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# Globalization, Fertility, and Marital Behavior in a Lowest-Low Fertility Setting

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**ABSTRACT** Declines in marriage and fertility rates in many developed countries have fostered research debate and increasing policy attention. Using longitudinal data from the German Socio-Economic Panel, we analyze the effects of exposure to globalization on fertility and marital behavior in Germany, which was a lowest-low fertility setting until recently. We find that exposure to greater import competition from Eastern Europe led to worse labor market outcomes and lower fertility rates. In contrast, workers in industries that benefited from increased exports had better employment prospects and higher fertility. These effects are driven by low-educated individuals, married men, and full-time workers and reflect changes in the likelihood of having any child (the extensive margin). We find evidence of some fertility postponement and significant effects on completed fertility, but we see little evidence of a significant impact on marital behavior. Our results inform the public debate on fertility rates in settings with lowest-low fertility, such as Germany, during the period under investigation.

**KEYWORDS** Globalization • Labor market outcomes • Fertility • Marriage

## Introduction

Over recent decades, major concerns about growing inequality in employment opportunities and earnings have arisen in advanced economies (Autor 2014). Many studies have documented how trade with China and other emerging economies has contributed to the rise in earnings inequality in the Western world (for a review, see Autor et al. 2016). Declines in marriage and fertility rates in many developed countries have also fostered research debate and increasing policy attention (Kohler et al. 2002; Stevenson and Wolfers 2007). Several studies have analyzed the possible relationship between labor demand shocks and fertility choices. Wilson (1996) and Wilson and Neckerman (1986) highlighted the role of job losses and, in particular, the secular decline of manufacturing in reshaping family structure. More recently, Autor et al. (2019) documented how the negative impacts of U.S. labor market shocks induced by increasing import competition from China affected men's marriage-market value and thus marriage and fertility rates. In a different setting, Keller and Utar (forthcoming)

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found that marriage and fertility rates increased among Danish female workers who were more exposed to fierce Chinese competition.

Despite this recent work, we still know relatively little about such relationships in other developed countries exposed to different population dynamics and different trade-induced labor market shifts. The main goal of this article is to study how labor market shocks driven by trade with Eastern Europe and China affected fertility and marital behaviors in Germany, a lowest-low fertility setting until recently (Anderson and Kohler 2015; Billari and Kohler 2004; Haub 2012; Kohler et al. 2002).

Germany provides an interesting case study. Its trade flows with Eastern Europe and, to a lesser extent, China increased dramatically in the 2000s, and previous research has shown that the effects on labor market outcomes in Germany differ from those observed in the United States (Dauth et al. 2014, 2017). Furthermore, over the period we consider (1991–2018), Germany had one of the lowest total fertility rates in Europe, dipping as low as 1.2 before stabilizing at approximately 1.35 by the late 2000s (Haub 2012).

In this study, we investigate how labor demand shocks stemming from rising exposure to trade competition influence family choices in Germany. We use individual-level longitudinal data from the German Socio-Economic Panel (SOEP) to investigate the labor market dynamics underlying the relationship between trade integration and family choices. We focus on the variation in exposure to trade between Germany and Eastern Europe, which is quantitatively more important than trade with China (Dauth et al. 2014). To identify trade effects, we draw on previous research that used trade flows with other high-income countries as instruments for the trade flows to the United States (Autor et al. 2019) and Germany (Dauth et al. 2014, 2017). Unlike Autor et al. (2019), we exploit individual-level variation in our longitudinal data from Germany. We perform a short-panel analysis capturing the effects of year-to-year changes in exposure to trade on fertility and marital outcomes. Keller and Utar (forthcoming) also used a yearly analysis in exploiting a quasi-experimental design based on the effects on Denmark of the textile trade liberalization, in which China benefited from the removal of textile quotas by entering the World Trade Organization (WTO). Their economy-wide empirical analysis used a long-difference (2009–1999) specification with individual-level data to explore variation in exposure to Chinese imports. Whereas only the import shock was relevant in their Denmark–China study, our economy-wide yearly analysis identifies both import and export shocks between Germany and Eastern Europe.

Consistent with previous evidence for Germany (Dauth et al. 2014; Huber and Winkler 2019), we find that both import and export shocks have significant effects on labor market outcomes and that these effects operate in opposite directions. Greater import competition lowers wages, hours worked, and the likelihood of employment. By contrast, greater export opportunities yield positive effects on labor market outcomes, which more than offset the negative effects of import competition. The labor market impacts are driven mostly by the rising trade relationship between Germany and Eastern Europe. In line with previous studies, we also find that workers in Germany were less affected by trade with China.

The import and export effects on labor market outcomes are concentrated among low-educated individuals and are driven by full-time employees. This evidence accords well with theoretical frameworks predicting that different types of low-skill

labor cannot easily move across industries and hence are affected by industry-specific import competition and rising export opportunities. In the analysis by gender, we find that the labor market effects are also concentrated among men; the effects on women are smaller and less precisely estimated. These patterns align with the evidence for the United States highlighting negative gender-specific employment effects of import shocks (Autor et al. 2019).

Our findings point to significant effects of trade exposure on fertility behavior. Consistent with our evidence regarding labor market outcomes, the impact varies with exposure to import competition or export opportunities and with education level. Although we find little or no evidence of effects on marital behavior (i.e., marriage, divorce, and cohabitation), the average change in imports from Eastern Europe throughout the study period (1991–2018) decreased fertility by 1.6 percentage points. The effects are concentrated among low-educated individuals and men and are driven by changes in marital fertility; we find no evidence of significant effects on nonmarital fertility. Exposure to imports led to a 1.5-percentage-point reduction in the probability of having a child (i.e., the extensive margin) but had no significant impact on the probability of having more than one child (i.e., the intensive margin). Although we find some evidence of fertility postponement, exposure to import competition significantly and negatively affected completed fertility. These results are consistent with recent evidence for the relationship between financial uncertainty and fertility decisions (Comolli 2017; Örsal and Goldstein 2010). The negative fertility effects are partly offset by exposure to greater exports to Eastern Europe. Our estimates reveal that the average change in exposure to exports during the study period led to a 1.1-percentage-point increase in the likelihood of having a child, although the effect is precisely estimated only when we focus on low-educated individuals. The beneficial effects of exports on fertility are again concentrated among low-educated individuals and driven by married individuals. Similar to our findings for imports, the results for export exposure reflect the increased likelihood of having any child (i.e., the extensive margin). Increased export exposure led to a significant rise in completed fertility, offsetting the adverse impact of import competition. Compared with situations in which only import shocks matter (Autor et al. 2019**;** Keller and Utar forthcoming), our results from Germany suggest a more nuanced role of trade-induced labor market shifts in family choices.

Our study speaks to a growing literature on the impact of labor demand shocks on life course choices (Ananat et al. 2013; Anelli et al. forthcoming; Autor et al. 2019; Black et al. 2013; Currie and Schwandt 2014; Kearney and Wilson 2018; Keller and Utar forthcoming; Lindo 2010; Schaller 2016). In particular, our work is closely related to two studies on the labor market effects of exposure to trade using German data. First, Dauth et al. (2014) found that the unprecedented rise in trade between Germany and the East (Eastern Europe and China) between 1988 and 2008 caused substantial job losses in import-competing industries, whereas regions specializing in export-oriented industries had even stronger employment gains. The authors found that most of these effects were driven by trade with Eastern European countries. Moreover, using individual-level data, they showed that overall, trade had a stabilizing effect on employment. Second, Huber and Winkler (2019) found that partners' risk sharing substantially reduced the inequality-increasing effect of international trade. Our identification strategy is closely related to that adopted in these previous studies.

