Journal of Population Economics (2023) 36:295–320
https://doi.org/10.1007/s00148-021-00855-0

ORIGINAL PAPER

Baby commodity booms? The impact of commodity shocks on fertility decisions and outcomes

Francisco Gallego[1] · **Jeanne Lafortune**[1]

Received: 30 September 2020 / Accepted: 14 May 2021 / Published online: 22 June 2021

Abstract

This paper uses international commodity prices and local natural resource endowments as a source of plausibly exogenous variation in local Chilean economic conditions to study how these shocks impact fertility behavior of families in a small, emerging open economy where non-marital fertility is common but parental obligations are not well enforced. We find that these commodity shocks lead to an increase in the number of births and the birth rate. We argue that these results are consistent with most women experiencing an income effect and a limited substitution effect from commodity booms. This is confirmed by looking at groups that would have experienced a larger income than substitution effect: higher-order births, births within marital relationships, and those by mothers who do not experience an increase in their employment probability respond more strongly to these commodity booms.

Keywords Fertility · Commodity booms · Cyclicality

JEL Classification J13 · J11

1 Introduction

How do shocks to local economic conditions alter decisions related to fertility and family formation? Many previous studies have documented that birth rates are pro-cyclical, suggesting a positive relationship between income and births (from Galbraith and Thomas 1941, Silver 1965, Ben-Porath 1973 to the more recent work of

Responsible editor: Terra McKinnish

✉ Jeanne Lafortune
jlafortune@uc.cl

Francisco Gallego
fgallego@uc.cl

[1] Instituto de Economía, Pontificia Universidad Católica de Chile, Santiago, Chile

Black et al. 2013, Lovenheim and Mumford 2013). The impact of economic prosperity on fertility decisions is theoretically complex. The impact of these booms is likely to involve a substitution and an income effect. The substitution effect stems from the fact that in a boom, labor market conditions are likely to improve, making women's fertility decisions more costly to them if they choose to step out of the labor force even temporarily to have children (see Schultz 1985, for an example in historical Sweden). On the other hand, economic prosperity also implies additional resources through labor markets, which couples can choose to devote to having more children if the latter are a normal good. Which effect dominates is thus an empirical question.

This question is complicated since decisions related to labor market outcomes, fertility, and marriage are likely made jointly and thus any correlation between these variables may not provide a causal relationship. The main challenge for such an estimation is that even at the national level, an anticipated increase in fertility can imply lower labor supply among women, inducing a negative relationship between measures of economic prosperity, such as the employment rate and fertility rates. Furthermore, existing studies have not been able to explore issues regarding to birth order and family characteristics using a single experiment. Our setting, in a small, emerging open economy hit frequently by external price shocks, offers us this opportunity. We use shocks to local geographical entities within Chile stemming from variation in international commodity prices and their relative exposure to those shocks to study the causal impact of local economic booms on fertility decisions and parental characteristics.

The context of Chile around the turn of the twenty-first century is an interesting one. Fertility rates in Chile have been falling, but the period we study is one in which the fertility rate decreased dramatically from around 2.5 to 1.9 in less than 20 years, falling below the replacement rate. This decrease was accompanied by an increase in female labor force participation, rising from around 30 to almost 50% at the end of our period. This is also the period when the gender gap in tertiary education reversed, with more women completing a post-secondary degree than men. As emphasized below, this is also a period when births outside of marriage became the norm rather than the exception.[1]

We use changes in international commodity prices between 1994 and 2011 as an exogenous shock to local economic conditions. We use a wide set of commodities since there is a large regional difference in natural resources in Chile. In contrast, Feigenberg (2021) uses shocks related to copper and the difference in copper resources across regions to explore the impact of aggregate income shocks on school pricing, and Álvarez et al. (2021) use shocks to metal prices to demonstrate that commodity booms lead to lower poverty in Chile. We first demonstrate that international price variations in different commodities have significantly and differentially affected regions in Chile. In particular, wood products, salmon, agriculture,

[1] While teenage pregnancies are a relevant problem with around 15% of births coming from that age group during our analysis, youth marriages are prohibited, with the minimum age of marriage being 16 for both genders with parental consent and 18 without.

and mining (each a staple product of different regions of Chile) all experienced different timing of economic shocks over the period analyzed in this paper. Since we are worried about regions specializing in a given commodity in response to international prices, we measure specialization in a Census previous to our period of interest. We then "allocate" the importance of each commodity shock over time to a county (comuna) depending on its past labor force participation in each sector. This period includes both a permanent increase in commodity prices and cyclical fluctuations. More interestingly, we find that there is important variation across counties in both the timing and intensity of commodity shocks. Given that Chile is a small producer in the world market, it is not likely that these shocks could be endogenous. Thus, we argue that this is a good setting for exploiting an exogenous shock in economic conditions.

We first show that changes in this "price index" significantly affected economic conditions at the local level. Employment, particularly in agriculture and mining, increases in counties when the commodities of which they are better endowed face higher international prices. We show evidence that for most women, these commodity booms are likely to have had a larger income than substitution effect since they seem to increase male more than female employment. Women without children are likely to experience the largest substitution effect since the largest wage cost for a woman would be for her first child. Women who already have children are less likely to be in the labor force and thus more likely to experience an income effect. Married women may also experience more of an income effect than single women since the latter are likely to face more difficulties in obtaining payments from the father of their children. We also document that income and substitution effects differ substantially depending on the educational attainment of the woman. Women in the lowest educational group are no more likely to be employed when faced with a positive commodity boom. However, we find that other members in their households are likely to face better employment prospects. Thus, for them, commodity prices appear to generate mostly an income effect. For women in the middle of the educational distribution, we find the complete opposite: their own employment increases but not that of their household members. This suggests that these women would mostly face a substitution effect and a limited income effect. Finally, for the most educated women, we observe that both their own employment and that of their household members are likely to increase. For them, we thus anticipate that both effects should be at play.

We then estimate the causal impact of these changes in economic conditions on fertility decisions using the census of all births in the country from administrative records. We match the predicted change in economic conditions due to the internal price shocks with the fertility outcomes at two points in time: conception and birth, at the quarterly level.[2] We find evidence that better economic conditions at the time

[2]Our strategy thus assumes that individuals who wish to conceive can do so within 3 months. This is a strong assumption since it can take up to a year to a fertile couple to conceive when they wish to do so. The speed at which one can conceive also critically depends on the method of contraception previously used. In 2017 (the only year for which we were able to find comprehensive information regarding the use of contraceptives in the adult population), only 54% of the surveyed adults declared using contraception and the most used method was condoms at 26%, followed by oral contraceptives at 16%. This pattern is likely

of conception increase the number of births and birth rates in the county, suggesting little role for the substitution effect and a much more important role for the income effect. No effect is found for economic conditions at the time of birth on the number of births. A 1% increase in the weighted price index of a county increases the number of births by 0.3% and the birth rate by 0.09 per 1000 woman (from a base of about 15). The average county in our sample experienced a change in their price index (above that of national variations) of 20%, implying that such a shock would generate an impact of about 12% of the average birth rate and about 6% in the number of births. When dividing the impact between a "permanent" and a "transitory" effect, we find that potential mothers respond more strongly to the permanent component than the transitory one.

The fact that more income could lead to higher fertility is central in a Malthusian framework. Studies using historical data from England (Lee and Anderson 2002), Sweden (Lagerlöf 2015), and Europe (Galloway 1988) showed evidence of this pattern historically as fertility responded to short-term variation in grain prices. Our study looks at the post-Malthusian era, where the link between income and fertility may have weakened, as discussed by Chatterjee and Vogl (2018).

