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Does Biology Drive Child Penalties? Evidence from Biological and Adoptive Families*

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Abstract

This paper investigates if the impact of children on the labor market trajectories of women relative to men — child penalties — can be explained by the biological links between mother and child. We estimate child penalties in biological and adoptive families using event studies around the arrival of children and almost forty years of adoption data from Denmark. Long-run child penalties in earnings and its underlying determinants are virtually identical in biological and adoptive families. This implies that biology is not important for child-related gender gaps. Based on additional analyses, we argue that our results speak against the importance of specialization based on comparative advantage more broadly.

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1 Introduction

Parenthood has large and persistent effects on the labor market outcomes of women, but not men. This holds across different households, across different countries and over time, making it one of the most robust findings in labor economics. Estimates of long-run *child penalties* in female earnings range from 20-25% in Scandinavian countries to 30% in the United States and a staggering 60% in Germany (Kleven *et al.* 2019a,b). In fact, most of the remaining gender inequality in high-income countries can be attributed to the unequal impacts of children on men and women (Kleven *et al.* 2019a, 2020).

Why are child penalties so large and persistent? While the literature estimating reduced-form impacts is fairly conclusive, research on the underlying mechanisms is much less developed. A traditional explanation focuses on the factor that make men and women obviously different: biology. Only women can bear and give birth to children, and only women have the option to breastfeed. One would certainly expect such factors to matter for the short-run impacts of children, say within the first year of child birth, but they could also matter for the long-run impacts.

Two sets of reasons point to the possibility of long-run impacts. First, the physiological implications of pregnancy, delivery and breastfeeding may extend beyond the short run. This could be due to post-partum health complications, and it could be due to the possible effects of changes in brain structure and hormonal levels on maternal attachment, as posited by a large literature in biology (see e.g., Numan & Insel 2003; Feldman *et al.* 2007; Hoekzema *et al.* 2017). Second, biology may affect long-run labor market outcomes through the dynamic effects of work interruptions. Interrupting work around pregnancy and infant child care may affect future earnings capacity through experience effects (such as human capital accumulation or signaling), and it may change preferences over family vs career. Indeed, the push for earmarked paternity leave in several countries is predicated on the idea that such leave may strengthen the bond between father and child, with longer-run implications for the division of child care.

Testing for the importance of biology requires separating the effects of *having* a child from the effects of *giving birth* to a child. A natural way of obtaining this separation is to compare child penalties in biological and adoptive families. However, any such investigation faces two challenges. The first challenge is statistical power: The best estimates of child penalties are based on event studies around the arrival of children, which require large panel data sets with information on labor market outcomes and children. This requirement is harder to satisfy for adopted children, because relatively few families adopt and data sources often do not record adoptions. We deal with this challenge by using Danish administrative data that contain exhaustive information on adoptions over almost forty years. The second challenge is identification: Adoptive families are a selected subsample of the population, implying that any differences in child penalties between biological and adoptive mothers may reflect selection rather than biology. We deal with this challenge by matching on a rich set of observables, showing that the matched samples display parallel pre-trends in the event studies.

We find large and persistent effects of children on gender gaps in both biological and adoptive families. Women and men evolve in parallel until the arrival of their first child, whether by birth or by adoption, and then diverge sharply and persistently. The short-run impacts are somewhat larger in biological families, but the long-run impacts are virtually identical. Ten years after birth, the child penalty in earnings is about 18% in both biological and adoptive families.[1] When investigating the underlying determinants of earnings — participation, hours worked, and wage rates — we find that biological and adoptive families are similarly impacted in those dimensions too. These findings provide evidence against the importance of the biological link between mother and child for explaining the gendered impacts of children.

More broadly, our results speak against comparative advantage in child care as an important driver of child penalties. Pregnancy and breastfeeding are the most obvious sources of such comparative advantage, and if these factors have no impact on long-run

[1]The long-run child penalties estimated here are slightly smaller than those estimated in Kleven *et al.* (2019a) for the full population. This is because we are reweighting biological families to match the characteristics of adoptive families, where the latter tend to have fewer children overall and therefore smaller child penalties.

child penalties, then other sources of comparative advantage may have no impact either. To further investigate the role of comparative advantage, we study heterogeneity in child penalties by the earnings potential of mothers relative to fathers in biological and adoptive families. The earnings potential is estimated based on detailed information about education level, education field, and labor market experience at the time of birth of the first child. Strikingly, we find that long-run child penalties are virtually unaffected by the relative earnings potential of women and men, and this holds in both biological and adoptive families. These findings suggest against the comparative advantage channel, and they are consistent with finding a zero effect of biological links between mother and child.

Our paper contributes to a large literature on gender inequality in the labor market (recently reviewed by Bertrand 2011 and Olivetti & Petrongolo 2016) and specifically to studies investigating the importance of parenthood (e.g., Bertrand *et al.* 2010; Angelov *et al.* 2016; Kleven & Landais 2017; Kleven *et al.* 2019a,b; Kuziemko *et al.* 2018). Moreover, our finding that biological and adoptive mothers experience the same long-run child penalties — even though adoptees arrive later and require less maternity leave — sheds light on a key finding in the literature on parental leave policies (reviewed by Olivetti & Petrongolo 2017). This literature finds that paid leave has no long-term impacts on female labor market outcomes and gender gaps (e.g., Lalive & Zweimüller 2009; Rossin-Slater *et al.* 2013; Lalive *et al.* 2014; Schönberg & Ludsteck 2014; Dahl *et al.* 2016). Our paper is consistent with this finding and go one step further: It suggests that we should expect limited long-term effects on maternal labor market outcomes from *any* policy or treatment that affects new mothers only temporarily, say in the first year or two following child birth.

Finally, our paper is related to Andresen & Nix (2019) who study child penalties in lesbian couples, where one partner is biologically linked to the child while the other partner is not. They find no long-term differences in child penalties between the biological mother and the "co-mother". In other words, biological links do not matter in couples where gender is held constant. An important advantage of studying adoptive couples over same-sex couples is that it gives a much larger and less selected sample of the population, yielding more precision and greater generalizability.[2]

[2]Regarding the selection argument, an important way in which same-sex parents differ from heterosex-

2 Empirical Specification and Data

2.1 Event Study Specification

We estimate the impact of biological and adopted children on the labor market outcomes of men and women using the event study approach of Kleven *et al.* (2019a). Specifically, we consider a balanced panel of parents observed in each year from 5 years before the arrival of their first child, by birth or by adoption, until 10 years after. We consider the following specification

$$Y_{it} = \boldsymbol{\alpha}' \boldsymbol{D}_{it}^{Event} + \boldsymbol{\beta}' \boldsymbol{D}_{it}^{Age} + \boldsymbol{\gamma}' \boldsymbol{D}_{it}^{Year} + \nu_{it}, \tag{1}$$

where Y_{it} is the outcome (e.g., earnings) of individual i at event time t. On the right-hand side, we use boldface to denote vectors. The first term includes event time dummies, indexed such that $t = 0$ denotes the year of arrival of the first child. We omit the dummy for $t = -1$, so that each $\alpha_t \in \boldsymbol{\alpha}$ measures the impact of children in a given year relative to the year before child arrival. The second and third terms include a full set of age and year dummies to control non-parametrically for lifecycle trends and time trends.[3] This specification is run separately for men and women, and for those with biological and adopted children.

