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Not so Disruptive after All: How Workplace Digitalization Affects Political Preferences*

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Abstract

New digital technologies are transforming workplaces, with unequal economic consequences depending on workers' skills. Does digitalization also cause divergence in political preferences? Using an innovative empirical approach combining individual-level panel data from the United Kingdom with a time-varying industry-level measure of digitalization, we first show that digitalization was economically beneficial for a majority of the labor force between 1997-2015. High-skilled workers did particularly well, they are the winners of digitalization. We then demonstrate that economic trajectories are mirrored in political preferences: Among high-skilled workers, exposure to digitalization increased voter turnout, support for the Conservatives, and support for the incumbent. An instrumental variable analysis, placebo tests and multiple robustness checks support our causal interpretation. The findings complement the dominant narrative of the "revenge of the left-behind": While digitalization undoubtedly produces losers, there is a large and often neglected group of winners who react to technological change by supporting the status quo.

JEL: P16, D72, O33, J31

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1 Introduction

Technological innovations have a long history of producing economic change and political upheaval (Caprettini and Voth, 2017; Mokyr, Vickers and Ziebarth, 2015; Boix, 2015). A recurring preoccupation is that new machines will replace human workers, create impoverishment, and produce political instability. This century-old concern is seeing a revival. Anxiety about automation is again widespread among the public. In 2017, according to the Eurobarometer, 72% of respondents agreed with the statement that digital technologies such as robots and artificial intelligence destroy jobs. Media pundits have voiced concerns that job displacement induced by new digital technologies has contributed to the recent political disruptions in many countries. An opinion piece in the New York Times, for example, draws on work in labor economics to argue that "robots [...] helped elect Trump" (Edsall, 2018). A central worry in the "fear of the robots" narrative is that workers displaced by digital technologies are turning against the political status quo.

Rigorous scholarly evidence on the political consequences of digitalization is still too scarce to draw firm conclusions. Recent work suggests that workers susceptible to automation demand more redistribution (Thewissen and Rueda, 2017), become more likely to vote for Trump (Frey, Berger and Chen, 2018) or increasingly support radical right parties (Dal Bó et al., 2018). Our contribution builds on this emerging literature and improves on it in three respects. The first concern is operationalization: Existing studies rely on indirect indicators of digitalization based on the prevalence of routine tasks in an occupation. This is in line with seminal contributions in labor economics, but time-invariant indicators do not capture changes in the penetration of technology at the workplace and do not allow disentangling the effect of risk of substitution by digital technologies from a myriad other occupational characteristics. A second limitation relates to identification and selection bias of workers into workplaces. The kind of worker who prefers more redistribution or is sympathetic to candidates such as Donald Trump might self-select into routine occupations or into manufacturing areas with high exposure to risk. In that case, the observed correlations between routine work and political attitudes would not necessarily be related to the introduction of digital technologies.

Our more substantive critique is that the focus in both public discourse and academic work on the losers of digitalization is overly narrow.[1] We do not dispute that a part of

[1] This is at least partly a consequence of an influential study estimating that digitalization puts almost

the population has difficulties to adapt to changing skill requirements in an increasingly digitalized work environment, or that digitalization eliminates jobs and produces losers. But an exclusive focus on citizens experiencing disadvantages might paint an incomplete picture of the political repercussions of economic modernization (see also Iversen and Soskice, 2019). It is at odds with standard economic theory, which claims that technological innovations increase productivity and wages – and can produce winners as well as losers. It is also at odds with historical experience, which has failed to produce the mass technological unemployment dreaded by Marx, Keynes, Leontief and many others. A more comprehensive understanding of the political consequences of digitalization should also study workers who benefit from it.

This paper improves on all three limitations in existing work. To address concerns about operationalization, we use a more direct measure of digitalization: time-varying indicators of ICT capital stocks at the industry-level (covering 1997-2015) taken from the EU KLEMS database (see also Michaels, Natraj and Van Reenen, 2014). For identification, we rely on rich individual-level panel data from the British Household Panel Study (BHPS) and the Understanding Society survey (UKHLS) and fixed effects models, which allow us to control for time-invariant individual and industry-level characteristics. An instrumental variable approach, a placebo analysis using non-ICT capital instead of ICT capital and multiple robustness checks add confidence to a causal interpretation of our findings. Finally, our approach is well-suited to study the large group of beneficiaries of technological change. Using a representative sample of the labor force, we can examine if digitalization in an industry affects the economic and political trajectories of workers and, crucially, if these effects vary depending on the workers' education level.

Our results suggest that digitalization at the workplace is economically beneficial for a majority of the workforce. ICT capital in an industry increases the salaries of all but the least educated workers and has limited adverse employment effects. Turning to political outcomes, our findings show that these distributive implications are reflected in individual political reactions to technological change. Faster than average digitalization is associated with increased (a) voter turnout, (b) support for the Conservative party, and (c) support for the incumbent, but only among winners of digitalization, that is the highly educated.[2]

every second job at risk of disappearing (Frey and Osborne, 2017). Although this figure has been questioned by later studies (see, e.g., Arntz, Gregory and Zierahn, 2016), the emphasis on displaced workers endures.

[2]As we discuss later, we find that education strongly conditions whether workers benefit or not from workplace digitalization. We also replicate the analysis distinguishing between workers in occupations with

Digitalization is unrelated or negatively related to the turnout rates of less educated workers and has no discernible effect on their support for parties.

To the best of our knowledge, this is the first paper to produce well identified individual-level effects of workplace digitalization on political outcomes using panel data.[3] We shed new light on the political consequences of digitalization by highlighting its multi-faceted effects. The finding that digitalization is economically beneficial for a majority of workers and that these workers increasingly support center-right mainstream and incumbent parties is in line with standard economic theory. This finding does not preclude that some sectors suffer in absolute or relative terms, and we indeed find evidence of economic polarization. Still, our paper brings attention to economic winners, a neglected population in the large literature on the political implications of structural change, and adds nuance to the gloomy picture in the "fear of robots" narrative. Technological change does not only shape politics by creating a reservoir of dissatisfied losers who find the political remedies offered by populist or anti-establishment parties appealing, but it can also increase support for the establishment among the large group of beneficiaries.

2 The political implications of technological change

A large theoretical and empirical literature in labor economics studies how advances in technology affect economic outcomes such as salaries, employment, and income inequality (e.g. Berman, Bound and Machin, 1998; Autor, Levy and Murnane, 2003; Autor, Katz and Kearney, 2006; Goldin and Katz, 2009; Goos, Manning and Salomons, 2009; Acemoglu and Restrepo, 2017). The effects of technological change on wages and employment depend on the net outcome of two countervailing forces (Acemoglu and Restrepo, 2018). The painful aspect of technological change is that it creates a displacement effect as machines start to perform tasks previously done by humans. The benign aspect is a productivity effect. New technologies complement workers, for example when they allow for quick communication with colleagues. They free up time spent doing dull tasks, which can be spent more productively. They reduce costs, generating economic growth and an increase

high or low routine-task intensity, but we do not find similarly strong moderation effects by routine task intensity.

[3] Most studies about the consequences of technological change in economics analyze *aggregate* level outcomes rather than the effects on the individual trajectories of workers (for an exception see Dauth et al., 2017). We also contribute to this literature by examining how workers' wages and probability of unemployment change when their industries digitalize.

in the demand for labor. Finally, new technologies create entirely new jobs, such as when computers generated demand for software engineers.

The net effect of these two forces on wages and employment is a priori uncertain. In the last two centuries, however, the productivity effect of technological has clearly dominated (Mokyr, Vickers and Ziebarth, 2015). Technological change, along with well-designed, complementary institutions, is the most important cause of the unrivaled growth in output and living standards since the Industrial Revolution. While perhaps less impressive than in the 1960s, the overall positive economic effect of technological innovation still holds today (Mokyr, 2018). The long-term macro picture might offer little consolation for workers displaced by technology, but it suggests that mass inmiseration due to technological change is rare and that most individuals have historically benefited from technology-driven productivity gains.

Average positive effects on wages are compatible with significant heterogeneity. The specific distributive effects depend crucially on the complementarities or substitution effects between new technologies and workers' skills. The last wave of technological innovation, which is characterized by the extension of information and communication technologies (we use the term *digitalization* to analytically distinguish from the more generic term of technological change), has mostly complemented highly educated workers while substituting less skilled workers and those in routine occupations (Autor, Levy and Murnane, 2003; Michaels, Natraj and Van Reenen, 2014; Goos, Manning and Salomons, 2009). We expect highly educated workers to benefit most from digitalization, but existing studies provide little guidance on the crucial question whether we should expect workers with low or mid levels of skills to become worse off when their workplace digitalizes. Most studies analyze the *aggregate* economic impact of technological change across countries, industries or regions rather than changes *within individuals*. For our purposes it is important to note that the well-documented reduction in jobs in mid-paying occupations does not necessarily imply that at the micro level workers with intermediate levels of skills suffer most. This is one of the empirical questions we set to explore.[4]

Despite the evident distributive consequences of digitalization, the political implications

[4]The observed aggregate reductions in mid-paying jobs can be driven by retirement (a non-traumatic way to exit the labor market) without replacement being concentrated in these jobs, and by exits to other jobs which are often higher paying (Dauth et al., 2017; Cortes, 2016). If both processes are at work, we should not necessarily observe that digitalization decreases the salaries or increases the probability to become unemployed of individuals with intermediate education levels.

of this economic transformation have received little academic attention (but see Thewissen and Rueda (2017), Frey, Berger and Chen (2018), and Dal Bó et al. (2018)). This stands in sharp contrast to the extensive literature on the implications of globalization and international trade (Margalit, 2011; Jensen, Quinn and Weymouth, 2017; Autor, Hanson and Majlesi, 2016; Colantone and Stanig, 2018*a*,*b*). This neglect is noteworthy since the empirical evidence suggests that technological change is the most important driver behind the transformation of the employment structure and outperforms international trade and migration as an explanation of the rise in inequality and job polarization (Jaumotte, Lall and Papageorgiou, 2013; Goos, Manning and Salomons, 2014).

To generate our hypotheses on political outcomes, we build on three core theories of political behavior – resource model of participation, spatial or ideological voting, and economic voting– which point to three distinct but in principle equally likely ways in which digitalization can affect political behavior. In all cases, we expect the effects of digitalization at the workplace to be heterogeneous depending on whether workers are likely to benefit from it or not. Previous work has proposed different reasons why digitalization may affect the political behavior of the disadvantaged (or "left behind"), but we are just as interested in the inversion of these theories' arguments, and hence discuss explicit expectations with respect to both less and highly educated workers.

We concentrate on education rather than on task content, i.e. the distinction between routine vs non-routine occupations dominant in economics (Autor, Levy and Murnane, 2003), for theoretical and empirical reasons. Education is a generally stable individual characteristic, as relatively few people acquire higher educational credentials after finishing schooling in young adulthood. Intra-individual stability makes education more suited for our longitudinal analysis than routine task intensity (RTI), which is measured on the level of occupations and changes as workers switch between different jobs. RTI is hence a fluid and potentially endogenous characteristic giving rise to varied trajectories.

More importantly, education should be correlated with individuals' unobserved cognitive skills and ability to learn and hence with their potential to adapt to and reap the benefits of the introduction of new digital technologies in the workplace. By contrast, it is unclear if the current RTI of a worker's job is informative about his or her ability to adapt to digitalization.[5] In our empirical setting, which interacts an industry-level measure of

[5] For instance, a highly educated routine worker (e.g. an accountant) may have the cognitive resources to adapt to the introduction of software that performs routine accounting tasks, and become more productive

digitalization with an individual trait capturing the capability to deal with this development, education is more informative about the ability to learn, retrain, and ultimately benefit from digitalization than routine task content of the current job. We support this claim with empirical evidence in section S3 where we show that education is a stronger moderator than RTI in predicting whether workers are positively or negatively affected by digitalization in their industries.

2.1 Digitalization and voter turnout

Our first expectation is that exposure to digitalization may affect participation in elections and that this effect is heterogeneous depending on whether workers benefit economically or are harmed by the introduction of new technologies in their workplaces. This can happen mainly through three mechanisms. The vast literature on the resource model of political participation (e.g. Verba, Schlozman and Brady, 1995) generates the expectation that economic hardship and a reduction of resources leads to lower turnout. If digitalization reduces wages among workers with less education who can be substituted by machines but increases wages among highly educated workers with skills that are complements to machines, we expect the political participation of these two groups to diverge when their workplaces digitalize.

A second mechanism with the potential to affect voter turnout is job insecurity or even job loss. In particular unemployment might lead to "political withdrawal" as citizens concentrate in solving more pressing problems than participation in elections (Rosenstone, 1982). Again, digitalization has contrasting effects on job prospects, as less educated workers become less secure in their jobs if the tasks they perform can be done by machines while the demand for highly educated workers increases if they become more productive. Although results about the relationship between unemployment and job security on voter turnout are mixed (Smets and Van Ham, 2013), recent evidence suggests that labor market vulnerability tends to go hand in hand with less political participation (Rovny and Rovny, 2017), and this demobilizing effect is especially pronounced in contexts where unemployment is not excessively high (Aytaç, Rau and Stokes, 2018), which is the case in the UK in the period we study. Importantly, research using similar longitudinal panel data shows that unemployment and insecurity can reduce political engagement (Emmenegger, Marx

at the same or another job, a process known as upskilling (Hershbein and Kahn, 2018).

and Schraff, 2017).

A third mechanism through which workplace digitalization can affect political participation is psychological. The realization that tasks previously performed by humans can be carried out by machines might undermine feelings of self-efficacy and self-esteem, which are important precursors of political engagement (Marx and Nguyen, 2016). Conversely, workers with complementary skills may become more central economically and become politically empowered as a result.

In sum, because of its material and psychological effects, we expect digitalization to increase political participation among highly educated workers and depress participation among the less educated, resulting in an increase in inequalities in voter participation.

2.2 Digitalization and party support

Beyond participation in elections, we also expect digitalization to shape preferences for political parties. Two core models in the study of political behavior, spatial or ideological voting models and economic voting models, point to two different predictions about the political consequences of digitalization for workers.

The first relevant stream of research is based on spatial models of voting, which depict political competition as a conflict about redistributive issues, and individual material circumstances as the main driver of individual policy preferences (e.g. Margalit, 2013; Rueda, 2005) and ultimately of party support (e.g. Iversen and Soskice, 2006). While theoretical models diverge in their attention to economic disadvantage (and hence demand for redistribution) or risk (and hence demand for insurance) (Rehm, Hacker and Schlesinger, 2012), in our case economic disadvantage and risks are bundled: Digitalization can depress wages and increase risk of displacement for workers who can be substituted by machines, who typically have low or middle levels of skills. It has the opposite effect on both dimensions for workers with complementary skills to machines, who are typically highly educated.

Workers affected by digitalization may change their preferences about parties through two channels. The first is changes in their material situation. Increases in income and job opportunities (or even in expectations) among workers with skills that are complementary to computers will reduce support for redistribution and increase support for parties that defend right-wing economic policies. Conversely, less educated workers at risk of substitution should become more supportive of parties that defend redistribution. These expectations are consistent with the findings reported in Thewissen and Rueda (2017), who show in cross-sectional analyses that workers who are likely to be negatively affected by digitalization demand more redistribution, even after introducing a wide range of controls.

An additional, more speculative, channel through which digitalization can affect support for parties is by altering attitudes towards the market and regulation. New technologies facilitate the creation of new markets where workers can directly offer goods and services. This direct exposure to markets shapes a more positive attitude if the worker sees herself as a beneficiary of the transformation. In addition, experience with technological disruption can make workers skeptical of the ability of government to regulate rapidly changing sectors, especially for those who expect their career perspectives to improve due to digitalization and hence have little motivation to demand regulation. Consistent with this intuition, a recent study of wealthy Americans' political preferences (Broockman, Ferenstein and Malhotra, 2017) found that Silicon Valley's tech entrepreneurs, a clear group of winners of digitalization, indeed oppose government regulation and display more market-friendly attitudes than other Democrats.[6]

The basic characterization of party competition described in these models applies to the UK. The main parties have clearly distinct positions on economic issues such as redistribution and social insurance. While the importance of class voting and the alignment of parties with income groups has declined since the 1970s (Evans and Tilley, 2017), the Conservative Party still defends more right-wing economic positions than the Labour Party, with the Liberal Democratic Party taking intermediary positions. In our setting, we expect digitalization to increase support for the Conservative Party among highly educated workers who become better off due to digitalization. Conversely, exposure to digitalization should increase support for the Labour Party among less educated workers.

A second stream of research suggests that changes in the economic standing of workers will affect support for the incumbent (Lewis-Beck and Stegmaier, 2000). In retrospective voting models (Fiorina, 1978), individual's economic situation influences their support for the incumbent through a simple punishment-reward mechanism. In the case of digitalization, this logic leads us to expect that highly educated workers who are positively affected by digitalization should become more likely to support the political status quo and hence

[6] One might ask why people who choose to work in the highly paid tech industry support the Democrats in the first place. However, party choice is the result of multiple considerations including moral issues, immigration and cosmopolitanism that may motivate tech elites to support a progressive rather than a conservative party.

the incumbent party. Conversely, less educated workers could become less supportive of the incumbent.

Some previous research is consistent with digitalization leading to an increase or decrease in support for the incumbent depending on how it affects different groups. Frey, Berger and Chen (2018) study vote for Donald Trump and argue that voters who most strongly feel the adverse consequences of automation might opt for radical political change, but their findings can also be interpreted through the prism of classical economic voting. Research about the political consequences of other structural transformations such as offshoring and trade with China finds that voters in negatively affected areas withdraw support for the incumbent party (Margalit, 2011; Jensen, Quinn and Weymouth, 2017; Autor, Hanson and Majlesi, 2016). Again, there is evidence from the UK confirming the relevance of economic voting in this context (Tilley, Neundorf and Hobolt, 2018).

