Essays on Labor Market Reforms and Health Economics

By

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Dedication

Dedicated To God,

To my husband, Sossou Simplice Adjisse, for your unwavering support and love.

To the TOSSOU family — Alain, Claudine, Armand, Marcel, Hospice, and Estelle — for your constant encouragement.

And to our wonderful children, whose joy and laughter inspire me every day.

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Summary

This dissertation consists of two chapters that examine how policy reforms shape labor market outcomes and child well-being, with a focus on institutional settings common in low-income countries.

The first chapter investigates the effects of Benin's 2017 labor market reform, which reduced dismissal costs and allowed the indefinite renewal of short-term contracts. Using harmonized labor force survey data and a two-way fixed effects strategy with neighboring West African countries as controls, the analysis estimates the reform's impact on employment structure, contract type, job tenure, and wages. The results show that the reform increased formal sector employment by 2.6 percentage points and reduced informal employment by 2.8 percentage points, representing a meaningful reallocation of labor in a context of high informality. It also led to greater use of permanent contracts, higher formal sector wages, and modest reductions in job tenure, particularly for short-term contract workers. These findings suggest that reducing firing costs can expand access to formal employment and raise earnings, even in the absence of net job creation.

The second chapter, coauthored with Lawrence Berger, Christine Durrance, and Jessica Pac, examines the effects of in-person schooling during the COVID-19 pandemic on child safety in Wisconsin. Using linked administrative data on child protective services (CPS) reports and Medicaid claims, the study employs event study and difference-in-differences models to estimate the impact of school reopenings. Results show that CPS reports, particularly from educators, increased following the return to in-person instruction, and diagnoses of child injuries rose as well. These findings reflect both enhanced surveillance and potential changes in underlying risk exposure.

Together, these chapters provide new evidence on how policy interventions influence economic and social outcomes in settings with limited institutional capacity. The results have implications for labor market regulation and child protection policy in both developing and developed contexts.

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Chapter 1

Labor Market Reforms, Flexibility, and Employment Transitions Across Formal and Informal Sectors

Selidji Caroline Tossou

1.1 Abstract

This paper investigates the 2017 labor market reform in Benin, which reduced firing costs and allowed firms to renew short-term contracts indefinitely. Using micro-data from the Harmonized Surveys on Households Living Standards and a two-way fixed effects approach with nearby countries as the control group, I assess the reform's impact on employment, worker tenure, contract types, and wages. The results reveal a 2.6 percentage point (24.5 percent) increase in formal sector employment and a 2.8 percentage point (3.2 percent) reduction in informal employment, indicating substantial labor market reallocation in a context of pervasive informality. Formal employment growth is particularly valuable in developing economies, where formal jobs offer stronger legal protections, access to social security, and expand the government's tax base. Tenure declined by 0.23 months for short-term contract workers, consistent with higher turnover, while it increased by 0.15 months for long-term contract holders. The likelihood of securing a permanent contract rose by 23.2

percentage points (41.6 percent) in the formal sector, suggesting that firms used long-term contracts to retain high-productivity workers. Wages in the formal sector increased by 33.6 USD per month on average, with gains observed for both short- and long-term contract holders. Taken together, these findings suggest that lower firing costs encouraged firms to expand formal employment and screen workers more actively, using short-term contracts for initial hires and upgrading high-performing workers. The study offers new evidence on the trade-offs between flexibility, job stability, and wages in a developing country context.

1.2 Author Disclosures

Short title: Labor Market Reforms and Employment Dynamics

Conflict of Interest Disclosures: The author reports no conflicts of interest.

Abbreviations: LSMS = Living Standards Measurement Study; USD = United States Dollar; ILO = International Labour Organization; GDP = Gross Domestic Product; DID = Difference-in-Differences.

Article Summary: This chapter evaluates the effects of a major 2017 labor market reform in Benin that reduced firing costs and increased contract flexibility. Using household survey data and quasi-experimental methods, the study estimates impacts on formal and informal sector employment, contract types, worker tenure, and wages.

What this Study Adds: The analysis documents significant labor market reallocation effects following the reform, particularly among short-term contract workers. It highlights how reduced dismissal costs affected firms' contract choices and worker retention, providing policy-relevant insights into employment flexibility and job quality in a developing economy context.

1.3 Contributors Statement

Selidji Tossou is the sole author of this chapter. She conducted the research, performed all empirical analyses and modeling, and wrote the manuscript. She is responsible for all content and analysis in the final version.

1.4 Introduction

Labor market regulations play a critical role in shaping employment outcomes, especially in developing economies where informality is widespread. In Benin, eight in ten individuals in the workforce operate in the informal sector. Informal employment accounts for an estimated 60 to 70 percent of GDP and is a key driver of economic activity (World Bank, 2023a; AfDB, 2022; ILO, 2017). By contrast, the formal sector is relatively small and heavily regulated, with high firing costs and rigid contract rules limiting firms' flexibility. Given that 36.2 percent of the population lives below the poverty line and nearly three-quarters of the workforce is underemployed, it is essential to understand how labor market reforms influence both formal and informal employment (INStaD, 2022; World Bank, 2023a).

In 2017, Benin implemented a labor market reform aimed at increasing flexibility. The reform reduced dismissal costs, particularly for long-term contracts, and eliminated the 48-month cap on the use of short-term contracts (STCs), allowing firms to renew them indefinitely. STCs offer firms greater flexibility and involve lower severance costs, while long-term contracts provide workers with stability, benefits, and legal protections. Before the reform, strict regulations limited firms' willingness to hire and adjust their workforce. By easing these constraints, the government aimed to encourage formal job creation. However, such reforms may also increase reliance on temporary arrangements and raise concerns about job stability and worker turnover. Understanding these trade-offs is central to evaluating the reform's effects.

This paper analyzes the reform's impact on employment, contract types, job tenure, and wages using a two-way fixed effects strategy. I use nationally representative data from the Harmonized Surveys on Household Living Standards and compare outcomes in Benin before and after the reform to those in similar West African countries that did not adopt comparable changes. I first establish balance on key covariates and show parallel trends in employment outcomes to validate the identification strategy. The analysis provides new evidence on how easing dismissal regulations affects labor markets in low-income countries where informality is prevalent.

The findings show that the reform resulted in substantial reallocation from informal to

formal employment. Formal sector employment increased by 2.6 percentage points (a 24.5 percent rise), while informal employment declined by 2.8 percentage points (a 3.2 percent reduction). Although total employment remained largely unchanged, the shift toward formality is economically and institutionally meaningful. Formal jobs tend to offer greater income security, access to social protection, and legal enforcement. For governments, a larger formal sector strengthens the tax base and improves policy enforcement. Even modest growth in formality can generate long-term gains if sustained.

The reform also altered the composition of employment contracts. The share of formal sector workers with long-term contracts rose by 23.2 percentage points (a 41.6 percent increase), and the overall share of permanent contracts increased by 4.6 percentage points (an 88.4 percent increase). These shifts were particularly large for women, rural residents, and unmarried individuals. This pattern suggests that firms used STCs to screen workers and upgraded productive employees to permanent roles, leveraging flexibility without fully abandoning employment stability.

Job tenure declined slightly after the reform, especially for STC workers, whose average tenure dropped by 0.23 months. Tenure among long-term contract workers increased by 0.15 months. These changes reflect both the influx of new hires with limited job histories and the greater ability of firms to dismiss underperforming workers. Although shorter tenure is often viewed as a negative outcome, higher turnover can also facilitate better worker-firm matching, skill acquisition, and income continuity through frequent reentry. In environments with limited unemployment insurance, formal job turnover may be preferable to unemployment or informality.

Wages in the formal sector rose following the reform. On average, monthly earnings increased by 33.6 USD, with gains observed for both short- and long-term contract holders. These increases likely reflect greater demand for productive workers and firms' efforts to retain talent. The wage differentials between contract types suggest that firms adjusted compensation in line with worker value and contract duration.

Overall, the results indicate that reducing firing costs encouraged firms to expand formal hiring, use STCs for initial screening, and selectively offer permanent contracts with better pay. These adjustments improved labor market efficiency without undermining the quality

of jobs. The findings contribute to a growing literature on employment protection reforms, including evidence from Latin America and sub-Saharan Africa. In contrast to studies where flexibility reforms had little effect on informality (e.g., Maloney, 2004), the Benin case shows that well-targeted reforms can produce meaningful shifts toward formality in highly informal settings.

In sum, the 2017 reform increased formal employment, expanded access to long-term contracts, and raised wages, while also increasing job turnover and reducing tenure for some workers. Complementary programs such as retraining or unemployment support could help mitigate risks for vulnerable groups. This paper provides new empirical insights into labor reform in a low-income country and offers policy lessons for similar contexts seeking to promote formal employment growth.

The remainder of the paper is organized as follows: Section 1.5 reviews related research. Section 1.6 outlines the 2017 reform in Benin. Section 1.7 describes the data and empirical strategy. Section 1.8 presents the findings. Section 1.9 interprets the results. Section 1.10 concludes.

1.5 Review of the Literature

The theory of job matching and worker turnover, first formalized by Jovanovic (1979), has long provided a foundational lens through which to understand labor market dynamics. This framework has inspired extensive research on how employment protection legislation affects job mobility, employment stability, and firm behavior. Much of this work has focused on high-income countries, especially in Europe, where employment regulations are stricter (Hijzen et al., 2013; Feldmann, 2009; Kahn, 2012; Noelke and Beckfield, 2017). These studies often find that stringent job security rules reduce both turnover and employment levels.

Reforms that reduce hiring and firing costs have generated mixed results across contexts. Bertola (1992) provides a theoretical model in which firing costs may increase or decrease employment depending on firm-specific discount rates and attrition. Empirically, Kugler (1999) finds that lower dismissal costs in Colombia increased transitions into and out of employment, especially in the formal sector. In North America, where labor markets are

more flexible, lower severance obligations have been associated with lower unemployment rates and faster adjustment (Aguirregabiria and Alonso-Borrego, 2014).

The literature has also addressed how regulatory reforms affect transitions into formal employment. Bruhn (2011) examines a business registration reform in Mexico and finds that simplifying registration increased firm creation and formalization. Similarly, De Mel et al. (2013) evaluate a formalization support program in Sri Lanka and show that while take-up was modest, formalization had positive effects on firms' perceptions and access to services. However, several studies caution that lowering administrative barriers alone may not be sufficient to induce widespread transitions into formality, particularly where enforcement is weak or labor protections remain costly.

A comprehensive review by Bruhn and McKenzie (2014) highlights that formalization decisions are shaped by a combination of regulatory, financial, and informational constraints. Their review emphasizes that entry reforms can promote formalization under the right institutional conditions, but that impacts vary significantly across countries and sectors. Importantly, most of this literature focuses on firm-level reforms rather than broader labor regulation changes affecting employer–employee relationships.

In sub-Saharan Africa, the evidence base remains limited. A few studies, such as those by the IMF and the World Bank, suggest that easing labor restrictions could reduce unemployment and increase formal job creation (Duval et al., 2021; World Bank, 2023b). However, informal employment dominates the region and enforcement capacity is limited, so the effects of reform may differ substantially from those in middle- and high-income countries.

This paper contributes to the literature by examining a large-scale labor market reform in Benin that reduced firing costs and removed restrictions on the use of short-term contracts. Unlike interventions focused narrowly on registration or tax compliance, this reform altered the employment relationship itself. The analysis provides new evidence on how such changes affect employment patterns, contract types, job duration, and wages in a labor market where informality is pervasive. In contrast to much of the Latin American evidence, which finds limited reductions in informality following flexibility reforms (Maloney, 2004; Levy, 2010), the results from Benin suggest that well-targeted employment regulation changes can induce meaningful labor market reallocation even in low-capacity environments.

1.6 Background

In Benin, major changes to labor market regulations were introduced in 2017, fundamentally altering the rules governing employment contracts and dismissal processes in the formal sector. The reform marked a significant policy shift by reducing severance pay for workers under long-term contracts, extending the legal use of short-term contracts (STCs), lowering compensation for unfair dismissal, and broadening the grounds for both individual and economic dismissals. These changes sharply reduced the firing costs faced by firms and marked a departure from the previous regulatory framework established in 1998. By redefining the terms of the employer-employee relationship, the reform aimed to increase labor market flexibility and promote private sector employment growth.

The formal sector in Benin is regulated and taxed, with explicit employment contracts and regular wages. Firms operating in this sector benefit from legal protections, access to formal credit, and eligibility for public procurement. These advantages, along with the potential for long-term productivity gains, offer strong incentives for formalization. Formal employment also allows firms to attract and retain skilled workers, who may value the stability, social security benefits, and legal protections that accompany long-term contracts. From a policy perspective, expanding formal employment has broader benefits. It increases tax revenues, improves compliance with labor and safety standards, and strengthens state capacity. The government explicitly aimed to promote formalization through the 2017 reform by lowering the costs associated with hiring and managing formal workers, thereby making formal jobs more accessible and sustainable.

By contrast, the informal sector, made up largely of small firms and self-employment, is not subject to institutional regulation. Informality is particularly high across Sub-Saharan Africa, and Benin is no exception. According to 2009 estimates from the National Statistics Agency, the informal sector accounted for as much as 70 percent of GDP. The prevalence of informality poses challenges for development. Governments struggle to collect taxes and fund services, and informal enterprises face limited access to finance, public programs, or legal recourse. They may also be more vulnerable to corruption and enforcement pressure. Formal firms, in turn, face unfair competition from informal operators and often bear a

disproportionate tax burden (Bank, 2015).

Benin's 2017 reform builds upon broader economic liberalization efforts that began in the early 1990s, supported by the World Bank and the International Monetary Fund. Earlier reforms helped restore macroeconomic stability and encouraged private investment. The 2017 labor law, known as Law 2017-05 of August 29, replaced the previous framework established under Law 98-004 of January 27, 1998, and introduced new provisions governing employment contracts and terminations. Key elements included lower compensation for unfair dismissal, elimination of the 48-month cap on STC renewals, and a revised severance structure.

The extension of STCs was among the most consequential changes. Under the previous legal framework, STCs could be renewed for up to four years, after which firms had to either convert the contract to long-term status or terminate the employment. The 2017 reform removed this cap, allowing indefinite renewals. This change effectively allowed firms to use temporary contracts as a long-term staffing tool. Because STCs do not require severance pay upon termination, firms gained greater flexibility to adjust their workforce based on demand. Employers are still required to provide two months' notice to avoid penalties. This is expected to increase turnover in the formal sector, especially for workers on STCs.

The reform also revised severance pay rules for long-term contract (LTC) workers. Previously, these workers were entitled to one month of severance per year of service, regardless of tenure. After the reform, severance became conditional on a minimum of 24 months of service and was scaled based on both tenure and salary, reducing the average cost of dismissals for firms.

Additionally, compensation for unfair dismissal was reduced. Under the earlier law, workers could receive between 6 and 12 months of gross salary as compensation. The new law lowered this to between 3 and 9 months, and STC workers were excluded from eligibility altogether. Workers with less than one year of service also became ineligible for unfair dismissal compensation.

These reforms mirror similar flexibility-enhancing changes adopted in other countries. For example, Spain introduced legislation expanding the use of fixed-term contracts and reducing severance obligations. Evidence from Spain suggests that such reforms led to greater turnover, a decline in long-term job relationships, and reduced on-the-job training, though

they also helped lower long-term unemployment.[1]

In sum, the 2017 labor reform in Benin represented a structural shift intended to modernize the labor market, reduce regulatory barriers to formal hiring, and create an environment more conducive to private sector growth. By lowering the costs of formal employment, the reform sought to make it more attractive for firms to hire formally, with potential long-term benefits for job quality, productivity, and state capacity.

1.7 Identification Strategy

1.7.1 Two-Way Fixed Effect Method

I exploit the timing of the 2017 labor reform in Benin and cross-country variation in labor regulations across West African countries using a two-way fixed effects model. In Benin, all workers in the formal sector are subject to the reform, while informal workers are not directly covered. However, I expect equilibrium effects in the labor market to affect informal workers as well, making them unsuitable as a control group. I instead use a set of comparable West African countries that did not undergo similar reforms as the control group to estimate the impact of reduced firing costs on key labor market outcomes.

