Bounding the Child Penalty

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  - The New York Times: "The Gender Pay Gap Is Largely Because of Motherhood" (C. C. Miller, 2017)
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  - Business Insider: "What's the major reason a woman might get paid less than men in the same field, and with the same education? Kids." (Kaplan, 2023)
- Academic discourse:
  - "Not surprisingly, children are the main contributors to women's labor supply changes." (Goldin, 2014)
  - "The effects of parenthood (. . .) account for most of the observed gender inequality in labor market outcomes" (Kleven et al., 2023)

Identification Challenge

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Leading methods only addresses each separately

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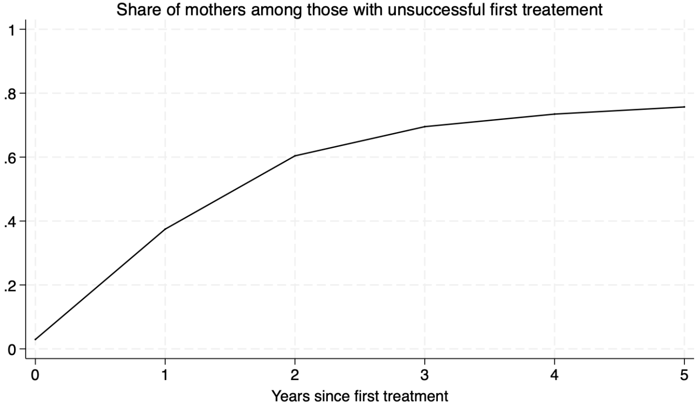
Very different results even in same samples My sample ES extern.

Motherhood Among Unsuccessfully Treated



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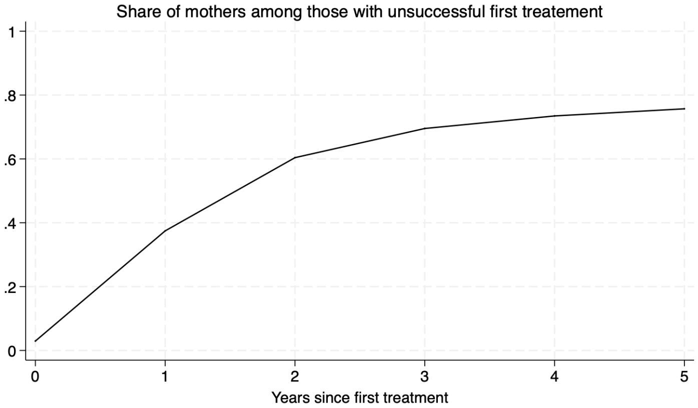

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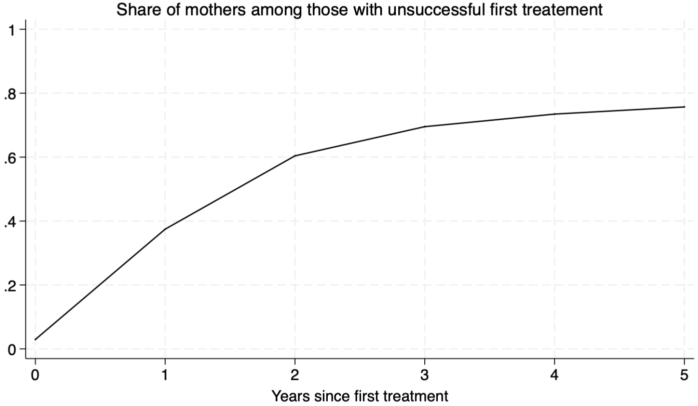

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$$\tau_{IV} = \tau_{Parenthood} - 3\tau_{Delay}$$

How much can we say about the causal effect of parenthood?

This Paper

1. Estimator that simultaneously addresses endogenous timing and dynamic effects
2. Empirical evidence using Dutch data

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- Narrowing bounds
  - Adapted DML estimator of Semenova (2020)

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For simplicity we are at $t = T$.

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$$(Y(1), Y(0), N, G) \perp D_j | P \geq j$$

- P - number of attempts
- D_j - success of attempt j

"Once sperm/eggs at attempt j are implanted, whether this results in a conception is as-good-as-random"

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Object of interest:

$$\tau_C = \mathbb{E}[Y(1) - Y(0) | N = 0]$$

Effect of motherhood for women reliant on treatments to conceive.

Counterfactuals discussion

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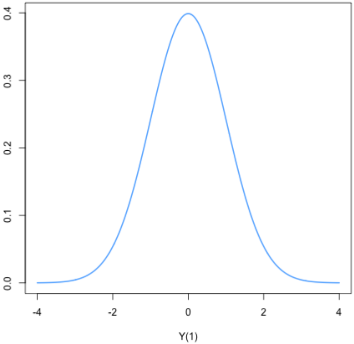
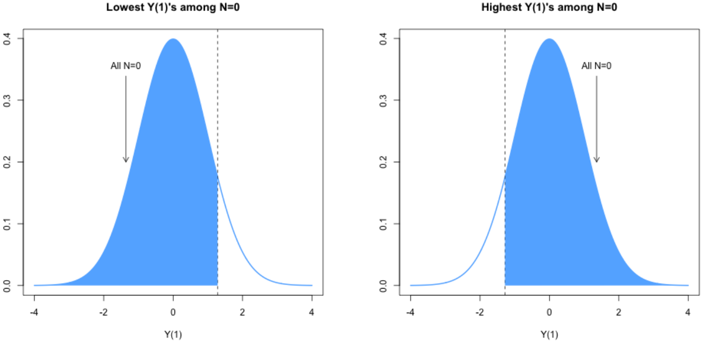
Intuition: Motherhood Outcome $Y(1)$

1. Identify distribution of motherhood outcomes using women with successful first treatment



Intuition: Motherhood Outcome $Y(1)$ (cont.)

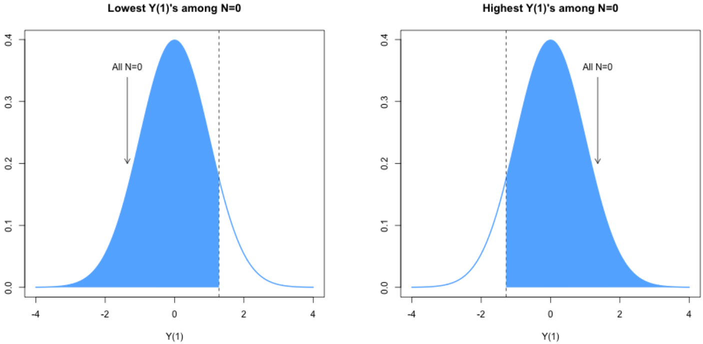
2. Estimate $\Pr(N = 0) = 0.9$ on control group
3. Assume most extreme distributions of types



Intuition: Motherhood Outcome $Y(1)$ (cont.)

4. The means of the two trimmed distributions give bounds:

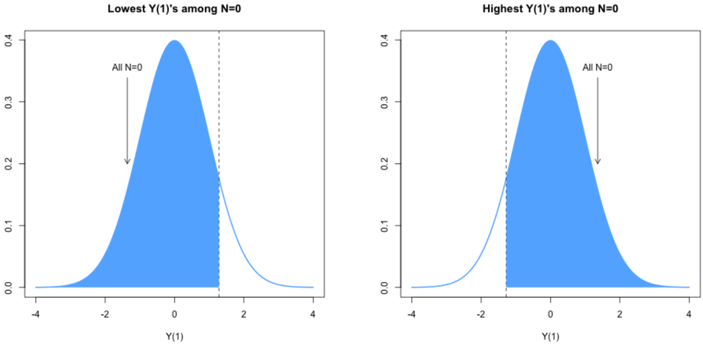
$$LB_{\mathbb{E}[Y(1)|N=0]} \leq \mathbb{E}[Y(1)|N=0] \leq UB_{\mathbb{E}[Y(1)|N=0]}$$



Intuition: Motherhood Outcome $Y(1)$ (cont.)

5. Bounds on the effect:

$$LB_{\tau_c} \leq \mathbb{E}[Y(1) - Y(0)|N = 0] \leq UB_{\tau_c}$$



Using covariates Estimator

Assisted Fertility Treatments

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- Relatively invasive procedure performed under sedation/anesthesia
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I exploit the moment embryos/sperm are transferred into the uterus

Is success as-good-as-random?