These two key expectations on the effects of digitalization for party support are drawn from well-established models in political behavior research. Both possibilities are *a priori* equally plausible and there is no theoretical reason to expect that one should apply but not the other.[7] The period we study covers governments by left- and right-wing political parties. Note that until 2010, when the Labour Party was in power, the prediction of spatial voting models is that highly educated workers should become more likely to support the Conservative Party due to economic self-interest but more likely to support the incumbent Labour Party due to egotropic economic voting. If both processes occur at the same time in the pre-2010 period, they would produce effects in opposite directions, potentially canceling each other out. By contrast, under Conservative government from 2010 onwards, the two processes produce reinforcing effects. An important implication is that the precise political consequences of digitalization depend on the specific political situation.

3 Data and descriptive overview

Our empirical analyses focus on the case of the UK, an established democracy at the frontier of technological innovation for which rich longitudinal micro-level data are available.

[7]Note that the processes discussed are based on general principles and can apply even in the absence of public debate about the issue of digitalization and technological change and even if workers do not actively reflect about this topic. The theoretical expectations could vary substantially if parties politicized the issue of digitalization. However, the party manifestos in the UK in the period covered by this study suggest that this topic was hardly mentioned.

3.1 Industry level measure of digitalization

To measure digitalization, we follow Michaels, Natraj and Van Reenen (2014), who use yearly changes in ICT capital stocks within industries.[8] We use the September 2017 release of the EU KLEMS dataset (Jaeger, 2017), which contains yearly measures of output, input and productivity for 40 industries in a wide range of countries, including the UK, and covers the period 1997 to 2017. The data is compiled using information from the national statistical offices and then harmonized to ensure comparability. Most importantly for our purposes, the database provides a breakdown of capital into ICT and non-ICT assets (O'Mahony and Timmer, 2009). This allows for the creation of time-varying, industry-specific indicators of digitalization based on ICT stocks.

Our measure of digitalization is constructed as follows:

$$\mathrm{D}_{j,t} = \frac{(\text{ICT capital stock in thousand GBP}_{j,t})}{(\text{Employees}_{j,t})}$$

Where ICT capital stock$_{j,t}$ is the sum of the fixed capital stocks in computing equipment, communications equipment, computer software and databases in industry j in year t, at constant 2010 prices.[9]

Figure 1 plots the evolution of our indicator of digitalization over time for the industries provided by EU KLEMS.[10] Some industries are disaggregated only at the 1-digit level (e.g. Agriculture, forestry and fishing), while for other industries EU KLEMS also breaks down the data at the more fine-grained 2-digit level (e.g. manufacturing is disaggregated into 11 categories such as "food products, beverages and tobacco").

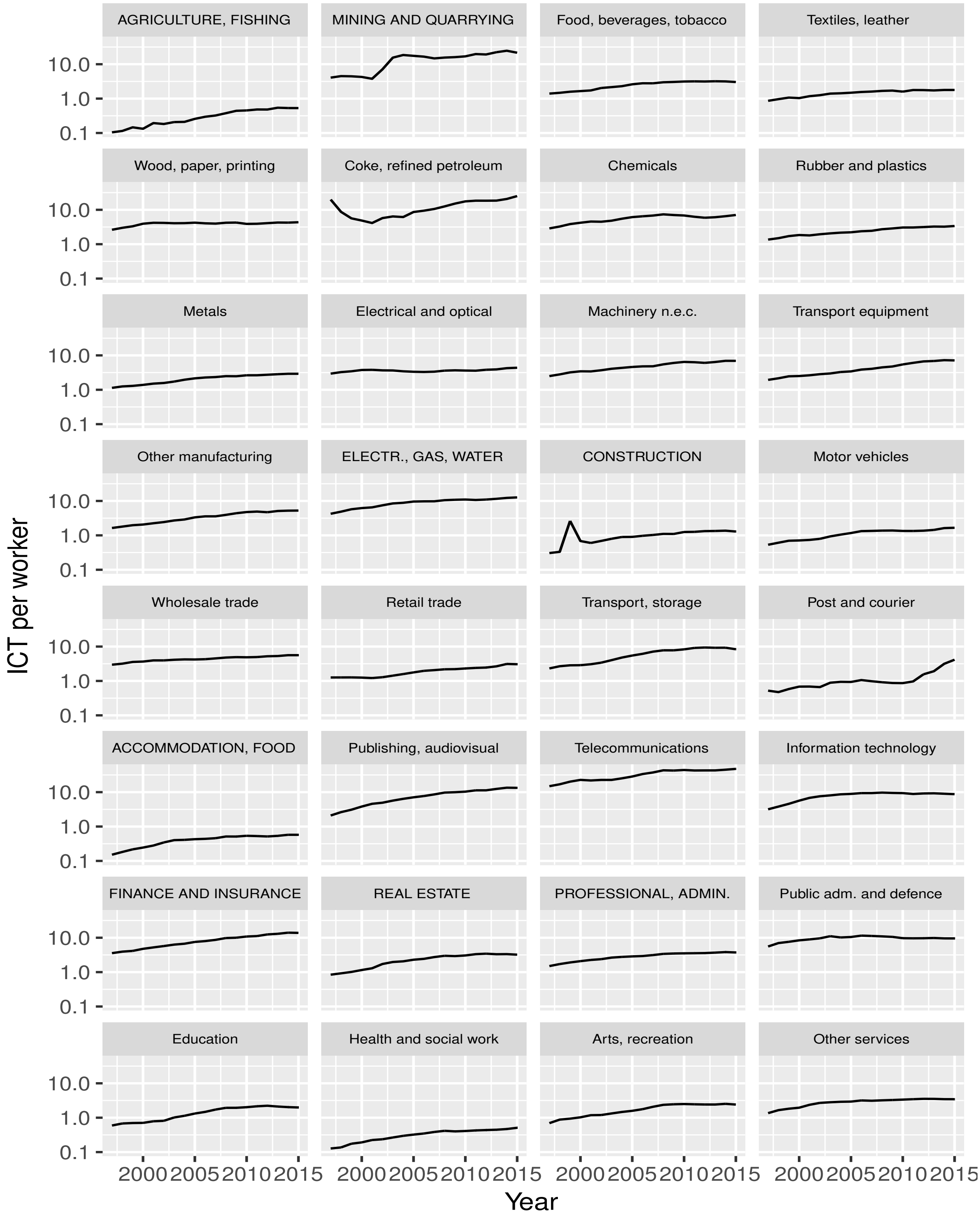
As expected, we see a general increase in the importance of digital technologies over time. The levels of ICT intensity also vary across industries in a sensible way (e.g. they are highest for telecommunications, or finance and insurance, as we would expect), adding to our confidence that the measure is valid. Note that the over time trend shown in the

[8] Our approach is also similar to Graetz and Michaels (2015) and Acemoglu and Restrepo (2017).

[9] Note that productivity-enhancing and potentially labor-saving investments can in principle affect our measure in two ways. First, they increase the numerator (the ICT capital stock) and second, they can reduce the denominator if labor-saving technologies are implemented and reduce the number of employees in the industry. This is a manifestation of the two-fold consequences of digitalization: It can be beneficial for workers by increasing productivity or threatening if it reduces labor demand. Our measure hence captures ICT intensity relative to labor in an industry, rather than ICT intensity in an absolute sense.

[10] EU KLEMS data is disaggregated by 40 industries based on the industry standard classification system used in the European Union (NACE rev1). For 3 industries, ICT data is missing or has only zero values which reduces our sample to 37. NACE codes are consistent with UK SIC codes provided in the BHPS, which allows for a comprehensive merge of the two datasets. The scale of the y axis is logged to facilitate visualization, but the analyses use the original variable, operationalized as discussed above.

Figure 1: Digitalization: ICT capital stock per employee, by industry



Note: Digitalization measured as yearly ICT capital stock per worker for the industries provided by EU KLEMS. Industries at the 1-digit level are written in capital letters, while industries at the 2-digit level are in lower case letters.

graph most likely understates the true degree of digitalization as ICT prices fell drastically over time. However, this does not invalidate our analysis since we will use year fixed effects and therefore focus on within year variation across industries in the adoption of ICT.

3.2 Individual-level survey data

We combine this measure of digitalization at the industry level with longitudinal data from the British Household Panel Study (BHPS) and the Understanding Society (UKHLS) survey. The BHPS is a longitudinal study that has interviewed about 10,000 individuals nested in 5,000 households drawn from a stratified random sample of the British population yearly from 1991 to 2008. In 2009 the BHPS was transformed into the Understanding Society (UKHLS) survey, with considerably expanded sample size (for a thorough description about survey design see Buck and McFall, 2011). Every year participants are asked detailed questions about their economic situation, current and past employment, as well as a few political questions.

We assign every worker the value of our measure of digitalization (ICT per worker) in his or her current industry. Because the 2017 EU KLEMS release only covers the period since 1997, we exclude respondents surveyed between 1991 and 1996 from our study. We also exclude respondents aged 65 and older (who should be less affected by changes in the labor market) and respondents less than 18 year old. From the remaining sample, 71.3% can be linked to one of 32 industries (NACE rev. 2). We exclude extraterritorial organizations and households as employers as there is no information on ICT capital stocks. Our final sample contains 276'855 for 60'029 individuals.[11]

The dependent variables in our analyses are a set of indicators of economic situation and political attitudes asked consistently over time by BHPS/UKHLS.

Wages: We compute hourly net wages in constant 2010 prices using the variable usual net pay per month, which is derived by BHPS/UKHLS staff using answers to detailed income questions and imputed if this information is missing. This is normalized by hours worked. Observations with less than half time employment (20 hours per week) are excluded from this analysis since there is considerable measurement error which leads to noise in our calculation if the denominator (hours worked) is small.

[11] The analyses do not include people not assigned to an industry, including students or the currently unemployed if no industry is reported, people who never enter the labor force, and people who have exited the labor force.

Unemployment: The employment status refers to the week when the respondent was interviewed. The surveys do not ask about unemployment spells between surveys, so we can only look at the moment of the interview, which is a lower bound for unemployment. Since we are interested in the effect of digitalization on the probability to *become* unemployed, we focus our analysis on the effect of current digitalization of a worker's industry on her probability of being unemployed at the time of the *next* interview.

Voter turnout: Our measure of voter turnout is self-reported participation in the last general election, which is asked in all waves until 2008 and then in 2010 and 2015.

Support for the Conservative Party and the Labour Party: We construct this variable using a series of questions asked every year on whether respondents consider themselves supporters of a party or (if they are not) if they feel closer to one political party than to the others. In the Supporting Information (SI) we also present the results about support for the Liberal Democratic Party and UKIP.

Support for the incumbent: We code respondents as supporters of the incumbent party if they supported the Labour Party before the government change in 2010 and the Conservative Party after it changed.[12]

Education is coded in six categories: university degree (26.6% of the sample in 2015); other higher degree (such as teaching or nursing, 12.4%), A-Level and other higher secondary qualifications (21%); General Certificate of Secondary Education, O-level and other lower secondary qualifications (19%); other qualifications (9.2%); and no formal qualifications (11.7%).

Table 1 presents the summary statistics of the main variables used in the analyses. The SI contains a detailed description of the evolution of all dependent variables over time for each educational group.

[12]Including LibDem as part of the government between 2010 and 2015 does not change the results. The BHPS/UKHLS asks other questions about political attitudes (such as attitudes towards the role of the government in the economy or nationalism), but only infrequently and mostly in the BHPS period before 2008. We concentrate on the variables for which we can obtain a longer time series.

Table 1: Summary Statistics

	Count	Mean	SD	Min	Max
ICT capital stock per worker	276855	2.14	2.46	0.05	25.30
ICT capital stock USA per worker (IV)	270019	28.73	83.89	0.18	1041.22
Non-ICT capital stock per worker (placebo)	276855	132.96	391.58	6.46	4955.94
Hourly wage	223760	9.41	23.94	0.00	5785.66
Probability to become unemployed	213823	0.02	0.15	0	1
Voted in the last general election	108880	0.70	0.46	0	1
Supports the Conservative Party	232121	0.22	0.41	0	1
Supports the Labour Party	232121	0.32	0.47	0	1
Age	276855	40.43	12.02	18	64
Female	276855	0.50	0.50	0	1
Year	276855	2008.54	5.08	1997	2015
Routine Task Intensity	264331	-0.26	0.78	-1.87	2.10
Industry in EUKLEMS categories	276855			1	38
Government region ID	275923			1	13
Observations	276855				

Note: ICT defined as "real fixed ICT capital stock (in 1000 GBP in constant 2010 prices) normalized by number of employees".

4 Estimation and identification

4.1 Fixed-effects model

We use individual industry-spell fixed-effects models to estimate the effects of digitalization in a worker's industry on labor market and political outcomes. To test the expectation that the effects of digitalization on labor market and political outcomes are heterogeneous depending on workers' education level, we estimate separate slopes for the effect of digitalization at the industry level for each education level. Our baseline specification is:

$$Y_{ijt} = \sum_{s*=1}^{6} I_{[S_{it}=s*]}\delta_{s*} + \sum_{s*=1}^{6} I_{[S_{it}=s*]}\theta_{s*} \times D_{jt} + \boldsymbol{\gamma}'\mathbf{C}_{it} + \eta_{ij} + \mu_t + \epsilon_{ijt} \qquad (1)$$

Where Y_{ijt} is the outcome of interest (economic or political) for individual i in industry j at time t. It is a function of six dummy variables $I_{[S_{it}=s*]}$, which take the value 1 if an individual has the corresponding education level and 0 otherwise. The coefficient vector $\boldsymbol{\delta}$ identifies separate intercepts for each education level.[13] We further add the time-varying measure of digitalization at the industry level (D_{jt}) described in section 3.1 which is interacted with the education level dummy variables $I_{[S_{it}=s*]}$ to estimate the heterogeneous effects of digitalization on our variable of interest across different education groups. Adding the six education levels as separate dummy variables allows us to estimate the effects of digitalization on individuals with different educational levels non-parametrically.[14]

We also add a vector $\mathbf{C}_{it}$ of individual-level controls. We only include age and age squared as controls in order to avoid post-treatment bias when controlling for time-varying covariates (such as socio-economic indicators) which may themselves be affected by changes in a workers' industry.

In our baseline specification, we include the term η_{ij}, a vector of individual by industry fixed effects (or industry-spell fixed effects) which captures all time-invariant variables that might affect labor market and political outcomes, self-selection of workers into specific workplaces, such as their gender, personality or family origin, as well as time-invariant industry-level characteristics. The industry-spell fixed effects include separate intercepts for the same individual in periods when he or she has worked in a different industry, which allows us to rule out that switchers to different industries are driving the results.[15] However, we also conduct extensive robustness checks to examine if our conclusions hold using alternative fixed effects models.

To allow for the correlation of error terms of the same individual over time and when they work in different industries we cluster the error term ϵ_{ijt} at the individual level.[16] Finally, we include a year fixed effect μ_t to account for common shocks and trends.

[13] For most individuals, the education level is constant in all waves of the study. In our fixed effect model, the coefficient vector $\boldsymbol{\delta}$ will only be identified by the few who upgrade their education level as education is otherwise absorbed by the individual fixed effect. Therefore, we do not focus on the direct effect of education when interpreting the results.

[14] Note that we chose to depart from the convention to include the direct effect of digitalization in the model (Brambor, Clark and Golder, 2006), even though the results would be numerically equivalent. If the direct effect were included, we would have to define one of the education level as the base-group to avoid colinearity. Coefficients would be relative to the base group: e.g. with a one unit increase in digitalization, the degree group earns X more relative to the no qualification group. However, we prefer to display the results leaving out the direct effect so that readers can immediately infer what is the effect of digitalization on a given education level and see if these effects are significantly different from zero: e.g. digitalization increases hourly wages of the degree group by X.

[15] This is important because differences in digitalization across industries are much larger than differences within industries from one year to another. Any changes occurring when workers move to a different industry (which may coincide with many other relevant changes besides digitalization) would dominate the more subtle effects of digitalization at a given workplace we are interested in.

[16] In section S8 of the SI, we report an alternative specification where we cluster standard errors at the level of the variation of the digitalization treatment, that is on the industry-year level.

This specification only uses over time variation in the level of digitalization within industries for workers who remain at the same industry for two or more periods to identify the effect of digitalization on political attitudes and other outcomes.

4.2 Instrumental variables approach

A key concern with our empirical approach is the possible endogeneity of our measure of digitalization. In particular, ICT capital stocks per worker in the UK, could be influenced by governmental policies that also affect workers' economic and political outcomes, such as policies adopted to shelter some industries from competition or subsidies to accelerate or slow down the adoption of digital technologies in some industries in response to their political power. In return, workers employed in that industry could have a more favorable view of the party in power.

To address this concern, we follow recent work on the Chinese import shock (Autor, Dorn and Hanson, 2013) and instrument our measure of ICT capital stocks per worker in the UK (D_{jt}) with an analogous measure from the USA (D_{jt}^{USA}).[17] Following previous literature (Colantone and Stanig, 2018*a*), we calculate our instrument as:

$$D_{j,t}^{USA} = \frac{(\text{ICT capital stock in the USA in thousand USD }_{j,t})}{(\text{Employees in the UK}_{j,t})}$$

In the second stage, $\tilde{D}_{jt}^{USA}$ represents digitalization in the UK instrumented with values from the US:

$$Y_{ijt} = \sum_{s*=1}^{6} I_{[S_{it}=s*]}\delta_{s*} + \sum_{s*=1}^{6} I_{[S_{it}=s*]}\theta_{s*} \times \tilde{D}_{jt}^{USA} + \gamma \mathbf{C}_{it} + \eta_{ij} + \mu_t + \epsilon_{ijt} \qquad (2)$$

The first stage of the IV analysis is strong (all F-statistics are larger than 75). This is to be expected given that the US is clearly at the technological frontier and competition and profit maximization motivate industries in other countries to adopt these productivity-enhancing technologies once they exist. Digital technologies adopted in an industry in the US are likely to be adopted in the UK as well, perhaps with a time lag.

The exclusion restriction of our IV strategy is that changes in ICT capital stocks in the USA do not produce changes in the economic outcomes or political views of workers from

[17] Unfortunately, the EUKLEMS dataset does not include data for two industries in the USA: telecommunications and wholesale and repair trade of motor vehicles and motorcycles.

the same industry living in the UK *if ICT stocks in the UK are held constant*. Channels other than technology diffusion are likely to impact workers in the UK too indirectly and too slowly to drive the effects we capture. Furthermore, given the unequal size of the countries, politics and economics in the UK are unlikely to affect the adoption of technology in the US.