Equation (1) is specified in levels rather than logs to keep observations with zero earnings and thus capture both intensive and extensive margin responses. We convert level effects into percentage effects by calculating

$$P_t \equiv \frac{\hat{\alpha}_t}{\mathsf{E}\left[\tilde{Y}_{it} \mid t\right]}, \tag{2}$$

where $\tilde{Y}_{it}$ is the predicted outcome when omitting the contribution of the event dummies. By running the estimations separately for men and women with biological and adopted

ual parents is that their child penalty (for the biological mother as well as the co-mother) converges to zero in the long run. This stands in sharp contrast to the large long-run child penalties observed for heterosexual parents, whether biological or adoptive.

[3] The conditions for causal identification of the short- and long-term impacts of children in this framework were laid out and validated in Kleven *et al.* (2019a).

children, we obtain four series of P_t. These series can be compared to estimate the impact of children on women relative to men — child penalties — in biological vs adoptive families across event time. This will shed light on the potential role of biology for short-run and long-run child penalties.

It is worth discussing two points on interpretation. First, differences in child penalties between biological and adoptive parents may not necessarily reflect biology alone, but also the differential selection of the two sets of parents. As we show, adoptive families tend to have their first child later, have fewer children overall, and have higher education and earnings. We deal with such selection issues by reweighting the sample of biological parents to ensure that the distribution of their background characteristics (x_B) exactly matches the distribution for the adoptive parents (x_A). Formally, we compute weights as the relative fraction of individuals with a certain set of characteristics in the two samples $(f(x_A)/f(x_B))$ and use these weights in the regression (1) and in the expectation in equation (2) for the biological sample. By reweighting only the biological sample, we are able to adjust for a potentially rich set of observables while losing power only in the power-abundant biological sample. This weighting strategy also implies that the treatment effects for the adoptive sample are unaffected by the choice of weights.

In our baseline specification, we reweight the biological sample to match the distribution of (i) year of arrival of the first child, (ii) years of arrivals of the second and third child (if any), and (iii) the total number of children in the adoptive sample. This ensures that we are comparing families who are treated by children in the same way. This is important because, even though the event studies are centered on the arrival of the first child, the longer-run impacts will capture the impact of subsequent children as well. Hence, finding that biological and adoptive families experience similar long-run child penalties would not be very informative if they were treated differently by subsequent children. In robustness checks presented in the appendix, we consider weighting schemes that include additional socio-economic characteristics.

Second, since adopted children do not arrive immediately after birth, there is a difference between event studies centered on child *arrivals* and event studies centered on child *births*. Our baseline specification is based on arrivals — the actual "event" for adoptive families — but a specification based on births would have merit as well. In particular, centering on births ensures that biological and adopted children have the same age at each event time, while centering on arrivals implies that adoptees are a little older (about one year older on average) at each event time. We consider specifications based on births in the appendix, showing that the long-run child penalties are virtually the same when doing this.

2.2 Data

Our analysis uses administrative data from Statistics Denmark (DST) covering the full population between 1980 and 2017. The DST data combine several administrative registers linked at the individual level through personal identification numbers. The data allow us to link individuals to their family members and contain detailed information on earnings, labor supply, education, children, and a range of other variables.

We focus on the impact of foreign adoptions throughout. Domestic adoptions are less common, the children tend to be older at arrival, and the adoptive parents often have a pre-existing link to the child (such as a step parent or aunt/uncle). Importantly, the adoption registry of Statistics Denmark only covers the period 1988-2009. Using this data alone would narrow the time window available for our event studies and reduce statistical power. We therefore augment the official records by identifying foreign adoptions outside the 1988-2009 window using information on country of origin and migration history. Specifically, we define foreign adoptees as individuals who fulfill the following conditions: (1) They were born in a non-western country, (2) they have two known parents born in a western country, (3) both parents had their legal address in Denmark (with no emigration record) at the time the child was born, and (4) the child has a recorded entry (immigration record) into the Danish Central Person Register after the date of birth.

To validate this procedure, Figure A.I in the online appendix compares our measure of adoptions to the official records during the time period where we have both. The figure shows that our measure captures the official numbers almost perfectly. Virtually all of our adoptees are also listed in the official records (no type II errors) and virtually no adoptees in the official records are missed by our measure (no type I errors).[4] We find around 400-600 adoptions per year, corresponding to 16,260 children between 1980-2017. About two-thirds of all foreign adoptees come from Asia, and about 40% of the Asian adoptees come from South Korea.[5]

We focus on parents whose first child arrives (by birth or by adoption) between 1985 and 2007, which gives us data for at least 5 years before and 10 years after parenthood in all families. We require that both parents are known, alive and reside in Denmark in each year of the event time window ($t = -5, ..., +10$). We impose no restrictions on the relationship status of the parents, including parents who are married, cohabiting, separated, divorced, or have not yet formed a couple in a given year. We also require that all subsequent children are of the same type as the first (adopted or biological) such that we are comparing purely biological to purely adoptive families, and we restrict attention to adoptive children arriving before the age of 5. These data restrictions leave us with around 527,000 first births in the biological sample and around 4,600 first arrivals in the adoptive sample.[6]

Our main outcome of interest is annual earnings. This includes income from wages, salaries, and self-employment. We also consider the impact of children on labor force participation, hours worked, and wage rates (earnings/hours worked). Our measures of hours worked and wage rates are based on administrative and third-party reported data from a mandated pension scheme called *Arbejdsmarkedets Tillægspension* (ATP), which requires employers to contribute on behalf of their employees based on individual hours worked.

Table 1 provides descriptive statistics on the mean and distribution of different background variables for children and their parents. We consider three different samples: adoptive families, biological families, and reweighted biological families. While other studies (e.g., Fagereng *et al.* 2019) have shown that foreign adoptees are as good as randomly allocated to adoptive parents, the table shows that the adoptive sample is selected

[4] The very small number of adoption in Statistics Denmark's data that are not identified by our procedure (type I errors) are primarily children where the entry date into the Central Person Register is assigned as the birth date.

[5] See Table A.I in the online appendix.

[6] See Table A.II in the online appendix.

differently than the raw biological sample. For example, adoptive parents tend to have their first child later, have fewer children in total, and have higher education and earnings. As shown in the table, our baseline weighting scheme ensures that the distribution of adoptive and biological families exactly matches in terms of treatment: Year of arrival of the first child, years to second and third child, and total number of children.

3 Results

3.1 Child Penalties in Biological vs Adoptive Families

Figure 1 shows the earnings impacts of parenthood on men and women in biological and adoptive families, respectively. Panel A considers all adoptees pooled, while Panel B considers adoptees split by their age at arrival. Each dot gives the percentage impact at event time t (relative to event time -1) based on the specification in (1)-(2). As described above, this specification controls non-parametrically for any underlying lifecycle and time trends, and it is implemented on a reweighted biological sample.

Consider first biological families. Relative to the underlying life-cycle and time trends, the earnings of men and women evolve in parallel until child birth and then diverge sharply. Female earnings drop by 25-30% immediately after child birth, while male earnings are unaffected. Women recover some of their earnings loss after infant child care, but they never catch back up to men. Ten years out, the child-induced earnings gap between mothers and fathers — the long-run child penalty — is equal to 17.3%.[7] These findings are well-known and hold across different countries (Kleven *et al.* 2019a,b).[8]

Consider then adoptive families. The main insight from Panel A of Figure 1 is that adoptive families are affected by parenthood in much the same way as biological families. The earnings of adoptive parents evolve in parallel before having children and then diverge sharply and persistently after having children. The short-run earnings impacts

[7] The long-run child penalty is measured as an average across event times 6-10 as the time series have reached a steady state by then.

