



# When do children enable retirement? Fertility, family support, and later-life labor supply in Indonesia

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## ABSTRACT

This paper examines whether and under what conditions fertility reduces later-life labor supply in Indonesia, where formal old-age protection remains limited and work often continues into advanced ages. Using the Indonesian Family Life Survey 2007 and 2014, we implement an instrumental-variables strategy based on differential reproductive-age exposure to the rollout of Village Contraceptive Distribution Centers across communities and cohorts.

The full-sample IV estimates are negative but statistically imprecise. By contrast, an additional child is estimated to reduce working status and log weekly hours among agricultural landholding households, while the corresponding estimates for landless households are smaller and statistically insignificant. Fertility also increases the probability that at least one linked child works in agriculture among landholding households, providing evidence consistent with family-labor substitution. Net intergenerational support rises most clearly among landless households, where later-life work does not decline, suggesting that financial support alone does not account for the observed labor-supply pattern. The findings indicate that additional children may enable reductions in later-life work when household production creates scope for intergenerational labor substitution. The estimates apply to fertility-responsive households rather than to all older Indonesians.

## Introduction

For many older adults in developing economies, retirement remains out of reach. Limited pension coverage and weak formal safety nets mean that work often continues late into the life course (UNDESA, 2018). In Indonesia, this pattern is particularly pronounced: a substantial share of individuals aged 70 and above remain economically active, often within informal or household-based production (Statistics Indonesia, 2023). In this setting, old-age security relies not only on continued work but also on family support, including transfers and other assistance from adult children (ADB et al., 2024; Phillips, 2002). However, as fertility declines and populations age, the sustainability of this family-based system becomes increasingly uncertain (Adioetomo and Pardede, 2025; Molyneux and Gertler, 2000).

A central question is whether having more children reduces the need for work in old age. In principle, children may support parents in later life through labor contributions to family production, financial transfers, and other forms of assistance, potentially reducing parents' reliance on

their own labor income (Hoddinott, 1992; Lillard and Willis, 1997; Rosenzweig and Wolpin, 1985). This view underlies a large literature that interprets children as a source of informal old-age security in settings with limited formal protection (Nugent, 1985). Yet the net effect of fertility is theoretically ambiguous. Intergenerational support may move in both directions as family needs and capacities change, because older parents may continue providing financial, practical, crises related assistance to adult children and grandchildren (Kreager and Schröder-Butterfill, 2008; Schröder-Butterfill, 2004). Larger families may therefore generate continuing obligations and care-related demands on parents' time and resources, so support received from children need not translate into lower labor supply in later life (Absor et al., 2024; Kim et al., 2021; Snopkowski and Sear, 2015). Fertility may also influence later-life work through long-run effects on parental health and functional capacity, although the direction of this pathway remains uncertain (Islam and Smyth, 2015).

This paper examines whether and under what conditions fertility reduces later-life labor supply in Indonesia. We argue that the effect

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depends importantly on the organization of household production and access to productive assets. In household production models, family and hired labor may be imperfect substitutes because of monitoring and supervision constraints, making the availability of family labor particularly relevant for household-based production (Deolalikar and Vijverberg, 1987; Singh et al., 1986). Agricultural land is therefore not only a form of wealth but also a productive asset that requires labor, supervision, and management. In landholding households, additional children may increase the availability of family labor for agricultural work or enable adult children to assume productive responsibilities previously undertaken by older parents. Among landless households, children may still provide transfers, care, or labor in other activities, but the direct scope for substituting for parental labor on household-owned agricultural land is more limited. More broadly, whether family support enables older parents to reduce work may also depend on whether assistance is offset by continuing obligations toward adult children and grandchildren (Kreager and Schröder-Butterfill, 2008; Schröder-Butterfill, 2004).

To estimate the causal effect of fertility, we exploit variation generated by Indonesia's family planning expansion. Specifically, we construct an instrumental variable based on differential exposure to the rollout of Village Contraceptive Distribution Centers (VDCs) across communities and birth cohorts, following Hatton et al. (2018). This variation captures differences in potential access to community-based contraceptive services during women's reproductive years and has been shown to predict completed fertility (Angeles et al., 2005; Gertler and Molyneaux, 1994; Miller and Babiartz, 2016). Using data from the Indonesian Family Life Survey (IFLS) 2007 and 2014, we implement a two-stage least squares strategy to estimate the effect of fertility on labor supply among individuals aged 55 and above.

The full-sample IV estimates are negative but statistically imprecise, providing limited evidence of a uniform fertility effect on later-life labor supply. The principal pattern instead appears by agricultural landholding status. Among landholding households, an additional child reduces both the probability of working and overall weekly labor supply, while the corresponding estimates for landless households are smaller and statistically insignificant. Additional heterogeneity by educational attainment suggests that the negative working-status effect may be more apparent among respondents with lower educational attainment, although the evidence is not sufficiently precise to establish a statistically significant difference across education groups.

The mechanism evidence is most closely aligned with family-labor substitution in agricultural production. Fertility increases the probability that at least one linked child works in agriculture among landholding households, the same group in which older parents reduce their labor supply. Because the data do not directly observe children taking over specific tasks, management responsibilities, or working hours, this evidence does not establish labor substitution conclusively. Net intergenerational support provides complementary evidence. Fertility increases positive net support and the amount of net transfers most clearly among landless households, yet parental labor supply does not decline in that group. By contrast, neither measure of net support increases significantly among landholding households. This contrast suggests that financial support alone is insufficient to account for the labor-supply pattern and that the productive context in which family support is provided may matter. Analyses of health-related work limitations and care-related assistance provide no affirmative evidence for these alternative explanations, although the estimates remain imprecise.

This paper contributes to two strands of literature. First, it adds to the emerging evidence on fertility and later-life labor supply in developing countries, which has been dominated by studies from China (Qiao and Wang, 2019; Rao and Zhang, 2024; Wang, 2025). Existing work

generally finds that higher fertility reduces later-life labor supply and often interprets this relationship through financial transfers and informal care. Rao and Zhang (2024), for example, show that higher fertility reduces later-life labor supply in China through increased support from children, particularly among rural parents and those with limited pension protection. We extend this literature by showing that the labor-supply implications of fertility depend on the productive context in which family support operates. In Indonesia, the negative labor-supply response is concentrated among landholding households, where children's participation in agricultural work increases with fertility.

Second, the paper contributes to the literature on intergenerational transfers and old-age support. A central question in this literature is whether transfers from adult children function as informal pensions that substitute for elderly labor (Lillard and Willis, 1997; Nugent, 1985). The empirical evidence, however, remains mixed, in part because family support networks are complex and bidirectional (Lee, 2020; Pezzin and Schone, 1997). In the Indonesian context, Cameron and Cobb-Clark (2008) show that transfers from children are, on average, weak substitutes for elderly labor supply. Our results complement this evidence by showing that greater net financial support does not necessarily reduce later-life work. Net support rises most clearly among landless households, whereas the labor-supply response is concentrated among landholding households, suggesting that financial transfers alone may be insufficient without corresponding changes in productive responsibilities.

Taken together, the findings highlight the uneven capacity of family-based old-age support to reduce continued work across household contexts. As fertility declines, formal old-age protection may become increasingly important where support from adult children does not translate into meaningful relief from work.

## Institutional settings

### *Old-age security, informality, and family support in Indonesia*

The institutional landscape of old-age security in Indonesia is highly segmented, with access to formal protection closely tied to employment status. Public sector employees, including civil servants, military, and police personnel, are covered by state-backed defined-benefit schemes through TASPEN and ASABRI. Formal private-sector workers are mandatorily enrolled in BPJS Ketenagakerjaan's old-age savings and pension programs. While participation remains voluntary for non-wage workers, the absence of employer contributions results in a coverage rate of only 0.4%, based on national social security and labor force statistics (Indonesia Financial Services Authority, 2022; Statistics Indonesia, 2022). Many rural and agricultural livelihoods fall into this segment, including farmers and unpaid family workers, helping explain why old-age protection remains especially limited outside the formal sector.

Non-contributory social assistance provides only a partial backstop. Such programs are generally means-tested and designed to alleviate basic hardship rather than replace labor earnings. Benefit levels are modest relative to typical living costs and are not structured as wage-replacement mechanisms. Public transfers therefore do not eliminate the need for continued work among most older adults outside the formal sector. The main rural-urban divide in retirement protection concerns pension access rather than different statutory replacement-rate formulas. In the 2023 Indonesia Longitudinal Ageing Survey, pension income was reported by only 1.4% of rural and 7.2% of urban older adults (ADB et al., 2024). Formal pension protection is therefore especially limited in rural areas and uncommon overall.

In the absence of comprehensive formal protection, old-age security also depends on intergenerational family support (Adioetomo and Mujahid, 2014). Such support is multidirectional rather than exclusively upward from adult children to ageing parents. Older parents may continue assisting adult children and grandchildren as family needs and capacities change, without this assistance necessarily forming part of a fixed reciprocal contract (Schröder-Butterfill, 2004). Support norms also vary across Indonesian family systems, with greater emphasis on independence and balanced exchange in East Java and stronger expectations of support from successful children among the Minangkabau (Kreager and Schröder-Butterfill, 2008). These community-based findings illustrate contextual variation rather than a single national norm.

This institutional configuration is reflected in the structure of older-age employment. Data from Statistics Indonesia (2023) show that later-life work is more common in rural than urban areas (63.34 versus 47.02%), with rural employment heavily concentrated in agriculture (72.56 versus 33.33%). Furthermore, the vast majority of elderly workers operate within the informal sector, particularly in rural regions where the rate reaches 90.88%. These patterns indicate that later-life work in Indonesia is often organized outside standard wage employment and is closely tied to agrarian livelihoods.

Agricultural land therefore represents a form of livelihood security that is distinct from formal retirement protection. As a productive asset, land can generate income and support household consumption, but its returns often require continued labor, supervision, or management. Landownership may thus sustain older households economically without automatically enabling withdrawal from work. This distinction motivates our focus on landholding status. Rural residence identifies where households live, whereas landholding identifies access to a productive agricultural asset. Although closely related, the two dimensions are conceptually distinct and are included separately in the empirical analysis.

