage share of workers aged 25–35 who are a household head or a partner of a household head (panel A), percentage share of workers aged 25–35 who were ever married (panel B), and the percentage share of workers aged 25–35 who have any children (panel C). For each variable, the residuals are rescaled by adding the variable's sample-wide average.

Table 1. Summary Statistics

Panel A. Household Finance and Consumption Survey (HFCS)

	Mean	Std Dev	*N*
Credit			
Applied for Credit Last 3 Years	0.290	0.454	145,137
Applicaton Rejected | Applied	0.122	0.327	39,481
Application Withheld Fearing Rejection	0.054	0.226	142,627
Obtained Credit	0.261	0.439	145,137
Has a Mortgage	0.388	0.487	145,137
Has New Mortgage	0.063	0.243	145,137
Debt/Income Ratio | Any Debt	1.478	1.894	86,730
Mortgage Interest Rate | Has Mortgage	3.300	2.211	41,837
Assets			
Has Own Household	0.851	0.356	145,137
Owns a House	0.610	0.488	145,137
Recent Homebuyer	0.045	0.207	145,137
House Value (EUR)| Recent Homebuyer	244,626	248,250	6,527
Total Value of Cars Owned (EUR)	8,541	26,646	145,137
Bought a Car in the Last 12M	0.152	0.359	98,665
Car Price (EUR) | Bought	12,448	12,849	14,937
Work			
Temporary Contract	0.134	0.341	145,137
Job Loss Risk	0.190	0.092	145,137
Employment Protection Index	2.448	0.548	136,945
Demographics			
Age	42.9	11.5	145,137
Male	0.502	0.500	145,137
Ever Married	0.684	0.465	145,137
Immigrant	0.090	0.286	145,137
Household Income (EUR, Gross)	55,103	86,940	145,137
Has Secondary Education	0.841	0.365	145,137
Has Tertiary Education	0.387	0.487	145,137

Table 1. Summary Statistics (continued)

Panel B. Panel Datasets (SHIW, EFF, DNBHS)

	Mean	Std Dev	*N*
Credit			
Applied for Credit	0.289	0.453	62,024
Applicaton Rejected	0.136	0.342	67,417
Expects to be Rejected	0.180	0.383	49,333
Assets			
Bought a House (Last 3Y)	0.028	0.166	50,569
Bought a Car (Last 3Y)	0.118	0.322	62,579
Work			
Temporary Contract	0.142	0.35	111,620
Got Permanent Contract (Last 3Y)	0.081	0.273	64,983
Demographics			
Age	46.1	14.2	111,620
Male	0.553	0.497	111,620
Ever Married	0.712	0.453	111,620
Household Income (EUR, Gross; ES, ND)	53,474	58,134	62,030
Household Income (EUR, Net; IT)	18,852	10,995	49,590
Has Secondary Education	0.721	0.448	111,620
Has Tertiary Education	0.278	0.448	111,620

Table 1. Summary Statistics (continued)

Panel C. Current Population Survey (CPS)

	Mean	Std Dev	*N*
Assets			
Owns a House	0.719	0.449	204,131
Work			
Temporary Work	0.034	0.181	204,131
Forced Temporary Work	0.021	0.142	204,131
Voluntary Temporary Work	0.011	0.106	204,131
Demographics			
Age	42.5	11.9	204,131
Male	0.501	0.500	204,131
Ever Married	0.859	0.453	204,131
Non-White	0.223	0.416	204,131
Household Income (USD, Gross)	59,748	41,305	190,221
Has Secondary Education	0.919	0.311	204,131
Has Tertiary Education	0.427	0.489	204,131

Notes: The table contains means, standard deviations, and observation counts of key variables from our HFCS, three-country panel, and CPS samples. The HFCS sample contains 145,137 workers for whom we observe country of residence, age, education, industry, total household income, sex, immigrant status, and occupation. The three-country panel contains data from Italy, Spain, and the Netherlands and up to 111,620 observations of up to 52,405 unique individuals. Gross income is not available for Italy, and net income is reported instead. The CPS sample contains 204,131 workers for whom we observe contingent work status, homeownership status, state of residence, age, education, industry, total household income, race, sex, and occupation, and who are heads of households or their spouses. Observation counts are lower for particular variables when (1) the question was only asked conditional on the answer to a previous question (e.g., application rejection is only relevant for workers who have applied for credit); (2) the question was not included in all countries in all waves of the survey; or (3) the data is missing.

