Local Human Capital Development and Firm Resilience: Evidence from the 1997 Indonesian Crisis

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Abstract

Do returns to human capital rise during crises? This paper examines whether human capital accumulation created by Indonesia's INPRES school construction program in the 1970s improved firm resilience in the 1997 Asian Financial Crisis. I find that each additional school per 1,000 children increased real labor productivity and output during the crisis period by 2.7 and 3.3 percent, respectively. INPRES induced changes in the local pre-crisis workforce composition that shifted workers toward more skill-intensive production work. I argue that these skills became particularly valuable during the crisis, when firms faced disruptions. Consistent with this mechanism, INPRES had larger effects in sectors with higher import intensity, where adjustment demands were greater. I show that the local abundance of skilled workers in high-INPRES districts kept wages relatively lower, enabling plants to retain more of them when they were most valuable.

JEL classification: O12, J24, D22, G01

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1 Introduction

> "[T]he ability to deal successfully with economic disequilibria is enhanced by education . . . this ability is one of the major benefits of education accruing to people privately in a modernizing economy." (Schultz, 1975)

There is a premium on human capital during economic crises. To deal with the disequilibrium of a crisis, firms must reallocate resources, adjust production decisions, and adapt to new economic conditions while minimizing losses. Their ability to navigate the crisis successfully depends on the speed and effectiveness of these decisions. Firms often respond by laying off workers to cut costs, securing additional financing to maintain cash flow, or implementing technological and organizational changes (Guimbert and Oostendorp, 2016; Kwapil, 2010; Westergård-Nielsen and Neamtu, 2012). Since workers are responsible for making and executing these decisions, the strength of a firm's crisis response is inevitably shaped by their skills.

The role of worker characteristics in firm performance during recessions has received little attention in the economics literature. The standard view, dating back to Schumpeter (1939), is that the strongest, most efficient firms survive. A natural extension of this idea is that highly skilled workers contribute to this survival by helping firms adapt. Indeed, following Schultz's (1975) argument, one could argue that skills matter the *most* during periods of crises, as that is when they are tested to their limits. The impact of a plant manager's decision to optimize production, the speed and precision with which a machine operator adapts to new technology, and the ability of a procurement specialist to secure critical raw materials amid supply chain disruptions all become even more important in a crisis. When firms are on the brink of shutdown, these factors can determine whether they adapt and survive or fail.

In this paper, I examine the impact of Indonesia's INPRES school construction program on human capital accumulation and its role in shaping plant performance during the 1997 Asian Financial Crisis. Between 1973 and 1979, the Indonesian government undertook an ambitious expansion of the education

system, doubling the stock of schools by constructing 61,800 new primary schools across the country. This large-scale intervention significantly improved educational attainment. Duflo (2001) found that the program increased schooling levels by 0.25 to 0.40 years, with each additional school per 1,000 children contributing 0.12 to 0.19 years of education.

In a later paper, Duflo (2004) examined the medium-run labor market effects of the INPRES program from 1986 to 1999. By 1986, thirteen years after the program began, the first cohort fully exposed to INPRES schools had turned 18 and begun entering the workforce, raising the overall share of workers with primary education. By the time the 1997 crisis struck, individuals from regencies (referred to as "districts" hereafter) with more INPRES schools had disproportionately benefited from the program and made up a growing share of the workforce, as older, less-educated workers exited. This provides a natural setting to assess whether improved human capital made firms more resilient to the crisis.

Despite exhibiting similar pre-crisis trends, plants in districts with higher INPRES intensity experienced smaller declines on average in output and labor productivity during the 1997 crisis than plants in lower-intensity districts. I interpret this pattern through what I call the human capital salience hypothesis, namely that the returns to human capital increase during periods of economic disruption. Under this view, the human capital accumulated through INPRES became especially valuable once plants were forced to reoptimize under stress, helping plants diagnose disruptions and implement adaptive measures during the crisis..

To test this hypothesis, I use data from the Statistik Industri (SI) census of manufacturing plants with 20 or more employees from 1987 to 1999, combined with data from Duflo (2001) on INPRES intensity at the district level. Using a difference-in-differences approach, I find that INPRES significantly improved plant performance during the crisis years (1997–1999). In my main within-plant specification, each additional INPRES school per 1,000 children in a plant's district increases labor productivity (real output per worker) by 2.7 percent and real output by 3.3 percent during the crisis period.

The estimated effects are larger in specifications with district fixed effects that capture the average effect

across all plants, including those that enter or exit the sample, than in within-plant specifications with plant fixed effects that identify effects among surviving plants. The empirical design isolates the INPRES impact through its interaction with the crisis, rather than through level differences in plant outcomes during normal times. The results are robust to specifications that include sector-by-year fixed effects and to restricting the sample to a balanced panel of 4,895 plants observed in every year from 1990 to 1999.

Event-study estimates show no evidence of differential pre-trends between plants in high and low-INPRES districts prior to 1997. The estimated effects emerge during the crisis period and are most pronounced in 1998. This timing aligns with the salience hypothesis, as skills generated by INPRES become most relevant once the crisis begins in the second half of 1997. The delayed timing likely also reflects lags in plants' diagnosis of disruptions and implementation of adaptive responses.

Plants in high-INPRES districts outperform particularly in sectors with high import intensity during the crisis, where production was more exposed to disruption and adjustment demands were greater. If the returns to human capital that INPRES helped produce are especially high when plants are dealing with a crisis, this differential effect across more disrupted sectors is exactly what one would expect. Interestingly, INPRES effects are weaker among exporters, plausibly because some of the effects of disruption were offset by the large rupiah devaluation for exporters during the crisis.

Given that compositional effects appear important for the size of the estimates, I also examine whether INPRES exposure affected plant exit during the crisis. Regressing an indicator for plant survival, defined as continued observation in the SI census, on INPRES exposure yields no statistically significant effect. A possible interpretation comes from Hallward-Driemeier and Rijkers (2013), who show, using the same Indonesian plant data, that the 1997 crisis substantially attenuated the relationship between productivity and survival.

I find the same pattern: lagged total factor productivity (TFP) is negatively associated with exit, but that this relationship weakens during the crisis period. Importantly, I find no evidence of adverse selection

into exit by INPRES exposure during the crisis. Plants in higher-INPRES districts are neither more nor less likely to exit, nor does INPRES exposure differentially affect the productivity-exit relationship. These results suggest that the estimated resilience effects are unlikely to be driven by differential selection into exit. Instead, they point to an intensive-margin channel in which plants in higher-INPRES districts adapt and reoptimize more effectively during the crisis, leading to higher output and output per worker.

I next assess the robustness of the estimated effects to a range of alternative explanations and sources of confounding. Because INPRES targeted areas with lower school enrollment in the 1970s, I examine whether the results are driven by pre-existing differences across plants or regions rather than by INPRES-induced human capital accumulation. I find that the estimated resilience effects are not driven by pre-crisis plant characteristics that might have given some plants greater scope to adjust during the crisis, including size, capital stock, or TFP (technical efficiency). Nor are the results driven by differences in pre-crisis ownership structure, including foreign or government ownership shares, or by business-group membership.

In addition, because the lower-enrollment districts targeted by INPRES were typically poorer and more rural, these areas may have differed in access to credit, infrastructure, or other determinants of firm performance even by the 1990s. Controlling for pre-crisis district-level averages of local capital intensity and financial dependence does not affect the results. The findings are also robust to accounting for sectoral, regional, and local price variation during the crisis by applying plant-level price deflators rather than a national GDP deflator.

Cross-district migration following the implementation of INPRES could bias the estimates if more educated or skilled individuals relocated in search of better opportunities. To address this concern, I control for district-level pre-crisis in-migration rates of INPRES-exposed cohorts using data from the 1990 population census. The results remain robust, suggesting that the findings are not driven by pre-crisis sorting of INPRES-educated workers across districts.

Finally, as an additional test of the relevance of INPRES-exposed cohorts, I combine district-level IN-

PRES school shares with the national share of exposed cohorts in the workforce to construct a Bartik (shift-share) exposure measure. I find that INPRES resilience effects during the crisis increase with the cohort share, consistent with the mechanism operating through plants' exposure to INPRES-exposed cohorts in the workforce.

Next, I examine what made INPRES-exposed cohorts especially valuable during the crisis. Their skills would have been particularly relevant if primary schooling facilitated subsequent skill acquisition, such as additional schooling, vocational training, or on-the-job learning. In other words, INPRES exposure may have been crisis-relevant precisely because it led to broader improvements in the productive capabilities of the local workforce prior to the crisis. Because the estimated INPRES effects cannot be explained by simple differences in basic educational attainment alone, I test whether they reflect broader human capital gains by examining pre-crisis occupational composition of the local workforce.

To do this, I use SAKERNAS labor force survey data from 1994–1996 and a cohort-by-program-intensity design similar to Duflo (2001). I find that INPRES exposure increased the likelihood that younger, exposed cohorts were employed in skilled production occupations. These occupations are defined primarily as ISCO-68 major groups 7 and 8 (craft and related trades; plant and machine operators and assemblers), along with a small number of closely related occupations. This pre-crisis occupational upgrading is consistent with INPRES increasing the local supply of skilled production workers, a margin that is likely important for plant-level adjustment during crisis-related disruptions.