Data

- Administrative data from Statistics Netherlands
  - Data on fertility treatments from 2013 to 2017
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- Sample of opposite sex couples cohabiting before first IUI attempt

Balance Treatment success Success prob. change

Results

Bounds

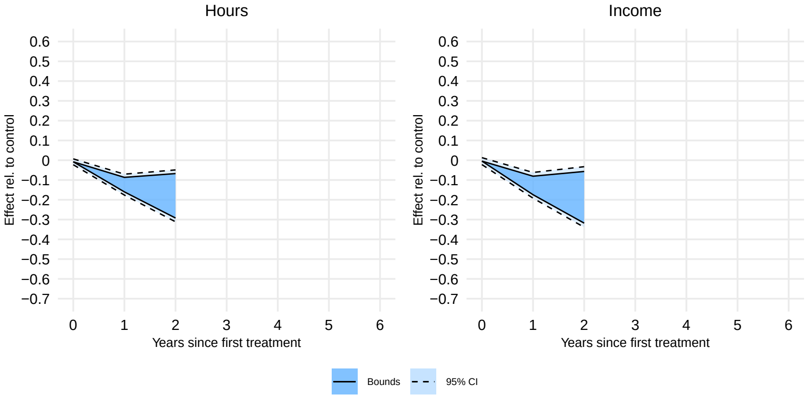

Figure 1: Bounds - short run

Baseline Lee bounds Absolute effects

Bounds

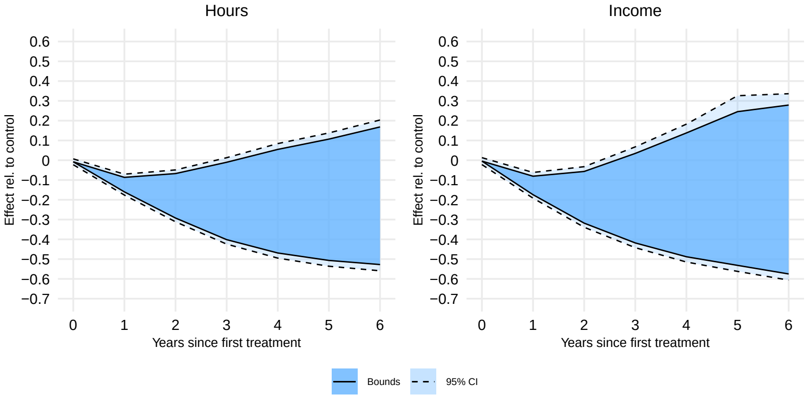

Figure 2: Bounds - medium run

Baseline Lee bounds Absolute effects

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- It may be reasonable to assume that they would have eventually conceived at least one child if all treatments had failed

Plausibility discussion Benefit of monotonicity Graphic intuition

Monotone Bounds

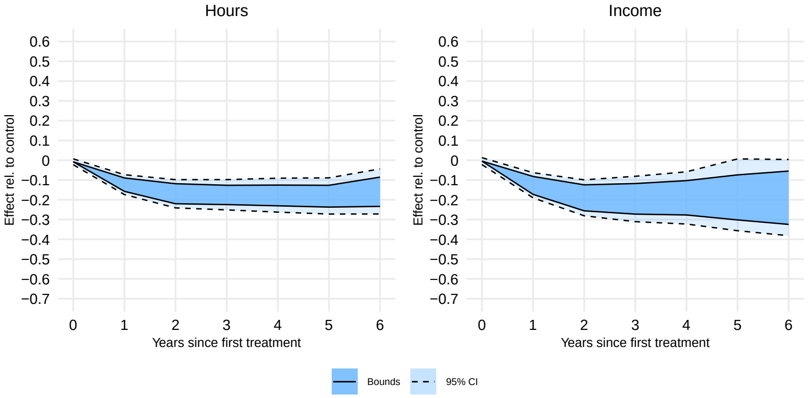

Figure 3: Monotone bounds for percent effects

Absolute How wide?

Monotone Bounds: Explaining Gender Inequality

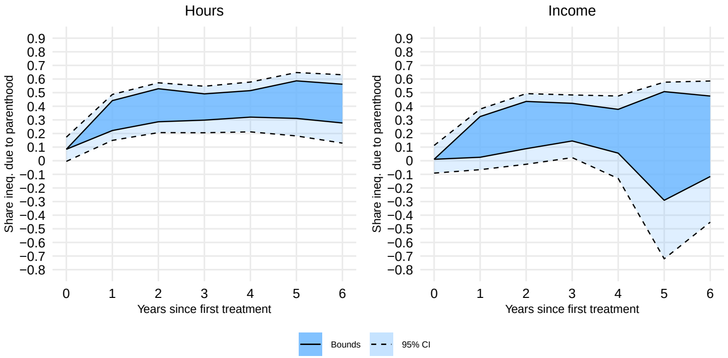

Figure 4: Share of gender inequality explained by parenthood

Extensions

Outcomes:

- Fatherhood Absolute Percentage
- Decomposing gender inequality Share explained by children
- Effect of delaying motherhood Absolute Cumulative

Existing estimators:

- Are existing estimates biased? IV-IVF equivalent Placebo event Placebo ineq.
- Are estimates less informative that existing? Confidence intervals

Robustness:

- Bias due to depression Counterfact. Depr. effect Bounds non-depr. Arguments
- Correcting for parental leave Max leave
- Inequality correcting for age De-aging partners
- Stable complier group Childless final period
- Estimator without DML Identification Effects
- Relaxing monotonicity Direction Partnered only

Other:

- Heterogeneity Covariates Willingness to try
- Population imputation* ES pop. Mother. imp. Childless imp. Effect imp. Gap

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  - Drives up to 60% (50%) of gender inequality in post-child work hours (earnings)

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  - IV-IVF might overstate penalty in the short run
  - ES might overstate penalty in both short and medium run

Appendix

Related Literature

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Main methodological ideas closely related to:

- Van den Berg & Vikström (2022): sequential treatment assignment.
- Lee (2005); Zhang & Rubin (2003): bounds with missing data.

Back Literature

Bensnes et al. (2023); Gallen et al. (2023)

Main idea:

1. Estimate effect in first period after treatment (while there are no later-mothers)
2. For individuals who are treated in second period, plug in estimate from the first
3. Repeat for all periods . . .

Required (intuitive) assumptions:

1. Effect must be similar between women who do and who do not enter motherhood later
2. Effect cannot vary over the life-cycle

Back

ToC

Literature (cont.)

Parenthood, labor market outcomes, and gender equality:

- Event studies: Angelov et al. (2016); Chung et al. (2017); Kleven, Landais, Posch, et al. (2019); Eichmeyer & Kent (2022),
- Abortion access: A. R. Miller (2011); Brooks & Zohar (2021)
- Infertility: Agüero & Marks (2008); Cristia (2008)
- Sibling sex mix: Angrist & Evans (1996); Iacovou (2001); Cruces & Galiani (2007); Maurin & Moschion (2009); Hirvonen (2009)
- Twins: Rosenzweig & Wolpin (1980); Bronars & Grogger (1994); Jacobsen et al. (1999); Vere (2011)
- Miscarriages: Hotz et al. (2005)

Methodological paper on dynamic compliance and treatment effects:

- Heckman et al. (2016); Han (2021); Van den Berg & Vikström (2022)

Back

Naive Comparison: IV vs ES

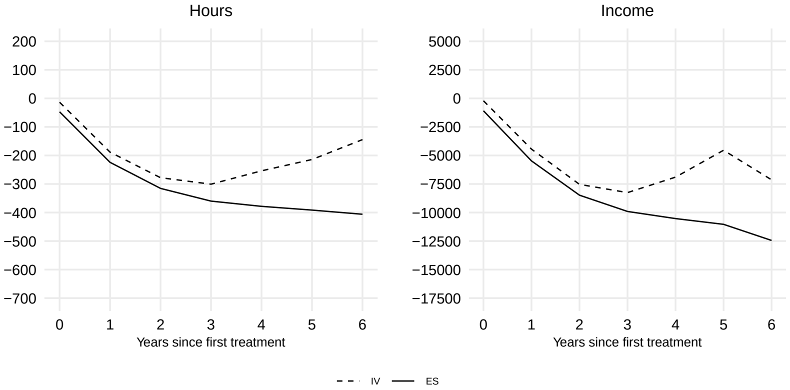

Figure 5: Comparison of IV and ES estimators using main sample

Back

ToC

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ToC

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Extrapolation requires carefully addressing mental health consequences of failure (and medical procedures)

Back (model) Back (extensions) Depr. effect Bounds non-depr. Arguments

ToC

Identification Intuition (cont.)

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Step 0:

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ToC

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3. Bounds on $\mathbb{E}[Y(1)|N=0]$

ToC

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Graph int. Coins Det. int Trimming int. Back

ToC

Estimator Intuition: Math with Coins

- Each individuals flips a coin once
- Some may chose to flip again if heads come up
- Number of flips (P) observed
- Y only revealed for those who never flip heads

$$\mathbb{E}[Y] = \mathbb{E}\left[\frac{1}{(1/2)^P} Y \, \mathbf{1}\{\text{no heads}\}\right]$$

Back

ToC

Formal Identification

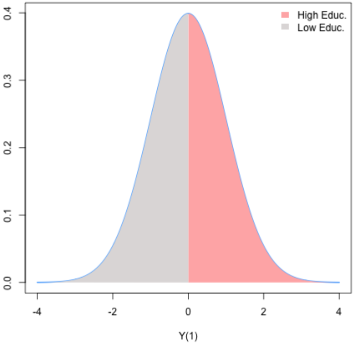
$$
\begin{aligned}
\Delta_L &= \mu_L - \mu_C \\
\Delta_U &= \mu_U - \mu_C \\
\mu_C &= \mathbb{E}\left[\frac{Y}{\prod_j^P (1 - p_j(X_j))}\middle| \mathbf{1}_{Child} = 0\right] \mathbb{E}\left[\prod_j^P (1 - p_j(X_j))\middle| \mathbf{1}_{Child} = 0\right] \\
\mu_L &= \mathbb{E}\left[\frac{Y}{p_1(X_1)}\middle| D_1 = 1, Y < y(1-s)\right] \mathbb{E}\left[p_1(X_1) | D_1 = 1, Y < y(1-s)\right] \\
\mu_U &= \mathbb{E}\left[\frac{Y}{p_1(X_1)}\middle| D_1 = 1, Y > y(s)\right] \mathbb{E}\left[p_1(X_1) | D_1 = 1, Y > y(s)\right] \\
y(q) &= G^{-1}(q) \\
G(q) &= \mathbb{E}\left[\frac{\mathbf{1}(Y \leq q)}{p_1(X_1)}\middle| D_1 = 1\right] \mathbb{E}[p_1(X_1) | D_1 = 1] \\
s &= \mathbb{E}\left[\frac{\mathbf{1}_{Child}}{\prod_j^P (1 - p_j(X_j))}\middle| W = 0\right] \mathbb{E}\left[\prod_j^P (1 - p_j(X_j))\middle| W = 0\right],
\end{aligned}
$$

where $W = 1 - \prod_{j=1}^P (1 - D_j)$.