However, to the best of our knowledge, the study by Keller and Utar (forthcoming) was the only one that employed individual-level longitudinal data to analyze how trade liberalization affected fertility and family choices. Using microdata on Danish firms and workers, they found that worse labor market opportunities owing to Chinese import competition led to higher parental leave-taking, higher fertility, more marriages, and fewer divorces. This pro-family shift was driven by women in their late 30s; the authors attributed this finding to the role of the "biological clock"—that is, women's desire to have children before the end of their childbearing years.[1]

Our analysis and results complement these recent studies by providing evidence from a low-fertility setting with labor market effects of trade shocks that differ from those observed in the United States and other advanced economies (Autor et al. 2019; Dauth et al. 2014). Overall, we find that globalization had a small negative effect on fertility because the negative impact of import competition more than offset the positive contribution of greater export opportunities.

Our results on the import effects differ from those obtained by Keller and Utar (forthcoming) for Denmark. Those authors found that greater import competition led to higher fertility. Differences in empirical strategy (i.e., they used a long-difference specification) might explain this divergence. Differences in family-oriented policies, parental leaves, and subsidies for childcare between Germany and Denmark during the period under investigation (Apps and Rees 2004; Seeleib-Kaiser and Toivonen 2011; Ziefle and Gangl 2014) might also explain the different results.

In addition, we find no evidence of significant effects on marriage, while there is some evidence that imports led to a decline in divorce among women. The lack of significant effects on marriage contrasts with the Autor et al. (2019) finding of negative effects of trade exposure on marriage rates but is consistent with findings of Kearney and Wilson (2018). We also find a decreasing, albeit nonsignificant, effect of import competition on cohabitation. The difference in our results compared with previous studies is likely explained by prevalent social norms in a context like Germany, which is characterized by relatively low marriage rates (Adler 1997).

## Theoretical Framework

Labor demand shifts, such as those due to exposure to trade shocks, likely influence fertility choices through changes in income and in the opportunity costs of having children. Neoclassical models of fertility suggest that because children are not easily substitutable, changes in income or economic opportunities will mostly result in income effects on fertility decisions. These models predict that as family income

[1] Our work relates to two other studies that investigated the relationship between trade and fertility choices at a more aggregate level. Bignon and García-Peñalosa (2018) found that fertility increased in French regions that were more exposed to protectionism in the agricultural sector during the nineteenth century. The mechanism behind their result differs from ours and is based on the quantity–quality trade-off (Galor and Weil 2000): trade protection in the agricultural sector weakened incentives to invest in education (quality), thereby leading couples to have more children (quantity). Do et al. (2016) showed that countries with a comparative advantage in female-intensive sectors (and hence a higher female-to-male wage ratio, which raises the opportunity costs of having children) exhibit lower fertility rates.

rises, fertility will increase (see Becker 1960; and for a recent review, see Doepke 2015). However, the trade-off in parents' preferences regarding the quantity and quality of children (as proxied by investments in each child at a given price) may weaken the relationship between income and fertility. Furthermore, previous studies demonstrated that improved economic opportunities may have different effects by the parent's gender. Given the monetary and time costs associated with fertility, labor market improvements may also lead to a fertility decline as the opportunity cost of having children increases. Women may be more responsive than men to changes in these opportunity costs because of the traditional division of chores within the household. As this brief discussion suggests, labor demand shocks may have a negative or a positive impact on fertility, depending on the strength of income and substitution effects.

The impact of greater exposure to international trade on fertility via labor demand is thus uncertain and ultimately an empirical question. Furthermore, the fertility elasticity with respect to demand shocks may be very different in low-fertility settings (Kohler et al. 2002). In fact, Billari and Kohler (2004) found that the emergence of lowest-low fertility during the 1990s significantly changed the relationship between traditional determinants of fertility and fertility outcomes.

Similarly, the effect of trade integration on marriage decisions is unclear. Worse economic opportunities for men may lower their value in the marriage market, thereby having a negative impact on marriage rates (Anelli et al. forthcoming; Autor et al. 2019). At the same time, decreased opportunities for women could result in lower opportunity costs of family life and induce women in more traditional societies to specialize in household activities. However, recent research suggests that in more modern societies, family formation may be less sensitive to economic conditions because the share of women specializing in domestic activities is decreasing and is more responsive to social norms (Kearney and Wilson 2018). Again, the effects of trade on marriage may be very different in a country such as Germany, where the importance of marriage has been declining over the last decades, particularly in East Germany (Klärner 2015).

The effects of trade-induced labor market shocks on fertility choices and marital status could differ substantially by gender and education. We expect that men will be more affected than women because they are employed in more tradable sectors. Consistent with this hypothesis, research showed that men suffered larger negative consequences of labor demand shocks (rising import competition and automation) than women (Anelli et al. forthcoming; Autor et al. 2019; Kearney and Wilson 2018). These changes in men's and women's relative market opportunities may have implications for fertility decisions (Ananat et al. 2013; Kearney and Wilson 2018; Schaller 2016; Shenhav 2021). For instance, consistent with neoclassical economic theory, Schaller (2016) found that improvements in men's labor market conditions are associated with increases in fertility, whereas improvements in women's labor market conditions have a fertility-decreasing impact (see also Autor et al. 2019; Gaddis and Pieters 2017). Further, neoclassical marriage models predict that as earning differences between men and women decline, marriage rates may decline (Becker and Lewis 1973; Bertrand et al. 2016; Bertrand et al. 2015). In their study on Denmark, Keller and Utar (forthcoming) found that increased exposure to competition from Chinese products led to a deterioration in women's labor market outcomes (relative to men's) and increased marriage and fertility rates, thus corroborating the neoclassical marriage model (Becker and Lewis

1973). Previous studies have suggested that the labor market consequences of trade exposure should be greater for men (e.g., Autor et al. 2019). Assuming that children are normal goods, fertility should fluctuate with income effects. Gender-specific shocks may also affect the likelihood of being in a stable relationship (marriage or cohabitation). The decline in men's relative economic stature may lead to a reduction in marriage (Anelli et al. forthcoming; Autor et al. 2019). However, marriage patterns are substantially shaped by context and social norms (Bertrand et al. 2016; Kearney and Wilson 2018). Furthermore, recent research on worker-level trade adjustment highlighted the role of gender differences in the market versus family choice in determining adjustment costs. Keller and Utar (forthcoming) found that labor demand shocks may result in significant long-run gender inequality, with children penalizing women more and with differential effects along the skill distribution.

The labor market effects of exposure to international trade are likely to vary with workers' skill levels, as suggested by both factor proportions (Heckscher-Ohlin and specific factors) and firm-level theories of trade (Adão et al. 2020; Helpman 2017; Kim and Vogel 2021; Wood 2018). If low-skilled workers are more specific to a particular industry than high-skilled workers (e.g., because of less general knowledge or human capital that could be used in different industries), they should be more affected by industry-specific trade shocks. We thus expect significant heterogeneity by skill level (as measured by educational attainment) in the impacts of trade on demographic outcomes through the labor market, with low-skilled workers being more affected by trade shocks than high-skilled workers.

