RATIONALITY & COMPETITION
CRC TRR 190

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Hannah Liepmann (Humboldt University Berlin)

Discussion Paper No. 69

January 24, 2018

Collaborative Research Center Transregio 190 | www.rationality-and-competition.de
Ludwig-Maximilians-Universität München | Humboldt-Universität zu Berlin
Spokesperson: Prof. Dr. Klaus M. Schmidt, University of Munich, 80539 Munich, Germany
+49 (89) 2180 3405 | info@rationality-and-competition.de

The Impact of a Negative Labor Demand Shock on Fertility - Evidence from the Fall of the Berlin Wall*

Hannah Liepmann†

January 23, 2018

Abstract

How does a negative labor demand shock impact fertility? I analyze this question in the context of the East German fertility decline after the fall of the Berlin Wall in 1989. I exploit differential pressure for restructuring across East German industries which led to unexpected, exogenous, and permanent changes to labor demand. I find that throughout the 1990s, women more severely impacted by the demand shock had relatively more children than their less-severely-impacted counterparts. Thus, the demand shock did not only depress the aggregate fertility level but also changed the composition of mothers. My paper shows that these two effects do not necessarily operate in the same direction.

Keywords: Fertility, Labor Demand Shock, Industrial Restructuring, East Germany

JEL Codes: J13, J23, P36

*I thank Alexandra Spitz-Oener for her support of this project. I also thank Henry Farber, Anette Fasang, Bernd Fitzenberger, Knut Gerlach, Albrecht Glitz, Heidi Hartmann, Ariane Hegewisch, Ilyana Kuziemko, Jürgen Kühl, Olivier Marie, Adriana Lleras-Muney, Jesse Rothstein, Daniel Schneider, Stephen Trejo, Till von Wachter, Felix Weinhardt; my doctoral colleagues Niko de Silva, Friederike Lenel, and Arne Thomas; and seminar participants at the Essen Health Conference, the IRLE at UC Berkeley, Humboldt-University, the CRC 190 Workshop in Schwanenwerder, SOLE Annual Meetings, the Lindau Nobel Laureate Meeting, and the seminar of the Berlin Network of Labor Economists for valuable comments and discussions. I am grateful to Daniela Hochfellner, Tatjana Mika, and Dana Müller for their generous support with the BASiD data; and to Vera Dahms for helpful advice on the GDR establishment survey. Financial support by the German Science Foundation (DFG) through the Research Training Group 1659 and the Collaborative Research Centers 649 and 190 is gratefully acknowledged. Access to the BASiD data was provided via on-site use at the Research Data Centre (FDZ) of the Institute for Employment Research (IAB). Results were reviewed by the FDZ/IAB to ensure data protection requirements. All errors are mine.

†Humboldt-University Berlin, Spandauer Str. 1, 10099 Berlin. E-mail: hannah.liepmann@hu-berlin.de

1 Introduction

In this paper, I revisit the question of how women's labor market situations impact childbearing decisions. From a neoclassical point of view, this is an empirical question, as there are opposing income and substitution effects (e.g., Becker, 1965; Gronau, 1977). Yet the endogeneity of individuals' labor market outcomes poses a key empirical challenge. I analyze the question in the context of East Germany after the fall of the Berlin Wall in 1989. Fertility in the formerly communist country plummeted after 1989 and recovered only slowly in later years (Figure 1).[1] The magnitude of this decline in East German fertility is unprecedented (Eberstadt, 1994). It stands in contrast to the relatively constant and already low West German fertility level.

The East German setting is particularly suited to study the effect of women's labor market prospects on fertility. To tackle the endogeneity problem, I exploit exogenous variation in the negative labor demand shock which hit East Germany as a consequence of the introduction of the market economy. The variation stems from differential pressure for restructuring across East German industries. A unique advantage of my empirical strategy results from the unexpectedness of the demand shock. In the former German Democratic Republic self-selection into industries was independent of later industrial restructuring. Instead, prior to German reunification, East German workers were accustomed to remarkably stable industrial employment structures and full employment guaranteed by the state. Furthermore, sorting into industries was exogenous to later industrial restructuring because of constrained job choice under central planning.

The nature of the labor demand shock represents another distinguishable feature of the East German setting. The shock is a one-time event which led to sharp and permanent structural changes to labor demand. This allows me to isolate how the effects of the shock evolved over time. Specifically, I show that in 1989, the East German employment distribution over industries strongly differed from economic structures in market economies. Within only a few years after reunification East German industrial employment structures converged to the market economy benchmark provided by West Germany. I exploit this fact to abstract from endogenous supply-side adjustments and to measure the varying intensity of the negative labor demand shock. While the exploited variation is clearly linked

[1] The decline was due to postponement of childbearing (Conrad et al., 1996; Lechner, 2001), but there was also a real reduction of births (Kreyenfeld, 2003; Goldstein and Kreyenfeld, 2011).

Figure 1: Total Fertility Rates by Region and Year, 1980-2012

Source: Human Fertility Database, Max Planck Institute for Demographic Research (Germany) and Vienna Institute of Demography (Austria), available at www.humanfertility.org (data downloaded in March, 2015). See Goldstein and Kreyenfeld (2011, p.454) for a similar graph. The total fertility rates are defined for each year as the unweighted sum of all age-specific birth rates for women in their childbearing years.

to the reunification of Germany, it resembles long-term changes to industrial employment structures which started earlier and were more gradual in market economies. This makes the variation relevant and informative beyond the context of East Germany.

My analysis is based on rich administrative data from German unemployment and pension insurance records, called BASiD. The panel structure of these data permits a detailed individual-level analysis of fertility over a relatively long time period of seventeen years. Moreover, in the BASiD data I am able to identify the significant fraction of East German women who migrated to West Germany. This allows me to study childbearing decisions of East German women rather than of women living in East Germany.

To preview the results, I first establish that the industry-level labor demand shock generated exogenous variation in individuals' labor market outcomes by increasing unemployment and inducing mobility across industries. I then show that this had an impact on the composition of mothers: Throughout the 1990s, women more severely impacted by the labor demand shock had relatively more children than their counterparts who were less

severely impacted. This composition effect is economically significant and it persists over the seventeen year period. The composition effect is moreover robust to evaluating the influence of migration to West Germany, child care, regional spill over effects, firm-level characteristics, and the presence of assortative mating. Furthermore, the composition effect is robust when older cohorts of East German women or, alternatively, West German women are used as a control group. Concerning effect heterogeneity, the industry-level shock impacted all qualification groups, including the highly skilled. Finally, my empirical estimates suggest a small permanent effect on completed fertility.

My results point to a trade-off between female careers and childbearing: In the uncertain economic environment in East Germany after reunification, women with more favorable labor market outcomes had relatively fewer children because they apparently were less willing to put their current jobs and future labor market prospects at risk. This mechanism affected the composition of mothers against the backdrop of a low aggregate fertility level. In this sense, the substitution effect dominated over the income effect in determining the composition of mothers.

As a general implication of my results, it is important to distinguish between the level effect and the composition effect of a labor demand shock on fertility. Indeed, my results show that the two effects do not necessarily operate in the same direction. In the East German context there was a pronounced negative level effect which depressed aggregate fertility after the fall of the Berlin Wall and among all groups of East German women (see Figure 1 above). This level effect was due to rapid systemic change after German reunification (Frejka, 2008). While the level effect cannot be attributed to a single factor alone, a major contributing factor was the aggregate negative labor demand shock.[2] I demonstrate in this paper that in addition to the level effect, women's relative labor market situations determined which women were relatively more likely to have children, thereby changing the composition of mothers.[3] This composition effect was positive, in the sense that women more severely impacted by the demand shock had relatively more children than their less-severely-impacted counterparts. The composition effect was not

[2]In this context, Chevalier and Marie (2017) emphasize the importance of elevated economic uncertainty in causing the decline in aggregate fertility (see also Eberstadt, 1994; Conrad et al., 1996; Sobotka et al., 2011). Arntz and Gathmann (2014) stress the importance of new opportunities. In my view, these two explanations are not mutually exclusive.

[3]Earlier research shows that aggregate economic conditions lead to such composition effects (Dehija and Lleras-Muney, 2004; Chevalier and Marie, 2017); whereas I focus on the impact of individuals' labor market outcomes.

strong enough to counterbalance the negative level effect. Nevertheless, the composition effect was meaningful in an economic sense.

My paper is related to three strands of literature. First, I build on previous studies on the East German fertility decline. Chevalier and Marie (2017) document the drop in East German fertility and find that it was more pronounced for older and more affluent women. They show that this caused poor educational outcomes for East Germans born in the years following the fall of the wall. Moreover, these authors stress feelings of economic uncertainty as a determinant for childbearing decisions. I regard my results as complementary, because they highlight another mechanism explaining the selection into motherhood. In contrast to Chevalier and Marie (2017), I exploit exogenous variation in women's employment situations and analyze how this impacts childbearing over time. Arntz and Gathmann (2014) focus on returns to experience in market economies and find that predicted motherhood wage penalties led to lower birth rates among East German women. Bhaumik and Nugent (2011) and Kreyenfeld (2010) document a negative impact of perceived employment uncertainty on birth rates of East German women. Finally, in accordance with my findings, Kohler and Kohler (2002) show that in Russia during the mid 1990s, less favorable labor market outcomes were in several cases positively correlated with fertility.

Second, this paper is related to two previous studies which exploit industry-level variation of changes to labor demand in the United States.[4] Schaller (2016) uses a Bartik-type instrumental variable strategy. Autor et al. (2017) focus on import competition from China.[5] With regard to fertility, both studies find that fertility tends to increase in regions where female labor market prospects decline. My contribution to this literature is twofold. To begin with, compared to the shift-share approaches employed in these previous studies, my measure for the demand shock exploits variation at the industry level but not based on geographic differences in initial industry concentration. Thus, my approach is not affected by serial correlation which is a potential concern in the context of shift-share analysis. In addition, the level of analysis in both previous studies is the regional level, whereas I investigate individual-level fertility. Specifically, I follow selected

[4]See also Perry (2004) who explores heterogeneity depending on women's qualification. Early contributions in this area are Schultz (1985) and Heckman and Walker (1990).

[5]Methodologically similar studies analyze shocks to family income or wealth. These studies exploit job displacement of husbands (Lindo, 2010), energy price shocks which increased male wages in a coal mining region (Black et al., 2013), or real estate price changes (Lovenheim and Mumford, 2013; Dettling and Kearney, 2014).

cohorts of women over time and analyze their childbearing decisions for the extensive and intensive margins of fertility. This enables me to show that the labor demand shock impacted the timing of childbearing, but also had a persistent impact on individual-level fertility in the long term. Such micro-level mechanisms have important implications for the life courses and labor market trajectories of women. These dynamics can also have long-term consequences, since parents' labor market outcomes affect children to the extent that socio-economic inequalities persist across generations.

Finally, this paper is related to studies analyzing plant closures, which arrive at different conclusions. For Finland, Huttunen and Kellokumpu (2016) show that female job loss decreases fertility. Del Bono et al. (2015) also find negative effects of job loss on the fertility of female white-collar workers in Austria, which they attribute to career disruptions.[6] This demonstrates the importance of the type of demand shock investigated. Mass layoffs have severe implications at individual and regional levels, but a significant fraction of displaced workers move into new jobs relatively quickly (Gathmann et al., 2017). Thus, after a plant closure, women seem to prioritize their reentry into employment before they have children. By contrast, a structural demand shock affecting entire industries may impact previously acquired human capital more permanently, thereby causing the composition effects I analyze in this paper.

The paper proceeds as follows. In the next section, I provide background information on the labor demand shock and its variation across industries. Section 3 contains a description of the data and sample. Section 4 includes the baseline empirical model. In Section 5, I establish that the labor demand shock impacted labor market outcomes. I analyze its effect on fertility in Section 6. In Section 7, I introduce two alternative control groups to demonstrate the robustness of the results. Section 8 focuses on heterogeneity by qualification level and age group. Section 9 concludes.

[6]Similarly, De la Rica and Iza (2005) show that the high prevalence of fixed-term contracts and a higher threat of job loss were associated with delayed childbearing in Spain.

2 Background and Empirical Strategy

2.1 Selection Into Industries

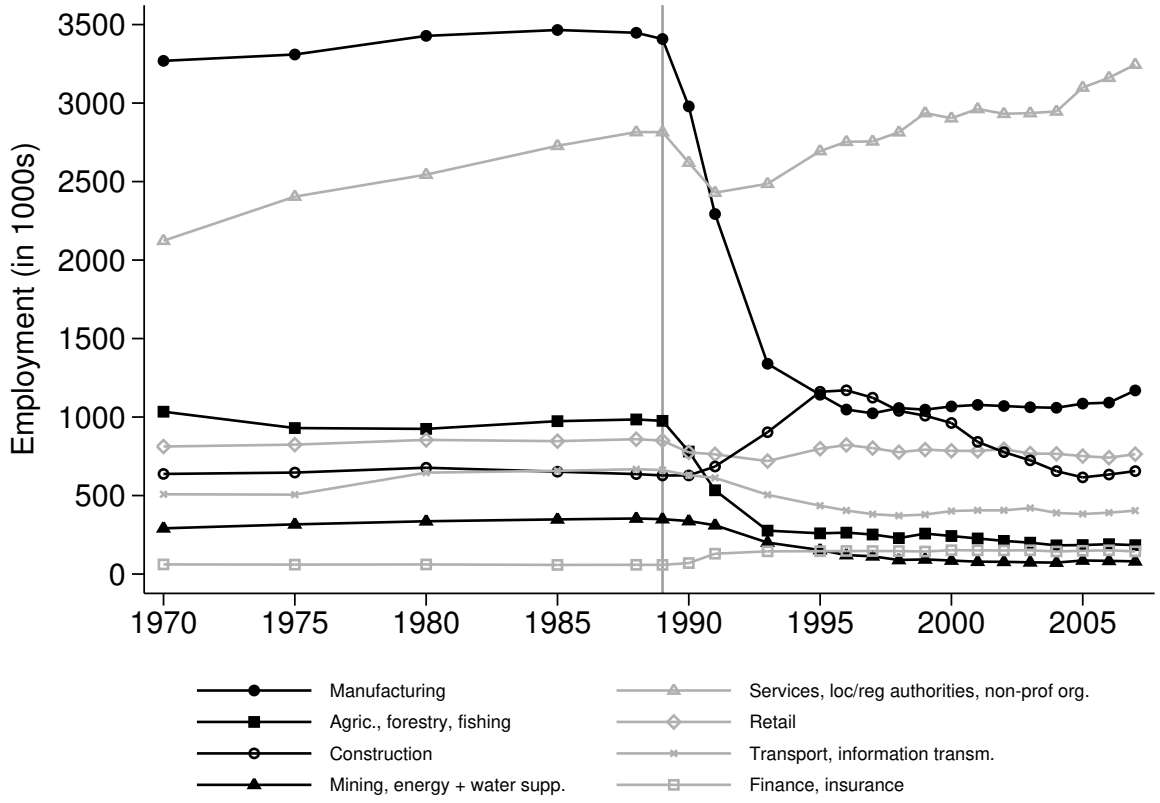
Since my empirical strategy exploits variation at the industry level, it is important that the reunification and economic integration of Germany were not anticipated. GDR citizens self-selected into jobs and industries independently of conditions that later prevailed in the market economy. A related argument has been made in the migration literature with regard to pre-determined occupational choices of migrants (Friedberg, 2001; Borjas and Doran, 2012; Prantl and Spitz-Oener, 2014).

In addition, self-selection into industries was constrained under central planning (Köhler and Stock, 2004; Baker et al., 2007; Fuchs-Schündeln and Masella, 2016). When Erich Honecker came to power in 1971, access to higher education was severely restricted. Very few students were allowed to obtain the school diploma which qualified for direct university admission. Apart from good performance in school, the demonstration of political loyalty towards the GDR regime and active membership in the "Free German Youth" were necessary prerequisites for being accepted to this school track. Career counseling was meant to influence individuals from an early age onwards to ensure that their occupational choices were made in accordance with available positions. In the sixth school year at the latest, students had to define their desired occupation for the first time. Applications for multiple apprenticeship positions were officially not possible. In sum, self-selection into industries was exogenous to the labor demand shock studied in this paper because German reunification was not anticipated and because job choices in the GDR were constrained.

