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Does Restricting Access to Credit Affect Learning Outcomes? Evidence from a Regulatory Shock to Microfinance in India

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Abstract

This study examines how restricted access to microfinance by households affects children's learning outcomes, utilizing a unique natural experiment that halted all microfinance operations in Andhra Pradesh (AP), India, in 2010. The analysis exploits quasi-random variation in district-level exposure to the shock in states other than AP, as the regulation affected lenders' liquidity nationwide. Using difference-in-differences and event study designs, we find a significant and persistent decline in children's learning outcomes. The restoration of credit access does not fully reverse these effects, highlighting the long-term consequences of short-term financial disruptions. As plausible mechanisms, we find a shift in enrollment from private to government schools, lower household spending on education, reduced food expenditure impacting nutrition, and a decline in mothers' employment potentially affecting intra-household resource allocation. Heterogeneity analysis reveals that the adverse effects are more prominent for girls and younger children. By focusing on the effects of regulatory restrictions rather than microfinance service provision, this study complements existing literature and provides a more comprehensive understanding of the socioeconomic impacts of microfinance.

Keywords: Microfinance regulation, Credit constraint, Learning outcomes, Schooling, Education, India.

JEL Codes: E51, G21, G28, I2, J16, R51.

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1 Introduction

The global microfinance market has expanded rapidly over the last few decades and has become an integral part of policy discussions due to its potential to alleviate poverty and promote economic development among low-income populations (Cull and Morduch, 2018).[1] This highlights the critical need to understand the effects of policy changes within the microfinance sector. Despite its growth and popularity, evidence regarding its impact remains mixed. While several studies highlight the positive general equilibrium effects of microfinance on economic well-being (Kaboski and Townsend, 2012; Burke et al., 2019; Fink et al., 2020), an increasing body of literature points to insignificant impacts on borrowers, suggesting that outcomes vary significantly depending on the context (Angelucci et al., 2015; Attanasio et al., 2015; Banerjee et al., 2019; Meager, 2019).[2] Nevertheless, microfinance continues to be valued for its role in providing liquidity and acting as a form of implicit insurance (Karlan and Zinman, 2011).

Beyond its direct economic impacts, microfinance has been studied for its broader socioeconomic effects, including health and education outcomes. However, there is limited evidence of its transformative effects on social indicators, particularly education (Banerjee et al., 2015; Tarozzi et al., 2015). Furthermore, the majority of existing studies have concentrated on the provision of microfinance, examining how access to financial services influences the economic well-being and social outcomes of low-income households. Research on the effects of microfinance regulation or the removal of access is less explored (Banerjee et al., 2018; Breza and Kinnan, 2021). Examining this aspect is crucial, given that the absolute impacts of providing versus removing access to microfinance may differ. Therefore, this paper aims to address this gap by analyzing the impact of the 2010 Andhra Pradesh (AP) microfinance regulation on children's learning outcomes, offering

[1] By the end of 2013, the sector had reached 211 million clients, including 114 million individuals living in extreme poverty (Reed et al., 2015).

[2] For instance, Banerjee et al. (2019) find that the positive effects of microfinance are observed only among entrepreneurs who already had a business before gaining access to microfinance. As a result, policymakers have begun advocating for a more targeted and selective approach to microlending.

insights into the wider socioeconomic implications of reduced access to microfinance.[3]

On October 15, 2010, the AP state government enacted an emergency ordinance to regulate the microfinance sector (Government of AP, 2010). This ordinance required microfinance institutions (MFIs) to immediately suspend their operations and obtain approval from local district authorities before they could restart disbursements or collections. These stringent regulatory measures significantly disrupted MFI operations, leading to widespread borrower defaults in the state. While the ordinance had a significant effect on lending within AP, it also triggered nearly immediate consequences on a national scale. In particular, Indian banks, which are key sources of funding for MFIs, largely stopped the issuance of new loans to these institutions across the country. During the period from 2010 to 2011, the Gross Loan Portfolio (GLP) of microlenders in India fell by around 20%, translating to a decline of more than USD 1 billion (Breza and Kinnan, 2021). Thus, MFIs that were heavily exposed to defaults in AP had to scale back their lending activities in other states that were not directly affected. This provides us with a unique natural experiment for examining the impact of microfinance regulation outside AP.

To examine the impact of microfinance restriction on children's learning outcomes, we utilize a district-level exposure measure from Breza and Kinnan (2021), which quantifies the extent to which each district in India was affected by the regulation. We merge this exposure measure with multiple rounds of nationally representative household surveys capturing various outcomes on children's education and household characteristics. Specifically, we utilize data from eleven rounds of the Annual Status of Education Report (ASER), spanning from 2006 to 2018, to form a district-level panel focusing on children's learning outcomes, particularly maths and reading test scores, which are the primary outcome variables in our study. Additionally, to explore the potential mechanisms underlying our main results, we combine the exposure measure with multiple rounds of the National Sample Survey (NSS), including the Employment and Unemployment Surveys,

[3]Andhra Pradesh is a state located in the southeastern part of India, with a population exceeding 50 million.

the Consumer Expenditure Surveys, and the Participation and Expenditure in Education Survey.

To effectively isolate the causal impact of AP regulation, our empirical strategy restricts the analysis to districts outside AP, thereby mitigating the influence of contextual confounding factors. Interestingly, the default issues observed in AP did not spread nationwide, and borrowers in other parts of the country continued to fulfill their loan obligations. We use a difference-in-differences specification as our primary empirical strategy to find the effects of microfinance regulation in AP on children's learning outcomes. The key identifying assumption is that after accounting for controls like the number of rural schools, rural population size, MFI lending levels in 2008 and 2010, distance from AP, and consumption and wage levels, households and individuals in these districts would have followed similar trends independent of their exposure to the AP microfinance regulation. To further complement our difference-in-differences specification, we implement an event study model to examine the evolution of relative effects.

Our analysis reveals a significant adverse impact of exposure to the AP regulation on children's learning outcomes. Specifically, a one standard deviation increase in exposure to the regulation results in a 0.081 standard deviation decline in maths test score and a 0.053 standard deviation decline in reading test score. Results from event study models further support these findings, showing that pre-regulation coefficients are near zero and jointly insignificant. However, from 2011 onwards, we observe a significant and persistent reduction in learning outcomes for children in districts with higher exposure to the shock. These findings hold up in a battery of robustness checks, such as removing districts bordering AP, limiting the sample to years around the regulation, excluding one state at a time from the sample, randomization inference, etc.

We also find evidence on four potential channels that may explain the decline in learning outcomes for children in more exposed districts: (1) a shift in enrollment from private to government schools, which are often perceived as lower in quality; (2) a reduction in household education expenditure, likely limiting access to educational resources; (3) a decrease in household food expenditure, potentially affecting children's nutrition;

and (4) a decline in mothers' employment participation, which may lead to underinvestment in children's education. Furthermore, the analysis demonstrates that the impact on children's learning outcomes varies by gender and age, with female children facing more severe consequences than their male counterparts and younger children experiencing more adverse effects compared to older children.

This paper contributes to several strands of the existing literature on microfinance, income shocks, and education. The majority of studies examining the socioeconomic impacts of microfinance consider the provision and expansion of microfinance services (Maldonado and González-Vega, 2008; DeLoach and Lamanna, 2011; Leatherman et al., 2012; You and Annim, 2014; Banerjee et al., 2015; Ghalib et al., 2015; Tarozzi et al., 2015; Baland et al., 2020), rather than exploring the implications of regulatory measures that limit access to such financial resources.[4] In contrast, our study specifically investigates how regulations restricting access to microfinance affect educational outcomes. This analysis is pertinent considering the possible asymmetry in the absolute impacts of expanding versus restricting access to microfinance.

By analyzing the effects of microfinance on children's learning outcomes, we also contribute to the literature on credit access and education in general. Existing research has largely concentrated on the impact of credit access on higher education, which usually involves larger financial commitments (Nielsen et al., 2010; Sun and Yannelis, 2016; Lucca et al., 2019). In contrast, our focus on school-age children offers insights into how credit access affects early childhood education, an area of critical policy relevance given the importance of early investment in long-term developmental outcomes (Cunha and Heckman, 2007). Further, our results showing that the shock had a larger negative impact on education outcomes of girls than boys add new evidence to the literature on how gender-neutral interventions can have a gendered effect (Evans and Yuan, 2022b).

[4] For instance, Leatherman et al. (2012), after reviewing various studies on microfinance, argues that the sector presents an underutilized opportunity for delivering health-related services to underserved populations. Similarly, Tarozzi et al. (2015) examines the effects of expanding access to microfinance on a range of socioeconomic outcomes, including educational attainment and indicators of women's empowerment.

By highlighting the potential role of reduced maternal employment in the decline of children's learning outcomes, we also contribute to the literature on the relationship between maternal employment and investment in children's human capital (Qian, 2008; Luke and Munshi, 2011; Afridi et al., 2016).

We also add to the growing body of literature on how different types of income shocks affect educational outcomes by examining a distinct shock induced by regulatory intervention in microfinance. While existing studies have largely focused on the effects of environmental and agricultural shocks (Jacoby and Skoufias, 1997; Jensen, 2000; Maccini and Yang, 2009; Björkman-Nyqvist, 2013; Shah and Steinberg, 2017; Garg et al., 2020), macroeconomic and financial shocks (Thomas et al., 2004; Duryea et al., 2007), natural disasters (Sacerdote, 2012; Andrabi et al., 2023), and COVID-19 pandemic-related disruptions (Engzell et al., 2021; Maldonado and De Witte, 2022; Moscoviz and Evans, 2022), our study highlights the impact of a policy-driven financial shock – the AP microfinance regulation – on children's learning outcomes. Unlike weather-driven or disaster-related shocks that are externally imposed, the microfinance regulation represents a policy-induced shock, which restricts households' access to credit and disrupts income flows, leading to reductions in educational investments and learning outcomes. More broadly, our findings contribute to the expanding body of knowledge on the impacts of microfinance and financial inclusion (Khandker, 2005; Angelucci et al., 2015; Attanasio et al., 2015; Banerjee et al., 2015; Tarozzi et al., 2015; Banerjee et al., 2018, 2019; Meager, 2019; Buera et al., 2021).

The remainder of this paper is organized as follows. Section 2 outlines the specific context and background of the study. Section 3 details the various datasets utilized in our analysis, while Section 4 explains our empirical strategy. In Section 5, we present the findings of the study, which include the main results, potential mechanisms, an analysis of heterogeneity, and robustness checks. Finally, Section 6 provides the concluding remarks.

2 Context

On October 15, 2010, the state government of Andhra Pradesh introduced an ordinance titled 'The Andhra Pradesh Micro Finance Institutions (Regulation of Money Lending) Act, 2010' to regulate the microfinance operations within the state (Government of AP, 2010). This ordinance effectively halted all microfinance activities, resulting in widespread defaults and a subsequent liquidity crisis affecting lenders nationwide. The ordinance mandated that microfinance institutions (MFIs) immediately cease operations, requiring them to secure authorization from district-level authorities before proceeding with any disbursements or collections. It also imposed several stringent restrictions: interest rates were capped at 100%, collections were mandated to occur in public spaces, and MFIs were banned from extending loans to self-help group (SHG) members.[5] These regulatory constraints severely curtailed MFI operations, resulting in near-total borrower defaults in AP. However, in August 2012, the Reserve Bank of India (RBI) relaxed the previously stringent provisioning norms, allowing AP-based MFIs to resume operations and stabilize their activities (Mint, 2013).

This ordinance was controversial for multiple reasons. As stated in the introduction of the Act, it aims to protect women SHG members from exploitation by MFIs, which were lending at exorbitant interest rates and using harsh recovery methods, leading to financial hardship and, in some cases, borrower suicides. In the months before the ordinance was enacted, there were extensive media reports highlighting multiple farmer suicides in AP, which were reportedly connected to over-indebtedness from MFIs (The Economic Times, 2010; The Hindu, 2010). However, critics argue that the government targeted MFIs as an easy solution to divert attention from more fundamental problems within the economy and farming sector. They claim that the legislation was driven more by political motives than by genuine regulatory concerns. Specifically, there was speculation that the ordinance was

[5] Self Help Groups (SHGs) in India are community-based groups, primarily of women, that pool savings to provide microloans for economic activities. Linked with formal banks, they play a crucial role in rural financial inclusion and women's empowerment.

intended to promote state-sponsored SHGs and fulfill clientelistic goals aimed at indebted rural voters rather than addressing real problems within the microfinance sector (Sriram, 2012; Rai, 2010; Roodman, 2010).

Although the ordinance had a clear impact on lending operations within AP, it also triggered almost immediate repercussions nationwide. Particularly, Indian banks, which are the key funding source for MFIs, significantly ceased issuing new loans to these institutions across India (Breza and Kinnan, 2021).[6] As a result, MFIs with large exposure to AP defaults had to cut back their lending activities in states that were not directly impacted. Interestingly, the default issues in AP did not extend throughout the country; borrowers elsewhere continued to meet their loan obligations. Consequently, our empirical strategy focuses exclusively on districts outside AP, as they were not directly impacted by the ordinance and where borrowers maintained their loan repayments.

3 Data

We primarily utilize three datasets: district-level microfinance exposure data sourced from the Microfinance Institutions Network (MFIN), the Annual Status of Education Report (ASER), and various rounds of the National Sample Survey (NSS). A detailed description of each dataset and the main variables used is provided in the subsequent subsections. The key outcome variables of our study, mathematics and reading test scores, are taken from the ASER, while the NSS is used to explore potential mechanisms underlying our primary findings.

