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A new look at technical progress and early retirement

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Abstract
Technical progress affects early retirement in two opposing ways. On the one hand, it increases real wages and thus produces an incentive to postpone retirement. On the other hand, it erodes workers' skills, making early retirement more likely. We re-examine the effect of technical progress on early retirement in the US. We measure technical change during the whole working life of the individuals and find that its effect on the probability of early retirement is non-monotonic. In particular, when technical change is small, the erosion effect dominates, but when it is large the wage effect dominates. These results may signal that the higher the technical change, the more willing are the elderly to retrain, which has direct policy implications for the design of elderly training programs.

Jel codes: J24, J26, O33

Keywords: Early retirement, Technical change, Training

1 Introduction

The labor participation rate for US individuals between 50 and 64 years old remains below 70% for males and 60% for females despite a general increase in life expectancy and health conditions, as we report in Appendix A: Fig. 1. Hence, there is a non-negligible fraction of individuals that exit the labor force well before they are 65. However voluntary or not, these early retirement decisions influence the economic dependency ratio of a country, that is, the ratio of retirees and unemployed over the employed. In the context of an aging population, policies aimed at decreasing this phenomenon are therefore important and call for a better understanding of the determinants of early retirement.

In this paper we reexamine the effect of technical change on early retirement. This effect can be both positive and negative. On the one hand, technical change can erode individual skills unless workers engage in training programs. On the other hand, higher technical levels may lead to higher individual productivity and therefore potentially to higher hourly compensations. These effects are especially relevant for the elderly because of both the life-cycle profile of wages and the potential age bias of technical change, as new technologies or forms of organization may affect negatively the employability of older workers. We focus on two key aspects of the relation between early retirement and technical change. We evaluate (i) the measure of technical change that is relevant for the obsolescence of elderly's abilities and (ii) the shape and size of its effect on early retirement decisions. We show empirically that the technical change that individuals face since they enter the labor market weighs more on their early retirement decisions than alternative measures of technical change, as we believe the erosion of skills starts from the very entry in the labor market. Moreover, we present extensive evidence that the relation between the technical change and the probability to retire early is non-linear.

We use US data from the Health and Retirement Study (HRS), a survey that follows around 37,000 adult individuals for 10 biennial waves between 1992 and 2010, with retrospective information on their job history. We merge this individual data with US World KLEMS aggregate data on sector TFP growth rates between 1948 and 2010. We associate to each individual the technical change he must have faced in the sector of specialization between the reported year of entry in the job market and the year of observation. We then check how this technical change affects the probability to retire early and compare the results to those obtained with other measures of technical change. We find that the effect of technical change on the probability of early retirement is non-monotonic. More precisely, there exists a threshold technical change around 85% of the distribution, below which early retirement depends positively on technical change, and above which it depends negatively. Our findings are robust to different empirical specifications and estimation methodologies as well as to considering various subsamples and alternative dependent variables.

Other papers have studied the effect of technical change on early retirement. Bartel and Sicherman (1993) (henceforth, BS) find that workers in industries with high technical change retire later than workers in industries with low technical change. They argue that industries that experience high technical change provide on-the-job training along the whole working life. This creates the incentives for workers to retire later in order to collect the returns on their training. Our results are in line with this explanation, as lifelong changes in productivity in the upper end of the distribution are associated with a decreasing probability of early retirement and changes in the lower end of the distribution are associated with an increasing probability. However, differently from BS we find that unexpected technical shocks in the last years of the working life are less relevant for early retirement decisions than the overall technical change that individuals experience since the entry in the labor market. Ahituv and Zeira (2011) identify the wage and erosion effects of technical change on early retirement. They develop a general equilibrium model where wages equalize across sectors. Then, aggregate technical change is responsible for a general wage increase that might reduce early retirement (wage effect), while the sector-specific technical change is associated to the erosion effect. They use this distinction in their empirical analysis to identify the erosion effect. More recently, Messe et al. (2014) use French data to investigate the effect of technical change and on-the-job training on retirement intentions. They find that technical change induces individuals to work longer in jobs with a high probability of skill upgrading. Our findings are in line with all these contributions and offer a general assessment of these possible explanations.

Our work is complementary to the literature that studies the effect of a growing elderly labor force on productivity. Sala-i-Martin (1996) proposes a model where, due to a positive externality in the average stock of human capital, it is socially optimal to encourage retirement when the difference between the skill level of the young and that of the old is large enough. This points to a reverse causality between early retirement and productivity. For example, Meyer (2011) finds that firms with a younger workforce benefit from a larger rate of technology adoption. There is also some evidence that the age composition of the labor force has an aggregate effect on productivity, as Feyrer (2007) and Werding (2008) point out. Since we consider technical changes that occur during the whole working life of individuals and, thus, before the individual early retirement decisions, our results are robust to this issue.

The literature has highlighted other explanations for the evolution of early retirement in the last decades.[1] Some examples are changes in Social Security programs and pension plans (Crawford and Lilien (1981), Blau (1994), Rust and Phelan (1997), Blundell et al. (2002), Coile and Gruber (2007), Vere (2011), Ferreira and dos Santos (2013), Vestad (2013), Atalay and Barrett (2015), Euwals and Trevisan (2014)), changes in the age and skill composition of the labor force (Blau and Goodstein (2010)), changes in workers' health status (McGarry (2004)), changes in leisure consumption choices (Kopecky (2011)), or the rise of the dual-earner family and the tendency of couples to retire around the same time (Gustman (2000), Maesta (2001), Coile (2004), Stancanelli (2012)). We include several regressors in our econometric specification in order to account for a wide range of these complementary explanations.

The paper is organized as follows. In Section 2 we present our data and estimation methodology. In Section 3 we report the estimation results and the robustness checks. In Section 4 we extend the benchmark results to different dimensions of individual heterogeneity and look at the whole transition out of the labor force. Section 5 draws the final conclusions. All figures and tables are in the Appendix.

2 Data and estimation methodology

2.1 Data

We use US data from the HRS, which consists of a national panel survey of individuals collected for the study of retirement and health among the elderly in the United States. The HRS contains information about around 37,000 individuals followed in 10 biennial waves from 1992 to 2010.[2] We have information about the labor status, personal characteristics, and details on the job history of the respondents. As retirement is an absorbing state, for each wave we keep only the observations that correspond to individuals that were working at the time of the previous wave, that is, two years prior to the interview year.[3]

Each individual reports the sector of specialization as the "sector with longest tenure." This sector is unique by construction for each individual.[4] For the vast majority of the observations, the sector of specialization coincides with the "current sector," which is also reported in the survey at each wave. However, we believe that the most relevant sector variable for the measurement of the technical change experienced by individual throughout their working history is the sector of specialization.[5] Our relatively large aggregation with only 13 sectors implies that the individuals that switch sectors across waves (and therefore may report at some point a current sector that differs from the sector with longest tenure) represent only around 11% of the observations.[6]

The US World KLEMS database Jorgenson et al. (2013) allows us to compute the TFP growth that occurred in each industry from any pair of years between 1948 to 2010.[7] In this way, we have the TFP growth in, say, the "Mining and Construction" industry from 1948 to 1949 (and to 1950, 1951,..., 2010), from 1949 to 1950 (and to 1951, 1952,..., 2010), and so on. We then assign to each individual in a certain survey year the TFP growth that occurred in his industry of specialization since the year he entered the job market. For example, we assign the TFP growth that occurred in the "Retail" industry from 1955 to 1998 to an individual that entered the labor market in 1955 who declares in 1998 that his industry is "Retail" (and is working two years prior to the survey).[8]

We focus on individuals who are between 50 and 64 years old at the time of the survey. Moreover, we limit our analysis to males only. Our sample consists then of an unbalanced panel of almost 22,000 observations for more than 6000 individuals. The panel spans 10 biennial survey years from 1992 to 2010, and individuals are distributed in 13 industries.[9] Individuals can be either retired (fully or partially) or not. Appendix B: Table 1 shows the distribution of the individuals in the sample by labor status and age group for the years 1992 (Wave 1) and 2006 (Wave 8).[10] The overall population in our sample shifts through the age groups over time, as the individuals aged 50-54 pass from 46% of the sample in 1992 to 27% of the sample in 2006, whereas the individuals aged 60-64 pass from 10% to 26%. Moreover, the aging population implies a higher percentage of retired people in 2006 (10%) than in 1992 (6%), and a correspondingly lower working population.

2.2 Estimation methodology

We want to estimate how much the probability of early retirement depends on the technical change that individuals face during their working life. The probability model consists of

$$\text{Prob}(\text{Retired}_{it} = 1) = f\left(\alpha + \beta_1 b_{it} + \beta_2 (b_{it})^2 + \gamma X\right), \tag{1}$$

where $\text{Prob}(\text{Retired}_{it} = 1)$ is the probability of individual i to be retired in period t,

$$b_{it} \equiv \ln\left(\text{TFP in sector of } i \text{ at } t\right) - \ln\left(\text{TFP in sector of } i \text{ at entry}\right)$$

is the technical change that occurred in the sector of specialization of each individual between the year of entry into the labor market and year t, and X are various controls for individual, sector, and aggregate characteristics. The function f may well be non-linear like in a logit model. We index the technical change b_{it} by i, as two individuals in period t that work in the same sector may differ in the amount of (sectoral) technical change they are subject to if they differ in the year of entry into the labor market.

The use of an individual measure of technical change is crucial. First, we believe that the obsolescence of individual skills starts from the very entry of individuals into the labor market. Enduring into a certain industry is the product of pre-entry individual skills like the education level and on-the-job training and learning (which we do not observe).[11] Second, early retirement occurs because the working-life horizon elderly individuals face is short enough to make on-the-job training too costly to bear compared to the potential benefits of remaining in the labor market, net of all demographic, health, and socio-economic characteristics. However, the focus on the last years before retirement does not take into account the progressive depreciation of individual skills and the role that individual work trajectories play in the decision to retire early. In other words, our measure of working-life technical change allows us to reduce the individual unobserved heterogeneity that is not captured by, e.g., individual fixed effects. Third, an individual measure generates the necessary heterogeneity in technical change across individuals that belong to the same sector that allows us to distinguish sectoral and time effects from the actual technical change faced by individuals.

The individual measure of technical change poses additional challenges with respect to an alternative, merely sector-specific measure of technical change. First, we may face an endogeneity problem due to omitted individual characteristics that may influence both the entry to the labor market (or the choice of the sector at entry) and the decision to retire at a certain point. In order to capture part of these unobserved individual characteristics, we control for a wide range of observed time-varying and time-invariant individual characteristics in the benchmark estimation. Moreover, we include a specification with individual fixed effects, which should control for at least those components of unobserved individual characteristics that do not change over time. Second, part of the results may be attributed to self-selection of workers, as for example highly skilled individuals may opt for sectors subject to high technical change and potentially high returns. If we suppose that the observable characteristics included in the benchmark specification are correlated with the self-selection, then we can look at subsamples for unique values of these observable characteristics. If estimates vary considerably across subsamples, then we have a signal of self-selection at work, or at least of a dimension of self-selection that is relevant for our exercise. If we suppose that unobserved individual characteristics may play a role in the self-selection of workers, then including individual fixed effects partially accommodates for this issue, as any time-invariant individual characteristics that contribute to the choice of entry in a specific year and in a specific sector are captured by the individual fixed effects. If instead we suppose that observed characteristics included in the benchmark specification are not correlated with the self-selection, then we can consider a specification with random effects. Lastly, we control for selection at exit with a survival model, which also explicitly accommodates for the structure of our dependent variable. Our benchmark estimates are robust to all these concerns. The discussion of these issues paves the way for several extensions that we leave to future research.

As the probability model in (1) relies on individual data that is merged with aggregate data, the errors that originate from its estimation may well be correlated across observations that belong to the same sector-year pair.[12] Hence, we cluster the errors at the sector-year level.

We measure the probability of early retirement with the dummy variable "Retired$_{it}$," which takes the value 1 if an individual i at time t reports to be either fully or partially retired and the value 0 if the individual is either working full-time, working part-time, or unemployed. The variable b_{it} represents the TFP growth that occurs in the sector where the individual is specialized between the year when the individual enters the labor market and the year of the survey. This measure of technical change ranges from 8% (1st percentile) to +364% (99th percentile), with a mean of 188%. By construction, b_{it} varies across sectors of specialization, years of the waves, and years of entry. In order to clarify the source of the variation, we report the distribution of b_{it} across sectors in Appendix A: Fig. 2, across waves in Appendix A:Fig. 3, and across years of entry in Appendix A: Fig. 4. All dimensions of variation present a reasonable amount of heterogeneity across individuals, with the exception of the individuals that enter the labor market from 1981 onwards (less than 5% of the observations).[13] The individuals that enter later in the sample are mostly the youngest. However, Appendix A: Fig. 5 shows that within each birth cohort there is sufficient variation. Hence, we conclude that our measure of technical change presents a rich variability across several dimensions. As the sources of variation in our measure of technical change are the sector-specific TFP processes, the years of entry into the labor force, and the years of waves, identification is achieved as long as at least one of these dimensions varies across observations.