The results we obtained are consistent with three recent studies that have looked at local income shocks and their link to fertility. Black et al. (2013) find that coal-rich US counties that experienced a boom due to rising energy prices had also an increase in completed fertility. Lovenheim and Mumford (2013) find that increases in a family home value increase the probability that a family will have a child, and Dettling and Kearney (2014) show that this is only the case for home owners, whereas higher housing prices decrease the fertility of renters. Kearney and Wilson (2018) look at the fracking boom and show that it increased fertility but not marriage. Compared to these studies, we argue that our setting is particularly interesting since we are looking at a context in which commodity shocks affected men and women differently: the shocks influenced more strongly employment rates for men and through this household resources available to women. This suggests that the income effect of the commodity boom would thus be likely to dominate the substitution effect for women. Furthermore, we are able to provide heterogeneous treatment effects that highlight the potential role of income versus substitution effects for different groups.

Unlike previous studies, we also document that the intensive margin of fertility, not the extensive margin, seems to respond to these shocks. The number of firstborns

to have been even stronger during most of the period of our study. This would imply that couples who wish to conceive would be able to abandon their method of contraception (or absence of it) easily leading to relatively quick impact on conceptions. Other studies have found short-term effects on births. Rodgers et al. (2005) find a short-term and local impact of the Oklahoma City bombing, Nandi et al. (2018) of the 2001 Gujarat earthquake. In poorer countries with worse access to modern contraception, the short-term effects are particularly visible. Alam and P ortner (2018) find that women have a lower probability of being pregnant within 7 months of a crop loss and lower fertility within 7 to 14 months. Davis (2017) also finds short-term impacts (within 2 years) of Hurricane Mitch. In the context of Chile, Hinojosa (2018) finds an immediate and short-lasting increase in births in the four months following the large earthquake of 2010. Similarly, Rau et al. (2017) find that a large increase in the prices of oral contraceptives led to very immediate increase in births. Nevertheless, we recognize that our impact may be underestimated if some couples need a few quarters to respond.

does not respond to the price shocks and the birth order rank and the number of children ever born to a woman increase, implying that the average increase in births we document comes mostly from the intensive margin. This suggests that while the timing of starting a family may not be very respondent to the economic conditions, the decision whether to have additional children does depend on income shocks. This again suggests little role for substitution versus income effects since the largest wage cost for a woman would be for her first child. This is also consistent with the very low levels of labor force attachment of mothers in Chile, suggesting that for those who already have children, the income effect is likely to be much more important than the substitution effect.

We further document that the number of births increases only for married women in these periods. This differs from the results of Kearney and Wilson (2018), where the fracking boom does not alter the fraction of births that occur out of marriage. The authors suggest that this is different than what happened with the coal boom in an earlier period because the stigma for out-of-wedlock births had decreased in the USA. However, we do see a fall in single motherhood in a sample that has a majority of birth occurring to unmarried mothers, suggesting that something else than social norms may explain the difference between the impact of fracking and coal. We hypothesize that this is because during this period Chile had limited rules in place to enforce child payments. Only in 1998 did children born out of wedlock obtain the same rights as those born within marriage, and a substantial reform to child payments only occurred in 2000 (for a discussion of the impact these changes may have had, see Martínez 2013). Those two dates occur within our sample. Even today, close to 80% of child support payments are not fulfilled. We thus suggest that part of the reason for Kearney and Wilson (2018)'s results may have been linked to the fact that modern USA has strong enforcement rules for child payments that do not require marriage for the income effect to operate.

In an important contribution compared to previous studies, our dataset also allows us to explore whether the characteristics of mothers and fathers change in a way that is consistent with the hypothesized changes in income and substitution effects for each group. When a county receives higher international prices for its commodities, fewer babies are born to parents in the middle of the educational distribution, suggesting a polarization in times of economic prosperity. This is exactly the group for which we predicted a limited income effect and a significant substitution effect. We also observe that this group experienced a strong fall in the number of firstborn births but no impact on births to single mothers. This is consistent with the substitution effect particularly affecting family expansion among married mothers. For low-skill women, we observe no increase in births of firstborn, but an increase in higher birth orders. This suggests that the income effect made these women more likely to expand their family. Furthermore, we observe a large fall in the number of births to single mothers in this skill group, suggesting that without marriage, these women did not experience a substantial income effect. Finally, for high-skill women, we do see an increase in the number of firstborns, implying that income effects for that group appear to have allowed some women to start a family. We also see an increase in the births to single mothers in that skill group, probably because these women are able to obtain more resources for their children even though they are not legally married

to the father of these children. This suggests that our interpretation of the aggregate effects as being driven by a dominant income effect can also explain differences by educational attainment.

Our paper makes several contributions to the literature. First, we use a plausibly exogenous shock to local economies in the context of an open economy. This allows us to identify effects that are not affected by obvious endogeneity problems. In contrast to the rest of the literature, we document the impact of current economic conditions using high-frequency data, allowing us to focus on the short-run effect of these shocks on fertility decisions.[3] Second, we are able to map the effect of commodity prices shocks to labor market opportunities of parents and other household members, which differently affect several groups. This allows us to identify how substitution and income effects may be affecting different economic agents. Third, our dataset allows us to identify how the characteristics of parents vary when faced with different economic conditions. In particular, we identify that most of the increased fertility occurs through the intensive margin. This allows us to confirm the fact that commodity booms may have both substitution and income effects and that those differentially influence some groups within the population. Finally, a significant contribution of this paper is that we can tackle all these dimensions of the effect of economic conditions on fertility outcomes in one study using the same source of exogenous variation.

The rest of the paper is organized as follows: the next section presents the empirical strategy and Section 3 discusses the data used in our paper. Section 4 then presents the empirical results and the last section concludes.

2 Empirical strategy

The key empirical relationship we wish to estimate is how local economic conditions may influence fertility decisions. Denote an outcome Y_{ct}, e.g., the average outcome of births in county c that were conceived at time t. We would like to correlate this with measures of how well the economy was doing, at that moment in time and in that particular geographical location. We consider commodity price shocks as an exogenous determinant of economic conditions (Z_{ct}). Given the panel nature of our dataset, we can think of the regression we estimate as:

$$Y_{ct} = \beta Z_{ct} + \nu_c + \delta_t + \varepsilon_{ct} \tag{1}$$

where ν_c identifies county fixed effects and δ_t time fixed effects. Thus, we identify the effect of Z_{ct} from within-county, within-time variation. The standard errors are clustered by county to allow flexibly for serial correlation within each county.

Using commodity prices allows us to solve the omission of relevant and unobservable factors biases in the estimation of β when using proxies of local economic activity, such as the employment rate. These omitted variables may be own income, household income, employability, education, etc. We are further plagued by a problem

[3] However, we also show indication that it seems to be the "permanent" component of the shock that elicits fertility responses.

of simultaneity since it may be that fertility influences the fraction of women in the labor force and through that, the employment rate.

We will thus propose an index of shocks generated by international commodity prices. This variable is a source of variation of local economic conditions that would be uncorrelated with local fertility patterns and labor supply decisions. However, since we want an index at the local geographical level, we must generate a way in which these international commodity prices affect different regions of a country at the same time. Our solution is to weight the impact of different commodities by the importance of each commodity in the local economy. While this can be done in many different ways, the easiest one and the one for which we have the best data is the employment share of these sectors in a period before shocks are measured. Formally, our commodity price index be

$$Z_{ct} = \sum_{i=1}^{7} \frac{N_{ci1992}}{N_{c1992}} * P_{it}$$

where N_{cit} represents the number of workers employed in sector i, in county c in quarter t, and P_{it} the price index of sector i over time.[4] Thus, our price index documents in which regions would the price of production have increased more rapidly if the industrial composition had remained the same as in 1992 since this is closest census to the data we are analyzing that is completely available in summary tables by Redatam, a subdivision of CEPAL (Redatam 2013). We thus prevent regions from increasing their involvement into a particular sector in response to price shocks to make sure that supply factors do not influence our measure.[5] We include 6 different sectors of relative importance in Chile, namely copper, other metal mining, agriculture, petroleum, fisheries, and forestry. We also include a residual sector which includes services and manufacturing so that the weights sum to 1 in each county. We assign to these sectors prices of commodities that are relevant to them: copper, metals, food, petroleum, fish flour, and cellulose, respectively. We obtain the first two from the International Monetary Fund (International Monetary Fund 2013) and the last four from SOFOFA (Sociedad de Fomento Fabril F.G. (SOFOFA) 2013). For the residuals, we assign it the Wholesale Price Index for domestic manufacturing, published by the Chilean Central Bank. We normalized all price indices to be equal to 1 in 2005.

