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Labor Market Adjustments to Population Decline: A Historical Macroeconomic Perspective, 1875-2019

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Abstract

Advanced economies will face population decline in the years and decades to come, particularly among those of working age. Yet, there is little empirical evidence of corresponding labor market implications. Tackling this shortcoming from a historical macroeconomic point of view, we compile a new dataset for sixteen advanced economies, covering demographic and labor market variables on an annual basis from 1875 to 2019. Based on a dynamic, nonlinear econometric model, we identify structural population shocks by using lagged births as external instruments for working-age population inflows and outflows, and trace the economic effects conditionally on the demographic regime. Our results suggest regime-specific differences: First, population decline quickly passes through to the labor market, translating into swifter disinvestment and decline in employment, but the effects of population growth take time. Second, in times of population decline, labor force participation increases as a response to reduced labor supply. Likewise, initially swift disinvestment tendencies decelerate. Consequently, we find only incomplete capital adjustment. Third, despite a declining labor supply, we find neither a decrease in unemployment nor any significant changes in wages as indicators of shortage. Finally, while population decline tends to depress total factor productivity, as also suggested by the literature, our results indicate that negative effects for economic growth are mitigated by increases in participation and the capital-labor ratio.

Zusammenfassung

In den kommenden Jahren und Jahrzehnten werden Industrienationen mit Bevölkerungsrückgängen, insbesondere im erwerbsfähigen Alter, konfrontiert sein. Dennoch gibt es bisher wenig empirische Evidenz zu entsprechenden Arbeitsmarktimplikationen. Wir adressieren diese Forschungslücke aus einer historischen, makroökonomischen Perspektive und stellen einen neuen Datensatz für sechzehn Industrienationen zusammen, der demografische und ökonomische Variablen von 1875 bis 2019 auf jährlicher Basis enthält. Auf Grundlage der Ergebnisse eines dynamischen, nichtlinearen ökonometrischen Modells und unter Zuhilfenahme verzögerter Geburten als externe Instrumente identifizieren wir strukturelle Bevölkerungsschocks und analysieren die ökonomischen Effekte von Bevölkerungsänderungen in Abhängigkeit des vorherrschenden demografischen Regimes. Unsere Ergebnisse deuten regimespezifische Unterschiede hin: Erstens, Bevölkerungsrückgang wirkt sich im Vergleich zu Bevölkerungswachstum rascher auf den Arbeitsmarkt aus, was sich insbesondere in Rückgängen von Investitionen und Beschäftigung widerspiegelt. Zweitens, in Zeiten von Bevölkerungsrückgang beobachten wir aber in der Folge und als Reaktion auf das sinkende Arbeitsangebot eine steigende Erwerbsbeteiligung sowie sich abschwächende Rückgänge von Investitionen. Drittens, trotz des sinkenden Arbeitsangebots finden wir weder einen Rückgang der Arbeitslosigkeit noch einen Anstieg von Löhnen. Während sich Bevölkerungsrückgang tendenziell negativ auf die Produktivität auswirkt, wie auch in der Literatur argumentiert wird,

deuten unsere Ergebnisse darauf hin, dass daraus resultierende negative Effekte auf das Wirtschaftswachstum durch eine erhöhte Erwerbsbeteiligung und Kapitalintensität abgefangen werden.

JEL classification

J11, J21, E22, E24

Keywords

Population decline, labor market adjustments, historical dataset, smooth transition regression, proxy VAR

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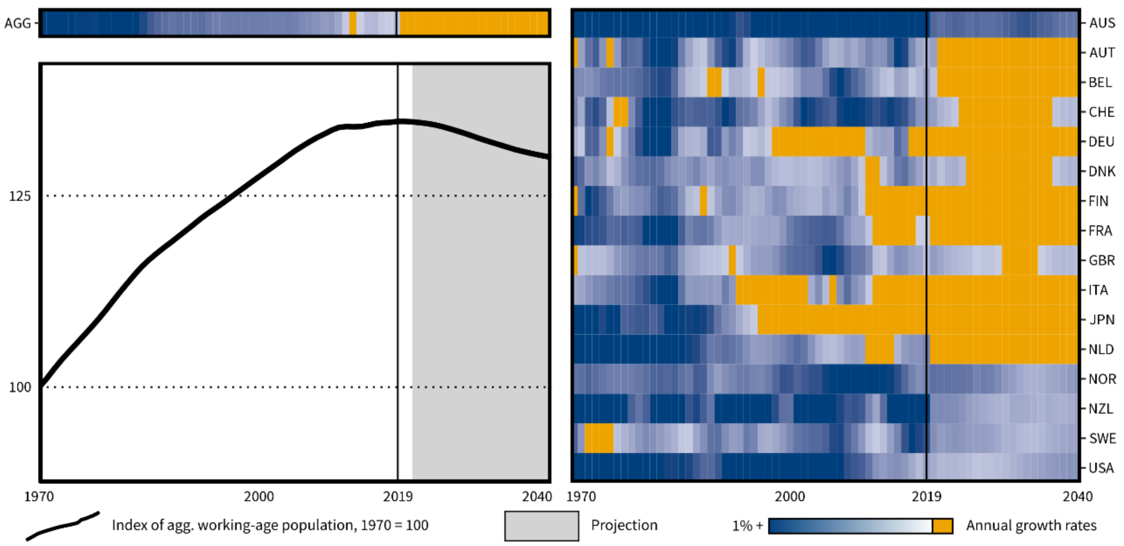
1 Introduction

A hitherto stylized fact, the perpetual growth of the population, is questioned in the short to medium and the long term by a range of demographic forecasts across countries (e.g., UN 2019). In the years and decades to come, depending on the scenario under consideration, advanced economies will face a stagnation and, sooner or later, a secular decline of their populations. The latter is expected to be particularly pronounced among those of working age. In fact, as the solid line and the color gradient in the left-hand side pane of Figure 1 illustrate, the aggregate working-age population of advanced economies has grown (blue) during the past five decades but has already passed its “tipping point” and is now in decline (orange). Additionally, as the data for individual countries in the right-hand side pane demonstrates, also the share of countries facing working-age population decline has seen a surge in the past years and is expected to increase further in the upcoming decades. Thus, in stark contrast to the more recent population history, the impending transformations will be pronounced, widespread, and enduring, providing a changed demographic context for a wide range of advanced economies.

Given the issue’s contemporary and future relevance across countries as well as the importance of demography for economic growth in general and the labor market in particular, questions about the economic implications of population decline emerge. Ultimately, the expected developments may challenge other supposedly stylized facts as well, such as the ever-accelerating growth of GDP (per capita) (Jones/Romer 2010) or the constant labor share in national income (Kaldor 1961). However, despite its occurrence or imminence in most advanced economies, there is substantial under-coverage among theoretical and empirical research on the economic implications of population decline, in general as well as with regard to the labor market.

Figure 1: Aggregate and individual working-age population growth across advanced economies, 1970–2040



Source: Author’s own calculations. For information on data sources, see Appendix A. © IAB

In formal economic modelling, most approaches assume a growing, or at least stagnant, population (Jones 2022). On the contrary, population decline and the accompanying implications have hardly been discussed as yet. In the existing literature, there have been some attempts to investigate the effects of demographic changes in Ramsey-type models (Brida/Accinelli 2007; Kajanovičová/Novotný/Pospíšil 2020), Solow-type models (Sasaki 2019), or endogenous and semi-endogenous growth models (Christiaans 2011; Jones 2022; Sasaki/Hoshida 2017). Among empirical studies, the under-coverage is even more distinct and may be explained by the fact that there have been comparatively few periods of population decline among advanced economies in the recent past, hampering the reliable identification of its effects. Consequently, existing macroeconomic research on the demography-economy nexus focuses on a variety of different issues: a multitude of empirical studies analyze the effects of population growth (see Headey/Hodge 2009 for a comprehensive meta-study), population ageing (e.g., Acemoglu/Restrepo 2017; Börsch-Supan 2008), or changing mortality, fertility, and human capital patterns (for many: Barro 1991, 1998; Barro/Lee 1994; Bloom/Williamson 1998; Hall/Jones 1999) on economic growth. From a more conceptual perspective, both the secular stagnation debate (Eggertsson/Lancastre/Summers 2019) and the unified growth theory (Cervellati/Sunde/Zimmermann 2017), among others, have addressed the role of demography for long-term economic development. But as in theory, population decline has not yet drawn explicit attention in the empirical literature.

Importantly, sparse contributions, such as the one more recently by Jones (2022), suggest that the economic effects of growth and decline in the population do not need to follow symmetrical paths. Yet, whether this applies to labor market issues as well – such as the behavior of wages, the capital utilization of firms, or the elasticity of labor supply when the labor force is declining – has hardly been addressed so far, neither in theory nor in empirics. To provide an empirically substantiated starting and orientation point for both policy and future research, such as the incorporation of labor market adjustments to population decline in formal modelling, we examine the effects of population decline on the labor market from a historical macroeconomic perspective.

Operationalizing our analysis consists of three key components. First, the occurrence of periods of actual population decline and the availability of labor market data do not necessarily coincide. As noted above, for most advanced economies, population shrinkage appears to be a rather new phenomenon. However, if we take a more historical perspective, even back to the second half of the 19th century, we are able to identify several periods of decline and low population growth, distributed across several countries. On the one hand, this suggests to empirically investigate population decline and its macroeconomic implications in a historical cross-country framework. On the other hand, economic data availability proves to be sparse in the very long run. To this end, we compiled a new historical dataset from a large number national and international sources. We collected information on population, births, real GDP, real wages, real investment, employment, unemployment, labor force participation, and hours worked for sixteen countries from more than 100 different sources, providing an annual coverage for seven countries from 1875 to 2019 and for nine from 1900. Second, the estimation must adequately address possible nonlinear interdependencies of macroeconomic variables conditional on the prevailing demographic regime. To account for this, we specify a panel smooth transition VAR (PSTVAR), thereby contributing to growing bodies of literature that rely on, first, cross-country settings (e.g., Aksoy et al. 2019), and second, regime-dependent methods (e.g., Auerbach/Gorodnichenko 2012) in analyzing dynamic interdependencies of macroeconomic aggregates. Third, tracing possibly nonlinear responses to population decline requires an appropriate identification of the structural population shock. By relying on external instruments, or proxy variables, we follow another strand of recent research (e.g., Gertler/Karadi 2015; Mertens/Ravn 2013; Stock/Watson 2018). Drawing on lagged births data as an instrument for working-age population inflows and outflows, we identify the contemporaneous effects of a structural population shock in times of population growth and decline and trace the corresponding impact of the structural shock using orthogonal impulse response functions.

Our findings indicate differences in the effects of population changes and corresponding adjustments across regimes. In general, population changes pass through to the labor market more quickly in times of decline, translating, inter alia, into a swifter decline in employment and disinvestment compared to times of growth. In the medium to long term, regime-specific adjustment processes unfold. In periods of population decline, labor force participation increases as a response to the initially quick reduction of labor supply, likewise disinvestment tendencies decelerate. By contrast, the effects of population growth unfold lagged but steadily. Notably, we do not find decreases in unemployment or any significant changes of wages as a shortage indicator in times of population decline. Thus, while population decline tends to depress total factor productivity, as also discussed by the literature, our findings indicate that corresponding negative effects for economic growth are mitigated by increases in participation and capital intensity.

The remainder of the paper is structured as follows. In section 2, we provide an illustrative overview of the role of population and labor force size in theoretical models to motivate the empirical investigation in this paper. Subsequently, and complementary to the survey on theoretical considerations, we provide some descriptive statistics on population decline in the past, introduce our historical dataset, and offer stylized evidence on trajectories of labor market variables during periods of decline in section 3. Based upon these two chapters, section 4 outlines a suitable nonlinear econometric strategy to identify (possibly) asymmetric effects of population changes during times of growth and decline. The corresponding results are presented and discussed in section 5. Section 6 demonstrates that the findings are robust. The last section concludes.

2 Theoretical Considerations

In a theoretical perspective, considerations on the economic effects of population decline depart from the fact that even in the simplest production function, $Y = F(K, L)$, the supplied amount of labor, L, is a crucial input determining economic growth. Thus, the relevancy of changes in L and the necessity of analyzing corresponding effects using macroeconomic growth models are evident. However, even though L is arguably closely connected to the size of the population, P, both are not identical – empirical evidence shows that neither participation rates are 100 percent nor working hours are evenly distributed across individuals and time (OECD 2022). But in economic research, questions concerning causes and effects of changes in the size and composition of L are often addressed separately, by different parts of the literature and model families, and are not necessarily linked to population decline. This section offers an illustrative, rather than exhaustive, overview to motivate the subsequent empirical investigation.

The role of (inelastic) labor supply in macroeconomic growth models

Typically, in a generic macroeconomic growth model (see standard textbooks, such as chapters 1 and 2 in Romer 2019), the population consists of a given number n of households with an identical number of household members, H_t, growing at a constant rate $g > 0$ over time:

$$H_t = H_0 e^{gt} \tag{1}$$

Here, each member of each household inelastically supplies one unit of labor, thus at each point t in time

$$P_t = nH_t = L_t, \tag{2}$$

i.e., the population size equals the size of the labor force. Thus $g = s$, meaning the growth rate of the population is identical to the growth rate of the labor supply, s. In the simplest case, firms, using the given labor as well as capital input, are subject to common factor prices, given a level of technology, A_t, and produce according to the identical production function. Consequently, total output results as $Y_t = (K_t, A_t L_t)$. In this setting, capital and labor are complements – thus, ceteris paribus, a decrease in L_t causes a proportional decrease in output. Yet, those effects may already differ when assuming a production function of the form $Y_t = (K_t^{\sigma}, A_t L_t^{1-\sigma})$ (Arrow et al. 1961), where σ is the elasticity of substitution between capital and labor. Here, depending on the value of σ, a decrease in L_t may be mitigated by exchanging for K_t and, thus, maintaining the output level even in the event of population decline.

Notably, theoretical macroeconomic research is still in rather early stages when it comes to analyzing the effects of population decline – only selected approaches deviate from the standard assumption of a constantly growing L_t and analyze the corresponding effects, addressing some of the issues raised above: Among other things, authors have investigated the effects of changes in the population growth rate in Ramsey-type growth models, for example when population growth is logistic (Brida/Accinelli 2007). Sasaki (2019) analyzes the consequences of negative population growth on the long run growth rate of per capita output using a Solow-type growth model. He demonstrates that, if in such a setting the elasticity of substitution is less than unity, economic growth exclusively depends on the rate of technological progress. Christiaans (2011) as well as Sasaki/Hoshida (2017) use semi-endogenous growth models to investigate the effects of population decline on output per capita. The results suggest varying responses of economic growth to negative population growth, inter alia depending on the assumed depreciation rate of capital. Sasaki (2023) uses a Solow growth model with automation capital and shows that the population decline and economic growth can coincide. Notably, the results indicate that the absolute value of population decline may play an important role.

In another recent contribution, Jones (2022) demonstrates that, in the case of population decline, endogenous and semi-endogenous growth models lead to stagnating living standards and knowledge. By taking one step further and endogenizing fertility, Jones (2022) shows that economic growth can only be resumed if the economy switches to an optimal allocation soon enough. Other recent contributions challenge the stagnation scenario. Strulik (2023) augments the model of Jones (2022) by endogenous education components and human capital as an input factor of production. Similarly, Boikos/Bucci/Sequeira (2023) build an R&D-based growth model with human capital accumulation and Bucci (2023) uses an endogenous growth model with human capital accumulation. In these theoretical settings, economic growth and population decline may coexist. Also, Elgin/Tumen (2012) elaborate on findings along this line.

Perspectives on (elastic) labor supply and demand

The insights delivered by those surveyed contributions are substantial, yet, as argued above, the existing literature tends to assume $P_t = L_t$. But labor supply side dynamics are more complex than inelastic labor supply assumptions suggest. Even descriptive empirical evidence demonstrates that neither participation behavior nor participation intensity are evenly distributed across the population. Consequently, unlike the inelasticity assumption in equation (2), actual labor supply is commonly understood as

$$a_t h_t P_t = L_t \tag{3}$$

where a_t is the labor force participation rate (extensive margin) and h_t is the average hours worked (intensive margin) at time t. Existing (micro- and macroeconomic) empirical and theoretical approaches have addressed these parameters in different settings, covering a wide range of issues, but have also investigated interactions of changing labor supply with the labor demand (firm) side.

For instance, a widely used way to think about the parameters a and h are labor supply elasticities. This is based upon the idea of an individual's (a household's) utility function, $U = f(c, l)$, encompassing consumption, c – which, in the absence of non-labor income, is solely determined by the wage rate and the supplied hours of work – and leisure, l. Approaches have analyzed elasticities of hours and participation in different settings, often suggesting positive effects of wage increases. This applies in particular, but not exclusively, on the extensive margin (among many: Ashenfelter/Doran/Schaller 2010; Bargain/Orsini/Peichl 2014; Blundell/Bozio/Laroque 2013; Chetty 2012; Evers/De Mooij/Van Vuuren 2008; Keane/Rogerson 2012; for a discussion on the variation in estimates of labor supply elasticities, see Bargain/Peichl 2016).

Intuitively, when assuming population decline is accompanied by labor supply decline, one may postulate an increase of wages as a shortage indicator. This assumption can be traced back, for example, to the literature on the wage curve, discussing a linear connection between higher unemployment and lower wages (Blanchflower/Oswald 1995), with the former usually perceived as a sign of underutilized labor supply. But, contrarily to this assumption, the standard law of labor demand suggests that rising wages reduce a firm's labor demand, depending on mediating factors such as substitutability (Hamermesh 1993; for a detailed survey on labor demand elasticities, see Lichter/Peichl/Siegloch 2015, for example). In a recent contribution, Bossler/Popp (2023) augment the law of labor demand by hiring costs. They demonstrate that not only rising wages but also general labor market tightness reduces the labor demand of firms, rather than increasing it, as searching becomes costlier – and labor market tightness may be seen as a conceivable implication of decreasing labor supply. These findings suggest that even if a slack labor market reduces wages, a tight labor market does not necessarily cause rising wages.

This brief illustrative overview demonstrates that from the existing body of theoretical literature, the economic effects of changes in the population size are difficult to derive and may depend on the interaction of a series of relevant factors, such as wages or hours worked, among other things[1], and this interaction may itself be state-dependent. This calls for empirical evidence on the causal effects of population shrinkage in order to learn about the potential future path of many economies and to inform further theory development.

3 Population Decline and the Labor Market: Some Descriptive Statistics from a New Historical Dataset

Working-age population decline: occurrence and characteristics

The under-coverage of population decline in (economic) research, as outlined above, is accompanied by similarly sparse descriptive statistics on the nature of population decline in the past; that is, the frequency of its occurrence, magnitude, distribution, and duration[2]. As the historical data for annual working-age population change among selected advanced economies from 1875 to 2019 in Table 1 demonstrates, only 154 of 2096 observations, or 7.3 percent, are decline years. Over the whole period covered, the median annual change of the working-age population was 0.85 percent, with 0.91 in growth years and –0.26 in decline years, and with the strongest increases in overseas migration destinations in the 19th century as well as the strongest decrease during Japan's ongoing decline since the 1990s.

A substantial share of the empirical literature using macroeconomic aggregates in (dynamic) panel models draws on time series starting in the 1960s, 1970s, or later, in particular in a cross-country perspective, with varying frequencies (e.g., Aksoy et al. 2019; Antonakakis/Chatziantoniou/Filis 2017; Canova/Ciccarelli/Ortega 2007; Comunale 2022; among others). Additionally, also labor market statistics across countries, most importantly information on unemployment, such as those delivered by the OECD, start around the mid-1960s or later, indicating this period as a somewhat natural starting point for empirical analyses. As the figures reveal, there has been more pronounced growth in the years before 1970 compared to those afterwards, vividly demonstrating the secular decline of population growth in the very long run. By contrast, population decline observations have been much more similar over time.

[1] Of course, numerous contributions have documented factors impacting both the supply and demand of labor beyond wages as the single determinant – such as the institutional setting and policies, for example in fostering or hampering female labor force participation (among many: Costa 2000; Cipollone/Patacchini/Vallanti 2014; for an exemplary survey see Abraham/Kearney 2020).

[2] Since working-age population is the one core determinant of labor supply, we focus on working-age population from here onwards, and, if not stated explicitly, we use the terms working-age population and population interchangeably.

Table 1: Descriptive statistics on annual working-age population changes from 1875 to 2019

	Annual working-age population change across countries (in %)											
	Total						Growth years			Decline years		
	n	Median	Mean	Min	Max	SD	n	Median	Mean	n	Median	Mean
1875–2019	2096	0.85	0.90	–1.40	10.63	0.77	1942	0.91	1.00	154	–0.26	–0.32
1875–1969	1296	1.05	1.10	–1.10	10.63	0.80	1238	1.08	1.16	58	–0.21	–0.28
1970–2019	800	0.54	0.59	–1.40	3.40	0.60	704	0.62	0.72	96	–0.30	–0.34

Note: Figures in this table encompass working-age population data from the sixteen countries covered by the historical dataset over the period 1874–2019 but exclude observations in war years (1914–1919 and 1939–1946).

Source: See Appendix A. © IAB

Table 2: Distribution of working-age population decline across countries from 1875 to 2019

Distribution of working-age population decline across countries			
Country	No. of decline observations	Country	No. of decline observations
JPN	25	NOR	6
AUT	22	GBR	5
ITA	22	NLD	4
DEU	21	CHE	2
FRA	12	DNK	2
SWE	12	AUS	0
FIN	11	NZL	0
BEL	10	USA	0

Note: See notes for Table 1.

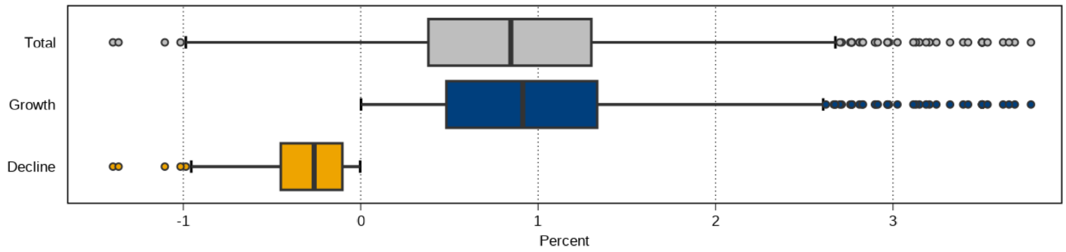
Source: Author's own calculations. For information on data sources, see Appendix A. © IAB

Importantly, 43 of the 96 decline observations after 1970 have occurred since 2010, impressively underlining the timeliness and importance of a close examination of population decline effects.