In the robustness checks we discuss alternative specifications and additional analyses that address other concerns including: placebo tests; models with individual, industry and year fixed effects (which include a single intercept per individual allowing us to track changes in outcomes when individuals change industries); lead models in which all dependent variables are measured one year later (allowing us to keep individuals who may have been displaced by technology or have exited their industries for other reasons); region by year fixed effects; models including controls for trade; and analyses of attrition, among others.

5 Results

This section reports the results of the main analyses in graphical form. The plots present the marginal effect of a one-unit increase in digitalization (a 1000 GBP increase in the ICT capital stock per worker or 0.4 standard deviations), for workers of different education levels.[18]

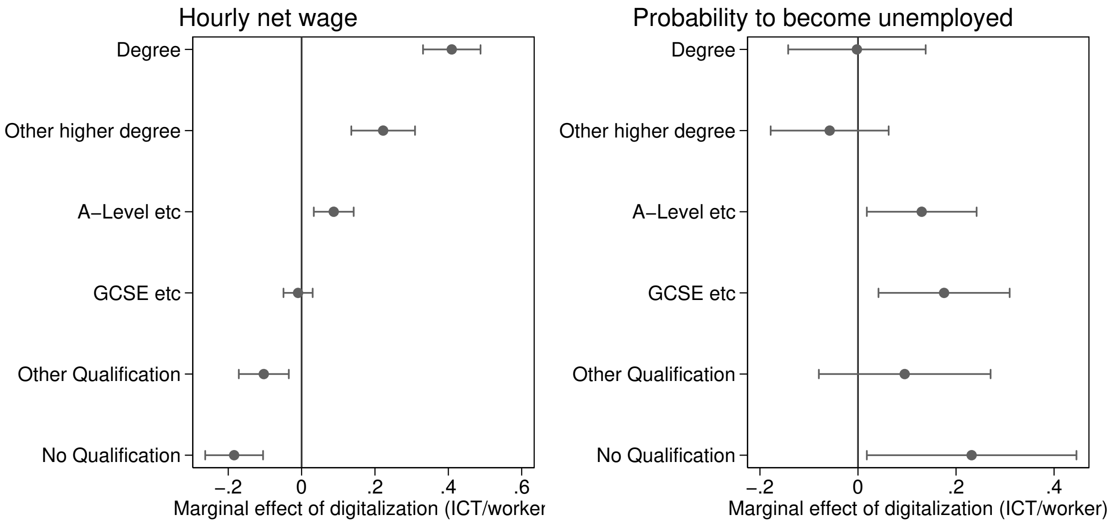
5.1 Winners and Losers: Digitalization and Labor Market Outcomes

The first part of our analysis tests our expectations about the distributive consequences of digitalization and helps validates our novel longitudinal approach. Figure 2 presents the marginal effects of our measure of digitalization on net hourly wages and the probability of unemployment at the time of the next interview for workers with varying levels of education.

Confirming our expectations, we find a strong positive effect of increases in digitalization in an industry on the hourly net wages of workers with higher education levels, especially university degrees. On the other hand, individuals with low levels of education

[18] The complete regression tables are presented in the section on instrumental variables and in the SI. The marginal effects presented in the main text are always estimated from column 1 (our main specification) of each table in the SI.

Figure 2: Effect of ICT capital stock increases on labor market outcomes



Note: Results show the marginal effect of one unit increase in digitalization (1000GBP in ICT capital/worker) on hourly net wages (left) and the probability to become unemployed (right). Hourly net wage calculated as monthly net wage in constant 2010 GBP normalized by average hour worked. Probability to become unemployed refers to being unemployed at the next interview in percentage points. The marginal effects are numerically equivalent to the interaction of digitalization and education groups of our main specification

or no qualifications experience a reduction in their hourly wages in periods when their industry digitalizes fast.[19] The coefficients can be interpreted as follows: a one unit increase in digitalization (1000 GBP ICT capital stock per worker) increases the average hourly net wage of a university graduate by 0.4 GBP which is equivalent to a yearly net wage increase of 768 GBP. By contrast, a one unit increase in digitalization decreases the average hourly wage of workers with no qualifications by 0.16 GBP or 312 GBP per year. In light of the conservative nature of these models, we consider the wage effects displayed in Figure 2 as strong evidence for heterogeneous effects of digitalization. It first and foremost benefits those who have the skills to thrive in a rapidly digitalizing world of work.

Second, we examine if digitalization affects the likelihood that workers will be unemployed the next time they are interviewed. In this case, we use lead models because we are interested in the probability of becoming unemployed in the future. We find some evidence that digitalization increases the likelihood that less educated workers become unemployed when they are reinterviewed after digitalization occurred. This finding is in line with the task-based literature emphasizing that primarily routine jobs in the middle and low end of the wage and education distribution are susceptible to automation (Autor, Levy and

[19] We tested if the differences in the effect of digitalization across education groups are statistically significant. All of them are, except for the difference between no qualification and other qualification.

Murnane, 2003; Goos, Manning and Salomons, 2009). However, the effects are substantively small. For example, a one-unit increase in our measure of digitalization, i.e. a 1000 GBP increase in the ICT capital stock per worker (0.4 std), is associated with an increase in the probability to report being unemployed at the next interview of 0.24 percentage points for the no qualification group. This constitutes a 7% increase in the odds to become unemployed from 1:30 to 1:28.5.

Our findings are in line with previous studies concluding that digitalization has limited effects on individual experiences with unemployment. Existing literature in labor economics has shown that while technological change is a powerful driver of a changing occupational structure, until now it has not had a strong negative impact on net employment (Autor, Dorn and Hanson, 2015). In an analysis using individual-level data from Germany (Dauth et al., 2017), finds that workers who started their employment trajectory in an industry that later became more robotized did not spend more days unemployed in subsequent years than other workers. For the UK, Kurer and Gallego (N.d.) show that most routine workers stay in their jobs and the decline in the share of routine jobs happens through retirement and lower entry rates rather than layoffs.[20]

Overall, the results of the first set of analyses on the economic consequences of digitalization confirms that it has strong distributional consequences. Digitalization produces income polarization between highly educated and less educated workers, although we find only weak adverse employment effects for workers with medium and low education levels. These results are congruent with previous findings in the literature, and suggests that our novel empirical approach is valid.

Our analysis yields two important take-away points. First, the impact of faster than average digitalization on hourly wages is positive for a majority of workers. Second, digitalization has unequal effects on highly and less educated workers, producing economic polarization. Those with a higher degree represent 39% of our sample in 2015 and are unambiguous economic winners, as digitalization increases their wages without any adverse employment effects. Adding workers holding A-Level certificates (upper secondary

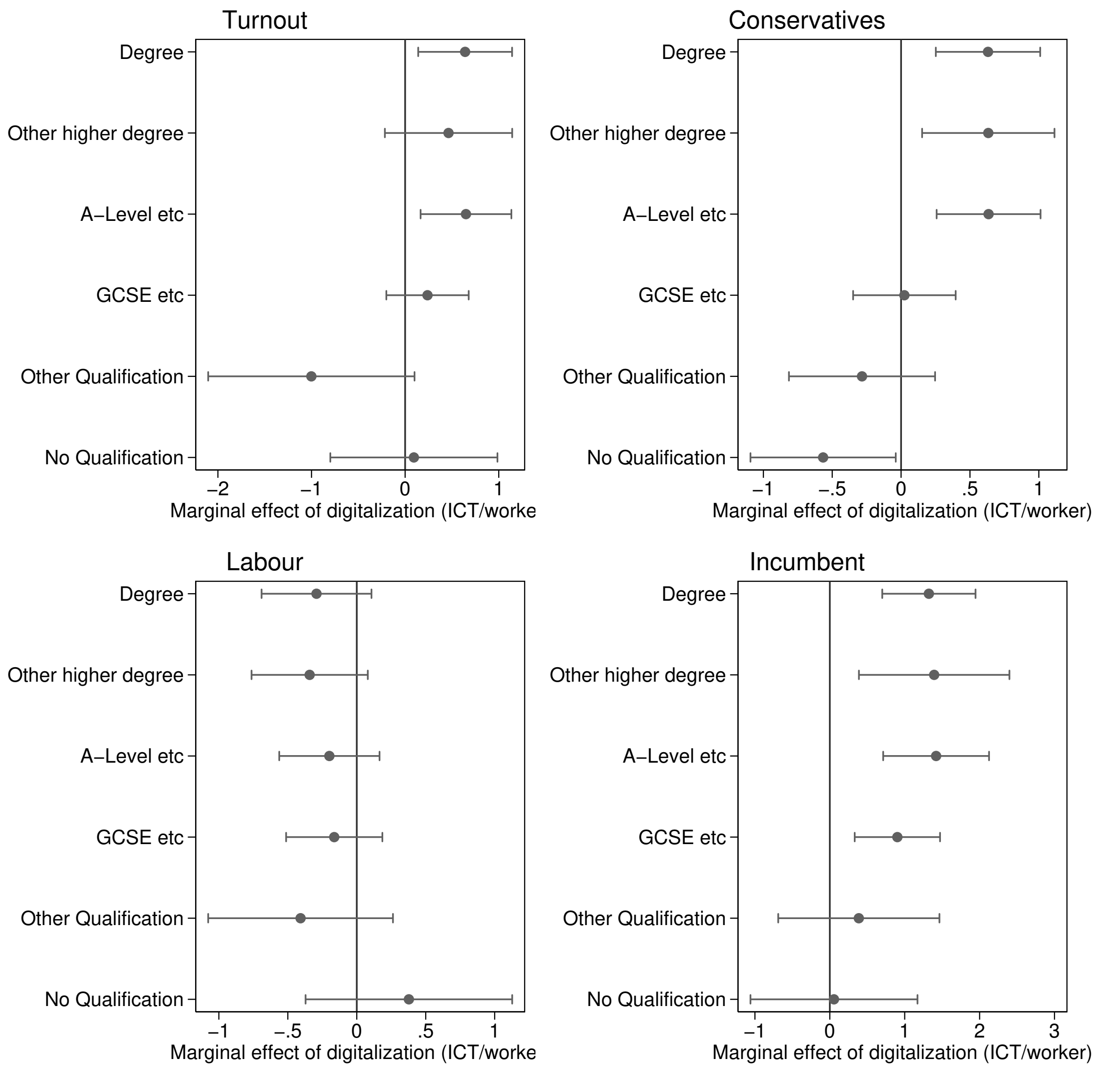
[20] A caveat is that information provided by the BHPS/UKHLS only refers to the individual employment situation in the week when they are interviewed. As explained above, the surveys do not ask about unemployment spells between surveys, so we do not observe if workers lost their job but found a new one before their next interview. Thus, our analyses cannot be interpreted as the impact on the probability of losing a job, only about being unemployed at the time of the next survey. A second caveat is that negative effects of digitalization on employment may still occur in some sectors, and they could be particularly concentrated in some sectors such as the ones that use specific labor-substituting technologies such as robots (Acemoglu and Restrepo, 2017).

education), whose wage gains come at the cost of slightly increased unemployment risk, this share increases to 61% of the population. Workers with secondary education (GCSE and similar) make for about a fifth of the population and experience neither positive nor negative income effects from digitalization. Unambiguous losers of digitalization, at least with regard to wages, are concentrated in groups with low formal educational credentials, which account for about 20% of the population.

5.2 Political outcomes

Our primary interest is in whether and how these distributive effects lead to changes in individual political behavior. Figure 3 presents the main results regarding voter turnout, support for the Conservative Party, for the Labour Party, and for the incumbent.

Figure 3: Effect of digitalization on political outcomes, industry-spells fixed effect specification



Note: Results show marginal effect of one unit increase in digitalization (1000GBP in ICT capital/worker) on probability to report to have voted or support a given political party. All results are in percentage points.

We find evidence of increasingly unequal political participation due to technological change. Highly educated workers in industries digitalizing more quickly become more likely to vote. A one unit increase in digitalization raises turnout among voters with university degrees by 0.64 percentage points. On the other hand, we find no effects or negative effects among less educated workers. Recent work has shown that the gaps in the turnout rates of citizens with high and low socio-economic status has increased over time in the UK and in other countries (Dalton, 2017; Heath, 2018). Our results suggest that digitalization contributes to increasing inequalities in voter turnout by (weakly) augmenting existing gaps. While we do not directly examine through which channels digitalization affects voter turnout, the findings are consistent with the material and psychological channels discussed above.

Next, we examine the relationship between digitalization and support for parties. The results provide clear evidence for increased support for the Conservatives among winners of technological change. For example, a 1000 GBP increase in the capital stock per worker is associated with an increase in support for the Conservatives of approximately 0.6 percentage points among the highly educated. For less educated workers, digitalization is associated with a reduction in support for the Conservatives.[21]

The results are consistent with our expectation that workers who benefit from digitalization may become more likely to support an economically right-wing party either due to self-interest reasons related to improvements in their material situation or to psychological channels in which positive experiences with creative destruction may workers hold more pro-market attitudes. The effect sizes are modest, but similar to other studies which usually find small effects of changes in economic circumstances on vote choices (Margalit, 2011; Colantone and Stanig, 2018*a*). It is worth emphasizing that the effects can accumulate over time, leading to more significant shifts in party support and that even modest changes in political behavior can be politically consequential as elections are often won by small margins.

With respect to support for the Labour Party, we do not find clear results. While the pattern is to some extent a weak mirror image of support for the Conservative party, the effects are weak and imprecisely estimated. This is true even among less qualified workers, which contrasts with previous research suggesting that losers of digitalization ask for more redistribution (Thewissen and Rueda, 2017). However, it should be noted that our industry-spell fixed-effect approach may underestimate the effects on the behavior of losers of digitalization since our analyses only capture political reactions of workers who remain in the labor market (see section 8.3 for an approach that includes displaced workers).

Finally, we also theorized plausible egotropic economic voting effects that are analytically distinct from voting decisions based on support or opposition to redistribution. The main hypothesis in this case is that through a simple reward-punishment mechanism, winners of digitalization become more likely to support the incumbent while losers withdraw

[21] The differences in the effects of digitalization for workers with university degrees and workers of the three lower education groups are statistically significant at conventional levels. The same is true for the difference between the top three education groups and the no qualification group.

support. The lower right panel of Figure 3 reports marginal effects of digitalization on support for the incumbent, defined as the Labour Party up to the elections in May 2010 and the Conservative Party afterwards. The results provide clear and strong evidence in line with the egotropic economic voting hypothesis: Being in a digitalizing environment increases the likelihood to support the incumbent, but only for highly educated workers (who benefit more from digitalization).

6 Interpretation: Analysis by period

Our analysis finds that digitalization increases support for the Conservative party and, even more clear-cut, for the incumbent among highly educated workers. In an attempt to more clearly distinguish between spatial voting and egotropic economic voting, we re-ran our analysis separately before and after the government change in 2010. Table 2 presents the results for each time period. For instance, column 1 reports the results about the effects of digitalization on support for the Labour Party when restricting the sample between 1997 and May 2010; column 2 reports the results between May 2010 and 2015; and column 3 reports the results for the whole period.

Our results are driven by the years after 2010. Column 1 shows that digitalization did not result in increased support for the Labour party during their period in government (until 2010). Columns 6 and 7, on the other hand, speaks in favor of an incumbency effect because the coefficients for incumbent voting are twice as large than for vote for Conservatives. Also, the Conservative Party did not benefit from digitalization when they were in opposition (pre-2010, column 4). If we count the Liberal Democrats as part of government for the years 2011 to 2014, the results on increased support for the incumbent among the winners of digitalization become even slightly stronger.

The findings are strongly consistent with the possibility that digitalization affects support for parties through two distinct mechanisms (spatial voting and economic voting), which can cancel each other out or reinforce each other depending on which party is in power. When the Labour Party governed, winners of digitalization could become more likely to support parties that oppose redistribution (the Conservative Party) and simultaneously become more likely to support the Labour Party because of pocketbook economic voting. Because the two mechanisms push in opposite directions, the effects cancel each

Table 2: Sub-period Analysis: Until May 2010 and after May 2010

	Vote for Labour			Vote for Conservatives			Incumenbent
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Pre May 2010	Post May 2010	Overall	Pre May 2010	Post May 2010	Overall	Overall
Degree × ICT	0.315	-0.700*	-0.292	0.198	0.930*	0.631**	1.155***
	(0.237)	(0.349)	(0.204)	(0.193)	(0.375)	(0.193)	(0.305)
Other higher degree × ICT	0.0101	-0.0930	-0.341	0.331	1.185**	0.633**	1.199*
	(0.317)	(0.421)	(0.215)	(0.324)	(0.448)	(0.245)	(0.466)
A-Level etc × ICT	0.000339	-0.340	-0.199	0.475*	1.040**	0.635***	1.284***
	(0.230)	(0.379)	(0.186)	(0.227)	(0.364)	(0.193)	(0.347)
GCSE etc × ICT	0.122	-0.301	-0.163	-0.111	0.682	0.0239	0.817**
	(0.219)	(0.402)	(0.178)	(0.263)	(0.391)	(0.190)	(0.283)
Other Qualification × ICT	-0.327	-0.491	-0.407	-0.288	0.521	-0.284	0.103
	(0.455)	(0.586)	(0.343)	(0.316)	(0.579)	(0.269)	(0.516)
No Qualification × ICT	0.171	0.00833	0.378	-0.404	-0.701	-0.566*	-0.00389
	(0.413)	(0.819)	(0.382)	(0.304)	(0.664)	(0.269)	(0.551)
Degree	-0.180	-1.234	3.347	-1.591	-10.55**	-7.278***	-8.847**
	(3.172)	(4.276)	(2.261)	(2.718)	(3.599)	(1.877)	(3.412)
Other higher degree	-1.166	-5.722	1.089	1.538	-11.17**	-4.835*	-8.494*
	(3.557)	(4.110)	(2.304)	(3.257)	(3.456)	(1.986)	(3.668)
A-Level etc	1.128	-3.667	1.001	-2.037	-10.10**	-6.151***	-8.498**
	(2.815)	(3.902)	(2.058)	(2.416)	(3.244)	(1.715)	(3.004)
GCSE etc	2.024	-0.491	1.746	-0.0882	-9.970***	-3.597*	-8.890**
	(2.764)	(3.683)	(1.924)	(2.524)	(3.028)	(1.684)	(3.021)
Other Qualification	-2.249	-0.260	0.0600	0.497	-5.107	-0.265	-1.625
	(2.246)	(3.243)	(1.789)	(2.264)	(2.736)	(1.661)	(2.495)
Age	0.247	0.449	0.592	0.0121	0.138	0.173	-0.0538
	(0.408)	(0.552)	(0.327)	(0.336)	(0.485)	(0.279)	(0.504)
Age × Age	0.00507	-0.0128***	-0.00518**	-0.00230	-0.00193	-0.00281	-0.00222
	(0.00263)	(0.00314)	(0.00176)	(0.00227)	(0.00278)	(0.00156)	(0.00293)
Constant	45.15**	52.50*	48.47***	11.25	26.13	17.02	67.09***
	(14.15)	(20.88)	(10.59)	(11.73)	(17.70)	(9.023)	(15.65)
Individual*Industry FE	X	X	X	X	X	X	X
Year FE	X	X	X	X	X	X	X
Region FE	X	X	X	X	X	X	X
Observations	105130	126190	231320	105130	126190	231320	231320

Note: The table reports the effect of digitalization on different party choices by education group before and after the government change in 2010. Columns (1) and (4) reports the pre-government change results for Labour and Conservative vote respectively. Columns (2) and (5) report on the results after the government change. Columns (3) and (6) cover the whole period. Column (7) reports the coefficient for incumbency. Standard error reported in parenthesis are clustered at the individual level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

other out and we find no effects of digitalization in this period. When the Conservative Party was in power, by contrast, both mechanisms push in the same pro-Conservative direction for winners of digitalization, resulting in more visible effects.