[8] The long-run child penalty of 17.3% estimated here is slightly smaller than the penalty of 19.4% estimated in Kleven *et al.* (2019a). This is due to the fact that biological families have been reweighted to match adoptive families.

are smaller in adoptive families than in biological families, but the long-run impacts are virtually the same. The long-run child penalty on adoptive mothers equals 18.0%, slightly larger than the long-run penalty of 17.3% on biological mothers. That is, even though adoptive mothers are not biologically linked to their children and are unaffected by aspects such as breastfeeding and postpartum health complications, they converge to long-run penalties at least as large as those for biological mothers.

Furthermore, the penalties on adoptive mothers feature little heterogeneity by their child's age at arrival as shown in Panel B. The different adoptive subsamples — those with early, intermediate, and late arrivals — line up closely throughout the event study window. Even adoptive mothers whose first child arrives after the age of one (two) experience a long-run penalty of 17.8% (16.2%), statistically indistinguishable from the penalty of 17.3% on biological mothers. In other words, the age of the child is not critical for the labor market impacts, at least not after the initial stage of breastfeeding and infant child care.[9]

In our baseline specification, the biological sample is reweighted to match the distribution of the total number and timing of children in the adoptive sample. This ensures that the two samples experience the same treatment. Figure A.III in the appendix investigates robustness to alternative weighting schemes. Without any weights, the long-run child penalty in biological families is about 4pp larger than in adoptive families, reflecting the fact that biological families have more children. By contrast, using richer weighting schemes than in the baseline, by adding pre-birth education or by adding both pre-birth education and pre-birth earnings, has virtually no impact on long-run penalties.

3.2 Anatomy of Child Penalties

In this section we investigate the anatomy of the large and persistent earnings impacts of both biological and adopted children. Figure 2 presents event studies of the three

[9]In Figure A.II in the online appendix, we replicate the analysis presented here when centering on child births instead of child arrivals. In this case, the short-run differences between biological and adoptive families are larger due to the delayed arrival of adoptees. When splitting adoptees by their age at arrival, the short-run impacts are staggered across ages as one would expect. Despite these short-run differences, however, the long-run impacts on biological and adoptive families are still very similar (and they are similar to those estimated when centering the analysis on arrivals in Figure 1).

underlying earnings determinants: hours worked conditional on working (Panel A), the wage rate conditional on working (Panel B), and the participation rate (Panel C-D). For hours worked and the wage rate, we find virtually identical child penalties throughout the event study window. In biological as well as adoptive families, the long-run hours penalty equals 7-8% and the long-run wage rate penalty equals 10-11%. Turning to the participation rate in Panel C, however, we observe a significant difference: the participation penalty is persistently smaller in adoptive families. The long-run participation penalty is about 4% for adoptive mothers and about 9% for biological mothers.

How do we reconcile these findings — i.e., identical hours and wage rate penalties, but a smaller participation penalty on adoptive mothers — with the previous finding that their long-run earnings penalty is the same? The answer is selection: the participation responses among adoptive mothers occur at higher earnings levels than the participation responses among biological mothers. To demonstrate this, Panel D shows the impact of children on an *earnings-weighted* participation rate. To avoid any confounding effects of intensive margin responses, we use a measure of potential earnings rather than actual earnings. Potential earnings are estimated from Mincer regressions of earnings on education level and experience within cells of education field. These regressions are run on the sample of men only (as they are unaffected by children), using the predicted values to measure potential earnings for both men and women.[10] We consider participation interacted with this measure of potential earnings as our outcome variable.

Comparing Panels C and D, we see that the earnings-weighted participation gap between biological and adoptive mothers is smaller than the raw gap at each event time. Moreover, the earnings-weighted gap converges to zero over time. As a result, the short-lived differences in earnings penalties between biological and adoptive families can be fully explained by extensive margin responses selected on earnings level. The selection-

[10]To be precise, using the rich Danish data we divide the sample into detailed education fields (140 fields such as "acting" or "physics"). For each education field, we run the following regression

$$\ln Y_{is} = \alpha Edu_{is} + \beta_1 Exp_{is} + \beta_2 Exp_{is}^2 + \gamma Year_s + \nu_{is}, \tag{3}$$

where Y_{is} is earnings of individual i in year s, Edu_{is} is a set of education level dummies (six levels from elementary school to PhD), Exp_{is} is experience (years since graduation), and $Year_s$ is a set of year dummies. The regressions are run on the population of men aged 15-55 between 1980-2017, using the predicted values (for men and women) as our measure of potential earnings.

adjusted participation responses are initially smaller for adoptive mothers than for biological mothers (partly due to differences in the need for maternity leave), but ultimately converge to the same level.[11]

The results from the graphical analysis presented thus far are summarized in Table 2. This table shows estimates of long-run child penalties (along with their standard errors) in earnings, hours worked, wage rates, raw participation, and earnings-weighted participation in biological vs adoptive families (Panel A) and in adoptive families by age at arrival (Panel B). The table highlights the striking degree of similarity in the long-run impacts on different margins (with the exception of the raw participation outcome) across families types.

3.3 Heterogeneity in Child Penalties by Comparative Advantage

A traditional explanation for the large and persistent child penalties on women focuses on specialization based on comparative advantage: women have a comparative advantage in child care, while men have a comparative advantage in market work. Our results have implications for this interpretation. The most obvious reason why women would have a comparative advantage in child care is based on the biological link between mother and child. The fact that only women can bear children and breastfeed almost certainly gives them a comparative advantage in the early stages of parenthood, and, as discussed in the beginning, it may also give rise to longer-lasting comparative advantage due to changes in earnings capacity and preferences. The absence of persistent differences in child penalties between biological and adoptive mothers run counter to these ideas. However, it is still possible that comparative advantage is important, but that the source of comparative advantage studied here (the biological link between mother and child) is short-lived, while other sources of comparative advantage are longer-lived. To investigate this point, this section presents evidence on heterogeneity in child penalties by comparative advantage.

Studying the role of comparative advantage requires a measure of male and female

[11]We find similar results if we reweight the biological sample to match the pre-arrival distributions of income and education of the adoptive parents.

earnings capacity within families. To avoid endogeneity of measured earnings capacity to children, one strategy would be to divide the sample by observed earnings prior to child birth. However, selecting subsamples on pre-birth earnings creates issues with mean reversion: If earnings consist of both permanent and transitory income components, we would be splitting the sample partly by transitory income shocks rather than by comparative advantage alone. To avoid such problems, we measure potential earnings using the Mincer earnings regression (3) discussed above. Estimating the coefficients of this regression on the sample of males, we predict the potential earnings of both males and females based on their education field, education level, and labor market experience. We then split the sample by relative female earnings potential within families just before the birth of the first child (at event time -1).

The results are shown in Figure 3. The figure shows earnings impacts of biological and adopted children, respectively, by whether the woman has a lower or higher earnings potential than the man at the time of child birth. If comparative advantage is important, we should see larger child penalties in families where the woman has a lower earnings potential. Instead we see that child penalties are unrelated to our proxy for comparative advantage: the long-run child penalties are similar for low-earning and high-earning mothers, and this holds in both biological and adoptive families.

Considering the same sample split, Panel C of Table 2 shows child penalty estimates in earnings and its underlying determinants by relative female earnings potential within families. These estimates confirm the absence of a significant comparative advantage channel. Finally, Figure A.IV in the online appendix shows that our findings are robust to alternative measures of relative earnings potential, including measures that are more forward-looking.

