Is the Motherhood Wage Penalty Universal? Evidence from Panel Data in Indonesia

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September 2018

This research investigates whether the "motherhood penalty" documented in many studies is universal using extremely rich longitudinal survey data from Indonesia. On one hand, studies have established that, in the U.S. and Europe, women earn less when they become mothers relative to comparable women without children, and this (Waldfogel, 1998; Budig and England, 2001; Davies and Pierre, 2005; Sigle-Rishton and Waldfogel, 2007). This wage penalty is also persistent in countries with generous parental leave and child support policies (Angelov et al., 2016; Kleven et al., 2017.) On the other hand, evidence from other parts of the world is relatively sparse and inconclusive, with some studies finding a motherhood penalty but others reporting a motherhood premium (Agüero and Marks, 2011; Piras and Rapani, 2005; Olarte and Peña, 2010; Orbeta, 2005).

This project is designed to address at least three key concerns in this literature. First, studies fail to account for selection into the labor force, selection in to motherhood and, importantly, the decision to work when a woman becomes a mother. If, ceteris paribus, higher earning women are more likely to work after giving birth, then failure to take this selection into account will result in upward biased estimates of the wage impact of motherhood; this is a particular concern in studies in middle and low income settings and likely explains the estimated motherhood premium in some of these regions. Exploiting longitudinal survey data that has followed the same respondents over 25 years, we compare the labor market outcomes of each mother before and after she gives birth to isolate the causal effect of motherhood on labor outcomes. We also describe the nature of selection into the labor force among mothers.

Second, one of the central hypotheses in this literature is that the motherhood penalty is linked to inflexible work conditions (Goldin, 2014; Goldin and Katz, 2016). We exploit the fact that almost half of Indonesia's female labor force participation is in the self-employed sector, where work conditions are typically more flexible than in the market sector (Maloney 2004). Most of these self-employed women work in agriculture or selling food and other goods prepared in the home. We test whether the motherhood penalty is different among women in the self-employed sector relative to the formal market sector and also assess whether women shift their time into self-employment around the time of becoming a mother.

Third, another key hypothesis in this literature is that much of the motherhood penalty is caused by work interruptions associated with childcare, and that this is likely mitigated for women who have help in the home. Indonesia – and many other developing countries – provides a good context to test this hypothesis since many households have three generations and extended family members often live together (Agüero and Marks, 2011). Exploiting the longitudinal dimension of the data, we show that living arrangements do not shift around the time a woman becomes a mother but document that a substantial fraction of households with reproductive age couples also have other women co-residing, including sisters, mothers, mothers-in-law, adolescent daughters and live-in servants. While this part of analysis is not yet completed, we plan to test whether the motherhood penalty differs for women living in households with other female co-residents.

Data are drawn from the Indonesia Family Life Survey (IFLS), an on-going longitudinal survey that has completed five waves since the baseline in 1993 which was selected to be representative of 83% of the population at the time. (Outer island and more remote provinces were excluded from the survey.) The survey is designed to track and interview all baseline respondents and their children born after the baseline and, in the four follow-ups, all household members, including new entrants, are individually interviewed. IFLS has been a pioneer among large-scale longitudinal studies in allocating resources to assure attrition in each wave is low and members of almost 90% of the original households have been interviewed in all 5 waves of the survey (Strauss et al., 2016).

This paper focuses on women aged 18-72 at the time of each IFLS wave, exploiting detailed information on labor market choices, birth histories and marital histories collected from individual interviews. Each respondent provides a complete marital and birth history so that the timing of each childbirth is known. Reliable earnings data is only collected for each survey year, so each woman has at most 5 labor market observations. Participants provide information about their primary job and, if they have one, their secondary job, including sector and type of work, hours worked and earnings. Earnings are made up of wage earnings including benefits for those in the market sector and profits for those in the self-employed sector. Earning and hours are annualized and summed over both primary and secondary job, and this is used to calculated hourly earnings. We use past-year outcomes because they tend to be much less volatile than past-month, especially for self-employment profits.

Our sample includes 59,607 observations and 22,754 women. Women are not working in 42.74% of these observations, employed in the market-wage sector in 22.51%, self-employed in 21.84% and engaging in unpaid work in 12.91%. Thus, to be included in a model with individual fixed effects, a respondent must have at least two labor market observations in which they were currently working for pay. Our final sample therefore includes 18,163 observations and 6,477 women.