Let i index individuals, with treatment defined as residing in Benin after the implementation of the reform. The treatment variable $T_{c(i)t(i)}$ equals 1 for individuals in Benin in post-reform years, and 0 otherwise:

$$T_{c(i)t(i)} = \begin{cases} 1, & \text{if } c(i) = \text{Benin and } t(i) \geq 2017 \\ 0, & \text{otherwise} \end{cases}$$

The main specification estimates the following two-way fixed effects model:

$$Y_i = \beta_0 + \beta_1 T_{c(i)t(i)} + \gamma_{c(i)} + \rho_{t(i)} + X_i' \alpha + \mu_i \tag{1.1}$$

[1] In Spain, labor reforms allowing for more flexible temporary contracts with lower severance pay led to a marked increase in temporary work. This expansion of temporary contracts decreased long-term unemployment but also contributed to reduced labor productivity, lower levels of on-the-job training, and increased wage inequality (Dolado et al., 2002; Toharia and Jimeno, 1993). This evidence suggests potential parallels for Benin, where reforms aimed at increasing flexibility may have similar effects on turnover and productivity.

where Y_i is the outcome of interest, $\gamma_{c(i)}$ and $\rho_{t(i)}$ are country and year fixed effects, X_i is a vector of individual-level controls, and μ_i is an error term. The coefficient β_1 captures the average treatment effect of the reform on Y_i.

1.7.2 Event Study Specification

To explore dynamic treatment effects and validate the parallel trends assumption, I estimate an event study specification:

$$Y_{it} = \alpha + \sum_{k \neq -1} \delta_k \mathbb{1}(t = k) \times \text{Benin}_i + \gamma_{c(i)} + \rho_{t(i)} + X_i' \theta + \epsilon_{it} \tag{1.2}$$

Here, δ_k denotes event-time coefficients relative to the reform year (with $k = -1$ as the omitted period). The interaction $\mathbb{1}(t = k) \times \text{Benin}_i$ captures the policy effect in each event year for individuals in Benin. This approach permits visual inspection of trends and temporal variation in the reform's effects.

1.7.3 Controls and Estimation Details

All regressions apply household sampling weights to ensure national representativeness. I control for urban residence, age, gender, education, household size, marital status, poverty status, and employment industry. Fixed effects for religion and commune account for cultural and geographic heterogeneity. Standard errors are clustered at the household level to account for within-household correlation.

1.7.4 Data

I use data from the Harmonized Surveys on Households Living Standards, part of the World Bank's Living Standards Measurement Study (LSMS) initiative. Conducted in 2016 and 2019, these surveys are nationally representative and cover Benin and several other West African countries. The surveys provide rich information on employment, income, and socio-demographic variables.

Although the data are cross-sectional, they were collected over multiple months in each

wave, with consistent fieldwork timing across geographic areas. I use the month of interview to construct a monthly event-time variable.[2] This monthly structure enables estimation of dynamic policy effects, despite the absence of panel data.

The analysis sample includes individuals aged 15 to 64 who were either employed or actively seeking work. Full-time students, retirees, and others outside the labor force are excluded. Outcomes are compared across repeated cross-sections rather than tracked at the individual level.

The LSMS data make it possible to distinguish between formal and informal employment. Workers are classified as formally employed if their employer contributes to the social security system, which is legally required in the formal sector. Informal workers do not receive such contributions. This definition closely mirrors the legal criteria for formality in Benin and reflects meaningful differences in job quality, benefits, and state oversight. Transitions into formality are therefore policy-relevant and often associated with improved job conditions, access to credit, and higher tax compliance.

Additional demographic variables such as age, gender, marital status, religion, education, and household size are used as controls in all regressions. Employment variables are measured with respect to the respondent's primary job, defined either as the job with a formal contract or, in the absence of any contract, the job generating the highest income.

Employment status is a binary variable equal to 1 if the individual was employed at the time of interview. Contract type distinguishes short-term from long-term agreements based on reported duration. Tenure measures how long a worker remained in their current job over the past 12 months, capped at 12. For unemployed individuals, non-employment duration captures the number of years since the last job ended. Monthly earnings are reported in local currency and converted to U.S. dollars.

The comparison group includes seven West African countries participating in the WAEMU LSMS harmonization project: Burkina Faso, Côte d'Ivoire, Guinea Bissau, Mali, Niger, Senegal, and Togo (see Figure A2). These countries share common monetary institutions, labor

[2]In figures and event study analyses, month –6 corresponds to January 2016, –5 to February 2016, and so on. Month 0 represents January 2019, and subsequent months follow sequentially. Note that there is a discontinuity between period –1 and period 0: the data come from two separate cross-sectional survey waves conducted in 2016 and 2019. As a result, the time index does not reflect consecutive calendar months across this gap.

market structures, and cultural features with Benin. Their inclusion provides a credible counterfactual for identifying the reform's effects. Importantly, these are the only countries available in the harmonized dataset, which ensures consistent variable definitions and survey implementation.

Table A1 summarizes demographic characteristics for Benin and the comparison countries in 2016. Beninese respondents averaged 34.5 years of age; 47 percent were male, and 47 percent lived in urban areas. Average household size was 6.5, and 60 percent of respondents had no formal schooling. Table A2 presents labor market outcomes, showing that 10.6 percent of workers were formally employed, 55.8 percent of formal workers held long-term contracts, average tenure was 9.5 months, and formal sector wages averaged 118.6 USD per month. These baseline differences motivate the inclusion of covariates and fixed effects to adjust for observable heterogeneity across countries and time.

1.8 Findings

1.8.1 Employment

The 2017 labor market reform in Benin, which reduced firing costs and expanded the legal use of short-term contracts, had notable implications for employment across both the formal and informal sectors. In the formal sector, where labor regulations are more binding, the reduction in dismissal costs gave employers greater flexibility to adjust their workforce in response to demand and economic conditions. This flexibility likely encouraged firms to expand their workforce, as the perceived risks of formal hiring were reduced. At the same time, broader use of short-term contracts introduced new concerns around job precariousness and employment stability.

Table A3 shows that the reform led to a 2.6 percentage point increase (SE = 0.004, $p < 0.001$) in the probability of working in the formal sector, corresponding to a 24.5 percent rise relative to the pre-reform mean. Informal sector employment declined by 2.8 percentage points (SE = 0.004, $p < 0.001$), a 3.2 percent reduction. These findings suggest that the reform reduced barriers to formal employment, encouraging a reallocation of workers from

the informal to the formal sector. This result aligns with the hypothesis that lower firing costs make formal hiring more attractive for firms, particularly when employers are allowed to rely on more flexible contract types.

Unadjusted trends, presented in Figures A3, A4, and A5, show the average employment rates over time for Benin and the comparison countries. These plots reveal stable and parallel pre-reform employment trajectories, lending support to the parallel trends assumption. After the reform, Benin experienced a clear increase in formal employment and a decline in informal work, shifts that are not observed in the control countries, indicating that these changes were likely driven by the reform.

Figure A6 illustrates the changing composition of employment across industries. In Benin, labor shifted away from traditional sectors such as agriculture and toward services and commerce. The formal sector expanded most notably in manufacturing and professional services, while informal sector employment remained concentrated in agriculture, though with some contraction.

The reallocation toward formal employment is a significant result, especially in a context like Benin where informality is pervasive. Moving workers into formal jobs is important for several reasons. Formal employment is associated with better working conditions, greater access to social protection, higher job stability, and stronger legal recourse. At the macroeconomic level, formalization expands the tax base, enhances regulatory oversight, and can improve productivity by enabling firms to access capital and contracts. These broader gains help explain why many governments, including Benin's, have long prioritized formalization as a development objective (Bruhn and McKenzie, 2014; Bruhn, 2011; De Mel et al., 2013).

These findings are consistent with international evidence on the effects of labor reforms. Bosch and Maloney (2010) document modest formalization effects of labor reform in Brazil. In Latin America more broadly, Maloney (2004) and Levy (2010) report that reforms often failed to reduce informality. In contrast, the results from Benin suggest that under the right conditions, such as high initial informality and reforms that lower hiring risk, the shift toward formal jobs can be meaningful. This supports the idea that labor flexibility, when paired with legal clarity on contract types, can facilitate formal job growth even in low-income settings.

While the reform did not significantly increase total employment, the reallocation from informal to formal work represents a major structural change. The 24.5 percent rise in formal employment is large relative to its pre-reform base and indicates substantial progress toward a more regulated labor market. This is consistent with the argument by Blanchard and Portugal (2001) that labor market reforms may change the composition of employment rather than total job numbers. Given the low initial level of formal employment in Benin, such compositional changes can be transformative over time, particularly if they are sustained and reinforced by complementary policies.

In summary, the 2017 labor market reform in Benin promoted a shift from informal to formal employment, reducing informality without compromising total employment. This reallocation is economically and institutionally significant in a country where informality dominates the labor force. By encouraging formal hiring and reducing regulatory frictions, the reform supported a central goal of development policy and contributed to long-term improvements in labor market structure.

1.8.2 Long-Term Contracts in the Formal Sector

Labor market flexibility reforms can influence the likelihood that workers obtain permanent contracts. By lowering firing costs, such reforms reduce the risks and costs associated with dismissals, which may lead firms to rely more heavily on temporary arrangements. This shift could reduce job stability and limit access to secure employment. However, flexibility may also allow firms to experiment with initial hires and selectively retain productive workers through permanent contracts.

Despite concerns about job precarity, Table A4 shows that the conditional probability of holding a permanent contract in the formal sector increased by 23.2 percentage points (SE = 0.020) following the reform, a 41.6 percent rise relative to the pre-reform mean of 55.7 percent. The unconditional probability of holding a permanent contract, across all workers, rose by 4.6 percentage points (SE = 0.004), an 88.4 percent increase relative to the pre-reform mean of 5.2 percent. These findings suggest that the reform not only expanded formal employment but also enhanced job stability within the formal sector, with more

workers securing long-term employment arrangements.

Event study results presented in Figures A7 and A8 reinforce this conclusion. The conditional probability of holding a permanent contract remained flat in the pre-reform period but rose sharply and persistently afterward. The unconditional probability also increased, primarily due to reallocation into the formal sector. However, the rise in the conditional probability demonstrates that firms were not simply hiring more workers, they were also more likely to offer long-term contracts within the formal sector.

Unadjusted trends in Figures A9 and A10 show similar patterns. Before the reform, Benin's trajectory for permanent contracts closely mirrored that of the control countries. After the reform, Benin experienced a distinct upward shift, while comparison countries did not, supporting the interpretation of a policy-driven effect.

The reform's impacts varied across demographic groups. Table A5 shows that women experienced a larger gain in the conditional probability of securing a permanent contract (28.0 percentage points) compared to men (21.1 percentage points), though the difference is not statistically significant (t-test = -1.48). Rural workers experienced a significantly larger increase (29.3 percentage points) than urban workers (19.0 percentage points), with a statistically significant difference (t-test = -2.04). This may reflect greater labor market competition in rural areas or more binding constraints on labor supply, prompting firms to offer stronger retention incentives.

Younger workers (under 35) also saw greater gains (25.1 percentage points) than older workers (19.3 percentage points), though this difference was not statistically significant. There were no meaningful differences by marital status.

Figure A11 provides additional context. In the formal sector, the distribution of job tenure shifted notably after the reform, with a larger share of workers reporting twelve months of tenure. This suggests that firms retained more workers for longer durations. A smaller upward shift is also visible in the informal sector, hinting at potential spillover effects, although the reform was not directly targeted there.

The finding that permanent contracts became more common is particularly important in a setting like Benin, where informal employment dominates and long-term contracts are historically rare. Formal contracts are typically associated with stronger legal protections,

access to social security, and greater income stability. By promoting formalization and supporting longer-term employment relationships, the reform may help lay the foundation for improvements in labor productivity, worker investment in skills, and compliance with tax and regulatory systems.

These results are consistent with recent studies showing that formalization reforms, when well designed, can support higher-quality job creation. For instance, Bruhn and McKenzie (2014) and De Mel et al. (2013) show that reducing regulatory burdens can lead to formalization without sacrificing job quality. In Benin, the evidence suggests that flexibility through short-term contracts did not lead to widespread precarity. Instead, firms used flexibility to screen new hires, while offering stable contracts to high-performing workers.

In summary, the 2017 labor market reform in Benin significantly increased the use of permanent contracts in the formal sector. Both conditional and unconditional probabilities rose following the reform, and these effects were especially pronounced for women, rural workers, and younger individuals. These findings challenge the assumption that labor flexibility necessarily leads to job insecurity. In contexts like Benin, flexibility and stability can coexist when reforms reduce hiring risk and allow firms to retain talent through long-term arrangements.

1.8.3 Tenure (Last 12 Months)

The 2017 labor market reform in Benin influenced the duration of employment across sectors and contract types. Tenure, measured as the number of months a worker remained in their primary job over the past 12 months,[3] serves as a key indicator of job stability. Because this outcome is top-coded at 12 months, it reflects short-run changes in employment duration rather than long-run employment trajectories.

As reported in Table A6, the reform led to an overall reduction in tenure of 0.158 months (SE = 0.059), or approximately 4.7 days. This corresponds to a 1.66 percent decrease relative to the pre-reform mean of 9.53 months, suggesting that increased labor market flexibility was associated with slightly shorter average job spells.

[3] Although tenure reflects employment during the previous 12 months, the 2019 survey occurred well after the August 2017 reform, ensuring that post-reform tenure effects are fully captured.

In the formal sector, the effects differ by estimation method. When analyzing only currently employed formal workers (conditional), tenure declined by 0.118 months (SE = 0.108), a small and statistically insignificant effect. However, the unconditional estimate, assigning zero tenure to workers outside the formal sector, increased by 0.153 months (SE = 0.039), a 15.45 percent rise from the pre-reform mean of 0.99 months. This compositional effect reflects the entry of new workers into the formal sector who began accumulating tenure, even as the average tenure among incumbents remained largely unchanged.

Short-term contract (STC) workers in the formal sector experienced more pronounced changes. Conditional tenure fell by 0.070 months (SE = 0.231), an effect that is not statistically significant. However, the unconditional estimate indicates a significant drop of 0.229 months (SE = 0.028), a 50.3 percent decrease from the pre-reform mean of 0.455 months. These findings suggest that although STCs facilitated broader formal sector hiring, they also led to shorter job durations, consistent with increased job mobility and turnover.

Tenure also declined in the informal sector. For informal STC workers, conditional tenure fell by 0.148 months (SE = 0.060), a 1.55 percent decline. Unconditional tenure dropped by 0.311 months (SE = 0.068), or 3.64 percent. While the reform targeted formal sector labor rules, these informal sector responses likely reflect spillover effects. Informal employers may have adapted to changes in worker expectations or competitive pressures. Alternatively, informal workers may have cycled more frequently between sectors as new opportunities emerged in the formal labor market.

Figure A12 presents event study estimates showing a clear post-reform decline in tenure, particularly in the informal sector. Pre-reform trends appear stable and parallel, while the post-reform drop is concentrated in Benin. Figure A13 further illustrates this pattern: control countries experienced little change in tenure, while Benin saw a noticeable decline following the reform. These dynamics reflect both a wave of new hires, who, by definition, have lower tenure and greater mobility among STC workers.

Histograms in Figure A11 provide additional insight. In the formal sector (left panel), more workers reported 12 months of tenure after the reform, suggesting that a subset experienced improved job stability. At the same time, there was a reduction in mid-range tenure (1 to 11 months), consistent with more frequent job transitions. In the informal sector (right

panel), the distribution shifted toward shorter spells, supporting the conclusion that the reform contributed to increased labor mobility, even outside the formal sector.

Although shorter job tenure may raise concerns about instability, it can also reflect improved labor market dynamism. Greater mobility allows workers to search for better job matches, gain varied work experience, and access higher-quality employment. Especially in a high-informality setting like Benin, even short stints in the formal sector can provide access to legal protections, skill development, and employer referrals. These transitions may therefore contribute positively to long-term labor market outcomes.