Moreover, occurrence differs across countries, as Table 2 proves. The list contains the number of decline observations per country as a total. Some have never faced working-age population decline in non-war years (Australia, New Zealand, United States), whereas others have repeatedly undergone shrinkage periods, although with differing duration and magnitude. Japan has experienced the most non-war years (25) of working-age population shrinkage, followed by Austria and Italy (22) as well as Germany (21) with some shorter sequences distributed across the whole observation period.

Notably, the distribution of observations in times of growth and decline exhibit different patterns. The median of all observations, 0.85 percent as shown in Table 1, is accompanied by an interquartile range from 0.38 to 1.30 percent, illustrated in Figure 2. Among growth observations, our data has an interquartile range from 0.48 to 1.33 percent, and among decline observations from –0.45 to –0.10 percent.

Figure 2: Distribution of annual population change observations in times of growth and decline



Note: Observations included correspond to Table 1. Whiskers indicate 1.5 IQR. Five outliers among the growth observations that are larger than 4 percent are not displayed for illustrative purposes.
Source: Author's own calculations. For information on data sources, see Appendix A. © IAB

Historical labor market dataset: a short overview

Yet, the exploration of historical economic dynamics across countries is a notoriously difficult task, particularly when focused on labor market issues. Well-known data collections such as the International Historical Statistics (Mitchell 2013) or Maddisons Historical Statistics (Bolt/van Zanden 2020) and their respective predecessors, among others, have settled the path for comparative historical economic research for decades. However, the availability of annual data in the very long run remained limited to selected variables. We have seen substantial improvements in recent years by compilations such as the Macrohistory Database (Jordà/Schularick/Taylor 2017) or the Long-Term Productivity Database (Bergeaud/Cette/Lecat 2016), both starting in the second half of the 19th century, covering a variety of advanced economies, and broadening the range of macroeconomic indicators. But the availability of annual information on variables such as unemployment is still strongly limited.

Based upon this finding, and in order to operationalize an analysis of macroeconomic labor market adjustments to population decline, we compiled a new historical annual labor market dataset, stemming from extensive data acquisition efforts. On the one hand, we draw both on existing macroeconomic and demographic databases, such as those quoted above, the Human Mortality Database (HMD 2023) or various OECD statistics. On the other hand, and more importantly, we rely on a vast number of individual (national) data sources and collections. Overall, the compilation combines information from more than 100 different sources.

The historical dataset covers sixteen advanced economies, seven of which starting from 1875 (Denmark, Germany, Netherlands, Norway, Sweden, United Kingdom, United States) and nine (Australia, Austria, Belgium, Finland, France, Italy, Japan, New Zealand, Switzerland) starting from 1900 due to limited data availability. The collection contains annual information on demographic and economic variables until 2019, which are

- population by age groups,
- real GDP,
- real wages,
- real investment,
- total employment,
- the unemployment rate, and
- average annual hours worked.

Table 3 provides a broad and descriptive overview of the dataset. Again, we excluded war years in the calculation of the descriptive statistics. The figures and the number of observations for population change differ from those presented above, as Table 3 displays only data for those country-year observations for which we also include labor market data. In Appendix A, we list the data sources for each variable, year, and country in detail. Additionally, we precisely document all preparation steps underlying the final dataset.

Table 3: Descriptive overview of the variables in the historical dataset

	Descriptive statistics					
	n	Median	Mean	Min	Max	SD
Working-age population, annual change (%)	1839	0.83	0.86	−1.40	3.62	0.69
Real GDP, annual change (%)	1839	2.95	3.10	−18.12	27.74	3.62
Real wages, annual change (%)	1839	1.81	2.37	−15.55	86.48	5.24
Real investment, annual change (%)	1839	3.66	4.15	−34.84	129.85	10.47
Employment, annual change (%)	1839	0.94	0.99	−15.77	22.15	1.97
Unemployment rate (%)	1839	3.88	4.79	0.01	34.60	3.83
Average annual hours, annual change (%)	1839	−0.31	−0.42	−13.17	11.44	1.47

Note: See notes for Table 1. Statistics shown here cover only those country-year observations for which the dataset contains information on all variables.

Source: Author's own calculations. For information on data sources, see Appendix A. © IAB

The labor market in times of decline: descriptive evidence

Now, as Figure 1 illustrates, population decline tends to occur consecutively, forming phases of shrinkage rather than single years. In the historical data, we are able to identify 34 periods of consecutive decline[3], whereby the median length was three years. The median *peak-to-through* (compare Reinhart/Rogoff 2014 on GDP changes in the course of financial crises) magnitude, i.e., the median cumulative decline in a shrinkage period, was −0.48 percent.

If decline usually persists over several years, one may also assume that a decline of the population does not affect the labor market only in a single year, but rather adjustment processes unfold over a longer time span. In Figure 3, the dynamics of working-age population as well as of the labor market variables in advance of and during periods of population decline are displayed as solid orange lines, with the levels[4] being indexed to the last year before the decline started ($t0$). The displayed dynamics are those of annual median values, covering six years prior to the decline period and five years of the decline period itself.[5]

Most notably, population growth rates had already been low prior to the respective decline. This implies the intuitively appealing fact that population decline is generally preceded by phases of

[3] Notably, for this illustrative purpose, we define a period to be one or more years of consecutive decline. A period starts whenever there is a decline of the population and there has not been a decline in the preceding two years, avoiding to count one period of decline as two due to very low growth in between. We exclude those periods that started during war years as defined above.

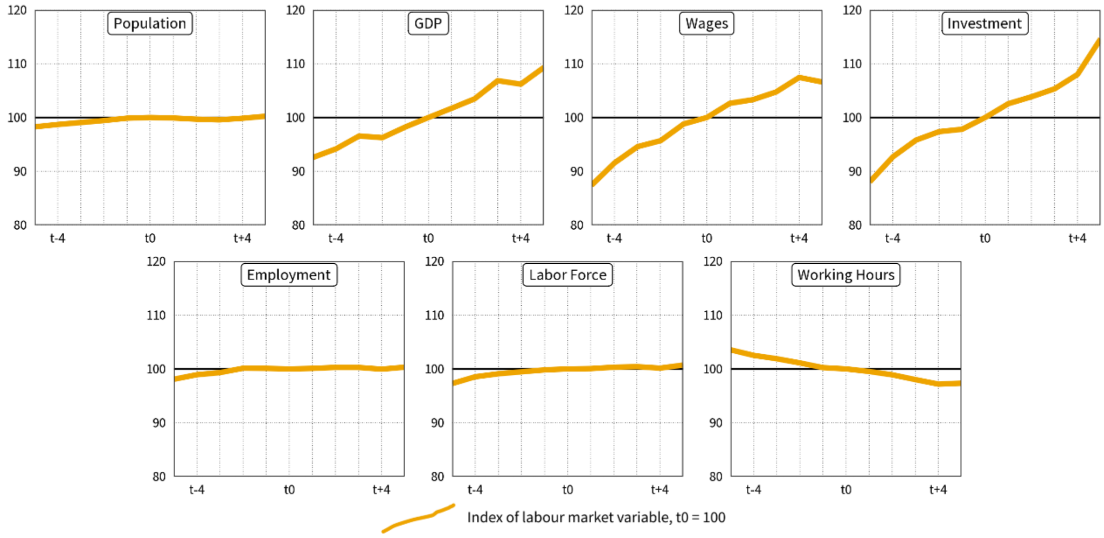
[4] Since we include the labor force rather than unemployment in the estimation outlined below, we display the labor force here as well.

[5] Notably, we display the median values of all periods; that is, both those that have ended earlier than five years and those that have ended later than the displayed horizon.

low growth, respectively stagnation. In other words, switches between a regime of strong, or at least average, population growth and a shrinkage regime take place generally more slowly than quickly. Moreover, the dynamics of labor market variables exhibit differences. For example, wages and hours worked closely stick to the pre-decline trend, indicating limited effects of population decline. On the other hand, there is low employment and labor force growth prior to the decline mirroring low population growth rates, followed by very similar patterns once decline occurs, suggesting a more pronounced effect for these variables.

Obviously, these findings are stylized, neither causal relations nor dynamic interdependencies of the examined macroeconomic aggregates are appropriately mirrored. Put differently, descriptive evidence as shown in Figure 3 does not allow the inference of the causal effects of population decline on labor market variables of interest, and it also does not consider how distinct and enduring a particular decline period has been.

Figure 3: Stylized evidence of labor market dynamics before and during population decline periods



Note: Figure 3 displays dynamics before, during and after periods of population decline as explained in the main text.
Source: Author's own calculations. For information on data sources, see Appendix A. © IAB

Towards an empirical framework

Thus, the short analysis in this section implies there are four directions that an empirical analysis aiming to carve out possibly nonlinear macroeconomic effects of (working-age) population decline needs to follow. First, a suitable empirical strategy must identify the causal effect of positive, respectively, negative population changes, distinguishing it from other shocks in the economy, and clearly examine the dynamic adjustment process over time that may differ in times of growth and decline. Second, simply distinguishing population growth and decline into two separate regimes does not account for empirically observed demographic developments. Rather, choosing an estimation setting that allows the impact of population changes to differ continuously from high to low growth to decline takes the existence of population stagnation before and after periods of decline into account. Third, the sparse occurrence of population decline calls for a cross-country perspective. Identifying nonlinear effects of population changes in growth and decline periods using an econometric model requires a sufficient number of observations for both, which is clearly not given when focusing on an individual economy. Even for countries that experienced comparatively many years of decline, a dynamic analysis including more than two or three variables with a sufficient number of lags quickly depletes its degrees of freedom for decline periods. Fourth, even in a cross-country perspective, there have been only few observations in the more recent past, calling to exploit the full variation of working-age population changes not only across countries but also over time, whenever reliable labor market data are available. In the following section, we propose an estimation framework addressing the mentioned necessities.

4 Econometric Strategy

As outlined, we exploit the time-series variation from multiple countries to identify possibly differing effects of population growth and decline, using an empirical strategy that permits the analysis of dynamic interdependencies conditional on the demographic regime. We draw on and expand different strands of the literature and introduce a suitable external instrument to identify the effects of a structural population shock in the economy. We divide this chapter into a series of subsections on nonlinear dynamic modelling, regime specification, shock identification, instruments, and impulse responses.

Capturing nonlinear effects: Panel Smooth Transition VAR (PSTVAR)

We start by specifying a panel VAR, and in doing so, we contribute to a growing body of literature making use of panel VARs in macroeconomics (e.g., Aksoy et al. 2019). Applying a vector autoregressive structure allows the flexible analysis of macroeconomic interdependencies without a priori imposing assumptions on the directions of effects (Canova/Ciccarelli 2013). Drawing on this literature, we specify our model in its linear version as

$$Y_{it} = \mu_i + \delta_t + AY_{i,t-1} + EX_{it} + u_{it} \tag{4}$$

with $i = 1, \dots, c$ and $t = 1, \dots, T$; c and T being the panel and time dimensions, respectively. Y_{it} is the vector of endogenous variables, μ_i and δ_t denote country- and time-fixed effects, respectively, and X_{it} represents country-year dummy variables to capture the effects of war and interwar periods[6]. A and E are coefficient matrices. Y_{it} comprises seven variables: the working-age population, real GDP, real wages, real investment, employment, the labor force, and average annual hours worked. All variables are included as log levels. Notably, given the inclusion of Y_{it} in levels, when allowing for a sufficient lag length, the VAR is able to capture level relations and flexibly form quasi-differences in the presence of unit roots (see, e.g., Sims/Stock/Watson 1990; more recently Weber/Weigand 2018).

Since the focus of the present paper is on the analysis of potentially different effects of population decline compared to population growth and the descriptive evidence suggests a

[6] This vector of dummy variables eliminates the effects of all observations from 1914 to 1922 as well as from 1929 to 1949, and is basically equal to removing those observations from the panel dataset completely. We keep these observations and eliminate the corresponding effects using dummy variables instead of excluding them in order to easily carry out the residual resampling. We additionally include a dummy for the German hyperinflation in 1923.

continuous rather than a threshold modelling approach, we combine our panel VAR with a nonlinear smooth transition structure. This follows a strand of literature using common vector autoregressive models and nonlinear extensions to account for regime-wise interdependencies of macroeconomic variables (e.g., Auerbach/Gorodnichenko 2012). Thus, our linear model in (4) is modified as follows

$$Y_{it} = \mu_i + \delta_t + [1 - P(q_{it})]GY_{i,t-1} + [P(q_{it})]DY_{i,t-1} + EX_{it} + u_{it} \quad (5)$$

where G and D are matrices holding the regime-dependent coefficients of the endogenous variables, and $P(q_{it})$ refers to the probability of experiencing population decline. This probability is given as

$$P(q_{it}) = \frac{exp[-\gamma(q_{it} - \kappa)]}{1 + exp[-\gamma(q_{it} - \kappa)]} \quad (6)$$

where q_{it} is the transition variable, γ defines the smoothness of the transition, and κ is a location parameter defining the value of q_{it} at which the regime-switch occurs.

Demographic regimes: transition variable and smoothing parameters

Given the scope of the paper, the transition variable q_{it} incorporates information on the prevailing demographic regime, i.e., population growth rates. However, from a conceptual perspective, the selection of an appropriate transition variable is not straightforward. Moreover, the smoothing and location parameters γ and κ are not predefined either. In the literature (e.g., Auerbach/Gorodnichenko 2012; Gehrke/Hochmuth 2021), authors tend to select a transition variable, define a switching point κ, and then calibrate γ such that the share of observations with probability $\geq 1 - \tau$ is close or equal to τ, which is the share of observation in the regime of interest, e.g., years of population decline.

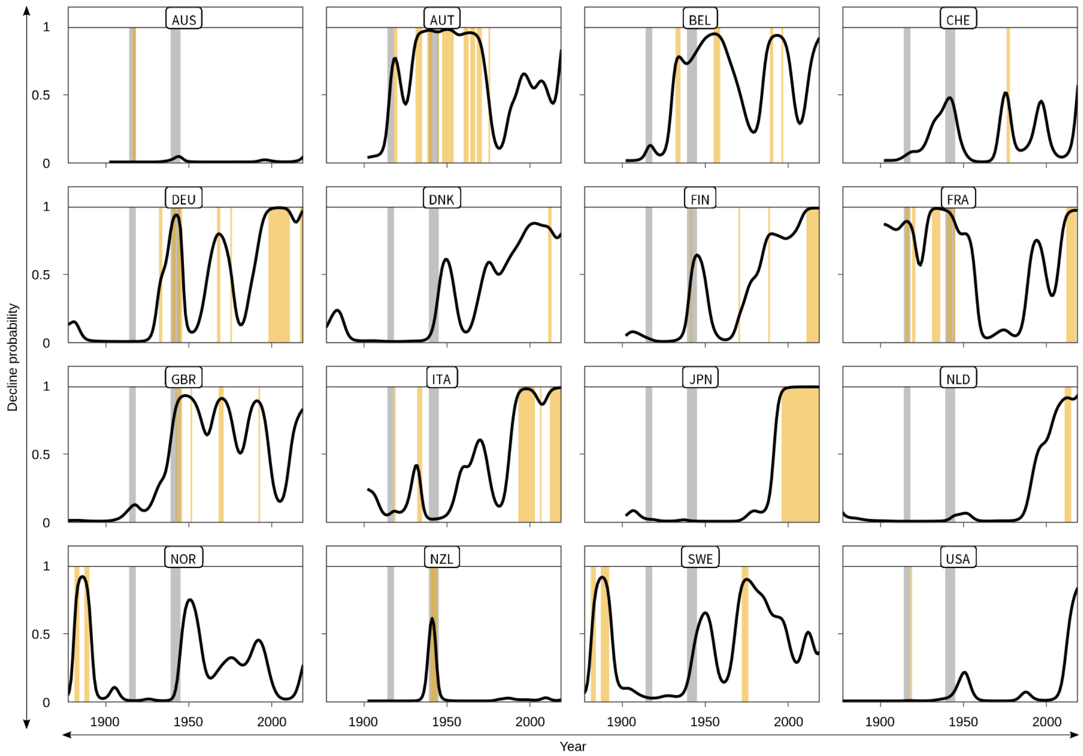
The selection of a transition variable mainly translates into the question of which period of population change is relevant to depict a demographic regime under which a labor market operates. To answer this question, we use the trend of annual population growth rates as delivered by the HP filter ($\lambda = 100$) as our transition variable. To avoid the common bias of the HP filter at the ends of the sample, we use population growth rates from 1860 to 2025, using data sources as outlined in Appendix A.

For the switching point, the literature tends to define $\kappa = 0$, which, in case of the usual z-standardization of the transition variable, implies a switch at the mean. In our case, the plausibility of this switching point (0.63 percent) is disputable, as it implies that the majority of the years after 1970 is closer to the decline than the growth regime (compare Table 1), i.e., working-age population decline has been more likely than growth. This contrasts the narrative that extensive working-age population decline is a rather recent phenomenon. To find a more suitable switching point, we rely on the distribution of the (original) annual population change rates in our dataset. We define $\kappa = Q_1^{POP,z}$, which is the lower quartile of growth observations across all years in the panel (0.48 percent) after the z-standardization of q_{it}. In the robustness section, we address this choice.

Eventually, for calibrating γ, we follow the standard procedure in the literature as outlined above. The share of population decline observation in the observations across all countries in the final panel is 8.4 percent. In accordance with the literature, and to ensure a sufficient number of observations in each regime, we calibrate to the original share of decline observations. We set γ such that $Pr[P(q_{it}) \geq 0.926] \approx 0.084$. This calibration exercise yields $\gamma = 3.85$. Figure 4 illustrates the distribution of the decline probabilities stemming from this specification across the countries in our panel. As intended, the calibration exercise creates decline probabilities coinciding with actual decline observations and allows for smooth changes from and to periods of growth.

Having distributed growth and decline weights across all observations in the sample, we are able to estimate the model equations-wise by OLS. We check for the appropriate lag length by relying on the BIC, and arrive at a lag length of $p = 2$.

Figure 4: Decline probabilities across the countries in the sample



Note: Shaded areas indicate war (gray) and decline (orange) years.
Source: Author's own calculations. © IAB

Identification of structural shocks by external instruments

However, to trace possibly nonsymmetrical labor market adjustments to population changes over time, we do not only estimate the reduced form, but we need to identify corresponding structural population shocks. Evidently, working-age population can be endogenous to economic variables, for example as push and pull factors driving migration. Indeed, estimations ignoring simultaneity in Appendix D demonstrate the importance of introducing instruments in the identification strategy.

In recent years, shock identification using external instruments has found widespread application (Gertler/Karadi 2015; Stock/Watson 2018). This approach exploits the well-known fact that the reduced form innovations, u_{it}, are a linear combination of structural shocks, ϵ_{it}:

$$u_{it} = S\epsilon_{it} \tag{7}$$

Analyses drawing on identification by external instruments refrain from identifying the full matrix S by imposing restrictions but rather focus only on the shock of interest, that is, only identify the corresponding column, s. To identify the structural shock of interest, $\epsilon_{1,it}$, appropriately, a suitable instrument, z_{it}, must satisfy the well-known conditions

$$E(\epsilon_{1,it}z_{it}) \neq 0 \tag{8}$$

$$E(\epsilon_{2:j,it}z_{it}) = 0 \tag{9}$$

While equation (8) states that z_{it}, must be relevant, i.e., correlated with the shock of interest, equation (9) requires the instrument to be exogenous to the remaining, unidentified shocks (Gertler/Karadi 2015).

The contemporaneous effects of a structural shock are estimated by two-stage least squares (2SLS). In the first-stage regression, we isolate the structural shock; that is, we regress the residuals of the equation of interest, here of the population equation, $\hat{u}_{1,it}$, on the instrument. In the second stage, we identify the contemporaneous impact of the structural shock by regressing the residuals of our j equations, with $j = 1, \dots, 7$, of the reduced-form estimation on the fitted values of the first stage. Notably, in the second stage, we weight the RHS by the respective regime probabilities.

More formally, we obtain the regime-dependent, contemporaneous impact of a structural shock using the instrument z_{it} by

$$\hat{u}_{1,it} = \alpha + \omega z_{it} + v_{it} \tag{10}$$

$$\hat{u}_{j,it} = \beta + \theta_j^G[1 - P(q_{it})]s_{it} + \theta_j^D[P(q_{it})]s_{it} + r_{it} \tag{11}$$

where $s_{it} = \hat{\hat{u}}_{1,it}$ is the structural shock in country i at time t, i.e., the fitted value obtained by estimating equation (10), ω is the coefficient in the first stage, and θ_j^G and θ_j^D hold the regime-dependent coefficients of interest. Importantly, by construction, this identification strategy scales the contemporaneous effects to a structural shock of one percent[7]. Yet, before estimating these regime-dependent, contemporaneous effects of a structural population shock as outlined, we need to find a valid instrument, meeting both the relevancy and exogeneity conditions.

Introducing a suitable instrument: lagged births

The development of a population can be written as $f(B, D, M)$, i.e., as a function of the three demographic components: births, deaths, and migration (Shryock/Siegel 1976). Given our

[7] This applies to the linear case. When identifying the contemporaneous effect in a nonlinear framework, as in equation (11), this coefficient may be different from 1. In our case, these differences are small. Thus, we manually scale the contemporaneous effects accordingly.

dataset starts in 1875, migration data is difficult, and for some countries impossible, to obtain, but information on natural population change – births and deaths – can be easily retrieved. However, as we strive to estimate the impact of a structural shock in the working-age population (15–64 years), the role of births differs from the role in the total population: The development of a working-age population can rather be written as $f(I, O, D, M)$, i.e., inflows into and outflows from the age group instead of births determine the size. Since we define the working-age population to be those aged 15 to 64 years, inflows in period t are persons aged 14 years in period $t-1$ and outflows in period t are persons aged 64 years in period $t-1$.

Notably, documenting annual births has a long tradition and corresponding time series are available starting from the early 19th century, and, by definition, a birth cohort always corresponds to a single age-year cohort in a given population. This observation suggests that inflows and outflows should be approximated by using lagged births data since, arguably, births lagged 15 and 65 years are an instrument that satisfies both conditions stated in equations (8) and (9). Nevertheless, the suitability of using births to identify a structural shock must take into account the interaction with the other two components of demographic changes, mortality and migration. Consequently, we conduct a series of preparatory steps.