In our preferred model, we separately regress past-year hours, earnings and wages on a set of indicator variables for whether or not a child was born in the past 1-3 months, 4-12 months, and 1-year intervals for the following 4 years (the excluded category is no live birth in the past 5 years). We control for age with 5-year-wide bins in order to sweep out life-cycle labor market trends, and include marriage bins that cover the same period as the childbirth bins to isolate the impact of childbirth independent of the impact of marriage. We also include demographic controls for the number of males and females living in the household aged 11-14, 15-24, 25-54 and over 54, as well as province fixed effects. By adding individual fixed effects, our identification of the motherhood penalty comes from changes within the individual over time, comparing labor market outcomes in the 5 years after a birth to labor market outcomes outside that period, and sweeping out any life-cycle trends associated with age and marriage.

A causal interpretation in the above model may still be flawed if the timing of childbirth is endogenously related to labor market outcomes. For example, if women decide to have children precisely when they anticipate an increase in earnings, then this could drive the observed lack of a motherhood penalty. To the extent that this anticipation is correlated with pre-birth trends, we would expect to see a change in labor market outcomes in the years leading up to a live birth, and we see no evidence of this.

Table 1 documents movements out of the labor force during motherhood, using fixed effects logit estimates for the likelihood of being in paid work (either market sector or self-employed) and including all of the controls from our main specification. Column 1 shows that women who recently had a live birth are significantly more likely to move out of paid work and into unpaid work and unemployment. Columns 2-3 estimate the same model separately for women with at most 6 years of education and for

women with more than 6 years of education. The reason that we use education rather than directly using income to document selection is that education is highly correlated with lifetime earnings, is mostly constant across the life cycle and is less endogenously related to work interruptions. These results show that selection out of the labor force is stronger for less-educated women, implying that OLS estimates of income and wages that do not account for selection into the workforce are likely biased upwards. Indeed, OLS estimates of the relationship between children and wages show a significant wage premium of 17% in the year after a live birth, and this premium disappears once individual fixed effects are included.

Columns 1-3 of Table 2 presents our main model estimated on all paid female workers. All outcomes are presented in terms of log*100. The estimates show a large and significant decrease in hours and earnings after a live birth that gradually returns to pre-birth value by year 5. (This is not noticeable in the past 1-3 months because the labor market outcomes cover the entire past-year, and we felt it was important to distinguish births that recently occurred from births that occurred greater than 3 months ago.) However, there is no statistically significant relationship between motherhood and wages.

Columns 3-6 and 7-9 present estimates from the same model, with the childbirth bins replaced with two interactions – one with an indicator variable for market-wage work and the other an indicator variable for self-employment work in the primary job. These sector-specific coefficients reflect variation in outcomes around childbirth specifically for women in those sectors, and allow us to distinguish potentially important heterogeneous effects between two very different types of work. Here we see that the hours and earnings penalty is strong within the market sector. It is similarly strong within the self-employment sector. However, the earnings penalty disappears faster for self-employed workers, indicating that self-employed mothers face a different motherhood penalty than mothers working in the market sector. Nevertheless, there is a lot of uncertainty in the self-employment outcomes, and these differences need to be further explored before any concrete conclusions can be drawn.

In the coming months we plan to extend the above analysis in a few key ways. First, we would like to extend the analysis to the work and earnings patterns of husbands. If there are substitution or specialization patterns occurring for women in the household during marriage and motherhood, then to ignore male incomes is to miss a big part of the story. We anticipate that an exploration of total family income and work patterns may help to explain the finding that self-employed women experience a decrease in hours but not earnings during motherhood. In particular, it is possible that self-employed women work on businesses that their husbands can assist with or co-manage, and the IFLS has business questionnaires that allow us to study this question in detail. This will also contribute to a large literature documenting the impacts of marriage and fatherhood on the incomes of men.

Second, we plan to explore in more detail the movements in and out of the labor force and between sectors leading up to and during motherhood. While we have not used this data yet, IFLS contains a detailed annual history of sector of employment, allowing us to precisely characterize both child birthdates and sector of employment over a 26-year period.

