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DOCTORAL THESIS

Essays in Applied Microeconomics

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Declaration of Authorship

I, Mischa KLEIS, declare that this thesis titled, “Essays in Applied Microeconomics”, submitted to Trinity College Dublin for the degree of Doctor in Philosophy (Ph.D.), has not been submitted as an exercise for a degree at this or any other university and it is entirely my work.

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Summary

This dissertation consists of three essays in applied microeconomics.

The first essay studies the effects of access to assisted reproductive technologies (ARTs) on fertility and labor market outcomes. Drawing on quasi-experimental variation in expected costs for ARTs and data from the German Microcensus, I find a weakly significant decrease in the probability to give birth for German women aged 30 to 34 in former East-Germany, while women's labor force participation remains unaffected. This result is in line with the hypothesis that access to infertility treatments may ease women's perception of the career-family tradeoff and may motivate them to postpone motherhood. For West-Germany, the findings suggest that while there is no significant impact of the policy intervention on German women's fertility behavior, it motivates women aged 30 to 34 to withdraw from the labor market - albeit the effect is only marginally significant. This result does not align with the theoretical considerations. In combination with the results on males, these observations challenge the validity of the obtained estimates in terms of reflecting the true effects of subsidizing ARTs on German women.

The second essay examines the impact of stricter regulation of a single input to the production of child care on the structural quality of the child care environment and children's development. Using a novel recording of legislative changes to maximum child-to-staff ratios (CSR) in early child care (ECC) – care for children aged 0-2 – I find that the CSR reforms have no statistically significant effect on the observed CSR. Further, my analysis uncovers no impact of the stricter regulations on other aspects of structural quality in child care, such as the qualification of pedagogical staff or the degree of staff fluctuation. Accordingly, I cannot confirm the presence of a child care market where regulations are enforced and binding. These results also suggest that child care providers refrain from engaging in cost-saving input substitution. Further, I show that the CSR reforms have no significant effect on children's development. Hence, the essay hints at a potentially limited scope of such policies to improve children's development.

The third essay contributes to the literature on sociodemographic segregation in child care participation and social mobility. Relying on my compilation of regional reforms to legislated CSR in ECC and representative survey data on ECC enrollment, I study the impact of

increased minimum quality standards for the provision of child care on sociodemographic enrollment gaps in ECC. I find that increased minimum standards in ECC had no significant effect on enrollment gaps by parental education and migration background. This suggests that such a policy may not be an appropriate measure to reduce segregation in child care enrollment and to improve social mobility.

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Contents

Declaration of Authorship	iii
Summary	v
Acknowledgements	vii
1 The Effects of Assisted Reproductive Technology on Fertility and Labor Market Outcomes	1
1.1 Introduction	1
1.2 Literature review	4
1.3 Theoretical framework	6
1.4 Institutional setting	8
1.5 Data	14
1.5.1 German fertility trends	14
1.5.2 Balancing	15
1.5.3 German Microcensus	18
1.6 Estimation strategy	19
1.7 Estimation results	23
1.8 Robustness Checks	23
1.8.1 Alternative definitions	23
1.8.2 The effects of subsidizing ARTs on males	25
1.8.3 Fixed effects	27
1.9 Conclusion	29
Appendices	33
1.A Data sources and definitions of variables	33

1.B	Full results from balancing regressions	35
1.C	States and counties by border and treatment status	44
2	Child-to-Staff Ratio Regulation and Child Development	45
2.1	Introduction	45
2.2	Institutional Background	49
2.2.1	Early Child Care in Germany	49
2.2.2	Child Care Quality Regulations in Germany	50
2.2.3	Reforms of the Child-to-Staff Ratio	52
2.3	Theoretical Framework	53
2.3.1	Theory on the Intensity of Staff Deployment and Further Determinants of ECC Quality	53
2.3.2	Theory on Child Development	55
2.4	Data	58
2.4.1	The Ländermonitor	59
2.4.2	The German Socio Economic Panel (SOEP)	59
2.4.3	Data from the Federal Statistical Office, the Federal Institute for Research on Building, Urban Affairs and Spatial Development, and the Betreuungsatlas	63
2.5	Estimation Strategy	63
2.5.1	Determinants of Adoption	64
2.5.2	Impact on the Intensity of Staff Deployment and Further Determinants of Child Care Quality	66
2.5.3	Impact on Child Development	71
2.6	Robustness Checks	74
2.6.1	Placebo Estimates: Impact on the Intensity of Staff Deployment and Further Determinants of Child Care Quality	74
2.6.2	Imputation Estimator: Impact on Child Development	78
2.7	Conclusion	81
	Appendices	83

2.A Synthetic Control Weights: Impact of CSR Reforms on Child Care Quality . .	83
2.B Impact of CSR Reforms on Child Development	86
3 Child Care Quality and Enrollment Gaps in Early Child Care	87
3.1 Introduction	87
3.2 Institutional Background	91
3.2.1 Early Child Care in Germany	91
3.2.2 Child Care Quality in Germany	94
3.2.3 Reforms of the Child-to-Staff Ratio	95
3.3 Data	97
3.3.1 The German Child Care Study (KiBS)	98
3.4 Estimation Strategy	104
3.4.1 Impact on Enrollment by Education	105
3.4.2 Impact on Enrollment by Migration Background	107
3.5 Robustness Checks	108
3.5.1 Placebo Estimates: Impact of the CSR reform in Bavaria on Sociode- mographic Enrollment Gaps	108
3.5.2 Placebo Outcome: Impact of the CSR reform in Bavaria on ECC De- mand by Sociodemographic Background	114
3.6 Conclusion	116
Appendices	119
3.A Synthetic Control Method: Weights of Control States and Predictor Variables	119
3.B Summary statistics by West-/East-Germany	123
3.C ECC Demand by Reason For Not Using Child Care by West-/East-Germany .	124
Bibliography	127

List of Figures

1.1	Wage trajectory conditional on the timing of family formation	7
1.2	Timeline of subsidizing ARTs in German states	10
1.3	Geographical distribution of state-level subsidies	13
1.4	Development fertility rates of German women by age group in West- and East-Germany	15
2.1	Impact of CSR Reforms on Intensity of Staff Deployment	68
2.2	Impact of CSR Reforms on Share staff without degree	69
2.3	Impact of CSR Reforms on Share Staff Employed Full-Time Deployment .	70
2.4	Impact of CSR Reforms on Child Development	74
2.5	Placebo Estimates: Impact of CSR Reforms on Intensity of Staff Deployment	76
2.6	Placebo Estimates: Impact of CSR Reforms on Share staff without degree	77
2.7	Placebo Estimates: Impact of CSR Reforms on Share Staff Employed Full-Time Deployment	78
3.1	Impact of CSR Reform on Enrollment by Education in Bavaria	106
3.2	Impact of CSR Reform on Enrollment by Migration Background in Bavaria	108
3.3	Placebo Estimates: Impact of CSR Reform on Enrollment by Education in Bavaria	111
3.4	Placebo Estimates: Impact of CSR Reforms on Enrollment by Migration Background in Bavaria	113
3.5	Impact of CSR Reform on Demand by Education in Bavaria	115
3.6	Impact of CSR Reforms on Demand Gap by Migration Background in Bavaria	117

List of Tables

1.1	States subsidizing infertility treatments	10
1.2	Summary of balancing tests by region	18
1.3	Summary statistics	19
1.4	The effect of subsidizing ARTs on the probability to give birth at balanced borders	24
1.5	The effect of subsidizing ARTs on the probability of labor force participation at balanced borders	24
1.6	Alternative definition: The effect of subsidizing ARTs on the probability to give birth at balanced borders	26
1.7	Alternative definition: The effect of subsidizing ARTs on the probability of labor force participation at balanced borders	26
1.8	Males: The effect of subsidizing ARTs on the probability of labor force participation at balanced borders	27
1.9	Border FE: The effect of subsidizing ARTs on the probability to give birth at balanced borders	28
1.10	County FE: The effect of subsidizing ARTs on the probability to give birth at balanced borders	29
1.11	Border FE: The effect of subsidizing ARTs on the probability of labor force participation at balanced borders	30
1.12	County FE: The effect of subsidizing ARTs on the probability of labor force participation at balanced borders	30
1.B.1	Results from Balancing Regressions by Subsample	35
1.B.2	Balancing Tests: West-West Bordering Counties, Pre-treatment	38
1.B.3	Balancing Tests: East-East Bordering Counties, Pre-treatment	41

1.C.1	States and counties by border and treatment status	44
2.1	Child-Staff-Ratio Age 0-2 Reforms in Germany	53
2.1	Mean differences: Treated and Control from SOEP data	62
2.1	Determinants of Adoption	65
2.1	Imputation Estimator: Impact of CSR Reforms on Child Development . .	80
2.A.1	Synthetic Bavaria Weights: Impact of CSR Reforms on Child Care Quality	83
2.A.2	Synthetic Berlin Weights: Impact of CSR Reforms on Child Care Quality .	84
2.A.3	Synthetic Brandenburg Weights: Impact of CSR Reforms on Child Care Quality	84
2.A.4	Synthetic Thuringia Weights: Impact of CSR Reforms on Child Care Quality	85
2.B.1	Impact of CSR Reforms on Child Development	86
3.1	Child-Staff-Ratio Age 0-2 Reforms in Germany	97
3.1	Summary statistics	98
3.2	ECC Enrollment and Demand by Parental Sociodemographic Background	99
3.3	ECC Demand by Reason For Not Using Child Care	101
3.4	Reasons For Not Using Child Care by Sociodemographic Background . .	104
3.1	Post/Pre MSPE Ratio of Placebo Estimates: Impact of CSR Reforms on Enrollment in Bavaria	110
3.A.1	Synthetic Bavaria Weights: ECC Enrollment by Parental Sociodemographic Background	119
3.A.2	Synthetic Bavaria Predictor Variables: ECC Enrollment (Less-educ)	120
3.A.3	Synthetic Bavaria Predictor Variables: ECC Enrollment (More-educ) . . .	120
3.A.4	Synthetic Bavaria Predictor Variables: ECC Enrollment gap (Education) .	121
3.A.5	Synthetic Bavaria Predictor Variables: ECC Enrollment (Not German) . .	121
3.A.6	Synthetic Bavaria Predictor Variables: ECC Enrollment (German language)	122
3.A.7	Synthetic Bavaria Predictor Variables: ECC Enrollment gap (Migration) .	122
3.B.1	Summary statistics West-Germany	123
3.B.2	Summary statistics East-Germany	123
3.C.1	ECC Demand by Reason For Not Using Child Care in West-Germany . .	124

3.C.2	ECC Demand by Reason For Not Using Child Care in East-Germany . . .	125
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Chapter 1

The Effects of Assisted Reproductive Technology on Fertility and Labor Market Outcomes

1.1 Introduction

When assessing the attitude of young women towards work life and family formation, it appears that many prefer the idea of exploiting their career opportunities over starting a family early (Bundesministerium für Familie, Senioren, Frauen und Jugend, 2014). With respect to fertility behavior in developed countries, this impression is backed by the statistics.¹ During the period from 2001 to 2012, women's average age at first birth in Germany increased from about 27 to 29 years (Kreyenfeld et al., 2011; Statistisches Bundesamt, 2012).² While this behavioral adjustment may enable more women to follow a career track, it leads to a more frequent occurrence of fertility problems at the time of intended family formation (Passet-Wittig et al., 2016).

At present, one in ten couples in Germany is affected by infertility and 25% of Germans report to be involuntarily childless (Bundesministerium für Familie, Senioren, Frauen und

¹Since fertility behavior is linked to social and economic development, the phenomenon of women postponing family development in order to pursue a career is mainly observed in developed countries. In developing countries that experience increases in female education and economic change, which leads to higher incomes and a stronger motivation to accumulate human capital, the opportunity cost of having children early rises. As a result, women in developing countries may reschedule their family development if societal norms and institutions permit (Bongaarts and Potter, 2013).

²In 2021, average age at first birth in Germany was about 31 years (Statistisches Bundesamt, 2023).

Jugend, 2015).³ In many cases, this medical condition manifests itself in severe physical and mental stress. For affected couples, the use of infertility treatments is considered as a promising last resort. With almost every fourth performed treatment cycle leading to a pregnancy in Germany on average, the chances of success seem to be reasonable (Geremek, 2014).⁴ But, limited cost coverage on the part of health insurance providers poses a financial burden on couples. Moreover, couples with insufficient financial means at their disposal may be factually excluded from access to infertility treatments (Bundesministerium für Familie, Senioren, Frauen und Jugend, 2015). Also for this reason, the member states of the European Union (EU) are requested to '[...] ensure the right of couples to universal access to infertility treatment' (European Parliament, 2008).⁵ Apart from this call on the EU-level, five states within Germany recently introduced subsidies for assisted reproductive technologies (ARTs) to break down financial barriers associated with the utilization of infertility treatments.

This paper studies the effects of subsidizing ARTs in Germany on fertility and female labor market outcomes. In general, the policy measure is expected to help couples with fertility problems to fulfill their desire to have children. In addition, women may reschedule their career and family development as fertility time horizons are reaching into later stages of their work life. For career-oriented women, making a decision on when to become a mother occurs amidst 'two competing forces' (Buckles, 2007) that emerge in the career-family trade-off.

Avoiding an interruption at early stages of a career motivates women to start a family later. The empirical evidence (see Miller, 2011; Kleven, Landais, and Sogaard, 2015; Adda, Dustmann, and Stevens, 2015) suggests that women with career breaks must expect lower life-time earnings. With women increasingly working in professional and highly paid professions, temporary absence of women from the labor force is nowadays more strongly penalized than it was in the past. Because of this, many career-oriented women wish to

³The official definition for clinical infertility according to the WHO is as follows: 'Failure to achieve a clinical pregnancy after 12 months or more of regular unprotected sexual intercourse.' (World Health Organization, 2018).

⁴In the US, 35% of performed treatment cycles result in a pregnancy (Geremek, 2014).

⁵This European Parliament resolution is a guideline for the member states.

postpone motherhood aspiring to accumulate human capital or ‘[...] obtain promotion before taking off for childbearing’ (Kroeger and La Mattina, 2015). However, being aware that with age comes a declining fertility and an increasing risk that a new-born child might face health issues, women feel pressure to become mothers earlier (Buckles, 2007).

With respect to the career-family tradeoff, the German policy measure probably promotes female career investments by mitigating the accompanying risk of infertility. As a result, younger women potentially regarding the subsidies for ARTs as ‘[...] insurance against age-related infertility’ (Gershoni and Low, 2015) may respond by scheduling family formation for an even later age. This implies that subsidizing ARTs may induce age-specific effects. In theory, older women of childbearing age, who are more likely to experience fertility problems, may show a higher probability to give birth and reduce their labor supply, while the opposite may be observed for younger women. Accordingly, the intervention increases demand for infertility treatments today as well as planned demand in later periods.

This paper is related to previous research on the consequences of introducing cost coverage for ARTs (Buckles, 2007; Schmidt, 2007; Ohinata, 2011; Lahey, 2012; Abramowitz, 2012; Machado and Galdeano, 2015; Gershoni and Low, 2015). It contributes by being the first to examine potential effects on women’s family and career planning for Germany. Compared to the large majority of the literature, which focuses on mandates that legally require employers to cover ARTs via employer-sponsored health insurance plans in the US, I study the German setting where subsidies for infertility treatments are financed by the regional public budget.

Both policy measures may have effects on fertility and indirectly influence female labor supply in response to changes in fertility. However, beyond that, the policy interventions in the US altered female labor market outcomes due to changes in labor demand and due to a response of labor supply, which was not only induced by fertility. For employers, the cost of employing older women of childbearing age who are more likely to draw on infertility treatments increased. As a result, labor demand for this group of women decreased. At the same time, labor supply among women in this group was positively affected by the additional employee benefit. Within the context of subsidizing ARTs in Germany, female labor market outcomes are not explicitly affected by changes in employment costs and employee benefits.

Consequently, this paper contributes to the literature by studying the German context where these channels are less impacted.

Empirically, I rely on a difference-in-differences (DD) approach to investigate changes in fertility and female labor market behavior. My identification strategy exploits variation in access to ARTs induced by the regional introduction of subsidies for ARTs in Germany. The policy measure generates variation across states and over time as the financial support is introduced in different states at different times. According to my preferred specification, which includes county fixed-effects, subsidizing ARTs is weakly related to a decrease in the probability to give birth of German women aged 30 to 34 without significantly affecting women's labor force participation in former East-Germany. Moreover, while fertility behavior of women in West-Germany appears to be unresponsive to the policy measure, there is a significant negative effect on labor force participation of German women aged 30 to 34 - albeit at a low level of statistical significance. The remaining of this paper is structured as follows: Section 2 provides an overview of the related existing literature. Section 3 describes the underlying theory. Section 4 introduces the German institutional setting in detail. Section 5 presents the data. In Section 6 describes the estimation strategy. Section 7 shows estimation results, while robustness checks are presented in Section 8. Finally, Section 9 presents a conclusion.

1.2 Literature review

The previous literature addresses the impact of an increase in cost coverage for ARTs on a variety of outcomes. With the exception of Gershoni and Low (2015) studying a policy change in Israel, research to date exclusively draws on the introduction of mandates to cover ARTs in the US.

According to the earlier theoretical considerations, subsidizing ARTs may result in age-specific changes in fertility and labor market outcomes of women. As regards the first-mentioned, Schmidt (2007) employs data from the Vital Statistics and the US Census to study first birth rates. The results obtained in an OLS difference-in-differences (DD) framework indicate that white women over 35 are responsive to the policy measure. For this subsample of women, higher cost coverage for infertility treatments appears to be associated with

an increase in first birth rates. This early finding may highlight the crucial role the policy measure plays in helping couples suffering from fertility problems.

Another related strand of the literature refers explicitly to the career-family tradeoff and focuses on investigating the timing of fertility. Correspondingly, Ohinata (2011) provides an insight into altered fertility behavior due to a shift of career interruptions. The paper addresses the subject with a proportional hazard model capturing the probability of becoming a mother at a given age. Further, the author primarily relies on the Panel Study of Income Dynamics (PSID) and the Current Population Survey (CPS). Showing that highly-educated white women exposed to the policy measure start a family significantly later in life, the study provides evidence that mandated insurance coverage for ARTs provokes women to postpone motherhood. Related research includes Abramowitz (2012), who uses a similar empirical framework, and Machado and Galdeano (2015) featuring an application of the synthetic control method to evaluate birth certificates from the National Vital Statistics System. Both studies confirm the earlier finding suggesting that financial support for ARTs is related to a delay in first birth among younger women.

Taking into consideration that the timing of giving birth has repercussions on labor supply, a small literature deals with the effect of mandated coverage for ARTs on female labor market outcomes. Buckles (2007) draws on CPS data and analyzes the percentage of women who were in the labor force in the week before being interviewed. Results from a probit model indicate increased (decreased) labor force participation for women in treated states before (after) age 35. This supports the idea that women may in fact delay having a child to follow a career track. By highlighting the interaction of labor demand and supply, the work of Lahey (2012) provides an important addition to the discussion of female labor market outcomes in this field. The author argues that cost coverage for ARTs results in older women of childbearing age becoming relatively more expensive to employ. However, there is an imbalance with the value of the mandates to this group of women being lower than the accompanied costs. Consistent with this hypothesis, using data from the CPS in an OLS triple difference (DDD) model, the findings indicate that women aged 28 to 42 experience a decrease in weeks worked and are – compared to the remaining working-age population –

more likely to not be in the labor force. In contrast to Buckles (2007), the revealed labor supply consequences are expected to not be induced by motherhood and accompanying career interruptions. According to the study, the effects originate from a decrease in wages that is not strong enough to compensate for a reduced labor demand and, thus, is unable to keep the amount of labor employed constant.

1.3 Theoretical framework

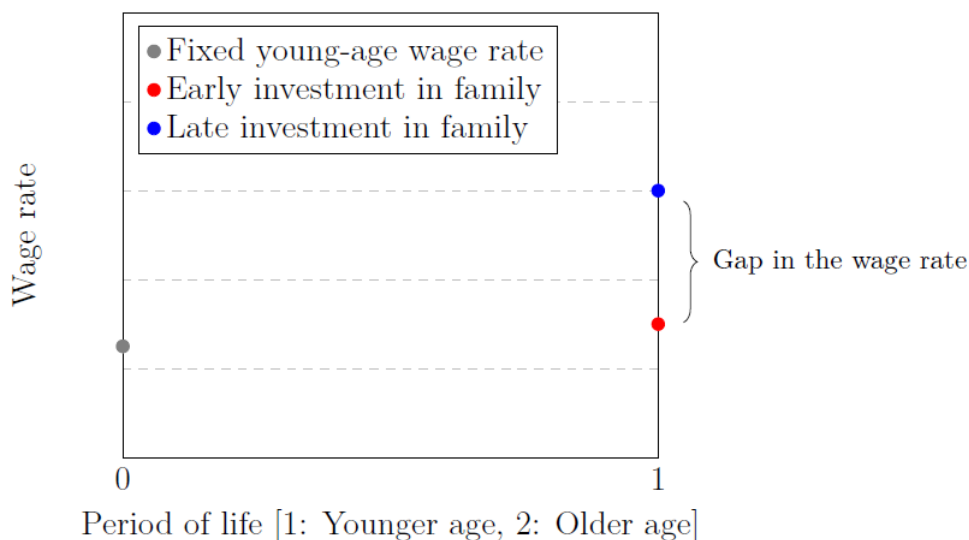
This section outlines a theoretical framework from which implications on the fertility and female labor market consequences of subsidizing ARTs can be derived. My framework borrows from Abramowitz (2017), who applies the theory of consumer and household behavior to the analysis of fertility behavior when facing a price change for ARTs. This theory, originally formulated in Becker (1993), models the decision-making process of a single individual that takes pleasure in consuming goods and spending free time on a variety of activities, but is constrained by income and time. Because these ‘pleasures’ require monetary and temporal expenditures, the individual must find the optimal balance between working in the market place and taking time off. While sacrificing time for work reduces available free time, it also translates into income that pays consumption and monetary costs of activities.

In the context of the career-family tradeoff and a co-payment to ARTs, a woman’s utility is determined by consumption and investment in family. The model considers two periods of life - the younger and the older childbearing age - and a woman chooses between an early or late family formation, which involves an interruption of work of a constant duration. For simplification, the utility derived from investment in family is independent of the period of life it takes place in.⁶ For each period, the respective time constraint ensures that total time available equals her time spent working plus time invested in family. At the young age, time worked is remunerated with a predetermined wage, while the older-age wage rate is conditional on whether the woman took a maternity-related break from work at the young age. Considering the positive relationship between human capital formation on the job and earnings established by Becker (1962), the model assumes that an absolute focus on working

⁶The potential preference of a woman for having a child early (late) can be included in the model by increasing (decreasing) the ‘[...] cutoff value[s] on the wage trajectory distribution [...]’ (Abramowitz, 2017).

at a young age is rewarded in the form of a higher wage (see Figure 1.1). Correspondingly, Miller (2011) finds reasons as to why becoming a mother early leads to a comparably lower subsequent wage in forfeited returns to experience and the depreciation of human capital.

FIGURE 1.1: Wage trajectory conditional on the timing of family formation



On the budget side, the income constraint states that a woman exhausts her potential income, which includes her non-labor income and earnings from market work, on consumption and investment in family. The price of consumption is normalized to 1 and constant over time. Family formation, however, involves expenditures that vary between the two periods. To take into account that a woman's fertility declines with age, monetary expenditures enter the model. These refer to the co-payment for ARTs and occur only - but necessarily - at the older age. It is assumed that a woman holds perfect information about this monetary expenditure. In addition, a fixed temporal expenditure accrues independent of age, which constitutes the opportunity cost of family formation. Because the opportunity cost is valued at '[...] the time used to produce utility' (Becker, 1993) by pursuing activities, this cost amounts to the wage rate.

Maximizing a woman's utility function with respect to the above-mentioned time and income constraints reveals two scenarios. Each scenario describes conditions that if fulfilled result in a woman starting a family early or late, respectively. In the first scenario, the loss associated with having a child is smaller at a young age than when being older. Mathematically, this implies that the sum of earnings foregone at a young age due to early investment

in family and the wage trajectory of the older-age period weighs less than earnings lost while investing in family at an older age plus the monetary expenditure for ARTs. When facing this first scenario, utility is maximized by having a career break at the young age and working the entire older-age period. In the second scenario, the opposite holds true with investment in family being more costly at a young age than when being older. Consequently, a woman focuses on working in the early period of life, while the older-age period makes provisions for family formation.

These theoretical considerations imply the existence of a cut-off value on female older-age wage trajectories, which determines whether women opt for early or late investment in family. As shown in Figure 1.1, the wage trajectory is quantified by the gap in the realized later-period wage rate between a woman investing early in family and her starting a family when being older. The second scenario with late family formation and the use of ARTs is observed for women whose careers feature a gap in the older-age wage rate greater than the cut-off value, hence, for women that can expect a significant reward for more work time at a young age. With respect to subsidizing ARTs, this implies that the probability to end in the second scenario becomes higher because the cut-off value of the older-age wage gap is reduced. Consequently, it is expected that a larger group of women decides to postpone having a child.

1.4 Institutional setting

In 2004, on the legal basis of the Statutory Health Insurance Modernization Act (in German, *Gesetz zur Modernisierung der gesetzlichen Krankenversicherung*), the German legislators adopted measures to permanently lower contribution rates of employers to the statutory health insurance (AOK Bundesverband, 2018). This cost reduction effort resulted in insured individuals being entitled to fewer health insurance benefits and facing significantly higher co-payments (Steinbronn, 2005).⁷ Before the health care reform was enacted, couples undergoing infertility treatments could expect to be practically fully reimbursed for incurred

⁷The majority of individuals in Germany is insured by the statutory health insurance. According to Dietrich and Wevers (2010), 86%, respectively 70.4 million people, were members of a statutory health insurance scheme in 2007.

costs.⁸ The new legislation stipulated that 50% of the costs for the first three treatment cycles are borne by the insurants, while eliminating any financial support for subsequently performed treatment cycles (Dietrich and Wevers, 2010).⁹

The overall costs for ARTs generally comprise physician consultations, services and prescribed drugs. In the case of a 50% co-payment, individuals making use of ARTs typically incur costs of about EUR 1500 (EUR 1700) per IVF (ICSI) treatment cycle. Correspondingly, a couple's out-of-pocket costs for a fourth, or any one subsequent non-funded cycle, may amount to EUR 3000 or more (Kinderwunschzentrum Altonaer Straße Hamburg, 2017). With a success rate per treatment cycle of around 23% in Germany, the reduction in health insurance benefits potentially confronted individuals facing infertility with significant financial challenges (Geremek, 2014).¹⁰ It comes as no surprise that subsequent years saw a decline of more than 50% in the number of performed treatment cycles and associated births (Dietrich and Wevers, 2010; Landtag Nordrhein-Westfalen, 2013).

As a measure for reducing the financial burden on individuals seeking infertility treatment, five of the 16 German states implemented subsidies for ARTs in the recent past. Henceforth, this paper refers to these five states as the 'treated states' and to the remaining states as the 'control states'. Figure 1.2 displays the timeline of subsidizing ARTs in Germany, while Table 1.1 features a detailed overview of the level of funding and the expected remaining co-payment per treatment cycle for treated and control states. Saxony (in 2009) and Saxony-Anhalt (in 2010) were the first two states to adopt state-level directives. In 2013, as a consequence of a federal directive, Lower Saxony, Mecklenburg-Vorpommern and Thuringia introduced financial support for ARTs.¹¹

⁸Before 2004, statutory health insurers provided a 100% coverage of costs for the first four treatment cycles. Moreover, no age requirements applied (Deutscher Bundestag, 2009).

⁹Since 2004, statutory health insurance providers offer a partial reimbursement of costs only if couples fulfill certain requirements, e.g. being married, the female applicant being aged from 25 to 39 and the male applicant being aged from 25 to 49. Refer to Book V of the Social Code (in German, *Sozialgesetzbuch Fünftes Buch*) available at Bundesministerium der Justiz und für Verbraucherschutz (2017) for a list of all requirements.

¹⁰The success rate per treatment cycle is here defined as the occurrence of a pregnancy.

¹¹To this day, besides the treated states, Berlin and Hesse arranged for subsidizing ARTs. While couples in Berlin benefit from financial funding since 2015, Hesse provides cost coverage since 2018. For the moment, I exclude the introductions of subsidies for ARTs in Berlin and Hesse from my analysis, because potential effects on fertility and labor market behavior set in at the end or out of the sample period.

FIGURE 1.2: Timeline of subsidizing ARTs in German states

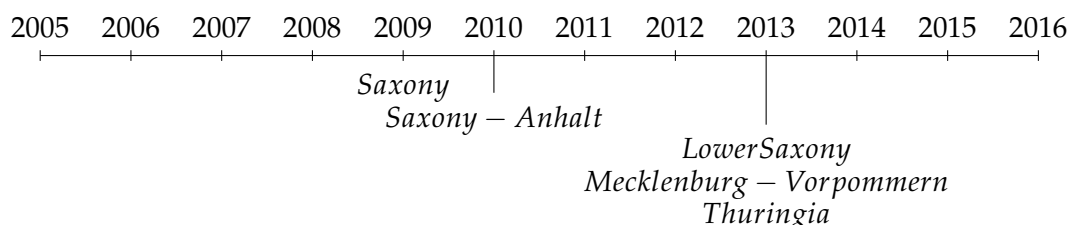


TABLE 1.1: States subsidizing infertility treatments

State Year subsidy adopted	Level of funding and expected remaining co-payment (per cycle) ^a			
	1st IVF/ICSI	2nd IVF/ICSI	3rd IVF/ICSI	4th IVF/ICSI
Saxony 2009	Level of funding			
Saxony-Anh. 2010	No subsidy	Max. EUR 800-900		Max. EUR 1600-1800
	Expected remaining co-payment			
	EUR 1520-1685	EUR 720-785		EUR 1440-1570
Lower Saxony Mecklenburg-V. Thuringia Saxony 2013	Level of funding			
	Max. EUR 750-900			Max. EUR 1600-1800
	Expected remaining co-payment			
	EUR 720-785			EUR 1440-1570
Saxony-Anh. 2014	Level of funding			
	Max. EUR 800-900			No subsidy
	Expected remaining co-payment			
	EUR 720-785			EUR 3040-3370
Control states ^b	Level of funding			
	No subsidy	No subsidy	No subsidy	No subsidy
	Expected remaining co-payment			
	EUR 1520-1685			EUR 3040-3370

Notes: The table shows German states, which introduced state-level support for ARTs, considered in my study. The following states adopted directives on financial support, but are excluded here: Berlin (2015) and Hesse (2018).

^a The expected remaining co-payment is the difference between expected costs for a treatment cycle of a married couple with statutory health insurance receiving a cost coverage of 50% by their health insurance provider and the maximum funding in the respective state. Kinderwunschzentrum Altonaer Straße Hamburg, 2017 provides figures for expected costs per treatment cycle.

^b The group of control states comprises: Baden-Wuerttemberg, Bavaria, Brandenburg, Bremen, Hamburg, Hesse, North Rhine-Westphalia, Rhineland-Palatinate, Saarland and Schleswig-Holstein.

Initially, in Saxony and Saxony-Anhalt every second to fourth treatment cycle was eligible for supplemental funding. However, in the course of the federal government establishing its own funding program in 2013, the two previously existing state-level directives were revised. Accordingly, the newly joined treated states Lower Saxony, Mecklenburg-Vorpommern and Thuringia, as well as Saxony (since mid 2013) subsidized the first to fourth treatment cycle, while Saxony-Anhalt (since early 2014) restricted financial support to the first to third cycle.

With respect to costs borne by a couple, the subsidies reduced the co-payment for a subsidized first to third IVF (ICSI) cycle by a maximum of EUR 800 (EUR 900) and for a subsidized fourth IVF (ICSI) cycle by a maximum of EUR 1600 (EUR 1800). Equivalently, this implies that eligible individuals in the treated states may lower their co-payment for the first three subsidized treatment cycle by about 25% and for a subsidized fourth cycle from 100% to about 50%.

The directives of the treated states stipulate that the provision of financial support for ARTs is conditional on the applicants meeting a number of eligibility requirements, which partially vary by state and over time. In all experimental states the female applicant must be aged from 25 to 39 and the male applicant must be aged from 25 to 49. Further, in most experimental states the condition of applicants being married was imposed. Only Saxony-Anhalt, as a step towards promoting equal rights for unmarried couples, required couples to be in a consensual union. In 2016, respectively 2017, all other experimental states adopted this up-to-date guideline by revising their directives. The directives also stipulate that requesting couples must be residents of the respective subsidizing federal state.¹² In addition, except for Saxony-Anhalt until 2014, all experimental states regulate where treatment may be performed to be regarded as eligible. This implied that infertility treatments were eligible only if being provided by a fertility clinic within the respective federal state. In Lower Saxony (since 2014) and Thuringia (since 2016) also treatments carried out in neighboring federal states are eligible for financial support. Lastly, Saxony, only temporarily, and Lower Saxony impose the condition that the couple has no entitlement to benefits from a private health insurance company.

¹²Until 2013, the directive of Saxony included the additional clause of being a resident for at least one year.

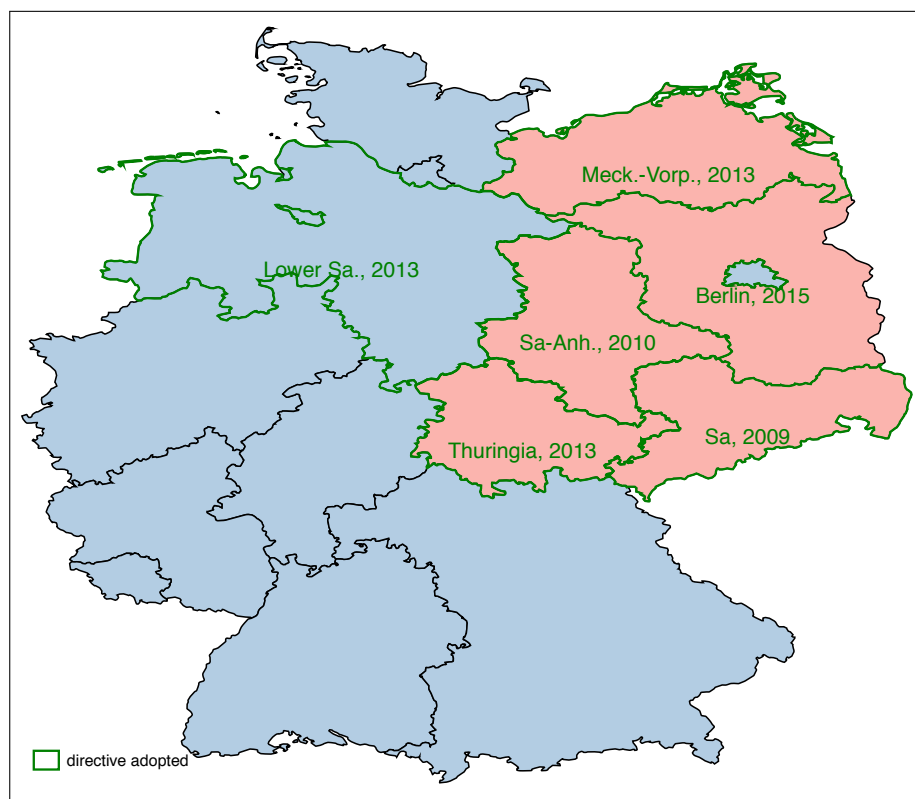
The actual utilization of the state-level funding is reflected in the number of subsidized treatment cycles. While roughly 550 treatment cycles were financially supported in 2009 by Saxony, in 2010 the figure increased by around 50% to a total of about 850 cycles as Saxony-Anhalt adopted the policy measure. In 2013, couples residing in the five experimental states already received funding for around 4600 treatment cycles and by 2016 the figure increased by more than 25% to 5900 subsidized cycles.¹³ Unfortunately, there is no information provided on the state-level with respect to the total number of completed treatment cycles (subsidized plus non-subsidized).