4 Conclusion

A recent literature documents large child penalties in female labor market outcomes, showing that these penalties can explain most of the remaining gender inequality in developed countries (see e.g., Kleven *et al.* 2019a,b). In this paper, we ask why the impacts of children are so large and gendered, focusing on traditional explanations rooted in biology and comparative advantage. Using Danish administrative data, we provide compelling event study evidence on child penalties in biological and adoptive families. Despite the existence of short-run differences in the child penalties of these two family types, they converge to the same penalty in the long run. This is true for earnings as well as for its underlying determinants.

Our findings provide evidence against the importance of biological links between mother and child for explaining child penalties. Moreover, since these biological links represent some of the most obvious sources of comparative advantage, they provide evidence against classic specialization stories. We provide further evidence on comparative advantage, showing that child penalties are unrelated to the relative earnings potential within families in both biological and adoptive families. Overall, this paper suggests that child-related gender inequality (i.e., most remaining gender inequality) cannot be understood through the lens of biology and incentive-based specialization, pushing towards a greater focus on preference formation, social norms and culture.

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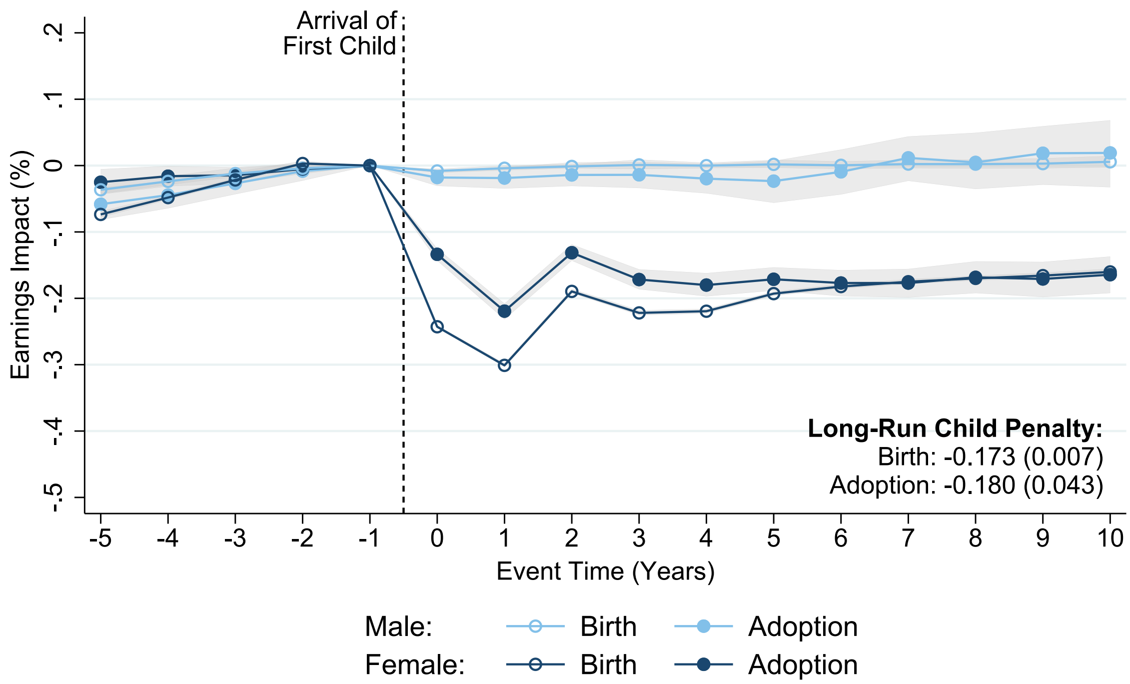
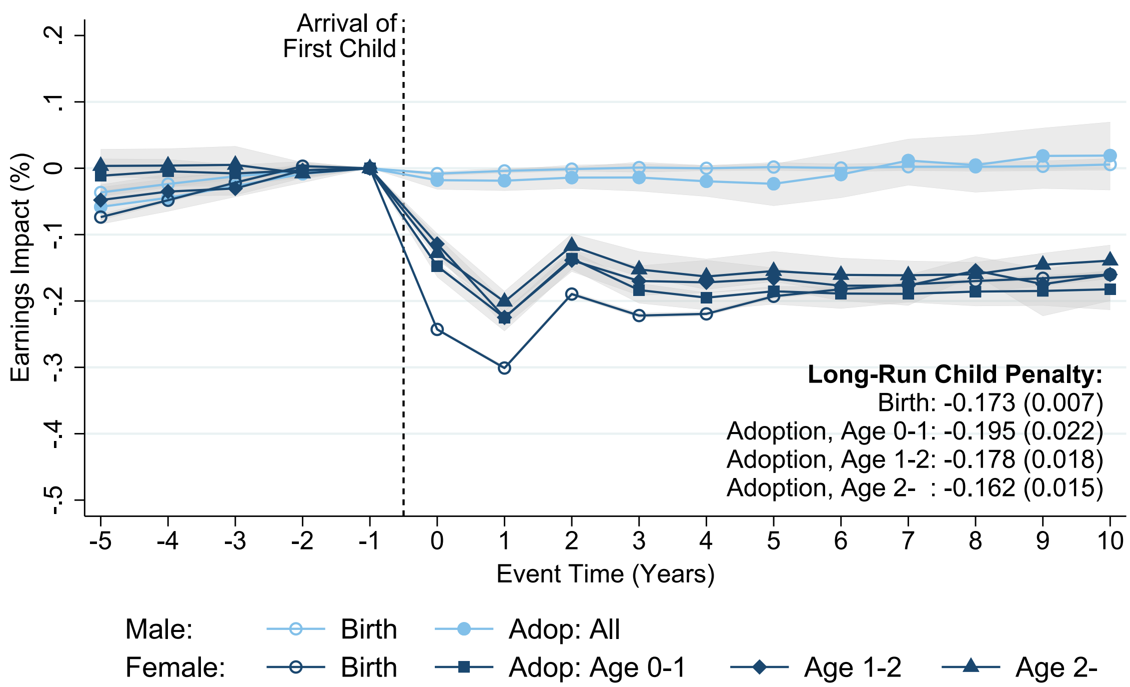
Figure 1: Child Penalties in Biological vs Adoptive Families