Back

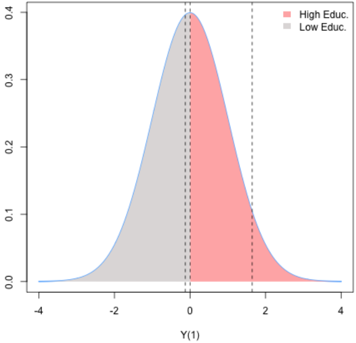
Tightening Bounds with Covariates

1. Separate distribution of motherhood outcomes into low and high education groups



Tighting Bounds with Covariates (cont.)

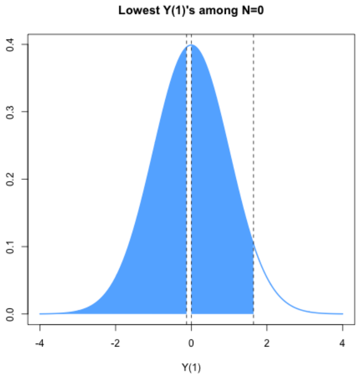
2. Estimate $\Pr(N = 0|high) = 0.9$ and $\Pr(N = 0|low) = 0.9$ on control group
3. Assume most extreme distribution of types within educ. groups



Tightening Bounds with Covariates (cont.)

4. The mean of the trimmed distribution gives new lower bound

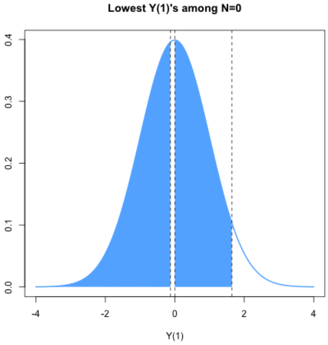
$$\mathbb{E}_{educ}[LB_{\mathbb{E}[Y(1)|N=0]}(educ)] \leq \mathbb{E}[Y(1)|N=0]$$

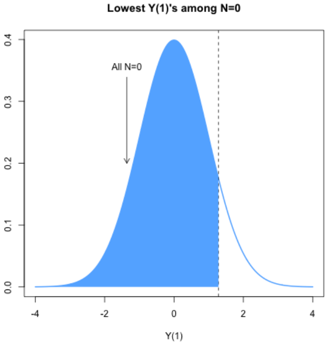


Comparing the Bounds

Conditional lower bounds is higher than unconditional:

$$\mathbb{E}_{educ}[LB_{\mathbb{E}[Y(1)|N=0]}(educ)] \geq LB_{\mathbb{E}[Y(1)|N=0]}$$





Comparing the Bounds (cont.)

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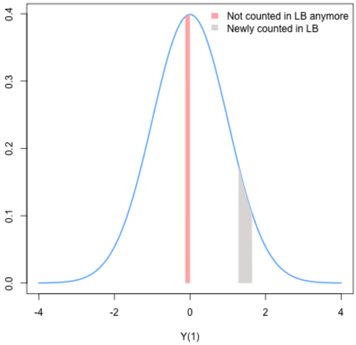

Estimation

$$m_L(data, \eta_0) = \frac{D_1}{e_1(X_1)} Y 1_{\{Y < q_1(s_0(X_1), X_1)\}} - \Pi_{j=1}^{P} \frac{1 - D_j}{1 - e_j(X_j)} SY$$

ToC

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\eta_0 &= \{s_0(x), q_1(u, x), e_j(x)\} \\
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$$\frac{1}{\sqrt{n}} \sum_i (m_L(data_i, \widehat{\eta}) - \mathbb{E}[m_L(data_i, \eta_0)]) \xrightarrow{d} ?$$

ToC

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$$\frac{1}{\sqrt{n}} \sum_i (g_L(data_i, \eta_0) - \mathbb{E}[m_L(data_i, \eta_0)]) \xrightarrow{d} N(0, \sigma^2)$$

Narrowing the Bounds with Covariates (cont.)

Alternative extension of Lee bounds (to my knowledge)

- Take some function $g(x)$
- $\mathbb{E}[g(X_1)|N = 0]$ can be identified on women who remain childless
- Take $\mathbb{E}[Y(1)|N = 0] = \mathbb{E}[g(X_1) + \varepsilon|N = 0]$
- Only need to bound $\mathbb{E}[\varepsilon|N = 0]$
- $g(X_1)$ can be directly chosen to minimize the spread in residuals, e.g. OLS of Y on X_1 for women with $D_1 = 1$.
- Since residuals typically have a narrower distribution, this gives narrower bounds.
- Also, likely to improve asymptotic efficiency: only need to compare residuals in both groups, $\mathbb{E}[g(X_1)|N = 0]$ is the same for treated and control.

Back (DML) Back (extensions)

ToC

Randomness in IUI and IVF

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Other (medical) factors relevant for IVF success appear to be largely unrelated to labor market outcomes (Lundborg et al., 2017).

Back

Treatment Success

- I impute treatment success as having a child born within 10 months after treatment without subsequent treatments in between.
  - Validated to reflect medical records of treatment success (Lundborg et al., 2017).

Back

Balance

Table 1: First treatment outcomes and descriptives

	Success (1)	Fail (2)	Difference (1)-(2)	Dif. cond. age FE (1)-(2) cond. age
Work (W)	0.881	0.863	0.018	0.008
	[0.324]	[0.344]	(0.009)	(0.009)
Work (P)	0.884	0.865	0.019	0.014
	[0.320]	[0.341]	(0.009)	(0.009)
Hours (W)	1239.696	1208.255	31.441	17.578
	[605.070]	[634.840]	(16.168)	(15.812)
Hours (P)	1473.383	1438.880	34.502	22.690
	[658.917]	[695.345]	(17.699)	(17.587)
Income 1000s € (W)	28.049	27.434	0.615	0.942
	[19.559]	[20.232]	(0.516)	(0.496)
Income 1000s € (P)	37.173	36.959	0.214	0.896
	[26.484]	[29.443]	(0.746)	(0.732)
Bachelor deg. (W)	0.608	0.605	0.002	0.018
	[0.488]	[0.489]	(0.013)	(0.012)
Bachelor deg. (P)	0.593	0.598	-0.004	0.008
	[0.491]	[0.490]	(0.013)	(0.012)
Age (W)	31.643	32.384	-0.741	
	[4.016]	[4.383]	(0.111)	
Age (P)	34.672	35.459	-0.787	
	[5.527]	[5.993]	(0.152)	
Observations	1,716	13,788		
Joint *p*-val.			0.000	0.536

Note: Labor market outcomes measured year before first treatment. (W) - woman, (P) - partner. Standard deviations in brackets. Standard errors in parentheses.

Balance in Later Treatments

Table 2: Balance in later treatments

	D2	D3	D4	D5	D6	D7	D8	D9	D10
Work (W)	0.013	-0.002	0.023	0.008	0.030	0.007	-0.008	0.016	0.041
	(0.009)	(0.010)	(0.011)	(0.012)	(0.013)	(0.014)	(0.017)	(0.019)	(0.026)
Work (P)	0.011	0.014	0.005	0.014	-0.004	-0.008	0.001	0.016	0.040
	(0.010)	(0.010)	(0.011)	(0.012)	(0.013)	(0.014)	(0.017)	(0.020)	(0.027)
Hours (W)	37.050	-0.615	45.477	39.327	68.596	25.780	-5.734	81.149	29.860
	(17.373)	(18.648)	(20.127)	(21.930)	(24.489)	(26.043)	(31.176)	(36.869)	(49.101)
Hours (P)	29.074	28.347	18.441	35.597	-7.332	-15.344	0.360	47.511	49.279
	(19.336)	(20.807)	(22.614)	(24.685)	(27.215)	(28.618)	(34.381)	(41.158)	(55.440)
Income 1000s € (W)	1.786	0.283	1.123	1.672	1.380	0.489	0.417	1.839	-0.297
	(0.548)	(0.592)	(0.647)	(0.710)	(0.786)	(0.831)	(1.030)	(1.240)	(1.714)
Income 1000s € (P)	0.221	1.277	1.588	1.125	-0.542	-0.370	1.567	1.001	-0.202
	(0.820)	(0.846)	(0.923)	(1.018)	(1.123)	(1.212)	(1.423)	(1.666)	(2.277)
Bachelor deg. (W)	0.002	0.026	-0.020	0.001	-0.003	0.003	0.023	-0.012	0.045
	(0.013)	(0.014)	(0.015)	(0.017)	(0.019)	(0.020)	(0.024)	(0.028)	(0.038)
Bachelor deg. (P)	0.005	0.010	0.011	0.007	-0.003	0.013	0.020	0.012	-0.014
	(0.013)	(0.014)	(0.016)	(0.017)	(0.019)	(0.020)	(0.024)	(0.029)	(0.039)
Age (W)	0.001	-0.007	-0.040	0.024	0.013	-0.001	-0.046	-0.027	-0.017
	(0.011)	(0.015)	(0.019)	(0.023)	(0.026)	(0.028)	(0.036)	(0.043)	(0.059)
Age (P)	0.001	-0.007	-0.040	0.024	0.013	-0.001	-0.046	-0.027	-0.017
	(0.011)	(0.015)	(0.019)	(0.023)	(0.026)	(0.028)	(0.036)	(0.043)	(0.059)
Observations	12,955	10,759	8,714	6,969	5,403	3,938	2,718	1,848	1,173
Joint *p*-val.	0.071	0.737	0.057	0.439	0.420	0.991	0.836	0.508	0.437

Note: Each column describes the differences between those treated successfully and those treated unsuccessfully at the respective treatment attempt conditional on fixed effects for age-at-treatment interacted with fixed effects for treatment type. Labor market outcomes and age measured year before first treatment. (W) - woman, (P) - partner. Standard errors in parentheses.