Table 2. Occupations with Most and Least Temporary Contracts and Job Loss Risk

Panel A. Share of Temporary Contracts

Most		Least	
Occupation	Percent	Occupation	Percent
Agricultural Laborers (92)	51.0	Science & Engin. Professionals (31)	7.3
Skilled Agricultural Workers (61)	25.6	IT Professionals (25)	6.8
Labourers in Mining, Manufact. (93)	23.9	Managers – Hospitality, Retail (14)	5.7
Food Preparation Assistants (94)	23.9	Production Managers (13)	3.3
Refuse Workers (96)	23.8	Administrative Managers (12)	2.9

Panel B. Job Loss Risk

Most		Least	
Occupation	Average	Occupation	Average
Agricultural Laborers (92)	0.27	Skilled Agricultural Workers (61)	0.12
Assemblers (82)	0.25	Health Associate Professionals (32)	0.10
Food Preparation Assistants (94)	0.23	Teaching Professionals (23)	0.10
Craft Workers – Food Processing (75)	0.22	Armed Forces – Officers (01, 02)	0.08
Craft Workers – Building and Related (71)	0.22	Armed Forces – Other Ranks (03)	0.04

Notes: Panel A lists occupations with the highest and lowest share of workers with temporary contracts. Panel B lists occupations for which permanent workers have the greatest and least job loss risk, as measured by workers' average assessment of their likelihood of job loss in the next 12 months, on a scale from 0 to 1. Each occupation's ISCO-08 classification code is listed in parentheses.

Table 3. Temporary Contracts and Credit

	(1) Obtained Credit	(2) Debt/Income	(3) Has New Mortgage	(4) Mortgage Interest Rate	(5) Applied for Credit	(6) Application Withheld	(7) Application Rejected	(8) Application Rejected or Withheld
Temporary Contract	-0.023***	-0.194***	-0.013***	0.102***	-0.016***	0.030***	0.041***	0.031***
	(0.003)	(0.022)	(0.002)	(0.033)	(0.004)	(0.002)	(0.007)	(0.003)
N	145,137	86,730	145,137	41,837	145,137	142,627	39,481	145,049
Dep. Var Avg	0.261	1.478	0.063	3.300	0.290	0.054	0.122	0.076
Demographic controls	X	X	X	X	X	X	X	X
Fixed Effects								
Occupation	X	X	X	X	X	X	X	X
Industry	X	X	X	X	X	X	X	X
Country	X	X	X	X	X	X	X	X
Year	X	X	X	X	X	X	X	X

Notes: This table reports estimates from linear regressions of various credit outcomes on an indicator for having a temporary contract and a set of controls. In all regressions, the controls include the worker's demographics (sex, immigrant status, age, education, and income percentile within the country) and fixed effects for the worker's occupation, industry, country, and year. The controls for age and income percentile are indicator variables for every integer value. The sample is limited to households with a positive debt balance in column 2, households with a mortgage in column 4, and households that applied for credit in column 7. In column 6, the outcome variable is an indicator for whether the household has withheld an application for credit because of fear of refusal. Standard errors, adjusted for clustering at the household level, are reported in parentheses. *** denotes significance at 1% level.

Table 4. Private Information About Job Loss Risk and Credit Rationing

	(1) Application Rejected	(2) Application Rejected or Withheld	(3) Mortgage Interest Rate
Temporary Contract	0.026***	0.021***	0.074*
	(0.009)	(0.005)	(0.044)
Temporary Contract × High Dispersion	0.024**	0.017***	0.049
	(0.011)	(0.006)	(0.066)
N	39,481	145,049	41,837
Dep. Var Avg	0.122	0.076	3.300
Demographic controls	X	X	X
Fixed Effects			
Occupation	X	X	X
Industry	X	X	X
Country	X	X	X
Year	X	X	X

Notes: This table reports estimates from linear regressions of credit rejections and interest rates on an indicator for having a temporary contract, its interaction with an indicator for occupations with above-median dispersion in workers' estimates of the likelihood of job loss, and a set of controls. In all regressions, the controls include the worker's demographics (sex, immigrant status, age, education, and income percentile within the country) and fixed effects for the worker's occupation, industry, country, and year. The controls for age and income percentile are indicator variables for every integer value. The sample is limited to households who have applied for credit in column 1 and households with a mortgage in column 3. Standard errors, adjusted for clustering at the occupation level, are reported in parentheses. *** denotes significance at 1% level, ** at 5% level, * at 10% level.