A: All Adoptees

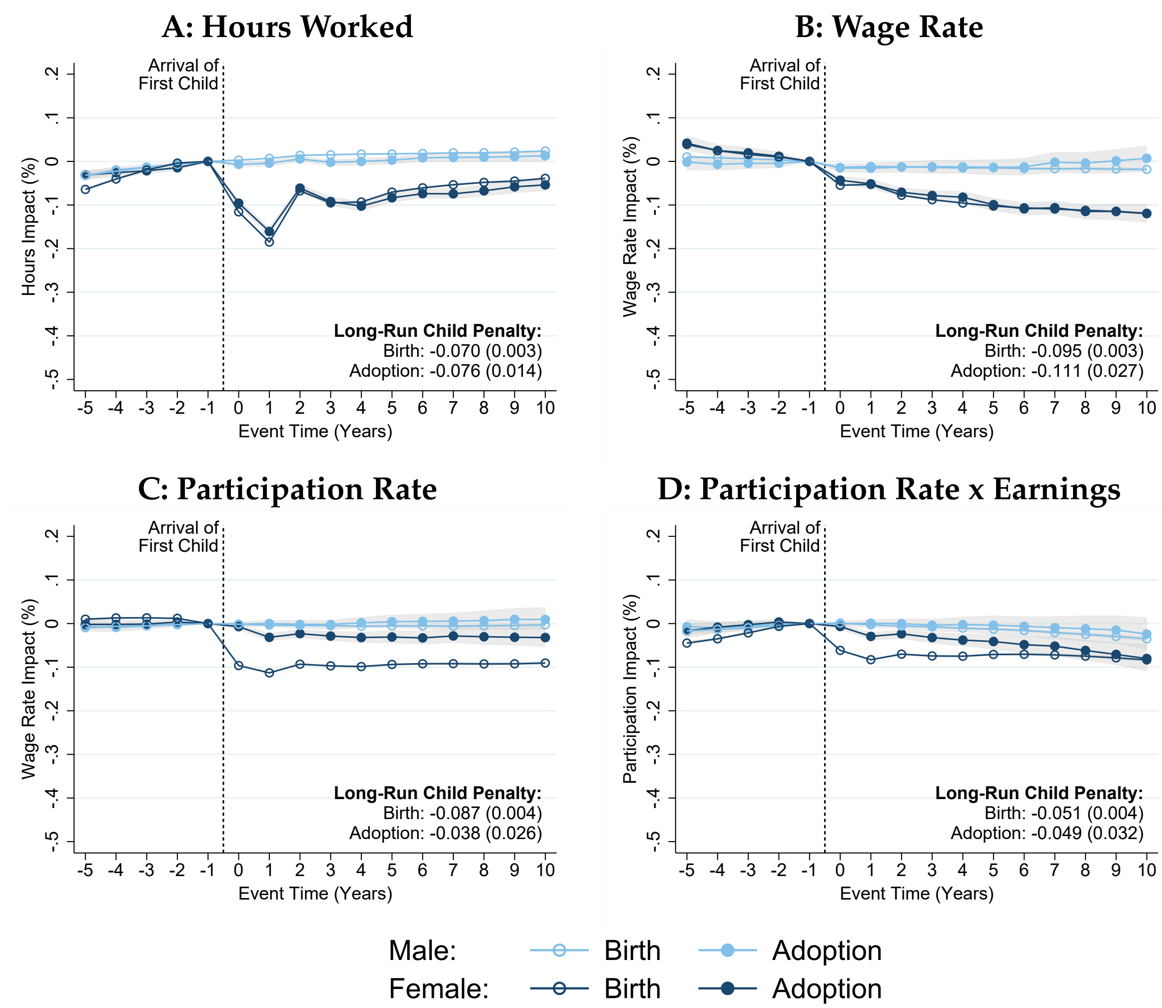


B: Adoptees by Age at Arrival



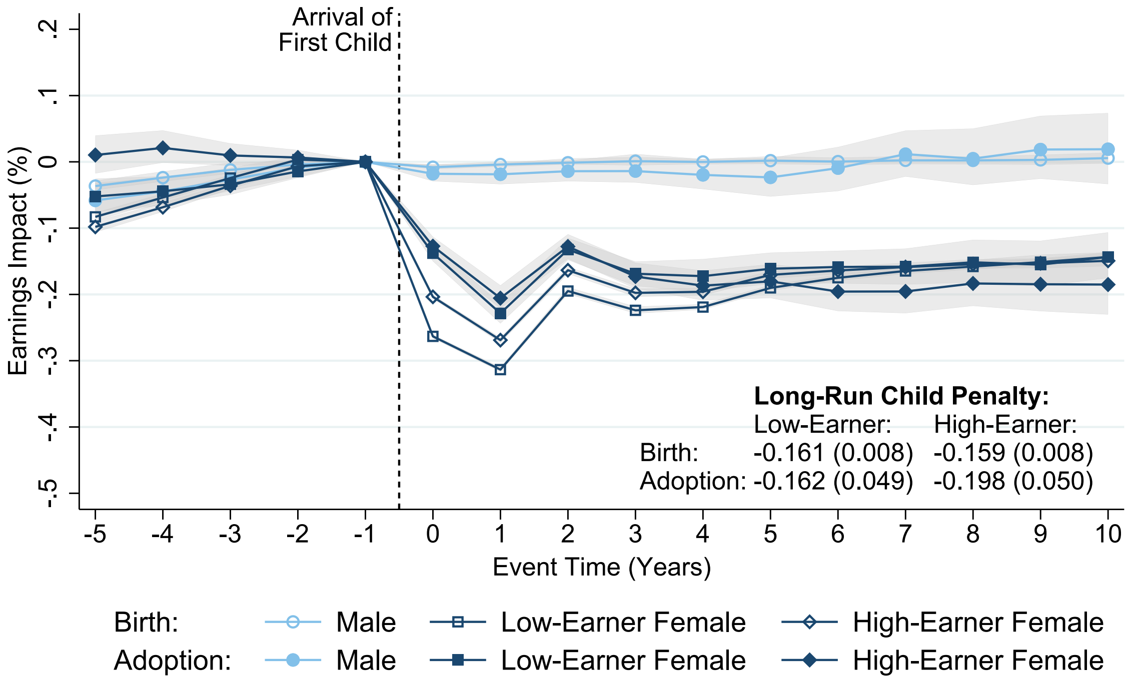
Notes: The figure shows the estimated impact of children (P_t) on earnings of men and women based on equations (1) and (2). The sample of biological parents is weighted so that it matches exactly the distribution of a) year of first child, b) years to subsequent children (if any) and c) the total number of children in the adoptive sample. Panel A shows the estimates for all adoptive couples together, while Panel B split the adopted mothers into three groups depending on the age at arrival of the first children. For this estimation we interact the event time dummies in equations (1) with dummies for belonging to each of the three age groups. Standard errors are bootstrapped (500 replications).

Figure 2: Anatomy of Child Penalties



Notes: The figure shows the estimated impact of children (P_t) on four different outcomes of men and women based on equations (1) and (2). The sample of biological parents is weighted as describe in Figure 1. Panel A shows the impact on hours worked (conditional on participation) using our *ATP* hours measure. Panel B shows the impact on the wage rate computed as annual earnings divided by annual ATP hours. We winsorize the wage rates at 0 and the within-year 99th percentile to deal with the measurement error in the hours measure and in particular the fact that self-employed can have very large positive or negative earnings with very small ATP hours. Panel C shows the impact on participation (postive ATP hours), while Panel D shows the impact on participation multiplied by our measure of potential earnings as described in footnote **??**. Standard errors are bootstrapped (500 replications).

Figure 3: Child Penalties by Relative Female Earnings Potential



Notes: The figure splits women into two groups depending on their relative within couple earnings potential. The estimation of individual earnings potentials is based on detailed records of completed educations as described in the text.