When studying regionally introduced policy measures, it is crucial to address the possibility that the decision-making process was endogenously driven. This means that there is the concern that state-level characteristics correlated with fertility and female labor market behavior determine whether a state is observed as a treated or control state. Since the prevailing economic and social environment of a German federal state is strongly related to its geographical location, it is valuable to investigate the geographical distribution of the introduced subsidies. Figure 1.3 shows a systematic pattern with the treated states being geographically located in the north-east of Germany. With the exception of Lower Saxony, all treated states are new federal states of Germany (in German, *Die neuen Bundesländer*). Before joining the Federal Republic of Germany (FRG) during the German reunification of 1990, these states belonged to the German Democratic Republic (GDR).¹⁴ It is known that the partition of Germany introduced profound differences in '[...] value structures, attitudes and beliefs' Goldstein and Kreyenfeld (2011) between the new and old states of Germany, which are related to the career-family tradeoff. Moreover, the states differ in key economic characteristics. Consequently, an important exercise of this paper lies in working out a credible counterfactual.

¹³The official figures on subsidized treatment cycles are: 552 (2009), 858 (2010), 923 (2011), 865 (2012), 4630 (2013), 5226 (2014), 4770 (2015), 5926 (2016). The large increase in subsidized treatment cycles observed from 2012 to 2013 may be explained by the fact that Lower Saxony, Mecklenburg-Vorpommern and Thuringia introduced financial support for ARTs in 2013.

¹⁴Hesse, one of the two treated states excluded from my study, is an old state of Germany. The second, Berlin is not officially termed a new federal state, but its areas of former East Berlin and West Staaken were previously part of the GDR.

FIGURE 1.3: Geographical distribution of state-level subsidies



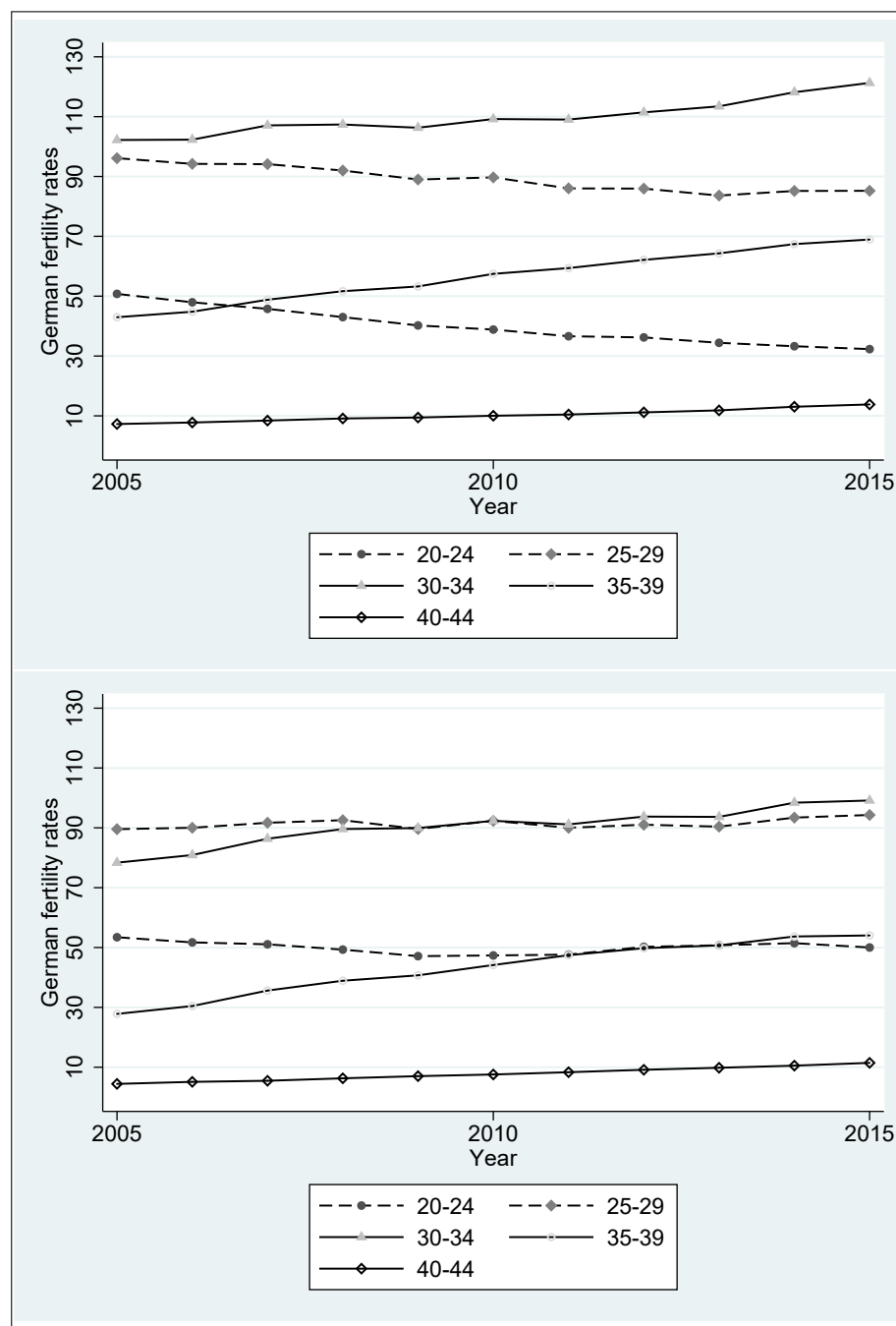
Notes: Figure displays the geographical distribution of adopted state-level subsidies in Germany. States that introduced subsidies for ARTs bordered in green. The year of adoption follows the name of the state. Subsidies introduced after 2013 are excluded from my analysis. Former FRG (GDR) territories highlighted in blue (red). Figure borrowed from Kleis (2017).

1.5 Data

1.5.1 German fertility trends

To get a first picture of fertility trends in Germany, I calculate age-group-specific fertility rates by combining information on birth counts with female population estimates. Figure 1.4 displays the development of German fertility rates by age group and separately for former West- and East-Germany. It becomes apparent that in West- and East-Germany fertility at younger ages is declining over time, while fertility at an advanced childbearing age is increasing. Accordingly, both regions show negative trends in fertility rates for the age group 20 to 24 and positive trends in fertility rates for the age groups 30 to 34 up to 40 to 44. Only for 25 to 29 year-olds, West- and East-Germany experienced differential developments. This may be the result of women increasingly pursuing professional careers and traditionally longer career interruptions following child birth in West than East-Germany. Accordingly, starting a family in West-Germany is relatively more strongly penalized in the labor market, which depresses current West-German fertility rates in this age group. While there is a drop in fertility in West-Germany, fertility rates in this age group remained quite stable in East-Germany over time. Considering the absolute rate of change in fertility, it can be seen that for most age groups the trends are more pronounced in former West- than East-Germany.

FIGURE 1.4: Development fertility rates of German women by age group in West- and East-Germany



Notes: Figure displays the development of birth rates of German women from 2005 to 2015. Source: Own calculation based on data from GSO.

1.5.2 Balancing

Motivated by the skewed geographical distribution of the subsidies outlined previously, this section addresses the potential endogeneity of the policy measures by carrying out balancing tests and proposing an approach to overcome the potential identification problem. In a

first step, I analyze how treated and control states compare in terms of potential determinants of fertility and female labor market behavior. At this stage, I run linear regressions of the following form:

$$x_i = \gamma + p \times D_i + v_i,$$

where the outcome variable x_i is a single characteristic used for comparison and D_i is a dummy variable dividing states into a treatment and control group. The coefficient of interest, p , reveals any significant differences in observables between treated and control states. Given the fact that most treated states are new federal states, which are characterized by distinct fertility and female labor market behavior for historical reasons, I further explore whether differences persist when restricting the sample to states from former West-Germany and East-Germany, respectively. An overview of the results of these regressions, which are carried out separately for 42 selected county-level characteristics, is displayed in panel A of Table 1.2.¹⁵ The county-level refers to *Landkreise* and *kreisfreie Städte*, which are regional authorities below the state-level that align with the level-3 administrative units in NUTS 3. For Germany, i.e. the unrestricted sample, the balancing tests reveal highly significant differences between treated and control states in 32 out of the 42 characteristics. Further, these differences in observables appear to be robust to restricting the sample to former West-German states as the regressions still show 28 highly significant coefficients. The sample of East-German states, however, is more balanced with considerably fewer significant differences.

Taking a closer look at the composition of uncovered differences between treated and control states, it can be seen that treated counties included in the unrestricted and West-German sample feature higher (lower) birth rates at a younger (older) age, a higher total fertility rate (TFR) as well as lower GDP levels and lower population density. The treated counties of the East-German sample are characterized by lower marriage and divorce rates, a higher TFR, a lower educational attainment of residents and less childcare utilization. This imbalance in county-level characteristics potentially represents a fundamental heterogeneity in the prevailing fertility and labor market environment between states that introduced

¹⁵Full regression results for the unrestricted, the West-German and the East-German sample are shown in the Appendix A.2.

the subsidies and the control states. The possibility that observables cannot fully capture differences in underlying career-family environments, poses a serious threat to identification. To take this potential identification problem into account, I draw on the discontinuous change in the availability of subsidies for ARTs at administrative borders in Germany. To the extent that there is no discontinuous change in the fertility and labor market environment at these state borders, analyzing outcomes of women residing in bordering counties will result in unbiased estimates.

Accordingly, I identify inner borders between German federal states and their bordering counties. Subsequently, I run regressions from the equation above separately for each state border that includes at least one treated state with the dummy variable D_i now indicating the side of a state border a county is located on. The results of these regressions are shown in panel B and C of Table 1.2.¹⁶ Table 1.C.1 in the Appendix displays states and counties by treatment status and by border. It should be noted that the number of counties of a given border is quite low. This implies that due to small sample sizes, the output on significant coefficients must be read with caution. When examining the results at West-German borders, it can be seen that all borders are more balanced than the unrestricted sample of West-Germany. However, within this group of borders, the borders of Lower Saxony to Bremen and Hamburg are quite unbalanced, while the other three borders appear to be more homogeneous in terms of the selected characteristics. For the East-German borders, the picture is mixed. In comparison to the full East-Germany sample, only the borders of Saxony/Saxony-Anhalt and Saxony-Anhalt/Thuringia are strictly more balanced. To ensure identification while facing a substantial imbalance in county-level characteristics between treated and control states, my empirical analysis resorts to the most balanced borders of my sample. My main sample includes only borders that show differences that are significant on the 1% (5%) level in less than 8 (11) out of the 42 characteristics. Applying this cutoff leads to a sample of counties from the following five borders: Lower Saxony/Schleswig-Holstein, Lower Saxony/Hessia, Lower Saxony/North Rhine-Westfalia, Saxony/Saxony-Anhalt and Saxony-Anhalt/Thuringia.

¹⁶Full regression results for all included state borders are shown in the Appendix A.2.

TABLE 1.2: Summary of balancing tests by region

Treated/Control	Number counties	Vars p<0.01	Vars p<0.05	Vars p<0.10	Main sample
Panel A:					
Whole Germany	383	32	35	36	No
West-Germany	316	28	31	33	No
East-Germany	66	8	8	11	No
Panel B: Borders West-Germany					
Lower Saxony/Schleswig-H.	3	0	8	9	Yes
Lower Saxony/Hessia	4	1	4	5	Yes
Lower Saxony/North Rhine-W.	18	4	10	11	Yes
Lower Saxony/Hamburg	3	6	11	22	No
Lower Saxony/Bremen	6	18	27	30	No
Panel C: Borders East-Germany					
Saxony/Saxony-A.	4	4	7	8	Yes
Saxony-A./Thuringia	11	7	8	12	Yes
Mecklenburg-V./Brandenburg	5	4	14	20	No
Saxony/Brandenburg	7	6	15	17	No
Saxony/Thuringia	7	9	10	14	No
Saxony-A./Brandenburg	7	10	13	23	No

1.5.3 German Microcensus

For studying fertility and labor market behavior in Germany, I resort to data from the German *Micro Census*. By covering about 380,000 households with 820,000 individuals, this dataset represents the largest household survey in Europe. The *Micro Census* features a representative sample size of 1% of the population in Germany and provides annual cross-sectional information on the individual level.¹⁷ The total of interviews is distributed evenly across the year, which implies a floating reference week. I use the *Micro Census* waves 2005 until 2016 and focus on German women aged from 20 to 49 years. Each woman is assigned to her relevant five-year age group. This results in the following age groups: 20 to 24, 25 to 29, 30 to 34, 35 to 39, 40 to 44 and 45 to 49. In my empirical analysis, I test whether women's career and family planning is affected by the state-level directives and allow the response to vary across age groups. It is only women in the second to fourth age group that meet the age requirements for financial support in the treated states. However, women aged 20 to 24 may adapt their behavior in anticipation of being eligible in the near future. At the same time,

¹⁷Since 2012, the data from the *Micro Census* can also be merged and analyzed - with limitations - as a rotational panel. Following the concept of partial rotation, survey units remain in the survey for four years and one quarter of the survey units is replaced each year.

responses of women aged 40 to 49 may be observed as, after time, these two age groups will include formerly eligible women. The time period considered before the adoption of the state-level directives serves for investigating pre-existing trends in the outcomes of interest. This allows evaluating whether the empirical analysis is likely to identify causal effects of the policy.

To capture fertility behavior, my outcome of interest is an indicator for giving birth. Because the *Micro Census* does not regularly survey births, I rely on information about household and family membership - and the age composition of their members. This means that I detect a birth for the preceding year, where a child born in the previous year is a member of the respective household or family.

The second outcome of interest is a binary variable indicating labor force participation. Table 1.3 features summary statistics for my sample of German women.¹⁸

TABLE 1.3: Summary statistics

Variable	Observations	Mean	SD	Min	Max
Child born t-1 is member of the household	115780	0.081	0.273	0	1
Child born t-1 is member of the family	115780	0.078	0.268	0	1
Labor force participation	116324	0.825	0.380	0	1
Labor force participation (alternative definition)	116324	0.814	0.389	0	1
Marital status: Unmarried	126462	0.358	0.480	0	1
Marital status: Married	126462	0.553	0.497	0	1
Education: High	126462	0.317	0.465	0	1
Urbanization: High	126448	0.327	0.469	0	1

Notes: The table displays summary statistics with data pooled over all waves. The sample comprises German women aged 20 to 49 who reside in counties located at balanced internal state borders. Source: Microcensus Waves 2005-2016

1.6 Estimation strategy

In consideration of the German institutional setting featuring a regional and successive introduction of subsidies for ARTs, a difference-in-differences (DD) approach is the natural choice to study fertility and female labor market outcomes. The DD estimator allows the

¹⁸Definitions of all variables can be found in the appendix.

identification of causal effects when a policy intervention affects only a fraction of the population and is adopted at a specific point in time (Blundell and Macurdy, 1999a). Hence, this empirical design exploits two dimensions of variation in expected costs for ARTs that emerged in the implementation process of the policy measure. The first dimension of variation is found in the regional nature of the subsidies. According to the eligibility conditions, only residents of the respective treated states may receive financial funding. Thus, it is this exclusive group of individuals that experiences the policy change. The second dimension used for identification is variation over time. In 2009, Saxony pioneered the financial support for ARTs. Saxony-Anhalt joined in 2010, Lower Saxony and Thuringia adopted subsidies in 2013.

The DD identification strategy assumes that treatment and control states experience equal time trends in the hypothetical absence of the policy intervention, while allowing for ‘permanent differences’ (Imbens and Wooldridge, 2007) between the two groups. In mathematical terms, this translates into calculating the mean difference in the outcome variable of the treatment and control states before the policy intervention and subtracting it from the difference between the two groups in the time period after treatment (Lechner et al., 2011). For this empirical approach to be providing ‘[...] behaviorally meaningful [...]’ (Blundell and Macurdy, 1999a) results, i.e. the true causal effects of a policy change, it is crucial that a set of identifying assumptions holds. The first key identifying assumption is termed the ‘parallel trend’ assumption. Because the DD strategy presents the treatment effect as the ‘deviation of the trend of the [...] outcomes of the treated from the trend of the [...] outcomes of the nontreated’ (Lechner et al., 2011), it is required that in a hypothetical scenario without the policy intervention, treatment and control states would be affected by the same time effects. The second identifying assumption describes the requirement of a stable composition of the treatment and control groups. This means that in each time period under investigation, the individuals representing each group are drawn from a ‘[...] comparable population’ (Meyer, 1995). A closer look at the time-invariant group effect imbedded in the DD model reveals that the occurrence of non-random changes in the groups would lead to biased estimates of the treatment effect.

The standard static DD estimator assumes that the treatment effect is constant over time. Accordingly, I estimate the following static DD model

$$\begin{aligned}
 O_{icbsta} = & \alpha_a (\text{Treatment}_s \times \text{Subsidy in effect}_{st}) \\
 & + \sum_s \beta_s \text{State}_s + \left[\sum_c \beta_c \text{County}_c \right] + \sum_t \gamma_t \text{Year}_t + \left[\sum_b \delta_b \text{Border}_b \right] \\
 & + \zeta_1 \mathbf{X}_i + \zeta_2 \mathbf{Y}_{ct} \\
 & + \epsilon_{icbsta},
 \end{aligned}$$

for all counties located at balanced internal state borders.¹⁹ The outcome of interest, O_{icbsta} , is a binary variable indicating whether a woman gave birth in the preceding year, respectively a woman's current-period labor force participation.²⁰ The subscripts denote observations for individual i residing in county c located at border b in state s , year t and age group a . The second-level interaction term (with coefficient α_a) identifies the static response to the policy with a binary variable, which differs between outcome variables. When analyzing retrospective births (labor force participation), it equals unity if an individual resides in a treated states and the subsidy for ARTs was introduced at least two years (one year) ago. The reason for not allowing an instant response to the policy measure lies in changes in fertility behavior and related changes in female labor market outcomes not occurring before the natural nine-month gestation period (Schmidt, 2007). The analysis is based on OLS estimation and the coefficients are estimated for each age group separately. This results in obtaining age-group-specific responses to the policy captured in α_a .

My baseline specification controls for state (β_s) and year fixed-effects (γ_t). In the sensitivity analysis, I augment the model by including county (β_c) and border (δ_b) fixed-effects, respectively. My preferred specification includes county fixed-effects, which allows controlling for any unobserved time-invariant differences across counties in characteristics that correlate with my outcome variables. In order to mitigate the risk of violation of the 'parallel

¹⁹These internal state borders are inner borders between German federal states. Counties located along an internal state border share this border with counties from neighboring federal states and may be adjacent to some of these counties.

²⁰In the sensitivity analysis, I take into account that a woman may be a member of a household where a child was born in the previous year, even though she may not be the child's mother. Thus, I proxy giving birth by a woman being a member of a family - within a household - in which a child was born in the previous year. Because the definition of a family in the *Micro Census* follows the two-generation rule, only parents and unmarried children can be members of the same family unit. Further, in the sensitivity analysis, I rely on male labor force participation as an additional outcome variable.

trend' assumption arising from residual heterogeneity in characteristics that are correlated with fertility and labor supply decisions, I add a battery of control variables.

On the individual level, the vector $\mathbf{X}_i$ controls for marital status, educational attainment and the level of urbanization of an individual's region of residence. By controlling for marital status, I take into account that marriage is typically accompanied by societal norms expecting a couple to have children. Further, I include educational attainment to capture an individual's labor market attachment. By adding the level of urbanization, I control for differences in societal norms between urban and rural areas.

The vector $\mathbf{Y}_{ct}$ appends county-level covariates. In selecting relevant characteristics, I closely follow Bauernschuster, Hener, and Rainer (2016). In order to capture local labor market conditions and economic stability, I consider employment rates by gender, the unemployment rate and the in- and out-commute rates.²¹ Moreover, I control for the local marriage rate to capture differences in societal norms and values. By including the share of 5-or more rooms apartments, I take into account that family formation may depend on the availability of sufficient living space. I also control for the vote shares of political parties to account for political orientation, which may reflect societal attitudes about women pursuing careers and family formation. Finally, I add a set of variables capturing access to childcare. As determinants of the childcare market, I rely on the number of available slots in childcare and the number of children in early childcare per woman aged 20 to 39 as well as the shares of children aged under 3 and 3 to 5 attending childcare. By controlling for access to childcare, I take into account that availability of childcare impacts the ability of an individual to achieve a balance between work and family.

The error term is denoted by ϵ and I calculate robust standard errors. In the sensitivity analysis, I apply bootstrap estimations instead to take into account that treatment in my research setting varies on the state level and that the number of state clusters is quite small.

²¹Since data on unemployment rates by gender is not available before 2008, I have to rely on the overall unemployment rate.

1.7 Estimation results

The results for the age-group-specific effects of subsidizing ARTs on the probability to give birth are summarized in Table 1.4. Assuming a static response to the policy intervention, the coefficients across age groups for West- and East-Germany are mostly insignificant. Only German women from former East-Germany aged 30 to 34 are significantly less likely to give birth due to the policy intervention - albeit at a low level of statistical significance.

The results on the probability of labor force participation, which are presented in Table 1.5, show a different picture. While the majority of coefficients is still insignificant, statistically relevant effects are uncovered for women in West-Germany in the age groups 30 to 34 and 35 to 39. However, for the age group 35 to 39, the estimate is only statistically significant at the 10% level. As a consequence of the subsidies, these women appear to be significantly less likely to participate in the labor force.

Against the background of the career-family tradeoff and the hypothesized effects of the subsidies, the results on fertility suggest that women in their early 30s from East-Germany may factually postpone motherhood. However, this change in fertility behavior does not lead to a sizable effect on women's labor force participation. This may be explained by the fact that childbearing in former East-Germany is associated with relatively short career interruptions. The results for West-Germany are difficult to explain within the context of the policy intervention. While women's fertility behavior appears to not be affected by the subsidies, women in their 30s withdraw from the labor market. This finding hints at differences in labor market trends between treated and control states in West-Germany that are not induced by the policy intervention and that result in confounded estimates.

1.8 Robustness Checks

1.8.1 Alternative definitions

In this section, I investigate whether my results are sensitive to alternative definitions of the outcome variables. Table 1.6 replicates the analysis on the probability to give birth. However, fertility is now proxied by a woman being a member of a family - within a household - in which a child was born in the previous year. Compared to the main analysis, this is a

TABLE 1.4: The effect of subsidizing ARTs on the probability to give birth at balanced borders

Dependent variable:	Child born in t-1 is member of the household					
Specification:	Static response					
Age group	20-24	25-29	30-34	35-39	40-44	45-49
West-Germany						
Subsidy	0.004	0.028	-0.016	0.021	0.013	0.015
SE	(0.013)	(0.017)	(0.020)	(0.016)	(0.010)	(0.009)
No. obs	11,822	11,171	11,723	14,134	18,273	20,282
R2	0.037	0.052	0.025	0.015	0.010	0.012
East-Germany						
Subsidy	0.005	-0.012	-0.062*	0.020	-0.001	0.014
SE	(0.040)	(0.040)	(0.034)	(0.028)	(0.023)	(0.018)
No. obs	3,435	3,926	3,983	4,336	5,682	6,999
R2	0.040	0.025	0.019	0.019	0.025	0.028
Control variables	yes	yes	yes	yes	yes	yes
State FE	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes
County FE	no	no	no	no	no	no
Border FE	no	no	no	no	no	no
SE	bootstrap	bootstrap	bootstrap	bootstrap	bootstrap	bootstrap

Notes: This table features estimation results for German women from OLS regressions based on *Micro Census* data from 2006 to 2016. The dependent variable is a binary variable, which equals unity if a woman is a member of a household, in which a child was born in the previous year. Levels of statistical significance: *** denotes significance at the 1% level; ** at the 5% level; and * at the 10% level. All regressions include state and year fixed-effects, as well as the control variables marital status, educational attainment, region, employment rates by gender, unemployment rate, in- and out-commute rates, marriage rate, share of 5-or more rooms apartments and the vote share by political party.

TABLE 1.5: The effect of subsidizing ARTs on the probability of labor force participation at balanced borders

Dependent variable:	Woman is participating in the labor force					
Specification:	Static response					
Age group	20-24	25-29	30-34	35-39	40-44	45-49
West-Germany						
Subsidy	-0.005	0.015	-0.053**	-0.033*	0.015	-0.011
SE	(0.026)	(0.021)	(0.023)	(0.019)	(0.015)	(0.014)
No. obs	11,633	10,912	11,595	14,606	18,755	19,764
R2	0.074	0.085	0.059	0.041	0.030	0.024
East-Germany						
Subsidy	0.033	-0.023	-0.004	-0.010	-0.030	-0.016
SE	(0.044)	(0.027)	(0.026)	(0.024)	(0.020)	(0.016)
No. obs	3,694	3,877	3,925	4,470	6,003	7,076
R2	0.063	0.050	0.041	0.037	0.029	0.039
Control variables	yes	yes	yes	yes	yes	yes
State FE	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes
County FE	no	no	no	no	no	no
Border FE	no	no	no	no	no	no
SE	bootstrap	bootstrap	bootstrap	bootstrap	bootstrap	bootstrap

Notes: This table features estimation results for German women from OLS regressions based on *Micro Census* data from 2005 to 2015. The dependent variable is a binary variable, which equals unity if a woman is employed or unemployed - unconditional of availability. Levels of statistical significance: *** denotes significance at the 1% level; ** at the 5% level; and * at the 10% level. All regressions include state and year fixed-effects, as well as the control variables marital status, educational attainment, region, employment rates by gender, unemployment rate, in- and out-commute rates, marriage rate, share of 5-or more rooms apartments and the vote share by political party.

tighter definition of giving birth because it rules out false double counting of a given birth in households that include more than one woman of childbearing age. The results based on this modified fertility proxy are qualitatively similar to the original definition. However, the positive coefficient for German women aged 25 to 29 from West-Germany is now weakly statistically significant, while the negative impact on women aged 30 to 34 from East-Germany turns insignificant. The finding that women in West-Germany aged mid to end 20 adapt their fertility behavior does not fit into the theoretical considerations on the potential effects of the subsidies. Hence, it raises the same concerns about unconfoundedness of the estimates as the results on labor force participation.

In the next step, I modify the definition of participating in the labor force by treating all individuals that are more than three months away from the workplace - except for the self-employed and family workers - as being inactive. The rationale behind this approach lies in this measure being more responsive to changes in fertility because it partially uncovers the labor market implications of being on maternity leave. The results shown in Table 1.7 are identical to the ones based on the original definition of labor force participation. The even more pronounced response of women from West-Germany aged 35 to 39 indicates that being more than three months on maternity leave is one of the causes of lower labor force participation.

1.8.2 The effects of subsidizing ARTs on males

To the extent that state-specific shocks to the labor market unconnected but coinciding with the introduction of the subsidies exist, the uncovered effects on fertility and female labor force participation may be falsely attributed to the policy measure. In order to assess whether my results suffer from endogeneity, I study the effects of subsidizing ARTs on male labor force participation in this section. Because employment of men is generally less responsive to fertility, the policy measure should exert a weaker effect on males. The results on the effects of subsidizing ARTs on males are displayed in Table 1.8. Most coefficients turn out insignificant. However, there is a negative and highly significant relation between the introduction of the subsidies and labor force participation of men aged 20 to 24 in West-Germany. This suggests that the policy measure was introduced in an environment where

TABLE 1.6: Alternative definition: The effect of subsidizing ARTs on the probability to give birth at balanced borders

Dependent variable:	Child born in t-1 is member of the family					
Specification:	Static response					
Age group	20-24	25-29	30-34	35-39	40-44	45-49
West-Germany						
Subsidy	0.008	0.028*	-0.016	0.020	0.013	0.015
SE	(0.012)	(0.017)	(0.020)	(0.016)	(0.010)	(0.009)
No. obs	11,822	11,171	11,723	14,134	18,273	20,282
R2	0.037	0.055	0.026	0.014	0.010	0.011
East-Germany						
Subsidy	-0.006	-0.014	-0.053	0.032	0.009	0.012
SE	(0.038)	(0.039)	(0.032)	(0.027)	(0.024)	(0.018)
No. obs	3,435	3,926	3,983	4,336	5,682	6,999
R2	0.039	0.024	0.017	0.020	0.024	0.026
Control variables	yes	yes	yes	yes	yes	yes
State FE	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes
County FE	no	no	no	no	no	no
Border FE	no	no	no	no	no	no
SE	bootstrap	bootstrap	bootstrap	bootstrap	bootstrap	bootstrap

Notes: This table features estimation results for German women from a Logistic regression based on *Micro Census* data from 2005 to 2016. The dependent variable is a binary variable, which equals unity if a woman is a member of a family, in which a child was born in the previous year. Levels of statistical significance: *** denotes significance at the 1% level; ** at the 5% level; and * at the 10% level. All regressions include state and year fixed-effects, state-specific linear and quadratic time trends, as well as the control variables marital status, education and the level of urbanization of the region of residence.

TABLE 1.7: Alternative definition: The effect of subsidizing ARTs on the probability of labor force participation at balanced borders

Dependent variable:	Woman is participating in the labor force					
Specification:	Static response					
Age group	20-24	25-29	30-34	35-39	40-44	45-49
West-Germany						
Subsidy	-0.008	-0.001	-0.048**	-0.047**	0.014	-0.015
SE	(0.026)	(0.023)	(0.024)	(0.020)	(0.016)	(0.015)
No. obs	11,633	10,912	11,595	14,606	18,755	19,764
R2	0.073	0.089	0.059	0.037	0.028	0.023
East-Germany						
Subsidy	0.035	-0.033	-0.013	0.000	-0.028	-0.011
SE	(0.046)	(0.031)	(0.029)	(0.024)	(0.020)	(0.016)
No. obs	3,694	3,877	3,925	4,470	6,003	7,076
R2	0.060	0.042	0.031	0.034	0.028	0.038
Control variables	yes	yes	yes	yes	yes	yes
State FE	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes
County FE	no	no	no	no	no	no
Border FE	no	no	no	no	no	no
SE	bootstrap	bootstrap	bootstrap	bootstrap	bootstrap	bootstrap

Notes: This table features estimation results for German women from OLS regressions based on *Micro Census* data from 2005 to 2015. The dependent variable is a binary variable, which equals unity if a woman is employed and less than three months away from workplace if applicable - or unemployed. Levels of statistical significance: *** denotes significance at the 1% level; ** at the 5% level; and * at the 10% level. All regressions include state and year fixed-effects, as well as the control variables marital status, educational attainment, region, employment rates by gender, unemployment rate, in- and out-commute rates, marriage rate, share of 5-or more rooms apartments and the vote share by political party.

TABLE 1.8: Males: The effect of subsidizing ARTs on the probability of labor force participation at balanced borders

Dependent variable:	Man is participating in the labor force					
Specification:	Static response					
Age group	20-24	25-29	30-34	35-39	40-44	45-49
West-Germany						
Subsidy	-0.070***	0.007	-0.020	-0.014	-0.014	0.002
SE	(0.023)	(0.017)	(0.014)	(0.012)	(0.009)	(0.009)
No. obs	12,501	10,944	11,134	14,177	18,530	20,010
R2	0.130	0.090	0.020	0.020	0.028	0.034
East-Germany						
Subsidy	0.018	0.039*	0.024	-0.011	0.006	0.014
SE	(0.027)	(0.021)	(0.019)	(0.014)	(0.017)	(0.014)
No. obs	4,399	4,286	4,054	4,639	6,267	7,153
R2	0.184	0.082	0.015	0.019	0.041	0.053
Control variables	yes	yes	yes	yes	yes	yes
State FE	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes
County FE	no	no	no	no	no	no
Border FE	no	no	no	no	no	no
SE	bootstrap	bootstrap	bootstrap	bootstrap	bootstrap	bootstrap

Notes: This table features estimation results for German men from OLS regressions based on *Micro Census* data from 2005 to 2015. The dependent variable is a binary variable, which equals unity if a woman is employed or unemployed - unconditional of availability. Levels of statistical significance: *** denotes significance at the 1% level; ** at the 5% level; and * at the 10% level. All regressions include state and year fixed-effects, as well as the control variables marital status, educational attainment, region, employment rates by gender, unemployment rate, in- and out-commute rates, marriage rate, share of 5-or more rooms apartments and the vote share by political party.

young adult males independent of education were less likely to participate in the labor market in the post-treatment period. For East-Germany, there is a weakly significant, positive effect on men of the age group 25 to 29. Overall, while it is reassuring that significant effects on men are scarce, the findings also hint at endogenous labor market trends, which challenge the validity of my results from the main analysis.

1.8.3 Fixed effects

In this section, I include additional fixed effects to the baseline model from the main analysis. At this point, I either add border- or county-FE, which control for any time-constant, unobserved border-, respectively county-specific, characteristics correlated with my outcome variables. My preferred specification includes county-FE. It should be noted that while my baseline model features bootstrap estimation, I rely on robust standard errors here.²²

²²I choose not to rely on bootstrap estimation in my models including border- or county-FE due to its greater computational demand in comparison to estimation with robust standard errors.

