The causal effect of an income shock on children's human capital

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Why do we care?

- Traits determined during childhood explain lifetime earnings
  - Cunha and Heckman (2007); Currie (2009)
- Inequalities during childhood are likely to lead to diverging destinies in adulthood and contribute to the intergenerational transmission of inequality
  - Black and Devereux (2011); Corak (2013); Black et al (2020)

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  - Black and Devereux (2011); Corak (2013); Black et al (2020)
- => Cash transfers to families with children: effective tool to prevent child poverty and mitigate growing socio-economic inequalities
  - OECD countries spend about 1.4% of GDP (OECD, Family Database, 2022)

What are the options?

- Highly targeted conditional schemes while cheaper, complicated to administer
  - About 20% of eligible taxpayers fail to claim the EITC (TIGTA 2018)
- Unconditional universal cash-transfer schemes while easier to administer, have lower payout to restrain program costs and may risk creating disincentives for work
  - This emphasis on negative behavioral effects of safety net programs is one explanation for low public expenditure in family benefits in the US, according to Aizer et al. (2022)

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=>Insufficient evidence on the long-term consequences of an unconditional cash transfer

Why do we care?

Biden's administration initiative to extend the Child Tax Credit has stalled

Research question

- What is the causal impact of receiving a generous unconditional cash transfer (just after birth) on children outcomes during middle childhood?

How do we answer this question?

- We use the natural experiment generated by a generous child benefit introduced unexpectedly in Spain on July 1, 2007
  - Universal and unconditional
  - Eligibility based on date of birth
  - Lump-sum 2,500-Euro payment to the mother right after birth.
    - About 11 percent of the median and 17 percent of the bottom quartile of *annual* household income
  - Almost full take up: over 91% (Gonzalez and Trommlerova 2021)
  - Introduced ex-post: no anticipation effects (e.g., timing of birth)
  - No simultaneous change in other child-related policies

What do we find?

- We fail to find any economically significant impacts from the benefit on children's later health and educational outcomes
  - High quality data allow us to reject impact sizes of the magnitude found on most previous studies
- We fail to find significant impacts on parental time and money investments in children
  - Maternal labor supply, childcare arrangements, partnership status
- We do find statistically significant increases in household expenditures on big-ticket items
  - In the context of a country with a wide safety net, these increases do not seem to further child development

Contribution

- To the extensive literature on the causal effect of income shocks on later child development (Almond et al. 2018; Cooper & Stewart 2021)
  - Universal and unconditional income shock: separate pure income effects from difficult-to-model substitution effects (Heckman & Mosso 2014)

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  - Other studies using unconditional income shocks
    - Some previous work has used variation stemming from lotteries (Cesarini et al 2016)
      - We contribute by studying a policy relevant income change (external validity: lottery players and lottery income)

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  - Other studies using unconditional income shocks
    - Some previous work has used variation stemming from lotteries (Cesarini et al 2016)
      - We contribute by studying a policy relevant income change (external validity: lottery players and lottery income)
    - Some recent papers study the impact of unconditional cash-transfers at birth (Barr et al. 2022, De Gendre 2021)
      - We contribute by studying a policy implemented retrospectively (no strategic manipulation of births) and in a different context (generous safety net)

Contribution

- To the literature on the causal impact of income on child outcomes during middle childhood
  - Very important overlooked period due to lack of administrative data: birth registers and adult social security registers (Almond et al 2018)
  - Few exceptions using high quality administrative data (Barr et al. 2022)
    - We contribute by using high quality administrative health and education data

Contribution

- To the recent debate on the potential negative labor market incentives of unconditional programs such as the universal basic income (Hoynes and Rothstein, 2019)
  - Theoretically: negative income effect on employment
  - Unanswered question in the empirical literature because many natural experiments are conditional on work (EITC, welfare-to-work experiments)
  - Empirical estimates from few experiments
    - Finnish Basic Income Experiment (Kangas et al 2020): short-term null results
    - Alaska Permanent Fund (Jones and Marinescu 2022): longer-term null results of permanent change in income
  - We provide evidence from a one-off policy shock in the long-run targeting recent mothers: no negative impact on female employment

Agenda

- ✓ Introduction
- Data
- Research design
- Main results: health and education outcomes
- Heterogeneity by age, sex, and socio-economic status
- Mechanisms
- Comparison to previous causal estimates
- Conclusion

Data Setting: Spain

- Spain's fact sheet (vis-a-vis the European Union)
  - 2nd by surface area, 4th by population, 4th country by GDP
- Spain's social safety net for families with children:
  - Universal, publicly-funded health care system
  - Free infant and primary education starting at age 3.
  - Generous parental leave: 16 weeks for mothers and 15 days for fathers in 2007