Table 1: Descriptive Statistics

	Adoptive Sample			Biological Sample			Weighted Biological Sample (Baseline Specification)		
	P25	**Mean**	**P75**	**P25**	**Mean**	**P75**	**P25**	**Mean**	**P75**
Children:									
Year of Arrival of First Child	1990.00	1995.85	2001.00	1990.00	1996.04	2002.00	1990.00	1995.85	2001.00
Years to Second Child	2.28	3.07	3.79	2.22	3.48	4.10	2.22	3.06	3.81
Years to Third Child	4.13	5.58	7.37	5.13	7.27	8.88	4.08	5.73	7.49
Total Number of Children	1.00	1.70	2.00	2.00	2.18	3.00	1.00	1.70	2.00
Age at Arrival of First Child	0.38	1.16	1.57	0.00	0.00	0.00	0.00	0.00	0.00
Mother:									
Age at First Child	32.00	35.43	38.00	25.00	27.51	30.00	25.00	28.60	31.00
Years of Schooling	13.00	13.63	15.50	12.00	12.58	14.00	12.00	12.67	14.00
Pre-Birth Earnings Rank	0.73	0.80	0.95	0.24	0.49	0.74	0.31	0.55	0.81
Father:									
Age at First Child	34.00	36.90	40.00	27.00	30.19	33.00	27.00	31.32	34.00
Years of Schooling	13.00	13.62	15.50	12.00	12.52	13.00	12.00	12.56	13.00
Pre-Birth Earnings Rank	0.65	0.76	0.94	0.25	0.50	0.75	0.29	0.54	0.79

Notes: The table shows descriptive statistics for our main sample as described in section 2.2. Our measure of years of schooling is based on individuals' highest educational attainment two years prior to the arrival of the first child. Pre-birth earnings rank is computed using the average earnings 1-5 years prior to the arrival of the first child and computed within gender and year of first child cells. In the last tree columns, we have weighted the biological sample to exactly match the distribution of a) year of first child, b) years to subsequent children (if any) and c) total number of children in the adopted sample. Note that as we match on whole years to subsequent children, the weighted biological sample does not match exactly when measuring years continuously.

Table 2: Child Penalties in Biological vs Adoptive Families

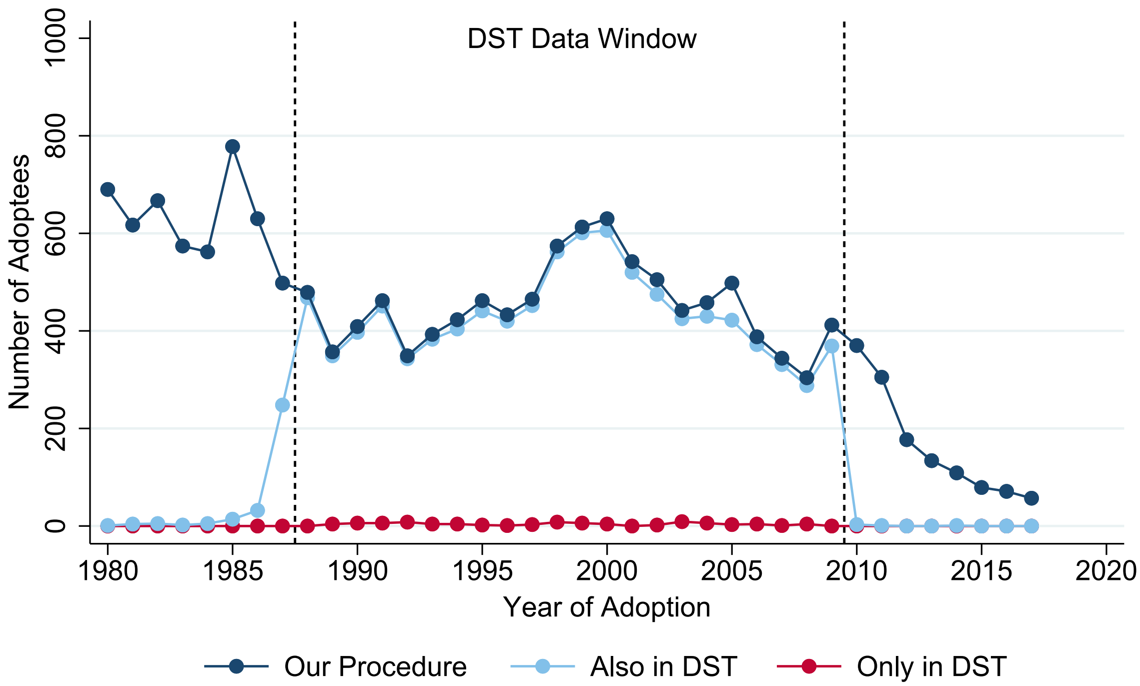
	Earnings	**Hours**	**Wage Rate**	**Participation**	**Participation x Earnings**
Panel A: Biological vs Adoptive Families					
Biological	-0.173	-0.070	-0.095	-0.087	-0.051
	(0.007)	(0.003)	(0.003)	(0.004)	(0.004)
Adoptive	-0.180	-0.076	-0.111	-0.038	-0.049
	(0.043)	(0.014)	(0.027)	(0.026)	(0.032)
Panel B: Adoptive Families by Child's Age at Adoption					
Before Age 1	-0.195	-0.084	-0.116	-0.039	0.053
	(0.047)	(0.016)	(0.030)	(0.029)	(0.037)
Between Age 1-2	-0.178	-0.064	-0.126	-0.043	-0.058
	(0.048)	(0.020)	(0.033)	(0.033)	(0.039)
After Age 2	-0.162	-0.066	-0.101	-0.038	-0.068
	(0.055)	(0.024)	(0.032)	(0.032)	(0.045)
Panel C: Biological and Adoptive Families by Relative Female Earnings Potential					
Biological Parents					
Low-Earner Female	-0.161	-0.064	-0.087	-0.096	-0.002
	(0.008)	(0.004)	(0.004)	(0.005)	(0.007)
High-Earner Female	-0.159	-0.060	-0.097	-0.050	-0.040
	(0.008)	(0.004)	(0.005)	(0.007)	(0.007)
Adoptive Parents					
Low-Earner Female	-0.162	-0.076	-0.099	-0.047	-0.010
	(0.049)	(0.018)	(0.031)	(0.030)	(0.039)
High-Earner Female	-0.198	-0.071	-0.124	-0.021	-0.080
	(0.050)	(0.023)	(0.037)	(0.038)	(0.046)

Notes: The table summaries our estimated long run child penalties from Figure 1-3. The estimations behind each panel are explained in the corresponding figure notes. The long run child penalties are computed as the average difference between the estimated impact of children (P_t) of men and women over event time 6 to 10. Standard errors are bootstrapped (500 replications).

Online Appendix (Not for Publication)

A Supplementary Figures and Tables

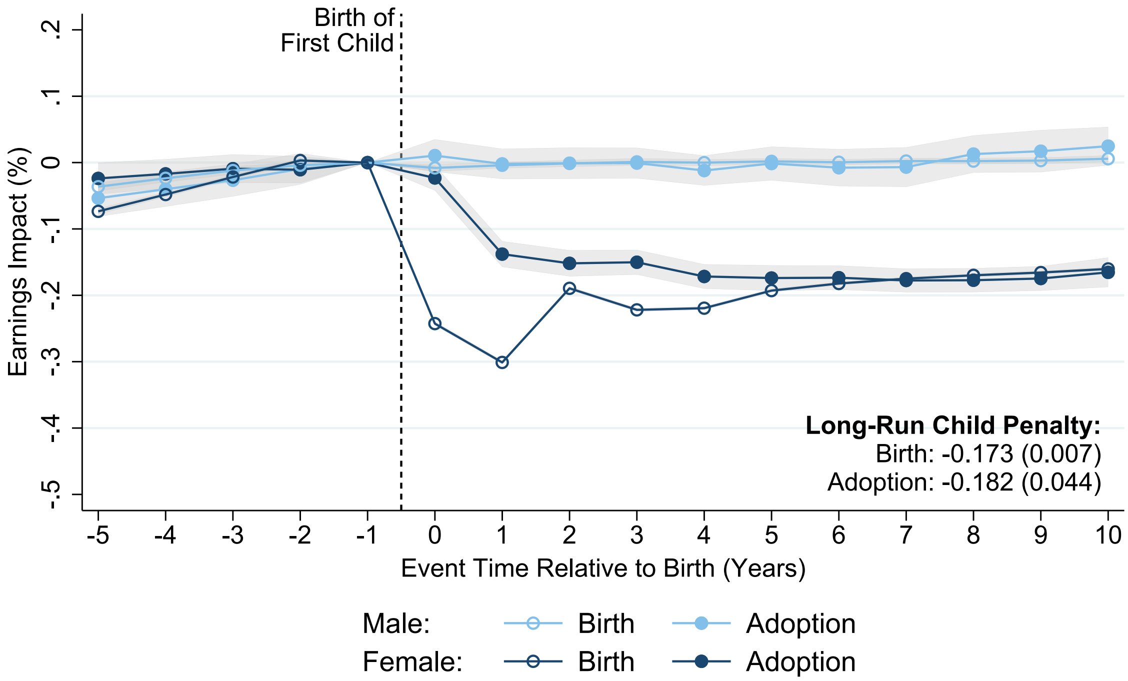
Figure A.I: Number of Foreign Adoptees: Our Procedure vs DST Registers