## Empirical Strategy

Previous studies analyzing the impact of trade largely relied on measures of geographical exposure to trade, instrumenting imports and exports exposure with a Bartik instrument that uses the initial industry distribution of employment across regions to build a measure of regional exposure to import and export shocks. Our online appendix presents results obtained using the regional exposure to imports and exports. However, our baseline specification exploits the longitudinal nature of the data and relies on the variation in industry exposure to trade over time (see also Huber and Winkler 2019). We use a short-run panel approach relating year-to-year changes in trade exposure to changes in labor market, fertility, and marital outcomes. The analysis is based on an unbalanced panel of workers observed from 1991 to 2018. Following Dauth et al. (2014), we compute trade exposure as the degree to which a two-digit industry is directly exposed to import competition and export expansions.[2] Because both export and the import dimensions have been shown to be relevant to the German setting (Dauth et al. 2014), we estimate the effects of import and export exposures on fertility and other family life choices. We assign to each individual the exposure to trade associated with the worker's initial industry observed in our sample. To mitigate concerns that the rising importance of trade with Eastern Europe and

[2] Computing trade exposure on the basis of a two-digit industry classification is a limitation of our data, particularly compared with other studies that could rely on employee–employer data sets and more granular industry classifications. Unfortunately, the SOEP data do not include narrower industry definitions.

China in the 1990s and most notably in the early 2000s (as Figure 1 later shows) may have affected the self-selection of workers into their initial industry, we restrict the sample to individuals who entered the labor market before 2000.

Similar to Huber and Winkler (2019) and Dauth et al. (2014), we normalize the trade flows by the industry wage bill in the first year the individual entered the sample to control for size differences between industries.[3] We fix the wage variable at the base year to rule out composition effects (i.e., changes in the relative labor demand at the industry level), which could be influenced by trade exposure. We estimate the following equation:

$$Y_{ijst} = \beta_1 IM_{jt-1} + \beta_2 EX_{jt-1} + \alpha X_{ijst} + \gamma_i + \delta_{st} + \theta_k + \lambda_{ot} + \epsilon_i, \quad (1)$$

where $Y_{ijst}$ denotes the outcome of interest—labor market outcomes (earnings, hours worked, employment, and labor force participation) and family choices (fertility behavior, marriage, divorce, and cohabitation)—for individual $i$, who had a first job in a NACE two-digit industry $j$, resided in federal state $s$ when entering the SOEP panel, and was interviewed in year $t$.

Our two main explanatory variables, $IM_{jt-1}$ and $EX_{jt-1}$, measure trade exposure to the East (i.e., Eastern Europe and, in additional estimations, China) at the industry level. They equal the value of imports ($IM$) and exports ($EX$) normalized by the total wage bill in the industry in the first year the individual entered the sample. The trade variables are lagged by one year. For both imports and exports, we sum the value of direct trade flows (i.e., those in the [manufacturing] industry $j$) with that of indirect trade flows through input–output linkages to downstream buyers and upstream sellers.[4] Adding indirect exposure through input–output linkages allows us to include individuals initially employed in service industries, whose exposure is only indirect through their sales to and purchases from manufacturing industries, because we do not have data on service trade flows for our sample period.

The coefficients of interest are $\beta_1$ and $\beta_2$, which capture the effects of import and export exposures, respectively. We focus on Eastern Europe and China as key trading partners because the rapid increase in trade with those countries (especially with Eastern Europe) has led to important changes in the German labor demand over the past decades (Autor et al. 2019; Dauth et al. 2014; Huber and Winkler 2019), which might have implications for family choices.[5]

We also estimate Eq. (1) by education level (college degree or higher vs. high school diploma or less) and by gender because these two dimensions of heterogeneity are possible theoretical mechanisms. Given that many women in Germany are part-time workers, we investigate the sensitivity of the results to the exclusion of part-time workers. Furthermore, we assess whether trade had any effect on the likelihood of working part-time.

[3] Other studies normalized trade flows by domestic absorption (e.g., Keller and Utar forthcoming).

[4] Each type of indirect exposure (downstream and upstream in the supply chain) is a weighted sum of trade flows in all other (manufacturing) industries, with weights equal to the share of industry $j$'s output used as inputs in a purchasing industry (downstream exposure) and of industry $j$'s input bought from a selling industry (upstream exposure) (Acemoglu et al. 2016).

[5] Unlike Huber and Winkler (2019), we do not consider partner's exposure to trade.

The term $X_{ijst}$ collects a set of control variables: age and age squared; household size; and in the regressions on the full sample, dummy variables for the individual's education.[6] All our estimates include individual fixed effects in the $\gamma_i$ term, which absorbs the influence of any unobserved time-invariant individual heterogeneity. The individual fixed effects net out important confounding factors that could bias our estimates. For example, individuals might sort themselves into industries with different levels of trade exposure based on predetermined characteristics, which can simultaneously affect their family choices. Individual fixed effects account for this selection bias. In addition, our choice to assign the trade exposure of the initial employment industry and to keep only individuals entering the sample before 2000—before trade flows between Germany and the East really took off—further mitigates the selection concerns regarding the movement of workers across industries in response to trade exposure. Because of these fixed effects, the identifying variation for our coefficients of interest comes from changes in import and export exposures within industries and over time.

Our specification also includes federal state × year fixed effects (initial state of residence in the panel, to rule out the influence of between-state migration endogenous to trade exposure), $\delta_{st}$. The inclusion of these effects controls for all possible state-level time-varying factors, thereby accounting for the possibility that regions that specialized in different industries experienced different time-varying shocks. One natural concern is that technological progress during 1991–2018 might be correlated with our measures of trade exposure. This concern may be particularly relevant to our case because women in Germany are overrepresented in nonmanual routine jobs (Gundert and Mayer 2012). To alleviate concerns of bias from technological progress, we add two sets of fixed effects to our specification. First, we include one-digit industry fixed effects, $\theta_k$, thereby exploiting only variation in trade exposure among individuals working in the same one-digit industry. These fixed effects absorb the influence of any changes occurring in technologies across one-digit industries during the period. Second, all our estimates control for one-digit ISCO occupation × year fixed effects, $\lambda_{ot}$, which account for initial occupation-specific shocks over time; here, we use occupation in the first year the individual was observed to net out possible bias from selection into the occupation that is endogenous to trade exposure. This set of fixed effects controls for the impact of technology on workers employed in occupations with different task content (e.g., how routine the tasks are and hence how susceptible they are to automation/computerization). Finally, $\epsilon_{ijst}$ represents an idiosyncratic error term. Throughout the analysis, we cluster standard errors at the two-digit industry level. We use a linear estimator for all regressions, even if the outcome variable is binary in most models. This choice accommodates the large dimensionality of the fixed effects used in the specifications.

Industry-level time-varying demand and productivity shocks may be correlated with both trade exposure and individual outcomes. Thus, even though our specification accounts for time-invariant unobserved heterogeneity through the individual fixed effects, our model may still suffer from endogeneity bias. To alleviate this concern, we adopt an IV approach (Autor et al. 2013, 2014), closely following Dauth

[6] Excluding the education dummy variables from Eq. (1) does not significantly alter the results. Approximately 14% of individuals changed their education during the study period.

et al. (2014) and Huber and Winkler (2019) in adapting the IV strategy to the German context. We instrument trade flows with Germany using trade with a group of other developed countries.

For import exposure, the objective is to isolate supply-driven changes in exports from China and Eastern Europe. The instrument is thus the direct and indirect exports of China and Eastern Europe to the other countries, normalized by the industry wage bill in the base period.[7] Therefore, the underlying identification assumption requires that demand shocks in the other developed countries are largely uncorrelated with demand shocks in Germany: the $\beta_1$ coefficient would rely on variation in the supply-side component of exports from the East. For exposure, we aim to net out the German supply part from the total variation in German exports to the East. Exports from the other developed countries to China and Eastern Europe are thus a valid instrument under the assumption that supply influences in those origin countries are orthogonal to the German supply.

