

# Bounding the Child Penalty

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- ▶ 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)

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**Leading methods only addresses each separately**



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1st child at 30

All mothers for 5 years

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All childless

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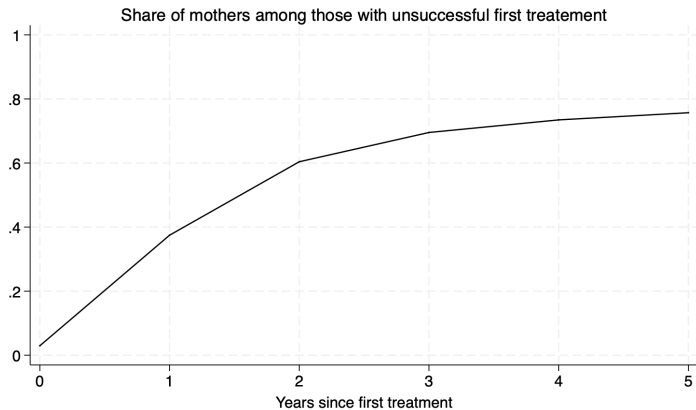
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**Very different results even in same samples**

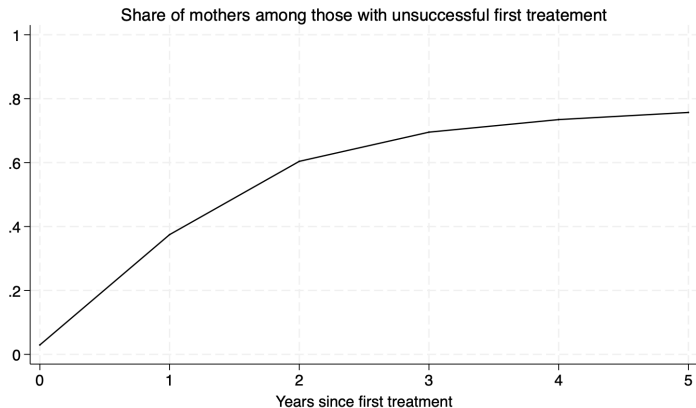
My sample

ES extern.

# Motherhood Among Unsuccessfully Treated

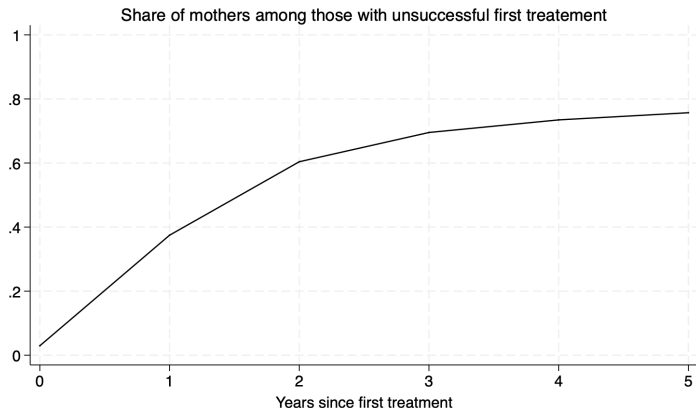


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$$\tau_{IV} = \tau_{Parenthood} - 3\tau_{Delay}$$

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

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2. Empirical evidence using Dutch data

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  - ▶ Minimal assumptions → see what is possible
  - ▶ Only crucial assumption: randomness in IVF success
    - ▶ Endogenous number of attempts
    - ▶ Endogenous non-IVF fertility
    - ▶ No restrictions on effects
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    - ▶ Bounds to handle non-IVF births
  - ▶ 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.



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$$D_1 = 1$$

$$\mathbb{E}[Y(1)|G = 2]$$

$$D_1 = 0, D_2 = 1$$

$$\mathbb{E}[Y(\textit{later})|G = 2]$$

$$D_1 = 0, D_2 = 0$$

$$\mathbb{E}[Y(0)|G = 2]$$



## Simple World: Max 2 Att., Only via IVF (Observed)

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$$\mathbb{E}[Y(0)|G = 2]$$

$$Pr(G = 1) =$$



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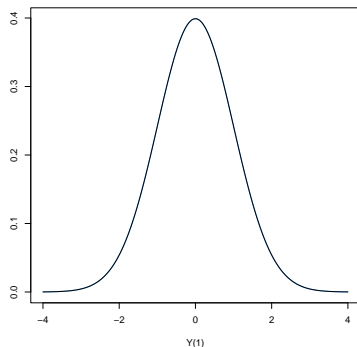
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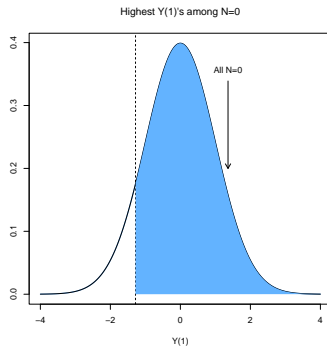
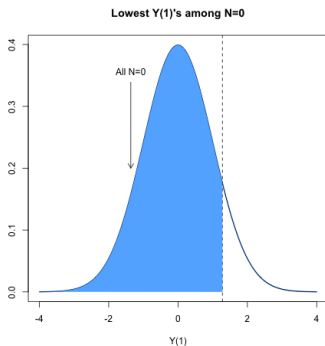
# Intuition: Motherhood Outcome $Y(1)$

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



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

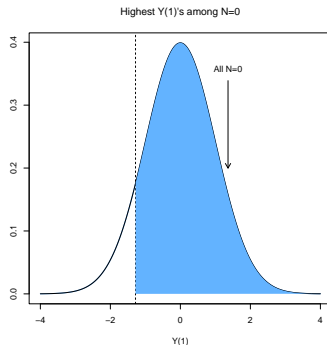
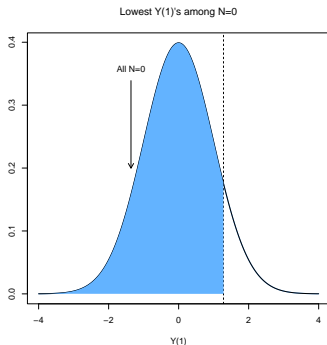
2. Estimate  $\Pr(N = 0) = 0.9$  on control group
3. Assume most extreme distributions of types



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

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

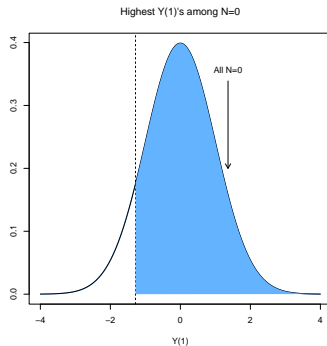
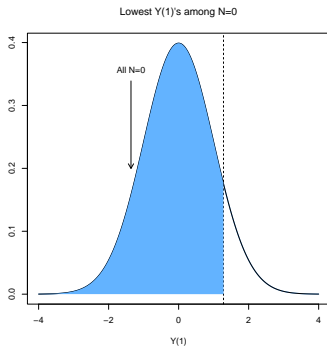
$$LB_{\mathbb{E}[Y(1)|N=0]} \leq \mathbb{E}[Y(1)|N=0] \leq UB_{\mathbb{E}[Y(1)|N=0]}$$



# 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

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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
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  - ▶ Labor market outcomes from 2011 to 2021
- ▶ Unlimited IUI and first 3 IVF attempts covered by mandatory insurance (later IVF attempts cost 2500-5000 Eur per cycle)
- ▶ Sample of opposite sex couples cohabiting before first IUI attempt