In sum, the 2017 reform modestly reduced average job tenure, with the sharpest effects among STC workers and in the informal sector. While this decline highlights the trade-off between flexibility and stability, it also signals increased employment dynamism. Policymakers could consider complementary measures such as skills training or support for contract upgrading to ensure that short spells serve as stepping stones toward more secure employment.

1.8.4 Non-Employment Spell in Years

The 2017 labor market reform in Benin also influenced non-employment durations, measured as the time in years since an individual last held a job.[4] Studying changes in these durations provides insight into whether increased labor market flexibility created longer interruptions in employment for certain groups, or instead allowed for quicker reentry.

Table A7 presents conditional estimates (restricted to currently non-employed individuals) and unconditional estimates (including all individuals aged 15 to 64). Among the currently non-employed, the average non-employment spell increased by 0.397 years (SE = 0.228), a 29.2 percent rise from the pre-reform mean of 1.36 years. However, this result is not statistically significant at the 95 percent level (confidence interval: –0.051 to 0.846). For the broader population, the unconditional estimate is a small increase of 0.010 years (SE = 0.005), or roughly 3.65 days. This change represents a 62.5 percent increase relative to the pre-reform mean of 0.016 years, but is also not statistically significant.

[4]This measure is based on self-reported time since the last job and may be subject to rounding or recall imprecision. Nonetheless, it remains a standard indicator of non-employment duration in the LSMS surveys.

The event study plot in Figure A14 shows stable non-employment durations before the reform, followed by a modest uptick in the post-reform period. The shift remains limited in size and does not suggest a sharp or sustained increase. This pattern is consistent with the regression results and indicates that while some individuals may have faced slightly longer reemployment lags, the overall structure of non-employment spells remained largely intact.

These results suggest that the reform did not systematically increase unemployment durations for the working-age population. For most workers, increased job mobility appears to have occurred without generating widespread long-term detachment from the labor market. The slightly longer spells observed among those already out of work may reflect greater selectivity by employers in a more flexible labor environment, or increased transitions associated with short-term contracts.

Importantly, increased turnover and shorter job spells should not be interpreted solely as negative outcomes. Greater labor mobility can promote better job matching, particularly in low-income settings where workers often accept suboptimal employment due to frictions or rigid regulations. Periods of non-employment may represent time spent searching for a more suitable job, pursuing new skills, or responding to changing family or economic conditions. In flexible labor markets, such transitions are part of a dynamic employment environment that, over time, may lead to improved earnings, better productivity matches, and more efficient labor allocation.

In summary, the 2017 reform did not generate significant increases in non-employment durations, and any adverse effects appear limited to a small subset of the workforce. These results reinforce the broader narrative that labor market flexibility, when thoughtfully implemented, need not result in widespread unemployment. Policymakers should nonetheless remain attentive to vulnerable workers and consider complementary policies such as job placement services or retraining programs to support individuals navigating periods between jobs.

1.8.5 Monthly Earnings in the Formal Sector

The 2017 labor market reform in Benin aimed to reduce firing costs and increase contractual flexibility, with potential implications for earnings in the formal sector. By granting firms greater discretion in managing their workforce, the reform may have altered how wages were used to attract and retain workers, particularly across different contract types.

Table A8 reports the two-way fixed effects estimates of the reform's impact on monthly earnings. Column (1) presents conditional estimates for individuals currently employed in the formal sector. The reform led to a significant increase of 33.6 USD per month (SE = 10.6), equivalent to a 28.3 percent rise over the pre-reform mean of 118.6 USD. This sizable increase suggests that employers responded to the new flexibility by offering higher pay to retain skilled or productive workers, especially in a more competitive and mobile labor environment.

By contrast, Column (2) shows the unconditional results, where earnings are set to zero for individuals not in formal employment. The estimated effect in this case is much smaller, just 1.42 USD (SE = 1.26), and not statistically significant. This pattern highlights that wage gains were concentrated among formal sector workers and did not extend broadly across the full labor force.

Column (3) focuses on short-term contract (STC) workers in the formal sector, who experienced an average earnings increase of 19.6 USD (SE = 9.7). Although the estimate is positive and economically meaningful, it is not statistically significant. This may reflect greater job turnover and lower bargaining power among STC workers. Nevertheless, it suggests that even for temporary positions, wages increased in the wake of the reform.

Among long-term contract (LTC) workers, Column (5) shows a statistically significant wage gain of 23.4 USD (SE = 11.6). These results reinforce earlier findings that firms used permanent contracts as a strategy to retain valuable employees, offering higher pay as part of that strategy. The wage premium for LTC workers indicates that flexibility in hiring did not necessarily come at the cost of compensation. Instead, firms appear to have used contractual stability and pay to build long-term employment relationships with select workers.

The event study in Figure A15 confirms these patterns. Prior to the reform, wages in the

formal sector were stable. Following implementation, there is a clear and sustained upward trend. In contrast, Figure A16, which includes the entire working-age sample, shows only a marginal increase in average earnings, consistent with the regression results.

These findings suggest that the reform incentivized firms to use wages strategically, differentiating pay across contract types to retain productivity-enhancing labor. In this way, the policy promoted more efficient wage setting and encouraged better job matching. Furthermore, wage increases may reflect compositional changes in the formal sector workforce, as the reform attracted higher-skilled individuals who were previously in the informal sector or out of work.

In summary, the reform generated meaningful wage increases for formal sector workers, especially those with long-term contracts. While these gains did not extend broadly to the informal sector or non-employed individuals, they indicate that firms used the added flexibility not just to cut costs, but also to invest in and retain productive employees. This suggests that flexibility-enhancing labor reforms can contribute to earnings growth when combined with employer incentives to build stable employment relationships.

1.9 Discussion

This study evaluates the effects of Benin's 2017 labor market reform, which reduced dismissal costs and lifted restrictions on the use of short-term contracts. The evidence shows that the reform meaningfully reshaped employment dynamics, contract structures, job duration, and wages, especially in a setting where informality is pervasive and regulatory barriers constrain firm behavior.

A central finding is the reallocation of workers from the informal to the formal sector. Formal sector employment rose by 2.6 percentage points (a 24.5 percent increase), while informal employment declined by 2.8 percentage points. In many low-income countries, informality reflects deep institutional weaknesses and limited state capacity. As such, even modest growth in formal employment is a valuable achievement. Formal jobs are more likely to offer legal protections, access to social security, and stable earnings, while also expanding the tax base and improving fiscal sustainability.

The reform also increased the prevalence of permanent contracts in the formal sector, with a 23.2 percentage point rise. This outcome challenges the common narrative that flexibility reforms inevitably produce precarious work. Instead, the results suggest that firms used short-term contracts to screen hires and awarded long-term contracts to productive workers. Women, rural residents, and younger individuals, groups often disadvantaged in labor markets, saw the largest gains in long-term contracting, indicating that the reform may have enhanced equity and inclusion.

Although job tenure declined slightly, particularly among short-term contract workers, this pattern is not necessarily negative. Higher turnover can reflect more efficient labor matching. For many workers, especially in developing economies, moving between jobs can improve skill acquisition, generate referrals, and offer better future matches. Compared to unemployment or long-term informality, dynamic transitions may be welfare-enhancing. Thus, the reform may have increased both firm-level efficiency and worker mobility.

Wage gains in the formal sector reinforce the view that flexibility and job quality are not mutually exclusive. Formal sector earnings rose significantly by 33.6 USD per month on average with gains observed for both short- and long-term contract workers. These increases likely reflect both firms' need to attract talent and compositional improvements in the formal workforce. Higher wages contribute to household welfare and may reduce poverty in the long run.

Nonetheless, several limitations must be acknowledged. First, the difference-in-differences design remains observational, and unmeasured confounding may bias estimates. Second, the data are cross-sectional rather than panel, preventing direct observation of job transitions. Third, wage and tenure effects may reflect both behavioral responses and shifts in the composition of the workforce. Finally, firm-level data would be useful for assessing changes in productivity or profitability.

Despite these caveats, the reform achieved key goals: expanding formal employment, improving contract stability, and raising wages without inducing job losses. These results suggest that well-designed reforms that enhance flexibility can support inclusive growth and labor market modernization, especially when accompanied by broader efforts to strengthen institutions and reduce informality.

Future research could examine longer-term effects, regional variation, and firm behavior. Policymakers may also consider complementary policies such as job training or portable benefits to ensure that flexibility yields broad-based and durable gains.

1.10 Conclusion

This paper examined the effects of the 2017 labor market reform in Benin, which lowered firing costs and removed restrictions on short-term contract renewals. Using nationally representative household data and a two-way fixed effects design with West African countries as controls, I analyzed how the reform affected employment, contract structure, tenure, and wages.

The results show that formal sector employment rose by 2.6 percentage points, while informal employment declined by 2.8 percentage points. This reallocation, in a context of high informality and labor underutilization, represents a meaningful policy success. Formalization increases access to stable jobs and legal protections, and helps expand the tax base. Even modest growth in the formal sector may yield cumulative long-run gains.

The reform also increased the use of long-term contracts by 23.2 percentage points within the formal sector. Women, rural workers, and younger individuals experienced the largest gains, suggesting improvements in equity and mobility. Contrary to concerns about precarious work, firms appear to have used short-term contracts for screening and offered stability to high-performing employees.

Average job tenure declined slightly, particularly among short-term contract workers. However, this reflects increased turnover, not labor market deterioration. Dynamic job mobility can improve matching, accumulate skills, and provide income in ways that long-term unemployment or informality cannot. Viewed in this light, the reform fostered more active labor market engagement.

Wages in the formal sector rose significantly by 33.6 USD per month on average; suggesting that firms offered higher pay to attract and retain workers. These gains were concentrated among long-term contract holders, but short-term workers also benefited. The results point to improved efficiency in wage setting and labor allocation.

Some outcomes may partly reflect compositional shifts, as more productive or educated individuals entered formal jobs after the reform. While observable characteristics are controlled for, the data do not allow full separation of selection and treatment effects.

Overall, the reform met its core objectives: increasing formal employment, improving contract quality, and raising wages, without compromising overall employment. These findings offer valuable lessons for policymakers in similar settings. Future reforms could build on this foundation by incorporating training programs, worker protections, and portable benefits to ensure that labor market flexibility remains both inclusive and sustainable.

Chapter 2

Effects of In-Person Schooling During the COVID-19 Pandemic on Child Safety

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2.1 Abstract

The COVID-19 public health crisis created many challenges; in particular, access to in-person instruction varied widely across states and districts in Fall 2020. This study examines the impact of returning to in-person schooling on child maltreatment reporting and related child safety measures during the COVID-19 pandemic. We use linked individual-level administrative data from Wisconsin, covering school-aged children and their siblings from 2019 to 2021. We estimate event study and difference-in-differences models at the child-quarter level, measuring outcomes using Child Protective Services (CPS) reports and Medicaid claims for injuries. Our models control for child, maternal, and economic characteristics, and standard

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errors are clustered at the school district level. We find that CPS reports increase when schools return to in-person instruction, predominantly driven by reports from educational professionals. We also detect increases in child injuries upon reopening, suggestive of effects on both surveillance and child safety. Heterogeneity analyses reveal disparities across racial and socioeconomic groups. Our results corroborate previous studies suggesting that in-person instruction promotes child safety. By extension, in the absence of such child safety protections, our findings stress the need for alternative child protection mechanisms during school closures. Moreover, our results emphasize the importance of distinguishing between increased detection and true changes in maltreatment prevalence.

2.2 Author Disclosures

Short title: COVID-19 and Child Safety

Conflict of Interest Disclosures: The authors report no conflicts of interest.

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Abbreviations: CPS = child protective services; UI = unemployment insurance; TANF = temporary assistance for needy families; SNAP = supplemental nutrition assistance program; SSI = Social Security Income; NH = non-Hispanic; WADC = Wisconsin Administrative Data Core.

Article Summary: This article studies the effects of in-person school reopenings during the COVID-19 public health crisis on child protective services reporting and child safety.

What's Known on this Subject: The COVID-19 public health crisis had dramatic impacts on health and well-being, reduced employment and earnings, and impacted children through access to school, child care, and medical care.

What this Study Adds: School reopenings are associated with increases in child protective service reporting as well as increases in child injuries, indicating both a surveillance and child safety impact of having virtual learning.

2.3 Contributors Statement

Selidji Tossou contributed to the data analysis and participated in drafting and revising the manuscript.

Dr. Christine Durrance collaborated on the study design, review of results, manuscript preparation, and reviewed the final manuscript.

Dr. Jessica Pac collaborated on the study design, review of results, manuscript preparation, and reviewed the final manuscript.

Dr. Lawrence Berger collaborated on the study design, review of results, manuscript preparation, and reviewed the final manuscript.

All authors approved of the final manuscript as submitted and agree to be held accountable for all aspects of the work.

2.4 Introduction

The COVID-19 pandemic in early 2020 led to widespread school closures across Wisconsin, significantly challenging parent's ability to balance work and childcare. Wisconsin declared a public health emergency on March 12, 2020, and on March 13, 2020, the Wisconsin Department of Health Services mandated the closure of all public and private schools for in-person instruction beginning March 18, 2020 (Evers, 2020; Wisconsin Department of Public Instruction, 2020). This closure, initially intended to last until April 6, was subsequently extended through the end of the 2019-2020 academic year. In Fall 2020, Wisconsin school districts were given the autonomy to decide whether to reopen for in-person instruction or continue virtually, leading to wide disparities in parents' childcare burdens across the state. School closures and county safer-at-home orders aimed to curb the spread of COVID-19, however, these actions required parents to balance employment, childcare, and education within the pandemic-spurred constraints, leading to various health, financial, work, and educational stressors. Many parents, especially mothers of school-aged children, faced increased childcare responsibilities due to school closures, leading to reduced work hours or complete withdrawal from the workforce (Bullinger et al., 2021). But these effects were disproportionately borne by low-income mothers, those with low levels of attained education, and those with service-based occupations that were commonly classified as "essential workers" (Garcia and Cowan, 2022; Ranji et al., 2021). In particular, Goldin (2022) argues that the effect of the pandemic were not necessarily worse for all women, but rather that women with young children and in frontline occupations were impacted most heavily by a lack of childcare and in-person work requirements (Goldin, 2022). These challenges deepened existing socioeconomic inequalities and led to increased enrollment in state-administered social welfare programs (Bitler, 2023).

In parallel, the lack of in-person instruction raised concerns about child wellbeing and safety, as teachers and school staff serve as primary reporters of child maltreatment (Baron et al., 2020). Fitzpatrick et al. (2020) underscore the critical surveillance role played by schools, estimating that school personnel account for nearly one-fifth of all child protective services (CPS) referrals in the U.S (Fitzpatrick et al., 2020). Referrals to child protective services (CPS) dropped sharply nationwide, including in Wisconsin, when schools transitioned to

remote learning (Bullinger et al., 2021). Baron et al. (2020) documented a 27% decrease in child maltreatment reports during the initial months of the pandemic, suggesting that school closures may have disrupted this primary reporting channel of school personnel (Baron et al., 2020). Indicators of severe child abuse showed a similar pattern: emergency department visits related to child maltreatment injuries declined significantly during the early pandemic period (Swedo et al., 2020). These pronounced declines coincided with remote learning, during which children had reduced interaction with teachers and other mandatory reporters. Wolf et al. (2024) found higher CPS referral rates in counties that resumed in-person education compared to those that remained virtual, although these differences were not statistically significant (Wolf, Nguyen, et al., 2024).

Emerging research shows that the availability of in-person schooling helped alleviate these burdens and improved parental employment. Hansen, Sabia, and Schaller (2024) find that in-person school reopenings in the U.S. were associated with a 3.5 percentage point increase in non-education employment and a 0.99-hour increase in conditional weekly work hours among married women with school-aged children. They also report a 2.1 percentage point reduction in non-employment due to caregiving, and a reduction in part-time work due to childcare, highlighting the role of school availability in supporting maternal labor supply (Hansen et al., 2024). Similarly, Beauregard et al. (2022) observed that primary school reopenings in Canada led to an 18 percentage point increase in the employment-at-work rate for single mothers, with particularly strong effects for those whose jobs could not be performed remotely (Beauregard et al., 2022).