3.1 Microfinance Exposure Data

This study investigates the impact of exposure to the microfinance regulation in AP on children's learning outcomes in districts outside AP. To do this, we require a district-level measure that quantifies the extent to which each district was exposed to the microfinance

[6] Between fiscal years 2010 and 2011, the Gross Loan Portfolio (GLP) of microlenders in India decreased by about 20%, amounting to more than USD 1 billion.

regulation implemented in AP. This measure is calculated based on the number and size of MFIs operating in both the district of interest and AP. First, for each lender, l, the fraction of its total Gross Loan Portfolio (GLP) that had been invested in AP at the beginning of October 2010, just before the regulation was enacted, was calculated.

$$fracAP_l = \frac{GLP_{l,AP,Oct_{2010}}}{GLP_{l,Total,Oct_{2010}}}$$

Then, for each district, d, an aggregate exposure measure was constructed by taking the weighted average of $fracAP$ across all lenders operating in that district. The weight used corresponds to each lender's relative share of the total GLP within the district.[7]

$$ExpAP_d = \sum_l fracAP_l * \frac{GLP_{l,d,Oct_{2010}}}{\sum_l GLP_{l,d,Oct_{2010}}} \tag{1}$$

This measure helps us to quantify how much each district was affected by the AP microfinance regulation, with the exposure being in the range of $[0, 1)$. For this data, we rely on Breza and Kinnan (2021), who obtained this proprietary information through the Microfinance Institutions Network (MFIN), the main trade association for for-profit MFIs in India. It is important to note that this measure is not based on the universal set of MFIs, as some institutions opted not to report their data. This introduces the potential for measurement error in the exposure variable. However, it is unlikely to create any concerns in our empirical strategy for multiple reasons. First, as highlighted by Breza and Kinnan (2021), key characteristics such as loan size and default rates do not vary significantly between MFIs that reported and those that did not. Moreover, within the reporting firms, exposed and unexposed MFIs do not vary significantly with respect to various characteristics of the MFI, supporting the internal validity of the estimates.[8] Second, while measurement error may exist, it is unlikely to be systematically correlated with educational outcomes; thus, in the presence of measurement error in our treatment

[7]For example, if a district is served by two MFIs, both providing 50% of the loans, and one lender has 0% of its portfolio in AP while the other has 50%, then $ExpAP_d = 0 * \frac{1}{2} + 0.5 * \frac{1}{2} = 0.25$.

[8]For more details, please refer to Table 1 in section III.A of (Breza and Kinnan, 2021, p. 1462).

variable, we may be identifying a lower bound of the true effect. Third, our event study results offer supporting evidence that trends in pre-regulation outcomes do not systematically vary with the exposure variable, further alleviating concerns about bias in our estimates.

Figure A.1 shows a map of the exposure levels across all districts in our sample, and Panel A in Table 1 provides the corresponding summary statistics. The table shows that 35% of districts had at least one exposed lender. In the main analysis, to simplify interpretation, we normalize this exposure variable to have a unit standard deviation. This allows the coefficient to represent the effect associated with a one standard deviation change in exposure to the AP microfinance regulation.

3.2 ASER Data

The Annual Status of Education Report (ASER) is one of the largest education-related household surveys conducted in India, initiated and led by the non-governmental organization *Pratham*.[9] ASER collects comprehensive data on children's schooling and foundational learning outcomes in rural India. Launched in 2005, ASER was conducted annually until 2014, covering nearly all rural districts across India. Since 2016, the survey has shifted to a biennial schedule, with interim years dedicated to specialized studies that explore particular aspects of children's learning. The survey is carried out from September to November and employs a two-stage sampling design. In the first stage, ASER selects 30 villages per district from the Census village directory[10] using the Probability Proportional to Size (PPS) method. In the second stage, 20 households are randomly selected from each chosen village, totaling 600 households per rural district, or approximately 300,000 households nationwide. This approach surveys around 600,000 children aged 3-16 across over 16,000 villages annually. Therefore, this sampling strategy provides representative data for the rural areas of every district in India; we also apply sampling

[9] *Pratham* is a leading non-governmental organization focusing on the education sector.

[10] The ASER surveys from 2005 to 2014 were based on the 2001 Census, whereas from 2016 onward, the 2011 Census has been used. We harmonize the district codes over the years.

weights in our regression analyses for accurate estimates.[11]

The survey specifically focuses on the enrollment of children aged 3-16 years and assesses basic arithmetic and reading competencies among children aged 5-16 years. ASER's household-based approach to data collection ensures the inclusion of all children within surveyed households, regardless of their enrollment status. Consequently, the survey captures data from children who have never attended school, those who have dropped out, and those enrolled in government, private, religious, or alternative educational institutions. Learning assessments are consistent across all children, regardless of age, enrollment status, or class level. The reading assessment is divided into four levels: (1) letter recognition, (2) common word recognition, (3) reading a paragraph with four simple sentences (Grade 1 level), and (4) reading a short story of 10 to 12 sentences (Grade 2 level).[12] This assessment is recorded on a scale of 1-5, with 1 indicating the inability to read even letters, and 5 representing the highest level of reading proficiency. Similarly, the arithmetic assessment includes four levels: (1) single-digit number recognition, (2) double-digit number recognition, (3) two-digit subtraction with borrowing, and (4) three-digit by one-digit division. The highest level tested aligns with the arithmetic competency expected in Grades 3 or 4, depending on the state. Additionally, basic household information, such as household size, parental education, and household assets, is also collected.

For our primary analysis, we utilize data from eleven rounds of ASER: 2006, 2007, 2008, 2009, 2010, 2011, 2012, 2013, 2014, 2016, and 2018. We include only children aged 6-16 from all rounds, as this corresponds to the formal schooling age. Maths and reading test scores are the two major outcome variables considered for this study. To account for age and round-specific variations, we standardize these test scores by age and round, ensuring consistency across different ages and ASER rounds. This standardization also aids in interpreting regression results in terms of standard deviations. Overall, our

[11] We also conduct the analysis without sampling weights as a robustness check, yielding similar results.

[12] ASER maths and reading tools are available at https://asercentre.org/process-documents/.

sample includes more than 3.05 million children, and Panel B of Table 1 presents summary statistics for our estimation sample. On a scale of 1 to 5, the average maths score is 3.37, while the average reading score is 3.71.

3.3 NSS Data

We use multiple rounds of nationally representative surveys conducted by the National Sample Survey Organization (NSSO) to investigate potential mechanisms underlying our primary findings. The NSSO collects various socioeconomic data from households using rigorous sampling methods.[13] Our analysis uses three rounds of both the Employment and Unemployment Surveys and the Consumer Expenditure Surveys: the 64th (2007-2008), 66th (2009-2010), and 68th (2011-2012).[14] Additionally, we incorporate data from the 64th round of the Participation and Expenditure in Education Survey, as the 64th round of the Employment and Unemployment Survey lacks information on school enrollment. The timing of these surveys, with two rounds pre-regulation and one post-regulation, allows us to apply a difference-in-differences method to examine the impact of exposure to the AP microfinance regulation on education outcomes.

We limit our analysis to rural households,[15] considering that microfinance is mainly intended for the rural population.[16] The main outcome variables from the NSS include school enrollment for children of school-going age, monthly household spending on education and food, and the detailed breakdown of these expenditures.[17] Summary Statistics

[13]We use sampling weights for the main analysis and show robustness without the weights.

[14]The NSSO employs three data collection methods: Uniform Reference Period (URP), Mixed Reference Period (MRP), and Modified Mixed Reference Period (MMRP). URP captures household expenditure over the past 30 days. MRP records expenditure on less frequently used items (e.g., clothing, education) for the past 365 days, while other items are recorded for the past 30 days. MMRP records certain food items for the past 7 days and uses MRP periods for other items. The MMRP method was introduced in the 66th round of the Consumer Expenditure Survey (2009-10) alongside MRP. Our study uses the MRP data from the 66th and 68th rounds to ensure comparability with the 64th round, which solely used MRP.

[15]Recall that ASER, which provides our primary outcome variables, is also based on rural households.

[16]As a robustness check, we also perform the same analysis for the urban sample, and generally, we don't find any significant effects.

[17]The household-level analysis is limited to households with at least one child aged 6 to 16, aligning with the school-age group considered in our individual-level analysis.

of key variables are provided in Panel C of Table 1. Approximately 81% of children aged 6-16 are enrolled in school, with 85% attending government schools and the remaining 15% attending private schools. On average, households spend INR 227 per month on education and INR 2,424 on food. In addition, we also use many variables from the NSS as controls in our analysis, such as age, gender, number of children in the household, household head's education level, social group, religion, and household type.[18]

4 Empirical Strategy

To estimate the impact of microfinance regulation on learning outcomes, we primarily employ the following difference-in-differences specification.

$$Y_{idt} = \alpha + \gamma_d + \delta_t + \beta \times ExpAP_d \times Post_t + X'_{idt}\zeta + \epsilon_{idt} \tag{2}$$

where Y_{idt} represents the outcome variable for individual i in district d at time t; γ_d and δ_t capture the fixed effects for district and survey round, respectively; $ExpAP_d$ is the measure of district d's exposure to the AP microfinance regulation as discussed in subsection 3.1, and β is the primary coefficient of interest.[19] To effectively isolate the effect of the microfinance regulation on the outcomes of our interest, the vector X controls for various individual, household, and pre-regulation district level characteristics. The pre-regulation district level characteristics are interacted with round dummies. These variables include the linear distance from the district centroid to AP, the rural population of the district in 2010 and its square, dummy variables representing quintiles of the Gross Loan Portfolio (GLP) for the years 2008 and 2010, the district's average rural per capita consumption in 2010, the district's average rural casual daily wage in 2010, and the count

[18] Household type refers to the primary source of livelihood and is categorized as self-employed, casual labour, and others. Social groups are classified into Scheduled Tribes (ST), Scheduled Castes (SC), Other Backward Classes (OBC), and others.

[19] We consider maths and reading test scores as continuous outcome variables for our main analysis. Later, we also consider binary indicators for different learning levels and estimate linear probability models with the same specification. This approach has been followed by other studies using ASER data (Chakraborty and Jayaraman, 2019; Lahoti and Sahoo, 2020).

of rural schools in the district in 2010. Our analysis uses repeated cross-sectional data instead of a household panel, forming a district-level panel with standard errors clustered at the district level.

To effectively capture the causal impact of exposure to the AP regulation, our empirical strategy limits the sample to districts outside of AP, thereby reducing the influence of contextual confounders. Given that the microfinance regulation was implemented in AP, one might consider a difference-in differences approach, taking AP as the treatment state and the remaining states as the control group. However, this design has two key limitations. First, the 100% default crisis reported in AP might have led to a short-term increase in income, raising concerns about the interpretation of the impact and the appropriateness of AP as the treatment group. Second, as observed by Breza and Kinnan (2021), the regulation also had indirect, general equilibrium effects on other states, which could challenge their suitability as a control group. On the other hand, our identification strategy relies on the differential changes in outcomes among individual or household cohorts in similar districts with varying exposure to the microfinance regulation. The key identifying assumption is that conditional on the controls included – such as the number of rural schools, rural population size, total MFI lending levels in 2008 and 2010, distance to AP, and levels of consumption and wages – households in these districts would have followed similar trends regardless of their exposure to the AP microfinance regulation.

In addition to our difference-in-differences specification, we employ an event study model to examine the evolution of relative outcomes, accounting for district-specific fixed effects and national trends over time. The model we estimate is as follows:

$$Y_{idt} = \alpha + \gamma_d + \delta_t + ExpAP_d \times \sum_{\substack{y=2006 \\ y \neq 2009}}^{y=2018} \beta_y \mathbf{I}[t = y] + X'_{idt}\zeta + \epsilon_{idt} \tag{3}$$

where the $ExpAP_d$ is the same exposure variable as defined in Equation 2. The indicator variables $\mathbf{I}[t = y]$ are year dummies, with the omitted reference category being $y =$

2009, representing the year immediately before the microfinance regulation in AP.[20] Each estimate of β_y quantifies the differential change in outcomes in year y, relative to the year just prior to the regulation, with differing degrees of exposure to the regulation. The rest of the variables included in Equation 3 are the same as in Equation 2.

5 Results

5.1 Main Results

In Table 2 and Figure 1, we present the main findings on the impact of exposure to microfinance regulation in AP on children's learning outcomes. In Table 2, Panel A reports results from the difference-in-differences specification, while Panel B presents results from the event study model. Our analysis reveals that exposure to AP microfinance regulation has had a significantly negative effect on children's learning outcomes. In Panel A of Table 2, we find that a one standard deviation increase in the exposure to the AP regulation corresponds to a 0.081 standard deviation decline in maths test score and a 0.053 standard deviation decline in reading test score, compared to similar districts that were not exposed to the regulation. Similarly, Figure 1 and Panel B of Table 2 show that pre-regulation event study coefficients are close to zero and statistically insignificant. A formal test for joint significance reveals that the pre-regulation coefficients are jointly insignificant (p-value: 0.7 (maths) and 0.61 (reading)), indicating no differential trends in learning outcomes across districts with varying levels of exposure.[21] However, from 2011 onwards, we observe a significant and persistent reduction in learning outcomes for children in districts with higher levels of exposure.[22]

[20] As in Equation 2, this analysis utilizes the same ASER survey data from the years 2006, 2007, 2008, 2009, 2010, 2011, 2012, 2013, 2014, 2016, and 2018.

[21] Additionally, we conduct a joint significance test using only pre-regulation data, limiting the sample of analysis to the 2006-2009 ASER rounds, with 2006 as the reference year. This test also shows that the pre-regulation coefficients are jointly insignificant (p-value: 0.78 (maths) and 0.64 (reading)).

[22] We do not find a significant coefficient for the year 2010, likely because the ASER survey is conducted between September and November each year, while the AP microfinance regulation was implemented in early October 2010. It is possible that the effects of the regulation took some time to manifest, thus explaining the insignificant coefficient for 2010.

To better understand the relative magnitude of our estimates, we compare them to effect sizes from other educational interventions in the literature. Kraft (2020), analyzing hundreds of interventions in high-income countries, reports a median effect size of 0.10 standard deviation for learning outcomes. The study also finds that larger sample sizes tend to yield smaller effects, with a median size of 0.03 standard deviation for studies with over 2,000 students compared to 0.24 standard deviation for those with 100 or fewer. Based on this, our estimated 0.08 standard deviation decline in maths score due to the AP microfinance regulation falls between the 70th and 80th percentiles. Similarly, Lortie-Forgues and Inglis (2019) report an average effect size of 0.06 standard deviation across 141 randomized controlled trials funded by the UK's Education Endowment Foundation and the U.S. National Center for Educational Evaluation and Regional Assistance. In low- and middle-income countries, Evans and Yuan (2022a) find a median effect size of 0.07 standard deviation for maths test score and 0.14 standard deviation for reading test score. Additionally, Chetty et al. (2014) estimate that a 0.08 standard deviation decline in student achievement corresponds to a 0.8% reduction in annual lifetime earnings.

The outcome variables used to measure learning achievements in our primary analysis are ordinal, capturing different levels of learning. As noted in Section 3.2, both the maths and reading assessments have four levels. To identify where the impacts are higher, we use binary variables representing each learning level as alternative dependent variables. We estimate linear probability models using the same specification as in the main analysis. The results show that the impact is smaller at lower levels and mostly increases in magnitude with higher levels of learning outcomes (Table A.1).