TABLE 1.9: Border FE: The effect of subsidizing ARTs on the probability to give birth at balanced borders

Dependent variable:	Child born in t-1 is member of the household					
Specification:	Static response					
Age group	20-24	25-29	30-34	35-39	40-44	45-49
West-Germany						
Subsidy	0.002	0.028	-0.017	0.021	0.012	0.014
SE	(0.014)	(0.017)	(0.020)	(0.015)	(0.011)	(0.009)
No. obs	11,822	11,171	11,723	14,134	18,273	20,282
R2	0.038	0.052	0.025	0.015	0.010	0.012
East-Germany						
Subsidy	0.003	-0.020	-0.051	0.019	-0.003	0.014
SE	(0.035)	(0.034)	(0.036)	(0.031)	(0.021)	(0.019)
No. obs	3,435	3,926	3,983	4,336	5,682	6,999
R2	0.040	0.025	0.020	0.019	0.025	0.028
Control variables	yes	yes	yes	yes	yes	yes
State FE	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes
County FE	no	no	no	no	no	no
Border FE	yes	yes	yes	yes	yes	yes
SE	robust	robust	robust	robust	robust	robust

Notes: This table features estimation results for German women from OLS regressions based on *Micro Census* data from 2006 to 2016. The dependent variable is a binary variable, which equals unity if a woman is a member of a household, in which a child was born in the previous year. Levels of statistical significance: *** denotes significance at the 1% level; ** at the 5% level; and * at the 10% level. All regressions include state, year and border fixed-effects, as well as the control variables marital status, educational attainment, region, employment rates by gender, unemployment rate, in- and out-commute rates, marriage rate, share of 5-or more rooms apartments and the vote share by political party.

The probability of giving birth

With respect to the effects on the probability to give birth, the results from estimations including additional fixed effects, which are displayed in Table 1.9 and 1.10 are quite similar to the baseline model. However, while the model including border-FE features now an insignificant response of German women aged 30 to 34 in East-Germany, the model with county-FE still presents a weakly statistically significant effect at the 10% level for this group of women.

The probability of labor force participation

Table 1.11 and 1.12 show regression results from the baseline model on the effects on labor force participation of German women with the additional fixed effects. It can be seen that the negative effect on women aged 30 to 34 in West-Germany is robust to the inclusion of border-FE. However, when controlling for county-FE, the negative effect becomes only weakly statistically significant. Further, for East-Germany, estimations with border-FE now

TABLE 1.10: County FE: The effect of subsidizing ARTs on the probability to give birth at balanced borders

Dependent variable: Specification: Age group	Child born in t-1 is member of the household Static response					
	20-24	25-29	30-34	35-39	40-44	45-49
West-Germany						
Subsidy	-0.019	0.023	-0.010	0.019	0.007	0.007
SE	(0.017)	(0.021)	(0.024)	(0.019)	(0.013)	(0.011)
No. obs	11,822	11,171	11,723	14,134	18,273	20,282
R2	0.040	0.054	0.027	0.016	0.011	0.012
East-Germany						
Subsidy	-0.008	-0.029	-0.075*	0.024	-0.001	0.001
SE	(0.037)	(0.037)	(0.040)	(0.034)	(0.024)	(0.021)
No. obs	3,435	3,926	3,983	4,336	5,682	6,999
R2	0.041	0.027	0.023	0.022	0.027	0.031
Control variables	yes	yes	yes	yes	yes	yes
State FE	no	no	no	no	no	no
Year FE	yes	yes	yes	yes	yes	yes
County FE	yes	yes	yes	yes	yes	yes
Border FE	no	no	no	no	no	no
SE	robust	robust	robust	robust	robust	robust

Notes: This table features estimation results for German women from OLS regressions based on *Micro Census* data from 2006 to 2016. The dependent variable is a binary variable, which equals unity if a woman is a member of a household, in which a child was born in the previous year. Levels of statistical significance: *** denotes significance at the 1% level; ** at the 5% level; and * at the 10% level. All regressions include county and year fixed-effects, as well as the control variables marital status, educational attainment, region, employment rates by gender, unemployment rate, in- and out-commute rates, marriage rate, share of 5-or more rooms apartments and the vote share by political party.

feature a negative response of women aged 40 to 44 - albeit an estimate with a lower level of statistical significance.

1.9 Conclusion

In the context of regional subsidies for ARTs in Germany, this paper addresses the hypothesis that access to infertility treatments may alter the career-family tradeoff for women and, as a consequence, influence fertility and female labor market outcomes. For East-Germany, results obtained from my preferred specification including county fixed-effects show a weakly significant negative effect of the subsidies on the probability to give birth for German women of the age group 30 to 34 in former East-Germany. Moreover, this response of fertility behavior is not accompanied by a measurable effect on women's labor force participation. This may be related to traditionally short career interruptions induced by fertility in former East-Germany. For West-Germany, the findings suggest that while

TABLE 1.11: Border FE: The effect of subsidizing ARTs on the probability of labor force participation at balanced borders

Dependent variable:	Woman is participating in the labor force					
Specification:	Static response					
Age group	20-24	25-29	30-34	35-39	40-44	45-49
West-Germany						
Subsidy	-0.002	0.015	-0.051**	-0.033	0.017	-0.009
SE	(0.026)	(0.023)	(0.022)	(0.020)	(0.016)	(0.014)
No. obs	11,633	10,912	11,595	14,606	18,755	19,764
R2	0.075	0.085	0.060	0.041	0.031	0.024
East-Germany						
Subsidy	0.036	-0.026	-0.005	-0.008	-0.031*	-0.017
SE	(0.039)	(0.031)	(0.026)	(0.025)	(0.019)	(0.017)
No. obs	3,694	3,877	3,925	4,470	6,003	7,076
R2	0.064	0.051	0.041	0.037	0.029	0.039
Control variables	yes	yes	yes	yes	yes	yes
State FE	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes
County FE	no	no	no	no	no	no
Border FE	yes	yes	yes	yes	yes	yes
SE	robust	robust	robust	robust	robust	robust

Notes: This table features estimation results for German women from OLS regressions based on *Micro Census* data from 2005 to 2015. The dependent variable is a binary variable, which equals unity if a woman is employed or unemployed - unconditional of availability. Levels of statistical significance: *** denotes significance at the 1% level; ** at the 5% level; and * at the 10% level. All regressions include state, year and border fixed-effects, as well as the control variables marital status, educational attainment, region, employment rates by gender, unemployment rate, in- and out-commute rates, marriage rate, share of 5-or more rooms apartments and the vote share by political party.

TABLE 1.12: County FE: The effect of subsidizing ARTs on the probability of labor force participation at balanced borders

Dependent variable:	Woman is participating in the labor force					
Specification:	Static response					
Age group	20-24	25-29	30-34	35-39	40-44	45-49
West-Germany						
Subsidy	-0.007	0.020	-0.044*	-0.037	-0.002	-0.016
SE	(0.030)	(0.026)	(0.026)	(0.024)	(0.019)	(0.016)
No. obs	11,633	10,912	11,595	14,606	18,755	19,764
R2	0.077	0.087	0.062	0.044	0.032	0.026
East-Germany						
Subsidy	0.018	-0.030	-0.011	0.000	-0.027	-0.022
SE	(0.044)	(0.035)	(0.031)	(0.027)	(0.021)	(0.020)
No. obs	3,694	3,877	3,925	4,470	6,003	7,076
R2	0.066	0.052	0.043	0.042	0.030	0.041
Control variables	yes	yes	yes	yes	yes	yes
State FE	no	no	no	no	no	no
Year FE	yes	yes	yes	yes	yes	yes
County FE	yes	yes	yes	yes	yes	yes
Border FE	no	no	no	no	no	no
SE	robust	robust	robust	robust	robust	robust

Notes: This table features estimation results for German women from OLS regressions based on *Micro Census* data from 2005 to 2015. The dependent variable is a binary variable, which equals unity if a woman is employed or unemployed - unconditional of availability. Levels of statistical significance: *** denotes significance at the 1% level; ** at the 5% level; and * at the 10% level. All regressions include county and year fixed-effects, as well as the control variables marital status, educational attainment, region, employment rates by gender, unemployment rate, in- and out-commute rates, marriage rate, share of 5-or more rooms apartments and the vote share by political party.

there is no significant impact of the policy intervention on German women's fertility behavior, it motivates women aged 30 to 34 to withdraw from the labor market - albeit the effect is only statistically significant at the 10% level. This result does not align with the theoretical considerations. In combination with the results on males, these observations challenge the validity of the obtained estimates in terms of reflecting the true effects of subsidizing ARTs on German women.

Appendix

1.A Data sources and definitions of variables

Outcome variables

- **Woman gave birth in the preceding year:** Binary variable that equals one if a woman is a member of a household, respectively family, in which a child was born in the previous year. Data: *Micro Census*.
- **Labor force participation (LFP):**
 - **LFP based on *Microcensus* concept and *GESIS* harmonization:** Binary variable that equals one if a woman is employed or unemployed - unconditional of availability. Data: *Micro Census*.
 - **LFP based on *International Labour Organization (ILO)* concept and *GESIS* harmonization:** Binary variable that equals one if a woman is employed or unemployed and available within the next two weeks. Data: *Micro Census*.
 - **LFP based on *Microcensus* concept and own harmonization:** Binary variable that equals one if woman is employed and less than three months away from workplace if applicable - or unemployed. Data: *Micro Census*.

German fertility trends, balancing and control variables

- **Individual-level data:**
 - **Marital status:** The marital status comprises four categories: unmarried, married, widowed, divorced. Marriage and registered partnerships are treated equally. Data: *Micro Census*.

-
- **Educational attainment:** Refers to the highest attained school-leaving qualification. The level of education comprises four categories: high, medium, low and not specified. Data: *Micro Census*.
 - **Region:** Refers to the level of urbanization of the region of residence: Urban, suburban, rural. Data: *Micro Census*.
 - **County-level data:**
 - **Employment rates by gender:** The employment rates by gender is provided by the *Federal Institute for Research on Building, Urban Affairs and Spatial Development (INKAR)*.
 - **Unemployment rate:** The unemployment rate is provided by the *Federal Institute for Research on Building, Urban Affairs and Spatial Development (INKAR)*.
 - **Share of employment in primary/tertiary sector:** The share of employment in primary /tertiary sector is provided by the *Federal Institute for Research on Building, Urban Affairs and Spatial Development (INKAR)*.
 - **In- and out-commute rates:** The in- and out-commute rates are provided by the *Federal Institute for Research on Building, Urban Affairs and Spatial Development (INKAR)*.
 - **Marriage rate:** The marriage rate provided by the *Federal Institute for Research on Building, Urban Affairs and Spatial Development (INKAR)*.
 - **Share of 5-or more rooms apartments:** The share of 5-or more rooms apartments is provided by the *Federal Institute for Research on Building, Urban Affairs and Spatial Development (INKAR)*.
 - **Population density:** Population density is provided by the *Federal Institute for Research on Building, Urban Affairs and Spatial Development (INKAR)*.
 - **GDP per capita:** GDP per capita is provided by the *Federal Institute for Research on Building, Urban Affairs and Spatial Development (INKAR)*.
 - **Vote share by political party:** Vote share by political party comprises six categories: CDU/CSU, Die Linke, FDP, Grüne, SPD and other. Data: *Federal Institute for Research on Building, Urban Affairs and Spatial Development (INKAR)*.

- **State-level data:**

- **Age-group-specific births:** The absolute number of births to all German mothers from a certain age group is obtained on the state-level from the *German population statistics* published by the *Federal Statistical Office*.

Age-group-specific population data: Age-group-specific population data is derived from state-level data included in the *German population statistics* and published by the *Federal Statistical Office*.

1.B Full results from balancing regressions

Results by subsample: Unrestricted, West-Germany, East-Germany

TABLE 1.B.1: Results from Balancing Regressions by Subsample

	balancingvars_all		
	GER	WW	EE
Emp_rate_m_05_09	-2.21	-1.75	0.46
p-value	0.00	0.02	0.65
Emp_rate_f_05_09	1.46	-2.55	0.17
p-value	0.01	0.00	0.83
Unemp_rate_05_09	4.41	2.03	1.11
p-value	0.00	0.00	0.09
Share_pri_sec_05_09	1.26	0.87	-0.41
p-value	0.00	0.00	0.41
Share_ter_sec_05_09	2.51	2.58	0.53
p-value	0.05	0.10	0.85
In_comm_05_09	-4.38	-4.80	0.43
p-value	0.00	0.00	0.87
Out_comm_05_09	-1.51	-1.24	-0.59
p-value	0.48	0.65	0.91
Marriages_05_09	-0.39	0.32	-1.22

p-value	0.03	0.16	0.00
Share_apart_4p_05_09	-4.24	5.02	-2.97
p-value	0.01	0.00	0.16
Pop_dens_05_09	-240.38	-331.74	212.52
p-value	0.00	0.00	0.12
GDP_pp_05_09	-7259.37	-5156.70	-96.30
p-value	0.00	0.01	0.94
Vote_share_CDUCSU_05_09	-6.86	-5.76	4.54
p-value	0.00	0.00	0.00
Avail_slots_childc_05_09	0.06	-0.01	0.09
p-value	0.00	0.16	0.00
Child_u3_in_care_05_09	0.03	-0.01	0.00
p-value	0.00	0.00	0.61
Share_in_care_u3_05_09	13.08	-3.22	-0.10
p-value	0.00	0.00	0.96
Share_in_care_3to5_05_09	-0.58	-6.48	-1.59
p-value	0.47	0.00	0.01
Share_emp_15to29_05_09	-1.44	-1.41	-0.14
p-value	0.00	0.00	0.73
Migbal_f_18_25_05_09	-19.48	-13.95	7.90
p-value	0.00	0.01	0.56
Migbal_m_18_25_05_09	-13.64	-11.93	5.83
p-value	0.00	0.00	0.57
Migbal_f_25_29_05_09	-9.64	-2.97	2.25
p-value	0.00	0.32	0.55
Migbal_m_25_29_05_09	-12.31	-7.19	4.36
p-value	0.00	0.01	0.37
Net_migr_f_05_09	-3.29	-1.39	-0.07
p-value	0.00	0.13	0.96
Net_migr_m_05_09	-3.27	-1.60	0.13

p-value	0.00	0.06	0.94
Divorces_05_09	-0.25	0.22	-0.49
p-value	0.00	0.00	0.00
Birthr_15_19_05_09	3.82	1.59	0.78
p-value	0.00	0.00	0.29
Birthr_40plus_05_09	-1.65	-0.51	0.33
p-value	0.00	0.01	0.26
Life_exp_05_09	-0.61	-0.57	0.15
p-value	0.00	0.00	0.45
Sex_ratio_20_39_05_09	-0.06	-0.01	0.01
p-value	0.00	0.06	0.49
Tot_fertr_05_09	0.02	0.06	0.05
p-value	0.08	0.00	0.00
School_leave_me_05_09	-10.82	-7.68	-3.58
p-value	0.00	0.00	0.00
School_leave_hi_05_09	3.61	-2.26	-4.63
p-value	0.00	0.04	0.00
School_leave_lo_05_09	1.43	1.11	0.75
p-value	0.00	0.00	0.11
Apprship_places_05_09	-0.23	0.36	1.05
p-value	0.47	0.28	0.14
Students_18_25_05_09	-6.26	-7.84	1.32
p-value	0.14	0.11	0.88
HH_inc_05_09	-225.36	-163.92	-2.87
p-value	0.00	0.00	0.85
Inf_mortality_06_09	0.10	0.77	-0.56
p-value	0.55	0.00	0.02
_0_2_FD_ccare_07_09	12.09	-1.43	-4.09
p-value	0.00	0.00	0.00
_3_5_FD_ccare_07_09	20.39	-10.96	-8.14

p-value	0.00	0.00	0.03
LFP_rate_f_08_09	1.72	-1.25	0.13
p-value	0.00	0.00	0.86
LFP_rate_m_08_09	-1.52	-1.39	1.00
p-value	0.00	0.03	0.23
Share_jobs_req_exp_10_16	-0.48	-0.50	0.21
p-value	0.10	0.19	0.77
Share_jobs_req_spec_10_16	-0.88	-1.10	0.65
p-value	0.00	0.00	0.02
N	383.00	316.00	66.00
Sign_1percent	32.00	28.00	8.00
Sign_5percent	35.00	31.00	12.00
Sign_10percent	36.00	33.00	13.00

Results for west-west bordering counties by border

TABLE 1.B.2: Balancing Tests: West-West Bordering Counties, Pre-treatment

	balancingvars_all				
	LS-HB	LS-HH	LS-SH	LS-HE	LS-NRW
Emp_rate_m_05_09	8.35	8.48	-2.76	3.26	-3.69
p-value	0.00	0.08	0.65	0.50	0.01
Emp_rate_f_05_09	1.67	-0.97	-1.19	3.68	-1.06
p-value	0.29	0.68	0.48	0.25	0.51
Unemp_rate_05_09	-3.79	-2.20	-0.04	-0.97	1.20
p-value	0.05	0.36	0.99	0.74	0.13
Share_pri_sec_05_09	1.15	1.72	0.15	1.80	0.55
p-value	0.02	0.09	0.66	0.11	0.04
Share_ter_sec_05_09	-6.86	-12.52	3.06	-9.54	4.14
p-value	0.24	0.15	0.01	0.36	0.17
In_comm_05_09	17.17	29.56	-2.91	-11.03	3.95
p-value	0.02	0.04	0.49	0.15	0.15
Out_comm_05_09	51.01	62.69	-3.95	-0.25	2.77

p-value	0.00	0.06	0.68	0.99	0.53
Marriages_05_09	0.53	1.77	0.46	0.81	0.01
p-value	0.15	0.26	0.53	0.06	0.95
Share_apart_4p_05_09	24.03	29.00	2.96	7.06	0.21
p-value	0.00	0.00	0.51	0.53	0.96
Pop_dens_05_09	-1328.90	-2158.81	16.59	-589.43	-28.40
p-value	0.00	0.01	0.74	0.46	0.83
GDP_pp_05_09	-20014.64	-32815.90	-1418.30	-7823.27	-1388.18
p-value	0.00	0.08	0.59	0.46	0.62
Vote_share_CDUCSU_05_09	7.27	6.90	-2.42	2.50	-4.22
p-value	0.00	0.00	0.63	0.14	0.19
Avail_slots_childc_05_09	0.06	0.05	0.01	-0.01	-0.00
p-value	0.01	0.13	0.88	0.71	0.77
Child_u3_in_care_05_09	-0.01	-0.02	-0.01	-0.01	-0.00
p-value	0.03	0.01	0.12	0.02	0.02
Share_in_care_u3_05_09	-5.77	-15.15	-1.66	-4.11	-1.10
p-value	0.00	0.01	0.39	0.02	0.15
Share_in_care_3to5_05_09	-2.86	5.62	5.85	-4.49	-1.31
p-value	0.13	0.07	0.04	0.01	0.46
Share_emp_15to29_05_09	1.29	0.72	0.29	-1.42	-0.17
p-value	0.00	0.56	0.71	0.26	0.88
Migbal_f_18_25_05_09	-80.92	-83.14	30.70	-36.67	4.50
p-value	0.00	0.00	0.45	0.45	0.67
Migbal_m_18_25_05_09	-71.28	-68.12	20.09	-33.92	4.96
p-value	0.00	0.01	0.54	0.45	0.59
Migbal_f_25_29_05_09	8.64	-6.79	-17.17	0.58	2.23
p-value	0.18	0.51	0.68	0.96	0.65
Migbal_m_25_29_05_09	-13.04	-25.71	1.80	-13.80	3.66
p-value	0.03	0.03	0.03	0.40	0.40
Net_migr_f_05_09	-1.71	-1.05	1.30	-2.51	0.26
p-value	0.24	0.76	0.51	0.34	0.77
Net_migr_m_05_09	-2.92	-1.42	2.10	-3.81	0.65
p-value	0.03	0.68	0.03	0.30	0.54
Divorces_05_09	0.31	0.27	0.25	-0.45	0.14
p-value	0.08	0.13	0.19	0.61	0.52
Birthr_15_19_05_09	-3.81	-2.85	-1.42	0.33	1.16
p-value	0.01	0.26	0.30	0.89	0.10

Birthr_40plus_05_09	-2.53	-3.40	1.03	-0.28	-0.24
p-value	0.01	0.12	0.04	0.87	0.47
Life_exp_05_09	0.03	0.53	0.58	-0.36	-0.59
p-value	0.90	0.57	0.62	0.54	0.02
Sex_ratio_20_39_05_09	-0.01	0.01	0.01	-0.04	0.01
p-value	0.57	0.92	0.10	0.28	0.56
Tot_fertr_05_09	0.23	0.26	-0.08	0.08	-0.09
p-value	0.00	0.06	0.64	0.11	0.03
School_leave_me_05_09	-0.11	-1.14	-16.11	-6.44	-2.34
p-value	0.93	0.05	0.05	0.13	0.05
School_leave_hi_05_09	-8.92	-17.23	5.17	2.06	-2.70
p-value	0.00	0.09	0.22	0.70	0.29
School_leave_lo_05_09	0.51	-2.71	-2.69	-0.26	2.39
p-value	0.56	0.26	0.04	0.90	0.00
Apprshp_places_05_09	0.00	2.09	2.04	3.86	3.55
p-value	1.00	0.44	0.01	0.01	0.03
Students_18_25_05_09	-58.44	-47.68	30.16	-40.05	8.16
p-value	0.00	0.02	0.56	0.41	0.44
HH_inc_05_09	-104.66	-118.23	-59.72	22.00	-116.83
p-value	0.10	0.54	0.84	0.71	0.02
Inf_mortality_06_09	0.83	0.18	1.17	-1.33	0.81
p-value	0.52	0.59	0.38	0.20	0.11
_0_2_FD_ccare_07_09	-4.56	-9.13	-0.82	-3.44	-2.58
p-value	0.00	0.06	0.40	0.19	0.00
_3_5_FD_ccare_07_09	-12.76	-20.58	0.87	-19.58	-15.22
p-value	0.00	0.06	0.77	0.15	0.00
LFP_rate_f_08_09	4.36	1.18	-2.99	3.53	-0.26
p-value	0.01	0.18	0.19	0.27	0.79
LFP_rate_m_08_09	7.03	7.22	-6.04	3.05	-1.90
p-value	0.00	0.16	0.30	0.44	0.11
Share_jobs_req_exp_10_16	-4.87	-3.28	-0.48	-0.68	1.23
p-value	0.00	0.10	0.83	0.79	0.15
Share_jobs_req_spec_10_16	-1.08	-2.86	-1.58	0.79	0.67
p-value	0.07	0.09	0.13	0.16	0.22
N	6.00	3.00	3.00	4.00	18.00
Sign_1percent	18.00	6.00	0.00	1.00	4.00
Sign_5percent	27.00	11.00	8.00	4.00	10.00

Sign_10percent	30.00	22.00	9.00	5.00	11.00
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Results for east-east bordering counties by border

TABLE 1.B.3: Balancing Tests: East-East Bordering Counties, Pre-treatment

	balancingvars_all					
	MV-BB	SA-THU	SA-SANH	SA-BB	SANH-BB	SANH-THU
Emp_rate_m_05_09	5.54	-0.45	-0.55	1.87	-0.93	-0.59
p-value	0.01	0.81	0.66	0.30	0.32	0.65
Emp_rate_f_05_09	6.74	1.57	3.11	3.79	-1.68	-0.99
p-value	0.03	0.44	0.26	0.11	0.31	0.62
Unemp_rate_05_09	-4.77	-0.04	-1.17	-2.75	4.04	2.37
p-value	0.06	0.98	0.63	0.13	0.05	0.24
Share_pri_sec_05_09	0.88	-1.39	1.34	-1.11	0.19	-0.63
p-value	0.51	0.14	0.25	0.42	0.81	0.37
Share_ter_sec_05_09	-4.93	6.89	1.07	8.24	4.38	2.41
p-value	0.02	0.09	0.71	0.05	0.09	0.57
In_comm_05_09	11.89	-1.32	-6.98	2.58	0.46	9.39
p-value	0.10	0.66	0.36	0.29	0.89	0.09
Out_comm_05_09	14.64	-3.08	-4.17	0.24	-0.54	8.50
p-value	0.09	0.43	0.50	0.89	0.90	0.15
Marriages_05_09	0.09	-0.95	-1.11	-1.17	0.13	0.36
p-value	0.76	0.11	0.28	0.04	0.61	0.18
Share_apart_4p_05_09	4.47	-3.41	-2.33	-2.98	-1.60	1.22
p-value	0.00	0.46	0.59	0.38	0.39	0.74
Pop_dens_05_09	-12.21	88.43	-6.19	57.80	-12.72	-56.97
p-value	0.58	0.20	0.86	0.03	0.22	0.45
GDP_pp_05_09	-3112.75	2609.58	-2327.00	-1097.30	-976.00	1784.73
p-value	0.04	0.11	0.59	0.66	0.41	0.54
Vote_share_CDUCSU_05_09	8.05	4.55	4.92	9.67	3.88	-0.18
p-value	0.00	0.00	0.01	0.00	0.00	0.86
Avail_slots_childc_05_09	-0.04	0.15	0.05	0.04	-0.03	0.14
p-value	0.01	0.00	0.21	0.24	0.07	0.00
Child_u3_in_care_05_09	0.01	-0.00	-0.02	-0.00	0.02	0.02
p-value	0.02	0.78	0.00	0.94	0.00	0.01
Share_in_care_u3_05_09	1.44	-3.51	-9.54	-3.84	9.04	7.89

p-value	0.28	0.52	0.01	0.13	0.00	0.01
Share_in_care_3to5_05_09	-2.92	-1.44	0.42	0.90	-3.51	-2.04
p-value	0.17	0.20	0.76	0.66	0.01	0.07
Share_emp_15to29_05_09	1.84	1.07	0.84	2.47	-0.98	-0.91
p-value	0.08	0.15	0.15	0.00	0.09	0.13
Migbal_f_18_25_05_09	-29.79	17.78	-8.54	11.69	-11.15	-7.14
p-value	0.11	0.11	0.12	0.25	0.21	0.54
Migbal_m_18_25_05_09	-19.10	8.84	-4.90	1.80	-7.06	-0.62
p-value	0.06	0.22	0.02	0.75	0.18	0.93
Migbal_f_25_29_05_09	1.78	7.16	4.38	18.50	-21.68	-1.14
p-value	0.88	0.09	0.43	0.01	0.05	0.80
Migbal_m_25_29_05_09	-10.36	7.60	0.59	8.93	-9.29	2.96
p-value	0.37	0.12	0.82	0.07	0.17	0.51
Net_migr_f_05_09	-5.48	3.27	1.75	3.41	-8.45	-1.24
p-value	0.34	0.01	0.42	0.03	0.05	0.37
Net_migr_m_05_09	-6.28	2.61	1.30	2.26	-7.98	-0.23
p-value	0.24	0.02	0.54	0.10	0.05	0.88
Divorces_05_09	-0.22	-0.63	-0.45	-0.80	-0.04	-0.16
p-value	0.27	0.00	0.15	0.00	0.78	0.27
Birthr_15_19_05_09	-1.82	0.57	-2.77	0.56	6.62	2.48
p-value	0.39	0.46	0.23	0.75	0.01	0.07
Birthr_40plus_05_09	-0.65	0.43	0.48	1.11	-1.30	-0.18
p-value	0.25	0.00	0.02	0.02	0.05	0.52
Life_exp_05_09	-0.26	0.11	0.54	1.02	-1.14	-0.34
p-value	0.31	0.75	0.28	0.02	0.00	0.35
Sex_ratio_20_39_05_09	-0.04	0.02	0.02	0.04	-0.03	-0.00
p-value	0.37	0.10	0.38	0.01	0.36	0.89
Tot_fertr_05_09	0.08	0.08	0.02	0.13	0.05	0.01
p-value	0.03	0.00	0.18	0.01	0.01	0.60
School_leave_me_05_09	-4.49	-4.11	-0.21	-4.21	-4.53	-2.85
p-value	0.01	0.00	0.51	0.07	0.01	0.00
School_leave_hi_05_09	-2.09	-0.93	1.62	-7.98	-3.65	-3.92
p-value	0.26	0.37	0.69	0.02	0.10	0.09
School_leave_lo_05_09	-0.22	1.55	-1.92	-0.40	0.69	3.67
p-value	0.86	0.01	0.29	0.64	0.31	0.00
Apprship_places_05_09	3.84	2.21	-2.38	1.12	4.95	1.48
p-value	0.04	0.15	0.00	0.60	0.00	0.02

Students_18_25_05_09	-0.18	13.09	-9.98	-1.91	13.09	3.50
p-value	0.44	0.18	0.42	0.82	0.00	0.61
HH_inc_05_09	52.91	39.13	53.74	32.48	-142.54	-9.31
p-value	0.35	0.08	0.29	0.32	0.03	0.67
Inf_mortality_06_09	0.85	-0.37	-0.73	-1.40	0.86	-1.09
p-value	0.11	0.61	0.10	0.03	0.08	0.16
_0_2_FD_ccare_07_09	1.43	-10.64	1.50	1.07	0.91	-9.39
p-value	0.22	0.06	0.48	0.66	0.63	0.01
_3_5_FD_ccare_07_09	6.33	-28.31	6.52	19.97	6.58	-34.07
p-value	0.03	0.00	0.02	0.00	0.07	0.00
LFP_rate_f_08_09	2.07	0.12	2.32	-0.33	-0.40	0.07
p-value	0.00	0.86	0.18	0.77	0.43	0.93
LFP_rate_m_08_09	1.51	1.09	1.83	-0.24	-1.90	-0.04
p-value	0.24	0.48	0.33	0.89	0.09	0.94
Share_jobs_req_exp_10_16	-2.32	0.33	-0.07	-0.38	1.91	0.91
p-value	0.04	0.74	0.91	0.77	0.12	0.34
Share_jobs_req_spec_10_16	-1.79	0.58	1.11	-0.28	-0.26	0.39
p-value	0.09	0.34	0.21	0.68	0.71	0.47
N	5.00	7.00	4.00	7.00	7.00	11.00
Sign_1percent	4.00	9.00	4.00	6.00	10.00	7.00
Sign_5percent	14.00	10.00	7.00	15.00	13.00	8.00
Sign_10percent	20.00	14.00	8.00	17.00	23.00	12.00

1.C States and counties by border and treatment status

TABLE 1.C.1: States and counties by border and treatment status

Panel A: Borders West-Germany		
Lower Saxony/Schleswig-H.		
State	Treatment status	Counties
Lower Saxony	Treated (2013)	Harburg, Lüneburg
Schleswig-H.	Never treated	Herzogtum Lauenburg
Lower Saxony/Hessia		
State	Treatment status	Counties
Lower Saxony	Treated (2013)	Northeim
Hessia	Never treated	Kassel (city), Kassel (district), Werra-Meißner
Lower Saxony/North Rhine-W.		
State	Treatment status	Counties
Lower Saxony	Treated (2013)	Diepholz, Hameln-Pyrmont, Holzminden, Nienburg/Weser, Schaumburg, Osnabrück (city), Emsland, County of Bentheim, Osnabrück (district)
North Rhine-W.	Never treated	Borken, Steinfurt, Warendorf, Gütersloh, Herford, Höxter, Lippe, Minden-Lübbecke
Panel B: Borders East-Germany		
Saxony/Saxony-A.		
State	Treatment status	Counties
Saxony	Treated (2009)	Leipzig, Nordsachsen
Saxony-A.	Treated (2010)	Burgenlandkreis, Saalekreis
Saxony-A./Thuringia		
State	Treatment status	Counties
Saxony-A.	Treated (2010)	Mansfeld-Südharz
Thuringia	Treated (2013)	Gera, Nordhausen, Sömmerda, Weimarer Land, Saale-Holzland-Kreis, Greiz, Altenburger Land

Notes: The table displays states and counties by internal state border and treatment status. The list only features balanced borders included in the empirical analysis. Panel A features states from all West-German balanced borders, while Panel B features states from all East-German balanced borders.

Chapter 2

Child-to-Staff Ratio Regulation and Child Development

2.1 Introduction

Many countries have been facing increased demand for early child care (ECC) - care for children aged under 3 (Felfe and Lalive, 2018). As governments work towards universal access and the share of children being exposed to ECC increases, researchers have become interested in the impact of early non-parental care on children's development. These studies produce mixed results suggesting that the effects of ECC enrollment depend on the study-specific quality of the care environment (Baker, Gruber, and Milligan, 2008; Datta Gupta and Simonsen, 2010; Herbst, 2013; Felfe and Lalive, 2018; Fort, Ichino, and Zanella, 2019).¹ While high-quality child care has the potential to promote children's development, children may be negatively affected by low-quality settings.

In light of these findings, the focus of the German government in the market for ECC has been shifting from aspects of quantity towards quality. In general, governments aiming to enhance the quality of ECC, introduce reforms that increase the minimum standards of regulated inputs to the production of child care that are associated with quality. Hence, governments target quality indirectly by regulating readily observed and measured structural features of the ECC environment, such as the child-to-staff ratio (CSR), the group size or the qualification of staff (Blau, 1999; Hotz and Xiao, 2011).

¹In these studies, the counterfactual mode of child care is either informal or private care.

The question rises whether reforms introducing a stricter regulation of selected inputs necessarily increase the overall quality of the ECC environment and promote children's development. In general, the reforms may only have consequences for the child care market and children if regulations are enforced and binding.² Besides, as minimum standards are typically increased only for a subset of quality characteristics, the reforms may motivate or force child care providers to engage in input substitution. For example, as a response to a stricter regulated CSR and an associated increase in personnel costs, child care providers may increasingly rely on lower qualified staff. Thus, reforms may lead to an undesirable deterioration of quality in more leniently regulated aspects of the ECC environment, and their impact on overall ECC quality and children's development is ambiguous (Blau, 2007).

3

In this paper, I analyze how stricter regulation of a single input to the production of child care in Germany impacts the structural quality of the child care environment and whether such reforms are associated with improved children's development. To address these questions, my study draws on variation in regulated minimum standards of CSR in ECC across regions of Germany over the time period from 2010 to 2017. In particular, I create a recording of regional reforms to the legislated CSR that I match with administrative panel data containing regional information on inputs to the production of child care that are associated with quality and individual-level data compiled from parental reports of children's development. To estimate the impact of the regulations on structural quality and children's development, I rely on the Synthetic Control Method (SCM) and on the Event-Study design.

My paper relates to the literature studying the impact of regulations in child care on the quality of the care environment (Leland, 1979; Blau, 1999; Blau, 2003; Blau, 2007; Hotz and Xiao, 2011; Ali, Herbst, and Makridis, 2021). These studies draw on the theory of regulations in competitive markets (Leland, 1979; Shapiro, 1986) and show that stricter regulations

²Interestingly, Ali, Herbst, and Makridis (2021) observe asymmetric effects of increased minimum standards in child care on compliance with regulations. In order to comply with stricter regulation of the CSR and group sizes, child care providers become more likely to violate the minimum requirements for staff education.