We extend to this literature by examining the effects of school reopening on child safety in Wisconsin. We leverage linked, individual-level administrative data—including CPS records and Medicaid claims for child injuries—combined with district-level data on instructional modes (in-person, hybrid, or virtual) in Fall 2020. This analysis provides critical insights into the protective role schools play for vulnerable children during public health crises.

2.5 Methods

2.5.1 Data

Our data are derived from the Wisconsin Administrative Data Core (WADC), a linked administrative database housed at the Institute for Research on Poverty. The WADC integrates records from child protective services (CPS), Medicaid claims, state social welfare programs (including the Supplemental Nutrition Assistance Program (SNAP), Unemployment Insurance (UI), Temporary Aid for Needy Families (TANF), and school enrollment records. The CPS data from the Wisconsin Department of Children and Families include demographic details of children with screened-in reports, alleged perpetrators, the nature of reported allegations, and the outcomes of investigations. We focus our analyses on investigated (including those reported by education professionals) and substantiated reports. Our primary outcome is investigated reports, as substantiated reports, in addition to reflecting variation in reporting behavior, capture agency-level preferences and resource capacity to respond to and investigate cases. We include substantiated reports for purposes of validation but prefer the investigated reports measure that is less subject to selection bias.

Additionally, we identify injury indicators from Medicaid claims to capture a different dimension of child safety and wellbeing. We implement the CDC's injury matrix to construct a series of binary indicators for any clinical visits (emergency department or otherwise) at which the child was noted to have an externally caused injury (see Table B1) (Centers for Disease Control and Prevention, 2014). The indicators capture motor vehicle and overexertion injuries that are mechanically more likely when children are not in school. We also include a narrower measure of injuries that are plausibly indicative of maltreatment (supervisory neglect), such as drowning, poisoning, firearm-related injuries, burns, and suffocation (Centers for Disease Control and Prevention, 2014). Though an imperfect proxy for child safety, these more objective measures offer additional insight into potential unreported cases of child maltreatment in light of previous work that found a decline in mandatory reporting during the COVID-19 pandemic (Shusterman et al., 2023). School enrollment data from the Wisconsin Department of Public Instruction (DPI), which are also included in the WADC,

allow us to track students' enrollment, grade level, and school district. We link these data with district-level information regarding reopening modalities from MCH Strategic Data. Our data also include a comprehensive set of covariates drawn from the WADC (see Table B2), such as past participation in social welfare programs and additional mother and child-level characteristics.

2.5.2 Sample

Our analytic sample consists of Wisconsin mothers with school-aged children (grades K-12) enrolled in public schools from the second quarter of 2019 through the fourth quarter of 2021, as well as all their siblings living in the same household. We restrict the enrolled children to those between the ages of 5 and 18 at the time of observation, consistent with compulsory school age in Wisconsin. After linking CPS, Medicaid, DPI school enrollment, and social welfare program records, our final dataset includes 643,759 unique children (enrolled students and their siblings) of 285,113 mothers, totaling 6,523,138 individual quarter-level observations (see Table B3). We define treatment status at the household level based on the school reopening modality experienced by enrolled school-aged children: a household was classified as treated if at least one school-aged child attended any form of in-person schooling (including hybrid instruction combining in-person and remote learning). This treatment was then extended to all siblings within the household, including those younger than school age, irrespective of their individual enrollment status, to capture the full household exposure to school reopening. For the injury-focused analyses, we constructed a Medicaid subsample limited to children enrolled in Medicaid for at least 75% of the observation period, resulting in a sample of 295,806 children with 3,090,898 individual-quarter level observations.

2.5.3 Empirical Approach

2.3 Empirical Approach

We employ both event study (ES) and difference-in-differences (DID) approaches to estimate the impact of school district reopening decisions on child safety. By including both CPS reports and injury-based indicators, the study offers a robust framework for evaluating child

safety in the context of school reopenings. This approach captures both officially reported cases of alleged maltreatment and broader patterns of injuries that may not enter the formal child welfare system, ensuring a more complete understanding of changes in child safety during the study period. We start with ES models of the following form:

$$Y_{idq} = \beta_0 + \sum_{k=-5}^{5} \beta_k \text{InPerson}_{ihdq+k} + \beta_2 X_{idq} + \theta_d + \delta_q + \varepsilon_{idq}$$

where Y_{idq} represents CPS involvement (investigation or substantiation) or an injury for child i, in district d, and year-quarter q. The key treatment variable, InPerson_{ihdq}, takes the value 1 if any school-aged child in household h attended a district offering any in-person instruction (fully in-person or hybrid) and 0 otherwise. The terms θ_d and δ_q are district and year-quarter fixed effects, respectively, controlling for time-invariant differences across districts and common time trends. The vector X_{idq} includes control variables such as number of siblings, number of children under five, the mother's age and education, the mother's CPS history prior to 2019, indicators for the 1st, 2nd, and 3rd quartiles of the mother's lagged quarterly earnings in 2019 Q1, and indicators for SNAP, W2, SSI, and Medicaid enrollment in the first quarter of 2019 Q1. Standard errors are clustered at the district level.

We also estimate DiD models that exploit variation in district-level instructional modalities during the COVID-19 public health crisis to identify the effect of in-person instruction on CPS involvement and child injuries. The primary identifying assumption of the DiD approach is that in the absence of school reopenings, districts that reopened for in-person learning and those that did not would have continued on the same trajectory over time.

$$Y_{idq} = \alpha + \beta \text{InPerson}_{ihdq} + \gamma X_{idq} + \theta_d + \vartheta_t + \varepsilon_{idq}$$

We begin by estimating a model for all school-aged children and their siblings in Wisconsin between Quarter 2 of 2019 and Quarter 4 of 2022. We additionally examine heterogeneous effects by mother's race (Black Non-Hispanic vs. White Non-Hispanic), region (rural vs. urban/suburban), education level (high school or less vs. college or more), and benefit receipt status (households receiving SNAP, TANF, Medicaid, or SSI vs. those not receiving benefits).

2.6 Results

Table B2 compares demographic and socioeconomic characteristics between the treatment group (children in districts with any in-person instruction) and the comparison group (children in districts without in-person instruction). Column (6) presents p-values from t-tests comparing the pre-treatment measures for those with and without in-person schooling. While the differences appear small in magnitude, the differences in all outcomes and covariates are statistically significant, emphasizing the importance of controlling for these in our ES and DiD models. One the whole, these descriptive statistics indicate that families in districts that reopened in-person (treatment districts) are modestly more socioeconomically advantaged than those that reopened virtually (comparison districts) and that comparison districts are disproportionately urban. We present summary statistics for the Medicaid sample (where we identify child injuries) in Table B4.

2.6.1 Regression results: CPS Investigations and Substantiations

Figure B1 presents event study results for all CPS investigations and substantiations and for CPS investigations and substantiations resulting from reports from educational professionals. The dotted vertical line in each panel denotes Q3 2020, the point at which districts had the option to return to in-person instruction. In addition to providing evidence of parallel trends prior to school reopenings, the figures show that investigated reports increased in districts that resumed in-person schooling, whereas such districts did not experience an increase in substantiations. Notably, however, when limiting to reports by education professionals, we find increases in both investigated reports and substantiated reports in districts that reopened with in-person instruction. The DiD results (Table B5) support these findings: on the whole, investigated reports from education professionals increased by 23.58 percent and substantiated reports from education professionals increased by 13.98 percent during the period in which districts implemented in-person schooling. Figure B2 shows very similar results when we condition on the Medicaid sample to compare directly with our child injury models. However, we prefer the ES estimates to the DiD models because the former assess

dynamic effects spanning each quarter of the post in-person reopening, rather than an average effect between post-reopening periods. Heterogeneity analysis in Figures B3-B6 reveals some disparities in the effects of school reopening on CPS outcomes across demographic and socioeconomic groups. We see a short-lived increase in CPS reports for Black children (Figure B3) but decreases in substantiated reports for Black children after in-person returns to school. We also see some evidence of increases in investigations for urban/suburban children (Figure B4), less educated mothers (Figure B5), and mothers receiving social welfare benefits (SNAP, TANF, Medicaid, and/or SSI) (Figure B6).

2.6.2 Regression results: Child Injuries

Figure B7 shows ES results for child injuries. Both total injuries and injuries suggestive of maltreatment increase following the return to in-person instruction, however the effect is more pronounced for total injuries. Table B6 shows the corresponding DiD coefficients. Specifically, in-person school reopenings are associated with a 7.68 percent increase in total injuries, suggesting that the observed increase in CPS investigations is driven by broader exposure risks, rather than a change in parenting behavior. Importantly, the estimated effect for injuries suggestive of maltreatment follows a similar pattern, though is not statistically significant, indicating that any increase in maltreatment-related harm is not as substantial as the increase in total injuries.

2.7 Discussion

Our findings provide important insight into how in-person schooling during the COVID-19 pandemic affected child safety. Using ES and DiD models, we show that the return to in-person instruction was associated with short-lived increases in child protective services (CPS) reports, particularly those made by educational professionals. These results suggest that school closures disrupted mandated reporting channels, potentially leading to under-reporting, while reopenings restored access to these detection mechanisms. Notably, we also observed an increase in externally-caused injuries following school reopenings among Medicaid-enrolled children. This suggests that increases in CPS reports may reflect both

enhanced detection and genuine increases in child safety concerns. However, the increase in injuries suggestive of maltreatment was modest compared to all injuries, suggesting that the observed increase may reflect a mix of mechanisms, including general exposure risks unrelated to abuse or neglect. Our estimated effects align with a broader pattern of post-reopening reporting recovery. For instance, Wolf et al. (2024) found increases in CPS referral rates in counties that resumed in-person instruction. Our findings show increases in both investigations and substantiated reports from educational professionals, suggesting a stronger reactivation of detection and substantiation mechanisms in our context. Finally, it is important to consider that results from our heterogeneity analyses are suggestive of widening disparities. Specifically, we find that Black children, children with lower-educated mothers, and those in benefit-receiving households saw greater increases in CPS investigations, but fewer substantiations. This pattern may reflect heightened scrutiny of certain groups, which is consistent with research on racial and socioeconomic disparities in CPS systems. Our analyses have several limitations that should be considered. These include the use of administrative data, which captures only documented cases of CPS involvement and child injury, and may reflect potential variation in CPS practices across districts. Additionally, given that our sample was drawn from Wisconsin, we cannot ensure generalizability to other states. Finaly, future research should explore other measures of child safety and longer-term outcomes associated with pandemic-era school disruptions using a wider range of data. These limitations notwithstanding, our findings underscore the protective role schools play in both detecting maltreatment and promoting child safety. At the same time, the disparate impacts observed across groups highlight the need for more equitable child protection strategies.

2.8 Conclusion

In-person schooling plays a vital role in promoting child safety, both by facilitating detection of maltreatment and mitigating broader risks to child safety. Our findings show that school reopenings during the COVID-19 pandemic led to increases in CPS reports and child injuries, particularly among vulnerable groups. As such, school closures likely left some children unseen and unsupported. Moving forward, it is essential to strengthen alternative

reporting and support systems during public health crises to ensure all children remain protected—regardless of instructional modality.

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Appendix A: Chapter 1

A.1 List of Tables

Table A1: Descriptive Characteristics of the Workforce in Benin and Other African Countries (Before and After Reform)

	Comparison Group		Benin		ttest
Survey Characteristics	Before	After	Before	After	P-value
Working Age	34.94	35.69	34.47	35.15	0.067
Gender (Male)	0.54	0.53	0.47	0.48	0.000
Urban area	0.38	0.39	0.47	0.48	0.000
Household size	8.50	8.19	6.51	6.48	0.000
Married	0.67	0.66	0.72	0.67	0.000
No Education	0.63	0.62	0.60	0.60	0.041
Primary School	0.19	0.17	0.19	0.19	0.951
Secondary School	0.16	0.19	0.18	0.18	0.000
Post Secondary School	0.02	0.03	0.03	0.03	0.005
Observations	110803	113964	16366	17474	127169

Note: This table presents descriptive statistics for the sample of workers from Benin and a comparison group of other African countries, before and after the 2017 labor market reform. It compares key characteristics such as age, gender, education level, and economic status across the two groups. The p-values from t-tests are provided to assess whether the differences between the two groups are statistically significant. The sample size is shown at the bottom of the table, with 110,803 observations for the comparison group and 16,366 observations for Benin.

Table A2: Employment Characteristics of Workers in Benin and Other African Countries (Before and After Reform)

	Comparison Group		Benin	
	Before	After	Before	After
Share of Workers				
Total Employment	97.53	97.35	98.40	98.35
Formal Sector Employment	12.71	12.41	10.59	13.34
Informal Sector Employment	84.82	84.94	87.81	85.01
Contract Type				
Long Term Contract (Conditional)	74.36	72.57	55.75	81.87
Long Term Contract (Unconditional)	10.38	09.00	05.24	09.14
Tenure in Last 12 Months				
Tenure Overall	8.220	8.225	9.532	9.700
Tenure Formal (Conditional)	9.434	10.010	10.515	9.865
Tenure Informal (Conditional)	8.271	8.211	9.556	9.864
Under STC Workers				
Tenure Overall	8.050	8.033	9.471	9.694
Tenure Formal (Conditional)	8.725	9.504	10.345	10.336
Tenure Informal (Conditional)	8.271	8.221	9.556	9.864
Non-Employment Spell in Years	1.407	1.460	1.361	1.552
Monthly Earnings in Formal Sector (USD)	148.663	155.321	118.602	93.491
Observations	110803	113964	16366	17474

Note: This table presents employment characteristics for workers in Benin and a comparison group of other African countries before and after the 2017 labor market reform. Columns for each group (comparison and Benin) show data from the periods before and after the reform. "Total Employment" reflects the percentage of individuals employed out of the total working-age population. "Formal Sector Employment" and "Informal Sector Employment" indicate the share of workers in each sector. "Long Term Contract (Conditional)" represents the percentage of workers with long-term contracts within the formal sector. "Long Term Contract (Unconditional)" provides the percentage of workers with long-term contracts across the entire sample, regardless of their employment sector. "Tenure in Last 12 Months" measures the average number of months workers have been in their current job over the last year. It is truncated above at 12 months. "Conditional" tenure metrics indicate the average tenure within specific sectors (formal or informal). Under STC Workers panel presents tenure statistics specifically for workers under short-term contracts. "Non-Employment Spell in Years" indicates the average duration of without employment among unemployed individuals, in years. "Monthly earnings in Formal Sector (USD)" reflects average earnings in the formal sector per month.

Table A3: Two way fixed effect result: Probability of Working (Unconditional)

	(1) Working at all	(2) Formal sector	(3) Informal sector
Benin × 2019	-0.00147	0.02629	-0.02777
se	(0.002)	(0.004)	(0.004)
ci	[-0.006,0.003]	[0.018,0.034]	[-0.036,-0.019]
Observations	258,599	258,599	258,599
Mean Pre-Treatment	0.984	0.106	0.873
Percentage Effect	-0.10%	24.53%	-3.21%

Note: This table presents the results of the impact of the 2017 labor market reform in Benin on the probability of working across different sectors. Column (1) shows the overall probability of working, column (2) focuses on the probability of working in the formal sector, and column (3) examines the probability of working in the informal sector. The coefficient for *Benin × 2019* represents the estimated effect of the reform. Mean Pre-Treatment reflects the average probability in the treatment group before the reform. Standard errors are in parentheses, and 95% confidence intervals are provided in brackets. All regressions are weighted using household weights and control for urban residence, age, gender, education, household size, marital status, industry type, and poverty level. Fixed effects for religion and commune are included. Robust standard errors are clustered at the household level.