Balance

Treatment success

Success prob. change

## Results

# Bounds

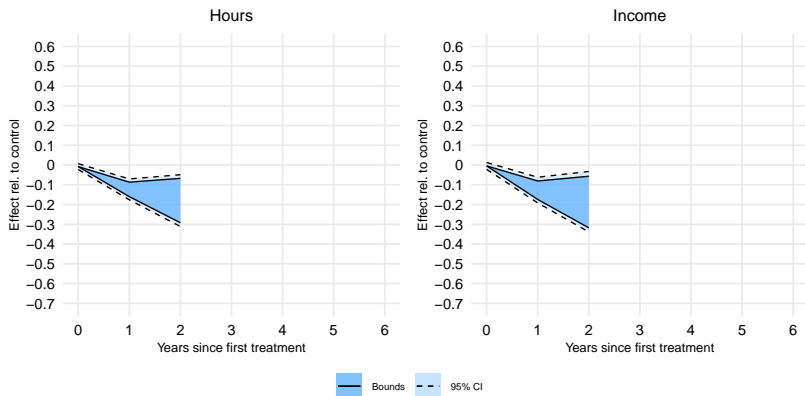


Figure 1: Bounds - short run

Baseline Lee bounds

Absolute effects

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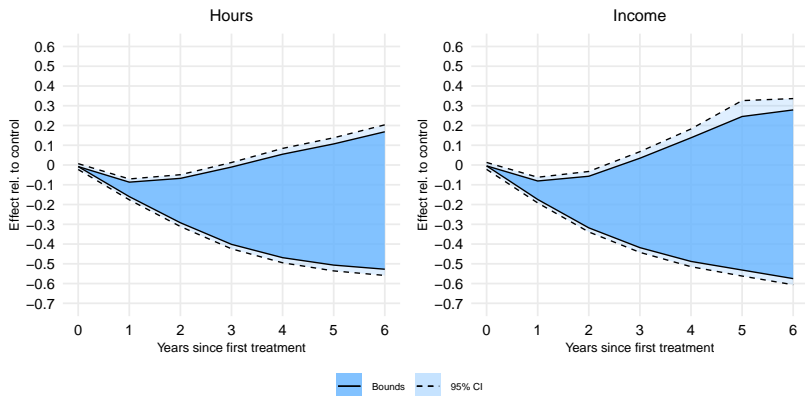


Figure 2: Bounds - medium run

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

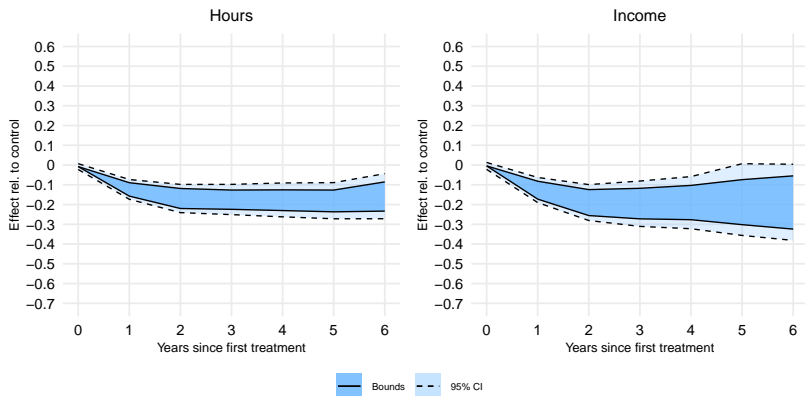


Figure 3: Monotone bounds for percent effects

Absolute

How wide?

# Monotone Bounds: Explaining Gender Inequality

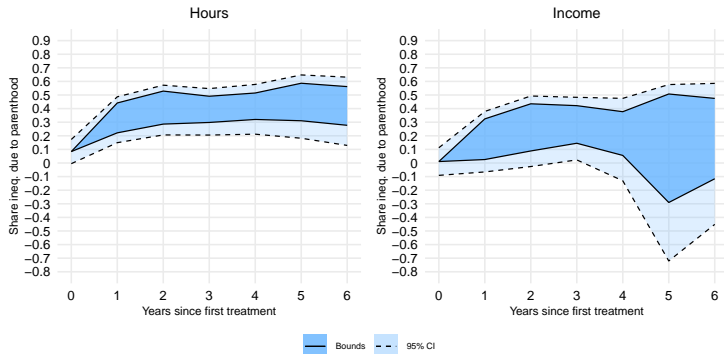


Figure 4: Share of gender inequality explained by parenthood

# 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 than 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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  - ▶ Under monotonicity: stable after 6 years
  - ▶ Drives up to 60% (50%) of gender inequality in post-child work hours (earnings)
- ▶ Comparison to existing approaches
  - ▶ IV-IVF might overstate penalty in the short run

# Conclusion

- ▶ Estimator for the career cost of children that simultaneously address selective timing and dynamic effects
  - ▶ Can be applied to other settings with dynamic and selective treatment assignment
- ▶ Application to Dutch data:
  - ▶ Sizable career impacts of parenthood in the first two years
  - ▶ Under monotonicity: stable after 6 years
  - ▶ Drives up to 60% (50%) of gender inequality in post-child work hours (earnings)
- ▶ Comparison to existing approaches
  - ▶ IV-IVF might overstate penalty in the short run
  - ▶ ES might overstate penalty in both short and medium run

# Appendix

Literature

Identification Math

Treatment Success

Balance

Type Shares

Trimming Shares

Monotonicity

Bound Width

Extensions

Application to Other Settings

References

## Related Literature

Gender inequality in labor market outcomes.

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**No paper to date addresses endogenous timing and dynamic (and heterogeneous) effects.**

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](#)

# 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)

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# Naive Comparison: IV vs ES

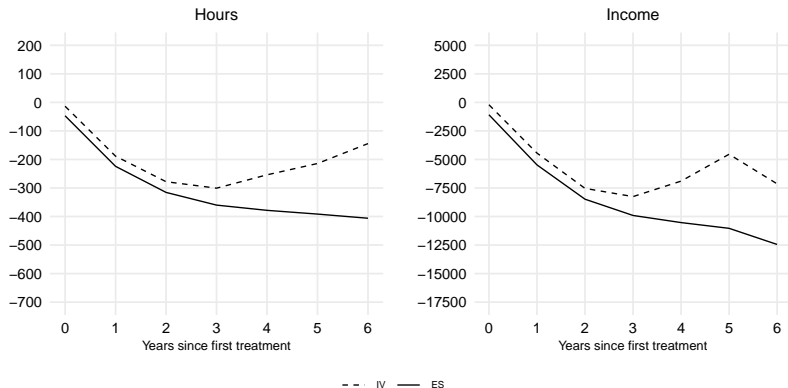


Figure 5: Comparison of IV and ES estimators using main sample

[Back](#)

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Broadly:

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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](#)

# Identification Intuition (cont.)

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Step 0:

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



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# 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

# Formal Identification

$$\Delta_L = \mu_L - \mu_C$$

$$\Delta_U = \mu_U - \mu_C$$

$$\mu_C = \mathbb{E} \left[ \frac{Y}{\prod_j^P (1 - p_j(X_j))} \middle| \mathbf{1}_{Child} = 0 \right] \mathbb{E} \left[ \prod_j^P (1 - p_j(X_j)) \middle| \mathbf{1}_{Child} = 0 \right]$$

$$\mu_L = \mathbb{E} \left[ \frac{Y}{p_1(X_1)} \middle| D_1 = 1, Y < y(1 - s) \right] \mathbb{E} [p_1(X_1) | D_1 = 1, Y < y(1 - s)]$$

$$\mu_U = \mathbb{E} \left[ \frac{Y}{p_1(X_1)} \middle| D_1 = 1, Y > y(s) \right] \mathbb{E} [p_1(X_1) | D_1 = 1, Y > y(s)]$$

$$y(q) = G^{-1}(q)$$

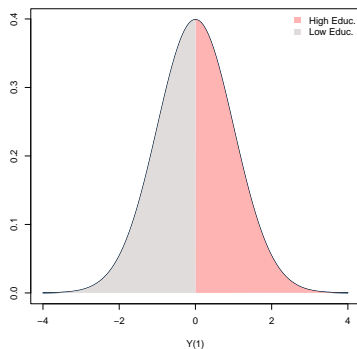
$$G(q) = \mathbb{E} \left[ \frac{\mathbf{1}(Y \leq q)}{p_1(X_1)} \middle| D_1 = 1 \right] \mathbb{E} [p_1(X_1) | D_1 = 1]$$

$$s = \mathbb{E} \left[ \frac{\mathbf{1}_{Child}}{\prod_j^P (1 - p_j(X_j))} \middle| W = 0 \right] \mathbb{E} \left[ \prod_j^P (1 - p_j(X_j)) \middle| W = 0 \right],$$

