Public child care and fertility in Germany

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Abstract

This study examines the impact of public child care supply on fertility. Microeconomic theory predicts a positive impact of the supply of affordable public child care on the decision to have a child. We utilize spatial and temporal variation of public child care coverage rates resulting from a child care reform in West Germany in 2005. With both low fertility rates and sparse public child care infrastructure in the pre-reform period, West Germany offers a suitable setting to test the theoretical relationship. Our Diff-in-Diff analysis of combined micro- and county-level panel data reveals that there is no significant effect of the expansion of public child care supply on fertility in the short-term. We neither find substantial effect heterogeneities nor evidence for selective migration into counties with more extensive expansion of public child care. In line with previous microlevel studies, we conclude that public child care expansion does not cause an immediate and strong fertility increase.

Keywords: Public child care, Fertility, Reform, Germany, Micro-level

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1 Introduction

In most developed countries, fertility has decreased within the 20th century and settled down below the replacement level. We also observe substantial and prevailing cross country differences in fertility: while total fertility rates (TFRs) of countries such as Norway, France, or the U.S. are close to the replacement level, other countries such as Italy, Japan, and Germany constantly show lower TFRs of around 1.3 in the last two decades (The World Bank 2016b). A fertility far below the replacement level coincides with predictable economic problems for the respective countries, for example with respect to pay-as-you-go pension schemes (Boersch-Supan & Ludwig 2010; Sinn 2004; Sinn & Uebelmesser 2002). Against this background, some countries have adopted family-friendly policies of Scandinavian countries which exhibit comparatively high fertility and maternal employment rates (OECD 2016a;b). For instance, while Japan introduced a paid maternity leave scheme not until 1992, Sweden had already introduced such a scheme in 1975. Germany introduced a universal public child care scheme as another key element of such policies in the mid-2000s, which is similar to the universal public child care scheme of Norway introduced within the 1970s (for an overview, see Gauthier 2007).

In the current study, we examine the consequences of exporting the "Scandinavian model" (Havnes & Mogstad 2011) of extensive family-friendly policies to other countries by taking the example of Germany. The child care policy change of this country offers the opportunity to investigate whether increasing public child care is an effective means to increase low fertility. Microeconomic theory predicts a positive impact of public child care supply on fertility (Becker & Lewis 1973; Ermisch 2016; Del Boca 2002; Borck 2010). Our study examines this relationship using a child care reform in West Germany in 2005, when a sparse public child care infrastructure coincided with low fertility and a gap between desired and realized fertility of the population at that time (Dorbritz et al. 2005). While the corresponding child care reform law was implemented at the federal level, the German counties were responsible for the expansion of child care slots. The federal government provided massive funds to the counties, which decided autonomously on the allocation of the means. This led to significant temporal and spatial variation of public child care coverage rates which allows us to apply differences-in-differences techniques. By using aggregated administrative information on public child care services in combination with micro-level panel data, we identify the causal effect of public child care availability on fertility decisions at the micro-level.

Our contribution to the literature is threefold. First, our identification strategy enables us to circumvent endogeneity problems of previous micro-level studies. For the most part, these studies reveal correlation patterns or use limited policy variation (e.g., Del Boca 2002; Hank & Kreyenfeld 2003). Second, we complement the existing macro-level evidence (e.g., Bauernschuster et al. 2016; Luci-Greulich & Thévenon 2013) as we model individual fertility decisions and address effect heterogeneities. Third, we are able to address the issue of selective migration in a distinct way compared to previous studies (e.g., Felfe & Lalive 2013). Selective migration of parents-to-be into regions with extensive public child care supply might bias the estimated effect of public child care coverage on fertility in our and other studies.

Our results do not show a quick and significant fertility reaction to public child care expansion, which is in line with previous micro-level studies. This result prevails prevails across a wide range of different subpopulations. Furthermore, our sensitivity checks do not indicate that migration into regions with extensive public child care supply is a relevant issue that would bias our results. Our analyses do not indicate that selective migration biases the relationship between public child care supply and fertility.

This paper is structured as follows. In Section 2 we briefly review existing empirical studies. The description of the institutional background and the relevant reform in Section 3 is followed by the presentation of our identification strategy in Section 4. Section 5 introduces our data, whereas Section 6 presents our main results. Corresponding robustness tests are given in Section 7, Section 8 summarizes the results and derives an implication for public policy.

2 Related studies

Within microeconomic theory, child care provision is a seldomly investigated determinant of family formation. The few existing theoretical models which deal with the impact of child care provision on fertility are based on the neoclassical theory of fertility as introduced by Becker (1960). Within the Anglo-Saxon context, theoretical research mostly focuses on the relationship between fertility decisions and the costs for private, market-based out-of-home child care services (Blau 2001; Ermisch 2016). Conversely, within European research, the focus is on the relationship between fertility decisions and both the provision and fees of public child care services. In Continental-European countries public child care services are more commonly utilized by families than private child care services. The respective models yield two main predictions. First, reducing the fees of public child care for young children or improving its quality has a positive impact on a women's choice to have children. Second, the positive effect is particularly pronounced for women with high opportunity costs of childbirth-related career interruptions, for example women with tertiary education (Del Boca 2002; Borck 2010). While theory clearly predicts a positive link, empirical evidence at the macro- and micro-level data however shows contradictory results.

Studies at the macro-level consistently find for different countries that public child care has a positive association with various aggregate fertility measures (Bonoli 2008; Bujard 2011; Castles 2003; D'Addio & d'Ercole 2005; Luci-Greulich & Thévenon 2013; Sleebos 2003). With regard to West Germany, a recent study shows a positive effect of an expansion of public child care for children under the age of three on birth rates resulting from two reforms in 2005 and 2008 (Bauernschuster et al. 2016). Using linear regression models with (county-) fixed effects, the results suggest that a 10 percentage points increase in the public child care coverage rate increases the number of births per 1,000 women aged 15 to 44 by on average 1.2. Conveyed to the micro-level, the authors calculate that a 10 percentage points increase leads women to have on average 0.04 more children.

Bauernschuster et al. (2016) attribute this effect to an increase in second and higher-order births, while there is no substantial impact on first births.

However, the mentioned studies have several shortcomings. First, these studies suffer from the general problems resulting from the usage of macro-level data such as the modifiable area unit problem or ecological failure, which further rules out the identification of effect heterogeneities (for an overview, see Dubé & Legros 2014). In the case of fertility, this line of studies does not tie on the level at which economic theory locates the fertility decision. More precisely, these studies do not investigate individual fertility decisions as such (e.g., the likelihood that a woman gives birth). Instead, the macro-level studies model the aggregated results of individual fertility decisions (e.g., the TFR). Furthermore, the child care coverage rate may be endogenous to aggregated fertility if policy makers react on changes in the birth rate with changes in the public provision of child care services.