In addition to pre-crisis occupational upgrading, I examine how plants in higher-INPRES districts adjusted their employment during the crisis. If INPRES-exposed cohorts were more valuable, plants would have had stronger incentives to hold on to them to help navigate the crisis, especially if the shock was perceived as temporary. I find that plants in higher-INPRES districts experienced higher employment growth, or contracted employment less, during the crisis. The workers they retained were also more likely to have completed primary education.

At the same time, production-worker wages fell more sharply during the crisis in higher-INPRES districts, widening the wage gap between nonproduction and production workers and making it easier for plants to retain skilled production workers. I outline a simple framework to interpret this pattern. The crisis reduces the marginal product of labor for both types of workers, but under the salience hypothesis, skilled workers are more useful for diagnosing disruptions and adapting production, so their marginal product falls by less. Because INPRES expanded the local supply of crisis-relevant production labor, plants were able to retain these workers while keeping wages low, contributing to stronger performance during the crisis.

This paper contributes to several strands of the economics literature. First, it contributes to the literature on the direct, indirect, and long-term impacts of the INPRES program, and large-scale investments in educational infrastructure more broadly. Following the seminal work by Duflo (2001, 2004), several papers have examined its effects, such as Martinez-Bravo (2017), who found that INPRES increased the education levels of local village heads, leading to greater provision of public goods. More recently, Akresh, Halim and Kleemans (2022) studied its long-term and intergenerational effects on outcomes such as employment sector, migration, marriage outcomes, and the education and living standards of women and their children. Hsiao (2024) also showed that labor mobility greatly amplified the wage gains from the program.

Second, it contributes to the literature on firm risk-coping strategies, resilience, and performance during economic crises, a topic that has received much attention in management research but remains understudied in development contexts. Most notably, Bartoloni, Arrighetti, and Landini (2021) examine firm survival probabilities conditional on firms adopting corporate strategies that emphasize skill accumulation. They find that Italian manufacturing firms that invested in human capital, innovation, or international expansion had higher survival rates following the 2008 Great Recession.

In the Indonesian context, Hallward-Driemeier and Rijkers (2013) use the same dataset as this paper to show that the 1997 crisis led to the exit of relatively productive firms but also led to the entry of more productive ones, partially offsetting the crisis's negative impact on aggregate productivity. Narjoko and Hill

(2007) also use the same dataset and find that foreign ownership, export orientation, and industry labor intensity were positively associated with firm survival after the crisis.

Finally, this paper closely relates to the literature on household resilience to shocks (Barrett et al., 2021). Recent studies, including Do, Nguyen and Grote (2025), D'Errico, Romano and Pietrelli (2018), and Smith and Frankenberger (2018), show that human capital and education play a significant role in enhancing households' adaptive capacity to negative shocks.

2 Context and Data

2.1 The 1997 Crisis in Indonesia

Indonesia experienced rapid industrial growth, averaging over 9 percent annually from the 1970s until the onset of the 1997 crisis (Narjoko and Hill, 2007), making it one of the fastest-growing economies in Southeast Asia. The country maintained low inflation, modest current account deficits, rapid export growth, and strong foreign exchange reserves. Given these conditions, many economists expected Indonesia to be relatively insulated from the crisis triggered by the devaluation of the Thai baht in July 1997 (Iriana and Sjöholm, 2002). However, the contagion spread quickly, and Indonesia became one of the hardest-hit countries, facing massive capital outflows, a sharp depreciation of the rupiah, surging inflation, and a severe economic contraction. GDP declined by 13 percent in 1998. Manufacturing was hit especially hard due to its reliance on imported inputs and exposure to exchange rate volatility (Hallward-Driemeier and Rijkers, 2013).

2.2 Plant Data

I use plant-level data from Indonesia's Annual Survey of Medium and Large Manufacturing, commonly referred to as Statistik Industri (SI), administered by Badan Pusat Statistik (BPS), covering the period 1987–1999. The survey is intended to cover the population of manufacturing establishments employing

at least 20 workers. The SI provides detailed annual information on output and inputs, employment and wages, ownership, energy use, inventories, and investment and fixed assets. Importantly for my analysis, the dataset also reports worker education levels from 1995 to 1997, which allows me to examine underlying channels and mechanisms.

The SI has broad coverage across regions and sectors, with plants classified using Klasifikasi Lapangan Usaha Indonesia (KLUI-1990) codes that are largely compatible with ISIC Revision 2. This structure allows me to control for 5-digit sector and district fixed effects and to estimate within-plant specifications with plant fixed effects, as plants are observed repeatedly over time. A limitation of the data is that it does not distinguish between firms and plants, so I cannot identify whether multiple plants are owned by the same entity.

Since district boundaries in Indonesia have changed frequently over this period, I harmonize them to their 1995 definitions to merge with the INPRES data. Additionally, the dataset includes product-level price and quantity information from 1994 onward, allowing me to test robustness to controlling for price changes by using plant-level deflators instead of a GDP deflator, following Cali et al. (2024, 2022).

2.3 INPRES and Labor Force Survey Data

To measure regional INPRES intensity, I use district-level data from Duflo (2001) on the rollout of the Sekolah Dasar INPRES primary school construction program. Launched in 1973, this large-scale initiative aimed to expand access to primary education by rapidly increasing the number of schools. Between 1973 and 1979, the government constructed 61,807 new primary schools, roughly doubling the stock of schools (and schools per capita). Program intensity varied substantially across districts (see Figure A.1 in Appendix A). The general allocation rule assigned more schools to districts with more primary-school-age children not enrolled in 1972, implying higher intensity in districts with lower baseline enrollment.

To examine pre-crisis occupational changes associated with INPRES, I use data from Indonesia's Na-

tional Labor Force Survey (SAKERNAS), also administered by BPS (Statistics Indonesia). SAKERNAS is a nationally representative household labor-force survey conducted annually in the 1990s. I pool the 1994–1996 waves and use individual-level demographic and occupational information, where occupations are coded using Indonesia's occupational classification (KBJI), which maps to ISCO-1968, to construct a measure of workers in skilled production occupations.

3 Descriptive Statistics

Table 1: Summary Statistics from INPRES and Plant Data

	Obs	Mean	Std. Dev.	Min	Max
District INPRES intensity (schools per 1000 kids)	290	2.33505	1.25235	0.59082	8.59827
District share of INPRES schools	290	0.00345	0.00185	0.00087	0.01270
	Obs	Mean	Std. Dev.	Min	Max
Total workers	161555	165.04	679.37	10.00	116052.00
Log total output	161555	13.17	2.04	3.40	23.14
Log output per worker	161555	9.07	1.33	-3.51	17.30
Log value added	161553	12.07	1.98	-80.78	22.44
Log total investment	80465	11.86	2.84	0.34	24.27
Log total machinery value	23735	11.12	2.53	0.69	21.19
Log total wages per worker	161414	7.01	0.88	-0.24	14.30
Share workers primary and above	51409	0.91	0.19	0.00	1.00
Share workers senior high and above	51409	0.28	0.29	0.00	1.00
Share workers bachelors and above	51409	0.01	0.03	0.00	0.59

Plant-level summary statistics are computed over plant-year observations (1987–1999).
Although SI targets establishments with $\geq$20 workers, some establishments report fewer workers in certain years.
Monetary variables are expressed in thousands of 1995 rupiah; logarithms are natural logs.
Worker education shares are the fraction of workers with at least primary, senior high, or tertiary education (1995–1997 only)

Table 1 shows summary statistics for key variables. There was substantial cross-district variation in the scale of the INPRES program. On average, district-level INPRES intensity is 2.34 schools per 1,000 children. When expressed as a share of total INPRES schools, each of the 290 districts received about 0.35 percent of schools on average.

The next part of the table summarizes the plant-level census data from 1987 to 1999. The average manufacturing plant employs 165 workers and produces output worth approx. 524 million rupiah (in 1995 prices), though there is wide variation in scale. One plant, likely a data error, reports 116,052 workers; the next largest has 42,649. Labor productivity, measured as the log of output per worker (in thousands), averages 9.07 with a standard deviation of 1.33, reflecting substantial productivity differences across plants. Worker education data, available for the 1995–1997 subsample, show that 91 percent completed at least primary school, 28 percent senior high, and only 1 percent attained tertiary education.

4 Empirical Strategy

To test whether INPRES increased plant resilience during the 1997 crisis, I employ a difference-in-differences framework that compares changes in plant outcomes before and after 1997 between districts with high and low INPRES intensity.

$$y_{isdt} = \alpha + \beta(\text{Post 1997} \times \text{INPRES})_{dt} + \nu_s + \lambda_t + \phi_g + \varepsilon_{isdt}. \tag{1}$$

Here, y_{isdt} denotes either labor productivity (real output per worker, in logs) or real output (in logs) of plant i in sector s, located in district d at time t. The variable INPRES captures the intensity of the INPRES school construction program, defined as the number of schools built per 1,000 children in district d. Post 1997 equals one in the crisis years (1997–1999) and zero in earlier years. ν_s, λ_t, and ϕ_g are sector, year, and unit fixed effects, respectively, where ϕ_g can be either district-specific or plant-specific fixed effects, depending on the specification. In an alternative specification, I replace ν_s and λ_t with sector-by-year fixed effects, $\nu_{s\times t}$, which flexibly absorb time-varying shocks common to all plants within a sector.