First, mortality patterns have changed substantially over the past two centuries (Davenport 2021). Correspondingly, the probability of a person reaching 15 and 65 years of age has been vastly different in the 19th century compared to the 20th and 21st century. An intuitive way of correcting for these changes and simultaneously relying on a variable of widespread availability is to weight births in a given year with some information on the life expectancy of newborns in the same year. However, life expectancy is typically calculated by using period mortality, i.e., the age-specific death rates in the same year or reference period (see, e.g., Anderton/Barrett/Bogue 1997; Shryock/Siegel 1976). But this does not account for the impact that drastic events have on age-specific death rates, e.g., such as the effect of wars on the mortality of those who have already entered working age, as well as for general improvements in health care and longevity over centuries. Consequently, we weight births lagged 15 and 65 years, denoted as $B_{i,t-15}$ and $B_{i,t-65}$, with the corresponding cohort survival rate, if available[8], denoted as q_{it}^{15} and q_{it}^{65}, thus $B_{i,t-15}^{*} = B_{i,t-15} * q_{it}^{15}$ and $B_{i,t-65}^{*} = B_{i,t-65} * q_{it}^{65}$.

Second, the contribution of fertility to population growth, here the contribution of inflows into and outflows from the working-age population, depends on the population size at a given point in time. Put differently, the same birth cohort might contribute to population change in vastly different ways when entering and exiting working age not only due to mortality, as it may also differ substantially when the in-between change of the population size was large, e.g., due to strong migration dynamics. We account for this by dividing births by the population level one year prior to the longest lag p in the VAR. More formally, this is

$$B_{it}^{*} = \frac{B_{i,t-15}^{*} - B_{i,t-65}^{*}}{P_{i,t-(p+1)}} \qquad (12)$$

Third, the model proposed in equation (5) encompasses the contribution of the natural component to overall working-age population growth. Now, in striving to isolate the structural

[8] Again, we document all data sources and adjustment steps in Appendix A in detail.

population shock in the residuals of the population equation by using lagged births as an instrument, we essentially address those innovations in the natural component of population change that have remained unexplained by the model. Put differently, rather than resembling the natural population change component already included in the VAR, an appropriate instrument should approximate only the idiosyncratic changes of innovations in this component. To this end, before using it as an instrument, we filter B_{it}^* by an autoregressive structure, with π being the corresponding coefficient, by country and year fixed-effects, and by country-year dummy variables – all of this analogous to equation (5). With a corresponding notation using an asterisk, this implies:

$$B_{it}^* = \mu_i^* + \delta_t^* + \pi B_{i,t-1}^* + E^* X_{it} + e_{it}^* \tag{13}$$

Since recorded births are flow data, we use only one lag to resemble the level structure as included in the VAR. Having estimated equation (13), we compute $\hat{e}_{it}^*$ and define $\hat{e}_{it}^* := z_{it}$, i.e., the residuals stemming from this filtering step serve as the instrument in our identification strategy.

Thus, in the identification step, we exploit the information from all reduced form residuals for which there are available data for births. Notably, this is the case for 90.6 percent of the observations. For the remaining 9.4 percent, estimated data exist. In Appendix B, we outline in detail across which countries and periods these estimated data points are distributed. Importantly, while we include all observations, based on estimated or observed births data, in the filtering equation (13) – in order to the estimate the trend correctly and equivalent to the VAR structure – we rely on the subsample of the 90.6 percent observations based on actual, observed births to identify the shock.

Now, by plugging z_{it} into equation (10) and retrieving the corresponding fitted values, we are able to isolate the structural population shock in the population equation residuals from the initial reduced-form estimation, as outlined above. Notably, in this first-stage estimation, we obtain an F statistic (HAC) of 164.0, demonstrating sufficient strength. Using the isolated shock in the second stage, as also outlined above, we obtain $\hat{\theta}_j^G$ and $\hat{\theta}_j^D$, which are the contemporaneous effects of a structural population shock on the j-th variable in the model – in times of population growth and times of population decline, respectively.

Using information on lagged fertility as an instrument to identify demographic changes has some precedents in the literature. Among others, Jaimovich/Siu (2009) use such data as an instrument for the age structure of the labor force and analyze corresponding effects on output volatility. Similarly, using information on survival rates has been applied as well. In a recent contribution, Maestas/Mullen/Powell (2023) analyze the effect of population aging on economic growth, the labor force, and productivity. To address endogeneity issues, they use data on lagged age structure as an instrument for the contemporaneous shares, weighted by corresponding survival probabilities. In this paper, we combine these existing approaches from the literature, augment them with a filtering exercise, and use the resulting series as an instrument to identify structural population shocks in the residuals of the VAR – rather than including the variable directly in our model.

Impulse response functions and bootstrapping

By stacking all $\hat{\theta}_j^G$ and $\hat{\theta}_j^D$ coefficients into two vectors, Θ^G and Θ^D, and using the estimated coefficient matrices from the reduced-form estimation, G and D, we are able to derive orthogonal impulse response functions and trace the effects of a structural population shock in times of population growth and in times of population decline. Importantly, by deriving orthogonal impulse response functions, we implicitly assume that the estimated system stays in the respective regime. Hence, as argued by Auerbach/Gorodnichenko (2012), the model is linear for each regime, and the corresponding impulse response functions do not depend on history (for details on impulse response functions and history dependence in the context of nonlinear multivariate models, see Koop/Pesaran/Potter 1996).

As we include the endogenous variables in log levels, we can easily derive the regime-dependent effects of structural population shocks on a range of additional variables: the labor force participation rate, the employment rate in the labor force, GDP per capita, productivity, the capital stock, and, consequently, the capital-labor ratio. In Appendix C, we outline in detail how we obtain these derived impulse response functions.

We construct 68 percent confidence intervals by applying recursive cross-sectional residual resampling with 5,000 draws. Following and building upon Jentsch/Lunsford (2022), we preserve the covariance of the structural population shock, the regime weights, and the instruments by resampling them simultaneously. We choose the block length to be equal to the lag length of the model, which corresponds to a block length of three.

In the robustness section, we address frequent questions appearing both in panel models and historical settings, such as cross-sectional dependence or parameter constancy, and demonstrate that our findings remain valid when explicitly accounting for those factors.

5 Results

Regime-dependent effects of structural population shocks

In this subsection, we report and analyze the impulse response functions to positive, respectively, negative population shocks[9]. Thereby, we address three varieties of (possible) asymmetries of these responses across regimes: in magnitude, in sign, and in timing. First, we present the results for the growth regime. Then, we do the same for the decline regime, and additionally include the mirrored point estimate from the growth regime. The latter enables a quick comparison in terms of symmetry; that is, what would the impulse response look like if it were symmetrical to the growth regime.

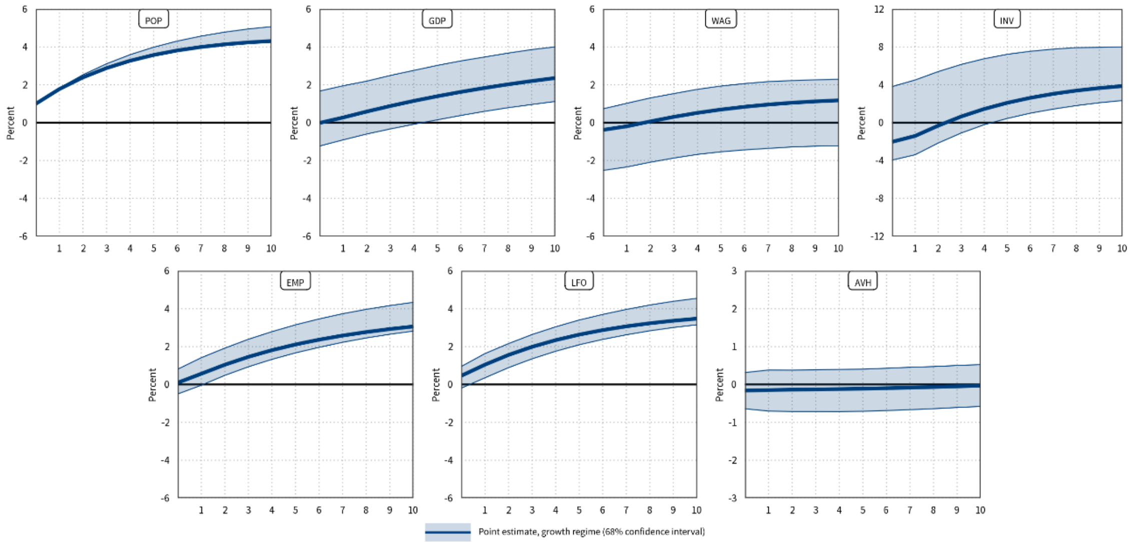
In Figure 5, the blue solid lines represent the impulse response functions of the point estimates of the level variables included in the baseline specification to a positive population shock. Since we include all variables in logs, the results can be interpreted as elasticities. Thus, the impulse response functions indicate the percent change of the respective variable to a 1 percent

[9] Notably, identifying structural population shocks does not rule out the existence of other structural shocks. In fact, the residuals still contain all other structural shocks, yet they remain unidentified.

population shock. The blue shaded areas indicate corresponding 68 percent confidence intervals. The plots permit the analysis of the effects over a horizon of up to ten years after the shock, i.e., from the short to the medium and long term.

As Figure 5 illustrates, the impulse response of the population variable to its own shock grows by up to 4 percent after ten years. This extent is important as it sets the benchmark to which the reaction of the other variables must be compared. It mirrors that population growth is persistent.

Figure 5: Impulse response functions to a positive population shock in times of growth



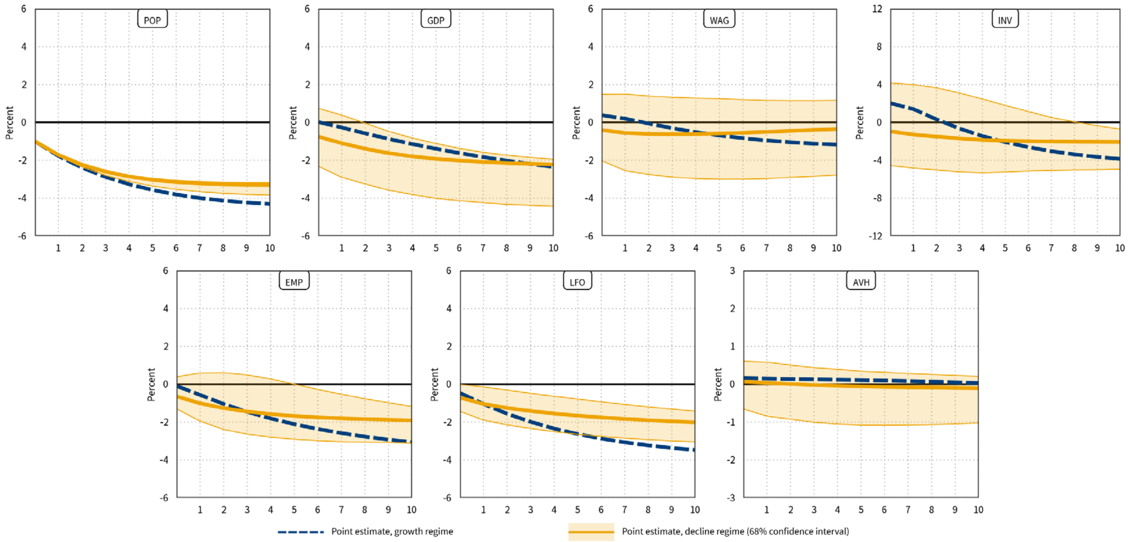
Note: POP = working-age population, GDP = real GDP, WAG = real wages, INV = real investment, EMP = employment, LFO = labor force, AVH = average annual hours worked.
Source: Author's own calculations. © IAB

The trajectories of the other impulse responses indicate that economic reactions to population shocks in periods of growth take time. Put differently, in periods of growth, population changes do not translate into economic reactions straightaway. However, the responses of GDP, of investment, and of the extensive margin of labor supply – employment and labor force – grow and become significant. By contrast, as the plot indicates, we do not find any significant effects on real wages or on average annual hours worked.

Below, Figure 6 offers the complementary analysis for the decline regime. The solid orange line indicates the impulse response functions of the point estimates of the level variables included in the baseline specification to a negative population shock of 1 percent in times of population decline. The orange shaded areas indicate the corresponding 68 percent confidence intervals. The dashed blue line indicates the mirrored point estimate from the growth regime, as given in Figure 5.

As Figure 6 shows, the impulse response of the population variable is less pronounced in the long term compared to the growth regime, flattening out at about 3.3 percent after ten years. Again, this is the benchmark to which the other results must be compared before drawing conclusions across regimes.

Figure 6: Impulse response functions to a negative population shock in times of decline



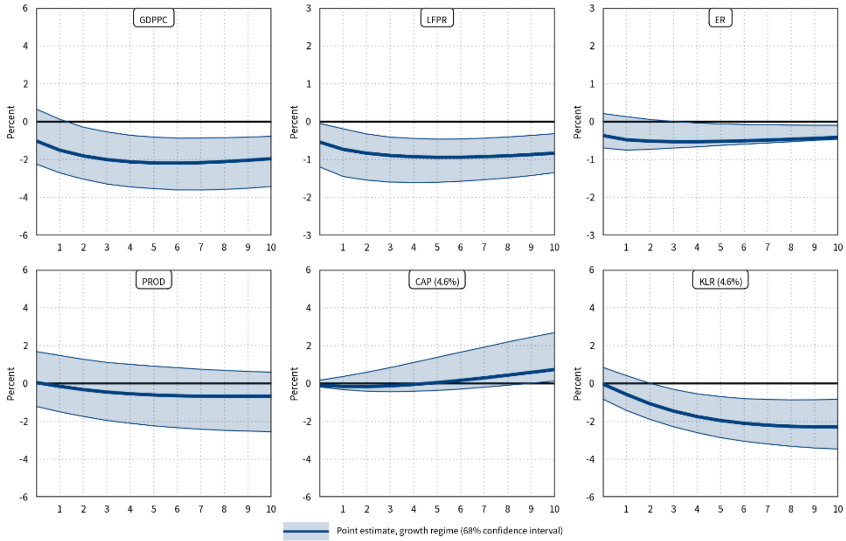
Note: For abbreviations, see notes of Figure 5.
Source: Author's own calculations. © IAB

Contrary to the growth regime, population changes translate more swiftly into the economy in times of decline. In the short term, this becomes visible in the point estimates of GDP, investment, and employment. In the medium to long term, the initial differences disappear or reverse. The effect on total GDP after ten years is similar in both regimes. The impulse responses for investment, employment, and the labor force flatten our more quickly – both in comparison to the growth regime but also in comparison to the trajectory of the population in the decline regime. These results suggest that economies have proven to be successful in cushioning the adverse effects of population decline on labor supply. Since this avoids further losses in the production factor labor, for the complementary production factor capital it also counteracts disinvestment tendencies, as mirrored in the corresponding impulse response. As in the growth regime, we do not find any significant changes of real wages or average annual hours worked.

So far, the analysis has focused on the results as straightforwardly provided by the baseline model. By deriving a series of additional impulse response functions, we are able to quantify these results in the form of well-known indicators, such as the labor force participation rate. Below, Figure 7 and Figure 8 allow for corresponding analyses. For the depreciation rate, we assume 4.6 percent (e.g., ECB 2006). We discuss variations of this deprecation rate in the next section.

As Figure 7 shows, a positive population shock in times of population growth causes GDP per capita to decline. The point estimate of the short-term effect indicates a pronounced but insignificant decline. After ten years, the effect arrives at a significant 2 percent decline. Importantly, this impulse response is calculated using the population variable in the model, which is the working-age population. Thus, the effect should rather be interpreted as GDP per capita of those of working age. The effect on GDP per capita measured using the total population depends on the ratio of those of working age to those of non-working age and, even more importantly, on the changes in the non-working-age population following the shock. Quantifying these effects is beyond the scope of this paper.

Figure 7: Additional impulse response functions to a positive population shock in times of growth

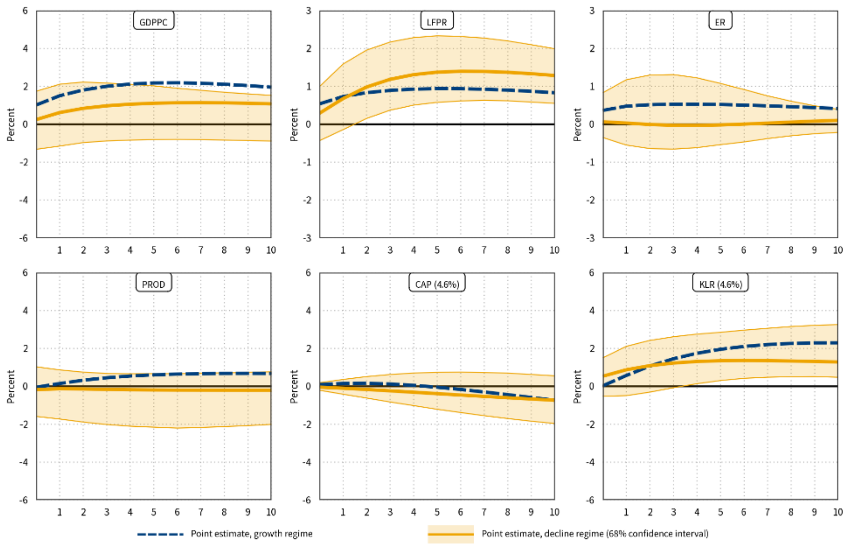


Note: GDPPC = GDP per capita, LFPR = labor force participation rate, ER = employment rate, PROD = productivity, CAP = capital stock, KLR = capital-labor ratio.
Source: Author's own calculations. © IAB

There are two possible explanations for this decline in output per capita. One is that labor does not increase as strongly as the population; that is, proportionally fewer people contribute to production. Looking at the level effects in Figure 5, this effect appears to be evident and is quantified in Figure 7, where the labor force participation rates decline significantly, by 0.8 percent in the long term. Moreover, the employment rate in the labor force declines significantly by 0.4 percent after ten years. Put differently, following a positive population shock in times of population growth, people participate less in the labor market, and those participating are less often in employment – i.e., the unemployment rate increases.

Another possible explanation is changes in productivity. The impulse response function for productivity – output per hour worked – suggests an effect close to zero in the short term, and subsequently a moderate decline by 0.7 percent, which is, however, not statistically significant. A certain decrease in productivity would be explained by a lower capital-labor ratio. Indeed, the capital stock increases only with a lag. As a consequence, we observe only an incomplete adjustment of the capital side after ten years according to the results of our model – which implies a lower capital-labor ratio. In fact, in a back-of-the-envelope calculation, when assuming a stylized Cobb-Douglas production function with an output elasticity of capital of one third, an isolated change in the capital-labor ratio of –2.3 percent would reduce productivity by 0.8 percent – which is very close to the model results.

Figure 8: **Additional impulse response functions to a negative population shock in times of decline**



Note: For abbreviations, see notes of Figure 7.
Source: Author's own calculations. © IAB

In Figure 8, the derived impulse response functions for the decline regime are visualized, again with the mirrored response from the growth regime as a dashed blue line. The increase in GDP per capita is not as pronounced as the response in the growth regime would suggest, arriving at around 1.1 percent after ten years, and not reaching statistical significance.

Again, there are two possible drivers for changes in GDP per capita: increasing employment participation or productivity. In Figure 8, the regime differences in the labor supply reaction in terms of participation and employment are well illustrated. The labor force participation quickly overtakes the mirrored response from the growth regime. After ten years, we see a substantial and significant increase of the labor force participation rate in times of decline by 1.3 percent, which is about 0.5 percentage points larger compared to the mirrored response from the growth regime. As outlined above, in times of growth, the unemployment rate rises as the labor force grows more strongly compared to employment. But in times of decline, by contrast, we observe much more similar changes of employment and the labor force. As a result, as Figure 8 visualizes, the employment rate remains roughly stable throughout the analyzed horizon, thus we find no evidence for any significant changes in the unemployment rate in times of decline.

Moreover, while productivity declines moderately in the growth regime, we do not observe any changes in periods of population decline. But as in the growth regime, we observe only incomplete capital adjustment, leading to an increased capital-labor ratio of about one percent after ten years. Since productivity does not increase and neither unemployment nor hours worked change noticeably, this implies that the observed rise in GDP per capita in periods of decline is mainly driven by changes at the extensive margin, i.e., increasing labor force participation.

However, the absence of productivity effects in the decline regime despite an increasing capital-labor ratio is noteworthy. Again, in a back-of-the-envelope calculation assuming a stylized Cobb-Douglas production function and an output elasticity of capital of one third, a 1.3 percent increase in the capital-labor ratio would imply an increase of productivity by 0.4 percent. But in

Figure 8, we observe even a slight decline. Consequently, there is some scope for other factors to exert negative effects on total factor productivity following a negative population shock.

Discussion: a view on the existing literature

One explanation may lie in the interaction of the capital stock and labor supply. If the latter decreases and, at the same time, the capital stock does not fall proportionately, we observe a rising capital-labor ratio, as outlined above. When expecting a rising capital-labor ratio to translate into higher productivity, one implicitly assumes that the additional capital per worker is fully utilized. But if capital utilization is neither exhaustive nor fixed – which is, in general, supported by empirical data (Gorodnichenko/Shapiro 2011) – productivity effects of changes in the capital-labor ratio due to population shocks may be limited. However, when attributing the whole divergence of productivity and the capital-labor ratio in times of population decline to underutilized capital, we would assume this effect to be persistent. This would be difficult to reconcile with the literature that analyzes the crucial role of fluctuations in capital utilization in order to absorb shocks in a business cycle – i.e., short-term – perspective (e.g., Burnside/Eichenbaum 1996).

This stresses mechanisms that are more fundamental than fluctuations at business-cycle frequency. Approaches such as Jones (2022) seek to model the endogenous dynamics leading to population decline and analyze, as a consequence of population decline, the implications for economic growth in the long run. Jones (2022) shows that in a regime of persistent population decline the diminishing number of people eventually leads to stagnating GDP per capita and productivity, as outlined above. While we do not analyze the long-run or steady-state dynamics of the economy analogously to theoretical models, the effects we are analyzing can be compared since they are conditional on the prevailing regime, i.e., on enduring population decline. Consequently, we use the way ideas evolve according to Jones (2022, see Table 1) and the calibration[10] therein in another back-of-the-envelope calculation and compare two scenarios: First, a scenario where there is a constant negative population growth rate, set to –0.5 percent, as done by Jones (2022). Second, a scenario where there is additional, exogenously induced population decline as given by the impulse response of the population from our baseline specification. By comparing the trajectories of knowledge in both cases, we observe that, compared to the first scenario, the additional population decline lowers productivity by about 0.3 percent after ten years.