Table A4: Probability of Having a Permanent Contract in the Formal Sector

	(1) Conditional Probability	(2) Unconditional Probability
Benin × 2019	0.232	0.046
se	(0.020)	(0.004)
ci	[0.193,0.271]	[0.039,0.053]
Observations	32,758	258,599
Mean Pre-Treatment	0.557	0.052
Percentage Effect	41.65%	88.46%

Note: This table displays the impact of the 2017 labor market reform in Benin on the probability of obtaining a permanent contract. Column (1) presents the conditional probability, focusing solely on workers in the formal sector, while column (2) shows the unconditional probability, encompassing all workers irrespective of sector. The interaction term *Benin* × *2019* captures the estimated effect of the reform on the likelihood of holding a permanent contract. Mean Pre-Treatment reflects the average probability in the treatment group before the reform. Standard errors are in parentheses, and 95% confidence intervals are provided in brackets. All regressions are weighted using household weights and include controls for urban residence, age, gender, education, household size, marital status, industry type, and poverty level. Fixed effects for religion and commune are also included. Robust standard errors are clustered at the household level.

Table A5: Probability of Having a Permanent Contract by Heterogeneity Characteristics (Conditional on Working in the Formal Sector)

	Gender		Urbanicity		Age		Marital Status	
	(1) Male	(2) Female	(3) Urban	(4) Rural	(5) ≤ 35	(6) > 35	(7) Married	(8) Not Married
Benin × 2019	0.211	0.280	0.190	0.293	0.251	0.193	0.222	0.261
se	(0.023)	(0.038)	(0.025)	(0.038)	(0.026)	(0.027)	(0.024)	(0.042)
ci	[0.165,0.256]	[0.206,0.354]	[0.142,0.239]	[0.218,0.369]	[0.201,0.301]	[0.139,0.246]	[0.175,0.268]	[0.177,0.344]
t-test	-1.48		-2.04		1.55		0.71	
Observations	21,840	10,918	21,928	10,830	15,565	17,193	22,676	10,082
Mean Pre-Treatment	0.554	0.563	0.585	0.492	0.519	0.602	0.588	0.453
Percentage Effect	38.07%	49.73%	32.48%	59.55%	48.36%	32.06%	37.76%	57.61%

Note: This table presents the estimated impact of the 2017 labor market reform in Benin on the probability of having a permanent contract, conditional on working in the formal sector, across various demographic and socioeconomic groups. Columns (1) and (2) represent the results for males and females, respectively; columns (3) and (4) show the results for urban and rural residents; columns (5) and (6) provide results by age group (≤ 35 and > 35); and columns (7) and (8) examine marital status (married and not married). The interaction term *Benin × 2019* captures the estimated effect of the reform. The row labeled "t-test" presents the t-statistic ($t = \frac{\text{coefficient}_1 - \text{coefficient}_2}{\sqrt{\text{se}_1^2 + \text{se}_2^2}}$) for the difference in coefficients between each pair of subgroups (e.g., male vs. female). A t-test value closer to zero suggests no significant difference between the subgroups, while a larger absolute value indicates a more significant difference. Standard errors are in parentheses, and 95% confidence intervals are provided in brackets. All regressions are weighted using household weights and include controls for urban residence, age, gender, education, household size, marital status, industry type, and poverty level. Fixed effects for religion and commune are also included. Robust standard errors are clustered at the household level.

Table A6: Tenure (Last 12 Months) for all workers

		Formal Sector (All)		Formal Sector (STC)		Informal Sector (STC)	
	(1) Overall Tenure	(2) Conditional	(3) Unconditional	(4) Conditional	(5) Unconditional	(6) Conditional	(7) Unconditional
Benin × 2019	-0.158	-0.118	0.153	-0.070	-0.229	-0.148	-0.311
se	(0.059)	(0.108)	(0.039)	(0.231)	(0.028)	(0.060)	(0.068)
ci	[-0.273,-0.042]	[-0.331,0.095]	[0.075,0.230]	[-0.524,0.384]	[-0.285,-0.174]	[-0.266,-0.031]	[-0.445,-0.177]
Observations	258,599	32,758	258,599	8,536	230,962	219,677	258,599
Mean Pre-Treatment	9.533	10.515	0.990	10.345	0.455	9.556	8.543

Note: This table presents the two way fixed effect results for worker tenure within the last 12 months, capturing how long workers have stayed in their current jobs at the time of the survey. Column (1) shows overall tenure across all workers and sectors. Columns (2) and (3) report tenure for all workers in the formal sector, where column (2) is conditional on being in the formal sector, and column (3) is unconditional, including all workers (with those not in the formal sector counted as having zero tenure). Columns (4) and (5) present tenure specifically for workers on short-term contracts (STC) in the formal sector, while columns (6) and (7) focus on tenure for short-term contract (STC) workers in the informal sector. The interaction term *Benin × 2019* captures the estimated effect of the 2017 labor market reform in Benin on worker tenure. Standard errors are in parentheses, and 95% confidence intervals are provided in brackets. Robust standard errors are clustered at the household level.

Table A7: Non-Employment Spell in Years (How long have you been Unemployed)

	(1) Conditional Unemployed Workers	(2) Unconditional All
Benin × 2019	0.397	0.010
se	(0.228)	(0.005)
ci	[-0.051,0.846]	[-0.001,0.020]
Observations	6,163	258,598
Mean Pre-Treatment	1.360	0.016

Note: This table presents the two way fixed effect results for Non-Employment spell duration in years, measuring how long individuals have been unemployed. Column (1) shows the conditional effect on Non-Employment spell for unemployed individuals, meaning only workers who are currently unemployed are included. Column (2) displays the unconditional effect, where the entire sample is included, with employed individuals having a non-employment spell of 0. The interaction term *Benin × 2019* captures the estimated impact of the 2017 labor market reform in Benin on the Non-Employment duration. Standard errors are in parentheses, and 95% confidence intervals are provided in brackets. Robust standard errors are clustered at the household level.

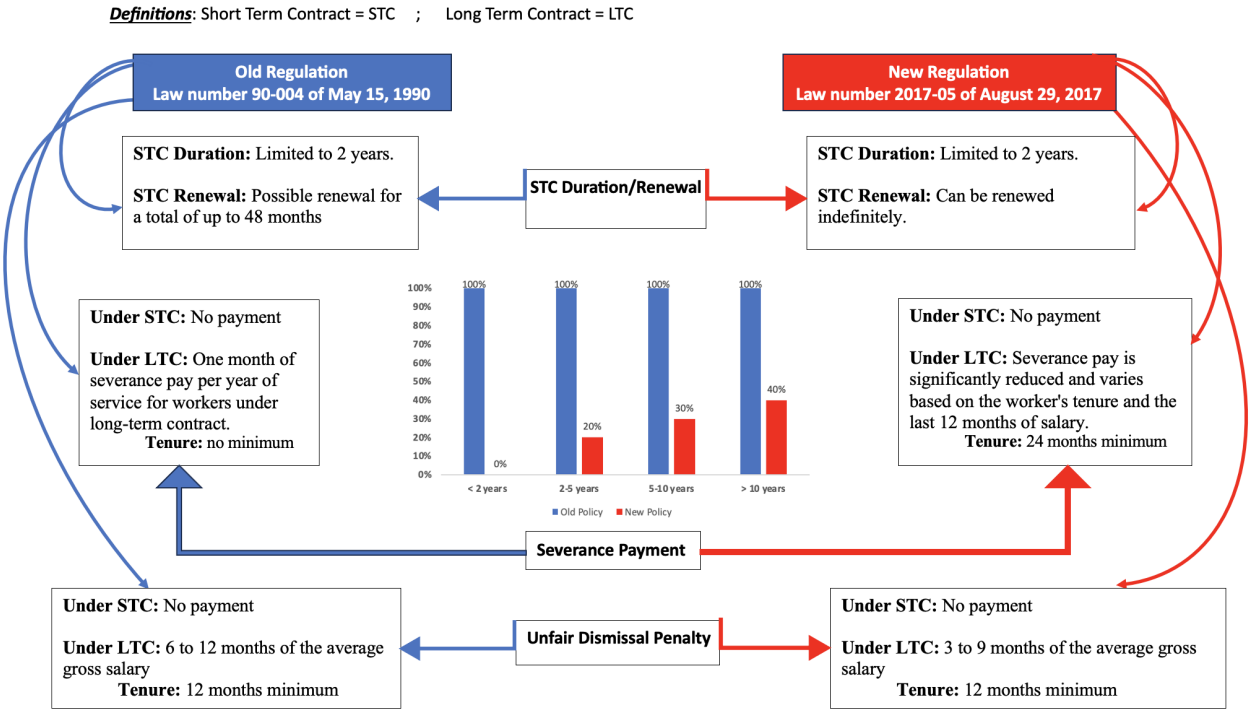
Table A8: Two way fixed effect Results for Monthly Earnings in the Formal Sector (in USD)

	All Workers		Short Term Contract		Long Term Contract
	(1) Conditional	(2) Unconditional	(3) Conditional	(4) Unconditional	(5) Unconditional
	Formal Sector	All Workers	Formal Sector	All Workers	All Workers
Benin × 2019	33.615	1.424	19.625	-0.678	23.411
se	(10.632)	(1.258)	(9.668)	(0.428)	(11.601)
ci	[12.720, 54.510]	[-1.046, 3.894]	[-0.607, 38.644]	[-1.526, 0.154]	[1.955, 50.320]
Observations	32,758	258,599	8,536	228,213	24,222
Mean Pre-Treatment	118.602	11.168	55.236	2.461	168.911

Note: This table presents the two way fixed effect results for monthly earnings in the formal sector, measured in USD after converting from FCFA using an exchange rate of 0.0017. Column (1) shows the conditional effect on wages for workers in the formal sector, while Column (2) presents the unconditional effect, considering all workers, including those not in the formal sector. Columns (3) and (4) present conditional and unconditional effects for workers with short-term contracts (STC) in the formal sector, while Column (5) shows the unconditional effect for workers with long-term contracts (LTC). The interaction term *Benin × 2019* captures the estimated impact of the 2017 labor market reform on wages in the formal sector. Standard errors are in parentheses, and 95% confidence intervals are provided in brackets. Robust standard errors are clustered at the household level.

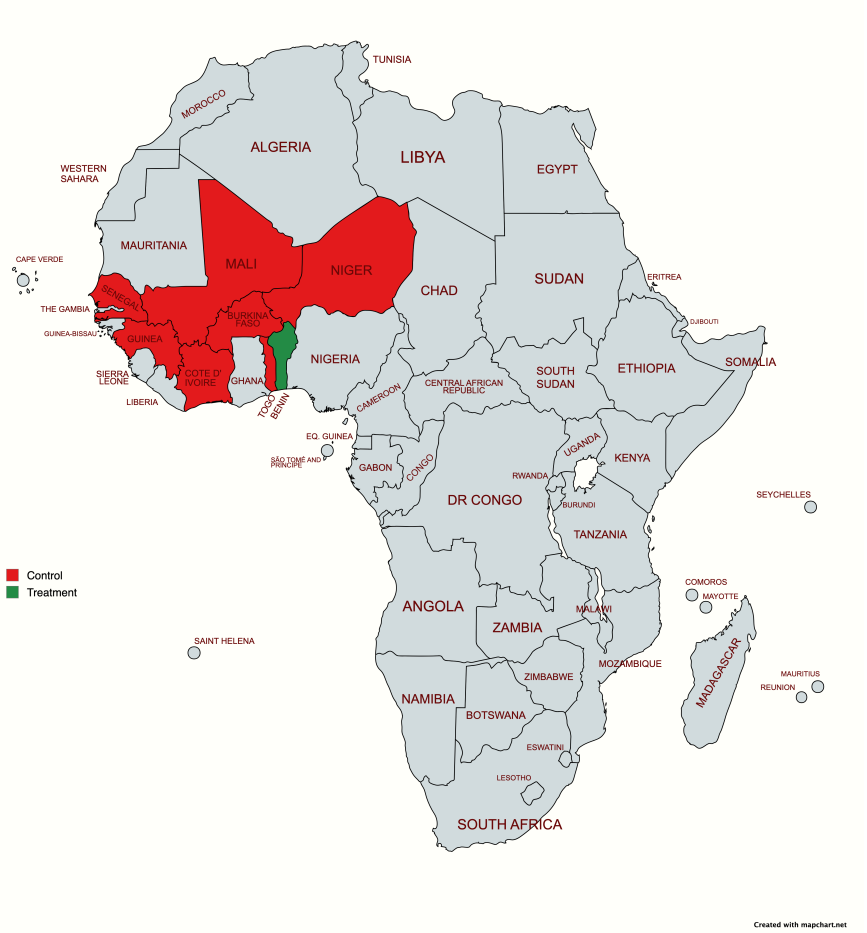
A.2 List of Figures

Figure A1: Old vs. New Labor Regulation in Benin: Key Changes



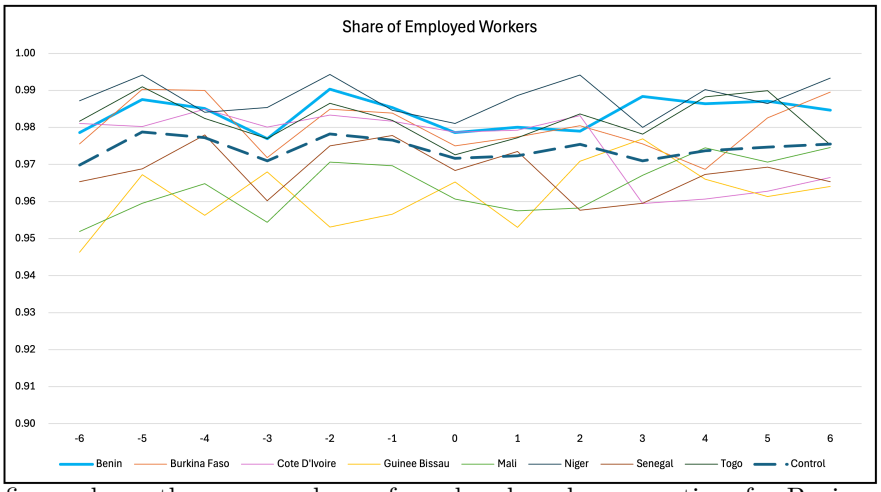
Note: This figure compares the key differences between Benin's old labor regulation (Law 90-004 of May 15, 1990) and the new labor regulation (Law 2017-05 of August 29, 2017). It highlights changes to the duration and renewal of short-term contracts (STC), severance pay, and unfair dismissal penalties. Under the new law, the renewal of STCs became indefinite, and severance pay for long-term contracts (LTC) was significantly reduced, particularly for workers with fewer years of service. This led to decreased costs for employers and increased labor market flexibility.

Figure A2: Treatment and Control Groups



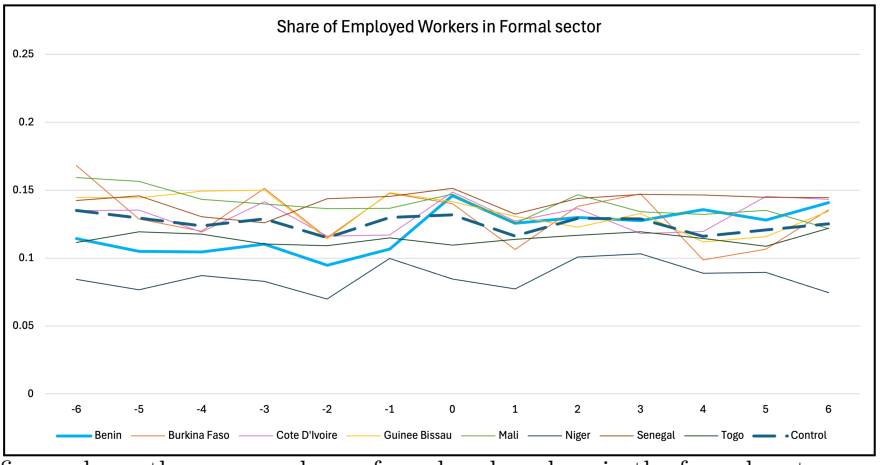
Note: This map highlights the countries used as treatment and control groups in the study. Benin, represented in green, is the treatment group where the 2017 labor market reform was implemented. The control group, marked in red, consists of West African countries that were not directly affected by the reform. These countries provide a comparative framework to assess the causal impact of the labor market reform on worker dynamics in Benin.