Finally, as discussed above, we plan to document whether the observed earnings penalty and reductions in labor supply are mitigated in households with other female co-residents. There is substantial evidence that family networks play a role in assisting with childcare in the household, and the IFLS presents an ideal context to study the extent to which this is related to female labor outcomes.

Table 1: Labor Force Participation during Motherhood

	Full Sample	**At Most 6 Yrs Educ**	**More Than 6 Yrs Educ**
	(1)	(2)	(3)
VARIABLES	Prob(paid work)	Prob(paid work)	Prob(paid work)
Child born in:			
past 1-3 months	0.32*	0.29*	0.35*
	(0.04)	(0.05)	(0.06)
past 4-12 months	0.35*	0.27*	0.44*
	(0.03)	(0.03)	(0.04)
past 13-24 months	0.44*	0.41*	0.46*
	(0.03)	(0.04)	(0.04)
past 25-36 months	0.58*	0.55*	0.59*
	(0.04)	(0.05)	(0.05)
past 37-48 months	0.75*	0.71*	0.77*
	(0.05)	(0.06)	(0.07)
past 49-60 months	0.85*	0.82*	0.85
	(0.05)	(0.06)	(0.08)
Observations	30,531	19,178	11,353
Number of individuals	8,432	4,892	3,540

* p<0.05

Logit odds ratios reported. All models include individual fixed effects.
Sample includes all female respondents age 18-72.

Table 2: Fixed Effects Motherhood Penalty Coefficients

	All Paid Work			**Market Sector Work**			**Self Employed Work**		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	ln(Hrs/Yr)	ln(Income)	ln(Wage)	ln(Hrs/Yr)	ln(Income)	ln(Wage)	ln(Hrs/Yr)	ln(Income)	ln(Wage)
Child born in:									
past 1-3 months	-1.7	7.5	9.1	0.3	-0.5	-0.8	-4.1	19.7	23.8
	(8.8)	(8.2)	(10.5)	(8.3)	(9.4)	(11.4)	(17.6)	(14.1)	(19.0)
past 4-12 months	-23.9*	-18.4*	5.5	-20.2*	-12.6	7.6	-29.1*	-26.1*	3.0
	(5.4)	(5.9)	(5.4)	(5.9)	(6.6)	(5.9)	(9.5)	(10.3)	(9.6)
past 13-24 months	-17.1*	-16.4*	0.7	-16.7*	-19.7*	-3.0	-17.4*	-13.2	4.3
	(4.5)	(5.0)	(4.5)	(5.5)	(6.1)	(5.1)	(6.9)	(7.7)	(7.3)
past 25-36 months	-10.2*	-13.3*	-3.2	-13.4*	-18.3*	-4.9	-6.2	-7.7	-1.5
	(4.0)	(4.7)	(4.6)	(5.2)	(5.8)	(5.4)	(6.0)	(7.2)	(7.7)
past 37-48 months	-11.4*	-9.6*	1.7	-5.8	-8.7	-2.9	-17.3*	-11.0	6.4
	(4.0)	(4.4)	(4.1)	(4.7)	(5.3)	(4.8)	(6.2)	(6.8)	(6.4)
past 49-60 months	-3.0	-3.3	-0.3	-0.3	-8.2	-7.9	-5.9	1.3	7.3
	(3.6)	(4.2)	(4.1)	(4.2)	(5.0)	(4.6)	(5.7)	(6.4)	(6.5)
Constant	710.3*	68.7	49.1	711.1*	67.4	47.1	711.1*	67.4	47.1
	(28.6)	(42.8)	(38.7)	(28.6)	(42.9)	(38.7)	(28.6)	(42.9)	(38.7)
Observations	18,163	18,163	18,163	18,163	18,163	18,163	18,163	18,163	18,163
R-squared	0.019	0.093	0.072	0.019	0.094	0.073	0.019	0.094	0.073
Number of individuals	6,477	6,477	6,477	6,477	6,477	6,477	6,477	6,477	6,477

Robust standard errors in parentheses

* p<0.05

Outcomes in logs*100. Sample includes all female paid workers age 18-72.
All models include individual fixed effects, age bins, marriage date bins, demographic controls, province fixed effects.

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