For example, let us assume that county A had 80% mining and 20% of agriculture and county B had no mining activity but 90% agriculture in 1992. Our price index thus would imply that when food prices are on the rise, the second county would have a larger predicted price index while it would not influence much county A. On the opposite, if copper prices are increasing, then county A would see its economic activity improve but not county B. Given that our regression includes fixed effects

[4]There is non-trivial weekly migration between the central zone of the country and some of the resources-rich areas. This will be captured in our employment share measure since this is obtained from the Census of population which records the location of an individual the night before the day of the Census.

[5]This does not imply that these changes did not occur. Changes in industrial composition at the county level will simply weaken the first stage relationship between our instrument and the endogenous variable.

for county and quarter, we are exploiting how the change over time in county A will differ from county B, taking into account that these two counties are very different and thus may have different levels of fertility.

What are the challenges with using this index as a valid source of exogenous variation? First, we may think that the importance of a commodity in a given county is endogenous to the changes in the prices that would be anticipated and somehow correlated with fertility decisions. However, we can easily think that the specialization levels of each region in Chile are much more likely to be correlated with natural resources endowment than with anticipated price shocks. Thus, it is unlikely that changes in endowments would be correlated with fertility decisions, in particular at a quarterly frequency, which is the time-horizon our data provides us with. There is also no evidence that 1992 would be a particularly special year in Chile's economy, thus leading us to believe that using the fractions from that year would not likely contaminate our results.

As is usual in this type of proxy, we must also show that the price indices are not responding to local conditions. Given that we use prices of commodities determined on world markets, it is difficult to believe that Chile, a relatively small economy, would have a large influence over those. The only good where Chile is a relatively large player is copper where it gathers about 35% of the world's production.[6] Despite that, the market for copper is determined on metal stock markets with strong competition forces. We thus see as unlikely that local economic conditions in copper-producing regions of Chile would significantly affect world prices. Furthermore, a large amount of fluctuations in the price of copper over the period we are analyzing was more linked to demand forces than supply.

Figure 1 shows graphically the type of variation we will employ. In panel A, we present the variation in the price index at the level of each commodity. We see that while all commodities were more volatile than the rest of the economy, there are differences across commodities. Before 2005, agriculture, forestry, and fisheries saw less of an increase in their prices than other commodities. The appreciation of prices post 2005 was much more marked for copper and other mining. Fisheries did not experience a boom between 2005 and 2009 but did so post 2010. Our instrument aggregates these fluctuations differently depending on the influence of each sector. While the agricultural sector is the largest commodity sector of the country, every county has a specialization that allows our index to differ depending on its industrial composition. In panel B, we show this for three specific counties. Andacollo is a county of the northern part of the country (what is often called "Small North") and is a mining town (gold and copper). Its price index will thus mostly reflect fluctuations in the price of mining commodities. Rio Claro is a county in the middle of the country that is well known for its agricultural products. It will thus mostly be influenced by agricultural prices. Finally, Guaitecas is a comuna in the southern part

[6]The second largest producer of copper is Peru which produces less than 15% of the world's production. China is next with around 10%, then the Democratic Republic of Congo and the USA, each with 8% of the world's production. Australia, Russia, Zambia, Mexico, Kazakhstan, Canada, and Poland all capture between 3 and 5%. Thus, the copper market includes a few large players and a number of smaller marginal producers which reduces the capacity of Chile to manipulate the price.

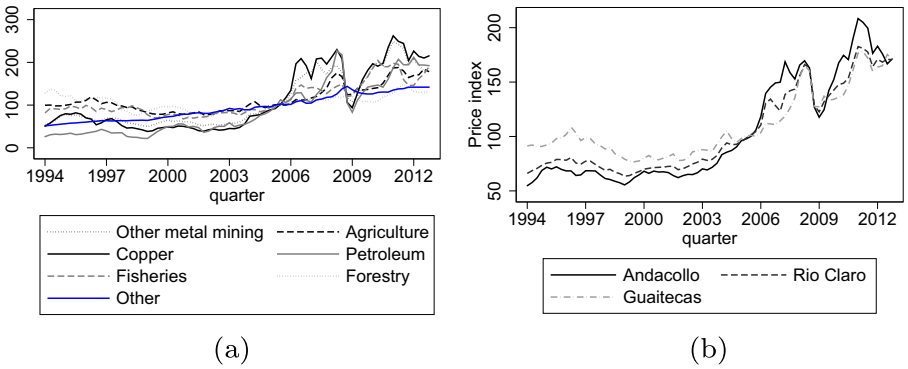

Fig. 1 Commodity price index for random sample of counties. **A** Commodity price indices. **B** Three specific countries

of the country that is very concentrated in the fisheries sector. While we observe the three counties experiencing an overall positive trend, they each have differences in the timing and the volatility they experienced. Guaitecas sees the lowest growth over this period, until the post 2010 fishery boom we discussed before. Rio Claro sees a faster growth before 2005 and then sees variations similar to Guaitecas in the post 2010 period. Finally, Andacollo almost replicates the time-series for copper and other mining prices. It is this type of variation that we will employ in our empirical strategy below.

3 Data

To perform the estimations we presented above, we need to measure birth outcomes at a high frequency in each county of Chile over a long enough horizon to observe commodity price fluctuations. We employ the census of all births in the country between 1994 and 2012, from the Departamento de Estadísticas e Información de Salud, DEIS, which is part of the Ministry of Health (Departamento de Estadísticas e Información de Salud 2013). While not freely accessible, researchers who justify the purpose of their query can obtain access through a web page. The database includes every single birth, codified by the date of birth of the child and the county of residence of the mother. The database is amazingly rich including information regarding the gender, weight, and gestational age of the baby and details regarding the delivery (doctor versus midwife, at home versus hospital, etc.). It also includes a large set of information regarding the parents: education, age, urbanicity, marital status, labor force status, and occupation. However, it does not include information about prenatal care nor actions taken by mothers during their pregnancy. Aggregating by quarter of conception and county leads to a group of 346 counties and 74 periods for a total of 24,890 observations. To construct natality rates, we must also count on the number of women in each county. We rely on the intercensal estimates of the Instituto Nacional de Estatisticas, INE (Instituto Nacional de Estadísticas 2013b).

In addition, we implement a robustness check of our empirical strategy using conditions in place at the moment of birth, given that our theoretical argument concerns conditions at the time of conception and not at the time of birth. In order to implement this exercise, we also aggregate the data by quarter of conception, quarter of birth, and counties. In general, quarters of conception have 2 and up to 3 different quarters of birth link to them (premature, on schedule and late births). This makes our database much larger reaching more than 50,000 observations. Nevertheless, most results are unchanged by the use of one aggregation or the other.

We also use data for several economic outcomes. We use two sources of information. First, we use employment rates at the regional level from INE (Instituto Nacional de Estadísticas 2013a). Since we do not have the information at the county-level, we assign to each county the data for the employment level of the region it belongs to. Second, we also use the household survey (CASEN), which is available at the county level but at a bi- or tri-anual basis, leaving us without the high frequency variation we sought to generate. Using this survey, we construct employment rates for the complete population, for different economic sectors and for men and women at the county-year level. This survey also allows us to identify variation in economic outcomes by parents' educational levels, which we exploit to understand mechanisms behind our results.