The coefficient β captures the differential effect of INPRES intensity on plant outcomes during the crisis years (1997–1999). In specifications with district fixed effects, β is identified from within-district

changes over time, but the sample composition may change due to plant entry and exit. In specifications with plant fixed effects, identification comes from within-plant changes over time, so β is identified using plants observed both before and during the crisis and can be interpreted as the effect for surviving plants. Since treatment intensity varies at the district level, standard errors are clustered at the district level.

Because the SI census is typically administered toward the end of each year or the beginning of the following year, I treat 1997 as part of the crisis period. Identification requires that, absent the crisis, outcomes in high and low-INPRES districts would have followed parallel trends. INPRES was not randomly assigned and was targeted toward areas with lower baseline enrollment. I discuss the resulting identification concerns and related robustness checks to address them in Section 5.3.

I treat the within-plant specification with plant fixed effects as my main specification, since it absorbs all time-invariant plant characteristics that could affect outcomes. A related implication is that, because INPRES varies at the district level, the level effect of INPRES_d is absorbed by district fixed effects (and by plant fixed effects in the within-plant specification). As a result, only the interaction between the crisis indicator and INPRES_d is identified in these specifications. This also implies that any association between INPRES intensity and plant outcomes in non-crisis years is not separately identified. In fact, consistent with INPRES being targeted to initially disadvantaged districts in the 1970s, the unconditional correlation between INPRES intensity and plant outcomes is negative in the 1990s. This pattern does not pose a threat to identification, as I show below that pre-crisis plant characteristics and trends do not differentially drive the estimated crisis effects.

Table 2: Impact of INPRES on Labor Productivity During the 1997 Crisis

	(1) District FE	(2) Plant FE	(3) Sector × Year FE	(4) Balanced panel
Post 1997 × INPRES	0.0482***	0.0270***	0.0195**	0.0387***
	(0.0149)	(0.00978)	(0.00962)	(0.0117)
Observations	161,547	153,559	153,337	48,944
District FE	Yes			
Year FE	Yes	Yes		Yes
Sector FE	Yes	Yes		Yes
Sector × Year FE	No	No	Yes	No
Plant FE	No	Yes	Yes	Yes
Balanced panel	No	No	No	Yes

Dependent variable is log real output per worker (labor productivity).
The treatment effect is identified from the interaction between INPRES intensity and an indicator for the crisis years (1997–1999).
Standard errors are clustered at the district level.
Column (4) restricts the sample to a balanced panel of 4,895 plants observed in every year from 1990 to 1999.
*** p<0.01; ** p<0.05; * p<0.10

Table 3: Impact of INPRES on Output During the 1997 Crisis

	(1) District FE	(2) Plant FE	(3) Sector × Year FE	(4) Balanced panel
Post 1997 × INPRES	0.0715***	0.0333**	0.0248*	0.0396***
	(0.0251)	(0.0136)	(0.0131)	(0.0134)
Observations	161,547	153,559	153,337	48,944
District FE	Yes			
Year FE	Yes	Yes		Yes
Sector FE	Yes	Yes		Yes
Sector × Year FE	No	No	Yes	No
Plant FE	No	Yes	Yes	Yes
Balanced panel	No	No	No	Yes

Dependent variable is log real output.
The treatment effect is identified from the interaction between INPRES intensity and an indicator for the crisis years (1997–1999).
Standard errors are clustered at the district level.
Column (4) restricts the sample to a balanced panel of 4,895 plants observed in every year from 1990 to 1999.
*** p<0.01; ** p<0.05; * p<0.10

5 Results

5.1 Labor Productivity and Output

Results from estimating the main specification in equation (1), which examines the impact of INPRES on plant outcomes during the 1997 crisis, are reported in Tables 2 and 3. The sample includes all plants in the SI census, excluding the small number of plants that appear to change districts at least once in the data. Column (1) shows estimates from the difference-in-differences specification with district fixed effects. Columns (2) through (4) include plant fixed effects and therefore identify within-plant impacts, which requires plants to be observed both before and during the crisis period (1997–1999). These specifications thus estimate the effect of INPRES exposure for surviving plants.

Column (3) replaces separate sector and year fixed effects with sector-by-year fixed effects to flexibly absorb time-varying shocks at the sector level, while column (4) further restricts the sample to a balanced panel of plants observed in every year from 1990 to 1999. By contrast, column (1), which includes district fixed effects but not plant fixed effects, does not condition on plant survival and instead captures the average effect across all plants, including those that enter or exit the sample.

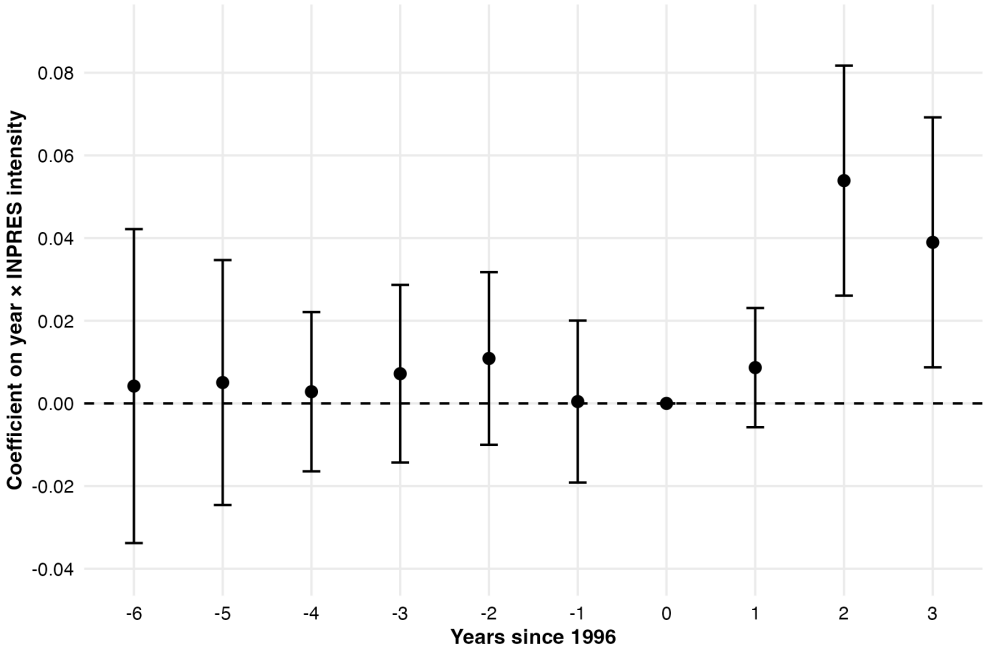
Across all specifications, the coefficient on the interaction between Post 1997 and INPRES intensity is positive and statistically significant, indicating that plants located in districts with greater INPRES exposure experienced higher labor productivity and output during the crisis years (1997–1999) relative to plants in districts with lower exposure. The results are robust to controlling for sector-by-year fixed effects and to restricting attention to a balanced panel, suggesting that they are not driven by differential sectoral trends or by changes in sample composition.

To ensure that these results are not driven by outliers, Table A.1 in the Appendix shows estimates from the full sample after excluding plants with labor productivity below the 5th or above the 95th percentile of the 1996 pre-crisis distribution. The estimated INPRES effects also remain robust to this restriction.

I treat the within-plant specification in column (2), which uses the largest sample of surviving plants, as my main specification. It implies that each additional INPRES school per 1,000 children in a plant's district is associated with a 2.7 percent increase in real output per worker and a 3.3 percent increase in total output during the crisis period. The fact that both labor productivity and total output rise suggests that these gains are not driven solely by employment reductions, but instead reflect relative plant-level outperformance during the crisis, consistent with human capital-driven resilience.

Figure 1 shows an event-study based on the within-plant specification in column (2), with labor productivity as the outcome. The coefficients on the pre-crisis leads are small and statistically insignificant, indicating no evidence of differential pre-trends prior to 1997. The estimated effects emerge during the crisis period and become more pronounced in 1998. This pattern is consistent with the salience hypothesis that INPRES-induced human capital gains became relevant once the crisis began in the second half of 1997, as well as with lags in plants' ability to implement protective or adaptive responses.

Figure 1: Event-Study of INPRES Effects on Labor Productivity, 1990–1999



Notes: Points are estimates of the coefficient on INPRES intensity interacted with each year dummy from the within-plant specification, with log real output per worker as the outcome; 1996 is the omitted year. Vertical bars show 95% confidence intervals with standard errors clustered at the district level. Regressions include plant, sector, and year fixed effects.

5.2 Sector Trade Exposure

A notable feature of the event study in Figure 1 is that the estimated INPRES effect on plant outcomes becomes most visible in 1998. This timing is consistent with the crisis unfolding in the second half of 1997 and with lags in firms' responses, as plants likely needed time to diagnose disruptions and implement operational adjustments.

This pattern is consistent with the human capital salience hypothesis. If the returns to worker skills rise primarily when plants face disruptions, then INPRES-driven differences in local human capital need not generate detectable performance gaps in normal times. Instead, these differences should become relevant once firms are forced to reoptimize under stress. A direct implication is that the effects of INPRES should vary systematically across sectors.

In particular, post-1997 resilience gains should be larger in sectors where disruption and adjustment demands were greatest, such as sectors with high import intensity that were more exposed to input shortages and exchange rate volatility. Effects may also vary by export orientation for similar reasons. By contrast, in sectors characterized by more routine production processes and limited reliance on imported inputs, crisis-period effects should be smaller.