Thus, the underlying mechanism as argued by Jones (2022) – fewer people produce fewer ideas, which exerts a negative effect on total factor productivity – is consistent with our findings. This applies to both the conceptual perspective but also to the attributed size effect (0.3 percent compared to 0.4 percent according to our model). Notwithstanding, in our model we find an increase rather than stagnation in GDP per capita in Figure 8 – which is explained by the combination of rising capital intensity and rising labor force participation. These mechanisms, that are absent in standard models, jointly offset possibly negative productivity effects due to a decreasing population size. Logically, these margins should be part of theoretical considerations on the odds of GDP stagnation.

[10] Jones assumes 2 percent annual TFP growth. This calibration suits our empirical data well, as we observe a median annual productivity change of 2.2 percent (only years that entered the estimation).

Other results discussed above have yet not been addressed explicitly in the context of population decline, but in the related literature. An established strand analyzes the wage and unemployment effects of shrinking cohort sizes. Another strand investigates the effects of emigration. If flexibility of labor demand is limited, shrinking cohort sizes may decrease crowding out effects and thus improve the labor market outcomes (Easterlin 1961). However, for firms, smaller youth entry cohorts may also lower the incentive to create new jobs (Shimer 2001), besides labor supply also aggregate demand effects have to be taken into account (Macunovich 1999) and the cohort effects may change over time (Zimmermann 1991). Similarly, other studies have found that emigration may increase the wages of stayers (e.g., Biavaschi 2013; Dustmann/Frattini/Rosso 2015). While the overall evidence is not unambiguous, even if one comes from the hypothesis that labor market outcomes deteriorate with cohort size (such as Berger 1985; Brunello 2010; Foote 2007; Garloff/Pohl/Schanne 2013) or are linked to emigration, the view of our study differs in one important aspect: the cohort shifts and emigration dynamics in the recent decades analyzed in the literature usually do not represent actual (working-age) population shrinkage, which is our focus. Evidently, the overall decline of the working-age population goes beyond cohort shrinkage or emigration, and thus we find distinct adjustment channels: no wage and unemployment reaction, but higher labor force participation and capital deepening.

Limitations: historical context and data quality

Breaking new ground with historical and comparative datasets is subject to a trade-off between the value of additional insights and measurement uncertainty. While we are confident that our investments in data quality are enough for the former to outweigh the latter, results and conclusions are subject to limitations. First and foremost, this can be attributed to the historicity of the data used. Both the quality of measurement of labor market indicators as well as underlying concepts and definitions (see, e.g., Romer 1986 or Piore 1987) have changed over time, which complicates, for example, the comparison of unemployment dynamics between the late 19th and the early 21st century. Moreover, for example, one key limitation in interpreting the absence of effects on wages is the data quality: Most historical wage series focus on urban unskilled laborers. In the literature, some argue that these wage series resemble overall wages in the economy quite well (e.g., Allen 2001). Still, given that urban unskilled laborers have represented only one part of the labor force, there are conceivable limitations in the interpretation.

Similarly, overall economic and social structures have changed. Put differently, economies and their labor markets have evolved strongly over the past 140 years, for example with regard to sectoral structure (among others, Herrendorf/Rogerson/Valentinyi 2014) or, in recent decades, due to automation (Carbonero/Ernst/Weber 2020) – and the same applies to social norms and values (e.g., Fernández 2013 or Humphries/Sarasúa 2012). While our dataset offers clear advantages for estimating effects that would otherwise be hard to measure, it is, due to the long time span, also subject to such transformations. We address potential interference of these changes over time for our estimation results in the upcoming robustness section.

Eventually, the external validity of our study is naturally linked to the range of population decline rates as observed in the past. As insights from theoretical models indicate (e.g., Sasaki 2023), the size of population decline might also play a role. The median of the decline observations in the sample is –0.26 percent, with an interquartile range from –0.45 to –0.10 percent, and with only few observations exceeding the –1 percent threshold (Figure 2). The value of –0.26 percent is close to the median of the projected annual changes in the aggregate of advanced economies until 2040 (UN 2019; see left-hand side of Figure 1). Still, there are countries where the projection suggests the median of the annual changes to be close to or even exceeding –1 percent (e.g., Italy, Japan, or Germany; see right-hand side of Figure 1). Given the insights from theory, such divergence must be considered when linking population projections to the results and conclusions presented in our paper.

6 Robustness

We check the plausibility of our results by applying a series of robustness checks, thereby addressing apparent and frequently discussed factors we haven't explicitly accounted for in our baseline specification. In each case, the corresponding plots can be found in Appendix E.

Parameter constancy over time

First, the data underlying our estimation covers a long period in which large-scale social, technological, and economic changes have occurred. This raises questions as to whether the "nature" of macroeconomic interdependencies might have changed over time and, as a consequence, questions the parameter constancy assumption embodied in our baseline specification. We use a straightforward approach to demonstrate the robustness of our findings by splitting the sample in 1950, which is the first post-war observation, and estimating a separate linear model for 1950–2019[11]. The corresponding Figure 12 can be found in Appendix E. The trajectories of the derived impulse responses are remarkably similar for both samples, with remaining differences mostly stemming from contemporaneous effects. Based upon this analysis, we argue that the assumption of parameter constancy is reasonable, thus our results appear to be robust.

Cross-sectional dependence

Second, advanced economies, and thus their labor markets, are anything but entities independent from each other – a fact that necessarily needs to be accounted for in a given empirical strategy. However, as argued by Pedroni (1999) and others, a common way to account for cross-sectional dependence is to demean over the cross-section, as done by introducing δ_t in the estimation above. Another way to account for cross-sectional dependence, and in doing so a robustness check, is to introduce an additional continuous regressor, such as world GDP (less a country's own; e.g., Comunale 2022). We test the robustness of our specification accordingly, calculating world GDP from GDP per capita and population data from Bolt/van Zanden (2020), and include the growth rate of the contemporaneous period as an exogenous predictor. The corresponding Figure 13 and Figure 14 can be found in Appendix E. For both regimes, the impulse

[11] Estimating nonlinear models for these subsamples is not feasible due to the lack of a sufficient number of decline observations.

responses are very similar to those of the baseline specification. Based upon these findings, we conclude that our results are robust in terms of unaccounted cross-sectional dependence.

Specification of the transition function

Third, in distributing growth and decline probabilities across the panel, we imposed a switching point assumption on the transition function. In order to investigate whether imposing different assumptions alters our results, we conduct a robustness check. Rather than assuming $\kappa = Q_1^{POP,G}$, we set $\kappa = Q_1^{POP}$, which is the lower quartile across all observations, growth and decline, and corresponds to a value of 0.37 percent. The corresponding Figure 15 and Figure 16 can be found in Appendix E. While the results for the growth regime for this robustness check are very similar to those of the baseline specification, the decline regime shows some differences. While the findings for the labor force participation rate are robust, the robustness check suggests a slight increase in unemployment. However, the confidence interval of the baseline specification is wide. Moreover, the robustness check suggests some differences in productivity, possibly driven by a longer-lasting increase in the capital-labor ratio. Nevertheless, the main results, that the labor supply reaction is more pronounced compared to the growth regime as well as a different effect on unemployment in the long run, still hold.

The impact of population ageing

Fourth, the effects of population ageing on the economy and the labor market have already drawn widespread attention in the literature (among many: Acemoglu/Restrepo 2017; Aksoy et al. 2019; Börsch-Supan 2008). In our baseline specification, we do not explicitly control for any age structure underlying the included working-age population. However, since we include year effects in our baseline specification and changes in the age structure exhibit strong similarities across advanced economies (see, e.g., Reher 2015 for a discussion on population ageing across countries due to *baby booms* and *busts*), we argue that most of these effects have already been captured. Still, to strengthen our argumentation, we conduct another robustness check by explicitly including information on age structure. Following Aksoy et al. (2019), we include contemporaneous shares of age groups as exogenous predictors[12]. The corresponding Figure 17 and Figure 18 can be found in Appendix E. The results show only very slight variations compared to the baseline specification. This confirms the expectation that age structure effects are no important disregarded factor.

Assuming varying depreciation rates

Fifth, we evaluate the sensitivity of the results for the capital-labor ratio to changed depreciation rates by assuming lower (2.5 percent) and higher (7 percent) values. We find these comparison values by following the assumptions of Xiao/Amaglobeli/Matsumoto (2021) for depreciation rates over time in low- and high-income countries. In Figure 9, we visualize the effects for both the growth and the decline regime simultaneously.

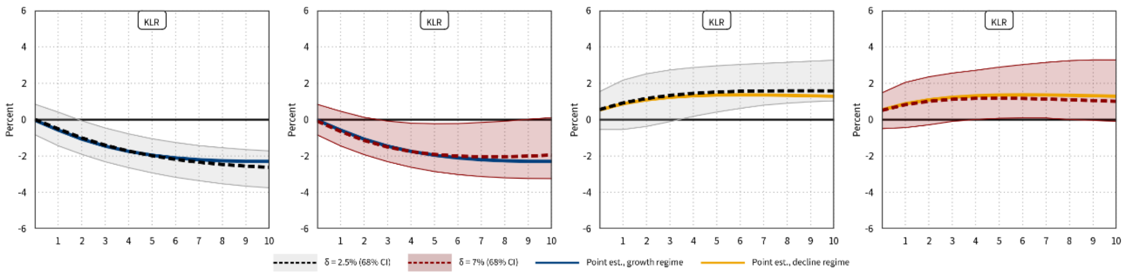
The black dotted lines indicate the respective trajectory of the capital-labor ratio in the point estimate when assuming a depreciation rate of 2.5 percent. The surrounding gray area is the

[12] Yet, as we are using lagged births as instruments, we already account for the implied change in the age structure due to inflows and outflows. To address the corresponding endogeneity issue, we include the shares of ten-year age groups among those aged 20–59 years as a proxy. In doing so, we only include information on age structure that is independent of inflows and outflows due to lagged births – and control for the effects of the accompanying heterogeneity.

corresponding 68 percent confidence interval. The red dotted line visualizes the impulse responses in the case of a 7 percent depreciation rate, again with the red area indicating the confidence interval. The blue and orange lines are the impulse responses from the point estimate in the benchmark specification (4.6 percent depreciation rate) for comparison.

The comparison of the results from the point estimates indicate only minor differences – with more pronounced changes for the 2.5 percent depreciation rate and less pronounced effects for the 7 percent depreciation rate case. But importantly, in the case of the 7 percent depreciation rate, only some intermediate years in both regimes indicate significant effects, the long-run effect is insignificant. Thus, the sensitivity analysis demonstrates that the significance of the effects of population changes on the capital-labor ratio are dependent upon the assumed depreciation rate. Put differently, the finding on significant long-run changes in the capital-labor ratio as a consequence of population shocks does not necessarily hold. Still, the point estimates suggest large effects, irrespective of the assumed depreciation rate.

Figure 9: Regime-dependent effects of varying assumptions on the depreciation rate of the capital stock



Note: For abbreviations, see notes of Figure 7.
Source: Author’s own calculations. © IAB

7 Conclusion

According to recent projections, most advanced economies will face population decline in the years and decades to come, providing a challenging demographic context in the short to the medium and long term. Notably, decline patterns are expected to be particularly pronounced among those of a working age. Although a decreasing population may have profound economic implications, above all in the labor market, there is still little theoretical and empirical evidence on this issue. We contribute to this sparse body of literature by focusing on the latter and compile a new historical dataset using more than 100 individual sources, containing information on demographics (population, births, mortality) and labor market variables (real GDP, real investment, real wages, employment, unemployment, participation rates, hours worked) to analyze the labor market effects of population decline from a macroeconomic point of view.

Notably, this research objective does not only call for combining information from several countries, but identifying causal effects of population changes and differentiating between times of demographic growth and decline. Tailoring our modelling approach to these requirements, we combine a reduced form panel model with an instrumental variable approach and a smooth transition specification. We identify structural population shocks in the reduced form residuals using lagged births as external instruments for working-age population inflows and outflows, and derive regime-dependent orthogonal impulse response functions to trace the effects of positive (negative) population shocks in the labor market in times of population growth (decline).

So far, the existing literature has relied on theoretical models to analyze the economic effects of population decline. Empirically, the effects have, as yet, been unclear, and our paper has addressed this gap. The results resemble the conclusion that maintaining economic growth is generally feasible and add additional insights: We find that population changes pass through to the labor market more quickly in times of decline compared to times of growth. Subsequently, regime-dependent adjustment processes unfold. Labor force participation increases as a response to the decline in labor supply, and in the long term it does so more strongly than it shrinks in times of growth. This rise in overall labor force participation likely plays a crucial role for further observed patterns, for example in decelerating initially swift disinvestment tendencies in times of population decline. By contrast, we find no significant changes in the unemployment rate as a response to population decline. Similarly, despite declining labor supply, we do not find any significant changes of wages as a shortage indicator over time. Eventually, while both our results and the existing economic literature on population decline point to negative effects on productivity, the findings of this paper suggest that corresponding negative effects for economic growth are mitigated by increases in participation and the capital-labor ratio.

Thus, the paper suggests that incorporating elastic labor supply into future model-based approaches that analyze the effects of population decline may enhance the resulting insights. Importantly, two properties should be considered: First, increases in participation are limited, eventually by the population size. Second, adjustments along the participation dimension may take time, as our results suggest. Similar results exist for other interventions. For example, there is evidence that active labor market policies (ALMPs) increase labor force participation rates (Escudero 2018) – but short-term effects of ALMPs are substantially smaller than medium- to long-term effects (Card/Kluve/Weber 2018 for ALMPs targeting persons inside the labor force).

Furthermore, and despite some caveats for the interpretation of the results as discussed in section 5, the paper offers additional contributions, both to the academic literature as well as in a broader policy perspective. Regarding the academic literature, the further contribution is threefold: Data relatedly, the paper offers a (partially) novel compilation of historical labor market data, providing both a suitable database for future research projects as well as a suitable starting point to improve existing, or create new, historical datasets. Methodically, it expands the existing body of literature by combining a proxy SVAR identification strategy with a nonlinear reduced form panel model. Substantively, it proposes to use, and implements, lagged births as a suitable instrument for the identification of population shocks. Jointly, these contributions permit, for the first time, the conducting of a comprehensive empirical analysis of the labor market effects of population decline from a macroeconomic perspective.

However, also in the broader policy context, the results bear importance. In light of the imminent population decline across advanced economies and corresponding discussions concerning labor supply shortages, findings such as increasing labor force participation in response to population decline are of crucial importance – and indicate that adjustments are feasible. This point must be qualified in its translation into policy. Clearly, the activation of individuals outside of the labor force is limited beyond a certain point; this applies, in particular, to demographic groups that already exhibit high participation rates. Therefore, adjustment is likely to become more critical the more the existing potential is exhausted. Furthermore, the systematic responses to demographic shocks as measured in the model based on empirical data include the political reactions that have appeared in the past. Therefore, if policymakers want to change the outcome, their measures would have to go beyond the typical reactions of the past.

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Appendix A

Note: The list of references for the data described below can be found at the end of Appendix A. Whenever we refer to the usage of "relative changes", we chain-link the respective time-series to the subsequent, more recent data.

Demographic variables

Population

Population by age group at mid-year. Population by broad age groups (0-14 years, 15-64 years, 65 years and above) at mid-year. For population data that is only available for another date, we follow the Human Mortality Database (HMD 2023) methodology and estimate the population as of mid-year by linear interpolation.

Australia 1860 – 1901: We use the relative changes of the total population from Gapminder (2015a) since the series by Bolt/van Zanden (2020) contains a break. | 1901 – 1921: We use the relative changes of population by age from Smith (2009), data taken from Hyndman (2017). | 1921 – 2019: HMD (2023) | 2019 – 2025: UN (2019)

Austria 1860 – 1870: We use the relative changes of the total population from Bolt/van Zanden (2020). | 1870 – 1914: We use the relative changes of population by age from Ediev/Gisser (2007). | 1914 – 1923: We use figures for the total population from Bolt/van Zanden (2020) as well as shares by broad age groups in 1914 and 1923 from Ediev/Gisser (2007). In between, we use information on births to obtain annual variation. See detailed explanation of construction of these series in the text for Germany below (1911-1950 period). For more fine-grained age groups among those in working age, we linearly interpolate the respective shares. | 1923 – 1940: We use the relative changes of the population by age from Ediev/Gisser (2007). | 1940 – 1947: We use figures for the total population from Bolt/van Zanden (2020) as well as shares by age groups in 1940 from Ediev/Gisser (2007) and in 1947 from HMD (2023). In between, we use information on births to obtain annual variation. See detailed explanation of construction of these series as given in the text for Germany below (1911-1950 period). | 1947 – 2019: HMD (2023) | 2019 – 2025: UN (2019)

Belgium 1860 – 1914: HMD (2023) | 1914 – 1919: We use figures for total population from Bolt/van Zanden (2020) as well as shares by broad age groups in 1914 and 1923 from HMD (2023). In between, we use information on births to obtain annual variation. See detailed explanation of construction of these series in the text for Germany below (1911-1950 period). For more fine-grained age groups among those in working-age, we linearly interpolate the respective shares. | 1919 – 2019: HMD (2023) | 2019 – 2025: UN (2019)

Denmark 1860 – 2019: HMD (2020) | 2019 – 2025: UN (2019)

Finland 1860 – 1866: We use the relative changes of the total population from Bolt/van Zanden (2020). | 1866 – 1878: We use the relative changes of the population by age from Statistics Finland (2020). The data is given for 31st December, we interpret the data to be representative for 1st January of the subsequent year – this procedure follows the literature (e.g., Klüsener et al. 2019). | 1878 – 2019: HMD (2023) | 2019 – 2025: UN (2019)

France 1860 – 2016: HMD (2023)| 2019 – 2025: UN (2019)

Germany 1860 – 1872: We use the relative changes of the total population from Bolt/van Zanden (2020). | 1872 – 1911: We use the relative changes of the population by age from Lösch (1936). Data is largely taken from the Gesis database, but we apply some adjustments (error correction) using the original source. The data is given for 31st December, we interpret the data to be representative for 1st January of the subsequent year. | 1911 – 1950: As there is no annual data on population by age, we use the relative changes from a newly constructed annual series: Since the data for the total population by Bolt/van Zanden (2020) contains level breaks, we use information from StBA (1972) for the population on the territory of the FRG in 1910, 1939, and 1946 to the scale their series. Then, we use information on population shares by broad age groups at census dates in 1919, 1925, 1933, 1939, and 1946 from Franzmann (2015) and for 1950 from Sensch (2004), original source is Rothenbacher (2002). For 1910, we use the shares by broad age groups as given in Lösch (1936). Using these shares as well as the scaled annual series for the total population from Bolt/van Zanden (2020), we obtain reference figures for age groups at census dates. To obtain annual variation between census dates, we use information on births. For those below the age of 15, we add births in the same year as inflows and subtract births lagged 15 years as outflows. For those aged 15 to 64 years, we add births lagged 15 years as inflows and subtract births lagged 65 years as outflows. For those aged 65 years and above, we add births lagged 65 years as inflows. We apply this procedure for each intercensal period and scale the obtained series by their respective deviation from the subsequent “reference figure”. We apply the resulting shares for age groups to the scaled annual series as outlined above to ensure consistency. The data for births is the same as outlined below. Information on lagged births is subject to the same adjustment for cohort mortality as outlined in the main text. For more fine-grained age groups, we linearly interpolate the respective shares, using the same sources as outlined for broad age groups in this paragraph. | 1950 – 1951: We use the relative change of the total population from Bolt/van Zanden (2020). | 1951 – 1956: We use the relative changes of the population by age from StBA (2023), data refers to East and West Germany. | 1956 – 2019: HMD (2023), up to 1990 relative changes for West Germany. To avoid a break due to the structural changes in the population after German re-unification, we calculate the relation of the age shares for total Germany and West Germany in 1990 and linearly interpolate its deviation back to 1956. We scale the population shares for West Germany by the resulting series and obtain population figures by age group by multiplication with the chain-linked overall working-age population. | 2019 – 2025: We use the relative changes of the population by age from UN (2019).