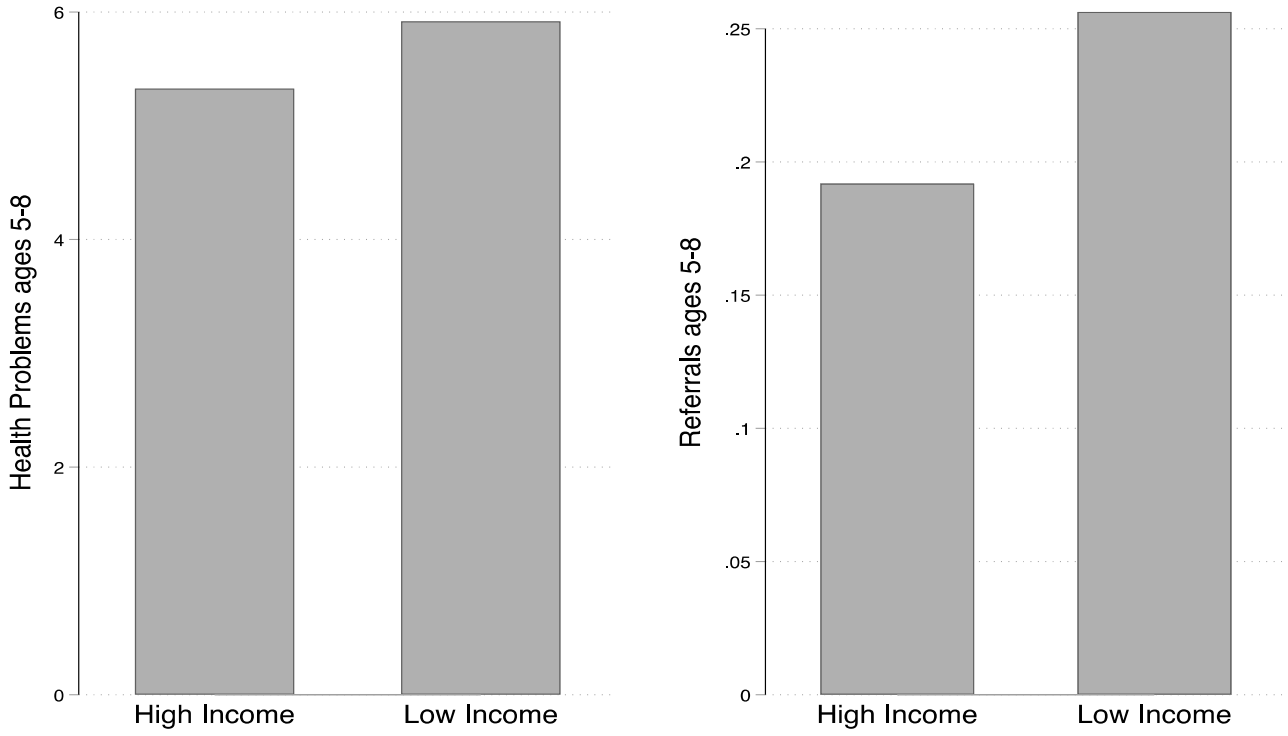
Data Overview: Main Registers

Panel A. Health Data

Primary care prescriptions data (BIFAP 2006-2011)	Visits, health problems (ICPC-2), referrals, prescriptions (ATC), anthropometric measures
Primary care clinical data (BDCAP 2011-2015)	Health problems (ICPC-2), referrals
Hospital Morbidity Survey (2006-2015)	Hospitalization rates by age and diagnosis (ICD-9)
Vital Statistics (2006-2007)	Number of births

Descriptives for Healthcare Data

Income gradient for health problems and referrals to specialists



- Similar income gradient also for hospitalizations
- We would expect that an increase in income would reduce healthcare utilization

Data Overview: Main Registers

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Primary care prescriptions data (BIFAP 2006-2011)	Visits, health problems (ICPC-2), referrals, prescriptions (ATC), anthropometric measures
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Hospital Morbidity Survey (2006-2015)	Hospitalization rates by age and diagnosis (ICD-9)
Vital Statistics (2006-2007)	Number of births
Panel B. Education Data	
Andalusian Diagnostic Tests-ADT (2013/14-2014/15)	Repeater, Math and Language Test Scores in 2nd year.
Catalonian Grades-CG (2013/14-2015/16)	Math, Spanish, English, and Catalan Grades in 2nd year, and Average Grades in 3rd year.

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Identification

- Difference-in-discontinuity design (Carneiro, Løken, and Salvanes, 2015 and Bertrand, Mogstad, and Mountjoy, 2020):

$$Y_i = \alpha + \gamma_1 Reform_i + \gamma_2 Post_i + \beta Reform_i * Post_i +$$