Conversely, micro-level studies are not prone to the described error sources. Evidence at the micro-level often contradicts the macro-level evidence on a positive causal effect in many countries. Kravdal (1996) illustrates that although the local child care coverage rate is positively associated with third births in Norway, this association becomes negligible if women's employment is controlled for. Havnes & Mogstad (2011) find in a sensitivity analysis accompanying their investigation of the relationship of public child care and maternal employment no association between the child care coverage rate and fertility for Norway as well. Rønsen (2004) shows that the provision of public child care is not associated with fertility in Finland and Norway, a result that also holds true for Sweden (Andersson et al. 2004). Schlosser (2011) finds that the introduction of free public preschool did not increase the fertility of Arab mothers in Israel. In contradiction to prevalent theories, Blau & Robins (1989) reveal that higher child care costs do not decrease the fertility of employed women in the U.S.. For that country, Lehrer & Kawasaki (1985) illustrate that public child care provision is not associated with fertility intentions of two-earner households. Moreover, in a cross-national study on seven European countries, Del Boca et al. (2009) find no association between child care availability and fertility.[1] With regard to West Germany, Hank & Kreyenfeld (2003) and Hank et al. (2004) find no association between child care availability and fertility using multilevel analysis. Based on a dynamic panel data model, Haan & Wrohlich (2011) estimate that child care availability is positively associated with first births, but negatively associated with higher-order births. The authors point to considerable effect heterogeneity, with higher educated women showing the strongest fertility response. By using a calibrated life cycle model, Bick (2010) finds no impact of public child care provision on the fertility of West German married women.

Although micro-level studies overcome the above mentioned shortcomings specific to macro-level data, the outlined micro-level studies similarly pertain methodological problems which we address in this paper. First, many micro-level studies suffer from limited policy variation with respect to public child care supply (e.g., Del Boca 2002; Hank & Kreyenfeld 2003; Hank et al. 2004; Kravdal 1996). We contribute to the micro-level evidence by utilizing significant policy variation resulting from a major child care reform. Second, the studies often do not account for the possibility of selective migration. Parents-to-be could systematically move to areas with higher increase in or level of public child care supply, thereby causing potentially upward-biased estimates of the impact of public child care supply on fertility in various studies (e.g., Del Boca et al. 2009; Schlosser 2011; Lehrer & Kawasaki 1985). We explicitly address this issue within a wide range of sensitivity checks.

[1] However, occasionally some studies do find a positive effect of public child care supply on fertility at the micro-level (e.g., see Del Boca 2002; Mörk et al. 2013).

3 Child care reforms

3.1 Background

Before the child care reforms for children under the age of three took place in the mid-2000s, Germany had already implemented child care policies similar to those of Scandinavian countries. In 1996, the federal government had introduced a legal claim for an (at least half-day) child care slot for children aged three to six. This policy reform resulted in consistently high coverage rates of kindergarten enrollment of at least 85 percent in East and West Germany with little temporal variation until today (Riedel 2007; Statistisches Bundesamt 2015).

At the same time, public child care slots for children under the age of three were severely rationed, given coverage rates close to zero in West Germany in 2002 (Hank et al. 2004).[2] The evidence suggests an excess demand for child care services before the reforms took place in the mid-2000s: for example, Fendrich & Pothmann (2007) estimate that 35 percent of West German mothers with children under the age of three stated demand for a child care slot, which exceeded the supply at that time by far.

3.2 Reform laws

The German federal government gave its first official commitment towards expanding public child care for children under the age of three at the Barcelona summit in March 2002 (Council 2002). Jointly with the other European Union member states, Germany committed itself to achieve a child care coverage rate of at least 33 percent by 2010.[3] This meeting did not result in any concrete terms of reference how to achieve this goal, i.e. with respect to funding, legal requirements, or responsibilities at the (sub-)national level.

[2] In the eastern part of Germany, the former German Democratic Republic, public child care infrastructure was introduced by the socialist regime in the 1970s, resulting in longstanding high coverage rates until today. See Buttner & Lutz (1990) for the fertility effect of this measure. The policy variation resulting from the reform we study has consequently been very limited in East Germany, which is why we focus on West Germany in our study.

[3] At that time, the German government consisted of a left-wing coalition of the Social Democratic Party (SPD) and the Green Party (Bündnis 90/Die Grünen).

As a consequence, German counties were not likely to anticipate the massive resources and responsibilities which they were about to be assigned.

In September 2004, the official draft for the first reform law at the federal level (Tagesbetreuungsausbaugesetz) was published. The reform law passed the German parliament in December 2004 and became effective in January 2005. The explicit goal was a child care coverage rate of 17 percent in West Germany by October 2010. Some 230,000 additional child care slots were estimated to be necessary to achieve this goal (Bundesregierung 2004).

During the child care expansion period between 2005 and 2010, a summit to coordinate child care expansion policies between the three federal levels (Krippengipfel) took place in April 2007: the local authorities, the federal states (Länder), and the federal government stated that a higher child care coverage rate than defined at the Barcelona summit should be targeted. The involved parties aimed to achieve a coverage rate of 35 percent until 2013 by creating 500,000 additional child care slots.[4]

Although the child care reform laws were passed at the federal level, the role of the federal government was rather limited in the child care expansion process: the German government set regulations for child care providers and provided a significant federal grant, but the financial resources for child care expansion were distributed by the federal states. The counties had the ultimate responsibility for child care expansion and administrated the expenditure of the allocated funds.

3.3 Organizing child care for children under the age of three

The newly created child care slots are heavily subsidized and offered at very low fees. On average, child care fees are covered by public subsidies (80 percent), private organizations

[4] The corresponding federal law (Kinderförderungsgesetz) both passed the parliament and became effective in December 2008. Its main component was the legal claim to an (at least half-day) child care slot for all children between the first until the third birthday. The legal claim became effective in August 2013 (Bundesregierung 2008). The current study, however, only investigates the Tagesbetreuungsausbaugesetz of 2005.