Italy 1860 – 1872: We use the relative changes of the total population from Bolt/van Zanden (2020). | 1872 – 2019: HMD (2023) | 2019 – 2025: UN (2019)

Japan 1860 – 1872: We use the relative changes of the total population from Bolt/van Zanden (2020). | 1872 – 1920: We use the relative changes of the population by age from Umemura et al. (1988), data taken from RCISSS (2016). However, in this source, only age-specific data for those aged 60 years and above is available. To be able to separate the age groups 15-64 years as well as 65 years and above, we use shares of those groups at census dates in 1884, 1893, 1903, 1913, and 1920 as given by Mitchell (2013). We linearly interpolate these shares and use the resulting series to separate the age group of those 60 years of age and above into those below and those above 65 years of age. The data is given for 31st December, we interpret the data to be representative for 1st January of the subsequent year. | 1920 – 1940: We use the relative changes of the population by age from Statistics Japan (2021). | 1940 – 1947: We use figures for the total population from Bolt/van Zanden (2020) as well as the shares by age groups in 1940 from Statistics Bureau of Japan (2000) and 1947 as outlined in the following. In between, we use information on births to obtain annual variation. See detailed explanation of construction of these series in the text for Germany above (1911-1950 period). | 1947 – 2019: Since HMD (2023) only offers data on Japanese only, we use data for Japanese and Non-Japanese population from Statistics Japan (2021) for 1st October and interpolate to mid-year values. For the years 1996-1999, 2001-2004, and 2011-2014 we use the relative changes from HMD (2023) due to breaks after census dates. See HMD (2023) for more detailed information. | 2019 – 2025: UN (2019)

Netherlands 1860 – 2019: HMD (2023) | 2019 – 2025: UN (2019)

Norway 1860 – 2019: HMD (2023) | 2019 – 2025: UN (2019)

New Zealand 1860 – 1871: We use the relative changes of the total population from Bolt/van Zanden (2020) for Australia. | 1871 – 1901: We use the relative changes of the total population from Bolt/van Zanden (2020). | 1901 – 1948: We use the relative changes of population by age (non-Maori) from HMD (2023). | 1948 – 2019: HMD (2023) | 2019 – 2025: UN (2019)

Sweden 1860 – 2019: HMD (2023) | 2019 – 2025: UN (2019)

Switzerland 1860 – 1861: We use the relative changes of the total population from Bolt/van Zanden (2020). | 1861 – 1875: We use the relative changes of population by age from BFS (2023). The data is given for 31st December, we interpret the data to be representative for 1st January of the subsequent year. | 1875 – 2019: HMD (2023) | 2019 – 2025: UN (2019)

United Kingdom 1860 – 1922: We use the relative changes of the population by age from HMD (2023), data refer to England, Wales, and Scotland. | 1922– 2019: HMD (2023) | 2019 – 2025: UN (2019)

United States 1860 – 1900: There is no series for population by age over this time period. We apply several data preparation steps in order to derive annual series that approximate population by age as closely as possible. We prepare the data as follows: We gather data on population by single age years at census dates in 1880 and 1890 from U.S. Bureau of the Census (1883) and U.S. Bureau of the Census (1895) and for 1900 from U.S. Bureau of the Census (2000). For 1880 and 1890, we smooth the shares of single age years by a centered five-year moving average to cope with age heaping. Additionally, we obtain shares for the 1870 census as follows: We use shares for five-year age groups as given by Mitchell (2013) and obtain the ratio of these shares between 1870 and 1880. We treat these ratios to be representative for the youngest cohort in each age group and linearly interpolate to derive shares for each individual cohort. We then scale the 1880 shares of single age years accordingly and obtain a distribution for 1870. Additionally, we use births data as outlined below. We interpolate cohort shares between census dates and use births to introduce new cohorts. We calculate the ratio of births to population aged 0 years in the census and linearly interpolate this share to scale the entry of new cohorts. Moreover, we scale the resulting overall population in each year by the series given by HSUS (2023). Also, before 1870, we assume all age groups to follow the relative changes of this series by HSUS (2023). | 1900 – 1948: We use the relative changes of population by age from U.S. Bureau of the Census (2000). | 1948 – 2019: HMD (2023) | 2019 – 2025: UN (2019)

Births

Live births per year.

Australia 1835 – 1850: We use the relative changes of births from Gapminder (2015b). | 1850 – 1860: We use the relative changes of births derived by using the births rates from Mitchell (2013) and the total population from Bolt/van Zanden (2020). | 1860 – 2004: HMD (2023)

Austria 1835 – 1871: We use the relative changes of births from Butschek (1992). | 1871 – 2004: HMD (2023)

Belgium 1835 – 1840: We use the relative changes of births derived by using the birth rates from Mitchell (2013) and the total population from Bolt/van Zanden (2020). | 1840 – 2004: HMD (2023)

Denmark 1810 – 1835: We use the relative changes of births derived using birth rates and population figures from Abildgren (2017). | 1835 – 2004: HMD (2023)

Finland 1835 – 2004: We use the relative changes of births from Statistics Finland (2023).

France 1835 – 2004: HMD (2023)

Germany 1810 – 1817: We use the relative changes of births from Gapminder (2015b). | 1817 – 1976: We use the relative changes of births from Sensch (2004), and scale the series from Sensch (2004) for 1943 and earlier with the ratio of births according on the territory of the FRG to StBA (1972). We fill the gaps in 1944 and 1945 as follows: For 1944, we use the relative change in the TFR according to BiB (2023). For 1945, we used the relative change according to StBA (1972). | 1976 – 1990: We use the relative changes of the sum of the births for West Germany and East Germany from HMD (2023). | 1990 – 2004: HMD (2023)

Italy 1835 – 1862: We use the relative changes of births from Gapminder (2015b). | 1862 – 2004: HMD (2023)

Japan 1835 – 1873: We use the relative changes of births from Gapminder (2015b). | 1873 – 2004: We use births from HMD (2023), except for 1941-1943, where we use the relative changes in births derived using the birth rate from Mitchell (2013) and the total population from Bolt/van Zanden (2020) as well as for 1944-1946, where we use the relative changes from Gapminder (2015b).

Netherlands 1810 – 1850: We use the relative changes of births from Smits/Horlings/van Zanden (2000). | 1850 – 2004: HMD (2023)

New Zealand 1835 – 1855: We use the relative changes of births from Gapminder (2015b). | 1855 – 1948: We use the relative changes of births from Statistics New Zealand (2002). | 1948 – 2004: HMD (2023)

Norway 1810 – 1846: We use the relative changes of births derived by using the birth rates from Mitchell (2013) and the total population from Bolt/van Zanden (2020). Population data for 1810-1820 was derived using the average annual growth rate from 1820-1830. | 1846 – 2004: HMD (2023)

Sweden 1810 – 2004: HMD (2023)

Switzerland 1835 – 1871: We use the relative changes of births from HSSO (2022a). | 1871 – 2004: HMD (2023)

United Kingdom 1810 – 1841: We use the relative changes of births from Gapminder (2015b). | 1841 – 1855: We use the relative changes of births for England and Wales from HMD (2023). | 1855

– 1922: We use the relative changes of births for England, Wales, and Scotland from HMD (2023). | 1922 – 2004: HMD (2023)

United States 1810 – 1855: We use the relative changes of births from Gapminder (2015b). | 1855 – 1909: We use the relative changes of births derived by using the birth rates from Mitchell (2013) and the total population from Bolt/van Zanden (2020). | 1909 – 1933: We use the relative changes of births from NCHS (2023). | 1933 – 2004: HMD (2023)

Cohort mortality

Mortality for each cohort up to 15 and up to 65 years of age. Available data sources do not cover all years in the sample, thus we augment cohort mortality rates by centered period death rates (by single age years) or use other data preparation methods outlined in detail below.

Australia 1835 – 1849: We assume the value of 1850. | 1850 – 1921: We use the ratio of UK cohort mortality in 1850 and 1921, chain-link, and linearly interpolate in-between. | 1921 – 1986: HMD (2020) | 1986 – 2001: We use the relative changes in the centered period death rates from HMD (2020). | 2002 – 2004: Assumed value of 2001.

Austria 1835 – 1947: We use the relative changes in the centered period death rates from Ediev/Gisser (2007). For missing observations in 1940-1946, we use the relative changes in period death rates as in WW I. | 1947 – 1987: HMD (2020) | 1987 – 2002: We use the relative changes in the centered period death rates from HMD (2020). | 2003 – 2004: We assume the value of 2002.

Belgium 1835 – 1841: We assume the value of 1841. | 1841 – 1919: We use the relative changes in the centered period death rates from HMD (2020). Moreover, we use the variation in the French period death rates for 1913-1918. | 1919 – 1988: HMD (2020) | 1988 – 2003: We use the relative changes in the centered period death rates from HMD (2020). | 2004: We assume the value of 2003.

Denmark 1810 – 1835: We assume the value of 1835. | 1835 – 1988: HMD (2020) | 1988 – 2004: We use the relative changes in the centered period death rates from HMD (2020).

Finland 1835 – 1878: We use the ratio of Swedish cohort mortality in 1835 and 1878, chain-link, and linearly interpolate in-between. | 1878 – 1988: HMD (2020) | 1988 – 2004: We use the relative changes in the centered period death rates from HMD (2020).

France 1835 – 1987: HMD (2020) | 1987 – 2004: We use the relative changes in the centered period death rates from HMD (2020).

Germany 1810 – 1818: We assume the value of 1819. | 1819 – 1871: We use the ratio of Austrian centered period mortality in 1819 and 1871 according to Ediev/Gisser (2007), chain-link, and linearly interpolate in-between. | 1871 – 2004: StBA (2017)

Italy 1835 – 1871: We assume the value of 1872. | 1872 – 1987: HMD (2020) | 1987 – 2004: We use the relative changes in the centered period death rates from HMD (2020).

Japan 1835 – 1849: We assume the value of 1850. | 1850 – 1947: We use the ratio of UK cohort mortality in 1850 and 1947, chain-link, and linearly interpolate in-between. | 1947 – 2003: HMD (2020) | 2004: We assume the value of 2004.

Netherlands 1810 – 1849: We assume the value of 1850. | 1850 – 1986: HMD (2020) | 1986 – 2001: We use the relative changes in the centered period death rates from HMD (2020). | 2002 – 2004: We assume the value of 2001.

New Zealand 1835 – 1840: We assume the value of 1841. | 1841 – 1901: We use the ratio of UK cohort mortality in 1841 and 1901, chain-link, and linearly interpolate in-between. | 1901 – 1948: We use the relative changes in the cohort mortality rates for the Non-Maori population from HMD (2020). | 1948 – 1983: HMD (2020) | 1983 – 1998: We use the relative changes in the centered period death rates from HMD (2020). | 1999 – 2004: We assume the value of 1998.

Norway 1810 – 1845: We assume the value of 1846. | 1846 – 1988: HMD (2020) | 1988 – 2003: We use the relative changes in the centered period death rates from HMD (2020). | 2004: We assume the value of 2003.

Sweden 1810 – 1988: HMD (2020) | 1988 – 2003: We use the relative changes in the centered period death rates from HMD (2020). | 2004: We assume the value of 2003.

Switzerland 1835 – 1876: We use the ratio of Austrian centered period mortality rates in 1819 and 1876 according to Ediev/Gisser (2007), chain-link, and linearly interpolate in-between. | 1876 – 1896: We use the relative changes in the centered period death rates from HMD (2020). | 1896 – 1986: HMD (2020) | 1986 – 2004: We use the relative changes in the centered period death rates from HMD (2020).

United Kingdom 1810 – 1840: We assume the value of 1841. | 1841 – 1922: We use the relative changes in the cohort death rates for England and Wales from HMD (2020). | 1922 – 1986: HMD (2020) | 1986 – 2001: We use the relative changes in the centered period death rates from HMD (2020). | 2002 – 2004: We assume the value of 2001.

United States 1810 – 1849: We assume the value of 1850. | 1850 – 1933: We use the ratio of UK cohort mortality in 1850 and 1933, chain-link, and linearly interpolate in-between. | 1933 – 1987: HMD (2020) | 1987 – 2001: We use the relative changes in the centered period death rates from HMD (2020). | 2002 – 2004: We assume the value of 2001.

Labor supply variables

In the model, we include information on employment and the labor force. Doing the latter inevitably requires information on unemployment. Moreover, aiming to analyze the effects of changes in a given population, here aged 15-64 years, the labor supply variables should be consistent with this age group definition. To address these requirements, we combine information on total employment, unemployment rates, and labor force participation rates by age from a large number of sources and conducted extensive data preparation. Below, we describe the process and the underlying sources in detail. Importantly, as indicated in each case below, for some observations the derivation of the (un-)employment value can only be understood by simultaneously referring to the detailed information on the sources and data preparation description of the respective other part of the labor force.

Employment

Employment, annual average.

Australia 1900 – 1950: We use the relative changes in total employment from Whiters/Endres/Perry (1985). | 1950 – 1978: We use the relative changes in total employment level from Feenstra/Inklaar/Timmer (2015). | 1978 – 2019: First, we obtain level figures for the labor force aged 15-64 years by multiplying participation rates from OECD (2022a) and the population aged 15-64 years as stated above. Then, second, we derive employment figures by subtracting those unemployed according to the unemployment rate as outlined below.

Austria 1900 – 1950: We use the relative changes in total employment from Butschek (1992), with linear interpolation for 1914-1915 and with replacement of the value in 1913 with the ratio of employment in 1913 and 1924 according to WIFO (1965). | 1950 – 1994: We use the relative changes in total employment from Feenstra/Inklaar/Timmer (2015). | 1994 – 2019: First, we obtain level figures for the labor force aged 15-64 years by multiplying participation rates from OECD (2022a) and the population aged 15-64 years as stated above. Then, second, we derive employment figures by subtracting those unemployed according to the unemployment rate as outlined below. We chain-link the break in the series in 2004 by using information on total employment from Feenstra/Inklaar/Timmer (2015).

Belgium 1900 – 1913: We use the labor force figures from Maddison (1982) for 1900 and 1913 and linearly interpolate in-between. We obtain the employment figures by subtracting unemployment as given by the unemployment rate outlined below. | 1913 – 1924: We use the relative changes in total employment from Bergeaud/Cette/Lecat (2016); see sources cited therein. We obtain the ratio to the subsequent and the preceding series, linearly interpolate these ratios, and scale the series by Bergeaud/Cette/Lecat (2016) accordingly. | 1924 – 1938 and 1950: We use the relative changes in the labor force figures from Maddison (1964) and the unemployment rate as outlined below to derive employment figures. | 1938 – 1946: We use labor force figures from Maddison (1964) for 1938, and for 1946 as outlined below. We linearly interpolate in-between and use the unemployment rate as outlined below to derive employment figures. | 1946 – 1950: We use the relative changes in the labor force figures by Clark (1957) and the values for the unemployment rate as outlined below to derive total employment. | 1950 – 2002: We use the relative changes in total employment from Feenstra/Inklaar/Timmer (2015). | 2002 – 2019: First, we obtain level figures for the labor force aged 15-64 years by multiplying participation rates from OECD (2022a) and the population aged 15-64 years as stated above. Then, second, we derive employment figures by subtracting those unemployed according to the unemployment rate as outlined below.

Denmark 1875 – 1900: We use the relative changes in total employment obtained by using labor force from Abildgren (2017) and the unemployment rate as outlined below. | 1900 – 1950: We use the relative changes in total employment from Abildgren (2017). | 1950 – 2002: We use the relative changes in total employment from Feenstra/Inklaar/Timmer (2015). | 2002 – 2019: First, we obtain level figures for the labor force aged 15-64 years by multiplying participation rates from OECD (2022a) and the population aged 15-64 years as stated above. Then, second, we derive employment figures by subtracting those unemployed according to the unemployment rate as outlined below.

Finland 1900 – 1950: We use the relative changes in total employment derived from information on unemployment levels and rate, provided by Statistics Finland on request. | 1950 – 1970: We use the relative changes in total employment from Feenstra/Inklaar/Timmer (2015). | 1970 –

2019: First, we obtain level figures for the labor force aged 15-64 years by multiplying participation rates from OECD (2022a) and the population aged 15-64 years as stated above. Then, second, we derive employment figures by subtracting those unemployed according to the unemployment rate as outlined below.

France 1900 – 1950: We use the relative changes in total employment from Villa (1993), series EMP. For missing observations in 1914-1918 and in 1940-1945, we use the series EMPE, which contains total firm employment. | 1950 – 2003: We use the relative changes in total employment from Feenstra/Inklaar/Timmer (2015). | 2003 – 2019: First, we obtain level figures for the labor force aged 15-64 years by multiplying participation rates from OECD (2022a) and the population aged 15-64 years as stated above. Then, second, we derive employment figures by subtracting those unemployed according to the unemployment rate as outlined below.

Germany 1875 – 1939: We use the relative changes in total employment from Hoffmann (1965) and Sommariva/Tullio (1987), data obtained from Rahlf (2015) and Sommariva/Tullio (1987); data points for 1876-1877 are linearly interpolated. For 1918-1919, we use the information on population given in Sommariva/Tullio (1987) and the data on total population as outlined above to cope with the boundary break. | 1939 – 1945: Again, we use the relative changes in total employment from Sommariva/Tullio (1987), original source is Hoffmann (1965), data with filled gaps. However, here we scale the data point in 1939 using data on population as outlined above and on labor force participation in 1939 and 1950 as well as on unemployment in both years as outlined below. In doing so, we are able to account for the structural break due to boundary changes. | 1946 – 1950: Available sources for this period suggest contradictory employment patterns (e.g., Sommariva/Tullio 1987 and Clark 1957). We derive relative changes in total employment as follows: We use information on unemployment in levels and in rates from Mitchell (2013) to find employment data for 1948-1950. Additionally, we use information on the relative changes in employment from BMA (1950) for 1946 and 1948. For 1947-1948, we use information on the labor force from Abelshauser (1975) and for unemployment from Galenson/Zellner (1957). | 1950 – 1970: We use the relative changes in total employment from Bundesbank (2023). | 1970 – 2019: First, we obtain level figures for the labor force aged 15-64 years by multiplying participation rates from OECD (2022a) and the population aged 15-64 years as stated above. Then, second, we derive employment figures by subtracting those unemployed according to the unemployment rate as outlined below. We chain-link the break in 1990-1991 by using information on total employment from Bundesbank (2020).

Italy 1900 – 1950: We use the relative changes in total employment from Giordano/Zollino (2016). | 1950 – 1970: We use the relative changes in total employment from Feenstra/Inklaar/Timmer (2015). | 1970 – 2019: First, we obtain level figures for the labor force aged 15-64 years by multiplying participation rates from OECD (2022a) and the population aged 15-64 years as stated above. Then, second, we derive employment figures by subtracting those unemployed according to the unemployment rate as outlined below.

Japan 1900 – 1920: We use the relative changes in total employment obtained by using labor force figures (the number of gainful workers, Japanese only, mid-year) aged 15 years and above from Umemura et al. (1988), data obtained from RCISSS (2016), and unemployment as outlined below. | 1920 – 1940: We use the relative changes in total employment obtained by using labor force figures (the number of gainful workers, 1st October) aged 15 years and above from

Umemura et al. (1988), data obtained from RCISSS (2016) and JSPS (2023), and unemployment as outlined below. | 1940 – 1950: We use the relative changes in total employment from Pilat (2002), data taken from van Leeuwen (2007). | 1950 – 1968: We use the relative changes in total employment from Feenstra/Inklaar/Timmer (2015). | 1968 – 2019: First, we obtain level figures for the labor force aged 15-64 years by multiplying participation rates from OECD (2022a) and the population aged 15-64 years as stated above. Then, second, we derive employment figures by subtracting those unemployed according to the unemployment rate as outlined below.

Netherlands 1875 – 1950: We use the relative changes in employment from CBS (2014). | 1950 – 1987: We use the relative changes in total employment from Feenstra/Inklaar/Timmer (2015). | 1987 – 2019: First, we obtain level figures for the labor force aged 15-64 years by multiplying participation rates from OECD (2022a) and the population aged 15-64 years as stated above. Then, second, we derive employment figures by subtracting those unemployed according to the unemployment rate as outlined below.

New Zealand 1900 – 1921: We use the relative change in total employment from Bergeaud/Cette/Lecat (2016). | 1921 – 1939: We use the relative changes in total employment from Rankin (1994); we interpolate to mid-year values. | 1939 – 1950: We use the relative changes in total employment from Bergeaud/Cette/Lecat (2016). | 1950 – 1986: We use the relative changes in total employment from Feenstra/Inklaar/Timmer (2015). | 1986 – 2019: First, we obtain level figures for the labor force aged 15-64 years by multiplying participation rates from OECD (2022a) and the population aged 15-64 years as stated above. Then, second, we derive employment figures by subtracting those unemployed according to the unemployment rate as outlined below.

Norway 1875 – 1900: We use the relative changes in total employment derived as the residual figures from the labor force and unemployment rate as outlined below. | 1900 – 1950: We use the relative changes in total employment from SSB (2020). | 1950 – 1970: We use the relative changes in total employment from Feenstra/Inklaar/Timmer (2015). | 1970 – 2019: First, we obtain level figures for the labor force aged 15-64 years by multiplying participation rates from OECD (2022a) and the population aged 15-64 years as stated above. Then, second, we derive employment figures by subtracting those unemployed according to the unemployment rate as outlined below.

Sweden 1875 – 1950: We use the relative changes in total employment from Krantz/Schön (2007) and Schön/Krantz (2012). | 1950 – 1960: We use the relative changes in total employment from Feenstra/Inklaar/Timmer (2015). | 1960 – 2019: First, we obtain level figures for the labor force aged 15-64 years by multiplying participation rates from OECD (2022a) and the population aged 15-64 years as stated above. Then, second, we derive employment figures by subtracting those unemployed according to the unemployment rate as outlined below.

Switzerland 1900 – 1913: We use the relative changes in total employment from HSSO (2022b) and chain-link the break in 1912-1913 with the growth rate from 1911-1912. | 1913 – 1950: We use the relative changes in total employment from HSSO (2022c). | 1950 – 1956: We use the relative changes in total employment from Feenstra/Inklaar/Timmer (2015). | 1956 – 1987: Since Swiss population data are available for the *de jure* population only with a lag (see, e.g., HMD 2023 for further information), the large inflow of foreign workers around 1960 (see, e.g., Clark 1983) is only mirrored in the employment, but not in the population statistics. To avoid inconsistency with the central variable of interest, the population, we adjust the employment series accordingly. More precisely, we create an appropriate series by removing those persons in employment with foreign citizenship who are not counted in the *de jure* population according to HSSO (2022d). For missing observations in 1972-1973, we use the relative changes in total employment from Feenstra/Inklaar/Timmer (2015). We use the relative changes of this adjusted employment series. | 1987 – 2010: We use the relative changes in total employment from Feenstra/Inklaar/Timmer (2015). | 2010 – 2019: First, we obtain level figures for the labor force aged 15-64 years by multiplying participation rates from OECD (2022a) and the population aged 15-64 years as stated above. Then, second, we derive employment figures by subtracting those unemployed according to the unemployment rate as outlined below.

United Kingdom 1875 – 1950: We use the relative changes in total employment from Bank of England (2017). | 1950 – 2004: We use the relative changes in total employment from Feenstra/Inklaar/Timmer (2015). | 2004 – 2019: First, we obtain level figures for the labor force aged 15-64 years by multiplying participation rates from OECD (2022a) and the population aged 15-64 years as stated above. Then, second, we derive employment figures by subtracting those unemployed according to the unemployment rate as outlined below.

United States 1875 – 1900: We use the relative changes in total employment from Vernon (1994). | 1900 – 1947: We use the relative changes in total employment from U.S. Bureau of the Census (1975), calculated by subtracting unemployed persons from total labor force, series D1-10. | 1947 – 1950: We use the relative changes in total employment from U.S. Bureau of the Census (1975), calculated by subtracting unemployed persons from total labor force, series D11-25. | 1950 – 1960: We use the relative changes in total employment from Feenstra/Inklaar/Timmer (2015). | 1960 – 2019: First, we obtain level figures for the labor force aged 15-64 years by multiplying participation rates from OECD (2022a) and the population aged 15-64 years as stated above. Then, second, we derive employment figures by subtracting those unemployed according to the unemployment rate as outlined below.