Table 5. Temporary Contracts and Consumption

	(1)	(2)	(3)	(4)	(5)
	Recent Homebuyer	Log(Home Value)	Bought a Car in the Last 12M	Log(Car Price)	Asinh(Total Value of Cars Owned)
Temporary Contract	-0.013***	-0.075***	0.000	-0.173***	-0.311***
	(0.002)	(0.024)	(0.004)	(0.032)	(0.030)
N	145,137	6,527	98,665	14,892	145,137
Dep. Var Avg	0.045	11.979	0.152	8.865	7.053
Demographic controls	X	X	X	X	X
Fixed Effects					
Occupation	X	X	X	X	X
Industry	X	X	X	X	X
Country	X	X	X	X	X
Year	X	X	X	X	X

Notes: This table reports estimates from linear regressions of various measures of durable consumption on an indicator for having a temporary contract and a set of controls. In all regressions, the controls include the worker's demographics (sex, immigrant status, age, education, and income percentile within the country) and fixed effects for the worker's occupation, industry, country, and year. The controls for age and income percentile are indicator variables for every integer value. The sample is limited to recent homebuyers in column 2 and people who bought a car in the last 12 months in column 4. The samples in columns 3 and 4 are smaller because a measure of having bought a car in the last 12 months is not available before 2013. The outcome in column 5 is the inverse hyperbolic sine of the total value of cars owned. Standard errors, adjusted for clustering at the household level, are reported in parentheses. *** denotes significance at 1% level; ** at 5% level; and * at 10% level.

Table 6. Within-Person Analysis

	(1) Applied for Credit	(2) Application Rejected	(3) Expects to Be Rejected
Temporary Contract	-0.019***	0.007*	0.019**
	(0.007)	(0.004)	(0.008)
N	62,024	67,417	49,333
Dep. Var Avg	0.289	0.136	0.173
Fixed Effects			
Individual	X	X	X
Income Percentile	X	X	X
Country × Age	X	X	X
Country × Education	X	X	X
Country × Occupation	X	X	X
Country × Industry	X	X	X
Country × Year	X	X	X

Notes: This table reports estimates from linear regressions of various credit outcomes on an indicator for having a temporary contract, an individual fixed effect, and a set of other controls. In all regressions, the controls include individual fixed effects, within-country income percentile, and country-specific fixed effects for age, education, occupation, industry, and year. The controls for age and income percentile are indicator variables for every integer value. See Appendix A for details on sample construction. Standard errors, adjusted for clustering at the household level, are reported in parentheses. *** denotes significance at 1% level; ** at 5% level; and * at 10% level.

Table 7. Transitions to Permanent Contract and Durable Consumption

	(1) New Homebuyer	(2) Bought a Car
New Permanent Contract	0.013***	0.019***
	(0.005)	(0.006)
N	50,659	62,579
Dep. Var Avg	0.028	0.118
Fixed Effects		
Income Percentile	X	X
Country × Age	X	X
Country × Education	X	X
Country × Occupation	X	X
Country × Industry	X	X
Country × Year	X	X

Notes: This tables reports estimates from linear regressions of house and car purchases on an indicator for moving from a temporary to permanent contract and a set of controls. In all regressions, the controls include within-country income percentile and country-specific fixed effects for age, education, occupation, industry, and year. The controls for age and income percentile are indicator variables for every integer value. See Appendix A for details on sample construction. Standard errors, adjusted for clustering at the household level, are reported in parentheses. *** denotes significance at 1% level.