#### *Indonesia's demographic transition and the family planning expansion*

As in other developing countries, Indonesia is currently on the threshold of an advanced demographic transition, with older persons approaching 10% of the total population (Adioetomo and Pardede, 2025). This accelerated population ageing and the subsequent pressure on elderly labor supply are closely linked to a profound, state-led fertility decline that began in the 1970s (Adioetomo, 2006). Between 1971 and the mid-2010s, the national total fertility rate (TFR) fell from 5.6 to approximately 2.4 births per woman. While broader economic change also mattered, the decline was strongly shaped by the state's family planning expansion following the establishment of the National Family Planning Coordinating Board (NFPCB) in 1970 (Hull, 2003).

The program was first implemented in Java and Bali before gradually expanding to cover nearly the entire country by the 1990s. The program was directed at the provincial level and implemented through existing administrative hierarchies, suggesting that local exposure was shaped more by the administrative rollout process than by household-level fertility or labor-supply decisions. A key early component was the rollout of Village Contraceptive Distribution Centers, managed by fieldworkers and local volunteers. These centers distributed subsidized contraceptive methods and were responsible for recruiting and retaining users (Adioetomo, 1993). Some informational and outreach functions were later also incorporated into integrated health posts (Posyandu), although these primarily focused on maternal and child health (Lubis, 2003).

Empirical studies of Indonesia's family planning expansion report differing estimates of its impact, in part because they examine different periods, outcomes, and levels of aggregation, and because fertility

decline in Indonesia also reflected broader socioeconomic change (Angeles et al., 2005; Gertler and Molyneaux, 1994; Hatton et al., 2018; Miller and Babiarz, 2016; Molyneaux and Gertler, 2000; Pitt et al., 1993). Taken together, the existing evidence suggests that improving local access to modern contraception through village-level distribution and subsidies contributed to fertility decline, even if the program was only one component of the broader transition. This motivates our focus on the staggered expansion of VDCs as a salient source of cohort and community variation in contraceptive exposure that underpins our empirical design.

#### **Data and methods**

##### *Sample*

This study draws on the Indonesian Family Life Survey (IFLS), a large longitudinal survey covering more than 30,000 individuals in 13 provinces that together represent over 80% of the national population (Strauss et al., 2016). The IFLS provides detailed socioeconomic, demographic, and health information at the individual and household levels, together with a Community-Facility Survey administered in each IFLS enumeration area. The Community-Facility Survey records whether a Village Contraceptive Distribution Center (VDC) was present in the community and the year in which it was established. These community-level records are linked to individual respondents through IFLS community identifiers. The survey also provides the contemporaneous community infrastructure measures used in the analysis, including access to electricity, community health centers, and piped water. Retrospectively reported fertility histories are used to construct complete family size.

The analysis uses the 2007 and 2014 IFLS waves and focuses on respondents aged 55 and older. This threshold corresponds to the statutory retirement age prevailing over much of the study period and captures individuals for whom continued employment and withdrawal from work are economically relevant outcomes. It also includes cohorts whose reproductive years overlapped with the expansion of Indonesia's family-planning program, generating variation in exposure to VDC availability during the reproductive life course. We restrict the sample to ever-married respondents and, in the preferred specification, to individuals with prior labor-market attachment, thereby focusing on later-life work continuation and withdrawal rather than lifetime non-participation. We also require residence information that permits a consistent assignment of historical VDC exposure and exclude observations with missing or inconsistent information on fertility, exposure, outcomes, controls, or other required covariates. The final analytical sample contains 3522 respondent-wave observations corresponding to 2644 unique respondents. Appendix Table A.1 documents the complete sample construction process. Supplementary estimates that include never-worked respondents or interdistrict movers produce substantively similar results.

##### *Variables and descriptive statistics*

##### *Later-life labor supply*

We measure later-life labor supply using two outcomes. The primary outcome is an indicator of whether an individual worked at any time during the previous twelve months. We adopt this extended reference period to account for the seasonal and irregular nature of later-life work in Indonesia. Relying on shorter windows, such as the previous week, risks misclassifying temporary inactivity as sustained labor-market withdrawal.

The second outcome is log weekly hours, defined as

$\ln(\text{weekly hours} + 1)$ . Respondents who did not work in the relevant survey wave are assigned zero weekly hours and remain in the estimation sample. This outcome therefore captures both the extensive margin of work participation and adjustments in hours worked. We retain zero-hour observations because restricting the sample to current workers would exclude transitions out of work and condition the analysis on an employment outcome that may itself be affected by fertility. Together, the two outcomes capture both complete withdrawal from work and broader reductions in labor supply.

#### *Children's participation in agricultural work*

To examine the principal mechanism, we construct an indicator for whether at least one linked child participates in agricultural work. This outcome captures whether additional fertility increases the availability of family labor in agricultural production, particularly among landholding households. Because the measure depends on successfully linked children and mechanically becomes more likely as the number of children increases, the estimates are interpreted as evidence consistent with family-labor substitution rather than as a complete measure of children's labor contribution.

#### *Intergenerational transfers*

To examine intergenerational support, we construct measures of economic flows between older parents and their children over the previous 12 months. Transfers received include the total value of cash and in-kind support from both co-resident and non-co-resident children, while transfers provided capture equivalent assistance from parents to their children. We combine these flows into net intergenerational transfers, defined as transfers received minus transfers provided. Because the resulting measure may take positive, zero, or negative values and is highly skewed, we use its inverse hyperbolic sine transformation. We also construct an indicator for positive net support, equal to one when transfers received exceed transfers provided. These measures capture whether older parents are net recipients of intergenerational support and the magnitude of the resulting net resource flow. They are interpreted as complementary evidence on family support rather than as a formal measure of mediation.

#### *Alternative mechanisms*

We examine two alternative explanations for fertility-related differences in later-life labor supply. First, we construct a composite index of chronic disease-related work limitation based on ten major illnesses. For each condition, respondents indicate whether they have ever been diagnosed and the extent to which it interferes with their ability to perform daily work or routine activities. The overall index sums up these severity scores across all reported conditions, capturing both the number and intensity of work-related constraints. Second, we measure care-related assistance provided by parents to their children using the annual number of days on which respondents report helping with household chores, childcare, or physical recovery. The measure covers assistance to both co-resident and non-resident children but does not separately identify grandchild care. Because the outcome is highly concentrated at zero and combines several types of assistance, the estimates are interpreted cautiously as changes in broad care-related responsibilities rather than as a direct measure of grandparental childcare.

#### *Control variables*

All regressions incorporate a comprehensive set of controls for socioeconomic status and baseline conditions, following standard specifications in the literature, such as those in [Rao and Zhang \(2024\)](#). These controls include individual and household characteristics, specifically

gender, marital status, years of schooling, pension ownership, household-head status, and the spouse's education, to account for differences in work capacity, household position, and access to formal old-age income. To account for structural labor-market differences, we further control for rural residence and local infrastructure conditions around the time of VCDC establishment, including access to electricity, piped water, and health clinics. The preferred specification also includes five-year age-group, province, and survey-wave fixed effects to flexibly account for nonlinear age patterns, time-invariant provincial differences, and common survey-wave effects.

To account more directly for observable historical conditions associated with VCDC placement, the preferred specification additionally includes three historical local characteristics. First, district-level labor-force participation is obtained from the 1971 Population Census ([Statistics Indonesia, 1972](#)). Second, district-level completed fertility is measured using the average number of children ever born among women aged 45–49 in the 1980 Population Census ([Statistics Indonesia, 1983](#)). Comparable completed-fertility information from the 1971 Census is available only at the provincial level, making the 1980 measure the earliest consistently available district-level fertility indicator that can be matched to IFLS locations. Third, we use the 1977 Ministry of Home Affairs village development classification, which categorized villages such as *Swadaya*, *Swakarya*, or *Swasembada*, corresponding respectively to lower, intermediate, and higher levels of development ([Ministry of Home Affairs, 1978](#)). We convert this classification into a normalized index ranging from zero to one, with higher values indicating greater historical village development, and aggregate it to the subdistrict level to match the finest historical geographic identifier available for IFLS communities. Together, these variables capture historical differences in local labor-market participation, fertility, and development that may have influenced VCDC establishment timing.

#### *Sample characteristics and the landholding profile*

[Fig. 1](#) compares working status and sectoral composition across the age profile by agricultural landholding status. Employment declines gradually with age in both groups, but landholding respondents remain more economically active throughout later life. Their employment is predominantly agricultural and persists into advanced ages, whereas landless respondents are more concentrated in non-agricultural activities and exhibit an earlier decline in employment. These patterns point to distinct forms of work organization across the two groups and motivate the subsequent focus on agricultural landholding as a potentially relevant source of heterogeneity.

[Table 1](#) further summarizes the main characteristics of the analytical sample and highlights several features relevant to the study. Formal old-age protection is extremely limited, with pension coverage of approximately 2% in both landholding and landless households. Fertility remains relatively high, with respondents having more than four children on average. Landholding households are substantially more rural and agriculturally oriented, although landless households are observed in both rural and urban areas. In the analytical sample, 74% of landholding households are located in rural areas, compared with 26% of landless households. Because rural residence is included separately in the empirical specification, the landholding distinction is not equivalent to a rural–urban classification.

Differences in educational attainment between the two groups are modest. Landholding respondents and their spouses have slightly fewer years of schooling on average, but the gap is not sufficiently large to characterize landholding as a simple proxy for broad socioeconomic advantage or disadvantage. [Fig. 2](#) provides a similar picture for current material living standards. The distributions of log real per capita

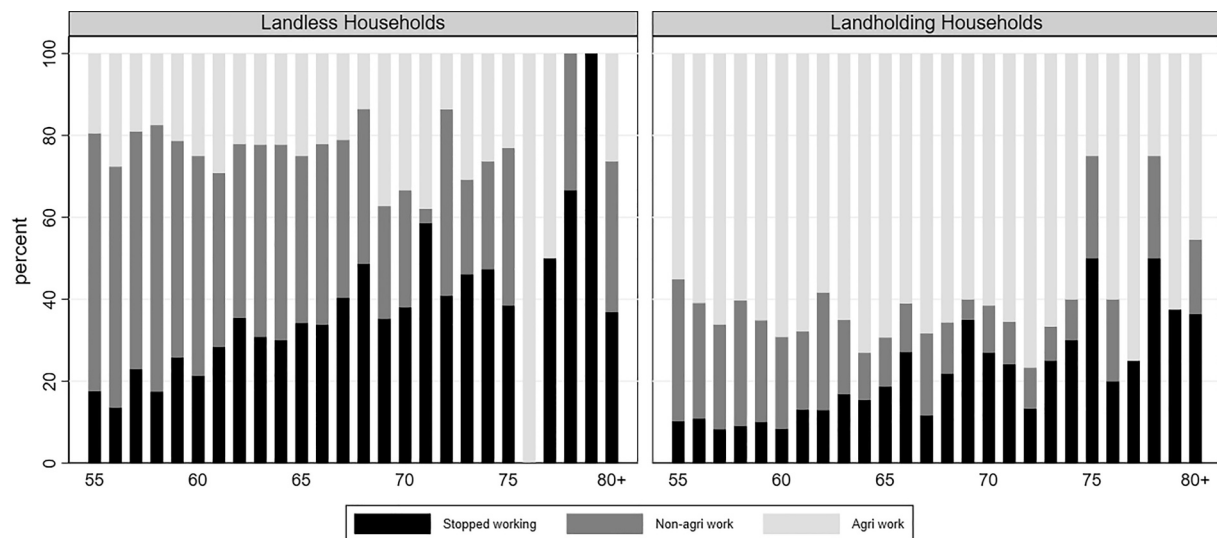


Fig. 1. Work status and sectoral composition by age and landholding status.