Table 1 presents the main characteristic of our basic sample. On average, we observe about 180 births per quarter per locality, translating into an average rate of 14 births per 1000 women. Of these births, about 42% are first births while the average birth makes the woman have her second child. This period is one where children are more and more likely to be born out of wedlock with, on average, about 57% of births being to unmarried mothers.[7] About 4% of both mothers and fathers report high-earning occupations. This is even lower than the fraction who completed university education (13% of women, 15% of men). On the other hand, 57% of mothers and 50% of fathers had not completed high school.[8] Average mother's age is about 26 years.

4 Empirical results

4.1 Commodity prices and economic outcomes

In order to understand how commodity booms can impact birth outcomes, we explore how our price index measure impacted economic variables. We demonstrate that there is a strong relationship between our predicted price index and the proxy for economic activity we have available for a big sample, namely the employment rate. Table 2 presents the relationship between economic activity and the commodity price index. We find a very strong relationship between the employment rate and the index,

[7] Chile now is the OCDE country with the most out-of-wedlock births at 71.1% in 2014.

[8] Father's educational attainment categories do not sum to 1 because some birth certificates do not report a father and his characteristics.

Table 1 Summary statistics

Variable	Observations	Mean	Standard Deviation
Fertility outcomes			
Births by county	24,938	181.14	295.41
Ln (births)	24,938	4.145	1.609
Birth rate	24,894	14.75	19.03
Firstborn	24,409	0.418	0.111
Number of children	24,409	2.019	0.336
Number of births	24,409	2.060	0.353
Mothers' characteristics			
Single	24,409	0.575	0.168
High-rank occupation	24,409	0.039	0.069
University educated	24,396	0.135	0.135
High school educated	24,396	0.294	0.150
Less than HS education	24,396	0.571	0.198
Age	24,383	25.797	1.691
Fathers' characteristics			
High-rank occupation	24,409	0.044	0.078
University educated	24,409	0.150	0.147
High school educated	24,349	0.278	0.153
Less than HS education	24,409	0.494	0.185

Authors' calculations at the county*quarter of conception level using information from the Census of Births (Departamento de Estadísticas e Información de Salud 2013)

whether we measure it at the regional level but at quarterly frequency, like in column (1), or at the county level but at an bi-annual frequency in the CASEN, like in column (2). We then show that the results are similarly strong at the beginning or at the end of the period (in the next two columns). Finally, we also show that we can exclude some key regions like the metropolitan region of Santiago or the North (where most of the mining industries are concentrated) and the results remain as strong, as can be seen in the last two columns.

In order to convince the reader that our price index does capture the margin we wish to measure, Table 3 further studies the effects of commodity prices by dividing the sample by the sector of the economy and gender. Results confirm the empirical validity of our index. Commodity-intensive sectors (agriculture and mining) expand when international price shocks raise the value of their output while manufacturing shrinks (it is negatively impacted by the terms of trade that are generated by commodity booms). Service sectors are unaffected. While not reported here, we observe limited impact on labor income conditional on employment, emphasizing the role of employment as being more relevant than wages for the average population.

Since our hypothesis is that the impact of the commodity boom on natality will depend on the relative strength of the substitution and income effect, it is important

Table 2 Impact of exogenous economic shocks on employment rate

Sample	(1) INE	(2) CASEN	(3) INE Before 2002	(4) INE After 2002	(5) INE Ex. Santiago	(6) INE Ex. North
Price index	0.025***	0.031**	0.024***	0.021***	0.018***	0.015***
	(0.005)	(0.014)	(0.008)	(0.006)	(0.007)	(0.005)
R^2	0.846	0.735	0.868	0.866	0.815	0.867
N	24,938	2259	12,132	12,806	21,090	21,756
Mean (s.e.) of	0.499	0.483	0.490	0.507	0.491	0.498
dependent variable	(0.040)	(0.069)	(0.037)	(0.041)	(0.039)	(0.040)

Each column represents a different regression as in Eq. 1. In columns 1 and 3 to 6, the outcome is the employment rate as measured by the INE at the regional-quarter level. In column 2, the outcome is the employment rate measured from CASEN at the bi- or tri-annual frequency by county. Column 3 includes only years before 2002, while column 4 includes only those after that year. Column 5 excludes the metropolitan region of Santiago, while column 6 excludes regions I, II, III, and IV. All regressions include time and county fixed effects. Robust standard errors clustered at the county level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

to detail whether female employment is more likely be affected by these booms. The last two columns of Table 3 show the impact of the price index on male and female employment respectively. As anticipated, given that the commodity sector is highly male-dominated, we observe that, on average, only the employment rate of males increased while that of women appears to be unaffected. Overall, these results would point to commodity prices impacting natality mostly through an income effect for mothers as long as resources can be transferred from fathers to mothers.

Table 3 Impact of exogenous economic shocks on employment rate by sector and gender, CASEN

	Agriculture (1)	Manufacturing (2)	Mining (3)	Services (4)	Men (5)	Women (6)
Price index	0.041**	-0.026***	0.015*	0.001	0.037**	0.012
	(0.017)	(0.007)	(0.008)	(0.017)	(0.018)	(0.019)
R^2	0.860	0.730	0.892	0.868	0.638	0.304
N	2259	2259	2259	2259	2259	2259
Mean (s.e.) of	0.143	0.048	0.013	0.278	0.669	0.304
dependent variable	(0.109)	(0.033)	(0.032)	(0.106)	(0.076)	(0.093)

Each column represents a different regression as in Eq. 1. The outcome is the employment rate, as measured in the CASEN, in the agricultural sector (column 1), in the manufacturing sector (column 2), in the mining sector (column 3), in the service sector (column 4), for men (column 5), and for women (column 6). Regressions include time and county fixed effects. Robust standard errors clustered at the county level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

4.2 Heterogeneity in income versus substitution effects

On average, we thus see that women may be faced with a stronger income than substitution effect. We now detail how this could differ by type of woman.

First, we may think that the relative size of the income and substitution effect may depend on the number of children already born to a woman. Women who already have children are much less likely to be in the labor force. The substitution effect they would face is thus less strong. On the other hand, the income effect could be affecting them more importantly. Women who are deciding when to start a family may particularly suffer from a substitution effect since the wage costs of having children appear immediately at the birth of one's first child (Kleven et al. 2019). We thus anticipate that the substitution effect would decrease the birth of firstborns while the income effect may increase that of births of higher birth order.

Second, the role played by the income and the substitution effects may differ depending on the marital status of the woman. Married mothers are likely to experience a stronger income effect as their husbands may experience higher incomes thanks to the commodity booms. Within marriage, transfer of resources is easier and guaranteed by law. For unmarried woman, the income effect is likely to be smaller since their spouse may not be present. Furthermore, Chile over this period had limited rules in place to enforce child payments. Only in 1998 did children born out of wedlock obtained the same rights as those born within marriage and a substantial reform to child payments only occurred in 2000 (for a discussion of the impact these changes may have had, see Martínez 2013). Those two dates occur within our sample. Even today, close to 80% of child support payments are not fulfilled by fathers. This suggests that the income effect is likely to be much stronger for married than unmarried mothers.

Finally, we can hypothesize that the income and substitution effects may differ strongly depending on the educational attainment of each woman. To explore that possibility, we constructed a sample of women aged 15–40 who were in a cohabiting or married relationship in each CASEN and separate them in each of our three educational categories.[9] The sample we obtain is older than our sample of new mothers (27.1-year-old on average) and is also less educated, because we are capturing older cohorts of women in a country where the gains in educational attainment have been fast-paced.[10] We regressed whether they were employed (in the first) and the fraction cohabiting adults in their households who were employed (in the second column) on our price index adding fixed effects for each county and year. We also include a control for age since working patterns are highly dependent of age. The results are presented in Table 4 where the first panel includes women with less than high school, the second focuses on high school–educated women, and the bottom panel on those with university education. For low-skill women, we document no gain in

[9]Results are similar but a bit less precise when using all women. In our dataset, more than 15% of all births in Chile occurred to women between the ages of 15 and 19, thus justifying our lower age limit. On the other hand, less than 2% of all births occurred to women above the age of 40.

[10]The fraction of women who are university-educated is 7%, 39% has a high school education while 54% have less than high school.