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ToC

Representative and Relevant Treatment group

Table 3: Full sample, relevant sample, and representative sample

	Success (1)	Fail (2)	Relevant (3)	Representative (4)	Success vs rep. (1)-(4)	Rel. vs rep. (3)-(4)
Work (W)	0.881	0.863	0.822	0.801	0.080	0.021
	[0.324]	[0.344]	[0.334]	[0.399]	(0.010)	(0.005)
Work (P)	0.884	0.865	0.850	0.783	0.101	0.066
	[0.320]	[0.341]	[0.344]	[0.412]	(0.010)	(0.005)
Hours (W)	1239.696	1208.255	1120.310	1071.721	167.975	48.589
	[605.070]	[634.840]	[583.894]	[697.609]	(16.879)	(8.254)
Hours (P)	1473.383	1438.880	1392.628	1245.385	227.998	147.243
	[658.917]	[695.345]	[663.323]	[793.411]	(19.197)	(9.376)
Income 1000s € (W)	28.049	27.434	24.925	20.903	7.146	4.021
	[19.559]	[20.232]	[15.086]	[17.981]	(0.435)	(0.213)
Income 1000s € (P)	37.173	36.959	35.002	27.544	9.630	7.459
	[26.484]	[29.443]	[23.998]	[28.685]	(0.694)	(0.339)
Bachelor deg. (W)	0.608	0.605	0.591	0.576	0.032	0.015
	[0.488]	[0.489]	[0.414]	[0.494]	(0.012)	(0.006)
Bachelor deg. (P)	0.593	0.598	0.582	0.554	0.040	0.029
	[0.491]	[0.490]	[0.416]	[0.497]	(0.012)	(0.006)
Age (W)	31.643	32.384	33.284	28.384	3.259	4.900
	[4.016]	[4.383]	[3.892]	[4.648]	(0.112)	(0.055)
Age (P)	34.672	35.459	36.327	28.384	6.288	7.943
	[5.527]	[5.993]	[3.924]	[4.655]	(0.113)	(0.055)
Observations	1,716	13,788	5,103	374,812		

Note: Labor market outcomes measured year before first treatment for main sample and year and 9 months before birth of first child for represenstative sample. Representative sample selected to match main sample by year of conception. Relevant sample consists of women in the main sample who remain childless weighted to account for differences in the probability to remain childless. (W) - woman, (P) - partner. Standard deviations in brackets. Standard errors in parentheses.

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Predicted Success Prob. per Treatment

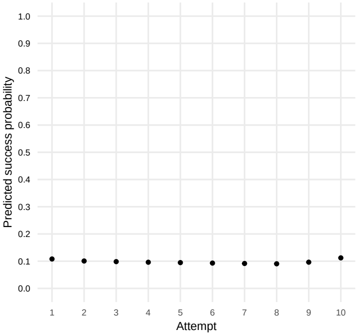

Figure 6: Predicted success probability holding X fixed at first attempt average

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Attempts

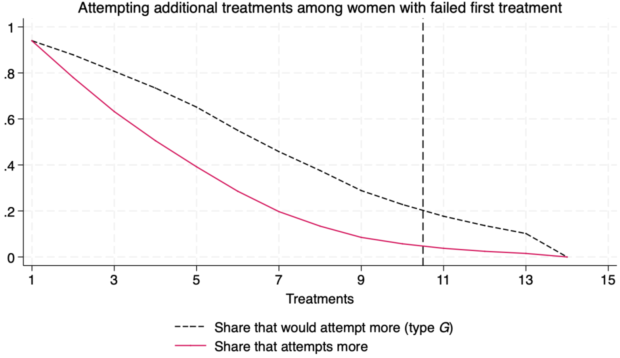

Figure 7: Number of treatments and type

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ToC

Non-treatment Conception by Type

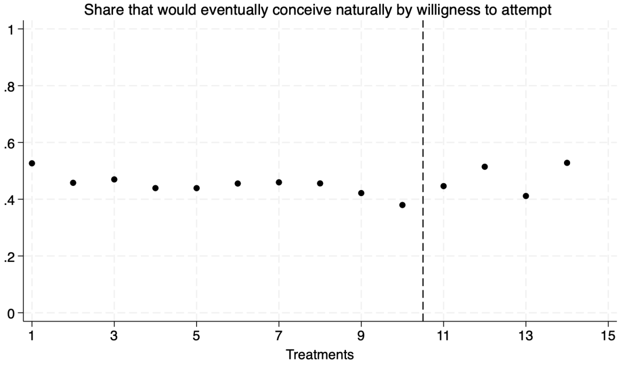

Figure 8: Conceiving naturally and willingness to attempt

Back

Trimming shares

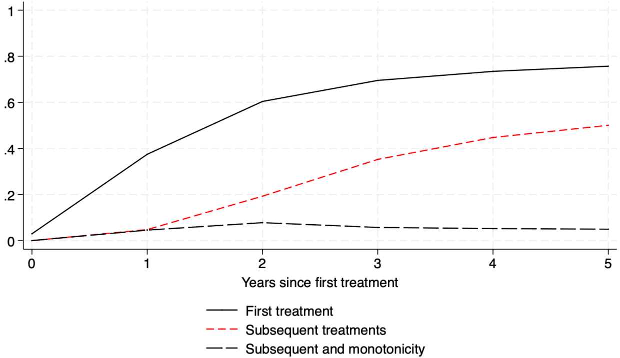

Figure 9: Trimming share under different information

Back

Correction Term

$$
\begin{aligned}
corr_L(data, \eta_0) &= q_1(s_0(X_1), X_1)\Pi_{j=1}^{P} \frac{1-D_j}{1-e_j(X_j)}(S - s(0, X_1)) \\
&- q_1(s_0(X_1), X_1)\frac{D_1}{e_1(X_1)}(1_{\{Y<q_1(s_0(X_1),X_1)\}} - s_0(X_1)) \\
&- \frac{D_1 - e_1(X_1)}{e_1(X_1)} z_L^{+}(1, X_1)s(0, X_1) \\
&+ \sum_k \mathbb{1}_{P\geq j}\Pi_j^{k-1}\frac{1-D_j}{1-e_j(X_j)}\frac{e_k(X_k)-D_k}{1-e_k(X_k)}(s_k(0, X_k)\beta_k(0, X_k)) \\
&+ \sum_k \mathbb{1}_{P\geq j}\Pi_j^{k-1}\frac{1-D_j}{1-e_j(X_j)}\frac{e_k(X_k)-D_k}{1-e_k(X_k)} q_1(s_0(X_1), X_1)(s(0, X_1) - s_k(0, X_k))
\end{aligned}
$$

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Bounds: Absolute

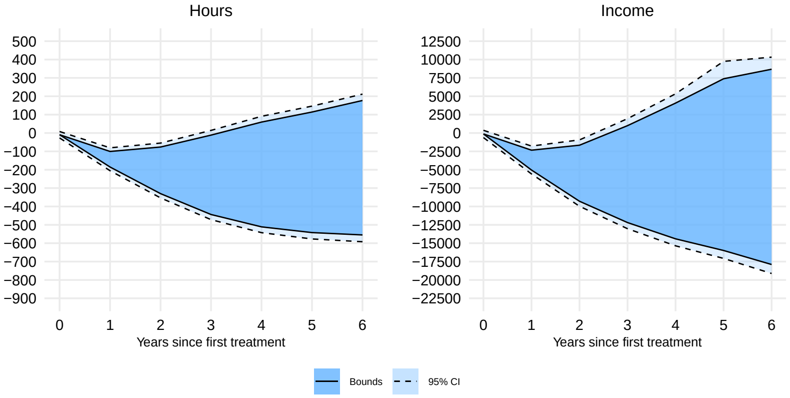

Figure 10: Bounds effects

Back

ToC

Bounds: Hours - Comparison to Baseline Lee Bounds

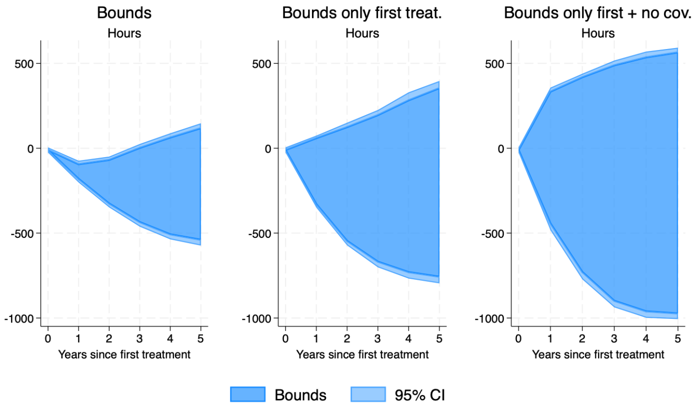

Figure 11: Comparison with baseline Lee: hours

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ToC

Bounds: Income - Comparison to Baseline Lee Bounds

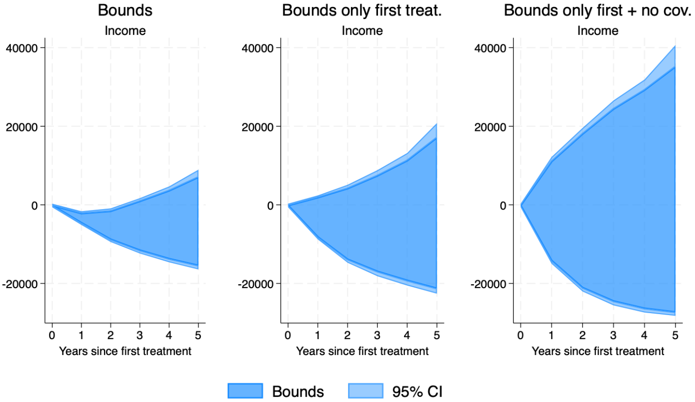

Figure 12: Comparison with baseline Lee: income

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ToC

Monotonicity (cont.)

