SUBJECTIVE JOB INSECURITY AND THE RISE OF THE PRECARIAT: EVIDENCE FROM THE UNITED KINGDOM, GERMANY, AND THE UNITED STATES

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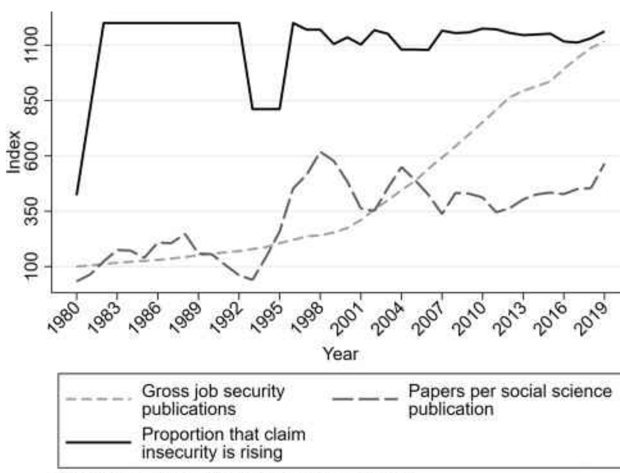
Abstract—There is a widespread belief that work is less secure than in the past, that an increasing share of workers are part of the "precariat." It is hard to find much evidence for this in objective measures of job security, but perhaps subjective measures show different trends. This paper shows that in the United States, the United Kingdom, and Germany, workers feel as secure as they ever have in the past 30 years. This is partly because job insecurity is very cyclical and (pre-COVID) unemployment rates very low, but there is also no clear underlying trend towards increased subjective measures of job insecurity. This conclusion seems robust to controlling for the changing mix of the labor force, and it is true for specific subsets of workers.

1. Introduction

THIS paper investigates the trends in self-perceived job security in the advanced industrial economies of the United Kingdom, Germany, and the United States over the past four decades. In discussions about the evolution of the labor market, it is common to hear what Hollister (2011) calls the "New Employment Narrative," or Standing (2011) the rise of the "precariat": the idea that security of employment has fallen substantially in recent decades. Putative causes include technological change (see Rifkin, 1995, for an early expression of this view) and globalization (Kalleberg, 2009), with an associated decline of manufacturing employment and unionization, and the rise of "nonstandard employment" that is, temporary or part-time work, zero-hour contracts, outsourcing, and other "flexible" work arrangements that are part of the "gig economy" (see, e.g., Davis, 2009; Fantasia & Voss, 2004; or Weil, 2014). Academics have become increasingly interested in job security as a research topic, and the overwhelming consensus in those papers is that insecurity has risen or has been rising. We extracted all the papers in Scopus with the phrases "job security" or "job insecurity" in their abstracts and used a natural language processing (NLP) model to select those that make a claim about trends in job insecurity and to classify whether the claim was that job insecurity was rising or falling. The blue line in figure 1 plots the three-year moving average of the gross number of papers making a claim about trending job insecurity, and the red line plots the number relative to the total number of social science publications in that year—each series is indexed to be equal to 100 in the base year of 1980. Both series show a rising level of interest. The green

FIGURE 1.—JOB SECURITY PUBLICATIONS ON SCOPUS OVER TIME



1980=100. Underlying data are 3-year moving averages.

line (on the right-hand axis) shows the fraction of papers that claim that insecurity has risen or is rising. This fraction has been above 90% since the mid-2000s; the raw sample proportion for the entire period since 1980 is 92%.

Many proponents of the "precariat" theory of labor markets point to the rising proportion of jobs with "nonstandard" characteristics (Standing, 2011; Kalleberg, 2018; and Thelen, 2019) as evidence that workers feel more insecure today than they did in the past. Mas and Pallais (2017) show, using experimental evidence from the United States, that workers' willingness to pay for the flexibility that is characteristic of these types of jobs is low. Datta (2019) performs a similar experimental exercise in the United Kingdom, and finds that around half of workers in flexible work arrangements would prefer to have more standard working arrangements. This work suggests that the rise of flexible employment may have more to do with the desire of employers to contract flexibly with their workers rather than vice versa. Nevertheless, though the rise of nonstandard working arrangements is well-documented, the magnitudes involved are often less than one might infer from the "precariat" argument. Using data from OECD (2015), Kalleberg (2018) notes that the growth rate of nonstandard jobs (defined as all temporary workers, those on part-time contracts, and own-account self-employed persons) in the United Kingdom was −0.5% between 1995 and 2007, and 3.2% between 2007 and 2013. In Germany, the growth rates over these periods were 12.7% and 2%, respectively, over the same time period. The same data in OECD (2015) show that the overall proportion of nonstandard work in the United Kingdom stayed virtually constant at around 33% from 1995 to 2013, while

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in Germany the nonstandard proportion rose from just under 30% to just under 40% over the same time period. In the United States, though Katz and Krueger (2019a) initially found a rise in independent contractors, freelancers, temporary workers, and fixed-term contract workers from 10.7% of the labor force in 2005 to 15.8% in 2015, a later paper from the same authors reassessed the data and found a smaller increase of at most a few percentage points (see Katz & Krueger, 2019b for a discussion of the survey weighting issues that caused the initial overestimate of the growth in alternative working arrangements). It is important to map the consequences of these changes in working arrangements across all three countries into what they mean for job insecurity as experienced by workers; this is what our paper does. It shows that, while some forms of nonstandard work are associated with greater job insecurity, the overall changes associated with any rises in the incidence of nonstandard work are small.

Other objective measures of job insecurity, for example, labor turnover rates (Neumark et al., 1999; Farber, 1999; Fujita, 2018; and Molloy et al., 2016) and the job tenure distribution (Neumark, 2000; Jaeger & Stevens, 1999; Hollister, 2011; and Bachmann & Felder, 2018), show little evidence of a rise in insecurity. Two recent papers, Molloy et al. (2020) and Copeland (2019), find no change in median job tenure of employees in the United States since the early 1980s—though the absence of change in median tenure does mask falling tenure among men (primarily through the 1990s), counteracted by rising tenure among women. These studies do not include the self-employed or "gig economy" workers, but the surveys we study for the UK and Germany contain information on job tenure for all workers, including these groups.

It is possible that objective measures of job security may not have changed but that workers feel more insecure, that is, subjective job security may have fallen. This would be a cause for concern, as there is evidence that self-perceived job insecurity, whether or not a termination is realized, has a detrimental impact on the worker's psychological health, stress levels, and job attitudes (see Ferrie, 2001 and Sverke et al., 2002 for summaries of the early literature, and Benach et al., 2014 or Laszlo et al., 2010 for more recent work). Some work has also found real effects of anticipated job loss: Hendren (2017) shows that fear of job loss lowers consumption and increases spousal labor supply even before job loss realization, while Basten et al. (2016) and Gallen (2013) both find evidence of increased saving in the years leading up to job loss or in an environment of increased layoff risk.

Most of the existing literature on subjective job security (Luebke & Erlinghagen, 2014; Erlinghagen, 2008; Green, 2009) focuses on cross-sectional analysis rather than long-term trends. It has found, for example, that temporary workers and those on fixed-term contracts report higher insecurity (Luebke & Erlinghagen, 2014; Keim et al., 2014). Some of the literature also compares perceived job security across countries, with mixed results: Hank and Erlinghagen (2011) conclude that factors like employment protection legislation and levels of social trust cannot significantly explain job security on an individual level. One of the only robust results that has emerged from these macrolevel studies is the correlation between the unemployment rate and the proportion of workers that feels insecure (Anderson & Pontusson, 2007; Erlinghagen, 2008; Schmidt, 1999; Luebke & Erlinghagen, 2014). There is little literature on longer-run trends in self-perceived job security, though Molloy et al. (2020) do have a brief discussion of trends in the United States using the General Social Survey, noting that changes in the job tenure distribution in the United States since the 1980s, discussed above, have not been associated with a rise in perceived job insecurity (and in fact, they find that the subjective job security of short-tenured workers has improved).

In this paper, we examine trends in subjective job security in three countries (the United States, the United Kingdom, and Germany) over the past four decades. The paper shows that, on the eve of the COVID-19 pandemic, workers in all three countries felt as secure as they ever have in the past 30 years. This is partly because job insecurity is very cyclical [e.g., rising in the Great Financial Crisis (GFC) beginning in 2007–2008 and almost certainly rising during the pandemic], and pre-COVID unemployment rates were very low, but there is also no clear underlying trend towards increased subjective measures of job insecurity. We find that adjusting for changes in demographic and other socioeconomic characteristics in the workforce makes little difference to the overall raw trend, and that the proportion of workers that feels insecure at any given point in time has not seen any sizable secular rise over the time period studied and shows a marked decline in the United Kingdom. This result is robust across all three countries in the sample, and we find very little heterogeneity in trends across different demographics and job types. Evidence from the rest of Europe suggests that these stylized facts are also likely to be true on the continent, with the proportion of workers that feel insecure today no higher than it was before the GFC in most European countries. Our findings call into question the validity of the "New Employment Narrative": by our accounting, there simply is not enough evidence that workers are more likely to feel insecure today than they did a few decades ago to support the claims made by those who promote narratives that emphasize the rise of the "precariat" as a new, highly insecure stratum of workers on flexible contracts.

The plan of the paper is as follows. Section II describes the data we use, explains how each survey measures subjective job security, and measures the raw trend in subjective job security over time in each of the three countries, comparing this trend to the unemployment rate and job separation rates. In section III, we adjust these raw trends to account for the changing demographic and socioeconomic characteristics of the workforce, and we show that the absence of a rise in subjective insecurity is robust to these adjustments. We also calculate the marginal effects of some of these worker characteristics on job security, confirming that job security varies across demographic and job categories. In section IV, we investigate whether the heterogeneity in levels that we find extends to heterogeneity in trends; we find that most subgroups have experienced remarkably similar trends, and that nearly all types of workers were experiencing record lows in subjective insecurity before the COVID-19 pandemic began. In section V, we look at broader evidence on job security in Europe, and we show that the proportion of workers that report feeling insecure has not risen in the vast majority of European countries since the GFC. Section VI considers the argument that the rise of the "precariat" is to be found in dimensions of security other than the subjective risk of job loss. We show that the level of general job satisfaction shows similar trends to those in subjective job security, suggesting there is no large determinant of job satisfaction missing from our analysis. Section VII concludes.

II. Data

This section describes each data set, the questions on perceived job insecurity (different in each survey), and the methodology used to construct a binary variable that indicates whether a respondent feels insecure in their job for the three data sets that do not ask respondents for a numerical probability of job loss.

A. *U.S. General Social Survey (GSS)*

For the United States, we use two surveys: the General Social Survey (GSS) and the Health and Retirement Study (HRS). The GSS is a repeated cross-sectional survey that has been conducted annually by the National Opinion Research Center at the University of Chicago since 1972, and aims to catalog Americans' attitudes towards various political, economic, and social issues.[1] The master data set which contains all waves of the survey has 64,814 individual observations. Not all respondents are employed, and not all respondents give answers to all of the questions that form the covariates used in the analysis below.

The GSS asks respondents if they are likely to lose their job on a scale of 1–4, with 1 being "very likely," 2 being "fairly likely," 3 being "not too likely," and 4 being "not likely." To enable a simpler presentation of results, we construct a binary variable that takes the value 1 for respondents that answer 1 or 2, and the value 0 if they respond 3 or 4.[2] The question was first asked in 1978 and subsequently asked in 1983, 1985, 1986, 1988–1991, 1993, and then every two years from 1994 to 2018. We drop observations that do not have data on the respondent's sex, age, race/ethnicity, marital status, country of birth, education, self-employment status, part-time work status, union membership, and employment industry, as well as those over the age of 65. In total, we have 11,170 observations covering the period from 1978 to 2018, with around 500 observations per year. We use the cross-sectional weight variable *wtssall* in all our analyses, except where noted.

B. *U.S. Health and Retirement Study (HRS)*

The HRS is a survey sponsored by the National Institute on Aging (grant number NIA U01AG009740) and is conducted by the University of Michigan.[3] It surveys a panel of Americans over the age of 50 every two years, collecting information on financial and labor market status, the health of the respondent, insurance coverage, and demographic characteristics. Since 1996, it has also asked respondents who are employed to assess the likelihood that they will lose their job in the next year, with the exception of the 2008 survey. We restrict our sample to those who are employed and of working age and who have nonmissing data on the same covariates as the GSS (with the exception of industry codes, which are masked in the public release version of the HRS), leaving an unbalanced panel from 1996 to 2016 with between 3,000 and 5,000 observations per year, for a total of 54,946 observations. We use the cross-sectional weight variable *wtresp* in all analyses, except where noted.

The HRS has the disadvantage that it covers a shorter sample period and more specific population than the GSS (it only samples workers over the age of 50), but it also possesses a number of advantages. First, it is a panel data set, so one can verify (as Hendren, 2017 does) whether those who report that their job is insecure are more likely to subsequently lose their job. Secondly, the form of the job security question is different from the other data sets. It asks respondents to give a numerical probability that they will lose their job in the next year. The wording of this question follows the recommendation of Manski (2004), who argues that expectation questions contain useful information, but that these questions would more usefully be formulated as explicit probabilities of events such as job loss.

The proportion of U.S. workers that report feeling insecure is plotted over time in figure 2 for the GSS, and the mean response in percentage points for the HRS. Consistent with the findings of Fullerton and Wallace (2007) and Molloy et al. (2020), this series does not demonstrate a noticeable secular trend in reported job security. Reassuringly, the trends are similar for both the GSS and HRS questions—a rise in the early 2000s and during the GFC (i.e., tracking the business cycle), but flat or falling otherwise—suggesting

[1] Smith, Tom W., Davern, Michael, Freese, Jeremy, and Morgan, Stephen L., General Social Surveys, 1972-2018 [machine-readable data file]. Principal Investigator, Smith, Tom W.; Co-Principal Investigators, Michael Davern, Jeremy Freese and Stephen L. Morgan; Sponsored by National Science Foundation. –NORC ed.– Chicago: NORC, 2019. 1 data file (64,814 logical records) + 1 codebook (3,758 pp.). – (National Data Program for the Social Sciences, no. 25).

[2] In appendix A.2, we show that the trend in the nonbinned responses does not affect our conclusions about the overall trend in job security.

[3] "Health and Retirement Study [RAND HRS Longitudinal File 2018 (v1)] public use data set. Produced and distributed by the University of Michigan with funding from the National Institute on Aging (grant number NIA U01AG009740). Ann Arbor, MI (2018)."

FIGURE 2.—JOB INSECURITY IN THE UNITED STATES, 1978–2018

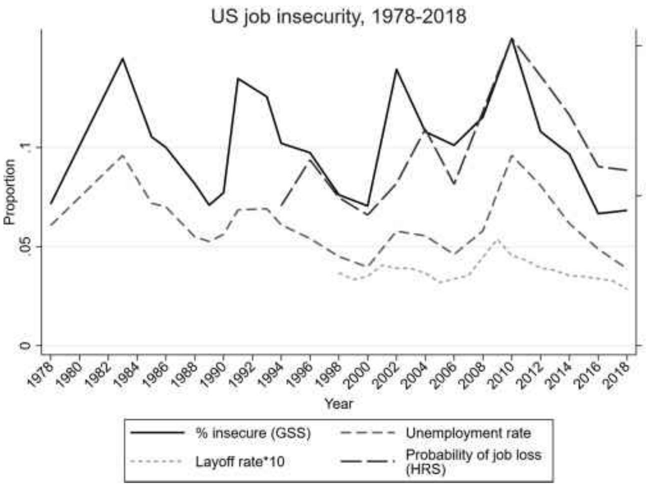
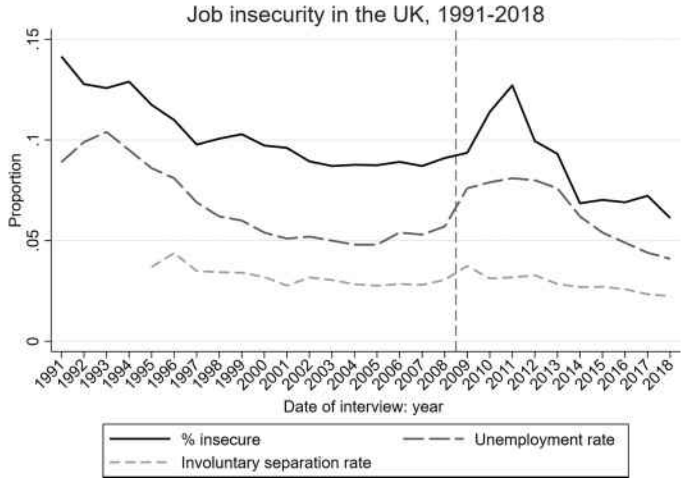


FIGURE 3.—JOB INSECURITY IN THE UNITED KINGDOM, 1991–2018



that the way the question is formulated does not affect changes over time. Figure 2 also plots the annual unemployment rate from FRED (on the left-sided axis), and the layoff rate from the CPS (on the right-sided axis, multiplied by 10 to aid comparison to the other series),[4] two more objective measures of job insecurity.

C. British Household Panel Survey (BHPS) and U.K. Household Longitudinal Survey (UKHLS)

The BHPS (1991–2009) and its successor survey, the UKHLS (2009–present), are longitudinal surveys of British households conducted by the Institute for Social and Economic Research at the University of Essex.[5] The 18 waves of the BHPS have 10,000–15,000 respondents per year, while the UKHLS has around 40,000–50,000 per wave, each of which spans two calendar years. Restricting the data to working-age, employed adults with data on the covariates listed in section IIA, plus temporary work status and job tenure, we have a total of 144,444 usable observations, with between 4,000 and 5,000 observations per year until the end of the BHPS. From 2009 on, single years have between 6,000 and 10,000 observations.[6] Because individual UKHLS waves span two calendar years, and because only the relative magnitudes of weights in a given year matter to our estimation procedures, we adjust the provided cross-sectional weight variable (*xr_wght* in the BHPS, *indinus_xw* in the UKHLS) by replacing observation i in calendar year t's weight w_{it} with $\frac{w_{it}}{\overline{w_t}}$, where $\overline{w_t}$ is the mean cross-sectional weight for observations in year t. Omitting this adjustment procedure does not affect our results.

The question on subjective job insecurity is different in the BHPS and the UKHLS. The BHPS asks respondents to rank how satisfied they are with their job security on a scale of 1–7, with 1 indicating "Not satisfied at all," 4 indicating "Neither satisfied nor dissatisfied," and 7 indicating "Completely satisfied."[7] We define insecurity as giving a response of 1, 2, or 3 to this job security question.[8] The UKHLS asks respondents how likely it is that they will lose their job in the next 12 months, on a scale of 1–4, with 1 indicating "Very likely," 2 indicating "Likely," 3 indicating "Unlikely," and 4 indicating "Very unlikely"; this question is only asked in even-numbered waves. The difference in the question leads to a break in the time series, which is especially problematic because it occurs at the same time as the financial crisis, a point at which there may have been large changes in job security.

To deal with this problem, we use the fact that in Waves 6 and 7 (1996 and 1997) of the BHPS, respondents are asked both the original BHPS question and the UKHLS question about the likelihood of job loss. We use this crosswalk to make the two surveys comparable by using Waves 6 and 7 to "translate" the 1–7 scale of the earlier BHPS question into the UKHLS question responses using the cross-tab of the two questions, conditional on other characteristics.[9] This process allows us to assign each individual giving a response to the BHPS question a probability that they would have given the response to the UKHLS question; see appendix A.1 for the technical details of this process and a discussion of the question of whether to include other covariates in the crosswalk. Using this approach, we can compute an estimate of the proportion of the workers in the BHPS who would have reported they felt insecure using the UKHLS question. Figure 3 plots our estimate of the

[4]Construction of this measure is described in appendix A.3.

[5]University of Essex, Institute for Social and Economic Research. (2020). Understanding Society: Waves 1–9, 2009–2018 and Harmonised BHPS: Waves 1–18, 1991–2009. [data collection]. 12th Edition. UK Data Service. SN: 6614, http://doi.org/10.5255/UKDA-SN-6614-13

[6]Because it falls in the gap between the end of the BHPS and the beginning of the UKHLS, 2009 has only 61 observations.

[7]Numbers in between do not have descriptions.

[8]Though, as in the U.S. case, none of the conclusions are sensitive to converting to a binary outcome.

[9]We implement the crosswalk in this direction because the UKHLS question is more similar to that asked in the United States and the German data.

proportion of U.K. workers feeling insecure over time, together with the U.K. unemployment rate and the involuntary separation rate (see appendix A.3 for calculation details). As in the United States, aggregate perceived job insecurity is cyclical, but with no long-run trend.

D. *German Socioeconomic Panel (SOEP)*

The German Socioeconomic Panel (SOEP) is an independent longitudinal survey conducted by DIW Berlin that aims to collect data that allow researchers to "…study processes of transformation and change in our society."[10] The survey has been conducted annually since 1984, and in 1992 the survey expanded to include respondents from East Germany in addition to the original West German respondents. The survey began by interviewing around 15,000 individuals per wave, rising to 30,000 per wave by 2000, and up to 60,000 per wave in recent years. Restricting the sample to working-age adults who answer the job security question and have nonmissing responses for the covariates described in section IIA,[11] plus job tenure, leaves 317,040 observations, with between 1,200 and 2,200 observations per year between 1985 and 1988, between 5,000 and 7,800 observations per year between 1989 and 1999, and between 11,000 and 17,000 observations per year between 2000 and 2018. We use the cross-sectional weight variable *phrf* in all analyses, except where noted.

Each year, SOEP asks whether the respondent is worried about his or her job security. Respondents can answer either 1 (very concerned), 2 (somewhat concerned), or 3 (not concerned). From these responses, we construct a binary variable that takes value 1 if the respondent answers 1 to the job security question, and value 0 if they give answer 2 or 3. The SOEP contains two other questions about the respondent's job security that are potentially relevant to our study of long-term trends in perceived insecurity—the first asks respondents how likely it is that they will lose their job on a scale of 1–4 with 1 indicating "Definitely," 2 indicating "Probable," 3 indicating "Improbable," and 4 indicating "Definitely not," while the second question asks the respondent to give a numerical probability (rounded to the nearest multiple of 10) that they will lose their job. However, because these two questions are asked irregularly[12] and not over the course of the entire sample, we focus on the first question and relegate analysis using the latter two to appendix A.4 to establish the robustness of our main conclusions.

[10]Socioeconomic Panel (SOEP), data for years 1984–2018, version 3y, SOEP, 2019, doi:10.5684/soep.v3y.; Goebel, Jan, Markus M. Grabka, Stefan Liebig, Martin Kroh, David Richter, Carsten Schröder, Jürgen Schupp. 2019. The German Socioeconomic Panel Study (SOEP). Jahrbücher für Nationalökonomie und Statistik / Journal of Economics and Statistics, 239(2), 345–360.