This pattern is exactly what I observe in Table 4. Plants in high-INPRES districts outperform particularly in sectors with higher pre-crisis import intensity, where production was more exposed to disruption during the crisis and required more complex adjustment. By contrast, INPRES effects are weaker for plants that were exporters before the crisis. A plausible explanation is that exporters benefited directly from the large rupiah devaluation, which offset some of the adverse effects of disruption and reduced the relative importance of local human capital for performance.

Table 4: INPRES Effects and Trade Exposure During the 1997 Crisis

	(1) Import Intensity (Sector)	(2) Export Intensity (Sector)	(3) Exporter (Plant)
Post 1997 × INPRES × Trade exposure	0.279**	0.000967	-0.0320**
	(0.123)	(0.0408)	(0.0151)
Post 1997 × INPRES	0.0127	0.0244**	0.0291***
	(0.0106)	(0.0106)	(0.0105)
INPRES × Trade exposure	-0.0285	0.0268	
	(0.146)	(0.0633)	
Post 1997 × Trade exposure	-0.357**	0.204**	0.217***
	(0.177)	(0.101)	(0.0318)
Observations	149,808	149,844	125,871
Year FE	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes
Plant FE	Yes	Yes	Yes

Dependent variable is log real output per worker (labor productivity).
Standard errors are clustered at the district level.
The coefficient on Post 1997 × INPRES × Trade exposure captures how the Post 1997 × INPRES effect varies with the pre-crisis trade exposure measure shown in the column heading.
In columns (1) and (2), trade exposure is sector-level import intensity and export intensity, measured as the sector mean share of raw materials that are imported or exported, respectively, in 1996. In column (3), trade exposure is an indicator for whether the plant is an exporter in 1996.
*** p<0.01; ** p<0.05; * p<0.10

5.3 Plant Exit and Total Factor Productivity

The effects shown in Tables 2 and 3 operate primarily on the intensive margin and reflect relative within-plant performance, with plants located in districts with greater INPRES exposure performing better during the crisis than plants in lower-exposure districts, relative to their own pre-crisis levels. The larger coefficient in the specification with only district fixed effects, which allows for changes in sample composition due to entry and exit, suggests that compositional effects may also contribute, for example through the exit of lower-performing plants or the entry of relatively stronger plants in high-INPRES districts. On the extensive margin, it is also possible that plants more exposed to INPRES were less likely to exit during the crisis, consistent with human capital-driven resilience.

However, regressing an indicator for plant survival, defined as continued observation in the SI census, on INPRES exposure yields no statistically significant relationship. To interpret this null result, it is useful to consider the nature of plant exit during the crisis. Hallward-Driemeier and Rijkers (2013), using the same SI census data, showed that the crisis significantly attenuated the relationship between productivity and survival, making exit less discriminating by productivity.

Table 5: Plant Exit and TFP During the 1997 Crisis

	(1) Exit_t	(2) Exit_t	(3) Exit_t
Post 1997 $\times$ $\log(\text{TFP})_{t-1}$	0.000817**	0.000807**	0.000334
	(0.000362)	(0.000361)	(0.000599)
Post 1997 $\times$ INPRES $\times$ $\log(\text{TFP})_{t-1}$			0.000179
			(0.000444)
Post 1997 $\times$ INPRES		-0.00100	-0.00268
		(0.000905)	(0.00455)
INPRES $\times$ $\log(\text{TFP})_{t-1}$			-0.000693*
			(0.000388)
$\log(\text{TFP})_{t-1}$	-0.00110**	-0.00109**	7.50e-05
	(0.000514)	(0.000514)	(0.000386)
Observations	81,084	81,084	81,084
Year FE	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes
District FE	Yes	Yes	Yes

Dependent variable is an indicator for plant exit in year t, defined as the plant being observed in year $t-1$ but not observed in the SI census in year t.
TFP is estimated separately by industry using the Wooldridge (2009) proxy-variable approach.
Standard errors are clustered at the district level.
*** p<0.01; ** p<0.05; * p<0.10

I retest this result and find the same pattern. I estimate total factor productivity using the Wooldridge (2009) proxy-variable approach, estimating industry-specific production functions and using intermediate inputs to address simultaneity between input choices and unobserved productivity shocks. Column (1) of Table 5 shows that lagged total factor productivity (TFP) is negatively associated with exit, but that this relationship attenuates during the crisis. Importantly for my main result above, columns (2) and (3) show

no evidence of adverse selection into exit by INPRES exposure during the crisis. In particular, INPRES exposure does not significantly predict exit in the post-1997 period, and the triple interaction indicates that the productivity-exit relationship does not differentially change with INPRES exposure during the crisis years.

This implies that the estimated resilience effects observed above are unlikely to be driven by differential selection into exit by INPRES exposure during the crisis. Instead, the evidence points to an intensive-margin channel in which plants in higher-INPRES districts adapt and reoptimize more effectively in response to crisis-related disruptions. The resulting effects manifest in higher output and output per worker during 1997–1999, reflecting continued operation and reduced output losses during the crisis. By contrast, such adaptive responses need not translate into higher Hicks-neutral efficiency, since TFP is a residual intended to capture shifts in underlying production technology. Consistent with this interpretation, Table A.2 in the Appendix shows no differential increase in TFP by INPRES intensity during the crisis.

5.4 Robustness

The INPRES school construction program of the 1970s was targeted toward areas with lower baseline school enrollment, which were typically poorer and more rural (Duflo, 2001). Although the firm outcomes used to identify the effects above are observed roughly two decades later, this targeting could still generate concerns about persistent regional differences or correlated policies that affect later firm performance.

In particular, differential targeting could have influenced the types of plants that emerged across regions, ownership structures, access to local finance and capital accumulation, or the pre-crisis characteristics and evolution of the local workforce, including migration patterns. Some of these channels may operate as mechanisms of the human capital salience effect studied in this paper, especially those related to changes in local workforce composition induced by INPRES. Understanding these channels is therefore of independent interest. Other channels, however, could act as confounders and potentially challenge a human capital

interpretation of the estimated resilience effects.

Below, I address these concerns directly by examining whether the estimated resilience effects are driven by pre-existing differences across plants or regions, rather than by INPRES-induced human capital accumulation.

5.4.1 Plant Characteristics

A first concern is whether the estimated resilience effects simply reflect that plants in high-INPRES districts were larger, more capital-intensive, or more productive prior to the crisis, giving them greater scope to adjust when the crisis hit. To address this concern, I extend the baseline specification by allowing crisis-period outcomes to vary with pre-crisis plant characteristics measured before 1997.

Specifically, Table A.3 in the Appendix extends the main regression by interacting the Post 1997 indicator with pre-crisis measures of plant size, capital stock, and TFP. Employment is measured in 1996, while capital stock and TFP are measured in 1995 because 1996 values are unavailable. This specification controls for the possibility that larger, more capital-intensive, or more productive plants performed better during the crisis regardless of INPRES exposure. I use TFP as a measure of baseline technological efficiency, which is unlikely to be a margin through which the human capital salience mechanism operates, as discussed above. The coefficient on Post 1997 $\times$ INPRES remains positive and statistically significant.

Table A.4 in the Appendix then allows the Post 1997 $\times$ INPRES effect to vary with these pre-crisis characteristics by including triple interactions. This tests whether the estimated INPRES resilience effect is concentrated among plants with greater adjustment capacity. The results provide little evidence that the INPRES effect is concentrated among plants with larger size, higher capital intensity, or higher baseline TFP.

5.4.2 Ownership and Group Membership

Another concern is whether plants in high-INPRES districts differ systematically in ownership structure or group affiliation in ways that could drive differential crisis performance. To address this, Table A.5 in the Appendix interacts the Post 1997 indicator with pre-crisis ownership shares and group membership, measured in 1996 and held fixed. The coefficient on Post 1997 $\times$ INPRES remains positive and statistically significant. In addition, allowing the Post 1997 $\times$ INPRES effect to vary with ownership and group affiliation provides little evidence that the estimated resilience effect is driven by foreign ownership, government ownership, or business-group membership.

5.4.3 Local Capital Intensity and Financial Dependence

Because INPRES was targeted in districts with lower baseline enrollment that were typically poorer and more rural, these areas may also have differed in pre-existing access to credit, infrastructure, or other determinants of firm performance. It is therefore important to verify that the estimated resilience effects are not simply capturing persistent district-level differences that predate the program.

Table A.6 in the Appendix addresses this concern by controlling for district-level baseline characteristics interacted with the Post 1997 indicator. Local capital intensity is measured using the average capital-labor ratio in 1995, while local financial dependence measures are based on average debt-to-assets, debt-to-output, and interest-to-output ratios in 1996. The coefficient on Post 1997 $\times$ INPRES remains positive and statistically significant across specifications, indicating that the main results are robust to controlling for differential crisis exposure associated with local capital intensity and financial dependence.

5.4.4 Local and Sectoral Price Changes

To account for sectoral, regional, and local price variation during the crisis, I re-estimate the main specifications using plant-level deflators instead of a national GDP deflator, following Cali et al. (2024, 2022). These

deflators are constructed using product-level price and quantity information available from 1994 onward, allowing prices to vary flexibly across plants and over time.

The results, reported in Table A.7, remain robust, with only a modest reduction in magnitude relative to the main estimates. This suggests that the estimated INPRES effects reflect real differences in plant performance during the crisis rather than being driven by differential price movements. Additional details on the construction of the plant-level deflators are provided in Appendix B.