Figure A3: Unadjusted Trend: Share of Employed Workers



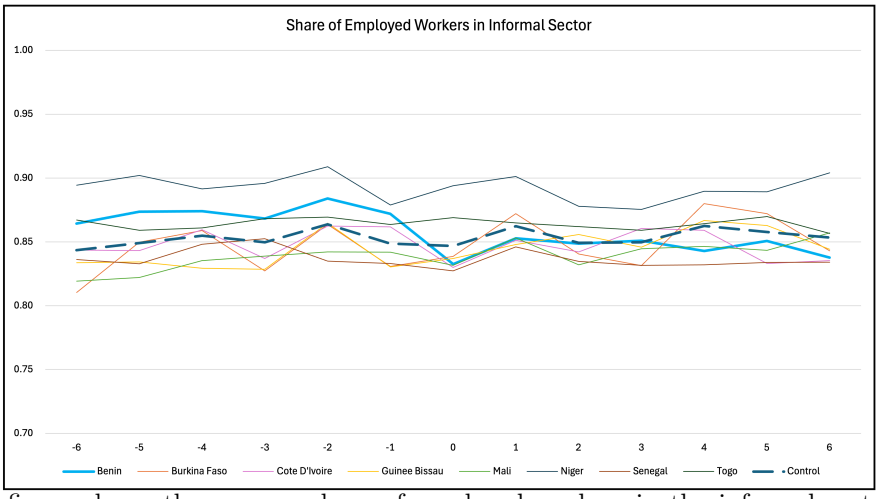
Note: This figure shows the average share of employed workers over time for Benin and several comparison countries (Burkina Faso, Côte d'Ivoire, Guinea Bissau, Mali, Niger, Senegal, and Togo). The x-axis represents pre-reform periods (–6 to –1) and post-reform periods (0 to 6), while the y-axis shows the share of employed workers. Solid lines represent individual country trends; the dotted line shows the comparison group mean. The LSMS data are repeated cross-sections collected over multiple months in each wave. Month –6 corresponds to January 2016, and month 0 to January 2019. The gap between period –1 and 0 reflects the time span between survey waves; individuals are not tracked over time. The sample includes working-age individuals (15–64) who were employed or unemployed and actively seeking work.

Figure A4: Unadjusted Trend: Share of Employed Workers in Formal Sector



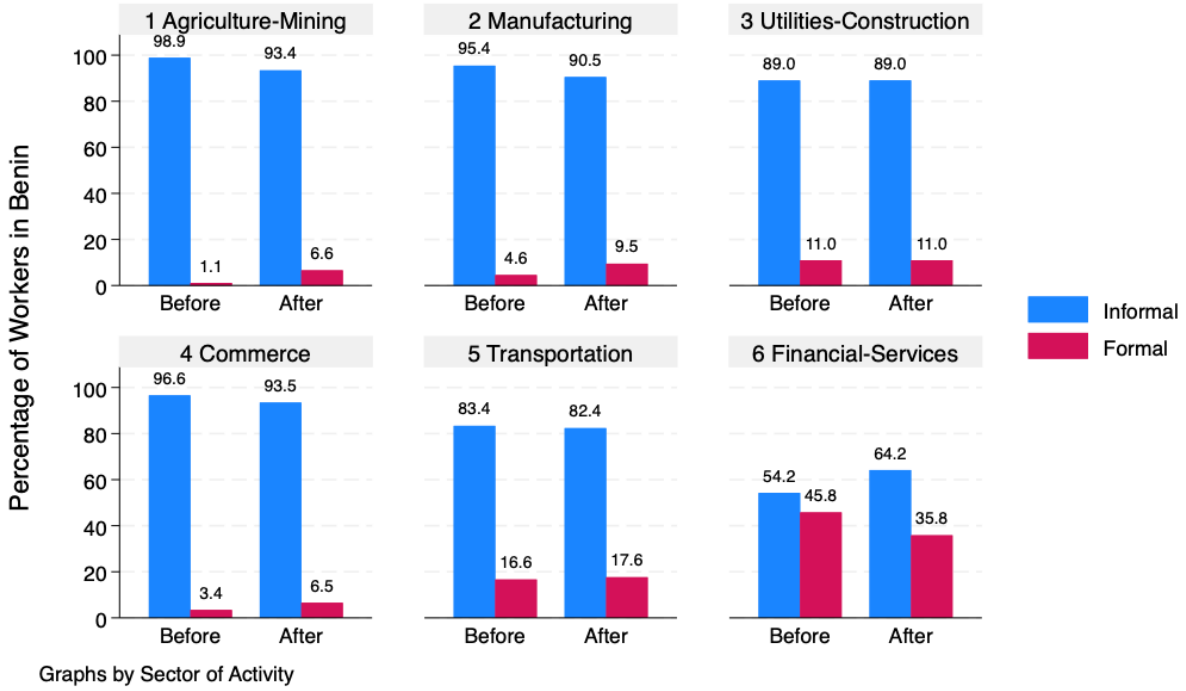
Note: This figure shows the average share of employed workers in the formal sector over time for Benin and several comparison countries (Burkina Faso, Cote d'Ivoire, Guinea Bissau, Mali, Niger, Senegal, and Togo). The x-axis represents pre-reform periods (–6 to –1) and post-reform periods (0 to 6), based on the month of survey interview. The y-axis shows the share of employed workers. Solid lines represent individual country trends; the dotted line represents the comparison group mean. Although the LSMS data are cross-sectional, each wave was collected progressively over several months. The monthly event-time variable is constructed using interview dates, with month –6 corresponding to January 2016 and month 0 to January 2019. The gap between period –1 and 0 reflects the time span between survey waves; individuals are not tracked over time. The sample includes working-age individuals (15–64) who were either employed or unemployed and actively seeking work at the time of the survey.

Figure A5: Unadjusted Trend: Share of Employed Workers in Informal Sector



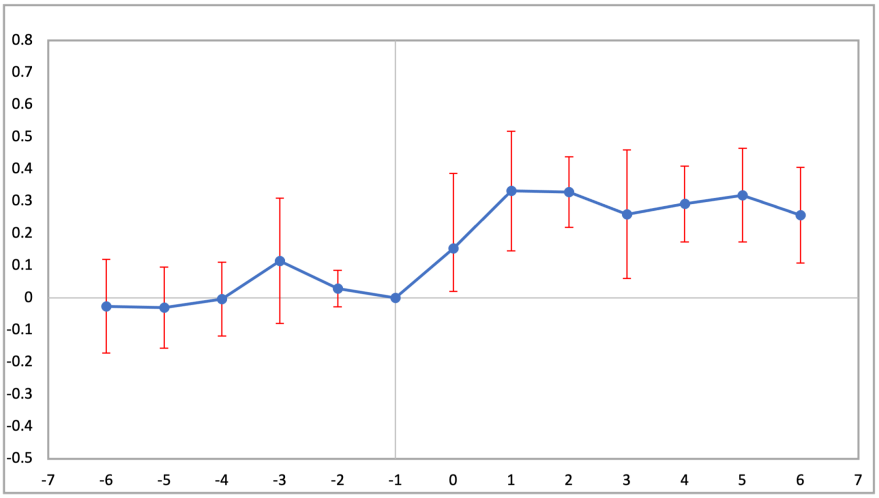
Note: This figure shows the average share of employed workers in the informal sector over time for Benin and several comparison countries (Burkina Faso, Cote d'Ivoire, Guinea Bissau, Mali, Niger, Senegal, and Togo). The x-axis represents pre-reform periods (–6 to –1) and post-reform periods (0 to 6), based on the month of survey interview. The y-axis shows the share of employed workers. Solid lines represent individual country trends; the dotted line represents the comparison group mean. Although the LSMS data are cross-sectional, each wave was collected progressively over several months. The monthly event-time variable is constructed using interview dates, with month –6 corresponding to January 2016 and month 0 to January 2019. The gap between period –1 and 0 reflects the time span between survey waves; individuals are not tracked over time. The sample includes working-age individuals (15–64) who were either employed or unemployed and actively seeking work at the time of the survey.

Figure A6: Workers Distribution Across Sectors in Benin



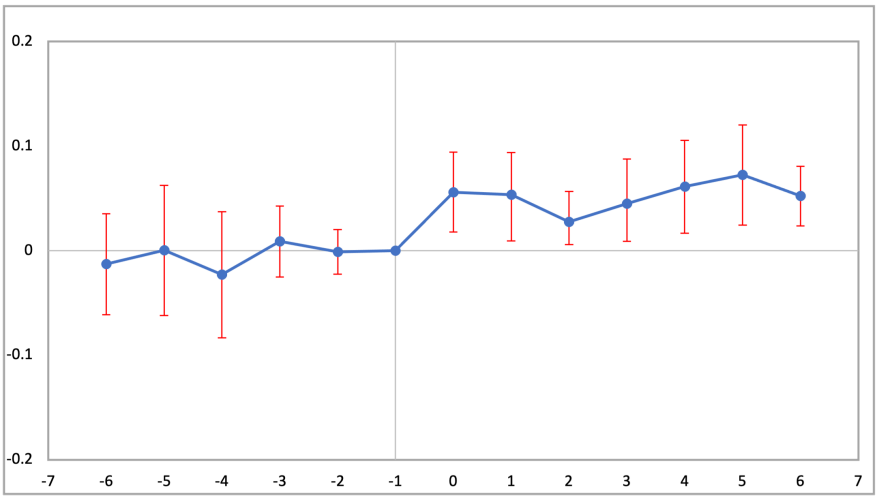
Note: This figure shows the distribution of workers across different economic sectors in Benin before and after the labor market reform. The x-axis displays two time periods labeled "Before" and "After" the reform. The y-axis represents the percentage of workers in each sector. Blue bars indicate informal employment, while red bars represent formal employment. Each subpanel corresponds to a specific industry: Agriculture-Mining, Manufacturing, Utilities-Construction, Commerce, Transportation, and Financial Services. The most notable shifts include an increase in formal employment within Agriculture-Mining, Manufacturing and Commerce sectors, and moderate gains in formal employment in Transportation. This distribution provides insight into the sector-specific impacts of the reform on formal and informal employment in Benin.

Figure A7: Event Study: Probability of Having a Permanent Contract for workers in the formal sector (Conditional)



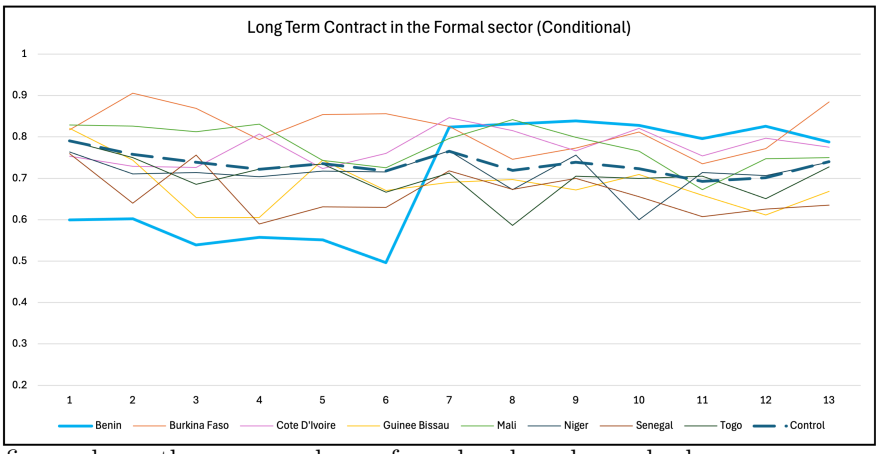
Note: This event study illustrates the conditional probability of having a long-term contract for workers in the formal sector. The horizontal axis displays periods relative to the reform implementation: lead periods from –6 to –1 represent the months of data collection before the reform, with –1 as the reference period. Periods from 0 to 6 are lags, indicating the months following the reform. Observations show the change in the probability of obtaining a long-term contract post-reform, relative to the pre-reform baseline. Although the LSMS data are cross-sectional, each wave was collected progressively over several months. The monthly event-time variable is constructed based on the interview date, with month –6 corresponding to January 2016 and month 0 to January 2019. The gap between period –1 and 0 reflects the time span between survey waves; individuals are not tracked over time. The sample includes working-age individuals (15–64) who were either employed or unemployed and actively seeking work at the time of the survey.

Figure A8: Event Study: Probability of Having a Permanent Contract (Unconditional)



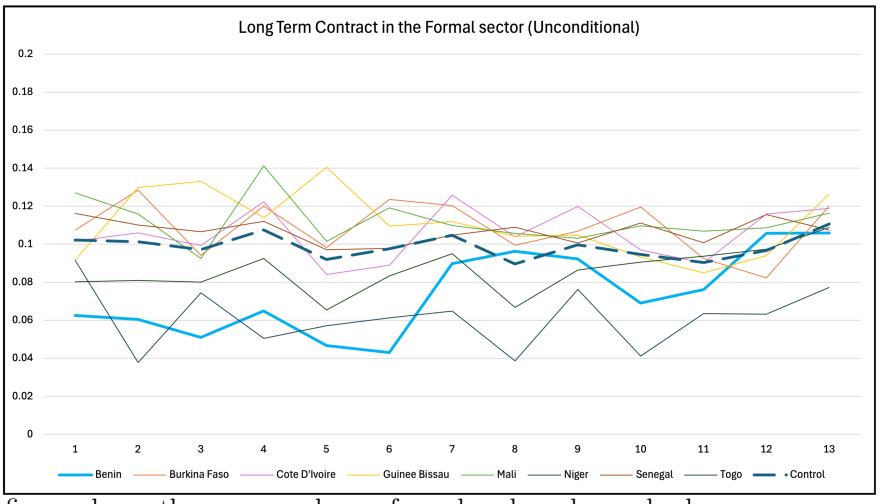
Note: This event study presents the unconditional probability of having a long-term contract for all workers, without restriction to those in the formal sector. The horizontal axis shows periods relative to the reform: –6 to –1 are lead periods based on the months of data collection prior to the reform, with –1 as the reference period. Periods from 0 to 6 are lags representing the post-reform months. The figure illustrates changes in the probability of obtaining a long-term contract following the reform, relative to the pre-reform baseline. Although the LSMS data are cross-sectional, each wave was collected progressively over several months. The monthly event-time variable is constructed from the interview date, with month –6 corresponding to January 2016 and month 0 to January 2019. The gap between period –1 and 0 reflects the time span between survey waves; individuals are not tracked over time. The sample includes working-age individuals (15–64) who were either employed or unemployed and actively seeking work at the time of the survey.

Figure A9: Unadjusted Trend: Probability of Having a Permanent Contract in the Formal Sector (Conditional)



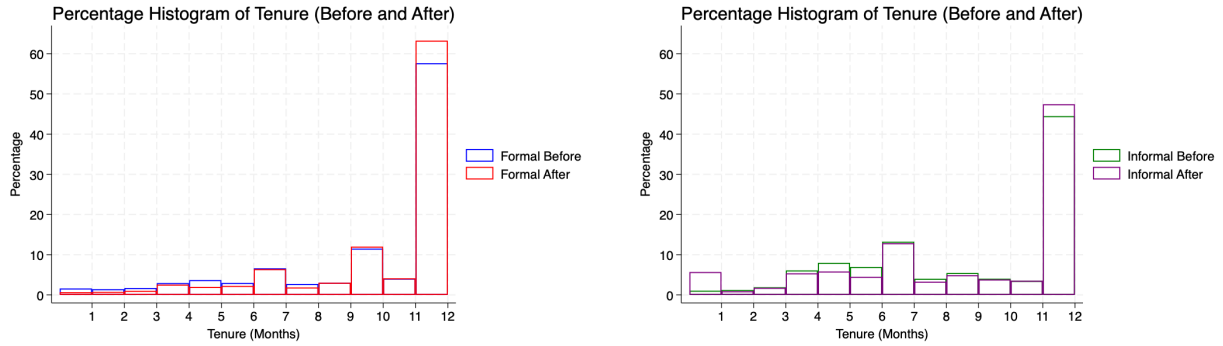
Note: This figure shows the average share of employed workers who have a permanent contract over time for Benin and several comparison countries (Burkina Faso, Cote d'Ivoire, Guinea Bissau, Mali, Niger, Senegal, and Togo). The x-axis represents time periods, with periods 1 to 6 as pre-reform and 7 to 13 as post-reform. The y-axis shows the share of employed workers. Solid lines represent individual country trends; the dotted line represents the combined mean for the comparison group. Although the LSMS data are cross-sectional, each wave was collected progressively over several months. The monthly event-time variable is based on the interview month, with month –6 corresponding to January 2016 and month 0 to January 2019. The gap between period –1 and 0 reflects the time span between survey waves; individuals are not tracked over time. The sample includes working-age individuals (15–64) who were either employed or unemployed and actively seeking work at the time of the survey.