³In a competitive child care market, stricter regulation may also result in lower supply and higher prices. This may have an isolated impact on the development of children whose parents are "priced out of the market" (Hotz and Xiao, 2011). However, to the extent that parents value the increased quality of child care, demand may increase and offset the negative effect on quantity demanded and supplied. As child care in Germany is heavily subsidized and non-competitive, I assume supply and prices to be less sensitive to reforms. However, this assumption has to be verified as a reform-induced selection of children into or out of childcare would bias my estimated impact on children's development.

motivate child care providers to substitute between inputs to the production of child care without having strictly positive implications for child care quality. Analyzing data from the hiring process of child care providers, Ali, Herbst, and Makridis (2021) provide recent empirical evidence on increased minimum standards for the CSR and group sizes in the context of the U.S. child care market. In line with the hypothesis of input substitution, the authors report that child care providers respond to stricter regulation by decreasing the educational requirements for potential employees. Similar to Ali, Herbst, and Makridis (2021), I rely on variation in regulated minimum standards across regions and over time to identify the causal effects of regulations on the quality of child care. In contrast with this literature, my study addresses quality regulations in a non-competitive and highly subsidized child care market where providers have access to limited financial resources. Hence, I contribute to the literature by deriving implications of regulations for a market structure that has not received much attention to date.⁴

This work also links to the literature on the relationship between structural quality in child care and children's development, which mainly focuses on child care for children aged 3 to 6. Even though there is a general consensus that children in high-quality care settings are more likely to benefit from attending child care (Almond and Currie, 2011; Baker, 2011; Ruhm and Waldfogel, 2012; Herbst, 2013), previous studies report mixed findings on the relevance of structural features of the care environment for child outcomes (Blau and Currie, 2006; Duncan and Magnuson, 2013). While studies on child care in the U.S. often reject the idea that structural quality is linked to child outcomes (Blau and Currie, 2006; Walters, 2015), research from other countries suggests that children's development is influenced by structural features, such as the CSR as well as the qualification, experience and gender composition of staff (Araujo et al., 2016; Bauchmüller, Gørtz, and Rasmussen, 2014; Gørtz, Johansen, and Simonsen, 2018).

Drange and Rønning (2020) contribute to the scarce evidence on the causal effect of structural quality in ECC on children's development from Norway. To overcome identification issues due to endogenous sorting of children into child care based on quality, the authors rely on a quasi-random assignment of child care slots. The findings suggest that children's

⁴Zierow (2017) studies family day care in Germany.

development is unaffected by structural features such as the CSR, qualification and tenure of staff. At the same time, a higher share of male staff and a lower sickness absence of staff appears beneficial for child outcomes.

My work relates to Drange and Rønning (2020) in terms of structural features of the child care environment considered in the analysis. Similar to their study, I also examine the CSR and the qualification of staff. Differences exist regarding the age group of children in child care and the employed measure of children's development. Drange and Rønning (2020) use a sample of children enrolled in child care from age 2 to 5. The outcomes of children are based on academic achievements in language and mathematics at primary-school age. Accordingly, the authors report the combined effect of exposure to a given quality in care for children aged under 3, i.e. ECC, and for children aged 3 to 5. In contrast, I study a sample of children that attends ECC without information about the duration of child care enrollment, and my measure of children's development is reported adaptive behavior of children in early kindergarten age. Hence, I aim at contributing to this literature by adding novel evidence on the relationship between structural quality in child care and early children's development.

My findings suggest that the CSR reforms have no statistically significant effect on the observed CSR. With respect to other aspects of structural quality, my analysis shows that the CSR reforms are not associated with an increase in lower-qualified pedagogical staff in child care and neither with a change in full-time employed staff. These results contrast with those from studies on regulations in competitive markets by rejecting the hypothesis of child care providers responding to the stricter regulation by substituting between inputs to the production of child care. Turning to child outcomes, I find that the increased minimum standards for the CSR have no significant impact on children's development. This is not in line with results from related European studies. Note, however, that the effects on children's development are obtained from a small sample and under the strict assumption of no reform-induced selection into or out of ECC. In sum, I cannot confirm that stricter CSR regulation in ECC is an effective policy measure to raise quality in a non-competitive and highly subsidized child care market. Moreover, my analysis of the German ECC market suggests

that the link between improved CSR in ECC and child outcomes is weak. Overall, the results hint at a potentially limited scope of such reforms to improve children's development in this child care setting.

The remainder of this paper is structured as follows. Section 2 describes the institutional background. Section 3 presents the theoretical considerations. Section 4 presents data, while Section 5 discusses the empirical strategy and the main results. Section 6 provides robustness checks, and Section 7 concludes.

2.2 Institutional Background

2.2.1 Early Child Care in Germany

In Germany, formal center-based child care is provided for children aged 0-2 (ECC) in early child care centers (*Krippen*) and for children aged 3-6 in kindergarten. The child care system follows a highly decentralized approach in which the federal, the state and the municipal governments as well as youth welfare offices and providers of child care take on different tasks and responsibilities (Jessen, Schmitz, and Waights, 2020). This implies that, like in the US and many other European countries, the responsible authorities are operating on different regional levels (European Commission, 2014; Ali, Herbst, and Makridis, 2021).

In contrast to the child care market in the US, which is characterized by monopolistic competition, the German child care system is not competitive (Berlinski et al., 2020). Most of the care centers are run by the municipality or other non-profit organizations, such as the church or welfare associations (Felfe and Zierow, 2018). As in many European countries, center-based care in Germany is predominantly financed by public subsidies that cover about 80% of the total operating cost of formal child care, while parental fees account for the remainder (European Commission, 2014).⁵ Moreover, in Germany, Switzerland, Italy and some Nordic countries, the amount of parental fees is regulated on the regional level (European Commission, 2014; Gørtz, Johansen, and Simonsen, 2018; Felfe and Lalive, 2018; Drange and Havnes, 2019). Thus, unlike in the US, where private firms set parental fees, parents in Germany are limited in their ability to select into child care of a better quality

⁵In some countries, like Cyprus, Malta and the United Kingdom, private providers are predominant. Hence, parents face higher fees in these countries.

by paying more when leaving aside the possibility of residential relocation (Zierow, 2017). Many German states charge fees that depend on income and the number of own children enrolled in care. For instance, similar to other European countries, Germany offers financial support to low-income families by reducing fees for child care (European Commission, 2014). With net costs of child care for a couple amounting to 11% of average wages, parental fees in Germany are lower than the EU average of 15% (Camehl, Schober, and Spiess, 2018).

For some time, the expansion of subsidized child care supply was high on the political agenda of Germany. Since 1996, children aged 3 and older have a legal entitlement to a slot in child care. As a result most children of kindergarten age are enrolled in formal child care. Until a few years ago, child care for children aged 0-2 was characterized by severe excess demand. This shortage in the supply of Early Childcare has been more pronounced in West Germany. In East Germany an extensive Early Child Care system was established during the existence of the German Democratic Republic. In 2005 (TAG) and 2008 (KiföG), the legislator passed two federal laws that made additional funding for care of children aged 0-2 available and created an obligation for regional governments to increase the provision of slots in Early Child Care. Since 2013, the legal entitlement to child care applies to children aged 1 and older. Due to these expansion efforts, the share of children aged 0-2 attending formal child care more than doubled between 2007 and 2015 (Camehl, Schober, and Spiess, 2018). More recently, the policymakers' focus in the market for child care is shifting more and more from aspects of quantity towards quality. As studies suggest that the scope of child care to promote child development depends on the quality of the experienced child care environment, the legislators are increasing the stringency of regulations on the provision of child care.

2.2.2 Child Care Quality Regulations in Germany

Regulations in the child care market often include characteristics that represent readily observed and measured determinants of child care quality. These regulated determinants of child care quality are aspects of the child care environment that shape the "nature of interactions between provider and children" (Blau, 1999). Similar to the US and Norway, regulations in Germany address structural features such as the child-to-staff ratio (CSR), the group

size, the qualification of staff, building requirements and pedagogical principles (Camehl, Schober, and Spiess, 2018).⁶ At the same time, some other Nordic countries refrain from defining minimum requirements for the CSR and group size (European Commission, 2014).

By requiring providers to offer child care that meets minimum standards of quality, legislators pursue the objective of ensuring children's wellbeing. Regulations are also motivated by the expectation that improved child care quality is conducive to children's health and development. In Germany, Switzerland, Italy, Norway and the US, the minimum quality requirements vary between regions (European Commission, 2014; Drange and Rønning, 2017; Ali, Herbst, and Makridis, 2021). Moreover, regulations may depend on the age of children. For example, in Germany, the US and Norway, the regulation of CSR and group sizes is more stringent for child care of children aged 0-2 than for care in kindergarten (Ali, Herbst, and Makridis, 2021). In Germany, local youth welfare services, which are operating on the county-level, monitor compliance with the regulations (Felfe and Zierow, 2018). However, for the US, where inspections to enforce regulations are carried out on the state-level, there is evidence of violated minimum requirements (United States Government Accountability Office, 2004; Blau, 2007; Ali, Herbst, and Makridis, 2021). In Germany, child care providers appear to comply with the regulations. But the possibility of imperfect enforcement and violations cannot be ruled out (Felfe and Lalive, 2018; Camehl, Schober, and Spiess, 2018). Even if quality regulations are perfectly enforced in the German child care market, it can be expected that child care providers do not exceed the minimum requirements considering that German child care features a non-competitive, underfunded market where providers are unable to set parental fees individually. Finally, unlike the US or the United Kingdom, the German child care market features no national accreditation, quality rating or improvement system. (Camehl, Schober, and Spiess, 2018; Blanden et al., 2021).

⁶Like the Nordic and other central European countries, Germany follows the social pedagogy tradition that puts emphasis on "socialization and learning through play" (Gørtz, Johansen, and Simonsen, 2018). This stands in contrast to the pre-primary pedagogic approach of many English- and French-speaking countries where children are exposed to formal learning processes to ensure a child's later school readiness (Drange and Rønning, 2020).

2.2.3 Reforms of the Child-to-Staff Ratio

Between 2010 and 2017 seven German states adopted reforms that increased the stringency of Child-to-Staff Ratios (CSR) in early child care (ECC).⁷ Table 3.1 provides an overview of these regulatory changes. Stricter regulations of the CSR were primarily introduced as a policy to improve structural quality (see Sachsen-Anhalt Landtag, 2017; Bundesministerium für Familie, Senioren, Frauen und Jugend, 2019; Evangelischer KITA-Verband Bayern, 2015).⁸ For some time, experts and pedagogues were criticizing lenient minimum requirements for the CSR in many states of Germany, which resulted in children being exposed to pedagogically inappropriate quality of child care (Sell, 2010; Viernickel et al., 2013). Hence, harmonization of the CSR across Germany was demanded to reduce regional disparities in educational opportunities (Bertelsmann Stiftung, 2014). In West-Germany, the CSR was reformed in Bavaria and Hamburg, while in East-Germany (including Berlin) variation in regulatory reform of the CSR is found in all states except for Mecklenburg-Western Pomerania. This highlights that backlog demand for CSR improvement was especially pronounced in East-Germany where, relative to former West-Germany, states traditionally featured higher childcare attendance rates. Because staffing costs represent the most relevant expense factor in childcare, East German states may have relied on relatively weaker CSR requirements with lower personnel deployment (Sachsen-Anhalt Landtag, 2017).

⁷Hesse introduced an updated method of calculation of the CSR. This regulatory change was not motivated by improving the CSR. The evidence suggests that the CSR was mostly unaffected by the reform. Thus, I consider Hesse as a never-treated state in the analysis. Further, Saarland is excluded from the analysis because it introduced a weaker CSR in 2014.

⁸For Saxony, official documents suggest that increasing the attractiveness of work in child care was an additional motive for changing the CSR.

TABLE 2.1: Child-Staff-Ratio Age 0-2 Reforms in Germany

State of Reform	Region	^a Year of CSR Reform	^b Legislated Child-Staff-Ratio	
			Pre-reform	^c Post-reform
Saxony-Anhalt	East	2013	1:6.8	1:6.7
Thuringia	East	2013	1:8.0	1:6.0
Bavaria	West	2015	1:5.8	1:5.5
Brandenburg	East	2015	1:7.5	1:6.9
Hamburg	West	2015	1:6.8	1:6.4
Berlin	West/East	2016	1:7.0	1:6.5
Saxony	East	2017	1:6.0	1:5.5

Notes: The table shows German states, which adopted Child-Staff-Ratio (CSR) reforms for children aged 0-2 between 2010 and 2017. Reading example legislated CSR: The pre-reform legislation in Saxony-Anhalt stipulated a ratio of 1 staff member for every 6.8 children. Source: Own calculations. ^a The year of the CSR reform refers to the first occurrence of a reform, which may be followed by successive CSR reforms of a given reform state. The year of the CSR reform refers to the date from which on the reformed CSR was enforced after a potential transition period. The reforms in Bavaria and Thuringia included a transition period (3-4 years). All states adopted the CSR reform in August/September (beginning of the school year) of the reform year with the exception of Hamburg which adopted the CSR reform in April. ^b The CSR are calculated as age-weighted averages for reform states with age-specific legislated CSR. ^c For a given reform state, the post-reform CSR refers to the CSR, which is adopted in the first occurrence of a reform. The legislated CSR may be altered by successive CSR reforms.

2.3 Theoretical Framework

2.3.1 Theory on the Intensity of Staff Deployment and Further Determinants of ECC Quality

Theoretical models of minimum quality standards in competitive markets hypothesize that stricter requirements for child care quality only impact child care markets where regulations are enforced and have a binding character in the sense that existing quality supplied by child care institutions falls below the standards stipulated by the upcoming, more stringent regulation. In that case, increasing the stringency of a subset of child care quality characteristics

raises the cost of providing child care, which may incentivize child care providers to pursue input substitution. Thus, with the stricter regulated input factors becoming more costly, child care providers may seek to lower the costs of more leniently regulated aspects of child care quality (Leland, 1979; Ali, Herbst, and Makridis, 2021; Blau, 2007).

In non-competitive and highly subsidized child care markets where most institutions do not operate for-profit, like in Germany and other European countries, the theoretical predictions may be different. However, if providers are underfunded and unable to set parental fees individually, most institutions may offer child care that does not exceed the mandatory minimum standards of quality (Evangelischer KITA-Verband Bayern, 2015). This implies that stricter regulation would be binding in such a child care environment. Hence, if compliance with the regulations is monitored and providers make an effort to comply with the imposed minimum requirements, reforms increasing the stringency of the CSR lead to higher personnel costs for which child care providers may compensate by increasingly relying on other inputs of a lower quality.⁹ There is supporting evidence showing that the CSR reforms resulted in an increase in the number of exceptional authorizations granted to providers to deviate from the minimum requirements for pedagogical staff (Landratsamt Ebersberg, 2019).

Beyond that, as increased minimum standards for the CSR lead to higher personnel deployment, input substitution may not only be motivated by cost minimization efforts, but also by child care providers' need to meet staffing requirements. For instance, in child care markets, where the hiring process faces a shortage of qualified staff, providers may have to draw more or less involuntarily on lower qualified candidates.¹⁰ In a similar way, CSR reforms may increase labor intensiveness in off-peak periods to which providers may have to respond by introducing more flexible working hours models, such as more part-time work.¹¹

In summary, the theoretical predictions from competitive markets according to which

⁹Bönisch and Tagge (2012) show that the cost of child care provision in Germany increases with the minimum requirements for the CSR.

¹⁰At the time of the CSR reforms, the German child care market experienced regional shortages of qualified staff. These shortages intensified in the following years. Since 2016, the number of unemployed pre-school teachers (*Erzieher*) in Germany is lower than the number of vacant positions (Deutsches Jugendinstitut E.V., 2017; Deutsches Jugendinstitut E.V., 2021).

¹¹These theoretical predictions hold under the assumption of child care supply being unaffected by the CSR reforms.

child care providers may respond to stricter CSR regulations with quality-harming input substitution to minimize costs may also apply to non-competitive and highly subsidized child care markets. Further, there is anecdotal evidence from Germany supporting the idea that such input substitution may also be driven by providers' need to meet staffing requirements. Accordingly, it is expected that the CSR reforms in Germany improve the effectively prevailing CSR, while the impacts on other aspects of the child care environment as well as on the overall quality are a-priori unclear.

2.3.2 Theory on Child Development

Turning to child outcomes, my analysis of the effect of stricter regulations of the CSR on children's development is motivated by a simple theoretical model that follows the model of skill production outlined in Cunha and Heckman (2007) and Cunha, Heckman, and Schennach (2010). Similar to Gensowski et al. (2020), θ_t denotes a child's skills obtained at the end of period t . In period zero, the skill level θ_0 is determined as follows:

$$\theta_0 = f(\theta_P, I_0), \quad (2.1)$$

where θ_P are parental skills, such as education, and I_0 are in-utero investments. In further periods, the skill level θ_t is a function of previous-period skills θ_{t-1} and public investments G_t made in early child care:

$$\theta_t = f(\theta_{t-1}, G_t), \quad (2.2)$$

where I assume that θ_t is increasing in both inputs.

Cascio (2014) notes that human capital formation depends on the process and structural quality of the child care environment. Hence, I define public investments G_t , which dictate early child care quality, as a function of process P_t and structural quality inputs S_t :

$$G_t = f(P_t, S_t), \quad (2.3)$$

where $\frac{\partial I_t}{\partial P_t} > 0$ and $\frac{\partial I_t}{\partial S_t} > 0$. Accordingly, higher inputs in process and structural quality raise early child care quality, public investments and are beneficial for the skill level of children.

Further, I model structural quality S_t as follows:

$$S_t = f(CSR_t, X_t), \quad (2.4)$$

where CSR_t is the effective prevailing CSR, and X_t is a vector capturing all other structural determinants of ECC quality. These include among others the group size, the qualification of pedagogical staff and the personnel continuity, i.e. low staff fluctuation (Sell, 2010). I rely on the common assumptions that structural quality S_t is decreasing in the CSR and group size, while it is increasing in staff qualification and personal continuity.

The CSR reforms represent random events that influence public investments G_t and child's skills θ_t indirectly via their effect on structural quality S_t . Based on the theory of minimum quality standards in child care markets, I expect the stricter regulations to potentially exert opposing effects:

1. In case stricter regulations are enforced and binding, the effective prevailing CSR CSR_t decrease. This, in turn, raises structural quality S_t and public investments G_t , which has a positive effect on the level of skills θ_t .
2. If the reforms motivate or force child care providers to engage in input substitution, inputs of other structural determinants captured in X_t are altered in such a way that structural quality S_t and public investments G_t decline. This deterioration has negative implications for a child's skills θ_t .

This implies that the effect of the CSR reforms on child development is undetermined and depends on whether the regulations are enforced and binding, whether input substitution is present and on the strength of the two potentially opposing effects.

I restrict the model to two periods: At the end of period zero, a child is born. During period 1, children are exposed to early child care and at the end of period 1, a measure of a child's skills is obtained. Accordingly, to capture the effect of the CSR reforms, I denote a child's skills (public investments) in the case of stricter CSR regulations with θ_1^T (G_1^T) and in the hypothetical absence of the reforms with θ_1^C (G_1^C). From this follows that skills in period 1 are either $\theta_1^T = f(\theta_1, G_1^T)$ if a child is exposed to a CSR reform or $\theta_1^C = f(\theta_1, G_1^C)$ in the counterfactual scenario without treatment (Dietrichson, Lykke Kristiansen, and Viinholt,

2020). Ultimately, the effect on child outcomes is represented by the skill difference $\beta = \theta_1^T - \theta_1^C$. This means that children's development benefits from a stricter regulation of the CSR if $\beta > 0$ and suffers from it if $\beta < 0$.

To analyze child development, studies draw on measures that may capture cognitive or non-cognitive dimensions of skills. Cognitive skills are typically obtained from academic performance, such as mathematics or language test scores (see Drange and Rønning, 2020), while assessments of non-cognitive skills may be obtained from household surveys, such as the National Longitudinal Survey of Children and Youth (see Baker, Gruber, and Milligan, 2008) or the SOEP (see Busse and Gathmann, 2020b), from teachers, e.g. in the National Pupil Database (see Blanden et al., 2017), or from general practitioners as in the case of the School Entry Examinations (SEE) (Felfe and Lalive, 2018). My analysis relies on adaptive behavior reported by parents. This outcome measure captures child development along the cognitive and non-cognitive dimensions of skills and relies on the Vineland Adaptive Behaviors Scales (VABS).¹² A potential concern is that parental reports may suffer from systematic reporting bias. For example, parents may have a biased perception of their child's development or report higher skills to meet society's expectations. However, Busse and Gathmann (2020b) state that several studies confirm the validity of parental reports and of the VABS in particular. Regarding the VABS, assessments show that parental reports positively correlate with teacher reports and with an infant development test performed by professional examiners (Felfe and Lalive, 2012; Sandner and Jungmann, 2016). With regard to the relevance of the VABS, there is evidence supporting the idea that skills measured on the VABS are positively associated with later life skill accumulation and adult outcomes. According to Schatz and Hamdan-Allen (1995), there is a correlation between VABS and IQ. Further, Szatmari et al. (2003) show that the measure of communication skills in VABS is related to communication skills of children aged 10 to 13 years.

My theoretical model excludes any postnatal investments made by parents, i.e. any parental decisions in period 1 that can be considered as promoting children's development.

¹²The VABS were first published by Sara Sparrow, Domenic Cicchetti and David Balla in 1984. They are based on the Vineland Social Maturity Scale by Edgar Arnold Doll. The VABS measure an individual's ability to perform everyday tasks essential for their personal functioning and social integration. Developed for the purpose of diagnosing developmental disabilities, the VABS have great significance for the field of child development research (Schmiade, Spiess, and Tietze, 2008; Felfe and Lalive, 2012).

In contrast to Dietrichson, Lykke Kristiansen, and Viinholt (2020), I impose the assumption that parents do not decide between sending their children to ECC or caring for them at home. This is a strong, but necessary assumption because my estimation strategy may only identify the true effects of the CSR reforms in the absence of reform-induced selection of children into or out of attending ECC. My third chapter suggests that the CSR reform in Bavaria has no statistically significant impact on ECC enrollment by sociodemographic background. I also refrain from modeling parental investments as a function of time spent at home nurturing the child, for example reading with children (see Gensowski et al., 2020). Accordingly, my analysis is based on the additional assumption that parenting at home is unaffected by the CSR reforms. If this assumption is violated, the estimated effect on children's development captures the sum of a direct effect from children being exposed to a different ECC quality and an indirect effect from a reform-induced response of parenting at home. In case this assumption holds, I can attribute the estimated impact on child outcomes entirely to the changes in the quality of the ECC environment.¹³

2.4 Data

To identify the causal effects of the CSR reforms, I rely on various data sets. For the analysis of German child care quality, I use state-level administrative data from the *Ländermonitor*; the analysis of children's development is based on individual-level survey data from the German Socio- Economic Panel (SOEP). In each case, I supplement these main data sets with state-level administrative data from the Federal Statistical Office, the Federal Institute for Research on Building, Urban Affairs and Spatial Development, and the *Betreuungsatlas*.

¹³An important extension to this paper would involve the inclusion of postnatal parental investments in the theoretical model and, thereby, allowing parental behavior to respond to the CSR reforms. Such an extension would also motivate an empirical study on the impact of the stricter regulation on parents' demand for child care and on parenting at home. This would be valuable as previous evidence suggests that parents respond to public investments in child care and school quality (see Gensowski et al., 2020; Cristian Pop-Eleches and Miguel Urquiola, 2011; Fredriksson, Öckert, and Oosterbeek, 2016). In my context, it may be possible to test the assumption of no reform-induced response of parenting at home using data from the German Family Panel (pairfam). The data include information on joint activities between parents and their children, e.g. reading books, telling stories, painting, singing or playing instruments (see Jessen, Waights, and Spieß, 2019).

2.4.1 The Ländermonitor

As part of the project “Ländermonitor”, the Bertelsmann Stiftung¹⁴ yearly publishes a compilation of data on the German child care environment from administrative statistics of the Statistics on Child and Youth Welfare, additional public statistics and from regularly surveying the federal state ministries in the area of child care. To analyze the impact of the CSR reforms on determinants of child care quality, I extract information on personnel resources keys in ECC¹⁵ reflecting the intensity of staff deployment, as well as the share of pedagogical staff without a vocational degree and the share of full-time employed staff in child care - including ECC and kindergarten. The share of pedagogical staff without a vocational degree is included as a proxy for the qualification of staff. In the context of early child care, pedagogical skills are essential and are typically acquired through vocational training or in a study programme.¹⁶ Further, I proxy the degree of staff fluctuation by the share of full-time employed staff in child care. Since full-time employees spend more time at work, I expect my measure to be related to personnel continuity, which allows staff to build stronger relationships with children through the provision of consistent and continuous child care.

2.4.2 The German Socio Economic Panel (SOEP)

The SOEP is an annual household panel commissioned by DIW Berlin.¹⁷ It provides researchers with representative individual-level data on private household members in Germany. Currently about 30,000 individuals are repeatedly surveyed. Individuals may exit from the survey due to moving abroad, due to death, or due to refusal to continue participation. The focus of the SOEP survey lies on gathering information about socio-economic and socio-demographic characteristics. The SOEP survey started in former West-Germany

¹⁴The Bertelsmann Stiftung is set up as an independent and politically nonpartisan foundation. By carrying out projects, studies and events, the foundation aims at promoting the public debate within the German democratic system.

¹⁵The personnel resources keys are defined as the ratio of contractually agreed childcare hours of children to the contractually agreed volume of employment of childcare staff.

¹⁶It is important to note that pedagogical staff without a vocational degree have not completed vocational training or a study programme.

¹⁷DIW stands for Deutsches Institut für Wirtschaftsforschung, i.e. The German Institute for Economic Research. The institute is a non-profit association based in Berlin. It analyzes domestic and international economic activities, provides research-based economic policy advice, and shares scientific knowledge with the general public.

in 1984 and former East-German households are included since 1991 (Wagner et al., 2008; Spiess et al., 2011).

Children's development

Since 2002 the SOEP includes an additional mother-child questionnaire where parents are asked to report adaptive behavior of their children at age 2-3. The reported adaptive behavior captures child development along the cognitive and non-cognitive dimensions of skills and is based on the Vineland Adaptive Behaviors Scales (VABS).

The questionnaire surveys four distinct subgroups of adaptive behavior: language skills, everyday skills, motor skills and social relationships. In each subgroup, assessment is based on five questions. For example, in the category "language skills", a question asks whether the child speaks in full sentences, and in the category "everyday skills", parents are asked to report whether the child uses scissors to cut paper. For each question, parents report the level of skill proficiency to which I award points accordingly: 2 points for mastered, 1 point for partially mastered, and 0 points for not mastered. Finally, I create an overall index by adding up a child's scores from the twenty questions ranging from 0 to 40 (see Busse and Gathmann, 2020a). Thus, there is a positive relationship between a child's skills and obtained scores.

For my empirical analysis, I restrict the sample to private households with children born according to birth-month specific cutoff dates and draw on SOEP survey waves that correspond to mother-child questionnaires of children born between 2009 and 2015.¹⁸

Additional information from the SOEP

The SOEP also provides detailed information on child care attendance. The survey asks parents whether their children are only cared for by members of the household or whether parents rely on any alternative mode of child care, such as publicly subsidized child care, a

¹⁸I only include observations with complete information on adaptive behavior, parenting intensity, age in months, weight, height. Further, I assume that children are enrolled in ECC (kindergarten) in the calendar year of their first (third) birthday (see also footnote 8). Because of this, I restrict the sample to children aged 37 to 43 (if born in January), 36 to 42 (if born in February), 35 to 41 (if born in March), ..., 26 to 32 (if born in December) months at the time of the interview. This birth-month specific sample restriction allows me to study reported adaptive behavior that is assessed not before attending the second half of the second ECC year and not later than one month before entering kindergarten.

professional child minder or another person outside the household (e.g. by relatives, friends or neighbors). Moreover, parents report the number of hours per week any alternative mode of child care is used. I use this information to create a variable indicating whether a child is cared for in publicly subsidized child care or by a professional child minder at least part-time, i.e. for more than 19 hours per week.

Motivated by the empirical evidence that parents typically enroll their children in ECC (kindergarten) in the calendar year of a child's first (third) birthday, identification in my empirical analysis is based on the simplifying assumption that children attend ECC for exactly two childcare years, which – excluding the August summer holidays - amounts to 22 months.¹⁹ In order to effectively control for potential correlates of adaptive behavior and the CSR reforms, I extract information on the following socio-economic and socio-demographic indicators of the child, the parents and the household: gender, weight, height and age of the child, gender, marital status, employment status, nationality and education of the surveyed parent, the number of persons and children in the household and household income.

Table 2.1 reports on the differences between parents and children of control and treated states from my SOEP sample. The summary statistics suggest that even though treated and control children reach similar scores in adaptive behavior, treated children are statistically significantly shorter and younger relative to children from the control states. In treated households, the parent responding to the survey is statistically more likely to be female and the respondents are statistically less likely to be married and more likely to be single. While education levels of parents are comparable across observations from treated and control states, the share of full-time employed parents from the treated states is statistically significantly higher relative to control and not working is less common. Finally, in my sample treated households are statistically significantly smaller, have less children and a lower income relative to control households. These descriptive findings highlight the necessity of including many of the above-mentioned socio-economic and socio-demographic indicators

¹⁹In more detail, my assumptions on ECC attendance are: 1) children enter ECC at the beginning of the month of the new child care year (September) in the autumn before (if born October to December) or after (if born January to August) their 1st birthday, and that 2) children exit ECC and are transferred to kindergarten in the autumn before (if born October to December) or after (if born January to August) turning 3. Kuehnle and Oberfichtner (2020) observe a related birth-month specific enrollment pattern for children of kindergarten age using the NEPS survey Starting Cohort 4. My own calculations based on the NEPS survey Starting Cohort 1 replicate their findings for children aged 0-2.

as control variables in my analysis of the impact of the CSR reforms on children's development.

TABLE 2.1: Mean differences: Treated and Control from SOEP data

	Mean Control	SD Control	Mean Treated	SD Treated	Difference	t	p-value
Overall Score in Adaptive Behavior	30.04	5.87	30.38	6.24	0.35	1.02	0.31
Gender Child							
Female	0.47	0.50	0.51	0.50	0.03	1.20	0.23
Weight of child (kg)	14.30	1.94	14.20	2.30	-0.11	-0.88	0.38
Height of child (cm)	95.55	5.43	93.75	6.34	-1.80	-5.41	0.00
Age of child (months)	34.50	3.83	33.20	4.00	-1.30	-5.92	0.00
Gender Parent							
Female	0.92	0.27	0.95	0.22	0.03	1.88	0.06
Marital Status of Individual							
Married	0.76	0.43	0.68	0.47	-0.08	-3.15	0.00
Single	0.19	0.40	0.25	0.43	0.05	2.26	0.02
Widowed	0.00	0.00	0.00	0.05	0.00	1.23	0.22
Divorced/Separated	0.04	0.20	0.06	0.25	0.02	1.75	0.08
Employment Level of Individual							
Full Time	0.17	0.38	0.26	0.44	0.08	3.55	0.00
Part Time	0.59	0.49	0.58	0.49	-0.00	-0.12	0.90
Not Working	0.24	0.43	0.16	0.37	-0.08	-3.54	0.00
Nationality Parent							
German	0.91	0.29	0.92	0.27	0.01	0.91	0.36
Education Parent							
In school	0.00	0.04	0.00	0.04	-0.00	-0.02	0.98
Level 1	0.09	0.29	0.08	0.27	-0.01	-0.77	0.44
Level 2	0.47	0.50	0.48	0.50	0.01	0.33	0.75
Level 3	0.42	0.49	0.44	0.50	0.02	0.61	0.54
Number of Persons in HH	3.76	1.00	3.56	0.81	-0.21	-4.13	0.00
Number of Children in HH	1.82	0.91	1.66	0.74	-0.16	-3.43	0.00
HH Post-Government Income	50977.46	44283.66	45767.23	23644.66	-5210.23	-2.70	0.01
Observations	633		657				

Notes: The table displays weighted mean differences and t-statistics for my sample of private households with completed mother-child questionnaires of children born between 2009 and 2015, aged 26 to 43 months (with birth-month specific cutoffs) at the time of the interview and attending ECC for more than 19 hours per week. Parental education refers to the CAS-MIN classification with the following categories: elementary (level 1), secondary (level 2) and higher (level 3). Columns 1-2 (3-4) report summary statistics for observations from the control (treated) states. Column 5 displays the calculation of the mean difference as treated minus control; columns 6-7 report the statistical significance of the difference. All observations are weighted with person-related weighting variables. Source: SOEP.

2.4.3 Data from the Federal Statistical Office, the Federal Institute for Research on Building, Urban Affairs and Spatial Development, and the *Betreuungsatlas*

To supplement my main data from the *Ländermonitor* and the SOEP, I use state-level aggregated data from the Federal Statistical Office, the Federal Institute for Research on Building, Urban Affairs and Spatial Development, and the *Betreuungsatlas*.²⁰ With respect to the general socio-economic environment, I draw on the employment rate of women and men aged 25 to 49, the marriage rate per thousand, inhabitants per square kilometer, GDP per capita, the conservative vote share and the share of employment in the primary sector.²¹ Moreover, I draw on the following additional indicators of the ECC environment: the share of pedagogically employed staff aged under 30 and 30 to 49 in child care, the share of public providers, the share of children in full-day ECC, the share of children in full-day kindergarten, and the shares of children aged 0-2 in ECC as well as of those aged 3-6 in kindergarten.

2.5 Estimation Strategy

In a first step, I study whether the decision of reforming the CSR is related to the general socio-economic and early childcare environment of a state. Following this, I estimate the impact of the CSR reforms on the actually observed CSR, which I proxy by the intensity of staff deployment in ECC. This allows an evaluation of the extent to which the reforms had a binding character and, thus, forced child care providers to intensify staff deployment. Subsequently, I analyze the impact of the reforms on further determinants of child care (ECC and kindergarten) quality. This aids in uncovering the overall implications for child care quality, which are a priori ambiguous. In theory, intensified staff deployment would promote child care quality. At the same time, however, the CSR reforms may have encouraged or forced child care providers to engage in input substitution adversely affecting child care quality.

²⁰The *Betreuungsatlas* is published by the working group "Statistics on Child and Youth Welfare". The working group processes information from administrative statistics of the Statistics on Child and Youth Welfare and is part of the research partnership between the German Youth Institute (DJI) and the Technical University of Dortmund.

²¹The primary sector includes agriculture, forestry and fishing.