The controls include personal characteristics such as age, race, education, sector experience (i.e., the span of time between year t and the year of entry), region of residence, health status, wealth level, marital status and working status of the spouse, presence of privately- or government-provided health insurance, access to a pension plan or current pension income, and occupation, cohort, and sector dummies. We include also the level of unemployment rate in the year of the survey to control for business cycle-related, time-varying aggregate effects. Appendix B: Table 2 reports the summary statistics of all the variables, together with other variables used in the robustness exercises later.

Our benchmark regressions consist of a pooled logit model and a linear probability model. The almost perfect coincidence between the marginal effects of the logit model and the linear probability model informs us about the structure of the function f of the probability model (1), which appears to be linear in the data. We compare the benchmark specification of the linear probability model with a panel linear probability model with random effects, a panel linear probability model with fixed effects, and the marginal effects of a survival model. Results are consistent across all estimations. Moreover, we perform several robustness checks. First, we consider alternative measures of technical change. Second, we control for the endogeneity of the entry decision. Third, we explore the structure of the relation between technical change and early retirement. In particular, we allow for a lower order polynomial (that is, a linear relation) and a higher order polynomial. Fourth, we check the robustness of the results to subsamples like the pre-crisis period. Fifth, we control for the effect of bridge jobs by eliminating the individuals that report a change in the sector of specialization or the occupation. Sixth, we collect a series of additional robustness checks.

Using the benchmark specification, we detail several potential dimensions of heterogeneity. Hence, we look at the relationship between early retirement and technical change across education levels, occupations, sectors, marital statuses, cohorts, age groups, genders, periods of permanence in the labor force, and training experiences. These exercises serve the purpose of both illustrating the heterogeneous effects within subsamples of the data and corroborating the identification of the key coefficients in the benchmark specification. For example, the sector experience is correlated by construction with our measure of technical change and may affect the early retirement decision directly through higher pension entitlements, wage profiles, and so on. Hence, it is important to see how our key coefficients describe the same nonlinear relationship between technical change and early retirement for both high and low levels of sector experience.

Lastly, we look at the effect of technical change on the whole transition from working full-time to working part-time, to unemployment, and eventually to early retirement. In this way, we show that the effects of technical change on the labor status relate mainly to the decision to retire early and have only marginal effects on the transition to part-time work and unemployment.

3 Empirical results

3.1 Main estimations

We present the main regressions in Appendix B: Table 3. Column (1) shows the results from a pooled logit, column (2) from an OLS, column (3) from a panel OLS with random effects, column (4) from a panel OLS with fixed effects, and column (5) from a survival model (a pooled complementary log-log model).

The benchmark specification in columns (1) and (2) yields a statistically significant dependence between technical change and early retirement. The relation appears non-monotonic, as the negative sign on the square of technical change implies a downward inversion for high-enough levels of technical change. In order to gauge the magnitude of this non-linearity, we plot the predicted probability of early retirement for different levels of technical change in Appendix A: Fig. 6. We compute the prediction using the pooled logit of column (1) for the case of a white single male with 30 years of experience, a pension, and no health insurance, while holding the rest of the variables at their mean. The probability of early retirement for example almost doubles from 3% to 6% if the technical progress experienced throughout the working life consists of a 150% change instead of a 50% change. The hump-shaped relation implies that the probability of early retirement reaches a maximum 7.7% in correspondence of a 260% technical change and decreases back to around 6% for the most extreme values of technical change.[14]

The rest of covariates affect early retirement in the same way as previously found in the literature and have straight-forward intuitions. Individuals delay the decision on retirement if the spouse is still working, in line with Baker and Benjamin (1999), Blau (1998), and Coile (2004), among others. In general, the wealthier the individual, the higher the likelihood of retirement (see Brown et al. (2010)). A higher individual wealth guarantees a higher buffer to face any idiosyncratic and aggregate shocks that may follow the retirement decision. A government health insurance plan is associated with a higher probability of early retirement, while an employer health insurance seems to have no statistical significant effects (if anything, the effect is negative in column (1)).[15] However, specifications that control for unobserved individual characteristics like in column (3) and column (4) stress the incentives towards a longer permanence in the labor force that an employer-provided health insurance conveys. Having a pension plan or already perceiving a pension income seems to reduce the incentives to abandon the labor force. Blundell et al. (2002) highlight how the opportunity cost of working is higher in this case, as having a pension may reduce the probability of early retirement if its benefits increase with tenure. The pension schemes may create additional returns to current wage income *vis-à-vis* future pension benefits. Moreover, the retirement decision yields a marginally lower increase in pension benefits when the individual has already started perceiving them.

Our results confirm the important role that the health status plays in the retirement decision, as in French (2005) and Ferreira and dos Santos (2013), among others. An increasingly bad health status makes individuals more likely to retire early. Moreover, there seems to be a relevant non-linearity in this relationship. Health statuses vary through 5 levels, from excellent to very good, to good, to fair, and eventually to poor. Passing from a very good to a good health status increases the probability of early retirement by around one percentage point, whereas passing from a good to a fair health status increases the probability of early retirement by 5 percentage points. Passing from a fair to a poor health status instead increases the probability of early retirement by more than 20 percentage points.[16]

We do not report race, foreign-born, geographical, education, occupation, age, cohort, sector, and sector experience dummies for the sake of brevity. Hispanic individuals tend to retire later than other reported races (Black/African American and White/Caucasian). The same happens to individuals born abroad and individuals resident in the South as opposed to resident in the North-East. The education level does not seem to have an impact on the early retirement decision, although high-school graduates are somewhat more likely to retire early. Managers and professionals seem to retire early just as much as clerks, less than those occupied in services, mechanics, constructors, and operators, and more than those occupied in farming, forestry, fishing, and armed forces. Not surprisingly, the older the individual, the more likely he is to retire early. The effect is again non-linear. The probability increases by 10 percentage points from 50 to 60 years old, and by a further 40 percentage points from 60 to 64. There does not seem to be any cohort effect, except for a slightly smaller probability (2 percentage points) for those born in the 5 years immediately after the World War (1946–1950). Also differences across sectors are mild. The individuals most likely to retire early are those working in the public administration (their probability is more than 11 percentage points higher than that of the individuals who are the least likely to retire early, that is, those in the Business and Repair industry), followed by Manufacturing and Transportation (between 9 and 7 percentage points higher), and the rest of the sectors (with no statistically significant differences). Sector experience has a negative effect on early retirement. This may capture either learning-by-doing, as individuals that accumulate more years of experience in the same sector tend to resist better the obsolescence of their skills, or a selection effect, as individuals that remain longer in the labor market are those more likely to keep working. In this respect, it is informative that the results do not change in the survival model of column (5), which partially controls for the selection at exit. Hence, we conclude that learning-by-doing is relevant for the early retirement decision.

The estimates from the model with random effects in column (3) are similar to the benchmark model's. The signs do not change, while the magnitudes become somewhat bigger. Hence, we conclude that those components of unobserved individual characteristics that are not correlated with the rest of the covariates do not play an important role. An exception is the employer-provided health insurance, whose coefficient becomes statistically significant and negative in sign. This may reflect the contractual terms of the employer's insurance that vary across employer-employee matches and are specific to the matches themselves rather than observable individual characteristics. The fixed-effects regression in column (4) delivers the strongest deviation from the benchmark, although still only in terms of coefficients' magnitude. The individual dummies displace all time-invariant race, foreign-born, education, occupation, cohort, and sector dummies. There is a total of 6,146 groups in the regression, with observations per group varying from 1 to 8 with a mean of 3.6. Almost 70% of the overall variance is due to the individual fixed effects, and we can safely reject the hypothesis that they are all jointly equal to zero. The coefficients on the technical change and its square do not change sign nor level of statistical significance, but they do change in magnitude. As a consequence, the non-linear relation predicted by the fixed-effects estimation becomes monotonic for all actual values of technical change, as the hump of the relation occurs for values of technical change that are double than the maximal observed value. This change in magnitude may be due to the fact that the year of entry does not change over time and is an important determinant of the individual TFP growth that we use as a measure of technical change. Hence, the individual fixed effects are likely to be correlated with our measure of technical change and may therefore bias the estimates of our key coefficient. The survival model in column (5) consists of a complementary log-log regression and confirms the scenario of the benchmark model. The zero outcomes are 19,228, while the nonzero 2628, and the final pseudolikelihood level and Wald test fall in the ballpark of column (1)'s.

We can rationalize the non-monotonic relation between early retirement and technical change with the two countervailing effects of technical change. Ahituv and Zeira (2011) propose to label an increase in the early retirement probability as an erosion effect. This effect represents the larger difficulty that individuals subject to higher technical innovation in the workplace face in order to remain in the labor market. A decrease in the early retirement probability is instead a wage effect, as a higher technical content increases the hourly productivity of workers and may thus induce an increase in the wage, which would in turn create further incentives for workers to remain in the labor force. Depending on the balance between the countervailing effects of erosion and wage developments, the effect of technical change on the probability to retire early is positive or negative. Previous studies are however quite silent on the shape that such a dependence may take, considering instead only the eventual sign of the relation. Our results suggest that the erosion effect is larger than the wage effect for most levels of technical change, except for high levels for which the wage effect dominates. Although the identification of the mechanism behind these results goes beyond the scope of this paper, let us briefly discuss a series of possible interpretations. First, Burlon and Vilalta-Bufí (2014) propose a model where the inverse-U shape emerges when the retraining cost is increasing and strongly concave with respect to technical change. Second, life-long learning seems important to reduce the cost of training. Hence, sectors with larger technical changes may also be those that offer life-long training to their workers, as in Bartel and Sicherman (1993) and Messe et al. (2014). Third, the magnitude of the observed technical change may proxy the underlying nature of different technological or organizational innovations. For instance, the introduction of computers and ICT technology could be the driver of large technical change as opposed to other more sector-specific innovations that might drive smaller technical changes. If the incentives to train are larger for more general-purpose technologies, this could explain the higher prevalence of training in sectors with large technical change and the inverse-U shape we find.

3.2 Robustness checks

We perform several robustness checks. In Appendix B: Table 4 we estimate the benchmark logit model substituting our working-life measure of TFP growth with alternative measures. In Appendix B: Table 5 we control for the potential endogeneity of our measure of technical change. In Appendix B: Table 6 we approximate the relation between technical change and early retirement with higher and lower order polynomials. In Appendix B: Table 7 we leave aside specific sections of the data that may drive the results. In Appendix B: Table 8 we restrict the estimation to individuals that do not change sector or occupation, so as to isolate the role of bridge jobs. Lastly, in Appendix B: Tables 9 and 10 we collect the rest of the robustness checks.

The use of alternative measures of technical change does not change the overall message. However, some alternatives seem to have a less clear impact on the decision to retire early. Column (2) of Appendix B: Table 4 reports the coefficients associated with the TFP change (and its square) that occurs 5 years prior to the survey year. This measure of technical change stresses the erosion of individual skills that may occur in the last years of working lives. More recent shocks may be more likely to be the sole drivers of retirement decisions. Bartel and Sicherman (1993) suggest that earlier shocks are potentially counteracted by training practices, while later shocks appear as surprises to the skill set of individuals. Column (2) suggests that the 5-years technical change does not have a statistically significant effect on the probability to retire early. The signs are the same as in the benchmark case that we report in column (1), although the coefficients are one order of magnitude larger due to the inherently smaller shocks of column (2). Hence, we conclude that technical changes that may occur in the last years are indeed relevant for the retirement decision but are not the only drivers. We do not report the same exercise with both measures of technical change in the same specification, although it yields a significant coefficient for our measure and a not significant coefficient for the alternative.[17]

In column (3) of Appendix B: Table 4 we divide our measure of technical change by the number of years that individuals spend in the labor force, that is, the sector experience. This variable proxies the mean technical change that each individual faces from his entry into the labor market to the time of the survey. This measure of technical change filters out the component of our measure of technical change that depends on the sector experience and is therefore likely to reflect more the intersectoral differences in TFP processes. The key coefficients have the same sign and similar statistical significance, and the larger magnitudes simply reflect the smaller average values of the independent variable. The predicted probability of early retirement reaches a maximum for a mean TFP growth of around 7.3%, with around 10.4% of the observations lying on the right side of this value, similarly to what happens in the benchmark case.

We can try to disentangle the contemporaneous effects of technical change on both the erosion of skills and the wage of individuals as previously done in the literature. For example, Ahituv and Zeira (2011) propose a way to disentangle these effects by distinguishing between the sector-specific component of technical change, which affects the erosion of skills alone and does not have a sufficient general-equilibrium impact to affect wages, and the aggregate component of technical change which instead affects both the individuals skills and the wages. We report in column (4) of Appendix B: Table 4 a similar exercise, where we associate to each individual in each wave both the aggregate and the sector-specific TFP growth that he faces since his entry in the labor market. We isolate the sector-specific component by subtracting to our measure of TFP growth the corresponding aggregate TFP growth that we derive from the KLEMS data. However, the aggregate measure is by construction more correlated with sector experience than our benchmark measure, as the only sources of variation left in the aggregate measure are the year of entry in the labor market and the year of the wave. Hence, we divide our sector-specific and aggregate measures by the sector experience as in column (3). Our results are consistent with the predictions of Ahituv and Zeira (2011), as the sector-specific technical change seems to have only a linear, positive erosion effect, while the aggregate technical change affects both the erosion of skills and the wage dynamics, thus causing a non-monotonicity in the relation that is similar to our benchmark result.