The IV strategy hinges on the assumption that the variation in trade with Eastern Europe and China picked up by the instrument is uncorrelated with German demand and supply shocks. Productivity changes in China and Eastern Europe are the main candidates to explain the instrument's validity. Autor et al. (2016) explained that the rise of China in the world economy resulted from productivity increases (e.g., related to internal reforms and migration) and lower trade costs (China's WTO entry). Similarly, Dauth et al. (2014) argued that the sudden, unexpected fall of the Iron Curtain exposed Germany to the transformation of former socialist countries in Eastern Europe into market economies. The transformation of the former socialist bloc triggered substantial productivity gains in those economies (Burda and Severgnini 2009). These countries also faced lower trade costs starting from the mid-1990s, following their 1995 entry into the WTO. Therefore, increasing German export and import volumes with Eastern Europe stemmed largely from the strongly rising productivity and accessibility of those trading partners rather than from changes in the German economy.

Following Dauth et al. (2014), we capture these productivity and trade cost shocks by including the trade between Eastern European countries and developed countries with an income level similar to Germany's, but we exclude all direct neighbors and members of the European Monetary Union. Thus, we exclude countries that likely experienced shocks similar to Germany's (e.g., France, Austria), which would challenge our identification strategy. Our final instrument group consists of Australia, Canada, Japan, New Zealand, Norway, Singapore, Sweden, and the United Kingdom. We show that the trade flows of those countries with the East represent a relevant instrument for German trade exposure, with the first-stage *F* statistic for all our estimates being well above the conventional levels.

## Data

We employ data from two main sources: the German Socio-Economic Panel and the United Nations Commodity Trade Statistics Database (Comtrade).

[7] In computing indirect trade flows, we always use the national input–output matrix for Germany.

### SOEP Data

The SOEP is a representative longitudinal data set surveying households and individuals in Germany since 1984. The data set contains several subsamples and is constructed to ensure the representativeness of the entire population in Germany. For a detailed description of the survey, see Wagner et al. (2007) and Goebel et al. (2019). A unique feature of this data source lies in its wide range of individual- and household-level information.

Of particular importance for our study is that the SOEP data contain information on household structure, marital status, and fertility histories. We use this information to create our main demographic outcomes of interest: a dummy variable for having a child in a given year and indicators for the individual's marital status (married, divorced, or cohabiting) at time *t*. Furthermore, our data set contains information on individuals' labor market outcomes, permitting us to investigate the potential mechanisms through which trade exposure affects fertility and marital behavior. We use four main labor market indicators: annual labor earnings, hours worked, employment, and labor force participation. The first two indicators are measured in the year before the survey, whereas the latter two are measured at the time of the survey.[8] The SOEP provides information on the industry in which the worker is employed based on the NACE two-digit classification. Our sample contains data for 56 industries. For each individual, we consider the first industry of employment observed in the SOEP data and keep it fixed over time.

To construct our working sample, we consider the survey years 1991–2018, after Germany's reunification and up to the latest available year of data. We keep only individuals aged 20–44 when the outcomes were measured because this is the age interval most relevant to women's fertility. Given our focus on labor market channels and following Huber and Winkler (2019), we apply additional data restrictions. We drop individuals who were self-employed, retired, civil servants, or students at the time of the survey. Finally, as mentioned earlier, we restrict the sample to individuals who entered the SOEP and had nonmissing information on their occupation before 2000.

After these restrictions, we obtain a final longitudinal sample of approximately 55,000 person-year observations with nonmissing occupation information resulting from roughly 6,500 individuals; the sample size varies depending on the outcome variable used in the regression model.[9] Table A.1 displays the descriptive statistics for the main variables used in the regressions (this table and all other tables and figures designated with an "A" are available in the online appendix). Approximately 4% of individuals report a birth in a given year (5% with a marital birth and 2% with a nonmarital birth). The percentages married, divorced, and cohabiting are approximately 62%, 6%, and 22%, respectively. Roughly 25% of individuals have a college degree,

[8] We consider employment status rather than transitions to and from employment (e.g., identifying transitions into employment through an indicator for the first year of an employment spell) to identify how exposure to trade might also affect the persistency of employment.

[9] The sample is larger when we analyze employment transitions (approximately 70,000 observations resulting from 7,000 individuals) because we include individuals with missing information on their current occupation.

which identifies the high-skilled workers in our heterogeneity analysis by education. On average, workers report a wage of approximately 23,000 euros in a given year and work close to 38 hours per week.

### Trade and Other Data

Data on international trade flows are drawn from the Comtrade database and cover the 1990–2018 period. These data include detailed information on commodity trade from more than 170 countries. Using the correspondence between the Standard International Trade Classification (revision 3) product codes and NACE codes provided by the United Nations Statistics Division, we harmonize industry and product classification to match these data with the NACE two-digit industry information available in the SOEP data. We convert trade flows for nonservice industries to current euros and combine them with the German input–output table for 1995 to compute indirect trade flows for each industry (discussed in detail later).[10]

Consistent with the literature on the labor market consequences of trade in Germany (e.g., Dauth et al. 2014, 2017), we consider two sets of trading partners: Eastern Europe and China. Figure 1 plots the evolution of total German exports to and imports from these two groups of countries over the entire period under investigation. Two important facts emerge. First, Germany's trade with these partners has increased substantially starting from the early 2000s. To minimize the risk of trade driving selection into industry, we thus consider only individuals who entered the sample before 2000 and keep their industry of employment in the first year fixed over time. Second, imports and exports with Eastern Europe have been consistently more important than those with China. As Dauth et al. (2014) demonstrated, this difference in quantitative importance is also reflected in its labor market effects: trade with Eastern Europe has had a more robust impact than trade with China. We thus focus on imports to and exports from Eastern Europe in our baseline regression analysis and briefly discuss the less important effects of trade with China. In the export and import variables used in Eq. (1), industry-level trade flows are normalized by the industry wage bill in the base period (i.e., the first year the individual enters the sample). Data on the total compensation of employees by industry are sourced from Eurostat.[11,12]

[10] We source the national input–output table for Germany from the World Input-Output Table database (https://www.rug.nl/ggdc/valuechain/wiod/?lang=en). We use the earliest available year of data, which is 1995.

[11] The industry classification used in the wage bill data is in NACE revision 2. We convert it to NACE revision 1 (the classification used in SOEP and in Comtrade) and allocate NACE revision 2 industries that span multiple NACE revision 1 industries using trade shares. The data on total compensation of employees by industry are available starting from 1995. We thus give the wage bill of that year to the first-year industry of individuals who entered before 1995.

[12] To make sure that our results are not influenced by outliers, we drop the top 1% of the trade regressors. These are implausibly large values that occur mainly in the last year of the sample (i.e., 2018). Adding these outliers scales down the point estimates without altering their statistical significance and the associated magnitudes.

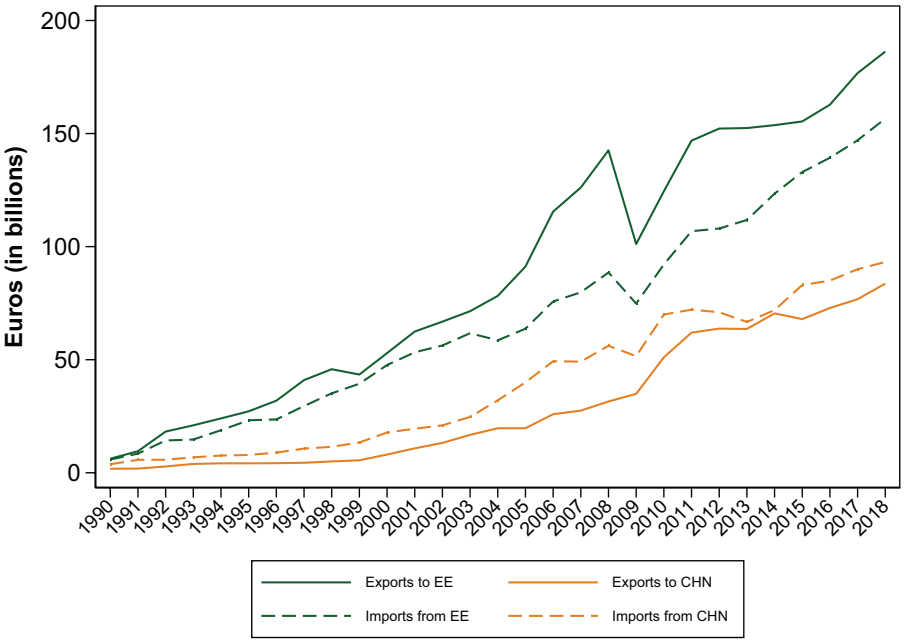


**Fig. 1** Trade between Germany and Eastern Europe (EE) and China (CHN). Trade values are in billions of current euros. The trade variables equal the sum of the direct and indirect (through input–output linkages) components.