Table 1

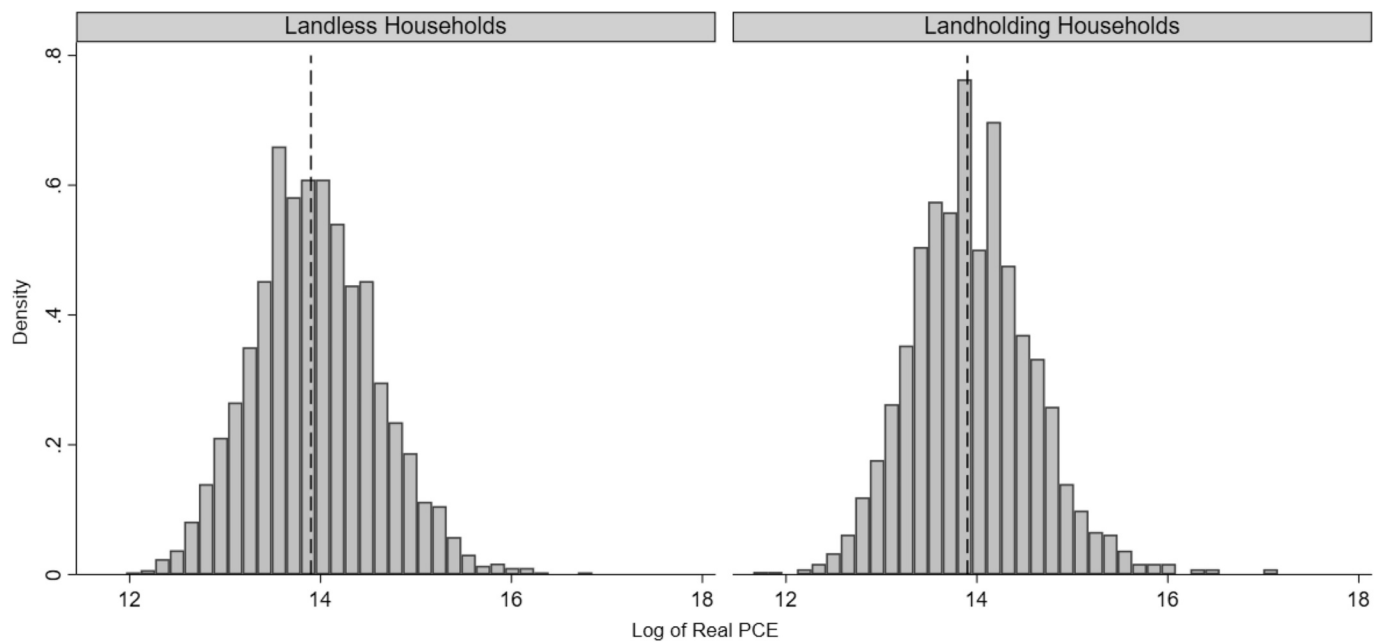
Descriptive statistics.

| Variables                                          | Full sample |           |        |       | Landholding households |           |        |       | Landless households |           |        |       |
|----------------------------------------------------|-------------|-----------|--------|-------|------------------------|-----------|--------|-------|---------------------|-----------|--------|-------|
|                                                    | Mean        | Std. Dev. | Min    | Max   | Mean                   | Std. Dev. | Min    | Max   | Mean                | Std. Dev. | Min    | Max   |
| Working Status (yes = 1)                           | 0.75        | 0.43      | 0      | 1     | 0.82                   | 0.39      | 0      | 1     | 0.69                | 0.46      | 0      | 1     |
| Log hours/week                                     | 2.7         | 1.53      | 0      | 5.13  | 2.88                   | 1.32      | 0      | 5.13  | 2.55                | 1.66      | 0      | 5.13  |
| Number of children                                 | 4.32        | 2.39      | 0      | 13    | 4.48                   | 2.42      | 0      | 13    | 4.18                | 2.36      | 0      | 13    |
| Elderly group                                      |             |           |        |       |                        |           |        |       |                     |           |        |       |
| 55–59                                              | 0.46        | 0.5       | 0      | 1     | 0.43                   | 0.5       | 0      | 1     | 0.48                | 0.5       | 0      | 1     |
| 60–64                                              | 0.3         | 0.46      | 0      | 1     | 0.31                   | 0.46      | 0      | 1     | 0.29                | 0.45      | 0      | 1     |
| 65+                                                | 0.24        | 0.43      | 0      | 1     | 0.26                   | 0.44      | 0      | 1     | 0.23                | 0.42      | 0      | 1     |
| Household Head (yes = 1)                           | 0.65        | 0.48      | 0      | 1     | 0.63                   | 0.48      | 0      | 1     | 0.66                | 0.47      | 0      | 1     |
| Male (yes = 1)                                     | 0.55        | 0.5       | 0      | 1     | 0.56                   | 0.5       | 0      | 1     | 0.55                | 0.5       | 0      | 1     |
| Years schooling                                    | 5.11        | 4.11      | 0      | 22    | 4.8                    | 3.98      | 0      | 19    | 5.37                | 4.19      | 0      | 22    |
| Spouse years schooling                             | 5.02        | 3.99      | 0      | 19    | 4.66                   | 3.79      | 0      | 19    | 5.31                | 4.12      | 0      | 19    |
| Rural (yes = 1)                                    | 0.48        | 0.5       | 0      | 1     | 0.74                   | 0.44      | 0      | 1     | 0.26                | 0.44      | 0      | 1     |
| Live separated with spouse (yes = 1)               | 0.14        | 0.35      | 0      | 1     | 0.11                   | 0.31      | 0      | 1     | 0.16                | 0.37      | 0      | 1     |
| Has a pension (yes = 1)                            | 0.02        | 0.15      | 0      | 1     | 0.02                   | 0.15      | 0      | 1     | 0.02                | 0.15      | 0      | 1     |
| IFLS4, 2007                                        | 0.38        | 0.48      | 0      | 1     | 0.39                   | 0.49      | 0      | 1     | 0.37                | 0.48      | 0      | 1     |
| IFLS5, 2014                                        | 0.62        | 0.48      | 0      | 1     | 0.61                   | 0.49      | 0      | 1     | 0.63                | 0.48      | 0      | 1     |
| Electricity availability (yes = 1)                 | 0.48        | 0.5       | 0      | 1     | 0.31                   | 0.46      | 0      | 1     | 0.63                | 0.48      | 0      | 1     |
| Pipe water availability (yes = 1)                  | 0.19        | 0.39      | 0      | 1     | 0.13                   | 0.34      | 0      | 1     | 0.24                | 0.43      | 0      | 1     |
| Central health clinic availability (yes = 1)       | 0.81        | 0.39      | 0      | 1     | 0.83                   | 0.38      | 0      | 1     | 0.8                 | 0.4       | 0      | 1     |
| Positive net intergenerational transfers (yes = 1) | 0.31        | 0.46      | 0      | 1     | 0.29                   | 0.46      | 0      | 1     | 0.31                | 0.46      | 0      | 1     |
| IHS net intergenerational transfers                | 2.58        | 9.34      | –22.80 | 23.61 | 2.13                   | 9.60      | –21.42 | 23.61 | 2.96                | 9.11      | –22.80 | 22.52 |
| Chronic disease work limitation                    | 1.67        | 2.63      | 0      | 23    | 1.52                   | 2.47      | 0      | 19    | 1.8                 | 2.76      | 0      | 23    |
| Timing instrument                                  | 0.58        | 0.24      | 0      | 1     | 0.56                   | 0.24      | 0      | 1     | 0.59                | 0.23      | 0      | 1     |
| Observations                                       | 3522        |           |        |       | 1596                   |           |        |       | 1926                |           |        |       |

expenditure overlap substantially and are centered near the overall sample median. Thus, although agricultural land is itself an important component of household wealth, landholding status is not sharply differentiated by current consumption expenditure, educational attainment, or formal pension access.

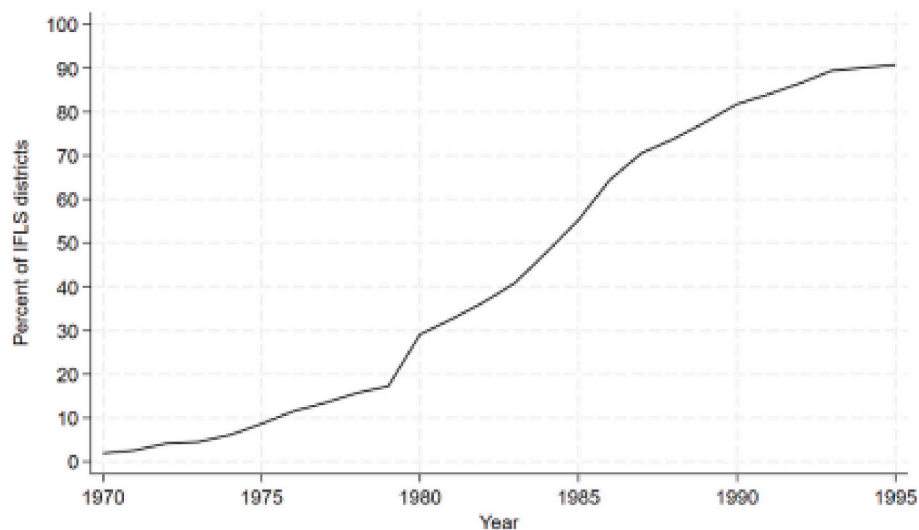
The more salient distinction concerns access to a productive agricultural asset and the organization of work around it. Landholding households combine greater rural concentration, sustained agricultural employment at older ages, and ownership of an asset that may require family labor, supervision, or management. By contrast, landless

households report slightly higher net intergenerational support on average, particularly in the amount of net transfers, yet they have less direct scope for adult children to assume responsibilities on household-owned agricultural land. These descriptive patterns suggest that financial support alone may not distinguish the two groups as clearly as access to a productive asset. We therefore interpret landholding primarily as access to an asset that may create opportunities for intergenerational labor substitution, rather than as a simple proxy for rural residence, current economic resources, educational attainment, pension coverage, or financial support from children.