Column (5) of Appendix B: Table 4 substitutes our measure of technical change based on TFP growth with an alternative measure based on changes in the value added per worker. We use Bureau of Economic Analysis' data with NAICS-coded industries.[18] We match individual and aggregate data in the same way as in the benchmark exercise. The value added per worker may better reflect potential developments in the individual wage since it refers more closely to the individual productivity of workers. The results suggest a milder dependence of early retirement from developments in the value added per worker than in the TFP, with a proportional reduction in the curvature of the relation. We reach the maximal probability of early retirement around the same growth rate of 260% as in the case of TFP growth. However, the predicted probability that results from such a technical change is around 4 percentage points lower than in the case of TFP changes, other things equal. This is in line with a relevant role for wage effects, as a measure that arguably is a more accurate proxy of individual wages than TFP yields a lower effect of the same growth rate on the probability to retire early. Hence, we conclude that alternative measures of technical change permit the exploration of different dimensions of the same dependence and help a more robust interpretation of the benchmark results.[19]

We cannot directly measure the wage effect, as we miss the correct counterfactual wage that retired individuals would enjoy if they remained in the labor market. However, we can still look at how the hourly wage of workers depends on our measure of technical change. Appendix A: Fig. 7 reports the (unconditional) fractional-polynomial prediction of the hourly wage for the individuals that work at the time of the survey as a function of our measure of technical change. The wage depends positively on technical change and the noise associated to this dependence increases for high-enough levels of technical change, where the average equilibrium wage may even decrease as a result of selection at exit, pension schemes, bargained-again labor contracts, and so on. The interpretation for which the wage effect is likely to dominate for high enough levels of technical change squares well with this evidence.

We address the endogeneity of the entry decision in Appendix B: Table 5. The decision to enter the labor market in a certain year may be driven by an unobserved individual characteristic that drives also the exit decision, thus inducing a spurious correlation between our measure of technical change and early retirement. Moreover, the individual fixed effects of the regression in column (4) of Appendix B: Table 3 are correlated by construction with the year of entry of individuals, which may induce a bias in the estimate of the effect of technical change on early retirement. Hence, we reduce the endogeneity of the entry decision by substituting our measure of technical change with two alternatives. In column (2) we report the regression with individual fixed effects where the technical change is measured from the year in which individuals have finished their education. We derive this year by adding to the birth year the reported years of education and the 6 years that precede the start of compulsory education. In column (3) we drop also the endogeneity of the education decision and simply measure technical change since individuals are 21 years old. However arbitrary, this measure is homogeneous across individuals that belong to the same cohort. Thus, the individual fixed effects are less correlated with technical change. The correlation between the individual dummies and the rest of the covariates drops by 10 percentage points from the benchmark fixed-effects regression to the regression in column (2) and by one additional percentage point to the regression in column (3). The resulting coefficients portray an even steeper relation between technical change and early retirement than in the benchmark fixed-effects regression. The relation is monotonic (for the realized values of our measure of technical change) and concave as in column (1), while the coefficients on the rest of the covariates remain unaltered.

In Appendix B: Table 6 we explore the shape of the relation between technical change and early retirement. In particular, we check whether our results are robust to the order of the polynomial that we use to approximate the dependence. The linear approximation of column (2) yields a positive and statistically significant coefficient associated to technical change. The linear predicted probability crosses exactly the non-linear prediction of the benchmark regression, while the rest of the covariates is virtually the same up to the third digit. In column (3) the polynomial of order 3 yields a coefficient for the cube of the technical change which is not statistically significant. Moreover, the magnitude of the three key coefficients implies a prediction that basically coincides with the prediction of the benchmark quadratic approximation within the actual support of technical changes. Hence, the quadratic term of benchmark regression alone seems to capture most of the non-linearity in the relation.

In Appendix B: Table 7 we explore relevant subsamples of the data. In particular, in column (2) we consider only positive TFP shocks so as to rule out the possibility that our results are driven by a few negative shocks. The estimates are close to the benchmark case. In column (3) we consider only the years before the Great Recession, that is, we exclude from the sample the surveys conducted in 2008 and 2010. These are years of deep economic turmoil, and individuals' retirement decisions might be driven by different factors, at least beyond what our proxy for the business cycle (the unemployment rate) might capture. The coefficient that changes the most is the one associated with the net wealth. We read this as a confirmation of how a higher wealth may work as a buffer against the adverse crisis shocks, as individuals with a higher wealth are more likely to drop out of the labor force after the crisis (when wage prospects and work conditions deteriorate) than before the crisis.

We restrict the estimation to those individuals that do not report any change of current sector or occupation across all surveys in Appendix B: Table 8. In this way, we identify those individuals that do not respond to technical change with a professional conversion to a different sector or occupation. In other words, we control for the use of so-called bridge jobs by elderly individuals that are close to the retirement age, which is one potential dimension of selection at exit. The estimates reveal a stronger effect of technical change on these individuals. Elderly individuals with a smaller set of alternatives to early retirement are more likely to retire early as a consequence of a technical change. The magnitude of the key coefficients are between almost two and three times the benchmark's, while the rest of the covariates are not relevantly different.

We gather the rest of the robustness checks in Appendix B: Tables 9 and 10. In column (2) of Appendix B: Table 9 we employ the benchmark specification using the survey weights of the 1992 wave. We omit for the sake of brevity the rest of the sensitivity analyses using the survey weights from the other waves as well as wave-by-wave regressions with the corresponding survey weights. The benchmark results are robust to all these variations. The only slight difference is in health status' coefficients, as a poor health status predicts a probability of early retirement that is 3 percentage points higher than in the benchmark regression. In column (3) in Appendix B: Table 9 we consider a winsorized version of our measure of technical change. We winsorize the distribution of individual TFP growths within each wave at the 1-st and 99-th percentiles. The results do not change even if we consider lower thresholds for the winsorization like 5-th and 95-th percentiles. In column (2) of Appendix B: Table 10 we substitute the cohort dummies of the benchmark specification (which imply one dummy every 5 birth years from 1927 to 1960) with birth-year dummies, with no consequence on the results. Also a larger aggregation (not reported) with dummies every 10 birth years does not yield different results. In column (3) of Appendix B: Table 10 we consider the one-year lag of all time-varying covariates, that is, marital and health status, insurance and pension endowment, residence, and sector experience. No significant change occurs in the key coefficients.

4 Extensions

4.1 Dimensions of heterogeneity

We explore different dimensions of heterogeneity across individuals. The following exercises serve two purposes. On the one hand, they illustrate several potential channels of heterogeneity across individuals that the literature finds relevant. On the other hand, the detail by different characteristics adds to the robustness of the results and sheds light on the key issues that our econometric set-up and our data pose. Appendix B: Table 11 looks at differences across education levels, Appendix B: Table 12 across occupations, Appendix B: Table 13 across sectors, Appendix B: Table 14 across marital statuses, Appendix B: Table 15 across cohorts, Appendix B: Table 16 across age groups, Appendix B: Table 17 across genders, Appendix B: Table 18 across levels of sector experience, and Appendix B: Table 19 across training experiences.

Appendix B: Table 11 splits the benchmark sample between individuals that have a college degree and individuals that do not have a college degree. In the benchmark logit regression, the coefficients associated with the dummies for the education levels are not statistically significant in general. However, the education level may have implications for our regressors rather than for our dependent variable. Our key regressors representing technical change are similar across the two subsamples. However, in the college sample government-provided health insurance, though still relevant, impacts the probability of early retirement by less than half the magnitude of the coefficient in the non-college sample. In the case of the wealth level, the impact even loses statistical significance. Also the impact of the health status is slightly different. College graduates seem to react early on to signs of declining health, as the coefficients of the dummies for less-than-excellent health statuses are higher in magnitude than for non-college graduates. However, the response to a poor health status is stronger in the sample of non-college graduates. We omit the sensitivity analyses on all the rest of subsamples by education level.

The second dimension of heterogeneity we explore is the profession. We separate individuals that report to be managers and specialized professionals from the rest of the sample in Appendix B: Table 12. If anything, we would expect managers and specialized professionals more likely to engage in training practices later in life, which would in turn help them to remain in the labor force even in presence of large technical changes in their sector of specialization. The unconditional summary statistics indicate that the percentage of managers and specialized professionals that retire early is around 4 percentage points lower than the percentage in other occupations, even though they are subject to technical changes that are on average 10 percentage points higher. There must be a higher wage effect for managers and specialized professionals. However, we do not find relevant differences across the two subsamples for our key coefficients. Hence, we deduce that the non-linear relation between early retirement and technical change captures sufficiently well the balance between erosion and wage effects within these subsamples. The rest of the covariates vary in a similar way as in the case of the subsamples by education level. We do not report more detailed splits across other occupations as they deliver a similar overall message.

In Appendix B: Table 13 we analyze the differences across sectors. As we derive our measure of technical change from both sector-specific and individual data, looking within each sector means that the variation of our measure is due exclusively to individual characteristics, that is, the timing of entry and exit from the labor market. We choose to isolate the manufacturing sector (durables and non-durables), the professional and related services sector, and the public administration sector from the rest. Professional and related services and public administration are two sectors that were subject to large technical change, as Appendix A: Fig. 2 shows. Manufacturing represents a large sector that comprises more than one fourth of the overall observations. We find that the relationship between technical change and early retirement is stronger in manufacturing and public administration. However, the level of technical change that maximizes the probability to retire early is around 260% like in the benchmark. The difference is that for that level of technical change an individual in the manufacturing sector or in the public administration is more than 10 percentage points more likely to retire early than an individual in a generic sector, other things equal. Individuals in the professional and related services are not only less prone to retire early in general but also less reactive to technical change, health concerns, marital status, and any of the other covariates. The general message is that the non-linear structure of the relation between early retirement and technical change holds even within each sector, although with slightly different magnitudes.

Appendix B: Table 14 suggests that unmarried individuals are more responsive to technical change than married individuals, perhaps because of joint retirement decisions that mitigate potential retirement incentives of the elderly. Appendix B: Table 15 looks at a sample split by cohort. We look at the benchmark specification depending on the year of birth and separate individuals born before World War II (up to 1939) from individuals born after World War II (after 1939). The coefficients are similar to the benchmark's, and the predicted probabilities across subsamples as a function of technical change coincide almost perfectly. We draw the same picture from Appendix B: Table 16, where we detail the relationship by age group. We do not find relevant differences in statistical significance and sign of the coefficients. The increasing magnitude of the effect of technical change for older age groups partly reflects the higher unconditional mean of the dependent variable, which we document in Appendix B: Table 1, but also a potential age bias of technical change. The rest of the covariates increases in magnitude as well, which suggests a higher relevance of the first interpretation.

Appendix B: Table 17 presents the results for the female sample. We do not consider females in the benchmark exercise in order to be consistent with the rest of the literature. Moreover, there is an important selection issue as the female labor force participation increases relevantly in the same period over which we compute our measure of technical change, so the results for the female sample are likely to be less robust to endogeneity concerns. The relation between technical change and early retirement is convex rather than concave for females, although for the actual values of our measure of technical change, the relation is simply positive and monotonic. Females seem to be seven times more responsive to their marital status than males. The lower female labor force participation coupled with the assortative matching in the marriage market reduces the outside options for elderly married women. Elderly female workers are more likely to slide into early retirement to follow their partners' retirement choices, an interpretation that is confirmed by the unchanged coefficient on the working status of the partner. If we look at the (unconditional) retirement probability by gender and by marital status, we find that married males are less likely to retire early than unmarried males, while married females are more likely retire early than unmarried females. Females are also four times more responsive to their wealth and twice more responsive to having a pension scheme or receiving a pension income than males. We interpret the reaction to marital status, wealth, and pension as evidence of relatively higher financial constraints and dependence faced by female individuals. Health is another important determinant for females' retirement choices in terms of both privately-provided health insurance and actual health status. The health status has an even more non-linear effect than in the males' sample. A female individual in poor health is almost 39 percentage points more likely to retire early than a female with excellent health, whereas a male in poor health was only 28 percentage points more likely than a male with an excellent health.

In Appendix B: Table 18 we explore the role of sector experience. Our measure of technical change is correlated by construction with the span of time that individuals pass in the labor force. However, its inclusion is important as it may affect the early retirement decision through other channels such as pension entitlements, resilience to further technology and organizational shocks, and so on. Hence, it is important to check whether our benchmark estimates report a nonlinear relation between technical change and early retirement even when we control for a nonlinear effect of sector experience. In column (2) we show that including dummies for the quartiles of the sector experience distribution instead of the yearly measure does not substantially alter the results. The predicted marginal effect is only slightly flatter than the benchmark case. In this case the correlation between our measure of technical change and the sector experience is arguably lower. The estimates of the dummies show a nonlinear, concave relationship between sector experience and early retirement. In column (3) and column (4) we use the specification of column (2) and look at the heterogeneous effects of technical change for levels of sector experience that are either higher or lower than the median value. By construction, only one dummy for the sector experience quartiles is left in each subsample. The overall concave effect is maintained, with a flatter response of early retirement for lower levels of technical change.