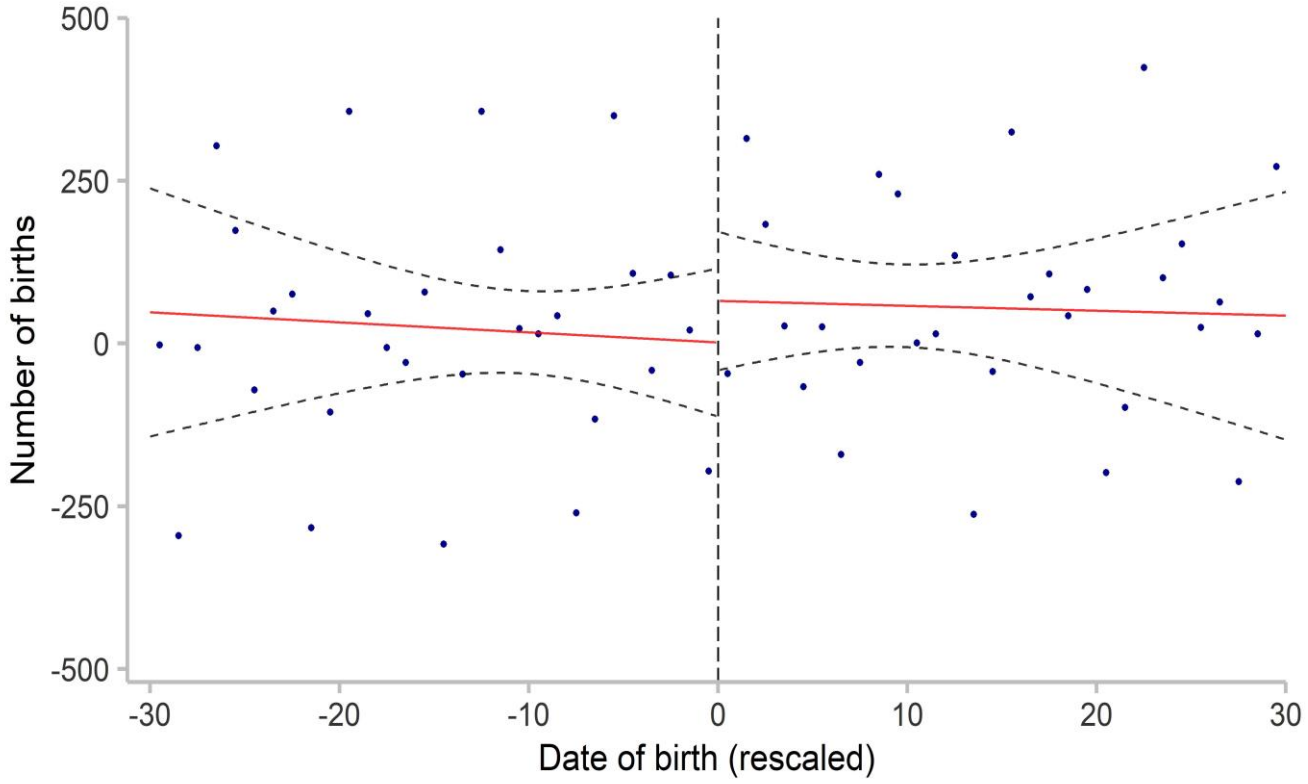
$$f(Date_i) * [\gamma_3 + \gamma_4 Reform_i + \gamma_5 Post_i + \gamma_6 Reform_i * Post_i] + \varepsilon_i$$

  - Y_i denotes the studied outcome of child *i*.
  - $Date_i$ is the running variable, defined with respect to July 1st each year
  - $Reform_i$ is an indicator variable equal to 1 if child i was born in the window surrounding the cutoff date July 1, 2007
  - $Post_i$ is an indicator variable that is equal to 1 if the child was born after the July 1st cutoff in either year (2006 and 2007)

Validity of the Research Design: Continuity at the July 1st threshold

- Absence of differential strategic sorting around the cutoff

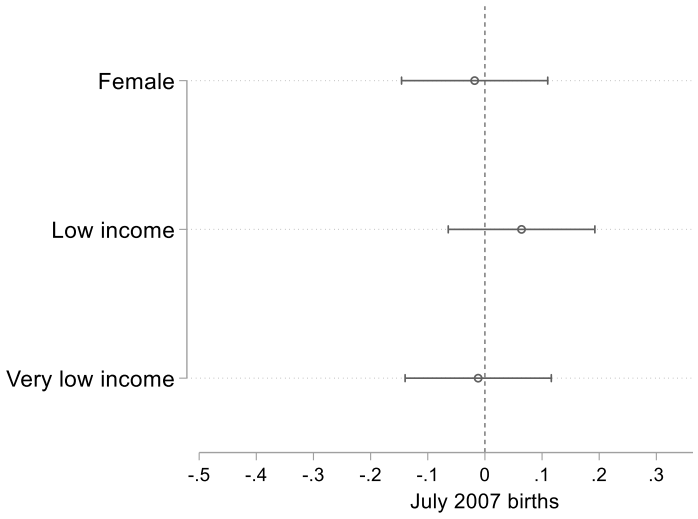
Impact of benefit eligibility on the number of births



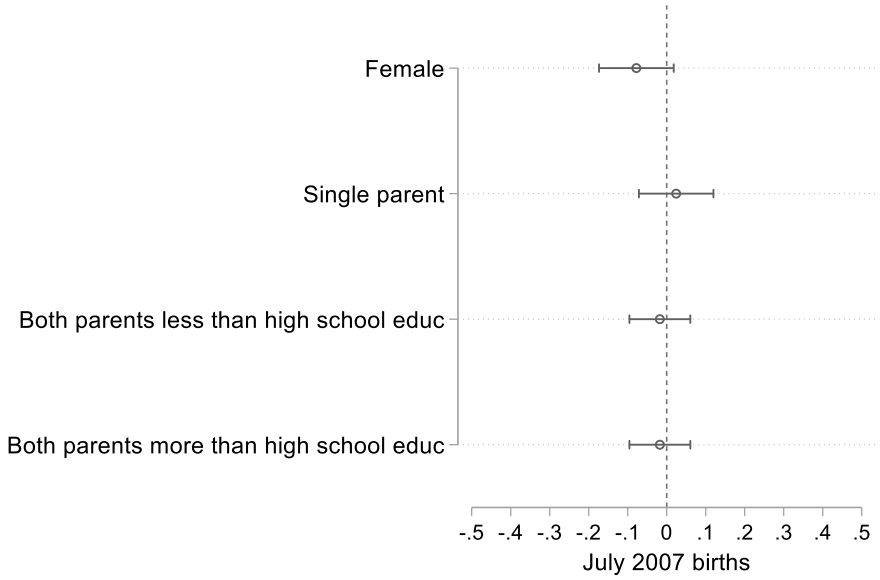
Validity of the Research Design: Continuity at the July 1st threshold