Table 4 Impact of exogenous economic shocks on employment, by education of the mother

	Less than high school diploma		High school diploma		University	
	Employed	Emp. of other adults in hh	Employed	Emp. of other adults in hh	Employed	Emp. of other adults in hh
	(1)	(2)	(3)	(4)	(5)	(6)
Price index	−0.006	0.039**	0.062**	−0.013	0.114**	0.069**
	(0.021)	(0.017)	(0.025)	(0.016)	(0.053)	(0.034)
R^2	0.095	0.195	0.081	0.177	0.197	0.208
N	143,045	143,045	111,241	111,239	18,145	18,143
Mean (s.e.) of dependent variable	0.205 (0.404)	0.634 (0.325)	0.391 (0.488)	0.671 (0.315)	0.601 (0.490)	0.713 (0.315)

Each column and panel represents a different regression as in Eq. 1. The outcomes of interest are a dummy for being employed in odd columns and the fraction of other adults in the household who are employed in even columns. Regressions are run at the individual level including time and county fixed effects and controls for age with robust standard errors clustered at the county*year level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

employment. However, we observe that other members of their households are likely to be more employed when faced with positive commodity shocks. We thus anticipate that these women would face stronger income effects than substitution effects. For those in the middle of our skill distribution, we find positive employment effects for women and no change in the probability of employment for other household members. This suggests that for this group, the substitution effect is likely to dominate. Finally, for high-skill women, we find a positive impact of commodity booms on both their own employment and that of other household members. This suggests that both income and substitution effects would be at play for this group.

We will thus explore whether these predictions hold in our empirical results.

4.3 Impact on natality

Having shown that our price index is strongly altering local economic conditions through employment changes, we now turn to exploring how these local economic shocks affected fertility-related outcomes. In Table 5, we show the effect of our commodity price index on several measures of fertility. In column (1), we look at the birth rate while in column (2), we present the log number of births. We find evidence that an exogenous shock to local economic conditions leads to higher fertility, as measured by both the birth rate and the log number of births. This replicates existing results causally estimated in other contexts, that is evidence that more income leads to more births. In terms of economic effects, an increase in the weighted price index of a county of 1% increases the number of births by 0.3% and the birth rate by 0.09 per 1000 woman (from a base of about 15). Given that the average county in our sample experienced a change of the price index of 20% over the period, this imply that such a

Table 5 Impact of exogenous economic shocks on fertility

	Birth rate	Log births				
		All	Firstborn	Higher order	Single moms	Married moms
	(1)	(2)	(3)	(4)	(5)	(6)
Price index	8.840*	0.269***	0.020	0.214***	0.090	0.243**
	(5.225)	(0.097)	(0.071)	(0.057)	(0.092)	(0.105)
R^2	0.145	0.967	0.957	0.965	0.961	0.951
N	24,894	24,938	24,409	24,409	24,938	24,938
Mean (s.e.) of	14.749	4.145	3.361	3.698	3.565	3.241
dependent variable	(19.033)	(1.609)	(1.506)	(1.450)	(1.576)	(1.652)

Each column represents a different regression as in Eq. 1. The outcome of interest is the number of births per fertile women in column 1, the log number of births in column 2, the log births that are firstborn in column 3, the log births that are not firstborns in column 4, the log births to single mothers in column 5, and the log births to married mothers in column 6. Regressions include time and county fixed effects. Robust standard errors clustered at the county level in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

shock translated into a 12% increase of the average birth rate and about 6% increase in the number of births. Our estimate for the birth rate is noisier than the one for the log births. This is likely to be due to the fact that the first one requires an estimate of the population of women in each county which is imprecise. This added noise in our outcome variable is likely to decrease the precision of our estimator as compared to the one of log births. One may worry that this result is spurious and driven by trends in the data. While it is really impossible here to use an event study methodology to claim that only economic conditions at the time of the conception are responsible for the effect we document, we find that including the values of the price index for the quarter previous to conception does not affect the magnitude of the coefficient of the conception's price index on the log number of births and the birth rate, although it decreases precision. Appendix Table 9 shows this.

As additional checks, we include region time quarter fixed effects to the regression. As shown in the top panel of Appendix Table 10, we find that the coefficients on birth rate and log births are similar in magnitude but loose their significance once we add this more demanding specification. Our regressions are also unweighted. To check whether this is driving our results, we replicate the results of Table 5 where we now weight by the population of each county in the bottom panel of Appendix Table 10. We find that, if anything, our results grow stronger by employing population weights.

One may be worried that the positive effect we find on natality would be due to migration of women to booming zones, leading to a higher number of babies being born because more women are locating there. This would not be the channel we are hypothesizing is at play. Appendix Table 11 shows that there is no indication that the total population nor the fraction of men in a given county increased in response to an increase in local economic conditions. We measure total population, in the first column, through the extrapolation provided in the Census; in the second column, we use

the CASEN survey.[11] The third column looks at the fraction of males in the economy. We find no statistically significant coefficient in the table. We continue by looking at whether the fraction of the population that is married increases. However, in CASEN, we can only jointly identify married and cohabiting couples. Nevertheless, we do not see a response to the fraction of individuals in such relationship. Finally, the last two columns show that there is no change in the fraction of the population in our two lowest educational category. The share of our excluded group (university) increases significantly in response to the shock but not by a large fraction. This suggests that the pattern we identify is not driven by migration.

4.4 Heterogeneity by birth order

However, this positive effect could be linked to either some women having more children or to more women deciding to have children. Our data, compared to previous work, has the richness to allow us to make this distinction, as shown in columns (3) and (4) in Table 5. Column (3) regresses the log births that were born from first time mothers in each quarter on our price index at the moment of conception while column (4) focuses on the log birth of those that were of higher birth order.[12] What our results suggest is that exogenous local income shocks do not lead to births to first-time mothers and more likely to increase higher-order births.[13] This is robust to the inclusion of region-quarter fixed effects and to weights, as shown in Appendix Table 10. This is in accordance to our predictions regarding the relative importance of the income and substitution effects depending on birth order. Only women who are already mothers appear to have a sufficiently large income effect to counteract the potentially negative substitution one. The magnitudes are such that the change in the price index experienced by the average county over this period altered the number of births to first time mothers by about 4% and increases the number of births that a mother has had by about 0.03.

4.5 Heterogeneity by marital status

The last two columns of Table 5 estimate the impact of better economic conditions on the log births to single and married mothers respectively. We find that the increase in babies conceived in times where local economies are benefited by international commodity price booms is concentrated exclusively in married mothers. The impact is noticeable with a 1% increase in the price index translating into a 0.25% higher

[11] Notice that the intercensal extrapolations for population we use in column (1) combine the use of linear interpolations for the 1992–1999 period and adjustments for mortality, fertility, and internal and international migration for the 2000–2011 period. Notice also that the data used in column (2) come from the population surveyed in the CASEN survey (which considers dwellings and not population as the basic unit of data collection) and therefore do not depend on intercensal extrapolations and would accurately measure migration.

[12] We obtain results that are consistent when using as an outcome variable the average rank of the birth. Results using the average rank of the child are extremely similar because of the low rate of stillbirths.

[13] In analyses not reported here but available upon request, we find that there is no effect of our price index on the fraction of children born in multiple births.

births to married women and only 0.09% (insignificant) to single mothers.[14] This could be because periods of booms lead to more marriages and, since there are more married individuals, more babies are born to married mothers, or because there are more births to married women than unmarried women, keeping their marital status as fixed. We find, when regressing the marriage rate against our proxy of economic shocks, that the coefficient is small and not statistically significant, suggesting that the second hypothesis is more likely to be at play.[15]

This raises the question about whether the effect we found on the extensive margin of fertility stems only from the fact that fewer single mothers are having children and that they have a higher propensity to have only one child. In analyses not reported here but available upon request, we find that while a fraction of the fall in firstborns is driven by the decrease in the fraction of births to single mothers, even within the group of single mothers, we find a significant and negative impact of better local economic conditions on the probability of having a firstborn. This suggests that for unmarried mothers, better economic conditions also appear to reduce the probability of starting a family. The effect for an increase in birth order is, correspondingly, larger and more significant for married mothers.