The final part of the analysis addresses child development where I estimate the impact of the CSR reforms on reported adaptive behavior. My analysis is limited to the overall index because it efficiently covers all four dimensions of adaptive behavior in a single measure. However, future work may take into consideration that the effects of the CSR reforms potentially differ by subgroup of adaptive behavior and may analyze each measure of child development separately. Throughout the empirical analysis, I rely on an identification strategy that exploits variation in the stringency of the CSR across states and over time.

2.5.1 Determinants of Adoption

To analyze the decision to increase the minimum standards of the CSR in more detail, I regress a binary variable indicating whether a state adopts a CSR reform in year t on the general socio-economic and early childcare environment. Observations are on the state-year level and results are displayed in Table 2.1. The socio-economic environment covers the unemployment rate, the employment rate of women 25 to 49, GDP per capita, inhabitants per square kilometer, the conservative vote share and the share of employment in the primary sector (in columns (1), (3), (4)). The early childcare environment is captured by the shares of children aged 0-2 in ECC, the share of children aged 0-2 in full-day ECC, the personnel resources keys in ECC, the share of staff aged 55 and older and the share of full-time employed staff (in columns (2), (5), (6)). In additional specifications (columns (3), (4), (5), (6)), I allow the effect of the determinants to differ by West/East Germany. In these regressions, I interact all characteristics with a binary variable indicating whether a state is a former West-German state. All control variables are lagged two years to take the time span of political decision-making processes into account.²²

²²When lagging all independent variables by one year instead, most of the coefficients are insignificant. Only the positive coefficient of the share of children aged 0-2 in ECC in specification (2) becomes statistically significant at the 5% level. The full estimation results are available upon request.

TABLE 2.1: Determinants of Adoption

	Adopt Child-Staff-Ratio Reform Policy					
	Germany		Differential effects by West/East			
	(1)	(2)	West	East	West	East
	(1)	(2)	(3)	(4)	(5)	(6)
General Socio-Economic Environment:						
Unemployment rate	-0.116		-0.012	-0.497**		
	(0.075)		(0.165)	(0.210)		
Female employment rate (25-U50)	0.004		0.005	-0.174***		
	(0.003)		(0.044)	(0.004)		
GDP per capita	0.000		-0.000	-0.000		
	(0.000)		(0.000)	(0.000)		
Inhabitants per km2	0.001*		0.001	0.001		
	(0.001)		(0.001)	(0.001)		
Conservative vote share	-0.058*		-0.058	-0.072**		
	(0.032)		(0.055)	(0.025)		
Share employment primary sec	0.227		-0.310	0.656		
	(0.907)		(0.821)	(1.262)		
Early Childcare Environment:						
Share children in ECEC U3		0.027			0.074	0.105
		(0.051)			(0.089)	(0.169)
Share children in Full-Day ECEC U3		0.098			0.368*	0.092
		(0.073)			(0.186)	(0.055)
Personnel Resources Key U3		-0.017			-1.330	0.052
		(0.363)			(0.899)	(0.687)
Share of staff aged 55 and older		0.052			-0.206	0.340
		(0.112)			(0.173)	(0.220)
Share of staff working full-time		-0.044			-0.198	-0.009
		(0.074)			(0.129)	(0.037)
Constant	1.884	-0.975	2.038		7.843	
	(1.222)	(3.051)	(2.277)		(5.884)	
Observations	104	50	104		50	
R-squared	0.254	0.402	0.353		0.494	

Notes: The dependent variable is an indicator equal to 1 if a state has adopted a CSR reform in year t and zero otherwise. All independent variables are lagged two years. As control variables in columns (1), (3) and (4) are included unemployment rate, employment rate of women 25 to 49, GDP per capita, inhabitants per square kilometer, conservative vote share and share of employment in the primary sector. In columns (2), (5) and (6), I only control for the share of children aged 0-2 in ECC, the share of children aged 0-2 in full-day ECC, personnel resources keys in ECC, share of staff aged 55 and older and the share of full-time employed staff. In specifications capturing differential effects by West/East Germany (columns (3)-(6)), all independent variables are interacted with an indicator equal to 1 if a state is a former West-German state. All specifications include state- and year-fixed effects. Standard errors are clustered at the state level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

With respect to the prevailing socio-economic environment, it appears that states with a higher population density and a lower conservative vote share are more likely to adopt a CSR reform. These coefficients are, however, only marginally statistically significant. When studying differential effects by West/East-Germany, the findings suggest that the selected characteristics are more strongly associated with the adoption decision for East-German than West-German states. For West-German states, none of the determinants reaches statistical significance. For East-German states, a lower unemployment rate, a lower female employment rate and a lower conservative vote share translate into a higher probability of adoption. Considering the ECC environment, the results suggest that most characteristics are not related to the probability of reforming the CSR. However, West-German states with a higher share of children in full-day ECC are more likely to introduce a stricter CSR regulation - albeit the relationship is only statistically significant at the 10% level. It should be noted, however, that the analysis of the determinants of adoption - and of the early childcare environment in particular - is based on a small sample, which may influence the results obtained. When I study differential effects of the socio-economic environment by West/East-Germany in columns (3) and (4), the number of observations is 56 for West-Germany and 48 for East-Germany. For the ECC environment, I rely on a sample of 28 observations for West- and 22 observations for East-Germany.

2.5.2 Impact on the Intensity of Staff Deployment and Further Determinants of Child Care Quality

To uncover the impact of the CSR reforms on the intensity of staff deployment, the qualification of staff and a proxy for the degree of staff fluctuation, I draw on the Synthetic Control Method (SCM), which was first introduced by Abadie and Gardeazabal (2003). The SCM is inspired by comparative case studies and the idea that an estimate of the effects of an event that occurs at an aggregate level and only affects one aggregate unit, such as a regional policy intervention, can be obtained from a comparison of the evolution of the outcome variables between the treated unit, i.e. affected by the event, and a control group of unaffected units. In order for this comparison to yield a credible estimate of causal effects, the units of the control group have to resemble the treated unit so that there exists a high degree

of co-movement in the evolution of the outcomes due to similar characteristics. Since single control units often lack similarity to the treated unit, the SCM departs from comparative case studies by relying on a combination of control units to provide a better comparison (Abadie et al., 2010; Abadie, 2020).

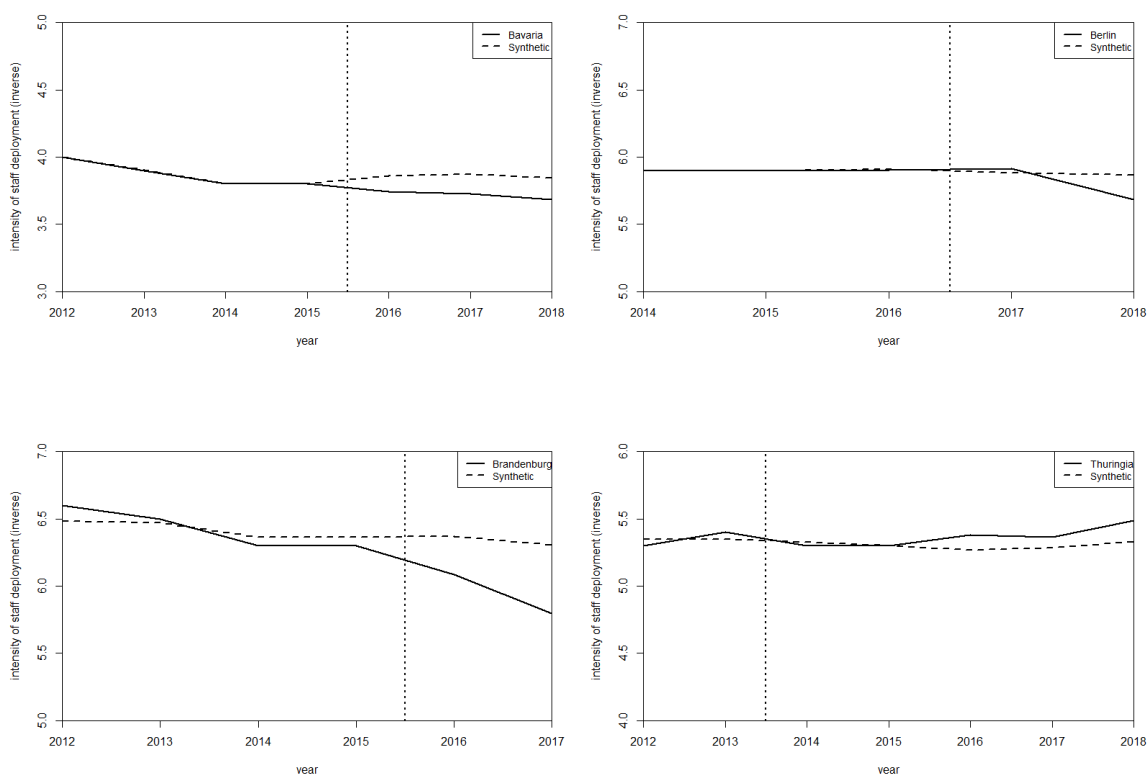
In my application of the SCM, I consider each CSR reform adopted by a different German federal state as a separate quasi-random experiment. Accordingly, I estimate the causal effects of the reforms for each treated state on its own. Based on a data-driven procedure, the SCM compares the evolution of the outcome variable in the treated state to a synthetic state, which is constructed as a weighted combination of control states. These weights define a synthetic state that best matches the treated state in terms of pre-treatment values of determinants and the pre-treatment evolution of the outcome variable. Hence, these determinants provide an assessment of the degree of similarity between the treated state and the control states. As determinants of the outcome variable, I choose two pre-treatment outcomes (2012 and 2013) and the covariates personnel resources keys, share of staff without a vocational degree, share of staff working full-time, share of staff aged under 25 years, share children in ECC aged under 3, share children in full-day ECC aged under 3, share of public child care providers, female employment rate (25-U50), population density and GDP per capita. The weights of the control states are reported in 2.A.

Figure 2.1 shows the results on the intensity of staff deployment by treated state.²³ The panels plot the evolution of the intensity of staff deployment over time for Bavaria, Berlin, Brandenburg, Thuringia and the corresponding synthetic counterpart. The figure shows that the intensity of staff deployment increases for Bavaria, Berlin and Brandenburg, while it decreases relative to the synthetic control for Thuringia. These increases in the intensity of staff deployment are observed in the first and second post-intervention period, while the decrease in Thuringia emerges in the longer run. It should be noted that a lower degree of co-movement in the evolution of the outcomes in the pre-treatment period (e.g. Brandenburg) may result in the SCM not being able to accurately predict the true counterfactual development of the treated state. Overall, the results suggest that in some treated states

²³I rely on personnel resources keys in care for children aged 0-2 as a measure of the intensity of staff deployment

child care providers were facing a binding more stringent CSR regulation where staff deployment intensified as a response. This response, considered in isolation, is assumed to be beneficial for the quality of the child care environment.

FIGURE 2.1: Impact of CSR Reforms on Intensity of Staff Deployment

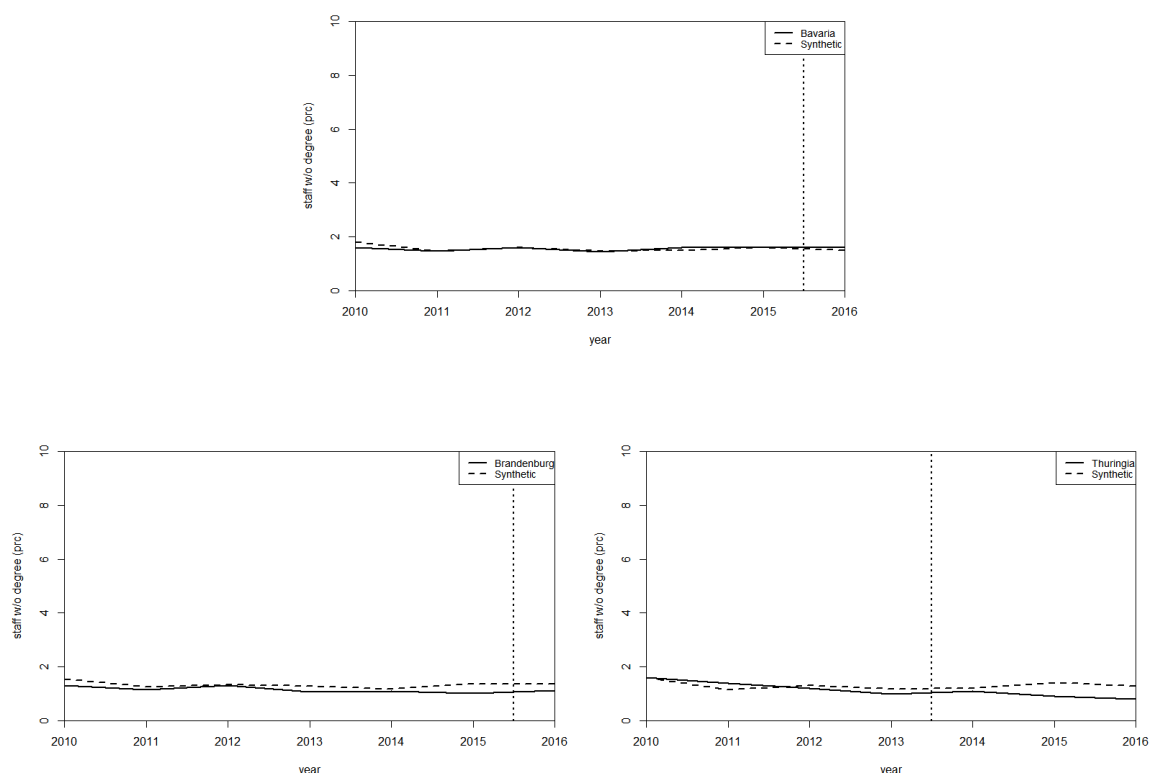


Notes: Figures show the evolution in the treated states Bavaria, Berlin, Brandenburg and Thuringia, respectively vs. their synthetic control of the intensity of staff deployment. The vertical line indicates the date of the CSR reform and separates the pre- and post-treatment periods. Source: Ländermonitor.

Figure 2.2 presents results on the share of pedagogical staff in child care without a vocational degree by treated state. The panels refer again to Bavaria, Brandenburg, Thuringia and the corresponding synthetic counterpart. Due to a lack of data, it is not possible to study the evolution of the share of pedagogical staff in child care without a vocational degree for Berlin. The figure suggests that the share of staff without a degree decreases relative to the synthetic control for Brandenburg and Thuringia, while it remains constant for Bavaria. In Brandenburg, the decline already emerges in the last pre-treatment period, while Thuringia experiences a lower share of staff without a degree in the second and third post-treatment period. The differential pre-treatment evolution of the outcomes in Brandenburg and its

synthetic control suggests that the SCM may not yield a precise estimate of the counterfactual development in the treated state. Overall, these findings reject the hypothesis that child care providers engaged in cost-saving input substitution. Hence, they speak against the idea that as a consequence of the stricter CSR regulation, providers increasingly reverted to staff without a degree to overcome the lack of hireable qualified staff or to ease financial constraints.

FIGURE 2.2: Impact of CSR Reforms on Share staff without degree

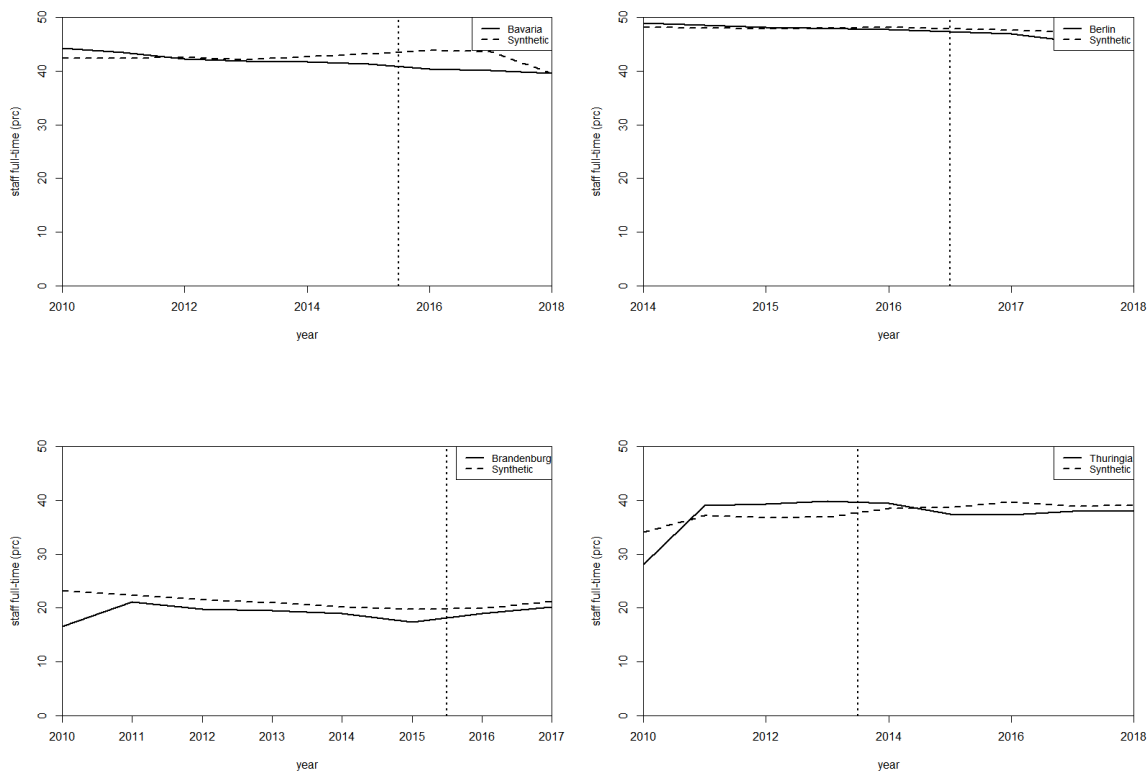


Notes: Figures show the evolution in the treated states Bavaria, Brandenburg and Thuringia, respectively vs. their synthetic control of the share of pedagogical staff in child care without a vocational degree. The vertical line indicates the date of the CSR reform and separates the pre- and post-treatment periods. Source: Ländermonitor.

Figure 2.3 shows the impact of the CSR reforms on the share of staff in child care working full-time for each treated state separately. The panels depict results for Bavaria, Berlin, Brandenburg and Thuringia, respectively. For Bavaria, Berlin and Thuringia, the figure shows a decline in the share of full-time employed staff relative to the synthetic control, while an increase is observed for Brandenburg. In Bavaria, Berlin and Brandenburg, the CSR reforms become effective in the first and second post-treatment period, while the impact emerges in Thuringia in the second and third year after treatment. It should be

mentioned that treated states (except for Berlin) and synthetic controls follow differential pre-trends, which may result in the SCM not being able to accurately construct the counterfactual development of the outcome variables. Still, the results imply that in the course of intensifying staff deployment to comply with stricter minimum standards, some providers substituted part-time for full-time employment in the production of child care. To the extent that lower work time reflects higher staff fluctuation, this input substitution negatively affected this aspect of child care quality.

FIGURE 2.3: Impact of CSR Reforms on Share Staff Employed Full-Time Deployment



Notes: Figures show the evolution in the treated states Bavaria, Berlin, Brandenburg and Thuringia, respectively vs. their synthetic control of the share of full-time employed staff. The vertical line indicates the date of the CSR reform and separates the pre- and post-treatment periods. Source: Ländermonitor.

Overall, the analysis in this section uncovered a positive effect of the CSR reforms on the effective prevailing CSR. This suggests that the reforms were binding and that child care providers attempted to comply with the new regulations. Considered in isolation, this intensified staff deployment is associated with an increase in ECC quality. Moreover, while input substitution appears to be absent concerning the qualification of staff, such a process

affected work time increasing staff fluctuation and reducing child care quality in this dimension. Thus, the CSR reforms lead to opposing effects on child care quality and the overall impact is uncertain. In the following analysis, I study child development and estimate the impact of the CSR reforms on reported adaptive behavior. Since the higher minimum standards for the CSR may have resulted in an unintended decline in overall child care quality, it is of importance to derive the reform-induced consequences for child development.

2.5.3 Impact on Child Development

To estimate the impact of the CSR reforms on child development, my estimation strategy compares differences in reported adaptive behavior between treated and control states before and after the reforms. Due to the staggered adoption of the policy and potentially dynamic treatment effects, I rely on an Event-Study design. This allows tracing out the evolution in outcomes with indicators for leads and lags of the reforms while controlling for time-invariant differences across states and birth months as well as trends over time. The estimation equation as follows:

$$O_{istm} = \alpha_s + \beta_t + \gamma_m + \sum_{\substack{y=-8 \\ y \neq -3}}^2 \delta_y T_s I(t - y_s^* = y) + \zeta_1 \mathbf{X}_i + \eta_1 \mathbf{Y}_s + \epsilon_{istm} \quad (2.5)$$

Here O_{istm} is reported adaptive behavior of child i , residing in state s , born in year t and month m . This outcome variable is the overall index ranging from 0 to 40, obtained by adding up scores (2 points: Skill mastered, 1 point: Skill partially mastered, 0 points: Not mastered) from the four distinct subgroups of adaptive behavior that measure the cognitive and non-cognitive dimension of skills. α_s is a state of current residence fixed effect, β_t is a year fixed effect and γ_m captures a month-of-birth fixed effect. $\mathbf{X}_i$ is a vector of control variables on the individual level.

These variables include gender of the child as well as weight, height and age in months at the time of reporting. The German SOEP also provides information about parents and the household. Hence, I control for the gender, marital status, employment status, nationality

and educational attainment of the interviewed parent. Further, I take the urbanization level of the region of residence, the number of persons and children in the household as well as household income into account.

Considering potentially confounding factors related to regional differences in the socio-economic or child care environment, $\mathbf{Y}_s$ is a vector of control variables on the state level. Characteristics describing the socio-economic environment cover the female and male employment rate (25-U50), the marriage rate per thousand, population density, GDP per capita and the conservative vote share. The child care environment is captured by the share of employed staff aged under 30 and aged 30 to 49, the share of public providers, the share of children in full-day ECC, the share of children in full-day kindergarten as well as the shares of children aged 0-2 in ECC and aged 3-6 in kindergarten. However, the CSR reforms may have impacted some variables of the child care environment, rendering these factors as "bad controls". Accordingly, I consider the age of employed staff, the share of public providers, the share of children in full-day ECC and kindergarten as well as children aged 3-6 in kindergarten as potentially endogenous.²⁴ Taking this into account, I estimate two specifications of the model. In the baseline model, the vector $\mathbf{Y}_s$ only captures variables of the socio-economic environment, while the extended model additionally includes all variables of the child care environment.

The coefficients of interest, δ_y , capturing the estimates of the treatment effects, are based on the interaction of a binary indicator, T_s , with event-year dummy variables, $I(t - y_s^* = y)$. The binary variable, T_s , identifies observations from treated states. It equals 1 if the child currently resides in a state that reformed its CSR regulation between 2008 and 2017, and zero otherwise. The event-year dummies, $I(t - y_s^* = y)$, are equal to 1 if measured from a child's birth year t , state s experienced a CSR reform y years ago. This means that $I = 1$ if and only if $(t - y_s^* = y)$, where y_s^* is the year in which state s reformed its CSR regulation. For the group of control states the event-year dummy variables are zero in all periods.

The last pre-treatment cohort in treated states comprises children born three years before

²⁴Regarding endogeneity of the ECC environment, I assume that the CSR reforms have no effect on ECC supply and demand at the extensive margin, which are represented by the shares of children aged 0-2 in ECC. Without this assumption, the composition of my sample may not remain stable over time due to a reform-induced selection of children into or out of childcare. This would render my estimates biased (Blundell and Macurdy, 1999b).

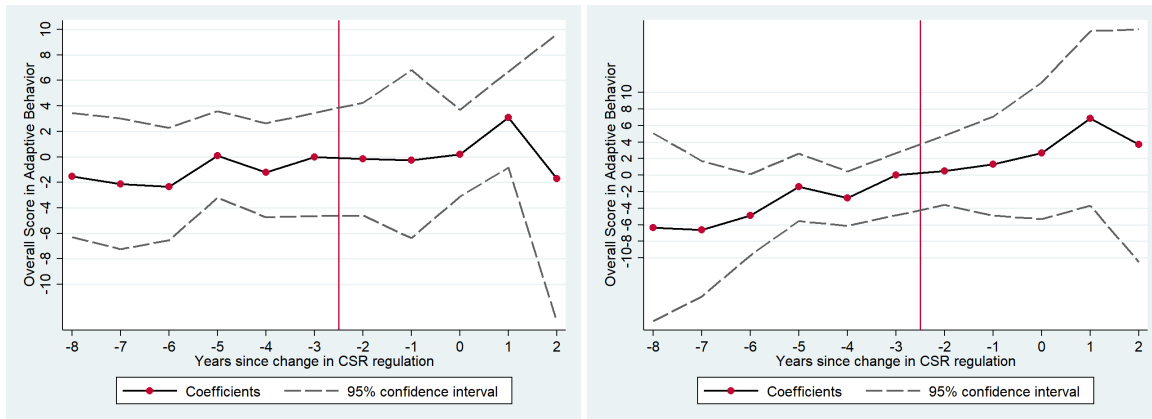
the reform year, y_s^* , where $y = -3$. From this follows that the pre-treatment period includes birth years three or more years before a CSR reform, while birth years two or less years before a new CSR regulation define the post-treatment period. Accordingly, I normalize the effect of the CSR reforms on the last pre-treatment cohort where $y = -3$ to zero and omit the corresponding event-year dummy. This implies that all children of cohorts born $y = -2$ or later are considered as treated. The method of estimation takes into account that the study design is based on a small number of treated and untreated state-level clusters. Hence, I calculate standard errors using the wild-cluster bootstrap procedure.²⁵

The coefficients of interest, δ_y , represent the deviation in outcomes of treated relative to control states in year y from the baseline outcomes of children born three years before the CSR reform where $y = -3$.

The Event-Study estimates of the impact of the CSR reforms on child development are shown in Figure 2.4. Table 2.B.1 in the Appendix also displays the Event-Study estimates of the impact of the CSR reforms on child development in table format. For both specifications, none of the point estimates for the indicators reaches a higher level of statistical significance. Table 2.B.1 shows that when excluding control variables of the ECC environment, there is a weak statistically significant positive effect on reported adaptive behavior in the first post-treatment period. For the specification including controls for the ECC environment, the coefficients for the post-treatment periods $y = 7$ and $y = -6$ are negative - but only statistically significant at the 10% level. With respect to the pre-treatment period, the results mainly suggest that the hypothesis of treated and control states following the same overall pretrend in adaptive behavior cannot be rejected based on the data available for my analysis. For the post-treatment period, the results hint at a statistically weak positive impact of the CSR reforms on adaptive behavior. Under the assumption that there is no reform-induced change in the composition of my sample, this finding suggests that child development may have benefited from the CSR reforms.

²⁵Considering the use of survey data, all regressions rely on person-related weighting variables to weight cross-sections of the SOEP waves.

FIGURE 2.4: Impact of CSR Reforms on Child Development



Notes: Figures report coefficients from the estimation of equation (1) for the specification excluding (including) covariates relating to the ECC environment on the left (right). The dependent variable is reported adaptive behavior. The coefficients represent the deviation in outcomes of children from treated relative to control states for cohorts born up to eight years before and two years after a CSR reform - as compared to the last pre-treatment cohort born three years before the CSR reform. The sample is comprised of children born between 2009 and 2015 from private households with completed mother-child questionnaires, aged 26 to 43 months (with birth-month specific cutoffs) at the time of the interview and attending ECC for more than 19 hours per week. All specifications include state-, year- and month-of-birth fixed effects. Standard errors are calculated using the wild-cluster bootstrap procedure. Source: SOEP.

2.6 Robustness Checks

2.6.1 Placebo Estimates: Impact on the Intensity of Staff Deployment and Further Determinants of Child Care Quality

To address concerns that estimates on the impact of the CSR reforms on determinants of child care quality from the previous analysis appearing large in magnitude may be insignificant and could simply be the result of the SCM not being able to accurately construct the counter-factual development of the outcome variables, I conduct placebo studies (Abadie et al., 2010). In these placebo studies, I iteratively apply the SCM to every state from the control group - separately for each CSR reform. Accordingly, in each iteration, I assign a placebo treatment and compare the evolution of the outcome variable in a state from the control group to its synthetic state where both units did not adopt a CSR reform.

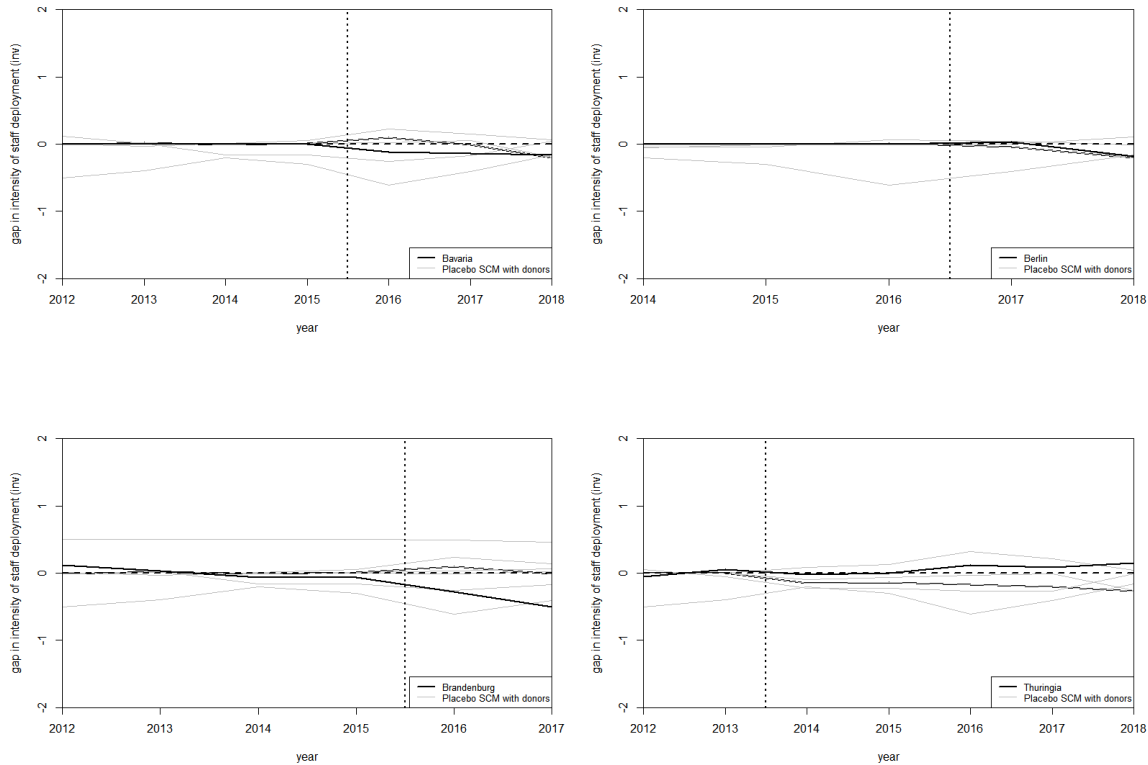
Assuming an absence of confounding events in control states, I consider my analysis as providing evidence of a significant effect of the CSR reforms on determinants of child care quality if the estimates of the placebo treatments are negligible in magnitude compared to ones obtained for the treated states. However, if the estimates of the placebo studies are similar in magnitude to the ones of the treated states, the estimated impacts of the CSR reforms are insignificant. In this case, the SCM appears to be unable to generate a more accurate match for the post-treatment period even in the absence of treatment and estimates of this magnitude could be generated solely by chance.²⁶

When evaluating the placebo studies, I focus on estimates where the SCM was able to construct a synthetic state that closely matches the pre-treatment evolution of the outcome variable in the placebo treated state. This follows from the assumption that an inaccurately reproduced pre-treatment path is accompanied by an inability to uncover the true counterfactual development. In each figure of this section, the gray lines show the gap in the outcome variable between each state from the control group and its synthetic state, while the black line plots the estimated gap for the treated state. Each figure presents results by treated state where panels refer to the CSR reform of Bavaria, Berlin, Brandenburg and Thuringia, respectively.

Figure 2.5 displays results for the placebo studies on the intensity of staff deployment. According to the figure, only the placebo studies for Brandenburg substantiate the suggestion of a positive impact of the CSR reforms on the intensity of staff deployment from the previous analysis. The estimated post-treatment gap in the intensity of staff deployment in Brandenburg is large in magnitude compared to the placebo estimates and, thus, less likely driven by chance - but statistically insignificant. For Bavaria, Berlin and Thuringia, however, the placebo studies yield estimates similar in magnitude to the ones of the treated states. Overall, this suggests that the impact of the CSR reforms on the intensity of staff deployment is insignificant.

²⁶It should be noted that confounding events in control states would offer another explanation for the placebo studies to yield estimates large in magnitude. In the context of my study, I consider regional changes in childcare fees regulation and expansions of the legal entitlement to a slot in childcare to younger children as potentially confounding events. Because these events occurred in Hamburg and Saxony-Anhalt, these states are excluded from the analysis.