[11]Though we do not have data on respondents' ethnicity or union membership status. For more details on the covariates we have for each data set, see section III and table 1 therein.

[12]The first is asked nine times between 1985 and 1998, and the second is asked nine times between 1999 and 2018.

FIGURE 4.—JOB INSECURITY IN GERMANY, 1985–2018

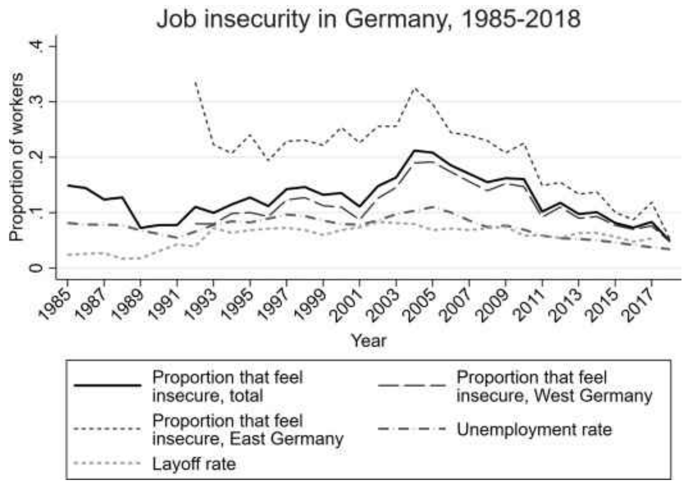


Figure 4 shows the proportion of German workers that feel insecure (East and West Germany combined and separately), overlaid with the annual unemployment rate and the involuntary separation rate.[13] Up to about 2005, job insecurity seemed to be rising, though it was much higher in the East than the West, almost certainly the result of the dislocation from reunification. But, after 2005 job insecurity is falling in both parts of Germany, and there is a noticeable convergence between East and West. The proportion of German workers that feel insecure today is the lowest in the sample period and over 15pp lower than its peak in the mid-2000s. Additionally, there is no long-term upward trend in involuntary separations, and our series has had a consistent negative trend since the early 2000s.

E. *Interpreting and Reconciling Trends in Subjective Job Security*

The graphs above tell us how people answer questions on subjective job security, but not what these responses mean. One concern is that questions on subjective expectations reflect little more than noise, as they are not based on actual behavior. However, Manski (2004) argues that questions on expectations often contain useful information. The simplest way to check this is to see whether variation in expectations is predictive of future events. In our context, we should check whether our subjective measures of job insecurity have any predictive power for actual subsequent job loss.[14] Using the panel structure of the HRS, BHPS/UKHLS, and SOEP data, we show in appendix B that insecurity today is strongly associated with job loss or job change tomorrow for all three countries. Thus, we can feel confident that the

[13]Details of the calculation process for these separation rates can be found in appendix A.3.

[14]Even if there were no predictive power, one might still be concerned about subjective measures of job security because of the stress costs of feeling insecure, as well as other anticipatory effects (see Hendren, 2017).

responses to the job security questions in our three data sets are informative about the changing (or unchanging) nature of objective job security, and do not simply illustrate changes in stress stemming from an ignorance of objective job loss probabilities on the part of workers.

Manski (2004) also argues that expectation questions that elicit subjective probabilities are to be preferred to other, vaguer questions. In our data sets, only the HRS takes this approach, but the fact that it has similar trends to the GSS (which, in turn, asks similar questions to the one in the British and German data) provides reassurance that the exact form of the question is not the main driver of our results.

Our results tell us what has been happening to subjective job insecurity, but not how this can be reconciled with other trends in labor market outcomes such as the decline in the labor share that has occurred in the United States and, to a smaller extent, Germany, though not in the United Kingdom (see, e.g., Pak & Schwellnus, 2019). In the model of Mortensen and Pissarides (1994), a fall in worker bargaining power that reduces the labor share reduces the rate of job destruction, increasing job security. However, appendix D shows that this prediction no longer necessarily holds in a modified version of Mortensen and Pissarides (1994) when one relaxes the assumption that all new matches are at the highest level of productivity. It is therefore possible to construct theoretical models in which there is any or no relationship between worker power and job security.

III. Adjusting for the Characteristics of Employment

Although there is little evidence in the raw data of rising job insecurity for any of the three countries we study, it is possible that this is because the structure of the workforce is changing in ways that lead to less overall job insecurity even as job insecurity is deteriorating for any individual worker. For example, if older workers feel more secure on average, an increase in the average age of employees would produce a downward trend in aggregate job insecurity (Molloy et al., 2020 show that the aging of the workforce can explain the fall in job-to-job mobility in the United States). To address this, this section estimates models for the probability that a worker reports feeling insecure in their job: $Pr(insecure = 1)$, controlling for socioeconomic and demographic characteristics of workers that may affect job security, while also including dummy variables for each year. The coefficients on the year dummies then tell us about trends in perceived job insecurity after accounting for changing worker characteristics, relative to a base year.

We use the following controls in these regressions. We include standard demographic controls for the individual: dummies for sex, ethnicity, immigrant status, marital status, a quadratic function of age, and a dummy for if the individual has completed a tertiary degree. We also control for various job characteristics: job tenure, a dummy variable for whether a worker is a temporary employee or on a fixed-term contract, a dummy for part-time work, and a dummy for self-employment (with and without employees in the United Kingdom, where we have data on the number of employees). For Germany, we also include a dummy for residing in the former East Germany as well as a dummy variable for being in marginal employment (sometimes known as a "mini job"), which became more common after the Hartz reforms and might be thought to be less secure. We also control for industry using single-digit International SOC codes for the United Kingdom and Germany, single-digit NAICS codes for the GSS, and manually constructed single-digit industry codes derived from the 1980 Census and 2002 and 2007 NAICS codes for the HRS. We do not have all variables for all countries: we are missing whether a worker is on a temporary contract and job tenure for both U.S. data sets, union membership, and ethnicity in Germany, and we lack data on the job security of the self-employed in the HRS, as well as in the United Kingdom after the BHPS transitioned to the UKHLS because the job insecurity question was no longer asked to the self-employed. The longitudinal nature of the HRS, BHPS/UKHLS, and SOEP data allows us to include individual fixed effects, which partial out time-invariant unobserved heterogeneity. Inclusion of fixed effects does not make a substantive difference to the trends, so in order to facilitate comparisons with the U.S. GSS, we choose to exclude the individual FEs and include the time-invariant demographic controls. Results from the equivalent regressions that include individual fixed effects can be found in appendix C.4.

Weighted summary statistics for the control variables can be found in table 1, with separate columns for pre- and post-2008 data for each survey. The rise of "nonstandard employment" is apparent, with a marked increase in part-time work in all three countries—however, trends in temporary work are more muted, higher in Germany by 2pp, but falling by around 0.5pp in the United Kingdom. The rise of short-term employment contracts in Germany can also be seen in the rise of marginal employment, from around 3.5% of our sample pre-2008 to nearly 7% post-2008. Self-employment has roughly the same prevalence pre- and post-2008 in the GSS (12%–13%) and the SOEP (10%–11%). It is also notable that mean job tenure in both the United Kingdom and in Germany is higher post-2008, perhaps consistent with Bachmann and Felder (2018)'s finding that crisis-era layoffs in Europe were concentrated among short-tenure workers, increasing the average tenure of remaining workers.

For the GSS and the SOEP, we estimate logistic regressions where the dependent variable is our binary measure of perceived job insecurity, and for the HRS we estimate a Linear Probability Model with the elicited probability of job loss (1–100) as the dependent variable; technical details of our approach for the GSS, HRS, and SOEP data can be found in appendix C.1. For the United Kingdom we use a different approach (based on the equivalence between a Poisson regression and a logit model) to handle the fact that we do not observe the "true" response to the BHPS job security question for the UKHLS portion of the sample, only an

TABLE 1.—DESCRIPTIVE STATISTICS, PRE- AND POST-2008

	GSS		HRS		BHPS		SOEP	
	pre-2008	post-2008	pre-2008	post-2008	pre-2008	post-2008	pre-2008	post-2008
Age	38.8	41.4	56.9	57.6	39.2	41	39.7	42.6
	(11.7)	(12.7)	(3.79)	(3.8)	(12.3)	(12.5)	(11.5)	(11.8)
Male	.529	.505	.479	.485	.525	.483	.576	.529
	(.499)	(.5)	(.5)	(.5)	(.499)	(.5)	(.494)	(.499)
Married	.628	.521	.732	.722	.575	.531	.589	.525
	(.483)	(.5)	(.443)	(.448)	(.494)	(.499)	(.492)	(.499)
Non-white	.165	.266	.135	.194	.0382	.078		
	(.371)	(.442)	(.342)	(.395)	(.192)	(.268)		
Immigrant	.0888	.127	.0818	.115	.0565	.1	.124	.14
	(.284)	(.333)	(.274)	(.319)	(.231)	(.3)	(.33)	(.347)
Tertiary degree	.257	.324	.549	.665	.474	.455	.27	.326
	(.437)	(.468)	(.498)	(.472)	(.499)	(.498)	(.444)	(.469)
Part-time worker	.167	.174	.118	.112	.245	.337	.153	.197
	(.373)	(.379)	(.322)	(.315)	(.43)	(.473)	(.36)	(.398)
Union member	.155	.115	.22	.188	.306	.293		
	(.362)	(.319)	(.414)	(.391)	(.461)	(.455)		
Temporary worker					.0689	.0641	.112	.132
					(.253)	(.245)	(.315)	(.339)
Self-employed without employees[a]	.129	.117			.119		.113	.1
	(.335)	(.322)			(.324)		(.316)	(.301)
Self-employed with employees					.0358			
					(.186)			
Marginally employed							.0362	.0688
							(.187)	(.253)
Job tenure					5.05	8.44	9.87	10.9
					(6.57)	(7.36)	(9.79)	(10.5)
N	8648	2521	29920	24518	84882	59562	164931	152109

[a]This row displays the mean for all self-employed workers for the United States and Germany.

estimate of the probability of a particular response; technical details of our approach for the BHPS and UKHLS data can be found in appendix C.2. The adjusted trends are marginal effects from the regressions, evaluated for each observation and then averaged over the sample, relative to the base year of 2001 (2002 for the United States, because the GSS and HRS lack data on job security in odd years). The base year is chosen to represent the pre-GFC baseline for job security. Because Germany experienced a pre-GFC downturn in the mid-2000s, which led to a rise in unemployment and perceived job insecurity, we have chosen 2001 as a base year that precedes this downturn.

A. *Trends in Adjusted Perceived Job Insecurity*

One striking feature of the results for all countries is the similarity of the unadjusted and adjusted trends; the correlation is .99 for the GSS, .97 for the HRS, .96 for the BHPS/UKHLS, and .99 for the SOEP. To summarize the results concisely, we use a simple linear regression model in which the unadjusted and adjusted marginal effects of the year dummies from the first-step regressions described above are regressed on a linear time trend (with time measured in decades because any trends are small) and the unemployment rate (to capture the cycle). Because the dependent variable in these regressions is estimated, we follow the advice of Lewis and Linzer (2005) in using heteroskedasticity-robust standard errors, as opposed to weighted least squares. To provide reassurance that these regressions are not missing some important aspect of the data, the unadjusted and adjusted trends in perceived job insecurity (i.e., the dependent variable in these regressions) are presented in appendix C.3. We also discuss and test other possible functional forms for the time trend, as there is no intrinsic reason that the secular trend in job security would be linear in time. Other functional forms yield qualitatively similar results, so for simplicity we use a linear time trend throughout the analysis in the main text.

Table 2 presents the results from these regressions for our four data sets, along with the R-squared and the aggregate insecurity level in the baseline year, in order to make it easier to assess the magnitude of the marginal effects. Coefficients should be interpreted as the percentage point change in job insecurity per unit change in the covariate, which corresponds to a one percentage point change in the unemployment rate, or ten years for the time trend. In all data sets there is a significant positive relationship between job insecurity and the unemployment rate, though there is some variation in the magnitude. The effect appears to be strongest in Germany, where a 1pp rise in the unemployment rate is estimated to raise the level of job insecurity by nearly 2pp. Our results here are in keeping with other papers that investigate trends in subjective job insecurity: Luebke and Erlinghagen (2014), Schmidt (1999), and Anderson and Pontusson (2007) all find the contemporaneous unemployment rate to be strongly predictive of subjective insecurity.

For the United States, the estimated underlying trend in job insecurity is positive, but the only trend coefficients that

TABLE 2.—LINEAR TIME TREND AND UNEMPLOYMENT RATE REGRESSION RESULTS

	U.S. (GSS)		U.S. (HRS)		United Kingdom		Germany	
	No controls	Controls	No controls	Controls	No controls	Controls	No controls	Controls
Unemployment rate*100	1.39***	1.4***	.868***	.898***	.688***	.628***	1.99***	1.87***
	(.175)	(.177)	(.0443)	(.0539)	(.133)	(.138)	(.15)	(.161)
Linear time trend*10	.38	.313	.84*	.964*	−1.23***	−1.18**	1.42***	1.08*
	(.337)	(.357)	(.282)	(.316)	(.319)	(.348)	(.377)	(.413)
N	22	22	12	12	27	27	34	34
R^2	0.607	0.602	0.883	0.883	0.823	0.777	0.791	0.733
Baseline year insecurity	13.9	13.9	15.4	15.4	9.6	9.6	11.1	11.1

This table displays the results of regressions of the average marginal effects of the year dummies (with and without controls) on the unemployment rate and a linear time trend using the GSS, HRS, BHPS/UKHLS, and SOEP data. Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, and *** $p < 0.001$.

are significantly different from zero are in the HRS. Even in this case, though, the magnitude of the time trend coefficient is very small—a 0.9pp increase in insecurity over 10 years without controls (0.87pp without controls), from a baseline of 15.4%. In the United Kingdom, the time trend is significantly negative in both regressions, implying a 10-year fall in job insecurity of around 1.2pp (baseline mean = 9.6%). For Germany the estimated underlying trends are positive and significantly different from zero, though again here the magnitudes are small; about a 1.4pp point rise in job insecurity over a decade without controls, and only 1.08pp after adjusting for composition. To put this in context, the effect of a decade in our estimation results is equivalent to the effect of a 0.5pp increase in the unemployment rate. Because the unemployment rate in Germany is lowest at the end of the sample period and has much bigger effects on job insecurity, the overall level of job insecurity is also lowest at the end of the sample period in spite of the small positive underlying trend.

To conclude, there is at most a weak positive underlying trend in job insecurity but a clear large impact of unemployment. The similarity of adjusted and unadjusted results suggests that compositional changes in the work force cannot explain the absence of a large increase in perceived job security in these three countries over the past four decades—in fact, the inclusion of these controls leaves even less evidence of a secular increase in job insecurity. However, personal and job characteristics are indeed correlated with reported levels of job insecurity, as the next section shows. Clarifying the magnitude of these differences in levels will help us understand why the change in the proportion of jobs with atypical working arrangements has not led to a concomitant rise in insecurity.

B. *The Impact of Personal and Job Characteristics on Perceived Job Insecurity*

Table 3 presents the marginal effects of selected covariates of interest on the probability of feeling insecure from the regressions estimated in section III. These estimates should be interpreted as the difference in the probability of feeling insecure for workers of the given type who would otherwise have job characteristics which would have led to a level of job insecurity at the level listed in the "baseline insecurity" row of table III.[15] We are agnostic about whether these relationships are causal. Many of our findings are in accordance with the findings of previous studies on the determinants of job security: temporary workers are much more likely to feel insecure than permanent workers (as in Clark & Postel-Vinay, 2009; Keim et al., 2014; and Luebke & Erlinghagen, 2014), and higher-educated workers are less likely to feel insecure. Immigrants and those of nonwhite ethnicity also feel more insecure. In the United States and Germany, the self-employed are less likely to feel insecure; this is also true of the self-employed with employees in the United Kingdom, while the coefficient for sole proprietors and freelancers is statistically insignificant and very close to zero, suggesting that self-employment's impact on insecurity depends on whether one has employees. The estimated effect of being in part-time employment also varies across countries: in the GSS, part-time workers appear to feel less secure, but in the United Kingdom and in Germany they are more secure. Interestingly, the marginally employed in Germany are significantly more secure than the nonmarginally employed by over 4pp.

The results in table 3 can help us understand why the unadjusted and adjusted trends are so similar. The impact of some demographic variables such as age and sex are relatively small, so a change in the demographic mix of employment would not be expected to change the overall level of job insecurity. Most other effects are no greater than 3 or 4pp, which is of modest magnitude relative to the baseline level of insecurity in the GSS (13.9%) and the BHPS/UKHLS (9.6%)—though in the United States (GSS), immigrants and nonwhites are likely to feel around 6pp more insecure than natives and whites. The largest effect by some distance is for being a temporary employee—an effect of 28pp in the U.K. data, and 9.9pp in the Germany data. One might expect these level differences to play an important role in explaining trends in job security, because it is a commonly held view that temporary work and other forms of

[15] This is the mean level of insecurity corresponding to the covariate values observed in the sample in the base year, the point at which we evaluate the marginal effects.

TABLE 3.—AVERAGE MARGINAL EFFECTS OF COVARIATES ON PROBABILITY OF INSECURITY

	U.S. (GSS)	U.S. (HRS)	United Kingdom	Germany
Temporary employee			.2812***	.0985***
			(.0091)	(.0041)
Self-employed without employees[a]	−.0488*		7.2e-04	−9.9e-04
	(.0197)		(.0099)	(.0032)
Self-employed with employees			−.0197*	
			(.0099)	
Immigrant	.0597***	.0437***	.0174	.0359***
	(.0159)	(.0064)	(.0093)	(.0026)
Nonwhite	.0652***	.0128**	.035***	
	(.0075)	(.0038)	(.0101)	
Part-time worker	.0677***	.0177***	−.0145	−.0273***
	(.0107)	(.0038)	(.0086)	(.0033)
Marginally employed				−.0422***
				(.0044)
Higher education degree	−.0719***	−.032***	−.0119	−.0188***
	(.0087)	(.0035)	(.0086)	(.0033)
Union active at workplace	.0327**	−.0415***	.013	
	(.0119)	(.0033)	(.0088)	
Male	.0179**	.0016	−.0101	−.0122***
	(.0059)	(.0015)	(.0079)	(.0017)
Age	.0031	.0089	.0028	.0087***
	(.0023)	(.0053)	(.0059)	(5.2e-04)
$(\text{Age}/10)^2$	−.0047	−.0085	−.0013	−.0092***
	(.0028)	(.0043)	(.0061)	(6.4e-04)
Length of job tenure			−2.9e-04	−.0013***
			(.006)	(1.2e-04)
N	11169	46903	144444	310738
Baseline insecurity	.1394	.1557	.0968	.1106

This table displays the marginal effects of covariates of interest on the probability of feeling insecure, derived from the logistic regressions specified in appendix B for the United States and Germany, and from the Poisson regression, specified in appendix C, used to analyze the UK data. a: This row gives the marginal effect for all self-employed (with and without employees) for the United States and Germany, as the GSS and SOEP lack data on the breakdown of the self-employed by number of employees. Standard error in parentheses. $^*p < 0.05$, $^{**}p < 0.01$, and $^{***}p < 0.001$.

nonstandard employment have been rising. However, not all types of nonstandard employment are associated with higher job insecurity (e.g., part-time work and self-employment in some countries, and marginal employment in Germany), and the types of nonstandard employment associated with job insecurity have not risen as much as often suggested. Together, these results imply that the rise of nonstandard work is of limited importance in explaining trends in perceived job insecurity. In appendix E, we present more formal quantitative evidence for this argument, using Oaxaca-Blinder decompositions (modified for our use of a binary dependent variable) and the reweighting methodology of DiNardo et al. (1996) to show that the compositional changes in the labor forces of our three countries that have occurred over the course of our sample, combined with the level differences in subjective job insecurity implied by the average marginal effects we estimated in our models in section IIIB, are not large enough to lead to big rises in aggregate job insecurity.

However, it is possible that our conclusions could be the result of using an empirical specification which allows different types of workers and jobs to be associated with different levels of job security, but requires the trends to be the same. Looking solely at the aggregate trend may obscure the fact that some sub-groups may now be experiencing greater insecurity than they did in the past. The next section therefore investigates whether there is important heterogeneity.

IV. Heterogeneity in Trends in Perceived Job Insecurity

There are obviously many subgroups that could be investigated, and a fishing expedition would undoubtedly uncover some with increased subjective job insecurity. Here, we focus on subgroups where there has been more concern about deterioration in the quality of work: temporary workers versus those on permanent contracts, part-time versus full-time workers, self-employed versus non-self-employed, men versus women, the university-educated versus less well-educated, the young (under 25) versus prime-aged (25–49) versus the old (over 50), and new starters (job tenure less than a year) versus those with tenure longer than a year. Our technical approach for adjusting the results for compositional changes is the same as before, but now we estimate the model on the subgroups; details of the approach can be found in appendix C. We again summarize the results using the simple regression where the estimated marginal effects of the year dummies are regressed on the unemployment rate and a decadal linear time trend, with robust standard errors. The coefficients on the unemployment rate are shown in table 4 and those on the linear trend in table 5. These coefficients should be interpreted as the percentage point change in insecurity (relative to the base year) associated with a 1pp change in the aggregate unemployment rate and over the course of 10 years, respectively. The original plots with the year dummy marginal effects and CIs for these subgroups can be found in appendix F.1.

TABLE 4.—AVERAGE MARGINAL EFFECT OF UNEMPLOYMENT RATE*100, BY SUBGROUP

	U.S. (GSS)	U.S. (HRS)	United Kingdom	Germany
Temporary workers			1.63***	2.59***
			(0.34)	(0.24)
Permanent workers			0.09	1.80***
			(0.20)	(0.16)
Part-time workers	2.55**	0.99***	0.32	1.43***
	(0.85)	(0.11)	(0.17)	(0.25)
Full-time workers	1.14***	0.88***	0.71***	1.99***
	(0.23)	(0.06)	(0.14)	(0.16)
New starters			1.66*	2.26***
			(0.65)	(0.27)
Non-new starters			0.63***	1.84***
			(0.14)	(0.15)
Males	1.60***	0.93***	0.71***	1.97***
	(0.29)	(0.05)	(0.15)	(0.15)
Females	1.26***	0.86***	0.53**	1.79***
	(0.16)	(0.07)	(0.15)	(0.18)
Over 50 y.o.	1.40***		0.29	1.56***
	(0.23)		(0.16)	(0.14)
25–49	1.14***		0.82***	1.95***
	(0.21)		(0.14)	(0.18)
Under 25 y.o.	3.40**		0.46**	2.47***
	(0.88)		(0.14)	(0.27)
Tertiary degree	0.56*	0.87***	0.72***	1.52***
	(0.20)	(0.07)	(0.13)	(0.18)
No tertiary degree	1.75***	0.95***	0.56*	1.99***
	(0.18)	(0.11)	(0.22)	(0.18)
Marginally employed				1.19*
				(0.51)
Nonmarginally employed				1.90***
				(0.16)
N	21	12	27	34

This table displays the average marginal effect of the linear time trend in a linear regression, where the dependent variable is the AMEs of the year dummies from the main regression specification (Poisson for the UK, logistic for the GSS and SOEP, linear regression for the HRS) with the sample restricted to the subgroup of interest. Standard errors in parentheses. $^{*}p < 0.05$, $^{**}p < 0.01$, and $^{***}p < 0.001$.