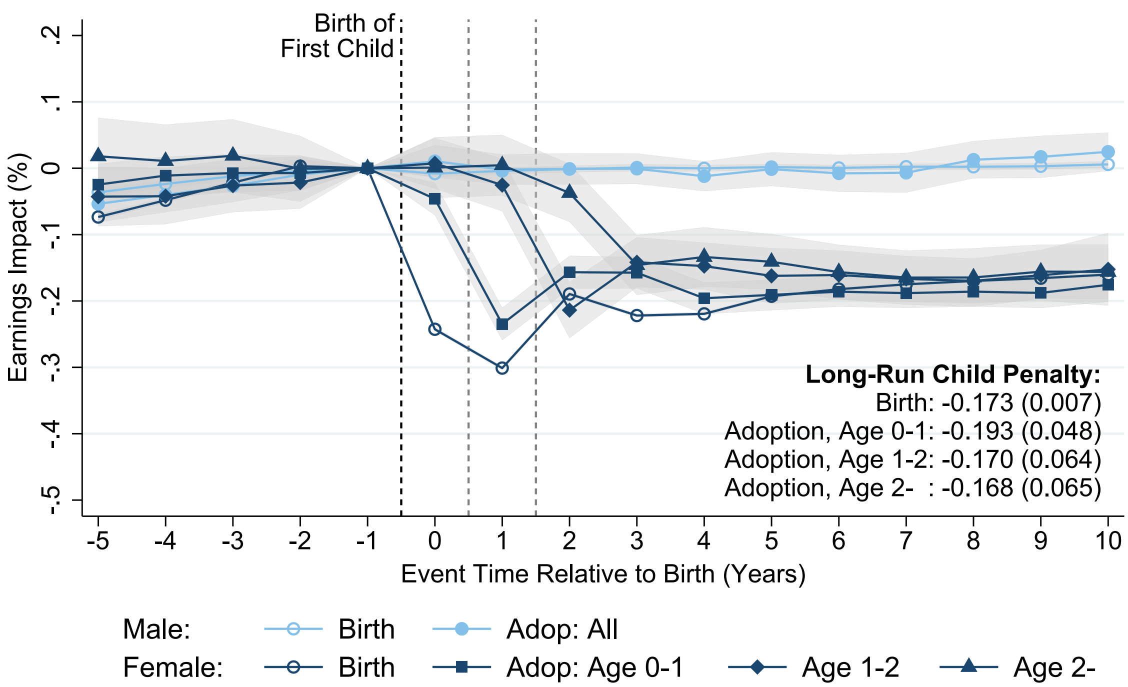
Notes: The figure compares our procedure for identifies foreign adoptees to the adoptees registered in Statistics Denmark's (DST) data available from 1988 to 2009. We identify a foreign adoptee as a child fulfilling the following conditions: 1) Born in a non-western country 2) Has two known parents, who are both born in a western country. 3) Both parents have their legal address in Denmark at the date of birth of the child (no emmigration record) 4) the child has a recorded entry (immigration record) into the Danish Central Person Register after the date of birth. Some of the adoptees, who entered Denmark prior to 1988, appear in DST's data.