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

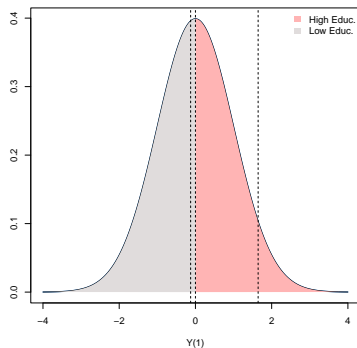
# Tightening Bounds with Covariates

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



## Tightening Bounds with Covariates (cont.)

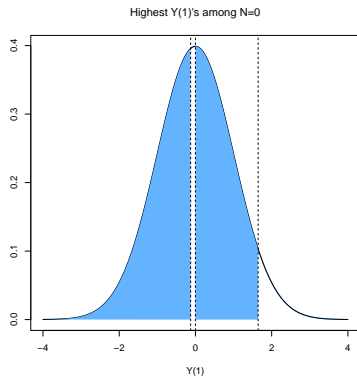
2. Estimate  $\Pr(N = 0|high) = 0.9$  and  $\Pr(N = 0|low) = 0.9$  on control group
3. Assume most extreme distribution of types within educ. groups



# Tightening Bounds with Covariates (cont.)

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

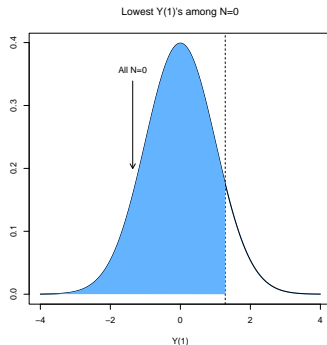
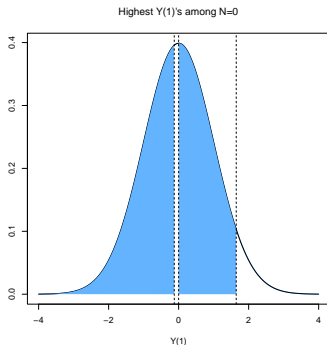
$$\mathbb{E}_{educ}[LB_{\mathbb{E}[Y(1)|N=0]}(educ)] \leq \mathbb{E}[Y(1)|N=0]$$



# Comparing the Bounds

Conditional lower bounds is higher than unconditional:

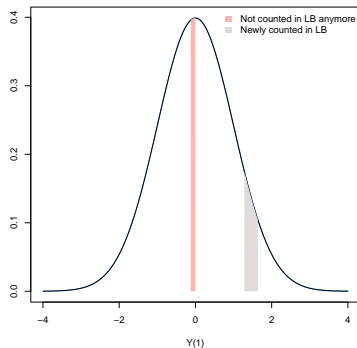
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## Estimation

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

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$$\eta_0 = \{s_0(x), q_1(u, x), e_1(x), \dots, e_J(x)\}$$

$$s_0(x) = Pr(N = 0 | X_1 = x)$$

$$q_1(u, x) = F_{Y(1)|X_1=x}^{-1}(u)$$

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$$\frac{1}{\sqrt{n}} \sum_i (m_L(data_i, \hat{\eta}) - \mathbb{E}[m_L(data_i, \eta_0)]) \xrightarrow{d} ?$$

## Inference

- ▶ Semenova (2020) develops DML estimator for Lee bounds
- ▶ When  $G = 1$ , Lee bounds = my approach
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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 \(extensions\)](#)

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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



**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 [0.324]	0.863 [0.344]	0.018 (0.009)	0.008 (0.009)
Work (P)	0.884 [0.320]	0.865 [0.341]	0.019 (0.009)	0.014 (0.009)
Hours (W)	1239.696 [605.070]	1208.255 [634.840]	31.441 (16.168)	17.578 (15.812)
Hours (P)	1473.383 [658.917]	1438.880 [695.345]	34.502 (17.699)	22.690 (17.587)
Income 1000s € (W)	28.049 [19.559]	27.434 [20.232]	0.615 (0.516)	0.942 (0.496)
Income 1000s € (P)	37.173 [26.484]	36.959 [29.443]	0.214 (0.746)	0.896 (0.732)
Bachelor deg. (W)	0.608 [0.488]	0.605 [0.489]	0.002 (0.013)	0.018 (0.012)
Bachelor deg. (P)	0.593 [0.491]	0.598 [0.490]	-0.004 (0.013)	0.008 (0.012)
Age (W)	31.643 [4.016]	32.384 [4.383]	-0.741 (0.111)	
Age (P)	34.672 [5.527]	35.459 [5.993]	-0.787 (0.152)	
Observations	1,716	13,788		
Joint <i>p</i> -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 (0.009)	-0.002 (0.010)	0.023 (0.011)	0.008 (0.012)	0.030 (0.013)	0.007 (0.014)	-0.008 (0.017)	0.016 (0.019)	0.041 (0.026)
Work (P)	0.011 (0.010)	0.014 (0.010)	0.005 (0.011)	0.014 (0.012)	-0.004 (0.013)	-0.008 (0.014)	0.001 (0.017)	0.016 (0.020)	0.040 (0.027)
Hours (W)	37.050 (17.373)	-0.615 (18.648)	45.477 (20.127)	39.327 (21.930)	68.596 (24.489)	25.780 (26.043)	-5.734 (31.176)	81.149 (36.869)	29.860 (49.101)
Hours (P)	29.074 (19.336)	28.347 (20.807)	18.441 (22.614)	35.597 (24.685)	-7.332 (27.215)	-15.344 (28.618)	0.360 (34.381)	47.511 (41.158)	49.279 (55.440)
Income 1000s € (W)	1.786 (0.548)	0.283 (0.592)	1.123 (0.647)	1.672 (0.710)	1.380 (0.786)	0.489 (0.831)	0.417 (1.030)	1.839 (1.240)	-0.297 (1.714)
Income 1000s € (P)	0.221 (0.820)	1.277 (0.846)	1.588 (0.923)	1.125 (1.018)	-0.542 (1.123)	-0.370 (1.212)	1.567 (1.423)	1.001 (1.666)	-0.202 (2.277)
Bachelor deg. (W)	0.002 (0.013)	0.026 (0.014)	-0.020 (0.015)	0.001 (0.017)	-0.003 (0.019)	0.003 (0.020)	0.023 (0.024)	-0.012 (0.028)	0.045 (0.038)
Bachelor deg. (P)	0.005 (0.013)	0.010 (0.014)	0.011 (0.016)	0.007 (0.017)	-0.003 (0.019)	0.013 (0.020)	0.020 (0.024)	0.012 (0.029)	-0.014 (0.039)
Age (W)	0.001 (0.011)	-0.007 (0.015)	-0.040 (0.019)	0.024 (0.023)	0.013 (0.026)	-0.001 (0.028)	-0.046 (0.036)	-0.027 (0.043)	-0.017 (0.059)
Age (P)	0.001 (0.011)	-0.007 (0.015)	-0.040 (0.019)	0.024 (0.023)	0.013 (0.026)	-0.001 (0.028)	-0.046 (0.036)	-0.027 (0.043)	-0.017 (0.059)
Observations	12,955	10,759	8,714	6,969	5,403	3,938	2,718	1,848	1,173
Joint <i>p</i> -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.