2.2 Employment Development by Economic Sector

As the market economy was introduced in the formerly communist country, East Germany experienced a sharp reduction in labor demand. This affected individual East German industries differently. I now discuss the differential impact of the labor demand shock across broadly defined economic sectors. At this level of aggregation I could compile reliable and consistently classified employment data spanning several decades. For 1970 to 1990, these data are based on the universe of all East German establishments, and for 1991 to 2007 the data source is the German Microcensus. The time series illustrates that the demand shock is particularly suited to the purposes of my research question.

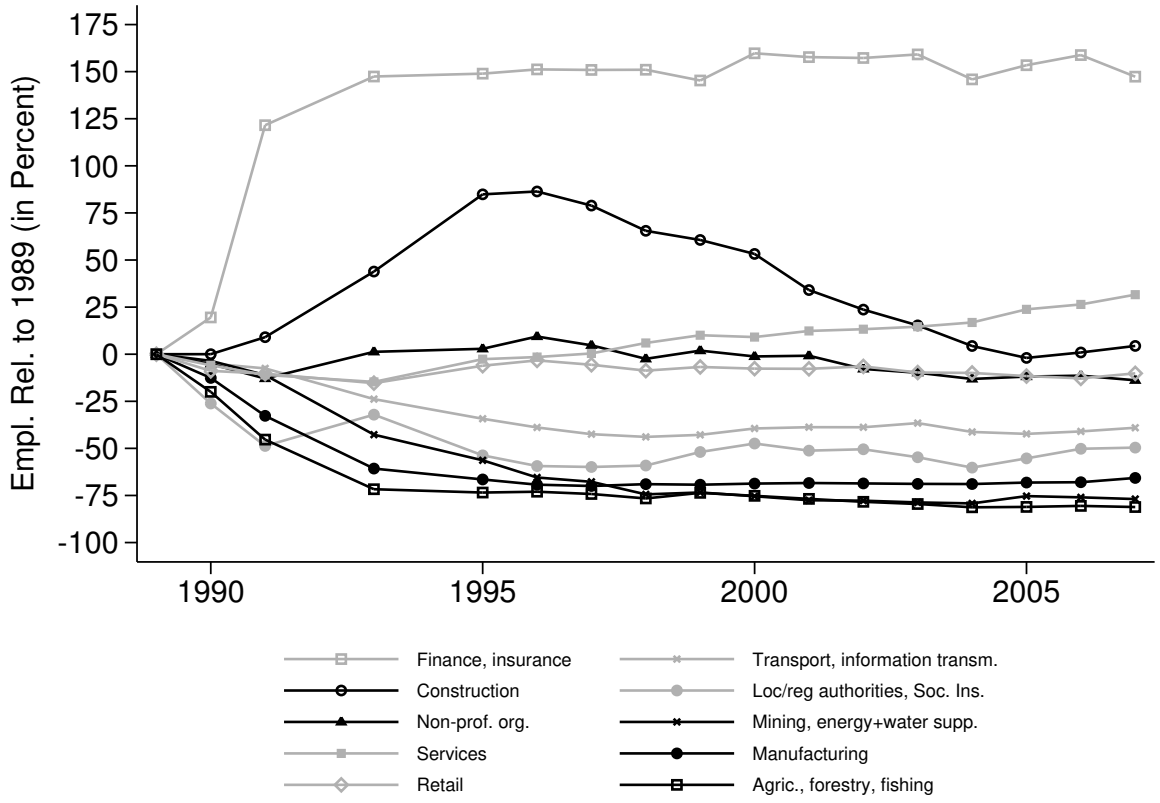
Figure 2: Employment in East Germany (in Thousands), By Economic Sector, 1970-2007 (Selected Years)



Source: *1970-1989:* Federal Statistical Office (1994), which recoded data for the universe of all GDR establishments (the so-called *Berufstätigenerhebung*) according to West German classification schemes, and additionally included persons working for the army, Ministry of the Interior, Socialist Unity Party, and the Ministry of State Security as these did not appear in official GDR statistics. *1990:* Bernien et al. (1996, p.16) based on the last *Berufstätigenerhebung* which also covered the universe of all East German establishments and refers to November 30th, 1990. *1991-2004:* Author's calculations based on Scientific Use Files of the Microcensus (a 0.7 percent sample of the population) for persons aged 15 and older living in East Germany (including East Berlin) at their main residence.

Figure 2 shows absolute East German employment by sector from 1970 to 2007. The figure reveals that before 1989 sectoral employment structures were remarkably stable in the GDR. This stability reflects that central planners pursued an extensive growth strategy, which was based on a mere expansion of production. There was no transition to an intensive growth strategy, which would have fostered productivity increases and corresponding adjustments of the sectoral structure. In the 1980s, the political authorities of the GDR failed to reallocate workers across sectors. One reason was that firms engaged in labor hoarding. Also, workers were reluctant to leave their firms as these had an important social function. The resulting sectoral stability was possible only because full employment was guaranteed by the state. This included full employment of women

Figure 3: Employment in East Germany Relative to 1989 (in Percent), By Economic Sector



Source: As in the previous figure. From 1989 onwards it is possible to split up the large category of services, local and regional authorities, and non-profit organizations.

(Grünert, 1996; Ritter, 2007).

There was a structural break due to German reunification, which unexpectedly and permanently changed the East German employment distribution over sectors. This is further illustrated in Figure 3, which displays relative employment changes by sector after 1989. Employment losses were especially drastic in agriculture, manufacturing, and "mining, energy and water supply" where employment declined by up to 75 percent until 1993. The second highest relative employment losses were in local and regional authorities, and in transport and information transmission. Much less pronounced employment losses occurred in retail, not-for-profit organizations, and services. The service sector even grew from 1993 onwards. Finally, the rather small finance and insurance sector grew strongly, and the construction sector experienced a boom which lasted until 1996.[7]

[7]To compare the East German case to a market economy, in Appendix Figure A1 I plot the development of West German employment by economic sector. In West Germany, changes to the sectoral

The East German employment decline after the fall of the wall and its variation across sectors were driven by three main phenomena (Lutz and Grünert, 1996). First and most importantly, the former GDR economy had to adjust to the fact that there were clear differences in economic structures between the GDR and market economies such as West Germany. It is crucial for my empirical strategy that, between East and West Germany, there were pronounced differences in the distribution of workers across broad sectors and more detailed industries. Second, employment declined due to migration to West Germany, early retirement schemes, and layoffs of workers with low performance who had been guaranteed jobs in the GDR. Finally, many workers in the so-called "Sector X" lost their jobs. These workers had previously been employed by the army, the Ministry of the Interior, the Ministry of State Security, and the Socialist Unity Party. Personnel replacements also impacted academic disciplines related to the economic and social system of the GDR.

2.3 Industry-Level Variation of the Labor Demand Shock

In the analysis, I will rely on variation of the labor demand shock at a level that is more detailed than broad sectors. Specifically, I will distinguish between 48 different industries. To give examples within the manufacturing sector, employment in the textiles and wearing apparel industries declined by more than 80 percent between 1989 and 1993, compared with a 57 percent employment decline in the food production industry, and a 41 percent decline in the chemical industry. Within services, the category "other services (consulting and related activities)" had declined by 19 percent by 1993, whereas the category "accommodation, homes, laundry, cleaning, waste collection" had increased by 40 percent.[8]

These relative changes in employment reflect both demand-side and supply-side adjustments. A credible identification strategy, however, should circumvent supply-side adjustments, as they are potentially endogenous. In particular, supply-side adjustments could be related to childbearing decisions. Thus, I derive an exogenous measure for the varying intensity of the labor demand shock where I exploit that the employment distribution

structure started earlier and were more gradual. There was no structural break in West Germany after the fall of the Berlin Wall.

[8]Note that 1993 was chosen as the reference year, because the major employment changes occurred up until 1993; also there was a change in the industry classification scheme after 1993 which could introduce measurement error at the level of these more detailed industries.

over industries differed strongly between East and West Germany in 1989. Specifically, I define the following measure of relative excess supply (RES):

$$RES_{j,89} = \frac{(Empl\_East_{j,89}/Empl\_East_{89}) - (Empl\_West_{j,89}/Empl\_West_{89})}{Empl\_East_{j,89}/Empl\_East_{89}}, \quad (1)$$

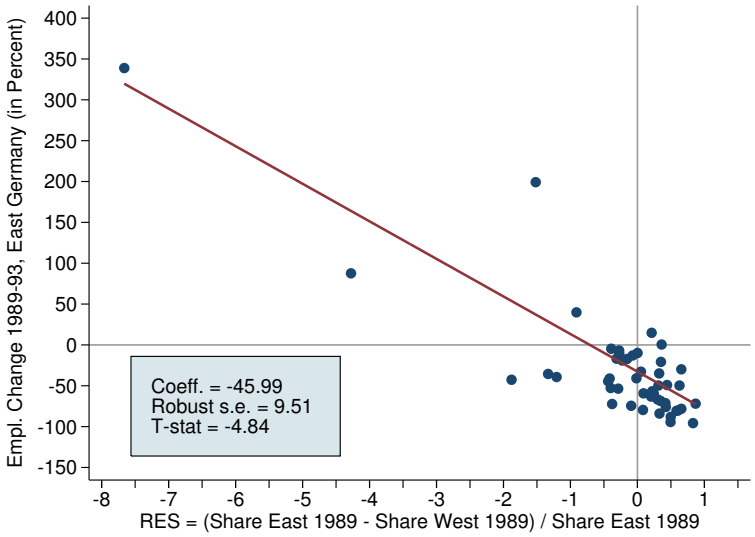
where $Empl\_East_{j,89}$ denotes the number of East German workers employed in an industry j in 1989, and $Empl\_East_{89}$ stands for total East German employment in 1989. $Empl\_West_{j,89}$ and $Empl\_West_{89}$ are defined analogously for West Germany. The numerator accounts for percentage point differences in East and West German industry shares in 1989. The larger the numerator is, the greater is the excess supply of East German workers in an industry j relative to the West German market economy benchmark. Accordingly, one can expect East German employment in j to decline. The denominator relates this percentage point difference to the relative size of an East German industry, since a given percentage point difference should matter the more, the smaller is an East German industry.

Importantly for identification, the RES measure is entirely based on differences in industrial employment structures that emerged because of divergent economic developments in East and West Germany during the separation of the country. Thus, the RES variable is exogenous to supply side adjustments after the fall of the wall, such as potentially selective fertility decisions, migration to West Germany, or other movements out of the East German labor force.

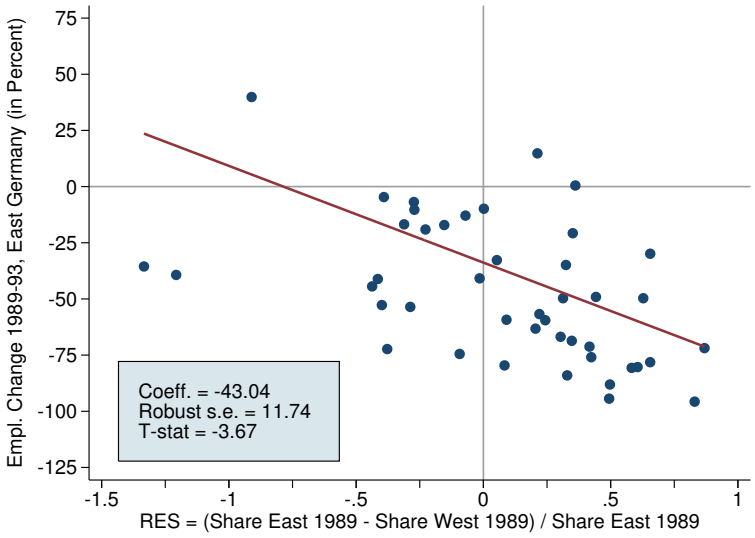
In panel (a) of Figure 4, I regress relative employment changes by industry on the RES measure. The figure illustrates the strength of the RES measure as a predictor for the relative employment decline of East German industries after 1989. Relative employment changes are captured by the percentage change in employment of an East German industry between 1989 and 1993. The figure confirms that relative employment changes of East German industries are indeed negatively correlated with 1989 employment differences in East and West German employment structures, as measured by the RES variable. As shown in Panel (b) of the same figure, where the four most extreme RES values are excluded, this negative relationship is not driven by outliers.[9]

[9]'Social insurance agencies' are excluded from the analysis but discussed in the Appendix. In 1989, only 0.07 percent of East German workers were employed in this industry. Its employment increased by 850 percent up to 1993. Most workers initially employed in this industry were replaced (Bernien et al., 1996), and the expansion of this industry had no positive impact on their later labor market success.

Figure 4: Correlation between the Relative East German Employment Change 1989 to 1993 (in Percent) and Relative Excess Supply in 1989, By Industry



(a) All industries (N=48)



(b) Excluding the four most extreme RES-values

Source: For East Germany in 1989 and 1993, the data sources are the same as in Figures 2 and 3. For West Germany in 1989, the data source is the Microcensus. See Appendix Table A3 for a list of the industries. The regression models are weighted using 1989 East German employment shares of industries as analytical weights. The y-axis displays change in percent, i.e., $100 * \frac{Empl\_East_{j,93} - Empl\_East_{j,89}}{Empl\_East_{j,89}}$. In panel (b), the four most extreme RES-values are excluded (these are the finance and insurance industries, "printing & reproduction," and "finishing trade") to emphasize that the relationship in panel (a) is not driven by outliers.

The negative relationship displayed in Figure 4 can be rationalized by two arguments. First, market forces led to a convergence of the distribution of East German employees to the West German standard. After all, West German industry structures had evolved such that the West German economy was relatively successful internationally. Second, as part

of the massive privatization of East German firms by the "Trust Agency," decisions were made on a case-by-case basis, while industrial policy concerns such as regional spill-over effects played a subordinate role. In this context, East German firms were frequently taken over by and integrated into West German firms belonging to the same industry. The East German firms were, for example, then established as suppliers of intermediary input goods (Wahse, 2003; Federal Institute for Special Tasks Arising From Unification, 2003).

To summarize, as a result of the fall of the wall, employment changes which occurred more gradually in market economies such as West Germany (Appendix Figure A1) took place within only a few years in East Germany. This makes the explored variation informative beyond the East German context. Given these gradual developments in West Germany, however, a potential objection is that the employment developments in East Germany occurred on their own and were unrelated to reunification. To investigate this, I conduct a placebo test. As an analogue to Figure 4, I regress West German employment changes on the RES measure (Appendix Figure A2). Reassuringly, the coefficient now becomes -2.23 (as opposed to -45.99 in the case of East Germany), which demonstrates that the variation exploited in this paper is clearly linked to German reunification. Essential for the identification strategy is the fact that East Germans did not anticipate the demand shock. At the point in time when former GDR citizens sorted into industries, industrial employment structures were stable, employment was guaranteed by the state, and sorting into industries was constrained as a result of limited job choice under central planning.

3 Data and Sample

3.1 Main Data: BASiD

The "Biographical Data of Social Security Insurance Agencies in Germany 1951-2009" (BASiD) combine data from the German Statutory Pension Insurance Scheme (RV), the Federal Employment Agency (BA) and the Institute for Employment Research (IAB). The basis of BASiD is the Sample of Insured Persons and their Insurance Accounts 2007 (*Versichertenkontenstichprobe, VSKT*) from the RV, which is merged with data from the BA and the IAB. The VSKT 2007 is a 1 percent sample of insured persons aged 15 to 67 at December 31, 2007 who are still alive and have an active pension insurance account. This

refers to persons who are covered by the pension insurance scheme but are not currently receiving pensions.[10] Insured persons contribute to their pension entitlements by means of employment, child care or elderly care, by receiving health insurance in case of long-term illness, or by receiving social benefits such as unemployment insurance.

The BASiD data have a rich panel structure. Up until 2007, they provide retrospective information on all spells and events which are relevant to the pension insurance, the unemployment insurance, or both. For the purposes of my study, the BASiD data have three major advantages. First, I can identify former GDR citizens in the data even if they moved to West Germany after the fall of the wall. As large proportions of young East German women migrated to West Germany during the 1990s (Hunt, 2006; Fuchs-Schündeln and Schündeln, 2009), this is an important feature. It enables me to include East German women who migrated to West Germany in my analysis. Second, the data provide accurate information on the month of birth of a woman's children, because childbearing entails contributions to pension entitlements. This is also true for births before 1989. Finally, sample sizes of BASiD are considerably larger than in alternative German panel data sources, which allows me to analyze the extensive and the intensive margin of fertility over a relatively long time period of 17 years.