Is monotonicity realistic?

Monotonicity (cont.)

Is monotonicity realistic?

- Yes, if families are determined to have at least one child.
  - Decreasing marginal returns to children.
  - Stronger sufficient assumption: success cannot increase total (natural) births.

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- Yes, if families are determined to have at least one child.
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- No, if first treatment success increases the likelihood of attempting to conceive naturally.

Monotonicity (cont.)

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  - Stronger sufficient assumption: success cannot increase total (natural) births.
- No, if first treatment success increases the likelihood of attempting to conceive naturally.
  - Couples may realize they are fertile and try more.

ToC

Monotonicity (cont.)

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  - Decreasing marginal returns to children.
  - Stronger sufficient assumption: success cannot increase total (natural) births.
- No, if first treatment success increases the likelihood of attempting to conceive naturally.
  - Couples may realize they are fertile and try more.
  - First child may "save the relationship" resulting in more attempts to conceive.

Monotonicity (cont.)

Is monotonicity realistic?

- Yes, if families are determined to have at least one child.
  - Decreasing marginal returns to children.
  - Stronger sufficient assumption: success cannot increase total (natural) births.
- No, if first treatment success increases the likelihood of attempting to conceive naturally.
  - Couples may realize they are fertile and try more.
  - First child may "save the relationship" resulting in more attempts to conceive.
- Robustness: restrict to only couples that stay together Effects

Back Benefit of monotonicity Graphic intuition

ToC

Benefit of Monotonicity

- Women who conceive a second child naturally are not the women who would remain childless.

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- This restricts which women among successfully treated could have remained childless had all treatment attempts failed.

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- This restricts which women among successfully treated could have remained childless had all treatment attempts failed.
- Narrower bounds.

Intuition Back

Monotonicity: Intuition (1)

Identify distribution of motherhood outcomes from group whose first treatment succeeds:

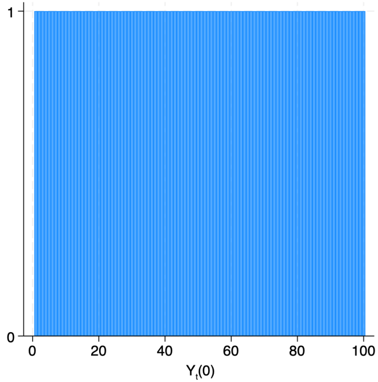

Figure 13: Distribution of potential motherhood outcomes

Back

ToC

Monotonicity: Intuition (2)

Without monotonicity: 20 with highest/lowest outcomes:

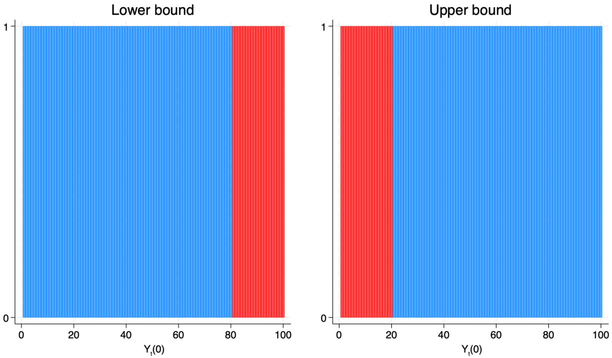

Figure 14: Distribution of potential motherhood outcomes

Back

ToC

Monotonicity: Intuition (3)

With monotonicity: first, drop 10 who conceive again naturally:

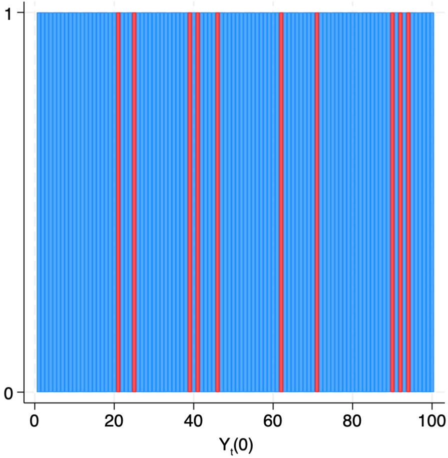

Figure 15: Distribution of potential motherhood outcomes

Monotonicity: Intuition (4)

With monotonicity: second, drop another 10 with highest/lowest outcomes:

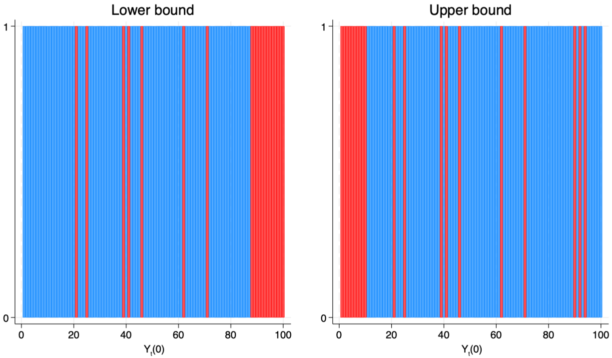

Figure 16: Distribution of potential motherhood outcomes

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Monotonicity: Intuition (5)

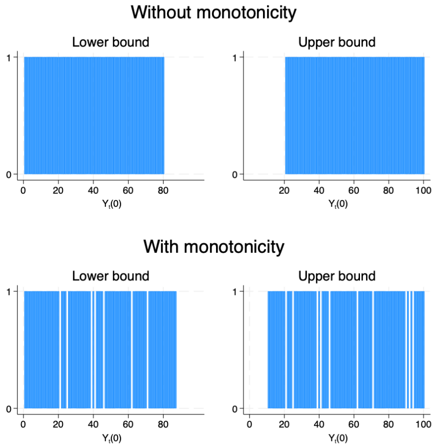

Figure 17: Distribution of potential motherhood outcomes

Monotone Bounds: Absolute

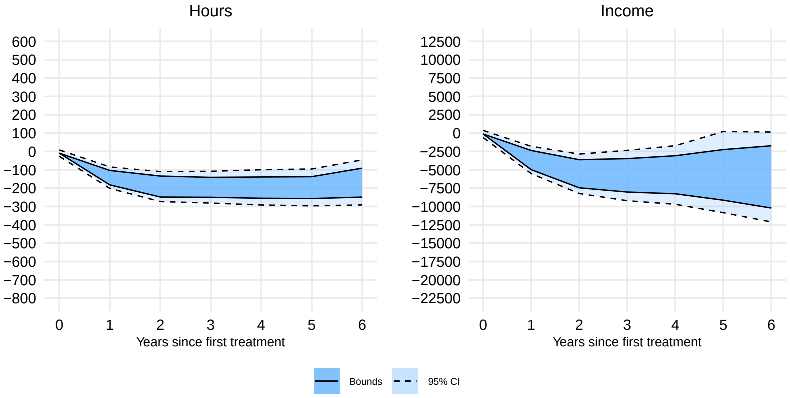

Figure 18: Monotone bounds: absolute terms

Back

How Wide are the Bounds?

6 years after first treatment:

- Bounds:
  - 1 SD of pre-treatment hours
  - 1 SD of pre-treatment earnings
- Monotone bounds:
  - 0.15 SD of pre-treatment hours
  - 0.25 SD of pre-treatment earnings

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ToC

Extensions

Outcomes:

- Fatherhood Absolute Percentage
- Decomposing gender inequality Share explained by children
- Effect of delaying motherhood Absolute Cumulative

Existing estimators:

- Are existing estimates biased? IV-IVF equivalent Placebo event Placebo ineq.
- Are estimates less informative that existing? Confidence intervals

Robustness:

- Bias due to depression Counterfact. Depr. effect Bounds non-depr. Arguments
- Correcting for parental leave Max leave
- Inequality correcting for age De-aging partners
- Stable complier group Childless final period
- Estimator without DML Identification Effects
- Relaxing monotonicity Direction Partnered only

Other:

- Heterogeneity Covariates Willingness to try
- Population imputation* ES pop. Mother. imp. Childless imp. Effect imp. Gap