In Appendix B: Table 19 we check for the different reaction that individuals reporting more than 100 h of training in their work life might have.[20] Consistently with the previous literature, we find that training practices help individuals to counteract the erosion effect of technical change. Thus, the effect of technical change on the early retirement of individuals that do engage in training is nil, which may be the net result of both the erosion and the wage effects.[21]

The exercises with education, professions, sectors, and marital status look at relevant ways to divide the sample and show us that potential self-selection concerns on observables are less severe than one may expect from our data structure. The key coefficients on technical change are robust to differentiation across observable characteristics that are potential dimensions of heterogeneous early retirement behavior. Whatever the unobservable characteristics that may drive the decision to reach a certain education level, to select a certain profession, or to specialize in a certain sector, they do not seem to affect the way in which the probability of early retirement depends on technical change. The exercises with cohorts, age groups, and genders show us instead that even a split based on an observable exogenous characteristic such as the year of birth does not relevantly alter the estimates. Important differences arise only when we look at the effect of training, which is structurally connected to the erosion that the technical change can cause on the ability of individuals to remain in the labor market. Receiving training makes early retirement not respond to technical change.

4.2 The process of transition out of the labor force

Technical change may have effects not only on the decision to retire early but also on the likelihood to maintain a full-time job. This may be especially true for the elderly, as the short work-life span left before retirement reduces the incentives of both workers and firms to invest in the maintenance of individual skills and tends to reduce the involvement of the elderly in the production process. In general, the transition of older individuals into retirement is not necessarily an abrupt shift from a full-time job to full retirement but may rather pass through a reduction in working hours, a different occupation or job, or unemployment.[22]

We look at the transition of individuals in our sample from working full-time to working part-time, to unemployment, and eventually to retirement. In Appendix B: Table 20 we keep the benchmark specification and change dependent variables. In column (1) we report the benchmark estimation with the dummy "Retired" as the dependent variable. In column (2) we look at the probability of being either retired or unemployed. In column (3) we extend further the definition of the dependent variable to include also the part-time workers. We do not observe significant changes of the key estimates across specifications, net of the level effect of a larger share of population reporting "1" as the value of the dependent variable. Appendix A: Fig. 8 reports the predicted probabilities of the estimations in column (1), column (2), and column (3) of Appendix B: Table 20 (the first prediction is the benchmark that we report also in Appendix A: Fig. 6 together with the relative confidence intervals). The shape of the relation coincides across dependent variables, and the function simply consists of an upward level effect. Hence, the technical change eminently affects the retirement decision rather than intermediate steps towards the exit from the labor force, like part-time work or unemployment. The health status seems to be another specific determinant of early retirement rather than a determinant of labor statuses in general. The coefficients associated to the rest of the covariates suggest that working part-time and unemployment depend more than early retirement on the marital status, the nature of health insurance and pension schemes, or the wealth level.[23]

5 Conclusions

We explore the role of technical change on early retirement decisions. We propose a measure of technical change that takes into account the whole working life of individuals, as we believe that the obsolescence of individual skills begins from the very entry into the labor force and that relevant, potentially unobserved elderly characteristics depend on their entire work-life history. We allow the shape of the relation between technical change and early retirement to be non-linear so as to accommodate for a wide range of possible explanations. We control for observed and unobserved individual characteristics and provide a large swath of robustness checks. Moreover, we present a series of exercises that analyzes several dimension of potential heterogeneity across individuals that belong to different education levels, occupations, sectors, marital statuses, cohorts, age groups, and training experiences. Lastly, we present evidence on how the technical change specifically affects the decision to leave the labor force by the elderly rather than intermediate labor statuses like working part-time or being unemployed.

Our contribution departs from the previous literature as we consider the technical change that each individual is subject to during his whole working life. Our results suggest that the erosion effect of technical change on individual skills is a pervasive feature of the early retirement decision. However, the contemporaneous effect on wages is a relevant component as well, especially for large enough technical changes. This leads to a non-monotonic relation between early retirement and technical change, with a downward tilt for high technical changes. Alternative measures that stress the role of wage effects confirm this interpretation.

Further research should use more extensive data to explore the mechanisms driving these results. A series of potential candidates includes the shape of retraining costs, the consequences of on-the-job training and life-long learning, and the heterogeneous effects of inherently different technological innovations. Structural modeling may help to better distinguish the contributions of the different channels and to encompass general equilibrium effects.

In the context of an aging society, policies may aim at delaying retirement and stimulate the participation of the elderly to the labor force. For example, an increase in workers' working horizon can stimulate elderly's labor force participation, as Staubli and Zweimüller (2013) and Brunello and Comi (2013) show for Austria and Italy, respectively. These policies should take into account our results. For example, individuals in sectors with larger technical change seem to be more prone to take up training programs and delay retirement. Therefore, training policies for the elderly may be more effective in these sectors.[24]

We omit at least two potentially important determinants of the early retirement decision in our analysis due to data limitations. First, previous studies like Johnson and Neumark (1997) find evidence of age discrimination in the work place in terms of both career prospects and the likelihood of job separations. Self-reported age discrimination may then be a relevant driver of the early retirement decision.[25] See Neumark (2009) for a review. Second, previous studies like Liebman and Luttmer (2012) have also assigned a specific role to individual expectations over future pension benefits and the rest of income components in shaping the labor supply of the elderly. We leave both extensions to future research.

In this paper we abstract from whether the retirement decision is voluntary or involuntary, although this aspect may be relevant for policy design. Dorn and Sousa-Poza (2010) show that involuntary early retirement is more widespread in Continental Europe as it depends also on employment protection legislation and not only on the generosity of pension schemes. Eichhorst (2011) stresses the importance of incentives for both workers and firms in the design of "active aging" reforms and explores the related political economy mechanisms.

Endnotes

[1] Maestas and Zissimopoulos (2010) provides an extensive review.

[2] We adopt the RAND HRS elaboration of this data. The RAND HRS Data file is an easy to use longitudinal data set based on the HRS data. It was developed at RAND with funding from the National Institute on Aging and the Social Security Administration. See RAND (2013). The original Health and Retirement Study public use database is produced and distributed by the University of Michigan with funding from the National Institute on Aging (grant number NIA U01AG09740). Ann Arbor, MI, (2014).

[3] In the data there is a negligible number of individuals that return back to the labor force after having retired. We exclude them from the analysis.

[4] Individuals that repeat the survey over time may report different sectors as the one with longest tenure. However, these individuals represent a negligible share of observations. Nevertheless, we assign to each individual the sector with longest tenure that is reported in the last available survey wave.

[5] The current sector may in fact reflect the process of exit from the work force, as individuals are first pushed out of their sector of specialization and later out of the labor force all together.

[6] We include a robustness exercise that considers only the individuals that do not switch sectors in Section 3.

[7] We aggregate the ISIC-coded industries of KLEMS so as to be compatible with the classification of the US Census Bureau adopted by the HRS. We report in Appendix C: Table 21 the details on the industry correspondence.

[8] In order to smooth out business cycle effects, we use the average TFP across three years, from the year before to the year after the entry into the labor market. We do the same for the TFP levels around the year of the wave.

[9] The HRS data does not allow for a more disaggregated industry classification that covers all the survey waves. A 19-sectors classification is available only from 2006 and only for some individuals.

[10] We ignore individuals that are either disabled or not in the labor force (and yet not retired), as they represent a negligible share of the population. We prefer to report the 2006 wave in order to avoid the crisis years, although the demographic and labor status dynamics do not change in 2008 and 2010.

[11] For a discussion on perceived skill obsolescence and its relevance even in early stages of individuals' working life, see for example Cedefop (2012) and Sanders et al. (2015).

[12] See Moulton (1990).

[13] The negative values of b_{it} for some of these observations are due to the drop in TFP induced by the crisis in 2008–2010, which dominates only for those individuals that enter the labor market in recent years.

[14] The interval 10%–360% supports 98% of the technical changes' distribution, with 14.6% of the observations concentrated on the right of the 260% maximum.

[15] See Behaghel et al. (2014) for a recent quantitative evaluation of the impact of disability insurance on early retirement behavior in France.

[16] See Jousten et al. (2014) for evidence health concerns' role in the decision to exit the labor market.

[17] We also do not report sensitivity analyses that feature only linear effects and different specifications of the set of dummies, as they deliver the same message as the one reported in Appendix B: Table 4. These exercises are available upon request.

[18] Appendix C: Table 21 reports the sector correspondence across the different classification schemes.

[19] The rest of the covariates do not change relevantly across the different specifications. This detail is available upon request.

[20] We have this question for the waves in 1992 and 1994 only, so we assign the value 1 to the dummy "Training" to all individuals in all waves that answered positively to this question in either the 1992 or the 1994 wave.

[21] For a detailed analysis of the determinants of training, see for example (OECD 1999, Table 3.12.)

[22] These dynamics may well depend on the labor market institutions and retirement schemes. Brunello and Langella (2013) show for example how increases in the minimum retirement age in Europe lead to a higher likelihood of a gradual transition into retirement. Euwals et al. (2012) analyze the effect of an institutional change on the possible pathways to retirement in the Netherlands.

[23] Technical change may affect the participation in the labor force of the elderly not only in the extensive margin but also in the intensive margin. The transition from full-time to part-time job may only partially take this into account (hours worked pass from an average of 46.6 h to an average of 29.7 h from the full-time to the part-time subsamples), and looking at the actual hours worked may be more informative on this margin. However, the unconditional correlation between hours worked and our measure of technical change in the sample of individuals that still work is nil (0.2% with no statistical significance).

[24] Policies that target early retirement should also take into account potential side effects of a longer work life, like health deterioration, as Hallberg et al. (2014) point out.

[25] Adams (2002) uses a question contained in the first wave of the HRS about age discrimination in job promotions, which only indirectly relates to early retirement decisions.

Appendix

Appendix A: Figures

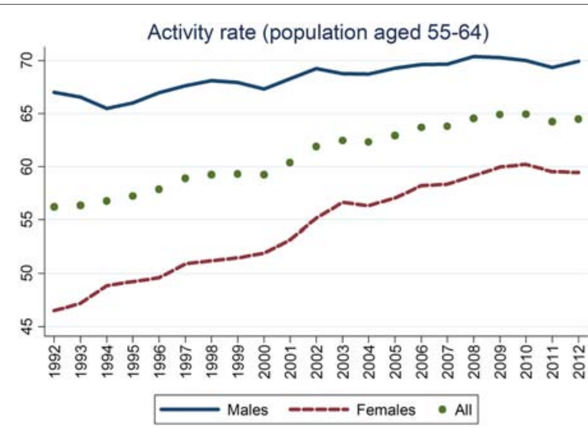

Fig. 1 US data from OECD (2013)