The BASiD data are well suited to study how women's labor market situations impact childbearing decisions. By contrast, information on births is unfortunately not available for men. A potential concern in this context is assortative mating. However, I later add imputed control variables for the presence and labor market prospects of spouses and show that the results are not driven by assortative mating. Moreover, it is a priori reasonable to expect that the labor market situation of East German women had a significant impact on their childbearing decisions. East German women and mothers have traditionally had a high labor force attachment (Rosenfeld et al., 2004). Among East German mothers with minor children in 1996, only 7.7 percent of mothers with partners and 2.2 percent of single mothers reported transfers from current partners, former partners or other relatives as their main income source (the corresponding figures for West Germany are 50.2 and 9.8 percent). While West German mothers relied much more often on their partners' incomes, for East German mothers the most important income sources were their own wages and salaries, followed by public transfers (Federal Statistical Office, 2010, p. 26). Adler

[10] German pension data have high coverage rates (Richter and Himmelreicher, 2008), but, as is typically the case for German administrative data, the self-employed and civil servants are not included.

(1997) emphasizes the reluctance of East German women to economically depend on their partners. Providing qualitative evidence, she even argues that economic independence from men would be a prerequisite for East German women to have children.

3.2 Sample Selection

3.2.1 Identification of East Germans

The selection of the sample of East German women requires three steps. In the first step, East and West German women are distinguished from one another. Here, I identify East Germans by exploiting the fact that contributory periods in East and West Germany yield different pension entitlements. Specifically, the sample includes women who prior to 1989 had at least one spell related to work or training in the dual system of apprenticeship in the GDR. Additionally, I only include women for whom no such spells were reported in West Germany prior to 1989.[11] These criteria ensure that the selected women were integrated in the East German labor market and that the labor demand shock studied in this paper was relevant to them. Those women who migrated to West Germany after the fall of the wall are kept in the sample to rule out selective attrition resulting from migration to the West.[12]

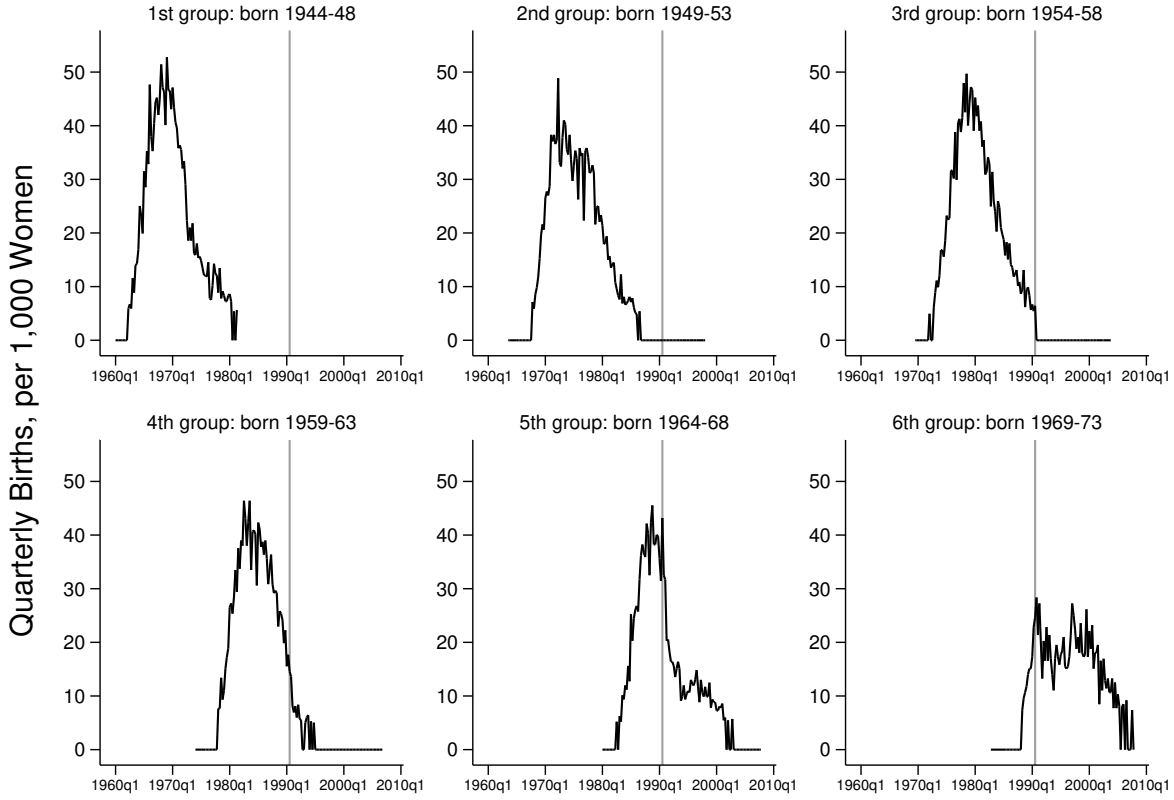
3.2.2 Cohort Choice

As a second sample selection criterion, I focus on cohorts born between 1959 and 1973. These cohorts were aged 17 to 31 at the end of 1990, and hence relatively young when the East German fertility decline became first apparent. The choice of these cohorts is motivated by the fact that in the former GDR women had their children at young ages, particularly in comparison with West German women (Huinink and Wagner, 1995; Goldstein and Kreyenfeld, 2011). Thus, the fertility of women who were older than 31 in 1990 (or born before 1959) was predetermined with regard to the labor demand shock. To illustrate this, Figure 5 shows the number of quarterly births per 1,000 East German

[11] I am grateful to Dana Müller whose Stata-routine I use to distinguish between East and West German spells. See also Grunow and Müller (2012).

[12] Focusing on migration from East to West Germany, the literature (Hunt, 2006; Fuchs-Schündeln and Schündeln, 2009; Arntz et al., 2014; Prantl and Spitz-Oener, 2014) neglects migration abroad. This corresponds to data provided by the Federal Statistical Office. Between 1991 and 2007, relative to a GDR population of around 16 Million and not even accounting for return migration, only 153,752 Germans (1 percent) migrated from East Germany to foreign countries. For 1990 such data do not exist.

Figure 5: Quarterly Number of Births per 1,000 East German Women, By Six Cohort Groups



Source: BASiD; based on women who, prior to November 1989, worked or did apprenticeship training in the GDR but not in West Germany, and including women who migrated to West Germany after November 1989. $N = 22,572$. Due to data protection, less than 20 absolute births per quarter had to be censored and are displayed here as zero births. The vertical line stands for the third quarter of 1990. See Section 3.1 for details on the data and Section 3.2.1 for details on the identification of East Germans.

women born between 1944 and 1973, based on the BASiD data and for six different cohort groups. The two oldest groups, born between 1944 and 1953, had already completed their fertility before 1989 (groups 1 and 2). Similarly, among the third group of women born between 1954 and 1958, only a small fraction gave birth immediately before the fall of the wall, and this fraction went down to almost zero after 1990 (group 3). By contrast, Figure 5 demonstrates that the fertility decisions of women born between 1959 and 1973 were impacted by the fall of the wall (groups 4-6). This is most drastically the case among the youngest group of women who were aged 17 to 21 at the end of 1990 (group 6). The collapse of the GDR caused these women to have children at higher average ages compared with their older counterparts. The fall of the wall also led to a drop in fertility among women born between 1959 and 1968 (groups 4 and 5). Therefore, I include groups

4 to 6, who were aged 17 to 31 at the end of 1990, in the main sample.

3.2.3 Final Selection Step

The final step in the selection of the sample arises from a peculiarity of the BASiD data. In this step, the sample is additionally restricted to women who on January 1st, 1991, worked in East Germany and have non-missing industry information. January 1st, 1991, which is about three months after the German reunification on October 3rd 1990, is the first point in time for which industry information is known for a subsample of East Germans. The final sample consists of 4,234 women.

To explain the final selection step, after reunification the East German labor administration was integrated into the West German administration as part of a complex process (Schmid and Oschmiansky, 2007). For some firms industry information was reported already in 1991, whereas it is available for all East German firms from 1992 onwards. It is, however, crucial to infer industry information for the earliest point in time possible, since industries are only observed for persons who work. Unemployment rates rapidly increased from 9.5 percent among East German women in mid-January of 1991 to 20.5 percent a year later (Federal Employment Agency, 2015) and a significant fraction of workers likely changed industries after they became unemployed. Figure 3 shows that some employment losses occurred already in the first year after the fall of the wall, but the most pronounced employment losses took place after 1991.[13] Using industry information from January 1st, 1991, thus seems to be a good approximation of industries prior to the fall of the wall. As far as the fertility analysis is concerned, it is also reasonable to exclude the year 1990. Given that fertility can respond to economic conditions only with a time lag of nine months, during 1990 births were largely independent of the fall of the wall in November of 1989.

It needs to be emphasized that industry information for January 1st, 1991, is missing for a large fraction of women who work on this day (there are 4,234 employed women with non-missing and 4,783 employed women with missing industry information). It is important that the missing information on industries is driven by firms not yet integrated into the social security system rather than by workers not reporting their industry. Workers sorted into firms prior to reunification and independently of later labor demand conditions.

[13]Note that the change in employment until 1990 in Figure 3 refers to the end of November, which is one month before January 1st, 1991.

Nevertheless, the fact that information on industry is missing for a large fraction of firms requires further methodological investigation. To begin with, the representativeness of the sample distribution over industries is a concern. For example, at the sectoral level, the sample share of women working in mining is suspiciously large and the sample share working in services appears to be too small. To correct for these discrepancies between the sample distribution and the population distribution, I use the Microcensus of 1991 as an auxiliary data source. In the Microcensus, I identify women born between 1959 and 1973 who live in East Germany and compile their distribution over industries.[14] For each woman from an industry j, I then calculate the following simple post-stratification weight:

$$w_j = \frac{share\_Microcensus_j}{share\_sample_j}, \tag{2}$$

where $share\_Microcensus_j$ is an estimate of the population share of this industry. Throughout the paper, I apply w_j as probability weights.[15]

In addition, it is possible that the firms included in the analysis are not a random sample. For example, larger firms might have been more likely to report their industry affiliation earlier. However, in a robustness test I control for a set of firm-level characteristics and show that these characteristics do not confound the impact of the labor demand shock on fertility.

Finally, there might be selective sample attrition because some women have already lost their jobs by January 1st, 1991. 8 percent of women are unemployed on this day. This is similar in magnitude to the official unemployment rate of 9.5 percent for East German women later in January of 1991. I discuss this specific group in more detail in Section B.2 in the Appendix. It turns out that the initially unemployed women are indeed a selective subgroup, since they include a high share of mothers with young children who were born immediately before the fall of the wall. At the same time, this corresponds to the intuition behind the findings for the main analysis, because it indicates that in these times of elevated uncertainty, childbearing was related to the risk of job loss.

[14] I use the Scientific Use File, which is a 0.7 percent representative sample of the population. Respondents are by law required to participate in the survey. The Microcensus was conducted in April, when unemployment had already increased strongly. Therefore the distribution is based on the current industry of employed women and the last industry of non-employed women.

[15] Weighted and unweighted results are consistent in a qualitative sense, but effects in unweighted estimations tend to be somewhat smaller and their statistical significance tends to be weaker.

4 Baseline Estimation

For woman i from industry $j = 1, ..., 48$ and years t, the following panel regression is estimated:

$$Y_{itj} = \beta_0 + \beta_1 RES_{j,89} + X'_{it}\beta_2 + X'_i\beta_3 + \gamma_t + \epsilon_{itj}, \tag{3}$$

where t refers to three separate time periods containing years 1991-94, 1995-99, or 2000-07. $RES_{j,89}$ is the measure for the intensity of the unexpected and exogenous labor demand shock as derived in Section 2.3. γ_t are time fixed-effects. Since the treatment is time invariant within industries, throughout the paper robust standard errors are clustered at the industry level (the number of clusters is 48).

I first establish that the RES demand shock impacted different labor market outcomes, which are discussed in the next section. I then analyze the impact on fertility, where Y_{itj} is a dummy variable equal to one if a woman gave birth in a given year.

To assess how the impact of the demand shock evolves over time, I focus on three distinct time periods. The first period of years 1991 to 1994 refers to the short term and includes the years during which aggregate East German fertility plummeted. The second period captures medium-term effects. It includes years 1995 to 1999 during which aggregate East German fertility increased again (for these aggregate fertility trends recall Figure 1 above). Finally, the third period is defined as years 2000 to 2007 and refers to the long term. The main parameter of interest, β_1, measures the average annual impact of the unexpected and exogenous labor demand shock during the respective time periods. β_1 thus summarizes annual effects in a way that is straightforward to interpret. I later augment the baseline specification to investigate dynamics in individual years and to assess how the effects accumulate over time.

Across specifications, time variant control variables are age and age squared. The other control variables are time constant. These include two qualification dummy variables referring to women who in 1991 had no formal qualification and to women who in 1991 had completed apprenticeship training, respectively. Women who in 1991 had graduated from university are the reference category. Further control variables are dummy variables for the number of children a woman had prior to 1991 (one child, two children and three or more children), and two dummy variables for whether a woman worked in a large city or very large city at the beginning of the 1990s, because fertility patterns as well

Table 1: Summary Statistics

Panel A: Labor Demand Shock			
RES	-.204	P90, P10 interval	[.583, -.911]
	(.993)	P75, P25 interval	[.324, -.391]

Panel B: Outcome variables			
(a) Annual averages	1991-94	1995-99	2000-07
Unemployment spell in t (0/1)	.274	.364	.299
Unemployment in t (months)	2.431	2.101	1.505
	(4.057)	(3.914)	(3.496)
Industry Change in t (0/1)	.182	.120	.059
Migration to West G. in t (0/1)	.032	.016	.022
Birth in t (0/1)	.040	.041	.019
1st Birth in t (0/1)**	.059	.051	.019
Higher-Order Birth in t (0/1)	.018	.021	.012
(b) End of a given year	1994	1999	2007
End of Period Childlessness (0/1)**	.766	.509	.355
Total Number of Births (#)	.163	.367	.522

Panel C: Control variables			
Age (here: in 1991)	25.68	2 children bf. 91 (0/1)	.264
	(4.11)	3 children bf. 91 (0/1)	.034
Low Qualification, 91 (0/1)	.255	Privileges in GDR (0/1)	.057
Med. Qualification, 91 (0/1)	.706	Appr. Training 91 (0/1)	.062
High Qualification, 91 (0/1)	.039	Working in large city, 91 (0/1)	.099
0 children bf. 91 (reference) (0/1)	.386	Working in very large city, 91 (0/1)	.141
1 child bf. 91 (0/1)	.316		
Sample size; i.e., individuals observed each year (1991-2007)			4,234

Notes: Panel A refers to the industry a woman worked in on January 1st, 1991. The sample is balanced with $N = 4,234$ individuals. In Panel B, "total number of births" pertains to births between 1991 and the end of a given year. Outcome variables marked by ** refer only to women who at the beginning of 1991 were still childless.

as industry structures might differ in rural versus urban areas. Another dummy variable for persons who in the GDR were entitled to privileged pensions serves as a proxy for closeness to the regime. This variable addresses the concern that for some workers labor market trajectories after reunification were shaped by their prior regime closeness rather than their initial industry. A final dummy variable captures whether a woman was still in apprenticeship training in 1991. A more detailed description of the definition and in some cases of the imputation of variables is provided in Appendix Table A2. Summary statistics are shown in Table 1.

5 Analysis of Labor Market Outcomes

I now establish that the RES demand shock generated exogenous variation in individuals' labor market outcomes (Table 2).[16] The first outcome variable is the incidence of unemployment, which is captured by a dummy variable equal to one if a women experienced an unemployment spell in a given year. The second outcome variable is the duration of unemployment expressed in months per year, which is set to zero for women without any unemployment spell. Third, industry changes are defined as a dummy variable equal to one if a woman started to work in a new industry in a given year. Finally, migration to West Germany is accounted for by a dummy variable equal to one in the year migration took place.

To facilitate the interpretation of results, I compare estimated effects for women who initially worked in industries subject to a severe labor demand shock with estimated effects for women who initially worked in industries which were less severely hit. Specifically, I compare women at the 90th percentile of the RES measure (which is 0.58 and stands for a severe labor demand shock) with women at the 10th percentile (which is -0.91 and implies that the labor demand shock was less severe). In terms of actual industries, the 90th percentile coincides with textile manufacturing, whereas the 10th percentile corresponds to lower-skilled services including cleaning and laundry workers. Throughout the paper, tables include rows labeled "P90 vs P10." In these rows, the difference in estimated effects between the 90th and the 10th percentile is reported.