ToC

Monotone Bounds: Women who Remain Childless

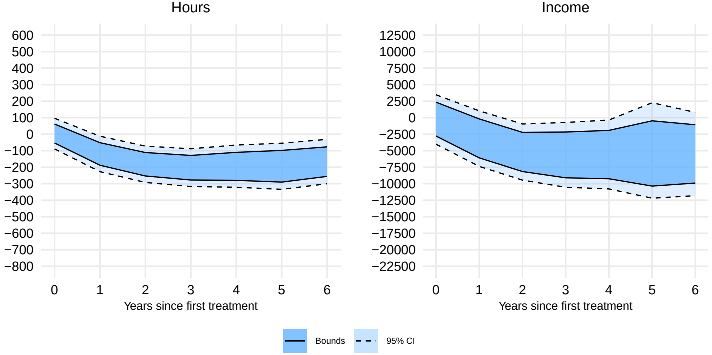

Figure 19: Monotone bounds using final status

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ToC

Event Study: Population vs IUI Sample

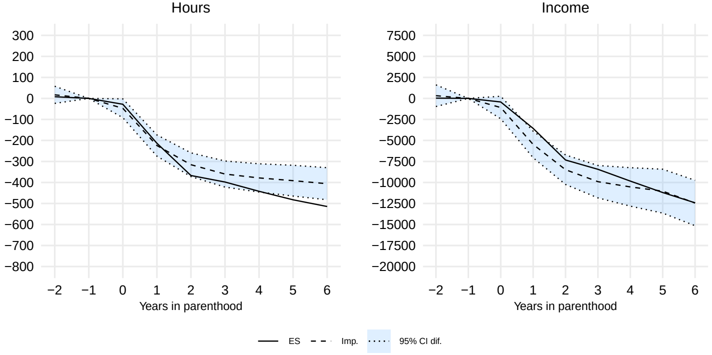

Figure 20: ES for population and women with first IUI success

Back (extensions) Back (intro)

ToC

Imputing Population Motherhood Outcomes Using IUI Sample

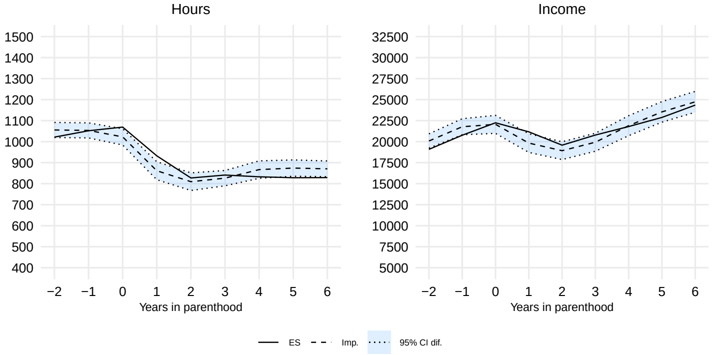

Figure 21: Population Outcomes vs IUI-imputation (age & education)

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ToC

Imputing Population Childless Outcomes Using IUI Sample

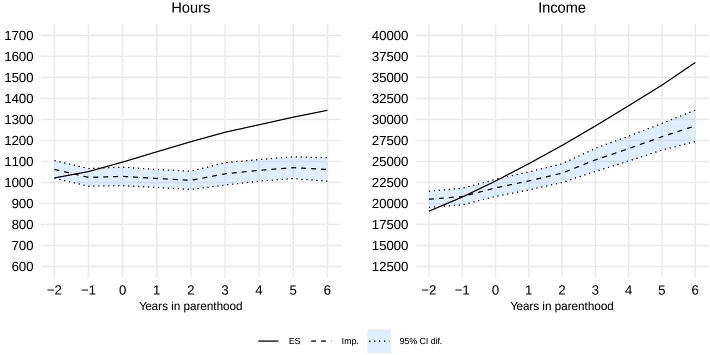

Figure 22: Population Outcomes vs IUI-imputation (age & education)

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ToC

Event Study vs IUI-imputation for Population

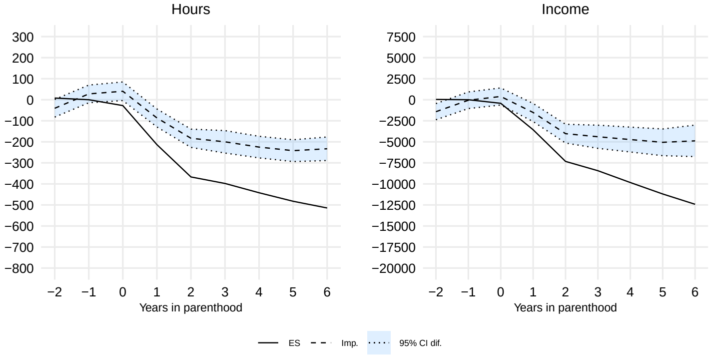

Figure 23: Event study vs IUI-imputation for population (age & education)

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ToC

Event Study vs IUI-imputation: Inequality

$$\text{Ineq. cause by children} = \frac{\tau}{\text{Ineq. w/o children} + \tau}$$

Event Study vs IUI-imputation: Inequality

$$\text{Ineq. cause by children} = \frac{\tau}{\text{Ineq. w/o children} + \tau}$$

ToC

Event Study vs IUI-imputation: Inequality

$$\text{Ineq. cause by children} = \frac{\tau}{\textcolor{red}{\text{Ineq. w/o children}} + \tau}$$

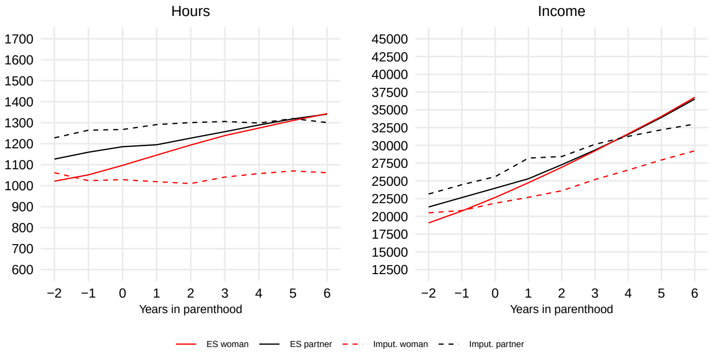

Figure 24: Event study vs IUI-imputation for population gaps (age & education), partner outcomes shifted 3 years

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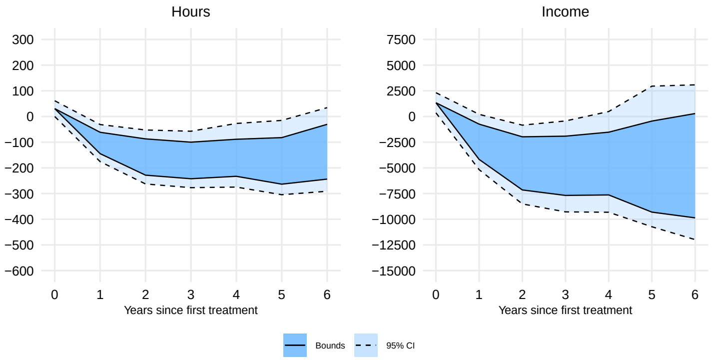
ToC

Simple estimator



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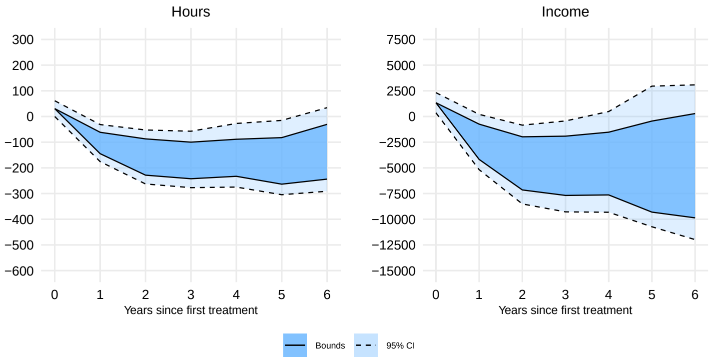
Simple estimator



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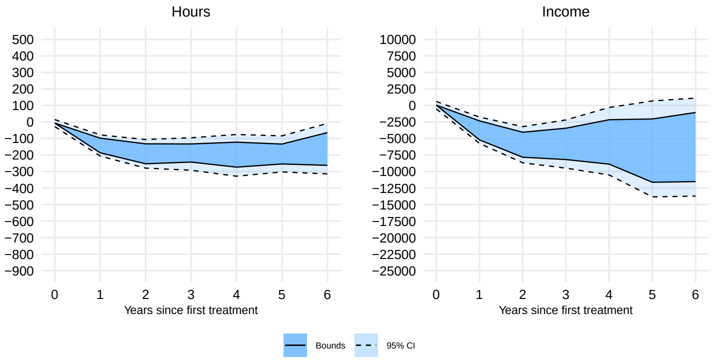
Relaxing Monotonicity Direction



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Heterogeneity by Covariates

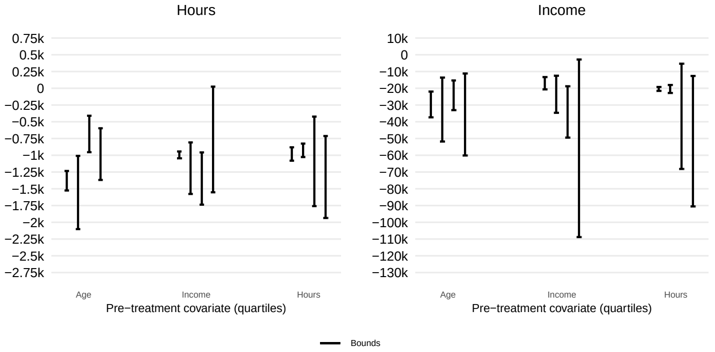

Figure 25: Cumulative outcomes after 6 years, pre-treatment covariates

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Heterogeneity by Willingness to Undergo Procedures