5.4.5 Migration

Another concern is that my identification strategy implicitly assumes that the district where a plant is located is also the district where its workers were educated. This assumption may be weakened by cross-district migration following the implementation of INPRES. Individuals may complete primary schooling in their district of childhood and later move, often to urban areas, in search of better job opportunities. Hsiao (2024) shows that labor mobility amplified wage gains associated with INPRES, and migration may be selective if more educated individuals are more likely to relocate to higher-return districts.

To address this concern, I re-estimate the main regressions while allowing crisis-period outcomes to vary with pre-crisis district-level mobility. Specifically, I add an interaction between the Post 1997 indicator and the district-level in-migration rate of individuals aged 18–22, measured in the 1990 population census. This age group is informative because, in 1990, individuals aged 18–22 corresponded to the first cohorts fully exposed to INPRES. The 1990 in-migration rate therefore captures mobility of INPRES-exposed cohorts prior to the 1997 crisis. Because migration may itself be influenced by INPRES, this control should be interpreted as isolating the component of the INPRES effect that operates independently of migration-related changes in local workforce composition.

Table A.8 in the Appendix shows that the results remain robust when controlling for these in-migration rates. The coefficient on Post 1997 $\times$ INPRES remains positive and statistically significant across speci-

fications, with magnitudes that are similar to the main estimates. Overall, these findings suggest that the main results are not driven by differential pre-crisis mobility or sorting of INPRES-exposed cohorts across districts.

5.4.6 Bartik Exposure of INPRES Cohorts

As an additional test of the relevance of INPRES-exposed cohorts during the crisis, I combine national demographic data from the UN World Population Prospects with district-level INPRES school shares to construct a Bartik (shift-share) exposure measure. The measure interacts (i) each district's fixed share of INPRES schools with (ii) the national share of fully treated cohorts (born in or after 1968) in the working-age population (ages 18–60), and is interacted with the post-1997 indicator.

The intuition is that district-level INPRES intensity is time-invariant, so the baseline difference-in-differences term delivers a constant treatment dose in all post-1997 years. The shift-share measure allows this dose to vary across post-1997 years as the national share of fully treated cohorts rises over time. If the main results operate through plant exposure to INPRES-exposed cohorts in the workforce, then the Post 1997 $\times$ INPRES effect should be stronger in years when the treated-cohort share is larger.

Table A.9 in the Appendix shows the results from this test. The coefficient on Post 1997 $\times$ INPRES $\times$ Shift is positive and statistically significant, indicating that the Post 1997 $\times$ INPRES effect increases with the cohort share. Because Shift is not centered, the coefficient on Post 1997 $\times$ INPRES reflects the effect evaluated at Shift $= 0$ rather than at observed cohort shares.

As a further robustness check, I also use the cohort-weighted exposure measure as an instrument for the baseline Post 1997 $\times$ INPRES term. Because the national cohort shares are common across districts and vary only modestly over 1997–1999, identification in this IV specification remains driven primarily by cross-district differences in INPRES shares. The IV estimates are similar in sign and magnitude to the main difference-in-differences estimates above.

5.5 From Schooling to Skills

5.5.1 Beyond Primary Completion

Beyond establishing that INPRES improved plant resilience during the crisis, it is important to understand what types of skills made INPRES-exposed cohorts particularly valuable. While primary schooling alone may enhance individuals' ability to process information, adapt to new tasks, and respond to changing economic conditions, this effect may be even stronger if primary education served as a foundation for subsequent skill acquisition, including further schooling, vocational training, or on-the-job learning. These cumulative human capital gains may have translated into broader improvements in productive capabilities prior to the crisis.

However, as shown in the descriptive statistics in Table 1, average levels of primary education among manufacturing workers were already high by the 1990s. Moreover, as emphasized by Duflo (2001), because INPRES targeted districts with low enrollment in the early 1970s, educational attainment in high-INPRES districts had not fully converged to that of low-INPRES districts by the mid-1990s. In the 1995 data, high-program regions still exhibit lower average schooling levels than low-program regions, even though exposed cohorts experienced larger schooling gains. Consistent with this pattern, Figure A.2 in the Appendix shows that in 1996, workers in above-median INPRES districts still slightly lagged behind those in below-median districts in the share completing primary education or higher, even though overall attainment was high in both groups.

At the same time, Duflo (2001) shows that INPRES increased not only primary completion but also, to a smaller extent, additional years of schooling beyond the primary level. As a result, average years of education and broader skill acquisition among exposed cohorts could rise more rapidly in high-INPRES districts even if extensive-margin differences in primary completion narrowed over time. This distinction suggests that the relevant human-capital channel may operate less through primary completion per se and

more through cumulative skill formation enabled by early schooling.

These patterns imply that differences in basic primary completion alone are unlikely to fully explain the estimated INPRES resilience effects. By the crisis years, primary attainment among manufacturing workers was already high in both high- and low-INPRES districts, leaving limited variation along this margin. To test this directly, Table A.10 in the Appendix re-estimates the main specifications while controlling for an interaction between the Post 1997 indicator and the plant's 1996 share of workers with at least primary education. The results remain similar to the baseline estimates reported in Table 2, indicating that the observed resilience effects are not driven solely by pre-crisis differences in primary education levels.

5.5.2 Pre-Crisis Occupational Upgrading

The evidence above suggests that the estimated resilience effects of INPRES are unlikely to operate solely through differences in basic primary education and instead point to broader forms of skill accumulation. One plausible channel is occupational upgrading. If INPRES-induced schooling facilitated further learning or skill development, exposed cohorts may have been more likely to sort into skilled production jobs that require greater technical competence and problem-solving ability.

To test this mechanism, I use data from the Indonesian Labor Force Survey (SAKERNAS) for 1994–1996 and focus on working-age cohorts that were differentially exposed to INPRES based on their age in 1974. Following Duflo (2001), I define the exposed cohort as individuals aged 2–6 in 1974, who were of primary-school age during the INPRES rollout, and the unexposed cohort as individuals aged 12–17 in 1974, who had largely completed primary schooling before the program began.

I construct an indicator for employment in skilled production occupations, defined primarily as ISCO-68 major groups 7 and 8 (craft and related trades; plant and machine operators and assemblers), along with a small set of closely related occupations from other groups. These occupations correspond closely to the types of production jobs observed in the SI manufacturing census. I then test whether INPRES exposure in-

creased the likelihood that younger cohorts worked in skilled production occupations by comparing exposed and unexposed cohorts within the same district-industry labor market.

The specification follows Duflo (2001)'s cohort-by-program-intensity design, adapted to occupational outcomes by conditioning on local industry labor markets. Specifically, I estimate:

$$\text{SkilledProd}_{idjt} = \alpha + \beta\,(\text{INPRES}_d \times \text{Young}_i) + X'_{idt}\Gamma + \alpha_{b(i)} + \alpha_{dj} + \alpha_{jt} + \varepsilon_{idjt}. \tag{2}$$

Here, $\text{SkilledProd}_{idjt}$ is an indicator equal to one if individual i in district d, industry j, and year t is employed in a skilled production occupation, and zero otherwise. Young_i is an indicator equal to one for the INPRES-exposed cohort (aged 2–6 in 1974) and zero for the older comparison cohort (aged 12–17 in 1974). The coefficient of interest, β, captures whether the difference in skilled production employment between younger and older cohorts is larger in districts with greater INPRES intensity. X_{idt} includes individual-level controls, including gender and an indicator for urban residence. $\alpha_{b(i)}$ denote birth-cohort fixed effects, α_{dj} are district-by-industry fixed effects that absorb time-invariant differences in local occupational structure, and α_{jt} are industry-by-year fixed effects that capture common industry-specific shocks over time. Standard errors are clustered at the district level.

This specification isolates whether occupational upgrading among INPRES-exposed cohorts occurs within relevant local industry labor markets. Table 6 shows the results. β is positive and statistically significant, indicating that, prior to the crisis, INPRES-exposed cohorts were more likely to work in skilled production occupations in districts with higher INPRES intensity. This pattern remains robust when additionally conditioning on primary school completion, suggesting that the shift toward skilled production is not explained solely by differences in basic schooling attainment. This pre-crisis occupational upgrading is consistent with INPRES increasing the local supply of skilled production workers, a margin that is likely important for plant-level adjustment during crisis disruptions.

Table 6: INPRES and Pre-Crisis Sorting into Skilled Production Occupations

	(1) Skilled production	(2) Skilled production
Young × INPRES	0.0137***	0.0142***
	(0.00461)	(0.00469)
Primary education completed		-0.0365***
		(0.00623)
Observations	71,726	71,726
Birth-year FE	Yes	Yes
District × Industry FE	Yes	Yes
Industry × Year FE	Yes	Yes

Dependent variable is an indicator equal to one if an individual is employed in a skilled production occupation, defined primarily by ISCO-68 major groups 7 (craft and related trades) and 8 (plant and machine operators and assemblers).
The main effect is identified from the interaction between INPRES intensity and an indicator for the exposed "young" cohort (aged 2–6 in 1974), with the "old" cohort (aged 12–17 in 1974) serving as the comparison group.
Sample includes working individuals observed in SAKERNAS, 1994–1996.
All specifications include individual controls for gender and urban residence.
Standard errors are clustered at the district level.
*** p<0.01; ** p<0.05; * p<0.10

5.6 Employment Adjustment During the Crisis

Given the evidence above that INPRES created a more skilled local workforce and increased plant resilience during the crisis, it is reasonable to expect that plants in higher-INPRES districts adjusted their employment differently when the shock hit. If cohorts exposed to INPRES were indeed more valuable during the crisis, plants may have had stronger incentives to retain them in order to better navigate crisis-related disruptions.