5.2 Mechanisms

This subsection investigates a few potential mechanisms that might explain the decline in learning outcomes of children in more exposed districts. Specifically, we explore four channels: a shift from private to government schooling, a reduction in household education expenditure, a decrease in household food expenditure, and a decline in mothers' employment.

5.2.1 Shift from Private to Government Schooling

In India, private schools are fee-charging, more expensive, and perceived by parents as better quality than government schools (Desai et al., 2009; Muralidharan and Sundararaman, 2015; Singh, 2015). The income shock triggered by the AP microfinance regulation may induce parents to enroll their children in government rather than private schools. To the extent private schools deliver better quality education, this shift in school choice may subsequently impact children's learning outcomes. Our findings, presented in column (3) of Table 3, indicate that children in more exposed districts are less likely to attend private schools. A one standard deviation increase in exposure to the regulation corresponds to a 2.4 percentage point decline in the likelihood of private school enrollment. This suggests that financial constraints imposed by the microfinance regulation might have discouraged households from enrolling their children in private schools.[23]

In response to the decline in private schooling, there is a significant increase in government school enrollment, as shown in column (2). Additionally, column (1) demonstrates a positive effect on overall school enrollment, driven by the increase in government school enrollment. A one standard deviation increase in exposure to the AP regulation is associated with a 1 percentage point increase in the likelihood of overall school enrollment, significant at the 10% level. Although the rise in overall enrollment may seem counterintuitive, it can be interpreted in several ways. First, consistent with the literature on the opportunity cost of schooling and child labour (Shah and Steinberg, 2017; Aggarwal, 2018; Edmonds and Theoharides, 2020), the income shock induced by the microfinance regulation may have reduced the incentive for children to leave school by restricting employment opportunities. Table A.2 provides supporting evidence for this claim, showing that the regulation is associated with a reduction in child labour; however, the effects are not statistically significant at conventional levels. Second, if the income shock made it difficult for households to afford basic necessities, they could ensure that their children

[23] We conduct a similar analysis on school enrollment using ASER data; the estimates are quite similar but lower in magnitude (Table A.3).

get a free meal by attending a school, especially a government school with the 'midday meal' scheme.[24] Third, as microfinance primarily targets informal, self-employed, and casual workers, the shock may have heightened the perceived value of education, prompting these workers to prioritize schooling for their children as a means to improve future career prospects in the formal sector.

5.2.2 Reduction in Household Education Expenditure

As discussed in Section 2, the microfinance regulation in AP led to the withdrawal of nearly USD 1 billion from the market, primarily due to banks stopping new loans to MFIs, which were heavily reliant on bank funding. This reduction in credit supply had a broader economic impact, resulting in lower employment, earnings, and consumption, even among non-borrowing households, through general equilibrium effects in rural labour markets. In this context, we investigate whether the regulation affected household monthly *education expenditure* and contributed to a decline in children's learning outcomes. Our findings, as presented in column (4) of Table 3, reveal that households in affected districts significantly cut back on education spending. A one standard deviation increase in exposure to the microfinance regulation is associated with a reduction of INR 39 (0.05 SD) in monthly household education expenditure, representing approximately a 13% decline over the mean, compared to non-exposed districts. This reduction may have adversely affected both the quality and quantity of educational resources available to children, thus explaining the decline in learning outcomes observed in our main findings.[25]

Additionally, the NSS consumer expenditure survey provides more detailed data on the components of annual education expenditure. Using this data, we look at how microfinance regulation affects these components, with the findings presented in Panel A

[24]In 1995, the Government of India launched the 'Midday Meal Scheme', offering free lunches to students in government schools. Studies show that this initiative has increased enrollment and retention, particularly among children from disadvantaged backgrounds (Drèze and Goyal, 2003; Chakraborty and Jayaraman, 2019). During the AP microfinance regulation, midday meal was universally implemented in all government schools of India.

[25]The same analysis is repeated in column (1) of Table A.4, with the expenditure data winsorized at the 99th percentile, producing similar results.

of Table 4. The results show that the major reduction in education spending is due to decreased expenditure on school fees. Given the predominantly free nature of government schools in India (Drèze and Sen, 2013), this decrease is likely associated with a shift from private to government schools. Consequently, the findings in Table 3 and Table 4 are consistent with each other.

5.2.3 Decrease in Household Food Expenditure

A well-established body of literature highlights the critical role of nutritional intake and dietary diversity in fostering cognitive development, particularly among children (Behrman and Rosenzweig, 2004; Grantham-McGregor et al., 2007). Nutrient-rich diets are essential not only for physical growth but also for enhancing cognitive abilities, which directly influence learning outcomes. The results in Column (5) of Table 3 indicate that the income shock triggered by the microfinance regulation has led to a decline in monthly household food expenditure. A one standard deviation increase in the exposure to the regulation is associated with a reduction of INR 71 (0.04 SD) in household food spending. This reduction likely compromises both the quality and quantity of food consumed by children, as lower expenditure on food may result in diets that are less diverse and nutritionally inadequate. Additionally, Panel B of Table 4 reveals a significant decrease in the consumption of cereals, vegetables, fruits, and nuts. Thus, the decline in learning outcomes in affected districts may, in part, be driven by reduced nutritional intake resulting from the economic constraints imposed by the regulation.

5.2.4 Decline in Mothers' Employment

Finally, we examine whether the AP microfinance regulation led to a decline in mothers' employment participation, given that women are usually the primary beneficiaries of microfinance institutions. If mothers' labour market outcomes are disproportionately affected by this shock, it could alter intra-household resource allocation, as mothers typically prioritize spending on their children's health and education more than fathers do (Thomas, 1990; Quisumbing and Maluccio, 2003). Several studies have documented the positive association between maternal employment and improved educational outcomes for children (Qian, 2008; Luke and Munshi, 2011; Afridi et al., 2016). Therefore, a decline in mothers' employment could disrupt household investments in education. Our analysis reveals a significant drop in mothers' employment participation (Table 5); a one standard deviation increase in exposure to the AP microfinance regulation corresponds to a 2.5 percentage point decline (i.e., 6.7% decline over the mean) in the likelihood of mothers' employment. This decline might alter household resource allocation priorities, potentially leading to underinvestment in children's education and reduced learning outcomes.[26]

5.3 Heterogeneity Analysis

Next, we investigate whether the impact of microfinance regulation on children's learning outcomes varies by gender and age. Examining these dimensions is particularly important given the context of gender bias (Lancaster et al., 2008; Sahoo and Klasen, 2021) and the concept of dynamic complementarity in early childhood investment (Cunha and Heckman, 2007). Thus, this subsection explores whether the regulation exacerbates existing gender inequalities or disproportionately impacts certain age cohorts.[27]

5.3.1 Heterogeneity by Gender

The decisions regarding educational spending for boys and girls are influenced by various factors, including household preferences (which may favour boys), perceived returns (such as lower female labour force participation), and overall budget constraints (Jayachandran, 2015). An income shock induced by microfinance regulation could lead to differential reductions in education expenditure based on the child's gender. This could further deepen existing inequalities and lead to differential effects on learning outcomes. To examine this, we assess whether exposure to the AP microfinance regulation had a

[26] The sample for this analysis is limited to married individuals aged 25 to 55 from households with at least one child in the school-going age range of 6 to 16 years.

[27] Following Feigenberg et al. (2023), the model for this analysis is specified by interacting the relevant heterogeneity variable with both the treatment variable and all other control variables.

differential impact on the learning outcomes of boys and girls. The results presented in Panel A of Figure 2 reveal that female children experience more adverse effects compared to their male counterparts. Specifically, Table 6 indicates that girls suffer an additional decline of 0.012 standard deviation in maths score and 0.01 standard deviation in reading score relative to boys. Further, coefficients from the event study model presented separately for boys and girls reveal a similar pattern (Figure A.2).[28]

Furthermore, we extend our heterogeneity analysis to explore whether similar gender differential effects are present within the potential mechanisms driving these findings. First, considering enrollment outcomes, Panel B of Figure 2 reveals significant gender differences in school choice. Column (5) of Table 6 indicates that girls experience an additional 2.3 percentage points lower likelihood of being enrolled in private schools compared to boys in response to the credit shock, suggesting that the financial pressures resulting from the regulation disproportionately affect girls' access to private schooling. Conversely, girls are more likely to be enrolled in government schools than their male counterparts, though the interaction coefficient for this effect is not statistically significant at conventional levels. However, we do not observe any significant gender-based variation in overall school enrollment, suggesting that while the regulation may influence the type of school children attend, it does not affect the overall likelihood of remaining in school. These results highlight that girls disproportionately bear the burden of the financial constraints imposed by the regulation, as households tend to prioritize shifting them from private to government schooling; this is likely to widen the existing gender gap in private school choice found by other studies (Maitra et al., 2016; Sahoo, 2017). This gender gap in private school enrollment may partially explain the differential impact of exposure to microfinance regulation on learning outcomes for boys and girls.

As discussed in Section 5.2, the next set of mechanisms, i.e., expenditures on education and food, are measured at the household level. To capture gender differences in the impact

[28]We also analyze the gender-differential effects within households by incorporating household fixed effects in the specification. The interaction coefficients remain negative for both maths and reading test scores; however, the estimate on maths test score is not statistically significant, as shown in Table A.5.

on these household-level outcomes, we interact the exposure variable with the proportion of female children in the household. The results are presented in columns (6)-(7) of Table 6.[29] The interaction coefficient for education expenditure is negative and statistically significant at the 10% level, suggesting that households with a higher proportion of female children reduce their education spending more in response to the credit shock. This result is consistent with the previous finding on the gender-differential effect on private school enrollment. In contrast, the interaction coefficient for food expenditure is positive and significant at the 10% level, suggesting that the reduction in food expenditure due to the shock is concentrated more among households with a higher proportion of boys.

A potential explanation for the contradictory findings on the gendered effects on education versus food expenditures is as follows. While education expenditure is measured only for young individuals pursuing education, food expenditure encompasses all household members. As households place a higher value on boys' education, the reduction in education expenditure is less when there is a higher proportion of boys; however, such households may compensate by reducing food expenditure – especially on food items consumed by adults. Such an adjustment in food expenditure is less needed when the proportion of girls is high, as the shock is absorbed by reducing girls' education expenditure in these households. Unfortunately, because individual-level data on food expenditure does not exist, we cannot empirically investigate whether the decline in food expenditure in households with a higher proportion of boys emanates from adults consuming less.

5.3.2 Heterogeneity by Age

Next, we investigate whether the impact of the microfinance regulation on children's learning outcomes varies by age. For this analysis, we categorize children into three age groups: 6-10, 11-13, and 14-16, which roughly correspond to the primary, middle, and secondary levels of schooling, respectively. Our findings, as illustrated in Panel A of Figure 3, indicate that younger cohorts – those aged 6-10 – are more adversely affected

[29]The variable 'proportion of female children' is constructed as the number of female children divided by the total number of children within the 6-16 age group.

by the microfinance regulation compared to older children. This pattern is consistent for both maths and reading test scores, with the corresponding regression coefficients presented in Table 7. These results underscore the critical importance of early childhood support, as children in younger age groups who experience reduced learning outcomes may require additional interventions later in life to recover from this setback.

Similar to our analysis of gender heterogeneity, we extend the examination of age-based heterogeneity in the primary outcome variables to potential mechanisms. Consistent with the patterns observed in our main heterogeneity analysis on learning outcomes, younger children experience a greater impact from the regulation regarding school choice. As shown in Panel B of Figure 3 and detailed in Table 7, younger children are significantly less likely to be enrolled in private schools compared to their older counterparts. On the other hand, younger children are more likely to be enrolled in government schools relative to older children. However, similar to our observations in the gender-based analysis, we do not find significant age-based differences in overall school enrollment rates. This indicates that the regulation's impact is primarily on the type of school attended rather than on the overall likelihood of school participation. These findings are consistent with the previously observed age-based differences in learning outcomes, offering an explanation for the varying impacts of the regulation on educational performance across age groups. For household education and food expenditure, the effect does not significantly vary with age groups.[30]

5.3.3 Heterogeneity by Exposure Intensity

In addition to examining the heterogeneity by children's gender and age, we further investigate whether the intensity of exposure to microfinance regulation has a differential effect on children's learning outcomes. Specifically, we assess whether the children from

[30] For these household-level outcomes, we consider the proportions of children in different age groups and use interaction terms to explore heterogeneity. For education expenditure, the interaction terms for higher age groups are negative, albeit statistically insignificant. Since older children incur higher expenditure on education, there may be more room for adjustment in their education expenditure in response to the shock. For food expenditure, there is no consistent pattern of age-based heterogeneity.

districts with higher exposure to the regulation experience more adverse effects compared to those with lower exposure. To evaluate this, we classify the exposed districts into four quantiles based on their exposure ratio to the regulation.[31] These quantiles are then interacted with a post-exposure dummy to capture the varying effects across districts. As expected, Figure A.4 and Table A.6 show that children from districts with the highest exposure to the regulation (Quantile 4) face more negative effects than those from districts with the least exposure (Quantile 1). These results emphasize the necessity of accounting for spatial variation in exposure when assessing the broader effects of financial regulations on human capital formation.

5.4 Robustness Analysis

To ensure the robustness of our main results, we conduct a few additional analyses. These include excluding districts bordering AP from the sample, limiting the ASER data to the 2007, 2009, and 2011 rounds, performing randomization inference, sequentially excluding states from the sample, conducting unweighted regressions, and restricting the analysis to urban households.

Removing Border Districts. First, to account for potential spillover effects associated with proximity to AP, we exclude districts that share a geographical border with AP from our estimation sample. The rationale behind this exclusion is that neighbouring districts might exhibit similarities to AP in terms of economic conditions, regulatory environments, or other contextual factors that could influence the outcomes under study. By excluding these bordering districts, we aim to accurately isolate the impact of the microfinance regulation enacted in AP, eliminating any indirect effects that may arise in the neighbouring areas. The regression results, presented in columns (1) and (2) of Table 8, show that even after removing these bordering districts, the results remain consistent with our original findings. This suggests that spillover effects from bordering districts are not driving the observed findings in our main analysis.

[31] Out of the 354 districts in our sample, 132 have an exposure ratio greater than 0. These 132 districts are categorized into four quantiles according to their exposure ratio.

Limiting to ASER 2007, 2009, and 2011. Second, we restrict our estimation sample to include only the ASER rounds from 2007, 2009, and 2011. This restriction is made to match the ASER data with the NSS rounds utilized in our mechanism analysis: 64 (2007-08), 66 (2009-10), and 68 (2011-12). This restriction ensures consistency across datasets and facilitates a more precise comparison. Columns (3) and (4) of Table 8 provide the results from this restricted sample, which are consistent with the initial findings.