TABLE 5.—AVERAGE MARGINAL EFFECT OF LINEAR TIME TREND, BY SUBGROUP

	U.S. (GSS)	U.S. (HRS)	United Kingdom	Germany
Temporary workers			0.22	−0.29
			(0.80)	(0.71)
Permanent workers			−2.37***	1.41***
			(0.48)	(0.35)
Part-time workers	−0.40	0.44	−1.53**	1.07
	(0.81)	(0.47)	(0.45)	(0.91)
Full-time workers	0.30	0.88*	−1.10**	1.09*
	(0.40)	(0.35)	(0.34)	(0.40)
New starters			2.30	0.94
			(1.86)	(0.62)
Non-new starters			−1.23**	1.22**
			(0.35)	(0.38)
Males	0.50	0.83*	−1.35***	0.92
	(0.47)	(0.32)	(0.35)	(0.46)
Females	−0.00	0.95*	−1.08*	1.38**
	(0.31)	(0.39)	(0.40)	(0.42)
Over 50 y.o.	0.51		−2.63***	0.99*
	(0.35)		(0.43)	(0.42)
25–49	0.15		−0.59	1.11*
	(0.45)		(0.37)	(0.45)
Under 25 y.o.	1.25		−0.72	1.93**
	(0.95)		(0.36)	(0.57)
Tertiary degree	−0.18	0.97*	−0.30	−0.06
	(0.39)	(0.36)	(0.31)	(0.46)
No tertiary degree	0.43	0.58	−1.72**	1.51**
	(0.38)	(0.44)	(0.58)	(0.46)
Marginally employed				−0.04
				(2.05)
Nonmarginally employed				1.08*
				(0.40)
N	21	12	27	34

This table displays the average marginal effect of the linear time trend in a linear regression, where the dependent variable is the AMEs of the year dummies from the main regression specification (Poisson for the UK, logistic for the GSS and SOEP, linear regression for the HRS) with the sample restricted to the subgroup of interest. Standard errors in parentheses. $^{*}p < 0.05$, $^{**}p < 0.01$, and $^{***}p < 0.001$.

The coefficients on the unemployment rate shown in table 4 are positive and significantly different from zero in almost every subgroup in every data set, confirming that subjective job insecurity is very cyclical. The job security of temporary workers appears to be more sensitive to the unemployment rate than permanent workers in the United Kingdom and in Germany (though we lack data on temporary employees in the U.S. data). Likewise, new starters experience more cyclical insecurity than incumbent employees in the United Kingdom and in Germany. Evidence on part-time workers versus full-time workers is mixed: in both U.S. data sets, the insecurity of part-time workers is more cyclical than that of full-time workers, but the opposite is true in the United Kingdom and in Germany.

The time trends in table 5, meanwhile, are mostly either negative or not significantly different from zero. The U.S. (GSS) time trends are all statistically insignificant and close to zero. The U.S. (HRS) results do show positive trends in some subgroups in line with the overall results reported earlier, but all of them indicate a rise in insecurity of less than 1pp over a decade. The United Kingdom results show negative trends for all subgroups analyzed with the exception of new starters (whose trend is not statistically significant). The German results generally show positive trends, though the magnitudes are again small. We find support for Bachmann et al. (2020)'s finding about younger cohorts' greater propensity to be atypically employed—they have the largest (significant) trend in any of the four data sets. However, this trend in under-25s is not present in the other countries, where younger cohorts have experienced either a negative trend or one insignificantly different from 0, and even in Germany, a test for the equality of trends across age groups does not allow us to reject the null that they are equal at the 5% level.

The group with the second-largest trend in the GSS, though statistically insignificant, is over-50s, a fact that helps reconcile the HRS results with those from the GSS. Because the HRS is significantly older (a mean age of 56, compared to 40 in the GSS), and older Americans may have experienced a greater (though still small) trend in insecurity, the increase in insecurity in the unadjusted HRS is expected—though again, the trend becomes insignificant when it is adjusted for composition, supporting our claim about the age structure influencing the aggregate trend.

It is notable that the variations in the estimated trends do not generally fit the impression of the groups who are commonly believed to be faring worse—for example, the small downward trend in insecurity for part-time workers is of greater magnitude than that for full-time employees in the United Kingdom and the U.S. (GSS; though it is statistically insignificant in the U.S. case). To take another example, in the United Kingdom, workers without a tertiary degree have experienced a greater fall in insecurity than their university-educated counterparts. In Germany, temporary workers, part-time workers, and new starters all have insignificant time trends, as do the marginally employed, whose trend is negative. Additionally, the pairs of subgroups that we analyze do not appear to have experienced vastly different secular trends in their insecurity over the course of the sample. In appendix F.2, we present p-values from a series of F-tests of the equality of these time trends for the subgroup pairs we study. In most cases, we cannot reject the null hypothesis that the time trends are equal for both subgroups. The main conclusion to be drawn from this exercise is that although there exist differences in levels of job security across subgroups, there is little variation in trends, and the differences that do exist are small in magnitude—no greater than a percentage point or two in terms of divergence over a period of ten years.

V. Broader European Evidence on Job Security

In this section, we briefly consider evidence on perceived job insecurity across a wider set of European countries using data from the European Working Conditions Survey (EWCS).[16] The EWCS is conducted every four to five years in 36 European countries, with a rotating panel of questions and approximately 1,000 respondents per country-year.[17] The 2005, 2010, and 2015 editions of the survey, which contain 106,572 complete responses, asked respondents to rate on a scale of 1–5 their agreement or disagreement with the following statement: "I might lose my job in the next six months." 1 indicates "strongly agree," 2 indicates "tend to agree," 3 indicates "neither agree nor disagree," and 4 and 5 indicate different strengths of disagreement. We drop observations that are missing information on the respondent's age, sex, employment contract type, self-employment status, job tenure, industry code, and highest education level attained, leaving us with 94,186 observations. Descriptive statistics for these variables can be found in appendix G.

Table 6 presents the proportion of each country's respondents that responded 1 or 2 to the question for each survey year, along with the percentage point change in the insecure proportion from 2005 to 2015. All but five countries experienced a rise in insecurity from 2005 to 2010, and 20 of the 36 countries experienced a fall from 2010 to 2015 as the recession's impact began to fade. The overall change between 2005 and 2015, however, is more heterogeneous across countries.[18] It is possible that the cycle can explain most of this; 2010 was shortly after the GFC and the Eurozone crisis lasted until at least 2015, affecting some countries much more than others.

To investigate more formally, table 7 presents results of regressions in which the job insecurity indicator is regressed

Table 6.—Job Insecurity in Europe

	2005	2010	2015	pp. change, 2005–2015
Belgium	9.2	16.0	15.3	6.1
Bulgaria	23.1	29.4	10.9	−12.2
Czech Republic	32.0	34.0	17.0	−15.0
Denmark	7.3	9.8	10.9	3.6
Germany	12.6		9.2	−3.4
Estonia	19.5	35.3	18.5	−0.9
Greece	20.9	19.5	20.8	−0.2
Spain	15.1	24.7	26.0	10.8
France	7.9	12.0	13.5	5.6
Ireland	9.9	25.4	15.7	5.8
Italy	9.0	13.8	20.7	11.7
Cyprus	14.3	19.1	14.2	−0.2
Latvia	18.9	31.4	19.5	0.6
Lithuania	23.0	40.9	13.6	−9.4
Luxembourg	5.6	9.1	11.1	5.6
Hungary	22.0	24.2	17.2	−4.8
Malta	15.4	18.3	9.3	−6.1
Netherlands	17.7	14.0	25.3	7.6
Austria	8.9	10.8	10.5	1.6
Poland	26.7	17.9	23.3	−3.5
Portugal	19.5	18.2	18.7	−0.8
Romania	16.1	25.3	16.8	0.7
Slovenia	27.4	27.3	27.2	−0.1
Slovakia	15.1	13.6	8.2	−6.8
Finland	13.1	15.6	15.5	2.5
Sweden	20.3	22.7	14.6	−5.7
United Kingdom	6.5	12.6	12.9	6.4
Croatia	19.4	26.8	18.3	−1.1
North Macedonia	.	28.6	19.7	.
Turkey	18.2	20.8	15.2	−3.0
Norway	6.7	10.4	10.5	3.8
Albania	.	11.7	13.9	.
Kosovo	.	18.9	.	.
Montenegro	.	20.4	17.3	.
Switzerland	12.2	.	11.7	−0.5
Serbia	.	.	22.9	.

Data from the European Working Conditions Survey Table shows proportion of workforce that feels insecure.

Table 7.—Job Insecurity Regression Results (Europe 2005–2015)

	(1)	(2)	(3)	(4)
2010	0.0251***	0.0270***	0.0319***	0.0281***
	(0.00196)	(0.00287)	(0.000896)	(0.00200)
2015	−0.00789***	−0.00523**	−0.00312**	−0.00405*
	(0.00217)	(0.00181)	(0.000979)	(0.00181)
Unemployment	0.610***	0.732***	0.508***	0.738***
rate*100	(0.0764)	(0.0934)	(0.0689)	(0.0724)
N	94186	94186	94186	94186
Country FEs	No	Yes	No	Yes
Other controls	No	No	Yes	Yes

This table displays coefficients and SEs from a logistic regression of the indicator for feeling insecure on the unemployment rate and year dummies, with 2005 as the base year. Demographic and job characteristic controls are included in models (3) and (4), and country fixed effects in (2) and (4). Standard errors in parentheses. $^{*}p < 0.05$, $^{**}p < 0.01$, and $^{***}p < 0.001$.

[16] European Foundation for the Improvement of Living and Working Conditions (2020). European Working Conditions Survey Integrated Data File, 1991–2015. [data collection]. 8th Edition. UK Data Service. SN: 7363, http://doi.org/10.5255/UKDA-SN-7363-8

[17] The European Social Survey (ESS) also contains a rotating question that asks respondents to assess the likelihood of losing their job in the near future. However, because only the 2004 and 2010 editions of the ESS ask this question, it is inferior to the EWCS as a data source for our purposes, and as such we only use the latter in our analysis.

[18] The population-weighted average change in insecurity over this period is 2pp.

on a combination of the unemployment rate, the demographic and job characteristic controls listed above, country fixed effects, and year dummies (as there are only three years of data, there is no point in including a time trend). Our results are in keeping with what we found in the United States, the United Kingdom, and Germany data: the unemployment rate is a statistically significant predictor of insecurity in Europe, even controlling for country fixed effects and year dummies that capture structural shifts in the likelihood of feeling insecure. The 2015 dummy is negative in all four regressions, indicating that by 2015 the level of insecurity had fallen back to 2005 levels. As in our results for the United States, the United Kingdom, and Germany, adding controls for the composition of the labor force hardly affects the results. Country fixed effects also have very little noticeable impact on the results (the 2015 dummy indicates a marginally smaller decline in insecurity when country FEs are included), though with country FEs included, the relationship between the unemployment rate and job insecurity appears to be more strongly positive. The general conclusion from this analysis of the EWCS is that workers in most European countries do not feel significantly more insecure in the post-GFC era than they did in the years leading up to it, after controlling for the level of unemployment, the composition of the labor force, and country-specific, time-invariant factors—and in fact, the inclusion of these other controls does not affect our conclusions.

VI. Other Dimensions of Job Security

This paper has focused on job security measured as risk of job loss, something that is undoubtedly important to many workers. However, Standing (2011) identifies eight dimensions of job security that define the "precariat," of which we have focused on only one. The OECD defines these other dimensions as earnings quality (including both level and volatility) and the quality of the working environment, which includes the "nature and content of the work performed," (lack of) risk to physical health, and workplace autonomy (OECD, 2014). It remains a possibility that the other constituent components of job quality and precarity are more important than subjective job insecurity and have deteriorated. Some scholars[19] theorize that the "post-" or "neo-Fordist" paradigm of production that has taken hold in advanced economies in the past few decades may have led to a fall in job quality along some dimensions like autonomy and challenge, despite rises in real wages. However, Green et al. (2013) find that perceptions of job quality have remained relatively stable in Europe, including the United Kingdom and Germany. Handel (2005) reports similar findings for the United States, and Bloom et al. (2017) find that earnings volatility in the United States has fallen by one-third since 1980, while Moffitt and Zhang (2020) conclude it has not changed for the past 30 years. There has also been little change in the generosity of social safety nets measured as replacement rates.[20]

One approach to address the argument that we are missing the most important parts of insecurity would be to try to estimate trends in other important dimensions of job security. Our ability to do this is limited by the fact that questions vary across surveys and there is a lack of the long runs of data needed to identify trends. In this section, we take a different approach and explore whether there are any trends in overall job satisfaction. Overall job satisfaction is likely to encompass dimensions of work other than security and is a useful summary measure of how workers feel about their jobs.

A. *Data and Methodology*

Each of our surveys (apart from the HRS) contains information on respondents' reported job satisfaction. The GSS asks its U.S. respondents to rate their work satisfaction on a scale of 1–4, with 1 indicating "very satisfied," 2 indicating "moderately satisfied," 3 indicating "a little dissatisfied," and 4 indicating "very dissatisfied."[21] The BHPS and UKHLS fortunately use the same question before and after the transition between surveys, asking respondents to rate their overall job satisfaction on a scale of 1 ("not satisfied at all") to 7 ("completely satisfied"), with 4 indicating neither satisfaction nor dissatisfaction. Finally, the German SOEP asks respondents to rate their satisfaction with their work on a scale of 0–10, with 0 being the lowest possible level of satisfaction and 10 being the highest.

Though job security might be considered a component of job satisfaction, our data suggests responses to the job satisfaction question provide new information not captured by the job security questions. In the BHPS sample, the correlation between job satisfaction and job security is .43, and in the UKHLS sample it is .19. In the GSS it is .16, and in the SOEP it is .17. These correlations suggest that there is a link between the two variables, but that job satisfaction also has other, non-security-related determinants.

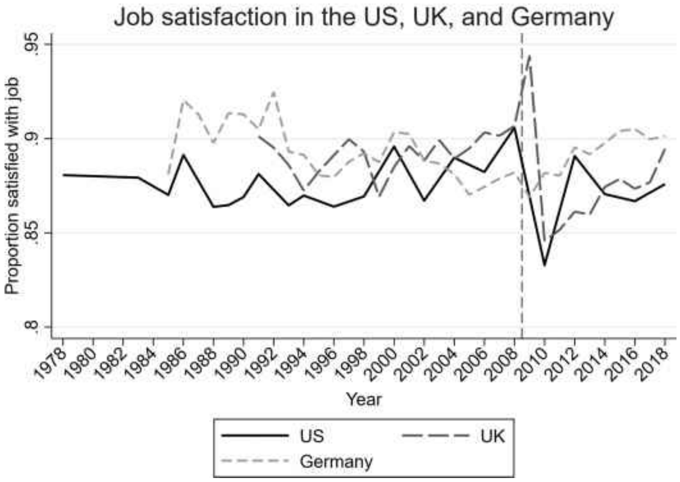
To investigate the trends in job satisfaction, we construct binary variables indicating satisfaction with one's job for each of the three data sets. For the GSS, the dummy takes a value of 1 if the respondent answers 3 or 4 to the job satisfaction question. BHPS/UKHLS respondents get a value of 1 if their response is greater than or equal to 4. SOEP respondents are deemed satisfied if they answer 5 or more to their question. The raw trends in the proportion of each country's labor force that reports dissatisfaction with their

[19]See Chapter 7 of Edgell et al. (2015) on job quality for an overview of the issues surrounding job quality, Chapter 8 of Gregg and Wadsworth (2011) for a focus on the United Kingdom, and Howell and Kalleberg (2019) for a recent survey of evidence from the United States.

[20]Net replacement rate in unemployment, OECD.stat (database). https://stats.oecd.org/Index.aspx?DataSetCode=NRR (accessed on 29 October 2021).

[21]We reverse the ordering of these categories to be consistent with the questions in the other surveys.

FIGURE 5.—JOB SATISFACTION IN THE UNITED STATES, THE UNITED KINGDOM, AND GERMANY



job over time are plotted in figure 5; there is no evidence of a trend fall in overall job satisfaction. Appendix H shows this conclusion is robust to controlling for characteristics. Visual inspection of the trends postrecession suggest a return to "normal" levels of job satisfaction in the decade since the crash.

VII. Conclusion

In this paper, we have shown that contrary to what Hollister (2011) calls the "New Employment Narrative," and despite the rise of nonstandard work arrangements, workers in the United States, the United Kingdom, and Germany feel as secure as they ever have in the past 30 years. This is partly because job insecurity is very cyclical, and (pre-COVID) unemployment rates have been very low, but there is also no marked underlying trend towards increased subjective measures of job insecurity. We have also demonstrated that there is an absence of a trend in perceived job security across workers with different types of working arrangements, different demographic characteristics, and different education levels—almost all subgroups along any dimension of interest feel approximately as secure in their jobs as they did in the early 2000s.

This conclusion should not be taken to mean that all is well in labor markets—only that there has not been a deterioration in the level of job insecurity. Wage inequality is higher in our three countries than 40 years ago (see, e.g., Acemoglu & Autor, 2011; Lindley & Machin, 2013; and Dustmann et al., 2009), though in the United States and the United Kingdom much of this change in the lower part of the income distribution occurred in the 1980s, and there has been stability since. Median real wages have been stagnant for a long period in the United States and more recently in the United Kingdom (OECD, 2019). Additionally, the labor share has fallen in the United States and Germany since the 1980s (Pak & Schwellnus, 2019; ILO & OECD, 2015). There are also dimensions of work other than insecurity and pay that may be deteriorating; though we find little trend in overall job satisfaction in our three countries, there is evidence from the United Kingdom that work is becoming more stressful, and that workers feel they have to work harder than ever, while also having less say in their working arrangements (Green et al., 2018; Gallie et al., 2018). To further our understanding of the dynamics of self-perceived job security and the impact of "atypical" work arrangements on these dynamics and on worker welfare more broadly, it would be useful to investigate whether job quality has adjusted along other margins, extending our preliminary analysis of overall job satisfaction in section VI. However, before COVID, workers in the United States, the United Kingdom, and Germany felt as secure in their jobs as they ever have in the past 30 years, and subjective job insecurity does not seem to be one of the biggest problems in labor markets.

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Online Appendix: "Subjective job insecurity and the rise of the Precariat: evidence from the United Kingdom, Germany, and the United States"

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February 25, 2022

Appendix A: Data Description

A.1 Crosswalk for the insecurity questions in the UK sample

For the UK, we have to take a different analytical approach to the US and German data because of the different questions asked in the BHPS and UKHLS. First, we have to have a crosswalk between the BHPS job insecurity question and the UKHLS one, using the two waves (6 and 7) which asked respondents both questions. As can be seen in Figure A.1 below, which plots the mean response to the UKHLS question by respondent's answer to the BHPS question for these two waves, the there is a clear relationship between answers to the two questions: respondents who say they are satisfied with their job security are less likely to say that they are likely to lose their job. The sample correlation between the responses to these two questions is .43.

In order to obtain the unconditional probabilities of a given response to the UKHLS job security question for the BHPS portion of the sample (which did not actually answer the question), we estimate a multinomial logit model with the UKHLS question's response as the dependent variable and the BHPS job security question and compositional controls as the independent variables:

$$Pr(UKHLS_i = j|BHPS_i = y) = \frac{e^{X_i'\beta_j+\theta_y}}{1+\sum_{k=2}^{4} e^{X_i'\beta_k}}$$

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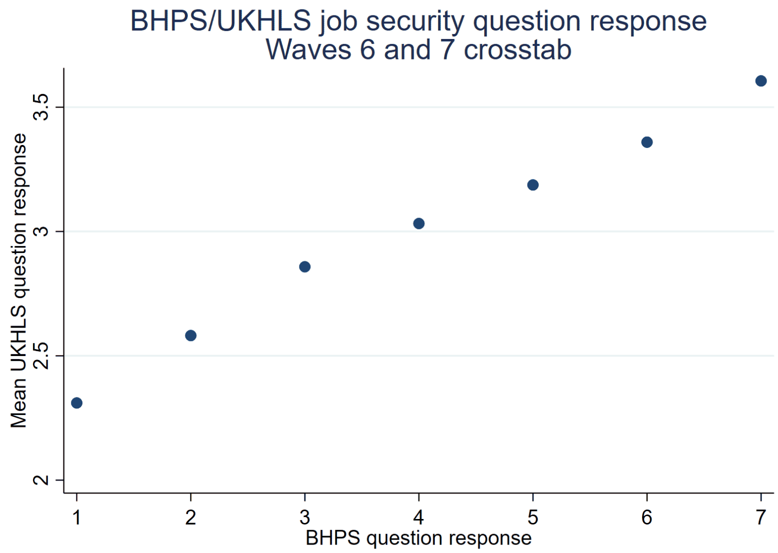

Figure A.1: Crosstab between UKHLS and BHPS job security questions, Waves 6 and 7

Where $UKHLS$ is the response to the UKHLS security question and $BHPS$ is the response to the BHPS security question, and the 3 β_k vectors in the denominator contain the θ vector of dummy variables indicating a given response to the UKHLS question, as well as the demographic controls used in the main analysis. The predicted probabilities $Pr(UKHLS_i \widehat{=} j|BHPS_i = y)$ from this model give the probability of responding a certain way to the UKHLS job security question, conditional on the respondent's characteristics and their answer to the job security question found in the BHPS. We then averaged these probabilities across the two-wave sample, with appropriate survey weights, in order to find the average probability of a UKHLS security response conditional on the BHPS security response.