To explore this, I test whether plants in higher-INPRES districts expanded employment more, or contracted less, than plants in lower-INPRES districts during the crisis. Table 7 re-estimates the main specifications using the annual log change in total employment as the dependent variable. The coefficient on Post 1997 × INPRES intensity is positive and statistically significant across specifications, indicating that greater INPRES exposure is associated with higher employment growth during the crisis years. Quantitatively, the estimates imply that one additional INPRES school per 1,000 children increases annual employment growth

Table 7: Impact of INPRES on Change in Plant Workforce During the 1997 Crisis

	(1) District FE	(2) Plant FE	(3) Balanced panel
Post 1997 × INPRES	0.0109***	0.00705**	0.00984***
	(0.00285)	(0.00323)	(0.00352)
Observations	118,709	114,819	44,048
District FE	Yes		
Year FE	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes
Plant FE	No	Yes	Yes
Balanced panel	No	No	Yes

Dependent variable is the annual log change in total employment.
Standard errors are clustered at the district level.
Column (3) restricts the sample to a balanced panel of 4,895 plants observed in every year from 1990 to 1999.
*** p<0.01; ** p<0.05; * p<0.10

by roughly 1 percent in the post-1997 period.

One possible interpretation of this pattern is labor hoarding. If skilled workers are particularly valuable for coping with temporary disruptions, plants may have incentives to retain them during the crisis rather than shedding labor to cut costs. This interpretation is most plausible if the workers being retained are those with skills that are especially useful for adjustment. Measuring these skills directly is challenging, however, because, as discussed above, they extend beyond formal education alone.

As a partial proxy, I examine changes in the composition of the workforce by educational attainment. In particular, the SI census reports the number of workers with primary education or higher for the years 1995–1997, which allows me to assess whether plants in higher-INPRES districts retained relatively more educated workers during the onset of the crisis. While the main resilience effects emerge most strongly in 1998, this timing is still informative: employment adjustments can occur more quickly than other operational responses to crisis-related disruptions. Moreover, the crisis began in mid-1997, and the SI census is typically conducted toward the end of the year or early in the following year, making 1997 a reasonable year to capture

early workforce adjustments.

Table 8: Impact of INPRES on the Number of Primary-Educated Workers During the 1997 Crisis

	(1) District FE	(2) Plant FE	(3) Balanced panel
Post 1997 × INPRES	0.0291**	0.0193***	0.0200**
	(0.0115)	(0.00725)	(0.00782)
Observations	51,257	47,449	38,825
District FE	Yes		
Year FE	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes
Plant FE	No	Yes	Yes
Balanced panel	No	No	Yes

Dependent variable is the log number of workers with at least primary education.
Regressions are estimated using data from 1995–1997, the years in which worker education is observed in the SI census.
Standard errors are clustered at the district level.
Column (3) restricts the sample to a balanced panel of 13,008 plants observed in every year from 1995 to 1997.
*** p<0.01; ** p<0.05; * p<0.10

Table 8 re-estimates the above specifications using the log number of workers with at least primary education as the dependent variable. The coefficient on Post 1997 × INPRES intensity is again positive and statistically significant across specifications, indicating that plants in higher-INPRES districts retained a larger stock of primary-educated workers during the crisis period.

Taken together, these results suggest that plants adjusted not only the size but also the composition of their workforce in response to the crisis, placing greater weight on retaining workers with skills that were valuable for coping with disruptions. This behavior is consistent with evidence that firms are less likely to let go of higher-skill workers during downturns, particularly when adjustment and re-hiring costs are high (Guimbert and Oostendorp, 2016). Similar patterns of selective labor retention were documented among Southeast Asian firms during the 1997 Asian Financial Crisis (Hallward-Driemeier, Dwor-Frécaut, and Colaco, 2000).

5.7 Wage Adjustment

If high-INPRES districts had a larger local supply of production workers with crisis-relevant skills, selective retention during the downturn could have occurred alongside weaker wage growth for those workers, especially if labor demand collapsed and plants could retain workers without raising wages. I therefore examine whether the crisis altered relative wages across worker types, comparing nonproduction roles (managers, technicians, clerical staff) to production roles (machine operators, assembly line workers).

Table 9: Impact of INPRES on the Wage Gap Between Nonproduction and Production Workers During the 1997 Crisis

	(1) District FE	(2) Plant FE	(3) Balanced panel
Post 1997 × INPRES	0.0547***	0.0392**	0.0437*
	(0.0181)	(0.0173)	(0.0259)
Observations	115,509	107,944	37,325
District FE	Yes		
Year FE	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes
Plant FE	No	Yes	Yes
Balanced panel	No	No	Yes

Dependent variable is the difference between the log average wage of nonproduction workers and the log average wage of production workers.
Standard errors are clustered at the district level.
*** p<0.01; ** p<0.05; * p<0.10

I re-estimate the main specification using the difference between the log average wage of nonproduction workers and the log average wage of production workers as the dependent variable. Table 9 shows the results. The coefficient on Post 1997 × INPRES is positive and statistically significant, implying that higher INPRES exposure is associated with a larger nonproduction–production wage gap during the crisis. Each additional INPRES school per 1,000 children increases the wage gap by about 4 percent among surviving plants.

This widening gap is driven by production-worker wages falling more sharply than nonproduction wages

Table 10: Impact of INPRES on Wage Changes by Worker Type During the 1997 Crisis

	(1) All workers	(2) Production workers	(3) Nonproduction workers
Post 1997 × INPRES	-0.0190**	-0.0256***	-0.00154
	(0.00828)	(0.00897)	(0.0115)
Observations	64,004	64,004	64,004
Year FE	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes
Plant FE	Yes	Yes	Yes

Dependent variable is the annual log change in average wages, reported separately for all workers, production workers, and nonproduction workers.
Standard errors are clustered at the district level.
*** p<0.01; ** p<0.05; * p<0.10

in high-INPRES districts. Table 10 confirms this by estimating the within-plant specification separately for log wage changes of all workers, production workers, and nonproduction workers. The results show that plants in high-INPRES districts experienced steeper overall wage declines during the crisis, driven primarily by larger declines in production-worker wages. This pattern is consistent with high-INPRES districts having a relatively abundant supply of production workers, allowing plants to retain them while keeping wages low during the crisis.

5.8 Interpreting the Adjustment Patterns

All of the evidence above points to a coherent pattern of human capital-based adjustment. Prior to the crisis, INPRES exposure shifted the local workforce toward more skill-intensive production jobs, the types of workers employed by plants in the SI census. When the crisis hit, plants in higher-INPRES districts were more likely to retain their workforce, and the workers they retained were more likely to be educated. At the same time, production-worker wages fell more sharply in higher-INPRES districts, widening the nonproduction–production wage gap. Together, these patterns suggest that plants placed greater value on crisis-relevant human capital during the downturn, and that INPRES increased the local abundance of these

skills, shaping both employment and wage adjustment during the 1997 crisis.

To help interpret these findings, consider a simple framework in which the crisis combines a negative demand shock with an increase in the relative value of crisis-relevant skills. Plant output is a function of capital and production labor, where production labor consists of higher and lower-human capital workers, W_H and W_L, who are imperfect substitutes. Output is given by $Y = A_t F(\phi_t W_H, W_L, \bar{K})$, where A_t is a demand shifter and ϕ_t captures the salience of skills. Prior to the crisis, $\phi_t = 1$. When the crisis hits, A_t falls, reducing the marginal product of both worker types. Under the human capital salience hypothesis, however, ϕ_t rises during the crisis, reflecting that skilled workers are relatively more productive at diagnosing disruptions and adapting production under stress.

As ϕ_t rises, the relative marginal product of W_H increases compared to W_L, even as demand weakens. Plants therefore have stronger incentives to retain W_H rather than letting them go. To connect this to wages, if high-INPRES districts have a larger effective local supply of crisis-relevant production labor, plants can retain more production workers while keeping wages low. In sum, the resilience effects of INPRES during the 1997 crisis likely reflect a combination of plants being able to retain more skilled workers and those skills becoming especially valuable for navigating disruptions during the crisis.

6 Conclusion

In this paper, I examine whether human capital accumulation from Indonesia's INPRES primary school construction program improved firm resilience during the 1997 Asian Financial Crisis. The crisis struck roughly a decade after the first cohorts exposed to INPRES began entering the workforce and directly benefiting from the program. Using manufacturing plant data from the Statistik Industri census and district-level data on INPRES intensity from Duflo (2001), I find that INPRES significantly improved plant performance during the crisis years (1997–1999). In my main within-plant specification, each additional INPRES school per 1,000 children in a plant's district increases labor productivity (real output per worker) by 2.7 percent

and real output by 3.3 percent during the crisis period.