Notes\* The vertical dashed line shows the median of log of real percapita expenditure for the full sample.

**Fig. 2.** Log of per capita expenditure by landholding status.



**Fig. 3.** Cumulative share of IFLS communities with a VCDC.  
(Source: Authors' calculations)

### Empirical strategy

#### Fertility instrument

Estimating the causal impact of fertility on later-life supply is complicated by endogeneity, because fertility may be correlated with unobserved preferences, family norms, and long-run economic conditions that also shape work in old age. Recent work on the long-run consequences of fertility has increasingly used early family planning expansions as a source of quasi-experimental variation (Chen and Fang, 2021; Rao and Zhang, 2024). In the Indonesian context, Hatton et al. (2018) were the first to exploit the staggered introduction of village-

level contraceptive distribution points as an instrument for fertility. We exploit differences in reproductive-age exposure to the staggered establishment of Village Contraceptive Distribution Centers across IFLS communities and birth cohorts. Valid establishment-year information is available for 284 of the 312 IFLS communities, with timing variation observed within every sampled province. The instrument therefore combines community-specific establishment timing with cohort differences in the share of reproductive years exposed to VCDC availability. Figs. 3 and 4 document the temporal and spatial variation underlying this design.

Building on this variation, we construct a cohort-specific exposure

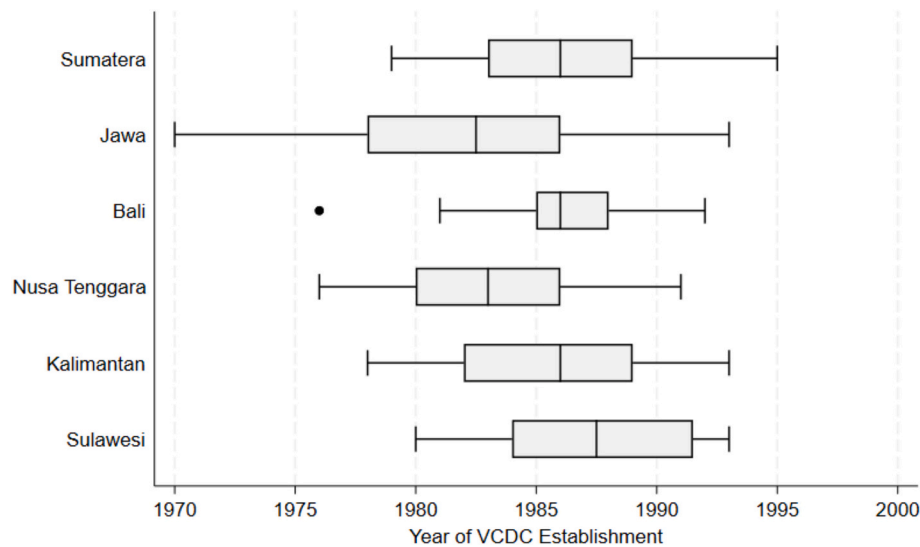


Fig. 4. Distribution of VCDC establishment years by island group.

**Table 2**  
Historical predictors of VCDC establishment timing.

| Variables                                                                            | Historical Labor market | Historical Fertility | Historical Development | Full model      |
|--------------------------------------------------------------------------------------|-------------------------|----------------------|------------------------|-----------------|
| Labor force participation, 1971 Census                                               | 0.059 (0.044)           |                      |                        | 0.030 (0.047)   |
| Children ever born, women aged 45–49, 1980 Census                                    |                         | 0.453 (0.634)        |                        | 0.571 (0.623)   |
| Historical village development, 1977 Ministry of Home Affairs Province Fixed Effects |                         |                      | −3.305** (1.488)       | −2.989* (1.600) |
| Observations                                                                         | 284                     | 284                  | 284                    | 284             |
| R-squared                                                                            | 0.178                   | 0.173                | 0.186                  | 0.190           |

*Notes:* The dependent variable is the calendar year in which the VCDC was established in the IFLS community. Labor-force participation and children ever born are measured at the district level, while historical village development is measured at the subdistrict level. A negative coefficient indicates earlier VCDC establishment. All specifications include province fixed effects. Heteroskedasticity-robust standard errors are reported in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

index that captures access to VCDCs during a woman's reproductive years. Following Wu and Li (2012), exposure is set equal to one for women who were younger than 15 when a VCDC was introduced in their community and to zero for women aged 49 or older at that time. For women between these ages, exposure is defined as the proportion of reproductive years, specifically ages 15 to 49, during which a VCDC was available in their locality:

$$\frac{49 - \text{age at VCDC rollout}}{49 - 15} \quad (1)$$

This measure, therefore, captures differences in contraceptive exposure across women who faced the program at different points in their reproductive life cycle. To reduce measurement error in assigning

program exposure, we exclude women whose district of residence at age 12 differs from their district of residence in the survey.

The validity of the instrument requires that reproductive-age VCDC exposure affect later-life labor supply only through completed fertility, conditional on the included covariates. A central concern is that community-level VCDC establishment may not have been random. Earlier placement may have reflected pre-existing local labor-market conditions, fertility patterns, or differences in local development and administrative capacity, each of which could also be related to later-life employment.

We examine this concern directly by testing whether VCDC establishment timing is associated with district labor-force participation in 1971, district completed fertility in 1980, and historical village development in 1977. As reported in Table 2, historical labor-force participation and completed fertility are not significantly associated with establishment timing, while historically more developed localities received VCDCs earlier. The rollout should therefore not be characterized as unconditionally random. We include all three historical measures in the preferred IV specification alongside community infrastructure controls, rural residence, and province, survey-wave, and five-year age-group fixed effects.

We also use Kinky Least Squares by Kripfganz and Kiviet (2021) to assess the sensitivity of the preferred IV estimates to potential violations of the exclusion restriction. This approach evaluates how the estimated fertility effects change under alternative values of the postulated correlation between the instrument and the structural error term. The results are reported in the robustness analysis.

We further assess the robustness of the results using alternative age controls, a demanding mother cohort-group-by-province specification, and several placebo tests. These exercises provide additional evidence that the findings are not readily explained by alternative age, cohort, or province-specific trends. Taken together, the preferred estimates rely on conditional identification rather than an assumption of fully random VCDC placement.

#### Model specification

We begin our analysis by estimating the following baseline specification:

$$L_{it} = \beta_0 + \beta_1 N_i + X_{it}'\beta_2 + R_{it}'\beta_3 + \mu_p + \lambda_t + \gamma_a + \varepsilon_{it} \quad (2)$$

where  $L_{it}$  denotes the labor-supply outcome of individual  $i$  observed in survey wave  $t$ , measured by either working status or log weekly hours. The latter is defined as  $\ln(\text{weekly hours} + 1)$ , includes nonworkers with zero hours, and therefore captures both the extensive and intensive margins of labor supply.  $N_i$  denotes the respondent's total number of children.  $X_{it}$  is a vector of individual- and household-level covariates, including respondent gender, educational attainment, spouse's education, household-head status, marital or spousal-living arrangement, and pension status.  $R_{it}$  includes rural residence, contemporaneous community infrastructure measures, and historical controls for the placement of family-planning services. The terms  $\mu_p$ ,  $\lambda_t$ , and  $\gamma_a$  denote province, survey-wave, and five-year age-group fixed effects, respectively, with respondents aged 65 and older grouped in the terminal age category. The error term is denoted by  $\varepsilon_{it}$ .

We first estimate Eq. (2) by ordinary least squares to describe the conditional association between fertility and later-life labor supply. Because completed fertility may be correlated with unobserved determinants of later-life work, we then estimate the model using two-stage least squares. The first-stage equation is:

$$N_i = \alpha_0 + \alpha_1 Z_i + X_i'\alpha_2 + R_i'\alpha_3 + \pi_p + \tau_t + \kappa_a + u_{it} \quad (3)$$

where  $Z_i$  is the timing-based measure of exposure to VCDC availability during the respondent's reproductive years. The terms  $\pi_p$ ,  $\tau_t$ , and  $\kappa_a$  denote the province, survey-wave, and age-group fixed effects in the first-stage equation. The second stage instruments the number of children in Eq. (2) using  $Z_i$ , with the same covariates and fixed-effect structure included in both stages. Standard errors are clustered at the district level to account for common shocks and within-district dependence.

Under the maintained instrumental-variable assumptions, the 2SLS estimates capture the effect of completed fertility on later-life labor supply for respondents whose fertility responded to differential exposure to the expansion of VCDC availability during their reproductive years. The estimates should therefore be interpreted as local effects for fertility-responsive households rather than as population-average effects for all older Indonesians. Because the instrument is continuous, this interpretation does not correspond to a single directly observable or discrete complier group.

We next examine whether the estimated relationship varies across household settings. The principal heterogeneity analysis distinguishes households according to agricultural landholding status, which captures access to a productive asset around which family labor and agricultural responsibilities can be organized. As an additional analysis, we examine heterogeneity by educational attainment.

**Table 3**

First stage estimates of VCDC exposure on completed fertility.

| Variables                                  | Full sample          | Landholding          | Landless             |
|--------------------------------------------|----------------------|----------------------|----------------------|
| Exposure to VCDC during reproductive years | −1.887***<br>(0.344) | −1.959***<br>(0.520) | −1.754***<br>(0.400) |
| Observations                               | 3522                 | 1596                 | 1926                 |
| First-stage F-statistic                    | 30.13                | 14.20                | 19.22                |

*Notes:* The dependent variable is completed fertility. All models use the preferred specification, including individual and household controls, contemporaneous community infrastructure measures, historical district labor-force participation, historical district fertility, the historical village-development index, rural residence, province fixed effects, survey-wave fixed effects, and five-year age-group fixed effects with a terminal category for ages 65 and older. Standard errors clustered at the district level are reported in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$ .