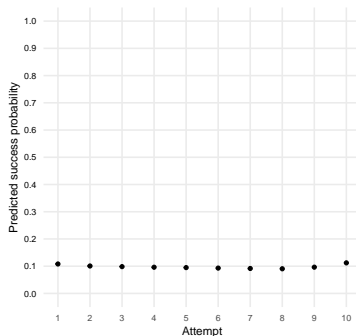
# Representative and Relevant Treatment group

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

# Predicted Success Prob. per Treatment



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

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# Attempts

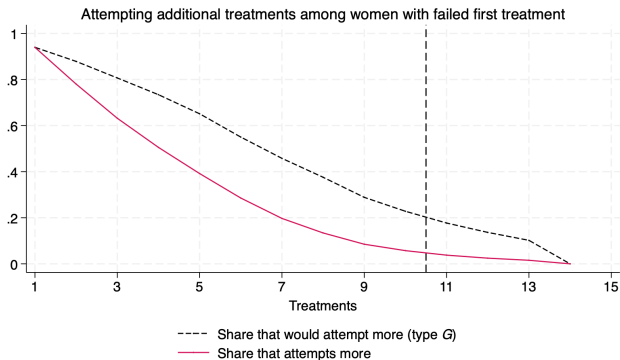


Figure 7: Number of treatments and type

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# Non-treatment Conception by Type

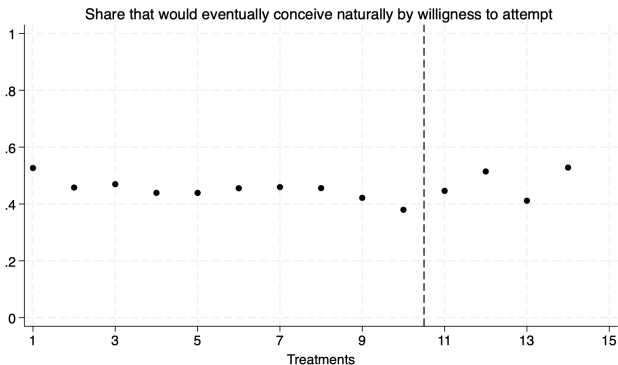


Figure 8: Conceiving naturally and willingness to attempt

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# Trimming shares

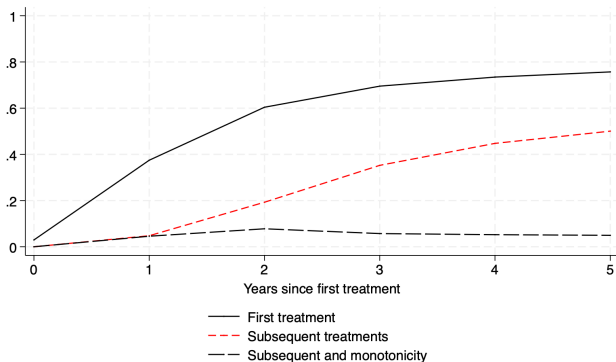


Figure 9: Trimming share under different information

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# Correction Term

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

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# Bounds: Absolute

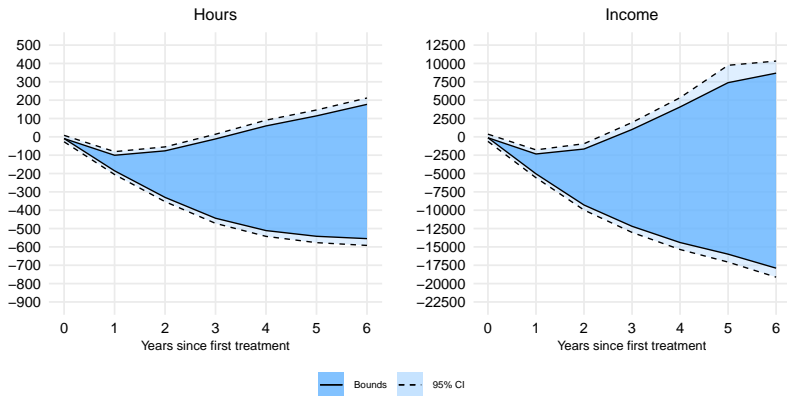


Figure 10: Bounds effects

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# Bounds: Hours - Comparison to Baseline Lee Bounds

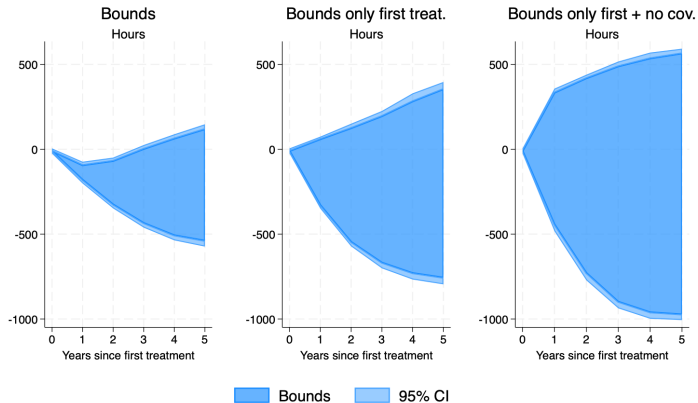


Figure 11: Comparison with baseline Lee: hours

# Bounds: Income - Comparison to Baseline Lee Bounds

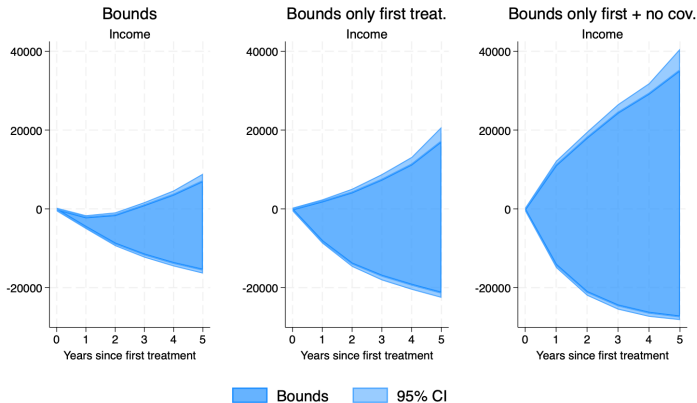


Figure 12: Comparison with baseline Lee: income

# 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.

# Monotonicity (cont.)

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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.

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  - ▶ Couples may realize they are fertile and try more.

# Monotonicity (cont.)

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- ▶ 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.)

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- ▶ 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

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Benefit of monotonicity

Graphic intuition

# 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.

# Benefit of Monotonicity

- ▶ Women who conceive a second child naturally are not the women who would remain childless.
- ▶ This restricts which women among successfully treated could have remained childless had all treatment attempts failed.
- ▶ Narrower bounds.