Importantly, our results are not driven by differential economic effects of digitalization in the two periods studied, which also coincide with the Great Recession. Additional analyses presented in section S4 in the SI show that the estimates of the effects of digitalization on hourly wages and unemployment are quite similar in the two periods studied.

In the same section S4 of the SI we also present results for the Liberal Democratic Party (LibDem) and UKIP. We argued previously that the LibDems constitute an intermediate case in terms of preference for redistribution and we thus did not expect to find effects of digitalization on voting for this party. This is indeed confirmed by the results. For UKIP we find a large point estimate for the no qualification group which is in line with the "revenge of the left behind" narrative. However, we do not want to over-stress this result as it is based on a small sample (UKIP was only included in the last three waves of the survey).

7 Instrumental variables analysis

As discussed in section 4.2, to mitigate concerns about endogeneity of our measure of digitalization, we instrument ICT capital stocks in the UK, with analogous data from the United States. Tables 3 and 4 present the results of the instrumental variables analysis next to the baseline results.

Table 3: Instrumental Variable Results: Economics

	Hourly net wage		Probability to become unemployed	
	(1)	(2)	(3)	(4)
	Main specification	Instrumental variable	Main specification	Instrumental variable
Degree × ICT	0.415***	0.595***	-0.00245	0.177
	(0.0419)	(0.100)	(0.0716)	(0.211)
Other higher degree × ICT	0.226***	0.582**	-0.0562	0.133
	(0.0451)	(0.212)	(0.0611)	(0.271)
A-Level etc × ICT	0.0907**	0.252*	0.133*	0.330
	(0.0284)	(0.111)	(0.0574)	(0.238)
GCSE etc × ICT	0.00386	0.0856	0.175*	0.532
	(0.0208)	(0.0863)	(0.0684)	(0.430)
Other Qualification × ICT	-0.106**	-0.0256	0.0987	0.393
	(0.0340)	(0.189)	(0.0895)	(0.263)
No Qualification × ICT	-0.163***	-0.140	0.242*	0.467
	(0.0397)	(0.124)	(0.109)	(0.459)
Degree	-2.281***	-2.586***	1.324	1.605
	(0.231)	(0.361)	(0.793)	(1.256)
Other higher degree	-2.126***	-3.032***	1.971*	2.105
	(0.234)	(0.561)	(0.798)	(1.239)
A-Level etc	-1.720***	-1.929***	0.815	1.075
	(0.183)	(0.305)	(0.684)	(1.102)
GCSE etc	-0.977***	-1.018***	0.970	0.771
	(0.164)	(0.261)	(0.652)	(1.195)
Other Qualification	-0.435**	-0.492	1.028	1.009
	(0.142)	(0.375)	(0.637)	(0.944)
Age	0.236***	0.231***	-0.369**	-0.380**
	(0.0383)	(0.0392)	(0.122)	(0.124)
Age × Age	-0.00317***	-0.00315***	0.00116	0.00129*
	(0.000233)	(0.000244)	(0.000620)	(0.000641)
Constant	0.402		11.01*	
	(1.109)		(4.392)	
Individual*Industry FE	X	X	X	X
Year FE	X	X	X	X
Region FE	X	X	X	X
Observations	193063	165046	213154	185136
First stage F-stat		110.0		111.7

Note: The table compares results of our main specification to an instrumental variable approach. Probability to become unemployed refers to the probability of being unemployed at the time of the next interview. It is reported in percentage points. Standard error reported in parenthesis are clustered at the individual level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 4: Instrumental Variable Results: Political outcomes

	Turnout		Conservatives		Labour		Incumbent	
	(1) Main	(2) IV	(3) Main	(4) IV	(5) Main	(6) IV	(7) Main	(8) IV
Degree × ICT	0.641*	1.550*	0.631**	2.264**	-0.292	0.452	1.323***	2.496
	(0.256)	(0.643)	(0.193)	(0.721)	(0.204)	(0.507)	(0.318)	(1.357)
Other higher degree × ICT	0.464	2.074*	0.633**	1.908**	-0.341	0.282	1.393**	2.492
	(0.346)	(1.025)	(0.245)	(0.685)	(0.215)	(0.670)	(0.513)	(1.273)
A-Level etc × ICT	0.650**	2.087*	0.635***	1.793**	-0.199	-0.595	1.419***	2.324*
	(0.246)	(0.973)	(0.193)	(0.609)	(0.186)	(0.545)	(0.361)	(0.914)
GCSE etc × ICT	0.239	1.313	0.0239	1.332	-0.163	0.426	0.902**	2.589**
	(0.225)	(0.931)	(0.190)	(0.685)	(0.178)	(0.641)	(0.290)	(0.963)
Other Qualification × ICT	-1.002	1.993	-0.284	1.568	-0.407	0.581	0.388	3.809
	(0.564)	(1.777)	(0.269)	(0.989)	(0.343)	(0.940)	(0.551)	(1.982)
No Qualification × ICT	0.0934	2.467	-0.566*	0.478	0.378	0.0424	0.0556	1.536
	(0.457)	(2.973)	(0.269)	(1.083)	(0.382)	(1.423)	(0.569)	(1.965)
Degree	-1.687	1.716	-7.278***	-7.966*	3.347	0.704	-11.60***	-10.48
	(3.321)	(6.108)	(1.877)	(3.140)	(2.261)	(3.723)	(3.513)	(5.702)
Other higher degree	-3.443	-2.339	-4.835*	-4.882	1.089	-1.199	-11.25**	-10.28
	(3.984)	(6.581)	(1.986)	(3.162)	(2.304)	(3.824)	(3.800)	(5.683)
A-Level etc	-5.841*	-3.936	-6.151***	-5.643*	1.001	1.565	-10.64***	-9.363
	(2.861)	(5.673)	(1.715)	(2.743)	(2.058)	(3.445)	(3.108)	(4.810)
GCSE etc	-4.832	-2.463	-3.597*	-3.895	1.746	-0.325	-11.38***	-12.06*
	(2.912)	(5.649)	(1.684)	(2.785)	(1.924)	(3.349)	(3.144)	(4.805)
Other Qualification	-0.389	-1.875	-0.265	-1.796	0.0600	-3.082	-3.000	-7.958
	(2.275)	(5.748)	(1.661)	(2.925)	(1.789)	(3.174)	(2.641)	(4.981)
Age	-1.542**	-1.494**	0.173	0.0966	0.592	0.664*	-0.0858	-0.00582
	(0.481)	(0.489)	(0.279)	(0.287)	(0.327)	(0.334)	(0.539)	(0.549)
Age × Age	-0.00853**	-0.00913**	-0.00281	-0.00218	-0.00518**	-0.00577**	-0.000580	-0.000336
	(0.00260)	(0.00284)	(0.00156)	(0.00163)	(0.00176)	(0.00184)	(0.00303)	(0.00312)
Constant	146.5***		17.02		48.47***		68.95***	
	(15.60)		(9.023)		(10.59)		(16.70)	
Individual*Industry FE	X	X	X	X	X	X	X	X
Year FE	X	X	X	X	X	X	X	X
Region FE	X	X	X	X	X	X	X	X
Observations	108146	85517	231320	197203	231320	197203	229320	195498
First stage F-stat		117.3		75.74		75.74		76.17

Note: The table compares results of our main specification to an instrumental variable approach. Column (1) and (2) compare the results for turnout, column (3) and (4) support for the Conservatives, column (5) and (6) support for Labour and column (7) and (8) support for the incumbent. All outcomes are in percentage points . For the instrumental variable models, the first-stage F-statistic for the instrument is reported at the bottom of the table. Standard error reported in parenthesis are clustered at the individual level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

All economic and political results remain qualitatively unchanged, although the instrumental variable approach tends to produce larger point estimates. Obtaining larger IV estimates is not infrequent (e.g. Dasgupta, 2018) and could be due to different reasons. A small part of the difference between our main specification and the IV is due to differences in the sample used. As explained in section 4.2, EUKLEMS does not provide data for two industries in the US resulting in a slightly smaller and more homogeneous sample. When we rerun the main analyses on the restricted sample, the coefficients become somewhat closer to the IV results. Measurement error may also contribute to explain the larger IV coefficients if ICT capital stocks are better measured in a larger economy like the US.

More substantively, the difference between the coefficients suggests that our measure of digitalization in the UK is indeed endogenous. One possible reason is that policy in the UK may work to limit the polarizing effects of digitalization on economic and political outcomes. Another reason could be that industrial policy in the UK might lead to an inefficient allocation of ICT investment across industries. Yet another explanation could be that trade unions pressure firms to mitigate the strongest symptoms of digitalization on workers' material and psychological well-being. All three processes would result in attenuation bias in our main specification.

8 Robustness Checks

We run a series of additional robustness checks in order to rule out alternative interpretations and further endogeneity concerns. These tests demonstrate that our findings are not driven by our choice of model specification and are robust to multiple modeling approaches. The full regression tables are presented in the SI.

8.1 Placebo Test

First, we need to rule out the possibility that an increase in ICT capital stocks simply reflects the fact that booming industries have a larger capacity to invest and offer their workers higher wages and better conditions. If the general propensity to invest of a sector has an effect on workers' economic outcomes and political preferences, this could invalidate our interpretation of our results. They would not capture the specific consequences of digitalization but rather the effect of working in a thriving industry.

To assess this possibility, we conduct a placebo test using non-ICT capital stock per worker as the main explanatory variable:

$$\text{Non-ICT capital intensity}_{jt} = \frac{\text{Total capital stock}_{jt} \text{ - ICT capital stock}_{jt}}{(\text{Employees}_{jt})}$$

Changes in an industry's non-ICT capital stock do not predict any of the outcomes we are interested in. As can be seen in column (3) in the tables presented in the SI, the coefficients are very small and imprecisely estimated. This was to be expected since we argued that investment in digitalization substitutes or complements labor in a specific way depending on their skill level. The same is not true for other kinds of capital investments (e.g. building a new production plant or buying a new office building).

The placebo test increases our confidence in the interpretation that the main results are driven specifically by ICT capital, since other kinds of capital do not affect workers' political preferences in a similar way.

8.2 Excluding outliers and allowing for regional heterogeneity

One might object that our results could be driven by a few rapidly digitalizing industries. To rule out this possibility, we excluded the three industries with the largest increase in digitalization in recent years (Telecommunications, Mining and Quarrying and Coke, Refined petroleum) in the models in column (4). The exclusion of these outliers does not change results. If anything, it even increases the precision of our estimates.

Relatedly, our results could also be driven by some particularly rapidly digitalizing regions such as the metropolitan area of London. To account for this, we interact the time fixed effects with regional dummies. Column (5) in the tables presented in the SI confirms that the results are not driven by these regions, as point estimates remain largely unchanged for all outcomes while standard errors decrease for some outcomes.

8.3 Including more workers who may have been displaced by technology

Another key concern is that our models are too restrictive towards losers and thus may underestimate the effects of digitalization because they miss the negative effects on workers who are displaced by digitalization and do not work in the same industry in the next period when they are re-interviewed. This could happen for two different reasons. If displaced workers drop out of the labor force they would not be assigned to an industry in the next interview and would therefore drop out of our analysis. If they switch to a different industry, the industry-spell fixed effects would absorb part of the effect of job displacement on economic and political outcomes. In any case, our models may fail to capture the effects of digitalization on some displaced workers workers.

We deal with this concern by relaxing the sample restriction in two ways and thus potentially capturing more losers: First, we replicate all analyses using lead models in which we examine how our measure of digitalization affects labor market and political outcomes measured at the time of the next interview. In this way, we keep in our sample all workers who may have been displaced by digitalization (and either exit the labor force or work in a different industry). This results in a slightly smaller sample, but the coefficients reported in column (6) confirm that the results remain unchanged when using leads. The only exception is voter turnout, as several of the coefficients of interest become statistically non-significant.

Second, we replicate all analyses using a unique individual fixed effect by respondent instead of industry-spell fixed effects. Using this approach, workers who change industries (perhaps in response to job displacement due to technology) contribute to the average estimates of the effect of digitalization on labor market and political outcomes, although workers who drop out of the labor force entirely are still excluded from the sample. The results are reported in column (7) in the tables in the SI. Although the polarizing effect of digitalization on wages is still clearly visible, this specification results in smaller estimates of the effects of digitalization on hourly pay for both highly and less educated workers. This was to be expected as using unique individual fixed effects adds large measurement error to our treatment variable which causes attenuation bias in the estimated coefficients.[22] An alternative explanation is that economic benefits of digitalization are reaped mostly by educated workers who stay in their industries while the costs may be borne also by less educated workers who choose to stay in the same industries. Using this specification, we do not find effects of digitalization on voter turnout, but we still observe that digitalization is associated with increased support for the Conservatives and the incumbent party among workers with more education.

8.4 Including controls for trade

A possible threat to identification is that our indicator of technology may be correlated with changes in international trade in an industry. In that case, our estimates would partially capture effects of international trade on economic outcomes and political behavior. However, previous work on the geography of trade shocks and technological change in the

[22] The variation in digitalization created by industry switches is much larger than the year to year variation for stayers which is problematic for two reasons. First, frequent back and forth switches between two industries within individuals is possibly due to measurement error in the interviews. Second, we theorize that a digitalizing workplace is what affects political attitudes, not the jumps when switching between highly and low digitalized industries.

US shows that the two types of shocks have largely distinct distributions in space (Autor, Dorn and Hanson, 2015), suggesting that there is limited overlap. In any case, we replicate all the analysis controlling for international trade in the industries for which we can collect data. Specifically, we use yearly UN Comtrade data on exports from China to the UK as an indicators of international trade.[23] This measure is only available for manufacturing industries, resulting in a much smaller sample size. The results presented in column (8) in the SI show that the results remain unchanged when controlling for changes in trade within the industries for which data are available.

8.5 Panel attrition

Attrition is a key concern in panel data analysis. In our case, one may worry that digitalization causes differential attrition rates between winners and losers. For instance, workers displaced by digitalization can be more likely to move and become more difficult to be located for reinterview. In addition, as discussed above, displacement may force workers to change industries. Higher attrition rates and more industry switches would both make it difficult for us to capture the adverse effects of digitalization, painting an exceedingly optimistic picture.

To examine if digitalization in an industry predicts sample attrition and industry switches, table S10 in the SI first presents the results of regressing the likelihood of dropping out of the sample or changing industries on ICT capital per worker. Next, we examine if these effects are heterogeneous for workers with different education levels by regressing both outcomes on the education dummies and the interaction of ICT capital per worker and education.

The results are reassuring as we do not find clear evidence that ICT capital per worker is associated with increased attrition. While the average effect of our key measure of digitalization is in fact negative, suggesting that workers in rapidly digitalizing industries are less likely to drop out of the panel, this difference is very small. Second, digitalization is not clearly associated with a stronger likelihood to change to a different industry in the next period for none of the education groups. In sum, differences between groups are small. It thus seems unlikely that differential attrition is driving our main results.

[23]The data is provided for different types of goods which we first crosswalk to SIC and from there to NACE rev. 2 codes which is used in EUKLEMS.

9 Discussion

The digital revolution is accompanied by two fears: that many workers will be displaced from their jobs and that this will lead to political unrest. Public debate and the scarce academic literature on this topic has primarily been concerned with its downsides and focused on the losers of technological progress. While this focus is comprehensible in the light of recent political disruptions, we contend that the imbalance in attention is at odds with standard economic theories emphasizing productivity gains as well as with historical experience, which has proved many gloomy projections wrong.

We document two main economic effects of digitalization. Contrary to pessimistic accounts, a majority of workers benefit economically from rapid digitalization in their industries. Yet, these benefits are not equally distributed and they disproportionately accrue to the highly educated. Our most novel finding is that these diverging economic trajectories are mirrored in diverging political trajectories. First of all, with respect to turnout, we observe that digitalization reinforces inequalities along education lines: The highly educated turn out more to vote if their sector digitalizes whereas we do not find such mobilizing effects among the less educated. Second, regarding party choice, the beneficiaries of digitalization become more likely to support the Conservative Party, in particular when they form the government. The large but often neglected pool of voters who benefit from technological innovation thus seems willing to support established mainstream parties and uphold the existing social contract.

There are several substantively relevant reasons why our results seem more optimistic about the economic and political consequences of technological change than previous work. First of all, we look at the average effects of a general set of technologies (ICT) on the workforce. This approach is likely to produce different results than if we had focused on the impact of specific technologies, such as industrial robots, that may have particularly strong displacement effects. In fact, Acemoglu and Restrepo (2017) show that industrial robots have strong negative effects on employment and wages, whereas the effects of increases in other ICT capital, such as computers per worker or investment in software and computers, are often *positive*. Clearly, some technologies have stronger labor-displacement effects than others. Relatedly, our approach leads us to include all sectors rather than mostly manufacturing, a sector which has seen particularly sharp reductions in employment in advanced economies, but is overall rather small.[24] Our coverage of all sectors with a more general measure of digitalization possibly facilitates identifying gains of technological change and results in a more optimistic picture.