I show that INPRES induced changes in the local pre-crisis workforce composition, shifting workers toward more skill-intensive production work whose skills became particularly valuable during the crisis. The effects are strongest in sectors with higher import intensity, where adjustment demands were greater. I further show that plants in higher-INPRES districts retained more workers during the crisis, and that the workers they retained were more likely to be educated. By expanding the local supply of crisis-relevant production labor, INPRES enabled plants to retain skilled workers while keeping wages low. Taken together, these findings suggest that human capital plays a central role in firm performance and becomes especially important during periods of economic disruption.

In addition to the contributions outlined in the introduction, this paper highlights that large-scale investments in basic human capital infrastructure can strengthen firm resilience by fostering broad skill development across the economy. The results suggest that human capital plays an important role in shaping how firms perform in the face of large shocks, not only by sustaining output but also by supporting employment. Such investments may therefore generate long-term economic gains by supporting both households and firms during crises. Future research could further explore how different dimensions of worker skills contribute to firm adaptability and survival during crises.

A Appendix: Figures and Tables

Figure A.1: Regional Distribution of Manufacturing Plants and INPRES Intensity

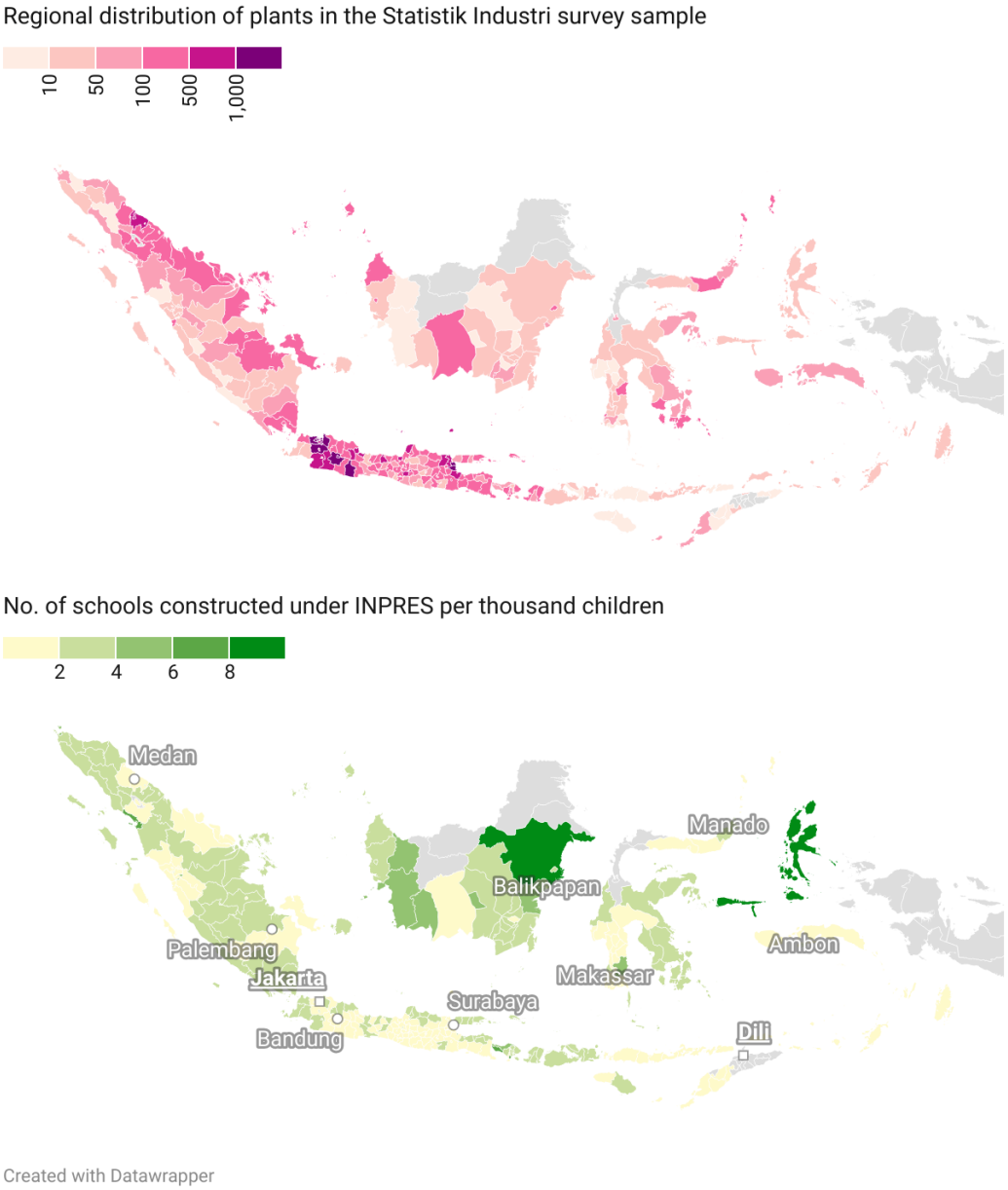


Table A.1: Impact of INPRES on Labor Productivity Using Trimmed Sample

	(1) District FE	(2) Plant FE	(3) Sector × Year FE	(4) Balanced panel
Post 1997 × INPRES	0.0470***	0.0265***	0.0200**	0.0311**
	(0.0131)	(0.00883)	(0.00779)	(0.0120)
Observations	143,692	135,754	135,472	35,893
District FE	Yes			
Year FE	Yes	Yes		Yes
Sector FE	Yes	Yes		Yes
Sector × Year FE	No	No	Yes	No
Plant FE	No	Yes	Yes	Yes
Balanced panel	No	No	No	Yes

Dependent variable is log real output per worker (labor productivity).
The treatment effect is identified from the interaction between INPRES intensity and an indicator for the crisis years (1997–1999).
Standard errors are clustered at the district level.
Sample excludes all plants with labor productivity below the 5th or above the 95th percentile of the 1996 pre-crisis distribution.
Column (4) restricts the sample to a balanced panel of 4,895 plants observed in every year from 1990 to 1999.
*** p<0.01; ** p<0.05; * p<0.10

Table A.2: Impact of INPRES on Total Factor Productivity During the 1997 Crisis

	(1) District FE	(2) Plant FE	(3) Sector × Year FE	(4) Balanced panel
Post 1997 × INPRES	0.0353*	0.0165	0.0156	0.00927
	(0.0180)	(0.0145)	(0.0134)	(0.0164)
Observations	132,790	125,917	125,775	44,030
District FE	Yes			
Year FE	Yes	Yes		Yes
Sector FE	Yes	Yes		Yes
Sector × Year FE	No	No	Yes	No
Plant FE	No	Yes	Yes	Yes
Balanced panel	No	No	No	Yes

Dependent variable is log(TFP).
Standard errors are clustered at the district level.
Column (4) restricts the sample to a balanced panel of 4,895 plants observed in every year from 1990 to 1999.
TFP is estimated separately by industry using the Wooldridge (2009) proxy-variable approach.
*** p<0.01; ** p<0.05; * p<0.10

Table A.3: INPRES Effects and Baseline Characteristics During the 1997 Crisis

	(1) No. of employees	(2) Capital stock	(3) TFP
Post 1997 × INPRES	0.0307***	0.0249**	0.0267***
	(0.0102)	(0.0100)	(0.0102)
Post 1997 × Baseline plant characteristic	0.0397***	0.00392**	0.00407
	(0.00717)	(0.00182)	(0.00271)
Observations	125,871	124,135	123,987
Year FE	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes
Plant FE	Yes	Yes	Yes

Dependent variable is log real output per worker (labor productivity).
Plant characteristics are measured prior to the crisis and held fixed. Employment is measured in 1996, while capital stock and TFP are measured in 1995 because 1996 values are unavailable.
Standard errors are clustered at the district level.
TFP is estimated separately by industry using the Wooldridge (2009) proxy-variable approach.
*** p<0.01; ** p<0.05; * p<0.10

Table A.4: Heterogeneity in INPRES Effects by Pre-crisis Plant Characteristics

	(1) No. of employees	(2) Capital stock	(3) TFP
Post 1997 × INPRES × Baseline plant characteristic	0.00112	-0.00377*	0.00501
	(0.00700)	(0.00211)	(0.00503)
Post 1997 × INPRES	0.0262	0.0673**	-0.0209
	(0.0291)	(0.0264)	(0.0464)
Post 1997 × Baseline plant characteristic	0.0378***	0.00958***	-0.00490
	(0.0129)	(0.00279)	(0.00934)
Observations	125,871	124,135	123,987
Year FE	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes
Plant FE	Yes	Yes	Yes

Dependent variable is log real output per worker (labor productivity).
Plant characteristics are measured prior to the crisis and held fixed. Employment is measured in 1996, while capital stock and TFP are measured in 1995 because 1996 values are unavailable.
Standard errors are clustered at the district level.
TFP is estimated separately by industry using the Wooldridge (2009) proxy-variable approach.
*** p<0.01; ** p<0.05; * p<0.10

Table A.5: INPRES Effects and Pre-crisis Ownership and Group Membership

	(1) Foreign ownership	(2) Government ownership	(3) Group membership
Post 1997 × INPRES	0.0274***	0.0307**	0.0283**
	(0.00970)	(0.0119)	(0.0119)
Post 1997 × Ownership or group indicator	0.300***	-0.0274	0.0855***
	(0.0499)	(0.0494)	(0.0317)
Observations	125,871	126,410	117,243
Year FE	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes
Plant FE	Yes	Yes	Yes