As shown in Table 2, the impact of the RES demand shock on unemployment is positive, significant, and it persists over time. In the short term of years 1991 to 1994, the implied differential increase in the incidence of unemployment is 7.0 percentage points on average per year when comparing a worker at the 90th percentile with a worker at the 10th percentile (Table 2, panel a, column 1). The average implied increase in unemployment duration per year is 0.48 months (panel b, column 1). When additional controls are added, the effect on unemployment is only slightly smaller (panels a and b, column 2). Over time, the effect decreases, but it remains positive even in the long term (panels a and b, columns 5 and 6). Besides unemployment, the RES demand shock also impacted mobility across industries. For the short-term period, a worker at the 90th percentile is

[16] Note that I now refer to the "RES demand shock" or the "RES measure" to distinguish the industry-level variation from the aggregate demand shock.

Table 2: Relative Excess Supply (RES) and Various Labor Market Outcomes, OLS Estimates (Panel Regressions)

	(1)	(2)	(3)	(4)	(5)	(6)
	1991-94		1995-99		2000-07	
(a) Dep. Var.: Unemployment Spell in t (0/1)						
RES	.047***	.040***	.039***	.033***	.021**	.017**
	(.015)	(.013)	(.009)	(.008)	(.008)	(.007)
P90 vs P10	.070	.059	.058	.050	.031	.025
(b) Dep. Var.: Unemployment Duration in t (months)						
RES	.319***	.264***	.286***	.240***	.174**	.140**
	(.101)	(.087)	(.057)	(.047)	(.072)	(.059)
P90 vs P10	.475	.394	.426	.358	.260	.208
(c) Dep. Var.: Industry Change in t (0/1)						
RES	.022***	.021***	.015***	.015***	.008***	.007***
	(.008)	(.008)	(.005)	(.005)	(.002)	(.002)
P90 vs P10	.033	.032	.023	.022	.012	.010
(d) Dep. Var.: Migration West in t (0/1)						
RES	-.002	-.002	.000	.000	.002**	.002***
	(.002)	(.002)	(.001)	(.001)	(.001)	(.001)
P90 vs P10	-.003	-.003	.000	.000	.003	.003
Age controls only	yes	-	yes	-	yes	-
Main controls	-	yes	-	yes	-	yes
Time FEs	yes	yes	yes	yes	yes	yes
N (each year)	4,234	4,234	4,234	4,234	4,234	4,434

Notes: Each coefficient is from a separate regression. "Age controls" are age and age squared. "Main controls" include age and its square as well as dummy variables for 1991 qualification, for children born prior to 1991, for living in a large or very large city in 1991, for GDR regime closeness, and for apprenticeship training in January 1991. Robust standard errors clustered at the industry level are in parentheses; ***, **, * refers to significance at the 1, 5, and 10 percent level, respectively. "P90 vs P.10" columns report the differences in the estimated effects between the 90th and the 10th percentile; which is equal to "coefficient" multiplied by 0.58-(-0.91)=1.49. Results are weighted using post-stratification weights as explained in Section 3.2.3.

estimated to be around 3.3 percentage points more likely to change industries in a given year than a worker at the 10th percentile (panel c, columns 1 and 2). Again, this effect decreases over time but remains positive.[17] Finally, there is no systematic association between the RES measure and the decision to migrate to West Germany (panel d of Table 2). This implies that the results on fertility presented below are not confounded by

[17]Participation in retraining programs could be another adjustment mechanism, but information on such programs is not available before 2000. Wage effects are also neglected, because after reunification East German wages were determined as part of a political process influenced by West German unions. Thus, wages exceeded market equilibria (Krueger and Pischke, 1995).

migration to West Germany.[18]

The impact of the RES demand shock on unemployment and mobility across industry is economically significant. This is particularly true if one keeps in mind that the RES measure exploits only one dimension of the labor demand shock. Moreover, it impacted unemployment and mobility across industries over a relatively long time period.

6 Baseline Fertility Analysis

6.1 Annual Births

Based on the previous section, it follows that for women who initially worked in industries subject to a relatively severe labor demand shock, labor market outcomes were less favorable and stable compared with their counterparts who initially worked in industries less strongly affected. I now assess how the demand shock impacted fertility and regress annual births on the RES demand shock measure. A distinction is again made between the short term, medium term, and long term. The results are reported in Table 3.

Throughout the 1990s, the RES demand shock had a positive impact on annual births. During the short-term period of years 1991 to 1994, East German women more severely impacted by the labor demand shock had relatively more children than their counterparts who were less severely impacted. Again, I compare the two extremes of women at the 90th percentile of the RES measure with women at the 10th percentile of the RES measure. Between those extremes, in the short-term period, the difference in the annual likelihood of having a child was 0.53 percentage points higher for women more severely impacted by the demand shock (Table 3, panel a, column 1). This effect is robust when further control variables are added (panel a, column 2). In the medium term of years 1995 to 1999, the RES demand shock had an even larger positive effect: During these years, women more severely impacted by the demand shock (90th percentile) were 0.76 percentage points more likely to have a child in a given year than women less severely impacted by the demand shock (10th percentile; see panel a, columns 3 and 4). Thus, throughout the 1990s, the RES labor demand shock had a positive impact on fertility. This positive effect is economically significant. To put it into perspective, the average annual birth rate during these years was 4 percent (Table 1). Finally, in the long term of years 2000 to

[18] I will investigate migration to West Germany in greater detail in Section 6.3.1.

Table 3: RES and Annual Births, OLS Estimates (Panel Regressions)

	(1)	(2)	(3)	(4)	(5)	(6)	N (each year)
	1991-94		1995-99		2000-07		
(a) Dep. Var: Birth in t (0/1)							
RES	.0036***	.0037***	.0051***	.0051***	-.0010	-.0009	4,234
	(.0009)	(.0012)	(.0007)	(.0007)	(.0009)	(.0011)	
P90 vs P10	.0053	.0055	.0076	.0076	-.0015	-.0013	
(b) Dep. Var: First Birth in t (0/1)							
RES	.0053**	.0053*	.0046***	.0042***	-.0035***	-.0033***	1,597
	(.0022)	(.0029)	(.0008)	(.0009)	(.0007)	(.0008)	
P90 vs P10	.0079	.0079	.0068	.0063	-.0052	-.0049	
(c) Dep. Var: Higher-Order Birth in t (0/1)							
RES	.0014*	.0016**	.0034***	.0034***	.0005	.0005	4,234
	(.0008)	(.0007)	(.0008)	(.0008)	(.0008)	(.0009)	
P90 vs P10	.0021	.0023	.0050	.0051	.0007	.0007	
Age cont. only	yes	-	yes	-	yes	-	
Main controls	-	yes	-	yes	-	yes	
Time FEs	yes	yes	yes	yes	yes	yes	

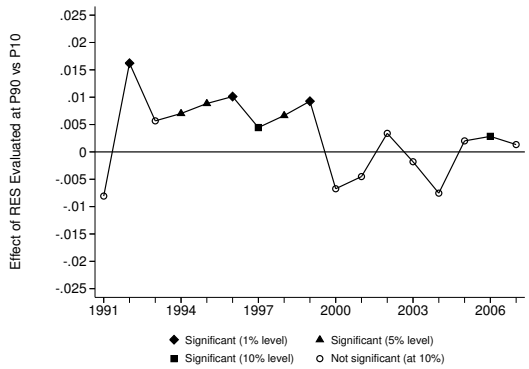
Notes: Explanations are analogous to Table 2. The dependent variables are equal to one whenever a woman gave birth in a given year and equal to zero otherwise. In Panel a, this refers to all births, in Panel b to first births, and in Panel c to higher-order births. Results on first births are for women who on January 1st, 1991 were still childless.

2007, point estimates suggest a negative, though statistically insignificant, impact of the RES demand shock on annual births: According to point estimates, women more severely impacted by the demand shock were on average 0.13 percentage points less likely to have a child in a given year (panel a, column 6).

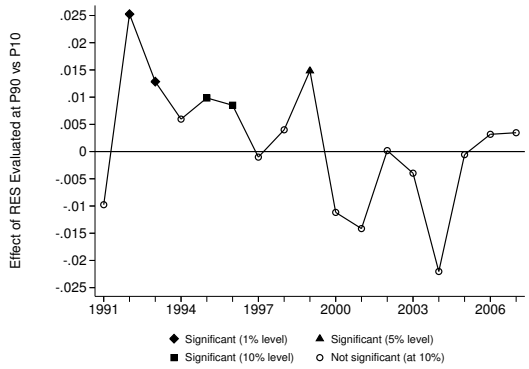
To assess whether these results differ by birth order, I separately regress first births and higher-order births on the RES measure (Table 3, panels b and c). With regard to first births, the RES demand shock had a pronounced positive effect throughout the 1990s (panel b, columns 1-4), but in the long-term period, this effect turns negative (panel b, columns 5-6). Thus, the RES demand shock appears to have impacted the timing of first births, and some of the effects that are found for the 1990s are later compensated for.

As far as higher-order births are concerned, there is a positive impact of the RES demand shock throughout the short and medium terms (panel c, columns 1-4). In the long term, point estimates are positive but statistically insignificant (panel c, columns 5-6), which indicates that shifting across time mattered less for higher order births. This suggests a

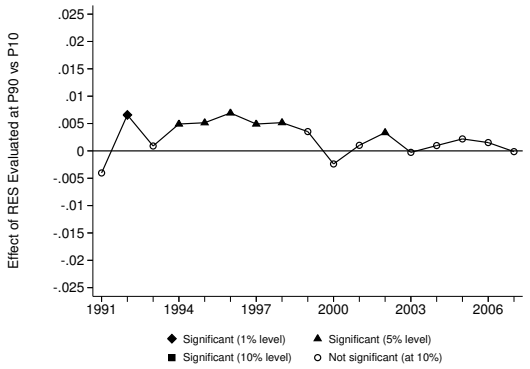
Figure 6: RES and Annual Births, Graphical Illustration of OLS Estimates (Panel Regression), by Year and Birth Order



(a) Dep. Var: Birth in t (0/1); all women



(b) Dep. Var: 1st birth in t (0/1); initially childless women only



(c) Dep. Var: Higher order birth in t (0/1); all women

Notes: The underlying regression models are based on data from all 17 years (1991 through 2007) combined. In panel a, the outcome variable is a dummy variable equal to one whenever a woman had a child in a given year and equal to zero otherwise. Outcome variables in panels b and c are analogously defined but capture 1st births and higher-order births, respectively. The effects refer to the interaction of the RES-variable with a time trend; and are evaluated at the 90th versus the 10th percentile. Included are time fixed effects and main control variables as used before (see Table 2).

persistent impact of the RES demand shock on higher-order births.

Finally, in Figure 6 I investigate the dynamics of the fertility effects for individual years and include data from all seventeen years combined. I interact the RES variable with a time trend, and again evaluate the estimated effects at the 90th versus the 10th percentile of the demand shock measure. Panel (a) of Figure 6 reveals that the impact of the RES demand shock on fertility occurred with a time lag of one year and was positive from 1992 up until 1999. Starting in 2000, the annual effects fluctuate around zero and are negative, though statistically insignificant, for several years. In panels (b) and (c) I repeat the same analysis by birth order. Again the graphs suggest that the RES demand shock impacted the timing of first births, whereas it had a persistently positive effect on higher-order

births. The persistence of the effects is assessed in more detail in the next section.

6.2 Persistence of the Effects over Time

So far, it can be concluded that the industry-level demand shock had a positive impact on annual births throughout the 1990s. A subsequent question is whether this pattern accumulates to persistent differences in fertility. Alternatively, the impact of the RES demand shock on fertility could vanish in the long term. This would imply that the demand shock only impacted the timing of childbearing.

To investigate this further, I adjust the empirical model to only include one cross-section at a time, namely the cross-sections of 1994, 1999, or 2007. The choice of these years is motivated as before, when I defined 1994 as the end of the short term, 1999 as the end of the medium term, and 2007 as the end of the long term.[19] The estimations contain all main control variables. For woman i, industry $j = 1, ..., 48$, and year t equal to 1994, 1999, or 2007, the cross-sectional regression model has the following form:

$$Y_{itj} = \beta_0 + \beta_1 RES_{j,89} + X'_{it}\beta_2 + X'_i\beta_3 + \epsilon_{itj}. \tag{4}$$

The first outcome Y_{itj} assesses the extensive margin of fertility. For this purpose, the sample is restricted to initially childless women. Y_{itj} is a dummy variable equal to one whenever a woman is still childless at the end of a given year, which is regressed on the RES measure. The corresponding results are displayed in panel (a) of Table 4. As one would expect based on the previous results, over time the RES demand shock decreases the likelihood that a woman is still childless. At the end of 1999, a woman at the 90th percentile of the RES measure is around 6 percentage points less likely to still be childless than a woman at the 10th percentile (Table 4, panel a, column 2). However, up until the end of 2007, the difference in childlessness becomes smaller and is no longer statistically significant (panel a, column 3). This implies that with regard to the decision of initially childless women to become mothers, the demand shock mostly impacted the timing of births. Those women more severely impacted by the demand shock had their first children earlier after they experienced the shock; whereas those less severely impacted postponed

[19] The year of 2007 is exogenously determined by the right censoring of the data. Admittedly, the choice of 1994 and 1999 as cutoffs is somewhat arbitrary. However, the aim is to analyze how the effects accumulate over time and the chosen approach is suitable to reveal this.

Table 4: RES and Total Number of Births/End of Period Childlessness, OLS Estimates (Cross-Sectional Regressions)

	(1) 1994	(2) 1999	(3) 2007
(a) Dep. Var: End of Period Childlessness (0/1)			
RES	-.0215*	-.0422***	-.0162
	(.0119)	(.0110)	(.0112)
P90 vs P10	-.0320	-.0629	-.0241
N	1,597	1,597	1,597
(b) Dep. Var: Total Number of Births (#)			
RES	.0146***	.0406***	.0334***
	(.0045)	(.0044)	(.0100)
P90 vs P10	.0217	.0605	.0497
N	4,234	4,234	4,234
Main controls	yes	yes	yes

Notes: Each regression is based on a single cross-section referring only to the year of 1994, or 1999, or 2007, respectively. Regarding "Total Number of Births," the dependent variable is the number of children born 1991-94 (column 1), born 1991-99 (column 2), and born 1991-2007 (column 3). "End of Period Childlessness" refers to a dummy variable equal to 1 if a woman is still childless at the end of a given year; here the sample is restricted to women who were childless at the beginning of 1991. Main controls and standard errors are defined as in Table 2.

childbearing more and became mothers at a later point in time.

By contrast, there is a persistent positive impact on the total number of children born even in the long term. This can be seen in panel (b) of Table 4, which refers to the intensive margin of fertility. The sample now includes all women. Here, the outcome variable Y_{itj} is the total number of children a woman had between the beginning of 1991 and the end of 1994, 1999, and 2007, respectively; which is again regressed on the RES measure. By the end of 1999, women experiencing a more severe labor demand shock have around 0.061 births more on average than women experiencing a less severe demand shock (panel b, column 2). This effect is economically significant. To put it into perspective, for years 1991 to 1999 the average number of children born per woman is 0.367 (see Table 1). Even by the end of 2007, there still is a persistent effect of the demand shock on the total number of children born. Between years 1991 to 2007, women more severely impacted by the demand shock have 0.050 children more on average (panel b, column 3). Again, this effect is economically meaningful, since the average number of births between 1991 and

2007 is 0.522 (Table 1).

6.3 Robustness of the Composition Effect

In the two previous sections, I have established that the RES demand shock altered the composition of mothers. Throughout the 1990s, those women more severely affected by the shock had relatively more children than their counterparts who were less severely affected. With regard to first births, this composition effect concerned the timing of childbearing but became smaller in the long term. With regard to the total number of births, the composition effect persisted over a period of seventeen years. I now investigate the robustness of these results. For the sake of brevity, most of the corresponding tables are displayed in the Appendix.

6.3.1 Migration to West Germany

To begin with, it is of interest how the results are impacted by migration to West Germany, which played an important role for the cohorts studied. Thus, I split the sample between those who stayed in East Germany and those who migrated to West Germany at some point before 2007 (see Appendix Table A4). The positive effect of the RES demand shock on births during the 1990s is confirmed for migrants and non-migrants. Yet the effects are stronger among non-migrants and persist in the long term only for this group. This is not surprising, as migration to West Germany was likely associated with new economic opportunities. For migrants it may have become less relevant which industries they were employed in initially. Nevertheless, to comprehensively assess the effects of the demand shock, I continue to include migrants in the following analysis.