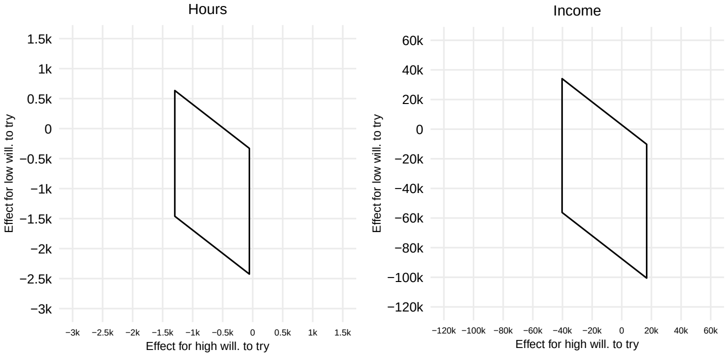

Figure 26: Cumulative outcomes 6 years after, G above or below 6

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Monotone Bounds: Excluding Depression

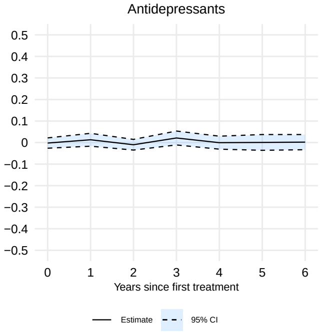

Figure 27: Sequential-IV estimates for effect on antidepressant take-up

Back (extensions) Back (model)

ToC

Monotone Bounds: Excluding Depressed

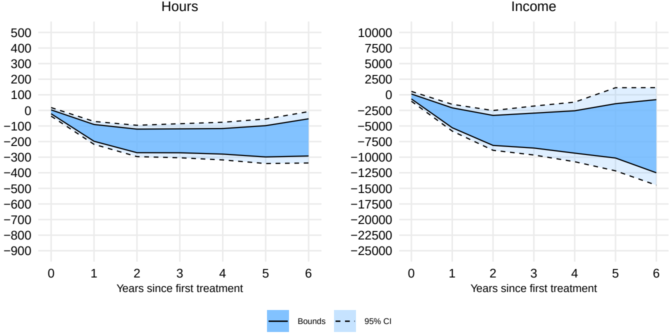

Figure 28: Monotone bounds for women who would not start antidepressants if they were to remain childless

Back (extensions) Back (model)

ToC

Arguments Regarding Mental Health

- Mental health consequences of not having a child are arguably a part of the relevant counterfactual
- IUI is significantly less invasive than IVF
- "Effects" of severe health shocks (including mental health shocks and psychical shocks that are followed by invasive treatments) are "relatively" small
- Partners' mental health might also suffer, which could alleviate concerns for inequality estimates

Back (extensions) Back (model)

ToC

Monotone Bounds: Assuming Maximum Leave

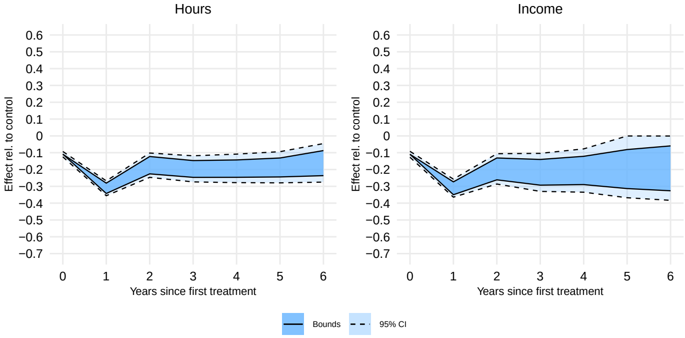

Figure 29: Monotone bounds scaling outcomes in years with childbirth by max. leave fraction

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Monotone Bounds: Correcting for Partner's age

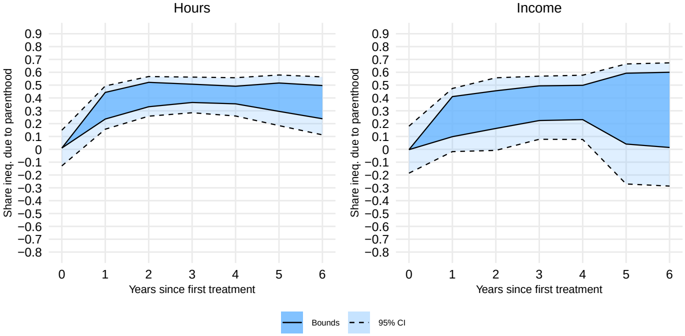

Figure 30: Monotone bounds using male income at same age as female

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Monotone Bounds: Fatherhood Penalty

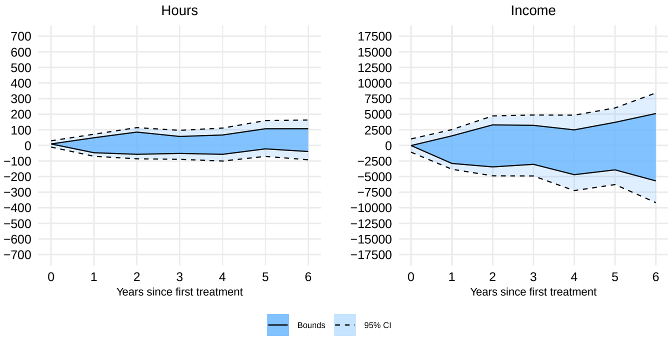

Figure 31: Monotone bounds for partners

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Monotone Bounds: Fatherhood Penalty in Percent

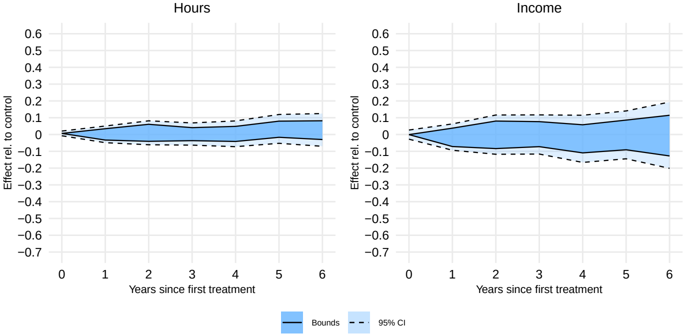

Figure 32: Monotone bounds for partners in percent

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Monotone Bounds: Explaining Gender Inequality

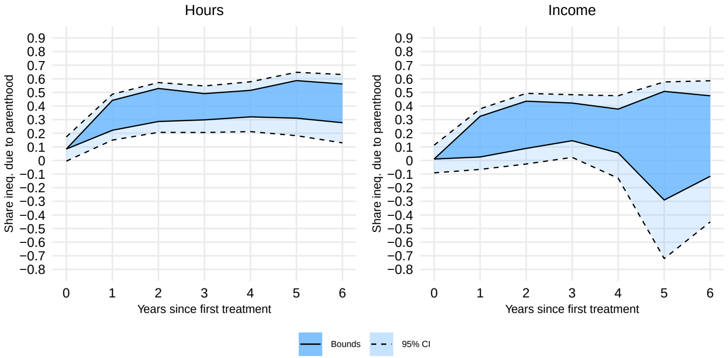

Figure 33: Share of gender inequality explained by parenthood

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Are Bounds Less Informative?