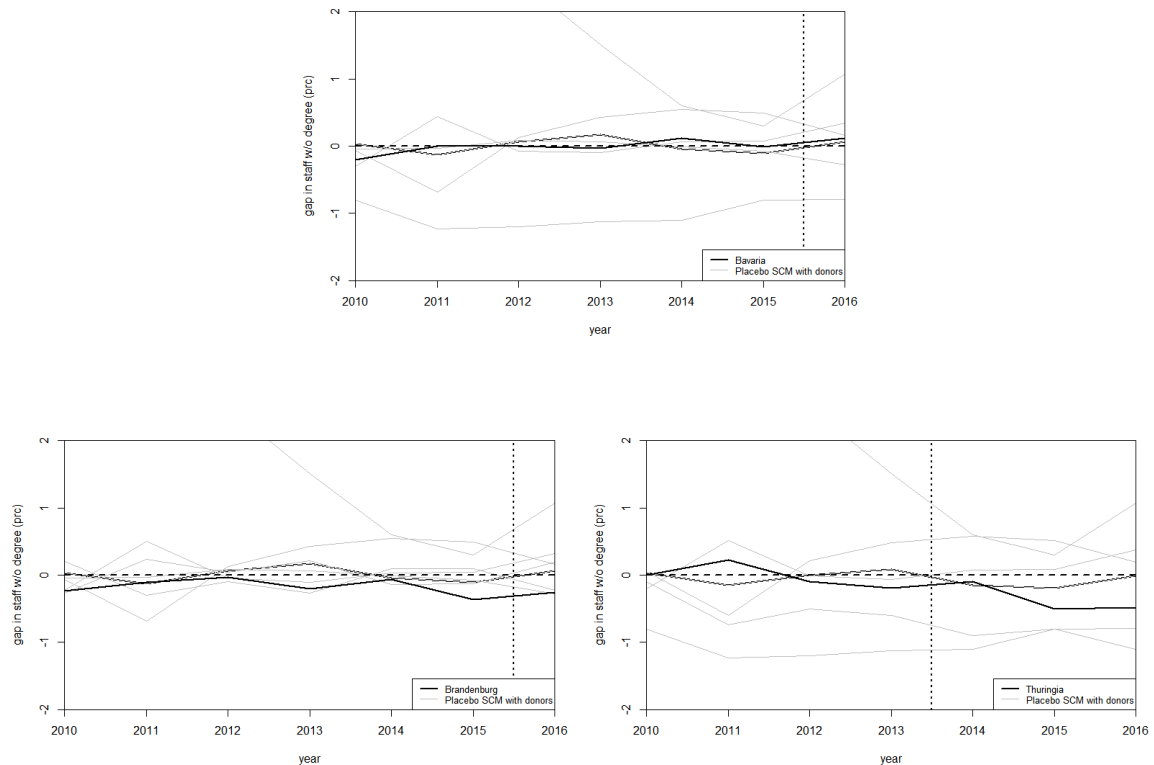
FIGURE 2.5: Placebo Estimates: Impact of CSR Reforms on Intensity of Staff Deployment



Notes: Figures show the evolution in the treated states Bavaria, Berlin, Brandenburg and Thuringia, respectively vs. their synthetic control of the intensity of staff deployment. The vertical line indicates the date of the CSR reform and separates the pre- and post-treatment periods. The light grey lines plot estimates for the placebo studies where the SCM is applied to one of the control states by assigning a placebo treatment. Source: Ländermonitor.

Results for the placebo studies on the gaps in shares of pedagogical staff in child care without a vocational degree by treated state are shown in Figure 2.6. In Bavaria, Brandenburg and Thuringia, the estimated impact of the CSR reforms on staff without a degree can be considered as insignificant as the post-treatment gaps of the placebo treatments are comparable in magnitude to the gaps obtained for the treated states. These results are in accordance with the conclusion of the previous analysis with child care providers not responding to the stricter CSR regulation by substituting input factors.

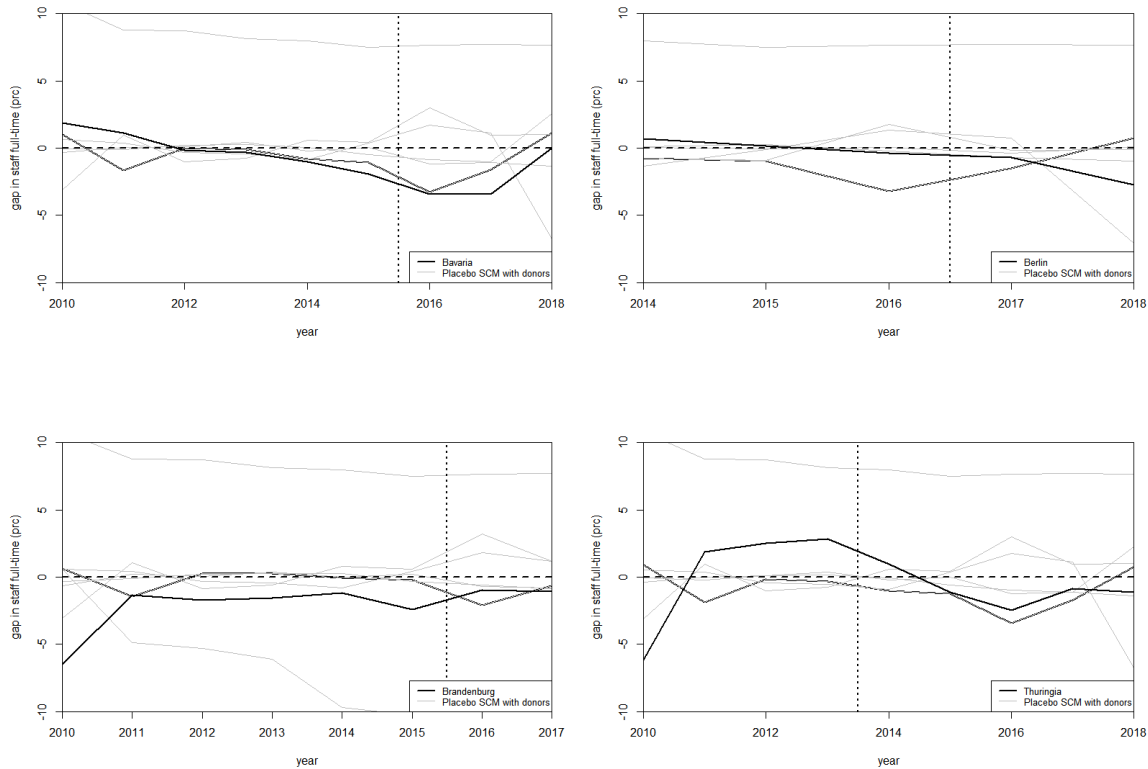
FIGURE 2.6: Placebo Estimates: Impact of CSR Reforms on Share staff without degree



Notes: Figures show the evolution in the treated states Bavaria, Berlin, Brandenburg and Thuringia, respectively vs. their synthetic control of the share of pedagogical staff in child care without a vocational degree. The vertical line indicates the date of the CSR reform and separates the pre- and post-treatment periods. The light grey lines plot estimates for the placebo studies where the SCM is applied to one of the control states by assigning a placebo treatment. Source: Ländermonitor.

Figure 2.7 features results on the placebo studies on the gaps in the share of staff in child care working full-time. With estimated placebo gaps of magnitude similar to the treated states, the declines in the share of full-time employed staff in Berlin and Thuringia, as well as the increase in Brandenburg appear to be insignificant. Only the decline in Bavaria is closer to statistical significance with the estimated gap for the treated state being larger in magnitude than the gaps of the placebo treatments. Compared to the mixed evidence from the previous analysis, the placebo studies suggest that the impact of the CSR reforms on the share of full-time employed staff is statistically insignificant.

FIGURE 2.7: Placebo Estimates: Impact of CSR Reforms on Share Staff Employed Full-Time Deployment



Notes: Figures show the evolution in the treated states Bavaria, Berlin, Brandenburg and Thuringia, respectively vs. their synthetic control of the share of full-time employed staff. The vertical line indicates the date of the CSR reform and separates the pre- and post-treatment periods. The light grey lines plot estimates for the placebo studies where the SCM is applied to one of the control states by assigning a placebo treatment. Source: Ländermonitor.

2.6.2 Imputation Estimator: Impact on Child Development

A recently growing literature addressing causal inference in conventional event-study designs such as Equation 2.5.3 demonstrates severe identification and estimation issues in case of heterogeneous treatment effects. Studying a setting with staggered adoption, Sun and Abraham (2020) show that estimated leads and lags are biased if dynamic treatment effects are heterogeneous across treated cohorts, i.e. across each group of individuals that shares the same treatment timing. My institutional setting features four different treated cohorts with two states adopting a CSR reform in 2013, three states in 2015, one state in 2016 and one state in 2017. I refrain from studying the impact of the CSR reforms in Hamburg, Saxony

and Saxony-Anhalt. Hamburg introduced an updated childcare fees regulation (see Jessen, Schmitz, and Waights, 2020), while Saxony-Anhalt expanded the legal entitlement to a slot in childcare to younger children. I expect both of these policies to be potentially confounding events. Further, Saxony introduced a CSR reform in 2017. Given that the latest wave of the SOEP provides data only up to and including 2018, the data set does not allow for an analysis of the impact of the CSR reform in Saxony on adaptive behavior in children at age 2-3. Considering that the legislated change of the CSR, which determines the intensity of treatment, varies across states within a given treated cohort, I assume that in my setting dynamic treatment effects are even heterogeneous within a given treated cohort.

To overcome these issues, I re-estimate Equation 2.5.3 using the “Imputation Estimator” proposed by Borusyak, Jaravel, and Spiess (2021), which places no restrictions on treatment effect heterogeneity.²⁷ The method estimates individual-specific treatment effects that are weighted to derive a single average treatment-on-the-treated (ATT) effect across all treated observations. Beyond that, the authors propose a test for parallel trends that draws on untreated observations only.²⁸ In this additional analysis, all regressions include state-, year- and month-of-birth fixed effects, and standard errors are clustered on the state-level.²⁹

²⁷The “Interaction-Weighted” estimator of Sun and Abraham (2020) is not suitable in my setting as it does not allow states within the same treatment cohort to follow different paths of dynamic treatment effects.

²⁸The sample of untreated observations includes individuals from control states as well as those from treated states in the pre-treatment period.

²⁹The “Imputation Estimator” uses the cross-sectional weights of the SOEP.

TABLE 2.1: Imputation Estimator: Impact of CSR Reforms on Child Development

	Overall Score in Adaptive Behavior	
	(1)	(2)
CSR adopted	-0.581 (1.445)	-1.406 (1.190)
Pre-trend (t-1)	-0.650 (1.029)	-0.030 (1.179)
Pre-trend (t-2)	-2.099** (0.980)	-1.168 (0.956)
Child care covariates	Excluded	Included
Cluster SE	State-level	
Observations	1,290	

Notes: The table displays estimates on the impact of CSR reforms on child development using the Imputation Estimator. The dependent variable is reported adaptive behavior. All specifications include state-, year- and month-of-birth fixed effects. Standard errors are clustered at the state level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

The results of the "Imputation Estimator" are reported in Table 2.1 in columns (1) and (2) for the two specifications, excluding and including child care control variables, respectively. The table features the estimated impact of the CSR reforms on children's development as well as a test for parallel trends one and two periods before treatment. When excluding child care control variables, the estimated impact on adaptive behavior is negative – but statistically insignificant. The test for parallel trends indicates a statistically significant, negative pretrend coefficient for lead two. This suggests that the assumption of parallel trends in treated and control states is partially violated as adaptive behavior was significantly lower in treated relative to control states two periods before treatment. Hence, the Event-Study design and the "Imputation Estimator" draw different conclusions regarding the validity of the parallel trends assumption. It is possible that the parallel trends assumption in the Event-Study design holds only due to the presence of heterogeneous treatment effects, which leads to biased pre-treatment estimates in that model.

Also the specification including child care covariates reveals a negative, but statistically insignificant, relationship between the CSR reforms and reported adaptive behavior. When checking the assumption of parallel trends, both pretrend coefficients are statistically insignificant. This implies that for this specification, the assumption of treated and control states following the same overall pre-treatment dynamics in adaptive behavior is not violated. The "Imputation Estimator" is my preferred specification for obtaining robust estimates of the effect of the CSR reforms on reported adaptive behavior. Based on the data available for my analysis, the impact of the CSR reforms on children's development appears to be insignificant and the assumption of parallel trends holds largely.

2.7 Conclusion

As a response to pedagogues and experts demanding an increase in the quality of ECC provision to promote children's development, Germany's regional governments are introducing stricter regulation of the CSR, which is one of several inputs to the production of child care. Even though the reforms potentially raise the quality of this stricter regulated aspect of ECC, fragmentally increased minimum standards may bring along the unintended side effect of deterioration in the quality of more leniently regulated inputs. Thus, the governments' responses may ultimately result in a decline in the overall quality of the ECC environment and may have negative effects on children's development. The empirical evidence on the impact of stricter regulation of a selected feature of ECC quality on other aspects of structural quality and children's development is scarce. With this paper, I provide new evidence from Germany by combining administrative data on structural quality in child care and survey data on the adaptive behavior of children with a novel recording of the legislated CSR. My analysis exploits quasi-experimental variation in the regulated minimum standards across German federal states over time to identify the causal impact of reforms to the CSR on the child care environment and children's development.

My findings show that the CSR reforms have no statistically significant effect on labor intensiveness. Accordingly, I cannot confirm that child care regulations in Germany are

enforced and binding. At the same time, I find no indication that the new regulations motivated or forced child care providers to engage in input substitution. The shares of lower-qualified and full-time employed staff appear to be unaffected by the reforms. This observation contradicts related studies showing that child care providers increasingly employ staff with lower qualifications (Ali, Herbst, and Makridis, 2021). With respect to child outcomes, my analysis suggests that the CSR reforms did not influence children's development. However, this finding must be treated with caution. To analyze children's development, I use a small sample that features compositional changes regarding relevant socio-economic and socio-demographic characteristics over time. Moreover, my identification strategy relies on the strict assumption of no reform-induced selection of children into or out of attending ECC. It should also be noted that the results may be due to the specific nature of the German ECC system.

In summary, in non-competitive and highly subsidized child care markets, policymakers may want to avoid stricter CSR regulations in ECC in an endeavour to raise structural quality. Further, as the impact of setting higher CSR minimum standards in ECC on child outcomes is weak, such reforms may not be the right measure to improve children's development. Future research in the context of the CSR reforms in Germany would benefit greatly from analyzing larger data sets. With access to state-level time series data on further structural aspects of ECC, such as group size or age of staff, researchers could obtain a more complete picture of the impact of the reforms on the German ECC environment. Further, studying a large, representative sample of children would permit making more meaningful statements on the consequences for children's development.

Appendix

2.A Synthetic Control Weights: Impact of CSR Reforms on Child Care Quality

TABLE 2.A.1: Synthetic Bavaria Weights: Impact of CSR Reforms on Child Care Quality

Control state	Outcome variable		
	Intensity of Staff Deployment	Staff Without Degree	Staff Employed Full-Time
Schleswig-Holstein	0	0.001	0
Bremen	0	0	0.507
North Rhine-Westphalia	0.467	0.239	0.493
Hesse	0.490	0.002	0
Rhineland-Palatinate	0	0.006	0
Mecklenburg-Vorpommern	0.042	0.752	0

Notes: The table displays the weights that the SCM assigns to each state from the control group in Figure 2.1, 2.2 and 2.3. These weights define synthetic Bavaria in the estimation of the impact of the CSR reform on child care quality. The control group is a non-exhaustive list of the 16 German federal states. States with confounding treatments or where correct determination of CSR legislation was unfeasible are excluded.

TABLE 2.A.2: Synthetic Berlin Weights: Impact of CSR Reforms on Child Care Quality

Control state	Outcome variable		
	Intensity of Staff Deployment	Staff Without Degree	Staff Employed Full-Time
Schleswig-Holstein	0.009	n/a	0.059
Bremen	0.008	n/a	0.089
North Rhine-Westphalia	0.009	n/a	0.719
Hesse	0.009	n/a	0
Rhineland-Palatinate	0.008	n/a	0
Mecklenburg-Vorpommern	0.958	n/a	0.132

Notes: The table displays the weights that the SCM assigns to each state from the control group in Figure 2.1, 2.2 and 2.3. These weights define synthetic Berlin in the estimation of the impact of the CSR reform on child care quality. The control group is a non-exhaustive list of the 16 German federal states. States with confounding treatments or where correct determination of CSR legislation was unfeasible are excluded.

TABLE 2.A.3: Synthetic Brandenburg Weights: Impact of CSR Reforms on Child Care Quality

Control state	Outcome variable		
	Intensity of Staff Deployment	Staff Without Degree	Staff Employed Full-Time
Schleswig-Holstein	0	0	0
Bremen	0	0	0
North Rhine-Westphalia	0	0	0
Hesse	0	0	0
Rhineland-Palatinate	0.045	0	0
Mecklenburg-Vorpommern	0	0.677	0
Saxony	0.955	0.323	1

Notes: The table displays the weights that the SCM assigns to each state from the control group in Figure 2.1, 2.2 and 2.3. These weights define synthetic Brandenburg in the estimation of the impact of the CSR reform on child care quality. The control group is a non-exhaustive list of the 16 German federal states. States with confounding treatments or where correct determination of CSR legislation was unfeasible are excluded.

TABLE 2.A.4: Synthetic Thuringia Weights: Impact of CSR Reforms on Child Care Quality

Control state	Outcome variable		
	Intensity of Staff Deployment	Staff Without Degree	Staff Employed Full-Time
Schleswig-Holstein	0	0	0
Bremen	0.259	0	0
North Rhine-Westphalia	0	0	0
Hesse	0	0	0
Rhineland-Palatinate	0	0	0.497
Mecklenburg-Vorpommern	0.741	1	0.503

Notes: The table displays the weights that the SCM assigns to each state from the control group in Figure 2.1, 2.2 and 2.3. These weights define synthetic Thuringia in the estimation of the impact of the CSR reform on child care quality. The control group is a non-exhaustive list of the 16 German federal states. States with confounding treatments or where correct determination of CSR legislation was unfeasible are excluded.

2.B Impact of CSR Reforms on Child Development

TABLE 2.B.1: Impact of CSR Reforms on Child Development

Dependent variable:	Reported adaptive behavior					
Specification:	(1)			(2)		
Years since CSR reform	Coeff	P value	95% CI	Coeff	P value	95% CI
Minus 8	-1.538	0.504	[-6.406, 3.284]	-6.329	0.242	[-17.491, 4.929]
Minus 7	-2.127	0.382	[-7.118, 2.942]	-6.627*	0.095	[-14.387, 1.185]
Minus 6	-2.354	0.255	[-6.260, 2.046]	-4.909*	0.056	[-9.888, 0.160]
Minus 5	0.087	0.960	[-2.762, 3.104]	-1.406	0.449	[-5.487, 2.826]
Minus 4	-1.205	0.458	[-4.735, 2.478]	-2.737	0.110	[-6.389, 0.703]
Minus 2	-0.171	0.917	[-4.370, 4.423]	0.492	0.797	[-3.768, 4.703]
Minus 1	-0.246	0.929	[-6.767, 7.155]	1.330	0.661	[-5.392, 7.421]
0 years	0.200	0.901	[-3.225, 4.041]	2.660	0.464	[-4.956, 10.729]
1 year	3.097*	0.091	[-0.520, 6.647]	6.844	0.203	[-4.051, 18.104]
2 years	-1.706	0.645	[-13.953, 9.685]	3.719	0.595	[-10.285, 17.897]
Constant	89.354			156.192		
No. obs	1,290			1,290		
R2	0.420			0.434		
Control variables	yes			yes		
Controls ECC	no			yes		
State FE	yes			yes		
Year FE	yes			yes		
Month-of-birth FE	yes			yes		
SE	bootstrap			bootstrap		

Notes: The table features Event-Study results from the estimation of Eq. 2.5.3 displayed in Figure 2.4. The dependent variable is reported adaptive behavior. Specification 1 (2) excludes (includes) covariates relating to the ECC environment. The coefficients represent the deviation in outcomes of children from treated relative to control states for cohorts born up to eight years before and two years after a CSR reform - as compared to the last pre-treatment cohort born three years before the CSR reform. The sample is comprised of children born between 2009 and 2015 from private households with completed mother-child questionnaires, aged 26 to 43 months (with birth-month specific cutoffs) at the time of the interview and attending ECC for more than 19 hours per week. Levels of statistical significance: *** denotes significance at the 1% level; ** at the 5% level; and * at the 10% level. All regressions include state-, year- and month-of-birth fixed effects. Standard errors are calculated using the wild-cluster bootstrap procedure. Source: SOEP.

Chapter 3

Child Care Quality and Enrollment Gaps in Early Child Care

3.1 Introduction

Research shows that an early childhood environment that offers learning and exploration opportunities is crucial for child development (Knudsen et al., 2006; Heckman and Mosso, 2014). With the sociodemographic background of parents largely determining the nature of a child's early environment, gaps in child development between children of less-educated or migrant parents and more advantaged peers are often observed at school entry (Jessen, Schmitz, and Waights, 2020). The effects of these gaps persist into adulthood, which is why disadvantaged children often experience less favorable long-term outcomes in the domains of health, educational attainment and earnings (Elango et al., 2016). Child care is expected to reduce these inequalities and promote social mobility by primarily enhancing child development among children from disadvantaged backgrounds (Blau and Currie, 2006; Havnes and Mogstad, 2015; Felfe and Lalive, 2018). However, children of less-educated or migrant parents are less likely to attend early child care (ECC) - care for children aged under 3, which limits its potential to equalize early childhood environments and child development across children from different sociodemographic backgrounds (Jessen, Schmitz, and Waights, 2020; Cornelissen et al., 2018).

Previous studies indirectly suggest that sociodemographic enrollment gaps in ECC are

associated with the stringency of quality regulations in child care.¹ However, the exact nature of this relationship, i.e. whether higher minimum standards extend or narrow these enrollment gaps, appears to be a priori undetermined and may be context-specific. Enrollment gaps may be aggravated in the absence of strict regulation where quality of ECC may be heterogeneous across child care centers. In such a child care environment, less-educated or migrant parents may refrain from using ECC if they have comparably less access to higher-quality ECC. For example, as a consequence of residential segregation, less-educated or migrant parents may reside in areas where child care centers tend to offer ECC of lower quality. Moreover, higher-quality centers may discriminate against these parents in the application process by preferably selecting children from advantaged sociodemographic backgrounds (Jessen, Schmitz, and Waights, 2020; Drange and Telle, 2021). Finally, in case of equal access to ECC quality, less-educated or migrant parents may in general have greater concerns about the quality of the child care environment (Jessen, Schmitz, and Waights, 2020).

At the same time, however, to the extent that stricter regulation of quality in ECC implies higher costs of providing child care, high minimum standards may be accompanied by large enrollment gaps. In this case, quality of ECC may be more homogeneous, but the child care environment may be characterized by lower supply and higher parental co-payments for child care. As far as lower supply equals a scarcity of child care places that introduces sociodemographic discrimination by child care providers, less-educated or migrant parents may face restricted access to ECC. Similarly, in such a scenario child care providers may prioritize children from working parents. If parents with lower education or migration background are less likely to be employed, they may also be disadvantaged in competition for slots in ECC. Moreover, if the reduction in supply is especially pronounced in areas with disadvantaged families, these parents may not have access to closely located ECC any longer. Finally, due to higher parental fees, these parents may be less able to afford child care for their children (Jessen, Schmitz, and Waights, 2020).²

In this paper, I investigate the effects of increased minimum quality standards for the

¹See Jessen, Schmitz, and Waights (2020) and Drange and Telle (2021). These studies do not directly examine the relationship between sociodemographic enrollment gaps and the stringency of quality regulations in ECC. Rather, I deduce a potential link from their works.

²Lower supply may also take the form of shorter opening hours. To the extent that less-educated and migrant parents attach a greater importance to long opening hours, these parents may be discouraged from using ECC.

provision of child care on sociodemographic enrollment gaps in ECC. My analysis relies on child care reforms that generated variation in the regulated maximum child-to-staff ratio (CSR) in ECC across regions and over time. More precisely, I compile an exhaustive data set of changes to maximum CSR from legal documents. This data set is then combined with information on child care enrollment and demand from representative survey data of children under the age of three in Germany.

My paper relates to the literature on the determinants of early sociodemographic segregation in ECC participation and social mobility. Previous research suggests that both supply- and demand-side constraints in the child care market may play an important role in explaining sociodemographic enrollment gaps in ECC. On the supply-side, inequalities in access to ECC may be the result of disadvantaged parents being more strongly affected by limited availability, quality and the financial burden of parental fees in child care (Bainbridge et al., 2005; Sibley et al., 2015; Drange and Telle, 2021). On the demand-side, parental preferences for ECC may differ by sociodemographic background. As far as less-educated or migrant parents may follow more traditional social norms on child rearing and caretaker's employment, these parents may have less need for formal child care (Pavolini and Van Lancker, 2018). Inequality in demand for ECC may also be the consequence of sociodemographic disparities in employment opportunities (Del Boca and Vuri, 2007). Finally, due to the importance of social networks and host country language skills for overcoming information constraints, disadvantaged parents may have less knowledge about the often complex child care programs and application processes (Eremenko and Unterreiner, n.d.; Hermes et al., 2021).

Using quasi-experimental variation in the scarcity of child care slots and parental fees in ECC, Jessen, Schmitz, and Waights (2020) provide recent evidence on the relevance of supply-side related factors in explaining sociodemographic enrollment gaps in Germany. The results show that a reduction in these potential access barriers narrows gaps by parental education. However, the enrollment gap by migration background appears to be unresponsive to these changes. Along with potential discrimination, Jessen, Schmitz, and Waights (2020) highlight the possibility that insufficient child care quality deters parents with migration background from enrolling their children in ECC. Similar to Jessen, Schmitz, and

Waight (2020), I draw on within-region and over-time variation to estimate the causal impact of a potential determinant of sociodemographic enrollment gaps in Germany. Moreover, my paper relies on the same survey data to study child care enrollment and demand of children under the age of three. In contrast to their study, I draw on a reform that introduces stricter quality regulation in ECC. Hence, I am able to shed light on a further supply-side related factor and examine whether a potential increase in child care quality has an effect on sociodemographic enrollment gaps.

Except for Jessen, Schmitz, and Waight (2020) and Hermes et al. (2021), most previous research provided correlational evidence of determinants of sociodemographic enrollment gaps in ECC (see e.g. Bainbridge et al., 2005; Sibley et al., 2015; Pavolini and Van Lancker, 2018). Hence, studies on causal relationships are scarce. Moreover, Jessen, Schmitz, and Waight (2020) demonstrate that a reduction in the scarcity of child care slots and in parental fees has no impact on the enrollment gap by migration background. Therefore, my paper contributes to the literature by analyzing the causal impact of increased minimum standards in ECC and thereby improves the understanding of the causal determinants of sociodemographic enrollment gaps in child care. Exploring whether stricter quality regulations reduce sociodemographic inequalities in ECC participation is of importance because such policies may have the potential to effectively promote social mobility.

In summary, I examine the hypothesis that sociodemographic enrollment gaps in ECC are responsive to stricter quality regulations in child care. Additionally, to the extent that increased minimum quality standards only have a positive effect on child care quality, I explore the idea that such policies specifically encourage parents with migration background to enroll their children in ECC.

I find that the effects of the child-to-staff ratio (CSR) reform on ECC enrollment gaps by parental education and by parental migration background are statistically insignificant. However, my analysis suggests a simultaneous change in parental demand for ECC and CSR regulation. Though, it still needs to be assessed whether this effect is statistically significant. Since potential consequences of the CSR reform relate to supply-side constraints, I assume that ECC demand remains largely unaffected by increased minimum standards in ECC quality. A response of ECC demand may imply that existence of confounding events

that impact parental preferences or employment opportunities cannot be ruled out. Overall, my findings suggest that stricter CSR regulation may not promote social mobility.

The structure of the paper is as follows. Section 2 introduces the institutional background. Section 3 presents the data. Section 4 features the empirical strategy and the main results, and Section 5 concludes.

3.2 Institutional Background

3.2.1 Early Child Care in Germany

The German child care system offers formal center-based child care for children under the age of three in early child care centers (*Krippen*).³ In Germany, like in many other countries, parental demand for ECC increased in recent years. In order to meet care needs of these parents, the German legislator adopted two federal laws, the TAG in 2005 and the KiföG in 2008. This resulted in more financial resources being allocated to child care for children under the age of three and regional governments being required to scale up their supply of ECC. In 2013, the expansion efforts were underlined by the introduction of a federal legal claim to care for children aged 1 and older. Despite a great expansion of ECC, the child care market in Germany, especially in former West Germany, features regional shortages where parental demand exceeds the number of supplied slots in ECC. In 2014, the share of children under the age of three enrolled in formal child care in Germany was about 32%. A closer look reveals regional differences. While former West Germany featured an enrollment rate of 27%, enrollment was 52% in former East Germany. Although similar to the OECD average, the ECC enrollment rate in Germany falls short of the levels observed in Scandinavian countries where every second child attends early care (OECD, 2017).

As in most European countries, ECC in Germany is provided by public and private entities. In Germany, ECC centers are predominantly owned and operated by non-profit

³In Germany, *Krippen* may operate as standalone facilities or may be integrated into kindergartens, where the kindergarten offers child care for children aged 3-6 alongside the provision of the *Krippe* (European Commission, 2014). An important goal of *Krippen* is to promote the development of the child into an independent and socially competent personality (Camehl, 2016). *Krippen* aim to achieve this goal by assuming educational responsibilities and following particular pedagogical concepts designed to foster children's analytical, language, and motor capabilities. The pedagogical concepts emphasize learning through play where children participate in circle games, reading, painting or physical activities (Felfe and Lalive, 2018).

organizations including public authorities, such as municipalities, and private providers, such as churches and welfare organizations. Commercial providers and employers that supply private, for-profit care play a minor role. For example, in 2014, 88% of ECC centers were non-profit, while for-profit centers accounted for the remaining 12% (Muehler, 2008; Felfe and Zierow, 2018).

The provision of ECC in Germany, like in many European countries, relies heavily on public funding. Thus, the low market share of for-profit ECC in Germany is partially explained by the fact that non-profit providers have been enjoying preferential treatment in terms of receiving subsidies. While non-profit ECC has always been comprehensively subsidized, for-profit centers were not eligible for public funding until 2005, and subsequently received subsidies only in some federal states of Germany (Muehler, 2008; Berger, Spieß, and Groh-Samberg, 2008). Regarding the existence of a non-subsidized private sector, the German ECC market bears resemblance with France, Italy and Switzerland, but differs from the ECC provision observed in the Nordic countries (European Commission, 2014).⁴ In 2006, public funding amounted to 79% of the total operating costs for center-based ECC, while 14% of the costs were covered by parents and 7% by other private organizations (Felfe and Lalive, 2018).

Parental co-payments for publicly subsidized ECC in Germany are subject to a national guideline that stipulates sliding-scale fees. The German federal states and municipalities are responsible for regulation of parental fees and implementation of this guideline whereby local authorities are free to decide on how parental fees are exactly determined. As a result, Germany, similar to Switzerland, Italy and some Nordic countries, features local caps on the amount of parental fees in child care (European Commission, 2014; Gørtz, Johansen, and Simonsen, 2018; Felfe and Lalive, 2018; Drange and Havnes, 2019). Due to putting a cap on prices at cost, child care provision in these countries differs from the US where the private sector sets higher parental fees to maximize profits (Berlinski et al., 2021). With heavy public subsidization and the minor private market, parental co-payments in Germany are assumed to be largely independent of demand and supply. However, private, for-profit ECC

⁴In some European countries, including Cyprus, Malta and the United Kingdom, children are predominantly enrolled in private, non-subsidized ECC settings. This implies that parental contributions to ECC are higher in these countries.

providers that are more often ineligible for public funding appear to charge higher parental fees.⁵ The criteria that local authorities rely on to determine parental fees differ across regions. German ECC centers may charge fees that depend on parental income, the number of own children enrolled in child care, and the hours of care. Similar to other European countries, Germany eases the financial burden on low-income families by offering reduced child care fees and fee exemptions (European Commission, 2014; Jessen, Schmitz, and Waights, 2020). With net costs of child care for a couple amounting to 11% of average wages, parental fees in Germany were comparable to Denmark but lower than the EU average of 15% and much lower than US with 35% (OECD, 2015). Between 2013 and 2015, private household expenditure for a child in ECC increased from 150 to 171 euros on average, which represents an increase of 14% (Schmitz, Spieß, and Stahl, 2017; Jessen, Schmitz, and Waights, 2020).⁶

Regarding the allocation of ECC places in Germany, parents are free to apply for care at any institution of their choice. Despite children aged 1 and older having a legal claim to a place in child care, admission is often based on selection criteria that vary across states, municipalities, and providers of child care. Similar local autonomy in defining rules for admission to child care is observed in many European countries (European Commission, 2014). In Germany, selection criteria in public institutions are sometimes designed to give priority to younger children, children with siblings already being enrolled in the institution, and children of parents with higher working hours. Admission of children to ECC centers operated by churches can be additionally based on the religious affiliation of families. Importantly, ECC providers may keep selection criteria private, which results in the allocation of ECC places being non-transparent (Schmitz, Spieß, and Stahl, 2017; Herzog and Klein, 2018).

⁵Child care institutions differ in amount of subsidies they receive from state and municipalities. As a consequence, they charge different fees.

⁶This increase in private household expenditure for child care can be partially explained by changes in the characteristics of parents with children in ECC and in the composition of their families. Parental fees are typically based on parental income, maternal employment, the number of own children enrolled in child care and hours of care. Among parents with children in ECC, income is higher, mothers are more likely to be employed and children are less likely to have siblings enrolled in child care in 2015 relative to 2013 (Schmitz, Spieß, and Stahl, 2017).

3.2.2 Child Care Quality in Germany

The quality of child care is evaluated based on whether a child care environment is supportive of children's wellbeing and human capital formation. The two main dimensions of child care quality are process and structural quality.⁷ Process quality refers to the immediate adult-child interactions and has a direct relation with child development. Structural quality is determined by inputs to the production of child care, i.e. the physical attributes of the care environment (Cascio, 2014; Ali, Herbst, and Makridis, 2021). Higher structural inputs increase the likelihood of higher process quality taking place.

In order to make sure that child care is delivered with a minimally acceptable quality and fosters child wellbeing and development, legislators focus on regulating structural quality where determinants are easily observed and measured (Blau, 1999). As in the US and Norway, quality regulations in Germany define minimum requirements for structural inputs such as the child-to-staff ratio (CSR), group size, teacher education and qualification, building requirements and pedagogical principles (Camehl, Schober, and Spiess, 2018).⁸ Interestingly, some countries, including Sweden and Denmark, leave the CSR and group size unregulated (European Commission, 2014). Since local authorities are responsible for setting quality regulations in Germany, minimum standards vary across states. Similarly, child care quality in the US is regulated by states, and by municipalities in Norway (European Commission, 2014; Drange and Rønning, 2017; Ali, Herbst, and Makridis, 2021). Further, regulations commonly vary by children's age. Accordingly, ECC in Germany, the US and Norway, is subject to stricter regulation of CSR and group sizes than care in kindergartens (European Commission, 2014; Ali, Herbst, and Makridis, 2021). In Germany, local youth welfare services at the county level control whether child care providers comply with the quality regulations (Felfe and Zierow, 2018). In contrast to the US, where inspections organized on the state level report that institutions violate minimum requirements, a high degree of compliance with quality regulations is observed among child care providers in

⁷A third component of child care quality is defined as orientation quality. Orientation quality refers to the normative orientation that guides pedagogic action by setting out concepts, convictions and values (Sell, 2010).

⁸Similar to the Nordic and other central European countries, the German child care system is oriented towards the social pedagogy tradition where focus lies on "socialization and learning through play" (Gørtz, Johansen, and Simonsen, 2018). By contrast, many child care in many English- and French-speaking countries follows the pre-primary pedagogic approach where formal learning processes prepare children for school (Drange and Rønning, 2020).