Unemployment

Unemployment rate in the labor force, annual average. For later years, we use OECD (2022b) for the unemployment rate among those aged 15-64 years. For earlier years, no age-specific information is available. Thus, we use various sources for the unemployment rate in the total labor force and assume that the relative changes in these unemployment rates are the same as for those aged 15-64 years. Put differently, for each country, we use OECD (2022b) as the benchmark series and chain-link the series for the unemployment rate in the total labor force for earlier years, as outlined below in detail, to the earliest value of OECD (2022b).

Australia 1900 – 1963: We use the unemployment rate from Whiters/Endres/Perry (1985). | 1964 – 1966: We use the unemployment rate from OECD (2016). | 1966 – 2019: We use the unemployment rate for those aged 15-64 years from OECD (2022b).

Austria 1900 – 1977: We use the unemployment rate from from Butschek (1992). We fill missing observations in 1914-1915 and 1917 by linear interpolation and in 1942-1945 by assuming the unemployment rate to stay at its 1941 level. | 1978 – 1994: We use the unemployment rate from OECD (2016). | 1994 – 2019: We use the unemployment rate for those aged 15-64 years from OECD (2022b).

Belgium 1900 – 1903: We use the relative changes in the unemployment rate among trade union members in Ghent from Galenson/Zellner (1957), data taken from Sensch (2014). | 1903 – 1912: We use the unemployment rate among trade union members from Galenson/Zellner (1957), data taken from Sensch (2014). | 1913: We use the unemployment rate from Maddison (1964). | 1914 – 1919: We apply linear interpolation. | 1920 – 1939: We use the unemployment rate from Goosens/Peeters/Pepermans (1988). | 1940 – 1944: We apply linear interpolation. | 1945 – 1949: We derive the unemployment rate by using labor force figures from Clark (1957), scaled by the interpolated ratio to Maddisons (1964) labor force figures in 1938 and 1950, and unemployment in levels from Mitchell (2013). We derive a value for the missing observation in Clarks (1957) series in 1945 by linear interpolation. | 1950 – 1959: We use the relative changes in the unemployment rate from Maddison (1982). | 1960 – 1983: We use the unemployment rate from OECD (2016). | 1983 – 2019: We use the unemployment rate for those aged 15-64 years from OECD (2022b).

Denmark 1875 – 1899: We use the relative changes in the unemployment rate from Abildgren (2005). | 1900 – 1960: We use the unemployment rate from Abildgren (2017). | 1960 – 1968: We use the unemployment rate from OECD (2005). We scale its values by obtaining ratios to the preceding and the subsequent sources in 1960 and 1969 and linearly interpolate this ratio in-between. | 1969 – 1982: We use the unemployment rate from OECD (2016). | 1983 – 2019: We use the unemployment rate for those aged 15-64 years from OECD (2022b).

Finland 1900 – 1920: We use the unemployment rate from Tiainen (1994), multiplied by the ratio of this series and Gryttens (2008) series in 1920. The Tiainen data was sent on request by Statistics Finland. | 1920 – 1938: We use the unemployment rate from Grytten (2008). | 1938 – 1950: We use the unemployment rate from Tiainen (1994), scaled by its ratios to the preceding series in 1938, the subsequent series in 1950 as well as their interpolation. | 1950 – 1959: We use the unemployment rate from Maddison (1982). | 1960 – 1963: We use the unemployment rate from OECD (2016). | 1963 – 2019: We use the unemployment rate for those aged 15-64 years from OECD (2022b).

France 1900 – 1950: We apply a three-step procedure: (1) We obtain the unemployment rate using unemployment figures and total employment from Villa (1993). (2) We take the quinquennial census unemployment rates from Galenson/Zellner (1957) as benchmark values. (3) We obtain the ratios of the census rates and the rates from Villa (1993) in (1), linearly interpolate between these observations and scale the series in (1) to obtain unemployment figures for this period. | 1950 – 1960: We use the unemployment rate from Maddison (1982). | 1960 – 1975: We use the unemployment rate from OECD (2016). | 1975 – 2019: We use the unemployment rate for those aged 15-64 years from OECD (2022b).

Germany 1875 – 1887: We use the ratio of the labor force to employment figures for benchmark years from Clark (1957) and scale the employment series as outlined above accordingly. For 1887, we obtain a benchmark value by using the same employment series and information on unemployment as outlined below. We linearly interpolate the labor force in the missing years and obtain unemployment figures by subtracting employment as outlined above. For 1875-1877, we assume the same unemployment rate as in 1877. | 1887 – 1914: We use the unemployment rate from Kuczynski (1962) and Kuczynski (1967), modified by Pierenkemper (1987) and tabulated by Hohls (1991), data taken from Sensch (2016). | 1915 – 1921: We use the unemployment rate by Mitchell (2013). | 1922 – 1939: We use the unemployment rate from Petzina/Abelshauser/Faust

(1978), data taken from Sensch (2016). | 1940 – 1945: For this period, there is no data available. The series of the unemployment rate by Petzina/Abelshauser/Faust (1978) shows a value of 0.5 percent for 1939. We follow the analysis of Kosche/Bach (1991), who suggest that the labor shortage intensified after 1940, additionally to large numbers of forced labor and recruited foreign workers, and assume that there was negligible unemployment during these times. Therefore, we set the unemployment rate to 0.5 percent throughout this period. | 1946 – 1949: We use the unemployment rate from Petzina/Abelshauser/Faust (1978) for 1949 and assume the same value back to 1946. | 1950 – 1970: We use the unemployment rate for West Germany from BA (2020). | 1970 – 2019: We use the unemployment rate for those aged 15-64 years from OECD (2022b).

Italy 1900 – 1913: We use the unemployment rate from Clark (1957), scaled by the 1913 ratio to Maddison (1964). We assume the missing value in 1900 to be equal to 1901. | 1913: We use the unemployment rate from Maddison (1964). | 1914 – 1928: We use the unemployment rate from Clark (1957), scaled by the ratios to Maddison (1964) in 1913 and 1929 as well as their interpolation. | 1929 – 1938: We use the unemployment rate from Maddison (1982), except for 1935-1936 for which we use the same source and procedure as in 1914-1928. | 1939 – 1945: We use the same source and procedure as in 1914-1928, except for the unemployment rate in 1946, which we derive as follows. | 1946 – 1949: We use unemployment levels from Mitchell (2013) and employment levels as outlined above to derive the unemployment rate, scaled by its 1950 ratio to the subsequent source. | 1950 – 1959: We use the unemployment rate from Maddison (1982). | 1960 – 1970: We use the unemployment rate from OECD (2016). | 1970 – 2019: We use the unemployment rate for those aged 15-64 years from OECD (2022b).

Japan 1900 – 1912: We assume the unemployment rate to be equal to the 1913 value. | 1913 – 1929: We use the relative changes in the unemployment rate from Clark (1957), whereby we linearly interpolate in 1914-1918. In using this series, we see a spike in the unemployment rate the early 1920s, which is in accordance with the literature (e.g., Kase 2004). | 1929 – 1939: We use the unemployment rate from Kato (2008). | 1940 – 1946: We derive the unemployment rate by linear interpolation between the preceding and the subsequent series. | 1947 – 1950: We use the relative changes of the unemployment rate from UN (1951). | 1950 – 1960: We use the unemployment rate from Maddison (1982). | 1960 – 1968: We use the unemployment rate from OECD (2016). | 1968 – 2019: We use the unemployment rate for those aged 15-64 years from OECD (2022b).

Netherlands 1875 – 1960: We use the relative changes of the unemployment rate from CBS (2014). | 1960 – 1971: We use the unemployment rate from OECD (2016). | 1971– 2019: We use the unemployment rate for those aged 15-64 years from OECD (2022b).

New Zealand 1900 – 1920: We apply a three-step procedure: (1) We retrieve the unemployment rate from Statistics New Zeland (2002) for 1901, 1906, 1911, and 1916 (consolidated series). (2) We obtain the number of unemployed assisted into employment from Martin (1996), taken from Statistics New Zeland (2002), and linearly interpolate to end-of-year values. (3) We obtain the ratios of, on the one hand, unemployment levels in benchmark years 1901, 1906, 1911, 1916 and for 1921 from Rankin (1994) – prepared as outlined below – using the rate in (1) and employment levels as outlined above, and, on the other hand, the level figures as given in (2). We linearly interpolate these ratios and use the results to scale the figures in (2). Using the scaled figures and employment as outlined above, we find the unemployment rate between benchmark years. For 1900, we assume the same unemployment rate as in 1901. | 1921 – 1939: We use the unemployment rate from Rankin (1994); we interpolate to mid-year values. | 1940 – 1951: We use unemployment levels from Mitchell (2013) and labor force figures as outlined above to find the unemployment rate. We scale this unemployment rate by its ratios to the preceding series in 1940, the subsequent series in 1951 as well as their interpolation. | 1951 – 1960: We use the relative changes in the unemployment rate derived from data on the workforce and unemployment from Rankin (1993); we interpolate to mid-year values. | 1960 – 1986: We use the unemployment rate from OECD (2016). | 1986 – 2019: We use the unemployment rate for those aged 15-64 years from OECD (2022b).

Norway 1875 – 1902: We apply a four-step procedure: (1) We obtain labor force figures from Central Bureau of Statistics of Norway (1966) for 1875, 1880, 1885, 1890, 1895, 1900 and, from the same source for 1877, 1887, 1899 by multiplying total GDP with GDP per person of the labor force provided therein. We linearly interpolate the remaining missing years. (2) We obtain level figures for males seeking public relief from Central Bureau of Statistics of Norway (1968). Due to the break in the series, we scale figures from 1886 onwards with the ratio 1885 to 1886. We use this variable as a proxy for the true unemployment level. (3) We use the relative changes to backward extend the subsequent absolute unemployment figures. We additionally scale these figures by a five-year moving average of the Swedish unemployment rate as outlined below, mirroring the close connection of both countries at that time, amplifying the dynamics underlying the figures of those seeking public relief. (4) We obtain the unemployment rate by calculating the ratio of (3) and (1). | 1903: We calculate a value for 1904 by multiplying the unemployment rate in 1904 using the 1903-to-1904-ratio of the seven-months-average of union unemployment from NOS (1949). | 1904 – 1920: We use the unemployment rate from Grytten (1994). | 1920 – 1939: We use the unemployment rate from Grytten (2008). | 1939 – 1949: We apply a four-step procedure: (1) We use unemployment levels given in SSB (1945), for 1939 and 1941, and SSB (1950), for 1946-1949, and we linearly interpolate the levels for missing observations in 1940 and in 1942-1946. (2) We use the total employment figures from the source indicated above and the unemployed levels in (2) to derive a series for the unemployment rate. (3) We scale this series by the ratio to Grytten (2008) in 1939, the ratio to the chain-linked unemployment rate from Galenson/Zellner (1957), whereby we use the value of Maddison (1982) as a benchmark in 1950, as well as the interpolation of those ratios. | 1950 – 1960: We use the relative changes in the unemployment rate from Maddison (1982). | 1960 – 1972: We use the unemployment rate from OECD (2016). | 1972 – 2019: We use the unemployment rate for those aged 15-64 years from OECD (2022b).

Sweden 1875 – 1911: We use the relative changes in the unemployment rate from Clark (1957). | 1911 – 1920: We use the relative changes in the unemployment rate from Molinder (2018) and Bengtsson/Molinder (2017). | 1920 – 1938: We use the unemployment rate from Grytten (2008). | 1939 – 1960: We use the relative changes in the unemployment rate from Molinder (2018) and Bengtsson/Molinder (2017). | 1960 – 1963: We use the unemployment rate from OECD (2016). | 1963 – 2019: We use the unemployment rate for those aged 15-64 years from OECD (2022b).

Switzerland 1900 – 1913: We use the relative changes in the ratio of job seekers to employed persons to derive information on unemployment dynamics. More precisely, we use job seekers in the city of Zurich from HSSO (2022e), chain-linked to the unemployment level in 1913, and derive the ratio to the employment series as outlined above. | 1913 – 1974: We use the unemployment rate from HSSO (2022c). | 1975 – 1991: We use the unemployment rate from OECD (2016). | 1991 – 2019: We use the unemployment rate for those aged 15-64 years from OECD (2022b).

United Kingdom 1875 – 1970: We use the unemployment rate from Bank of England (2017), per cent of total workforce in the United Kingdom. | 1971 – 1984: We use the unemployment rate from OECD (2016). | 1984 – 2019: We use the unemployment rate for those aged 15-64 years from OECD (2022b).

United States 1875 – 1899: We use the unemployment rate from Vernon (1994). | 1900 – 1959: We use the unemployment rate from Weir (1992), data taken from Ramey/Francis (2009). | 1960 – 2019: We use the unemployment rate for those aged 15-64 years from OECD (2022b).

Labor force participation

Labor force participation rate of those aged 15-64 years, annual average. Here, we indicate the sources which we use to obtain information to approximate the labor force participation rate of those aged 15-64 years in earlier years, and how we use these data to scale employment and unemployment levels as outlined above.

Introducing benchmark values for labor force participation rates is associated with both advantages and disadvantages. One advantage is that the final series delivers more accurate patterns of age-specific participation rates over time than the relative changes of a total employment series without adjustment would provide. For example, the PWT series for total employment in Austria after 1950 (Feenstra/Inklaar/Timmer 2015) uses the same relative changes as employment in Butschek (1992). Notably, employment in Butschek (1992) encompasses only dependent employees but not those being self-employed and suggest a strong increase of employment after WW II. Treating the series as representative for overall employment among working-age in Austria, this would lead to low levels of participation rates among those in working-age in 1950 – far below the figures suggested by ILO (1977a) and others. At the same time, the series for self-employment in Austria by Butschek (1974) for the period 1961-1971 shows that the number of those being self-employed declined by more than a quarter within ten years, suggesting that at least some of the increase in dependent employment may be driven by the decline of self-employment. Thus, introducing benchmark values accounts for this shortcoming.

By contrast, the disadvantage that this procedure implies is that, by construction, some annual change rates are amplified while others are attenuated. If the original change rates are very close to zero, some may flip signs. In the course of preparing the dataset as outlined, we have evaluated the impact of introducing benchmarks and find that the annual change rates of our adjusted series are still very strongly correlated to the original change rates.

Australia 1900 – 1950: As benchmark values, we use the age-specific labor force participation rates for census dates in 1911, 1921, and 1933 from ABS (1917), ABS (1925), and ABS (1937) as well as another benchmark value for 1900 by using the 1900-to-1911-ratio from New Zealand and for 1950 from ILO (1977a). We obtain the ratio of these benchmark values to the labor force participation rate derived from the (unadjusted) data on total employment and the unemployment rate as outlined above. In-between the benchmark years, we linearly interpolate the ratio. We use the resulting series to scale the (unadjusted) data on total employment and the unemployment rate as outlined above. | 1950 – 1978: As benchmark values, we use the age-specific labor force participation rate from ILO (1977a) (1950, 1955, 1960, 1965). We obtain the ratio of these benchmark values to the labor force participation rate derived from the (unadjusted) data on total employment and the unemployment rate as outlined above. For 1978, we assume a ratio of 1. In-between the benchmark years, we linearly interpolate the ratio. We use the resulting series to scale the (unadjusted) data on total employment and the unemployment rate as outlined above.

Austria 1900 – 1950: As a benchmark value, we use the age-specific labor force participation rate (for those aged 11 to 70) for the census in 1910 from K.K. Statistische Zentralkommission (1916) as well as another benchmark for 1950 from ILO (1977a). We obtain the ratio of this benchmark value to the labor force participation rate derived from the (unadjusted) data on total employment and the unemployment rate as outlined above. In-between the benchmark years, we linearly interpolate the ratio. We use the resulting series to scale the (unadjusted) data on total employment and the unemployment rate as outlined above. | 1950 – 1994: As benchmark values, we use the age-specific labor force participation rate from ILO (1977a) (1950, 1955, 1960, 1965, 1970). We obtain the ratio of these benchmark values to the labor force participation rate derived from the (unadjusted) data on total employment and the unemployment rate as outlined above. For 1994, we assume a ratio of 1. In-between the benchmark years, we linearly interpolate the ratio. We use the resulting series to scale the (unadjusted) data on total employment and the unemployment rate as outlined above.

Belgium 1950 – 2002: As benchmark values, we use the age-specific labor force participation rate from ILO (1977a) (1950, 1955, 1960, 1965, 1970). We obtain the ratio of these benchmark values to the labor force participation rate derived from the (unadjusted) data on total employment and the unemployment rate as outlined above. For 2002, we assume a ratio of 1. In-between the benchmark years, we linearly interpolate the ratio. We use the resulting series to scale the (unadjusted) data on total employment and the unemployment rate as outlined above.

Denmark 1950 – 2002: As benchmark values, we use the age-specific labor force participation rate from ILO (1977a) (1955, 1960, 1965, 1970). We obtain the ratio of these benchmark values to the labor force participation rate derived from the (unadjusted) data on total employment and the unemployment rate as outlined above. For 2002, we assume a ratio of 1. In-between the benchmark years, we linearly interpolate the ratio. We use the resulting series to scale the (unadjusted) data on total employment and the unemployment rate as outlined above.

Finland 1950 – 1970: As a benchmark, we use the age-specific labor force participation rate from ILO (1977a) for 1950. We obtain the ratio of these benchmark value to the labor force participation rate derived from the (unadjusted) data on total employment and the unemployment rate as outlined above. For 1970, we assume a ratio of 1. In-between the benchmark years, we linearly interpolate the ratio. We use the resulting series to scale the (unadjusted) data on total employment and the unemployment rate as outlined above.

France 1900 – 1950: As benchmark values, we use the age-specific labor force participation rates for census dates in 1906, 1911, 1921, 1931, and 1946 from Maruani/Meron (2012) and for 1950 from ILO (1977a). We obtain the ratio of these benchmark values to the labor force participation rate derived from the (unadjusted) data on total employment and the unemployment rate as outlined above. In-between the benchmark years, we linearly interpolate the ratio. We use the resulting series to scale the (unadjusted) data on total employment and the unemployment rate as outlined above. | 1950 – 2003: As benchmark values, we use the age-specific labor force participation rate from ILO (1977a) (1950, 1955, 1960, 1965). We obtain the ratio of these benchmark values to the labor force participation rate derived from the (unadjusted) data on total employment and the unemployment rate as outlined above. For 2003, we assume a ratio of 1. In-between the benchmark years, we linearly interpolate the ratio. We use the resulting series to scale the (unadjusted) data on total employment and the unemployment rate as outlined above.

Germany 1895 – 1950: As benchmark values, we use the age-specific labor force participation rates for 1895, 1907, 1925, 1933, and 1939 from Long (1958) and for 1950 from ILO (1977a). We obtain the ratio of these benchmark values to the labor force participation rate derived from the (unadjusted) data on total employment and the unemployment rate as outlined above. In-between the benchmark years, we linearly interpolate the ratio. We use the resulting series to scale the (unadjusted) data on total employment and the unemployment rate as outlined above. | 1950 – 1990: As benchmark values, we use the indexed age-specific labor force participation rate from ILO (1977a) (1950, 1955, 1960, 1965, 1970). We obtain the ratio of these benchmark values to the labor force participation rate derived from the (unadjusted) data on total employment and the unemployment rate as outlined above. For 1978, we assume a ratio of 1. In-between the benchmark years, we linearly interpolate the ratio. We use the resulting series to scale the (unadjusted) data on total employment and the unemployment rate as outlined above.

Italy 1901 – 1950: As a benchmark value, we use the age-specific labor force participation rate for the census in 1901 from Fuà (1976) as well as another benchmark for 1950 from ILO (1977a). We obtain the ratio of this benchmark value to the labor force participation rate derived from the (unadjusted) data on total employment and the unemployment rate as outlined above. In-between the benchmark years, we linearly interpolate the ratio. We use the resulting series to scale the (unadjusted) data on total employment and the unemployment rate as outlined above. | 1950 – 1970: As a benchmark value, we use the age-specific labor force participation rate from ILO (1977a) for 1950. We obtain the ratio of these benchmark values to the labor force participation rate derived from the (unadjusted) data on total employment and the unemployment rate as outlined above. For 1970, we assume a ratio of 1. In-between the benchmark years, we linearly interpolate the ratio. We use the resulting series to scale the (unadjusted) data on total employment and the unemployment rate as outlined above.

Japan 1920 – 1950: As a benchmark value, we use the age-specific labor force participation rate for 1920, derived by using age-group specific rates from Umemura (1962) and population by Statistics Japan (2021), as well as another benchmark for 1950 from ILO (1977b). We obtain the ratio of this benchmark value to the labor force participation rate derived from the (unadjusted) data on total employment and the unemployment rate as outlined above. In-between the benchmark years, we linearly interpolate the ratio. We use the resulting series to scale the (unadjusted) data on total employment and the unemployment rate as outlined above. | 1950 – 1968: As benchmark values, we use the age-specific labor force participation rate from ILO (1977b) (1950, 1955, 1960, 1965). We obtain the ratio of these benchmark values to the labor force participation rate derived from the (unadjusted) data on total employment and the unemployment rate as outlined above. For 1968, we assume a ratio of 1. In-between the benchmark years, we linearly interpolate the ratio. We use the resulting series to scale the (unadjusted) data on total employment and the unemployment rate as outlined above.

Netherlands 1950 – 1987: As benchmark values, we use the age-specific labor force participation rate from ILO (1977a) (1950, 1955, 1960, 1965, 1970). We obtain the ratio of these benchmark values to the labor force participation rate derived from the (unadjusted) data on total employment and the unemployment rate as outlined above. For 1987, we assume a ratio of 1. In-between the benchmark years, we linearly interpolate the ratio. We use the resulting series to scale the (unadjusted) data on total employment and the unemployment rate as outlined above.

New Zealand 1901 – 1950: As benchmark values, we use the indexed age-specific labor force participation rates for 1901, 1906, 1911, 1921, 1926, 1936, and 1945 from Long (1958) and for 1950 from ILO (1977a). We obtain the ratio of these benchmark values to the labor force participation rate derived from the (unadjusted) data on total employment and the unemployment rate as outlined above. In-between the benchmark years, we linearly interpolate the ratio. We use the resulting series to scale the (unadjusted) data on total employment and the unemployment rate as outlined above. | 1950 – 1970: As benchmark values, we use the indexed age-specific labor force participation rate from ILO (1977a) (1950, 1955, 1960, 1965, 1970). We obtain the ratio of these benchmark values to the labor force participation rate derived from the (unadjusted) data on total employment and the unemployment rate as outlined above. For 1970, we assume a ratio of 1. In-between the benchmark years, we linearly interpolate the ratio. We use the resulting series to scale the (unadjusted) data on total employment and the unemployment rate as outlined above.