Randomization Inference. Third, we perform a randomization inference test for the effect of exposure to the AP microfinance regulation on children's learning outcomes based on ASER data from 2006 to 2018. This analysis involves 500 iterations in which the continuous exposure variable is randomly reassigned across 354 districts.[32] This approach allows us to determine whether the observed association between exposure to the AP microfinance regulation and children's learning outcomes could be attributable to random variation.[33] Figure 4 presents the results from this randomization inference analysis. The distribution of coefficients from the randomization inference is centered around zero, while the actual coefficient observed in our study is far from this distribution. This indicates that the actual coefficients are unlikely to arise from spurious correlations, thereby providing reassurance about the validity of our main findings.

Excluding One State at a Time. Fourth, we conduct a sequential exclusion of states to test for regional dependencies in our results. In this exercise, we systematically remove one state at a time from the sample and re-estimate the model to assess whether the exclusion of any particular state substantially changes our findings.[34] This approach helps to identify whether our results are driven by any specific state or regional context, ensuring that our conclusions are not unduly influenced by the characteristics or policies

[32]In each iteration, 132 districts are assigned a positive value greater than zero, while the remaining districts are assigned a value of zero.

[33]If the observed effects were purely due to chance, we would expect many of the iterations to produce similar patterns of association between the exposure variable and learning outcomes. Conversely, a significant deviation of the actual coefficients from the distribution of coefficients produced by randomization would suggest that the observed effects are not due to random chance.

[34]Our sample consists of 21 states, of which only 16 are considered in this analysis. The remaining five states - Assam (0.17%), Meghalaya (0.64%), Pondicherry (0.41%), Sikkim (0.18%), and Tripura (0.22%) - are not considered due to their limited representation, collectively accounting for just 1.44% of the total sample.

of a single area. The results of this analysis, presented in Table A.7, are consistent with our original findings. Across 16 different regressions, the coefficients remain significant at the 1% level. Specifically, we observe that a one standard deviation increase in exposure to the AP microfinance regulation is associated with a 0.06 to 0.09 standard deviation decline in maths test score and a 0.04 to 0.06 standard deviation decline in reading test score. These results reinforce the robustness of our main findings and suggest that the observed impacts are not driven by any specific state.

Unweighted Regressions. Fifth, we run unweighted regressions as an additional robustness check to validate our findings. The results of the unweighted regressions, presented in Table A.8, are consistent with our original findings using ASER sampling weights. This further supports the robustness of our findings, regardless of the application of sampling weights. Additionally, we re-estimated our mechanism analyses without applying NSS sampling weights. The results, shown in Table A.9, are consistent with those obtained using sample weights.

Urban Sample. Finally, we extend our analysis to urban households to offer a comparative perspective on the effects observed in rural markets. While the rest of this paper focuses on rural households, this robustness check helps to understand how the AP microfinance regulation impacts urban areas, where credit markets are generally more integrated and diversified (Mohan, 2006). However, since ASER is exclusively a rural household survey, we are unable to perform this check on our primary outcome variables – maths and reading test scores. Instead, we focus on the main mechanisms contributing to the observed negative effects on learning outcomes, such as private school enrollment and educational expenditure. Given the greater access to alternative credit options and the lower dependency on microfinance institutions in urban areas, the regulation is expected to have a minimal effect on urban localities. As expected, the findings in Panel A of Table 9 show no statistically significant effect on private school enrollment or educational expenditure for the urban sample.

Further, we subdivide the urban sample into two groups: households residing in districts within 150 km of the state capital and those located more than 150 km away. This distinction aims to address concerns that urban areas further from the state capital may be less integrated into credit markets, making them more comparable to rural regions. We repeat the same analysis for each subsample separately, with results presented in Panels B and C of Table 9. As expected, urban households situated farther from the state capital experience negative impacts from the microfinance regulation similar to those found in rural areas. In contrast, urban households closer to the state capital do not experience any significant adverse effects.

Additional Robustness Checks. In Table A.10, we present results using raw test scores, ranging from 1 to 5, without standardization. The coefficients remain consistent with our main findings reported in Table 2. For ease of interpretation, in Table A.11, we also provide results based on a binary indicator for the presence of any lender with exposure to the AP regulation ($ExpAP_d > 0$). The findings indicate that children in districts with an AP-exposed lender experienced a decline of 0.17 standard deviation in maths test score and 0.11 standard deviation in reading test score. Additionally, to address concerns about potential coefficient attenuation with the inclusion of more post-regulation years, we limit our analysis to a shorter time frame, ending in 2014. The results presented in Table A.12 are similar to our main findings reported in Table 2.

6 Conclusion

The findings of this study provide evidence of the far-reaching consequences of financial disruptions on children's educational outcomes. Using the 2010 AP microfinance crisis as a natural experiment, we demonstrate that restricted access to microfinance led to a significant decline in children's learning outcomes, with a 0.08 standard deviation reduction in maths score and a 0.05 standard deviation decline in reading score. This decline has substantial implications when considered within the broader context of educational interventions, placing it around the median effect size observed in similar studies (Lortie-Forgues and Inglis, 2019; Kraft, 2020; Evans and Yuan, 2022a). To facilitate a more effective comparison with the effect sizes of other similar negative shocks to education, Garg et al. (2020) find that high temperatures reduce maths and reading test scores by 0.03 and 0.02 standard deviations, respectively. In another context, Sacerdote (2012) reports declines of 0.07 to 0.20 standard deviation in test scores among students displaced by Hurricanes Katrina and Rita in the United States. Andrabi et al. (2023) find that children living within 20 km of the earthquake zone scored 0.31 standard deviation lower than those more than 20 km away following the 2005 earthquake in Pakistan. In the context of the COVID-19 pandemic, studies find a learning loss of 0.08 standard deviation in the Netherlands (Engzell et al., 2021), 0.17 and 0.19 standard deviations in maths and language in Belgium (Maldonado and De Witte, 2022), and losses ranging from 0.05 to 0.17 standard deviations in the United Kingdom (Moscoviz and Evans, 2022). Compared to these effect sizes, our study also reveals larger negative effects, underscoring the considerable impact of restricting access to microfinance on educational outcomes.

Our findings add a new dimension to the existing literature on microfinance and education. While most of existing studies find no significant or transformative impact of microfinance provision on education-related outcomes, such as education expenditure, school enrollment, and learning achievements (Attanasio et al., 2015; Banerjee et al., 2015, 2019; Tarozzi et al., 2015), we offer new evidence from a different perspective. Instead of focusing on providing credit, we examine the effects of removing access to credit on these outcomes. Unlike the largely insignificant effects observed with the provision of credit, our study reveals significant and negative impacts on education because of removing access to microfinance. Our results also underscore the importance of early educational investments and the long-term effects of financial disruptions on learning outcomes. Furthermore, this research suggests a potential new area for exploration. An important aspect to consider is how reduced learning outcomes may impact future employment opportunities, illustrating how financial constraints during schooling years can shape long-term economic prospects.

In conclusion, this research highlights the unintended consequences of financial regulation on broader socioeconomic outcomes. Although these regulations aim to enhance stability and protect vulnerable populations, they can inadvertently restrict access to vital resources like credit, negatively impacting education and future economic opportunities. Our findings also suggest that the income shock had a disproportionately higher effect on girls, exacerbating existing gender gaps. Therefore, policymakers should consider both the direct and indirect effects of financial regulations, striving for a balance that maintains stability while ensuring access to resources essential for long-term growth and development.

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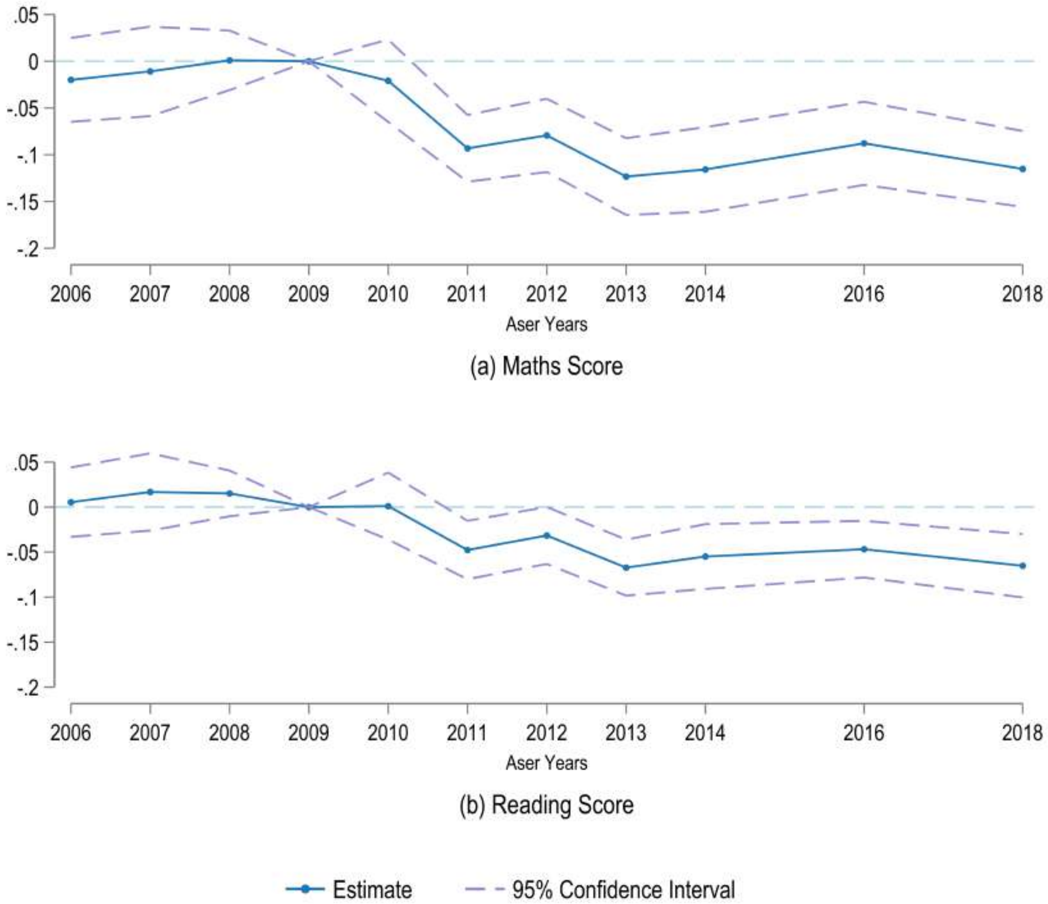
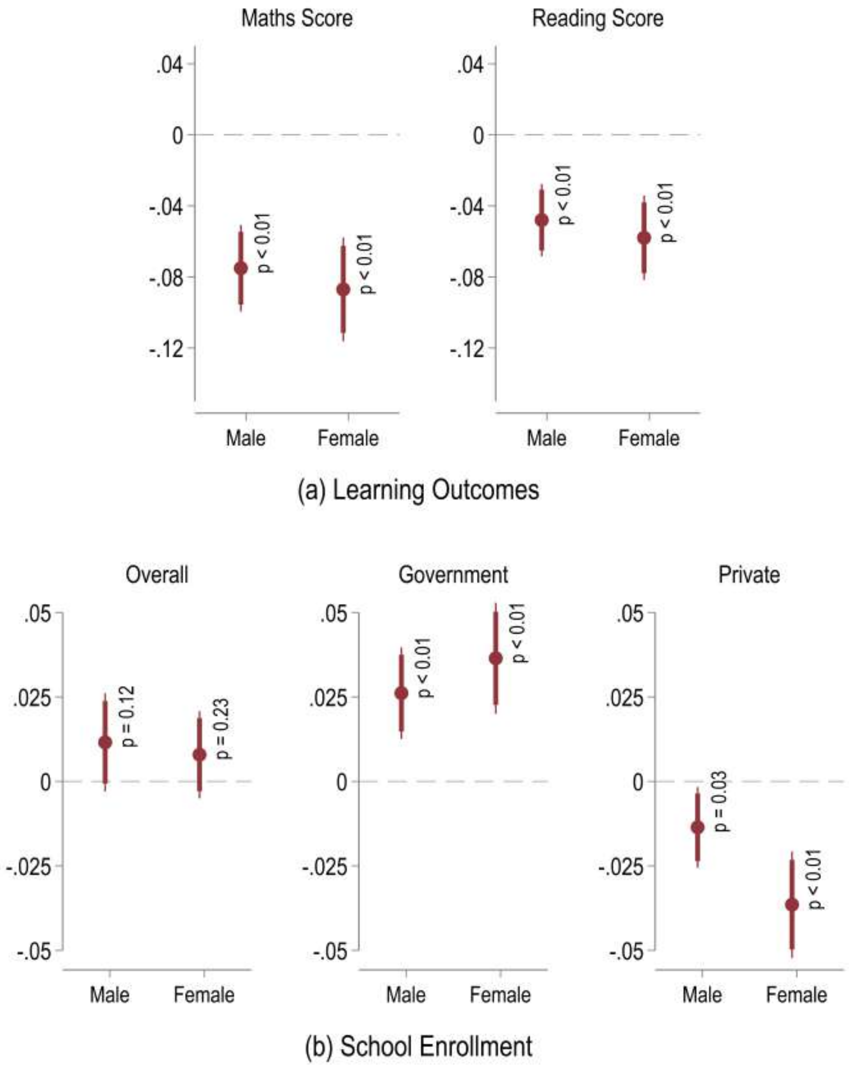
Figures and Tables

Figure 1: Effects of Exposure to the AP Regulation on Learning Outcomes - Event Study Plot



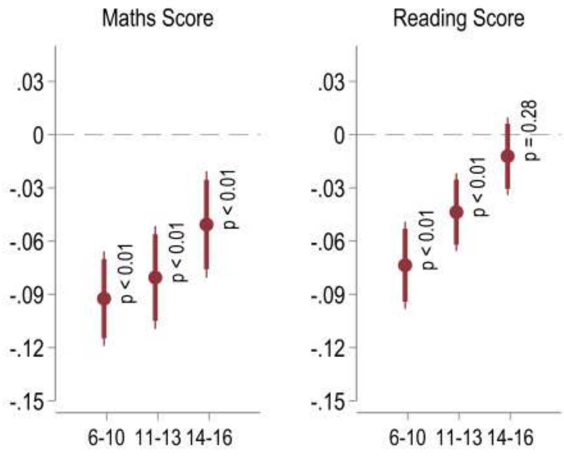
Notes: The graph illustrates the event study plots for learning outcomes, utilizing data from ASER rounds conducted in the years 2006 through 2018. The upper panel presents the event study coefficients for maths score, while the lower panel displays the coefficients for reading score. The solid lines represent the year-specific coefficients derived from the regressions of the outcome variable against the continuous exposure measure. Dashed lines depict the 95% confidence intervals. Round 2009 serves as the omitted category. Each regression includes control variables such as the linear distance from the district centroid to AP, rural population and its square in 2010, dummy variables for GLP quintiles in 2008 and 2010, average rural per capita consumption and casual daily wage in 2010, and the count of rural schools in 2010, all interacted with the round variable. Additionally, the model includes controls for sex and quintiles of household size. It also accounts for round and district fixed effects. Standard errors clustered at the district level, and observations are weighted by ASER sampling weights.