(7 percent), and parental contributions (13 percent).[5] The allocated means are substantial: total operating costs of child care for children under the age of three amounted to over 14 billion euros (15.6 billion $) in 2006. In general, parental contributions range between about 0 and 600 euros (670 $) per month and child (Bauernschuster et al. 2016). Furthermore, charges are staggered by family size and income so that poor and/or large families have to pay lower fees. In some cases, such as social assistance receipt of the family, the federal employment agency covers the costs. The fees are not standardized at the federal level since subsidized day care centers are not only run by the counties but by various institutions such as non-profit organizations, religious charities, or private providers (Felfe & Lalive 2013).[6]

4 Identification strategy

The child care expansion involved many complex tasks for local politicians. For example, the counties were responsible for the assessment and projection of local child care demand and for the coordination between the respective authorities. In the late phase of the reform (e.g., 2009-2010), regional differences in the demand can be expected to be the main driver of differing regional public child care coverage growth.[7] By contrast, in the early phase of the reform (e.g., 2005-2008), regional differences in public child care coverage growth are likely to originate from instrumental problems which are arguably independent to changes in micro-level fertility: non-influenceable obstacles such as shortages in construction ground or insufficiently qualified staff for child care centers, which varied

[5] Public subsidies consist of federal, state, and county funds.

[6] Public child care for children under the age of three had been severely rationed before the reforms, followed by long waiting lists for day care centers. A private market for child care did virtually not exist before 2005 due to very strict regulations of child care providers at the state level (Bauernschuster et al. 2016; Felfe & Lalive 2013). For the period between January 2005 and July 2013, that is, before the legal claim became effective, day care centers had to give preference for children of dual earner families or families with unemployed persons on the waiting lists (Bundesministerium für Familie, Senioren, Frauen und Jugend 2004).

[7] For example, when the legal claim became effective in 2013 we still observe striking differences in child care coverage rates (Statistisches Bundesamt 2015).

significantly across counties, exacerbate the expansion (Huesken 2011). This unique situation within the first years of child care expansion serves as the basis for our identification strategy. We therefore choose the year 2008 as the end of our observation period, which is significantly before the end of the public child care expansion period. By that, we only use variation in public child care coverage rates resulting from the first reform law of 2005 (Tagesbetreuungsausbaugesetz). We choose the year 2002 as the beginning of our observation period since this is the closest point in time before the reform for which data on child care coverage rates are available.[8]

In our observation period from 2002 until 2008, we are not aware of any significant changes in family policy which could be relevant for fertility, except the parental leave reform in 2007.[9] The new parental leave benefits, however, were introduced nationwide and at the same time. Thus, this policy change can be controlled for by the time-fixed effects which we will include in the empirical model. Importantly, there was no crowding out of public spending on the major benefits for families such as child benefits due to the child care reforms.

Our methodology consists of a differences-in-differences (DD) framework to identify the causal effect of public child care supply on the individual fertility decisions of German women. Importantly, we do not use this framework to evaluate the impact of the 2005 reform as such on fertility.[10] Our study does not follow the usual logic of the rich body of reform effect evaluation literature which uses DD methodology. Instead, we use the variation in child care coverage rates resulting from the described instrumental problems at the county-level to investigate the general relationship between public child care supply and fertility, which is different from estimating the effect of a single, isolated reform.

[8] Data on child care coverage rates are available for the years 1998, 2002, and 2006-2014.

[9] For the fertility effect of this reform see Raute (2014) and Cygan-Rehm (2016).

[10] This is hardly possible because a second child care reform law (Kinderförderungsgesetz) was passed during the child care expansion period scheduled by the first reform law (2005-2010). To the best of our knowledge, there is no data which would allow us to do so, such as data which differentiates the newly created child care slots by the respective funding source.

Following Havnes & Mogstad (2011), we measure the treatment effect by comparing the change in the average fertility outcome of women living in counties which expand child care coverage extensively to the respective change of counties which expand child care coverage only marginally. More precisely, counties with below-median absolute increase in the child care coverage rate serve as control group, whereas counties with above-median absolute increase in the child care coverage rate serve as treatment group. Our identification strategy is based on the following equation:

$$y_{it} = \beta\, \mathit{treat}_{it} + \gamma\, \mathit{post}_t + \delta\, (\mathit{treat}_{it} \cdot \mathit{post}_t) + {x_{it}}'\, \alpha + \epsilon_{it}, \tag{1}$$

for female individuals $i = 1, ..., N$ observed in time periods $t = 1, ..., T$. The dependent variable y_{it} denotes woman i's fertility outcome in time period t. We consider a binary variable of having a birth 0/1 in the respective year as fertility measure. $treat_{it}$ constitutes a binary variable determining the assignment to treatment and control group. Women living in counties which show above-median absolute increase in child care coverage over time serve as treatment group ($treat_{it} = 1$). Correspondingly, women living in countries which show below-median absolute increase in child care coverage over time serve as control group ($treat_{it} = 0$). The binary variable $post_t$ equals 0 in the pre-reform period, i.e. 2002, and equals 1 in the post-reform period, i.e. 2008. $treat_{it} \cdot post_t$ denotes an interaction term of the variables $treat_{it}$ and $post_t$, which only equals 1 for individuals observed both in the treatment group and in the post-reform period. The corresponding parameter δ quantifies the causal impact of the child care reform on fertility. As control variables (x_{it}) we add socio-demographic characteristics such as household income, year of birth, migration background, education, employment status, and county of residence to the model. Further, county characteristics such as gross domestic product (GDP) per capita, population density, and public debt are included. In our baseline regression we use the ordinary least squares (OLS) technique to estimate equation 1.

5 Data

Our empirical strategy requires the combination of macro- and micro-level data. Variation in child care coverage information over time and between counties serves as a means for treatment assignment at the macro-level. Merging the macro-level data with the micro-level data enables us to identify treatment effects at the individual-level.

5.1 Macro-level data and assignment of treatment status

A key feature of our analysis is the generation of a treatment and a control region at the macro-level.[11] The DJI Regionaldatenbank provides macro-level data on children-, youth- and family-related topics (Bayer 2010). This data base integrates administrative data from different sources. We use this data source to collect macro-level data on yearly child care coverage rates for all West German counties from 1998 to 2010.[12] The child care coverage rate is defined as the number of children under the age of three in subsidized daycare facilities divided by the total number of children under the age of three.[13] In order to define treatment and control group, we calculate the increase in the child care coverage rate between 2002 and 2008 separately for each county. The calculated median increase in child care coverage rates amounts to 8.82 percentage points.[14]

Figure 1 displays the development of child care coverage rates from 1998 to 2010 by treatment status. Both control and treatment group start at roughly the same average coverage rate and experience an upward trend after 2002.[15] However, a gap emerges over

[11] As reliable information on local child care coverage rates at the macro-level exists, we use the same identification strategy as Havnes & Mogstad (2011). Of course, information on child care utilization is available at the micro-level in the German Socio-Economic Panel as well. Nevertheless, the number of mothers with children under the age of three is too small to calculate reliable coverage rates based on the survey data, only.