To identify the most dynamic industries in terms of trade patterns, Figures A.1 and A.2 display, respectively, the average yearly percentage change in the import and export variables (*IM* and *EX* in Eq. (1), which are normalized by the wage bill) with Eastern Europe. This yearly variation within industries closely relates to the variation that we exploit to identify our estimates of interest in Eq. (1). Two main observations are worth noting. First, trade between Germany and Eastern Europe increased in all industries during the study period, with some variation in the speed of the yearly increases across industries. Second, manufacturing industries are among the most exposed to imports and exports. The correlation between the export and import variables is strong, at .57 (Spearman rank correlation coefficient), suggesting that the effects of the two variables can be distinguished empirically but that it is also important to consider the overall impact of exposure to imports and exports.

## Results

### Effects of Trade Exposure on Labor Market Behavior

We first reexamine the impact of trade exposure on the labor market outcomes of German workers in our sample period (1991–2018). Huber and Winkler (2019) performed a similar analysis for the years 1993–2008. As described earlier, in each

**Table 1** Effects of trade exposure on labor market outcomes, by education and gender: 2SLS estimates

| | Pooled | Low-Educated | High-Educated | Males | Females |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| A. Income | | | | | |
| Import exposure (Eastern Europe) | –0.352* | –0.336* | –0.079 | –0.436** | 0.087 |
| | (0.137) | (0.161) | (0.183) | (0.116) | (0.356) |
| Export exposure (Eastern Europe) | 0.286** | 0.293* | 0.063 | 0.335** | –0.015 |
| | (0.092) | (0.111) | (0.131) | (0.082) | (0.219) |
| Number of observations | 52,180 | 39,185 | 12,784 | 28,525 | 23,634 |
| Mean of dependent variable | 23,075 | 20,456 | 31,190 | 27,778 | 17,398 |
| SD of dependent variable | 14,701 | 11,694 | 19,288 | 15,610 | 11,129 |
| First-stage *F* statistic, import | 89.33 | 92.10 | 27.46 | 110.90 | 25.78 |
| First-stage *F* statistic, export | 79.24 | 66.87 | 31.47 | 76.95 | 56.93 |
| B. Hours Worked | | | | | |
| Import exposure (Eastern Europe) | –0.255* | –0.178 | –0.201 | –0.297** | 0.001 |
| | (0.116) | (0.141) | (0.175) | (0.100) | (0.299) |
| Export exposure (Eastern Europe) | 0.191* | 0.154† | 0.128 | 0.235** | 0.007 |
| | (0.075) | (0.091) | (0.128) | (0.072) | (0.174) |
| Number of observations | 54,080 | 40,622 | 13,255 | 29,166 | 24,894 |
| Mean of dependent variable | 38.13 | 37.37 | 40.50 | 42.56 | 32.93 |
| SD of dependent variable | 12.88 | 12.69 | 13.08 | 10.66 | 13.30 |
| First-stage *F* statistic, import | 96.30 | 97.70 | 29.08 | 112.30 | 32.04 |
| First-stage *F* statistic, export | 78.66 | 68.30 | 32.88 | 77.94 | 59.98 |
| C. Employment | | | | | |
| Import exposure (Eastern Europe) | –0.285** | –0.385** | –0.045 | –0.226* | –0.206 |
| | (0.094) | (0.105) | (0.155) | (0.108) | (0.208) |
| Export exposure (Eastern Europe) | 0.171* | 0.234** | –0.006 | 0.151† | 0.093 |
| | (0.071) | (0.080) | (0.108) | (0.085) | (0.136) |
| Number of observations | 70,893 | 54,981 | 15,713 | 34,667 | 36,211 |
| Mean of dependent variable | 0.740 | 0.710 | 0.846 | 0.834 | 0.649 |
| SD of dependent variable | 0.439 | 0.454 | 0.361 | 0.372 | 0.477 |
| First-stage *F* statistic, import | 52.71 | 54.69 | 34.00 | 99.20 | 17.28 |
| First-stage *F* statistic, export | 70.58 | 60.89 | 34.68 | 76.53 | 53.34 |

*Notes:* Standard errors, shown in parentheses, are clustered at the industry level. All models include individual, year × federal state, year × occupation, and one-digit industry fixed effects. Further controls include age and its quadratic term, indicators for education, and household size.

†$p < .10$; *$p < .05$; **$p < .01$

regression, we include a set of individual-level controls, worker fixed effects, federal state × year fixed effects, one-digit industry fixed effects, and occupation × year fixed effects.

Table 1 reports the two-stage least-squares (2SLS) estimates of the effects of trade exposure on wages (in logs), hours worked (in logs), and employment for the full

sample and by education group and gender. In light of the previous evidence, we expect low-skilled workers to be more affected by trade exposure.[13]

We find that increased exposure to import competition from Eastern Europe has a significant negative effect on wages, hours worked, and the probability of being employed. We find the opposite effect for greater export opportunities: higher income, more hours worked, and a higher likelihood of being employed.

We use our point estimates—semi-elasticities in the regressions with log wages and log hours worked as outcome variables—to simulate the average change in wages and hours worked that would arise if individuals were exposed to the mean variation in the trade exposure variables between 1991 and 2018.[14] The estimates using the pooled sample in column 1 of panel A imply that rising import exposure from Eastern Europe decreases wages by 5%, whereas rising export exposure increases wages by 5.5%. These findings are consistent with results found by Huber and Winkler (2019) for a different demographic group (e.g., they included individuals up to age 65) and period (1993–2008).

The effects on hours worked are reported in panel B. For the pooled sample (column 1), the average exposure to imports leads to a 3.5% reduction in working hours. This reduction is offset by the percentage increase predicted by the average rise in exposure to exports.

We also find that trade with Eastern Europe has significant labor market effects on the extensive margin: exposure to more import competition from Eastern Europe reduces the likelihood of being employed in a given year by 4 percentage points (see column 1, panel C). This negative effect is partially offset by the positive effect of greater export opportunities (3.3 percentage points). These findings that trade exposure affects primarily the income and job stability of German workers accord well with the evidence on the wage premium by exporting firms (Egger et al. 2013) and on job stabilizing effect of exposure to trade in Germany (Dauth et al. 2014). Table A.2 displays evidence that import exposure leads to a decline in labor force participation, whereas the effect of export exposure is less precisely estimated.

As shown in column 2 of panel A of Table 1, the trade-induced impacts are most visible for low-educated workers (i.e., those without a college degree). By contrast, the impacts for high-skilled workers are smaller and statistically indistinguishable from zero (see column 3). The effect on hours worked (i.e., the intensive margin) is less precisely estimated when results are reported by education (see columns 2 and 3): the effect on employment rates is slightly more important among low-skilled workers than for the full sample.