The Law of Iterated Expectations allows us to find the unconditional probability of responding a certain way to the BHPS security question for those who did not actually answer the question, i.e. those in the UKHLS sample. This unconditional probability can be calculated as follows:

$$Pr(UKHLS_i = j) = \sum_{k=1}^{7} Pr(UKHLS_i = j|BHPS_i = k)Pr(BHPS_i = k)$$

$Pr(BHPS = k)$ can be calculated empirically from the BHPS sample space by dividing the number of respondents who answered a certain way to the question by the total number of respondents to the question. We already obtained the conditional probabilities of answering a certain way on the UKHLS security question from the multinomial logit model above; all that remains is to sum the product of these two probabilities over the seven possible responses to the BHPS security question to obtain the desired unconditional probabilities.

There is some question over whether we ought to include covariates in this imputation procedure. It is possible that by excluding covariates from the crosswalk, the imputed values from which we then regress on those same covariates in Sections 3 and 4, we introduce bias in the same manner as imputing missing wages without including union status in the imputation procedure, then using the imputed values to estimate union wage effects (see Hirsch and Schumacher (2004) for a discussion of this type of bias). We include the covariates in our crosswalk in order to avoid this potential source of bias, but in practice the inclusion or exclusion of the covariates makes little difference to our results. In Figure A.2, we plot the proportion of the UK labor force that feels insecure over time, constructing the series both with and without covariates in the crosswalk, with the x-axis restricted to the period of time for which we have to impute the insecure proportion. Clearly, the series are virtually identical.

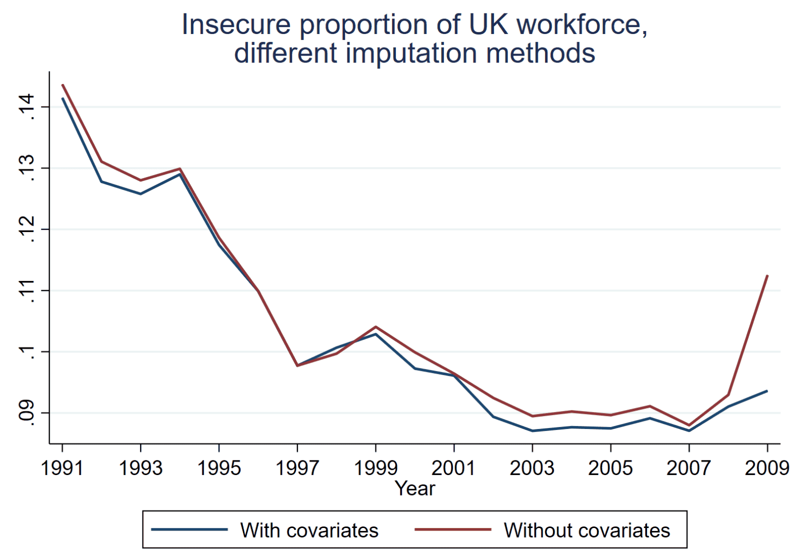

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Figure A.2: Insecure proportion of UK workforce, 1991-2009 (differing imputation methods)

A.2 Distribution of non-binned responses to job security question over time

In the main text, we analyze the GSS, BHPS/UKHLS, and SOEP job security data by binning the possible responses in such a way that allows us to construct a binary variable for whether or not the respondent feels insecure or not (we do not do this for the HRS, which asks a Manski-style question about the numerical probability of job loss in the next year). In this Appendix, we plot the non-binned, raw responses to the job security question in each of these three surveys over time. Though we do not conduct any direct statistical analysis here, visual inspection shows that binning the responses as we do in the main analysis does not obscure any underlying trends that might call into question our conclusions about the lack of a rise in job insecurity over time.

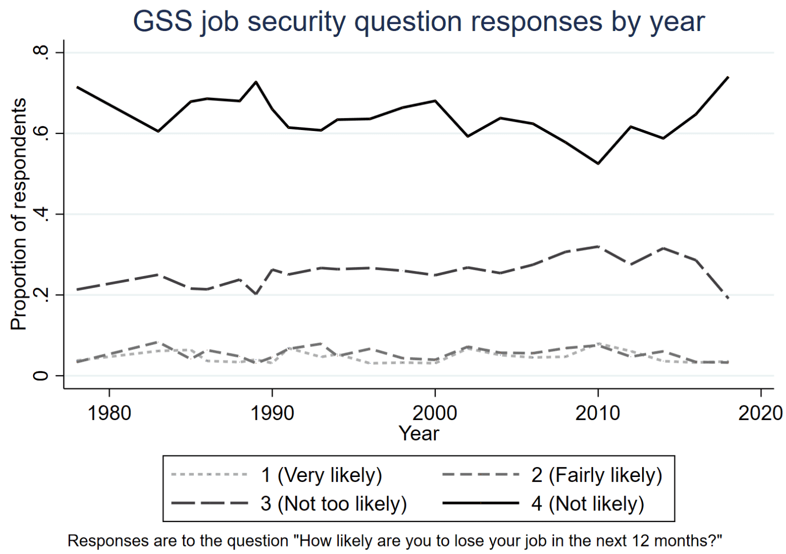

Figure A.3: Non-binned responses to job security question, GSS

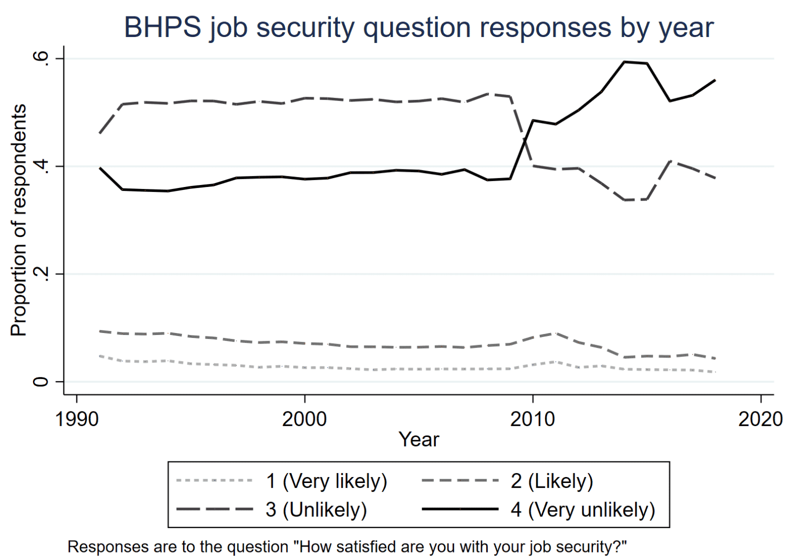

Figure A.4: Non-binned responses to job security question, BHPS/UKHLS

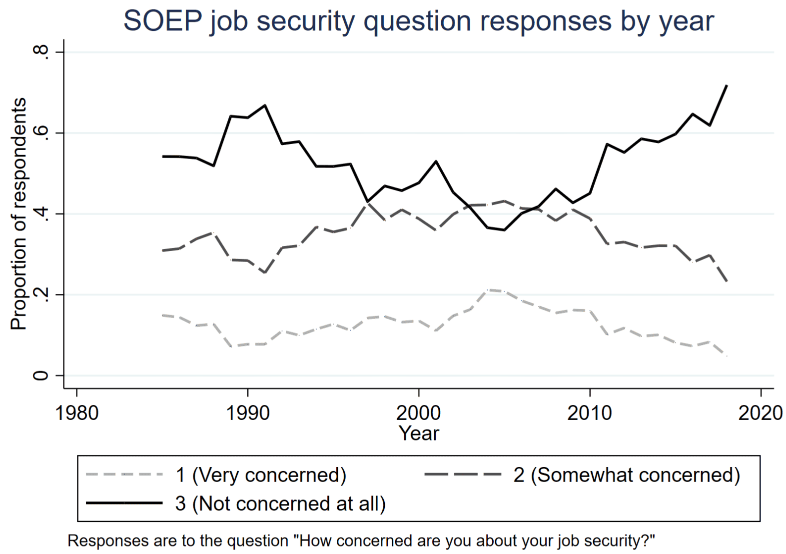

Figure A.5: Non-binned responses to job security question, SOEP

A.3 Separation rates

This sub-appendix describes the method we use to construct separation rates for each of the three countries, and presents evidence that there are not any trends in separations, voluntary or involuntary, that would call into question our conclusions about falling job insecurity. We define the total separation rate for time period t as $s_t = \frac{u^s_{t+1}+d_{t+1}+j_{t+1}}{e_t}$, where u^s_{t+1} is the number of unemployed persons at the beginning of $t+1$ who have been unemployed for one period or less (the short-term unemployed), d_{t+1} is the number of labor force non-participants at $t+1$ who were participants at t, j_{t+1} is the number of employed persons at $t+1$ who were in different jobs at t, and e_t is the number of employed persons at the beginning of t.

Because our separation rate series measure different concepts for each country (our US series is monthly, the UK series is quarterly, and the German series is yearly), they are not comparable across countries in terms of levels, but they are consistently calculated over time for a given country, and so they can provide useful information about how the likelihood of leaving one's job (in the aggregate and, for the US and the UK, just involuntarily) has changed over the period of our sample. We can also compare our own separation rate series with series from other sources, to make sure that they are consistent with the findings of others on this subject.

United States

For the US, we use the Current Population Survey (CPS), 1998-2018,[1] to construct monthly separation, layoff, labor force dropout, and job-to-job transition rates. The CPS asks respondents about their labor force status, and if they are employed, whether they are in the same job as they were during the previous month. If the respondent is unemployed, they are asked to give a reason. Because of the CPS' monthly panel structure (respondents spend 4 months in the sample, 8 months out, and then another 4 months in), we can match respondents across months and construct flow data on those who have become unemployed, dropped out of the labor force altogether, or transitioned from one job to another in the last month, as well as the overall stock of employment in a given month.

Because the "reason for unemployment" question does not have an explicit "involuntary job loss" response, we focus on layoffs (which is a possible response) as our measure for involuntary separations. We define u^s_{t+1} as the number of unemployed persons at the beginning of month $t+1$ of the CPS who were employed in month t, d_{t+1} as the number of labor force non-participants at $t+1$ who were employed at t, and j_{t+1} as the number of employed persons at $t+1$ who were in employed in a different job at t. In calculating these flows, we use the longitudinal weight variable (*compwt*) recommended by IPUMS for analyses meant to reproduce monthly BLS statistics. This weighting variable is available from 1998, so we start our series there. We also exclude respondents who are in the 1st or 5th month of their time in the CPS sample, because they cannot be linked to their previous-month labor force status. Using these monthly estimates, we can calculate monthly separation probabilities as defined above for both total separations (regardless of the reason for separation) and layoffs, as well as dropout rates ($\frac{d_{t+1}}{e_t}$) and job-to-job transition rates ($\frac{j_{t+1}}{e_t}$). We make one adjustment to the total separation rate series and the job-to-job transition rate series, substituting in the job-to-job transition rate series found in Fujita et al. (2020), also derived from the monthly CPS, which adjusts for a known downward bias in CPS-derived job transition rates (see the aforementioned paper for more detail on this bias and the adjustment procedure). In Figure 3, the total and layoff rates plotted with the unemployment rate and the overall insecure proportion of the workforce are yearly averages of these monthly rates.

In Figure A.6, we plot the components of our CPS-derived separation probabilities for the US (though the job-to-job transition rates are from Fujita et al. (2020)). Total separations have trended downward consistently from the beginning of our series, and the layoff rate has also hovered a bit below 1%, rising during the recession but falling back to pre-recession levels shortly thereafter. The separation-to-unemployment rate has also been on a downward trend since the recession, and is currently at its lowest level of the entire series.

[1] Sarah Flood, Miriam King, Renae Rodgers, Steven Ruggles and J. Robert Warren. Integrated Public Use Microdata Series, Current Population Survey: Version 8.0. Minneapolis, MN: IPUMS, 2020. https://doi.org/10.18128/D030.V8.0

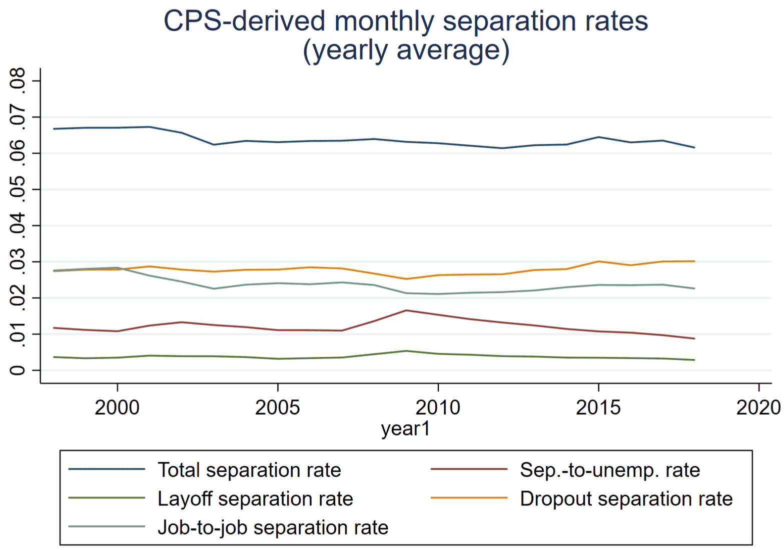

Figure A.6: CPS-derived separation rates for the United States

We also compare our CPS-derived series to separation probabilities from other sources. Figure A.7 plots the yearly average of our total and involuntary monthly separations series along with those calculated by Shimer (2012) and the total and involuntary non-farm separation rate (seasonally adjusted) from the Job Openings and Labor Turnover Survey (JOLTS) conducted by the US Department of Labor[2]. Shimer's series captures total separation probabilities and goes back to 1948, but ends in 2007. JOLTS provides both total and involuntary separations on a monthly basis, beginning in 2000. These three series provide a reference point in assessing the plausibility of our CPS-derived rates. There are obvious differences in levels for each of the three total separations series and the two involuntary separations series, but they do move together–the correlation between our CPS measure and Shimer's measure is .80, and the correlation of the two involuntary separations series is .71. The fall in involuntary separations after the recession found in the JOLTS series is consistent with the fall/leveling-out of our CPS-derived measure of the layoff rate.

[2] Job Openings and Labor Turnover Survey - Total and involuntary nonfarm separation rates, seasonally adjusted. Bureau of Labor Statistics (2021). https://www.bls.gov/jlt/data.htm (Accessed 10 May 2021).

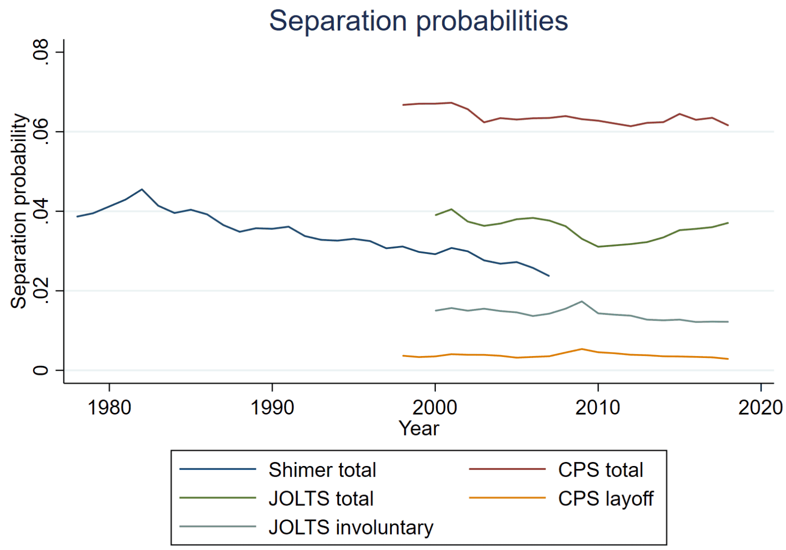

Figure A.7: Separation rates for the United States (CPS, Shimer, JOLTS)

United Kingdom

For the UK, we use the Quarterly Labour Force Survey (QLFS)[3], which asks similar questions to the CPS, to construct separation probabilities analogous to those for the US, but on a quarterly basis rather than a monthly one. Figure 4 plots a yearly average of these total and involuntary separation rates. Figure A.8 presents the yearly average of our QLFS-derived quarterly separation probabilities from 1993 to 2019, with each component of the total separation rate (separation-to-unemployment, separation-to-nonparticipation, and job-to-job flows) also plotted separately. Job-to-job separations are the largest component of the total separations series, and the involuntary separation rate has trended downward since a peak during the recession; it is now lower than it was in the years leading up to the crisis.

[3]Office for National Statistics, Social Survey Division, Northern Ireland Statistics and Research Agency, Central Survey Unit. (2021). Quarterly Labour Force Survey, 1992-2020. 22nd Edition. UK Data Service. SN: 6727, http://doi.org/10.5255/UKDA-SN-6727-23

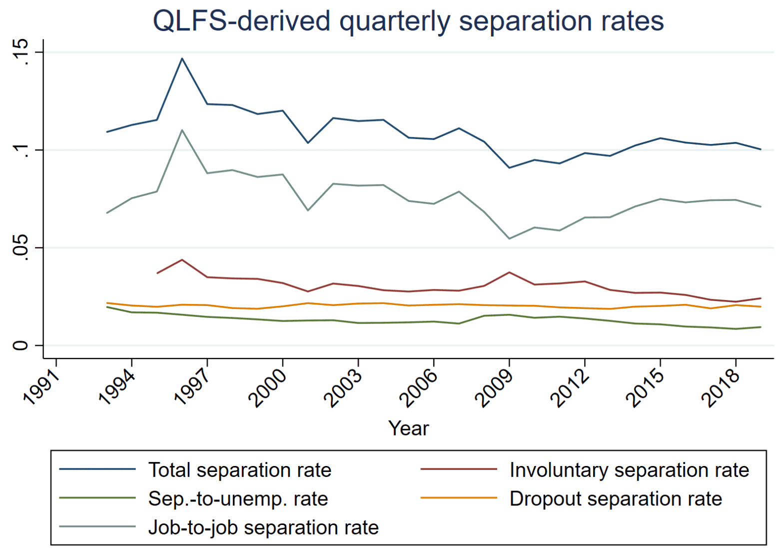

Figure A.8: QLFS-derived separation rates for the UK

Germany

For Germany, we use the panel structure of the SOEP to construct flow data on the number of separations and layoffs for each year of the sample, along with the stock of employment, to construct separation and involuntary separation probabilities. The survey asks respondents if they have left a job since the beginning of the previous year, and if so, to explain why they left the position by choosing from a list of possible reasons. We define involuntary separations here as those who put either "dismissed by employer", "place of work closed", or "temporary work contract ended" as their reason for leaving. We include the final reason listed, the end of a temporary contract, to capture the possibility that there has been a rise in labor market churn owing to the rise of temporary work and "mini-jobs" in Germany since the early 2000s, something that a simply layoff rate might not pick up. We construct yearly employment stocks using the provided cross-sectional weights (*phrf*), and adjust these weights by multiplying them by the inverse probability of remaining in the sample until the next wave (*pbleib*) in order to calculate separations for that year.

Our calculated series are displayed in Figure A.9 below. Both series rise in the early 1990s, with the reunification of Germany and the addition of residents of the former East Germany to the sample, before leveling out in the mid/late 1990s, and beginning a slight decline in the early 2000s. There is no indication from our constructed series that involuntary separations have risen in Germany over the past 25 years, nor do total separations appear to be on the rise.

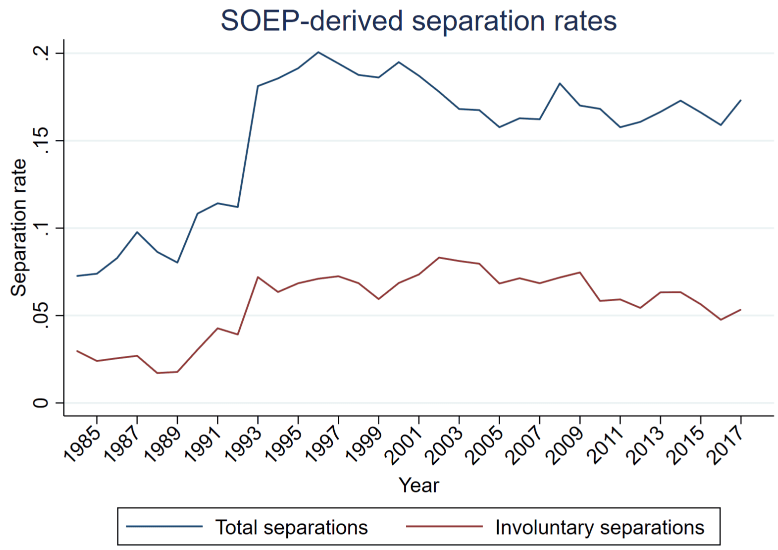

Figure A.9: Total separation rate for Germany

A.4 Other job security questions in SOEP

As mentioned in the main text, the German SOEP contains two additional questions on the respondent's subjective job security. The first asks respondents how likely it is that they will lose their job on a scale of 1 to 4 with 1 indicating "Definitely", 2 indicating "Probable", 3 indicating "Improbable", and 4 indicating "Definitely not", while the second question asks the respondent to give a numerical probability (rounded to the nearest multiple of 10) that they will lose their job. These questions are not asked consistently through the SOEP; the first question is asked in 1985, 1987, 1989, 1991-1994, 1996, and 1998, while the second question is asked in 1999, 2001, 2003, 2005, 2007, 2009, 2013, 2015, and 2018. Nevertheless, as a robustness check, and in keeping with Manski (2004)'s recommendation that expectations be elicited by surveys using numerical probabilities, we can perform some of the analyses we implemented in the main text using the responses to these questions, to see if our conclusions about the absence of a secular trend in job insecurity hold.

Figure A.10 below plots the average level of insecurity by year using each of our three measures from the SOEP. For comparability purposes, we construct a binary variable from the first alternative question, which takes a value of 1 if the response is 1 or 2 ("Definitely" or "Probable"), and 0 otherwise. The probability of job loss question is plotted by scaling the probabilities between 0 and 1. As is evident in the figure, the three series tend to move together, particularly the probability of job loss and our preferred insecurity measure (for which we have data covering the entire sample period). Though the spike in insecurity in 1992 (when residents of the former East Germany were added to the sample) is much larger using the second measure than it is using the first measure, this large discrepancy in levels is an outlier, and the two similarly-worded questions are otherwise highly correlated.