[12] Data on child care coverage rates is not available for the years 1999-2001 and 2003-2005.

[13] In this study, we use public child care coverage rates as a measure for public child care supply. Of course, the coverage rates do not only reflect the supply side but also the utilization of public child care. Despite that, the number of child care slots can roughly be equated with the number of enrolled children due to the massive excess demand for child care services in our investigated period (Fendrich & Pothmann 2007).

[14] Treatment status does not change substantially if the mean is chosen instead of the median.

[15] Since both groups have on average the same low coverage rate in 2002, selection of parents-to-be into the treatment group is not likely to be a relevant issue in the pre-reform period.

time. Over the time period from 2002 to 2008 the treatment group expands average child care coverage rates by 6.22 percentage points more than the control group does.[16]

Figure 2 depicts the development of aggregate fertility, i.e. the number of births per 1,000 women in the age group 20 to 45, by treatment status. For the entire period of time we observe the average fertility of the control group to be higher than the average fertility of the treatment group. Aggregate fertility decreases until 2006, but experiences an upward trend in the subsequent years in both groups. The constant gap between control and treatment group in the pre-reform period provides suggestive graphical evidence for a common time trend with respect to fertility before the reform. Furthermore, we observe that the gap between control and treatment counties slightly narrows in the post-reform period.

5.2 Micro-level data and sample selection

The micro-level data we use is the German Socio-Economic Panel (SOEP), which contains rich individual information on for example labor market participation, education, migration, and fertility. We use the SOEP Remote System (Knies & Spiess 2007) to merge the micro-level data with the administrative macro-level data on child care coverage rates. This procedure allows us to determine the assignment of individuals to either treatment or control group.

We use the birth biography data set of the SOEP to collect information on the female respondents' fertility. The data comprises information on the calendar year and month of every birth the respondents gave in the past. We define the individual fertility outcome as

[16] The average child care coverage rate in the treatment group amounts to 2.31 in 2002 and to 15.00 in 2008. Respective values for the control group are 2.01 in 2002 and 8.48 in 2008. Thus, the increase in the average child care coverage rate is $(15.00 - 2.31) - (8.48 - 2.01) = 6.22$ percentage points higher for the treatment group.

a binary variable measuring whether or not a woman gave birth in the respective calender year (birth 1/0). Births comprise any form of parity (first, second, and higher order births).[17]

Key covariates exploited in the regression are defined and coded as follows. The household's income is measured as the real net monthly equivalent income, i.e. the real net monthly income divided by the square root of the number of persons living in the household. Further control variables are the cohort (coded as indicator variables for each cohort) and whether or not the individual has a direct or indirect migration background (migrant 1/0). The highest schooling degrees are categorized into the following four groups: high (*Abitur* and *Fachabitur*), middle (*Realschule*), basic (*Hauptschule*), and other. In addition, we define the binary variable university degree 1/0. Indicators for full-time employment, regular part-time employment, vocational training, marginal and irregular part-time employment, and no employment account for the individual employment status. In addition, 325 county fixed effects and time-varying county characteristics such as GDP per capita, population density, and public debt add to extended specifications of the model.

Following the sample selection procedure of the existing literature, our sample consists of married females aged 20 to 45 who reside in West Germany. We chose to focus our analysis on married women since this group shows a higher likelihood of fertility intentions.[18] The baseline analysis restricts the sample to the pre- and post-reform periods 2002 and 2008. Furthermore, observations without information on included covariates are deleted (about 9% of the sample). Our final sample comprises 3,722 individual-year observations.[19]

Table 1 displays in column (1) to column (4) the sample means of the dependent variable and the covariates by treatment status and time period. Table 2 depicts the differences

[17] In our analysis, we do not differentiate between different parities since the number of birth events is too low to investigate specific parities in our subsample estimations.

[18] The share of legitimate births on the total number of births was close to 80 percent in our investigated reform period (Pötzsch & Emmerling 2008).

[19] We observe 2,257 women in 2002 and 1,465 women in 2008. In 2002, 1,142 women belong to the treatment group, whereas in 2008 714 women belong to the treatment group. In our sample, we observe 2,026 women in one of the two periods and 848 women as repeated observations in both periods.

in means between treatment group, control group, and time periods as well as the results of t-tests we ran to check whether the calculated differences are equal to zero. Columns A and B display differences in means between post- and pre-reform periods for the treatment and the control group, respectively. We observe that both the treatment and the control group experience significant temporal variation within our observation period in the child care coverage rate ($p<0.01$) and in other variables such as the GDP per capita or the employment status. Columns C and D show differences in means between the control and the treatment group for pre- and post-reform periods, respectively. Both in the pre-reform period as well as in the post-reform period there is significant spatial variation between the treatment and the control group. For instance, Column C shows that the treatment group has a significantly higher share of women with a university degree in the pre-reform period ($p<0.05$). This could violate the exogeneity assumption of cross-sectional models and prohibits for example the usage of the simple OLS estimator. However, most importantly, the treatment and the control group do not differ in their intertemporal variation on the observed variables (see column E). For example, the treatment and the control group do not experience significantly different trends in secondary schooling ($p>0.10$). Column E shows that the treatment status is exogeneous with respect to the intertemporal changes of all observed variables. Only the change in the child care coverage rate differs significantly between the treatment and the control group ($p<0.01$). Both provides suggestive evidence of the suitability of our chosen DD framework.

6 Empirical results

6.1 Main results

Table 3 presents our main results in three separate specifications. We estimate a pooled ordinary least squares model for two survey years (2002 and 2008) and stepwise include socio-demographic correlates of fertility, county-fixed effects, and time-varying county characteristics. In all specifications listed in this paper, standard errors are clustered at the county level and robust to heteroscedasticity.[20] The baseline model presented in column (I) controls for the family income, cohort fixed effects, migration background, education, and employment status. The model given in column (II) further comprises county fixed effects. In column (III) we additionally include time-varying county characteristics such as the GDP per capita, population density, and public debt.