In columns 4 and 5 of Table 1, we explore the heterogeneity of trade exposure by gender; see Table A.3 for the breakdown by education and gender, and see Tables A.5 and A.6 for the corresponding ordinary least-squares (OLS) coefficients. Overall, we

[13] The first-stage $F$ statistics reported at the bottom of each panel of Table 1 show that our instruments are well above the conventional thresholds for strong instruments.

[14] We take the average of simulated changes across individuals throughout the study period. The average change in our measures of import and export exposure with Eastern Europe are 0.139 and 0.193, respectively. We then interpret our estimates considering the impact of the average changes in import and export exposure observed throughout the period studied.

find that men experience the largest effects on income, hours worked, and employment; they drive the negative effects obtained in the full sample. Among men, rising import competition leads, on average, to a 6% decline in wages. These losses are fully compensated by the gains from rising export opportunities.

As an additional margin of adjustment in the labor market, we report the effects of our trade variables on the likelihood of working part-time (Table A.7). We find that exposure to imports leads to a reduction in part-time work, whereas exports, if anything, increase it. Our results on labor market outcomes are largely driven by full-time workers (see columns 1 and 2, Table A.8).

We focus on trade flows with Eastern Europe because previous research documented that exposure to trade with Eastern Europe had a significantly larger effect on the German labor market than that with China (Dauth et al. 2014, 2017). The main results analyzing the effects of opening trade with China (Tables A.9 and A.10) tend in the same direction. However, the implied effects are smaller and are not precisely estimated compared with those for trade with Eastern Europe.

In sum, these results confirm evidence presented by Dauth et al. (2014) and Huber and Winkler (2019), who showed that in Germany, both import and export exposures matter in assessing the labor market effects of trade with Eastern Europe. Consistent with the literature, we also show that the beneficial effects of export exposure on income are slightly larger than the negative effects of import competition. The net impact on employment and work hours, however, is close to null. All the labor market effects are concentrated among low-skill workers and men.

### Effects on Fertility and Marital Behavior

Table 2 displays the 2SLS estimates of the effects of trade exposure on fertility outcomes by education group and gender; Table A.11 displays the corresponding OLS estimates. Consistent with our results for labor market outcomes, we find heterogeneous impacts between exposure to trade with Eastern Europe.

Increased import competition reduces fertility, whereas exposure to greater export opportunities increases fertility, although the latter effect is less precisely estimated and is significant only for low-skilled workers. The estimates in column 1 of Table 2 imply that the average exposure to imports throughout the 1991–2018 period reduces the probability of a birth by 1.6 percentage points—a 0.06-percentage-point reduction per year. The rise in exports to Eastern Europe implies a 1.1-percentage-point increase in the probability of a birth—an increase of 0.04 percentage points per year. These effects are larger among low-educated individuals (column 2) and men (column 4); they are smaller and less precisely estimated among high-educated individuals (column 3) and women (column 5).[15,16]

[15] Table A.12 decomposes the results by gender and education. Because of our sample restrictions, the double split by education and gender produces fairly small samples for high-educated individuals. The point estimates in the corresponding subgroups are high (in absolute value) but very imprecisely estimated. Thus, these results must be interpreted with caution.

[16] Our empirical analysis takes the worker as the unit of observation. Huber and Winkler (2019) adopted a household approach and found that trade shock led to significant risk-sharing effects, reducing the

**Table 2** Effects of trade exposure on fertility, by education and gender: 2SLS estimates

| | Pooled | Low-Educated | High-Educated | Males | Females |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| A. Overall Fertility | | | | | |
| Import exposure (Eastern Europe) | –0.120* | –0.128* | –0.110 | –0.125* | –0.078 |
| | (0.046) | (0.055) | (0.137) | (0.051) | (0.081) |
| Export exposure (Eastern Europe) | 0.061† | 0.070† | 0.032 | 0.057 | 0.043 |
| | (0.033) | (0.041) | (0.089) | (0.038) | (0.055) |
| Number of observations | 51,664 | 38,609 | 12,868 | 26,181 | 25,463 |
| Mean of dependent variable | 0.042 | 0.040 | 0.049 | 0.054 | 0.031 |
| SD of dependent variable | 0.202 | 0.197 | 0.216 | 0.225 | 0.173 |
| First-stage *F* statistic, import | 95.19 | 96.19 | 29.11 | 116.00 | 32.09 |
| First-stage *F* statistic, export | 81.93 | 70.64 | 33.44 | 82.73 | 61.00 |
| B. Marital Fertility | | | | | |
| Import exposure (Eastern Europe) | –0.123† | –0.124† | –0.126 | –0.151* | –0.065 |
| | (0.064) | (0.065) | (0.139) | (0.072) | (0.085) |
| Export exposure (Eastern Europe) | 0.079† | 0.086† | 0.053 | 0.096† | 0.038 |
| | (0.046) | (0.050) | (0.095) | (0.055) | (0.058) |
| Number of observations | 35,597 | 25,982 | 9,521 | 17,789 | 17,766 |
| Mean of dependent variable | 0.051 | 0.049 | 0.057 | 0.069 | 0.034 |
| SD of dependent variable | 0.220 | 0.216 | 0.231 | 0.252 | 0.181 |
| First-stage *F* statistic, import | 120.20 | 129.90 | 22.46 | 96.24 | 71.76 |
| First-stage *F* statistic, export | 89.78 | 97.50 | 22.84 | 69.10 | 177.40 |
| C. Nonmarital Fertility | | | | | |
| Import exposure (Eastern Europe) | –0.021 | –0.060 | 0.011 | –0.013 | 0.069 |
| | (0.067) | (0.063) | (0.208) | (0.088) | (0.143) |
| Export exposure (Eastern Europe) | 0.003 | 0.018 | 0.011 | –0.014 | –0.057 |
| | (0.048) | (0.047) | (0.120) | (0.061) | (0.096) |
| Number of observations | 15,634 | 12,267 | 3,150 | 8,157 | 7,410 |
| Mean of dependent variable | 0.022 | 0.021 | 0.025 | 0.022 | 0.022 |
| SD of dependent variable | 0.145 | 0.142 | 0.155 | 0.145 | 0.146 |
| First-stage *F* statistic, import | 26.07 | 22.41 | 14.78 | 54.19 | 8.99 |
| First-stage *F* statistic, export | 29.21 | 21.53 | 22.61 | 65.25 | 9.60 |

*Notes:* Standard errors, shown in parentheses, are clustered at the industry level. All models include individual, year × federal state, year × occupation, and one-digit industry fixed effects. Further controls include age and its quadratic term, indicators for education, and household size.

†$p < .10$; *$p < .05$

worker-level impact of trade shocks on earnings inequality. Our conceptual framework suggests that changes in labor market opportunities and income might affect family choices irrespective of the individual's position in the income distribution. Empirically, our results indicate that *individuals'* labor market outcomes respond significantly to trade shocks even with any risk-sharing intrahousehold adjustment operating in the background.

**Table 3** Effects of trade exposure on first child vs. higher order children: 2SLS estimates

| | Pooled | First Child | Second+ Child |
|---|---|---|---|
| | (1) | (2) | (3) |
| Import Exposure (Eastern Europe) | –0.120* | –0.107* | 0.024 |
| | (0.046) | (0.050) | (0.086) |
| Export Exposure (Eastern Europe) | 0.061† | 0.061† | –0.065 |
| | (0.033) | (0.035) | (0.058) |
| Number of Observations | 51,664 | 31,708 | 19,667 |
| Mean of Dependent Variable | 0.042 | 0.033 | 0.055 |
| SD of Dependent Variable | 0.202 | 0.177 | 0.228 |
| First-Stage *F* Statistic, Import | 95.19 | 57.06 | 80.80 |
| First-Stage *F* Statistic, Export | 81.93 | 59.45 | 94.88 |

*Notes:* Standard errors, shown in parentheses, are clustered at the industry level. All models include individual, year × federal state, year × occupation, and one-digit industry fixed effects. Further controls include age and its quadratic term, indicators for education, and household size.