[Intuition](#)

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# Monotonicity: Intuition (1)

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

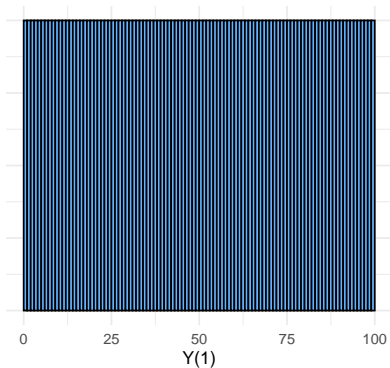


Figure 13: Distribution of potential motherhood outcomes

## Monotonicity: Intuition (2)

Without monotonicity: 20 with highest/lowest outcomes:

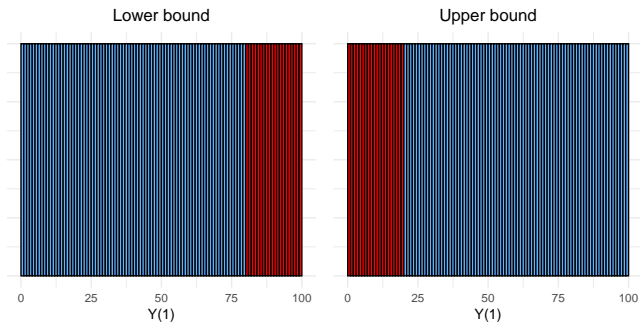


Figure 14: Distribution of potential motherhood outcomes

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## Monotonicity: Intuition (3)

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

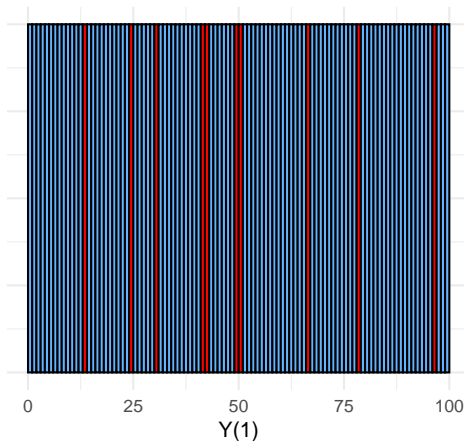


Figure 15: Distribution of potential motherhood outcomes

## Monotonicity: Intuition (4)

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

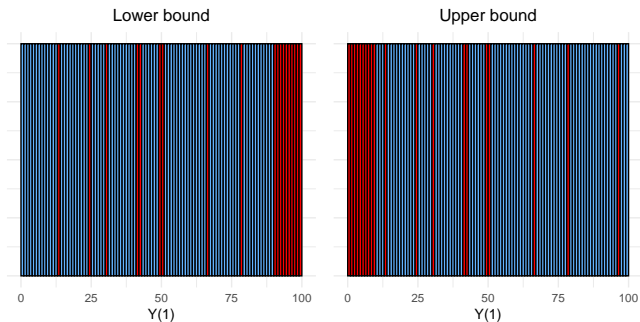


Figure 16: Distribution of potential motherhood outcomes

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## Monotonicity: Intuition (5)

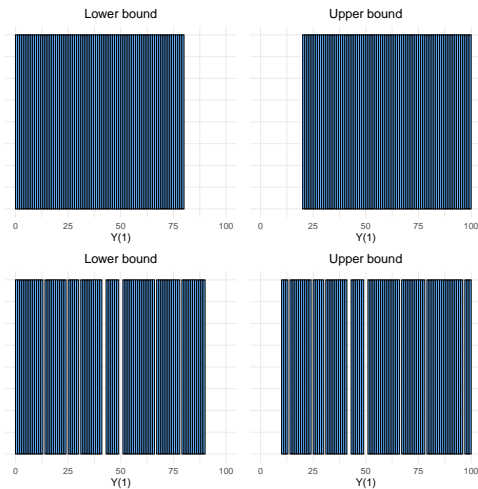


Figure 17: Distribution of potential motherhood outcomes

# Monotone Bounds: Absolute

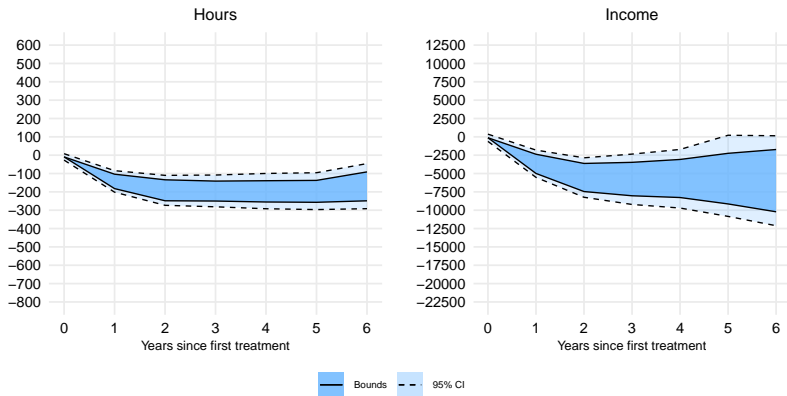


Figure 18: Monotone bounds: absolute terms

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# 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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# 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 than 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

# Monotone Bounds: Women who Remain Childless

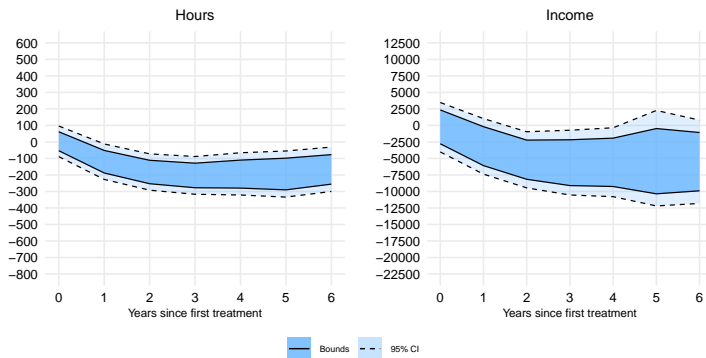


Figure 19: Monotone bounds using final status

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# Event Study: Population vs IUI Sample

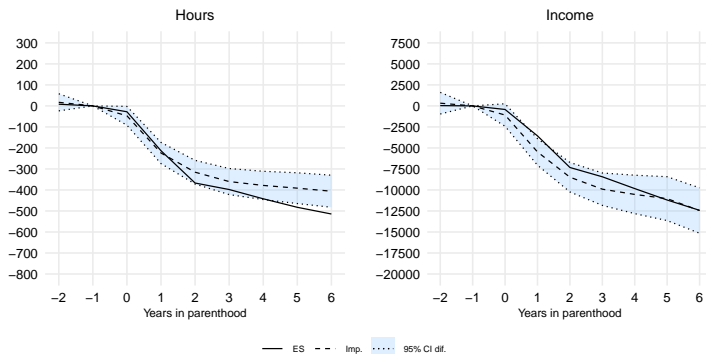


Figure 20: ES for population and women with first IUI success

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# Imputing Population Motherhood Outcomes Using IUI Sample

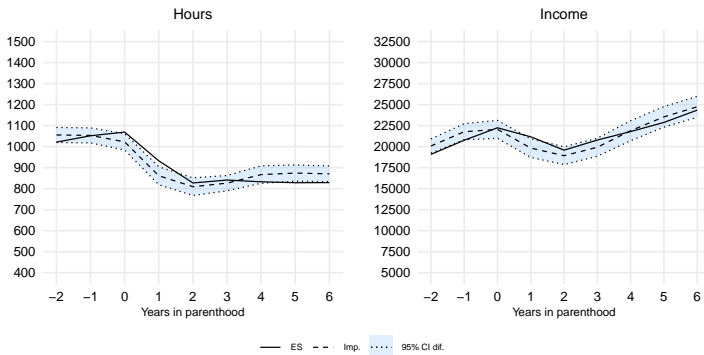


Figure 21: Population Outcomes vs IUI-imputation (age & education)

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# Imputing Population Childless Outcomes Using IUI Sample