The table illustrates that the estimates of the treatment effect, i.e., the coefficient of the interaction term between the treatment region indicator and the post-reform indicator, are rather stable. In all specifications the models yield statistically non-significant and positive effects of public child care expansion on the probability of having a birth within the respective calender year. For example, the estimate of our fully specified model presented in column (III) suggests that the 4.96 percentage points increase in the child care coverage rate in the treatment counties relative to the comparison counties in our sample (see Table 1) caused a 1.57 percentage points increase in the probability of having a birth.

To provide a more common interpretation of our results, the estimates need to be divided by the difference in increases in average child care coverage rates between treatment and control countries, i.e., 4.96 percentage points. Thus, column (III) suggests that if the child care coverage rate increases by one percentage point, the probability of having a birth increases by 0.32 ($0.0157/4.96 = 0.0032$) percentage points. Nevertheless, the estimated effect is statistically non-significant (p>0.10).

[20] Results do not change if we apply standard errors clustered at the individual level or robust standard errors only. We also estimated logit models to check whether our results are sensitive to the chosen estimation method. The sign and significance of the logit and OLS estimates were identical in all model specifications (results not shown here).

6.2 Subsample estimations

In order to check effect heterogeneities, we run separate estimations for different educational, ethnic, and age groups. Table 4 displays our subsample estimations with all covariates exploited in the model.[21] We find that the estimates for the treatment effect differ slightly by educational and migration background as well as by age.

With respect to education, a one percentage point increase in the child care coverage rate only causes a 0.26 ($0.0125/4.89 = 0.0026$) percentage points increase in the probability of having a birth for those without a tertiary degree. The respective effect for those who have a tertiary degree amounts to 1.11 ($0.0577/5.18 = 0.0111$) percentage points. A similar pattern appears when regarding different secondary schooling degrees. For all educational groups the estimated treatment effects are not statistically significant different from zero.

With respect to migration background, we observe a small effect for natives, the effect for women with migration background is nonetheless close to zero. These effects are, however, not significantly different from zero. The estimate suggests for immigrants that a one percentage point increase in the child care coverage rate only causes a 0.13 ($0.0062/4.79 = 0.0013$) percentage point increase in the probability of having a birth. The analysis of different immigrant groups seems promising, but our limited sample size does not allow to investigate these heterogeneous effects further.

Although we did not find that public child care expansion causes a fertility response in the short-run, there might be a significant effect in the long-run. Nevertheless, investigating this long-run effect by widening the time frame would be critical: the instrumental problems, which have caused the variation in child care coverage rates between counties within the first years of the child care expansion (Huesken 2011) are likely to be already

[21] The presented results are comparable to the results of the fully specified model given in column (III) of Table 3. The table further shows the differences in increases in average child care coverage rates between treatment and control countries for the respective groups which are needed to interpret the group-specific effects correctly.

solved in later years. For example, issues like spikes in local demand for child care services or other factors whose relationship to fertility is unclear might drive the variation in the years close before the legal claim became effective (e.g., 2013). Thus, in the late phase of child care expansion the variation in child care coverage rates might not be exogenous anymore. Therefore we desist from investigating the long-run effect. Nevertheless, separate regressions by age groups while keeping the time frame narrow can give suggestive evidence for a time-lagged impact. While the younger age group (age 20 to 30) might show a time-lagged fertility response of the reform, the older age group (age 31 to 45) is likely to react faster because of the foreseeable closing biological window which remains to realize fertility intentions. For the younger age group, the estimate suggests that a one percentage point increase in the child care coverage rate causes a decline of 0.93 ($-0.0461/4.95 = -0.0093$) percentage points in the probability of having a birth. Conversely, for the older age group the sign of the treatment effect is positive. Again, we do not find evidence for heterogeneity in the statistically insignificant treatment effect since the corresponding p-values are greater than 0,1.

7 Robustness tests

In order to test the robustness of our main results, we performed eight sensitivity checks with the corresponding results given in Table 5.

First, we check whether our results are sensitive to the chosen observation period. Therefore, we add one pre-reform wave and one post-reform wave to the estimation sample. While in the baseline specification we chose a two-waves (years 2002 and 2008) specification of our DD estimation, we exploit both the years 2002 and 2003 as pre-reform period and both the years 2008 and 2009 as post-reform period in a robustness test. Panel A of Table 5 indicates that the inclusion of the years 2003 and 2009 only marginally effects our main results.

Second, we check how the results change when we exploit an alternative definition of treatment assignment. Our baseline DD estimations are based on a specific mechanism which assigns counties to either the treatment or the control group. We defined treatment (control) counties as those counties whose increase in the child care coverage rate between the years 2002 and 2008 was above (below) the median increase of all counties over that time period. Thus, our DD results may be sensitive to the chosen mechanism of assignment. In a robustness test we define treatment counties as those whose increase in the child care coverage rate lies above the 75 percent percentile (12.12 percentage points) and control counties as those whose increase lies below the 25 percent percentile (6.73 percentage points). Thereby excluding counties whose increase lies between the 25 and 75 percent percentile, this new definition leads to a larger difference in increases in average child care coverage rates between treatment and control counties than in our baseline model. Panel B of Table 5 shows that the estimate slightly increases due to this new definition but continues to be non-significant.

Third, we address the issue of selective migration into treatment counties, which could be a potential source of bias of our estimated treatment effect. Parents-to-be could systematically move to counties with higher increase in or level of child care coverage. Such behavior might generate reverse causality: if fertility is associated with higher child care coverage rates, our models would then yield upward-biased estimates. Instead of the current calender year, we use in this robustness test the county in which a woman lived in the year 2000 to assign her to the treatment or control group. Panel C of Table 5 shows that the results barely change if we exploit the county of residence five years before the reform took place to determine the assignment to the treatment or control group. This result is compatible with two studies addressing selective migration with respect to public child care infrastructure in West Germany which utilized a different methodology (Bauernschuster et al. 2016; Felfe & Lalive 2013).

Fourth, we check whether our results hold if we apply a broader age definition to the women in our sample. Our estimation sample in the baseline model consists of women between 20 and 45 years of age since teenagers and women above the age of 45 rarely give birth in Germany (The World Bank 2016a). Nevertheless, as the reproductive phase is somewhat longer we also checked whether our results are sensitive to the chosen age span. We therefore include women between 17 and 49 years of age in our new sample. Panel D of Table 5 shows that our estimates remain mostly unaffected.

Fifth, we include unmarried women in our sample. The estimation sample of the baseline model comprises only married women. Our results might be sensitive to this decision. Panel E of Table 5 indicates that the estimates slightly decrease and remain statistically insignificant if unmarried women are included.