6.3.2 Child Care and Regional Spillover Effects

Next, I investigate whether the results are driven by regional spillover effects. The availability of childcare could be relevant in this context. The GDR was a state incentivizing and even requiring full-time employment of mothers through comprehensive public child care (Rosenfeld et al., 2004). Post-reunification, public child care became the responsibility of local municipalities. Its availability declined in East Germany, even though it was still markedly higher than in West Germany (Kreyenfeld, 2003, p. 310). However, in municipalities more severely impacted by the demand shock, one would expect the

availability of child care to be relatively lower. This should decrease regional birth rates and lead to underestimation of the effect of the RES demand shock on the composition of mothers. While the reduced availability of child care is one plausible reason for the East German fertility decline as a whole; it is not a plausible cause for the finding that women more severely impacted by the demand shock had more children on average.

Independent of child care, it could still be the case that the RES demand shock impacted fertility through the channel of regional spillover effects. Such spillover effects would be a consequence of the labor demand shock, but one that would indirectly operate through differential changes in regional economic prospects. To assess whether this channel impacts the results, in Appendix Table A5 I include detailed fixed effects accounting for the municipalities women lived in at the beginning of the 1990s (*Kreise* in German). The inclusion of these fixed effects does not change the results significantly. Point estimates for results on childlessness are very similar to those presented earlier, and point estimates for the total number of children born become only slightly smaller. This suggests that the channel of regional spillover effects played only a small role in explaining the results.

6.3.3 Missing Information on Industry

My sample includes women whose firms were integrated into the social security system and reported their industry affiliation already at the beginning of 1991. A potential concern is that firm-level characteristics, such as firm size, determined which firms were more likely to report their industry affiliation earlier. I therefore analyze whether firm-level characteristics confound the results. In Appendix Table A6, I control for the set of 1991 firm-level characteristics included in the BASiD data. These characteristics are the number of employees working at a firm and its square, the share of female workers, a dummy variable capturing whether a firm employed more than 5 percent of low-skilled workers, the share of workers younger than 30, and the share older than 50. Some of the firm-level characteristics are correlated with fertility. In particular, fertility was higher among women employed in female-dominated firms or in firms with a relatively large share of low-skilled workers. Importantly, the impact of the RES demand shock on fertility is qualitatively and quantitatively robust to the inclusion of the firm-level control variables. This indicates that the potential selectivity of firms does not confound my results.

6.3.4 Assortative Mating

The previous analysis has focused on women while neglecting their spouses. This could be a problem if the labor demand shock experienced by women was correlated in systematic ways with employment prospects of their spouses. Because the East German economy was characterized by large firms, it is implausible to assume that assortative mating played no role. Drawing on the Microcensus and focusing on East German women who were married in 1991, I find that there is indeed some evidence of assortative mating. This is shown in Appendix Figure A3, where, at the industry level, average RES-values for husbands are regressed on women's RES-values. On average, the RES measure for husbands increases by 0.12 if women's RES-values are increased by a factor of 1 (panel a).

However, microeconomic theory as well as several recent empirical studies (Lindo, 2010; Black et al., 2013; Autor et al., 2017; Schaller, 2016), suggest that negative shocks to men's labor market prospects merely operate through the income effect and hence decrease fertility. Given that these insights apply to the East German context, the presence of assortative mating should lead to an underestimation of the positive effect the RES demand shock had on the composition of mothers.

To the extent possible, I test these expectations empirically. In Table 5 I include control variables for husbands. I again relied on the German Microcensus. Using this auxiliary data source, at the industry level I compiled the fraction of women living with a spouse in 1991. Additionally, for married women I inferred the 1991 RES-value of their spouses. At the industry-level, I then calculated the average RES-value of spouses. I merged the two industry-level variables - the fraction living with a spouse in 1991 and average 1991 RES-values of spouses - with the main BASiD data.[20] These control variables were imputed for 1991 only, because partnership formation in later years could have been influenced by the labor demand shock. It turns out that average RES-values of spouses have an insignificant effect in the short-term period. In the medium- and long-term periods, point estimates suggest that average RES-values of spouses operate in the expected direction. This is tentative evidence that indeed negative shocks to male employment prospects decrease fertility. As a result, the impact of female labor market prospects, as captured by the RES demand shock experienced by women, increases. By 2007, women at the 90th percentile of the demand shock are now estimated to have had 0.099 children more on average than

[20]This is inspired by Perry (2004) and Raute (2017) who similarly impute information about spouses.

Table 5: RES and Total Number of Births/End of Period Childlessness, OLS Estimates (Cross-Sectional Regressions), Including Imputed Control Variables for Labor Market Prospects of Spouses

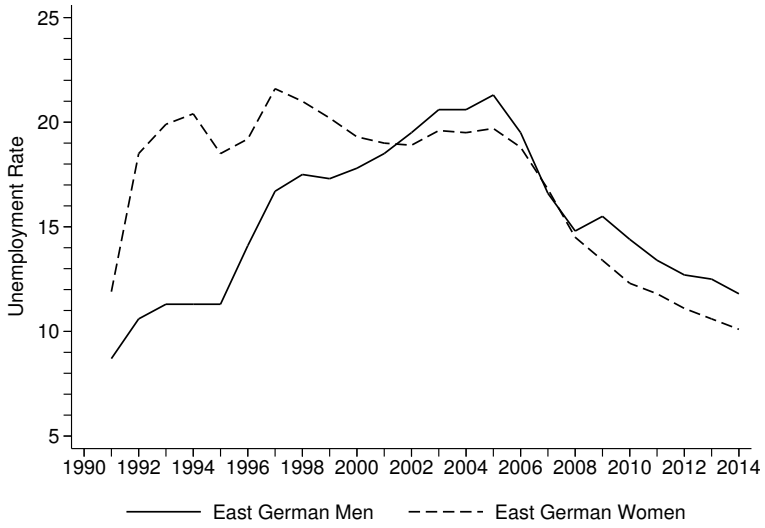
	(1) 1994	(2) 1999	(3) 2007
(a) Dep. Var: End of Period Childlessness (0/1)			
RES	-.0182	-.0458***	-.0308**
	(.0156)	(.0153)	(.0150)
P90 vs P10	-.0271	-.0683	-.0459
Mean RES-value of spouses	-.0059	.0382	.1182
	(.0855)	(.1062)	(.0972)
P90 vs P10	-.0025	.0161	.0497
Share living with spouse	-.3113**	-.1453	-.0455
	(.1458)	(.2179)	(.1891)
N	1,596	1,596	1,596
(b) Dep. Var: Total Number of Births (#)			
RES	.0131*	.0421***	.0664***
	(.0067)	(.0080)	(.0123)
P90 vs P10	.0195	.0628	.0990
Mean RES-value of spouses	.0110	-.0097	-.2655**
	(.0419)	(.0616)	(.1179)
P90 vs P10	.0046	-.0041	-.1115
Share living with spouse	.0199	-.0642	-.1151
	(.0926)	(.1254)	(.1682)
N	4,229	4,229	4,229
Main controls	yes	yes	yes

Notes: As in Table 4, but adding imputed control variables for husbands. These variables are based on the Scientific Use File of the 1991 Microcensus, from which women born 1959 to 1973 living in East Germany were selected. For these, the share living with a spouse as well as average RES-values of spouses (calculated for married women only) was imputed at the industry level. These two imputed variables were merged to the main sample according to a woman's initial industry.

their counterparts at the 10th percentile (panel (b) of Table 5, column (3)). The effect on long-term childlessness also becomes larger in magnitude. At the end of 2007, women experiencing a more severe labor demand shock are on average 4.6 percentage points less likely to still be childless (panel (a), column (3)).

Why should male labor market prospects only impact fertility in the medium- and long-

Figure 7: Unemployment Rates in East Germany, 1991-2015, By Gender



Source: Federal Employment Agency (2015). Unemployment rates refer to June 30th of a given year and are defined relative to the dependent labor force.

term periods? A possible reason is that East German men were most strongly hit by the labor demand shock only from the mid-1990s onwards. After 1995 and up until 2006, male unemployment rates increased to dramatic levels. By contrast, unemployment rates among East German women reached dramatic levels already in 1992 (see Figure 7). Particularly in the medium- and long-term periods, not accounting for male employment prospects seems to lead to a downward bias of the effect of female labor market prospects on the composition of mothers.

Average RES-values are only an imperfect approximation of the labor demand shock spouses experienced. For example, this proxy variable could capture both the labor market situation of actual spouses of married women and the labor market situation of potential partners of single women. A comprehensive analysis of how men's labor market situations impact childbearing decisions is beyond the scope of this paper. At the very least, however, the findings presented in Table 5 cannot be reconciled with the view that the presence of assortative mating drives the positive effect the demand shock had on the composition of mothers.

6.3.5 Industries with Strong Employment Growth

Four East German industries were subject to a strong positive labor demand shock.[21] Because these industries were small in the GDR, their employment growth after reunification was pronounced. A natural question is how this impacts my results.

In Appendix Table A7 I exclude women who were initially employed in one of these industries. The patterns of the results are similar to those presented earlier. But an important difference is that the persistence of the effect on the total number of children born until 2007 becomes smaller. It appears that for the majority of women the shock mostly impacted the timing of births. To investigate this further, in Appendix Table A8 I include imputed control variables for spouses (see the previous section for details). Now the effect on the total number of children born becomes persistent in the long term as well.[22] Assortative mating seems to exert a relatively strong downward bias, potentially because assortative mating plays a stronger role when the industries with particularly favorable employment growth are excluded (panel (b) of Appendix Figure A3). Therefore, the results remain robust, but the persistence of the effect on the total number of children born now depends on whether I account for assortative mating.

6.3.6 Placebo Test based on Previous Births

Finally, even though the negative labor demand shock was unexpected and exogenous, a natural concern is the potential impact of systematic and unobserved differences associated with the intensity of the labor demand shock. This would be a problem if these characteristics, and not the demand shock, caused differences in fertility. As a first approach to addressing this objection, in panel (a) of Appendix Table A9 I display the results of a placebo test. The test investigates prior births which are predetermined with regard to the labor demand shock. The outcome variables are a dummy capturing childlessness at the end of 1990 as well as the total number of children born up until this point. When these predetermined variables are regressed on the RES measure, the coefficients are statistically insignificant. The point estimates are moreover relatively small in comparison to the mean of the outcome variables. This suggests that the labor demand shock was not

[21] These industries include the finance and insurance industries, "printing & reproduction," and "finishing trade" (see Figure 4 above).

[22] In this specification with husband control-variables, coefficients are imprecisely estimated (Appendix Table A8, columns 1-3), but the precision increases with the inclusion of firm-level control variables (columns 4-6).

associated with unobservable factors causing systematic differences in childbearing.[23] To address the concern of unobservable characteristics more rigorously, in the next section the effects are evaluated against a control group of older cohorts.

7 Composition Effect versus Level Effect

In the previous section I established the robustness of the composition effect: Throughout the 1990s, East German women more severely impacted by the labor demand shock had relatively more children than their counterparts who experienced less severe labor demand shocks. That is, the substitution effect dominated the income effect in determining the composition of mothers. An interpretation that rationalizes this finding is that after reunification there was a high degree of general economic uncertainty in East Germany. Up until 2006, the unemployment rates of East German men and women were at dramatic levels (see Figure 7). The prevalence of unemployment was also high among the cohorts of women included in my analysis (see Table 1). Those women with relatively better labor market prospects might have been less willing to put their jobs at further risk. As a consequence, they had relatively fewer children.

After reunification, East Germans were integrated into the West German social security system (for details see Schmid and Oschmiansky, 2007). Unemployment benefits and other benefits were relatively generous, at least in comparison to benefit levels in the United States, or in comparison to the situation in Germany today. This might have been one institutional factor explaining why women with less favorable labor market outcomes had relatively more children.

The fact that the RES demand shock had a positive impact on the composition of mothers is puzzling given that East Germany experienced an unprecedented decline in aggregate fertility. In the analysis below, I resolve this apparent contradiction by demonstrating that the labor demand shock impacted both the level of fertility and the composition of

[23] In panels (b) to (d) of the sample table, I repeat the placebo test but split the main sample into three different cohort groups. Again, I find that there are no systematic differences in predetermined births. In particular, point estimates for women born 1959-63 and 1964-68 have opposing signs (these cohorts are of particular interest because they were old enough such that a significant share of women had children prior to reunification). The only statistically significant effect concerns the pre-determined childlessness of women born 1964-68, but in this case, women employed in industries that were later subject to a severe labor demand shock were in fact more likely to be childless in 1990. Note also that the placebo test yields similar results if I only include main control variables, or if I additionally control for imputed variables for husbands, firm-level characteristics, and regional fixed effects (details are available from the author).

mothers. The composition and level effects need to be distinguished, since they did not operate in the same direction.

7.1 Using Older Cohorts as a Control Group

As previously discussed, it is still possible that, across industries, women differ in unobservable characteristics correlated with fertility. To account for this possibility, I define the main sample of women born between 1959 to 1973 as treated cohorts and use older East German women born between 1944 and 1958 as a control group. The fertility of this control group was completed before the fall of the Berlin Wall (recall Figure 5) and serves as a benchmark. The identifying assumption requires that the potential endogeneity of industries is constant between treated cohorts and their older counterparts. Thus, the approach accounts for systematic unobserved differences among women in highly affected versus less affected industries under the assumption that these differences are the same for the treatment and the control group. The control group is a natural choice, because the socialization of treatment and control group took place in the GDR and both groups selected into industries within the same institutional environment. The comparison to this control group is moreover informative as it clarifies that the labor demand shock impacted fertility through a level and a composition effect.

To explain the approach in more detail, I also define three time periods for the control group: 1976 to 1979 (short term), 1980 to 1984 (medium term), and 1985 to 1992 (long term). During these intervals, women in the control group were of the same ages as the treated cohorts during the time periods included in the baseline analysis (1991 to 1994, 1995 to 1999, and 2000 to 2007). The following difference-in-differences model is then estimated separately for the respective short-, medium-, and long-term periods:

$$Y_{itj} = \beta_0 + \beta_1 RES_{j,89} + \beta_2 treated\_c_i + \beta_3 treated\_c_i * RES_{j,89} + X'_{it}\beta_4 + X'_i\beta_5 + \epsilon_{itj}. \quad (5)$$

For treated cohorts, t stands for 1994, 1999, or 2007; for control cohorts it refers to 1979, 1984, or 1992. $treated\_c_i$ is equal to one if a woman belongs to the treatment group and equal to zero otherwise. Control variables are analogous to the main controls used in the baseline analysis. Note that in the following, I no longer include imputed control variables for husbands, since these variables are not available for the control group. Thus, I provide

lower-bound estimates of the composition effect.

j denotes the industry a woman worked in, which for both treatment and control groups is defined as her industry on January 1st, 1991. As in the previous section, I distinguish between the extensive and the intensive margins of fertility. The first outcome is end of period childlessness, which is a dummy variable equal to one whenever an initially childless woman is still childless at the end of the short-, medium- or long-term period. The second outcome is the total number of children a woman had between the beginning of the short-term period and the end of a given period.

Regarding main coefficients, β_1 controls for unobserved differences across industries that are correlated with fertility. β_2 captures differences in fertility between the treatment and control group when the two groups were of the same ages. This reflects the impact of the aggregate labor demand shock as well as any other factor changing general fertility trends. For example, pro-natalist policies took effect in 1972 and 1976 in East Germany and coincided with higher birth rates among women in the control group (Huinink and Wagner, 1995). This does not invalidate the choice of the control group, as long as these policies - or other factors impacting fertility trends - did not have a differential impact across industries. Finally, β_3 measures the impact of the RES demand shock on the fertility of treated cohorts relative to the control group. Under the identifying assumption stated above, β_3 estimates the impact of the RES demand shock on fertility net of the influence of a presumed endogeneity of industries. Therefore, β_3 is the crucial parameter when assessing the robustness of the baseline results (specifically the results presented in Table 4 of Section 6; which I will refer to in the discussion below).