- Absence of a differential impact for pre-determined variables

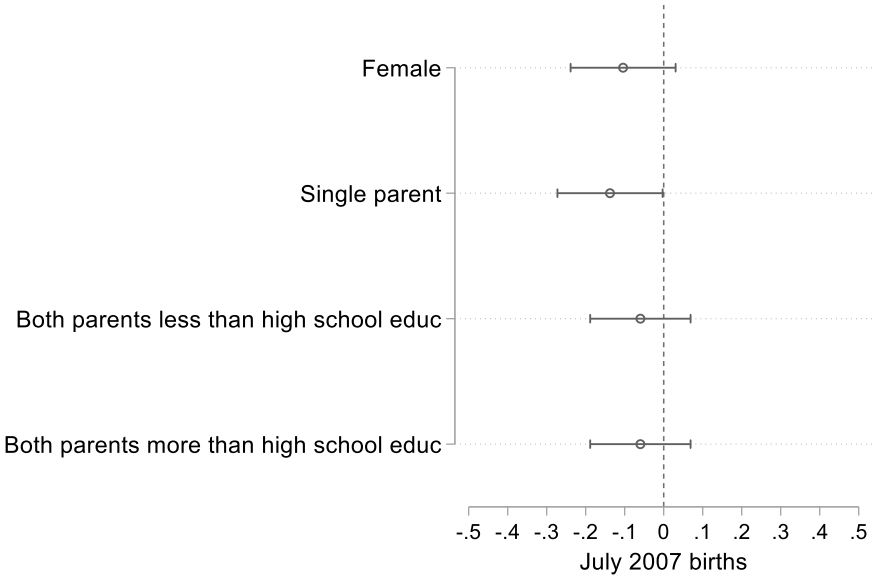
Impact of benefit eligibility



Clinical Primary Care



Andalusian Tests



Catalonian Grades

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Main Results: Primary healthcare outcomes

	Health Problems	Referrals
Panel A. Primary Healthcare Outcomes Ages 0-4. BIFAP		
Effect	-0.139	0.074
	(0.557)	(0.099)
Mean/SD	23.402/15.269	1.508/2.699
CI in sd units	(-0.08, 0.06)	(-0.04, 0.10)
Panel B. Primary Healthcare Outcomes Ages 5-8. BDCAP		
Effect	0.499	-0.019
	(0.398)	(0.052)
Mean/SD	5.362/6.349	0.218/0.754
CI in sd units	(-0.04, 0.20)	(-0.16, 0.11)

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We can reject reductions in the number of health problems larger than 0.08 s.d. units

Main Results: Hospitalization outcomes

	All Stays	Respiratory	Infections
Effect	0.031	0.016	0.009
	(0.037)	(0.012)	(0.008)
Mean/SD	0.694/0.056	0.128/0.016	0.101/0.014
CI in % units	(-5.9, 14.9)	(-5.8, 30.8)	(-6.6, 24.4)
Observations	122	122	122
Linear Trend	Yes	Yes	Yes

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Observations	122	122	122
Linear Trend	Yes	Yes	Yes

We can rule out reductions in hospitalization rates larger than 6.6%

Main Results: School outcomes

	Spanish (standardized)	Math (standardized)
Panel A. Andalusian Diagnostic Tests		
Effect	-0.064	-0.048
	(0.050)	(0.046)
CI	(-0.16, 0.03)	(-0.14, 0.04)
Panel B. Catalan Grades		
Effect	-0.125*	-0.042
	(0.075)	(0.070)
CI	(-0.27, 0.02)	(-0.18, 0.09)

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	(0.075)	(0.070)
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We can discard improvements in cognitive outcomes larger than 0.03 and 0.02 s.d. units in Spanish and 0.04 and 0.09 s.d. units in Math

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Heterogeneity

- No impacts by:
  - Age of the child
  - Sex of the child
  - Socioeconomic status