Figure 2: Heterogeneity by Gender

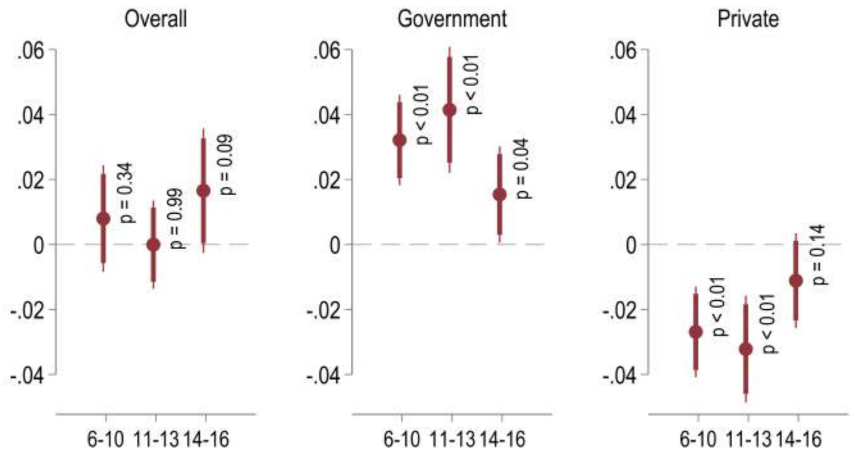


Notes: The figure illustrates the heterogeneous effects of exposure to the AP microfinance regulation on children's learning outcomes and school enrollment. The upper panel, using ASER data (2006-2018), highlights differential impacts by gender in maths and reading scores, while the lower panel, based on NSS education round 64 and employment rounds 66 and 68, illustrates the differential impacts of gender on overall, government, and private school enrollment. Point estimates are represented by dots, with thicker and thinner lines indicating 90% and 95% confidence intervals, respectively. Each regression includes control variables such as the linear distance from the district centroid to AP, rural population and its square in 2010, dummy variables for GLP quintiles in 2008 and 2010, average rural per capita consumption and casual daily wage in 2010, and the count of rural schools in 2010, all interacted with the round variable. Additionally, we control for the district level total number of rural schools, rural government schools, and rural private schools in 2009-10, each interacting with the round variable in their respective models for overall, government, and private school enrollment. For the government and private school enrollment models, estimation is performed for children who are enrolled in a school. The model further incorporates the household and individual level controls. It also accounts for round and district fixed effects. Standard errors clustered at the district level, and observations are weighted by their respective sampling weights.

Figure 3: Heterogeneity by Age Group



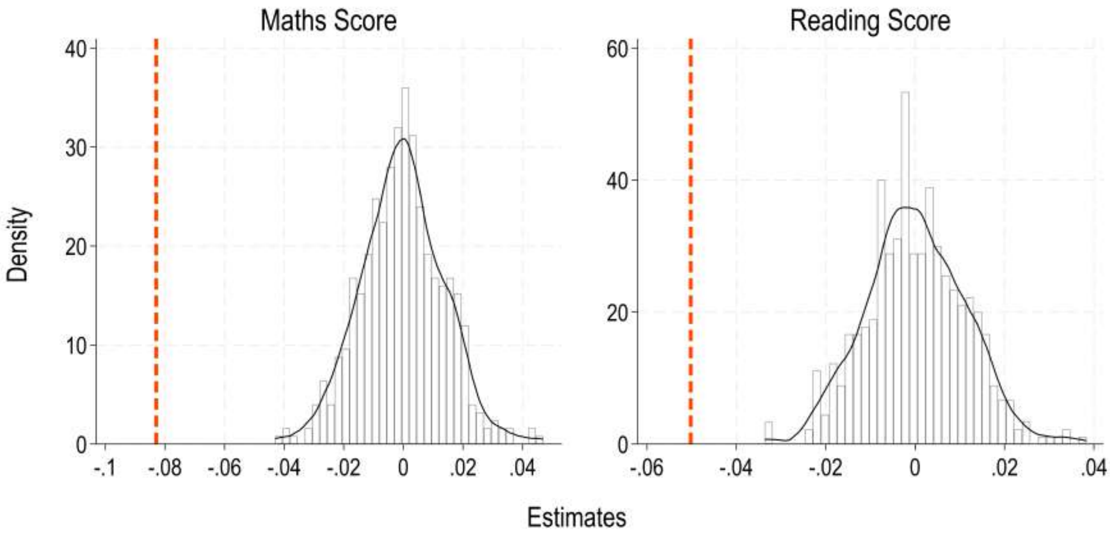
(a) Learning Outcomes



(b) School Enrollment

Notes: The figure illustrates the heterogeneous effects of exposure to the AP microfinance regulation on children's learning outcomes and school enrollment. The upper panel, using ASER data (2006-2018), highlights differential impacts by age groups in maths and reading scores, while the lower panel, based on NSS education round 64 and employment rounds 66 and 68, illustrates the differential impacts by age groups on overall, government, and private school enrollment. Point estimates are represented by dots, with thicker and thinner lines indicating 90% and 95% confidence intervals, respectively. Each regression includes control variables such as the linear distance from the district centroid to AP, rural population and its square in 2010, dummy variables for GLP quintiles in 2008 and 2010, average rural per capita consumption and casual daily wage in 2010, and the count of rural schools in 2010, all interacted with the round variable. Additionally, we control for the district level total number of rural schools, rural government schools, and rural private schools in 2009-10, each interacting with the round variable in their respective models for overall, government, and private school enrollment. For the government and private school enrollment models, estimation is performed for children who are enrolled in a school. The model further incorporates the household and individual level controls. It also accounts for round and district fixed effects. Standard errors clustered at the district level, and observations are weighted by their respective sampling weights.

Figure 4: Robustness Check: Randomization Inference



Notes: The figure presents the results of the randomization inference test for the effects of exposure to the AP microfinance regulation on children's learning outcomes, based on ASER data from 2006 to 2018. We carry out 500 iterations where the exposure variable is randomly shuffled among 354 districts, such that 132 districts are assigned a value greater than 0 in each iteration, while the rest are assigned a value of 0. The dashed red line represents the coefficient obtained from the regression using the actual exposure data. Each regression includes control variables such as the linear distance from the district centroid to AP, rural population and its square in 2010, dummy variables for GLP quintiles in 2008 and 2010, average rural per capita consumption and casual daily wage in 2010, and the count of rural schools in 2010, all interacted with the round variable. Additionally, the model includes controls for sex and quintiles of household size. It also accounts for round and district fixed effects, with standard errors clustered at the district level.

Table 1: Summary Statistics

	Obs (1)	Mean (2)	Std Dev (3)	Min (4)	Max (5)
Panel A: Treatment Variables from Balance Sheet Data (District-Level)					
Exposure Ratio	354	0.08	0.13	0.00	0.43
Any Exposed Lender	354	0.35	0.48	0.00	1.00
Panel B: Outcome Variables from Aser (Individual-Level)					
Maths Score	3058204	3.37	1.31	1.00	5.00
Reading Score	3067727	3.71	1.43	1.00	5.00
Maths Score (Standardized)	3058204	-0.07	1.03	-3.82	3.36
Reading Score (Standardized)	3067727	-0.04	1.04	-4.25	2.96
Panel C: Outcome Variables from NSS (Individual/Household-Level)					
Enrolled in School (Age 6-16)	121708	0.81	0.39	0.00	1.00
Enrolled in Govt School (Age 6-16)	98405	0.85	0.36	0.00	1.00
Enrolled in Private School (Age 6-16)	98405	0.15	0.35	0.00	1.00
HH Monthly Expenditure: Education	64616	227.01	511.60	0.00	79500.00
HH Monthly Expenditure: Food	68060	2423.86	1310.38	0.00	34606.00
HH Annaul Education Expenditure - Books	52408	485.56	871.97	0.00	35000.00
HH Annaul Education Expenditure - School Fee	52408	1258.24	4556.50	0.00	500000.00

Notes: Panel A variables are taken from balance sheet information obtained through MFIN. The 'Exposure Ratio' measures the extent to which each district was affected by the AP microfinance regulation, as defined in Equation 1. The variable 'Any Exposed Lender' is a binary indicator created from the continuous variable 'Exposure Ratio,' where it takes the value 1 if the exposure ratio exceeds 0 and 0 otherwise. Panel B variables are coming from ASER rounds conducted between 2006 and 2018, while Panel C variables are drawn from three rounds of NSS Surveys: the 64th (2007-2008), 66th (2009-2010), and 68th (2011-2012).

Table 2: Effects of Exposure to the AP Regulation on Learning Outcomes

	Maths score std. (1)	Reading score std. (2)
Panel A - (DiD Model)		
Exposure Ratio × Post 2010	-0.081***	-0.053***
	(0.013)	(0.011)
Panel B - (Event Study Model)		
Exposure Ratio × 2006	-0.020	0.005
	(0.023)	(0.020)
Exposure Ratio × 2007	-0.011	0.017
	(0.024)	(0.022)
Exposure Ratio × 2008	0.001	0.015
	(0.016)	(0.013)
Exposure Ratio × 2010	-0.021	0.001
	(0.022)	(0.019)
Exposure Ratio × 2011	-0.093***	-0.048***
	(0.018)	(0.016)
Exposure Ratio × 2012	-0.079***	-0.032*
	(0.020)	(0.016)
Exposure Ratio × 2013	-0.123***	-0.067***
	(0.021)	(0.016)
Exposure Ratio × 2014	-0.116***	-0.055***
	(0.023)	(0.018)
Exposure Ratio × 2016	-0.088***	-0.047***
	(0.023)	(0.016)
Exposure Ratio × 2018	-0.115***	-0.065***
	(0.021)	(0.018)
Control mean	-0.0414	-0.00961
Control SD	1.006	1.025
Observations	3058204	3067727

Notes: The outcome variables are taken from various rounds of ASER conducted in the years 2006 through 2018. Panel A displays coefficients from regressions using a difference-in-differences specification, while Panel B presents coefficients from separate regressions using an event study specification (Year 2009 serves as the omitted category). Age-wise standardization for both maths and reading scores was applied within each year. Both columns include control variables such as the linear distance from the district centroid to AP, rural population and its square in 2010, dummy variables for GLP quintiles in 2008 and 2010, average rural per capita consumption and casual daily wage in 2010, and the count of rural schools in 2010, all interacted with the round variable. Additionally, the model includes controls for sex and quintiles of household size. It also accounts for round and district fixed effects. Standard errors clustered at the district level, and observations are weighted by ASER sampling weights. The symbols *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table 3: Mechanisms: School Enrollment and Expenditure Outcomes

	Enrolled in School (1)	Enrolled in Government School (2)	Enrolled in Private School (3)	HH Monthly Education Expenditure (4)	HH Monthly Food Expenditure (5)
Exposure Ratio × Post 2010	0.010*	0.031***	-0.024***	-38.892***	-71.121***
	(0.006)	(0.007)	(0.006)	(8.230)	(25.616)
Control mean	0.833	0.767	0.230	290.4	2729.3
Control SD	0.373	0.423	0.421	753.8	1739.0
Observations	121708	98405	98405	64616	68060

Notes: The outcome variables in columns (1)-(3) are taken from NSS education round 64 and employment rounds 66 and 68, while columns (4)-(5) are based on employment rounds 64, 66, and 68. In columns (2) and (3), we examine government and private school enrollment, conditional on being enrolled in a school. All columns include control variables such as the linear distance from the district centroid to AP, the rural population and its square in 2010, dummy variables for GLP quintiles in 2008 and 2010, and the average rural per capita consumption and casual daily wage in 2010, all interacted with the round variable. Additionally, we control for the district level total number of rural schools, rural government schools, and rural private schools in 2009-10, each interacted with the round variable in columns (1), (2), and (3), respectively. The model further incorporates household-level controls, including social group, religion, household type, household head's education, number of children, and quintiles of household size. In columns (1)-(3), individual-level controls include age and sex. The model also accounts for the survey month, round, and district fixed effects. Standard errors are clustered at the district level, and observations are weighted by NSS sampling weights. The symbols *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table 4: Effects of Exposure to the AP Regulation on Educational and Food Expenditures

	Panel A: Household Annual Education Expenditure					
	Books/ Journals (1)	Newspapers/ Periodicals (2)	Stationary (3)	School Fees (4)	Private Tuition/ Coaching (5)	Total Education Expenses (6)
Exposure Ratio × Post 2010	-75.183***	-5.651	1.504	-257.638***	-5.981	-349.282***
	(18.986)	(4.352)	(14.050)	(66.830)	(29.148)	(99.184)
Control mean	537.7	104.3	376.9	1900.6	446.5	3443.1
Control SD	1130.7	396.6	828.5	7306.4	1752.2	8826.3
Observations	52408	52408	52408	52408	52408	52408
	Panel B: Household Monthly Food Expenditure					
	Cereals & Cereal Products (1)	Pulses & Pulse Products (2)	Milk & Milk Products (3)	Vegetables, Fruits & Nuts (4)	Egg, Fish & Meat (5)	Other Foods (6)
Exposure Ratio × Post 2010	-23.581***	1.234	-6.723	-20.408***	-8.620	-13.024*
	(8.506)	(2.739)	(11.132)	(5.392)	(7.635)	(7.106)
Control mean	695.7	185.9	507.7	455.1	245.3	639.6
Control SD	441.2	151.1	811.4	327.2	389.8	629.8
Observations	68060	68060	68060	68060	68060	68060