Another reason why our conclusions may be relatively optimistic is related to our empirical approach. We deliberately study the political implications of digitalization on the active labor force, not on the population as a whole, and we study individual effects, which can differ from contextual effects. Using a longitudinal approach, we find little indication of political disruption but rather business as usual. We do not include in our sample retired or disabled people, students or people doing housework, even though workplace digitalization may affect them through various channels including the changes in communities and spillovers within the household. Some segments of this population might react more negatively, e.g. workers who lose their job and cannot find a new one or young citizens with troubles entering the labor market in the first place, although the size of these groups is too small to produce large differences. Finally, contextual effects can operate even on people in the labor force in ways that are not captured by our empirical approach. For these reasons, we do not make inferences based on our findings to population-wide political effects.

To conclude, our findings reveal a complex and presumably more realistic picture of the political consequences of technological change. The innovative empirical analysis provides abundant and robust evidence that digitalization is economically beneficial for a majority of the labor force. And it is politically consequential in two contrasting ways: First, the large group of winners become more likely to support incumbent mainstream parties and thus can act as a stabilizing force in democratic systems. Second, while we do not find evidence of an anti-establishment backlash as a reaction to digitalization, we demonstrate that the economic polarization associated with digitalization is accompanied by differential political effects on winners and losers of this process. The resulting divergence in political behavior between the two groups could translate quite directly into increasing political polarization. All in all, however, the implications of digitalization at the workplace are more multi-faceted than the simple narrative of the "revenge of the left-behind" suggests.

[24] In the US, the workforce in the manufacturing sector has never exceeded 20% in the post-war period and currently represents less than 13%, a figure that according to the Office for National Statistics stands below 10% in the UK.

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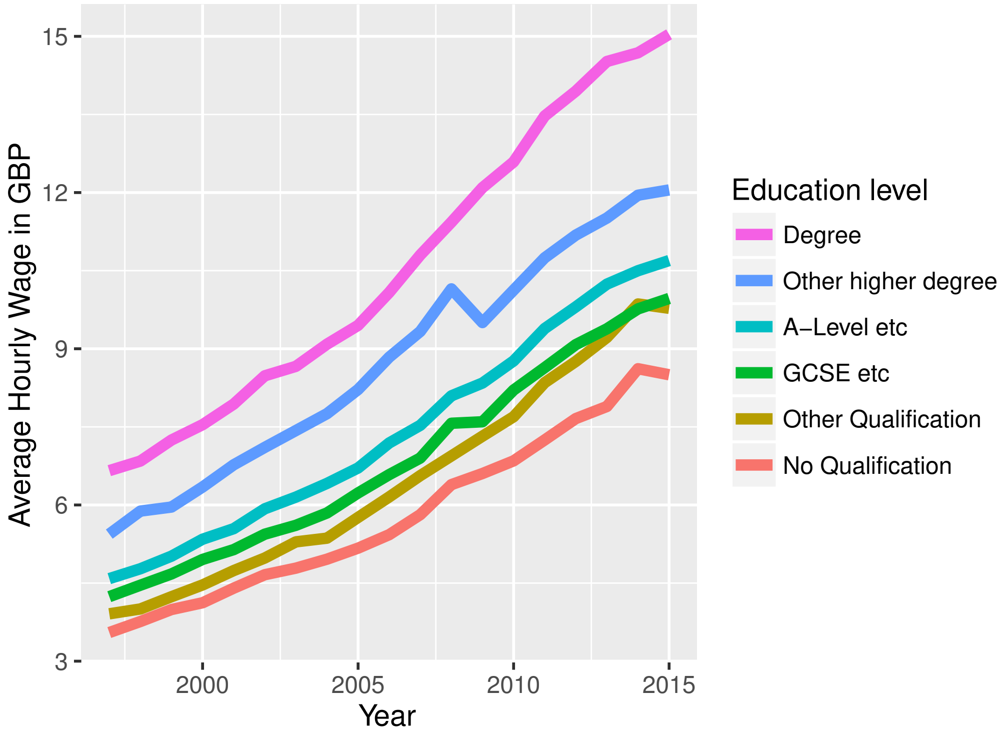
SUPPORTING INFORMATION

Table of Contents

S1 Description of the data

This section presents the longitudinal evolution of our dependent variables between 1997 and 2015, dividing the sample by education level. Figure S1 plots the average net hourly wage. As in the main analysis, we use constant 2010 prices. The wages of all educational groups have increased over time. In the period until the financial crisis, the growth was largely similar for all income groups, but there is a divergence after the crisis between respondents with university degrees and the rest.

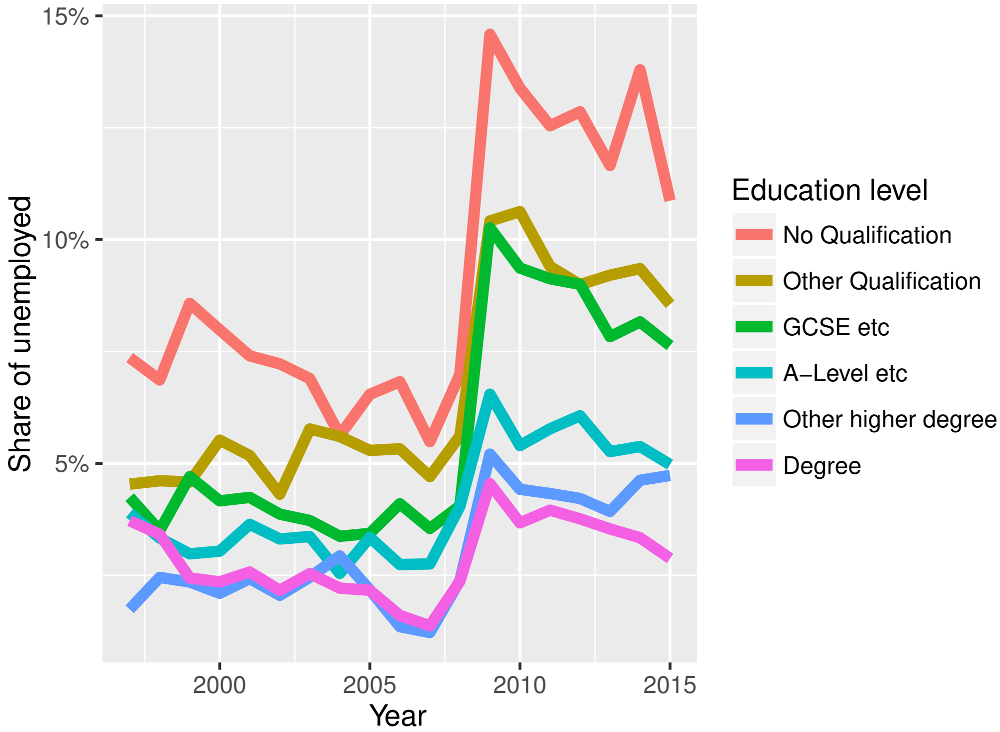
Figure S1: Average hourly net wage by education



Note: Hourly net wage calculated as monthly net wage in constant 2010 prices normalized by average hour worked. Workers with less than half time employment (<20h) excluded. In 2009, BHPS is changed into US which results in the inclusion of new households into the sample.

Figure S2 presents the percentage of respondents who were unemployed in the week when the interview was conducted. Here again we observe some divergence, as increases in unemployment after the crisis were particularly visible among citizens with less education. Note that unemployment shares in our actual sample are smaller because those who stay unemployed for two periods are not captured by our operationalization.

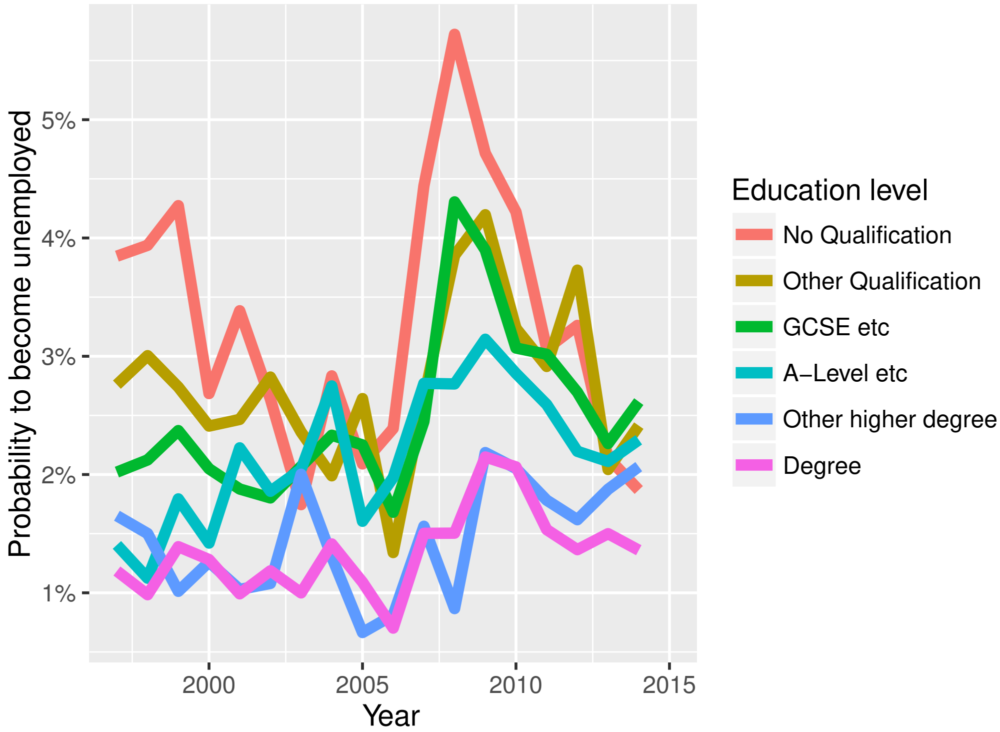
Figure S2: Share unemployed by education



Note: Share unemployed at the time of the interview.

Figure S3 describes the probability to become unemployed (i.e. to be unemployed at the time of next interview). Again, we see that less educated respondents are more likely to become unemployed and there is an increase after the financial crisis of 2008.

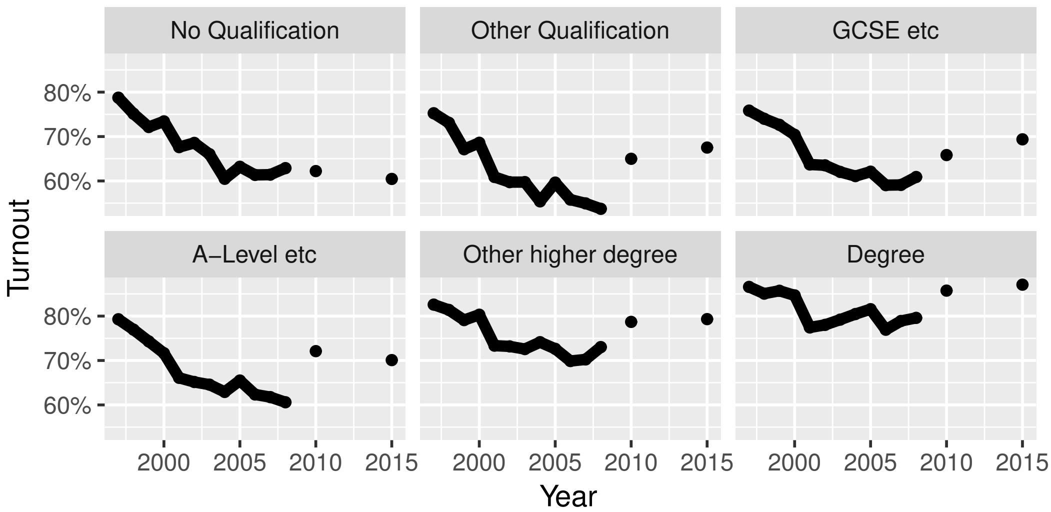
Figure S3: Probability to become unemployed in the next period by education



Note: Average probability to become unemployed in the next interview for different education groups. Currently unemployed and respondents without any industry assignment are excluded to ensure equivalence with the main analysis. In 2009, BHPS is changed into US which results in the inclusion of new households into the sample.

Figure S4 plots reported turnout for different education levels. Note that this was only asked infrequently after 2008. There was a steady decline in turnout until the mid 2000s and then a partial recovery. Turnout is consistently higher for the highly educated.

Figure S4: Reported voter turnout by education

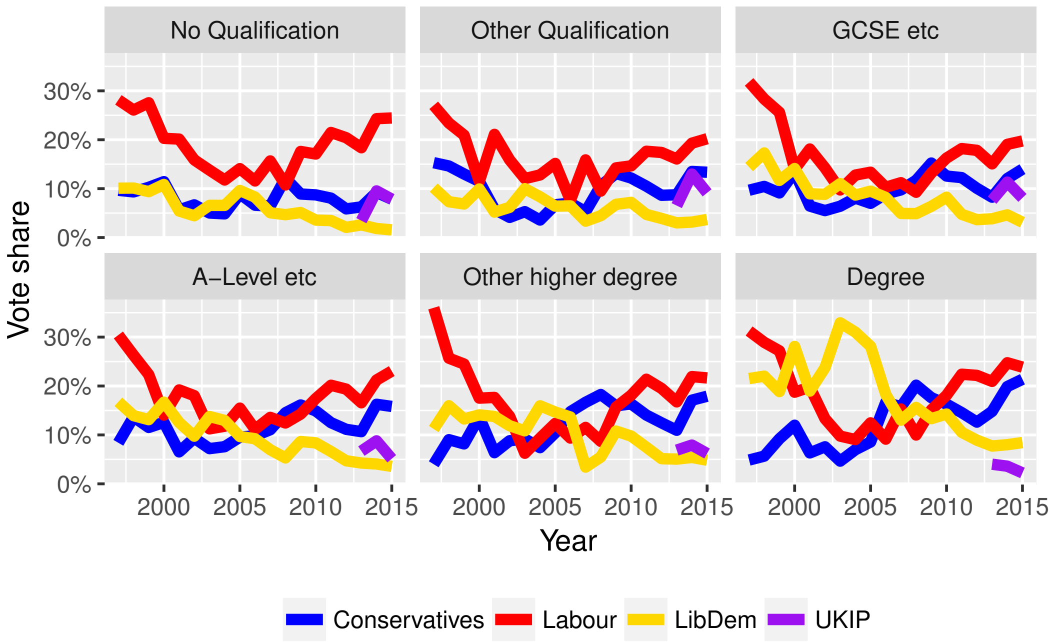


Note: Participation in elections was asked in all waves of BHPS which ended in 2008. In the Understanding Society Survey, participation in elections was only asked in 2010 and 2015.

Figure S5 plots the average support for the political parties included in the analyses:

the Conservative Party, the Labour Party, as well as the Liberal-Democratic Party, and UKIP (since 2013). We observe a markedly different evolution of support for parties for different education groups, with support for the Conservatives having grown most among workers with university degrees, at the expense of the Liberal-Democratic Party. Some of the time trends will be captured by the year fixed effects.

Figure S5: Support for political parties by education



Note: Vote shares calculated based on sample responses answering they voted for the respective party divided by the number of responses for any party including other parties not reported here.

S2 Crosswalking and Merging Data Sets

The BHPS, UKHLS and the EU KLEMS datasets are provided using different classifications, which we address by constructing cross-walks. We are able to match the 2007 version of the Standard Industrial Classification (SIC07), used between 2009 and 2015 comprehensively to the classification scheme used by EU KLEMS (NACE Rev. 2). We also manually construct cross-walks from SIC 1992, used in 1994, 1997 and from 2001 to 2008, and are able to match the vast majority of respondents. Between 1991 and 2001 the BHPS used the SIC 1980, which differs markedly from the following versions. We use another crosswalk to translate SIC-80 codes into SIC-92 codes, which then allows to merge the remaining years of EU-KLEMS data. This procedure generates an individual-level data set with information on ICT capital per industry ranging from 1997 to 2015.

S3 Comparison of RTI and education as key dimension

In this section, we show that while education is a strong moderator predicting if workers stand to gain or lose from workplace digitalization, RTI seems to be less relevant.