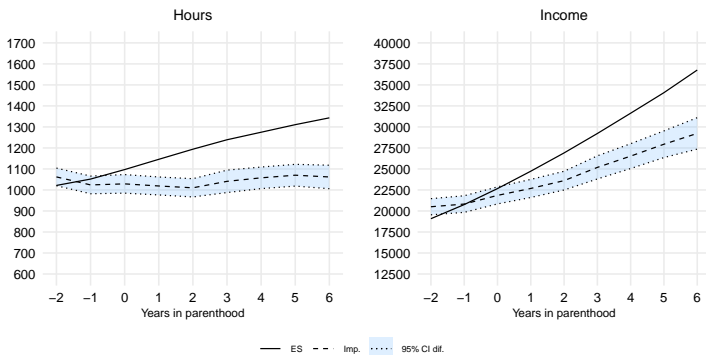
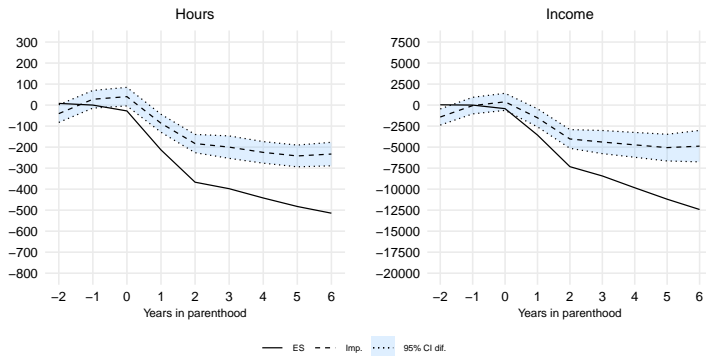


Figure 22: Population Outcomes vs IUI-imputation (age & education)

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# Event Study vs IUI-imputation for Population



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

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# 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

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

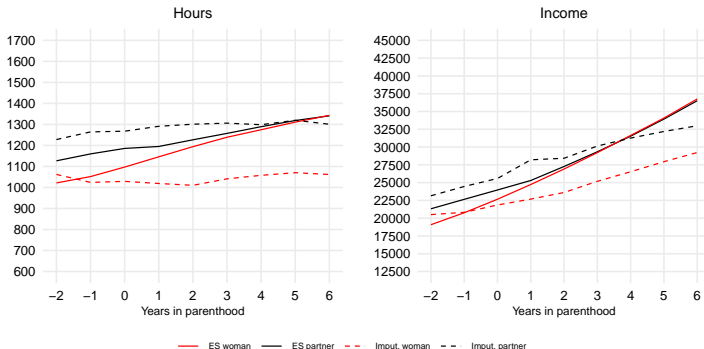
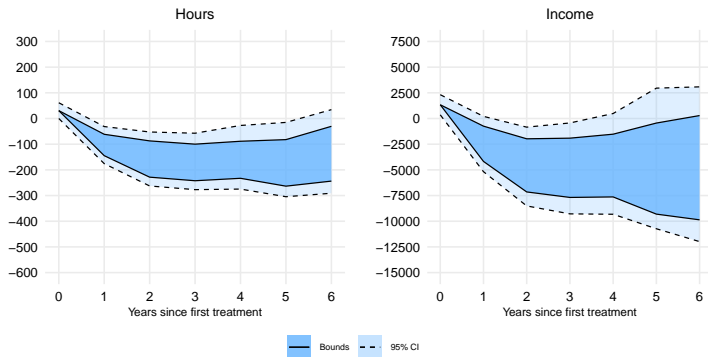


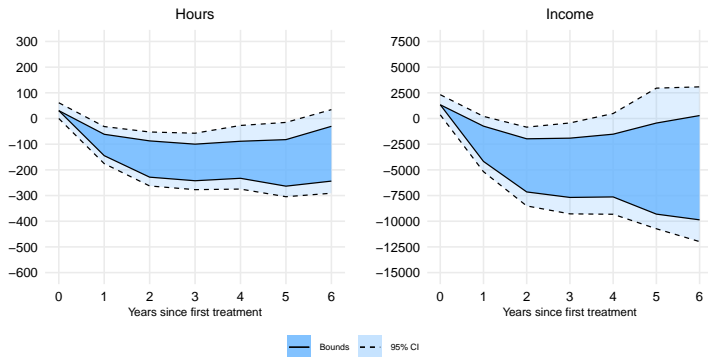
Figure 24: Event study vs IUI-imputation for population gaps (age & education), partner outcomes shifted 3 years

# Simple estimator



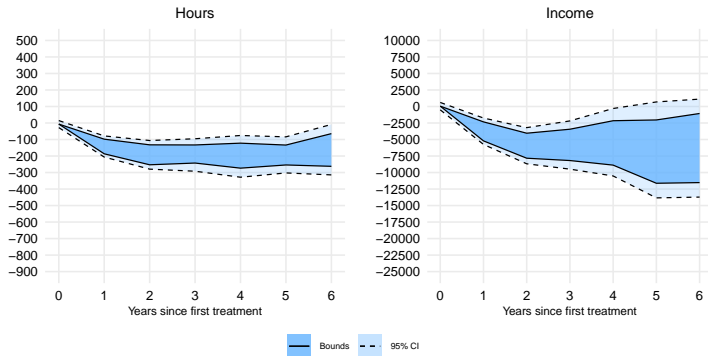
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# Simple estimator



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# Relaxing Monotonicity Direction



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# Heterogeneity by Covariates

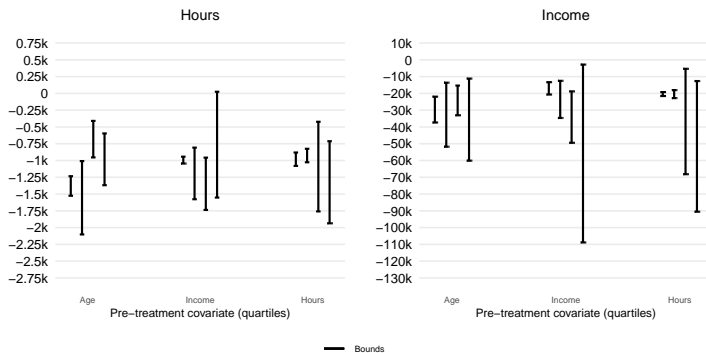


Figure 25: Cumulative outcomes after 6 years, pre-treatment covariates

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# Heterogeneity by Willingness to Undergo Procedures

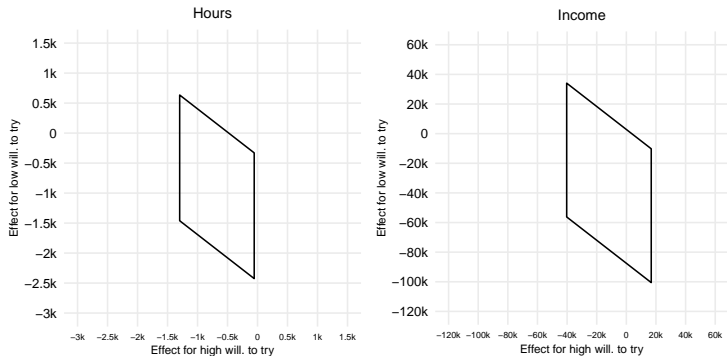


Figure 26: Cumulative outcomes 6 years after,  $G$  above or below 6

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# Monotone Bounds: Excluding Depression

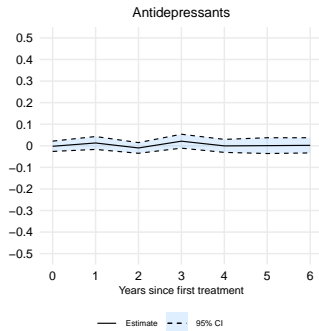


Figure 27: Sequential-IV estimates for effect on antidepressant take-up

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# Monotone Bounds: Excluding Depressed

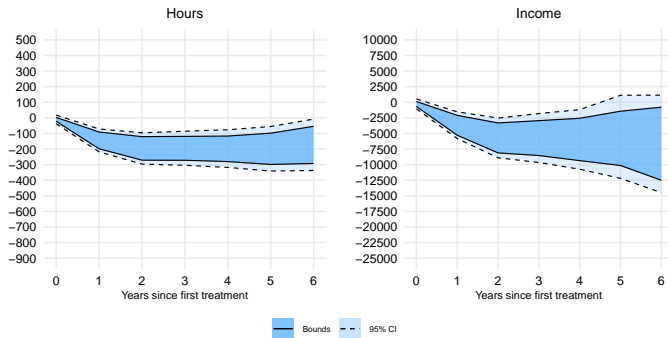


Figure 28: Monotone bounds for women who would not start antidepressants if they were to remain childless