This differs from the results of Kearney and Wilson (2018) who find that the fracking boom increased fertility but did not reduce the share of birth that were out-of-wedlock. We argue that the difference here is not because there is a large stigma to having a child out-of-wedlock in Chile (as the authors argue was the case in the 1970s during the coal boom) since the majority of births in our sample occur out of wedlock. However, despite non-marital births being very frequent, legal obligations of unmarried fathers were relatively weak over our period of analysis compared to modern US period. We thus suggest that when economic conditions improve employment of men, women who chose to have another child are those with a formal commitment that ensures that the better economic prospects of the fathers can translate to better economic conditions for their child. Said differently, only women who can benefit substantially from the income effect of the commodity booms have more children.

4.6 Heterogeneity by educational level

We next check whether the identity of parents changed in some observable characteristics. We anticipate that women for which the substitution effect dominates would be likely to decrease their fertility in response to commodity booms but that women for whom the income effect dominate would have more children. We use education as a dividing criteria since we found different patterns in the impact of commodity booms on employment by educational attainment in Table 4.[16] While not presented here, we find no evidence of changes in the age distribution of parents, suggesting

[14] These results are not robust to the inclusion of region-quarter fixed effects but are to the use of population weights.

[15] Results not presented here but available in Irarrázaval (2013).

[16] We also divided the sample by occupational status of the mother and father. We find that the log births increased for both but more strongly for those who had higher occupational status.

that these effects are not linked to older or younger parents being more represented when a boom occurs.

To do this, we repeat our analysis of Table 5 but dividing the sample by education. The first panel of Table 6 looks at those with less than high school education, the middle one at those that have high school education and the bottom panel to those who have some tertiary education. The first column regresses the log of births by education of the mother. It finds that the log birth significantly decreased for women in the middle educational category, who was the one for which we had found strong substitution effects and no income effect. For the other two groups, we observe a positive but insignificant increase. Column (2) divides the birth by the educational

Table 6 Impact of exogenous economic shocks on fertility, by education of the mother

	Log births mothers (1)	Log births fathers (2)	Log births firstborn (3)	# of Children (4)	Log births single moms (5)
	Less than high school diploma				
Price index	0.012	−0.143	−0.005	0.112**	−0.276***
	(0.099)	(0.103)	(0.090)	(0.050)	(0.079)
R^2	0.954	0.951	0.901	0.179	0.931
N	24,925	24,878	24,925	24,227	24,925
Mean (s.e.) of	3.531	3.526	2.664	2.194	3.119
dependent variable	(1.484)	(1.459)	(1.268)	(0.393)	(1.312)
	High school diploma				
Price index	−0.226*	−0.319**	−0.237**	0.087*	−0.006
	(0.133)	(0.135)	(0.101)	(0.050)	(0.131)
R^2	0.917	0.909	0.849	0.121	0.854
N	24,925	24,878	24,925	22,407	24,925
Mean (s.e.) of	2.746	2.679	2.386	1.767	2.472
dependent variable	(1.827)	(1.836)	(1.333)	(0.345)	(1.393)
	University				
Price index	0.057	0.149	0.528***	-0.022	1.574***
	(0.126)	(0.123)	(0.116)	(0.070)	(0.174)
R^2	0.923	0.923	0.823	0.099	0.760
N	24,925	24,878	24,925	21,696	24,925
Mean (s.e.) of	1.925	2.023	1.831	1.716	1.772
dependent variable	(1.139)	(1.905)	(1.355)	(0.502)	(1.275)

Each column and panel represents a different regression as in Eq. 1. The outcomes of interest are the log births to mothers (column 1) and fathers (column 2) with the indicated educational attainment, the log of firstborn births to mothers with the indicated educational attainment in column 3, the number of children ever born to mothers in column 4, and the log birth to single mothers in column 5. Regressions are run at the county level including time and county fixed effects with robust standard errors clustered at the county level in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

attainment of the father and finds similar patterns although the low-educational category is negative. The magnitudes suggest that an increase of 1% in the local price index decreases the number of births of middle-skill parents by around 3%. This is consistent with our interpretation of income versus substitution effects.

We now explore whether the other heterogeneity results we presented differ across educational groups in a way that would lend credence to the dual impact of commodity booms. To do so, we re-estimate all of our regressions but separately by educational attainment group of the mother. We focus on a few outcomes but the results are similar when looking at others. Specifically, we look at the log number of firstborn births in column (3), to the number of children in column (4), to the log births to single mothers in column (5).

The third column of Table 6 suggests that the decrease in the log of firstborn births conceived in a given quarter is particularly strong for women with high school diplomas, which is consistent with the existence of a substitution effect for these mothers. On the other hand, those most educated saw an increase in the number of births of firstborn babies. This would suggest that the income effect was particularly strong for these women. Similarly, the increase in the intensive margin is found most strongly for the lowest two educational groups of the mothers. For university-educated women, we do not see a statistically significant pattern.

We then look at the log births born to a single mother and find very different results depending on the level of education of the mothers. The null aggregate pattern is observed for the middle educational categories. For the lowest educational group, we find that the log births to single mothers significantly decreased. This group is the one where obtaining support from non-married fathers is probably the most difficult. However, we observe that for university-educated women, the log births increased for single mothers. This group is likely to have a much stronger capacity to extract resources from non-married partners.

Overall, we thus find little evidence to believe that the aggregate results we documented in the main part of the paper are simply due to a changes in the educational attainment of women and not also to changes in behavior of these different women in our sample. In fact, our results suggest that different types of income and substitution effects explain the pattern we observe at the aggregate level: low-educated women and university-educated women face increases in the employment rate of those living with them that induce them to have more children. The only group for which there seems to be a strong substitution effect is for women with high school diplomas.

4.7 Birth versus conception

While we have so far argued that our results highlight the fact that when economic conditions are better, some individuals decide to conceive more babies, an alternative interpretation would be that when economic conditions are better, more babies are carried to term. Furthermore, the fact that more babies are born to parents with given characteristics could not be due to the fact that these parents decide to conceive more children but that economic conditions at the time of birth are influencing what they report on the birth certificate.

Table 7 Impact of exogenous economic shocks at conception and birth

	Log births				
	All	Firstborn	Higher order	Single moms	Married moms
	(1)	(2)	(3)	(4)	(5)
Price index	0.584***	0.404***	0.536***	0.433***	0.643***
at conception	(0.138)	(0.113)	(0.118)	(0.136)	(0.132)
Price index	−0.066	−0.098	−0.047	−0.127	−0.128
at birth	(0.132)	(0.107)	(0.113)	(0.130)	(0.127)
R^2	0.547	0.619	0.601	0.567	0.575
N	50,620	50,645	50,645	50,645	50,645
Mean (s.e.) of	3.177	2.473	2.733	2.585	2.290
dependent variable	(1.711)	(1.511)	(1.563)	(1.702)	(1.746)

Each column represents a different regression as in Eq. 1 but where we have two price indices for the time of birth and the time of conception. Each observation is at the level of quarter of birth and quarter of conception by county. The outcome of interest is the log number of all births in column 1, of firstborn births in column 2, of higher-order births in column 3, of births to single mothers in column 4, and of births to married mothers in column 5. Regressions include time and county fixed effects. Robust standard errors clustered at the county level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

To explore this in more details, we first reconstruct our data by quarter of conception AND quarter of birth. Given that babies are born at different gestational age, this implies that we usually have 2 or 3 quarters of birth for every quarter of conception. We then generate another price index which this time measures exogenous shocks at the time of birth, instead of conception. Given that the gestation period could be endogenous, we also repeat the exercise measuring the quarter of expected birth, assuming full term. These measures at time of conception and at time of (actual or predicted) birth are clearly correlated but still provide some differences that can be exploited. Thus, the thought experiment is now the following. Take two babies that were born in the same quarter of birth but were conceived in different quarters. One of these babies had a quarter of conception that had better local economic conditions than the other because of shifts in international commodity prices. Are the characteristics of these two babies likely to be different?