Sixth, we investigate how our results change if we exclude the largest cities in West Germany. Our main results could be driven by changes between rural and urban counties or respective migration patterns. For instance, there was a trend among young and wealthy individuals to move into urban areas during our investigated time period (Geppert & Gorning 2010). This trend might be related to fertility. Therefore, we exclude women living in West German cities counting more than 500,000 inhabitants (Hamburg, Munich, Cologne, and Frankfurt) from the sample. Panel F of Table 5 shows that our results are basically unaffected.

Seventh, we estimate the effect of a placebo reform. A central assumption of our DD estimates is that treatment and control group follow the same time trend in fertility in the absence of child care expansion. To check whether our estimates are biased by differential time trends, we estimated the effect of a placebo reform which hypothetically took place in the pre-reform period. Panel G of Table 5 presents the re-estimation of our modified baseline model, where the variable $post_t$ equals 0 for the year 2001 and equals 1 for the year 2004. The estimation yields a very small (0.0088) and insignificant ($p>0.10$) coefficient, suggesting that the time trends in treatment and control group were basically the same with respect to fertility.

Eighth, we switch from our baseline model with a dichotomous treatment status variable to a model with a continuous treatment variable. Instead of the treatment indicator, the post-reform indicator, and the corresponding interaction term we include the local public child care coverage rate as a continuous variable and year indicators in the empirical model. We thereby avoid problems regarding the specific definition of treatment and control group and exploit the full variation in public child care coverage rates. Additionally we include the survey waves of 2006 and 2007 in order to increase our sample size.[22] Panel H of Table 5 indicates that the estimate is still statistically non-significant ($p>0.10$).

To sum up, our specification checks show that the positive and statistically non-significant effect of public child care expansion on the probability of having a birth prevails across different model specifications.

8 Summary and conclusion

In this article we examine whether there is an impact of public child care supply on fertility decisions. Although microeconomic theory predicts a positive effect, we show that the existing empirical evidence is inconclusive. To empirically investigate this relationship, we draw on a policy change in a country with initially low public child care supply and low fertility. We utilize a child care reform in West Germany in 2005 which allocated massive federal funds for the purpose of public child care expansion. Due to the administration of these funds by the counties, this reform led to considerable temporal and spatial variation of public child care coverage rates.

The methodology we apply utilizes this exogenous variation to investigate the impact of public child care supply on fertility. Our differences-in-differences analysis of combined administrative county-level data on public child care coverage rates and micro-level panel survey data does not indicate a significant, immediate fertility response of married women. Subsample estimations do not reveal any heterogeneities in this effect. Finally, we show that there is no evidence for selective migration with respect to regional public

[22] Data on child care coverage rates are not available for the years 2003 to 2005.

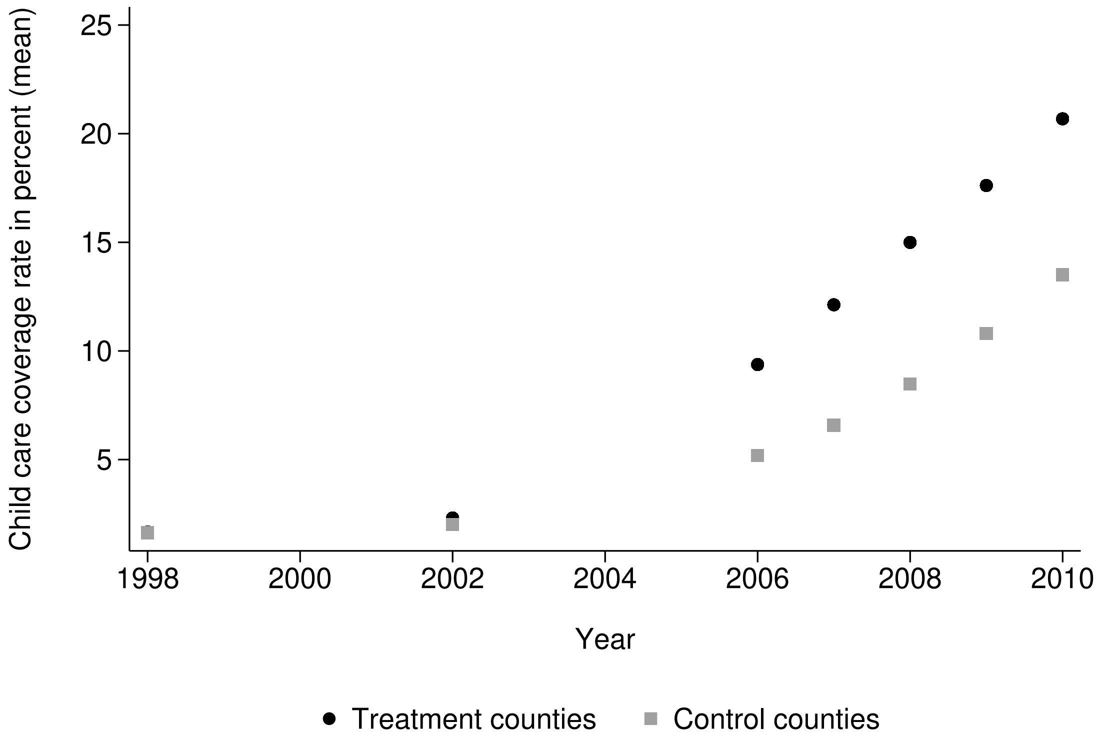
child care supply. Our study is in line with most micro-level studies on the issue, but contradicts existing macro-level evidence which overwhelmingly shows a positive impact. Future research should explain this contradiction which we have shown to prevail across many studies on the relationship between public child care and fertility.

Despite this, the qualitative finding that increasing public child care provision does not spike fertility prevails: for instance, a recent macro-level study estimates the overall effect of the same reform on the West German TFR, which has been between 1.3 and 1.4 for decades, to be 0.12 (Bauernschuster et al. 2016). This effect is by far too low to reach for example Norway's TFR (1.8) or the replacement level TFR which is around 2.1 for Germany (Espenshade et al. 2003). The obviously weak positive or non-existent impact of public child care supply in West Germany is in line with evidence from other countries (Gauthier 2007).

These results should be noticed by policy makers and the scholars who have argued in favor of exporting the "Scandinavian model" of extensive family-friendly policies as such or specific elements of it to other countries (e.g., see Del Boca 2002; Kreyenfeld et al. 2002; Spiess & Wrohlich 2005). Consider that fertility, maternal employment, and other outcomes are subjects of public interest on which the Scandinavian countries perform comparatively well. We argue that adopting Scandinavian policies will not necessarily help other countries to quickly achieve this performance. The universal public child care provision as a main element of these policies has turned out not to be an effective means for that with respect to fertility in the short-run. There is growing evidence that the same holds true for maternal employment (Havnes & Mogstad 2011). Both examples show that policy makers should be careful with respect to such policy adaption or policy imitation attempts. However, we cannot conclude at this point that public child care expansion is an ineffective means to increase fertility since we only investigated short-run effects. We leave the final verdict to future research, which should address long-term effects of public child care supply.