†$p < .10$; *$p < .05$

The negative effects of import exposure and the positive impacts of export exposure are driven by the effect on marital fertility (panel B). However, we find no evidence of significant effects of import or export exposure on nonmarital fertility (panel C), a result consistent with Autor et al.'s (2019) findings for the United States. Our results are also in line with Becker's model and evidence from previous studies analyzing the effects of income shocks on fertility (Black et al. 2013; Dettling and Kearney 2014; Lindo 2010; Lovenheim and Mumford 2013).

As shown in Table 3, these results are driven by the effects on the likelihood of having any child (i.e., extensive margin). Column 1 presents the estimates on the pooled sample (the same sample shown in column 1, panel A of Table 1). The effect is substantially unchanged when we restrict the analysis to the event of having a first child (column 2, Table 3).[17] We find no significant effect when we restrict the sample to those with at least one child and consider the likelihood of having more than one child (i.e., intensive margin; column 3). The average increase in exposure to exports leads to a 1.1-percentage-point increase in the probability of having the first child. Table A.13 reports the analysis by gender. The estimates are more precisely estimated for men than for women, but they do not differ significantly across the subsamples. Table A.8 shows that, consistent with our findings for labor market outcomes, our main results on fertility are driven by full-time workers (see columns 3–5).

Table 4 shows that our results hold when we focus on completed fertility by restricting the sample to individuals born before 1974, who were at least age 45 before the end of our study period.[18] We examine the effect of trade exposure on individual-level completed fertility (the number of children in the last year an individual was observed

[17] We restrict the sample to individuals reporting their first child or who reported having had no children in the year of the interview.

[18] In our sample, 95% of fathers had a child before age 44, and 95% of mothers had a child before age 42.

**Table 4** Effects of trade exposure on completed fertility (number of children): 2SLS estimates

| | Pooled | Males | Females |
|---|---|---|---|
| | (1) | (2) | (3) |
| Import Exposure (Eastern Europe) | –0.471* | –0.486† | –0.295 |
| | (0.223) | (0.281) | (0.282) |
| Export Exposure (Eastern Europe) | 0.614** | 0.539* | 0.320 |
| | (0.172) | (0.218) | (0.217) |
| Number of Observations | 10,482 | 4,950 | 5,532 |
| Mean of Dependent Variable | 0.823 | 1.006 | 0.660 |
| SD of Dependent Variable | 1.033 | 1.151 | 0.884 |
| First-Stage *F* Statistic, Import | 337.40 | 292.20 | 75.40 |
| First-Stage *F* Statistic, Export | 445.80 | 348.50 | 119.70 |

*Notes:* Robust standard errors are shown in parentheses. The sample is restricted to individuals born before 1974. All models include age and its quadratic term, indicators for education, and state fixed effects.

†$p<.10$; *$p<.05$; **$p<.01$

in the panel), keeping the trade variables at the first year an individual was observed in the sample. The average increase in exposure to imports throughout the study period reduces completed fertility by 8% among workers, whereas exposure to exports leads to a 12% increase in completed fertility. These findings suggest a net percentage increase in overall completed fertility due to trade exposure. Consistent with the earlier analyses, the results on completed fertility are driven by men (columns 2 and 3, Table 4). Using the average exposure to imports and exports for each individual throughout the study period (instead of exposure in the first year an individual entered the sample) yields similar results (Table A.14).

Exposure to trade with Eastern Europe did not significantly affect marital behavior (Table 5), except for a negative and marginally significant effect on divorce. The result for divorce is driven by women and is consistent with the notion that marriage's risk-sharing benefits are countercyclical (e.g., Shore 2010). We find that import exposure leads to a 6% reduction in cohabitation, whereas export exposure leads to an increase of approximately 11%. These results, although imprecisely estimated, are in line with recent findings highlighting the role of social norms and context in shaping family formation (see Adler 1997; Kearney and Wilson 2018). See Table A.16 for the corresponding OLS estimates and Table A.17 for the breakdown by gender and education.

In sum, the evidence suggests that greater exposure to imports worsens labor market outcomes and reduces the likelihood of having children. By contrast, exposure to greater export opportunities enhances labor market prospects and increases fertility. Consistent with our findings for labor market outcomes, we find no evidence of significant effects of exposure to trade with China on fertility and marital behavior (Tables A.18 and A.19). This evidence is consistent with income effects on fertility choices: the decision to have children correlates with the direction of trade-induced income changes.

**Table 5** Effects of trade exposure on marital behavior, by education and gender: 2SLS estimates

| | Pooled | Low-Educated | High-Educated | Males | Females |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| A. Marriage | | | | | |
| Import exposure (Eastern Europe) | –0.019 | 0.053 | –0.223 | –0.020 | 0.178 |
| | (0.071) | (0.095) | (0.148) | (0.071) | (0.138) |
| Export exposure (Eastern Europe) | –0.010 | –0.026 | 0.064 | –0.028 | –0.114 |
| | (0.054) | (0.071) | (0.107) | (0.056) | (0.095) |
| Number of observations | 51,941 | 38,473 | 13,265 | 27,713 | 24,208 |
| Mean of dependent variable | 0.619 | 0.595 | 0.692 | 0.620 | 0.618 |
| SD of dependent variable | 0.486 | 0.491 | 0.462 | 0.485 | 0.486 |
| First-stage *F* statistic, import | 90.51 | 92.68 | 30.22 | 132.30 | 31.33 |
| First-stage *F* statistic, export | 85.08 | 73.60 | 33.81 | 85.28 | 65.31 |
| B. Divorce | | | | | |
| Import exposure (Eastern Europe) | –0.108† | –0.113 | –0.091 | –0.069 | –0.227† |
| | (0.062) | (0.079) | (0.098) | (0.055) | (0.130) |
| Export exposure (Eastern Europe) | 0.065 | 0.060 | 0.067 | 0.053 | 0.115 |
| | (0.047) | (0.060) | (0.059) | (0.042) | (0.094) |
| Number of observations | 54,965 | 41,382 | 13,371 | 29,543 | 25,402 |
| Mean of dependent variable | 0.058 | 0.062 | 0.045 | 0.043 | 0.074 |
| SD of dependent variable | 0.233 | 0.240 | 0.208 | 0.203 | 0.262 |
| First-stage *F* statistic, import | 98.08 | 99.02 | 29.58 | 112.90 | 32.67 |
| First-stage *F* statistic, export | 79.80 | 69.10 | 33.48 | 78.63 | 60.97 |
| C. Cohabitation | | | | | |
| Import exposure (Eastern Europe) | –0.101 | –0.104 | 0.049 | –0.106 | –0.228 |
| | (0.121) | (0.123) | (0.204) | (0.138) | (0.197) |
| Export exposure (Eastern Europe) | 0.130 | 0.110 | 0.092 | 0.147 | 0.202 |
| | (0.097) | (0.097) | (0.142) | (0.125) | (0.132) |
| Number of observations | 54,904 | 41,320 | 13,371 | 29,487 | 25,397 |
| Mean of dependent variable | 0.222 | 0.226 | 0.207 | 0.202 | 0.245 |
| SD of dependent variable | 0.416 | 0.418 | 0.405 | 0.402 | 0.430 |
| First-stage *F* statistic, import | 97.61 | 98.94 | 29.76 | 115.10 | 32.13 |
| First-stage *F* statistic, export | 79.88 | 69.23 | 33.70 | 78.90 | 61.16 |

*Note:* Standard errors, shown in parentheses, are clustered at the industry level. All models include individual, year × federal state, year × occupation, and one-digit industry fixed effects. Further controls include age and its quadratic term, indicators for education, and household size.