Notes: In Panel A, the outcome variables are based on NSS consumption rounds 64, 66, and 68, while those in Panel B come from employment rounds for the same periods. All columns include control variables such as the linear distance from the district centroid to AP, the rural population and its square in 2010, dummy variables for GLP quintiles in 2008 and 2010, and the average rural per capita consumption and casual daily wage in 2010, all interacted with the round variable. The model also incorporates household-level controls, including social group, religion, household type, household head's education, number of children, proportion of female children, and quintiles of household size. Additionally, it accounts for survey month, round, and district fixed effects. Standard errors are clustered at the district level, and observations are weighted by NSS sampling weights. The symbols *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table 5: Mechanisms: Mothers' Employment

	All Individuals		Parents Sample	
	Employment (1)	Paid Work (2)	Employment (3)	Paid Work (4)
Exposure Ratio × Post 2010	0.002	0.006	0.004	0.007
	(0.004)	(0.005)	(0.004)	(0.005)
Exposure Ratio × Post 2010 × Female	-0.026**	-0.024**	-0.029**	-0.026**
	(0.012)	(0.010)	(0.012)	(0.011)
Marginal Effects				
Male	0.002	0.006	0.004	0.007
	(0.004)	(0.005)	(0.004)	(0.005)
Female	-0.024**	-0.018**	-0.025**	-0.020**
	(0.010)	(0.008)	(0.010)	(0.009)
Control mean (Male)	0.956	0.896	0.955	0.909
Control SD (Male)	0.205	0.306	0.207	0.288
Control mean (Female)	0.368	0.264	0.374	0.272
Control SD (Female)	0.482	0.441	0.484	0.445
Observations	139675	139675	128600	128600

Notes: Outcome variables come from NSS employment rounds 64, 66, and 68, measuring employment participation based on daily status. The sample for this analysis is limited to married individuals aged 25 to 55 from households with at least one child in the school-going age range of 6 to 16 years. In columns (3) and (4), the sample is further restricted to include only parents. All columns include control variables such as the linear distance from the district centroid to AP, the rural population and its square in 2010, dummy variables for GLP quintiles in 2008 and 2010, the average rural per capita consumption and casual daily wage in 2010 and the total number of rural schools in the district, all interacted with the round variable. The model further incorporates household-level controls, including social group, religion, household type, household head's education, number of children, and quintiles of household size. Individual-level controls include age and sex. The model also accounts for the survey month, round, and district fixed effects. Standard errors are clustered at the district level, and observations are weighted by NSS sampling weights. The symbols *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table 6: Heterogeneity by Gender

	Maths score std. (1)	Reading score std. (2)	Enrolled in School (3)	Enrolled in Government School (4)	Enrolled in Private School (5)	HH Monthly Education Expenditure (6)	HH Monthly Food Expenditure (7)
Exposure Ratio × Post 2010	-0.075***	-0.048***	0.012	0.026***	-0.014**	-24.110**	-91.549***
	(0.012)	(0.010)	(0.007)	(0.007)	(0.006)	(9.315)	(32.751)
Exposure Ratio × Post 2010 × Girl	-0.012**	-0.010**	-0.004	0.010	-0.023***		
	(0.005)	(0.004)	(0.007)	(0.007)	(0.007)		
Exposure Ratio × Post 2010 × Prop Girl Child						-26.892*	61.529*
						(14.543)	(36.664)
Control mean	-0.000108	-0.0190	0.848	0.750	0.247	330.9	2885.8
Control SD	0.991	1.013	0.359	0.433	0.432	617.8	1455.9
Observations	3058204	3067727	121708	98405	98405	63981	67421

Notes: Outcome variables in columns (1)-(2) are from various rounds of ASER(2006-2018), columns (3)-(5) use NSS education round 64 and employment rounds 66 and 68, and columns (6)-(7) rely on NSS employment rounds 64, 66, and 68. In columns (4) and (5), we examine government and private school enrollment, conditional on being enrolled in a school. The variable 'Prop Girl Child' refers to the proportion of female children within the household, calculated as the number of girls aged 6 to 16 relative to the total number of children in that age range. All columns include control variables such as the linear distance from the district centroid to AP, the rural population and its square in 2010, dummy variables for GLP quintiles in 2008 and 2010, and the average rural per capita consumption and casual daily wage in 2010, all interacted with the round variable. Additionally, we control for the total number of rural schools, rural government schools, and rural private schools in 2009-10, each interacted with the round variable in columns (3), (4), and (5), respectively. The model further includes household and individual level controls. It also accounts for round and district fixed effects. Standard errors clustered at the district level, and observations are weighted by their respective sampling weights. The symbols *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table 7: Heterogeneity by Age Group

	Maths score std. (1)	Reading score std. (2)	Enrolled in School (3)	Enrolled in Government School (4)	Enrolled in Private School (5)	HH Monthly Education Expenditure (6)	HH Monthly Food Expenditure (7)
Exposure Ratio × Post 2010	-0.092***	-0.074***	0.008	0.032***	-0.027***	-24.628***	-61.035*
	(0.014)	(0.013)	(0.008)	(0.007)	(0.007)	(8.168)	(35.214)
Exposure Ratio × Post 2010 × (Age 11-13)	0.012	0.030***	-0.008	0.009	-0.005		
	(0.008)	(0.008)	(0.010)	(0.009)	(0.007)		
Exposure Ratio × Post 2010 × (Age 14-16)	0.042***	0.061***	0.009	-0.017**	0.016*		
	(0.011)	(0.009)	(0.012)	(0.008)	(0.009)		
Exposure Ratio × Post 2010 × Prop Child (Age 11-13)						-9.112	14.384
						(17.296)	(44.565)
Exposure Ratio × Post 2010 × Prop Child (Age 14-16)						-16.458	-6.938
						(18.130)	(36.861)
Control mean	-0.0121	0.00979	0.912	0.754	0.243	438.1	2928.8
Control SD	0.990	1.014	0.284	0.431	0.429	831.7	1413.6
Observations	3058204	3067727	121708	98405	98405	63684	67132

Notes: Outcome variables in columns (1)-(2) are from various rounds of ASER(2006-2018), columns (3)-(5) use NSS education round 64 and employment rounds 66 and 68, and columns (6)-(7) rely on NSS employment rounds 64, 66, and 68. In columns (4) and (5), we examine government and private school enrollment, conditional on being enrolled in a school. All columns include control variables such as the linear distance from the district centroid to AP, the rural population and its square in 2010, dummy variables for GLP quintiles in 2008 and 2010, and the average rural per capita consumption and casual daily wage in 2010, all interacted with the round variable. Additionally, we control for the total number of rural schools, rural government schools, and rural private schools in 2009-10, each interacted with the round variable in columns (3), (4), and (5), respectively. The model further incorporates household and individual level controls. It also accounts for round and district fixed effects. Standard errors clustered at the district level, and observations are weighted by their respective sampling weights. The symbols *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table 8: Robustness Check: Exclude Border Districts and Limit ASER to the 2007, 2009, and 2011 Rounds

	Excluding Border Districts		Limiting to 2007, 2009, and 2011 Data	
	Maths score std. (1)	Reading score std. (2)	Maths score std. (3)	Reading score std. (4)
Exposure Ratio × Post 2010	-0.079***	-0.053***	-0.083***	-0.053***
	(0.014)	(0.012)	(0.017)	(0.014)
Control mean	-0.0379	-0.000836	-0.0343	-0.00977
Control SD	1.008	1.025	0.994	1.015
Observations	2911616	2920687	989249	994240

Notes: Data for the outcome variables come from various ASER survey rounds conducted from 2006 to 2018. Age-wise standardization for both maths and reading scores was applied within each year. In the first two columns, we exclude districts that share a geographical border with Andhra Pradesh (AP). The last two columns restrict the ASER data to the 2007, 2009, and 2011 rounds to align with the NSS rounds used in our analysis of mechanisms. Each regression includes control variables such as the linear distance from the district centroid to AP, rural population and its square in 2010, dummy variables for GLP quintiles in 2008 and 2010, average rural per capita consumption and casual daily wage in 2010, and the count of rural schools in 2010, all interacted with the round variable. Additionally, the model includes controls for sex and quintiles of household size. It also accounts for round and district fixed effects. Standard errors clustered at the district level, and observations are weighted by ASER sampling weights. The symbols *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

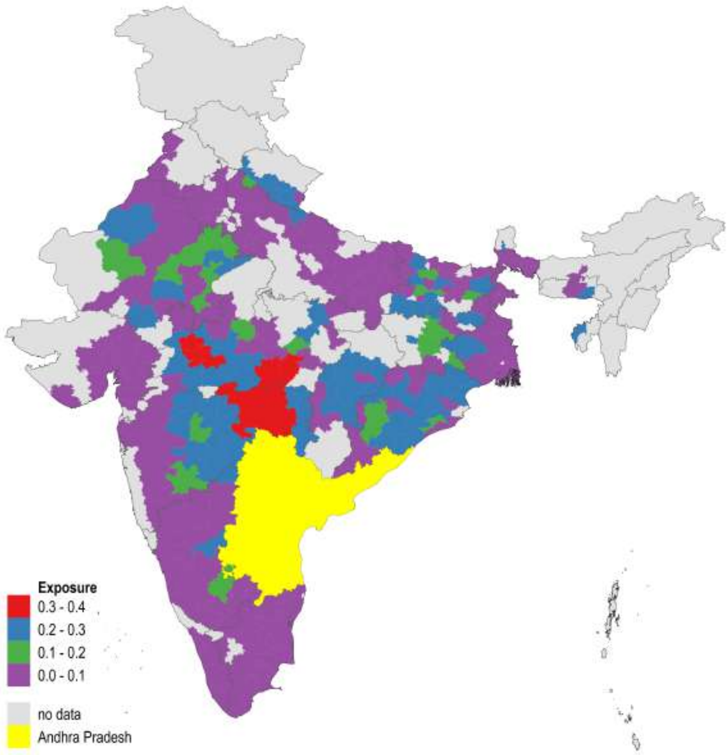
Table 9: Robustness Check: Urban Sample

	Enrolled in School (1)	Enrolled in Government School (2)	Enrolled in Private School (3)	HH Monthly Education Expenditure (4)	School Fees (5)
Panel A - Overall Urban					
Exposure Ratio × Post 2010	0.012**	0.007	-0.015	-14.522	-403.454
	(0.006)	(0.011)	(0.011)	(25.211)	(247.108)
Control mean	0.829	0.646	0.351	764.9	6606.1
Control SD	0.377	0.478	0.477	1681.1	18419.4
Observations	61588	50701	50701	67758	59625
Panel B - Close to the State Capital					
Exposure Ratio × Post 2010	-0.008	-0.016	0.018	35.318	397.245
	(0.011)	(0.018)	(0.019)	(54.214)	(645.385)
Control mean	0.843	0.692	0.306	822.0	6949.0
Control SD	0.363	0.462	0.461	1815.7	19490.6
Observations	27003	22330	22330	31511	27214
Panel C - Far From the State Capital					
Exposure Ratio × Post 2010	0.016**	0.015	-0.031**	-16.552	-460.496
	(0.007)	(0.013)	(0.013)	(39.568)	(305.060)
Control mean	0.815	0.599	0.396	696.4	6194.6
Control SD	0.389	0.490	0.489	1501.0	17037.8
Observations	34585	28371	28371	36247	32411

Notes: The outcome variables are taken from various rounds of NSS surveys, 64, 66, and 68. In Panel A, the sample includes only households residing in urban areas. Panel B restricts the sample to urban households in districts where the centroid lies within 150 km of the state capital, while Panel C focuses on urban households in districts located more than 150 km away. In all columns, controls include the linear distance from the district centroid to AP, the urban population and its square in 2010, dummy variables for GLP quintiles in 2008 and 2010, and the average urban per capita consumption and casual daily wage in 2010, all interacted with the round variable. Additionally, we control for the district level total number of urban schools, urban government schools, and urban private schools in 2009-10, each interacted with the round variable in columns (1), (2), and (3), respectively. The model also incorporates household-level controls, including social group, religion, household type, household head's education, number of children, and quintiles of household size. Columns (1), (2), and (3) incorporate individual-level controls such as age and gender. Additionally, it accounts for survey month, round, and district fixed effects. Standard errors are clustered at the district level, and observations are weighted by NSS sampling weights. The symbols *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

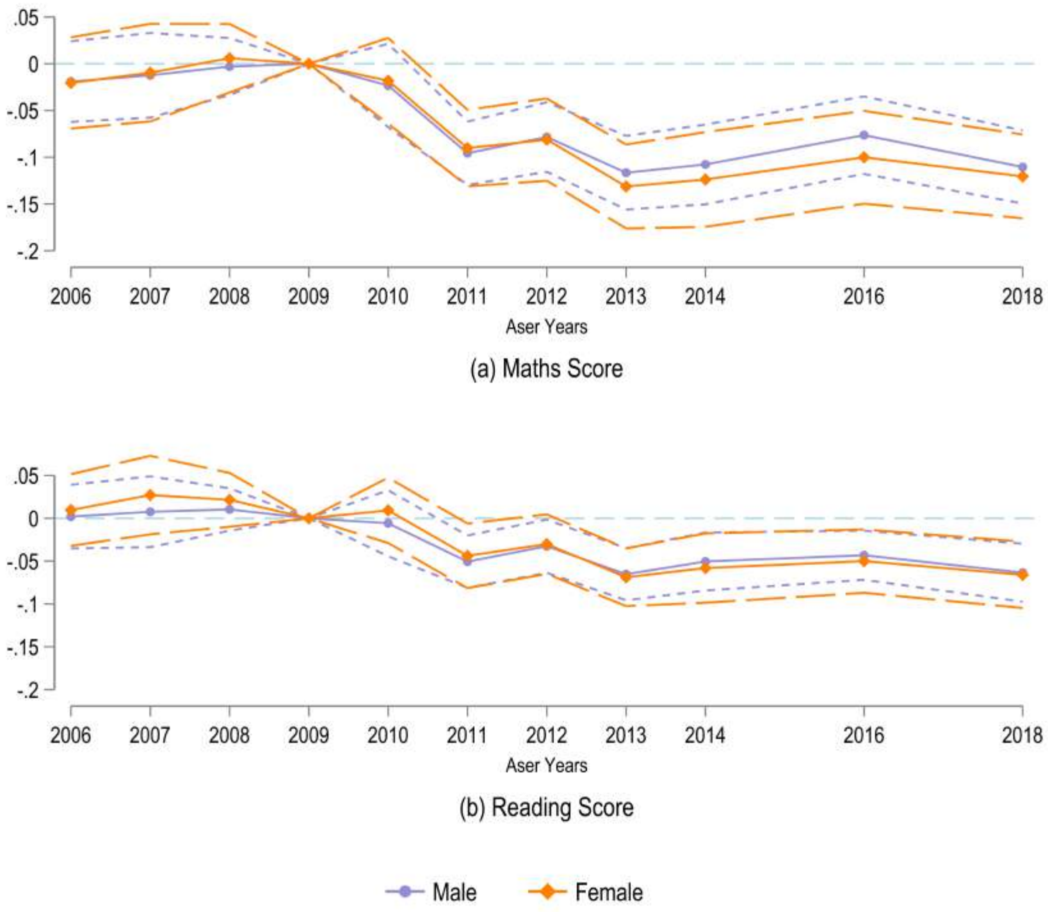
Appendix.