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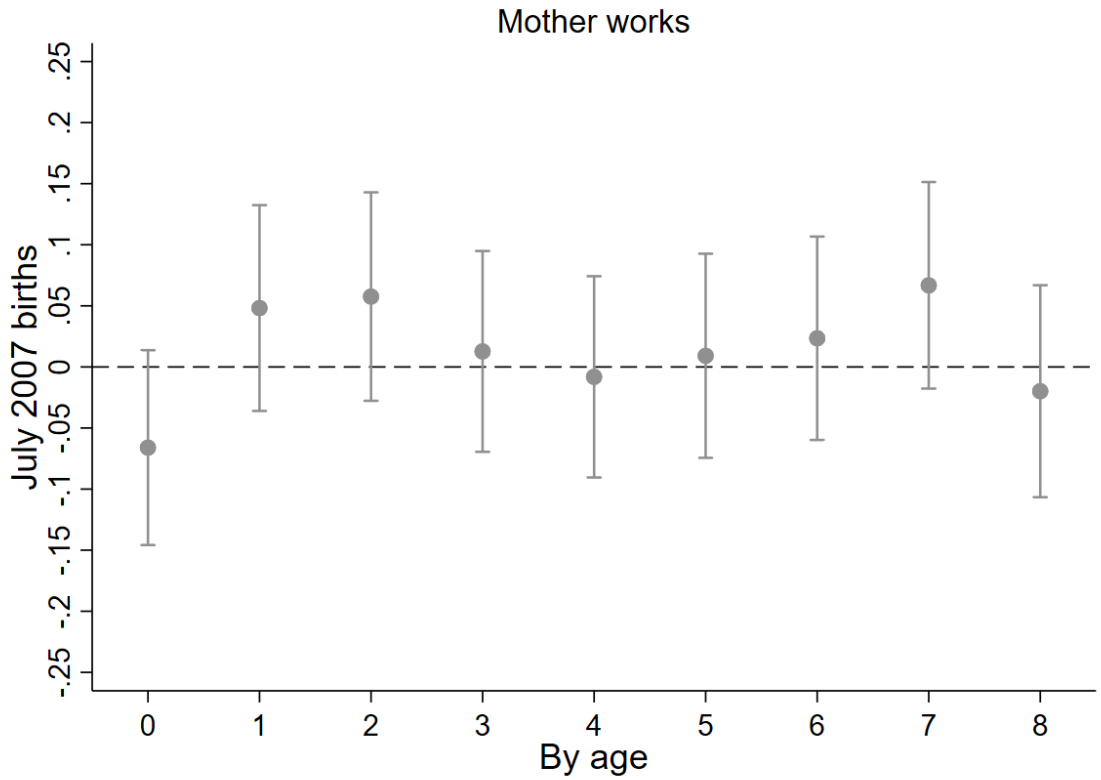
Mechanisms

- No impacts on:
  - Parental time investments, including
    - Maternal labor supply and childcare arrangements
  - Family structure (except for less divorces during first year)
  - Subsequent fertility