Figure A.II: Child Penalties Centered on the Birth of the First Child

A: All Adoptees

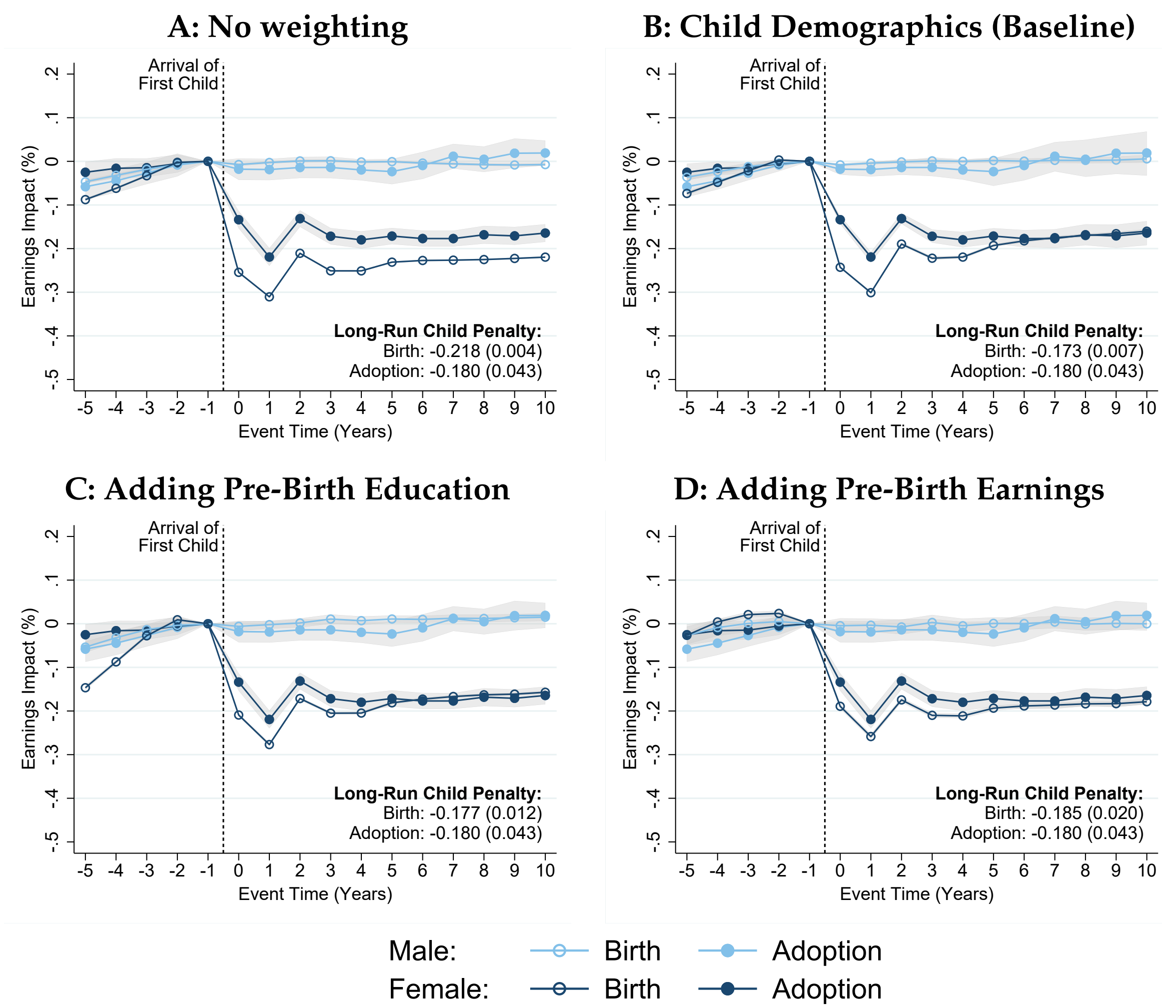


B: Adoptees by Age at Arrival



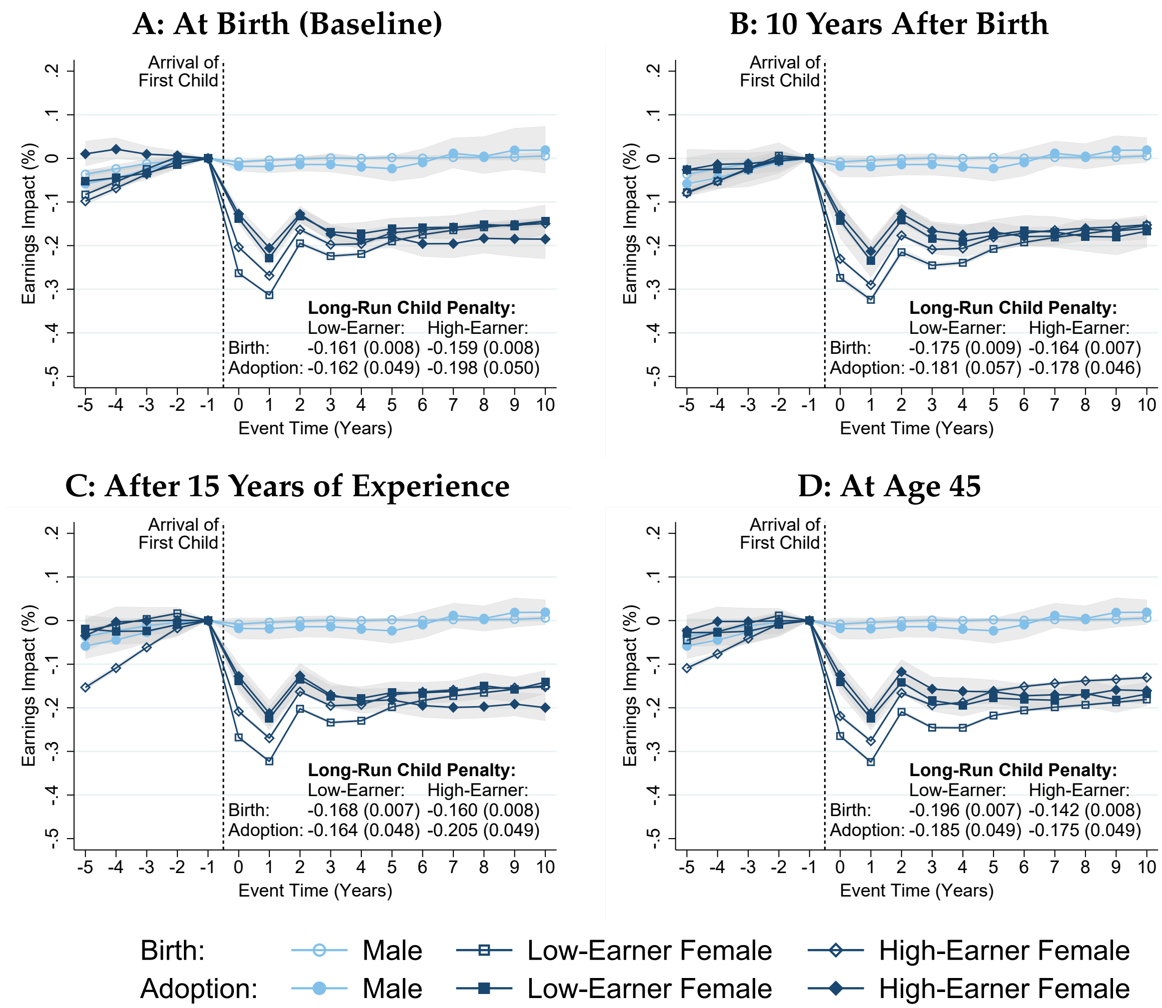
Notes: The figure replicates the estimated impact of children (P_t) on earnings of men and women in Figure 1 with event time defined relative to birth instead of the arrival of the child. For that reason, the adopted children does not arrival immediately after birth. The sample of biological parents is weighted as describe in Figure 1. Panel A shows the estimates for all adoptive couples together, while Panel B split the adopted mothers into three groups depending on the age at arrival of the first children. The grey vertical lines in Panel B indicate the latest/earliest time of arrival for the adopted children in the three age groups.

Figure A.III: Child Penalties Under Different Weighting Schemes



Notes: This figure replicates the estimated impact of children (P_t) on earnings of men and women in Figure 1, Panel A under different weighting schemes. Panel A shows the raw unweighted event studies. Panel B repeats our baseline specification in which the biological sample is reweighted to exactly the distribution of a) year of first child, b) years to subsequent children (if any) and c) the total number of children in the adoptive sample. Panel C adds pre-arrival education level (six groups measured at event time -2). Panel D adds both pre-arrival education level and pre-arrival earnings deciles (computed within birth cohort using average earnings between event time -5 to -1).

Figure A.IV: Child Penalties by When Female Earnings Potential is Measured



Notes: This figure replicates the estimated impact of children (P_t) on earnings of men and women in Figure 1, Panel A under different weighting schemes. Panel A shows the raw unweighted event studies. Panel B repeats our baseline specification in which the biological sample is reweighted to exactly the distribution of a) year of first child, b) years to subsequent children (if any) and c) the total number of children in the adoptive sample. Panel C adds pre-arrival education level (six groups measured at event time -2). Panel D adds both pre-arrival education level and pre-arrival earnings deciles (computed within birth cohort using average earnings between event time -5 to -1).

Table A.I: Number of Adoptees by Continent of Birth and Decade of Adoption

	1980-89	1990-99	2000-10	2010-17	Total
Africa	**90**	**130**	**680**	**670**	**1,570**
Asia	**5,070**	**2,690**	**2,910**	**490**	**11,160**
- China	0	300	1,230	150	1,680
- India	690	830	640	50	2,210
- South Korea	3,500	870	350	60	4,780
- Sri Lanka	550	180	20	20	760
Eastern Europe	**10**	**360**	**160**	**0**	**540**
Latin America	**680**	**1,410**	**770**	**140**	**3,000**
- Colombia	490	1,030	510	90	2,120
Total	**5,850**	**4,580**	**4,520**	**1,300**	**16,260**

Notes: The table breaks down the total number of foreign adoptees that arrived in Denmark between 1980-2017 based on their country of origin and decade of arrival. Eastern Europe includes Bulgaria, Yugoslavia, Romania, Hungary, Russia, Belarus, Romania with the majority coming from Bulgaria and Romania after the fall of the Soviet Union.

Table A.II: Sample Restrictions and Sample Sizes

	Adoptive Sample	Biological Sample
Individuals with known parents born 1980-2017	16,260	2,454,200
Only individuals born between 1985-2007	11,110	1,552,700
Only first born children	5,760	685,800
All subsequent children of same type	5,130	684,900
Arrived before age 5	4,990	684,900
Balanced on event time	**4,580**	**527,100**

Notes: The table shows how our sample restrictions affect the number of children included in the analysis starting from the total population born between 1980-2017.