Figure A.1: Exposure of Districts to the AP Regulation



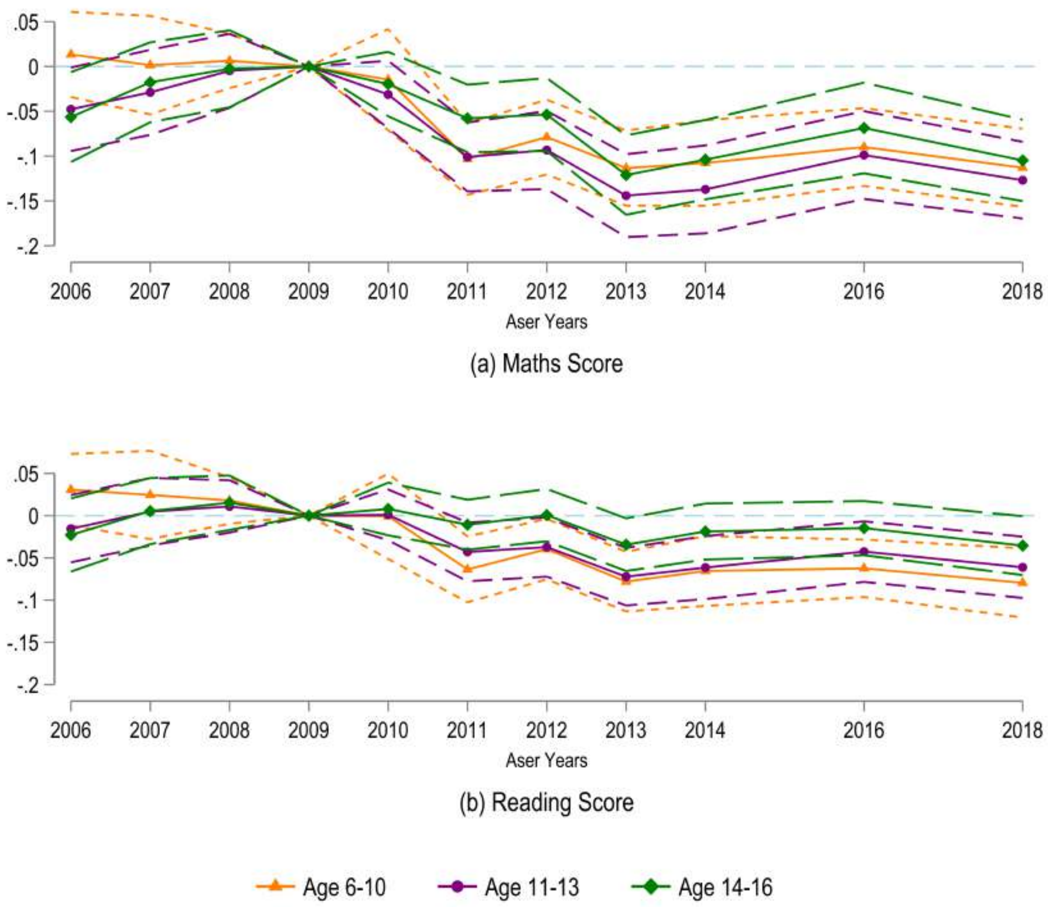
Notes: The figure illustrates the variations in district-level exposure across India to the AP microfinance regulation, as measured by a continuous exposure variable defined in Equation 1.

Figure A.2: Heterogeneity by Gender - Event Study Plot



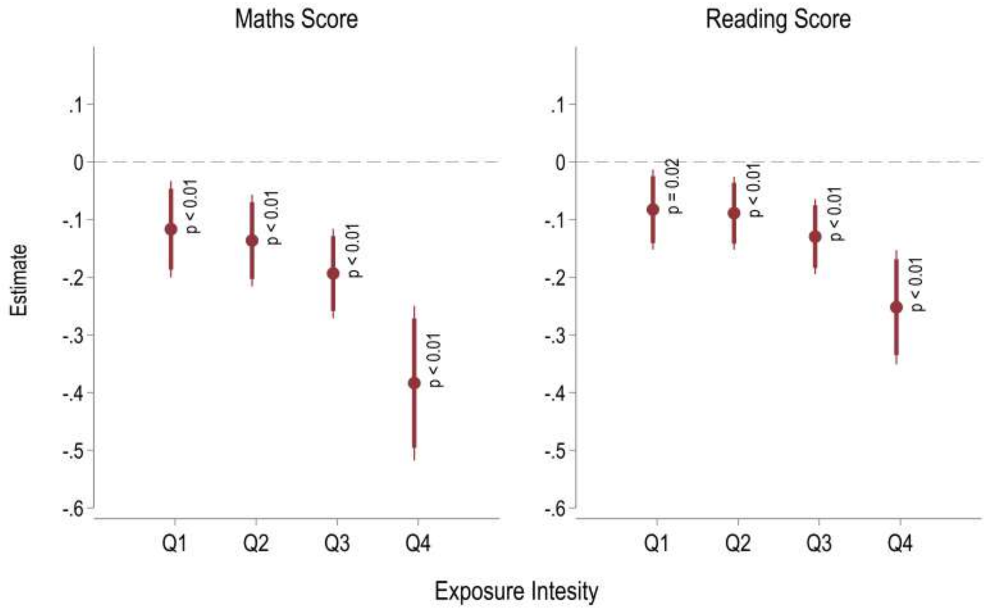
Notes: The graph presents event study plots for learning outcomes separately by gender, using ASER data from 2006 to 2018. The upper panel presents the event study coefficients for maths score, while the lower panel displays the coefficients for reading score. The color lavender denotes male children, and orange represents female children. The solid lines represent the year-specific coefficients derived from the regressions of the outcome variable against the continuous exposure measure. Dashed lines depict the 95% confidence intervals. Round 2009 serves as the omitted category. Each regression includes control variables such as the linear distance from the district centroid to AP, rural population and its square in 2010, dummy variables for GLP quintiles in 2008 and 2010, average rural per capita consumption and casual daily wage in 2010, and the count of rural schools in 2010, all interacted with the round variable. Additionally, the model controls for quintiles of household size. It also accounts for round and district fixed effects. Standard errors clustered at the district level, and observations are weighted by ASER sampling weights.

Figure A.3: Heterogeneity by Age Groups - Event Study Plot



Notes: The graph presents event study plots for learning outcomes separately by age group, using ASER data from 2006 to 2018. The upper panel presents the event study coefficients for maths score, while the lower panel displays the coefficients for reading score. Children in the 6-10 age range are represented by the color orange, those in the 11-13 age range are shown in purple, and children aged 14-16 are depicted in green. The solid lines represent the year-specific coefficients derived from the regressions of the outcome variable against the continuous exposure measure. Dashed lines depict the 95% confidence intervals. Round 2009 serves as the omitted category. Each regression includes control variables such as the linear distance from the district centroid to AP, rural population and its square in 2010, dummy variables for GLP quintiles in 2008 and 2010, average rural per capita consumption and casual daily wage in 2010, and the count of rural schools in 2010, all interacted with the round variable. Additionally, the model controls for quintiles of household size. It also accounts for round and district fixed effects. Standard errors clustered at the district level, and observations are weighted by AESR sampling weights.

Figure A.4: Heterogeneity by Exposure Intensity



Notes: The figure illustrates the heterogeneous effects of varying levels of exposure to the AP microfinance regulation on children's learning outcomes, using ASER data from 2006 to 2018. In this analysis, the exposed districts are categorized into four quantiles based on their exposure intensity, with Q1 representing the lowest level of exposure and Q4 representing the highest. Point estimates are represented by dots, with thicker and thinner lines indicating 90% and 95% confidence intervals, respectively. Each regression includes control variables such as the linear distance from the district centroid to AP, rural population and its square in 2010, dummy variables for GLP quintiles in 2008 and 2010, average rural per capita consumption and casual daily wage in 2010, and the count of rural schools in 2010, all interacted with the round variable. Additionally, the model includes controls for sex and quintiles of household size. It also accounts for round and district fixed effects. Standard errors clustered at the district level, and observations are weighted by ASER sampling weights.

Table A.1: Robustness Check: Different Levels of Learning Outcomes Represented by Binary Indicators

	Maths Proficiency Levels				Reading Proficiency Levels			
	Single-Digit (1)	Double-Digit (2)	Subtraction (3)	Division (4)	Letter (5)	Word (6)	Paragraph (7)	Story (8)
Exposure Ratio × Post 2010	-0.008***	-0.020***	-0.030***	-0.014***	-0.006***	-0.012***	-0.014***	-0.016***
	(0.002)	(0.004)	(0.005)	(0.004)	(0.002)	(0.004)	(0.004)	(0.004)
Control mean	0.919	0.746	0.476	0.263	0.899	0.745	0.610	0.459
Control SD	0.273	0.435	0.499	0.440	0.301	0.436	0.488	0.498
Observations	3058204	3058204	3058204	3058204	3067727	3067727	3067727	3067727

Notes: The outcome variables are taken from various rounds of ASER conducted in the years 2006 through 2018. All columns include control variables such as the linear distance from the district centroid to AP, rural population and its square in 2010, dummy variables for GLP quintiles in 2008 and 2010, average rural per capita consumption and casual daily wage in 2010, and the count of rural schools in 2010, all interacted with the round variable. Additionally, the model includes controls for age, sex and quintiles of household size. It also accounts for round and district fixed effects. Standard errors clustered at the district level, and observations are weighted by ASER sampling weights. The symbols *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table A.2: Effects of Exposure to the AP Regulation on Child Labor

	Participating in Labour Force: Principal Status (1)	Participating in Labour Force: Daily Status (2)	No of Days: Spent in Labour Force (3)
Exposure Ratio × Post 2010	-0.001	0.000	-0.003
	(0.003)	(0.003)	(0.018)
Control mean	0.0345	0.0421	0.260
Control SD	0.183	0.201	1.286
Observations	138317	138317	138317

Notes: The outcome variables are taken from NSS employment rounds 64, 66, and 68. Columns (1), (2), and (3) represent labor force participation according to principal activity status in the NSSO, labor force participation based on daily status, and the number of days per week engaged in the labor force from daily status, respectively. The analysis is limited to children aged 6 to 16, who are within the school-going age group. All columns include control variables such as the linear distance from the district centroid to AP, the rural population and its square in 2010, dummy variables for GLP quintiles in 2008 and 2010, the average rural per capita consumption and casual daily wage in 2010 and the total number of rural schools in the district, all interacted with the round variable. The model further incorporates household-level controls, including social group, religion, household type, household head's education, number of children, and quintiles of household size. Individual-level controls include age and sex. The model also accounts for the survey month, round, and district fixed effects. Standard errors are clustered at the district level, and observations are weighted by NSS sampling weights. The symbols *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table A.3: Effects of Exposure to the AP Regulation on School Enrollment

	Enrolled in School (1)	Enrolled in Government School (2)	Enrolled in Private School (3)
Exposure Ratio × Post 2010	0.006***	0.019***	-0.009**
	(0.002)	(0.005)	(0.004)
Control mean	0.942	0.626	0.374
Control SD	0.234	0.484	0.484
Observations	3345171	3122255	3122255

Notes: The outcome variables are taken from various rounds of ASER conducted in the years 2006 through 2018. In columns (2) and (3), we examine government and private school enrollment, conditional on being enrolled in a school. All columns include control variables such as the linear distance from the district centroid to AP, the rural population and its square in 2010, dummy variables for GLP quintiles in 2008 and 2010, and the average rural per capita consumption and casual daily wage in 2010, all interacted with the round variable. Additionally, we control for the total number of rural schools, rural government schools, and rural private schools in 2009-10, each interacted with the round variable in columns (1), (2), and (3), respectively. The model further includes controls for age, sex and quintiles of household size. It also accounts for round and district fixed effects. Standard errors clustered at the district level, and observations are weighted by ASER sampling weights. The symbols *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table A.4: Effects of Exposure to the AP Regulation on Educational and Food Expenditures (Winsorized)

	HH Monthly Education Expenditure (1)	HH Monthly Food Expenditure (2)
Exposure Ratio × Post 2010	-31.541***	-64.087***
	(7.041)	(24.483)
Control mean	264.2	2683.5
Control SD	523.9	1349.9
Observations	64616	68060

Notes: The outcome variables are taken from the NSS employment rounds 64, 66, and 68, and are winsorized at the 99th percentile. Both columns include control variables such as the linear distance from the district centroid to AP, the rural population and its square in 2010, dummy variables for GLP quintiles in 2008 and 2010, and the average rural per capita consumption and casual daily wage in 2010, all interacted with the round variable. The model also incorporates household-level controls, including social group, religion, household type, household head's education, number of children, proportion of female children, and quintiles of household size. Additionally, it accounts for survey month, round and district fixed effects. Standard errors are clustered at the district level, and observations are weighted by NSS sampling weights. The symbols *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table A.5: Heterogeneity by Gender (with HH Fixed Effects)

	Maths score std. (1)	Reading score std. (2)
Exposure Ratio × Post 2010 × Girl	-0.002	-0.010***
	(0.004)	(0.004)
Control mean	-0.000108	-0.0190
Control SD	0.991	1.013
Observations	2391912	2401904

Notes: The outcome variables are taken from various rounds of ASER conducted in the years 2006 through 2018. The model includes controls for the linear distance from the district centroid to AP, the rural population and its square in 2010, dummy variables for GLP quintiles in 2008 and 2010, average rural per capita consumption and casual daily wage in 2010, and the number of rural schools in 2010, all interacted with the round variable. The model further includes controls for sex and quintiles of household size. It also accounts for household, round, and district fixed effects. Standard errors clustered at the district level, and observations are weighted by ASER sampling weights. The symbols *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table A.6: Heterogeneity by Exposure Intensity

	Maths score std. (1)	Reading score std. (2)
Exposure Quantile 1 × Post 2010	-0.117***	-0.083**
	(0.043)	(0.035)
Exposure Quantile 2 × Post 2010	-0.136***	-0.089***
	(0.041)	(0.032)
Exposure Quantile 3 × Post 2010	-0.194***	-0.130***
	(0.039)	(0.033)
Exposure Quantile 4 × Post 2010	-0.384***	-0.252***
	(0.068)	(0.050)
Control mean	-0.0414	-0.00961
Control SD	1.006	1.025
Observations	3058204	3067727