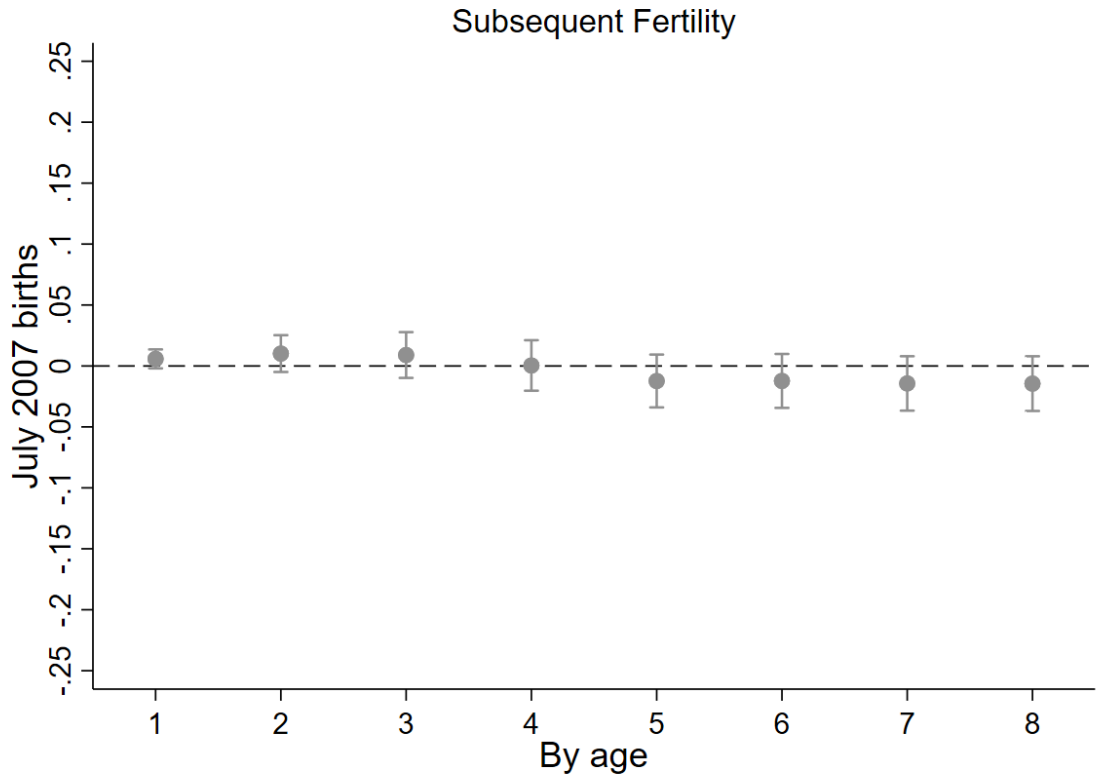
Mechanisms

Parental time investments and family structure

Labor force participation



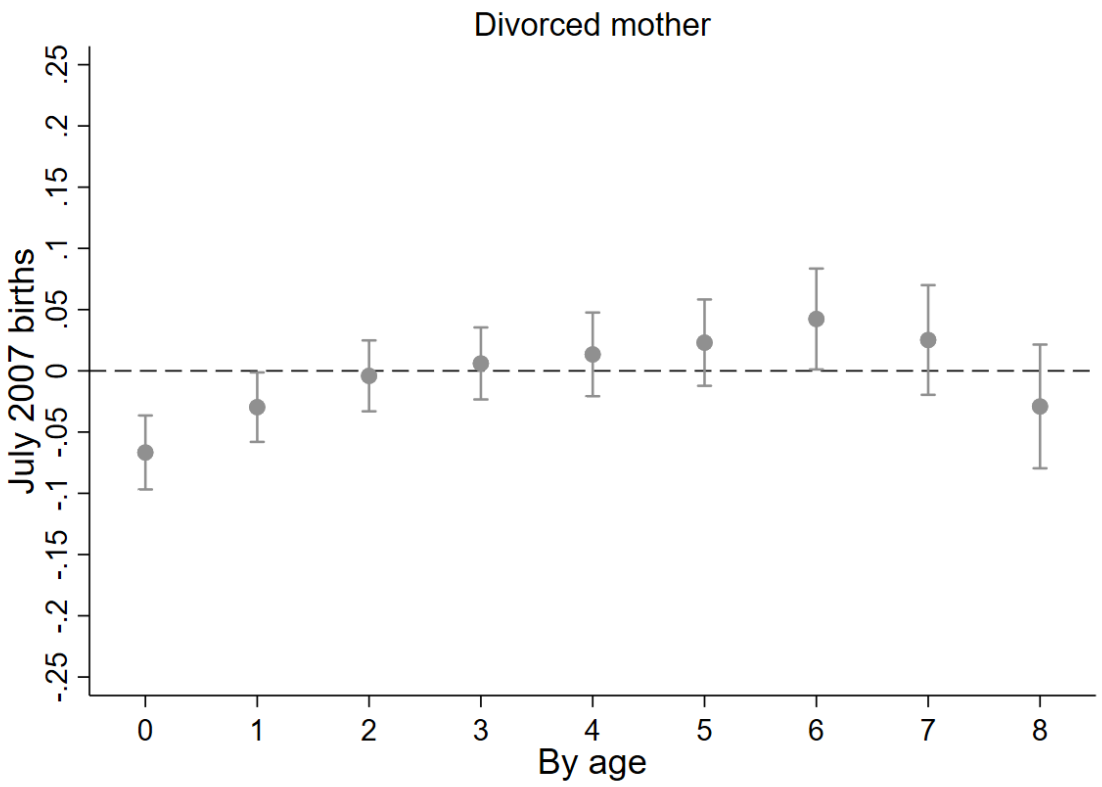
Subsequent fertility



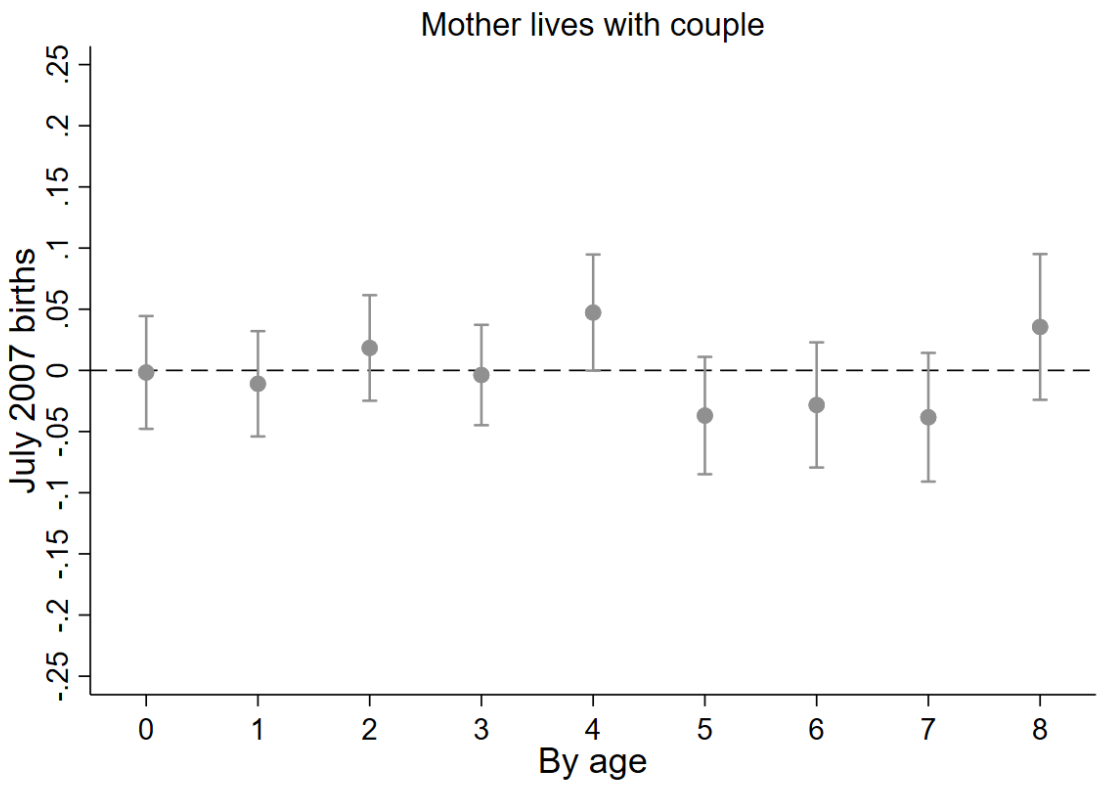
Mechanisms

Parental time investments and family structure

Divorced mother



Partnered mother



Mechanisms

- Significant impacts on:
  - Expenditure on big-ticket items

	Total Expenditure	Child-related Expenditure	Food Expenditure	Expenditure on Big-Ticket Items				
				Any	Appliances	Furniture	Home Repairs	Vehicles
Effect	0.0164	0.131	0.167	0.970**	1.088**	0.798	0.781*	0.703
	(0.095)	(0.176)	(0.107)	(0.468)	(0.512)	(0.577)	(0.461)	(0.585)
CI	(-0.2, 0.2)	(-0.2 ,0.5)	(-0.0, 0.4)	(0.5 , 1.9)	(0.1, 2.1)	(-0.3, 1.9)	(-0.1, 1.7)	(-0.4, 1.8)
Obs.	488	488	488	488	488	488	488	488