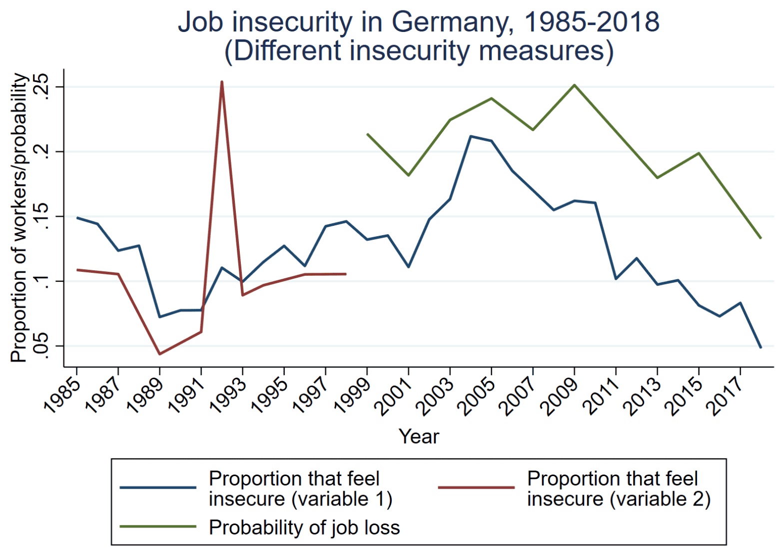

Figure A.10: Job insecurity in Germany with different measures

We also run the same regressions as found in Section 3 using the two alternative job security questions as the dependent variable; the only difference is that in the specification with the elicited probability of job loss on the left hand side, we run a linear regression rather than a logistic one (as in the HRS analysis, where we also have the respondent's elicited probability of job loss) We plot the marginal effects of the year dummies in these regressions below in Figure A.11, along with those from the baseline specification using our preferred measure of job insecurity. We use 1999 as the base year in the baseline specification and the specification with the probability of job loss as the dependent variable, and 1998 in the specification with the first alternative question as the dependent variable, to ensure levels are as comparable as possible (though it is the trend that we are more concerned with). As is evident from the figure, our conclusions about the lack of an upward trend in job insecurity in Germany over the last 30-40 years still hold even with the alternative dependent variables, both with and without controls.

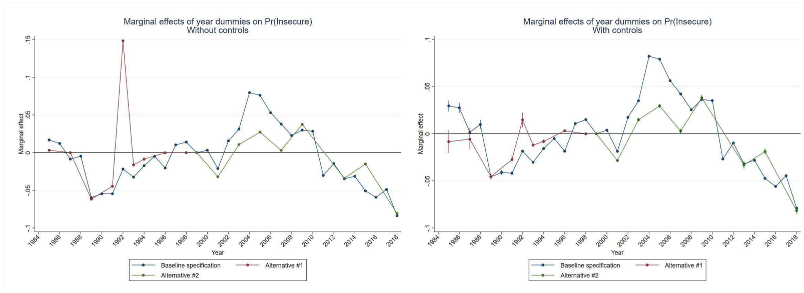

Figure A.11: SOEP year dummies with different measures

Appendix B: Predictive content of subjective job security

As discussed in the main text, our analysis is more meaningful if perceived job security is correlated with job loss in the near future. We can test the hypothesis that the two variables are unrelated by running logistic regressions of the following form using the HRS, BHPS, and SOEP panel datasets:

$$Pr(Y_{it} = 1) = \frac{e^{\gamma I_{it-1} + x'_{it-1}\beta}}{1 + e^{\gamma I_{it-1} + x'_{it-1}\beta}}$$

Where Y_{it} is a dummy variable that equals 1 if individual i is not the same job at time t as she was at time $t-1$, I_{it-1} is a fixed effect for individuals that feel insecure in $t-1$, and X_{it-1} is the matrix of covariates from our main specification (discussed in Section 3), chosen in order to adjust our estimates for demographics and job characteristics. If perceived insecurity is predictive of job loss, γ should be positive.

All three regressions suggest a highly significant relationship between Y_{it} and γ : in the HRS regression, $\gamma_{it-1} = 1.290$ ($S.E. = .079$), in the BHPS regression, $\gamma_{it-1} = .623$ ($S.E. = .0383$), and in the SOEP regression, $\gamma = .567$ ($S.E. = .0292$). Table B.1 below translates these coefficients into semi-elasticities of unemployment with respect to previous-period job insecurity: a 1pp increase in the likelihood of job loss gives a 1.15pp increase in the probability of next-period unemployment in the HRS sample, while in the binary-question datasets, feeling insecure increases the likelihood of next-period unemployment by 43.5% in the BHPS/UKHLS data, and by 52.7% in the SOEP sample. These results demonstrate that self-assessed job security does contain useful information about the likelihood of job loss in the near future.

Table B.1: Elasticity/semi-elasticity of unemployment

	HRS	BHPS/UKHLS	SOEP
Insecurity	1.153***	0.435***	0.527***
	(0.0702)	(0.0268)	(0.0270)
N	41414	35558	269091

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Appendix C: Main regression analysis details

In this Appendix, we discuss the regression models we use to analyze the trends in job insecurity in each of our four countries, explain how we use these models to adjust for the changing composition of the labor force over time, and present the full set of year dummy marginal effects from these regressions.

C.1 Logistic/LPM regressions using the GSS, HRS, and SOEP datasets

For the GSS and the SOEP, with the insecurity indicator as the dependent variable, we run logistic regressions of the following form using each dataset by itself:

$$Pr(insecure_i = 1) = \frac{e^{x_i'\beta}}{1 + e^{x_i'\beta}}$$

Where the dependent variable is the probability that worker i feels insecure. We have two main specifications for this model. The first is simply a regression using a vector of year dummies as the independent variables. The resulting marginal effects from this model, $\frac{\partial Pr(insecure_i=1)}{\partial x_{year}}$, simply capture the difference in the expected probability of feeling insecure in year x and the probability in the base year. These marginal effects capture the raw trend in insecurity over the course of the sample. If they are significantly different from zero, we can interpret this as a significant difference in the probability of job insecurity in that year, relative to the base year.

The second specification uses the year dummies as independent variables, but also controls for socioeconomic and job characteristics of the worker. We control for sex, age and age squared, race/ethnicity (missing for Germany), marital status, country of birth, education, self-employment status, part-time work status, union membership (missing for Germany), and employment industry (plus job tenure and temporary work status for the UK and Germany data). We also include industry fixed effects. For dummy covariates, we calculate average marginal effects by calculating the predicted probability of feeling insecure for each observation with the dummy switched on and off, and then averaging those individual marginal effects over the whole sample, giving an average marginal effect. For continuous covariates, we compute the predicted probabilities for each observation at their observed covariate values, then again after increasing the covariate of interest by one unit, and averaging those differences over the sample as in the dummy variable case. Net of these demographic and socioeconomic controls, the marginal effect of the year dummies should tell us whether or not there has been an underlying change in the baseline probability of feeling insecure within job types and demographic cells. If the marginal effects of these year dummies are similar across both specifications, we may conclude that changes in the composition of employment plays only a small role in in explaining changes over time.

Because the dependent variable in the HRS is a number between 1 and 100, we use the Linear Probability Model (LPM) to analyze the impact of year dummies and covariates on the probability of feeling insecure. The specification is as follows:

$$Y_{it} = \alpha + \theta_t + x_{it}^{'}\beta + \varepsilon_{it}$$

Where Y_{it} is the elicited probability of losing one's job in the next 12 months, θ_t is a year effect for year t, $x_{it}^{'}$ is a vector of demographic controls, and ε_{it} is an error term.

C.2 FE Poisson-logistic regression equivalence using the BHPS/UKHLS datasets

Because we had to impute the probabilities of answering a given way to the BHPS job security question for the UKHLS sample, we do not observe what their true response would have been had they been explicitly asked the question. As such, we cannot construct a binary variable indicating insecurity as we could for the GSS and SOEP. One way around this problem is to transform the data slightly in order to exploit the well-known equivalence between multinomial logit models and Poisson models with individual-year fixed effects (Palmgren (1981); Baker (1994); Lang (1996)).

The model we use to analyze the UK data is a typical Poisson regression, specified as follows:

$$E[Y_i|X_i] = e^{X_i'\beta + \epsilon_i}$$

Where X is a matrix of socioeconomic and demographic controls and an individual-year fixed effect, and Y is a binary variable taking value 1 if the observation (of which there are 4 for each individual-year), henceforth sub-observation, corresponds to the true realized response to the job security question (recall that there are 4 possible responses to the UKHLS job security question). For the UKHLS portion of the sample, Y is the imputed probability of giving that response. The controls matrix X contains the controls described in Section 3, interacted with a vector of 3 dummy variables, one for each possible response minus the base category, each of which takes a value of 1 if that sub-observation in question corresponds to that possible response. The $4 \times K$ matrix X is illustrated below for one individual-year, with the first column indicating the sub-observation's response, v_x indicating that the sub-observation corresponds to response x, with $x = 4$ as the base category, and with K control variables x_{ik}:

$$\begin{bmatrix} v_1 * x_{1i} & v_1 * x_{2i} & ... & v_1 * x_{Ki} \\ v_2 * x_{1i} & v_2 * x_{2i} & ... & v_2 * x_{Ki} \\ v_3 * x_{1i} & v_3 * x_{2i} & ... & v_3 * x_{Ki} \\ 0 & 0 & ... & 0 \end{bmatrix}$$

The coefficient vector β thus has $3K$ individual coefficients, since one response acts as the base category. This model may seem unnecessarily complex, but the advantage is that we are able to back out the average marginal effect of a given variable on the probability of responding a certain way to the question using the estimated coefficients by computing the expected probability of each response category for every respondent–with and without the covariate of interest switched on for dummy variables, and at the observed value as well as after changing the value by a small amount for continuous covariates–and then computing the difference between those probabilities and averaging those differences over the sample. We can therefore compare our UK results to the Germany and US results.

C.3 Summary linear regressions

In this appendix, we provide some further detail on the linear regression we use as summary measures for our analysis in Sections 3 and 4 of the main text. Figures C.1 through C.4 show the estimated average marginal effects and standard errors of the year dummies from the logistic/LPM and the FE-Poisson regressions that form the basis for the dependent variable in the regressions in Table 2. These AMEs are very similar with and without controls, implying (as much of our other analysis corroborates) that changing labor force composition has not substantially impacted subjective job security over the course of our sample.

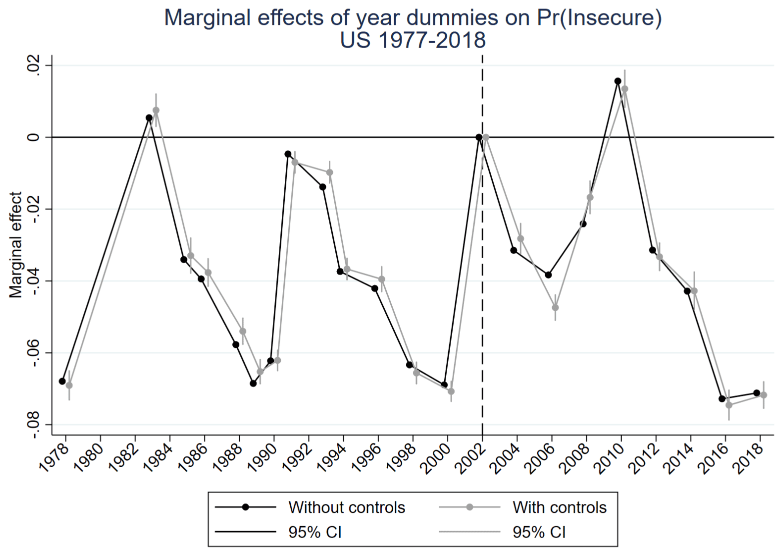

Figure C.1: Marginal effects of year dummies in US regressions (GSS)

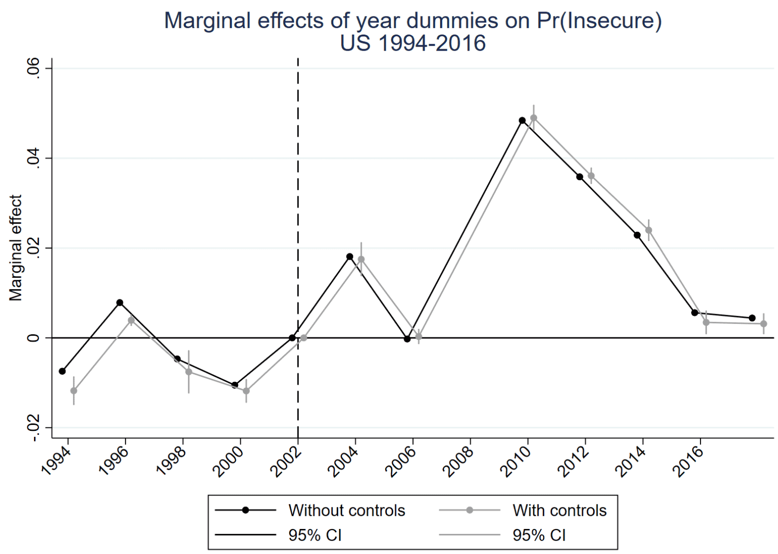

Figure C.2: Marginal effects of year dummies in US regressions (HRS)

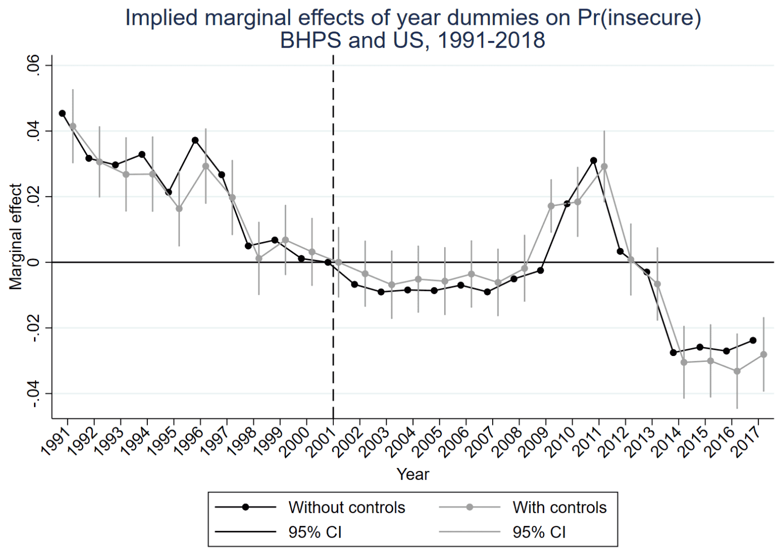

Figure C.3: Marginal effects of year dummies in UK regressions

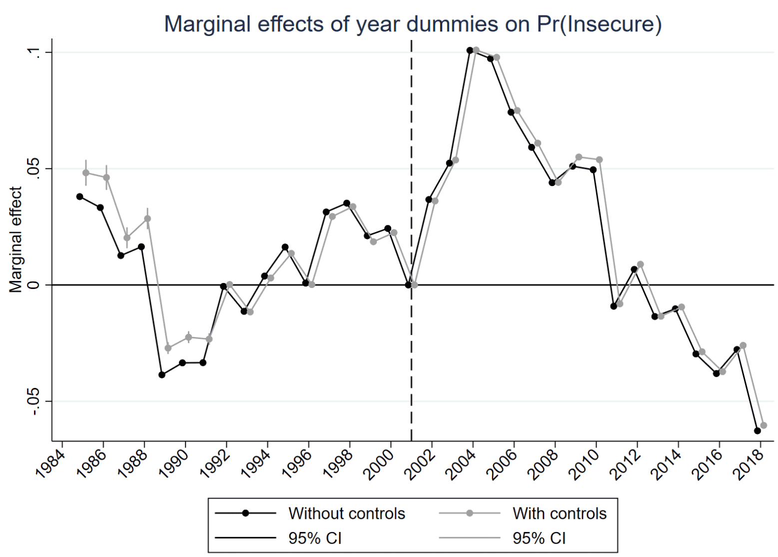

Figure C.4: Marginal effects of year dummies in Germany regressions

We also consider the possibility that the underlying time trend in job insecurity is nonlinear. We therefore test the plausibility of these alternative functional forms by running the linear regressions found in Sections 3 and 4, regressing the average marginal effect of the year dummies from the nonlinear regressions with controls on the unemployment rate and linear and quadratic time trends, along with a linear spline function with a knot in 2008, to capture a potential shift in insecurity at the time of the financial crisis. For Germany, we also test a linear spline with a knot in 2005, when the last of the Hartz reforms took effect, along with a function with a trend break in 2005. Results from these regressions are displayed in Tables C.1-C.4 below, and show that our results are not sensitive to the functional form chosen for time. For the quadratic function, we re-parameterize the time variable to be equal to $year - 2016$, so that the coefficient on the linear term can be interpreted as the marginal effect of time in 2016, a measure of current trends. In the UK results, the marginal effect of all functions of time that we test is negative over nearly all plausible values of t, and is strongly negative in the most recent years of the sample. The linear spline gives two negative coefficients, the latter of which is statistically significant. In the US (GSS) data, the quadratic trend has a significant negative coefficient that makes the marginal effect negative from approximately 2000, while the pre/post 2008 linear spline shows that the trend is effectively 0 post-2008, and very small pre-2008. The US (HRS) results show very similar magnitudes across functional forms–the linear trend is marginally positive but very small, and almost exactly equal in 2016 to the linear time trend estimated in column (1). The linear spline yields a small but positive trend pre-2008, and a trend that is effectively 0 (.07pp over 10 years) post-2008. Finally, in our Germany results, the linear trend is positive but small, while the quadratic function implies a positive marginal effect of time since approximately 1996, and a slightly larger one in 2016. For Germany we also report some other specifications to allow for a possible impact of the Hartz labor market reforms in the early 2000s. We report the result of two splines, one with a knot at 2005 (column 3) and the other with a knot at 2008 (column 4); both give insignificant, small trends in both the pre- and post- periods, with magnitudes similar to the simple linear time trend. Finally, in column (5) we include an interaction term between the time trend and a dummy variable for a level change in 2005, along with the dummy itself; this, like the trend coefficients, is insignificantly different from zero.

Because on the whole our results are qualitatively similar in magnitude and direction across most possible functional forms of time, we choose to use a linear time trend in the main text to summarize our findings.In no case do the trends differ greatly from each other, and nearly all functional forms imply a negative trend in the later years of the sample.

Table C.1: UK: Time trend and unemployment rate regression results

	(1) AME	(2) AME	(3) AME
Unemployment rate*100	.628***	.817***	.613***
	(.138)	(.16)	(.145)
t	-1.18**	-3.41**	
	(.348)	(.97)	
t^2		-1.02*	
		(.478)	
Linear spline (pre-2008)			-.473
			(.381)
Linear spline (post-2008)			-.113**
			(.0365)
N	27	27	27
R^2	0.777	0.834	0.781

This table displays the results of regressions of the average marginal effects of the year dummies (with controls) on the unemployment rate and various functional forms of time using the BHPS/UKHLS data. In the quadratic functional form, t is re-parameterized to be equal to *year* - 2016. Standard errors in parentheses. *$p < 0.05$, **$p < 0.01$ ***$p < 0.001$

Table C.2: US (GSS): Time trend and unemployment rate regression results

	(1) AME	(2) AME	(3) AME
Unemployment rate*100	1.4***	1.52***	1.36***
	(.177)	(.204)	(.173)
t	.313	-2.63**	
	(.357)	(.844)	
t^2		-.874**	
		(.224)	
Linear spline (pre-2008)			1***
			(.213)
Linear spline (post-2008)			.00862
			(.0341)
N	22	22	22
R^2	0.602	0.755	0.648

This table displays the results of regressions of the average marginal effects of the year dummies (with controls) on the unemployment rate and various functional forms of time using the GSS data. In the quadratic functional form, t is re-parameterized to be equal to *year* - 2016. Standard errors in parentheses. *$p < 0.05$, **$p < 0.01$ ***$p < 0.001$

Table C.3: US (HRS): Time trend and unemployment rate regression results

	(1) AME	(2) AME	(3) AME
Unemployment rate*100	.898***	.837***	.943***
	(.0539)	(.0705)	(.041)
t	.964*	.992**	
	(.316)	(.273)	
t^2		-.647	
		(.47)	
Linear spline (pre-2008)			1.35***
			(.232)
Linear spline (post-2008)			.0655*
			(.0203)
N	12	12	12
R^2	0.883	0.911	0.940

This table displays the results of regressions of the average marginal effects of the year dummies (with controls) on the unemployment rate and various functional forms of time using the HRS data. In the quadratic functional form, t is re-parameterized to be equal to *year* - 2016. Standard errors in parentheses. *$p < 0.05$, **$p < 0.01$ ***$p < 0.001$

Table C.4: Germany: Time trend and unemployment rate regression results

	(1) AME	(2) AME	(3) AME	(4) AME	(5) AME
Unemployment rate*100	1.87***	2.45***	2.15***	1.62***	1.98***
	(.161)	(.239)	(.401)	(.309)	(.387)
t	1.08*	5.94**			
	(.413)	(1.77)			
t^2		1.5**			
		(.525)			
Linear spline (pre-2008)			.5		
			(1.04)		
Linear spline (post-2008)			2.88		
			(2.29)		
Linear spline/trend break (pre-2005)				1.49	-.59
				(.778)	(.946)
Linear spline/trend break (post-2005)				-1.27	.145
				(2.36)	(2.63)
Post 2005					3.72
					(5.87)
N	34	34	34	34	34
R^2	0.733	0.781	0.737	0.739	0.806

This table displays the results of regressions of the average marginal effects of the year dummies (with controls) on the unemployment rate and various functional forms of time using the SOEP data. In the quadratic functional form, t is re-parameterized to be equal to *year* - 2016. Column (4) uses a linear spline, while Column (5) uses a trend break. Standard errors in parentheses. *p $<$ 0.05, **p $<$ 0.01 ***p $<$ 0.001

C.4 Marginal effects of year dummies in regression with individual fixed effects

Figures C.5 through C.7 plot the results from our main specifications for the US (using the HRS), UK, and Germany with individual fixed effects included as covariates; time-invariant demographic controls like sex and race are excluded. Visual inspection of these plots compared to those in the main text demonstrates that the estimated trends in job security are not affected by the inclusion of individual fixed effects. The HRS regression without controls shows a higher level of insecurity relative to the base year, but with controls our point estimates indicate insecurity no higher than the base year (though standard errors are large). There is a large difference in levels depending on the inclusion of individual FEs in the SOEP regressions. In particular, the magnitude of the deviations from the base year are larger in the FE models, with maxima and minima on the order of 20 to 35 percentage points. However, our conclusions about the overall trend in subjective job security over the past four decades are unaltered by these level differences. There is essentially no difference in levels in the UK regressions, and the trend is also the same.