**Table 4**

Fertility and later-life labor supply.

|                         | OLS                  |                     | Instrumental Variable (IV) |                   |
|-------------------------|----------------------|---------------------|----------------------------|-------------------|
|                         | Working Status       | Log hours/ week     | Working Status             | Log hours/ week   |
| Number of children      | −0.011***<br>(0.003) | −0.031**<br>(0.013) | −0.032<br>(0.020)          | −0.108<br>(0.076) |
| Observations            | 3522                 | 3522                | 3522                       | 3522              |
| First-stage F-statistic |                      |                     | 30.13                      | 30.13             |

*Notes:* The dependent variables are an indicator equal to one if the respondent worked during the reference period and log weekly hours, defined as  $\ln(\text{weekly hours} + 1)$ . The latter includes non-workers with zero hours and therefore captures both the extensive and intensive margins of labor supply. All specifications include individual and household controls, contemporaneous community infrastructure measures, historical placement controls, rural residence, province fixed effects, survey-wave fixed effects, and five-year age-group fixed effects, with respondents aged 65 and older grouped in the terminal category. Standard errors clustered at the district level are reported in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$ .

**Table 5**

Fertility and later-life labor supply by agricultural landholding status.

|                                        | OLS                  |                    | Instrumental variable (IV) |                     |
|----------------------------------------|----------------------|--------------------|----------------------------|---------------------|
|                                        | Working status       | Log hours/ week    | Working status             | Log hours/ week     |
| <i>Panel A: Landholding households</i> |                      |                    |                            |                     |
| Number of children                     | −0.014***<br>(0.004) | −0.028*<br>(0.015) | −0.064**<br>(0.030)        | −0.196**<br>(0.100) |
| Observations                           | 1596                 | 1596               | 1596                       | 1596                |
| First-stage F-statistic                |                      |                    | 14.20                      | 14.20               |
| <i>Panel B: Landless households</i>    |                      |                    |                            |                     |
| Number of children                     | −0.009*<br>(0.005)   | −0.032<br>(0.020)  | −0.021<br>(0.031)          | −0.102<br>(0.122)   |
| Observations                           | 1926                 | 1926               | 1926                       | 1926                |
| First-stage F-statistic                |                      |                    | 19.22                      | 19.22               |

*Notes:* The dependent variables are an indicator equal to one if the respondent worked during the reference period and log weekly hours, defined as  $\ln(\text{weekly hours} + 1)$ . Log weekly hours includes non-workers with zero hours and therefore captures both the extensive and intensive margins of labor supply. All specifications include individual and household controls, contemporaneous community infrastructure measures, historical placement controls, rural residence, province fixed effects, survey-wave fixed effects, and five-year age-group fixed effects with a terminal category for respondents aged 65 and older. Landholding status is defined according to whether the respondent's household owns agricultural land. Standard errors clustered at the district level are reported in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$ .

## Results and discussion

### First-stage relationship between VCDC exposure and fertility

Table 3 reports the first-stage relationship between exposure to VCDC availability during women's reproductive years and completed fertility across the full sample and key subsamples. The instrument strongly predicts fertility in all specifications. The first-stage F-statistic is 30.13 in the full sample and remains above the conventional threshold of 10 in the landholding and landless subsamples, at 14.20 and 19.22, respectively. These values indicate that the timing-based exposure measure retains substantial predictive power after the inclusion of

historical placement controls and the other covariates in the preferred specification.

The estimated coefficients indicate that greater exposure to VCDC availability is consistently associated with lower completed fertility. Because the exposure measure ranges from zero to one, the full-sample coefficient can be interpreted as the predicted difference in completed fertility between hypothetical no exposure and full reproductive-age exposure. This comparison spans the full range of the instrument, however, and should not be interpreted as representing a typical change in exposure. Fig. A.1 further shows that average completed fertility declines across increasing levels of VCDC exposure, supporting the plausibility of a monotonic first-stage relationship.

#### Baseline estimates for fertility and later-life labor supply

Table 4 reports the OLS and IV estimates of the relationship between fertility and later-life labor supply. The OLS estimates indicate that an additional child is associated with a 1.1-percentage-point reduction in working status and a 0.031-log-point reduction in weekly hours. The preferred IV estimates are also negative and larger in magnitude, at  $-0.032$  and  $-0.108$ , but are not statistically distinguishable from zero. The full-sample results therefore provide limited evidence of a uniform average effect among fertility-responsive households represented by the IV estimates. This aggregate pattern may nevertheless mask heterogeneity across household production settings, which we examine next by agricultural landholding status and, subsequently, educational attainment.

#### Heterogeneity analysis by agricultural landholding

Table 5 examines heterogeneity in the relationship between fertility and later-life labor supply by agricultural landholding status. Both the OLS and IV estimates suggest a clear contrast between landholding and landless households, although the IV results provide a more credible

**Table 6**  
Fertility and later-life labor supply by educational attainment.

|                                                       | OLS                       |                          | Instrumental Variable (IV) |                     |
|-------------------------------------------------------|---------------------------|--------------------------|----------------------------|---------------------|
|                                                       | Working Status            | Log hours/ week          | Working Status             | Log hours/ week     |
| <i>Panel A: Less than completed primary education</i> |                           |                          |                            |                     |
| Number of children                                    | $-0.016^{***}$<br>(0.004) | $-0.045^{**}$<br>(0.018) | $-0.051^*$<br>(0.029)      | $-0.105$<br>(0.097) |
| Observations                                          | 1796                      | 1796                     | 1796                       | 1796                |
| First-stage F-statistic                               |                           |                          | 15.30                      | 15.30               |
| <i>Panel B: Completed primary education or higher</i> |                           |                          |                            |                     |
| Number of children                                    | $-0.005$<br>(0.005)       | $-0.011$<br>(0.019)      | $-0.006$<br>(0.031)        | $-0.068$<br>(0.114) |
| Observations                                          | 1726                      | 1726                     | 1726                       | 1726                |
| First-stage F-statistic                               |                           |                          | 25.99                      | 25.99               |

**Notes:** The dependent variables are an indicator equal to one if the respondent worked during the reference period and log weekly hours, defined as  $\ln(\text{weekly hours} + 1)$ . Log weekly hours includes non-workers with zero hours and therefore captures both the extensive and intensive margins of labor supply. Respondents are classified according to whether they completed primary education. All specifications include individual and household controls, contemporaneous community infrastructure measures, historical placement controls, rural residence, province fixed effects, survey-wave fixed effects, and five-year age-group fixed effects with a terminal category for respondents aged 65 and older. Standard errors clustered at the district level are reported in parentheses.

\*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$ .

**Table 7**  
Availability and coverage of the child-agricultural-work outcome.

|                                                                     | Full sample      | Landholding      | Landless         |
|---------------------------------------------------------------------|------------------|------------------|------------------|
| Final respondent-wave observations                                  | 3522             | 1596             | 1926             |
| Child agricultural-work outcome available, N (% of baseline sample) | 3334<br>(94.66%) | 1512<br>(94.74%) | 1822<br>(94.60%) |
| At least one linked child works in agriculture, N (% of available)  | 931<br>(27.92%)  | 626<br>(41.40%)  | 305<br>(16.74%)  |
| Mean share of reported children linked, capped at 100%              | 76.87%           | 73.78%           | 79.45%           |

**Notes:** The child agricultural-work outcome equals one if at least one linked child is observed working in agriculture in the corresponding IFLS wave and zero otherwise. Because employment information is observed only for linked children, the indicator is an incomplete measure of observed child agricultural participation. Percentages are calculated relative to the analytical or available sample as indicated. The linked-child recovery rate is the average percentage of reported children successfully linked to IFLS individual records, capped at 100%.

basis for causal interpretation. Among landholding households in Panel A, an additional child reduces the probability that an older individual remains in work by 6.4 percentage points and log weekly hours by 0.196 log points. Both estimates are statistically significant at the 5% level. The participation effect is economically meaningful relative to the subgroup working rate of approximately 82%, corresponding to about 8% of that mean. It is also broadly comparable in order of magnitude to the 9.9-percentage-point effect reported by Rao and Zhang (2024) for parents aged 60 and older in China, although our estimate applies specifically to agricultural landholding households. Because log weekly hours includes nonworkers with zero hours, the hours estimate captures changes across both the extensive and intensive margins rather than a pure percentage reduction in hours among workers.

By contrast, the landless estimates are smaller and statistically insignificant. This pattern is consistent with fertility reducing later-life labor supply primarily in households where agricultural land creates greater scope for family-labor substitution, although the subgroup contrast is not itself a formal test of coefficient equality.

#### Additional heterogeneity analysis

Table 6 presents additional heterogeneity by educational attainment, a predetermined indicator of long-run socioeconomic position that is less susceptible to endogeneity than current income, expenditure, or wealth. Among respondents with less than completed primary education, the IV coefficient for working status is negative and marginally significant, while the log-hours coefficient is negative but imprecise. The corresponding estimates for respondents with at least completed primary education are smaller and statistically insignificant. The pattern suggests that the negative fertility effect may be concentrated among respondents with lower educational attainment, although the difference across education groups is not established statistically.

**Table 8**  
Fertility and the probability that at least one linked child works in agriculture.

|                         | Full sample        | Landholding        | Landless         |
|-------------------------|--------------------|--------------------|------------------|
| Number of children      | 0.079**<br>(0.034) | 0.096**<br>(0.043) | 0.029<br>(0.037) |
| Observations            | 3334               | 1512               | 1822             |
| First-stage F-statistic | 31.38              | 14.50              | 24.69            |

**Notes:** The dependent variable equals one if at least one linked child is observed working in agriculture and zero otherwise. All specifications include individual and household controls, contemporaneous community infrastructure measures, historical placement controls, rural residence, province fixed effects, survey-wave fixed effects, and five-year age-group fixed effects with a terminal category for respondents aged 65 and older. Standard errors clustered at the district level are reported in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$ .

Appendix Table A.2 reports gender-specific estimates, although the male and female subsamples are not directly comparable because male respondents are included only when linked to a female partner with available fertility histories, whereas female respondents are not subject to the same linkage requirement. The coefficient is negative and precisely estimated among men, while the female estimate is inconclusive because the instrument is extremely weak in that subsample.

#### Children's participation in agricultural work

We construct an indicator equal to one if at least one linked child is observed working in agriculture in the corresponding IFLS wave and zero otherwise. Because employment information is available only for children who can be linked to individual IFLS records, the measure captures observed rather than complete child agricultural participation, and a value of zero does not necessarily imply that none of the respondent's unlinked children works in agriculture. As shown in Table 7, the outcome is available for 3334 of the 3522 respondent-wave observations, with nearly identical coverage among landholding and landless households at 94.74 and 94.60%, respectively. Among observations with available information, 41.40% of landholding households have at least one linked child working in agriculture, compared with 16.74% of landless households. The mean share of reported children successfully linked is lower among landholding than landless households, at 73.78 and 79.45%, respectively. Because employment is not observed for unlinked children, however, the direction and magnitude of any resulting measurement bias cannot be determined definitively.