Table 8. Job Loss Risk, Credit, and Consumption

	(1) Applied for Credit	(2) Application Rejected	(3) Debt/Income	(4) Has New Mortgage	(5) Mortgage Interest Rate	(6) Recent Homebuyer	(7) Log(Home Value)	(8) Asinh(Total Value of Cars Owned)
Job Loss Risk	-0.068***	0.180***	-0.874***	-0.053***	0.480***	-0.057***	-0.350***	-1.388***
	(0.014)	(0.024)	(0.079)	(0.007)	(0.136)	(0.007)	(0.109)	(0.198)
N	145,137	39,481	86,730	145,137	41,837	145,137	6,527	145,137
Dep. Var Avg	0.29	0.122	1.478	0.0629	3.3	0.045	11.98	7.056
Implied Effect of Temporary Contract	-0.016	0.043	-0.210	-0.013	0.115	-0.014	-0.084	-0.333
Percent of Coefficient Explained	102%	105%	108%	98%	113%	105%	112%	107%
Demographic controls	X	X	X	X	X	X	X	X
Fixed Effects								
Occupation	X	X	X	X	X	X	X	X
Industry	X	X	X	X	X	X	X	X
Country	X	X	X	X	X	X	X	X
Year	X	X	X	X	X	X	X	X

Notes: This table reports estimates from linear regressions of various credit and consumption outcomes on job loss risk and a set of controls. Job loss risk is the average level reported by workers in the same occupation with the same type of employment contract. In all regressions, the controls include the worker's demographics (sex, immigrant status, age, education, and income percentile within the country) and fixed effects for the worker's occupation, industry, country, and year. The controls for age and income percentile are indicator variables for every integer value. The sample is persons applying for credit in column 2, those with positive debt balance in column 3, those with a mortgage in column 5, and recent homebuyers in column 7. The implied effect of having a temporary contract is the product of the coefficient on Job Loss Risk and 0.24, the average difference in job loss risk between permanent and temporary workers. The percent of coefficient explained is the ratio of this implied effect and the estimated effect of having a temporary contract in Table 3 for credit outcomes and Table 5 for consumption outcomes. Standard errors, adjusted for clustering at the occupation-contract type level, are reported in parentheses. *** denotes significance at 1% level.

Table 9. Employment Protection-Induced Job Security: Impact on Credit and Consumption

Panel A. Likelihood of Job Loss

	(1) Self-Reported Likelihood of Losing a Job in the next 12 months
Job Loss Risk × Employment Protection	-0.234*** (0.065)
N	40,667
Dep. Var Avg	14.97

Panel B. Credit and Consumption

	(2) Applied for Credit	(3) Application Rejected	(4) Debt/Income	(5) Has New Mortgage
Job Loss Risk × Employment Protection	0.156*** (0.054)	-0.240** (0.0941)	0.957** (0.390)	0.032 (0.027)
N	118,041	32,845	71,618	118,041
Dep. Var Avg	0.297	0.109	1.445	0.0664

	(6) Mortgage Interest Rate	(7) Recent Homebuyer	(8) Log(Home Value)	(9) Asinh(Total Value of Cars Owned)
Job Loss Risk × Employment Protection	-2.150*** (0.699)	0.068** (0.033)	1.180** (0.450)	2.739** (1.167)
N	35,423	118,041	5,620	118,041
Dep. Var Avg	3.218	0.0477	12.06	7.119

Notes: This table reports estimates from linear regressions of various outcomes on the degree of employment protection, the interaction of job loss risk and the degree of employment protection, and a set of controls in a sample of workers with permanent contracts. Job loss risk varies by occupation and employment protection varies by country-year. In all regressions, the controls include the worker's demographics (sex, immigrant status, age, education, and income percentile within the country) and fixed effects for the worker's occupation, industry, country, and year. The controls for age and income percentile are indicator variables for every integer value. The main effect for job loss risk is absorbed by the occupation fixed effect. In Panel A, the outcome variable is the individual's self-reported likelihood of job loss. In Panel B, the outcome variables are measures of the respondent's credit and consumption. The sample is persons that applied for credit in column 3, those with positive debt balance in column 4, those with a mortgage in column 6, and recent homebuyers in column 8. Standard errors, adjusted for clustering at the occupation level, are reported in parentheses. *** denotes significance at 1% level and ** denotes significance at 5% level.