Mechanisms

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Obs.	488	488	488	488	488	488	488	488

Consistent with evidence on how EITC recipients spend their refunds (Goodman-Bacon & MacGranahan 2008)

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Comparison to previous studies

- We selected papers
  - Included in the latest literature review studies (Almond et al 2018, Cooper and Stewart 2020).
  - Set in Europe and North America estimating causal effects
  - Based on a natural experiment or randomized control trial experiment
    - Akee et al (2010), Milligan and Stabile (2011), Duncan et al (2011), Dahl and Lochner (2012, 2017), Black et al (2014), Aizer et al (2016), Cesarini et al. (2016)
  - Plus two very recent papers involving income shocks at birth
    - De Gendre et al. (2021) and Barr et al. (2022)

Comparison to previous studies

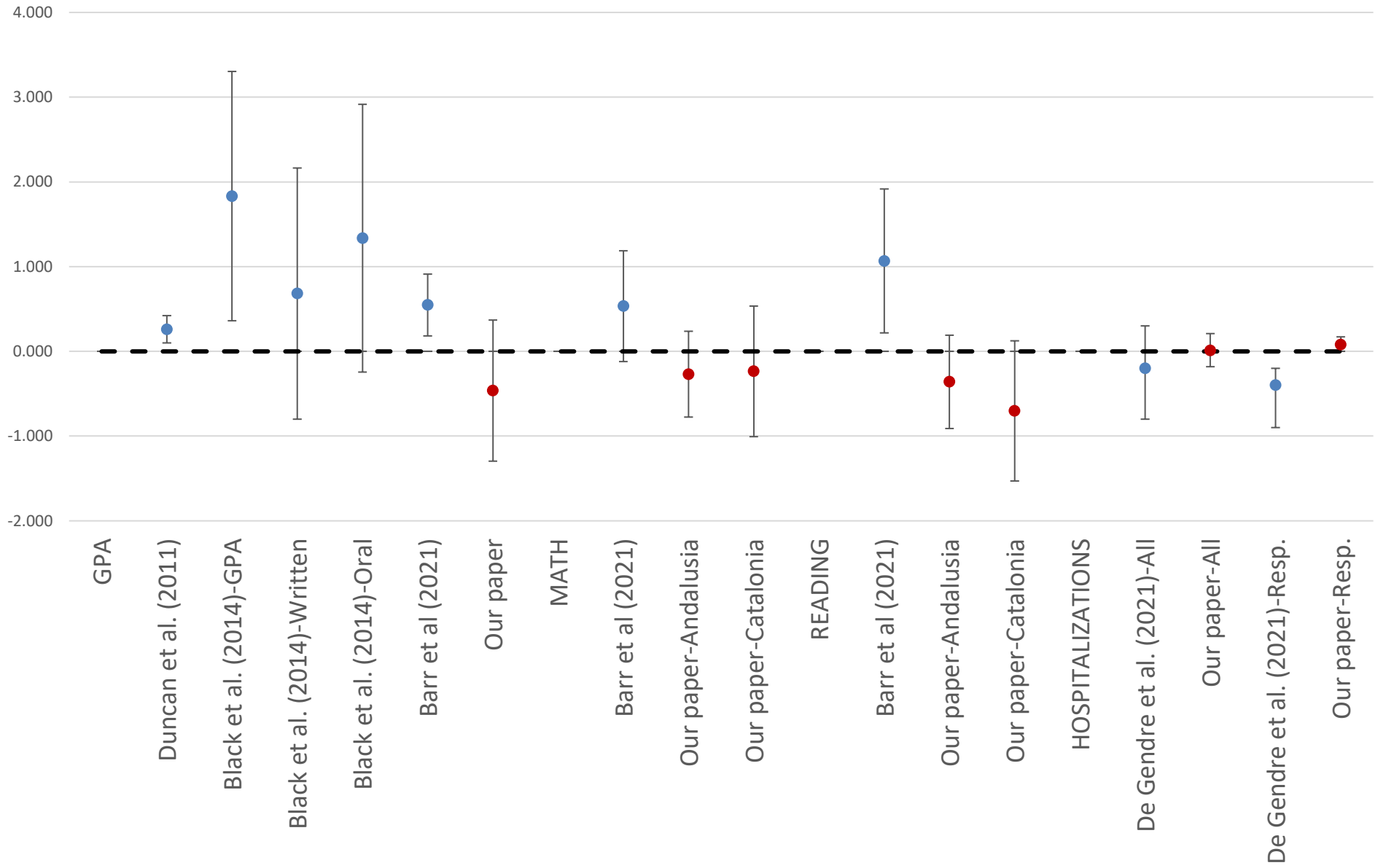
- Most papers report positive income effects on cognitive outcomes, with impacts ranging between 0.05 and 0.37 standard deviation units per $1,000 increase in annual income
  - Exception Cesarini et al (2016): Swedish lottery winners
- Evidence more mixed for health outcomes
  - Positive impacts (Aizer et al 2016)
  - No impacts (Milligan and Stabile 2011)
  - Both negative and positive impacts (Cesarini et al 2016)