Table 8 shows IV estimates of fertility and the probability that at least one linked child works in agriculture. The positive full-sample relationship between fertility and children's agricultural work is most clearly expressed among landholding households. An additional child increases the probability that at least one linked child works in agriculture by 7.9 percentage points in the full sample and by 9.6 percentage points among landholding households, with both estimates statistically significant at the 5% level. By contrast, the corresponding estimate for landless households is 2.9 percentage points and statistically indistinguishable from zero. The first-stage F-statistics remain above the conventional threshold of 10 in all three samples.

These results should be interpreted cautiously. Because the outcome records whether at least one linked child works in agriculture, part of the association may mechanically reflect having more children who could potentially enter agricultural work. The measure also does not directly observe the transfer of tasks, farm-management responsibilities, or working hours from parents to children. Nevertheless, among landholding households, higher fertility is associated with greater observed child agricultural participation and lower parental employment. This

**Table 9**  
Fertility and net intergenerational support.

| Sample group | Positive net support | IHS net intergenerational transfers | Observations | First-stage F-statistic |
|--------------|----------------------|-------------------------------------|--------------|-------------------------|
| All sample   | 0.058**<br>(0.023)   | 0.61<br>(0.510)                     | 3522         | 30.13                   |
| Landholding  | 0.045<br>(0.028)     | 0.354<br>(0.664)                    | 1596         | 14.2                    |
| Landless     | 0.088***<br>(0.033)  | 1.346**<br>(0.638)                  | 1926         | 19.22                   |

**Notes** Each coefficient reports the IV estimate of the number of children. Positive net support equals one when transfers received from children exceed transfers provided to children. Net transfers are transformed using the inverse hyperbolic sine. All specifications use the preferred model with historical placement controls and five-year age-group fixed effects. District-clustered standard errors are reported in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

**Table 10**  
Fertility and alternative mechanisms.

|                                                              | Full sample       | Landholding      | Landless          |
|--------------------------------------------------------------|-------------------|------------------|-------------------|
| <i>Panel A. Health-related work limitations</i>              |                   |                  |                   |
| Number of children                                           | -0.060<br>(0.122) | 0.212<br>(0.185) | -0.289<br>(0.217) |
| Observations                                                 | 3522              | 1596             | 1926              |
| <i>Panel B. Care-related assistance provided to children</i> |                   |                  |                   |
| Number of children                                           | 1.052<br>(3.486)  | 4.781<br>(5.009) | -3.526<br>(5.900) |
| Observations                                                 | 3522              | 1596             | 1926              |
| First-stage F-statistic                                      | 30.13             | 14.2             | 19.22             |

**Notes:** Panel A reports 2SLS estimates for self-reported health-related limitations on the respondent's ability to work. Panel B reports estimates for the annual number of days on which respondents provided assistance to children involving household chores, childcare, or physical recovery. All models use the preferred specification and include the historical placement controls, individual, household, and community covariates, province and survey-year fixed effects, and five-year age-group fixed effects. Standard errors clustered at the district level are reported in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$ .

alignment is consistent with family-labor substitution, without establishing the mechanism conclusively.

#### Intergenerational transfers

Table 9 examines whether fertility changes net intergenerational support in ways that may help explain later-life labor-supply responses. In the full sample, an additional child increases the probability that older parents receive positive net support, although the estimated effect on the amount of net transfers is imprecise. The contrast by landholding status is more informative. Among landholding households, fertility reduces later-life work, yet neither the probability of positive net support nor the amount of net transfers increases significantly. For this group, the accompanying rise in the probability that at least one linked child works in agriculture provides the strongest available evidence consistent with family-labor substitution. Among landless households, by contrast, fertility increases both positive net support and the amount of net transfers but does not reduce later-life labor supply.

This contrasting pattern suggests that greater net financial support alone is insufficient to account for the labor-supply response. Rather, the productive context in which support is provided appears to matter. Children may enable older parents to reduce work when they can assume labor or management responsibilities in family-based agricultural production. Among landless households, financial support may provide only limited relief relative to household needs and may therefore be insufficient to reduce the need for continued work. In this respect, the results complement Cameron and Cobb-Clark (2008) by suggesting that the limited substitutability they document between children's transfers and older parents' labor supply may arise because financial support does not automatically translate into effective relief from work. We therefore interpret the transfer results as complementary evidence on intergenerational resource flows rather than as evidence from a formal mediation analysis.

#### Alternative mechanisms

We examine whether the labor-supply results may reflect changes in parents' health-related work capacity or increased care-related responsibilities toward children. Panel A of Table 10 shows no statistically significant relationship between fertility and the composite index of chronic disease-related work limitation in the full sample or by landholding status. Panel B similarly provides no affirmative evidence that additional fertility increases care-related assistance provided by parents

**Table 11**

Robustness to alternative controls for age and cohort effects.

| Outcome: Working Status     |                        |                     |                     |                   |
|-----------------------------|------------------------|---------------------|---------------------|-------------------|
|                             | Five-year age-group FE | Continuous age      | Age and age squared | Birth-year FE     |
| <i>Panel A: Full Sample</i> |                        |                     |                     |                   |
| Number of children          | −0.032<br>(0.020)      | −0.023<br>(0.018)   | −0.018<br>(0.022)   | −0.010<br>(0.023) |
| Observations                | 3522                   | 3522                | 3522                | 3522              |
| First-stage F-statistic     | 30.13                  | 32.17               | 25.29               | 23.76             |
| <i>Panel B: Landholding</i> |                        |                     |                     |                   |
| Number of children          | −0.064**<br>(0.030)    | −0.053**<br>(0.027) | −0.064*<br>(0.034)  | −0.049<br>(0.035) |
| Observations                | 1596                   | 1596                | 1596                | 1596              |
| First-stage F-statistic     | 14.20                  | 13.97               | 9.90                | 8.70              |
| <i>Panel C: Landless</i>    |                        |                     |                     |                   |
| Number of children          | −0.021<br>(0.031)      | −0.012<br>(0.030)   | 0.003<br>(0.034)    | 0.007<br>(0.036)  |
| Observations                | 1926                   | 1926                | 1926                | 1926              |
| First-stage F-statistic     | 19.22                  | 22.41               | 17.63               | 15.07             |

| Outcome: Log working hours  |                        |                   |                     |                   |
|-----------------------------|------------------------|-------------------|---------------------|-------------------|
|                             | Five-year age-group FE | Continuous age    | Age and age squared | Birth-year FE     |
| <i>Panel A: Full Sample</i> |                        |                   |                     |                   |
| Number of children          | −0.108<br>(0.076)      | −0.059<br>(0.071) | −0.051<br>(0.081)   | −0.033<br>(0.089) |
| Observations                | 3522                   | 3522              | 3522                | 3522              |
| 1st stage F-statistic       | 30.13                  | 32.17             | 25.29               | 23.76             |
| <i>Panel B: Landholding</i> |                        |                   |                     |                   |
| Number of children          | −0.196**<br>(0.100)    | −0.143<br>(0.091) | −0.178<br>(0.113)   | −0.133<br>(0.116) |
| Observations                | 1596                   | 1596              | 1596                | 1596              |
| 1st stage F-statistic       | 14.20                  | 13.97             | 9.90                | 8.70              |
| <i>Panel C: Landless</i>    |                        |                   |                     |                   |
| Number of children          | −0.102<br>(0.122)      | −0.055<br>(0.116) | −0.023<br>(0.126)   | −0.024<br>(0.138) |
| Observations                | 1926                   | 1926              | 1926                | 1926              |
| 1st stage F-statistic       | 19.22                  | 22.41             | 17.63               | 15.07             |

**Notes:** The table reports robustness to alternative controls for age and cohort effects. The baseline specification includes five-year age-group fixed effects; alternative specifications use continuous age, age and age squared, or birth-year fixed effects. All other controls are unchanged. Robust standard errors clustered at the district level are reported in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$ .

**Table 12**

Fertility and later-life labor supply with mother cohort group x province fixed effects.

| Outcome                 | Full sample       | Landholding        | Landless          |
|-------------------------|-------------------|--------------------|-------------------|
| Working status          | −0.040<br>(0.038) | −0.091*<br>(0.054) | −0.016<br>(0.069) |
| Ln hours/week           | −0.069<br>(0.143) | −0.252<br>(0.182)  | −0.035<br>(0.262) |
| Observations            | 3522              | 1596               | 1926              |
| First-stage F-statistic | 8.880             | 4.585              | 3.921             |

**Notes:** Each row reports the coefficient on the number of children from a separate IV regression. The dependent variables are an indicator equal to one if the respondent worked during the reference period and log weekly hours, defined as  $\ln(\text{weekly hours} + 1)$ . All specifications include the individual and household controls, contemporaneous community infrastructure measures, historical placement controls, rural residence, survey-wave fixed effects, age-group fixed effects, and mother-cohort-group-by-province fixed effects used in the preferred specification. Standard errors clustered at the district level are reported in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$ .

to their children. The assistance estimates are imprecise and vary in direction across the landholding and landless subsamples. Moreover, the outcome is highly concentrated at zero, combines several forms of assistance, and does not separately identify grandchild care. The available measures therefore provide no clear evidence that declining work capacity or increased parental assistance explains the main labor-supply results, although the imprecision and measurement limitations prevent us from ruling out these channels.

### Robustness analysis

We begin the robustness analysis by assessing the sensitivity of the preferred IV estimates to possible violations of the exclusion restriction using kinky least squares (KLS). Appendix Fig. A.2 plots the estimated fertility coefficients and their 95% confidence intervals under alternative postulated correlations between completed fertility and the structural error term. The accompanying KLS exclusion diagnostics indicate that  $\rho = 0$ , corresponding to valid exclusion, is included in the 95% set of correlations compatible with the data for the full, landholding, and landless samples. Consequently, the preferred estimates therefore remain compatible with valid exclusion under the maintained specification. We interpret this result as supportive rather than definitive because compatibility with  $\rho = 0$  does not establish that the exclusion restriction holds or rule out unobserved determinants of VCDC rollout.