7.2 Difference-in-Differences Results

The results of the difference-in-differences analysis are shown in Table 6. The table reveals pronounced differences in fertility between treatment and control groups at the same ages. Among initially childless women, women in the treatment group are 25 percentage points more likely to still be childless at the end of the short-term period than their older counterparts (Table 6, panel a, column 1, $\hat{\beta}_2$). By the end of the long-term period, the difference in childlessness between treated and control groups has become smaller (panel a, column 3, $\hat{\beta}_2$). This is in line with the finding that part of the East German fertility decline after the fall of the wall was caused by postponement of first births (Conrad et al.,

Table 6: RES and Fertility Relative to the Control Group of Older East German Cohorts, Difference-in-Differences Estimates (based on cross-sections)

	(1) Short term	(2) Med. Term	(3) Long Term	N
(a) Dep. Var: End of Period Childlessness (0/1)				
RES ($\hat{\beta}_1$)	.0040	.0015	.0046	3,190
	(.0222)	(.0131)	(.0048)	
Treated Cohorts ($\hat{\beta}_2$)	.2509***	.2256***	.1215***	
	(.0210)	(.0193)	(.0151)	
RES x Treated Cohorts ($\hat{\beta}_3$)	-.0209	-.0457***	-.0223**	
	(.0214)	(.0122)	(.0087)	
P90 vs P10	-.0311	-.0681	-.0332	
|P90 vs P10| / $\hat{\beta}_2$	.1240	.3019	.2733	
(b) Dep. Var: Total Number of Births (#)				
RES ($\hat{\beta}_1$)	-.0106*	.0007	.0063	8,878
	(.0057)	(.0068)	(.0112)	
Treated Cohorts ($\hat{\beta}_2$)	-.2419***	-.3220***	-.2791***	
	(.0108)	(.0157)	(.0257)	
RES x Treated Cohorts ($\hat{\beta}_3$)	.0227***	.0381***	.0258	
	(.0060)	(.0067)	(.0165)	
P90 vs P10	.0338	.0568	.0385	
P90 vs P10 / $\lvert\hat{\beta}_2\rvert$	.1397	.1764	.1379	
Main Controls	yes	yes	yes	

Notes: "Short term" stands for years 1991-94 for treated cohorts born 1973-59; and for years 1976-79 for the control group born 1944-58. "Medium term" refers to 1995-99 (treatment) and 1980-84 (control); and "long term" to 2000-07 (treatment) and 1985-92 (control). Only the final year of each of the three time periods is included. In panel a, "end of period childlessness" refers to a dummy variable equal to one whenever an initially childless woman is still childless at the end of a given period. In panel b, "Total Number of Births" refers to the number of children born between the beginning of the first period and the end of the final year of the short, medium, and long term period, respectively. Control variables are defined as before, except for qualification now standing for the highest qualification a woman achieved (since 1976 qualification variables are not available for the control group). For the sake of comparability, I use the same post-stratification weights as before.

1996; Goldstein and Kreyenfeld, 2011).

In addition, there are strong differences in the total number of births. Compared with the control group, by the end of the long-term period, women in the treatment group had on average 0.279 children less (panel b, column 3, $\hat{\beta}_2$). This clearly shows the relevance

of the negative level effect on aggregate fertility. The level effect can be attributed to the aggregate demand shock and to systemic change after German reunification more generally. The results presented here even seem to indicate that the level effect was so pronounced that it reduced completed fertility among women in the treatment group. Due to the right-censoring of the data in 2007, however, it is beyond the scope of this paper to analyze comprehensively whether this is indeed the case.

Importantly, the results on the impact of the RES demand shock on fertility are robust. Recall that $\hat{\beta}_1$ captures the presumed endogeneity of industries. The parameter is small according to point estimates and almost always statistically insignificant. Thus, a potential endogeneity of industries is not a concern. The only exception is the impact of the RES measure on the total number of births during the initial period (panel b, column 1, $\hat{\beta}_1$). In this case $\hat{\beta}_1$ is in fact negative. If anything, the positive impact of the RES demand shock on the fertility of the treatment group in the short term has been slightly underestimated in Table 4 of Section 6.

The other parameter of interest reflects the differential impact of the RES demand shock on the fertility of the treatment group relative to the control group, and net of a presumed endogeneity of industries ($\hat{\beta}_3$, see the interaction terms in Table 6). $\hat{\beta}_3$ thus captures the composition effect identified in the baseline analysis. According to the composition effect, the intensity of the labor demand shock experienced by women shaped the composition of mothers. Reassuringly, the conclusions drawn are similar to those discussed before. I again find that women more severely impacted by the industry-level demand shock had relatively more children. One difference worth noting is that the negative impact of the RES demand shock on childlessness at the end of the long-term period is now slightly stronger and the statistical significance of this effect has increased compared with the baseline analysis ($\hat{\beta}_3$, panel b, column 3). The opposite is true with regard to the positive impact of the RES demand shock on the total number of children born until the end of the long-term period ($\hat{\beta}_3$, panel a, column 3). The results now suggest that in the long term a woman at the 90th percentile of the demand shock measure had 0.039 births more on average than a women at the 10th percentile of the measure (compared with the previously estimated difference of 0.050 births, see Table 4). The statistical significance decreased (the p-value is now 0.125). While the difference-in-differences analysis thus changes some details of a nuanced interpretation of the effects, overall it confirms the

persistence of the positive impact of the demand shock on fertility.[24] This shows that the main results are not invalidated by endogeneity.

Finally, a direct comparison of the estimates for $\hat{\beta}_2$ and $\hat{\beta}_3$ in Table 6 leads to a more differentiated interpretation. First, the level effect (captured by $\hat{\beta}_2$) and the composition effect (captured by $\hat{\beta}_3$) operate in different directions. The RES demand shock changed the composition of mothers, but against the backdrop of a generally low fertility level. This makes the distinction between the composition effect and the level effect particularly relevant. Second, the level effect is in general relatively large in comparison to the composition effect (as evaluated at the 90th versus 10th percentile). With regard to the total number of births, and depending on the time period analyzed, the magnitude of the composition effect amounts to 14 to 18 percent of the magnitude of the level effect. As far as first births are concerned, and again depending on the time period analyzed, the composition effect amounts to 12 to 30 percent of the level effect (see Table 6). While the composition effect was economically significant, it was not strong enough to counterbalance the low aggregate fertility level after the fall of the wall.

7.3 Common Trends Assumption

In the last section, β_3 measured the differential impact of the RES demand shock on fertility of the treatment group relative to the control group. The approach requires that estimates for β_3 are driven by the intensity of the negative labor demand shock after 1991. β_3 should not be confounded by any unobservable trend affecting women in the treatment and control groups differently *across industries*. To test and support this assumption, I repeat the previous analysis for predetermined birth outcomes (childlessness at the beginning of the short term period as well as the total number of children born up until this point). This test should yield small and insignificant estimates for β_3. Results reported in Table 7 highlight that this is indeed the case and support the validity of the difference-in-differences approach.

[24]The interpretation is the same when I right censor the data three years earlier for both treatment and control groups to avoid that the control group is observed after 1989 (not shown). Note, however, that the control group had almost completed their fertility in 1989 (Figure 5); and was therefore included until 1992.

Table 7: RES and Predetermined Fertility Outcomes Relative to the Control Group of Older East German Cohorts, Test of the Common Trend Assumption (based on cross-sections)

	(1) Prior childlessness (0/1)	(2) Prior total no of births (#)
RES ($\hat{\beta}_1$)	-.0015	.0148
	(.0061)	(.0121)
Treated Cohorts ($\hat{\beta}_2$)	.1085***	-.1896***
	(.0098)	(.0226)
RES x Treated Cohorts ($\hat{\beta}_3$)	.0019	.0045
	(.0106)	(.0246)
P90 vs P10	.0029	.0067
N	8,878	8,878
Main Controls	yes	yes

Notes: The model has the same difference-in-differences structure as in Table 6. To test the common trends assumption, the outcome variable in column (1) is a dummy variable equal to 1 whenever a women was still childless before the beginning of the respective short term period (for women in the treatment group this refers to the beginning of 1991 and for women in the control group this refers to the beginning of 1979). In column (2), the outcome is the total number of children born before the respective short term period. Control variables for the number of children born before the short term are obviously dropped; otherwise explanations and control variables are the same as in Table 6.

7.4 Comparison to West German Women

Relying on older cohorts of East German women as a control group, I have provided evidence against the objection that women's unobserved characteristics might drive the results. Another reason for objection are unobserved factors potentially associated with industry characteristics. Across industries, the introduction of the market economy may have changed working conditions differentially, such as work-family friendly policies. This would be a problem if these conditions rather than the demand shock were causing the results presented so far. To account for this, West German women are a natural comparison group. West German women were exposed to working conditions in a market economy, but were not impacted by the demand shock specific to German reunification. At the same time, West German women experienced more gradual and potentially anticipated changes to the industrial structure. To the extent that the RES variable picks up these more gradual changes, we can think of the comparison to West German women

Table 8: RES and Total Number of Births, OLS Estimates (Cross-Sectional Regressions), By Qualification in 1991

	(1) 1994	(2) 1999	(3) 2007	N
(a) Low qualification				
RES	.0322***	.0637***	.0695***	1,147
	(.0092)	(.0133)	(.0203)	
P90 vs P10	.0480	.0949	.1036	
(b) Medium qualification				
RES	.0073	.0285***	.0165	2,929
	(.0085)	(.007)	(.0115)	
P90 vs P10	.0109	.0425	.0246	
(c) High qualification				
RES	.0306**	.1156***	.1203***	158
	(.0131)	(.0139)	(.0145)	
P90 vs P10	.0236	.0890	.0926	
Main controls	yes	yes	yes	

Notes: Explanations are analogous to Table 4. I refer to women without formal qualification, with apprenticeship degrees, and to graduates, respectively. As before, "Total Number of Births" means the number of children born between 1991 and the end of 1994, 1999, or 2007. I no longer control for qualification.

as a conservative approach providing a lower bound for the effects among East German women. For this purpose, I draw a sample of West German women belonging to the same cohorts as and defined analogously to the East German main sample. I then repeat a difference-in-differences analysis similar to before, but this time comparing East German women (the treatment group) with West German women (the control group). Thus, the comparison is across regions, not across time. For the sake of brevity, I report and discuss the corresponding results in the Appendix A.1.2. The conclusions drawn before remain valid when West German women are used as the control group.

8 Effect Heterogeneity

8.1 Qualification Groups

In the final part of the analysis, I investigate effect heterogeneity. To analyze differences by qualification level, Table 8 presents results separately for women who at the beginning

of the 1990s had no formal qualification, had completed apprenticeships, or had graduated from university. Since, in the last section, endogeneity of industries was shown not the be a major concern, I return to the main sample of women born between 1959 and 1973. For each qualification level, I separately regress the total number of children born between 1991 and the end of the short-, medium-, and long-term periods on the RES measure.

The RES demand shock had a significant positive impact on the total number of children born by women with low qualification levels. A pronounced impact is already found for the short-term period (Table 8, panel a, column 1). This is consistent with Chevalier and Marie (2017) who find that, in general, low-skilled East German women were relatively more likely to have children immediately after the fall of the Berlin Wall. I now show that this general reaction to uncertain times corresponds to the within-group reaction of low-skilled women to the RES demand shock. For high-skilled women, effects are also positive and significant (panel c). Note, however, that among the relatively young cohorts included in the sample only a small fraction had already graduated from university at the beginning of 1991. As a result, the sample size for women graduates is relatively small and the results should be interpreted with some caution. For both low-skilled and high-skilled women, effects persist even in the long term (panels a and c, column 3). By contrast, effects are smaller and do not persist in the long term among medium-skilled women (panel b).

It is striking that the positive effects of the demand shock pertain to all qualification groups and that the effects are more pronounced at the "qualification extremes." In order to assess underlying mechanisms, in Appendix Table A10, I investigate how the RES demand shock impacted labor market outcomes by qualification level. The table documents two important findings. First, the impact of the demand shock on unemployment is decreasing in qualification levels. The demand shock had a strong and persistent effect on the likelihood of unskilled women to become unemployed. Thus, in the generally uncertain economic situation in East Germany, reentry into employment was especially difficult for low-skilled women. One explanation that rationalizes my findings is that low-skilled women with better economic opportunities were less willing to put their jobs at risk. As a consequence, they had relatively fewer children.

Second, the impact of the demand shock on the mobility across industries is increasing in qualification levels. Among women graduates, there was a strong and immediate mobility

Table 9: RES and Total Number of Births, OLS Estimates (Cross-Sectional Regressions), By Age Groups

	(1) 1994	(2) 1999	(3) 2007	N
(a) Aged 34-38 at the end of 2007				
RES	.0236*	.0745***	.0588*	1,144
	(.0120)	(.0154)	(.0324)	
P90 vs P10	.0370	.1170	.0922	
(b) Aged 39-43 at the end of 2007				
RES	.0024	.0311*	.0285	1,457
	(.0131)	(.0155)	(.0202)	
P90 vs P10	.0036	.0463	.0425	
(c) Aged 44-48 at the end of 2007				
RES	.0094*	.0179***	.0154**	1,633
	(.0050)	(.0052)	(.0067)	
P90 vs P10	.0140	.0266	.0230	
Main controls	yes	yes	yes	

Notes: Explanations are analogous to Table 4. As before, "Total Number of Births" means the number of children born between 1991 and the end of 1994, 1999, or 2007.

response, since those high-skilled women more severely impacted by the demand shock were very likely to switch industries already in the short-term period. This largely ensured them against the risk of becoming unemployed; and in the medium-term period, those who had switched jobs were more likely to become mothers. Again note, however, that these results are based on a relatively small sample of high-skilled women and should be interpreted with some caution.

8.2 Age Groups

So far, I have found that the positive effect of the RES demand shock persists even after a relatively long period of 17 years. At the end of this period in 2007, the cohorts I focused on are 34 to 48 years old. While it is possible that some of the identified effects are counterbalanced by later births, childbearing among East German women is not very common after age 34 (recall Figure 5 above). It is therefore unlikely that the composition effect completely vanished after the analyzed time period.

In this context, it is informative to differentiate the effects by age groups. Table 9 shows

that the effect of the RES demand shock on fertility is decreasing in age. Intuitively, this can be rationalized by the fact that the younger women were, the fewer children they tended to have prior to 1991. Therefore, the impact on fertility was stronger for younger women. However, a small persistent effect is found even for the oldest group of women who were aged 44 to 48 in 2007 (Table 9, panel c). This last result is of particular interest, because it is impossible that many women belonging to the oldest age group had children after 2007. The result suggests that the RES demand shock had an impact on their completed fertility.[25]

9 Interpretation and Conclusion

In this paper, I analyze how women's labor market situations impact childbearing decisions. I analyze this question in the context of East Germany after the fall of the Berlin Wall. To circumvent the endogeneity of individuals' labor market outcomes, I exploit exogenous variation of the negative labor demand shock which hit East Germany as a result of the introduction of the market economy. The variation stems from differential pressure for restructuring across industries. Industrial restructuring in East Germany was pronounced and entirely unexpected. It led to permanent shifts in employment structures and it generated exogenous variation in individuals' labor market outcomes. Importantly, industrial restructuring occurred independently of previous sorting of East German workers into industries.

I find that the negative labor demand shock had an impact on the composition of mothers. Throughout the 1990s, East German women more severely impacted by industrial restructuring had relatively more children than their counterparts who were less severely impacted. The comparison with older cohorts of East German women reveals that the composition effect was not strong enough to counterbalance the low East German fertility level after the fall of the wall. However, the composition effect was meaningful in an economic sense.

A priori, it is theoretically ambiguous how women's labor market situations impact childbearing decisions. From a neoclassical point of view, there are opposing income and substitution effects. The findings presented in this paper point to the dominance of the

[25]For completeness, in Appendix Table A11, the effects of the demand shock on labor market outcomes are shown separately by age group. There are no major differences by age group.

substitution effect in determining the composition of mothers. The substitution effect suggests a trade-off between pursuing a career and raising children. This implies a decrease in fertility as economic opportunities for women improve. After the fall of the wall, economic prospects were generally uncertain in East Germany. In this situation, childbearing likely increased the actual or at least the perceived risk of job loss. As a result, it is plausible that East German women with more favorable employment conditions were relatively less willing to give up their current labor market situations.