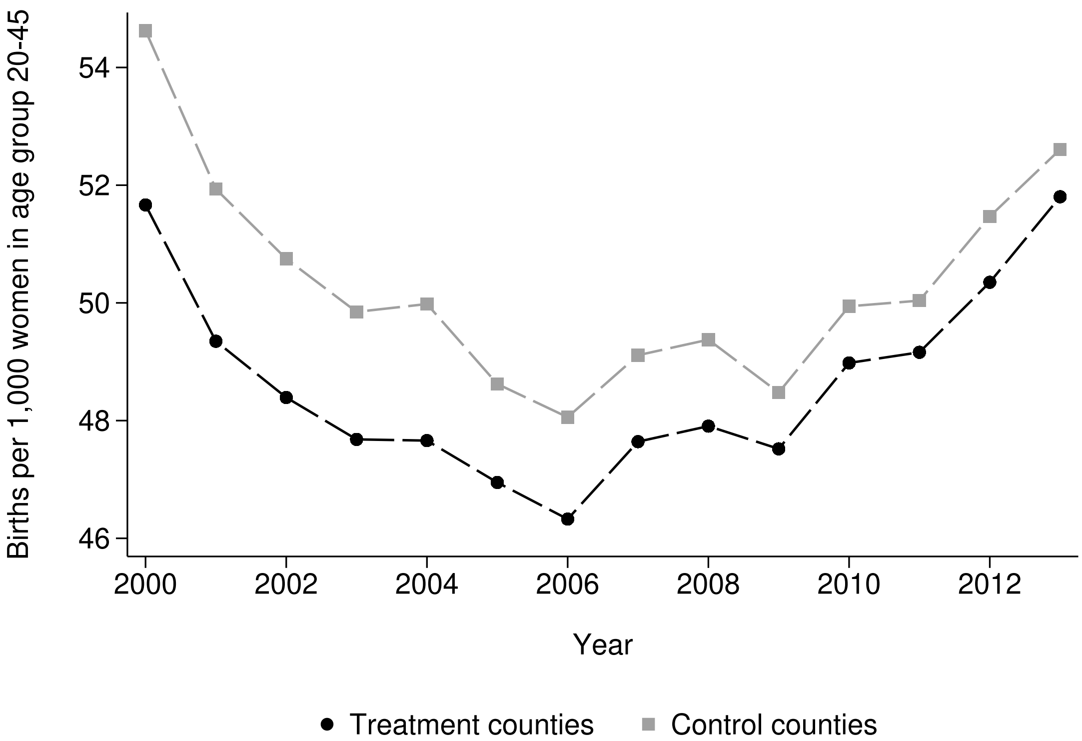
9 Appendix: figures and tables

Figure 1: Child care coverage rates in treatment and control counties, in percent



Notes: The figure displays the development of child care coverage rates over the years 1998 to 2010 separately for treatment and control counties. Counties with above-median increase in child care coverage rates serve as the treatment group, whereas counties with below-median increase in child care coverage rates serve as the control group.
Source: Statistical Offices of the German Länder (Statistische Landesämter) for the years 1998, 2002, 2006-2010; own calculations.

Figure 2: Births per 1,000 women aged 20 to 45 in treatment and control counties



Notes: The figure displays the development of births per 1,000 women aged 20 to 45 over the years 2000 to 2013 separately for treatment and control counties. In the raw data, we observe aggregate fertility for all West German counties over the respective time period. Counties with above-median increase in child care coverage rates serve as the treatment group, whereas counties with below-median increase in child care coverage rates serve as the control group.
Source: Statistical Offices of the German Länder (Statistische Landesämter) for the years 2002 and 2008; German Federal Statistical Office for the years 2000-2013; own calculations.

Table 1: Sample means by treatment status and time period

	Treated before (1)	Treated after (2)	Non-treated before (3)	Non-treated after (4)
Birth 0/1	0.056	0.069	0.065	0.068
Child care coverage rate	2.682	12.474	2.043	6.873
Household income	2056.925	1883.373	1830.171	1767.169
Cohort	1965.506	1970.445	1965.390	1970.715
Migrant 0/1	0.287	0.284	0.268	0.276
University degree 0/1	0.163	0.203	0.128	0.153
Secondary schooling				
High school degree 0/1	0.272	0.314	0.229	0.277
Middle school degree 0/1	0.354	0.359	0.352	0.386
Basic school degree 0/1	0.246	0.217	0.272	0.228
Other 0/1	0.128	0.111	0.147	0.109
Employment status				
Full-time 0/1	0.229	0.218	0.195	0.201
Regular part-time 0/1	0.322	0.375	0.309	0.324
Vocational training 0/1	0.007	0.004	0.002	0.001
Irregular part-time 0/1	0.095	0.120	0.100	0.160
No employment 0/1	0.347	0.282	0.394	0.314
County characteristics				
Population density	786.960	839.299	719.371	702.013
GDP per capita (in 1,000)	29.015	33.320	24.952	29.080
Debt (in 1,000,000)	0.397	0.374	0.315	0.345
Number of observations	1,142	714	1,115	751

Notes: The table depicts means, differences in means, and the differences in differences of means for exploited variables. We use t-tests to check whether the differences equal to zero. ***, **, and * indicate p-values smaller than 0.01, 0.05, and 0.1. The definitions of exploited variables are given in Section 5.2.
Source: Statistical Offices of the German Länder (Statistische Landesämter) and SOEP for the years 2002 and 2008; own calculations.