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# 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

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# Monotone Bounds: Assuming Maximum Leave

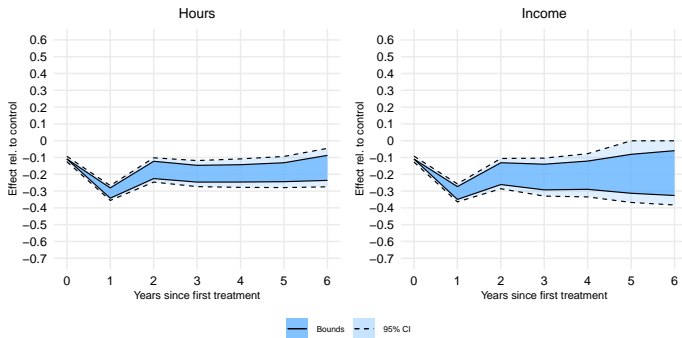


Figure 29: Monotone bounds scaling outcomes in years with childbirth by max. leave fraction

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# Monotone Bounds: Correcting for Partner's age

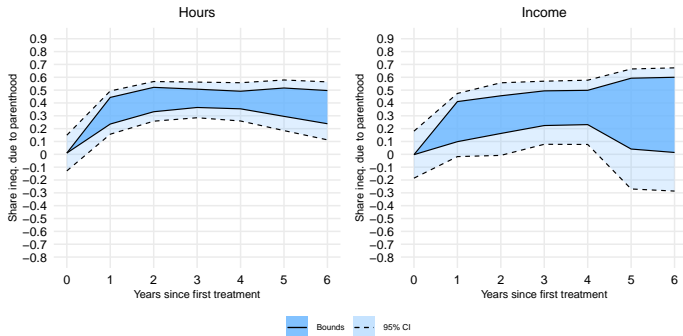


Figure 30: Monotone bounds using male income at same age as female

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# Monotone Bounds: Fatherhood Penalty

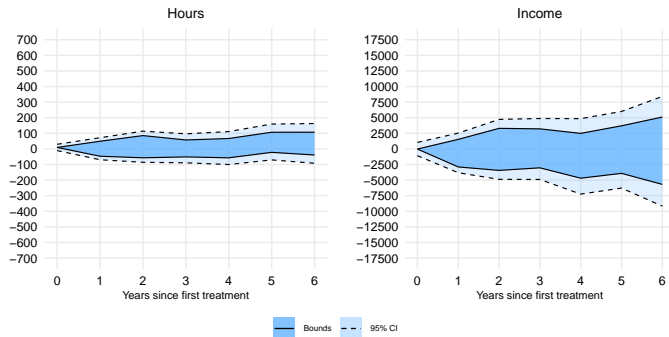


Figure 31: Monotone bounds for partners

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# Monotone Bounds: Fatherhood Penalty in Percent

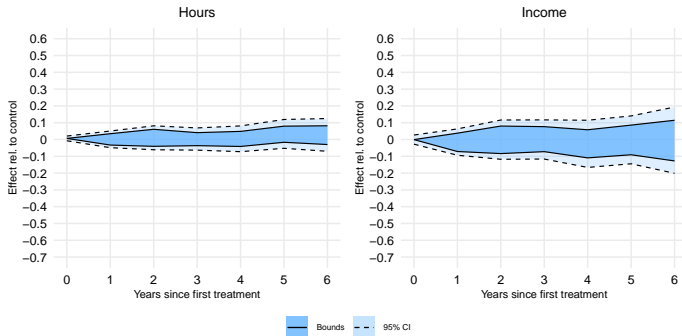


Figure 32: Monotone bounds for partners in percent

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# Monotone Bounds: Explaining Gender Inequality

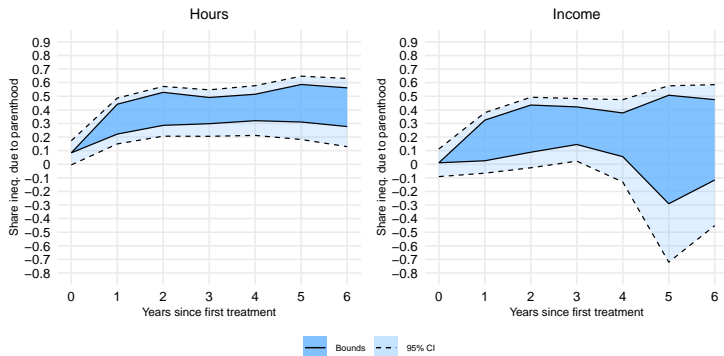


Figure 33: Share of gender inequality explained by parenthood

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# Are Bounds Less Informative?