The industry-level variation I exploit is relevant and informative beyond the context of East Germany, since it resembles long-term changes to industrial employment structures which started earlier and were more gradual in market economies. A remaining question is whether similar fertility effects can be expected in economic environments where the aggregate labor demand shock is absent. In accordance with my findings, for the United States Autor et al. (2017) and Schaller (2016) also find a relative increase in fertility in regions with declining female labor market prospects compared with regions where female labor market prospects develop more favorably. This suggests that the trade-off between female careers and child-rearing determines the composition of mothers across economic environments. In my paper, I could exploit individual-level panel data. I have shown that labor demand shocks impact the timing of births but that the effects in part persist over the life courses of individual women.

Finally, a general implication of my results is that labor demand shocks impact fertility both through a level effect and a composition effect. In East Germany, the negative aggregate demand shock and elevated economic uncertainty depressed aggregate fertility levels. This level effect was very pronounced as it coincided with the rapid systemic changes implied by the reunification of Germany. By contrast, the impact of the labor demand shock on the composition of mothers was positive. East German women more severely impacted by the labor demand shock had relatively more children than their counterparts who were less severely impacted. A distinction between the level effect and the composition effect is crucial in general, particularly because this paper has shown that the two effects do not necessarily operate in the same direction.

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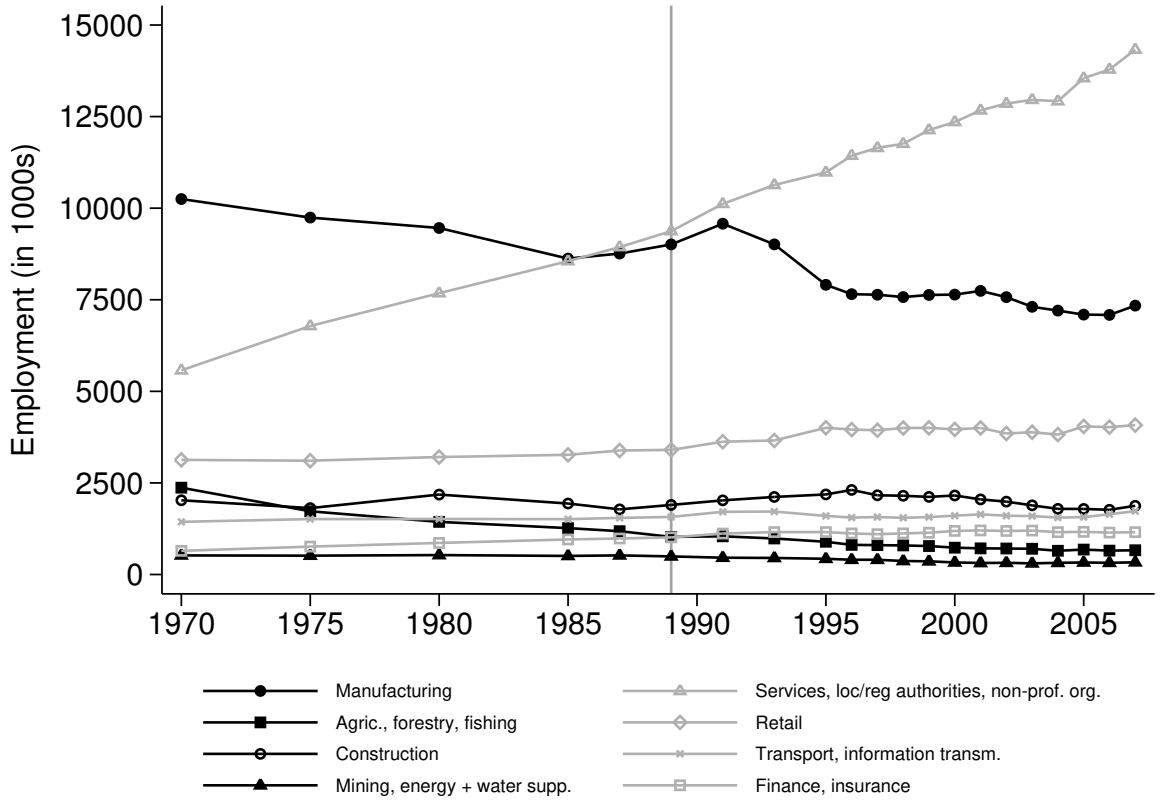
A Appendix: For Online Publication

A.1 Comparison with Results for West Germany

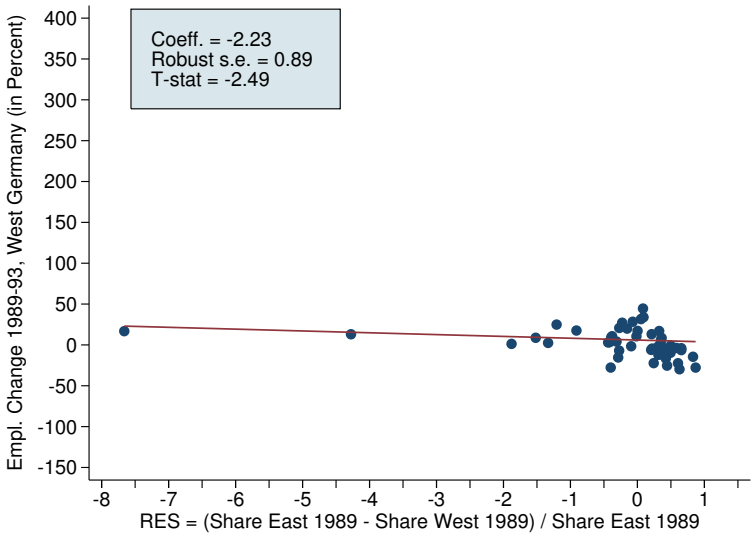
A.1.1 Employment by Sector and More Detailed Industries

Figure A1: Employment in West Germany (in Thousands), By Economic Sector, 1970-2007 (Selected Years)



Source: *1970, 1975, 1980:* Provided by Federal Statistical Office and based on the (West German) Microcensus. *1985 and later:* Author's calculations based on the Scientific Use Files of the Microcensus (a 0.7 percent sample of the population) for persons aged 15 and older living in West Germany at their main residence; weighted by Microcensus weights. The increase and decline in manufacturing employment between 1989 and 1995 can be explained by new investment opportunities in East Germany, followed by the impact of a now more competitive environment in which firms could move production to central and eastern European countries (see, e.g., Dustmann et al., 2014, p. 182).

Figure A2: Correlation between the Relative West German Employment Change 1989 to 1993 (in Percent) and Relative Excess Supply in 1989, By Industry



Source: West German analogue for Figure 4, panel (a). $N = 48$, the regression model is weighted using 1989 West German employment shares of industries as analytical weights. The y-axis displays change in percent, i.e., $100 * \frac{Empl\_West_{j,93} - Empl\_West_{j,89}}{Empl\_West_{j,89}}$.

A.1.2 Difference-in-Differences Analysis with West German Women

The sample of West Germans is defined as an analogue to the main sample of East German women (cf. Section 3.2). Thus, I select West German women born from 1959 to 1973 who worked in West Germany on January 1st, 1991 in an industry $j = 1, ..., 48$. The difference-in-differences analysis is defined as follows:

$$Y_{itj} = \beta_0 + \beta_1 RES_{j,89} + \beta_2 treated\_c_i + \beta_3 treated\_c_i * RES_{j,89} + X'_{it}\beta_2 + X'_i\beta_3 + \epsilon_{itj}, \quad (6)$$

where t stands for 1994, 1999, or 2007; and $treated\_c_i$ is equal to one for East German women and equal to zero for West German women. I regard this as a conservative test, since West German women experienced more gradual and potentially anticipated changes to their industry structures. Results are reported in Table A1.[26]

To compare these with older results, I refer to the analysis relying on older cohorts as a control group. Thus, I compare $\hat{\beta}_3$ in Table 6 with $\hat{\beta}_3$ in Table A1. According to this comparison, estimates for β_3 follow the same pattern as before. With regard to childlessness, the long term impact of the RES demand shock is now estimated to be smaller. With regard to the total number of children born, the impact is now slightly larger. Thus, again some nuances change, but overall the conclusions are similar whether I only exploit within-group variation for the main sample (as in the baseline analysis in Section 6), whether I use older cohorts of East German women as a control group (as in Section 7), or whether I perform a conservative analysis using West German women as a control group (as here in the Appendix).

[26]Note also that when I assess the "common trend assumption" based on predetermined fertility outcomes before 1991, estimates for β_3 are small and statistically insignificant; as they should be (details are available on request).

Table A1: RES and Fertility Relative to the Control Group of West German Cohorts, Difference-in-Differences Estimates (based on cross-sections)

	(1) 1994	(2) 1999	(3) 2007	N
(a) Dep. Var: End of Period Childlessness (0/1)				
RES ($\hat{\beta}_1$)	-.0087***	-.0114***	-.0094**	20,718
	(.0019)	(.0038)	(.0036)	
Treated Cohorts ($\hat{\beta}_2$)	-.0532***	-.1050***	-.0929***	
	(.0152)	(.0181)	(.0162)	
RES x Treated Cohorts ($\hat{\beta}_3$)	-.0153	-.0359***	-.0096	
	(.0105)	(.0120)	(.0141)	
P90 vs P10	-.0229	-.0536	-.0143	
(b) Dep. Var: Total Number of Births (#)				
RES ($\hat{\beta}_1$)	.0079***	.0142**	.0096	26,913
	(.0019)	(.0069)	(.0097)	
Treated Cohorts ($\hat{\beta}_2$)	-.0672***	-.1061***	-.1172***	
	(.0135)	(.0193)	(.0179)	
RES x Treated Cohorts ($\hat{\beta}_3$)	.0107	.0323***	.0287*	
	(.0070)	(.0074)	(.0153)	
P90 vs P10	.0159	.0481	.0427	
Main Controls	yes	yes	yes	

Notes: Regressions are based on cross-sections, which, refer to 1994 (colum 1), or 1999 (column 2), or 2007 (column 3), for both treatment and control groups. In panel a, "end of period childlessness" refers to a dummy variable equal to one whenever an initially childless woman is still childless at the end of a given year. In panel b, "Total Number of Births" refers to the number of children born between the beginning of 1991 and the end of 1994, 1999, or 2007, respectively. Control variables are defined as before, though I dropped control variables specific to East Germany (such as the proxy for prior regime closeness).

B Description of Variables

Table A2: Variables and Underlying Concepts

Unemployment:
Because unemployment transfers are in part means tested (Fitzenberger and Wilke, 2004), unemployment refers to spells with such transfers (unemployment benefits or assistance) or to spells merely entailing contributions towards pensions. Unemployment variables are defined for calendar years; unemployment spells covering multiple calendar years were thus split.

Industry Change:
Defined as occurring whenever a woman works in a new industry for the first time.

Migration West:
Based on a Stata-routine by Dana Müller. East or West German residence is inferred from employment and unemployment spells and related pension entitlements. There may be gaps in the data; thus, the point of time of migration is determined as the last day of the last spell in East Germany.

Birth: Inferred from month of birth of a woman's children.

Age: Inferred from a woman's month of birth.

Qualification: Imputed and defined following Fitzenberger et al. (2006).

Children Born Before 1991:
Inferred from month of birth of a woman's children.

Large/very large city:
Cities are based on the 2009 regional classification scheme; in several cases the definition of city boundaries is broader than at the beginning of the 1990s. Moreover based on the first entry when regional information was non-missing. Large cities are defined as Chemnitz, Halle, Magdeburg, Erfurt and Rostock; very large cities are Berlin, Leipzig, and Dresden.

GDR regime closeness:
Proxied by GDR pension privileges. Around one third of these pensions were paid to "Sector X" employees and two-thirds to persons in positions considered important, including part of the intellectual elite and pedagogues (Schmähl, 2007).

Apprenticeship Training:
Dummy variable equal to one for women who on January 1st, 1991, participated in an apprenticeship training program.

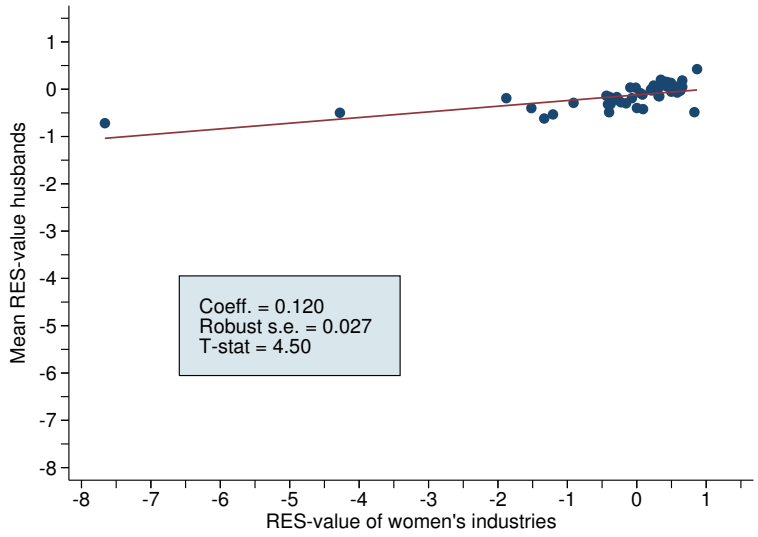
Table A3: List of Industries in Ascending Order of RES

Industries j=1,...,48:
Insurance; Monetary intermediation; Printing, reproduction; Finishing trade; Construction & repair of road vehicles etc.; Drawing & cold-rolling of metals, custom steel forming; Accommodation, homes, laundry, cleaning, waste collection etc.; Wholesale, wholesale on a fee or contract basis; Chemical industry, manufacturing & processing of fissible & fertile material; Manuf. of iron, sheet metal & metal products; Retail sale; Manuf. of plastics products, processing of rubber products; German Federal Postal Administration; Precision mechanics, optics, Manuf. of watches & clocks; Local & regional authorities; Human health activities, veterinary activities; Other services (consulting & related activities); Other transport; Manuf. & processing of pulp, groundwood pulp, paper, paperboards; Iron & steel producing industry; Manufacturing & processing of wood; Facture of structural metal products, Manuf. of railway vehicles; Non-profit organizations; Manuf. of non-ferrous & semi-finished non-ferrous metal products; Manuf. of tobacco products; Electrical engineering, repair of domestic appliances; Main construction; Manuf. of food products; Fine ceramics, Manuf. & processing of glass; Mechanical engineering; Inland, sea & coastal water transport; Education, science, culture, publishing; Manuf. of wearing apparel; Manuf. of beverages; Distribution of electricity, gas, water, long-distance heating & related activities; Industrial market gardening & farming, forestry, fishing; Quarrying & processing of stone & minerals; Foundry; Coal mining; Manuf. of office machinery, accounting & computing machinery; Manuf. of musical instruments, games, toys, fountain pens etc.; Manuf. of textiles; Manuf. of leather; Building & repairing of ships; Agriculture; Railway organizations; Manuf. of refined petroleum products; Other mining

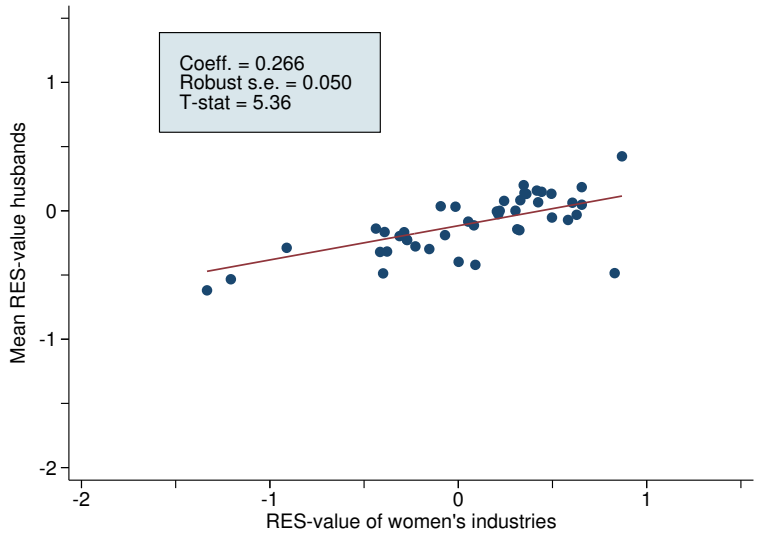
The industry classification scheme corresponds to the German 1979 Classification of Economic Activities.

B.1 Extensions to the Main Analysis

Figure A3: Correlation between Women's RES-Values (by Industry) and Average RES-Values of their Husbands, 1991, Married Women Only



(a) All industries (N=48)



(b) Excluding the four most extreme RES-values

Notes: Based on the Microcensus of 1991. The sample includes East German women born 1959 to 1973 who have a husband. The regression model is weighted using industry shares.