Norway 1950 – 1970: As benchmark values, we use the indexed age-specific labor force participation rate from ILO (1977a) (1950, 1955, 1960, 1965, 1970). We obtain the ratio of these benchmark values to the labor force participation rate derived from the (unadjusted) data on total employment and the unemployment rate as outlined above. For 1970, we assume a ratio of 1. In-between the benchmark years, we linearly interpolate the ratio. We use the resulting series to scale the (unadjusted) data on total employment and the unemployment rate as outlined above.

Sweden No additional adjustment for age-specific participation rates.

Switzerland No additional adjustment for age-specific participation rates.

United Kingdom 1911 – 1950: As benchmark values, we use the age-specific labor force participation rates (14-64 years) for census dates in 1911, 1921, 1931, and 1939 from Long (1958) and for 1950 from ILO (1977a). We obtain the ratio of these benchmark values to the labor force participation rate derived from the (unadjusted) data on total employment and the unemployment rate as outlined above. In-between the benchmark years, we linearly interpolate the ratio. We use the resulting series to scale the (unadjusted) data on total employment and the unemployment rate as outlined above. | 1950 – 2004: As benchmark values, we use the age-specific labor force participation rate from ILO (1977a) (1950, 1955, 1960, 1965, 1970). We obtain the ratio of these benchmark values to the labor force participation rate derived from the (unadjusted) data on total employment and the unemployment rate as outlined above. For 2004, we assume a ratio of 1. In-between the benchmark years, we linearly interpolate the ratio. We use the resulting series to scale the (unadjusted) data on total employment and the unemployment rate as outlined above.

United States 1890 – 1950: As benchmark values, we use the age-specific labor force participation rates for census dates in 1890, 1900, 1910, 1920, 1930, and 1940 from Long (1958) and for 1950 from ILO (1977a). We obtain the ratio of these benchmark values to the labor force participation rate derived from the (unadjusted) data on total employment and the unemployment rate as outlined above. In-between the benchmark years, we linearly interpolate the ratio. We use the resulting series to scale the (unadjusted) data on total employment and the unemployment rate as outlined above. | 1950 – 1960: As a benchmark value, we use the age-specific labor force participation rate from ILO (1977a) for 1950. We obtain the ratio of these benchmark values to the labor force participation rate derived from the (unadjusted) data on total employment and the unemployment rate as outlined above. For 1960, we assume a ratio of 1. In-between the benchmark years, we linearly interpolate the ratio. We use the resulting series to scale the (unadjusted) data on total employment and the unemployment rate as outlined above.

Average annual hours worked

Average annual hours worked per person employed. After 1950 until 2019, we rely on Feenstra/Inklaar/Timmer (2015) for all countries wherever data are available. Before 1950, data with an annual frequency are sparse. Thus, for the period before 1950, and if not stated explicitly otherwise, we use the benchmark estimates for average annual hours worked by Huberman/Minns (2007) and combine them with the variation from other sources, as similarly done by others, e.g., Bergeaud/Cette/Lecat (2016).

Australia 1900 – 1950: We use the variation of the average working week in manufacturing from Gilmore (2021). | 1950 – 2019: Feenstra/Inklaar/Timmer (2015)

Austria We obtain benchmark values by using information from Maddison (1982), scaled by the bias of the Maddison estimates compared to the Huberman/Minns (2007) estimates for Germany. 1900 – 1929: We use the values for 1900 and 1929 from Gilmore (2021) as a reference point and use the variation of German average annual hours worked as derived below in-between. | 1929 – 1950: We use the variation of the average working week in manufacturing from Gilmore (2021), with linear interpolation for 1938-1946. | 1950 – 2019: Feenstra/Inklaar/Timmer (2015)

Belgium 1900 – 1913: We use the variation of the average working week in manufacturing in France from Gilmore (2021). | 1913 – 1939: We use the variation of the average working week in manufacturing from Gilmore (2021) and linearly interpolate the year 1921. | 1939 – 1950: We use the variation of the average working week in manufacturing in France from Gilmore (2021). | 1950 – 2019: Feenstra/Inklaar/Timmer (2015)

Denmark 1875 – 1929: We use the variation resulting from the average annual changes in both the average working week in manufacturing from Gilmore (2021) as well as in the agreed annual working time from Abildgren (2017). | 1929 – 1950: We use the variation in the agreed annual working time from Abildgren (2017). | 1950 – 2019: Feenstra/Inklaar/Timmer (2015)

Finland 1900 – 1950: We use the relative changes in hours worked from Bergeaud/Cette/Lecat (2016), data sent on request. | 1950 – 2019: Feenstra/Inklaar/Timmer (2015)

France 1900 – 1919: We use the variation in the average working week in industry from Villa (1993), with linear interpolation for the years 1914 – 1918. | 1919 – 1939: We use the variation in the average working week from Villa (1993). | 1940 – 1945: We use the variation in the average working week in industry from Villa (1993). | 1946 – 1950: We use the variation in the average working week from Villa (1993). | 1950 – 2019: Feenstra/Inklaar/Timmer (2015)

Germany 1875 – 1950: We use the variation in the average weekly working hours in the industry from Schröder (1980), Meinert (1958), and BMAS (2012), data taken from Sensch (2015), with linear interpolation for the years 1915-1918, 1925, and 1945-1949. | 1950 – 2019: Feenstra/Inklaar/Timmer (2015)

Italy 1900 – 1913: We apply linear interpolation of benchmark estimates by Huberman/Minns (2007). | 1913 – 1920: We use the variation in average annual hours worked by Clark (1957). | 1921 – 1927: We use the variation in average weekly hours worked in manufacturing from Gilmore (2021), scaled by the interpolated ratio to Clarks (1957) figures in 1920 and 1928. | 1928 – 1950: We use the variation in average annual hours worked by Clark (1957). | 1950 – 2019: Feenstra/Inklaar/Timmer (2015)

Japan Here, we do not use benchmark estimates but rely on the relative changes of the following sources: 1900 – 1905: We assume the level of 1906. | 1906 – 1918: We use the relative changes in the hours series from Nihon Rodo Undo Shiryo Iinkai (1959), derived by multiplying days worked per year and hours worked per year, and scale by overlapping information with Clark (1957) in 1913. | 1918 – 1927: We use the relative changes in average annual hours worked by Clark (1957). | 1927 – 1950: We use the relative changes in the average weekly working hours in manufacturing from Gilmore (2021), with linear interpolation for 1946. | 1950 – 2019: Feenstra/Inklaar/Timmer (2015)

Netherlands 1875 – 1913: We use the variation in the average weekly working hours in manufacturing from Gilmore (2021), whereby we assume 1902-1903 to be constant. | 1913 – 1939: We use the variation in average annual working hours from Clark (1957). | 1939 – 1950: We use the variation in the average weekly working hours in manufacturing from Gilmore (2021). | 1950 – 2019: Feenstra/Inklaar/Timmer (2015)

New Zealand 1900 – 1970: We use the relative changes in hours worked from Bergeaud/Cette/Lecat (2016), data sent on request. | 1970 – 2019: Feenstra/Inklaar/Timmer (2015)

Norway Here, we do not use benchmark estimates but rely on the relative changes of the following sources: 1900 – 1937: We use the average variation in the series as outlined for Denmark above and for Sweden below; for 1908-1910 we only use relative changes from Denmark. | 1937 – 1950: We use the variation in the average weekly working hours in manufacturing from Gilmore (2021). | 1950 – 2019: Feenstra/Inklaar/Timmer (2015)

Sweden 1875 – 1938: We use the variation in average annual working hours from Clark (1957), except for 1900-1913, where we assume constant hours, following Bergeaud/Cette/Lecat (2016). | 1938 – 1950: We use the variation in the average weekly working hours in manufacturing from Gilmore (2021) up to 1952, with linear interpolation for the years 1947-1952. | 1950 – 2019: Feenstra/Inklaar/Timmer (2015)

Switzerland 1900 – 1950: We use the variation of the average working week in manufacturing from Gilmore (2021). | 1950 – 2019: Feenstra/Inklaar/Timmer (2015)

United Kingdom 1875 – 1950: We use the variation in the average weekly working hours of all in employment from Bank of England (2017). | 1950 – 2019: Feenstra/Inklaar/Timmer (2015)

United States 1875 – 1890: We linearly interpolate the benchmark estimates from Huberman/Minns (2007). | 1890 – 1919: We use the variation in the average weekly hours in manufacturing from HSUS (2023). | 1919 – 1950: We use the variation in the average weekly hours of production or nonsupervisory workers on private payrolls in manufacturing from HSUS (2023). | 1950 – 2019: Feenstra/Inklaar/Timmer (2015)

Other economic variables

Real GDP

Total real gross domestic product. We derive a series for real GDP by multiplying total population, derived as described above, with a given real GDP per capita value. From 1950 to 2019, we use GDP per capita values that result from dividing real GDP by total population from PWT 10.01 national accounts data (Feenstra/Inklaar/Timmer 2015). Before 1950, we use the relative changes in the real GDP per capita series by Bolt/van Zanden (2020). The only exception is Germany, where we use the relative changes in real GDP per capita from Bundesbank (2023) for the period 1950-1991.

Real wages

Real wage, hourly. Deriving a consistent dataset for real wages across space and time is a difficult task. We combine a multitude of sources to derive series for real wages that (1) approximate hourly real wages as closely as possible and (2) are consistent within a country. The latter implies that our series do not allow to conduct PPP analyses across countries. This shortcoming reflects the fact that PPP adjusted wages across space and time are a complex research field in its own (e.g. de Zwart/van Leeuwen/van Leeuwen-Li 2014). Below, we specify the frequency which the used wages indicators refer to. If this frequency is anything else than hourly, we divide accordingly by information delivered by our average annual hours worked (AVH) time series. For example, to adjust weekly wages, we divide them by (AVH/52), which approximates average weekly hours worked. We do not account for the changing nature of working days and weeks across space and time, which is beyond the scope of this paper (for a discussion, see Huberman/Minns 2007).

Australia 1900 – 1900: We use the relative changes in the weekly nominal wages from Jordà/Schularick/Taylor (2017) and deflate using the CPI series from the same source. | 1990 – 2019: We use the average annual real wages from OECD (2021).

Austria 1900 – 1912: We use the relative changes in the nominal wages in Vienna from Cvrcek (2013) and deflate by the CPI series from Hubmann/Jobst/Maier (2020). | 1912 – 1924: We assume the wage series to follow the relative changes in Germany. | 1924 – 1937: We obtain the sum of all wages as the share in nominal GDP from WIFO (1965) and divide by the total annual hours worked using employment and hour worked time series as outlined above and below. Then, we use the CPI series from Hubmann/Jobst/Maier (2020) to deflate the nominal values. | 1937 – 1948:

We use the ratio of German real wages between 1937 and 1948 and linearly interpolate in-between. | 1948 – 1950: We apply the same procedure as for 1924-1937. | 1950 – 1958: We use the relative changes in the nominal monthly wages from WIFO (1958) and deflate using the CPI series from the same source. | 1958 – 1990: We use the relative changes in the hourly nominal earnings in manufacturing from OECD (2019) and deflate using the CPI series from Hubmann/Jobst/Maier (2020). | 1990 – 2019: We use the average annual real wages from OECD (2021).

Belgium 1900 – 1938: We use the relative changes in the annual real wages by Williamson (1995). We linearly interpolate the missing years in 1915-1919. | 1938 – 1947: We use the relative changes in the hourly nominal wages from Jordà/Schularick/Taylor (2017) and deflate using the CPI series from the same source. We linearly interpolate the missing years from 1942 to 1946. | 1947 – 1959: We use the relative changes in the daily real wages from Williamson (1995). | 1959 – 1990: We use the relative changes in the weekly nominal wages from Jordà/Schularick/Taylor (2017) and deflate using the CPI series from the same source. | 1990 – 2019: We use the average annual real wages from OECD (2021).

Denmark 1875 – 1990: We use the relative changes in the hourly nominal wages from Jordà/Schularick/Taylor (2017) and deflate using the CPI series from the same source. | 1990 – 2019: We use the average annual real wages from OECD (2021).

Finland 1900 – 1913: We use the relative changes in the daily real wages in manufacturing from Heikkinen (1997). | 1913 – 1914: We use the relative changes in the nominal wages from Hjerppe (1989) and deflate using the CPI series from Jordà et al (2017). | 1914 – 1950: We use the relative changes in the nominal wages from Jordà/Schularick/Taylor (2017) and deflate using the CPI series from the same source, frequency unclear. | 1950 – 1990: We use the relative changes in the hourly nominal wages from Jordà/Schularick/Taylor (2017) and deflate using the CPI series from the same source. | 1990 – 2019: We use the average annual real wages from OECD (2021).

France 1900 – 1938: We use the relative changes in the nominal wages from Jordà/Schularick/Taylor (2017) and deflate using the CPI series from the same source; frequency unclear. | 1938 – 1939: We use the relative changes in the daily real wages from Williamson (1995). | 1939 – 1948: We use the relative changes in the hourly nominal wages from Jordà/Schularick/Taylor (2017) and deflate using the CPI series from the same source. | 1948 – 1990: We use the relative changes in the weekly nominal wages from Jordà/Schularick/Taylor (2017) and deflate using the CPI series from the same source. | 1990 – 2019: We use the average annual real wages from OECD (2021).

Germany 1875 – 1914: We use the relative changes in the annual nominal wages from Jordà/Schularick/Taylor (2017) and deflate using the CPI series from the same source. | 1914 – 1918: We use the relative changes in the real wages from Williamson (1995), frequency unclear. | 1918 – 1924: We use the relative changes in the weekly real wages from Pierenkemper (1984), data taken from Sensch (2012). | 1924 – 1991: We use the relative changes in the annual nominal wages from Jordà/Schularick/Taylor (2017) and deflate using the CPI series from the same source. | 1991 – 2019: We use the average annual real wages from OECD (2021).

Italy 1900 – 1914: We use the relative changes in the hourly nominal wages from Jordà/Schularick/Taylor (2017) and deflate using the CPI series from the same source. | 1914 – 1943: We use the relative changes in the daily nominal wages from Jordà/Schularick/Taylor

(2017) and deflate using the CPI series from the same source. | 1943 – 1950: We use the relative changes in the daily real wages from Williamson (1995). | 1950 – 1990: We use the relative changes in the weekly nominal wages from Jordà/Schularick/Taylor (2017) and deflate using the CPI series from the same source. | 1990 – 2019: We use the average annual real wages from OECD (2021).

Japan 1900 – 1945: We use the relative changes in the nominal daily wages from Jordà/Schularick/Taylor (2017) and deflate using the CPI series from the same source. | 1945 – 1947: We use the relative changes in the nominal hourly wages from Mitchell (2013) and deflate by the CPI series from Jordà/Schularick/Taylor (2017). | 1947 – 1950: Available data sources for this period indicate stark differences in the development of real wages, e.g. for 1949-1950 ranging from a 10 percent decline to a 36 percent increase (see van Leeuwen 2007, ILO 1952, among others). These differences appear to be attributed to using wages either in agriculture or in industry. To avoid using biased change rates for the period under consideration due to relying on one or another, we use changes in both agriculture and manufacturing weighted by the share of agriculture in overall employment. All relevant data – agricultural and manufacturing wages, the CPI, and the share in employment in 1950 – are taken from ILO (1952). | 1950 – 1957: We use the relative changes in the hourly nominal wages from Mitchell (2013) and deflate using the CPI series from Jordà/Schularick/Taylor (2017). | 1957 – 1990: We use the relative changes in the nominal weekly wages from Jordà/Schularick/Taylor (2017) and deflate using the CPI series from the same source. | 1990 – 2019: We use the average annual real wages from OECD (2021).

Netherlands 1875 – 1913: We use the relative changes in the nominal wages from Jordà/Schularick/Taylor (2017) and deflate using the CPI series from the same source, frequency unclear. | 1913 – 1948: We use the relative changes in the daily nominal wages from Jordà/Schularick/Taylor (2017) and deflate using the CPI series from the same source. | 1948 – 1990: We use the relative changes in the weekly nominal wages from Jordà/Schularick/Taylor (2017) and deflate using the CPI series from the same source. | 1990 – 2019: We use the average annual real wages from OECD (2021).

New Zealand 1900 – 1940: We use the relative changes in the real wages from Greasley/Oxley (2004). | 1940 – 1985: We use the relative changes in the real weekly earnings from Statistics New Zealand (2002). | 1985 – 1990: We use the relative changes in the hourly nominal wages from Mitchell (2013) and deflate using the CPI series from OECD (2020). | 1990 – 2019: We use the average annual real wages from OECD (2021).

Norway 1875 – 1990: We use the relative changes in the annual nominal wages from Jordà/Schularick/Taylor (2017) and deflate using the CPI series from the same source. | 1990 – 2019: We use the average annual real wages from OECD (2021).

Switzerland 1900 – 1990: We use the relative changes in the hourly nominal wages from Jordà/Schularick/Taylor (2017) and deflate using the CPI series from the same source. | 1990 – 2019: We use the average annual real wages from OECD (2021).

Sweden 1875 – 1949: We use the relative changes in the hourly earnings of males from Prado (2010) and deflate using the CPI series from Jordà/Schularick/Taylor (2017). | 1949 – 1990: We use the relative changes in the weekly nominal wages from Jordà/Schularick/Taylor (2017) and deflate using the CPI series from the same source. | 1990 – 2019: We use the average annual real wages from OECD (2021).

United Kingdom 1875 – 1990: We use the relative changes in the annual nominal earnings from Jordà/Schularick/Taylor (2017) and deflate using the CPI series from the same source. | 1990 – 2019: We use the average annual real wages from OECD (2021).

United States 1875 – 1890: We use the relative changes in the annual nominal wages from Jordà/Schularick/Taylor (2017) and deflate using the CPI series from the same source. | 1890 – 1990: We use the relative changes in the hourly nominal earnings from Jordà/Schularick/Taylor (2017) and deflate using the CPI series from the same source. | 1990 – 2019: We use average annual real wages from OECD (2021).

Investment

Total real investment. We collect data on real investment for the period 1960-2019 for all countries from IMF (2021). Before 1960, we use the relative changes from various sources. We use data on gross fixed capital formation, if available, and on gross capital formation if not. Eventually, we scale the series by using the ratio of real investment to real GDP from IMF (2021) in 2019, multiplied by the real GDP series as outlined above, as the benchmark value.

Australia 1900 – 1960: We use the relative changes in real total fixed investment from Butlin (1977). We linearly interpolate the data to 31st December. For 1900-1901, we assume the same change rate as for 1901-1902. | 1960 – 2019: IMF (2021)

Austria 1900 – 1913: Due to the absence of an investment series for that period, we use the relative changes in the real fixed capital stock from Schulze (2005). | 1913 – 1924: We use the relative changes in real gross fixed capital formation from Handler/Merth/Morwind (1968). | 1924 – 1937: We use the relative changes in real gross fixed capital formation from WIFO (1965). | 1937 – 1948: We use the relative changes in real gross fixed capital formation from Handler/Merth/Morwind (1968). | 1948 – 1960: We use the relative changes in real gross fixed capital formation from WIFO (1965). | 1960 – 2019: IMF (2021)

Belgium 1900 – 1960: We use the relative changes in nominal gross fixed capital formation from van Meerten (2003), deflated by the CPI series from Jordà/Schularick/Taylor (2017). | 1960 – 2019: IMF (2021)

Denmark 1900 – 1960: We use the relative changes in real gross capital formation from Abildgren (2017). | 1960 – 2019: IMF (2021)

Finland 1900 – 1960: We use the relative changes in nominal gross fixed capital formation from Hjerppe (1989), deflated by the GDP deflator from the same source. | 1960 – 2021: IMF (2021)

France 1900 – 1920: We use the relative changes in nominal gross capital formation from Villa (1993), deflated by the CPI series from Jordà/Schularick/Taylor (2017). | 1920 – 1935: We use the relative changes in nominal gross fixed capital formation, derived by multiplying the share of GFCF in GDP from van Meerten (2003) with nominal GDP from Villa (1993), deflated by the CPI series from Jordà/Schularick/Taylor (2017). | 1935 – 1960: We use the relative changes in real non-residential gross investment from Maddison (1994). | 1960 – 2019: IMF (2021)

Germany 1875 – 1913: We use the relative changes in nominal gross capital formation, derived by multiplying the share of GCF in GDP and nominal GDP from Jordà/Schularick/Taylor (2017), deflated by the CPI series from the same source. | 1913 – 1920: We assume real investment to follow real GDP. | 1920 – 1960: We use the relative changes in real non-residential gross investment from Maddison (1994). | 1960 – 1991: We use the relative changes in real gross fixed capital formation from Bundesbank (2023). | 1991 – 2019: IMF (2021)

Italy 1900 – 1960: We use the relative changes in real gross fixed capital formation from Baffigi (2011). | 1960 – 2019: IMF (2021)

Japan 1900 – 1960: We use the relative changes in real non-residential gross investment from Maddison (1994). | 1960 – 2019: IMF (2021)

Netherlands 1875 – 1913: We use the relative changes in real gross fixed capital formation from Smits/Horlings/van Zanden (2000). | 1913 – 1960: We use the relative changes in real gross fixed capital formation from Groote/Albers/de Jong (1996). | 1960 – 2019: IMF (2021)

New Zealand 1900 – 1914: We use the relative changes in nominal gross capital formation from Statistic New Zealand (2002), deflate by using the five-year moving average of the Australian CPI series from Jordà/Schularick/Taylor (2017), and chain-link to the subsequent source for New Zealand from BIS (2022). | 1914 – 1960: We use the relative changes in nominal gross capital formation from Statistics New Zealand (2002), deflated using the CPI series from BIS (2022). | 1960 – 2019: IMF (2021)

Norway 1875 – 1960: We use the relative changes in real gross capital formation from Grytten (2004), updated by Norges Bank (2018). | 1960 – 2019: IMF (2021)

Sweden 1875 – 1960: We use the relative changes in real gross fixed capital formation by Krantz/Schön (2007) and Schön/Krantz (2012). | 1960 – 2019: IMF (2021)

Switzerland 1900 – 1948: We use the relative changes in real gross capital formation from HSSO (2022f). | 1948 – 1960: We use the relative changes in nominal gross capital formation, derived by multiplying the share of GCF in GDP from Jordà/Schularick/Taylor (2017) and nominal GDP from BFS (2022), deflated by the CPI series from Jordà/Schularick/Taylor (2017). | 1960 – 2019: IMF (2021)

United Kingdom 1875 – 1960: We use the relative changes in real non-residential gross investment from Maddison (1994). | 1960 – 2019: IMF (2021)

United States 1875 – 1960: We use the relative changes in real non-residential gross investment from Maddison (1994). | 1960 – 2019: IMF (2021)

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Appendix B

Overview of observations excluded in the shock identification procedure

As outlined in the main text, we exclude a number of observations from the identification of the structural population shock. In the table below, we provide information on the country, period, and reason for each case. Additionally, we also indicate how many observations are effectively lost – i.e., have not already been excluded due to, e.g., being a war year.