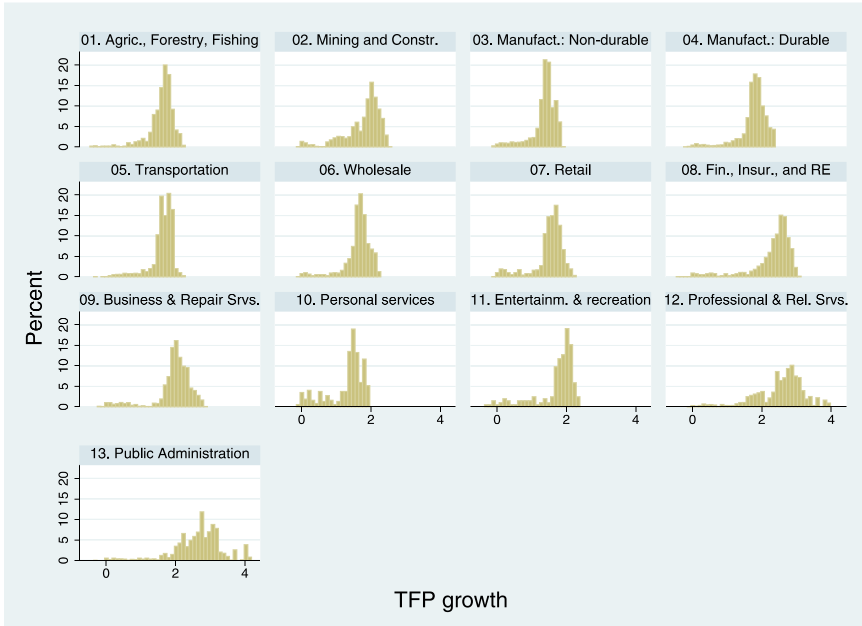

Fig. 2 Technical change faced by individuals across sectors

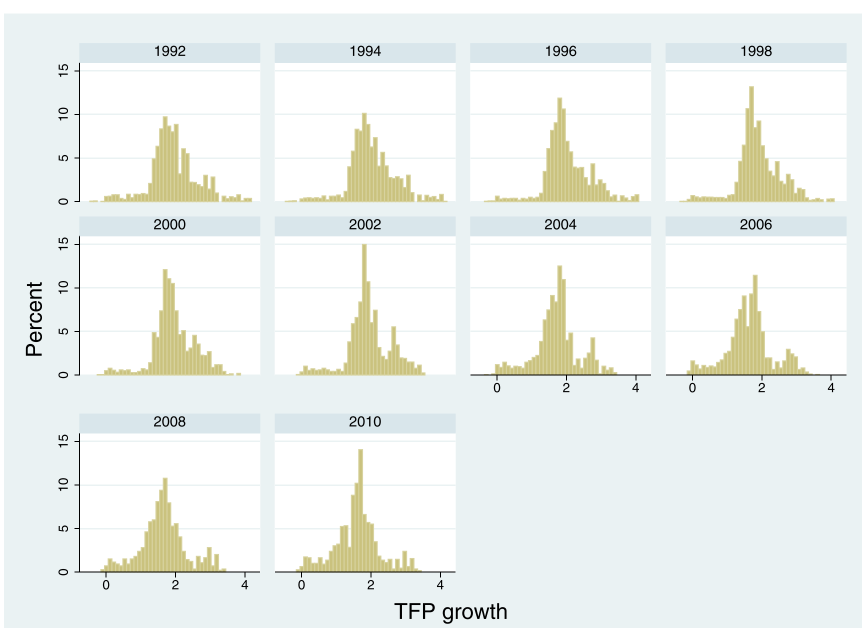

Fig. 3 Technical change faced by individuals across waves

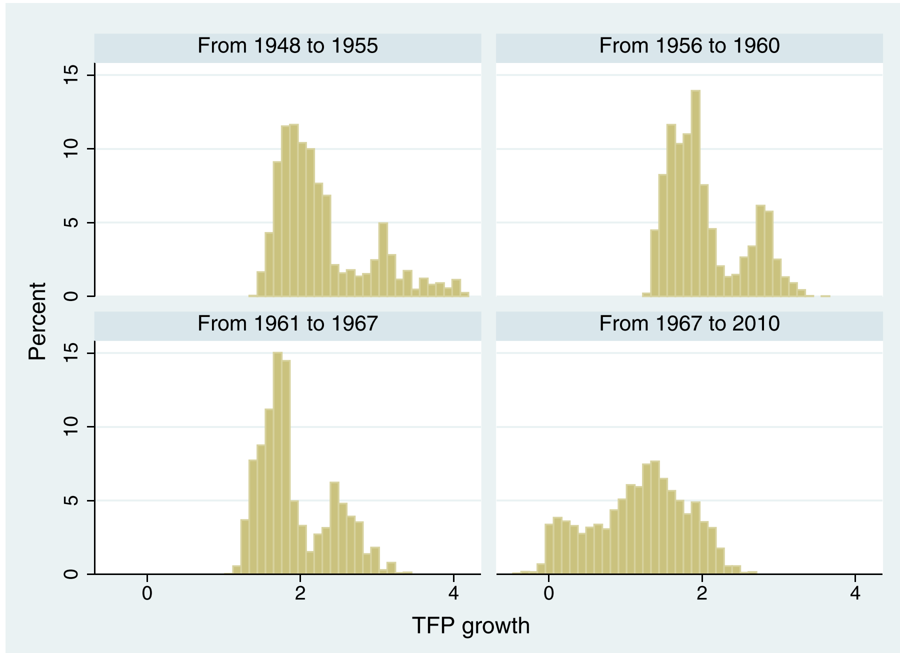

Fig. 4 Technical change faced by individuals across years of entry (quartiles of the distribution)

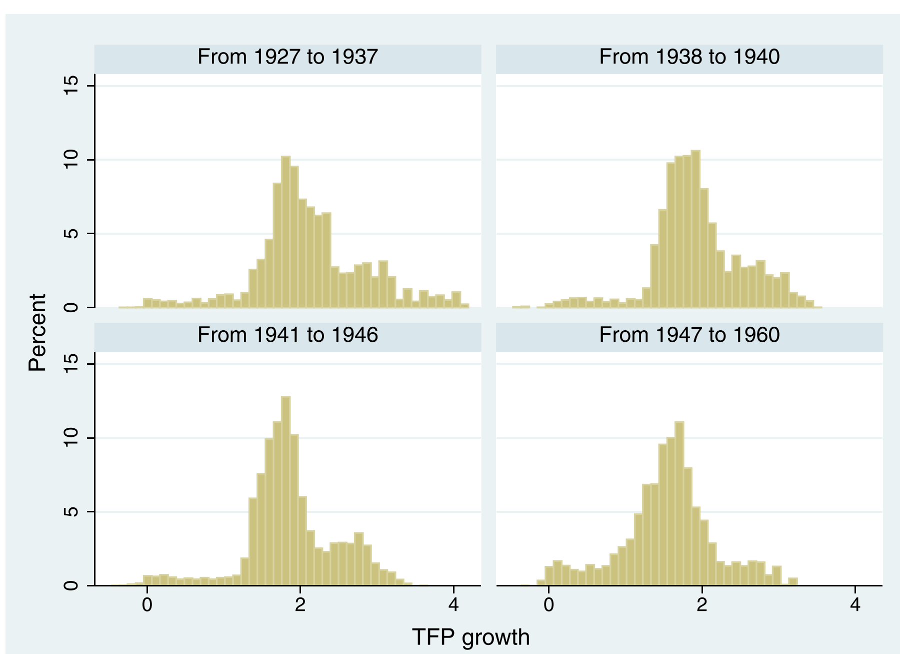

Fig. 5 Technical change faced by individuals across birth cohorts (quartiles of the distribution)

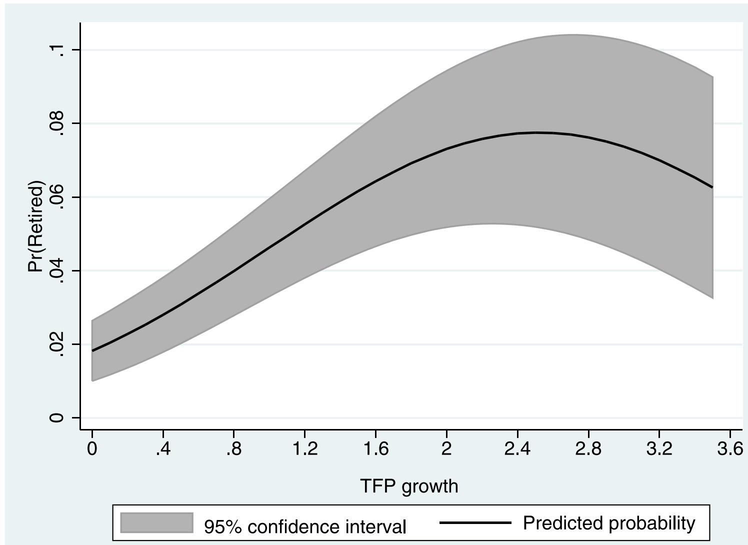

Fig. 6 Predicted probability of early retirement as a function of technical change based on the logit model of column (1) in Appendix B: Table 3

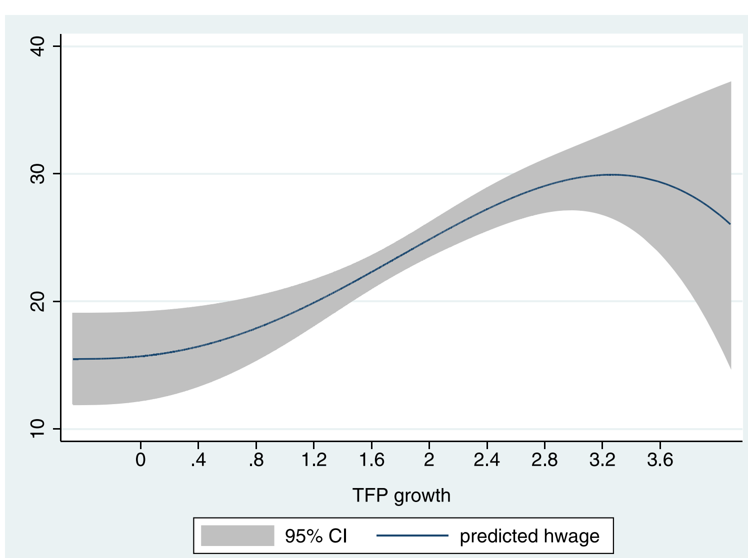

Fig. 7 Fractional-polynomial prediction of hourly wage of working individuals as a function of technical change

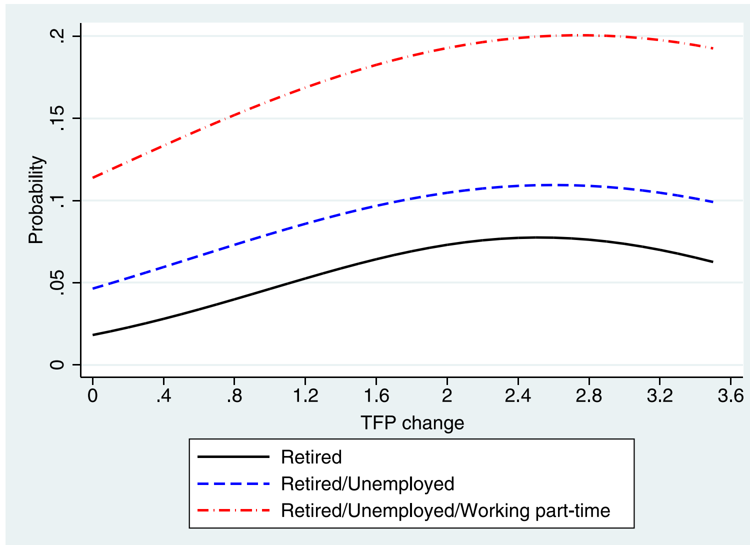

Fig. 8 Predicted probability of early retirement, unemployment, and working part-time as a function of technical change based on the logit models of column (1), column (2), and column (3) in Appendix B: Table 20

Appendix B: Tables

Table 1 Labor status by age group for 1992 and 2006

1992 (Wave 1)		Age group		
Labor status	50–54	55–59	60–64	Total
Working Full-Time	0.4014	0.3656	0.0804	0.8474
Working Part-Time	0.0286	0.0247	0.0047	0.0580
Unemployed	0.0144	0.0169	0.0032	0.0346
Retired	0.0135	0.0339	0.0127	0.0600
Total	0.4580	0.4412	0.1009	1
2006 (Wave 8)		Age group		
Labor status	50–54	55–59	60–64	Total
Working Full-Time	0.2439	0.3906	0.1812	0.8157
Working Part-Time	0.0142	0.0298	0.0149	0.0588
Unemployed	0.0080	0.0105	0.0032	0.0218
Retired	0.0085	0.0326	0.0627	0.1037
Total	0.2745	0.4635	0.2619	1

Notes: We use survey weights for each wave

Table 2 Summary statistics

Variable	Mean	Std. Dev.	Min.	Max.
Retired (d)	0.120	0.325	0	1
TFP change	1.884	0.664	–0.468	4.086
Age	57.440	3.694	50	64
Years of education	12.923	3.201	0	17
Health level	2.464	1.049	1	5
Birth year	1941.4	6.051	1927	1960
Race: Black (d)	0.122	0.328	0	1
Race: Hispanic (d)	0.103	0.304	0	1

Table 2 Summary statistics (*Continued*)

Foreign born (d)	0.109	0.312	0	1
Married (d)	0.828	0.377	0	1
Spouse working (d)	0.529	0.499	0	1
Private health insurance (d)	0.796	0.403	0	1
Government health insurance (d)	0.066	0.249	0	1
Net wealth (deflated, in $100,000s)	0.040	0.066	−0.045	2.548
Pension plan or pension income (d)	0.599	0.490	0	1
Sector experience	36.989	8.379	0	51
Training (d)	0.155	0.362	0	1
Residence: Midwest (d)	0.259	0.438	0	1
Residence: South (d)	0.402	0.490	0	1
Residence: West (d)	0.182	0.386	0	1
Occupation: Managers and professionals (d)	0.336	0.472	0	1
Occupation: Clerical and sales (d)	0.142	0.349	0	1
Occupation: Services (d)	0.066	0.249	0	1
Occupation: Farming, forestry, and fishing (d)	0.043	0.202	0	1
Occupation: Mechanics, constructors, operators (d)	0.395	0.489	0	1
Occupation: Armed forces (d)	0.019	0.136	0	1
Sector: Agriculture, forestry, fishing (d)	0.043	0.203	0	1
Sector: Mining and Construction (d)	0.109	0.312	0	1
Sector: Manufacturing Non-durable (d)	0.090	0.286	0	1
Sector: Manufacturing Durable (d)	0.168	0.374	0	1
Sector: Transportation (d)	0.105	0.307	0	1
Sector: Wholesale (d)	0.053	0.225	0	1
Sector: Retail (d)	0.081	0.272	0	1
Sector: Finance, Insurance, and Real Estate (d)	0.049	0.215	0	1
Sector: Business and Repair Services (d)	0.055	0.228	0	1
Sector: Personal services (d)	0.017	0.129	0	1
Sector: Entertainment and recreation (d)	0.009	0.096	0	1
Sector: Professional and related services (d)	0.144	0.351	0	1
Sector: Public Administration (d)	0.077	0.267	0	1
TFP change (last 5 years)	0.134	0.125	−0.126	0.602
Mean TFP change	0.051	0.018	0.005	0.136
Mean aggregate TFP change	0.050	0.012	0.009	0.137
Mean sector-specific TFP change	0.001	0.013	−0.021	0.035
Value added per worker change	1.153	1.375	−0.48	6.591