Table A4: RES and Total Number of Births/End of Period Childlessness, OLS Estimates (Cross-Sectional Regressions), By Migration Status

	Non-Migrants			Migrants		
	(1)	(2)	(3)	(4)	(5)	(6)
	1994	1999	2007	1994	1999	2007
(a) Dep. Var: End of Period Childlessness (0/1)						
RES	-.0144	-.0451***	-.0363**	-.0404**	-.0360	.0398*
	(.0159)	(.0109)	(.0158)	(.0190)	(.0252)	(.0207)
P90 vs P10	-.0214	-.0673	-.0541	-.0615	-.0548	.0605
N	1,055	1,055	1,055	542	542	542
(b) Dep. Var: Total Number of Births (#)						
RES	.0108	.0435***	.0399***	.0231**	.0290***	.0092
	(.0076)	(.0055)	(.0111)	(.0087)	(.0105)	(.0195)
P90 vs P10	.0160	.0648	.0595	.0344	.0432	.0137
N	3,060	3,060	3,060	1,174	1,174	1,174
Main controls	yes	yes	yes	yes	yes	yes

Notes: As in Table 4. A woman is defined as being a "migrant" if she migrated to West Germany before 2007; all other women are "non-migrants."

Table A5: RES and Total Number of Births/End of Period Childlessness, OLS Estimates (Cross-Sectional Regressions), Controlling for Regional Spillover Effects

	(1)	(2)	(3)	
	1994	1999	2007	N
(a) Dep. Var: End of Period Childlessness (0/1)				
RES	-.0189*	-.0417***	-.0179*	1,516
	(.0110)	(.0093)	(.0090)	
P90 vs P10	-.0281	-.0621	-.0267	
(b) Dep. Var: Total Number of Births (#)				
	.0107***	.0373***	.0286***	4,088
	(.0031)	(.0040)	(.0084)	
P90 vs P10	.0159	.0555	.0425	
Main Controls	yes	yes	yes	
Municipality FEs	yes	yes	yes	

Notes: As in Table 4. The regressions now contain detailed fixed effects accounting for the municipality a woman lived in at the beginning of the 1990s (*Kreise* in German). In a few cases, this information was missing, which explains the slightly smaller sample sizes.

Table A6: RES and Total Number of Births/End of Period Childlessness, OLS Estimates (Cross-Sectional Regressions), Including Firm-Level Control Variables

	Dep. Var: End of Period Childlessness (0/1)			*Dep. Var: Total Number of Births (#)*		
	(1)	(2)	(3)	(4)	(5)	(6)
	1994	1999	2007	1994	1999	2007
RES	-.0275**	-.0458***	-.0204*	.0158***	.0407***	.0367***
	(.0118)	(.0103)	(.0105)	(.0038)	(.0045)	(.0098)
P90 vs P10	-.0410	-.0682	-.0304	.0235	.0606	.0547
Firm-Level Control Variables						
Share Female	-.1661**	-.2000***	-.2006***	.0996***	.1296***	.2062***
	(.0724)	(.0648)	(.0563)	(.0287)	(.0303)	(.0545)
Share low-skilled	-.0899**	-.1402*	-.1043*	.0380**	.0641	.1181**
$> 5\%$ (0/1)	(.0389)	(.0755)	(.0532)	(.0184)	(.0473)	(.0581)
Share younger 30	.1659	-.0007	-.1525	-.0068	.0222	.2087*
	(.1130)	(.0871)	(.1177)	(.0701)	(.0753)	(.1214)
Share older 50	-.0591	-.2135	-.2843	.1357	.2480	.3373*
	(.1659)	(.1862	(.1180)	(.0905)	(.1707)	(.1826)
Firm Size	1.06E-05	4.89E-06	5.33E-06	-4.97E-06	-7.74E-06	-1.2E-05
	(8.80E-06)	(1.24E-05)	(9.27E-06)	(5.97E-06)	(7.75E-06)	(9.79E-06)
Firm Size Sq.	-3.45E-10	-2.87E-11	-4.80E-11	2.54E-10	4.95E-10	4.70E-10
	(3.23E-10)	(5.10E-10)	(3.83E-10)	(2.25E-10)	(3.18E-10)	(3.85E-10)
N	1,582	1,582	1,582	4,196	4,196	4,196
Main controls	yes	yes	yes	yes	yes	yes

Notes: As in Table 4, but controlling for 1991 firm-level characteristics. Coefficients on firm size are in scientific notation, where for example E-05 denotes 10^{-5}. Firm size is defined as the number of employees.

Table A7: RES and Total Number of Births/End of Period Childlessness, OLS Estimates (Cross-Sectional Regressions), Excluding Industries with Strong Employment Growth

	(1) 1994	(2) 1999	(3) 2007
(a) Dep. Var: End of Period Childlessness (0/1)			
RES	-.0647***	-.0639**	-.0175
	(.0185)	(.0260)	(.0226)
P90 vs P10	-.0983	-.0971	-.0266
N	1,557	1,557	1,557
(b) Dep. Var: Total Number of Births (#)			
RES	.0188*	.0420***	.0115
	(.0095)	(.0112)	(.0331)
P90 vs P10	.0192	.0428	.0117
N	4,135	4,135	4,135
Main contr.	yes	yes	yes
Firm contr.	yes	yes	yes

Notes: As in Table 4, but excluding women initially employed in one of the four industries with the strongest employment growth. After weighting, the share of women employed in these industries is 3.2 percent. I include 1991 firm-level characteristics (as in Table A6).

Table A8: RES and Total Number of Births, OLS Estimates (Cross-Sectional Regressions), Excluding Industries with Strong Employment Growth and Including Imputed Control Variables for Spouses

	(1) 1994	(2) 1999	(3) 2007	(4) 1994	(5) 1999	(6) 2007
Dep. Var: Total Number of Births (#)						
RES	.0013	.0402	.0721*	.0157	.0515*	.1215***
	(.0196)	(.0294)	(.0404)	(.0175)	(.0261)	(.0347)
P90 vs P10	.0013	.0410	.0735	.0160	.0525	.1239
N	4,167	4,167	4,167	4,130	4,130	4,130
Main Controls	yes	yes	yes	yes	yes	yes
Husb. Control Variables	yes	yes	yes	yes	yes	yes
Firm Control Variables	-	-	-	yes	yes	yes

Notes: As in Table 4, but excluding women initially employed in one of the four industries with the strongest employment growth. I include imputed control variables for spouses (as in Table 5). In columns 4 to 6, I additionally include firm-level control variables (as in Table A6).

Table A9: RES and Predetermined Fertility Outcomes, Placebo Test (Cross-Sectional Regressions)

	(1) Prior childlessness (0/1)	(2) Prior total no of births (#)
(a) All women born 1959-1973		
RES	-.0020	.0213
	(.0058)	(.0163)
P90 vs P10	-.0029	.0317
Mean of dep. var.	.3860	.9515
N	4,234	4,234
(b) Women born 1959-1963		
RES	-.0086	.0468
	(.0133)	(.0312)
P90 vs P10	-.0135	.0735
Mean of dep. var.	.1182	1.5250
N	1,633	1,633
(c) Women born 1964-1968		
RES	.0247**	-.0252
	(.0120)	(.0162)
P90 vs P10	.0368	-.0376
Mean of dep. var.	.3257	.9148
N	1,457	1,457
(d) Women born 1969-1973		
RES	-.0157	.0192
	(.0121)	(.0132)
P90 vs P10	-.0233	.0285
Mean of dep. var.	.0856	.1561
N	1,144	1,144
Main Controls	yes	yes

Notes: The model has the same structure as Table 4. The outcome variable in column (1) is a dummy variable equal to 1 whenever a women was still childless at the beginning of 1991. In column (2), the outcome is the total number of children born before 1991. Control variables for the number of children born before 1991 are obviously dropped; otherwise explanations and control variables are the same as in Table 4.

Table A10: RES and Various Labor Market Outcomes, OLS Estimates (Panel Regressions), by Qualification in 1991

	Dep. Var: Unemployment Spell in t (0/1)			*Dep. Var: Industry Change in t (0/1)*		
	(1)	(2)	(3)	(4)	(5)	(6)
	1991-94	1995-99	2000-07	1991-94	1995-99	2000-07
(a) Low qualification						
RES	.0538***	.0386***	.0282***	.0163***	.0217***	.0114***
	(.0148)	(.0104)	(.0092)	(.0052)	(.0044)	(.0039)
P90 vs P10	.0802	.0575	.0421	.0242	.0323	.0170
N (each year)	1,147	1,147	1,147	1,147	1,147	1,147
(b) Medium qualification						
RES	.0338**	.0282***	.0119*	.0216**	.0122**	.0049***
	(.0130)	(.0088)	(.0067)	(.0095)	(.0056)	(.0017)
P90 vs P10	.0503	.0420	.0177	.0322	.0181	.0073
N (each year)	2,929	2,929	2,929	2,929	2,929	2,929
(c) High qualification						
RES	.0143	.0098	.0105	.0405***	.0051	.0047
	(.0187)	(.0101)	(.0064)	(.0102)	(.0052)	(.0028)
P90 vs P10	.0110	.0076	.0081	.0312	.0039	.0036
N (each year)	158	158	158	158	158	158
Main controls	yes	yes	yes	yes	yes	yes
Time FEs	yes	yes	yes	yes	yes	yes

Notes: As in Table 2, but separated by qualification group. Also within qualification groups, the RES demand shock had no systematic impact on the likelihood of migrating to West Germany (data not shown).

Table A11: RES and Various Labor Market Outcomes, OLS Estimates (Panel Regressions), by Age Groups

	Dep. Var: Unemployment Spell in t (0/1)			*Dep. Var: Industry Change in t (0/1)*		
	(1)	(2)	(3)	(4)	(5)	(6)
	1991-94	1995-99	2000-07	1991-94	1995-99	2000-07
(a) Aged 34-38 at the end of 2007						
RES	.0480	.0284**	.0204*	.0156*	.0204***	.0075***
	(.0300)	(.0128)	(.0103)	(.0089)	(.0041)	(.0021)
P90 vs P10	.0754	.0446	.0320	.0245	.0320	.0117
N (each year)	1144	1144	1144	1144	1144	1144
(b) Aged 39-43 at the end of 2007						
RES	.0210	.0316***	.0187*	.0177*	.0154***	.0028
	(.0145)	(.0097)	(.0111)	(.0090)	(.0055)	(.0024)
P90 vs P10	.0314	.0471	.0279	.0263	.0229	.0041
N (each year)	1,457	1,457	1,457	1,457	1,457	1,457
(c) Aged 44-48 at the end of 2007						
RES	.0459***	.0355***	.0120	.0277***	.0115*	.0090***
	(.0097)	(.0067)	(.0100)	(.0083)	(.0061)	(.0029)
P90 vs P10	.0684	.0529	.0179	.0412	.0172	.0135
N (each year)	1,633	1,633	1,633	1,633	1,633	1,633
Main controls	yes	yes	yes	yes	yes	yes
Time FEs	yes	yes	yes	yes	yes	yes

Notes: As in Table 2, but separated by age group. Also within age groups, the RES demand shock had no systematic impact on the likelihood of migrating to West Germany (data not shown).

B.2 Initially Unemployed Women

Figure A4: Annual Number of Births per 1,000 East German Women, By Employment Status at the Beginning of 1991

Notes: Group definitions and sample sizes are: (1) 9,017 women were employed on January 1st, 1991 ("employed, all"). (2) Of those employed on January 1st, 1991, 4,234 women had non-missing industry information ("employed, industry known"). This is the main sample used in the analysis. (3) An additional 807 women were unemployed on January 1st, 1991 ("unemployed"). The discrepancy between the first two groups motivates the use of weights in order to make the sample representative by industry; whereas weighting cannot address any selective attrition resulting from unemployment. Note also that 2 percent of women had already migrated to West Germany by January 1st, 1991. Unfortunately, due to its small sample size (N=223), I was not allowed to display birth data for this group. This is because results based on less than 20 absolute births per year and subgroup were censored. For the same reason, the line for initially unemployed women is for years 1980 to 1998 only.

Eight percent of women had already become unemployed by January 1st, 1991, the day for which the sample was drawn. Across and within industries, these women should be among those workers with the highest risk of job loss. Since their initial industry is not known, it is unfortunately not possible to include them in the main analysis. Nevertheless, interesting fertility patterns are found for this subgroup.

To illustrate this further, in Figure A4, annual births per 1,000 women are displayed for women who work at the beginning of 1991 as well as for women who have already lost their jobs by then. It turns out that the initially unemployed women have disproportionately high birth rates in the three years prior to reunification (1987 to 1989). Those East German women who were among the first losing their jobs were relatively likely to have young children below the age of four. This corresponds to legal regulations: The comprehensive job guarantee for all workers in the GDR was officially abolished in June of 1990 (Richardi, 2007, p. 356); whereas special dismissal protection and job guarantees for pregnant women and for women on maternity leave (which originate from the West German labor law) were introduced in East Germany only at the beginning of 1991 (ibid, p. 359). Overall, the association in Figure A4 suggests that in these times of elevated economic uncertainty, childbearing was related to the risk of job loss.[27]

Given that the initially unemployed women had relatively many children before 1991, one might have expected relatively low subsequent birth rates for this subgroup. This is not

[27]From 1991 onwards, dismissal protection for mothers only covered the time of maternity leave; it moreover did not apply to women employed in firms which closed down.

the case, since from 1991 onwards, birth rates of the initially unemployed women are very similar to birth rates of the initially employed women. Accounting for the pre-determined number of children already born, initially unemployed women thus have relatively high post-reunification birth rates. This seems to correspond to the main findings, according to which the experience of a more severe demand shock leads to higher birth rates.

B.3 Women Employed in Social Security Agencies

Table A12: Extreme Cases: Social Security Agencies Compared With Insurance and Financial Intermediation, Various Outcomes, OLS Estimates (Panel Regressions, Unweighted)

	(1)	(2)	(3)	(4)	(5)	(6)
	1991-94		1995-99		2000-07	
(a) Dep. Var.: Unemployment Spell in t (0/1)						
Soc. Sec. Agencies	.0211	.0219	.0817***	.0515	.0430*	.0214
	(.0321)	(.0371)	(.0291)	(.0322)	(.0222)	(.0243)
(b) Dep. Var.: Industry Change in t (0/1)						
Soc. Sec. Agencies	.1468***	.1196***	.0333	.0421*	.0273**	.0232*
	(.0294)	(.0321)	(.0205)	(.0242)	(.0107)	(.0125)
(c) Dep. Var.: Birth in t (0/1)						
Soc. Sec. Agencies	.0210	.0064	.0286***	.0299***	-.0035	-.0006
	(.0140)	(.0178)	(.0101)	(.0093)	(.0081)	(.0076)
Age controls only	yes	-	yes	-	yes	-
Main controls	-	yes	-	yes	-	yes
Time FEs	yes	yes	yes	yes	yes	yes
N (each year)	228	228	228	228	228	228

Notes: Each coefficient is from a separate regression. It refers to a dummy variable equal to one for women who worked in social security agencies on January 1st, 1991; this dummy variable is equal to zero for women who worked in insurance or financial intermediation on January 1st, 1991. Control variables are analogous to Table 2. Robust standard errors are in parentheses; ***, **, * refers to significance at the 1, 5, and 10 percent level, respectively. No weights were used.

My analysis excludes women initially employed in social security agencies. Social security agencies practically did not exist in the GDR. Employment in this industry therefore increased by more than 850 percent between 1989 and 1993. The corresponding RES value is similarly extreme. Despite this strong employment growth, women initially employed in social security agencies were frequently replaced by new workers. This was part of the complete reorganization of social security agencies after the reunification of Germany (Bernien et al., 1996).

Once I include women initially employed in social security agencies, the economic significance of the RES demand shock decreases somewhat. To illustrate this further, in Table A12 I compare outcomes of women initially employed in social security agencies with outcomes of women initially employed in insurance and monetary intermediation industries. These are two other examples of industries which expanded strongly after reunification,

although less so than social security agencies. Table A12 reveals that women initially employed in social security agencies comparatively often experienced unemployment and, in particular, changed industries relatively often. At the same time, their medium-term birth rates were relatively high. In this sense, for women initially employed in social security agencies, less favorable or stable labor market outcomes are again associated with higher medium-term birth rates; but, this stands in contrast to the exceptional employment development of their initial industry.