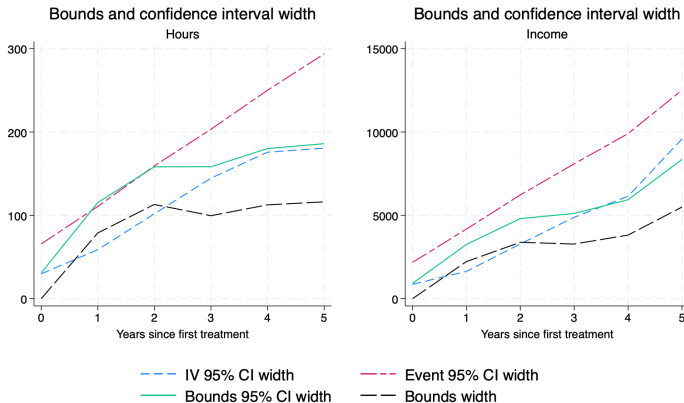


Figure 34: Confidence intervals for different methods

# Monotone Bounds and IV

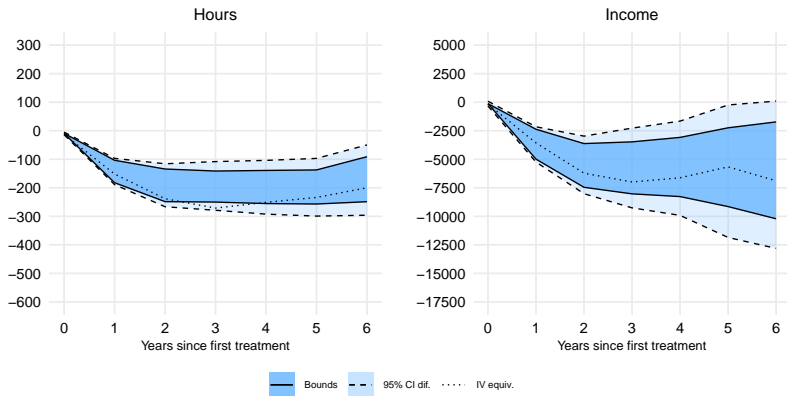


Figure 35: Bounds and IV equivalent for the same population

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# Placebo Event

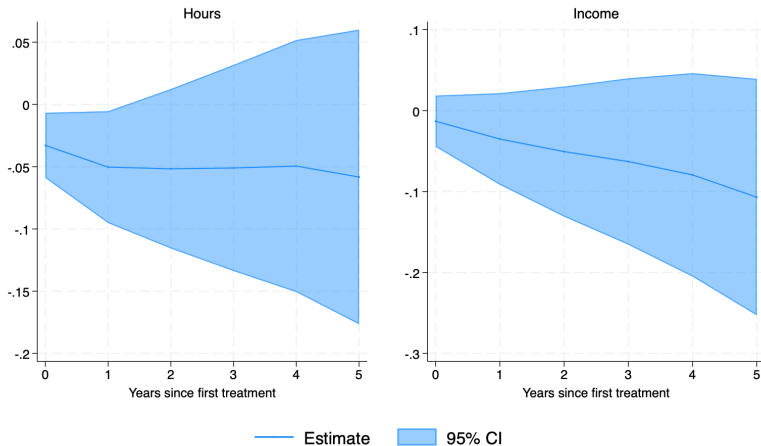


Figure 36: Placebo event study

# Inequality treating ES bias as causal

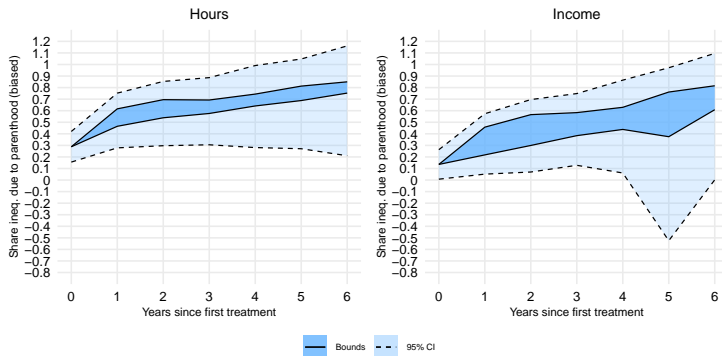


Figure 37: Placebo effects as share of bounds for the same population

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

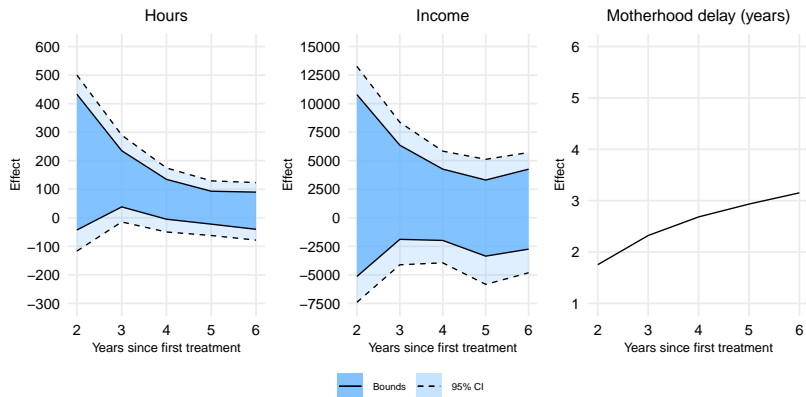


Figure 38: Effect of delaying relative to motherhood at first attempt

**Opposite of what is frequently assumed!**

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# Cumulative effect of Delaying Motherhood

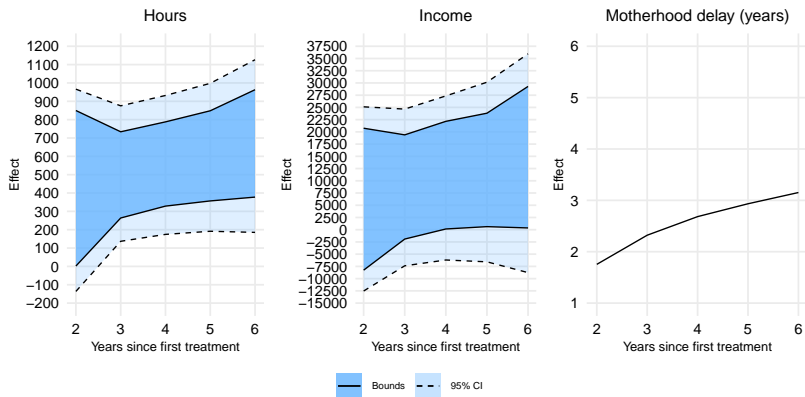


Figure 39: Effect of delaying relative to motherhood at first attempt

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# Monotone Bounds: Women who Remain Childless

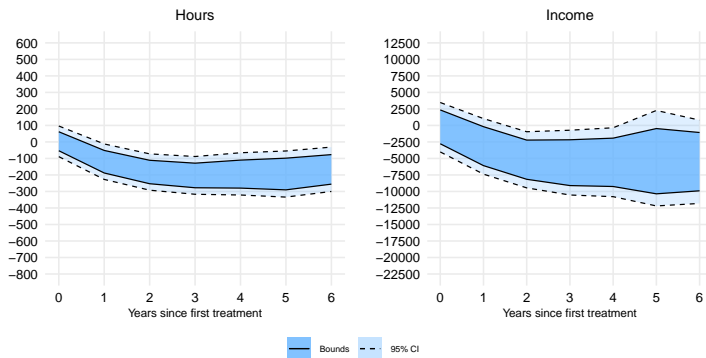


Figure 40: Monotone bounds using final status

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# Relaxing Monotonicity to Partnered Women

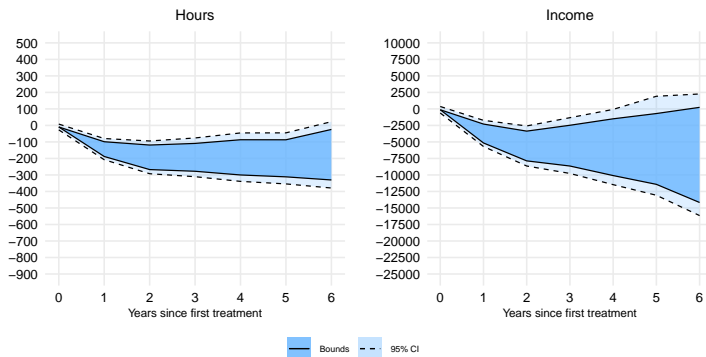


Figure 41: Monotone bounds using women who stay partnered

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[Back \(monotonicity\)](#)

# Testing the Plug-in Approach

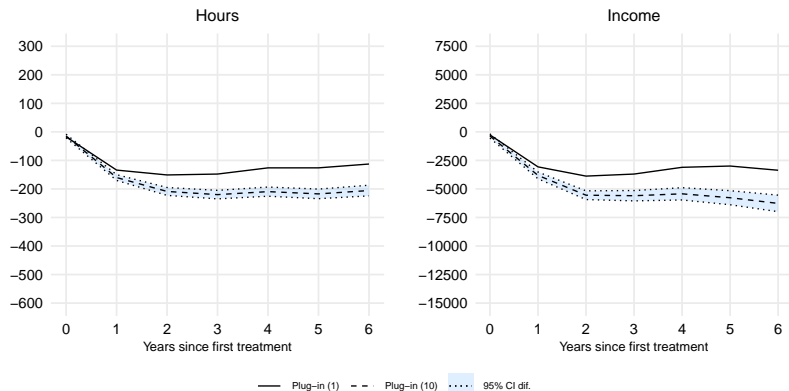


Figure 42: Plug-in estimators exploiting different number of treatments

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# 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.

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Few examples:

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

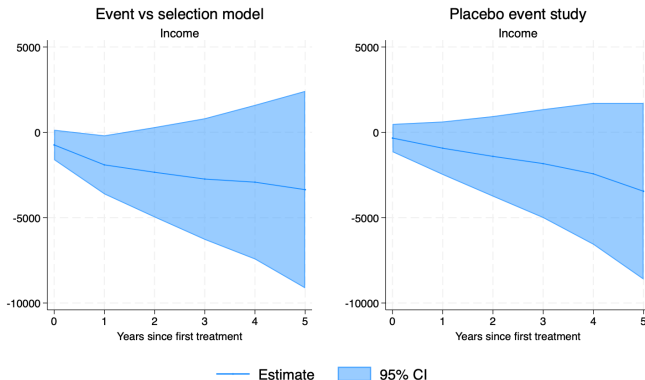
Examples

# 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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