Notes: All variables have 21,856 non-missing observations, except for the mean TFP changes that have 21,853 observations. Mean TFP changes are winsorized at the 1-st and 99-th percentiles

Table 3 Benchmark regressions

	(1)	(2)	(3)	(4)	(5)
Dep. var.: Retired	Logit	OLS	Random efx	Fixed efx	Survival
TFP growth	0.075***	0.091***	0.122***	0.464***	0.059***
	(0.010)	(0.011)	(0.026)	(0.097)	(0.009)
TFP growth squared	−0.015***	−0.019***	−0.029***	−0.033*	−0.013***
	(0.002)	(0.003)	(0.008)	(0.017)	(0.002)
Married (d)	0.009**	0.017***	0.026***	0.014	0.007*
	(0.004)	(0.006)	(0.007)	(0.011)	(0.004)
Spouse working (d)	−0.042***	−0.057***	−0.065***	−0.106***	−0.037***
	(0.004)	(0.005)	(0.006)	(0.009)	(0.004)
Emp. health ins. (d)	−0.001	0.002	−0.038***	−0.092***	−0.000

Table 3 Benchmark regressions (*Continued*)

	(0.005)	(0.008)	(0.012)	(0.021)	(0.004)
Gov. health ins. (d)	0.127***	0.185***	0.175***	0.195***	0.103***
	(0.013)	(0.016)	(0.028)	(0.033)	(0.010)
Wealth	0.062***	0.061*	0.073	0.258**	0.048**
	(0.023)	(0.035)	(0.070)	(0.111)	(0.020)
Pension (d)	−0.079***	−0.098***	−0.159***	−0.210***	−0.070***
	(0.009)	(0.011)	(0.022)	(0.022)	(0.008)
Very good health (d)	0.014***	0.014***	0.006	0.013**	0.014***
	(0.005)	(0.005)	(0.004)	(0.005)	(0.005)
Good health (d)	0.026***	0.027***	0.016***	0.031***	0.025***
	(0.005)	(0.005)	(0.006)	(0.010)	(0.005)
Fair health (d)	0.076***	0.082***	0.069***	0.087***	0.066***
	(0.010)	(0.008)	(0.007)	(0.012)	(0.009)
Poor health (d)	0.279***	0.274***	0.250***	0.258***	0.213***
	(0.026)	(0.016)	(0.015)	(0.020)	(0.024)
(Pseudo) R-squared	(0.239)	0.191	0.183	0.558	
Within			0.284		
Between			0.162		
Observations	21,856	21,856	21,856	21,856	21,856

Notes: All models except column (4) include race, foreign-born, geographical, education, occupation, age, cohort, and sector dummies, as well as controls for the unemployment rate in the survey year and the sector experience. The symbol (d) indicates discrete change of dummy variable from 0 to 1. Statistical significance is represented by * for $p < 0.10$, ** for $p < 0.05$, and *** for $p < 0.01$. Standard errors are clustered at the sector-wave level (at the sector level in Model (3) with random effects and Model (4) with fixed effects). Column (1) and column (5) report the marginal effects of logit and complementary log-log regressions. The individual fixed effects of column (4) replace all time-invariant dummies and impose the exclusion of the age dummies because of the collinearity with sector experience

Table 4 Robustness on different measures of technical change

Dep. var.: Retired	(1) Benchmark	(2) 5-years	(3) Mean growth	(4) Mean Sect. & Aggr.	(5) VA/workers
TFP growth	0.075***				
	(0.010)				
TFP growth squared	−0.015***				
	(0.002)				
TFP growth (last 5 years)		0.126			
		(0.078)			
TFP growth (last 5 years) sq		−0.186			
		(0.187)			
Mean TFP growth			5.309***		
			(0.674)		
Mean TFP growth sq			−36.559***		
			(4.636)		
Mean sector-specific TFP growth				1.188**	
				(0.551)	
Mean sector-specific TFP growth sq				−12.756	
				(16.159)	
Mean aggregate TFP growth				9.052***	
				(1.079)	
Mean aggregate TFP growth sq				−60.109***	
				(7.797)	

Table 4 Robustness on different measures of technical change (*Continued*)

VA per worker growth					0.034***
					(0.010)
VA per worker growth sq					–0.007***
					(0.002)
Pseudo R-squared	0.239	0.238	0.244	0.249	0.238
Observations	21,856	21,856	21,853	21,853	21,856

Notes: All models have the same specification of column (1) in Table 3 except for the measure of technical change. Statistical significance is represented by * for $p < 0.10$, ** for $p < 0.05$, and *** for $p < 0.01$. Standard errors are clustered at the sector-wave level. All models report the marginal effects of logit regressions. Column (3) and column (4) report 21,853 observations because three observations correspond to a nil sector experience. Mean TFP changes are winsorized at the 1-st and 99-th percentiles

Table 5 Robustness on measures of technical change with exogenous entry

	(1)	(2)	(3)
Dep. var.: Retired	Benchmark FE	End of educ.	21 years old
TFP growth	0.464***		
	(0.097)		
TFP growth squared	–0.033*		
	(0.017)		
TFP growth (from end of educ.)		0.529**	
		(0.175)	
TFP growth (from end of educ.) sq		–0.048	
		(0.032)	
TFP growth (from 21 yo)			0.711***
			(0.162)
TFP growth (from 21 yo) squared			–0.090**
			(0.032)
Married	0.014	0.017	0.014
	(0.011)	(0.012)	(0.011)
Spouse working	–0.106***	–0.107***	–0.107***
	(0.009)	(0.009)	(0.009)
Emp. health ins.	–0.092***	–0.091***	–0.092***
	(0.021)	(0.021)	(0.021)
Gov. health ins.	0.195***	0.192***	0.195***
	(0.033)	(0.032)	(0.033)
Wealth	0.258**	0.253**	0.261**
	(0.111)	(0.112)	(0.111)
Pension	–0.210***	–0.210***	–0.210***
	(0.022)	(0.022)	(0.022)
Very good health (d)	0.013**	0.013**	0.013**
	(0.005)	(0.005)	(0.005)
Good health (d)	0.031***	0.031***	0.031***
	(0.010)	(0.009)	(0.010)
Fair health (d)	0.087***	0.090***	0.087***
	(0.012)	(0.012)	(0.012)
Poor health (d)	0.258***	0.273***	0.257***
	(0.020)	(0.020)	(0.020)
R-squared	0.558	0.547	0.558
Observations	21,856	21,183	21,856

Notes: All models include individual fixed effects and geographical dummies, as well as controls for the unemployment rate in the survey year and the sector experience. Statistical significance is represented by * for $p < 0.10$, ** for $p < 0.05$, and *** for $p < 0.01$. Standard errors are clustered at the sector level. The lower number of observations in column (2) is due to a few individuals in our sample that end their education before 1948, that is, before the year from which we are able to compute the TFP growth rates

Table 6 Robustness on different-order polynomials

Dep. var.: Retired	(1) Benchmark	(2) Linear	(3) Order 3
TFP growth	0.075***	0.017***	0.093***
	(0.010)	(0.005)	(0.023)
TFP growth squared	−0.015***		−0.026**
	(0.002)		(0.013)
TFP growth cube			0.002
			(0.002)
Married (d)	0.009**	0.009**	0.009**
	(0.004)	(0.004)	(0.004)
Spouse working (d)	−0.042***	−0.042***	−0.042***
	(0.004)	(0.004)	(0.004)
Emp. health ins. (d)	−0.001	−0.000	−0.001
	(0.005)	(0.005)	(0.005)
Gov. health ins. (d)	0.127***	0.127***	0.127***
	(0.013)	(0.013)	(0.013)
Wealth	0.062***	0.066***	0.061***
	(0.023)	(0.023)	(0.023)
Pension (d)	−0.079***	−0.078***	−0.079***
	(0.009)	(0.009)	(0.009)
Very good health (d)	0.014***	0.015***	0.014***
	(0.005)	(0.005)	(0.005)
Good health (d)	0.026***	0.026***	0.026***
	(0.005)	(0.005)	(0.005)
Fair health (d)	0.076***	0.077***	0.076***
	(0.010)	(0.010)	(0.010)
Poor health (d)	0.279***	0.279***	0.279***
	(0.026)	(0.026)	(0.026)
Pseudo R-squared	0.239	0.237	0.239
Observations	21,856	21,856	21,856

Notes: All models include race, foreign-born, geographical, education, occupation, age, cohort, and sector dummies, as well as controls for the unemployment rate in the survey year and the sector experience. Statistical significance is represented by * for $p < 0.10$, ** for $p < 0.05$, and *** for $p < 0.01$. Standard errors are clustered at the sector-wave level. All models report the marginal effects of logit regressions

Table 7 Robustness on different subsamples

Dep. var.: Retired	(1) Benchmark	(2) Only positive shocks	(3) W/o crisis years
TFP growth	0.075***	0.081***	0.069***
	(0.010)	(0.010)	(0.011)
TFP growth squared	−0.015***	−0.016***	−0.014***
	(0.002)	(0.002)	(0.002)
Married (d)	0.009**	0.009**	0.009**
	(0.004)	(0.004)	(0.004)
Spouse working (d)	−0.042***	−0.042***	−0.041***
	(0.004)	(0.004)	(0.004)
Emp. health ins. (d)	−0.001	−0.000	−0.004
	(0.005)	(0.005)	(0.005)
Gov. health ins. (d)	0.127***	0.126***	0.124***
	(0.013)	(0.013)	(0.013)

Table 7 Robustness on different subsamples (*Continued*)

Wealth	0.062***	0.062***	0.043*
	(0.023)	(0.023)	(0.026)
Pension (d)	−0.079***	−0.079***	−0.075***
	(0.009)	(0.009)	(0.010)
Very good health (d)	0.014***	0.014***	0.013***
	(0.005)	(0.005)	(0.005)
Good health (d)	0.026***	0.026***	0.024***
	(0.005)	(0.005)	(0.006)
Fair health (d)	0.076***	0.077***	0.079***
	(0.010)	(0.010)	(0.011)
Poor health (d)	0.279***	0.280***	0.280***
	(0.026)	(0.026)	(0.029)
Pseudo R-squared	0.239	0.239	0.242
Observations	21,856	21,757	19,373

Notes: All models include race, foreign-born, geographical, education, occupation, age, cohort, and sector dummies, as well as controls for the unemployment rate in the survey year and the sector experience. Statistical significance is represented by * for $p < 0.10$, ** for $p < 0.05$, and *** for $p < 0.01$. Standard errors are clustered at the sector-wave level. All models report the marginal effects of logit regressions. Column (2) has less observations due to unreported years of schooling

Table 8 Robustness on sector and occupation switchers

	(1)	(2)	(3)
Dep. var.: Retired	Benchmark	W/o sector switch	W/o occup. switch
TFP growth	0.075***	0.256***	0.125***
	(0.010)	(0.029)	(0.015)
TFP growth squared	−0.015***	−0.036***	−0.021***
	(0.002)	(0.004)	(0.003)
Married (d)	0.009**	0.014***	0.011***
	(0.004)	(0.004)	(0.004)
Spouse working (d)	−0.042***	−0.043***	−0.043***
	(0.004)	(0.004)	(0.004)
Emp. health ins. (d)	−0.001	−0.001	−0.000
	(0.005)	(0.006)	(0.006)
Gov. health ins. (d)	0.127***	0.129***	0.131***
	(0.013)	(0.014)	(0.013)
Wealth	0.062***	0.077***	0.104***
	(0.023)	(0.028)	(0.028)
Pension (d)	−0.079***	−0.091***	−0.092***
	(0.009)	(0.010)	(0.010)
Very good health (d)	0.014***	0.015***	0.017***
	(0.005)	(0.005)	(0.005)
Good health (d)	0.026***	0.026***	0.027***
	(0.005)	(0.006)	(0.006)
Fair health (d)	0.076***	0.079***	0.080***
	(0.010)	(0.011)	(0.011)
Poor health (d)	0.279***	0.295***	0.290***
	(0.026)	(0.026)	(0.028)
Pseudo R-squared	0.239	0.254	0.249
Observations	21,856	19,404	19,638

Notes: All models include race, foreign-born, geographical, education, occupation, age, cohort, and sector dummies, as well as controls for the unemployment rate in the survey year and the sector experience. Statistical significance is represented by * for $p < 0.10$, ** for $p < 0.05$, and *** for $p < 0.01$. Standard errors are clustered at the sector-wave level. All models report the marginal effects of logit regressions