Table 7 shows, for the same outcomes as in Table 5, the results of combining both price indices in a single regression.[17] The first column suggests that our aggregate fertility result is solely driven by changes in economic conditions at conception and not at birth. This is not surprising given the low level of perinatal mortality in Chile. columns (3) and (4) then examine our results on extensive and intensive margins. We find that the positive impact of economic conditions at the moment of conception

[17]We exclude the birth rate since it is not obvious what would be measured by dividing the number of births conceived in quarter i and born in quarter j by the population of females.

continues to positively impacts the births of higher-order babies but now also positively affects (although to a lower degree) the births of firstborns. Given the high correlation between the two price index measures, we see this as suggesting that our results are not driven by economic conditions at birth but rather a change in the family fertility's decisions. Finally, the last columns explores whether our compositional measures may also depend on the conditions at birth. We continue to find that good economic times at the moment of conception positively increase the births to married mothers but the impact on unmarried mothers is now positive and significant, although lower in magnitude. The results suggest that our interpretation that more married women choose to have children in good economic times is maintained.

One could be worried about the fact that if gestational age is endogenous, using the price index at the time of actual birth would be problematic. In Appendix Table 9, column (4) shows that the log births do not respond to the average of the price index in the 3 quarters following conception. Since birth would occur in one of these three quarters with certainty, this is additional evidence that the effect does not stem from a response to conditions at the time of birth but rather at the time of conception. Furthermore, we also repeated the exercise of Table 7 using the average gestation as the time of birth for all births in our dataset. We find a similar pattern as the one displayed above when using the price index at this quarter of expected birth although quarter of expected birth has a stronger impact on birth order outcomes.[18]

4.8 Trends versus short-term fluctuations

As we argued in the introduction, no other study on this topic has been able to explore such high-frequency birth data to explore the relationship between cycles and fertility outcomes. While this is an advantage, it also questions whether our results are driven by "trend" shifts in local economic conditions or by more short-term variations. To explore this, we separate our local price index into 2 components (trend and residual) using a Hodrik-Prescott filter. This is only possible because of our high-frequency data. We have limited capacity to do such decomposition since the ingredients of our price index are common across all regions. Nevertheless, we obtain significant differences by local area. The results using each ingredient of that decomposition in our main outcomes are presented in Table 8. What can be seen there is that while both the trend and the cycle component of our price index appear to influence fertility outcomes in a consistent manner, the results are substantially more precise for the trend than the cycle component. This would suggest that the longer-term variations in local economic conditions appear to drive the main fertility results more strongly than very short-term fluctuations. This also gives support to the idea that income effects are more important than substitution effects in explaining our results. As an alternative, we also use a moving average specification, using 12 months before and after the conception to measure the "trend" component and the difference between

[18] Results available upon request.

Table 8 Impact of exogenous economic shocks: trend versus cycle

	Birth rate	Log births				
		All	Firstborn	Higher order	Single moms	Married moms
	(1)	(2)	(3)	(4)	(5)	(6)
Price index	6.624	−0.095*	−0.123**	−0.023	−0.069	−0.069
(cycle)	(6.980)	(0.054)	(0.063)	(0.048)	(0.056)	(0.074)
Price index	9.801	0.428***	0.083	0.318***	0.158	0.379**
(trend)	(6.124)	(0.151)	(0.102)	(0.082)	(0.139)	(0.162)
R^2	0.145	0.967	0.957	0.965	0.961	0.951
N	24,894	24,938	24,409	24,409	24,409	24,409

Each column represents a different regression as in equation (2) but where we have two independent variables of interest: the cycle and trend of the price index. Both of these were obtained from a Hodrik-Prescott filter where the trend corresponds to the estimated trend and the "cycle" is obtained from the residuals. The outcome of interest is the number of births per fertile women in column 1, the log number of births in column 2, the log births that are firstborn in column 3, the log births that are not firstborns in column 4, the log births to single mothers in column 5, and the log births to married mothers in column 6. Regressions include time and county fixed effects. Robust standard errors clustered at the county level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

that moving average and the actual conception price index as the "cycle" component with similar results.[19]

5 Conclusions and future research

Small open economics are subject to external commodity price shocks that affect their economic conditions. We take advantage of the combination of commodity price shocks faced by the Chilean economy with variation in local natural resources endowments to construct a source of plausibly exogenous variation in local economic conditions in the 1994–2012 period. This creates the opportunity to study how these shocks (and their substitution and income effects) affect fertility and family formation. Moreover, we use several sources of data to identify effects on different margins and to document whether income or substitution effects prevail and for whom.

Overall, we find that births are procyclical. We find evidence indicating that these impacts are due to shocks at the time of conception and not at the time of birth. Moreover, we are able to show that this is much more due to families getting larger than to new families forming. These results shed important light over the relative opportunity costs faced by women starting a family versus increasing their number of children. We think that this is important policy lesson since this margin has been hidden previously for lack of data.

[19] Table omitted here but available upon request.

The impacts are concentrated in mothers with low and high skills. The patterns we find can be explained by low skill women facing only income effects, middle skills women facing only substitution effects, and high skills women reacting more to income than substitution effects. This suggests that when trying to understand the link between economic conditions and fertility, one must understand the relative impact these may have on through substitution and income effects.

Finally, one important result we find is the fact that births within marriage are particularly responding, despite a majority of births occurring to unmarried mothers. We emphasize that this may be linked to poor enforcement of parental obligations in Chile. In addition, this result also highlights the potential beneficial impact of local economic conditions on fatherly presence at the moment of birth. This is particularly interesting given the beneficial impact seen in other settings of better involvement of fathers. We think that this is an interesting avenue of future research.

Appendix A: Additional Tables

Table 9 Impact of exogenous economic shocks on fertility, adding lags, and leads

	Birth rate (1)	Log births (2)	Log births (3)	Log births (4)
Price index	7.739	0.165**	0.161*	0.175***
	(7.027)	(0.082)	(0.086)	(0.064)
Price index, lag	1.072	0.086		
	(7.186)	(0.085)		
Price index, lead 1 month			0.039	
			(0.113)	
Price index, lead 2 months			0.077	
			(0.107)	
Price index, lead 3 months			0.023	
			(0.084)	
Price index, lead 1–3 months				0.122
				(0.090)
R^2	0.148	0.968	0.968	0.967
N	24,559	24,601	23,927	24,601
Mean (s.e.) of	14.749	4.145	4.145	4.145
dependent variable	(19.033)	(1.609)	(1.609)	(1.609)

Each column and panel represents a different regression as in Eq. 1 but includes in addition to the price index at the time of conception, the price index the quarter before conception in columns 1 and 2, and the price index post-conceptions in columns 3 and 4. The outcomes of interest are the birth rate in column 1 and the log number of births in columns 2 to 4. Regressions include time and county fixed effects. Robust standard errors clustered at the county level in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 10 Impact of exogenous economic shocks on fertility, robustness checks

	Birth rate	Log births				
		All	Firstborn	Higher order	Single moms	Married moms
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Adding region-quarter fixed effects					
Price index	10.693	0.220	−0.155**	0.132*	0.171	0.210
	(6.630)	(0.163)	(0.072)	(0.070)	(0.142)	(0.164)
R^2	0.227	0.969	0.960	0.968	0.963	0.954
N	24,894	24,938	24,409	24,409	24,409	24,409
	Panel B: Weighting by population					
Price index	2.331**	0.153***	0.005	0.184***	0.005	0.139**
	(1.007)	(0.058)	(0.064)	(0.054)	(0.061)	(0.070)
R^2	0.145	0.967	0.962	0.970	0.968	0.959
N	24,894	24,938	24,386	24,386	24,890	24,890