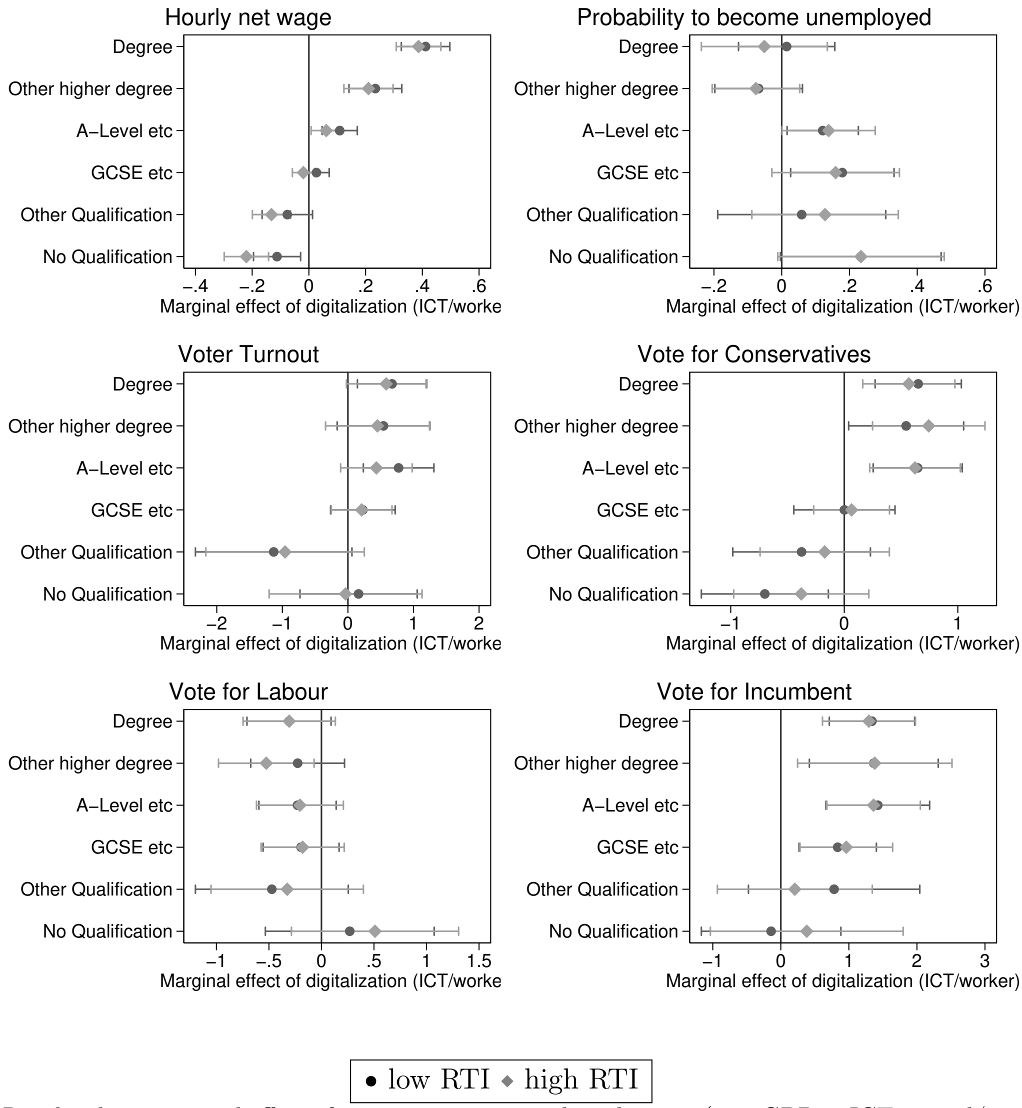
Specifically, we created occupation-specific RTI scores from ONET data following the standard approach of Autor and Dorn (2013), i.e. subtracting log abstract and log manual content from log routine content of each occupation, and relying on a crosswalk by Hardy and colleagues (2018) to merge data with European occupational codes. We then split the observations in high and low RTI groups if they are above or below the median of RTI in the sample.

Figure S6 shows that high RTI workers in general benefit less from digitalization in terms of wages, as we would expect, but the differences are not statistically significant. By contrast, the strong education gradient suggests that digitalization affect highly and less educated workers in very heterogeneous ways. We learn from this analysis that when looking at individual trajectories, education seems to be a more important source of heterogeneity in the impact of digitalization than RTI.

Given the strong emphasis in the economics literature on the distinction between routine and non-routine occupations, this finding is somewhat surprising. However, this literature looks mostly at aggregate level economic outcomes and we discuss in the text several reasons why our within-individual effects may diverge. We believe that education may be a better proxy than RTI for the ability of workers to adapt to and benefit from digitalization. RTI may predict which jobs are more likely to be partially or fully conducted by machines, but it does not predict well if the individual worker performing a job will benefit or lose from digitalization. The difference between the aggregate level and micro level results are worth further empirical exploration.

In any case, the empirical findings reported here are a strong motivation for our decision of concentrating on education as the key moderator of the effects of workplace digitalization on economic and political outcomes.

Figure S6: Main outcomes split by high and low RTI



Note: Results show marginal effect of one unit increase in digitalization (1000GBP in ICT capital/worker) on hourly wage, probability to become unemployed and probability to report to have voted or support a given political party. All results except for the hourly wage are in percentage points. High RTI and low RTI is defined relative to the median RTI of the sample.

S4 Economic Effects Before and After the 2010 Government Change

Table S1 shows a sub-period analysis for our economic outcomes. It compares the results for hourly net wages and the probability to become unemployed for the time before and after the government change in 2010. The results are comparable to the composite effects. Main difference seems to be that in the 2010 onwards period, low educated workers did not seems to lose out in terms of wages in absolute term when they were effected by digitalization. Nevertheless, digitalization decreased their relative wage performance as the effect of digitalization on the wages of the higher educated increases over time.

Table S1: Economic effects pre and post Government change in May 2010

	Hourly Wage		Unemployment	
	(1)	(2)	(3)	(4)
	Pre May 2010	Post May 2010	Pre May 2010	Post May 2010
Degree × ICT	0.338***	0.441***	-0.0577	-0.0268
	(0.0391)	(0.0546)	(0.108)	(0.112)
Other higher degree × ICT	0.176***	0.279***	-0.145	-0.154
	(0.0473)	(0.0535)	(0.0859)	(0.131)
A-Level etc × ICT	0.0526*	0.198***	0.203	0.170
	(0.0261)	(0.0460)	(0.107)	(0.122)
GCSE etc × ICT	-0.0252	0.138**	0.217*	0.0921
	(0.0212)	(0.0488)	(0.0979)	(0.152)
Other Qualification × ICT	-0.104**	0.0714	0.131	-0.0402
	(0.0359)	(0.0994)	(0.142)	(0.213)
No Qualification × ICT	-0.201***	0.0283	0.237	0.179
	(0.0473)	(0.0874)	(0.132)	(0.251)
Degree	-1.439***	-2.272***	3.243*	1.174
	(0.238)	(0.432)	(1.270)	(1.954)
Other higher degree	-1.437***	-1.794***	4.712***	2.357
	(0.258)	(0.373)	(1.363)	(1.792)
A-Level etc	-1.196***	-1.542***	1.335	0.366
	(0.161)	(0.438)	(1.010)	(1.712)
GCSE etc	-0.756***	-0.673	1.627	0.894
	(0.163)	(0.364)	(1.035)	(1.621)
Other Qualification	-0.302*	-0.404	1.289	2.018
	(0.137)	(0.273)	(0.868)	(1.676)
Age	0.214***	0.305***	-0.264	-0.152
	(0.0316)	(0.0715)	(0.169)	(0.230)
Age × Age	-0.00299***	-0.00428***	-0.000129	-0.000346
	(0.000255)	(0.000413)	(0.000927)	(0.00148)
Constant	0.440	3.563	4.106	7.603
	(1.001)	(2.586)	(6.523)	(8.939)
Id*Ind FE	X	X	X	X
Year FE	X	X	X	X
Region	X	X	X	X
Observations	88960	104103	104604	108550

Note: All columns use our main specification. Column (1) and (2) report a reoport a sub-erpiod analysis for net hourly wages (calculated as monthly net wage in constant 2010 prices normalized by average hour worked. Workers with less than half time employment (<20h) excluded). Column (3) and (4) report a sub-period analysis for probability to become unemployed in percentage points (ie. to be unemployed at the next interview conditional on currently working). Standard error reported in parenthesis are clustered at the individual level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

S5 Robustness checks: Full tables

This section presents the tables discussed in the main text and the robustness checks.

For the sake of completeness, we also add a cross-sectional OLS regression including only industry and year fixed effects to see how between-worker differences in ICT intensity relate to our outcomes (column 9). Results have to be interpreted with a large grain of salt as we now cannot control for unobserved worker-level characteristics anymore. Instead, except for the inclusion of a gender dummy, we tried to stay as close as possible to our main specification to ensure the comparability of results while avoiding post-treatment bias. The results for political outcomes are surprisingly similar to the fixed-effects specification. Especially, they confirm the finding that digitalization increase support for the Conservatives for the incumbent among highly educated workers.

Regarding economic outcomes, the results change slightly. The highly educated are still the main beneficiaries when it comes to wages. However, looking at unemployment, less educated people already working in digitalized industries appear to benefit from digitalization as they have lower probabilities to become unemployed. This is somewhat counter-intuitive and seemingly opposite to our findings from the baseline specification. Yet, the two diverging results make sense considering the different nature of the two analyses. The cross-sectional analysis shows that working in an already digitalized industry reduces the risk of unemployment whereas the fixed-effects specification shows that for a given worker in a given industry, increasing digitalization might threaten the jobs of less educated workers if tasks are automated.

Table S2: Net hourly wages in GBP

	(1) Main	(2) IV	(3) Placebo	(4) Region*Year FE	(5) Excl. outliers	(6) Lead	(7) ID FE	(8) Trade	(9) Cross Sect
Degree × ICT	0.415***	0.595***	-0.000941	0.402***	0.514***	0.391***	0.185***	0.515***	0.154***
	(0.0419)	(0.100)	(0.000741)	(0.0395)	(0.0533)	(0.0491)	(0.0190)	(0.0883)	(0.0102)
Other higher degree × ICT	0.226***	0.582**	-0.000685	0.226***	0.287***	0.225***	0.138***	0.359***	0.131***
	(0.0451)	(0.212)	(0.000569)	(0.0446)	(0.0663)	(0.0537)	(0.0207)	(0.0669)	(0.0126)
A-Level etc × ICT	0.0907**	0.252*	-0.00105*	0.0885**	0.129**	0.0854**	0.105***	0.211*	0.151***
	(0.0284)	(0.111)	(0.000496)	(0.0283)	(0.0427)	(0.0315)	(0.0173)	(0.0928)	(0.00868)
GCSE etc × ICT	0.00386	0.0856	-0.000828	0.00130	0.0181	0.00722	0.0632***	0.0503	0.133***
	(0.0208)	(0.0863)	(0.000464)	(0.0207)	(0.0341)	(0.0228)	(0.0156)	(0.0415)	(0.00874)
Other Qualification × ICT	-0.106**	-0.0256	-0.00150**	-0.115***	-0.105*	-0.103**	0.0551**	-0.0779	0.110***
	(0.0340)	(0.189)	(0.000545)	(0.0341)	(0.0419)	(0.0319)	(0.0199)	(0.0589)	(0.0120)
No Qualification × ICT	-0.163***	-0.140	-0.00144**	-0.168***	-0.174***	-0.132***	0.0191	-0.195*	0.0490***
	(0.0397)	(0.124)	(0.000505)	(0.0389)	(0.0507)	(0.0388)	(0.0232)	(0.0923)	(0.0122)
Degree	-2.281***	-2.586***	-0.842***	-2.232***	-2.492***	-1.929***	-1.262***	-2.721***	4.802***
	(0.231)	(0.361)	(0.194)	(0.230)	(0.249)	(0.242)	(0.179)	(0.636)	(0.0523)
Other higher degree	-2.126***	-3.032***	-1.303***	-2.107***	-2.265***	-2.015***	-1.669***	-2.508***	2.670***
	(0.234)	(0.561)	(0.181)	(0.234)	(0.263)	(0.252)	(0.181)	(0.671)	(0.0527)
A-Level etc	-1.720***	-1.929***	-1.335***	-1.696***	-1.771***	-1.581***	-1.484***	-2.000***	1.626***
	(0.183)	(0.305)	(0.149)	(0.184)	(0.198)	(0.177)	(0.145)	(0.442)	(0.0426)
GCSE etc	-0.977***	-1.018***	-0.792***	-0.967***	-1.002***	-0.960***	-0.901***	-1.346***	0.988***
	(0.164)	(0.261)	(0.140)	(0.168)	(0.177)	(0.155)	(0.136)	(0.360)	(0.0399)
Other Qualification	-0.435**	-0.492	-0.404***	-0.396**	-0.443**	-0.431**	-0.491***	-0.370	0.443***
	(0.142)	(0.375)	(0.117)	(0.142)	(0.148)	(0.133)	(0.121)	(0.330)	(0.0474)
Age	0.236***	0.231***	0.288***	0.267***	0.229***	0.277***	0.279***	0.155	0.463***
	(0.0383)	(0.0392)	(0.0399)	(0.0392)	(0.0382)	(0.0425)	(0.0394)	(0.0813)	(0.00582)
Age × Age	-0.00317***	-0.00315***	-0.00337***	-0.00318***	-0.00315***	-0.00344***	-0.00345***	-0.00185***	-0.00472***
	(0.000233)	(0.000244)	(0.000238)	(0.000233)	(0.000233)	(0.000278)	(0.000211)	(0.000451)	(0.0000750)
Imports								0.0000307	
								(0.0000350)	
Female									-1.217***
									(0.0242)
Constant	0.402	0.557	-0.835	-0.310	0.678	-0.122	-0.458	2.231	-8.093***
	(1.109)	(1.168)	(1.174)	(1.160)	(1.111)	(1.202)	(1.127)	(2.371)	(0.161)
Individual*Industry FE	X	X	X	X	X	X		X	
Year FE	X	X	X		X	X	X	X	X
Region FE	X	X	X		X	X	X	X	X
Year*Region FE				X					
Individual FE							X		
Industry FE							X		X
Observations	193063	187972	193063	193063	190056	151927	193063	34961	193063

Note: Hourly net wage calculated as monthly net wage in constant 2010 prices normalized by average hours worked. Workers with less than half time employment (<20h) excluded. Column (1) is our main specification with industry-spell fixed-effects. In column (2), we instrument ICT with data from the USA. Column (3) uses non-ICT capital per worker as main regressor. Column (4) is equivalent to the main specification with adding region by year fixed-effects. In column (5) we exclude the most digitalized industries. Column (6) uses the lead of the dependent variable. Column (7) uses individual fixed-effects and industry fixed effects. Column (8) includes a control for trade. Column (9) is a cross sectional analysis without individual fixed effects. Standard error reported in parenthesis are clustered at the individual level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table S3: Probability to become unemployed

	(1) Main	(2) IV	(3) Placebo	(4) Region*Year FE	(5) Excl. outliers	(6) Lead	(7) ID FE	(8) Trade	(9) Cross Sect
Degree × ICT	-0.00245	0.177	0.00000172	-0.000390	-0.0930	0.130	0.0531	-0.169	0.0118
	(0.0716)	(0.211)	(0.000685)	(0.0715)	(0.0794)	(0.114)	(0.0455)	(0.0997)	(0.0247)
Other higher degree × ICT	-0.0562	0.133	0.000936	-0.0457	-0.157	0.00838	0.0678	-0.230	0.0217
	(0.0611)	(0.271)	(0.00101)	(0.0613)	(0.0899)	(0.0752)	(0.0748)	(0.157)	(0.0294)
A-Level etc × ICT	0.133*	0.330	-0.000252	0.144*	0.132*	0.137	0.152**	-0.0780	0.000546
	(0.0574)	(0.238)	(0.000901)	(0.0580)	(0.0656)	(0.0953)	(0.0518)	(0.130)	(0.0267)
GCSE etc × ICT	0.175*	0.532	-0.000350	0.179**	0.165	0.254**	0.100	0.0466	-0.00449
	(0.0684)	(0.430)	(0.000748)	(0.0683)	(0.0908)	(0.0911)	(0.0511)	(0.105)	(0.0293)
Other Qualification × ICT	0.0987	0.393	-0.000299	0.0988	0.0159	0.102	0.0589	-0.0554	-0.00787
	(0.0895)	(0.264)	(0.00135)	(0.0897)	(0.0937)	(0.116)	(0.0886)	(0.285)	(0.0460)
No Qualification × ICT	0.242*	0.467	-0.000254	0.234*	0.245	0.323*	0.0202	0.0685	-0.0671
	(0.109)	(0.459)	(0.00120)	(0.109)	(0.138)	(0.162)	(0.0857)	(0.168)	(0.0462)
Degree	1.324	1.605	0.608	1.309	1.591*	0.783	-1.854*	3.420	-2.234***
	(0.793)	(1.256)	(0.744)	(0.794)	(0.812)	(1.005)	(0.849)	(1.933)	(0.210)
Other higher degree	1.971*	2.105	1.033	1.960*	2.237**	2.085*	-0.996	3.480	-1.896***
	(0.798)	(1.239)	(0.744)	(0.796)	(0.834)	(1.011)	(0.888)	(2.722)	(0.217)
A-Level etc	0.815	1.075	0.639	0.802	0.822	1.119	-0.887	1.030	-1.656***
	(0.684)	(1.102)	(0.634)	(0.684)	(0.708)	(0.877)	(0.753)	(1.392)	(0.212)
GCSE etc	0.970	0.771	0.853	0.944	1.023	1.033	-0.304	0.119	-1.092***
	(0.652)	(1.195)	(0.591)	(0.650)	(0.685)	(0.841)	(0.721)	(1.617)	(0.211)
Other Qualification	1.028	1.009	0.666	0.996	1.243	1.561*	-0.408	1.136	-0.700**
	(0.637)	(0.944)	(0.568)	(0.637)	(0.650)	(0.774)	(0.718)	(1.861)	(0.257)
Age	-0.369**	-0.380**	-0.369**	-0.373**	-0.380**	-0.278	-0.411***	-0.453	-0.446***
	(0.122)	(0.124)	(0.124)	(0.124)	(0.123)	(0.144)	(0.124)	(0.332)	(0.0236)
Age × Age	0.00116	0.00129*	0.00119	0.00119	0.00106	0.00202**	0.00366***	0.00295	0.00467***
	(0.000620)	(0.000641)	(0.000619)	(0.000621)	(0.000623)	(0.000778)	(0.000621)	(0.00176)	(0.000273)
Imports								0.0000628	
								(0.000120)	
Female									-0.498***
									(0.0740)
Constant	11.01*	10.77*	11.79**	11.40*	11.48**	6.936	12.61**	0.226	12.42***
	(4.392)	(4.526)	(4.488)	(4.499)	(4.427)	(4.373)	(4.213)	(11.16)	(0.627)
Individual*Industry FE	X	X	X	X	X	X		X	
Year FE	X	X	X		X	X	X	X	X
Region FE	X	X	X		X	X	X	X	X
Year*Region FE				X					
Individual FE							X		
Industry FE							X		X
Observations	213154	207754	213154	213154	210135	172419	213154	34645	213154

Note: Probability to become unemployed in percentage points among those currently working. Column (1) is our main specification with industry-spell fixed-effects. In column (2), we instrument ICT with data from the USA. Column (3) uses non-ICT capital per worker as main regressor. Column (4) is equivalent to the main specification with adding region by year fixed-effects. In column (5) we exclude the most digitalized industries. Column (6) uses the lead of the dependent variable. Column (7) uses individual fixed-effects and industry fixed effects. Column (8) includes a control for trade. Column (9) is a cross sectional analysis without individual fixed effects. Standard error reported in parenthesis are clustered at the individual level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table S4: Voted in last general elections