Notes: The outcome variables are taken from various rounds of ASER conducted in the years 2006 through 2018. In this analysis, the exposed districts are categorized into four quantiles based on their exposure intensity, with Quantile 1 representing the lowest level of exposure and Qunatile 4 representing the highest. The model includes controls for the linear distance from the district centroid to AP, the rural population and its square in 2010, dummy variables for GLP quintiles in 2008 and 2010, average rural per capita consumption and casual daily wage in 2010, and the number of rural schools in 2010, all interacted with the round variable. The model further includes controls for sex and quintiles of household size. It also accounts for round and district fixed effects. Standard errors clustered at the district level, and observations are weighted by ASER sampling weights. The symbols *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table A.7: Robustness Check: Excluding One State at a Time

	BH (1)	CG (2)	GJ (3)	HR (4)	JH (5)	KA (6)	KL (7)	MP (8)	MH (9)	OD (10)	PB (11)	RJ (12)	TN (13)	UT (14)	UP (15)	WB (16)
Maths Score																
Exposure Ratio × Post 2010	-0.092***	-0.083***	-0.082***	-0.081***	-0.083***	-0.061***	-0.081***	-0.067***	-0.065***	-0.090***	-0.081***	-0.094***	-0.071***	-0.081***	-0.068***	-0.088***
	(0.017)	(0.014)	(0.014)	(0.013)	(0.014)	(0.013)	(0.014)	(0.012)	(0.014)	(0.014)	(0.013)	(0.014)	(0.014)	(0.014)	(0.016)	(0.013)
Control mean	-0.044	-0.042	-0.035	-0.064	-0.042	-0.037	-0.081	-0.038	-0.044	-0.042	-0.041	-0.046	-0.069	-0.041	0.094	-0.051
Control SD	1.005	1.007	1.007	1.002	1.006	1.016	1.010	1.006	1.011	1.007	1.006	1.005	1.030	1.006	0.939	1.001
Observations	2695298	2967123	2925869	2935130	2925549	2836747	2991045	2796210	2825900	2847272	3030079	2800920	2872282	3009354	2504204	2950693
Reading Score																
Exposure Ratio × Post 2010	-0.052***	-0.056***	-0.051***	-0.053***	-0.055***	-0.046***	-0.053***	-0.042***	-0.040***	-0.060***	-0.053***	-0.065***	-0.045***	-0.053***	-0.051***	-0.056***
	(0.016)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.010)	(0.011)	(0.012)	(0.011)	(0.011)	(0.012)	(0.011)	(0.013)	(0.011)
Control mean	-0.011	-0.011	-0.011	-0.026	-0.010	-0.001	-0.046	-0.007	-0.022	-0.012	-0.010	-0.014	-0.021	-0.010	0.120	-0.023
Control SD	1.025	1.026	1.027	1.027	1.025	1.032	1.034	1.026	1.031	1.026	1.025	1.026	1.052	1.025	0.936	1.021
Observations	2703342	2976339	2934850	2944077	2934415	2845869	2999914	2805193	2835046	2856161	3039442	2809332	2881327	3018693	2513133	2959661

Notes: The outcome variables are taken from various rounds of ASER conducted in the years 2006 through 2018. The first panel presents results on maths score, while the second panel displays results on reading score. Each column excludes observations from the state indicated at the top of the column. All columns include control variables such as the linear distance from the district centroid to AP, rural population and its square in 2010, dummy variables for GLP quintiles in 2008 and 2010, average rural per capita consumption and casual daily wage in 2010, and the count of rural schools in 2010, all interacted with the round variable. Additionally, the model includes controls for age, sex and quintiles of household size. It also accounts for round and district fixed effects. Standard errors clustered at the district level, and observations are weighted by ASER sampling weights. The symbols *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table A.8: Robustness Check: Unweighted Regressions

	Maths score std. (1)	Reading score std. (2)
Panel A - (DiD Model)		
Exposure Ratio × Post 2010	-0.080***	-0.049***
	(0.014)	(0.011)
Panel B - (Event Study Model)		
Exposure Ratio × 2006	-0.017	0.006
	(0.022)	(0.019)
Exposure Ratio × 2007	-0.014	0.008
	(0.021)	(0.018)
Exposure Ratio × 2008	-0.004	0.013
	(0.015)	(0.012)
Exposure Ratio × 2010	-0.025	-0.004
	(0.018)	(0.016)
Exposure Ratio × 2011	-0.088***	-0.045***
	(0.019)	(0.016)
Exposure Ratio × 2012	-0.083***	-0.033**
	(0.020)	(0.016)
Exposure Ratio × 2013	-0.121***	-0.062***
	(0.020)	(0.015)
Exposure Ratio × 2014	-0.124***	-0.057***
	(0.020)	(0.016)
Exposure Ratio × 2016	-0.091***	-0.042***
	(0.021)	(0.015)
Exposure Ratio × 2018	-0.115***	-0.064***
	(0.021)	(0.017)
Control mean	-0.0122	0.00522
Control SD	1.000	1.012
Observations	3058204	3067727

Notes: The outcome variables are taken from various rounds of ASER conducted in the years 2006 through 2018. Panel A displays coefficients from regressions using a difference-in-differences specification, while Panel B presents coefficients from separate regressions using an event study specification (Year 2009 serves as the omitted category). Age-wise standardization for both maths and reading scores was applied within each year. Both columns include control variables such as the linear distance from the district centroid to AP, rural population and its square in 2010, dummy variables for GLP quintiles in 2008 and 2010, average rural per capita consumption and casual daily wage in 2010, and the count of rural schools in 2010, all interacted with the round variable. Additionally, the model includes controls for sex and quintiles of household size. It also accounts for round and district fixed effects, and standard errors clustered at the district level. The symbols *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table A.9: Robustness Check: Unweighted Regressions for Mechanism Analysis

	Enrolled in School (1)	Enrolled in Government School (2)	Enrolled in Private School (3)	HH Monthly Education Expenditure (4)	School Fees (5)
Exposure Ratio × Post 2010	0.011**	0.026***	-0.023***	-36.500***	-338.120***
	(0.004)	(0.005)	(0.005)	(8.079)	(70.489)
Control mean	0.843	0.748	0.248	391.9	2799.1
Control SD	0.363	0.434	0.432	971.2	9633.4
Observations	121708	98405	98405	64616	52408

Notes: The outcome variables are taken from various rounds of NSS surveys, 64, 66, and 68. In all columns, controls include the linear distance from the district centroid to AP, the rural population and its square in 2010, dummy variables for GLP quintiles in 2008 and 2010, and the average rural per capita consumption and casual daily wage in 2010, all interacted with the round variable. Additionally, we control for the district level total number of rural schools, rural government schools, and rural private schools in 2009-10, each interacted with the round variable in columns (1), (2), and (3), respectively. The model also incorporates household-level controls, including social group, religion, household type, household head's education, number of children, and quintiles of household size. Columns (1), (2), and (3) incorporate individual-level controls such as age and gender. Additionally, it accounts for survey month, round, and district fixed effects. Standard errors are clustered at the district level. The symbols *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table A.10: Robustness Check: Learning Outcomes Without Standardization

	Maths score (1)	Reading score (2)
Panel A - (DiD Model)		
Exposure Ratio × Post 2010	-0.081***	-0.058***
	(0.014)	(0.013)
Panel B - (Event Study Model)		
Exposure Ratio × 2006	-0.028	0.012
	(0.024)	(0.022)
Exposure Ratio × 2007	-0.011	0.017
	(0.026)	(0.024)
Exposure Ratio × 2008	0.002	0.016
	(0.017)	(0.014)
Exposure Ratio × 2010	-0.021	0.001
	(0.023)	(0.020)
Exposure Ratio × 2011	-0.095***	-0.051***
	(0.019)	(0.018)
Exposure Ratio × 2012	-0.081***	-0.033*
	(0.021)	(0.018)
Exposure Ratio × 2013	-0.129***	-0.075***
	(0.022)	(0.017)
Exposure Ratio × 2014	-0.120***	-0.059***
	(0.024)	(0.020)
Exposure Ratio × 2016	-0.089***	-0.049***
	(0.024)	(0.018)
Exposure Ratio × 2018	-0.119***	-0.072***
	(0.022)	(0.020)
Control mean	3.404	3.714
Control SD	1.265	1.427
Observations	3058204	3067727

Notes: The outcome variables are taken from various rounds of ASER conducted in the years 2006 through 2018. Panel A displays coefficients from regressions using a difference-in-differences specification, while Panel B presents coefficients from separate regressions using an event study specification (Year 2009 serves as the omitted category). Both columns include control variables such as the linear distance from the district centroid to AP, rural population and its square in 2010, dummy variables for GLP quintiles in 2008 and 2010, average rural per capita consumption and casual daily wage in 2010, and the count of rural schools in 2010, all interacted with the round variable. Additionally, the model includes controls for age, sex and quintiles of household size. It also accounts for round and district fixed effects. Standard errors clustered at the district level, and observations are weighted by ASER sampling weights. The symbols *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table A.11: Robustness Check: Exposure Dummy

	Maths score std. (1)	Reading score std. (2)	Maths score (3)	Reading score (4)
Panel A - (DiD Model)				
Any exposed lender × Post 2010	-0.170***	-0.115***	-0.173***	-0.129***
	(0.029)	(0.023)	(0.030)	(0.027)
Panel B - (Event Study Model)				
Any exposed lender× 2006	-0.038	0.016	-0.054	0.029
	(0.047)	(0.041)	(0.049)	(0.046)
Any exposed lender× 2007	-0.017	0.023	-0.018	0.021
	(0.050)	(0.045)	(0.054)	(0.050)
Any exposed lender× 2008	0.028	0.066**	0.030	0.072**
	(0.032)	(0.026)	(0.033)	(0.028)
Any exposed lender× 2010	-0.044	-0.010	-0.047	-0.014
	(0.045)	(0.039)	(0.046)	(0.042)
Any exposed lender× 2011	-0.194***	-0.106***	-0.199***	-0.116***
	(0.039)	(0.036)	(0.041)	(0.040)
Any exposed lender× 2012	-0.172***	-0.076**	-0.177***	-0.084**
	(0.044)	(0.035)	(0.047)	(0.039)
Any exposed lender× 2013	-0.239***	-0.123***	-0.249***	-0.138***
	(0.046)	(0.034)	(0.048)	(0.038)
Any exposed lender× 2014	-0.228***	-0.107***	-0.235***	-0.115***
	(0.047)	(0.038)	(0.050)	(0.042)
Any exposed lender× 2016	-0.180***	-0.095***	-0.186***	-0.105***
	(0.047)	(0.034)	(0.050)	(0.037)
Any exposed lender× 2018	-0.225***	-0.120***	-0.232***	-0.134***
	(0.045)	(0.038)	(0.047)	(0.042)
Control mean	-0.0414	-0.00961	3.404	3.714
Control SD	1.006	1.025	1.265	1.427
Observations	3058204	3067727	3058204	3067727

Notes: The outcome variables are taken from various rounds of ASER conducted in the years 2006 through 2018. Panel A displays coefficients from regressions using a difference-in-differences specification, while Panel B presents coefficients from separate regressions using an event study specification (Year 2009 serves as the omitted category). In the first two columns, learning outcomes have been standardized, whereas in the last two columns, they are presented in their original form. All columns include control variables such as the linear distance from the district centroid to AP, rural population and its square in 2010, dummy variables for GLP quintiles in 2008 and 2010, average rural per capita consumption and casual daily wage in 2010, and the count of rural schools in 2010, all interacted with the round variable. Additionally, the model includes controls for sex and quintiles of household size. It also accounts for round and district fixed effects. Standard errors clustered at the district level, and observations are weighted by ASER sampling weights. The symbols *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.

Table A.12: Robustness Check: Restricting ASER Data to Rounds from 2006 to 2014

	Maths score std. (1)	Reading score std. (2)	Maths score (3)	Reading score (4)
Panel A - (DiD Model)				
Exposure Ratio × Post 2010	-0.076***	-0.048***	-0.076***	-0.053***
	(0.013)	(0.011)	(0.013)	(0.012)
Panel B - (Event Study Model)				
Exposure Ratio × 2006	-0.018	0.006	-0.027	0.013
	(0.023)	(0.020)	(0.024)	(0.022)
Exposure Ratio × 2007	-0.010	0.017	-0.010	0.018
	(0.024)	(0.022)	(0.026)	(0.024)
Exposure Ratio × 2008	0.001	0.015	0.002	0.017
	(0.016)	(0.013)	(0.017)	(0.014)
Exposure Ratio × 2010	-0.020	0.002	-0.021	0.002
	(0.022)	(0.019)	(0.023)	(0.020)
Exposure Ratio × 2011	-0.092***	-0.047***	-0.094***	-0.050***
	(0.018)	(0.016)	(0.019)	(0.018)
Exposure Ratio × 2012	-0.080***	-0.032*	-0.081***	-0.033*
	(0.020)	(0.016)	(0.021)	(0.018)
Exposure Ratio × 2013	-0.124***	-0.067***	-0.129***	-0.074***
	(0.021)	(0.016)	(0.022)	(0.017)
Exposure Ratio × 2014	-0.115***	-0.055***	-0.120***	-0.059***
	(0.023)	(0.018)	(0.024)	(0.020)
Control mean	-0.0461	-0.0153	3.431	3.723
Control SD	1.008	1.027	1.274	1.418
Observations	2627707	2636381	2627707	2636381

Notes: The outcome variables are taken from various rounds of ASER conducted in the years 2006 through 2014. Panel A displays coefficients from regressions using a difference-in-differences specification, while Panel B presents coefficients from separate regressions using an event study specification (Year 2009 serves as the omitted category). In the first two columns, learning outcomes have been standardized, whereas in the last two columns, they are presented in their original form. All columns include control variables such as the linear distance from the district centroid to AP, rural population and its square in 2010, dummy variables for GLP quintiles in 2008 and 2010, average rural per capita consumption and casual daily wage in 2010, and the count of rural schools in 2010, all interacted with the round variable. Additionally, the model includes controls for sex and quintiles of household size. It also accounts for round and district fixed effects. Standard errors clustered at the district level, and observations are weighted by ASER sampling weights. The symbols *, **, and *** indicate significance at the 10%, 5%, and 1% levels, respectively.