Each column represents a different regression as in Eq. 1. The outcome of interest is the number of births per fertile women in column 1, the log number of births in column 2, the log births that are firstborn in column 3, the log births that are not firstborns in column 4, the log births to single mothers in column 5, and the log births to married mothers in column 6. Regressions include time and county fixed effects. Panel A includes also region*quarter fixed effects. Regressions in panel A are unweighted while they are weighted by population in panel B. Robust standard errors clustered at the county level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 11 Impact of price index on total population, by gender, marital status, and educational attainment

	Log(pop.)	Log(pop.)	% male	% Married/ cohabitating	% less than high-school	% high-school diploma
	(1)	(2)	(3)	(4)	(5)	(6)
Price index	−0.028	−0.001	0.006	−0.021	−0.024	−0.015
	(0.071)	(0.061)	(0.007)	(0.021)	(0.027)	(0.025)
Source	Census	CASEN	CASEN	CASEN	CASEN	CASEN
R^2	0.994	0.995	0.528	0.564	0.858	0.795
N	24,894	2258	2259	2259	2259	2259
Mean (s.e.) of	8.441	8.720	0.489	0.783	0.530	0.403
dependent variable	(1.518)	(1.318)	(0.025)	(0.073)	(0.157)	(0.129)

Each column and panel represents a different regression as in Eq. 1. The outcomes of interest are the log of the population in column 1 and 2, while it is the fraction of the population that is male in column 3, the fraction married or cohabitating in column 4, the fraction with less than high school in column 5, and the fraction with high school diploma in column 6. The first column is computed using Census data (at the quarter-county level) while all others employ CASEN data (at the bi- or tri-annual county level). Regressions include time and county fixed effects. Robust standard errors clustered at the county level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Acknowledgements Fernanda Rojas provided excellent research assistance. We also benefited from the work by Maria de la Paz Irarrázaval for her Master's thesis written at EH Clio Lab. We would like to thank seminar participants at the 2017 SECHI meetings and the EH Clio Lab 2018 conference, editor Terra McKinnish, and two anonymous referees for valuable comments and suggestions.

Funding We would like to thank FONDECYT (Project 1170956) for financial support.

Data availability All replication files can be found at: https://github.com/fagallego/babycommoditybooms.

Declarations

Competing interests The authors declare no competing interests.

Disclaimer All errors are our own.

References

Alam SA, P ortner C (2018) Income shocks, contraceptive use, and timing of fertility. J Dev Econ 131(C):96–103. https://doi.org/10.1016/j.jdeveco.2017.10.007

Álvarez R, Álvaro Garcá?a-Mará?n, Ilabaca S (2021) Commodity price shocks and poverty reduction in chile. Resources Policy 70:101177. https://doi.org/10.1016/j.resourpol.2018.04.004

Ben-Porath Y (1973) Short-term fluctuations in fertility and economic activity in israel. Demography 10(2):185–204. https://doi.org/10.2307/2060813

Black DA, Kolesnikova N, Sanders SG, Taylor LJ (2013) Are children "normal"? Rev Econ Stat 95(1):21–33. https://doi.org/10.1162/REST_a_00257

Chatterjee S, Vogl T (2018) Escaping malthus: Economic growth and fertility change in the developing world. Am Econ Rev 108(6):1440–67. https://doi.org/10.1257/aer.20170748

Davis J (2017) Fertility after natural disaster: Hurricane mitch in nicaragua. Population and environment 38(4):448–464. https://doi.org/10.1007/s11111-017-0271-5

Departamento de Estadísticas e Información de Salud (2013) Estadísticas de Natalidad. Recovered on July 10, 2013, at http://www.deis.cl/descargar-bases-de-datos/

Dettling LJ, Kearney MS (2014) House prices and birth rates: the impact of the real estate market on the decision to have a baby. J Public Econ 110:82–100. https://doi.org/10.1016/j.jpubeco.2013.09.009

Feigenberg B (2021) Priced out: Aggregate income shocks and school pricing in the chilean voucher market. Econ Inq 59(2):696–721. https://doi.org/10.1111/ecin.12945

Galbraith VL, Thomas DS (1941) Birth rates and the interwar business cycles. J Am Stat Assoc 36(216):465–476. http://www.jstor.org/stable/2278953

Galloway PR (1988) Basic patterns in annual variations in fertility, nuptiality, mortality, and prices in pre-industrial europe. Popul Stud 42(2):275–303. https://doi.org/10.1080/0032472031000143366

Hinojosa J (2018) El efecto de los desastres naturales en la fecundidad: Los hijos del terremoto del 2010 en chile. Master's Thesis available at http://economia.uc.cl/wp-content/uploads/2019/01/tesis-HinojosaCorral-2018.pdf

Instituto Nacional de Estadísticas (2013) Encuesta nacional del empleo. Recovered September 9, 2013, at http://www.ine.cl/canales/chile-_estadistico/familias/laborales.php

Instituto Nacional de Estadísticas (2013) Estadísticas vitales. Recovered October 11, 2013, at http://www.ine.cl/canales/chile-_estadistico/demogra%1Ca_y_vitales/estadisticas_vitales/estadisticas_vitales.php

International Monetary Fund (2013) Primary commodity prices. Recovered August 10, 2013, at http://www.imf.org/external/np/res/commod/index.aspx

Irarrázaval MP (2013) El impacto causal de un shock económico en la natalidad: Un estudio para 1994-2011 en chile

Kearney MS, Wilson R (2018) Male earnings, marriageable men, and nonmarital fertility: Evidence from the fracking boom. Rev Econ Stat 100(4):678–690. https://doi.org/10.1162/rest_a_00739

Kleven H, Landais C, Søgaard JE (2019) Children and gender inequality: Evidence from denmark. American Economic Journal: Applied Economics 11(4):181–209. https://doi.org/10.1257/app.20180010
Lagerlöf N-P (2015) Malthus in sweden. The Scandinavian Journal of Economics 117(4):1091–1133. https://onlinelibrary.wiley.com/doi/pdf/10.1111/sjoe.12119
Lee R, Anderson M (2002) Malthus in state space: Macro economic-demographic relations in english history, 1540 to 1870. J Popul Econ 15(2):195–220. https://doi.org/10.1007/s001480100091
Lovenheim MF, Mumford KJ (2013) Do family wealth shocks affect fertility choices? evidence from the housing market. Rev Econ Stat 95(2):464–475. https://doi.org/10.1162/REST_a_00266
Martínez AC (2013) Intrahousehold allocation and bargaining power: Evidence from chile. Econ Dev Cult Chang 61(3):577–605
Nandi A, Mazumdar S, Behrman JR (2018) The effect of natural disaster on fertility, birth spacing, and child sex ratio: evidence from a major earthquake in India. J Popul Econ 31(1):267–293. https://doi.org/10.1007/s00148-017-0659-7
Rau T, Sarzosa M, Urzúa SS (2017) The children of the missed pill. Working Paper Series 23911, National Bureau of Economic Research
Redatam C (2013) Censo de población y vivienda 1992. Recovered August 10, 2013, at http://www.cepal.org/es
Rodgers JL, John CAS, Coleman R (2005) Did fertility go up after the oklahoma city bombing? an analysis of births in metropolitan counties in oklahoma, 1990-1999. Demography 42(4):675–692. http://www.jstor.org/stable/4147334
Schultz TP (1985) Changing world prices, women's wages, and the fertility transition: Sweden, 1860-1910. J Polit Econ 93(6):1126–1154. https://doi.org/10.1086/261353
Silver M (1965) Births, marriages, and business cycles in the united states. J Polit Econ 73(3):237–255. http://www.jstor.org/stable/1829038
Sociedad de Fomento Fabril F.G. (SOFOFA) (2013) Precio de commodities. Recovered August 14, 2013, at http://web.sofofa.cl/informacioneconomica/indicadores-economicos-/estructura-de-la-industria/precio-commodities/