Having examined exclusion restriction sensitivity, Table 11 evaluates whether the results are sensitive to alternative controls for age and cohort effects by replacing the preferred five-year age-group fixed effects with continuous age, age and age squared, and birth-year fixed effects, while holding the sample and all other controls constant. The working-status estimates for landholding households remain negative and similar in magnitude across specifications, ranging from −0.049 to −0.064, although precision declines as the controls become more demanding and the first-stage F-statistic falls below 10 under the quadratic-age and birthyear fixed-effects specifications. The corresponding estimates for log weekly hours also remain negative, ranging from −0.133 to −0.196, but are less precisely estimated outside the preferred specification. Overall, the directional pattern is preserved, but the evidence is stronger for the decision to remain in work than for adjustments in weekly hours, while highly flexible cohort controls reduce instrument strength by absorbing part of the variation used for identification.

As a demanding robustness exercise, Table 12 augments the

**Table 13**  
Robustness of the fertility estimates to controlling for the share of sons.

|                             | Working status      |                     | Log weekly hours    |                     |
|-----------------------------|---------------------|---------------------|---------------------|---------------------|
|                             | Baseline            | + Share of sons     | Baseline            | + Share of sons     |
| <i>Panel A. Full sample</i> |                     |                     |                     |                     |
| Number of children          | −0.036*<br>(0.020)  | −0.035*<br>(0.021)  | −0.118<br>(0.076)   | −0.116<br>(0.077)   |
| Share of sons               |                     | 0.040<br>(0.031)    |                     | 0.142<br>(0.115)    |
| Observations                | 3400                | 3400                | 3400                | 3400                |
| First-stage F-statistics    | 30.54               | 30.59               | 30.54               | 30.59               |
| <i>Panel B. Landholding</i> |                     |                     |                     |                     |
| Number of children          | −0.066**<br>(0.031) | −0.065**<br>(0.030) | −0.200**<br>(0.099) | −0.198**<br>(0.099) |
| Share of sons               |                     | 0.057<br>(0.042)    |                     | 0.181<br>(0.144)    |
| Observations                | 1550                | 1550                | 1550                | 1550                |
| First-stage F-statistics    | 13.48               | 13.54               | 13.48               | 13.54               |
| <i>Panel C. Landless</i>    |                     |                     |                     |                     |
| Number of children          | −0.024<br>(0.032)   | −0.023<br>(0.032)   | −0.112<br>(0.123)   | −0.110<br>(0.123)   |
| Share of sons               |                     | 0.029<br>(0.043)    |                     | 0.112<br>(0.161)    |
| Observations                | 1850                | 1850                | 1850                | 1850                |
| First-stage F-statistics    | 23.59               | 23.73               | 23.59               | 23.73               |

**Notes:** The table reports IV estimates estimated on the valid-share sample. The baseline columns reproduce the preferred specification on this restricted sample without controlling children's gender composition, while the adjacent columns add the share of sons among children ever born. The valid-share sample includes respondents with at least one child for whom the reported numbers of sons and daughters are available, internally consistent, and sum to the total number of children used in the main analysis. The share of sons is defined as the number of sons divided by the total number of children ever born. The preferred estimates based on the unrestricted analytical sample of 3522 respondent-wave observations are reported separately in Tables 3 and 4. All specifications include the individual and household controls, contemporaneous community infrastructure measures, historical placement controls, rural residence, province fixed effects, survey-wave fixed effects, and age-group fixed effects used in the preferred specification. Standard errors clustered at the district level are reported in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$ .

preferred specification with mother-cohort-group-by-province fixed effects, thereby absorbing province-specific cohort patterns that may coincide with the expansion of family-planning services. This highly restrictive specification removes a substantial share of the variation used for identification. The coefficients remain negative for landholding households, with the working-status estimate being significant at the 10% level, whereas the corresponding estimates for the full and landless samples are statistically insignificant. The log-hours estimates are also imprecisely estimated throughout. More importantly, the first-stage F-statistics fall below the conventional threshold of 10 in all samples, indicating substantially weaker identification once these granular fixed effects are introduced. We therefore interpret this specification as an over-saturated stress test rather than as confirmatory evidence. Its main value is that the negative direction of the landholding estimates persists, while the weak first stage underscores the limits of the identifying variation under such demanding controls.

Next, Table 13 examines whether the preferred fertility estimates are sensitive to children's gender composition. Restricting the analysis to respondents with valid information on the numbers of sons and daughters leaves the main pattern of results unchanged. Holding this sample fixed, adding the share of sons produces virtually no change in the fertility estimates, their precision, or the first-stage statistics. The share-of-sons coefficient is statistically insignificant throughout, while the negative effects among landholding households remain stable. Overall, the results indicate that the main findings are not materially driven by observed differences in children's gender composition.

To further examine whether the estimates may reflect broader compositional differences rather than fertility-related mechanisms, Appendix Table A.3 reports placebo tests using adult height and retrospectively reported childhood health before age 15. Across both placebo outcomes, the IV estimates are statistically insignificant in the full, landholding, and landless samples, and the conclusions are unchanged when the analysis is restricted to one observation per respondent. The height coefficients vary in sign across landholding groups, while the childhood-health estimates are close to zero in the full and landholding samples and show no stable pattern across sample definitions. These results provide no systematic evidence that instrument-induced fertility is associated with the early-life health and physical-endowment proxies examined. The findings should nevertheless be interpreted cautiously because adult height is only an indirect proxy for early-life conditions, childhood health is retrospectively reported, and the respondent-level childhood-health specification for landholding households has a first-stage F-statistic slightly below 10.

## Conclusion

This paper examines whether and under what conditions fertility reduces later-life labor supply in Indonesia, where formal old-age protection remains limited and family support continues to play an important role. Using data from the Indonesian Family Life Survey and an instrumental-variable strategy based on differential reproductive-age exposure to the expansion of Village Contraceptive Distribution Centers, we estimate the relationship between completed fertility and working status and log weekly hours among older respondents.

The full-sample IV estimates are negative but statistically imprecise, providing limited evidence of a uniform fertility effect on later-life labor supply. The main pattern instead emerges across agricultural landholding status. Among landholding households, an additional child reduces both the probability of working and overall weekly labor supply, whereas the corresponding estimates for landless households are smaller and statistically insignificant. Additional heterogeneity by educational attainment suggests that the negative working-status effect may be more apparent among respondents with lower educational attainment, although the evidence is not sufficiently precise to establish a statistically significant difference across education groups.

The mechanism evidence is most closely aligned with family-labor substitution in agricultural production. Among landholding households, fertility increases the probability that at least one linked child works in agriculture, the same household context in which older parents reduce their labor supply. The data do not directly observe children taking over specific tasks, farm-management responsibilities, or working hours, so this evidence does not establish labor substitution conclusively. Net intergenerational support provides complementary rather than mediating evidence. Fertility increases positive net support and the amount of net transfers most clearly among landless households, yet later-life labor supply does not decline in that group. By contrast, net support does not increase significantly among landholding households,

where the labor-supply response is concentrated. This contrast indicates that financial support alone does not quantitatively account for the observed labor-supply pattern and that the productive context in which family support is provided may matter. The health and caregiving analyses likewise provide no affirmative evidence that declining work capacity or increased assistance to children explains the main findings, although these estimates remain imprecise.

These results should be interpreted within the limits of the research design. VCDC establishment was not unconditionally random, as historically more developed localities received services earlier. The preferred specification therefore conditions on observable historical fertility, labor-market, and development characteristics, and the estimates rely on conditional identification rather than fully random program placement. The KLS sensitivity analysis, placebo outcomes, and alternative specifications provide no systematic evidence that the findings are driven by the observable threats examined, but unobserved political, administrative, or development-related determinants of rollout cannot be ruled out. In addition, the demanding mother-cohort-by-province specification substantially weakens the first stage. Because the instrument is continuous, the IV estimates should be interpreted as local effects for fertility-responsive households rather than as population-average effects for all older Indonesians.

More broadly, the findings underscore that family-based old-age security provides uneven protection across household contexts. Declining family size may reduce the potential for adult children to assume productive responsibilities in landholding households, while landless households may remain vulnerable because greater intergenerational support does not necessarily reduce the need for continued work. Formal old-age income protection may therefore become increasingly important where family support does not translate into meaningful relief from work. The findings point to potentially different old-age security challenges across household contexts without

identifying or ranking specific policy instruments. These implications are necessarily local to the fertility-responsive households represented by the IV estimates and should not be read as forecasts of how all older Indonesians will respond to future fertility decline.

#### CRediT authorship contribution statement

**Muhammad Iqbal Nasution:** Writing – review & editing, Writing – original draft, Visualization, Validation, Software, Methodology, Investigation, Formal analysis, Data curation, Conceptualization. **Rus'an Nasrudin:** Writing – review & editing, Validation, Supervision, Methodology, Investigation, Formal analysis, Data curation, Conceptualization. **Mohamad Ikhsan:** Writing – review & editing, Validation, Supervision, Conceptualization. **Dwini Handayani:** Writing – review & editing, Validation, Supervision, Formal analysis, Conceptualization.

#### Declaration of generative AI and AI-assisted technologies in the writing process

During the preparation of this work, the authors used ChatGPT and Grammarly to assist with proofreading and grammar correction. After using this tool, the authors reviewed and edited the content as needed and take full responsibility for the content of the publication.