Comparison to previous studies

Potential explanations for difference in estimates:

- Size of the income shock
  - Our study about $180 annuitized permanent income
  - Other papers with similar annuitized cash sizes do find positive impacts
    - Aizer et al (2016): $430, Duncan et al (2011): $350; Black et al (2014): $250; Barr et al. (2022): $60; De Gendre et al. (2021): $90

Comparison to previous studies



Comparison to previous studies

Potential explanations for difference in estimates:

- Heterogeneity in the outcomes measured and/or age at measurement
  - Considerable overlap: survey data (Milligan & Stabile 2011, Duncan et al 2011) and administrative data (Barr et al 2022)

Comparison to previous studies

Potential explanations for difference in estimates:

- Heterogeneity in the outcomes measured and/or age at measurement
  - Considerable overlap: survey data (Milligan & Stabile 2011, Duncan et al 2011) and administrative data (Barr et al 2022)
- Different targeted populations
  - No impact for low-income samples, unlike Akee et al (2010), Duncan et al (2011), Aizer et al (2016), Dahl & Lochner (2017)

Comparison to previous studies

Potential explanations for difference in estimates:

- Spanish income supplements and Swedish lotteries not conditional on household time use investments or expenditures
  - Unlike policies studied in other papers using conditional cash transfers (Duncan et al 2011, Black et al 2014) or in-kind programs (Deming 2009; Chetty et al. 2016) as documented by Hendren & Sprung-Keiser (2022)

Conclusion

- We show that the child benefit did not have any significant impact on children's human capital and well-being
  - from birth to middle childhood.
- We also show that it did not have any significant impact on any of the main mechanisms
  - Maternal labor force participation, partnerships, and subsequent fertility
- We find suggestive evidence of increased expenditure on big-ticket items as a result of the bonus
  - These investments did not translate into better parental employment outcomes, unlike Barr et al (2022) for the US

Conclusion

- We contribute to the debate on what types of policies are more likely to be effective in improving children's development
  - In-kind transfers (tied to a specific expenditure) might be more effective (Hendren and Sprung-Keyser 2020)
  - Cash-transfers received more regularly might allow better investments (Parolin et al 2021)
  - Benefits received during pregnancy might be more successful (Amarante et al 2016, Hoynes et al 2016)
- Spanish baby-bonus effective in increasing overall fertility and health at birth of subsequent offspring
  - As shown by Gonzalez (2013) and Gonzalez and Trommlerova (2021, 2022)

Conclusion

- Results interpreted in the context of a country with a very generous safety net for families with children
  - Caution if results extrapolated to different contexts
- We cannot know whether the effects on children's health and cognition remain latent for some time before re-emerging
  - As found by the previous early intervention programs literature (Garces et al 2002; Deming 2009; Chetty et al. 2016)

Thank you

Validity of the Research Design: Constant Pre-trends

- The impact of being born after the cutoff is constant over time

	Health Problems	Referrals
Effect	-0.158	0.001
	(0.321)	(0.046)
Mean/SD	3.998/4.809	0.159/0.624
Observations	14,510	14,510
Std. Coefficient	-0.033	0.002
Controls	No	No
Linear Trend	Yes	Yes

- Primary care data: placebo comparing 2006 with 2005

Validity of the Research Design: Constant Pre-trends

- The impact of being born after the cutoff is constant over time

	Math	Spanish
Effect	-0.039	0.014
	(0.044)	(0.043)
Observations	28,508	28,507
Controls	No	No
Linear Trend	Yes	Yes

- Education data: placebo comparing 2008 with 2007

Heterogeneity by socioeconomic status

	Health Problems 5/8	Referrals 5/8	Hospitalizations
Panel A. Low Income			
Effect	1.078**	0.022	0.074
	(0.510)	(0.062)	(0.052)
Mean/SD	5.946/6.712	0.261/0.831	0.811/0.082
Observations	9,811	9,811	122
CI in sd units	(0.01, 0.30)	(-0.12, 0.17)	(-3.4, 21.7,)
Panel B. High Income			
Effect	-0.221	-0.108	-0.013
	(0.770)	(0.093)	(0.042)
Mean/SD	5.259/6.080	0.204/0.705	0.568/0.057
Observations	4,527	4,527	122
CI in sd units	(-0.28, 0.21)	(-0.41, 0.10)	(-16.7, 12.2)

Heterogeneity by socioeconomic status

	Math in Andalusia	Math in Catalonia	Spanish in Andalusia	Spanish in Catalonia
Panel A. Low Income				
Effect	-0.047	-0.046	-0.093	-0.119
	(0.064)	(0.103)	(0.056)	(0.114)
Mean/SD	-0.143/1.064	-0.187/0.999	-0.169/1.049	-0.163/1.001
Observations	14,465	6,199	14,485	6,204
CI in sd units	(-0.17, 0.08)	(-0.24, 0.15)	(-0.20, 0.02)	(-0.34, 0.10)
Panel B. High Income				
Effect	-0.026	0.042	-0.007	-0.043
	(0.055)	(0.097)	(0.063)	(0.111)
Mean/SD	0.216/0.816	0.348/0.854	0.250/0.817	0.314/0.871
Observations	13,373	4,606	13,394	4,608
CI in sd units	(-0.13, 0.08)	(-0.15, 0.23)	(-0.13, 0.11)	(-0.26, 0.17)