Germany (Felfe and Lalive, 2018).⁹

The average quality of child care in Germany is assessed as not particularly high (Tietze et al., 2012). There has been severe criticism that state-level regulations for structural quality are too lenient and mostly fail to comply with expert recommendations, such as the guidelines of the National Association for the Education of Young Children (NAEYC) Early Childhood Program Standards and Accreditation Criteria (Bertelsmann Stiftung, 2014; Becker and Schober, 2017; Camehl et al., 2015). In contrast to the US or the United Kingdom, child care provision in Germany is not subject to a qualitative rating or an improvement system (Camehl, Schober, and Spiess, 2018; Blanden et al., 2021). The German child care environment is also characterized by a high degree of variation in quality across states, counties and child care institutions (Tietze et al., 2012; Camehl et al., 2015; Schober and Spiess, 2015). This heterogeneity may be accompanied by unequal access to child care quality with less-educated or migrant parents being relegated to care of lower minimum standards. As in Norway, children of disadvantaged sociodemographic backgrounds are more likely to be enrolled in lower quality child care in Germany (Drange and Telle, 2021; Lehl, Kuger, and Anders, 2014; Stahl, Schober, and Spiess, 2018).¹⁰ This may be explained by residential segregation as far as less-educated or migrant parents may find comparably less child care provision of higher quality close to where they live.¹¹ However, discrimination according to which higher-quality centers prefer to take in advantaged children may also be present (Drange and Telle, 2021; Lehl, Kuger, and Anders, 2014).

3.2.3 Reforms of the Child-to-Staff Ratio

As a response to experts calling for improvements in structural quality of ECC, seven German states introduced child care reforms that increased minimum standards for the child-to-staff ratio (CSR) in ECC between 2010 and 2017 (see Sachsen-Anhalt Landtag, 2017; Bundesministerium für Familie, Senioren, Frauen und Jugend, 2019; Evangelischer KITA-Verband

⁹Camehl, Schober, and Spiess (2018) note that enforcement issues and violations relating to quality regulations may also exist in the German child care market.

¹⁰This observation is interesting considering that child care providers receive additional funding in sociodemographically disadvantaged areas (Stahl, Schober, and Spiess, 2018).

¹¹Stahl, Schober, and Spiess (2018) note that disadvantaged parents have a stronger preference for proximity when selecting a child care institution. This may enhance the effect of residential segregation on inequality in access to child care quality.

Bayern, 2015).¹² Table 3.1 provides an overview of all changes in CSR regulation. CSR reforms were introduced in Bavaria, Hamburg, Saxony-Anhalt, Thuringia, Brandenburg, Berlin and Saxony. This shows that CSR regulation was predominantly amended in former East-German states. Compared to former West-German states, these states feature lower process quality and historically lower minimum CSR requirements (Tietze et al., 2012). The stricter regulation of the CSR likely increased the costs for a place in ECC. For Saxony-Anhalt and Thuringia, there is evidence that this cost increase was passed on the parents where parental fees increased as well (Kreikenbom and Lesser, 2013; Sachsen-Anhalt Landtag, 2017; Waldner, 2018).

Because data on ECC enrollment and its determinants is only available for the period 2012 to 2017, I restrict my analysis to reforms adopted between 2013 and 2015. Accordingly, I do not study the impact of changes to CSR legislation in Berlin and Saxony. In this way, I ensure that the data set covers at least two periods after a given CSR reform. I consider this as important because effects of the CSR reforms may occur with a delay or may vary over time. Moreover, I refrain from studying Hamburg where childcare fees regulation changed in 2014. This policy represents a confounding event that has an independent effect on ECC enrollment (see Jessen, Schmitz, and Waights, 2020). Accordingly, the policy obstructs the correct evaluation of the impact of the CSR reform. Ultimately, this results in my analysis focusing on Bavaria, Saxony-Anhalt, Thuringia and Brandenburg.

¹²For Saxony, official documents suggest that increasing the attractiveness of work in child care was an additional motive for changing the CSR.

TABLE 3.1: Child-Staff-Ratio Age 0-2 Reforms in Germany

State of Reform	Region	^a Year of CSR Reform	^b Legislated Child-Staff-Ratio	
			Pre-reform	^c Post-reform
Saxony-Anhalt	East	2013	1:6.8	1:6.7
Thuringia	East	2013	1:8.0	1:6.0
Bavaria	West	2015	1:5.8	1:5.5
Brandenburg	East	2015	1:7.5	1:6.9
Hamburg	West	2015	1:6.8	1:6.4
Berlin	West/East	2016	1:7.0	1:6.5
Saxony	East	2017	1:6.0	1:5.5

Notes: The table shows German states, which adopted Child-Staff-Ratio (CSR) reforms for children aged 0-2 between 2010 and 2017. Reading example legislated CSR: The pre-reform legislation in Saxony-Anhalt stipulated a ratio of 1 staff member for every 6.8 children. Source: Own calculations. ^a The year of the CSR reform refers to the first occurrence of a reform, which may be followed by successive CSR reforms of a given reform state. The year of the CSR reform refers to the date from which on the reformed CSR was enforced after a potential transition period. The reforms in Bavaria and Thuringia included a transition period (3-4 years). All states adopted the CSR reform in August/September (beginning of the school year) of the reform year with the exception of Hamburg which adopted the CSR reform in April. ^b The CSR are calculated as age-weighted averages for reform states with age-specific legislated CSR. ^c For a given reform state, the post-reform CSR refers to the CSR, which is adopted in the first occurrence of a reform. The legislated CSR may be altered by successive CSR reforms.

3.3 Data

To study ECC enrollment and its determinants, I rely on household-level survey data from the German Child Care Study (KiBS) of the German Youth Institute (DJI). This data source provides detailed information on children's child care situations from the parents' perspective. My evaluation of the impact of the CSR reforms on ECC enrollment is based on the KiBS and supplementary administrative data on the state-level from the Ländermonitor ¹³,

¹³The "Ländermonitor" is a project carried out by the Bertelsmann Stiftung. Within this project the Bertelsmann Stiftung annually publishes data on the German child care environment obtained from administrative statistics of the Statistics on Child and Youth Welfare, additional public statistics and from surveying the federal

the Federal Statistical Office, the Federal Institute for Research on Building, Urban Affairs and Spatial Development, and the *Betreuungsatlas*.

3.3.1 The German Child Care Study (KiBS)

The KiBS is an annual survey of households with children conducted by the DJI since 2012. Due to the large scale of the survey, researchers are provided with panel data that is representative on the state level. It focuses on child-specific care arrangements from the parent's perspective. This includes asking parents about their use of child care, needs for care, the perceived quality of child care and reasons for refraining from using child care. Moreover, the KiBS collects detailed information on the sociodemographic background of parents. In my empirical analysis, I focus on children under age three and use information from the KiBS waves covering 2012 to 2017. My estimation sample comprises 74,569 children under age three. Table 3.1 provides summary statistics of the full sample.¹⁴

TABLE 3.1: Summary statistics

Variable	Observations	Mean	SD	Min	Max
ECC enrollment	74,569	0.314	0.464	0	1
ECC demand	74,569	0.441	0.496	0	1
<i>Family background</i>					
More-educated (university entrance qualification)	74,569	0.534	0.499	0	1
No migration background (German is main language at home)	74,569	0.838	0.368	0	1
Age of child (in months)	74,569	18.227	9.335	0	35
Child is female	74,569	0.487	0.5	0	1
Respondent is married	74,569	0.783	0.412	0	1

Notes: The table displays summary statistics with data pooled over all waves. All observations and values are weighted. Source: KiBS Waves 2012-2017

ECC Enrollment

The KiBS survey asks parents to report who typically cares for their child: the parents, the grandparents, a nanny/an au pair, a formal child care institution, or a professional childminder. Drawing on this information, I define ECC enrollment as being cared for in a formal

state ministries in the area of child care. The Bertelsmann Stiftung is an independent and nonpartisan foundation. The foundation implements projects, conducts scientific studies and hosts events to contribute to the public debate in Germany.

¹⁴Table 3.B.1 and 3.B.2 in the Appendix show summary statistics for West- and East-Germany separately.

setting such as a child care institution or by a professional childminder. Table 3.2 features ECC enrollment by parental education and migration background. The descriptive findings suggest that significant sociodemographic enrollment gaps exist in the German child care context. In Germany, 38% of children aged 0-2 from higher educated parents attend formal child care, while only 24% of those from less-educated parents are enrolled in ECC. Similarly, enrollment of children aged 0-2 from parents with no migration background is 38% compared to 22% of children aged 0-2 from parents with a migration background.

TABLE 3.2: ECC Enrollment and Demand by Parental Sociodemographic Background

	Obs	Mean	Parental education			Parental migration background		
			More	Less	Diff	No	Yes	Diff
<i>Germany</i>								
ECC Enrollment	74569	0.31	0.38	0.24	-0.14***	0.33	0.22	-0.12***
ECC Demand	74569	0.44	0.48	0.40	-0.08***	0.44	0.44	0.00
<i>West-Germany</i>								
ECC Enrollment	46915	0.27	0.34	0.19	-0.15***	0.28	0.20	-0.08***
ECC Demand	46915	0.40	0.45	0.36	-0.09***	0.40	0.43	0.03***
<i>East-Germany</i>								
ECC Enrollment	27654	0.51	0.52	0.50	-0.02**	0.52	0.38	-0.15***
ECC Demand	27654	0.59	0.58	0.60	0.02**	0.59	0.55	-0.04**

Notes: The table displays differences in ECC enrollment and demand by parental sociodemographic background. Parental education is classified into the following categories: More-educated (Respondent parent has university entrance qualification), less-educated (Respondent parent has no university entrance qualification). Parental migration background is classified into the following categories: No (German is main language at home), yes (German is not main language at home). Differences ("Diff") are reported with statistical significance (***) $p < 0.01$, ** $p < 0.05$, * $p < 0.1$). All values are weighted means. Source: KiBS Waves 2012-2017

When investigating ECC enrollment separately in former West-German and East-German states, marked differences become apparent. For West-Germany, pronounced enrollment gaps by parental education and migration background are observed. East-Germany features an enrollment gap by migration background that is comparable to West-Germany. However, the enrollment gap by education is of small magnitude - albeit still statistically significant. Thus, enrollment gaps in former West-Germany occur along both sociodemographic dimensions, while in former East-Germany parental migration background is the

key factor correlating with ECC attendance.

ECC Demand

The data set also contains information about parents' need for child care irrespective of whether they currently rely on child care or not. Following Jessen, Schmitz, and Waights (2020), I draw on these reports to obtain a measure of child care demand. Focusing on parents whose children are not enrolled in child care and analyzing differences in ECC demand by parents' reported reasons for refraining from using child care, Jessen, Schmitz, and Waights (2020) suggest that this measure is sensitive to demand-side constraints in the child care market and largely indicative of parental preferences.

In Table 3.3, I replicate and expand on this descriptive analysis.¹⁵ I assume that reasons, such as employing more pedagogical staff in a group or reducing group sizes, correlate with the intended consequences of the reforms. Further, the table includes reasons that correlate with potential unintended outcomes of the CSR reforms, such as higher costs or lower ECC supply and availability. For completeness, I add two further important reasons, which remain uncategorized. My evaluation is in line with Jessen, Schmitz, and Waights (2020) and confirms the idea that parents with a weaker preference for child care are less likely to report a need for care. For example, only 12% of parents who state that they refrain from using child care because their child is too young, i.e. parents who give an affirmative answer, have a demand for child care, compared with 53% of parents who answer this question in the negative. Similarly, 14% of parents who report not using child care because they are home themselves express a need for child care, while 22% of parents with a negative answer to this question do.

¹⁵In comparison to Jessen, Schmitz, and Waights (2020), I additionally consider a survey question where parents report if they would use ECC if more pedagogical staff were in a group. Further, I append the latest wave of the KiBS data from 2017. In Tables 3.C.1 and 3.C.2 of the Appendix, I also report ECC demand by reason for not using child care for West- and East-Germany separately.

TABLE 3.3: ECC Demand by Reason For Not Using Child Care

	Affirmative answer		Negative answer		Diff
	Obs	Demand	Obs	Demand	
<i>Reasons related to intended effects CSR reforms</i>					
Would use ECC if more pedagogical staff in group	2879	0.40	9783	0.10	0.30***
Would use ECC if groups were smaller	4803	0.39	18286	0.13	0.26***
Reason for not using ECC: Pedagogical quality insufficient	1090	0.20	9432	0.19	0.01
<i>Reasons related to unintended effects CSR reforms</i>					
Reason for not using ECC: Costs	6770	0.30	30360	0.15	0.15***
Reason for not using ECC: Did not get a slot	3834	0.69	22555	0.11	0.58***
Reason for not using ECC: Opening hours not suitable	2571	0.38	34416	0.16	0.22***
Reason for not using ECC: I am home myself	15288	0.14	11166	0.22	-0.08***
<i>Other reasons</i>					
Would use ECC if an institution was located nearby	3952	0.55	19051	0.12	0.43***
Reason for not using ECC: Child is too young	31287	0.12	6384	0.53	-0.41***

Notes: The table is a replication and extension of Jessen, Schmitz, and Waights (2020). The table displays ECC demand by response (affirmative or negative) to questions relating to reasons for not using child care. Affirmative responses to multiple reasons are possible for a given survey respondent. Reading example: ECC demand equals 40% among parents who state that they would use ECC if more pedagogical staff were in a group, while ECC demand is 10% among parents that give a negative answer to this question. Differences ("Diff") are reported with statistical significance (*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$). All observations and values are weighted. Source: KiBS Waves 2012-2017

At the same time, supply-side constraints, such as limited availability or parental fees, do not prevent parents from reporting a need for child care. Accordingly, 69% of parents who report that they are not using ECC because they did not get a slot express a need for child care. This compares to a share of 11% with ECC demand among parents who answer this question in the negative. The same pattern is observed when asking if costs are the reason for not using ECC where 30% (15%) of parents who give an affirmative (negative) answer to this question, report a demand for child care. Regarding limited ECC quality and reasons for not using child care that relate to the intended effects of the CSR reforms, Jessen, Schmitz, and Waights (2020) argue that concerns about group sizes represent supply-side constraints to some degree. This implies that having this quality concern does not severely depress reporting a demand for ECC. The same conclusion can be drawn from parental reports on whether they would use ECC if more pedagogical staff were in a group. My analysis shows that 40% (10%) of parents who give an affirmative (negative) answer to this question, have a

demand for ECC. Hence, because reasons for not using child care that relate to the intended and unintended effects of the CSR reform represent supply-side constraints, I expect that the CSR reforms may not induce a strong response in ECC demand.

To assess the importance of ECC demand as a potential determinant of sociodemographic enrollment gaps in German ECC, I study whether differences in ECC demand across parental sociodemographic backgrounds exist. Accordingly, Table 3.2 also presents ECC demand by parental education and migration background. Like Jessen, Schmitz, and Waights (2020), I find that higher-educated parents report a statistically significantly higher ECC demand than less-educated parents in Germany (48% versus 40%). At the same time, my descriptive results show no difference in ECC demand by migration background (44% versus 44%).

For West-Germany, the gap in ECC demand by parental education reflects the result for the whole of Germany (45% versus 36%). However, ECC demand of parents with no migration background is statistically significantly lower than demand of parents with a migration background (40% versus 43%). Interestingly, for East-Germany, gaps in ECC demand appear to work in the opposite direction. In East-Germany, higher-educated parents have a statistically significantly lower ECC demand than less-educated parents (58% versus 60%), while parents with no migration background report a statistically significantly higher demand in comparison to migrant parents (59% versus 55%). Moreover, when comparing ECC enrollment and demand across the table, it becomes apparent that gaps in enrollment are quantitatively larger than gaps in demand.

Overall, these descriptive findings suggest that differences in ECC demand by parental education may be one important determinant of ECC enrollment gaps in West-Germany. However, the ECC enrollment gap by parental migration background in West-Germany cannot be well explained by gaps in ECC demand. For East-Germany, the same conclusion applies to the ECC enrollment gap by parental education. The ECC enrollment gap by migration background observed in East-Germany, though, may partially be a consequence of differences in ECC demand. The results support the hypothesis that supply-side constraints including limited ECC quality, i.e. determinants other than ECC demand, may play an important role for ECC enrollment gaps.

Reasons For Not Using ECC

Table 3.4 features parents' reported reasons for refraining from using child care by education and migration background. This allows an evaluation of whether responses of those parents to potential consequences of the CSR reform are likely to differ by sociodemographic background. Regarding reasons that relate to intended effects of increased minimum standards in ECC, disadvantaged parents are statistically significantly more likely to report that they would use ECC if child care quality was higher. For example, 17% (17%) of more-educated (German) parents report that they would use ECC if more pedagogical staff were in a group, compared with 22% (33%) of less-educated (migrant) parents. It also becomes apparent that differences are quantitatively larger by migration background. With respect to reasons that relate to unintended consequences of the CSR reforms, disadvantaged parents are also statistically significantly more likely to report that they refrain from using ECC for reasons that correlate with potential unintended consequences of the CSR reform. While 13% (17%) of more-educated (German) parents report that they refrain from using ECC because of costs, parental fees are mentioned among 23% (24%) of less-educated (migrant) parents.

TABLE 3.4: Reasons For Not Using Child Care by Sociodemographic Background

	Obs	Mean	Parental education			Parental migration background		
			More	Less	Diff	No	Yes	Diff
<i>Reasons related to intended effects CSR reforms</i>								
Would use ECC if more pedagogical staff in group	12662	0.20	0.17	0.22	0.05***	0.17	0.33	0.16***
Would use ECC if groups were smaller	23089	0.18	0.16	0.19	0.03***	0.15	0.29	0.14***
Reason for not using ECC: Pedagogical quality insuff.	10522	0.11	0.11	0.11	-0.00	0.10	0.14	0.04**
<i>Reasons related to intended effects CSR reforms</i>								
Reason for not using ECC: Costs	37130	0.18	0.13	0.23	0.10***	0.17	0.24	0.07***
Reason for not using ECC: Did not get a slot	26389	0.11	0.10	0.12	0.01**	0.10	0.15	0.05***
Reason for not using ECC: Opening hours not suitable	36987	0.07	0.06	0.08	0.02***	0.07	0.08	0.02**
Reason for not using ECC: I am home myself	26454	0.61	0.53	0.68	0.15***	0.60	0.63	0.02*
<i>Other reasons</i>								
Would use ECC if an institution was located nearby	23003	0.13	0.10	0.16	0.06***	0.10	0.28	0.18***
Reason for not using ECC: Child is too young	37671	0.85	0.87	0.84	-0.02***	0.86	0.83	-0.03***

Notes: The table is a replication and extension of Jessen, Schmitz, and Waights (2020). The table displays the share of affirmative responses to questions relating to reasons for not using child care by parental sociodemographic background. Classification of parental education: More-educated (Respondent parent has university entrance qualification), less-educated (Respondent parent has no university entrance qualification). Classification of parental migration background: No (German is main language at home), yes (German is not main language at home). Affirmative responses to multiple reasons are possible for a given survey respondent. Reading example: 17% of more-educated parents state that they would use ECC if more pedagogical staff were in a group, while 22% of less-educated parents give an affirmative answer to this question. Differences ("Diff") are reported with statistical significance (***) $p < 0.01$, ** $p < 0.05$, * $p < 0.1$). All observations and values are weighted. Source: KiBS Waves 2012-2017

Jessen, Schmitz, and Waights (2020) argue that the sociodemographic differences observed for reasons that relate to supply-side constraints in the child care market are likely to reflect the fact that disadvantaged parents are more strongly affected by limited availability, quality and the cost of child care. In summary, it appears that parents of children who are currently not enrolled in ECC may respond more strongly to both, intended and unintended consequences of the CSR reforms. In case that an increase in minimum standards only results in higher child care quality, I expect that sociodemographic enrollment gaps - especially by migration background - may narrow.

3.4 Estimation Strategy

Firstly, I study the impact of the CSR reform in Bavaria on sociodemographic enrollment gaps in ECC. This paper only features results for Bavaria. Unfortunately, the other case

studies, Saxony-Anhalt, Thuringia and Brandenburg, do not allow for a credible estimation of the causal effects of stricter CSR regulation. The control states of these other case studies lack similarity to the treated states in terms of pre-reform enrollment gaps and the factors that influence them. Therefore, the units of the control group do not provide a good measure of counterfactual enrollment gaps in the absence of the CSR reforms.¹⁶ Subsequently, as a robustness check, I also investigate whether the child care reform is associated with changes in demand for ECC by parental education and migration background.

To estimate the effects of the CSR reform on sociodemographic enrollment and demand gaps in ECC, I use the Synthetic Control Method (SCM) by Abadie and Gardeazabal (2003). To be precise, I compare the evolution of the outcome variables between Bavaria, the treated unit, and a synthetic counterfactual constructed from a weighted combination of untreated units. Similar to Jessen, Schmitz, and Waights (2020), I use selected pre-treatment outcomes and the covariates unemployment, inhabitants per km², GDP per capita and the share of migrants as predictors of the outcome variables. Based on these predictors, the SCM constructs a synthetic counterfactual that closely resembles the treated unit Bavaria. The SCM relies on a generalized difference-in-differences model where the potential outcomes of untreated units are generated according to a linear factor model as follows (from Abadie et al., 2010):

$$Y_{it}^N = \delta_t + \theta_t \mathbf{Z}_i + \lambda_t \boldsymbol{\mu}_i + \epsilon_{it} \quad (3.1)$$

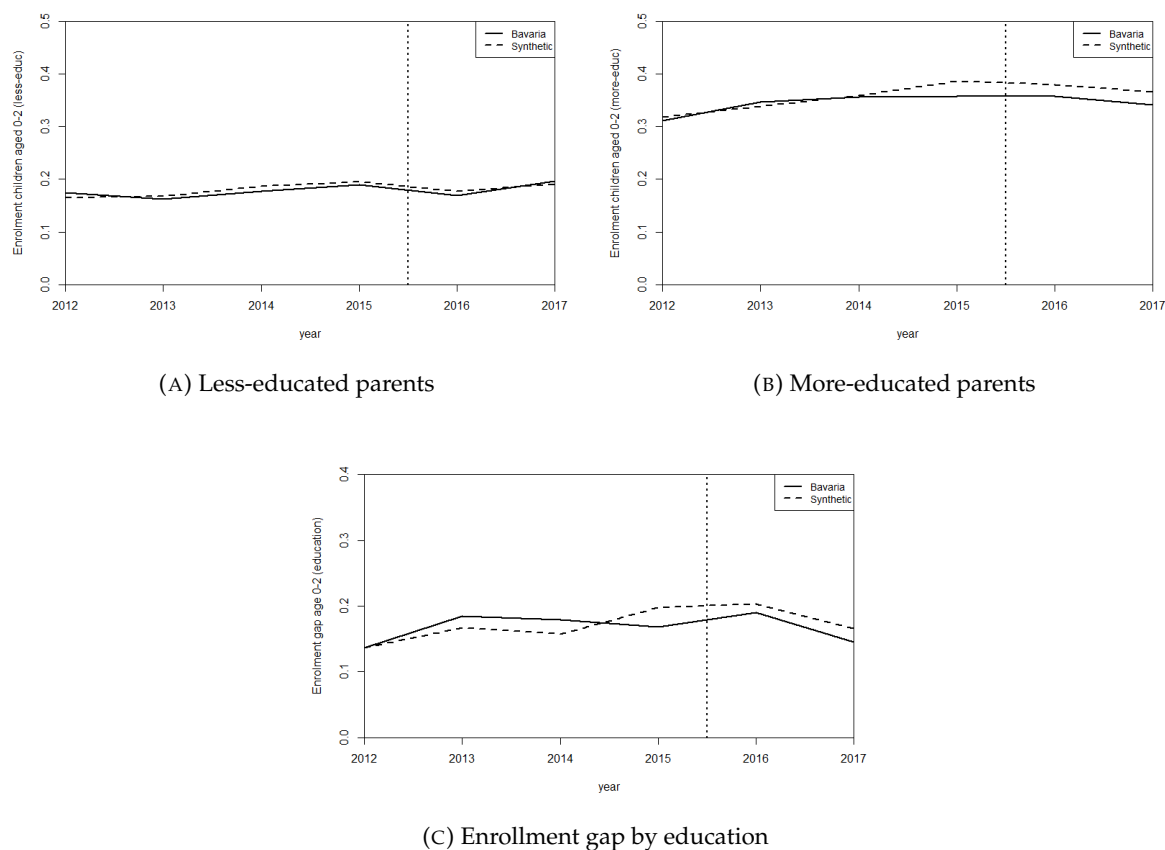
where δ_t is an unknown common factor with constant factor loadings across units, $\mathbf{Z}_i$ is a vector capturing the predictor variables, which are assumed to be unaffected by the treatment, θ_t is a vector of unknown parameters, λ_t is a vector of unobserved common factors, $\boldsymbol{\mu}_i$ is a vector of unknown factor loadings, and the error terms ϵ_{it} are unobserved transitory shocks at the state level with zero mean.

3.4.1 Impact on Enrollment by Education

Figure 3.1 shows the results on enrollment by parental education. The three panels plot the evolution of ECC enrollment of children from less-educated parents, more-educated

¹⁶Results on the impact of the reforms in Berlin, Brandenburg, Saxony-Anhalt, Thuringia are available upon request.

FIGURE 3.1: Impact of CSR Reform on Enrollment by Education in Bavaria



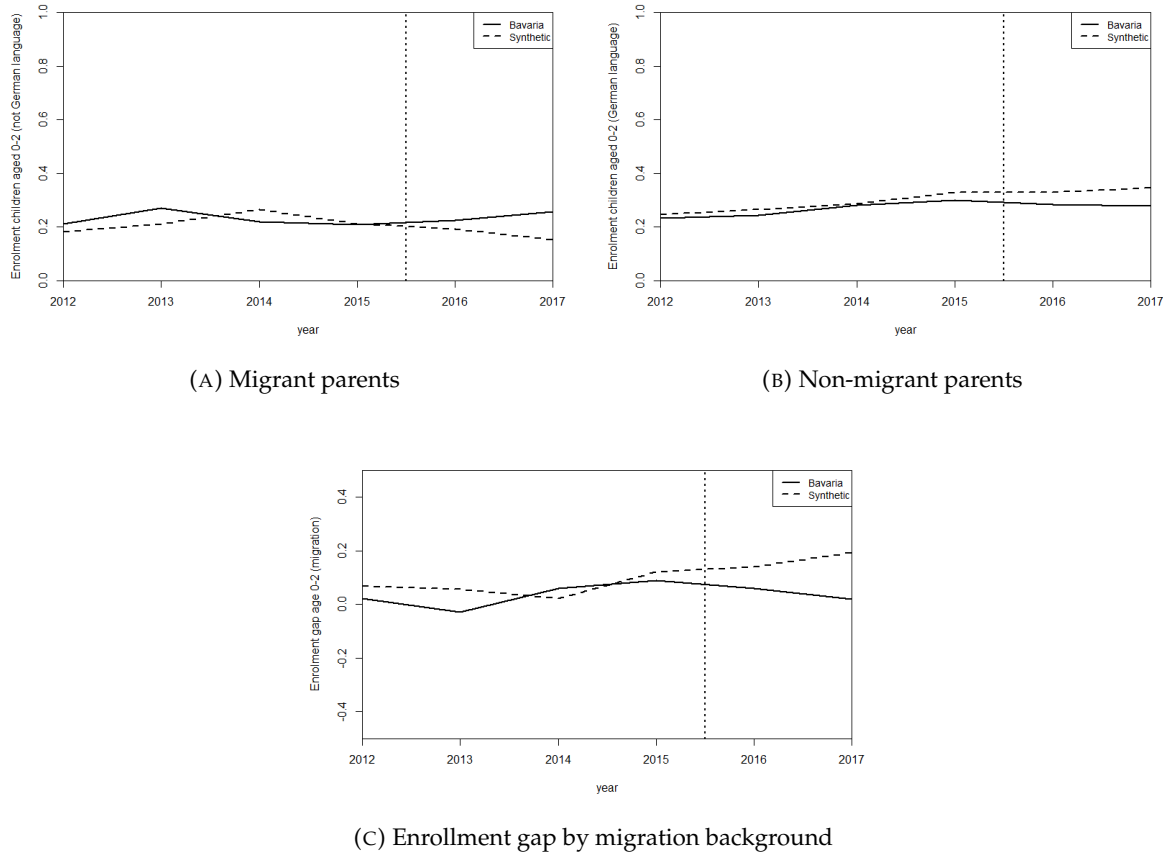
Notes: Figure 3.1 shows the estimated effect of the CSR reform on ECC enrollment by parental education in Bavaria. The three panels plot the evolution of ECC enrollment of children from less-educated parents, more-educated parents and the ECC enrollment gap over time for Bavaria and the synthetic counterpart. The vertical line indicates the date of the CSR reform and separates the pre- and post-treatment periods. State-level values are weighted means. Source: KiBS.

parents and the ECC enrollment gap over time for Bavaria and the synthetic counterpart. The figure suggests that enrollment from less-educated parents marginally decreased in the first post-treatment period, while it increased relative to the synthetic control in the second post-treatment period. For children from more-educated parents, enrollment appears mostly unaffected by the CSR reform in the post-treatment periods. Overall, the enrollment gap increased relative to the synthetic counterpart in the first post-treatment period - and marginally decreased thereafter. These results indicate that stricter regulation of ECC quality may be accompanied by mixed effects on enrollment gaps by education.

3.4.2 Impact on Enrollment by Migration Background

Figure 3.2 features results on enrollment by parental migration background. Again, the three panels plot the evolution of ECC enrollment of children from migrant parents, i.e. parents where German is not the main language at home, non-migrant parents, i.e. parents where German is the main language at home, and the ECC enrollment gap over time for Bavaria and the synthetic counterpart. The results suggest that relative to the synthetic controls, enrollment from migrant parents increased, while enrollment from non-migrant parents decreased. The effects occur in the first- and second post-treatment period. As a consequence, the enrollment gap by parental migration background decreases. Hence, these results differ from the findings on enrollment by education and suggest that increased minimum standards in child care quality may narrow enrollment gaps by parental migration background.

FIGURE 3.2: Impact of CSR Reform on Enrollment by Migration Background in Bavaria



Notes: Figure 3.2 shows the estimated effect of the CSR reform on ECC enrollment by parental migration background in Bavaria. The three panels plot the evolution of ECC enrollment of children from migrant parents (i.e. parents where German is not the main language at home), non-migrant parents (i.e. parents where German is the main language at home) and the ECC enrollment gap over time for Bavaria and the synthetic counterpart. The vertical line indicates the date of the CSR reform and separates the pre- and post-treatment periods. State-level values are weighted means. Source: KiBS.

3.5 Robustness Checks

3.5.1 Placebo Estimates: Impact of the CSR reform in Bavaria on Sociodemographic Enrollment Gaps

Following the previous empirical analysis, questions arise as to whether my application of the SCM yields estimates of the true impact of the CSR reform on enrollment gaps and whether the estimated effects are significant. The answers to these questions hinge on the

ability of the SCM to replicate the evolution of the outcome in the treated state in the hypothetical absence of the CSR reform, i.e. in the counter-factual scenario. To assess the performance of the SCM, I implement placebo studies. These placebo studies take the form of an iterative process during which I perform the SCM on each state of the control group of Bavaria. Within each instance, I assign a placebo treatment to a specific control state and allow the SCM to draw on the remaining units of the control group to create the synthetic counterpart. Then, I obtain a placebo estimate for each control state of Bavaria separately by comparing the evolution of the outcome in this state to the evolution of the outcome in its synthetic counterpart.

The magnitude of the placebo estimates relative to the estimate obtained for the treated state, Bavaria, is indicative of the probability of confounding events in control states being present and of the probability that the estimated impact of the CSR reform in Bavaria occurred by chance. In case of the SCM providing estimates of a similar magnitude for both, the placebo treatments and the treated state, events other than the CSR reform may have produced the result on the impact on enrollment gaps and the impact is considered as not significant. However, if the estimate of the treated state is large in magnitude relative to the placebo estimates, it is unlikely that the estimated impact of the CSR reform in Bavaria is biased by confounding events in control states and the treatment effect is considered as significant (Abadie and Gardeazabal, 2003; Abadie et al., 2010). In each figure of this section, I separately depict the estimated effects for the treated state, Bavaria, and for each placebo treatment assigned to a specific control state of Bavaria.

Figure 3.3 and 3.4 display placebo estimates for the impact of the CSR reform in Bavaria on sociodemographic enrollment gaps. The figures feature gray and black lines. Each gray line plots the estimated gap in the outcome of a placebo treated state and its synthetic counterpart, while the black line refers to the estimated gap for the treated state. In addition to carrying out a visual inspection of the placebo estimates, I compare the estimate obtained for the treated state to the placebo estimates by studying post/pre CSR reform ratios of the mean squared prediction error (MSPE). Here, a large post/pre ratio of the MSPE indicates that the SCM produces a large gap between the outcomes of the (placebo) treated state and the synthetic counterpart in the post-treatment period relative to the gap in outcomes in the

pre-treatment period. Table 3.1 presents the post/pre ratios of the MSPE for all estimations.