Table 2: Differences in sample means

Differences (see Table 1)	(2)-(1)	(4)-(3)	(3)-(1)	(4)-(2)	Diff-in-diff
	A	B	C	D	E
Birth 0/1	0.013	0.003	0.009	-0.001	0.009
Child care coverage rate	9.792 ***	4.830 ***	-0.639 ***	-5.601 ***	4.962 ***
Household income	-173.552 ***	-63.001	-226.754 ***	-116.203 *	-110.550
Cohort	4.939 ***	5.325 ***	-0.116	0.270	-0.386
Migrant 0/1	-0.003	0.007	-0.019	-0.009	-0.010
University degree 0/1	0.040 **	0.025	-0.035 **	-0.050 ***	0.015
Secondary schooling					
High school degree 0/1	0.041 *	0.048 **	-0.044 **	-0.037	-0.007
Middle school degree 0/1	0.005	0.034	0.001	0.028	-0.029
Basic school degree 0/1	-0.029	-0.044 **	0.026	0.011	0.015
Other 0/1	-0.017	-0.038 **	0.019	-0.001	0.021
Employment status					
Full-time 0/1	-0.010	0.006	-0.034 **	-0.017	-0.017
Regular part-time 0/1	0.053 **	0.014	-0.013	-0.052 **	0.039
Vocational training 0/1	-0.003	-0.001	-0.005 **	-0.003	-0.002
Irregular part-time 0/1	0.025 *	0.059 ***	0.005	0.039 **	-0.034
No employment 0/1	-0.065 **	-0.079 ***	0.047 **	0.033	0.014
County characteristics					
Population density	52.339	-17.358	-67.589 *	-137.286 ***	69.697
GDP per capita (in 1,000)	4.305 ***	4.127 ***	-4.063 ***	-4.241 ***	0.178
Debt (in 1,000,000)	-0.024	0.030 **	-0.082 ***	-0.028	-0.054
Number of observations	1,856	1,866	2,257	1,465	3,722

Notes: Columns A and B display differences in means between post- and pre-reform periods for the treatment and the control group, respectively. Columns C and D show differences in means between the control and the treatment group for pre- and post-reform periods, respectively. Column E displays the difference of column A and column B which is equivalent to the difference of column C and column D. We use t-tests to check whether the differences equal to zero. ***, **, and * indicate p-values smaller than 0.01, 0.05, and 0.1. The definitions of exploited variables are given in Section 5.2.
Source: Statistical Offices of the German Länder (Statistische Landesämter) and SOEP for the years 2002 and 2008; own calculations.

Table 3: Effect of public child care on fertility

	(I)	(II)	(III)
Treat·Post	0.0130	0.0133	0.0157
	(0.0147)	(0.0152)	(0.0152)
Socio-demographic characteristics	yes	yes	yes
County fixed effects	no	yes	yes
County characteristics	no	no	yes
Number of observations		3,722	

Notes: Each coefficient represents a separate linear regression. All regressions include a constant, the treat and post indicators. Socio-demographic characteristics include the covariates family income, cohort (32 indicators), migration background 0/1, schooling (4 indicators) and tertiary degree 0/1, and employment status (5 indicators). Standard errors are clustered at the county level.
Source: Statistical Offices of the German Länder (Statistische Landesämter) and SOEP for the years 2002 and 2008; own calculations.

Table 4: Heterogeneous treatment effects

	Treatment effect	Standard error	Diff-in-diff child care coverage rate	Number of observations
Tertiary education				
Tertiary degree	0.0577	0.0650	5.183	589
No tertiary degree	0.0125	0.0154	4.886	3,133
Secondary schooling				
High school degree	0.0425	0.0477	4.688	998
Middle school degree	0.0438	0.0308	4.670	1,343
Basic school degree	0.0012	0.0293	5.581	910
Other	-0.0136	0.0666	5.444	471
Migration background				
No migration background	0.0280	0.0225	5.037	2,685
Migration background	0.0062	0.0339	4.790	1,037
Age				
Between 20 and 30	-0.0461	0.0919	4.951	582
Between 31 and 45	0.0223	0.0158	4.959	3,140

Notes: The table presents heterogeneous treatment effects with coefficients, corresponding standard errors, the difference in the differences in child care coverage rate (comparable to the last column of Table 2) for the respective group, and the number of observations given in the first, second, and third column. Estimation results refer to the model with all covariates exploited (comparable to model (III) in Table 3). The first panel splits the sample by tertiary education of the woman, whereas the second panel gives treatment effects for different highest schooling degrees of the woman. Panel three shows treatment effects by migration background; panel four splits the sample into younger (age 20 to 30) and older (age 31 to 45) women. All regressions include a constant, the treat and post indicators. Socio-demographic characteristics include the covariates family income, cohort (32 indicators), migration background 0/1, schooling (4 indicators) and tertiary degree 0/1, and employment status (5 indicators). Standard errors are clustered at the county level.
Source: Statistical Offices of the German Länder (Statistische Landesämter) and SOEP for the years 2002 and 2008; own calculations.

Table 5: Sensitivity tests

	(I)	(II)	(III)
Panel A: Pooled regression			
Treat·Post	0.0061	0.0057	0.0111
	(0.0109)	(0.0113)	(0.0113)
Number of observations		7,226	
Panel B: Alternative treatment assignment			
Treat·Post	0.0210	0.0167	0.0214
	(0.0208)	(0.0224)	(0.0240)
Number of observations		1,714	
Panel C: Selective migration into treatment counties			
Treat·Post	0.0076	0.0104	0.0121
	(0.0146)	(0.0150)	(0.0153)
Number of observations		3,342	
Panel D: Broader age group			
Treat·Post	0.0104	0.0117	0.0141
	(0.0120)	(0.0122)	(0.0121)
Number of observations		4,570	
Panel E: Inclusion of unmarried women			
Treat·Post	0.0120	0.0102	0.0126
	(0.0105)	(0.0110)	(0.0110)
Number of observations		6,268	
Panel F: Exclusion of large cities			
Treat·Post	0.0140	0.0143	0.0159
	(0.0151)	(0.0157)	(0.0154)
Number of observations		3,582	
Panel G: Placebo reform			
Treat·Post	0.0154	0.0128	0.0088
	(0.0143)	(0.0153)	(0.0155)
Number of observations		4,136	
Panel H: Continuous treatment variable			
Treat·Post	0.0005	0.0025	0.0030
	(0.0007)	(0.0019)	(0.0019)
Number of observations		7,209	
Socio-demographic characteristics	yes	yes	yes
County fixed effects	no	yes	yes
County characteristics	no	no	yes

Notes: The table presents the treatment effect, the corresponding standard error, and the number of observation for each of our eight robustness tests. All regressions include a constant, the treat and post indicators. Socio-demographic characteristics include the covariates family income, cohort (32 indicators), migration background 0/1, schooling (4 indicators) and tertiary degree 0/1, and employment status (5 indicators). Standard errors are clustered at the county level.
Source: Statistical Offices of the German Länder (Statistische Landesämter) for the years 2002 and 2008; and SOEP for the years 2000-2004, 2006-2009; own calculations.

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