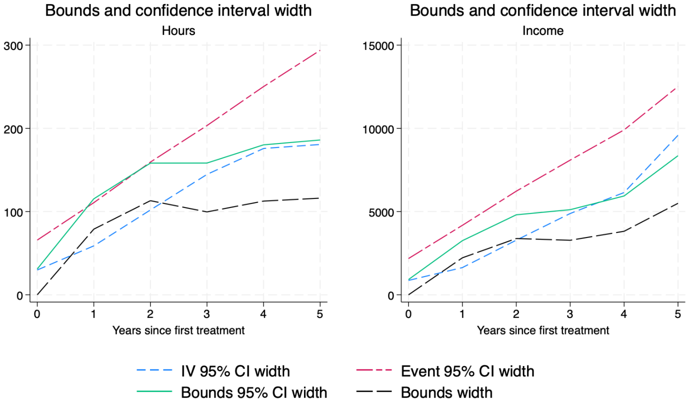

Figure 34: Confidence intervals for different methods

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Monotone Bounds and IV

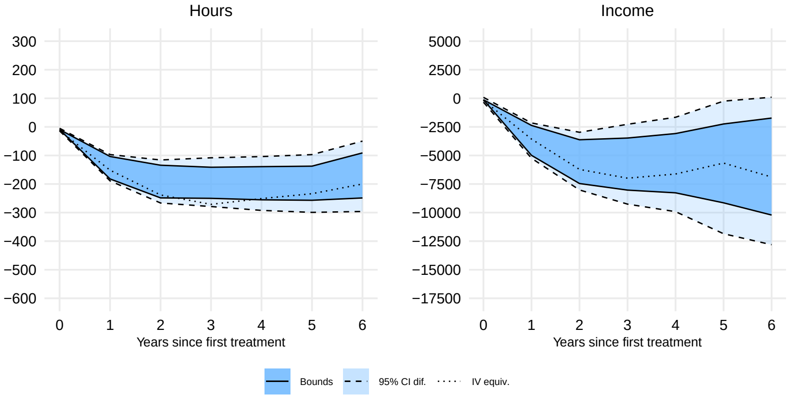

Figure 35: Bounds and IV equivalent for the same population

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Placebo Event

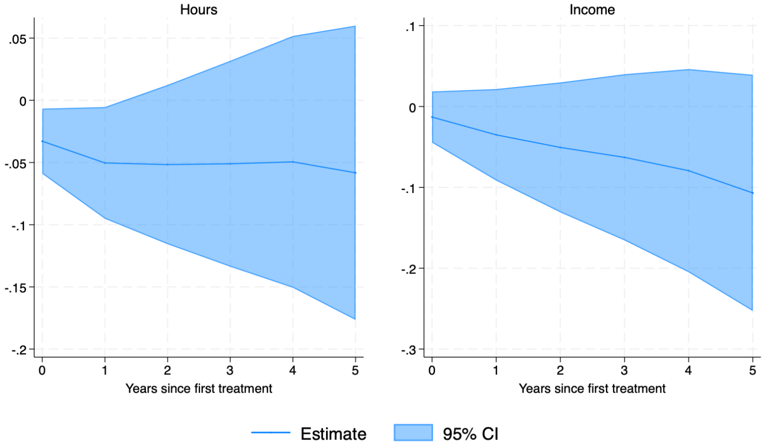

Figure 36: Placebo event study

Back Placebo as causal

ToC

Inequality treating ES bias as causal

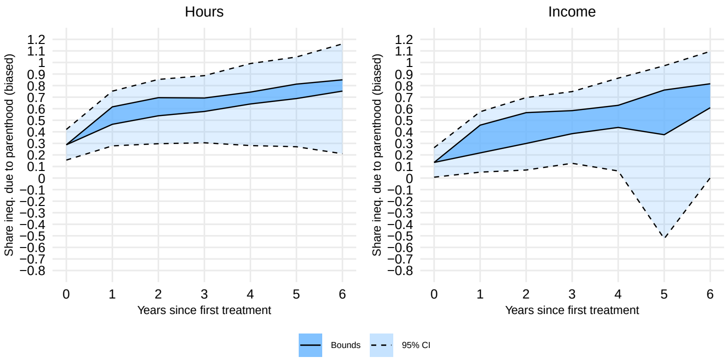

Figure 37: Placebo effects as share of bounds for the same population

Two possible interpretations:

- Event study severely overstates the penalty
- Large share of penalty is due to trying to conceive not parenthood per se

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Yearly effect of Delaying Motherhood

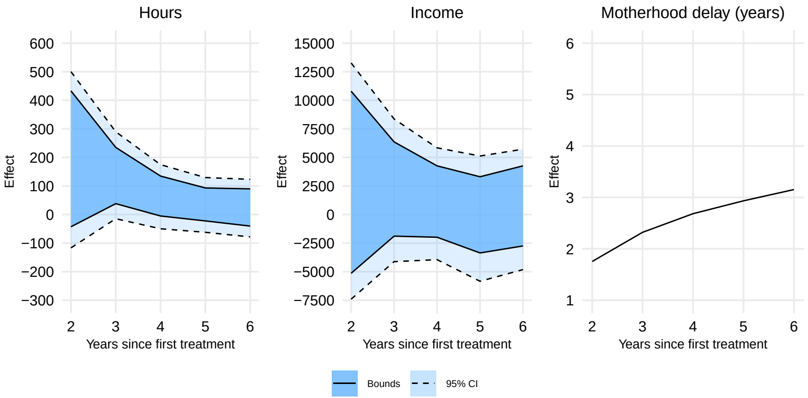

Figure 38: Effect of delaying relative to motherhood at first attempt

Opposite of what is frequently assumed!

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ToC

Cumulative effect of Delaying Motherhood

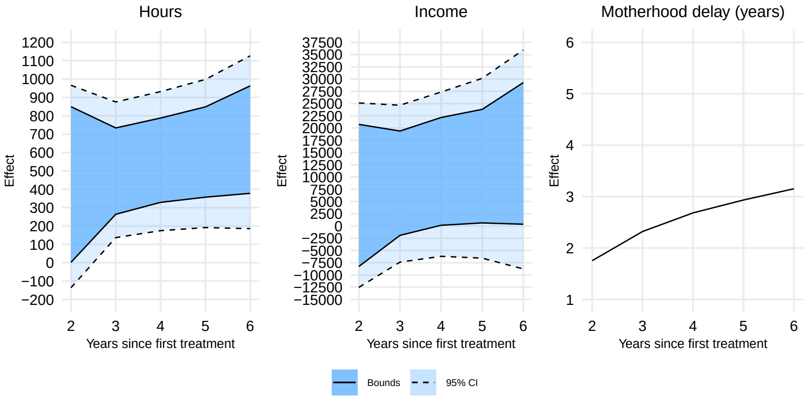

Figure 39: Effect of delaying relative to motherhood at first attempt

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Monotone Bounds: Women who Remain Childless

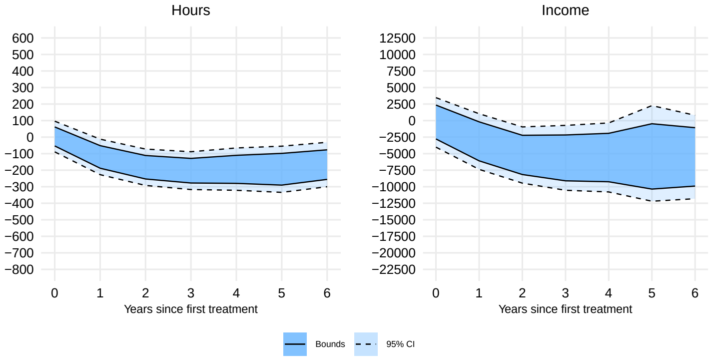

Figure 40: Monotone bounds using final status

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Relaxing Monotonicity to Partnered Women

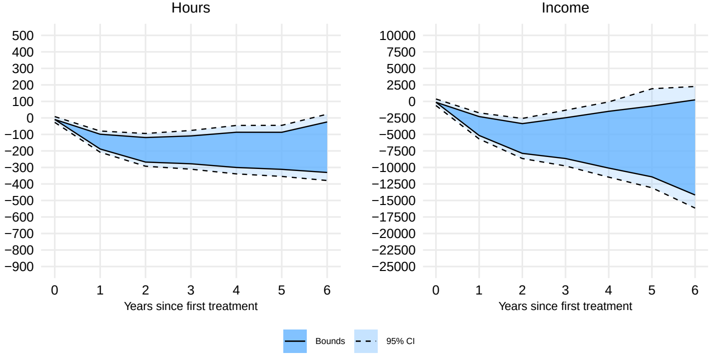

Figure 41: Monotone bounds using women who stay partnered

Back (extensions) Back (monotonicity)

ToC

Testing the Plug-in Approach

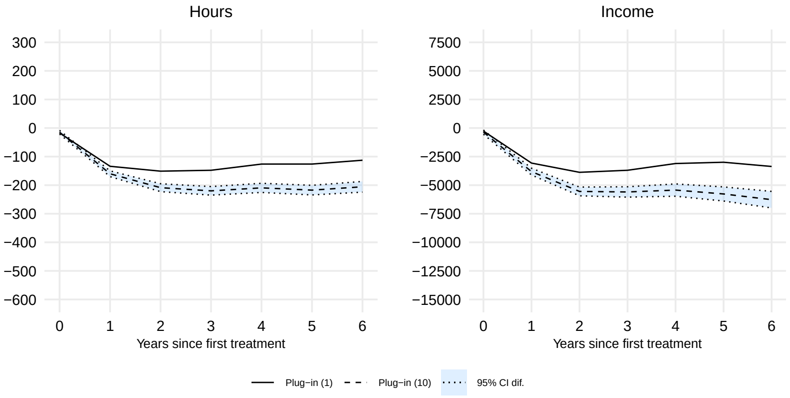

Figure 42: Plug-in estimators exploiting different number of treatments

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ToC

Application to Other Settings

Key features:

1. Dynamic treatment effects.
2. Individuals (may choose to) "re-apply" for random treatment assignment.
3. Some may obtain treatment endogenously.

Application to Other Settings

Key features:

1. Dynamic treatment effects.
2. Individuals (may choose to) "re-apply" for random treatment assignment.
3. Some may obtain treatment endogenously.

Few examples:

- Education, medical trials, research grants, job training.

Examples

Application to Other Settings (Examples)

- Education: grade retention, school admission lotteries, special and gifted education programs.
- Medical trials: individuals assigned to control may eventually choose to participate in other medical trials or selectively receive a different treatment.
- Research grants: after unsuccessful application can apply for another or receive funding other ways.
- Job training: those not assigned to training may re-apply, some assignments may be non-random.

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Estimated Bias and Placebo Event Study

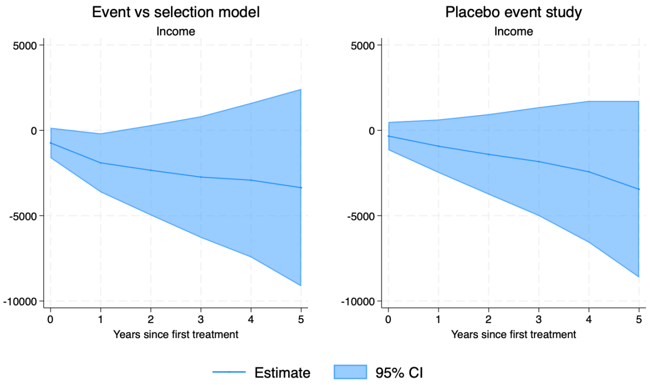

Figure 43: Difference between selection model estimate and event study estimate compared to placebo event study estimate

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