TABLE 3.1: Post/Pre MSPE Ratio of Placebo Estimates: Impact of CSR Reforms on Enrollment in Bavaria

State	Parental education			Parental migration background		
	Less	More	Gap	Not German	German	Gap
Bavaria (treated)	0.90	2.18	0.72	3.52	7.74	6.20
<i>Placebo treated</i>						
Schleswig-Holstein	2.06	2.12	4.96	21.47	4.92	1778.46
Bremen	7.47	2.99	2.42	0.27	0.18	0.75
North Rhine-Westphalia	0.12	0.21	1.44	0.05	0.80	0.02
Hesse	1.82	0.04	1.17	9.64	14.85	0.44
Rhineland-Palatinate	3.63	1.26	20.00	7.15	7.39	4.12
Mecklenburg-Vorpommern	1.04	0.62	2.55	0.78	0.78	1.36

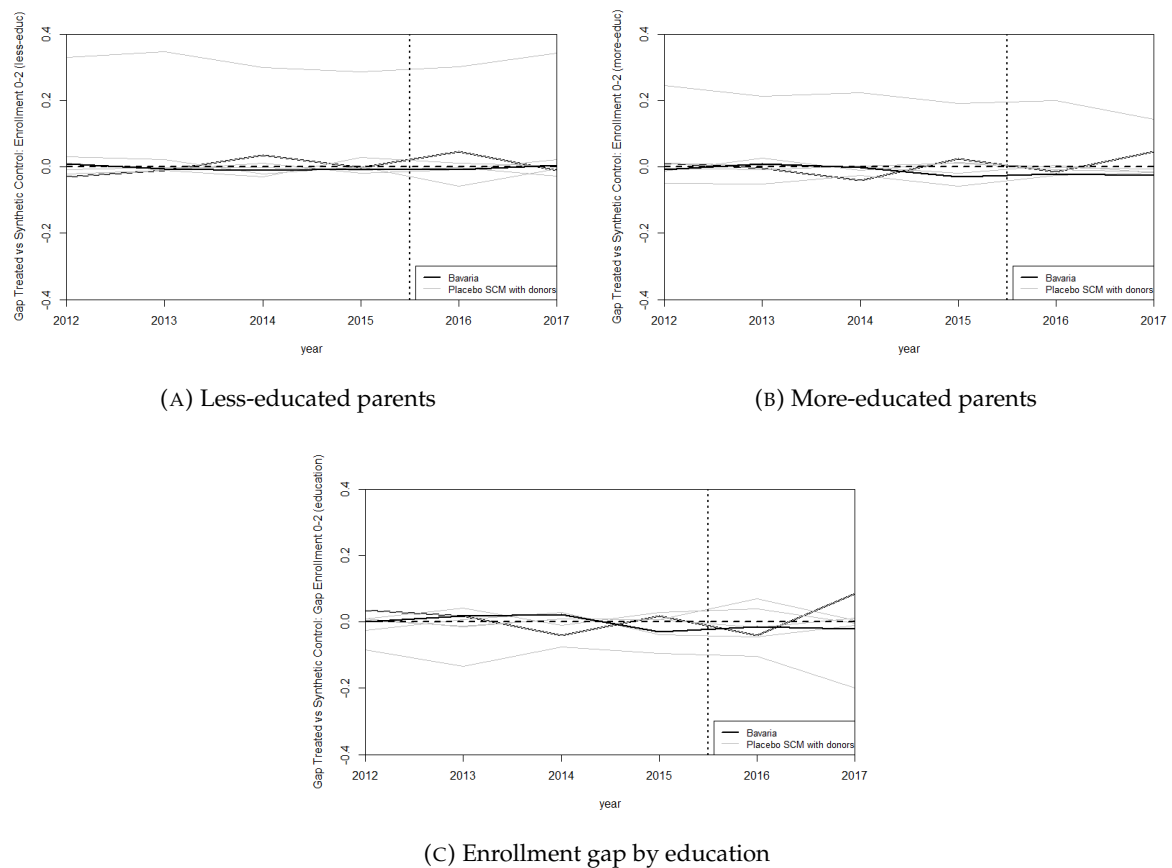
Notes: The table displays post/pre ratios of the mean squared prediction error (MSPE) for the placebo estimates featured in Figure 3.3 and 3.4. The MSPE is calculated as the average of squared differences between ECC enrollment in a given placebo treated state and in its synthetic control. The terms "pre" and "post" refer to the average during the pre- and post-treatment period, respectively. Source: KiBS Waves 2012-2017.

Placebo Estimates: Enrollment Gap by Education in Bavaria

Figure 3.3 displays the placebo estimates for the impact of the CSR reform in Bavaria on the enrollment gap by education. Regarding the enrollment of children from less-educated parents, the estimated pre- and post-treatment gaps of Bavaria visually appear negligible in magnitude relative to the gaps estimated for the placebo treated states. The minor gap estimated for Bavaria in the pre-treatment period indicates that the SCM is able to create a synthetic Bavaria that closely matches the evolution of enrollment in Bavaria in the years before the CSR reform. However, the minor gap in outcomes in the post-treatment period and the post/pre ratios of the MSPE suggest that the estimated impact of the CSR reform is statistically insignificant. Most post/pre ratios calculated for the placebo treated states are quantitatively larger than the ratio obtained for Bavaria. Assuming an absence of confounding events in control states, this suggests that when assigning a placebo treatment to one of the seven control states at random, the probability of observing an estimate as large as the one of Bavaria would be very high ($6/7 = 0.857$). Thus, the enrollment of children

from less-educated parents appears to be unaffected by the CSR reform. The large estimates for the placebo treated states also suggest a further investigation of potentially confounding events in control states.

FIGURE 3.3: Placebo Estimates: Impact of CSR Reform on Enrollment by Education in Bavaria



Notes: Figure 3.3 shows placebo estimates of the CSR reform on ECC enrollment by parental education in Bavaria. The three panels plot the estimated difference between the (placebo) treated state and its synthetic counterpart in ECC enrollment of children from less-educated parents, more-educated parents and in the ECC enrollment gap over time. The black line refers to the estimated differences in outcomes between Bavaria and its synthetic counterpart. The gray lines plot the estimated differences in outcomes between a given placebo treated state and its synthetic counterpart for each control state. The vertical line indicates the date of the CSR reform and separates the pre- and post-treatment periods. State-level values are weighted means. Source: KiBS.

With respect to the enrollment of children from more-educated parents, the estimated pre-treatment gap of Bavaria is visually perceived as comparable to the gaps estimated for the placebo treated states. However, in the post-treatment period, the gap of Bavaria is large relative to the gaps observed for the placebo treatments. This finding in isolation suggests

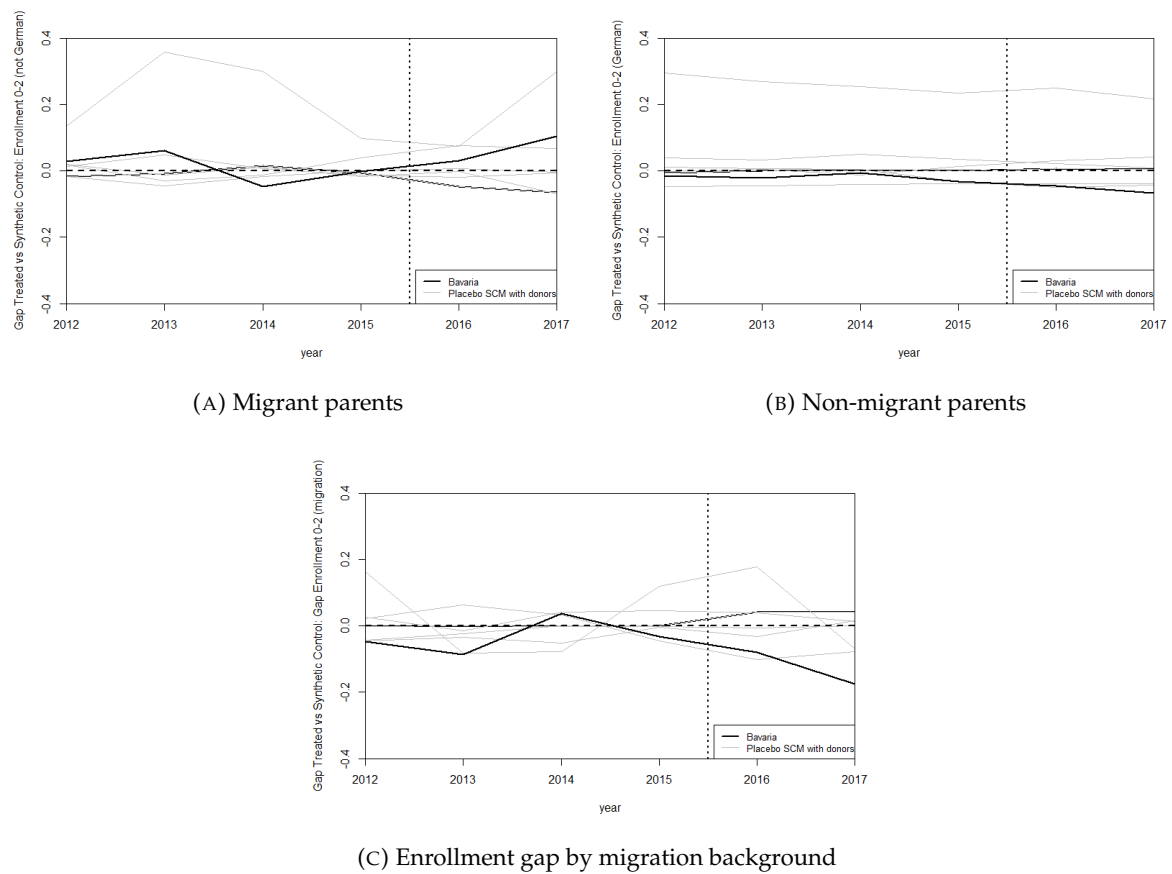
that the gap observed in the post-treatment period may not be created by an inability of the SCM to generate the counter-factual scenario. Studying the post/pre ratios of the MSPE reveals that the estimated impact of the CSR reform in Bavaria on enrollment of children from more-educated parents is closer to statistical significance than the estimate obtained for less-educated parents. The ratio of Bavaria is quantitatively larger than most ratios of placebo treated states. This implies that, in the absence of confounding events in control states, the estimated impact of the CSR reform is less likely to be driven by chance. The probability of observing an estimate as large as the one for more-educated parents in a placebo treated state is $2/7 = 0.286$. Accordingly, the impact of the CSR reform on enrollment of children from more-educated parents is not significant at conventional levels of statistical significance.

The visual inspection of the enrollment gap by parental education suggests that the estimated pre- and post-treatment gaps of Bavaria are comparable in magnitude to the gaps estimated for the placebo treated states. Thus, it is likely that the observed post-treatment gap is only a consequence of the SCM not being able to create a synthetic Bavaria that better matches the evolution of the outcome over time. The fact that all post/pre ratios of the MSPE are quantitatively larger for the placebo treated states than for Bavaria hints at a statistical insignificance of the estimated impact of the CSR reform. Again, the possibility of confounding events in control states should receive further attention. Especially in the case of Rhineland-Palatinate, where the post/pre ratio is exceptionally large. In summary, after considering the placebo studies, I assume that the enrollment gap by parental education was largely unaffected by the CSR reform in Bavaria.

Placebo Estimates: Enrollment Gap by Migration Background in Bavaria

Figure 3.4 presents the placebo estimates for the impact of the CSR reform in Bavaria on the enrollment gap by migration background. When inspecting the enrollment of children from migrant parents, it appears that the estimated pre- and post-treatment gaps of Bavaria are similar in magnitude to the gaps estimated for the placebo treated states. The large gaps in the pre-treatment period suggest that the SCM fails to find suitable control states for the synthetic counterfactual. The post/pre ratios of the MSPE reveal that the estimated impact of the CSR reform is statistically insignificant. Because some ratios of the placebo treated

FIGURE 3.4: Placebo Estimates: Impact of CSR Reforms on Enrollment by Migration Background in Bavaria



Notes: Figure 3.4 shows placebo estimates of the CSR reform on ECC enrollment by parental migration background in Bavaria. The three panels plot the estimated difference between the (placebo) treated state and its synthetic counterpart in ECC enrollment of children from less-educated parents, more-educated parents and in the ECC enrollment gap over time. The black line refers to the estimated differences in outcomes between Bavaria and its synthetic counterpart. The gray lines plot the estimated differences in outcomes between a given placebo treated state and its synthetic counterpart for each control state. The vertical line indicates the date of the CSR reform and separates the pre- and post-treatment periods. State-level values are weighted means. Source: KiBS.

states are larger than Bavaria's, it is statistically not improbable to observe a ratio of the magnitude of Bavaria's even in the absence of a CSR reform. These findings suggest that the impact of the CSR reform in Bavaria on enrollment of children from migrant parents is statistically insignificant.

With respect to the enrollment of children from non-migrant parents, the estimated pre-treatment gap of Bavaria appears smaller relative to the gaps estimated for the placebo treated states, while the post-treatment gap of the treated state seems large relative to the

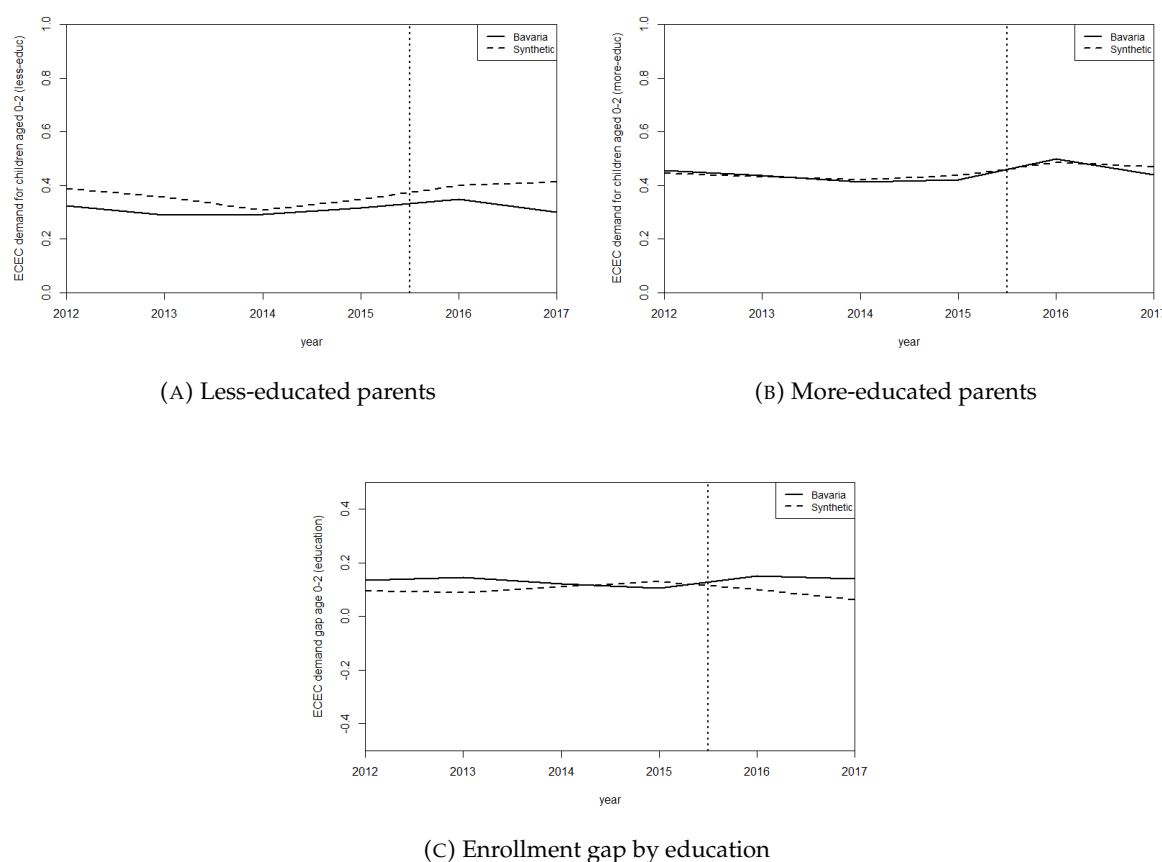
gaps observed for the placebo treated states. Hence, the SCM is able to relatively successfully recreate the pre-treatment evolution of the outcome in Bavaria and the post-treatment gap may represent a significant effect of the CSR reform. The post/pre ratios of the MSPE confirm this impression. Only the placebo treatment in Hesse generates a larger ratio than the CSR reform in Bavaria. This implies that while the estimated impact of the CSR reform in Bavaria on the enrollment of children from non-migrant parents is closer to being significant, it still falls short of statistical significance at conventional levels.

When analyzing the enrollment gap by migration background, the estimated pre-treatment gap of Bavaria is visually comparable to the gaps estimated for the placebo treated states. For the post-treatment period, however, the gap estimated for the treated state is of a larger magnitude than the gaps obtained for the placebo treated states. This finding hints at a potential impact of the CSR reform on the enrollment gap by migration background. Most post/pre ratios of the MSPE are quantitatively smaller for the placebo treated states than for Bavaria. The probability of observing a ratio as large as Bavaria's when randomly assigning the treatment is $2/7 = 0.286$. The post/pre ratio of 1778.46 for Schleswig-Holstein underlines the importance of evaluating the possibility of confounding events in control states. The results suggest that the effect of the CSR reform on the enrollment gap by migration background is less likely to be driven by chance, but remains statistically insignificant.

3.5.2 Placebo Outcome: Impact of the CSR reform in Bavaria on ECC Demand by Sociodemographic Background

The descriptive analysis of ECC demand in Section 3.3.1 suggests that ECC demand strongly responds to changes in parental preferences, which represent demand-side constraints in the child care market. At the same time, the measure appears to be less sensitive to supply-side constraints, such as parental concerns about limited ECC availability, quality or parental fees. Since potential intended and unintended consequences of the CSR reforms represent supply-side constraints, I expect that the policies should not induce strong responses in ECC demand. Thus, in the following, I consider demand for ECC as a placebo outcome.

FIGURE 3.5: Impact of CSR Reform on Demand by Education in Bavaria



Notes: Figure 3.5 shows the estimated effect of the CSR reform on ECC demand by parental education in Bavaria. The three panels plot the evolution of ECC demand of less-educated parents, more-educated parents and the evolution of the ECC demand gap over time for Bavaria and the synthetic counterpart. The vertical line indicates the date of the CSR reform and separates the pre- and post-treatment periods. State-level values are weighted means. Source: KiBS.

Placebo Outcome: Impact on ECC Demand by Parental Education in Bavaria

Figure 3.5 presents results on demand for ECC by parental education. The three panels plot the evolution of ECC demand from less-educated parents, more-educated parents and the ECC demand gap over time for Bavaria and the synthetic counterpart. The estimates indicate that demand from less-educated parents decreased in the second post-treatment period. For children from more-educated parents, demand increased in the first post-treatment period, while it decreased relative to the counterfactual in the second post-treatment period.

Overall, the figure suggests that the demand gap by education increased, which can be primarily attributed to the strong rise in ECC demand from more-educated parents in the first post-treatment period in Bavaria. Considering this, the changes in ECC demand

by parental education, as well as in the demand gap suggest that the policy may have been accompanied by a coincident change in parental preferences for ECC. While I do not directly measure parental preferences, I assume that ECC demand serves as a proxy measure for such preferences.

Placebo Outcome: Impact on ECC Demand by Parental Migration Background in Bavaria

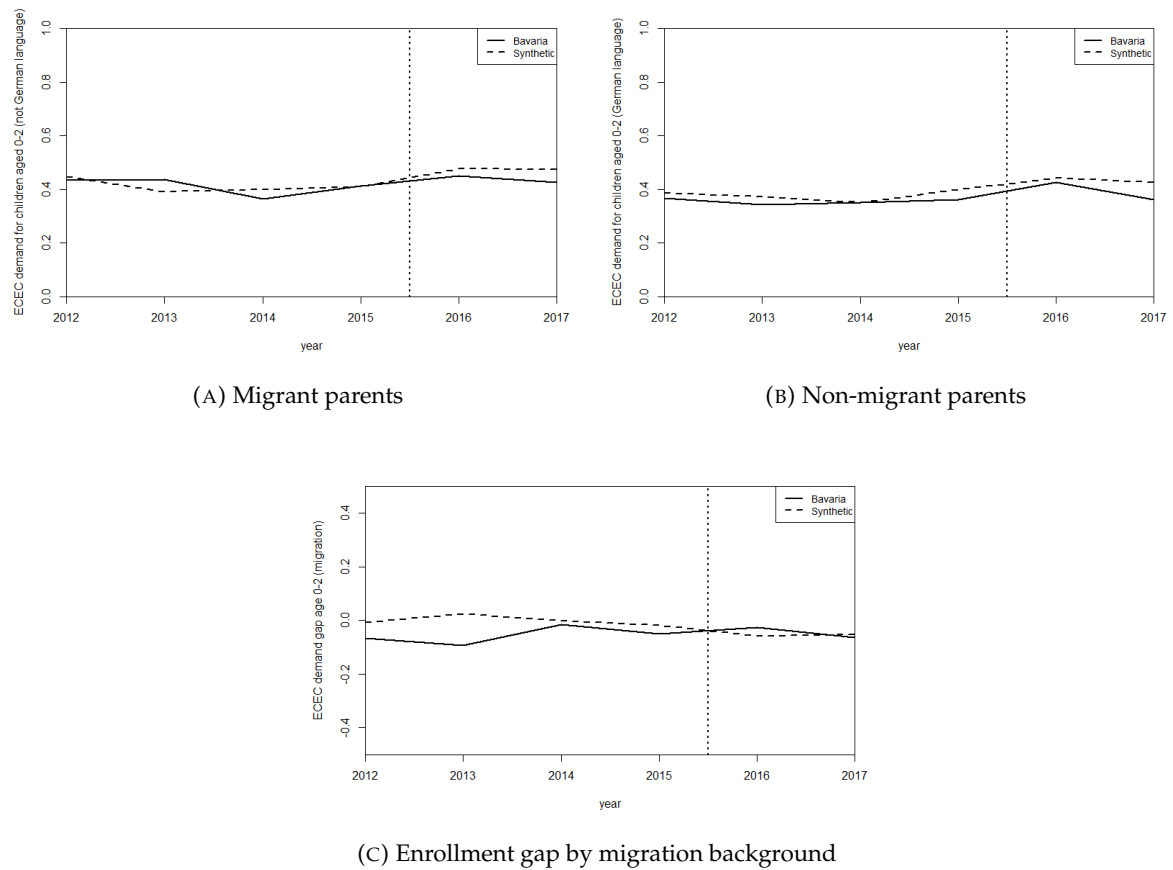
Figure 3.6 shows results on demand for ECC by parental migration background. Again, the three panels plot the evolution of ECC demand of children from migrant parents, i.e. parents where German is not the main language at home, non-migrant parents, i.e. parents where German is the main language at home, and the ECC demand gap over time for Bavaria and the synthetic counterpart.

The figure suggests that demand from parents with a migration background decreased in the post-treatment periods. For children from non-migrant parents, demand increased in the first post-treatment period, while it decreased relative to synthetic Bavaria in the second post-treatment period. Consequently, the demand gap by education increased (decreased) in the first (second) post-treatment period. Similar to the findings on demand by parental education, it is unlikely that ECC quality changes induced by the CSR reform are the explanation for the post-treatment evolution of demand by parental migration. Thus, changes in parental preferences or employment opportunities by migration background for ECC may have occurred after the child care reform.

3.6 Conclusion

Children from less-educated and migrant parents are less likely to participate in ECC. This results in early inequalities in child development and limited social mobility. The stringency of quality regulations in ECC may be a determinant of sociodemographic segregation in ECC enrollment. Since changes to minimum standards may introduce opposing effects in theory, the impact of stricter quality regulation is a priori undetermined and remains an empirical question. With this study, I provide empirical evidence from a child care reform in Germany by combining survey data on children's enrollment and parental demand for

FIGURE 3.6: Impact of CSR Reforms on Demand Gap by Migration Background in Bavaria



Notes: Figure 3.6 shows the estimated effect of the CSR reform on ECC demand by parental migration background in Bavaria. The three panels plot the evolution of ECC demand of migrant parents (i.e. parents where German is not the main language at home), non-migrant parents (i.e. parents where German is the main language at home) and the evolution of the ECC demand gap over time for Bavaria and the synthetic counterpart. The vertical line indicates the date of the CSR reform and separates the pre- and post-treatment periods. State-level values are weighted means. Source: KiBS.

ECC with a unique recording of changes to legislated child care quality over time. More precisely, my empirical analysis relies on quasi-experimental variation in regulated minimum standards for CSR in one German state over time to identify the causal impact of quality regulations on ECC enrollment and demand.

My findings suggest that the effects of the CSR reform in Bavaria on enrollment gaps by parental education and by parental migration background are statistically insignificant. Further, the child care reform may be accompanied by changes in parental demand for ECC. However, it remains an open question whether the effects of the CSR reform on parental demand are statistically significant. But the possibility that changes in parental preferences or employment opportunities, which are not induced by the policy change, are the driving force behind the observed variation in enrollment gaps cannot be ruled out. It should be noted, however, that I do not directly observe parental preferences. Moreover, it is possible that my results can be attributed to the specific nature of the German ECC system. In summary, increased minimum standards in ECC may not be an effective means of promoting social mobility.

Appendix

3.A Synthetic Control Method: Weights of Control States and Predictor Variables

TABLE 3.A.1: Synthetic Bavaria Weights: ECC Enrollment by Parental Sociodemographic Background

State	Parental education			Parental migration background		
	Less-educ	More-educ	Gap	Not German	German	Gap
Schleswig-Holstein	0	0	0	0	0	0
Bremen	0.088	0	0	0	0	0
North Rhine-Westphalia	0.193	0.005	0	0	0.055	0
Hesse	0.719	0.784	0.952	0.999	0.945	1
Rhineland-Palatinate	0	0.211	0.048	0	0	0
Mecklenburg-Vorpommern	0	0	0	0	0	0

Notes: The table displays the weights that the SCM assigns to each state from the control group in Figure 3.1 and Figure 3.2. These weights define synthetic Bavaria in the estimation of the impact of the CSR reform on ECC enrollment. The control group is a non-exhaustive list of the 16 German federal states. States with confounding treatments or where correct determination of CSR legislation was unfeasible are excluded.

TABLE 3.A.2: Synthetic Bavaria Predictor Variables: ECC Enrollment (Less-educ)

Predictor variables	Bavaria	Synthetic Bavaria	Sample Mean
Personnel resources keys (0-2)	3.875	3.810	4.067
Unemployment rate	3.725	6.622	8.050
Population density	179.514	445.477	472.073
GDP per capita	41318.250	40318.214	34521.000
Share of migrants	10.092	12.424	8.754
ECC Enrollment (less-educ) 2014	0.178	0.187	0.266
ECC Enrollment (less-educ) 2015	0.189	0.196	0.273

Notes: The table displays mean values of predictor variables for Bavaria, synthetic Bavaria and the full sample. The SCM relies on these predictor variables to define synthetic Bavaria in the estimation of the impact of the CSR reform on ECC enrollment of children from less-educated parents in Figure 3.1.

TABLE 3.A.3: Synthetic Bavaria Predictor Variables: ECC Enrollment (More-educ)

Predictor variables	Bavaria	Synthetic Bavaria	Sample Mean
Personnel resources keys (0-2)	3.875	3.876	4.067
Unemployment rate	3.725	5.618	8.050
Population density	179.514	271.053	472.073
GDP per capita	41318.250	39138.347	34521.000
Share of migrants	10.092	11.853	8.754
ECC Enrollment (more-educ) 2014	0.357	0.359	0.376
ECC Enrollment (more-educ) 2015	0.358	0.386	0.403

Notes: The table displays mean values of predictor variables for Bavaria, synthetic Bavaria and the full sample. The SCM relies on these predictor variables to define synthetic Bavaria in the estimation of the impact of the CSR reform on ECC enrollment of children from more-educated parents in Figure 3.1.

TABLE 3.A.4: Synthetic Bavaria Predictor Variables: ECC Enrollment gap (Education)

Predictor variables	Bavaria	Synthetic Bavaria	Sample Mean
Personnel resources keys (0-2)	3.875	3.914	4.067
Unemployment rate	3.725	5.659	8.050
Population density	179.514	283.975	472.073
GDP per capita	41318.250	40640.414	34521.000
Share of migrants	10.092	12.633	8.754
ECC Enrollment gap (education) 2014	0.179	0.157	0.110
ECC Enrollment gap (education) 2015	0.168	0.198	0.130

Notes: The table displays mean values of predictor variables for Bavaria, synthetic Bavaria and the full sample. The SCM relies on these predictor variables to define synthetic Bavaria in the estimation of the impact of the CSR reform on the ECC enrollment gap by parental education in Figure 3.1.

TABLE 3.A.5: Synthetic Bavaria Predictor Variables: ECC Enrollment (Not German)

Predictor variables	Bavaria	Synthetic Bavaria	Sample Mean
Personnel resources keys (0-2)	3.875	3.925	4.067
Unemployment rate	3.725	5.675	8.050
Population density	179.514	288.033	472.073
GDP per capita	41318.250	41065.383	34521.000
Share of migrants	10.092	12.853	8.754
ECC Enrollment (not German) 2014	0.219	0.266	0.248
ECC Enrollment (not German) 2015	0.208	0.211	0.215

Notes: The table displays mean values of predictor variables for Bavaria, synthetic Bavaria and the full sample. The SCM relies on these predictor variables to define synthetic Bavaria in the estimation of the impact of the CSR reform on ECC enrollment of children with migrant parents (i.e. parents where German is not the main language at home) in Figure 3.2.

TABLE 3.A.6: Synthetic Bavaria Predictor Variables: ECC Enrollment (German language)

Predictor variables	Bavaria	Synthetic Bavaria	Sample Mean
Personnel resources keys (0-2)	3.875	3.909	4.067
Unemployment rate	3.725	5.811	8.050
Population density	179.514	300.647	472.073
GDP per capita	41318.250	40753.178	34521.000
Share of migrants	10.092	12.724	8.754
ECC Enrollment (German) 2014	0.280	0.286	0.339
ECC Enrollment (German) 2015	0.299	0.330	0.366

Notes: The table displays mean values of predictor variables for Bavaria, synthetic Bavaria and the full sample. The SCM relies on these predictor variables to define synthetic Bavaria in the estimation of the impact of the CSR reform on ECC enrollment of children with non-migrant parents (i.e. parents where German is the main language at home) in Figure 3.2.

TABLE 3.A.7: Synthetic Bavaria Predictor Variables: ECC Enrollment gap (Migration)

Predictor variables	Bavaria	Synthetic Bavaria	Sample Mean
Personnel resources keys (0-2)	3.875	3.925	4.067
Unemployment rate	3.725	5.675	8.050
Population density	179.514	288.061	472.073
GDP per capita	41318.250	41069.262	34521.000
Share of migrants	10.092	12.856	8.754
ECC Enrollment gap (Migration) 2014	0.061	0.022	0.091
ECC Enrollment gap (Migration) 2015	0.090	0.122	0.151

Notes: Notes: The table displays mean values of predictor variables for Bavaria, synthetic Bavaria and the full sample. The SCM relies on these predictor variables to define synthetic Bavaria in the estimation of the impact of the CSR reform on the ECC enrollment gap by parental migration background in Figure 3.2.

3.B Summary statistics by West-/East-Germany

TABLE 3.B.1: Summary statistics West-Germany

Variable	Observations	Mean	SD	Min	Max
ECC enrollment	46,915	0.266	0.442	0	1
ECC demand	46,915	0.404	0.491	0	1
<i>Family background</i>					
More-educated (university entrance qualification)	46,915	0.519	0.5	0	1
No migration background (German is main language at home)	46,915	0.821	0.384	0	1
Age of child (in months)	46,915	18.228	9.328	0	35
Child is female	46,915	0.488	0.5	0	1
Respondent is married	46,915	0.833	0.373	0	1

Notes: The table displays summary statistics for West-Germany with data pooled over all waves. All observations and values are weighted. Source: KiBS Waves 2012-2017

TABLE 3.B.2: Summary statistics East-Germany

Variable	Observations	Mean	SD	Min	Max
ECC enrollment	27,654	0.511	0.5	0	1
ECC demand	27,654	0.588	0.492	0	1
<i>Family background</i>					
More-educated (university entrance qualification)	27,654	0.594	0.491	0	1
No migration background (German is main language at home)	27,654	0.911	0.285	0	1
Age of child (in months)	27,654	18.223	9.366	0	35
Child is female	27,654	0.483	0.5	0	1
Respondent is married	27,654	0.58	0.494	0	1

Notes: The table displays summary statistics for East-Germany with data pooled over all waves. All observations and values are weighted. Source: KiBS Waves 2012-2017

3.C ECC Demand by Reason For Not Using Child Care by West-/East-Germany

TABLE 3.C.1: ECC Demand by Reason For Not Using Child Care in West-Germany

	Affirmative answer		Negative answer		Diff
	Obs	Demand	Obs	Demand	
<i>Reasons related to intended effects CSR reforms</i>					
Would use ECC if more pedagogical staff in group	2136	0.40	7163	0.11	0.29***
Would use ECC if groups were smaller	3453	0.40	13296	0.14	0.26***
Reason for not using ECC: Pedagogical quality insufficient	800	0.21	6658	0.20	0.01
<i>Reasons related to unintended effects CSR reforms</i>					
Reason for not using ECC: Costs	5919	0.30	21302	0.16	0.15***
Reason for not using ECC: Did not get a slot	2681	0.70	16892	0.11	0.59***
Reason for not using ECC: Opening hours not suitable	2208	0.39	24876	0.17	0.22***
Reason for not using ECC: I am home myself	12103	0.14	7523	0.24	-0.10***
<i>Other reasons</i>					
Would use ECC if an institution was located nearby	2841	0.56	13851	0.12	0.44***
Reason for not using ECC: Child is too young	22667	0.13	4889	0.52	-0.39***

Notes: The table is a replication and extension of Jessen, Schmitz, and Waights, 2020. The table displays ECC demand in West-Germany by response (affirmative or negative) to questions relating to reasons for not using child care. Affirmative responses to multiple reasons are possible for a given survey respondent. Reading example: ECC demand equals 40% among parents who state that they would use ECC if more pedagogical staff were in a group, while ECC demand is 10% among parents that give a negative answer to this question. Differences ("Diff") are reported with statistical significance (***) $p < 0.01$, ** $p < 0.05$, * $p < 0.1$). All observations and values are weighted. Source: KiBS Waves 2012-2017

TABLE 3.C.2: ECC Demand by Reason For Not Using Child Care in East-Germany

	Affirmative answer		Negative answer		Diff
	Obs	Demand	Obs	Demand	
<i>Reasons related to intended effects CSR reforms</i>					
Would use ECC if more pedagogical staff in group	743	0.37	2620	0.07	0.30***
Would use ECC if groups were smaller	1350	0.35	4990	0.10	0.25***
Reason for not using ECC: Pedagogical quality insufficient	290	0.15	2774	0.15	-0.01
<i>Reasons related to unintended effects CSR reforms</i>					
Reason for not using ECC: Costs	851	0.27	9058	0.14	0.12***
Reason for not using ECC: Did not get a slot	1153	0.63	5663	0.08	0.55***
Reason for not using ECC: Opening hours not suitable	363	0.34	9540	0.15	0.19***
Reason for not using ECC: I am home myself	3185	0.15	3643	0.16	-0.01
<i>Other reasons</i>					
Would use ECC if an institution was located nearby	1111	0.48	5200	0.09	0.39***
Reason for not using ECC: Child is too young	8620	0.09	1495	0.60	-0.51***

Notes: The table is a replication and extension of Jessen, Schmitz, and Waights, 2020. The table displays ECC demand in East-Germany by response (affirmative or negative) to questions relating to reasons for not using child care. Affirmative responses to multiple reasons are possible for a given survey respondent. Reading example: ECC demand equals 40% among parents who state that they would use ECC if more pedagogical staff were in a group, while ECC demand is 10% among parents that give a negative answer to this question. Differences ("Diff") are reported with statistical significance (***) $p < 0.01$, ** $p < 0.05$, * $p < 0.1$). All observations and values are weighted. Source: KiBS Waves 2012-2017

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