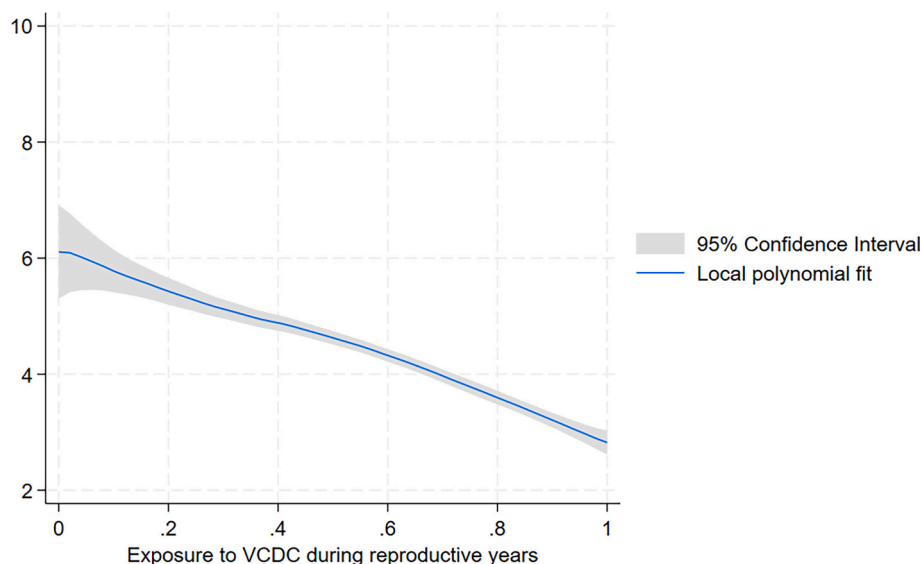
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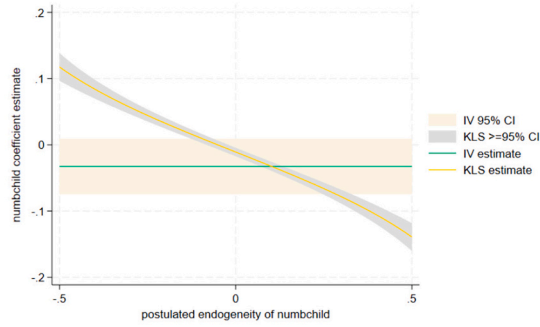
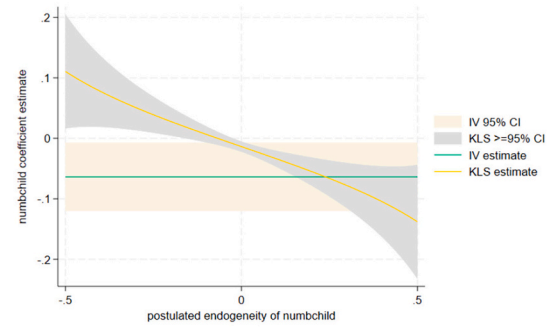
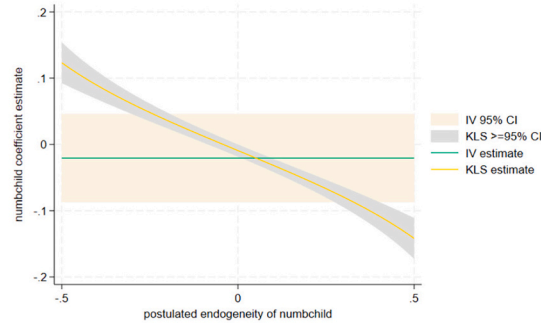
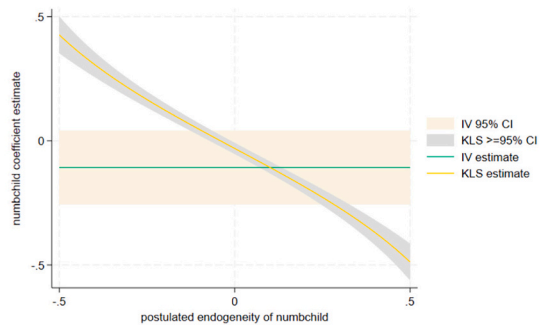
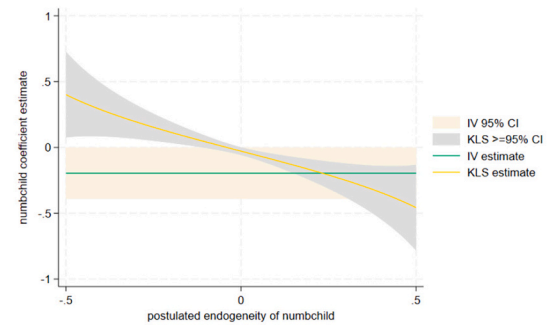
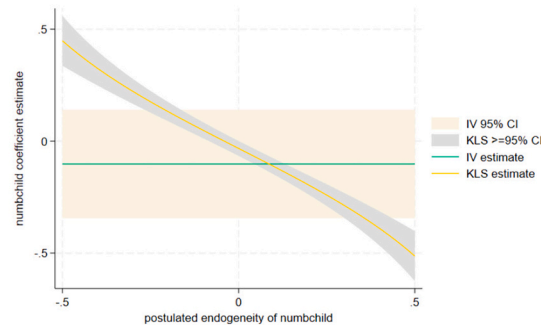
The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

## Appendix A. Additional Analyses and Supporting Information



**Fig. A.1.** Completed fertility across reproductive-age exposure to VCDC.

**Notes:** The horizontal axis shows the proportion of reproductive years exposed to VCDC availability, and the vertical axis shows completed fertility measured by the total number of children. The solid line presents a local polynomial fit using individual-level observations, with the shaded area indicating the 95% confidence interval. The fitted relationship declines across the observed exposure range, supporting the plausibility of a monotonic first-stage relationship.

**Outcome: Working Status***Panel A. Full sample**Panel B. Landholding households**Panel C. Landless households***Outcome: Log hours/week***Panel A. Full sample**Panel B. Landholding households**Panel C. Landless households***Fig. A.2.** Sensitivity of the preferred IV estimates to alternative endogeneity assumptions.

**Notes:** The figure reports KLS sensitivity analyses for log hours/week in the full sample, landholding households, and landless households. The horizontal axis represents alternative postulated endogeneity correlations for completed fertility. The orange line and gray shaded area show the KLS estimates and 95% confidence intervals, while the green line and light shaded area show the conventional IV estimate and its 95% confidence interval. All models use the preferred specification, including the historical placement controls. The KLS exclusion diagnostic includes zero in the 95% set of correlations compatible with valid exclusion in all three samples. The results indicate compatibility with valid exclusion but do not prove that the exclusion restriction holds.

**Table A.1**

Construction of the analytical sample.

| Sample-construction step                                              | Observations retained | Observations removed |
|-----------------------------------------------------------------------|-----------------------|----------------------|
| All person-wave records in IFLS4 and IFLS5                            | 108,870               | –                    |
| Respondents aged 55 or older                                          | 13,915                | 94,955               |
| Ever-married respondents                                              | 13,755                | 160                  |
| Valid fertility-history information                                   | 6763                  | 6992                 |
| Recorded VCDC establishment year and valid exposure information       | 5854                  | 909                  |
| Consistent residence and district information for exposure assignment | 4592                  | 1262                 |
| Complete community and historical controls                            | 4522                  | 70                   |
| Respondents with prior labor market attachment                        | 4279                  | 243                  |
| Complete parent-level covariates                                      | 3796                  | 483                  |
| Final complete-case analytical sample                                 | 3522                  | 274                  |

**Notes:** The table reports the sequential construction of the pooled person-wave analytical sample from IFLS4 and IFLS5. The number of observations removed at each stage is conditional on all preceding restrictions and therefore depends on the order in which the criteria are applied. The final analytical sample contains 3522 respondent-wave observations corresponding to 2644 unique respondents. Valid VCDC exposure requires information on the timing and locality of VCDC availability relevant to the respondent's reproductive years. The prior labor market attachment restriction excludes respondents who had never worked and focuses the preferred analysis on later-life work continuation and withdrawal.

**Table A.2**

Fertility and working status by respondent gender.

|                         | Full sample       | Men                 | Women            |
|-------------------------|-------------------|---------------------|------------------|
| Number of children      | –0.032<br>(0.020) | –0.034**<br>(0.016) | 0.039<br>(0.332) |
| Observations            | 3522              | 1951                | 1571             |
| First stage F-statistic | 30.13             | 58.38               | 0.34             |

**Notes:** The table reports IV estimates of the effect of the number of children on working status for the full analytical sample and separately for male and female respondents. The gender-specific models use the preferred specification, including individual and household controls, contemporaneous community infrastructure measures, historical placement controls, rural residence, province fixed effects, survey-wave fixed effects, and age-group fixed effects. Respondent gender is omitted from the gender-specific regressions. Fertility histories are reported by women; male respondents are therefore included only when they can be linked to a female partner with available fertility information, whereas female respondents are not subject to the same linkage requirement. The male and female subsamples are consequently not directly comparable. Standard errors clustered at the district level are reported in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$ .

**Table A.3**

Placebo tests using early-life health proxies.

|                                                                         | Pooled person-wave sample |              |                         | One observation per respondent |              |                         |
|-------------------------------------------------------------------------|---------------------------|--------------|-------------------------|--------------------------------|--------------|-------------------------|
|                                                                         | IV coefficient            | Observations | First-stage F-statistic | IV coefficient                 | Observations | First-stage F-statistic |
| <i>Panel A. Adult height measured at the earliest available wave</i>    |                           |              |                         |                                |              |                         |
| Full sample                                                             | –0.121<br>(0.655)         | 3521         | 30.13                   | –0.218<br>(0.761)              | 2643         | 28.08                   |
| Landholding households                                                  | –1.253<br>(1.086)         | 1596         | 14.20                   | –1.727<br>(1.399)              | 1156         | 10.23                   |
| Landless households                                                     | 0.869<br>(0.765)          | 1925         | 19.20                   | 0.727<br>(0.901)               | 1487         | 21.90                   |
| <i>Panel B. Retrospectively reported childhood health before age 15</i> |                           |              |                         |                                |              |                         |
| Full sample                                                             | 0.002<br>(0.062)          | 2930         | 26.62                   | 0.036<br>(0.073)               | 2145         | 23.95                   |
| Landholding households                                                  | –0.017<br>(0.090)         | 1366         | 13.04                   | 0.021<br>(0.103)               | 966          | 9.53                    |
| Landless households                                                     | 0.115<br>(0.098)          | 1564         | 17.86                   | 0.136<br>(0.108)               | 1179         | 18.53                   |

**Notes:** The table reports IV estimates of the effect of the number of children on two placebo outcomes capturing early-life health and physical endowments. Panel A uses adult height, measured in centimeters at the respondent's earliest available IFLS observation. For respondents observed in multiple waves, the height measure is taken from the wave in which they were first observed, thereby capturing attained adult height at the youngest available age and reducing potential measurement changes associated with height loss at older ages. The mean age at the earliest height observation is approximately 45 years. Panel B uses a retrospectively reported index of childhood health before age 15, with higher values indicating worse childhood health. The pooled specifications retain all eligible respondent-wave observations, whereas the respondent-level specifications retain one eligible observation per respondent. In the respondent-level specifications, the earliest available height record is retained for Panel A, while the latest eligible analytical record is retained for Panel B because childhood-health information is collected only in IFLS5. All specifications include the individual and household controls, contemporaneous community infrastructure measures, historical placement controls, rural residence, province fixed effects, survey-wave fixed effects, and age-group fixed effects used in the preferred specification. Standard errors clustered at the district level are reported in parentheses. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.10$ .

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