†$p < .10$

## Robustness Checks and Heterogeneity Analyses

We conduct several sensitivity checks and heterogeneity analyses. Table A.15 presents the estimates of the effects of imports and exports exposure on labor market outcomes and fertility, leveraging the local variation in trade exposure at the level of regional policy regions (*Raumordungsregionen* [ROR]) in 1996. This alternative

empirical strategy follows the commuting-zone approach of Autor et al. (2013). The trade values varying by industry and year are allocated to regions according to the distribution of employment across regions and industry in 1996.[19] Employment data are drawn from Federal Employment Agency Statistics (*Bundesagentur für Arbeit Statistik*). Because 1996 is the first year for which these data are available at a finer geographical level than the federal state, we cannot use earlier years as a base to construct our regional-level trade exposure variables. Following Autor et al. (2013) and Dauth et al. (2014), we use other countries' import and export flows as an instrument for local import and export exposure, respectively, in Germany.[20]

Results confirm the negative effects of imports on labor market outcomes and fertility (columns 1–5, Table A.15). The implied magnitudes are larger than those in our baseline specification, suggesting that local general equilibrium effects (i.e., outside the individual's industry of initial employment) exacerbate the direct effects. In particular, the average change in exposure to imports throughout the period leads to a 13.8% reduction in wages and a 3.4-percentage-point decline (4% relative to the sample mean) in the likelihood of being employed. The average exposure to exports yields a 10.5% increase in income and a 3.8-percentage-point increase in the employment probability.[21] Examining fertility, we find that the average exposure to imports from Eastern Europe throughout the period leads to a 3.2-percentage-point decrease in fertility (0.13 percentage points per year), whereas the average exposure to exports increases fertility by 2.6 percentage points (0.10 percentage points per year). We also confirm the lack of significant effects of trade on marital outcomes (columns 6–9, Table A.15).

A potential concern is that our findings may be confounded by the effects of the Great Recession.[22] To dispel this concern, we repeat our main analyses while excluding the 2008–2018 period (Tables A.20–A.23). The estimated coefficients on the trade variables remain fairly stable relative to the benchmark specification.

We also report results on fertility behavior obtained using alternative age-groups: 17–44 (Table A.24), 20–40 (Table A.25), and 20–50 (Table A.26). Overall, the results are very similar to those obtained using our baseline sample of individuals aged 20–44 (see Table 2). In our benchmark specification, we use one-year lagged values of the exposure to imports and exports to predict the effect of exposure on labor market outcomes and family behavior. Table A.27 shows results for a longer lag structure.[23] The point estimates of the effect of imports on labor market outcomes are similar overall.

[19] Our measure of import exposure is calculated as follows:

$$\sum \lambda_{j,\,r,\,1996} \times IMP_{GER,jt}, \tag{2}$$

where λ is the ROR's ($r$) share of workers in industry $j$ in 1996, and $IMP_{GER,jt}$ is the national-level imports in industry $j$ in year $t$. Similarly, our measure of export exposure is

$$\sum \lambda_{j,\,r,\,1996} \times EXP_{GER,jt}. \tag{3}$$

[20] Germany has 96 regional policy regions, as defined by the Federal Office for Building and Regional Planning based on their economic interlinkages. For detailed information on SOEP regional data, see Knies and Spiess (2007).

[21] The average changes in our measures of regional import and export exposure from Eastern Europe are 2.9 and 3.2, respectively.

[22] In contrast to other European countries, Germany recorded a very mild recession as measured by unemployment and GDP changes.

[23] The inclusion of additional lags further restricts our sample size.

The effect of imports on fertility, if anything, increases in absolute value. In contrast, the effect of exports on labor market outcomes is smaller and less precisely estimated, while the point estimates of the effect of exports on fertility are similar.

Table A.28 displays the heterogeneity of the results using an alternative definition of education based on the tracking decisions pupils made at the transition from primary to secondary school (Krause and Schüller 2014; Zimmermann et al. 2013). Highly educated individuals are those with higher (academic) or intermediate secondary education, and less educated individuals are those with lower secondary education (basic track, or *Hauptschulabschluss*). Results confirm that relative to highly skilled individuals, less skilled individuals are more affected by the labor market consequences of imports from and exports to Eastern Europe, thereby leading to a larger negative effect (in absolute value) of imports on fertility (columns 4 and 5, panels A and B). In addition, we examine heterogeneous effects by occupational type, comparing blue-collar and white-collar workers. Columns 6 and 7 of panels A and B show that the effects of trade exposure on labor market outcomes and fertility are concentrated among individuals in blue-collar jobs.

Tables A.29–A.32 show the results when we include ROR × year fixed effects (instead of the more aggregated federal state × year fixed effects), which account for time-specific shocks at the ROR level. Reassuringly, the main results are unchanged.

We also experimented with earlier cutoffs for the year of entry to define our working sample. Table A.33 shows the robustness of our main findings for income and fertility when we restrict the sample to individuals entering the SOEP before 1995 or 1990. Overall, we find very similar results.

Finally, we conduct a falsification test using lagged data for all our labor market and demographic outcomes of interest. We estimate the impact of trade exposure on outcomes lagged by 10 years. Importantly, we find no evidence of significant effects of trade exposure (Tables A.34–A.37). This placebo test lends further support to a causal interpretation of the effect of trade on the labor market, fertility, and marital behavior.

## Conclusion

In this study, we investigated the effects of globalization on labor markets, fertility, and marital behavior by exploiting longitudinal data and within-worker variation in exposure to trade. Previous studies have not examined the impacts of trade on fertility and marital behavior in a lowest-low fertility, high-income context. Our focus on Germany fills this gap.

To identify the effects of trade flows, we follow the strategy adopted by Autor et al. (2014) and Autor et al. (2019). We first confirm the results of previous studies finding heterogeneous effects of imports and exports on the German labor market. Our main contribution is exploring the consequences of globalization on fertility and marital behavior. We find that exposures to imports and exports have very different effects on family choices. Increased exposure to import competition from Eastern Europe lowers fertility. This effect is at least partly offset by the positive impact of exposure to greater export opportunities. The observed import and export effects are largely driven by the low-educated workers and men. Further, the fertility effect is

mostly seen in the probability of having any child (extensive margin). Although we find evidence of some fertility postponement, exposure to imports negatively affects completed fertility—an effect that is again compensated by the positive influence of exports. However, we find no evidence of significant changes in marital patterns, despite some evidence that imports led to a decline in divorce among women. These findings are consistent with neoclassical fertility models, which highlight the role of income effects: workers who experience negative labor market outcomes because of import competition reduce fertility, whereas workers who improve their labor market stance owing to greater export opportunities increase fertility. Because German trade with Eastern Europe is primarily intraindustry, most workers are likely to experience both effects to some extent: negative effects from imports and positive effects from exports.

Germany's low natality rate has been a major concern for politicians for decades. The fertility behavior effects of negative labor demand shocks due to, for instance, import competition are nonnegligible and should not be overlooked. Policies tackling the demographic deficit by extending parental leave or increasing child allowances may mitigate the adverse demographic impact of labor demand shocks. Our analysis omits the possible influence of domestic policies on the impact of labor market shifts on family choices. Future research might thus investigate the role of family-oriented policies in mediating the effects of labor market shocks on demographic behavior and life course choices. ■

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