Figure A10: Unadjusted Trend: Probability of Having a Permanent Contract in the Formal Sector (Unconditional)



Note: This figure shows the average share of employed workers who have a permanent contract over time for Benin and several comparison countries (Burkina Faso, Cote d'Ivoire, Guinea Bissau, Mali, Niger, Senegal, and Togo). The x-axis represents time periods, with periods 1 to 6 as pre-reform and 7 to 13 as post-reform. The y-axis shows the share of employed workers. Solid lines represent individual country trends; the dotted line represents the combined mean for the comparison group. Although the LSMS data are cross-sectional, each wave was collected progressively over several months. The monthly event-time variable is based on the interview month, with month –6 corresponding to January 2016 and month 0 to January 2019. The gap between period –1 and 0 reflects the time span between survey waves; individuals are not tracked over time. The sample includes working-age individuals (15–64) who were either employed or unemployed and actively seeking work at the time of the survey.

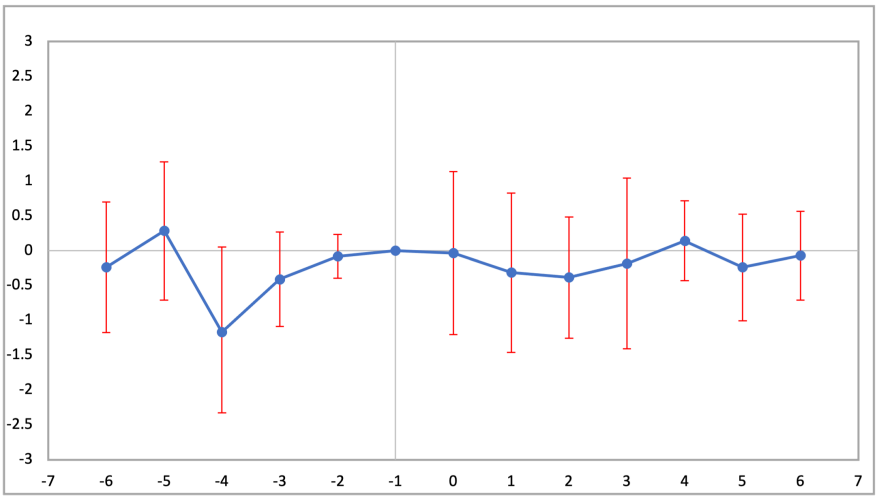
Figure A11: Tenure Histograms: Formal and Informal Sectors Before and After the Reform



(a) Formal Sector (b) Informal Sector

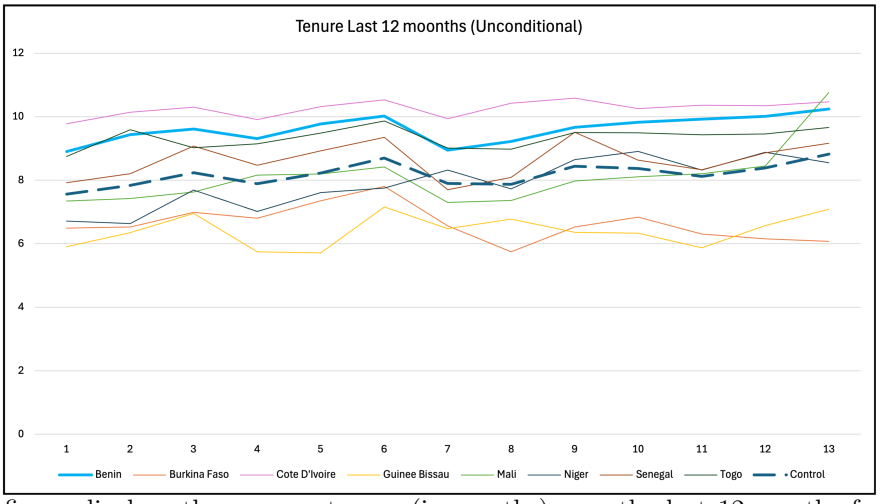
Note: The histograms display the distribution of job tenure (in months) among workers in the formal and informal sectors before and after the reform. In the formal sector (left panel), the post-reform distribution (red) shows a notable increase in the percentage of workers with 12 months of tenure, indicating greater retention or job stability following the reform. The informal sector (right panel) also exhibits a shift in tenure distribution, with more workers reaching higher tenure levels (shown in purple) after the reform compared to the pre-reform period (green). These patterns suggest that the reform may have influenced both formal and informal sector job tenure.

Figure A12: Event Study: Tenure Last 12 Months (overall)



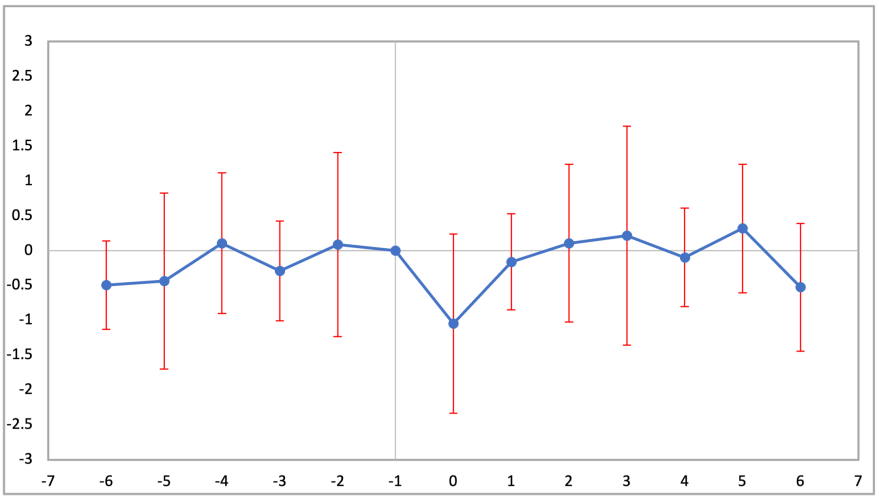
Note: This event study illustrates the average tenure (in months) over the last 12 months for workers across all sectors, without restriction to formal or informal employment (unconditional tenure). The x-axis represents time periods relative to the reform implementation, where –6 to –1 are lead periods, indicating months of data collection before the reform, with –1 as the reference period. Periods from 0 to 6 are lags, representing months after the reform. The y-axis shows the deviation in tenure from the reference period, allowing us to observe changes in tenure trends post-reform compared to the pre-reform baseline. Although the LSMS data are cross-sectional, each wave was collected progressively over several months. The monthly event-time variable is constructed based on the interview month, with month –6 corresponding to January 2016 and month 0 to January 2019. The gap between period –1 and 0 reflects the time span between survey waves; individuals are not tracked over time. The sample includes working-age individuals (15–64) who were either employed or unemployed and actively seeking work at the time of the survey.

Figure A13: Unadjusted Trends: Tenure Last 12 Months (Unconditional)



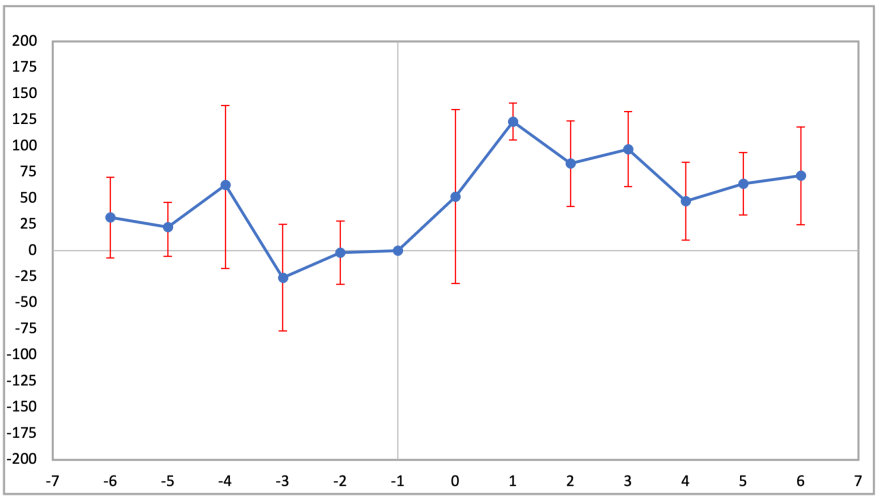
Note: This figure displays the average tenure (in months) over the last 12 months for workers across different countries, irrespective of their sector of employment (unconditional tenure). Each line represents a country, with Benin indicated by a solid cyan line. The control group average is shown with a dashed line. The x-axis represents time periods, with periods 1 to 6 as pre-reform and 7 to 13 as post-reform. The y-axis shows the average tenure. This visualization compares tenure trends across countries—Benin, Burkina Faso, Cote d'Ivoire, Guinea Bissau, Mali, Niger, Senegal, and Togo—both individually and relative to the control. Although the LSMS data are cross-sectional, each wave was collected progressively over several months. The monthly event-time variable is based on the interview month, with month –6 corresponding to January 2016 and month 0 to January 2019. The gap between period –1 and 0 reflects the time span between survey waves; individuals are not tracked over time. The sample includes working-age individuals (15–64) who were either employed or unemployed and actively seeking work at the time of the survey.

Figure A14: Event Study: Non-Employment Spell in Years (Conditional)



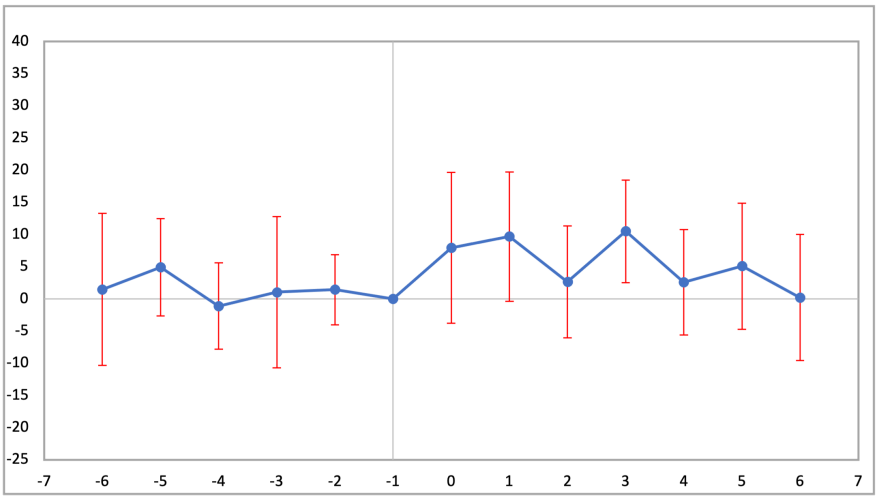
Note: This event study illustrates the average duration of unemployment spells in years for workers who are currently unemployed (conditional sample). The x-axis represents time periods relative to the reform implementation, where –6 to –1 are lead periods indicating months of data collection before the reform, with –1 serving as the reference period. Periods from 0 to 6 are lags, representing months of data collection following the reform. The y-axis shows the deviation in unemployment spell duration from the reference period, allowing us to observe changes in unemployment duration trends post-reform compared to the pre-reform baseline. Although the LSMS data are cross-sectional, each wave was collected progressively over several months. The monthly event-time variable is constructed based on the interview date, with month –6 corresponding to January 2016 and month 0 to January 2019. The gap between period –1 and 0 reflects the time span between survey waves; individuals are not tracked over time. The sample includes working-age individuals (15–64) who were either employed or unemployed and actively seeking work at the time of the survey.

Figure A15: Event Study: Monthly Earnings in the formal sector in USD (Conditional)



Note: This event study illustrates the average monthly earnings for workers who are currently in the formal sector (conditional sample). The x-axis represents time periods relative to the reform implementation, where –6 to –1 are lead periods indicating months of data collection before the reform, with –1 serving as the reference period. Periods from 0 to 6 are lags, representing months of data collection following the reform. The y-axis shows the deviation in wages from the reference period, allowing us to observe changes in earnings trends post-reform compared to the pre-reform baseline. Although the LSMS data are cross-sectional, each wave was collected progressively over several months. The monthly event-time variable is constructed based on the interview date, with month –6 corresponding to January 2016 and month 0 to January 2019. The gap between period –1 and 0 reflects the time span between survey waves; individuals are not tracked over time. The sample includes working-age individuals (15–64) who were either employed or unemployed and actively seeking work at the time of the survey.

Figure A16: Event Study: Monthly Earnings in the formal sector in USD (Unconditional)



Note: This event study illustrates the average monthly earnings in the formal sector across all workers, without restriction to workers in the formal sector (unconditional sample). The x-axis represents time periods relative to the reform implementation, where –6 to –1 are lead periods, with –1 as the reference period. Periods from 0 to 6 are lags, representing months of data collection post-reform. The y-axis shows the deviation in wages from the reference period, allowing us to observe changes in earnings trends following the reform. Although the LSMS data are cross-sectional, each wave was collected progressively over several months. The monthly event-time variable is constructed based on the interview month, with month –6 corresponding to January 2016 and month 0 to January 2019. The gap between period –1 and 0 reflects the time span between survey waves; individuals are not tracked over time. The sample includes working-age individuals (15–64) who were either employed or unemployed and actively seeking work at the time of the survey.

Appendix B: Chapter 2

B.1 List of Tables

Table B1: List of ICD Codes for Injury Outcomes

External Injury	**ICD-10 Codes**
Cut/Pierce	W25–W29, W45, W46, X78, X99, Y28, Y354
Drowning	W65–W74, X71, X92, Y21
Fall	W00–W19, X80, Y01, Y30
Fire/Flame	U013, X00–X09, X76, X97, Y26, Y363
Hot Object/Scald	X10–X19, X77, X98, Y27
Firearm	U014, W32–W34, X72–X74, X93–X95, Y22–Y24, Y350
Machinery	W24, W30–W31
Motor Vehicle Traffic	V30–V794, V30–V799, V81–V821, V83–V860, V83–V863
Other Pedal Cycle	V10–V11, V12–V140, V190–V199
Other Pedestrian	V01, V02–V040, V05, V06, V090–V099
Other Land Transport	V20–V280, V290–V293, V30–V793, V800–V809, V81–V829, V83–V869, V879–V899, X82, Y03, Y32
Other Transport	U011, V90–V99, Y361
Natural/Environmental	W42–W43, W53–W64, W92–W99, X20–X39, X51–X57
Overexertion	X50
Poisoning	U016–U017, X40–X49, X60–X69, X85–X90, Y10–Y19, Y352
Struck by/Against	W20–W22, W50–W52, X79, Y00, Y04, Y29, Y353
Suffocation	W75–W84, X70, X91, Y20
Other Specified	U010, U012, U015, U030, W23, W35–W41, W44, W49, W85–W91, X75, X81, X96, Y02, Y05–Y07, Y25, Y31, Y351, Y355, Y360, Y362, Y364–Y368, Y85
Not Specified	U019, U039, X59, X84, Y09, Y34, Y357, Y369, Y899

Notes: This table presents the ICD-10 codes used to classify injury outcomes. Injury categories were constructed based on Medicaid claims data, following standard classification guidelines. ICD codes correspond to external cause of injury (E-codes) and mechanisms of injury. Ranged codes (e.g., W65-W74) indicate a continuous sequence of codes covering a similar injury type. 'Other' categories (e.g., Other Transport, Other Land Transport, Not Specified) include injuries not captured by more specific classifications. The subset of injuries suggestive of maltreatment includes drowning, fire and flame injuries, firearm-related injuries, poisoning, suffocation, and hot object or scald injuries. Full details on coding methodology and category definitions are available upon request.