Table 9 Additional robustness checks

Dep. var.: Retired	(1) Benchmark	(2) Survey weights	(3) Winsorized
TFP growth	0.075***	0.064***	
	(0.010)	(0.021)	
TFP growth squared	−0.015***	−0.014***	
	(0.002)	(0.005)	
Win. TFP growth			0.078***
			(0.010)
Win. TFP growth squared			−0.015***
			(0.002)
Married (d)	0.009**	0.008	0.009**
	(0.004)	(0.007)	(0.004)
Spouse working (d)	−0.042***	−0.054***	−0.042***
	(0.004)	(0.005)	(0.004)
Emp. health ins. (d)	−0.001	−0.005	−0.001
	(0.005)	(0.007)	(0.005)
Gov. health ins. (d)	0.127***	0.128***	0.127***
	(0.013)	(0.019)	(0.013)
Wealth	0.062***	0.039	0.062***
	(0.023)	(0.049)	(0.023)
Pension (d)	−0.079***	−0.086***	−0.079***
	(0.009)	(0.009)	(0.009)
Very good health (d)	0.014***	0.008	0.014***
	(0.005)	(0.009)	(0.005)
Good health (d)	0.026***	0.024**	0.026***
	(0.005)	(0.009)	(0.005)
Fair health (d)	0.076***	0.085***	0.076***
	(0.010)	(0.018)	(0.010)
Poor health (d)	0.279***	0.309***	0.279***
	(0.026)	(0.044)	(0.026)
Pseudo R-squared	0.239		0.239
Observations	21,856	13,539	21,856

Notes: All models include race, foreign-born, geographical, education, occupation, age, cohort, and sector dummies, as well as controls for the unemployment rate in the survey year and the sector experience. Statistical significance is represented by * for $p < 0.10$, ** for $p < 0.05$, and *** for $p < 0.01$. Standard errors are clustered at the sector-wave level. All models report the marginal effects of logit regressions

Table 10 Additional robustness checks

Dep. var.: Retired	(1) Benchmark	(2) Year-by-year cohort	(3) Lagged controls
TFP growth	0.075***	0.075***	0.059***
	(0.010)	(0.010)	(0.015)
TFP growth squared	−0.015***	−0.015***	−0.015***
	(0.002)	(0.002)	(0.004)
Married (d)	0.009**	0.009**	−0.008
	(0.004)	(0.004)	(0.006)
Spouse working (d)	−0.042***	−0.042***	−0.027***
	(0.004)	(0.004)	(0.005)
Emp. health ins. (d)	−0.001	−0.001	0.002
	(0.005)	(0.005)	(0.007)

Table 10 Additional robustness checks (*Continued*)

Gov. health ins. (d)	0.127***	0.128***	0.072***
	(0.013)	(0.013)	(0.016)
Wealth	0.062***	0.062***	0.043
	(0.023)	(0.024)	(0.036)
Pension (d)	−0.079***	−0.079***	0.002
	(0.009)	(0.009)	(0.006)
Very good health (d)	0.014***	0.014***	0.024***
	(0.005)	(0.005)	(0.007)
Good health (d)	0.026***	0.025***	0.041***
	(0.005)	(0.005)	(0.008)
Fair health (d)	0.076***	0.075***	0.095***
	(0.010)	(0.010)	(0.014)
Poor health (d)	0.279***	0.277***	0.264***
	(0.026)	(0.026)	(0.036)
Pseudo R-squared	0.239	0.241	0.153
Observations	21,856	21,826	15,693

Notes: All models include race, foreign-born, geographical, education, occupation, age, cohort, and sector dummies, as well as controls for the unemployment rate in the survey year and the sector experience. Statistical significance is represented by * for $p < 0.10$, ** for $p < 0.05$, and *** for $p < 0.01$. Standard errors are clustered at the sector-wave level. All models report the marginal effects of logit regressions. Column (2) includes year-by-year cohort dummies instead of 5 years-by-5 years cohort dummies that we use in the benchmark regression. Column (3)'s time-varying covariates are all lagged by one period except for TFP growth and the unemployment rate

Table 11 Heterogeneity by education level

Dep. var.: Retired	(1) Benchmark	(2) College graduates	(3) Others
TFP growth	0.075***	0.080***	0.082***
	(0.010)	(0.018)	(0.013)
TFP growth squared	−0.015***	−0.014***	−0.017***
	(0.002)	(0.003)	(0.003)
Married (d)	0.009**	0.012**	0.009
	(0.004)	(0.005)	(0.005)
Spouse working (d)	−0.042***	−0.049***	−0.040***
	(0.004)	(0.007)	(0.004)
Emp. health ins. (d)	−0.001	0.002	−0.001
	(0.005)	(0.008)	(0.006)
Gov. health ins. (d)	0.127***	0.058***	0.151***
	(0.013)	(0.020)	(0.016)
Wealth	0.062***	0.023	0.104***
	(0.023)	(0.025)	(0.037)
Pension (d)	−0.079***	−0.083***	−0.077***
	(0.009)	(0.012)	(0.010)
Very good health (d)	0.014***	0.020***	0.009
	(0.005)	(0.006)	(0.006)
Good health (d)	0.026***	0.045***	0.015**
	(0.005)	(0.009)	(0.006)
Fair health (d)	0.076***	0.105***	0.067***
	(0.010)	(0.024)	(0.011)
Poor health (d)	0.279***	0.238***	0.275***
	(0.026)	(0.077)	(0.028)
Pseudo R-squared	0.239	0.209	0.254
Observations	21,856	5,985	15,850

Notes: All models include race, foreign-born, geographical, education, occupation, age, cohort, and sector dummies, as well as controls for the unemployment rate in the survey year and the sector experience. Statistical significance is represented by * for $p < 0.10$, ** for $p < 0.05$, and *** for $p < 0.01$. Standard errors are clustered at the sector-wave level. All models report the marginal effects of logit regressions

Table 12 Heterogeneity by occupation

Dep. var.: Retired	(1) Benchmark	(2) Managers and professionals	(3) Others
TFP growth	0.075***	0.070***	0.064***
	(0.010)	(0.024)	(0.013)
TFP growth squared	−0.015***	−0.013***	−0.012***
	(0.002)	(0.005)	(0.003)
Married (d)	0.009**	0.010**	0.007
	(0.004)	(0.005)	(0.005)
Spouse working (d)	−0.042***	−0.045***	−0.035***
	(0.004)	(0.013)	(0.005)
Emp. health ins. (d)	−0.001	0.009	−0.007
	(0.005)	(0.005)	(0.006)
Gov. health ins. (d)	0.127***	0.069***	0.149***
	(0.013)	(0.023)	(0.016)
Wealth	0.062***	0.021	0.097***
	(0.023)	(0.020)	(0.035)
Pension (d)	−0.079***	−0.069***	−0.076***
	(0.009)	(0.020)	(0.009)
Very good health (d)	0.014***	0.015**	0.010
	(0.005)	(0.006)	(0.007)
Good health (d)	0.026***	0.030***	0.019***
	(0.005)	(0.011)	(0.007)
Fair health (d)	0.076***	0.067***	0.073***
	(0.010)	(0.023)	(0.012)
Poor health (d)	0.279***	0.225***	0.288***
	(0.026)	(0.072)	(0.028)
Pseudo R-squared	0.239	0.216	0.260
Observations	21,856	7,310	14,519

Notes: All models include race, foreign-born, geographical, education, occupation, age, cohort, and sector dummies, as well as controls for the unemployment rate in the survey year and the sector experience. Statistical significance is represented by * for $p < 0.10$, ** for $p < 0.05$, and *** for $p < 0.01$. Standard errors are clustered at the sector-wave level. All models report the marginal effects of logit regressions

Table 13 Heterogeneity by sector

Dep. var.: Retired	(1) Benchmark	(2) Manufactg.	(3) Prof. svcs.	(4) P.A.	(5) Rest
TFP growth	0.075***	0.185***	0.040*	0.184***	0.055***
	(0.010)	(0.055)	(0.024)	(0.046)	(0.015)
TFP growth squared	−0.015***	−0.036*	−0.005	−0.033***	−0.015***
	(0.002)	(0.022)	(0.005)	(0.009)	(0.006)
Married (d)	0.009**	0.017*	0.000	0.032**	0.005
	(0.004)	(0.009)	(0.006)	(0.014)	(0.005)
Spouse working (d)	−0.042***	−0.052***	−0.031***	−0.068***	−0.037***
	(0.004)	(0.009)	(0.010)	(0.022)	(0.004)
Emp. health ins. (d)	−0.001	−0.014	0.000	0.003	0.003
	(0.005)	(0.013)	(0.007)	(0.022)	(0.006)
Gov. health ins. (d)	0.127***	0.226***	0.060***	0.059	0.124***
	(0.013)	(0.033)	(0.017)	(0.041)	(0.015)
Wealth	0.062***	0.210***	−0.019	0.305*	0.038
	(0.023)	(0.072)	(0.036)	(0.160)	(0.029)

Table 13 Heterogeneity by sector (*Continued*)

Pension (d)	−0.079***	−0.137***	−0.062***	−0.311***	-0.041***
	(0.009)	(0.023)	(0.019)	(0.046)	(0.009)
Very good health (d)	0.014***	0.023*	0.011	−0.004	0.012*
	(0.005)	(0.014)	(0.008)	(0.015)	(0.006)
Good health (d)	0.026***	0.033**	0.025***	0.021	0.019***
	(0.005)	(0.016)	(0.008)	(0.015)	(0.007)
Fair health (d)	0.076***	0.081***	0.076***	0.062*	0.075***
	(0.010)	(0.024)	(0.021)	(0.034)	(0.014)
Poor health (d)	0.279***	0.213***	0.159***	0.331***	0.322***
	(0.026)	(0.052)	(0.044)	(0.097)	(0.036)
Pseudo R-squared	0.239	0.273	0.167	0.280	0.253
Observations	21,856	5,612	3,034	1,685	11,389

Notes: All models include race, foreign-born, geographical, education, occupation, age, and cohort dummies, as well as controls for the unemployment rate in the survey year and the sector experience. We leave sector dummies only in column (1), column (2), and column (5). Statistical significance is represented by * for $p < 0.10$, ** for $p < 0.05$, and *** for $p < 0.01$. Standard errors are clustered at the sector-wave level in column (1), column (2), and column (5), and at the wave level in column (3) and column (4). All models report the marginal effects of logit regressions

Table 14 Heterogeneity by marital status

	(1)	(2)	(3)
Dep. var.: Retired	Benchmark	Married	Unmarried
TFP growth	0.075***	0.070***	0.106***
	(0.010)	(0.011)	(0.025)
TFP growth squared	−0.015***	−0.014***	−0.019***
	(0.002)	(0.003)	(0.005)
Married (d)	0.009**		
	(0.004)		
Spouse working (d)	−0.042***	−0.045***	
	(0.004)	(0.004)	
Emp. health ins. (d)	−0.001	0.010**	−0.050***
	(0.005)	(0.004)	(0.013)
Gov. health ins. (d)	0.127***	0.130***	0.120***
	(0.013)	(0.016)	(0.023)
Wealth	0.062***	0.040	0.138**
	(0.023)	(0.026)	(0.068)
Pension (d)	−0.079***	−0.083***	−0.059***
	(0.009)	(0.009)	(0.013)
Very good health (d)	0.014***	0.010**	0.040**
	(0.005)	(0.005)	(0.020)
Good health (d)	0.026***	0.019***	0.063***
	(0.005)	(0.005)	(0.022)
Fair health (d)	0.076***	0.070***	0.101***
	(0.010)	(0.009)	(0.034)
Poor health (d)	0.279***	0.268***	0.311***
	(0.026)	(0.030)	(0.062)
Pseudo R-squared	0.239	0.240	0.262
Observations	21,856	18,094	3,762

Notes: All models include race, foreign-born, geographical, education, occupation, age, cohort, and sector dummies, as well as controls for the unemployment rate in the survey year and the sector experience. Statistical significance is represented by * for $p < 0.10$, ** for $p < 0.05$, and *** for $p < 0.01$. Standard errors are clustered at the sector-wave level. All models report the marginal effects of logit regressions

Table 15 Heterogeneity by cohort

Dep. var.: Retired	(1) Benchmark	(2) Pre-war cohort	(3) Post-war cohort
TFP growth	0.075***	0.084***	0.070***
	(0.010)	(0.018)	(0.011)
TFP growth squared	−0.015***	−0.018***	−0.013***
	(0.002)	(0.003)	(0.003)
Married (d)	0.009**	0.006	0.012***
	(0.004)	(0.008)	(0.004)
Spouse working (d)	−0.042***	−0.053***	−0.033***
	(0.004)	(0.006)	(0.005)
Emp. health ins. (d)	−0.001	−0.008	0.005
	(0.005)	(0.010)	(0.005)
Gov. health ins. (d)	0.127***	0.155***	0.105***
	(0.013)	(0.019)	(0.017)
Wealth	0.062***	0.037	0.059***
	(0.023)	(0.040)	(0.018)
Pension (d)	−0.079***	−0.093***	−0.067***
	(0.009)	(0.017)	(0.006)
Very good health (d)	0.014***	0.011	0.019***
	(0.005)	(0.008)	(0.006)
Good health (d)	0.026***	0.025***	0.028***
	(0.005)	(0.008)	(0.007)
Fair health (d)	0.076***	0.092***	0.065***
	(0.010)	(0.014)	(0.013)
Poor health (d)	0.279***	0.325***	0.237***
	(0.026)	(0.041)	(0.035)
Pseudo R-squared	0.239	0.229	0.240
Observations	21,856	11,088	10,768