Table 4: Details on excluded births observations

Country	Years	Reason	No. of obs. excl.
AUS	1900-1916	For this period, only estimated annual data from Gapminder (2015b) is available for outflows.	12
AUT	1954-1955	The inflows in this period are strongly driven by the unprecedented increase in births in Austria following the annexation ("Anschluss") by Germany in 1939. We categorize these observations as outliers.	2
AUT	2004-2005	The outflows in this period are strongly driven by the unprecedented increase in births in Austria following the annexation ("Anschluss") by Germany in 1939. We categorize these observations as outliers.	2
DEU	1875-1883	For this period, only estimated annual data from Gapminder (2015b) is available for outflows.	7
GBR	1875-1907	For this period, only estimated annual data from Gapminder (2015b) is available for outflows.	31
ITA	1900-1928	For this period, only estimated annual data from Gapminder (2015b) is available for outflows.	18
JPN	1900-1939	For this period, only estimated annual data from Gapminder (2015b) is available for outflows.	18
JPN	1959-1963	For this period, only estimated annual data from Gapminder (2015b) is available for inflows.	5
JPN	2009-2013	For this period, only estimated annual data from Gapminder (2015b) is available for outflows.	5
NZL	1900-1921	For this period, only estimated annual data from Gapminder (2015b) is available for outflows.	12
USA	1900-1921	For this period, only estimated annual data from Gapminder (2015b) is available for outflows.	37
			149

Source: Author's own calculations. For information on data sources, see Appendix A. © IAB

Appendix C

Additional impulse response functions

With our econometric strategy, we are able to compute impulse response functions to a structural population shock for each of the seven endogenous variables in our model. Moreover, we may obtain the impulse response functions to an additional series of relevant labor market indicators, as reported in Figure 7 and Figure 8 in section 5 of the main text, by linking the coefficients of the main results (Figure 5 and Figure 6) to each other. Below, a detailed outline of the calculation of each of these additional impulse response functions is given.

GDP per capita

Let GDP_t and POP_t be the estimated elasticities of a structural population shock on real GDP and on the working-age population at time t. Then, the elasticity for GDP per capita in the same period, $GDPpc_t$, is given by

$$GDPpc_t = GDP_t - POP_t \tag{A1}$$

Labor force participation rate

Let LF_t be the estimated elasticity of a structural population shock on the labor force at time t. Then, the elasticity for the labor force participation rate in the same period, $LFPR_t$, is given by

$$LFPR_t = LF_t - POP_t \tag{A2}$$

Employment rate (in the labor force)

Let EMP_t be the estimated elasticity of a structural population shock on employment at time t. Then, the elasticity for the employment rate in the labor force in the same period, ER_t, is given by

$$ER_t = EMP_t - LF_t \tag{A3}$$

Labor productivity

Let AVH_t be the estimated elasticity of a structural population shock on average hours worked at time t. Then, the elasticity for productivity in the same period, $PROD_t$, is given by

$$PROD_t = GDP_t - (EMP_t + AVH_t) \tag{A4}$$

Capital stock

Let $INVEST_0$ be the estimated elasticity of a structural population shock on investment in period 0 and let δ be the annual depreciation rate. Departing from a steady-state assumption, that is, in the absence of the structural population shock there would be no changes in the variables under consideration, then the ratio of annual investment to the capital stock is equal to δ. This assumption allows the derivation of the contemporaneous effect of a structural population shock on the capital stock, CAP_0, as

$$CAP_0 = INVEST_0 \cdot \delta \tag{A5}$$

Subsequently, from period t onwards, with in this case $t = 1, \ldots, H$ and H is equal to the chosen horizon length for computing the impulse response functions, the effect is given by

$$CAP_t = CAP_{t-1} - CAP_{t-1} \cdot \delta + INVEST_t \cdot \delta \tag{A6}$$

K-L ratio

Eventually, the elasticity for the capital-labor ratio at time t, KLR_t, is given by

$$KLR_t = CAP_t - (EMP_t + AVH_t) \tag{A7}$$

Appendix D

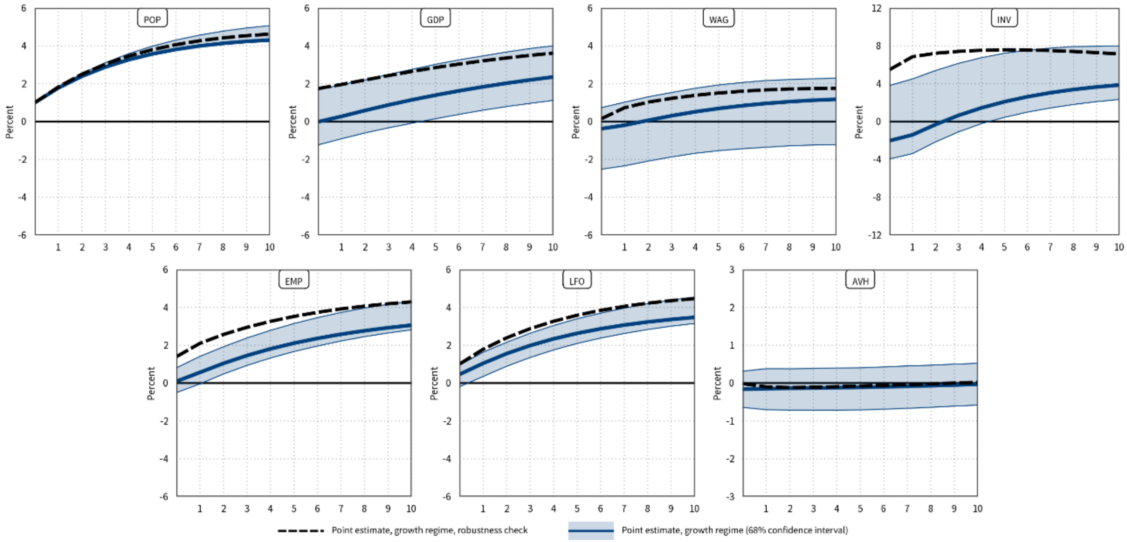
Identification of structural population shocks without instruments

The identification of structural population shocks is of key importance for our analysis. We do this in order to ensure that we estimate the causal effect of a population change and avoid biased results due to endogeneity. Possible sources of bias range from GDP and wages to employment, among others, all of which can be assumed to possibly exert effects on population, for example as push and pull factors driving migration dynamics.

Below, we illustrate the results of an identification without instruments. We follow the common identification strategy and impose a lower triangular matrix. In doing so, we assume that the population shock affects all variables in the model in the same period, but not vice versa. We implement the identification of regime-dependent shocks by regressing the residuals of each equation on the residuals of the population equation, weighted by the corresponding regime probabilities. Figure 10 and Figure 11 visualize the results of this specification. As illustrated, this identification strategy appears to capture an additional correlation between population and the other variables in the model. In particular, for GDP, employment, and investment we see far-stronger contemporaneous effects. As noted above, observing an additional strong correlation between population and these variables is consistent with typical push-pull frameworks. Particularly, given that reverse causality of economic variables on population is positive, the figures display exactly the bias one would have expected when ignoring simultaneity. Moreover, as an example, the supposedly strong effects of population on GDP cannot necessarily be reconciled with existing evidence (e.g., Headey/Hodge 2009).

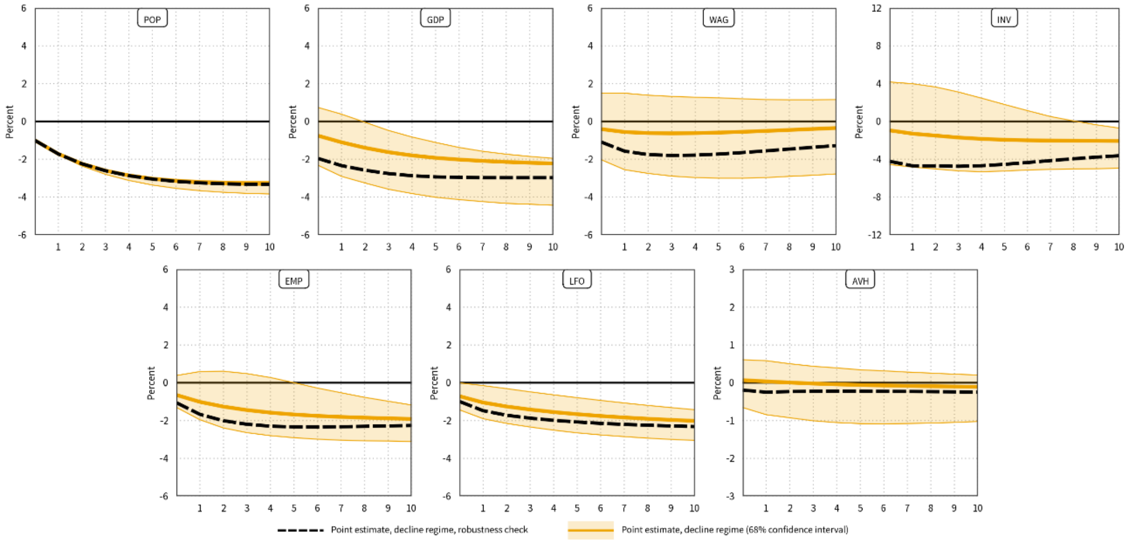
Thus, the results based solely on a lower triangular matrix in comparison to our baseline results underpin the need for an instrument-based identification strategy.

Figure 10: Robustness check for identification of contemporaneous effects, growth regime



Note: For abbreviations, see notes of Figure 5.
Source: Author's own calculations. © IAB

Figure 11: Robustness check for identification of contemporaneous effects, decline regime

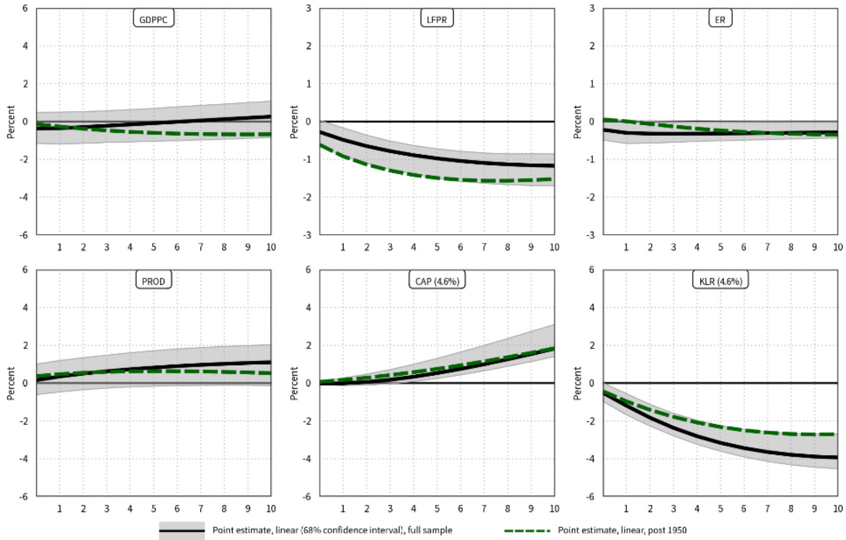


Note: For abbreviations, see notes of Figure 5.
Source: Author's own calculations. © IAB

Appendix E

Robustness: parameter constancy

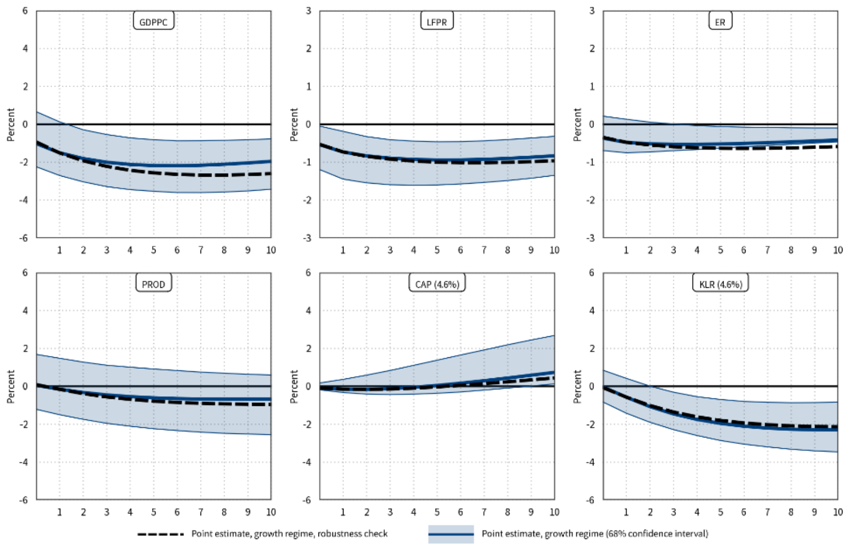
Figure 12: Robustness check for parameter constancy, post-1950 vs. full sample



Note: For abbreviations, see notes of Figure 7.
Source: Author’s own calculations. © IAB

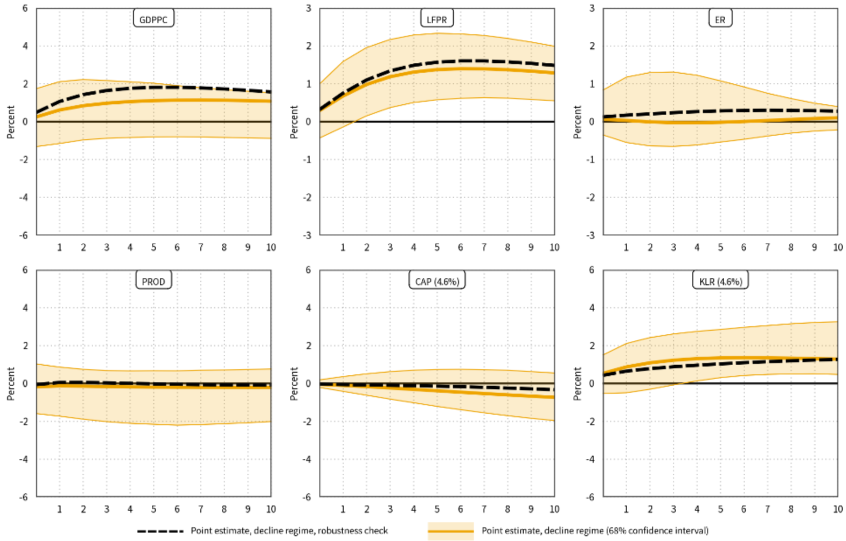
Robustness: cross-sectional dependence

Figure 13 :Robustness check for cross-sectional dependence, including world GDP, growth regime



Note: For abbreviations, see notes of Figure 7.
Source: Author’s own calculations. © IAB

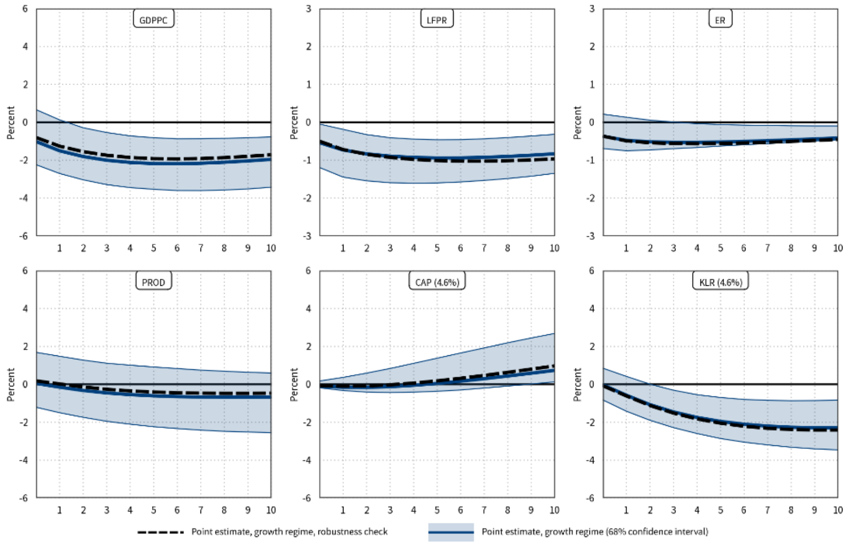
Figure 14: Robustness check for cross-sectional dependence, including world GDP, decline regime



Note: For abbreviations, see notes of Figure 7.
Source: Author's own calculations. © IAB

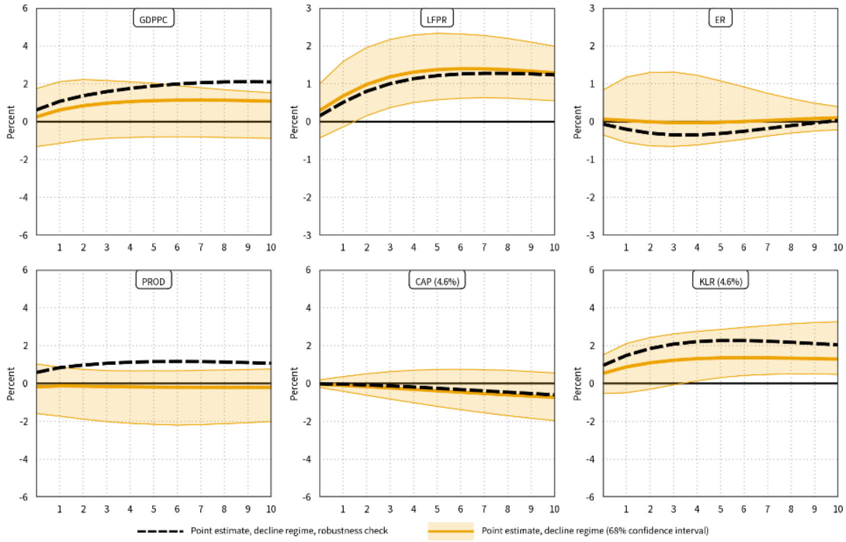
Robustness: specification of transition function

Figure 15: Robustness check for the specification of the transition function, growth regime



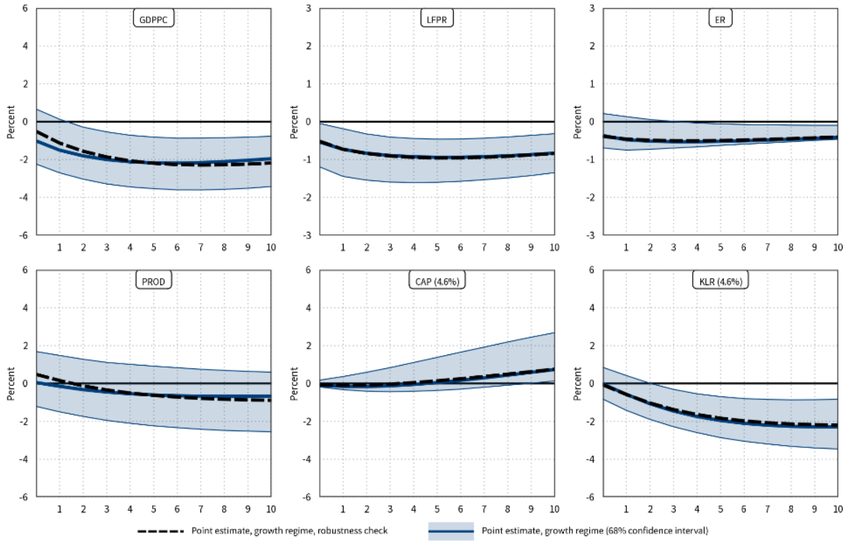
Note: For abbreviations, see notes of Figure 7.
Source: Author's own calculations. © IAB

Figure 16: Robustness check for the specification of the transition function, decline regime



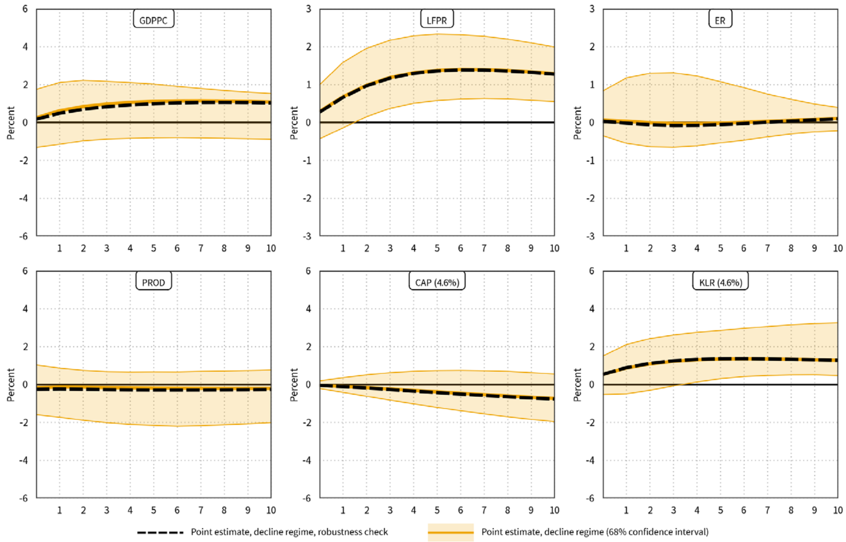
Note: For abbreviations, see notes of Figure 7.
Source: Author's own calculations. © IAB

Figure 17: Robustness check for the impact of the age structure, growth regime



Note: For abbreviations, see notes of Figure 7.
Source: Author's own calculations. © IAB

Figure 18: Robustness check for the impact of the age structure, decline regime



Note: For abbreviations, see notes of Figure 7.

Source: Author’s own calculations. © IAB

Figures

Tables

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