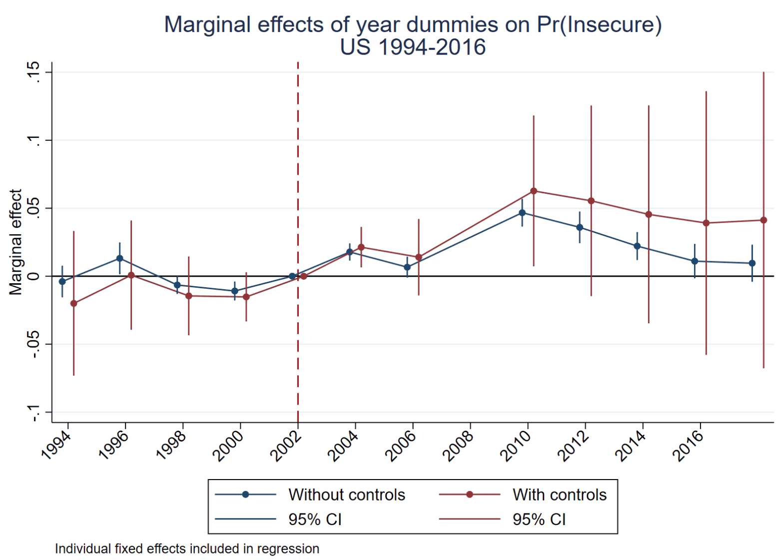

Figure C.5: Marginal effects of year dummies in US (HRS) regressions, with individual FEs

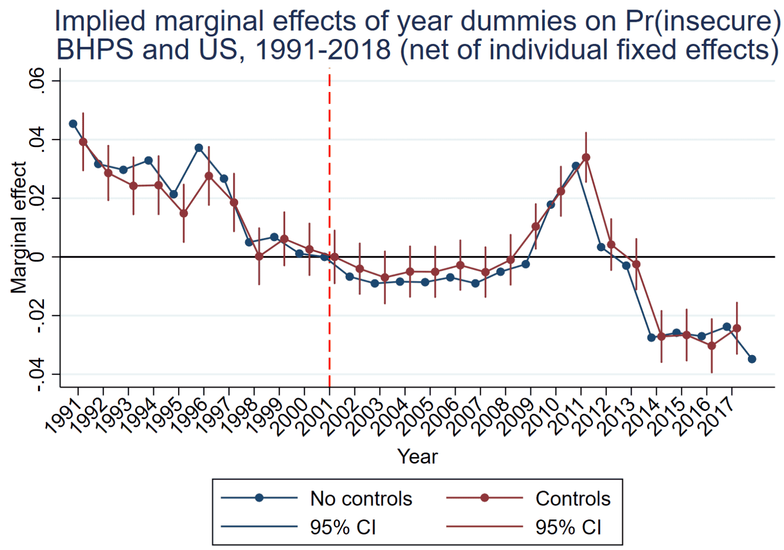

Figure C.6: Marginal effects of year dummies in UK regressions, with individual FEs

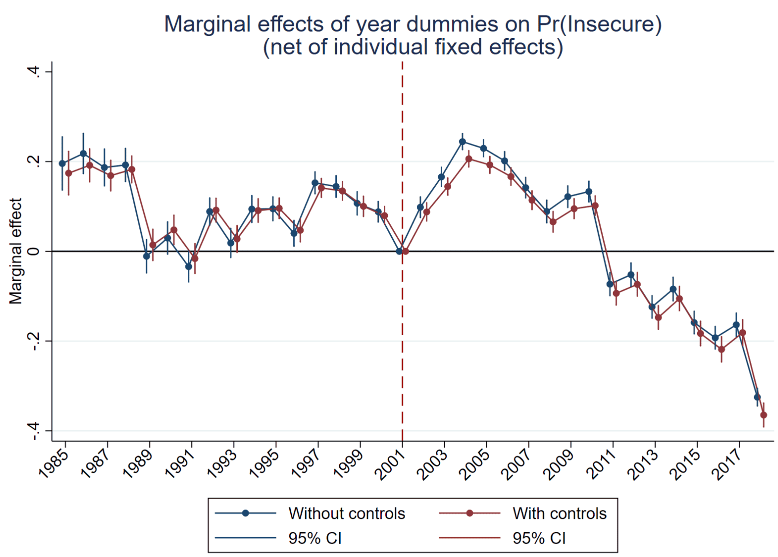

Figure C.7: Marginal effects of year dummies in Germany regressions, with individual FEs

Appendix D: A Modified Mortensen-Pissarides Model of Job Destruction

In the popular model of endogenous job destruction of Mortensen and Pissarides (1994), an increase in worker bargaining power leads to a fall in the labor share and a fall in job destruction so that that job insecurity (the likelihood of losing one's job) falls. One might then conclude that our finding of no clear trend in job insecurity is inconsistent with the fall in the labor share observed in the US and, to a lesser extent, Germany (though the labor share in the UK appears to have risen). In this Appendix, we show it is possible to break the tight link between job destruction and worker bargaining power if some assumptions in Mortensen and Pissarides (1994) are tweaked.

In Mortensen and Pissarides (1994), all new jobs are created at the highest possible level of productivity, and productivity shocks can only reduce productivity with jobs being destroyed when productivity falls below the job destruction threshold. In this Appendix, we modify the model to assume that the productivity distribution in new jobs is drawn from the same distribution as productivity after a productivity shock. This is not an unreasonable change: the assumption that all new jobs are created at the highest possible level of productivity has the counter-factual prediction that hires from unemployment should have the highest wage.

In Mortensen and Pissarides (1994), productivity in a job is written as $p + \sigma\varepsilon$, where ε is a standardized idiosyncratic component and σ the standard deviation of productivity shocks. There is a constant arrival rate of productivity shocks, λ, and the new level of productivity is drawn from a distribution $F(x)$ with upper bound ε_u. All new jobs are assumed to be created at the upper bound, but we will modify this by assuming that the productivity in each new match is drawn from $F(x)$; this implies that matches with very low realized productivity will never be consummated. Define the lowest productivity at which job creation occurs as ε_d; this will also be the threshold at which job destruction will occur and is where the surplus from a job is zero. We will assume that a lower value of ε_d, which is a lower job destruction rate, is synonymous with increased job security. With our assumptions the value of a vacancy can be written as:

$$rV = -c + q\left(\frac{v}{u}\right)\int^{\varepsilon_u} max\left[J(x) - V, 0\right] dF(x) \tag{1}$$

where r is the interest rate, c is the cost of posting a vacancy, $J(x)$ is the value of a filled job that has idiosyncratic component x, and q is the matching rate for vacancies that depends on labor market tightness

$\frac{v}{u}$. Free entry of vacancies will ensure that $V = 0$ in equilibrium, in which case (1) can be written as:

$$\int_{\varepsilon_d}^{\varepsilon_u} J(x)\,dF(x) = \frac{c}{q} \tag{2}$$

where we have also used the fact that ε_d is the lowest level at which jobs will be created. The value of a filled job can be written as:

$$rJ(\varepsilon) = p + \sigma\varepsilon - w(\varepsilon) + \lambda\int_{\varepsilon_d}^{\varepsilon_u} J(x)\,dF(x) - \lambda J(\varepsilon) \tag{3}$$

The value of a job to a worker, $W(\varepsilon)$, can be written as:

$$rW(\varepsilon) = w(\varepsilon) + \lambda\int_{\varepsilon_d}^{\varepsilon_u} W(x)\,dF(x) - \lambda W(\varepsilon) + \lambda F(\varepsilon_d)\,U \tag{4}$$

where U is the value of unemployment. The value of unemployment can be written as:

$$rU = b + \frac{vq}{u}\int_{\varepsilon_d}^{\varepsilon_u} [W(x) - U]\,dF(x) \tag{5}$$

where b is the value of leisure. Following Mortensen and Pissarides (1994), we define total match surplus as:

$$S(\varepsilon) = J(\varepsilon) + W(\varepsilon) - U \tag{6}$$

Combining (3), (4), (5), and (6) we have that:

$$(r+\lambda)\,S(\varepsilon) = p + \sigma\varepsilon - b + \lambda\int_{\varepsilon_d}^{\varepsilon_u} S(x)\,dF(x) - \frac{vq}{u}\int_{\varepsilon_d}^{\varepsilon_u} [W(x) - U]\,dF(x) \tag{7}$$

Differentiating leads to:

$$(r+\lambda)\,S'(\varepsilon) = \sigma \tag{8}$$

We also follow Mortensen and Pissarides (1994) in assuming a sharing rule so that:

$$W(\varepsilon) - U = \beta S(\varepsilon)\,,\, J(\varepsilon) = (1-\beta)\,S(\varepsilon) \tag{9}$$

Using this, (2) can be written as:

$$\int_{\varepsilon_d}^{\varepsilon_u} S(x)\,dF(x) = \frac{c}{q(1-\beta)} \tag{10}$$

Using (8), integrating the left-hand side by parts, and using the fact that surplus is zero at the job destruction point, we have that:

$$\int_{\varepsilon_d}^{\varepsilon_u} [1 - F(x)]\, dx = \frac{c(r+\lambda)}{q(1-\beta)\sigma} \tag{11}$$

This is the job creation equation; it implies a negative relationship between ε_d and labor market tightness. A fall in worker bargaining power makes the job creation curve shift up. Using (8) and (9) in (7), we have that:

$$(r+\lambda) S(\varepsilon) = p + \sigma\varepsilon - b + \frac{\sigma\left[\lambda - \beta \frac{vq}{u}\right]}{(r+\lambda)} \int_{\varepsilon_d}^{\varepsilon_u} [1 - F(x)]\, dx \tag{12}$$

The job destruction threshold is where $S(\varepsilon_d) = 0$. Using this in (12) and using (11) leads to the job destruction equation:

$$p + \sigma\varepsilon_d = b - \frac{\left[\lambda - \beta \frac{vq}{u}\right]}{(1-\beta)} \frac{c}{q} \tag{13}$$

This is an upward-sloping relationship between ε_d and labor market tightness. How this curve is affected by a fall in worker bargaining power is now ambiguous. For a given level of labor market tightness, a fall in β leads to a rise (resp. fall) in job destruction depending if the arrival rate of productivity shocks is greater (resp. smaller) than the arrival rate of job offers for the unemployed. The ambiguous nature of the shift is not in the original Mortensen and Pissarides (1994) specification, where all new jobs are at the highest level of productivity. This ambiguity in how the job destruction curve shifts means that a fall in bargaining power no longer unambiguously reduces the job destruction threshold so does not necessarily increase job security of workers. The realized shift depends on the parameters of the model.

There are other possible models where falls in worker power do not necessarily lead to a rise in job security. In the simpler model of Pissarides (1990) the risk of job loss is an exogenous parameter uninfluenced by any other aspects of the model. There, a reduction in worker bargaining power leads to a fall in the labor share, but no change in job destruction. However, this might be thought to be an uninteresting result because job destruction is exogenous in this model. There are still other possible mechanisms: if workers are risk averse and so dislike job loss, a fall in bargaining power would likely lead to lower wages and more job destruction, potentially off-setting the effect at work in the original Mortensen and Pissarides (1994) model. The general conclusion is that it is not fundamentally inconsistent with theory if we observe changes in worker power together with little or no change in job security.

Appendix E: Compositional changes and subjective job security

In this Appendix, we extend our analysis of the impact of job characteristics and demographics on subjective insecurity by using a series of decomposition and reweighting exercises to show that the realized compositional changes in the labor force over the course of our sample do not explain much of the underlying trend in subjective job security. We utilize two methodologies common in the literature: the re-weighting approach of DiNardo et al. (1996) and a modified Oaxaca-Blinder decomposition.

E.1 Reweighting for covariate balance

We implement the reweighting-based analysis originally detailed in DiNardo et al. (1996), which allows us to estimate what the trend in job insecurity would have been if the balance of individual characteristics were kept the same as in some base year $t = t_{base}$ for the entire time series. The advantage of this approach is that it allows for a very flexible relationship between characteristics and job insecurity in each year, not restricting it to the linear functions we have used in the reported estimates. The first step of this analysis is to calculate a propensity score for each observation, estimating $p = Pr\left(t = t_{base}|X\right)$ using a logistic regression with the same worker characteristics X as in the job security regressions. We then multiply the original cross-sectional weights for each observation by $\frac{p}{1-p} * \frac{1-Pr(t=t_{base})}{Pr(t=t_{base})}$, upweighting those observations that have covariate values similar to those found in the base year, and downweighting those that do not. The final step is to regress our insecurity measure on the set of year dummies (with the same link function as in the main job security regressions for each dataset), weighting each observation by these new weights. The marginal effects of these dummies give the yearly mean of the insecurity measure if the balance of covariates in that year were the same as in the base year.

In the figures below, we plot the marginal effects of these year dummies for each of our four datasets, with a selection of different base years for the covariate balance, along with the main job security series from the regressions in Section 3, to investigate how much of an impact changing labor force composition could feasibly have on our results. As is clear from the plots, the choice of base year makes very little difference to the trend, and in all cases, the adjusted trend is almost identical to the unadjusted trend. This reinforces our conclusion that even accounting for changing labor force composition, workers do not feel more insecure today than they did in the past.

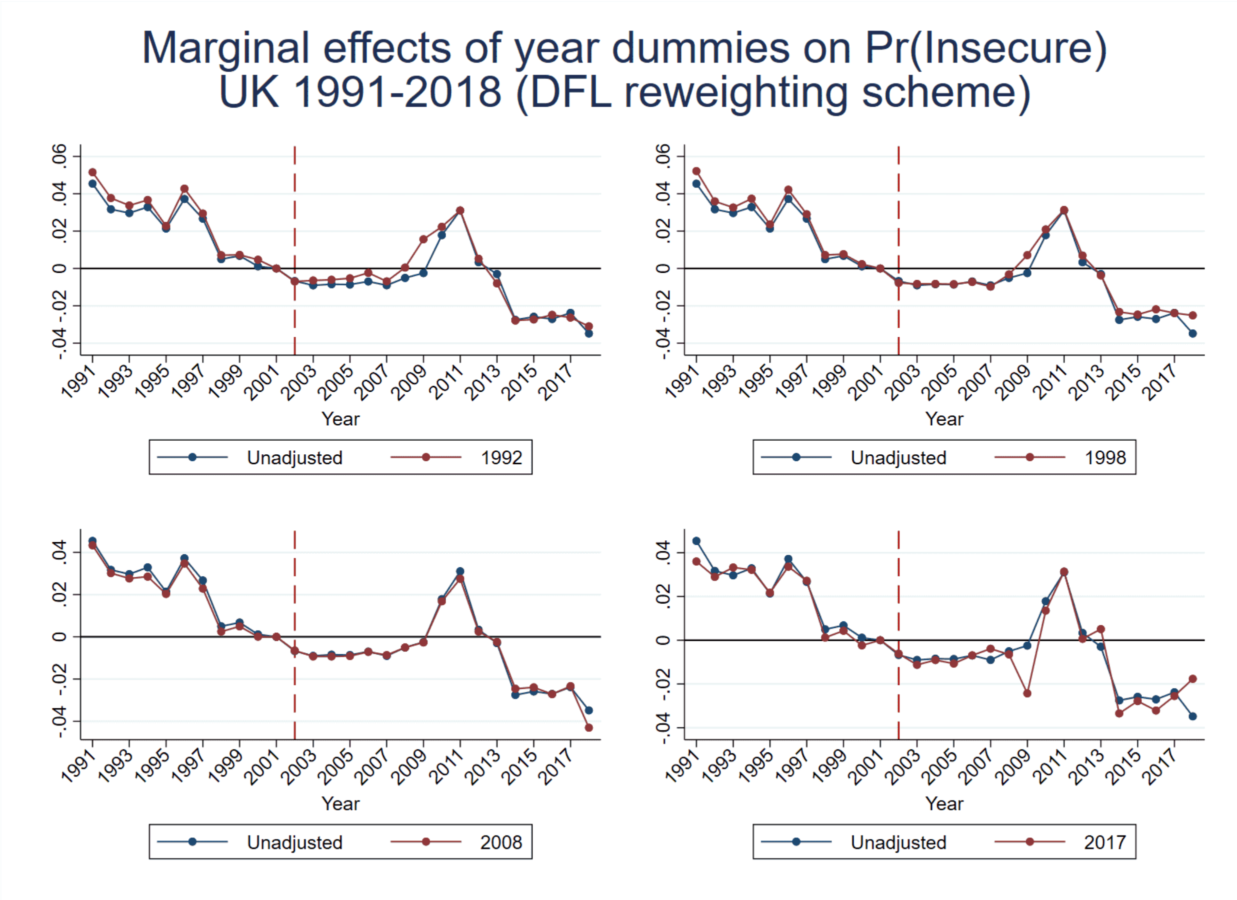

Figure E.1: DFL-weighted marginal effects of year dummies, UK

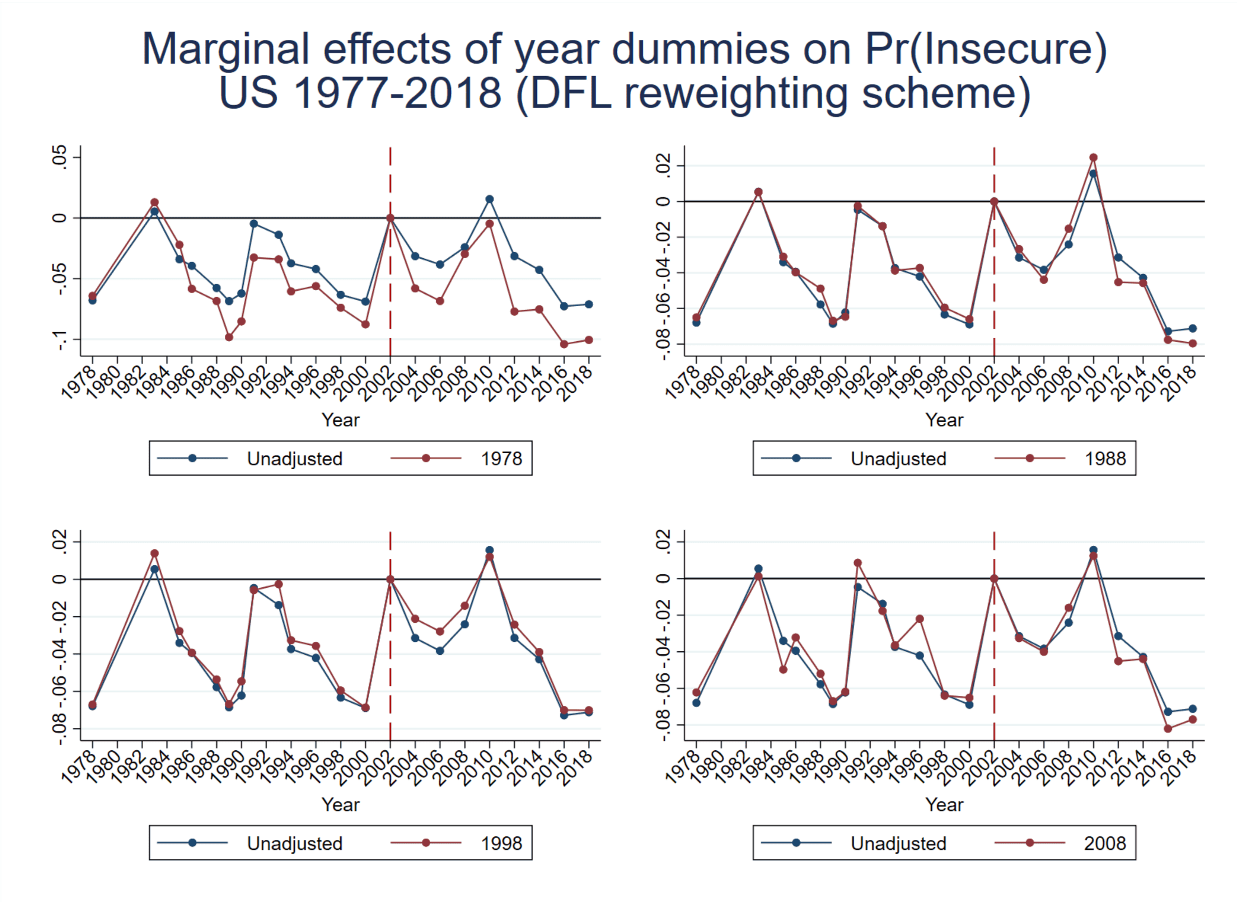

Figure E.2: DFL-weighted marginal effects of year dummies, US (GSS)

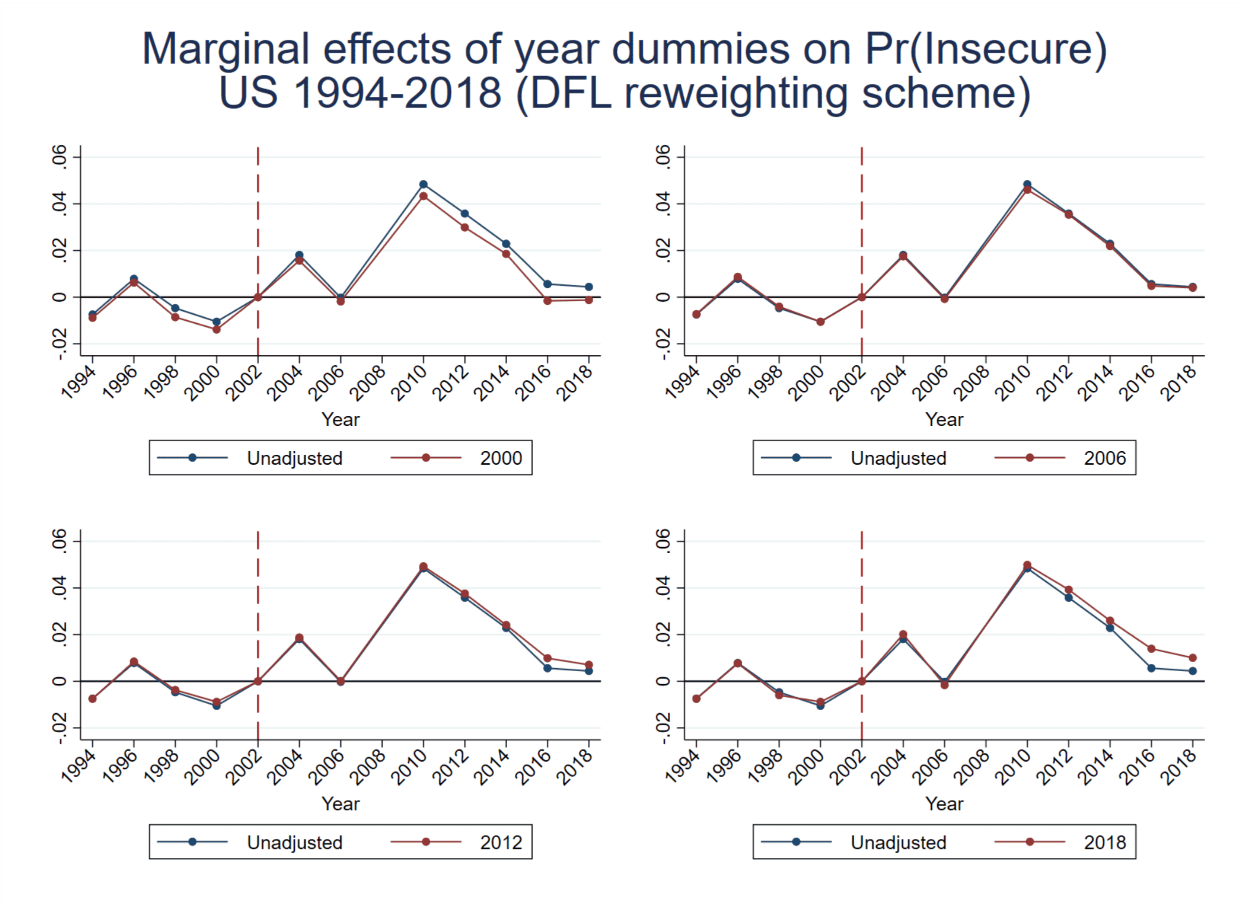

Figure E.3: DFL-weighted marginal effects of year dummies, US (HRS)