	(1) Main	(2) IV	(3) Placebo	(4) Region*Year FE	(5) Excl. outliers	(6) Lead	(7) ID FE	(8) Trade	(9) Cross Sect
Degree × ICT	0.641*	1.550*	0.00783	0.557*	1.035**	0.327	0.351*	0.339	-0.0183
	(0.256)	(0.643)	(0.00509)	(0.250)	(0.366)	(0.277)	(0.145)	(0.707)	(0.108)
Other higher degree × ICT	0.464	2.074*	0.00714	0.460	0.881	0.780	0.259	0.127	-0.141
	(0.346)	(1.025)	(0.00439)	(0.342)	(0.537)	(0.404)	(0.186)	(0.597)	(0.129)
A-Level etc × ICT	0.650**	2.087*	0.00632	0.661**	0.964**	1.018***	0.436**	-0.0299	0.160
	(0.246)	(0.974)	(0.00623)	(0.246)	(0.353)	(0.286)	(0.148)	(0.493)	(0.114)
GCSE etc × ICT	0.239	1.313	-0.00203	0.199	-0.221	0.195	0.343*	-0.0427	0.153
	(0.225)	(0.931)	(0.00491)	(0.224)	(0.382)	(0.248)	(0.152)	(0.460)	(0.116)
Other Qualification × ICT	-1.002	1.993	-0.00527	-1.076	-0.785	-0.142	0.175	-1.922	-0.427*
	(0.564)	(1.777)	(0.00772)	(0.563)	(0.546)	(0.439)	(0.243)	(1.158)	(0.179)
No Qualification × ICT	0.0934	2.467	0.00244	0.132	0.201	0.435	0.658*	-1.306	0.297
	(0.457)	(2.973)	(0.00571)	(0.455)	(0.641)	(0.473)	(0.264)	(0.787)	(0.186)
Degree	-1.687	1.716	-1.734	-1.462	-2.343	-3.553	-1.159	-19.36*	22.57***
	(3.321)	(6.109)	(3.242)	(3.306)	(3.417)	(3.451)	(2.861)	(9.726)	(0.705)
Other higher degree	-3.443	-2.339	-4.107	-3.816	-4.179	-7.720	-2.900	-23.57*	14.91***
	(3.984)	(6.582)	(3.771)	(3.950)	(4.151)	(4.082)	(3.372)	(11.24)	(0.763)
A-Level etc	-5.841*	-3.936	-5.393*	-5.782*	-6.231*	-6.129*	-3.624	-15.15*	10.93***
	(2.861)	(5.674)	(2.727)	(2.838)	(2.967)	(2.900)	(2.508)	(6.365)	(0.688)
GCSE etc	-4.832	-2.463	-4.564	-4.704	-3.816	-3.522	-4.726	-10.68	6.097***
	(2.912)	(5.650)	(2.780)	(2.888)	(3.057)	(3.048)	(2.490)	(6.651)	(0.679)
Other Qualification	-0.389	-1.875	-2.130	-0.0428	-0.428	0.197	-1.146	4.441	2.627**
	(2.275)	(5.749)	(2.027)	(2.250)	(2.307)	(2.370)	(1.816)	(5.113)	(0.827)
Age	-1.542**	-1.494**	-0.892	-0.950*	-1.599***	0.604	-1.149**	-1.229	1.959***
	(0.481)	(0.490)	(0.484)	(0.484)	(0.484)	(0.499)	(0.440)	(1.152)	(0.0786)
Age × Age	-0.00853**	-0.00913**	-0.00925***	-0.00884***	-0.00805**	-0.0107***	-0.00900***	0.0000360	-0.0112***
	(0.00260)	(0.00284)	(0.00259)	(0.00259)	(0.00262)	(0.00266)	(0.00227)	(0.00632)	(0.000940)
Imports								-0.000504	
								(0.000512)	
Female									0.112
									(0.296)
Constant	146.5***	142.0***	126.6***	127.7***	148.0***	86.34***	129.4***	136.8***	14.16***
	(15.60)	(16.91)	(15.89)	(15.91)	(15.73)	(17.51)	(15.23)	(39.14)	(2.157)
Individual*Industry FE	X	X	X	X	X	X		X	
Year FE	X	X	X		X	X	X	X	X
Region FE	X	X	X		X	X	X	X	X
Year*Region FE				X					
Individual FE							X		
Industry FE							X		X
Observations	108146	105171	108146	108146	106403	95363	108146	19908	108146

Note: Probability to report to have voted in last general election in percentage point. Column (1) is our main specification with industry-spell fixed-effects. In column (2), we instrument ICT with data from the USA. Column (3) uses non-ICT capital per worker as main regressor. Column (4) is equivalent to the main specification with adding region by year fixed-effects. In column (5) we exclude the most digitalized industries. Column (6) uses the lead of the dependent variable. Column (7) uses individual fixed-effects and industry fixed effects. Column (8) includes a control for trade. Column (9) is a cross sectional analysis without individual fixed effects. Standard error reported in parenthesis are clustered at the individual level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table S5: Support for the Conservative Party

	(1) Main	(2) IV	(3) Placebo	(4) Region*Year FE	(5) Excl. outliers	(6) Lead	(7) ID FE	(8) Trade	(9) Cross Sect
Degree × ICT	0.631**	2.264**	0.00376	0.593**	0.769**	0.596**	0.424***	1.124	0.322***
	(0.193)	(0.721)	(0.00252)	(0.192)	(0.258)	(0.201)	(0.0991)	(0.593)	(0.0719)
Other higher degree × ICT	0.633**	1.908**	0.00596*	0.597*	1.044***	0.719**	0.212	-0.162	0.169*
	(0.245)	(0.685)	(0.00294)	(0.245)	(0.298)	(0.255)	(0.125)	(0.589)	(0.0827)
A-Level etc × ICT	0.635***	1.793**	0.00510	0.599**	1.147***	0.650***	0.347***	0.210	0.188*
	(0.193)	(0.609)	(0.00301)	(0.190)	(0.269)	(0.195)	(0.0998)	(0.346)	(0.0745)
GCSE etc × ICT	0.0239	1.332	0.00176	-0.00535	0.450	0.192	0.220*	-0.800*	0.261***
	(0.190)	(0.685)	(0.00301)	(0.186)	(0.250)	(0.178)	(0.108)	(0.378)	(0.0778)
Other Qualification × ICT	-0.284	1.568	-0.00446	-0.381	-0.143	-0.216	0.122	-0.736	-0.0524
	(0.269)	(0.989)	(0.00569)	(0.277)	(0.329)	(0.266)	(0.137)	(0.516)	(0.106)
No Qualification × ICT	-0.566*	0.478	-0.00194	-0.538*	-0.580	-0.415	-0.125	-1.258	-0.196
	(0.269)	(1.083)	(0.00405)	(0.268)	(0.331)	(0.276)	(0.150)	(0.842)	(0.108)
Degree	-7.278***	-7.966*	-5.339**	-7.093***	-7.235***	-7.173***	-5.052**	-20.44***	8.601***
	(1.877)	(3.140)	(1.778)	(1.881)	(1.915)	(1.842)	(1.568)	(5.861)	(0.429)
Other higher degree	-4.835*	-4.882	-3.233	-4.775*	-5.572**	-7.672***	-3.162	-4.073	11.32***
	(1.986)	(3.162)	(1.807)	(1.981)	(2.041)	(2.043)	(1.626)	(6.528)	(0.472)
A-Level etc	-6.151***	-5.643*	-4.499**	-6.066***	-7.181***	-8.134***	-4.775***	-7.242	9.454***
	(1.715)	(2.744)	(1.605)	(1.728)	(1.763)	(1.629)	(1.435)	(3.955)	(0.421)
GCSE etc	-3.597*	-3.895	-3.196*	-3.583*	-4.468**	-5.801***	-3.714**	-0.369	7.152***
	(1.684)	(2.786)	(1.592)	(1.694)	(1.722)	(1.617)	(1.375)	(4.527)	(0.418)
Other Qualification	-0.265	-1.796	0.386	0.112	-0.452	-2.407	-0.648	1.685	4.054***
	(1.661)	(2.925)	(1.396)	(1.653)	(1.706)	(1.556)	(1.251)	(5.239)	(0.509)
Age	0.173	0.0966	0.310	0.258	0.147	0.218	0.124	1.085	0.179***
	(0.279)	(0.287)	(0.282)	(0.282)	(0.282)	(0.297)	(0.264)	(0.701)	(0.0469)
Age × Age	-0.00281	-0.00218	-0.00313*	-0.00264	-0.00247	-0.00591***	-0.00153	-0.00706	0.00242***
	(0.00156)	(0.00163)	(0.00156)	(0.00156)	(0.00157)	(0.00173)	(0.00134)	(0.00406)	(0.000582)
Imports								0.000204	
								(0.000281)	
Female									-0.158
									(0.187)
Constant	17.02	16.95	11.89	13.81	17.52	18.62	13.80	-6.264	10.30***
	(9.023)	(9.497)	(9.286)	(9.306)	(9.106)	(10.00)	(8.327)	(21.68)	(1.453)
Individual*Industry FE	X	X	X	X	X	X		X	
Year FE	X	X	X		X	X	X	X	X
Region FE	X	X	X		X	X	X	X	X
Year*Region FE				X					
Individual FE							X		
Industry FE							X		X
Observations	231320	225834	231320	231320	228252	191163	231320	35775	231320

Note: Probability to report to support the Conservative Party in percentage point. Column (1) is our main specification with industry-spell fixed-effects. In column (2), we instrument ICT with data from the USA. Column (3) uses non-ICT capital per worker as main regressor. Column (4) is equivalent to the main specification with adding region by year fixed-effects. In column (5) we exclude the most digitalized industries. Column (6) uses the lead of the dependent variable. Column (7) uses individual fixed-effects and industry fixed effects. Column (8) includes a control for trade. Column (9) is a cross sectional analysis without individual fixed effects. Standard error reported in parenthesis are clustered at the individual level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table S6: Support for the Labour Party

	(1) Main	(2) IV	(3) Placebo	(4) Region*Year FE	(5) Excl. outliers	(6) Lead	(7) ID FE	(8) Trade	(9) Cross Sect
Degree × ICT	-0.292	0.452	-0.00503	-0.310	-0.203	-0.105	-0.178	-0.863	-0.494***
	(0.204)	(0.507)	(0.00301)	(0.201)	(0.267)	(0.208)	(0.101)	(0.849)	(0.0791)
Other higher degree × ICT	-0.341	0.282	-0.00186	-0.305	-0.349	-0.302	-0.000246	-0.462	-0.291**
	(0.215)	(0.670)	(0.00375)	(0.219)	(0.300)	(0.256)	(0.111)	(0.400)	(0.0895)
A-Level etc × ICT	-0.199	-0.595	-0.00443	-0.177	-0.509	-0.289	-0.196	0.0511	-0.448***
	(0.186)	(0.545)	(0.00374)	(0.184)	(0.265)	(0.195)	(0.106)	(0.543)	(0.0815)
GCSE etc × ICT	-0.163	0.426	-0.00509	-0.155	-0.476	-0.238	-0.157	0.534	-0.621***
	(0.178)	(0.641)	(0.00406)	(0.179)	(0.259)	(0.179)	(0.110)	(0.487)	(0.0869)
Other Qualification × ICT	-0.407	0.581	-0.00540	-0.384	-0.657	-0.137	0.0613	0.676	-0.328**
	(0.343)	(0.940)	(0.00754)	(0.341)	(0.393)	(0.336)	(0.157)	(0.795)	(0.118)
No Qualification × ICT	0.378	0.0424	-0.00655	0.328	0.259	0.248	0.228	0.430	-0.0537
	(0.382)	(1.424)	(0.00395)	(0.380)	(0.493)	(0.455)	(0.191)	(0.554)	(0.145)
Degree	3.347	0.704	1.636	3.159	2.458	2.124	3.491	9.883	0.559
	(2.261)	(3.723)	(2.067)	(2.258)	(2.352)	(2.381)	(2.000)	(6.067)	(0.564)
Other higher degree	1.089	-1.199	-0.970	0.912	0.671	-0.714	-0.0846	2.603	-4.689***
	(2.304)	(3.825)	(2.092)	(2.310)	(2.413)	(2.469)	(2.064)	(5.762)	(0.598)
A-Level etc	1.001	1.565	-0.477	0.696	1.176	0.143	1.173	-1.378	-2.950***
	(2.058)	(3.445)	(1.882)	(2.064)	(2.151)	(2.170)	(1.887)	(4.804)	(0.551)
GCSE etc	1.746	-0.325	0.544	1.644	1.959	0.440	1.002	-0.358	-3.599***
	(1.924)	(3.350)	(1.786)	(1.927)	(2.020)	(2.082)	(1.770)	(4.262)	(0.548)
Other Qualification	0.0600	-3.082	-1.809	-0.0993	0.369	-0.508	-0.645	-3.475	-4.382***
	(1.789)	(3.175)	(1.517)	(1.784)	(1.851)	(1.928)	(1.473)	(4.625)	(0.645)
Age	0.592	0.664*	0.429	0.448	0.621	0.461	0.666*	0.424	0.484***
	(0.327)	(0.334)	(0.329)	(0.329)	(0.329)	(0.353)	(0.315)	(0.781)	(0.0533)
Age × Age	-0.00518**	-0.00577**	-0.00487**	-0.00505**	-0.00523**	-0.0000660	-0.00533***	0.00437	-0.00553***
	(0.00176)	(0.00184)	(0.00175)	(0.00176)	(0.00178)	(0.00195)	(0.00153)	(0.00423)	(0.000649)
Imports								-0.000229	
								(0.000349)	
Female									-1.684***
									(0.210)
Constant	48.47***	47.78***	57.50***	55.60***	48.03***	32.88**	45.04***	36.41	41.20***
	(10.59)	(11.21)	(11.09)	(11.14)	(10.69)	(11.63)	(9.960)	(24.53)	(1.573)
Individual*Industry FE	X	X	X	X	X	X		X	
Year FE	X	X	X		X	X	X	X	X
Region FE	X	X	X		X	X	X	X	X
Year*Region FE				X					
Individual FE							X		
Industry FE							X		X
Observations	231320	225834	231320	231320	228252	191163	231320	35775	231320

Note: Probability to report to support the Labour Party in percentage point. Column (1) is our main specification with industry-spell fixed-effects. In column (2), we instrument ICT with data from the USA. Column (3) uses non-ICT capital per worker as main regressor. Column (4) is equivalent to the main specification with adding region by year fixed-effects. In column (5) we exclude the most digitalized industries. Column (6) uses the lead of the dependent variable. Column (7) uses individual fixed-effects and industry fixed effects. Column (8) includes a control for trade. Column (9) is a cross sectional analysis without individual fixed effects. Standard error reported in parenthesis are clustered at the individual level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table S7: Support for the Incumbent

	(1) Main	(2) IV	(3) Placebo	(4) Region*Year FE	(5) Excl. outliers	(6) Lead	(7) ID FE	(8) Trade	(9) Cross Sect
Degree × ICT	1.323***	2.496	0.0103	1.245***	1.957***	1.005**	0.968***	0.324	0.781***
	(0.318)	(1.357)	(0.00735)	(0.312)	(0.469)	(0.363)	(0.164)	(0.790)	(0.0808)
Other higher degree × ICT	1.393**	2.492	0.00693	1.423**	2.488***	1.095	0.898***	0.434	0.792***
	(0.513)	(1.273)	(0.00650)	(0.470)	(0.557)	(0.562)	(0.213)	(1.011)	(0.0918)
A-Level etc × ICT	1.419***	2.324*	0.00324	1.376***	2.156***	1.036*	0.885***	-0.0190	0.672***
	(0.361)	(0.914)	(0.00591)	(0.327)	(0.418)	(0.411)	(0.186)	(0.754)	(0.0828)
GCSE etc × ICT	0.902**	2.589**	-0.00142	0.853**	1.479***	1.060**	0.712***	-0.255	0.514***
	(0.290)	(0.963)	(0.00617)	(0.278)	(0.447)	(0.332)	(0.173)	(0.496)	(0.0876)
Other Qualification × ICT	0.388	3.809	-0.00992	0.206	0.599	0.337	0.913***	-1.386	0.662***
	(0.551)	(1.982)	(0.00920)	(0.554)	(0.664)	(0.540)	(0.250)	(1.282)	(0.119)
No Qualification × ICT	0.0556	1.536	-0.0227*	-0.0409	0.315	0.220	0.640*	-1.051	0.284*
	(0.569)	(1.965)	(0.0104)	(0.567)	(0.745)	(0.623)	(0.277)	(0.936)	(0.138)
Degree	-11.60***	-10.48	-11.91***	-10.63**	-12.28***	-8.791*	-10.08***	-17.16*	2.731***
	(3.513)	(5.702)	(3.189)	(3.436)	(3.725)	(3.751)	(2.959)	(8.648)	(0.542)
Other higher degree	-11.25**	-10.28	-10.98***	-10.76**	-13.07***	-10.61**	-9.963**	-21.40*	2.243***
	(3.800)	(5.683)	(3.295)	(3.659)	(3.921)	(4.038)	(3.131)	(9.780)	(0.582)
A-Level etc	-10.64***	-9.363	-10.27***	-10.19***	-11.76***	-8.971**	-8.894**	-11.96	1.493**
	(3.108)	(4.811)	(2.854)	(3.051)	(3.253)	(3.277)	(2.718)	(6.705)	(0.533)
GCSE etc	-11.38***	-12.06*	-11.90***	-11.22***	-11.89***	-11.74***	-11.86***	-16.31**	0.335
	(3.144)	(4.806)	(2.845)	(3.052)	(3.331)	(3.331)	(2.710)	(6.275)	(0.530)
Other Qualification	-3.000	-7.958	-4.544*	-3.135	-2.648	-2.978	-3.390	1.166	-1.746**
	(2.641)	(4.981)	(2.274)	(2.602)	(2.740)	(2.758)	(2.168)	(6.341)	(0.630)
Age	-0.0858	-0.00582	-0.0488	-0.159	-0.191	-0.660	-0.0591	-1.879	0.556***
	(0.539)	(0.549)	(0.528)	(0.529)	(0.542)	(0.546)	(0.538)	(1.341)	(0.0518)
Age × Age	-0.000580	-0.000336	-0.00138	-0.000436	0.000221	-0.00135	-0.0000848	-0.00449	-0.00411***
	(0.00303)	(0.00312)	(0.00295)	(0.00296)	(0.00305)	(0.00349)	(0.00268)	(0.00791)	(0.000636)
Imports								-0.00163*	
								(0.000640)	
Female									-0.298
									(0.204)
Constant	68.95***	64.55***	86.60***	86.77***	71.55***	67.88***	66.78***	120.1**	34.85***
	(16.70)	(17.49)	(17.32)	(17.36)	(16.86)	(17.16)	(16.88)	(40.17)	(1.613)
Individual*Industry FE	X	X	X	X	X	X		X	
Year FE	X	X	X		X	X	X	X	X
Region FE	X	X	X		X	X	X	X	X
Year*Region FE				X					
Individual FE							X		
Industry FE							X		X
Observations	229320	223879	229320	229320	226288	189965	229320	35482	229320

Note: Probability to report to support the incumbent in percentage point. Until May 2010, Labour is coded as the incumbent whereas the Conservatives after 2010. Column (1) is our main specification with industry-spell fixed-effects. In column (2), we instrument ICT with data from the USA. Column (3) uses non-ICT capital per worker as main regressor. Column (4) is equivalent to the main specification with adding region by year fixed-effects. In column (5) we exclude the most digitalized industries. Column (6) uses the lead of the dependent variable. Column (7) uses individual fixed-effects and industry fixed effects. Column (8) includes a control for trade. Column (9) is a cross sectional analysis without individual fixed effects. Standard error reported in parenthesis are clustered at the individual level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

S6 Other political outcomes

The following tables report the results of additional analyses examining if digitalization affects support for the Liberal Democratic Party and UKIP.