Notes: All models include race, foreign-born, geographical, education, occupation, age, cohort, and sector dummies, as well as controls for the unemployment rate in the survey year and the sector experience. Statistical significance is represented by * for $p < 0.10$, ** for $p < 0.05$, and *** for $p < 0.01$. Standard errors are clustered at the sector-wave level. All models report the marginal effects of logit regressions

Table 16 Heterogeneity by age group

Dep. var.: Retired	(1) Benchmark	(2) Age 50–54	(3) Age 55–59	(4) Age 60–64
TFP growth	0.075***	0.024***	0.064***	0.148***
	(0.010)	(0.008)	(0.013)	(0.033)
TFP growth squared	−0.015***	−0.004*	−0.012***	−0.029***
	(0.002)	(0.002)	(0.003)	(0.008)
Married (d)	0.009**	0.002	0.012**	0.013
	(0.004)	(0.002)	(0.005)	(0.013)
Spouse working (d)	−0.042***	−0.006**	−0.032***	−0.107***
	(0.004)	(0.002)	(0.005)	(0.010)
Emp. health ins. (d)	-0.001	0.002	0.011**	−0.028*
	(0.005)	(0.002)	(0.005)	(0.016)
Gov. health ins. (d)	0.127***	0.047***	0.140***	0.198***
	(0.013)	(0.018)	(0.023)	(0.026)
Wealth	0.062***	0.025***	0.128***	-0.054
	(0.023)	(0.008)	(0.031)	(0.074)

Table 16 Heterogeneity by age group (*Continued*)

Pension (d)	−0.079***	−0.034***	−0.082***	−0.130***
	(0.009)	(0.006)	(0.011)	(0.016)
Very good health (d)	0.014***	0.009*	0.013**	0.025*
	(0.005)	(0.005)	(0.006)	(0.015)
Good health (d)	0.026***	0.009*	0.024***	0.049***
	(0.005)	(0.005)	(0.007)	(0.015)
Fair health (d)	0.076***	0.055***	0.059***	0.132***
	(0.010)	(0.018)	(0.012)	(0.023)
Poor health (d)	0.279***	0.150***	0.320***	0.328***
	(0.026)	(0.047)	(0.036)	(0.045)
Pseudo R-squared	0.239	0.272	0.179	0.171
Observations	21,856	5,304	9,528	7,014

Notes: All models include race, foreign-born, geographical, education, occupation, age, cohort, and sector dummies, as well as controls for the unemployment rate in the survey year and the sector experience. Statistical significance is represented by * for $p < 0.10$, ** for $p < 0.05$, and *** for $p < 0.01$. Standard errors are clustered at the sector-wave level. All models report the marginal effects of logit regressions

Table 17 Heterogeneity by gender

	(1)	(2)	(3)
Dep. var.: Retired	Benchmark	Only females	Both genders
TFP growth	0.075***	0.012	0.029***
	(0.010)	(0.010)	(0.008)
TFP growth squared	−0.015***	0.006***	−0.001
	(0.002)	(0.002)	(0.002)
Male (d)			−0.036***
			(0.004)
Married (d)	0.009**	0.063***	0.041***
	(0.004)	(0.004)	((0.003)
Spouse working (d)	−0.042***	−0.062***	−0.054***
	(0.004)	(0.005)	(0.004)
Emp. health ins. (d)	−0.001	−0.018***	−0.011***
	(0.005)	(0.005)	(0.004)
Gov. health ins. (d)	0.127***	0.143***	0.155***
	(0.013)	(0.012)	(0.010)
Wealth	0.062***	0.269***	0.173***
	(0.023)	(0.045)	(0.032)
Pension (d)	−0.079***	−0.200***	−0.157***
	(0.009)	(0.006)	(0.006)
Very good health (d)	0.014***	0.006	0.012***
	(0.005)	(0.005)	((0.004)
Good health (d)	0.026***	0.022***	0.029***
	(0.005)	(0.006)	(0.004)
Fair health (d)	0.076***	0.105***	0.107***
	(0.010)	(0.011)	(0.008)
Poor health (d)	0.279***	0.389***	0.374***
	(0.026)	(0.023)	(0.016)
Pseudo R-squared	0.239	0.246	0.232
Observations	21,856	24,859	46,715

Notes: All models include race, foreign-born, geographical, education, occupation, age, cohort, and sector dummies, as well as controls for the unemployment rate in the survey year and the sector experience. Statistical significance is represented by * for $p < 0.10$, ** for $p < 0.05$, and *** for $p < 0.01$. Standard errors are clustered at the sector-wave level. All models report the marginal effects of logit regressions

Table 18 Heterogeneity by sector experience

Dep. var.: Retired	(1) Benchmark	(2) Quartiles	(3) Short exp.	(4) Long exp.
TFP growth	0.075***	0.050***	0.026***	0.097***
	(0.010)	(0.009)	(0.009)	(0.034)
TFP growth squared	−0.015***	−0.009***	−0.004	−0.012**
	(0.002)	(0.002)	(0.003)	(0.006)
Sector experience	−0.001***			
	(0.000)			
Sect. exp. 2nd q (d)		0.015**	0.010***	
		(0.006)	(0.004)	
Sect. exp. 3rd q (d)		−0.008		
		(0.006)		
Sect. exp. 4th q (d)		−0.031***		−0.055***
		(0.006)		(0.009)
Married (d)	0.009**	0.010***	0.005	0.016**
	(0.004)	(0.004)	(0.004)	(0.007)
Spouse working (d)	−0.042***	−0.041***	−0.022***	−0.064***
	(0.004)	(0.004)	(0.004)	(0.006)
Emp. health ins. (d)	−0.001	−0.001	−0.003	0.001
	(0.005)	(0.005)	(0.004)	(0.009)
Gov. health ins. (d)	0.127***	0.127***	0.122***	0.131***
	(0.013)	(0.013)	(0.018)	(0.019)
Wealth	0.062***	0.054**	0.047**	0.053
	(0.023)	(0.023)	(0.022)	(0.043)
Pension (d)	−0.079***	−0.078***	−0.063***	−0.097***
	(0.009)	(0.009)	(0.009)	(0.012)
Bad health 2/5 (d)	0.014***	0.014***	0.017***	0.011
	(0.005)	(0.005)	(0.006)	(0.009)
Bad health 3/5 (d)	0.026***	0.025***	0.025***	0.026***
	(0.005)	(0.005)	(0.007)	(0.008)
Bad health 4/5 (d)	0.076***	0.075***	0.086***	0.067***
	(0.010)	(0.010)	(0.014)	(0.013)
Bad health 5/5 (d)	0.279***	0.276***	0.290***	0.262***
	(0.026)	(0.027)	(0.036)	(0.038)
Pseudo R-squared	0.239	0.244	0.270	0.220
Observations	21856	21856	10925	10684

Notes: All models include race, foreign-born, geographical, education, occupation, age, cohort, and sector dummies, as well as a control for the unemployment rate in the survey year. Statistical significance is represented by * for $p < 0.10$, ** for $p < 0.05$, and *** for $p < 0.01$. Standard errors are clustered at the sector-wave level. All models report the marginal effects of logit regressions

Table 19 Heterogeneity by training

Dep. var.: Retired	(1) Benchmark	(2) With training	(3) W/o training
TFP growth	0.075***	0.044	0.078***
	(0.010)	(0.041)	(0.009)
TFP growth squared	−0.015***	−0.005	−0.016***
	(0.002)	(0.008)	(0.002)
Married (d)	0.009**	0.025**	0.007*
	(0.004)	(0.011)	(0.004)
Spouse working (d)	−0.042***	−0.047***	−0.041***

Table 19 Heterogeneity by training (*Continued*)

	(0.004)	(0.011)	(0.004)
Emp. health ins. (d)	−0.001	0.006	−0.002
	(0.005)	(0.014)	(0.005)
Gov. health ins. (d)	0.127***	0.132***	0.126***
	(0.013)	(0.035)	(0.013)
Wealth	0.062***	0.080	0.056**
	(0.023)	(0.077)	(0.023)
Pension (d)	−0.079***	−0.097***	−0.077***
	(0.009)	(0.021)	(0.008)
Very good health (d)	0.014***	0.007	0.015***
	(0.005)	(0.015)	(0.005)
Good health (d)	0.026***	0.021	0.027***
	(0.005)	(0.016)	(0.006)
Fair health (d)	0.076***	0.085***	0.076***
	(0.010)	(0.027)	(0.011)
Poor health (d)	0.279***	0.440***	0.255***
	(0.026)	(0.077)	(0.026)
Pseudo R-squared	0.239	0.276	0.235
Observations	21,856	3,390	18,462

Notes: All models include race, foreign-born, geographical, education, occupation, age, cohort, and sector dummies, as well as controls for the unemployment rate in the survey year and the sector experience. Statistical significance is represented by * for $p < 0.10$, ** for $p < 0.05$, and *** for $p < 0.01$. Standard errors are clustered at the sector-wave level. All models report the marginal effects of logit regressions

Table 20 Effect of technical change on different labor statuses

	(1)	(2)	(3)
Dep. var.:	Retired	Retired/Unemp.	Retired/Unemp./Part-time
TFP growth	0.075***	0.061***	0.066***
	(0.010)	(0.012)	(0.016)
TFP growth squared	-0.015***	-0.012***	-0.012***
	(0.002)	(0.003)	(0.004)
Married (d)	0.009**	0.009*	0.005
	(0.004)	(0.005)	(0.007)
Spouse working (d)	-0.042***	-0.050***	-0.055***
	(0.004)	(0.005)	(0.006)
Emp. health ins. (d)	-0.001	-0.023***	-0.066***
	(0.005)	(0.007)	(0.010)
Gov. health ins. (d)	0.127***	0.123***	0.137***
	(0.013)	(0.014)	(0.018)
Wealth	0.062***	-0.013	0.078*
	(0.023)	(0.032)	(0.041)

Table 20 Effect of technical change on different labor statuses (*Continued*)

Pension (d)	-0.079***	-0.138***	-0.197***
	(0.009)	(0.011)	(0.013)
Very good health (d)	0.014***	0.015**	0.008
	(0.005)	(0.006)	(0.008)
Good health (d)	0.026***	0.033***	0.037***
	(0.005)	(0.006)	(0.007)
Fair health (d)	0.076***	0.089***	0.109***
	(0.010)	(0.011)	(0.012)
Poor health (d)	0.279***	0.292***	0.302***
	(0.026)	(0.025)	(0.024)
Pseudo R-squared	0.239	0.214	0.175
Observations	21,856	21,856	21,856

Notes: All models include race, foreign-born, geographical, education, occupation, age, cohort, and sector dummies, as well as controls for the unemployment rate in the survey year and the sector experience. Statistical significance is represented by * for $p < 0.10$, ** for $p < 0.05$, and *** for $p < 0.01$. Standard errors are clustered at the sector-wave level. All models report the marginal effects of logit regressions

Appendix C: Data

We construct the correspondence between HRS and KLEMS industries.[26] We map the 13-industries Census 2002 classification scheme used in the HRS into the 31 ISIC (rev 3) codes used for the KLEMS data. This permits us to merge HRS's individual data with KLEMS's sector data. We report the correspondences in column (1) of Table 21. We compute analogous measures of value added per worker using BEA data. We follow Nolte et al. (2013) to construct the correspondence between US Census industries and NAICS codes. Column (2) of Table 21 indicates the correspondence across Census codes and NAICS industries.

Table 21 Correspondence across classification schemes

HRS industries (Census 2002 codes)	(1) KLEMS industries (31 ISIC rev 3)	(2) BEA industries (NAICS 2002 codes)
01.Agric/Forest/Fish	A, B	11
02.Mining and Constr	C, F	21, 23
03.Mnfg: Non-durable	D15–D25	31, 32 (exc. 321 and 327)
04.Mnfg: Durable	D26–D37	33, 321, 327
05.Transportation	E, I60–I64	22, 48, 49 (exc. 491), 51
06.Wholesale	G50–G51	42
07.Retail	G52	44, 45
08.Finance, Ins, and RE	J, K70	52, 53
09.Busns/Repair Svcs	K71–K74	54, 55, 56
10.Personal Services	H	72, 81
11.Entertn/Recreatn	O	71
12.Prof/Related Svcs	M, N	6
13.Public Administr	L	NA, 491

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The IZA Journal of Labor Policy is committed to the IZA Guiding Principles of Research Integrity. The authors declare that they have observed these principles.

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