Dependent variable is log real output per worker (labor productivity).
Ownership and group membership measures are based on 1996 values and held fixed. Foreign and government ownership are measured as ownership shares, and group membership is an indicator for whether the plant belonged to a business group in 1996.
Standard errors are clustered at the district level.
*** p<0.01; ** p<0.05; * p<0.10

Table A.6: INPRES Effects and Pre-crisis Local Capital Intensity and Financial Conditions

	(1) log(K/L)	(2) Debt/assets	(3) Debt/output	(4) Interest/output	(5) All controls
Post 1997 × INPRES	0.0297***	0.0317***	0.0272***	0.0271***	0.0314***
	(0.0109)	(0.0112)	(0.00996)	(0.00981)	(0.0115)
Post 1997 × Control	0.0123	0.0884	2.22e-05	0.186	
	(0.0219)	(0.0956)	(0.000168)	(0.396)	
Observations	153,437	153,069	153,454	153,460	153,019
Year FE	Yes	Yes	Yes	Yes	Yes
Sector FE	Yes	Yes	Yes	Yes	Yes
Plant FE	Yes	Yes	Yes	Yes	Yes

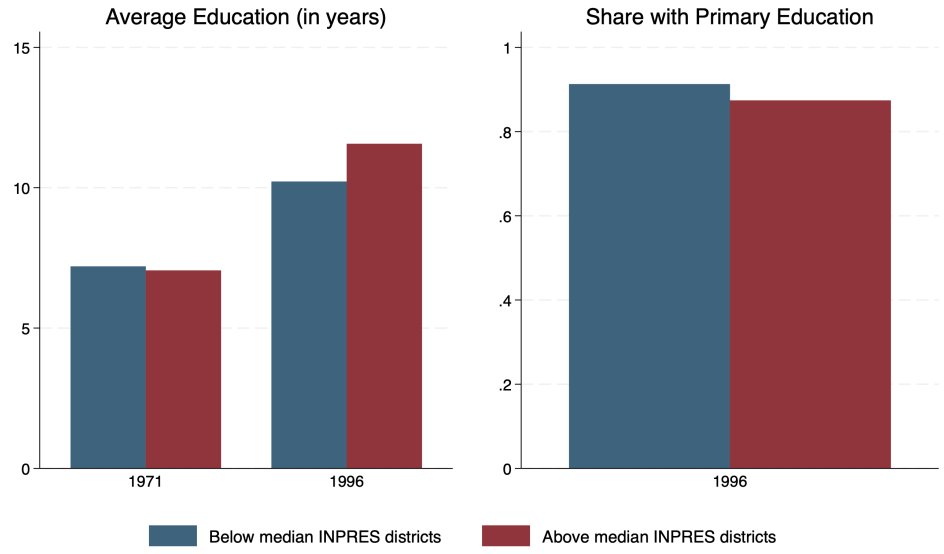
Dependent variable is log real output per worker (labor productivity).
Each column controls for a pre-crisis district-level characteristic interacted with the Post 1997 indicator, as indicated by the column title. Capital intensity is measured in 1995, while financial exposure measures are based on 1996 values.
Column (5) includes all pre-crisis controls jointly.
Standard errors are clustered at the district level.
*** p<0.01; ** p<0.05; * p<0.10

Table A.7: INPRES Effects on Labor Productivity Using Plant-level Price Deflators

	(1) District FE	(2) Plant FE	(3) Sector × Year FE	(4) Balanced panel
Post 1997 × INPRES	0.0366***	0.0217**	0.0162**	0.0223**
	(0.0107)	(0.00869)	(0.00802)	(0.00886)
Observations	90,691	86,967	86,811	42,460
District FE	Yes			
Year FE	Yes	Yes		Yes
Sector FE	Yes	Yes		Yes
Sector × Year FE	No	No	Yes	No
Plant FE	No	Yes	Yes	Yes
Balanced panel	No	No	No	Yes

Dependent variable is log real output per worker (labor productivity), deflated with plant-specific Tornqvist price indices.
The treatment effect is identified from the interaction between INPRES intensity and an indicator for the crisis years (1997–1999).
Standard errors are clustered at the district level.
Column (4) restricts the sample to a balanced panel of 7,077 plants observed in every year from 1994 to 1999.
*** p<0.01; ** p<0.05; * p<0.10

Figure A.2: Pre-INPRES and Pre-Crisis Education Levels



Notes: The 1971 data are from the Population Census (for working adults) and sourced from Duflo (2001). The 1996 values are from the SI census and are for plant workers.

Table A.8: INPRES Effects Controlling for Pre-crisis In-migration

	(1) District FE	(2) Plant FE	(3) Sector × Year FE	(4) Balanced panel
Post 1997 × INPRES	0.0445**	0.0291**	0.0206*	0.0255**
	(0.0172)	(0.0123)	(0.0120)	(0.0107)
Post 1997 × In-migration rate	-0.189	-0.0105	-0.0197	-0.318***
	(0.213)	(0.152)	(0.133)	(0.0978)
Observations	160,683	152,735	152,513	48,924
District FE	Yes			
Year FE	Yes	Yes		Yes
Sector FE	Yes	Yes		Yes
Sector × Year FE	No	No	Yes	No
Plant FE	No	Yes	Yes	Yes
Balanced panel	No	No	No	Yes

Dependent variable is log real output per worker (labor productivity).
Regressions control for pre-crisis district-level in-migration rates interacted with the Post 1997 indicator. In-migration is measured using the 1990 census as the five-year in-migration share of individuals aged 18–22.
The in-migration measure captures pre-crisis mobility of cohorts fully exposed to INPRES during schooling (born in or after 1968).
Standard errors are clustered at the district level.
Column (4) restricts the sample to a balanced panel of 4,895 plants observed in every year from 1990 to 1999.
*** p<0.01; ** p<0.05; * p<0.10

Table A.9: INPRES Effects and the National Share of Treated Cohorts During the 1997 Crisis

	(1)	(2)
	District FE	Plant FE
Post 1997 × INPRES × Shift	0.748**	0.664**
	(0.364)	(0.307)
Post 1997 × INPRES	-0.282*	-0.258**
	(0.153)	(0.129)
INPRES × Shift	-0.0142	-0.0691
	(0.0684)	(0.0647)
Observations	161,547	153,559
District FE	Yes	
Year FE	Yes	Yes
Sector FE	Yes	Yes
Plant FE	No	Yes

Dependent variable is log real output per worker (labor productivity).
The coefficient on Post 1997 × INPRES × Shift captures how the Post 1997 × INPRES effect varies with Shift, defined as the national share of fully treated cohorts (born ≥ 1968) in the working-age population (ages 18–60).
Standard errors are clustered at the district level.
*** p<0.01; ** p<0.05; * p<0.10

Table A.10: INPRES Effects Controlling for Baseline Primary Education During the 1997 Crisis

	(1) District FE	(2) Plant FE	(3) Sector × Year FE	(4) Balanced panel
Post 1997 × INPRES	0.0515***	0.0277**	0.0175*	0.0352***
	(0.0170)	(0.0111)	(0.0104)	(0.0133)
Post 1997 × Baseline primary education	0.102	0.0189	-0.0689	-0.0820
	(0.172)	(0.115)	(0.103)	(0.130)
Observations	161,399	153,460	153,236	48,944
District FE	Yes			
Year FE	Yes	Yes		Yes
Sector FE	Yes	Yes		Yes
Sector × Year FE	No	No	Yes	No
Plant FE	No	Yes	Yes	Yes
Balanced panel	No	No	No	Yes

Dependent variable is log real output per worker (labor productivity).
Baseline primary education is the plant's 1996 share of workers with at least primary education, interacted with Post 1997 and held fixed.
Standard errors are clustered at the district level.
*** p<0.01; ** p<0.05; * p<0.10

B Appendix: Construction of Plant-Level Price Deflators

To account for sectoral, regional, and local price variation during the crisis period, I construct plant-level price deflators using product-level price and quantity data from 1994 onward. The methodology follows Cali et al. (2024, 2022), which builds on Eslava et al. (2004) and Mertens (2019).

The raw dataset requires substantial preprocessing. Units of measurement vary across years for the same products, and in some cases, units are ambiguous or not easily convertible into consistent metrics such as volume, weight, count, area, or length. To ensure consistency, I first retain only products that appear in all years for each plant. I then keep only products with clear and convertible units that can be standardized into one of five categories: volume, weight, count, area, or length. Finally, following Cali et al. (2024), I drop products where the implied unit price changes by more than a factor of 10 between consecutive years.

Using the cleaned dataset, I construct a Tornqvist price index for each plant to capture variation in price changes across its 9-digit products. The plant-level index is defined as:

$$\pi_{it} = \prod_{p=1}^{n} \left(\frac{P_{pit}}{P_{pit-1}} \right)^{0.5(S_{pit}+S_{pit-1})} \pi_{it-1},$$

where P_{pit} is the unit value (total revenue divided by total quantity) of product p in plant i at time t, and S_{pit} is the share of product p in the plant's total product sales at time t. The price index π_{it} evolves as a weighted geometric mean of product-level price growth, with weights equal to the average sales share in periods t and $t-1$.

For missing values, I follow Cali et al. (2024) and impute price growth using the average change observed in the same sector and district. I use these plant-level deflators to adjust outcome variables and re-estimate the main regression specifications. Results using these adjusted outcomes are reported in Table A.7.

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