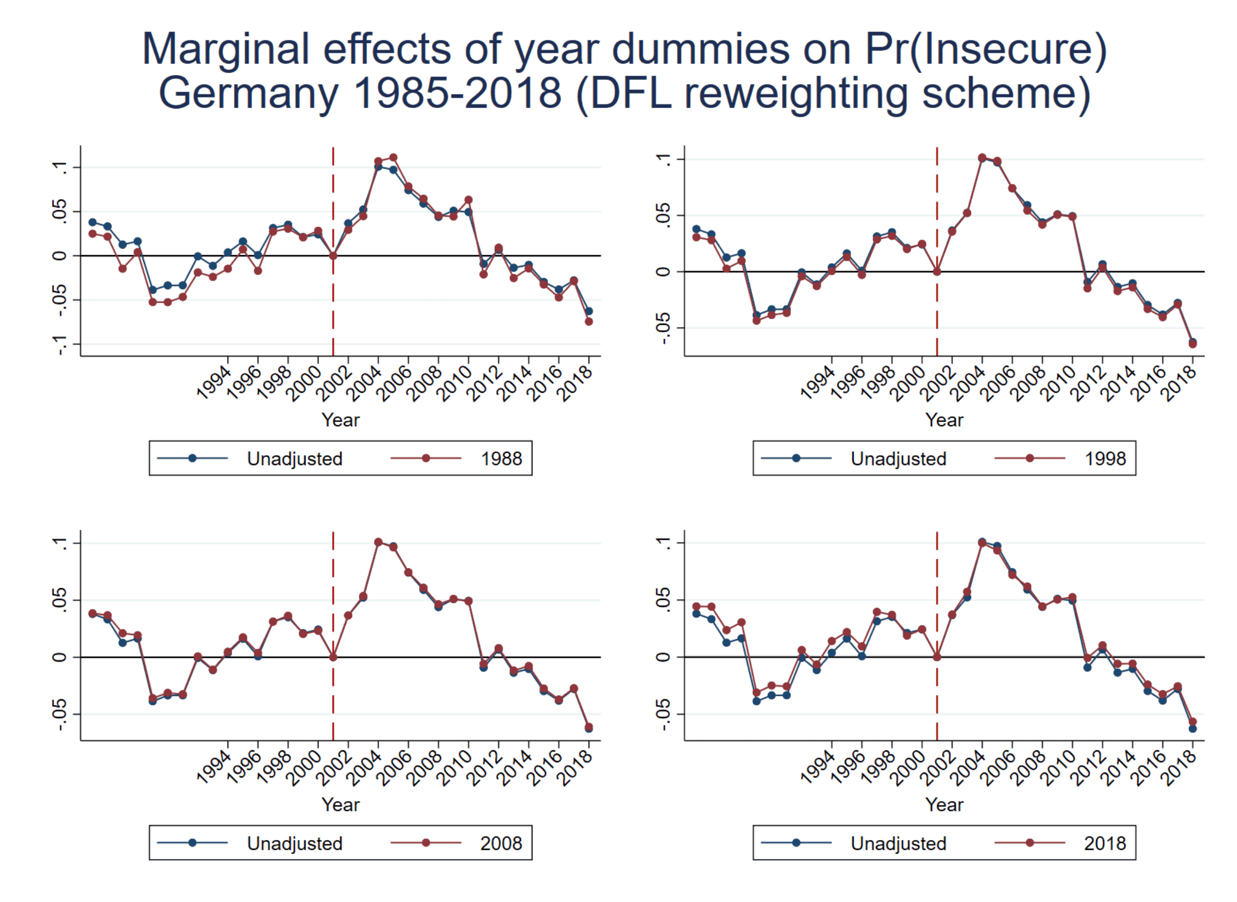

Figure E.4: DFL-weighted marginal effects of year dummies, Germany

E.2 Oaxaca-Blinder decomposition

Next, we implement a decomposition in the style of Powers et al. (2011), which is an extension of the gender wage gap regressions of Oaxaca and Ransom (1994)[4] to nonlinear models, allowing us to attribute differences in aggregate job insecurity across different years to changing labor force composition (endowments) and changing levels of insecurity within a characteristic cell (coefficients). Specifically, in analyzing the difference in insecurity between some base year s and some year in the future t, Powers et al. (2011) decomposes the difference in aggregate insecurity between the two years as follows:

$$\overline{Y_t} - \overline{Y_s} = \left\{\overline{F\left(X_t\beta_t\right)} - \overline{F\left(X_s\beta_t\right)}\right\} + \left\{\overline{F\left(X_s\beta_t\right)} - \overline{F\left(X_s\beta_s\right)}\right\}$$

[4]Which in turn is a generalization of the decompositions found in the seminal Oaxaca (1973) and Blinder (1973).

Where $\overline{Z_y}$ is the mean value of Z in year y, $F(\cdot)$ is a once-differentiable function mapping a linear combination of X to Y, and β_y is a vector of "returns", in terms of insecurity, to the covariate matrix X_y in year y. The first term on the right hand side represents the contribution of the difference in endowments across the two years, using the coefficient vector in year t as the baseline "returns" vector, and the second term represents the contribution of the difference in coefficients between year t and year s, as if the distribution of endowments were held fixed at their year s level. We model $F(X_t\beta_t)$ through the link functions we use in the regressions found in Section 3 of the main text (logistic for the GSS and the SOEP, and linear regression for the HRS), running the regressions on samples restricted to the year in question through Powers et al. (2011)'s *mvdcmp* Stata package. As Oaxaca and Ransom (1999) points out, the covariate-by-covariate breakdown of the decomposition depends on the reference category when sets of dummy variables are included in the covariates. We therefore apply the deviation contrast normalization set out in Yun (2005), which applies an ANOVA-type centered-effects restriction to the exhaustive sets of dummy variables on the right hand side of the regression and is built into the Stata package we use. Unfortunately, because there is no easily-implemented extension of the Oaxaca-Blinder method to models with high dimensional fixed effects, we cannot perform this analysis on the UK data–though the results from the DiNardo-Fortin-Lemieux reweighting above suggest that changing endowments play little to no role in explaining UK job security trends.

The tables below display the raw difference in aggregate security, the portion of that difference that can be attributed to changing characteristics (endowments), the portion that can be attributed to changing coefficients, and the covariate-by-covariate breakdown of the impact of changing endowments. One first thing to note when reading these tables is that in some pairs of years, the raw difference in insecurity is quite small. The absence of large variation in insecurity from year to year can itself partly explain why compositional effects have not altered the trend in job security over the course of our sample. A second thing to note is that the effect of endowments (i.e., compositional effects) is close to or insignificantly different from 0 in all year-pair comparisons that we study–at most, compositional effects contributed 1pp to the difference in insecurity between the later and earlier year, in the GSS between 1994 and 2016. Differences in the endowments of individual covariates, too, contribute almost nothing to explaining the difference in insecurity across years. Most of the raw differences in aggregate insecurity can be explained instead by coefficient differences between years. We interpret this finding as evidence that it is primarily the economic cycle that drives subjective insecurity. We have already shown in Section 4 of the main text that in nearly all cases, subgroups of the labor force and different worker types have experienced nearly identical trends in subjective insecurity, and given this conclusion about the homogeneity of trends across worker types, we feel confident in interpreting the changing coefficients' contribution to the decomposition as reflecting different underlying economic conditions in the different years (which we cannot control for directly, as the regressions are cross-sectional, and the unemployment rate invariant within a given year). Thus, the main conclusion to be drawn from both the Oaxaca-Blinder analysis and the DiNardo-Fortin-Lemieux analysis above is that changing labor force composition has played little to no role in affecting trends in aggregate job insecurity in any of our three countries over the course of the sample.

Table E.1: Oaxaca-Blinder decomposition results: US (GSS)

	1978-1994	1994-2016	1978-2016
Raw difference	0.0427**	-0.0343	0.0048
	(0.0165)	(0.0177)	(0.0143)
Coefficients	0.0384	-0.0467**	-0.0131
	(0.0211)	(0.0172)	(0.0150)
Endowments	0.0043	0.0123*	0.0179
	(0.0115)	(0.0054)	(0.0101)
Individual covariates:			
Self-employed	0.0012	0.0000	-0.0000
	(0.0009)	(0.0004)	(0.0001)
Part-time worker	0.0010	0.0052	0.0081
	(0.0007)	(0.0035)	(0.0092)
Union member	-0.0001	0.0011	0.0033
	(0.0033)	(0.0036)	(0.0107)
Tertiary degree	-0.0032	-0.0016	-0.0164
	(0.0049)	(0.0012)	(0.0233)
Male	0.0018	0.0020	-0.0032
	(0.0030)	(0.0014)	(0.0044)
Married	0.0005	0.0041	0.0101
	(0.0031)	(0.0069)	(0.0155)
Non-white	0.0066	-0.0003	-0.0007
	(0.0063)	(0.0048)	(0.0123)
Immigrant	0.0026	0.0018*	0.0166
	(0.0037)	(0.0009)	(0.0174)
Age	0.0454	-0.0001	-0.0074
	(0.0328)	(0.0003)	(0.0395)
$(\text{Age}/10)^2$	-0.0387	0.0018	0.0105
	(0.0274)	(0.0067)	(0.0424)

This table displays the contribution of composition and coefficient differences to the overall difference in aggregate job insecurity between selected years, derived from the nonlinear Oaxaca-Blinder decomposition found in Powers et al. (2011). Standard errors in parentheses. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$

Table E.2: Oaxaca-Blinder decomposition results: US (HRS)

	1994-2010	2010-2016	2000-2016
Raw difference	0.0822***	-0.0725**	-0.0279
	(0.0229)	(0.0265)	(0.0244)
Coefficients	0.0755***	-0.0778**	-0.0274
	(0.0209)	(0.0272)	(0.0214)
Endowments	0.0067	0.0054***	-0.0005
	(0.0063)	(0.0016)	(0.0041)
Individual covariates:			
Part-time worker	-0.0002	-0.0004	-0.0002
	(0.0002)	(0.0002)	(0.0001)
Union member	0.0023***	0.0006***	0.0020***
	(0.0006)	(0.0002)	(0.0006)
Tertiary degree	-0.0051*	-0.0001	-0.0018
	(0.0022)	(0.0001)	(0.0012)
Male	0.0002	0.0000	0.0000
	(0.0002)	(0.0002)	(0.0002)
Married	0.0002	0.0000*	0.0000*
	(0.0003)	(0.0000)	(0.0000)
Non-white	0.0012*	0.0001	0.0002
	(0.0006)	(0.0005)	(0.0009)
Immigrant	0.0006***	0.0017***	0.0023***
	(0.0002)	(0.0004)	(0.0006)
Age	0.0019	0.0161	0.0039
	(0.0080)	(0.0213)	(0.0052)
$(\text{Age}/10)^2$	-0.0010	-0.0140	-0.0036
	(0.0060)	(0.0206)	(0.0053)

This table displays the contribution of composition and coefficient differences to the overall difference in aggregate job insecurity between selected years, derived from the nonlinear Oaxaca-Blinder decomposition found in Powers et al. (2011). Standard errors in parentheses. $*p < 0.05$, $**p < 0.01$, $***p < 0.001$

Table E.3: Oaxaca-Blinder decomposition results: Germany

	1986-2004	2004-2018	1986-2018
Raw difference	0.0647***	-0.1617***	-0.0970***
	(0.0131)	(0.0066)	(0.0122)
Coefficients	0.0783***	-0.1616***	-0.1005***
	(0.0155)	(0.0069)	(0.0129)
Endowments	-0.0136	-0.0001	0.0034
	(0.0084)	(0.0018)	(0.0045)
Individual covariates:			
Temporary worker	-0.0129***	0.0000	-0.0275
	(0.0027)	(0.0009)	(0.1711)
Self-employed	-0.0011	0.0000	-0.0001
	(0.0017)	(0.0001)	(0.0009)
Part-time worker	-0.0030	0.0000	0.0029
	(0.0017)	(0.0002)	(0.0182)
Marginally employed	-0.0034**	-0.0000	-0.0059
	(0.0013)	(0.0000)	(0.0381)
Job tenure	-0.0121**	-0.0000	-0.0175
	(0.0047)	(0.0003)	(0.1148)
Tertiary degree	-0.0018	-0.0001	-0.0180
	(0.0014)	(0.0011)	(0.1142)
Male	0.0017	-0.0000	-0.0003
	(0.0009)	(0.0000)	(0.0042)
Immigrant	0.0021***	0.0001	0.0112
	(0.0006)	(0.0010)	(0.0701)
Age	0.1669***	0.0009	0.4433
	(0.0271)	(0.0159)	(2.7123)
$(\text{Age}/10)^2$	-0.1406***	-0.0008	-0.3578
	(0.0244)	(0.0157)	(2.1966)
Married	-0.0044**	0.0000	-0.0026
	(0.0016)	(0.0002)	(0.0157)

This table displays the contribution of composition and coefficient differences to the overall difference in aggregate job insecurity between selected years, derived from the nonlinear Oaxaca-Blinder decomposition found in Powers et al. (2011). Standard errors in parentheses. *$p < 0.05$, **$p < 0.01$, ***$p < 0.001$

Appendix F: Heterogeneity analysis details

This Appendix presents the marginal effects from the logit and Poisson models of insecurity for the subgroups analyzed in Section 4, as well as the p-values from joint F tests of the equality of time trends across the subgroups in question.

F.1 Marginal effects of year dummies by demographic group

In the main text, we summarize our results on insecurity by demographic group by regressing the marginal effects described above on the unemployment rate and a linear time trend, but we show the calculated marginal effects of the year dummies, and their standard errors, in the plots below. These marginal effects should be interpreted as deviations in the probability of feeling insecure from the base year, indicated in the figures by a dashed red line. In some cases, the standard errors are quite large, owing to both the small yearly shares of non-standard workers and the number of other controls we use in the regressions, but as in the aggregate regressions, compositional effects are almost negligible, and the point estimates for the year dummy marginal effects in otherwise-identical regressions are extremely similar, and more precisely estimated. We present only the composition-adjusted figures here.

Insecurity by permanent/ temporary job status

Figure F.1 presents the compositionally-adjusted trends in insecurity for temporary and permanent workers in the UK and in Germany (we lack data on job security for temporary workers in the US). In both countries, temporary workers actually feel more secure than they did before the GFC, but this appears especially true in the UK, where the security of temporary workers has risen by over 10pp (admittedly a noisy estimate because the share of workers on temporary contracts is quite small and what variation we have is reduced by the number of other controls we include) in the years since the recession, even while the security of permanent workers returned to its pre-GFC level.

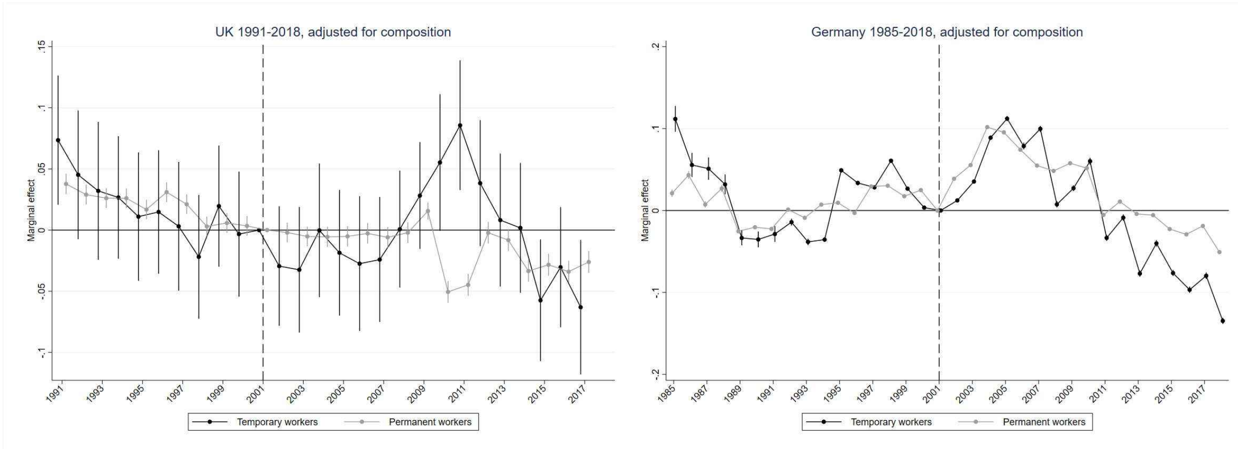

Figure F.1: Marginal effects of year dummies, by temporary work status

Insecurity by full-time/part-time status

Figure F.2 shows the marginal effects of the year dummies in our regressions for each of the US, UK, and Germany for part time workers. Particularly in the UK and Germany, part-time and full-time workers have experienced the same underlying trend in subjective job security over the course of the sample. In the US, while these subgroups may have experienced slightly different underlying trends, insecurity today is around the pre-recession level for both groups. Interestingly, part-time workers appear to have borne much of the rise in GFC-era insecurity in the US. This heterogeneity appears to be absent in the UK and Germany, where insecurity rose relatively uniformly across the two subgroups during the 2008-2011 period.

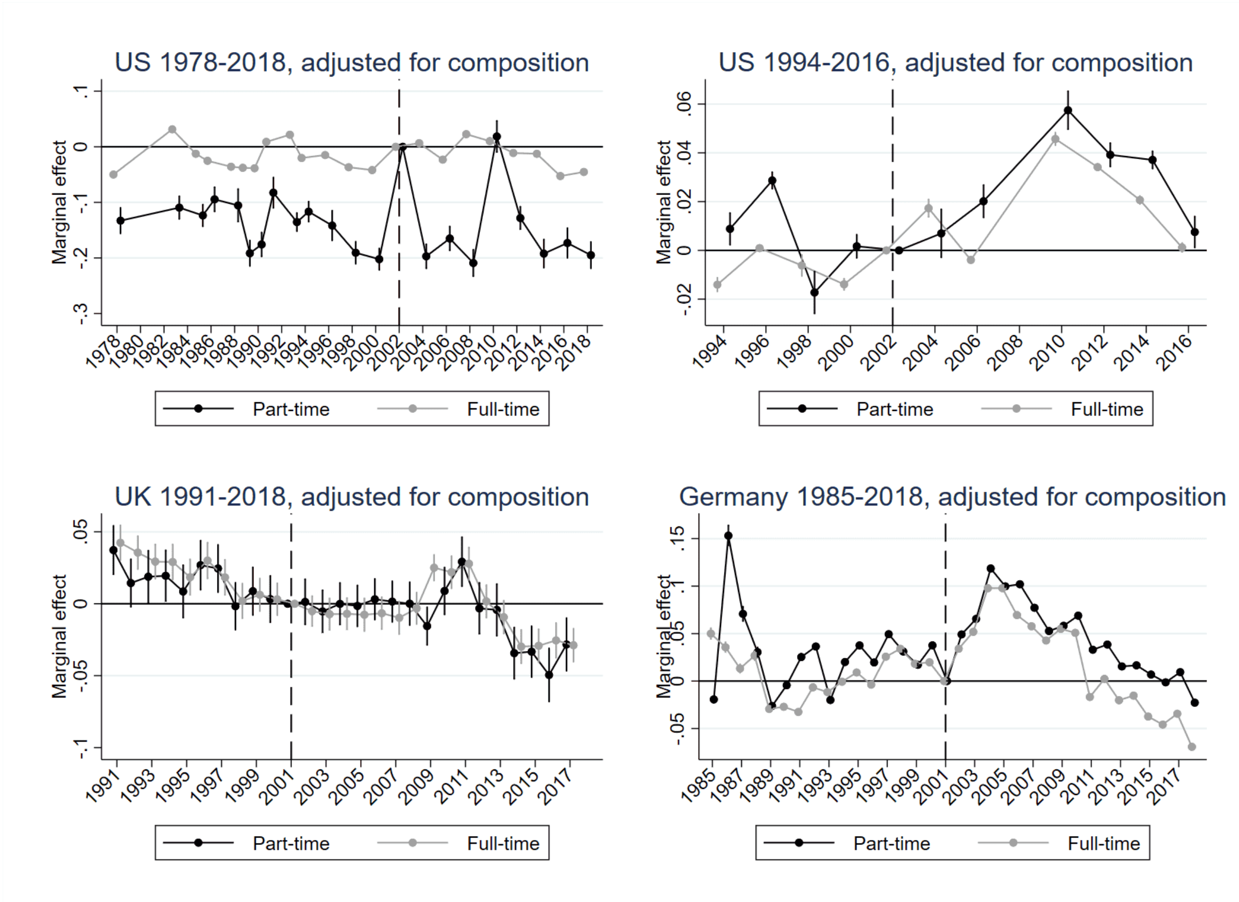

Figure F.2: Marginal effects of year dummies, by part-time work status

Insecurity by sex

Figure F.3 presents the adjusted probabilities of feeling insecure by sex for the US, UK, and Germany as deviations from their pre-GFC levels. The results from all three countries indicate that insecurity followed a similar trend for men and women over the entire sample period, and has returned to its pre-recession levels for both groups.