First, we do not find a change in the support for the Liberal Democratic Party among workers who experience digitalization. The Liberal Democratic Party is a centrist party, that includes both classical economic liberals as well as social-democrats. The two main wings have varying strengths across constituencies and over time. One possible interpretation of this finding is that these different factions within the party cancel each other out. It is furthermore noteworthy that it seems that Libdem could not capitalize from an incumbency advantage.

Second, we find some tentative evidence for increased UKIP support (only asked since 2013) among the lowest qualified respondents in our sample, which would be consistent with the possibility that digitalization makes losers more likely to support anti-establishment parties, in this case from the radical right. Among workers with no formal qualification, an increase in ICT intensity produces a substantively large increase in the likelihood to support UKIP. However, the point estimates are never significant . Furthermore, the results have to be interpreted with caution since they are based on a relatively small sample and a very short period of time, as the option to report support for the UKIP is only provided in the latest three waves of the Understanding Society survey.

Table S8: Support for the Liberal Democratic Party

	(1) Main	(2) IV	(3) Placebo	(4) Region*Year FE	(5) Excl. outliers	(6) Lead	(7) ID FE	(8) Trade	(9) Cross Sect
Degree × ICT	-0.0489	-1.650*	-0.00213	-0.0511	-0.0975	-0.0914	-0.0850	-0.656	0.165**
	(0.132)	(0.656)	(0.00217)	(0.132)	(0.204)	(0.159)	(0.0753)	(0.403)	(0.0527)
Other higher degree × ICT	0.0557	-0.934	-0.00173	0.0700	0.0293	-0.0600	-0.104	-0.0856	0.0666
	(0.172)	(0.648)	(0.00258)	(0.171)	(0.251)	(0.202)	(0.0857)	(0.377)	(0.0549)
A-Level etc × ICT	0.181	0.140	-0.000508	0.215	0.190	0.348**	-0.0749	0.102	0.209***
	(0.125)	(0.652)	(0.00206)	(0.125)	(0.182)	(0.134)	(0.0858)	(0.282)	(0.0521)
GCSE etc × ICT	0.0646	-0.932*	-0.00325	0.0816	0.122	0.174	0.0241	-0.0880	0.278***
	(0.129)	(0.447)	(0.00230)	(0.130)	(0.195)	(0.120)	(0.0806)	(0.476)	(0.0561)
Other Qualification × ICT	0.281	-0.527	0.00398	0.310	0.311	0.183	-0.0790	0.0760	0.216**
	(0.184)	(0.623)	(0.00469)	(0.182)	(0.228)	(0.215)	(0.139)	(0.377)	(0.0693)
No Qualification × ICT	0.276	0.213	0.00251	0.207	0.344	-0.0184	-0.0460	0.353	0.0756
	(0.218)	(0.796)	(0.00263)	(0.212)	(0.288)	(0.308)	(0.108)	(0.304)	(0.0721)
Degree	3.331*	7.052**	2.979*	3.299*	3.762**	2.033	3.152**	5.166	9.176***
	(1.355)	(2.358)	(1.204)	(1.356)	(1.425)	(1.590)	(1.205)	(3.340)	(0.320)
Other higher degree	3.046*	5.208*	3.027*	2.972*	3.467*	0.849	2.812*	1.494	4.711***
	(1.442)	(2.382)	(1.299)	(1.441)	(1.506)	(1.723)	(1.320)	(3.652)	(0.335)
A-Level etc	2.697*	1.974	2.889**	2.538*	2.995*	1.069	3.614***	1.762	3.380***
	(1.135)	(2.015)	(1.008)	(1.133)	(1.183)	(1.347)	(1.053)	(2.267)	(0.293)
GCSE etc	1.142	3.189	1.241	0.967	1.394	0.949	1.682	-0.236	1.403***
	(1.079)	(1.921)	(0.949)	(1.074)	(1.136)	(1.264)	(0.997)	(2.332)	(0.287)
Other Qualification	0.672	2.048	0.304	0.237	0.613	0.234	1.069	3.569	0.609
	(1.042)	(1.936)	(0.901)	(1.037)	(1.069)	(1.328)	(0.909)	(2.653)	(0.344)
Age	-0.0538	-0.0531	-0.169	-0.170	-0.0566	-0.0971	-0.174	-0.496	-0.256***
	(0.240)	(0.246)	(0.241)	(0.241)	(0.242)	(0.262)	(0.231)	(0.566)	(0.0333)
Age × Age	0.00163	0.00158	0.00189	0.00188	0.00152	0.00173	0.00277*	-0.00135	0.00382***
	(0.00128)	(0.00134)	(0.00128)	(0.00128)	(0.00129)	(0.00146)	(0.00115)	(0.00282)	(0.000409)
Imports								-0.0000653	
								(0.000215)	
Female									0.868***
									(0.136)
Constant	1.249	0.978	3.936	3.769	1.143	10.88	6.149	6.717	7.421***
	(7.368)	(7.668)	(7.657)	(7.678)	(7.438)	(8.267)	(7.074)	(18.67)	(0.961)
Individual*Industry FE	X	X	X	X	X	X		X	
Year FE	X	X	X		X	X	X	X	X
Region FE	X	X	X		X	X	X	X	X
Year*Region FE				X					
Individual FE							X		
Industry FE							X		X
Observations	231320	225834	231320	231320	228252	191163	231320	35775	231320

Note: Probability to report to support the Liberal Democratic Party in percentage point. Column (1) is our main specification with industry-spell fixed-effects. In column (2), we instrument ICT with data from the USA. Column (3) uses non-ICT capital per worker as main regressor. Column (4) is equivalent to the main specification with adding region by year fixed-effects. In column (5) we exclude the most digitalized industries. Column (6) uses the lead of the dependent variable. Column (7) uses individual fixed-effects and industry fixed effects. Column (8) includes a control for trade. Column (9) is a cross sectional analysis without individual fixed effects. Standard error reported in parenthesis are clustered at the individual level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table S9: Support for UKIP (only asked since 2013)

	(1) Main	(2) IV	(3) Placebo	(4) Region*Year FE	(5) Excl. outliers	(6) Lead	(7) ID FE	(8) Trade	(9) Cross Sect
Degree × ICT	0.00310	-1.679	0.0178	0.0491	-0.262	0.503	-0.282	-1.033	0.171
	(0.344)	(1.347)	(0.0120)	(0.343)	(0.561)	(0.505)	(0.314)	(0.748)	(0.364)
Other higher degree × ICT	-0.792	-2.701	-0.0182	-0.816	-0.347	-1.228	-0.235	-2.217	0.253
	(0.598)	(1.644)	(0.0148)	(0.596)	(0.818)	(0.751)	(0.351)	(1.709)	(0.364)
A-Level etc × ICT	-0.863	-3.234	-0.0193	-0.854	-0.612	-0.162	-0.295	-2.832	0.204
	(0.484)	(1.697)	(0.0148)	(0.483)	(0.604)	(0.453)	(0.328)	(1.926)	(0.363)
GCSE etc × ICT	0.267	-1.789	-0.0172	0.244	0.0883	0.260	-0.383	1.312	0.321
	(0.648)	(1.787)	(0.0142)	(0.649)	(0.778)	(0.502)	(0.350)	(1.706)	(0.369)
Other Qualification × ICT	-1.302	-1.140	-0.0202	-1.292	-1.269	0.142	-0.396	2.518	0.374
	(1.030)	(2.246)	(0.0301)	(1.027)	(1.175)	(1.067)	(0.418)	(5.370)	(0.381)
No Qualification × ICT	2.035	3.176	0.0214	2.049	2.088	1.796	0.108	15.45	0.394
	(1.262)	(2.909)	(0.0373)	(1.257)	(1.365)	(1.089)	(0.593)	(10.47)	(0.401)
Degree	8.463	14.51	3.234	8.237	9.348	2.552	1.455	84.42	-3.779***
	(5.248)	(7.896)	(5.025)	(5.249)	(5.380)	(4.260)	(4.170)	(59.43)	(0.778)
Other higher degree	10.21*	17.00*	5.697	10.39*	9.093	8.819*	0.199	76.49	-1.056
	(5.109)	(7.504)	(4.857)	(5.110)	(5.131)	(4.144)	(4.081)	(57.21)	(0.825)
A-Level etc	9.526	17.97*	4.323	9.517	9.147	5.996	0.978	75.34	-0.111
	(4.864)	(7.486)	(4.569)	(4.867)	(4.954)	(3.849)	(3.946)	(56.76)	(0.806)
GCSE etc	7.289	14.75*	4.968	7.409	7.998	0.802	1.812	72.40	1.232
	(5.047)	(7.291)	(4.808)	(5.054)	(5.133)	(4.173)	(3.941)	(55.82)	(0.822)
Other Qualification	14.42*	17.78*	8.593	14.38*	14.54*	5.285	5.623	63.79	1.013
	(5.960)	(8.057)	(5.571)	(5.974)	(6.047)	(3.661)	(4.674)	(49.11)	(0.968)
Age	1.308*	1.401*	1.250*	1.269*	1.273*	0.111	1.072	4.003	-0.0163
	(0.589)	(0.601)	(0.590)	(0.589)	(0.595)	(0.615)	(0.565)	(2.129)	(0.0562)
Age × Age	-0.00600	-0.00657	-0.00548	-0.00565	-0.00533	-0.00634	-0.00577	-0.0102	0.000972
	(0.00477)	(0.00484)	(0.00479)	(0.00479)	(0.00479)	(0.00491)	(0.00456)	(0.0166)	(0.000692)
Imports								0.000111	
								(0.000713)	
Female									-1.529***
									(0.203)
Constant	-36.82	-40.87	-32.01	-36.54	-37.02	6.739	-14.94	-175.6	6.898***
	(21.89)	(24.48)	(21.84)	(21.87)	(22.13)	(19.03)	(21.70)	(94.06)	(1.874)
Individual*Industry FE	X	X	X	X	X	X		X	
Year FE	X	X	X		X	X	X	X	X
Region FE	X	X	X		X	X	X	X	X
Year*Region FE				X					
Individual FE							X		
Industry FE							X		X
Observations	55172	53988	55172	55172	54519	53537	55172	7195	55172

Note: Probability to report to support the United Kingdom Independence Party in percentage point. Column (1) is our main specification with industry-spell fixed-effects. In column (2), we instrument ICT with data from the USA. Column (3) uses non-ICT capital per worker as main regressor. Column (4) is equivalent to the main specification with adding region by year fixed-effects. In column (5) we exclude the most digitalized industries. Column (6) uses the lead of the dependent variable. Column (7) uses individual fixed-effects and industry fixed effects. Column (8) includes a control for trade. Column (9) is a cross sectional analysis without individual fixed effects. Standard error reported in parenthesis are clustered at the individual level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

S7 Attrition

Table S10 tests if differential attrition is a concern. We see that workers who experience digitalization are on average *less* likely to leave the sample, and this effect is driven at least partially by the university educated. We never find that digitalization makes workers *more* likely to leave the sample or change industry which is reassuring.

Table S10: Attrition

	Leave sample		Change industry	
	(1)	(2)	(3)	(4)
ICT	-0.000553**		0.0000154	
	(0.000169)		(0.000204)	
Degree × ICT		-0.00274*		0.000595
		(0.00131)		(0.00178)
Other higher degree × ICT		-0.000809		-0.00147
		(0.00185)		(0.00200)
A-Level etc × ICT		-0.000310		0.00106
		(0.00128)		(0.00146)
GCSE etc × ICT		0.000636		-0.0000339
		(0.00121)		(0.00156)
Other Qualification × ICT		0.000685		-0.00265
		(0.00247)		(0.00342)
No Qualification × ICT		0.00374		0.00823
		(0.00297)		(0.00422)
Degree		0.110***		0.0627**
		(0.0174)		(0.0241)
Other higher degree		0.108***		0.0470*
		(0.0188)		(0.0235)
A-Level etc		0.0830***		0.00142
		(0.0156)		(0.0207)
GCSE etc		0.0488**		0.00971
		(0.0155)		(0.0205)
Other Qualification		0.0409**		0.0222
		(0.0143)		(0.0193)
Age		0.0279***		-0.0162***
		(0.00292)		(0.00340)
Age × Age		-0.000196***		0.000130***
		(0.0000129)		(0.0000176)
Constant	0.0538***	-0.753***	0.285***	0.481***
	(0.00501)	(0.0913)	(0.00810)	(0.109)
Id*Ind FE	X	X	X	X
Year FE	X	X	X	X
Region	X	X	X	X
Observations	253814	253814	220829	220829

Note: Column (1) reports the direct effect of ICT intensity on probably to leave the sample. Column (2) reports the effect of ICT intensity on the probability to leave the sample by education group. Column (3) reports the direct effect of ICT on the probably to change industries. Column (4) reports the effect of ICT on the probably to change industries by education group. Standard error reported in parenthesis are clustered at the individual level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

S8 Alternative Clustering

Table S11 shows that our results are robust when we cluster standard errors at the industry-year level rather than the individual level. Given that our main variable of interest is the interaction between education (an individual-level characteristic) and digitalization (a time-varying industry characteristic), it is not straightforward which level of clustering is preferable. This table shows that when clustering at the industry-year level, standard errors tend to be somewhat smaller than in the results presented in the main text.

Table S11: All outcomes with standard errors clustered at the industry-year level

	(1) Hourly wage	(2) Unemployed	(3) Turnout	(4) Conservative	(5) Labour	(6) Incumbent
Degree × ICT	0.415***	-0.00245	0.641**	0.631***	-0.292	1.323***
	(0.0393)	(0.0919)	(0.224)	(0.161)	(0.166)	(0.385)
Other higher degree × ICT	0.226***	-0.0562	0.464	0.633**	-0.341	1.393***
	(0.0371)	(0.0742)	(0.340)	(0.208)	(0.189)	(0.393)
A-Level etc × ICT	0.0907***	0.133	0.650*	0.635***	-0.199	1.419***
	(0.0218)	(0.0725)	(0.259)	(0.163)	(0.157)	(0.340)
GCSE etc × ICT	0.00386	0.175*	0.239	0.0239	-0.163	0.902***
	(0.0183)	(0.0860)	(0.279)	(0.186)	(0.176)	(0.267)
Other Qualification × ICT	-0.106***	0.0987	-1.002	-0.284	-0.407	0.388
	(0.0313)	(0.104)	(0.573)	(0.236)	(0.347)	(0.429)
No Qualification × ICT	-0.163***	0.242*	0.0934	-0.566*	0.378	0.0556
	(0.0370)	(0.103)	(0.499)	(0.225)	(0.313)	(0.426)
Degree	-2.281***	1.324	-1.687	-7.278***	3.347	-11.60***
	(0.257)	(0.852)	(3.246)	(1.833)	(2.066)	(3.153)
Other higher degree	-2.126***	1.971*	-3.443	-4.835*	1.089	-11.25***
	(0.220)	(0.881)	(3.823)	(1.889)	(2.168)	(3.005)
A-Level etc	-1.720***	0.815	-5.841*	-6.151***	1.001	-10.64***
	(0.157)	(0.702)	(2.931)	(1.589)	(1.832)	(2.529)
GCSE etc	-0.977***	0.970	-4.832	-3.597*	1.746	-11.38***
	(0.116)	(0.703)	(2.913)	(1.547)	(1.959)	(2.514)
Other Qualification	-0.435***	1.028	-0.389	-0.265	0.0600	-3.000
	(0.123)	(0.687)	(2.537)	(1.444)	(1.701)	(2.153)
Age	0.236***	-0.369**	-1.542**	0.173	0.592	-0.0858
	(0.0324)	(0.125)	(0.525)	(0.291)	(0.353)	(0.434)
Age × Age	-0.00317***	0.00116	-0.00853**	-0.00281	-0.00518**	-0.000580
	(0.000206)	(0.000696)	(0.00287)	(0.00146)	(0.00185)	(0.00201)
Constant	0.402	11.01*	146.5***	17.02*	48.47***	68.95***
	(0.946)	(4.434)	(17.62)	(8.319)	(10.08)	(13.26)
Individual*Industry FE	X	X	X	X	X	X
Year FE	X	X	X	X	X	X
Region FE	X	X	X	X	X	X
Observations	193063	213154	108146	231320	231320	229320

Note: All columns use the main specification. Column (1) reports the results for hourly wage, column (2) for the probability to become unemployed, column (3) for voter turnout, column (4) for vote for the Conservatives, column (5) for vote for Labour and column (6) for vote for the incumbent. Standard error reported in parenthesis are clustered at the industry-year level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$