Table 10. Controling for Unemployment Insurance Generosity and Marginal Tax Rates

	(1) Applied for Credit	(2) Application Rejected	(3) Debt/Income	(4) Has New Mortgage
Job Loss Risk	0.169***	-0.218**	1.102***	0.037
× Employment Protection	(0.057)	(0.095)	(0.412)	(0.025)
N	118,041	32,845	71,618	118,041

	(5) Mortgage Interest Rate	(6) Recent Homebuyer	(7) Log(House Value)	(8) Asinh(Total Value of Cars Owned)
Job Loss Risk	-1.520**	0.0648**	0.785	2.327**
× Employment Protection	(0.608)	(0.029)	(0.477)	(1.067)
N	35,423	118,041	5,620	118,041

Notes: This table reports estimates from linear regressions of various outcomes on the degree of employment protection, the interaction of job loss risk and the degree of employment protection, and a set of controls in a sample of workers with permanent contracts. The specification is the same as in Table 9 but also includes controls for the unemployment insurance (UI) replacement rate and the marginal tax rate as well as interactions of those rates with job loss risk. Both measures are from the OECD and correspond to a single person without children earning 2/3 of the average wage. The UI replacement rate is for a person who has worked 2 months and includes social assistance and housing benefits. The marginal tax rate is for a person working between 50% and 100% of full time work. *** denotes significance at 1% level, ** at 5% level, and * at 10% level.

Table 11. Forced Versus Voluntary Temporary Work

	Owns a Home	
	(1)	(2)
Temporary	-0.044***	
	(0.005)	
Forced Temporary		-0.069***
		(0.007)
Voluntary Temporary		0.001
		(0.008)
N	204,131	204,131
Dep. Var Avg	0.719	0.719
p-value	-	< 0.001
Demographic controls	X	X
Fixed Effects		
Occupation	X	X
Industry	X	X
State	X	X
Year	X	X

Notes: This table reports estimates from linear regressions of an indicator variable for owning a home on measures of temporary work and a set of controls. The specification in column 1 includes a single indicator for all types of temporary work, whereas the specification in column 2 contains separate indicators for whether the temporary worker prefers to work on a permanent contract ("Forced") or a temporary contract ("Voluntary"). The *p*-value in column 2 corresponds to a test of the null hypothesis that the coefficients on the two temporary work indicators are equal. In both regressions, the controls include the worker's demographics (sex, race, age, education, and family income) and fixed effects for the worker's occupation, industry, state, and year. The age controls are indicator variables for every integer value, and the family income controls are indicator variables for 20 income ranges. Standard errors, adjusted for clustering at the household level, are reported in parentheses. *** indicates significance at 1% level.

Table 12. Contract Type and Family Formation

	Panel A. Temporary Contracts		
	(1)	(2)	(3)
	Lives on Own	Married	Has Children
Temporary Contract	-0.092***	-0.047***	-0.018**
	(0.006)	(0.007)	(0.007)
N	32,009	32,009	32,009

	Panel B. Within-Person Analysis		
	(4)	(5)	(6)
	Starts Own Household	Gets Married	Becomes Parent
New Permanent Contract	0.014*	0.024**	0.007
	(0.008)	(0.011)	(0.012)
N	9,934	9,956	9,765

	Panel C. Job Loss Risk		
	(7)	(8)	(9)
	Lives on Own	Married	Has Children
Job Loss Risk	-0.401***	-0.193***	-0.065**
	(0.029)	(0.027)	(0.030)
N	32,009	32,009	32,009

	Panel D. Employment Protection-Induced Job Security		
	(10)	(11)	(12)
	Lives on Own	Married	Has Children
Job Loss Risk	0.163	0.694***	0.464***
× Employment Protection	(0.144)	(0.157)	(0.164)
N	23,987	23,987	23,987

Notes: This table reports estimates from linear regressions of various outcomes on measures of temporary contract status or job loss risk and a set of controls in a sample of young adults aged 25 to 35 years. In panels A, C, and D, the dependent variables are indicators for living in their own household (away from their parents), being married, and having any children. In Panel B, the outcomes are indicators for transitions into those states. Except for the dependent variables, the specifications in Panels A, B, C, and D are the same as those in Tables 3, 7, 8, and 9, respectively. *** denotes significance at 1% level, ** at 5% level, and * at 10% level.