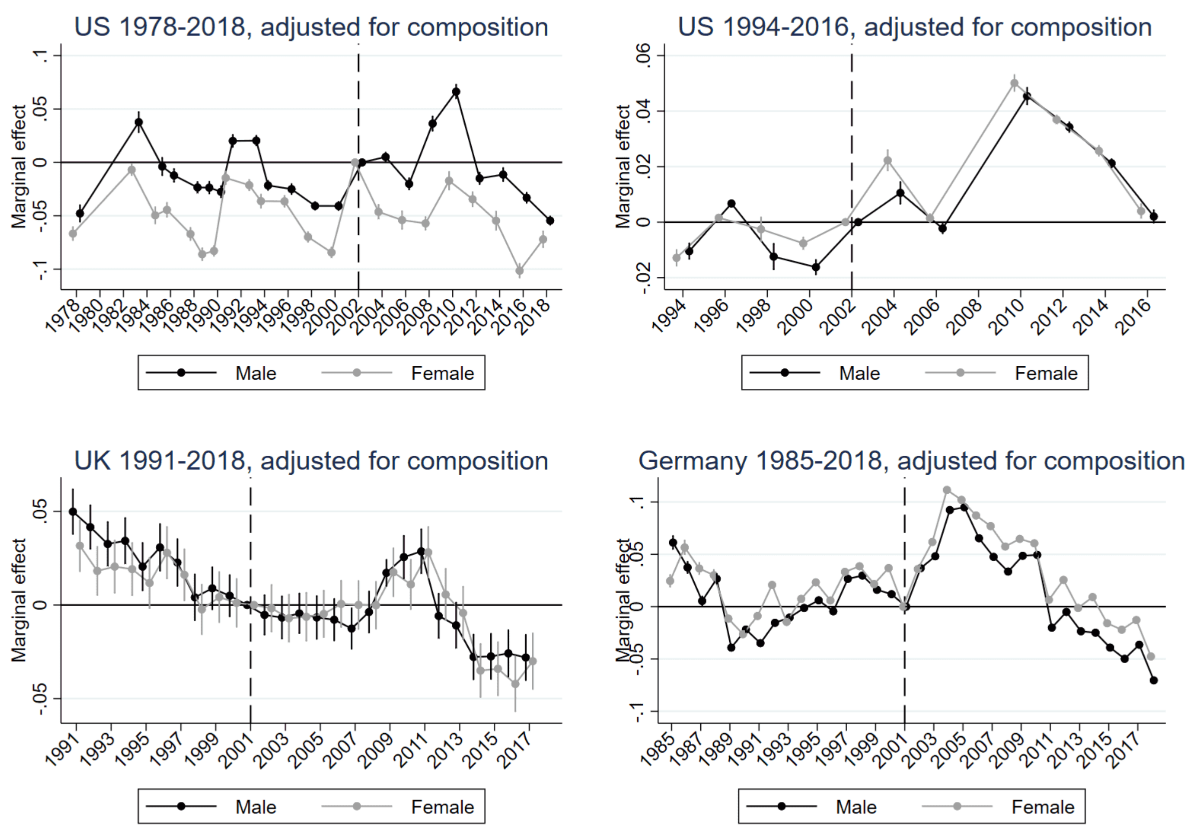

Figure F.3: Marginal effects of year dummies, by sex

Insecurity by self-employed status

Figure F.4 presents the adjusted probabilities of feeling insecure by self-employed status for the US (GSS), UK, and Germany. Note that we do not have data on the subjective job security of the self-employed for the UKHLS portion of the UK sample. In all three cases, insecurity follows a similar trend for the self-employed and the non-self-employed. In the UK, insecurity clearly rose relatively more during the GFC for the self-employed than for employees, but we do not have the data to track the trend after 2009.

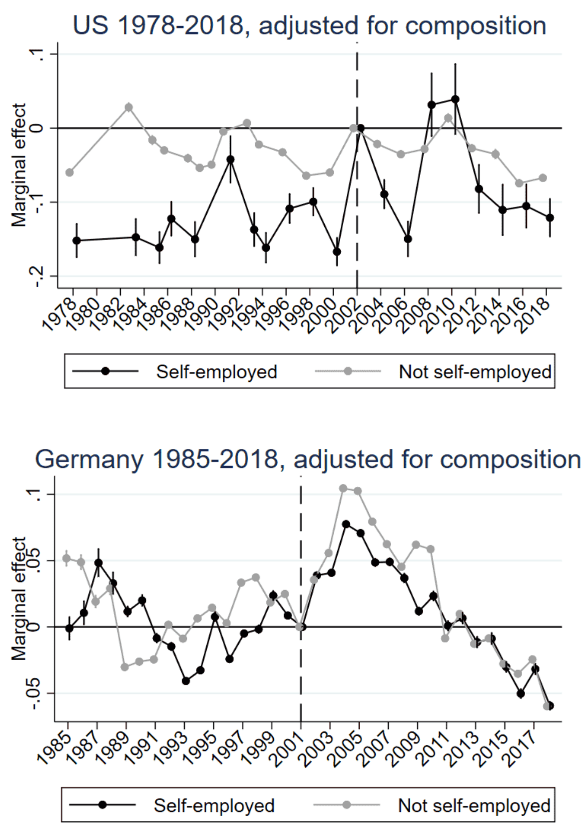

Figure F.4: Marginal effects of year dummies, by self-employed status

Insecurity by education level

Figure F.5 presents the adjusted probabilities of feeling insecure, broken down by level of educational attainment, for the US, UK, and Germany as deviations from their pre-GFC levels. In each country studied, workers with and without tertiary degrees experienced remarkably similar trends in their job security, and in all three countries each subgroup has largely seen their job security return to pre-recession levels–in the HRS, university-educated workers are experiencing insecurity above their base year level (though only by about 2pp).

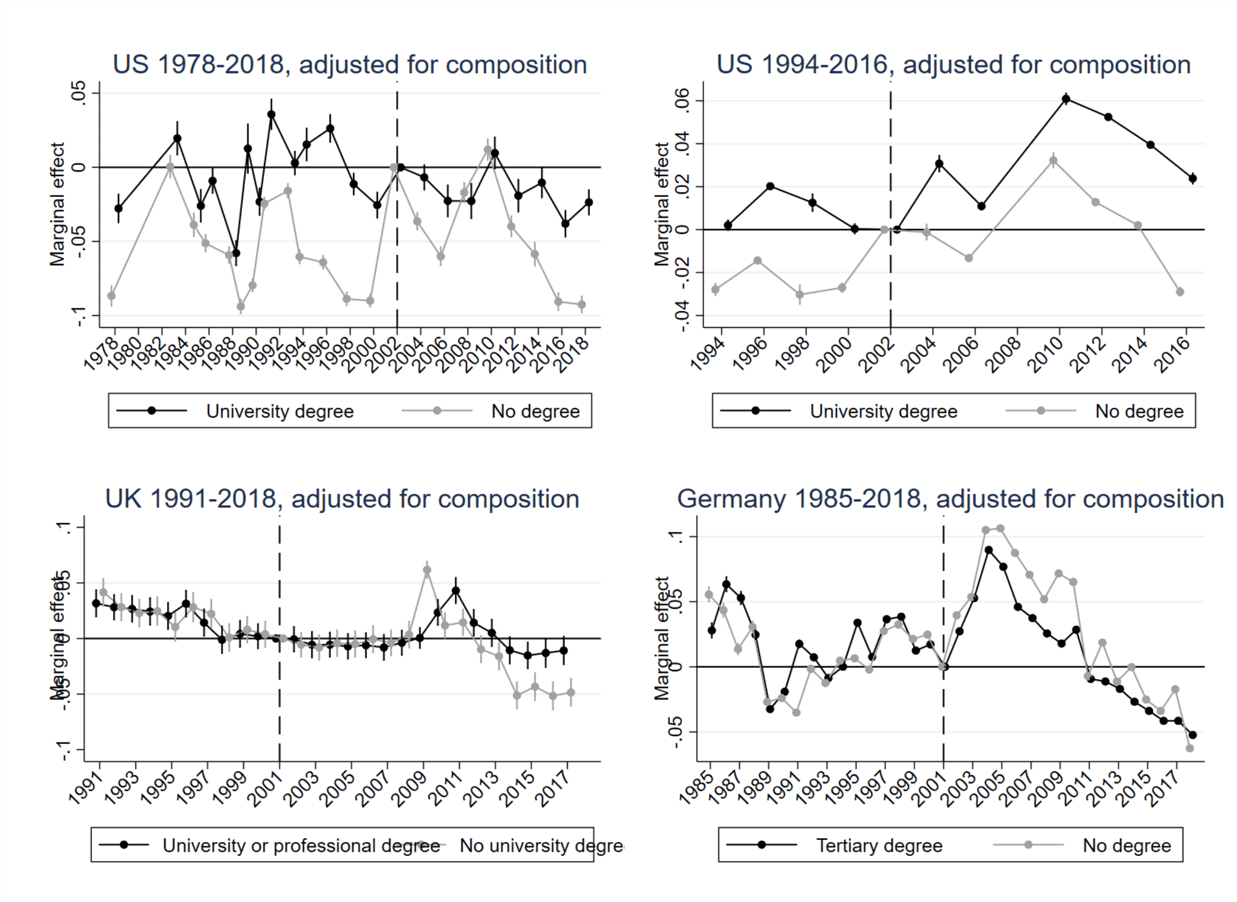

Figure F.5: Marginal effects of year dummies, by education level

Insecurity for new starters

Bachmann and Felder (2018) found that layoffs in Europe during the period 2002-2012 were concentrated among workers of short tenure, increasing the average job tenure via a compositional shift. Analogously, if long-tenured workers feel more secure in their jobs, layoffs of short-tenured workers would mechanically increase the proportion of workers that feel secure in our framework. To test this hypothesis using our UK and German data on job tenure, we first restrict the sample to workers who started their jobs in the year they were surveyed (new starters). The second subsample consists of workers who did not start their job in the survey year. The figure below show that the pre-recession trends in job security over time for these two subgroups are not substantially different in either country, and although the financial crisis and recession appears to have increased insecurity for new starters more than for incumbent workers, in both countries insecurity has returned to pre-recession levels for both groups of workers.

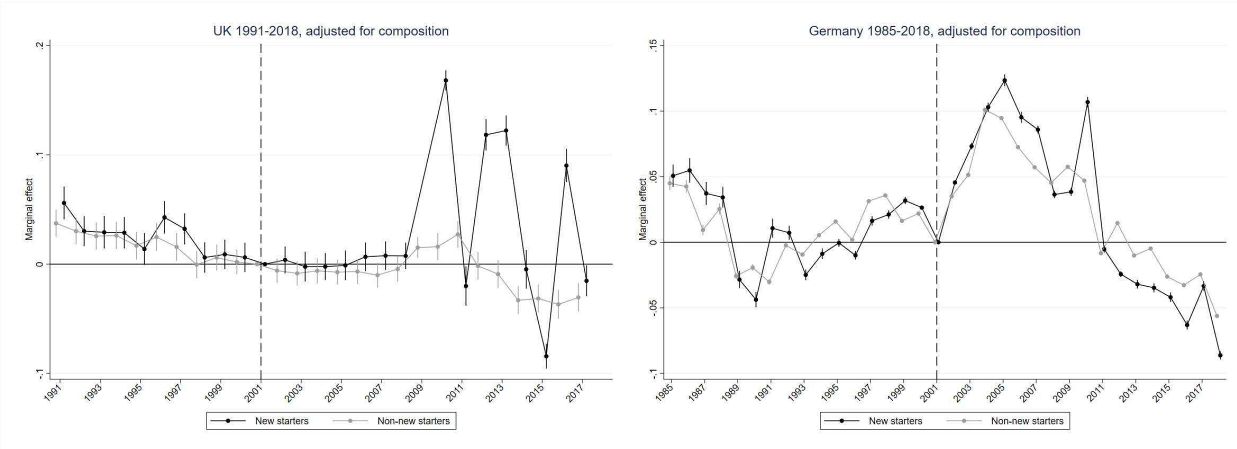

Figure F.6: Marginal effects of year dummies, by job tenure

Insecurity by age

Finally, Figure F.7 presents the marginal effect of the year dummies for the US (GSS), UK, and Germany by age group. Trends are similar for all age groups in all three countries. In the GSS, over-50s appear to have been relatively more insecure in the aftermath of the financial crisis than their under-50 counterparts, which can help to explain the differences in trends between the aggregate GSS and the aggregate HRS–the HRS sample consists primarily of Americans over 50, and their prolonged experience of relatively higher insecurity than younger workers means that a younger HRS sample would yield similar results to the GSS. Meanwhile, despite Bachmann et al. (2020)'s finding that younger cohorts are more likely to be atypically employed in Germany, the trend in subjective insecurity has been largely the same for those above and below 25 in Germany and the other two countries that we study–although we do find that after controlling for unemployment, German under-25s have experienced a positive secular trend in insecurity, a test of equality of the trends for the three age groups does not allow us to reject the null that the trends are the same across age groups (see Table F.1 in the next section).

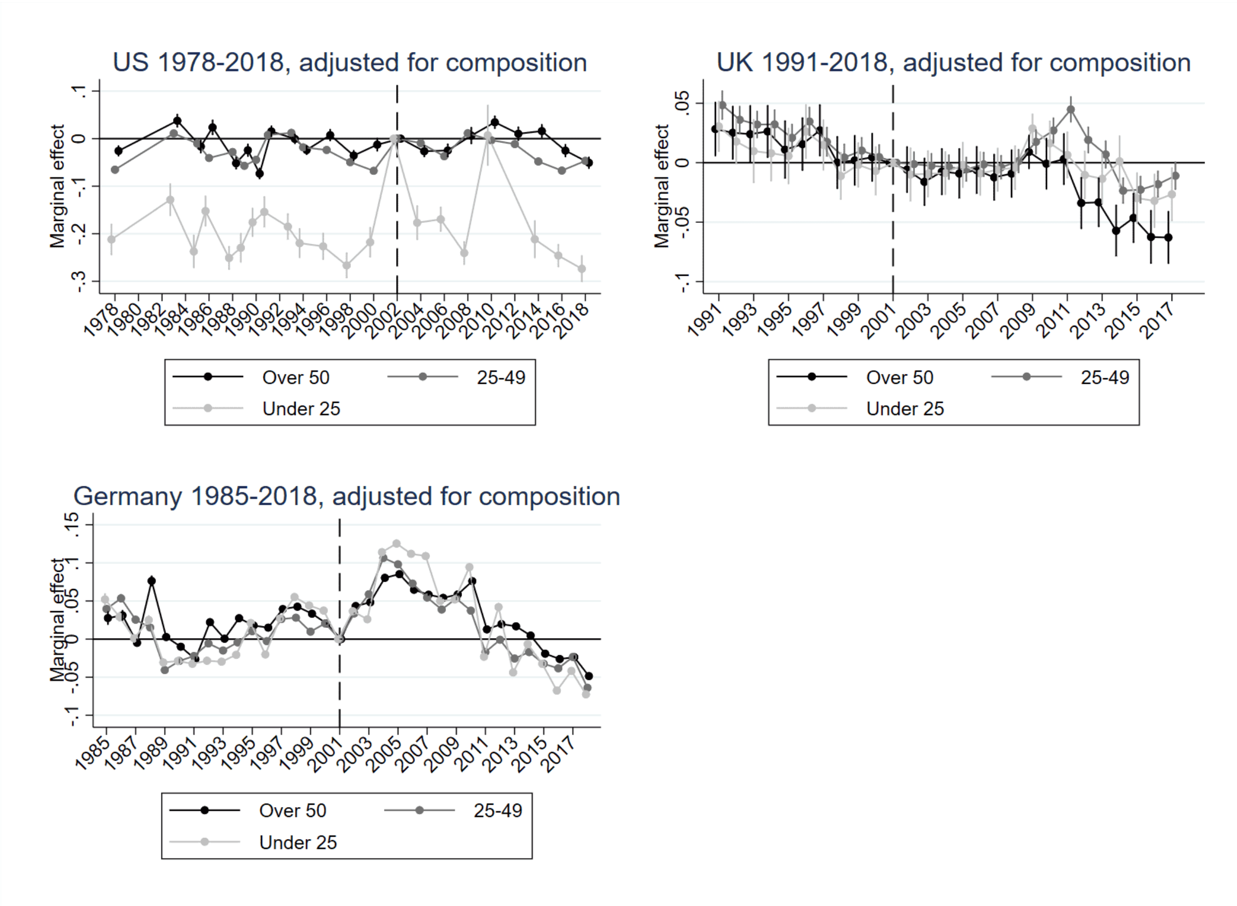

Figure F.7: Marginal effects of year dummies, by age group

F.2 P-values for equal time trends

Table F.1 below displays the p-values from a series of F tests conducted to test the equality of the estimated time trends for pairs of subgroups (e.g. male and female, part-time and full-time, etc.) from the linear regressions summarized in Tables 4 and 5 in the main text. In the majority of cases, subgroups have not experienced statistically different time trends.

Table F.1: P-values for test of equal time trends by subgroup

	US (GSS)	US (HRS)	UK	Germany
Temporary vs. permanent workers			0.01**	0.03*
Parttime vs. fulltime workers	0.45	0.46	0.45	0.99
New starters vs. incumbent workers			0.07	0.70
Male vs. female	0.38	0.81	0.61	0.45
Over-50s vs. 25-49 vs. under-25s	0.56		0.00***	0.39
Tertiary degree holders vs. no degree	0.27	0.49	0.04*	0.02*
Marginally- vs. non-marginally employed				0.59
N	21	12	27	34

This table displays the p-value for an F test that the time trends for the two subgroups in question are equal. *$p < 0.05$, **$p < 0.01$, ***$p < 0.001$

Appendix G: EWCS summary statistics

The table below presents summary statistics for the covariates included in the job security regressions using EWCS data in Section 5. Means and standard deviations for each of the covariates used in the regression analyses are broken out by year.

Table G.1: EWCS descriptive statistics (by year)

	2005	2010	2015
Age	39.5	39.9	41.3
	(11.5)	(11.5)	(11.7)
Male	.556	.558	.527
	(.497)	(.497)	(.499)
Tertiary degree	.261	.313	.354
	(.439)	(.464)	(.478)
Part-time worker	.179	.194	.221
	(.383)	(.396)	(.415)
Temporary worker	.105	.106	.113
	(.306)	(.308)	(.316)
Self-employed	.157	.155	.14
	(.364)	(.362)	(.347)
Job tenure	9.49	9.46	9.75
	(9.66)	(9.51)	(9.6)
N	26714	36229	36410

Appendix H: Job satisfaction regression results

In this Appendix, we present the marginal effects of the year dummies in the job satisfaction regressions in Section 6. It is clear that in all three countries, job satisfaction has not deteriorated in recent years.

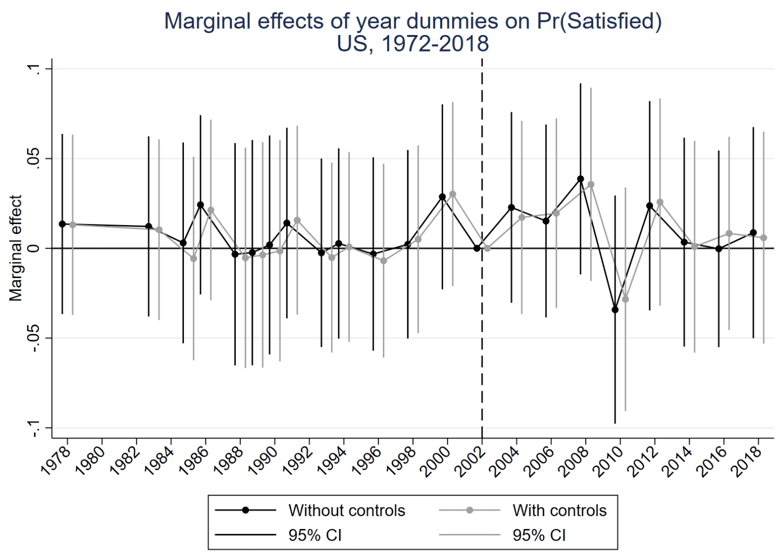

Figure H.1: Marginal effect of year dummies in US (GSS) regressions

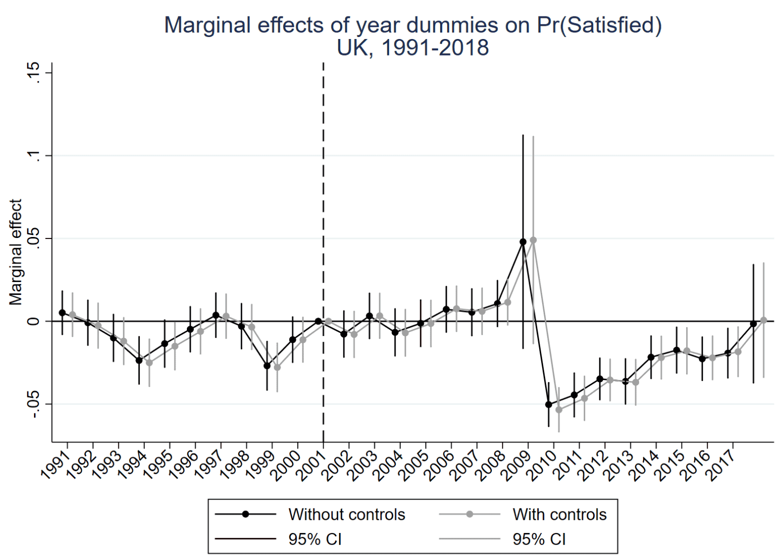

Figure H.2: Marginal effect of year dummies in UK regressions

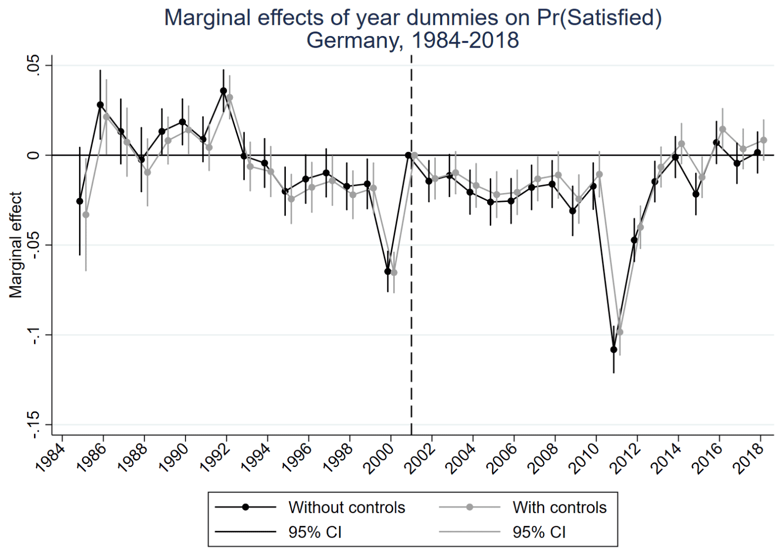

Figure H.3: Marginal effect of year dummies in Germany regressions

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