Table B2: Summary Statistics

		Comparison Group		Treatment Group		
Variable	Full (1)	Pre (2)	Post (3)	Pre (4)	Post (5)	P-value (4)-(2)
Outcomes Variables (Full Sample)						
Investigated Reports	0.00789	0.00905	0.00758	0.00778	0.00742	[0.00000]
Substantiated Reports	0.00112	0.00084	0.00070	0.00132	0.00127	[0.00000]
N (Full Sample)	6,523,138	1,260,485	1,046,459	2,321,113	1,895,081	3,581,598
Substantiated Reports	0.14144	0.09313	0.09267	0.16996	0.17148	[0.00000]
N (Investigated cases)	51,458	11,404	7,931	18,069	14,054	29,473
Outcomes Variables (Medicaid Sample)						
All Injuries	0.09103	0.07828	0.07664	0.09785	0.10231	[0.00000]
Injuries Suggestive of Maltreatment	0.01153	0.01081	0.01034	0.01205	0.01225	[0.00000]
N (Medicaid Sample)	3,090,898	670,025	552,408	1,029,940	838,525	1,699,965
Child Characteristics						
Female	0.484	0.486	0.486	0.483	0.484	[0.000]
Age	10.609	10.148	10.663	10.483	11.041	[0.000]
Mother's Age at Birth	27.098	26.747	26.966	27.149	27.342	[0.000]
Children Under 18	2.850	2.985	3.017	2.757	2.780	[0.000]
Children Under 5	0.695	0.753	0.824	0.618	0.680	[0.000]
Mother's Race						
White Non-Hispanic	0.585	0.388	0.381	0.695	0.692	[0.000]
Black Non-Hispanic	0.147	0.296	0.302	0.064	0.066	[0.000]
Hispanic	0.147	0.193	0.194	0.122	0.123	[0.000]
Native Non-Hispanic	0.027	0.017	0.017	0.033	0.033	[0.000]
Asian Non-Hispanic	0.056	0.074	0.075	0.046	0.046	[0.000]
Mother's Education						
No High School	0.201	0.258	0.258	0.169	0.169	[0.000]
High School	0.578	0.557	0.559	0.588	0.589	[0.000]
More than High School	0.035	0.033	0.032	0.037	0.036	[0.000]
Mother's CPS involvement before 2019	0.104	0.117	0.111	0.101	0.096	[0.000]
2019 Q1 Benefits and Wages						
1st quartile in quarterly wages	0.296	0.305	0.298	0.294	0.291	[0.000]
2nd quartile in quarterly wages	0.270	0.279	0.303	0.251	0.270	[0.000]
3rd quartile in quarterly wages	0.211	0.210	0.205	0.214	0.210	[0.000]
Received SNAP	0.324	0.413	0.411	0.276	0.277	[0.000]
Received TANF	0.015	0.026	0.027	0.009	0.009	[0.000]
Received SSI	0.029	0.042	0.041	0.022	0.021	[0.000]
Received Medicaid	0.382	0.437	0.435	0.352	0.353	[0.000]
County Type						
Urban	0.339	0.550	0.556	0.222	0.222	[0.000]
Suburban	0.146	0.082	0.081	0.182	0.182	[0.000]
Rural	0.515	0.368	0.363	0.597	0.596	[0.000]
N	6,523,138	1,260,485	1,046,459	2,321,113	1,895,081	3,581,598

Notes: WADC 2019 Q2 – 2021 Q4. This table presents demographic and economic characteristics for the treatment group (children in districts with any in-person instruction) and the comparison group (children in districts without in-person instruction). The sample covers both the pre-treatment period (2019 Q2 – 2020 Q3 before school reopened) and the post-treatment period (2020 Q4 – 2021 Q4 after school reopened). Columns 1–5 report mean values for each subgroup, while column 6 presents p-values from t-tests comparing the pre-treatment characteristics of the treatment and comparison groups. Most outcome variables are calculated using the full sample, except for substantiated reports, which are measured within the subsample of investigated cases. As a result, substantiated reports should not be directly compared with outcomes derived from the full sample. Injury outcomes are based on Medicaid claims and are constructed using the sample of Medicaid-enrolled children. The injury outcomes include all injuries recorded in Medicaid claims as well as a subset classified as suggestive of maltreatment, including drowning, fire and flame injuries, firearm-related injuries, poisoning, suffocation, and hot object/scald injuries, which are commonly associated with neglect or abuse. P-values, reported in brackets in Column 6, reflect statistical tests comparing pre-treatment characteristics between the treatment and comparison groups.

Table B3: Exclusion Restrictions

Restriction	**Individual-Quarter Observations**	**Unduplicated Children**	**Unduplicated Mothers**
Initial sample: Mothers with children enrolled in public K–12 schools (Q2 2019 – Q4 2021)	5,029,078	474,933	285,113
Inclusion of non-enrolled siblings (merged via mother IDs)	8,276,047	770,112	285,113
Exclusion of children older than 18 years	6,523,138	643,759	285,113
Final sample after merging CPS records and resolving unmatched records due to eligibility criteria	6,523,138 (Final Analytic Sample)	643,759	285,113
Medicaid subsample: children enrolled in Medicaid $\geq$ 75% of the study period	3,090,898 (Final Medicaid subsample)	295,806	137,087

Notes: WADC 2019 Q2 – 2021 Q4. This table summarizes the steps in constructing the analytic sample, beginning with all mothers with school-aged children enrolled in public K-12 schools and their siblings. Children older than 18 were excluded. The Medicaid subsample specifically facilitates analyses of child injury outcomes.

Table B4: Summary Statistics (Medicaid Sample)

		Comparison Group		Treatment Group		
Variable	Full (1)	Pre (2)	Post (3)	Pre (4)	Post (5)	P-value (4)-(2)
Outcomes Variables (Medicaid Sample)						
Investigated Reports	0.00994	0.01057	0.00876	0.01017	0.00993	[0.01186]
Substantiated Reports	0.00120	0.00081	0.00078	0.00133	0.00164	[0.00000]
N (Medicaid Sample)	3,090,898	670,025	552,408	1,029,940	838,525	1,699,965
Substantiated Reports (Rate)	0.12102	0.07693	0.08890	0.13046	0.16531	[0.00000]
N (Investigated cases)	30,723	7,084	4,837	10,478	8,324	17,562

Notes: WADC 2019 Q2 – 2021 Q4. This table presents summary statistics for the treatment group (children in districts with any in-person instruction) and the comparison group (children in districts without in-person instruction), focusing on Medicaid-enrolled children. The sample covers both the pre-treatment period (2019 Q2 – 2020 Q3 before school reopened) and the post-treatment period (2020 Q4 – 2021 Q4 after school reopened). Columns 1–5 report mean values for each subgroup, while column 6 presents p-values from t-tests comparing the pre-treatment characteristics of the treatment (column 4) and comparison (column 2) groups. Investigated reports are calculated using the full Medicaid sample, whereas substantiated reports are measured only within the subsample of investigated cases. As a result, substantiated reports should not be directly compared with outcomes derived from the full sample. Covariates included in the estimation are listed in Table 1 and also include district and year-quarter fixed effects P-values, reported in brackets in Column 6, reflect statistical tests comparing pre-treatment characteristics between the treatment and comparison groups.

Table B5: DID Results for CPS Outcomes – Investigations and Substantiations

	All Reporters		**Educational Reporters**	
	(1) Investigated Reports	(2) Substantiated Reports	(3) Investigated Reports	(4) Substantiated Reports
DID Estimate	0.0011	0.0042	0.0005*	0.0376***
SE	0.0006	0.0071	0.0002	0.0107
Mean of Dependent Variable	0.00789	0.14144	0.00212	0.26889
Percent Effect	13.94%	2.97%	23.58%	13.98%
Observations	6,523,138	51,458	6,523,138	51,458

*Notes: WADC 2019 Q2 – 2021 Q4. This table presents Difference-in-Differences (DID) estimates assessing the impact of in-person schooling on child protective services (CPS) reports. Covariates included in the estimation are listed in Table 1 and also include district and year-quarter fixed effects. The estimates are reported separately for all reporters and for education-type reporters, including daycare center staff, home daycare providers, school administrators, school counselors, teachers, licensed/certified daycare providers, speech therapists/audiologists, occupational therapists, and social workers. The dependent variables include investigated reports, substantiated reports (Column 2), investigated reports from education reporters (Column 3), and substantiated reports from education reporters (Column 4). Investigated reports refer to any CPS referral that was formally investigated, while substantiated reports reflect cases where maltreatment was confirmed. Substantiated reports are measured only within the subsample of investigated referrals and should not be directly compared to outcomes derived from the full sample. Standard errors are clustered at the district level. Statistical significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.*

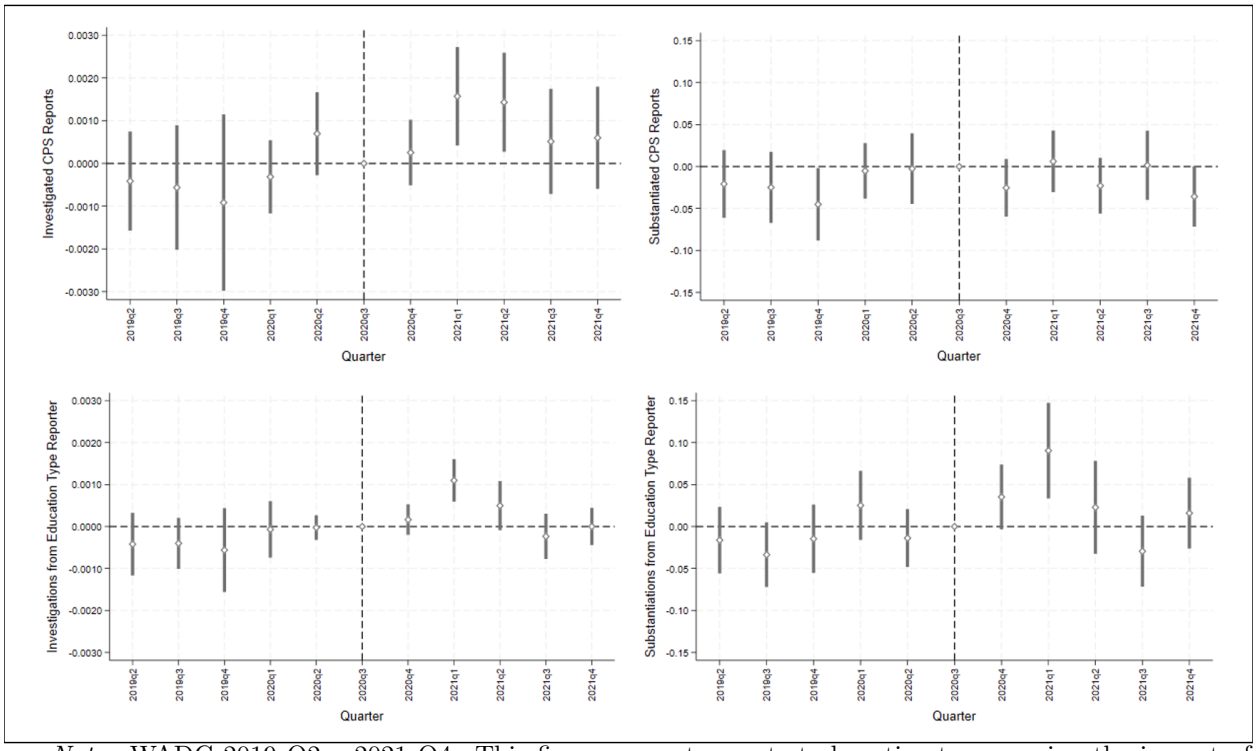
Table B6: DID Results for Injury Outcomes

	All Injuries	Injuries Suggestive of Maltreatment
DID Estimate	0.00598**	0.00069
SE	0.00216	0.00035
Mean of Dependent Variable	0.0782836	0.0108145
Percent Effect	7.68%	6.73%
Observations	3,090,898	3,090,898

*Notes: WADC 2019 Q2 – 2021 Q4. This table presents Difference-in-Differences (DID) estimates assessing the impact of in-person schooling on child injury outcomes, using a sample of Medicaid-enrolled children. The dependent variables include all injuries and injuries suggestive of maltreatment. Injury outcomes are recorded in Medicaid claims. Injuries suggestive of maltreatment include categories typically considered indicators of potential neglect or abuse, such as drowning, fire and flame injuries, firearm-related injuries, poisoning, suffocation, and hot object/scald injuries. Detailed ICD codes used to define maltreatment and injury categories are provided in the Appendix Table A1. The DID estimate represents the estimated effect of in-person schooling on each outcome. Covariates included in the estimation are listed in Table 1 and include district and year-quarter fixed effects. Standard errors are clustered at the district level. Statistical significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.*

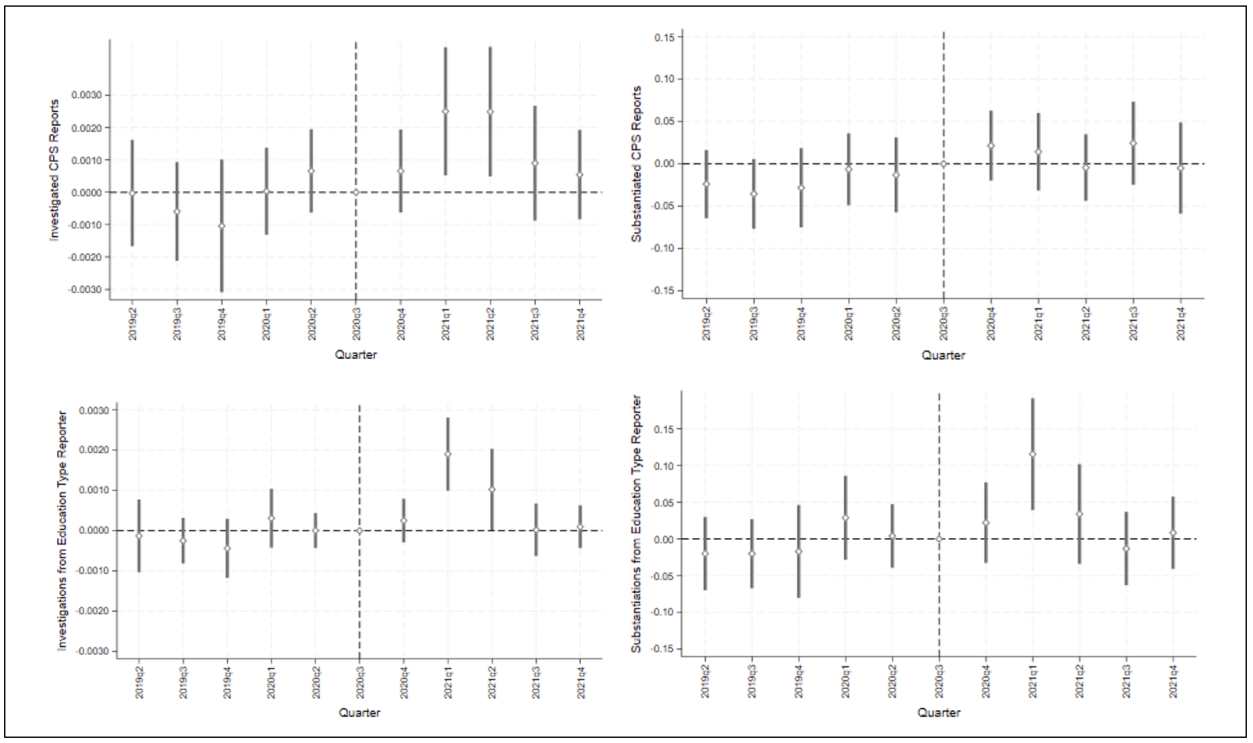
B.2 List of Figures

Figure B1: Event Study: CPS Reports – Investigations and Substantiations



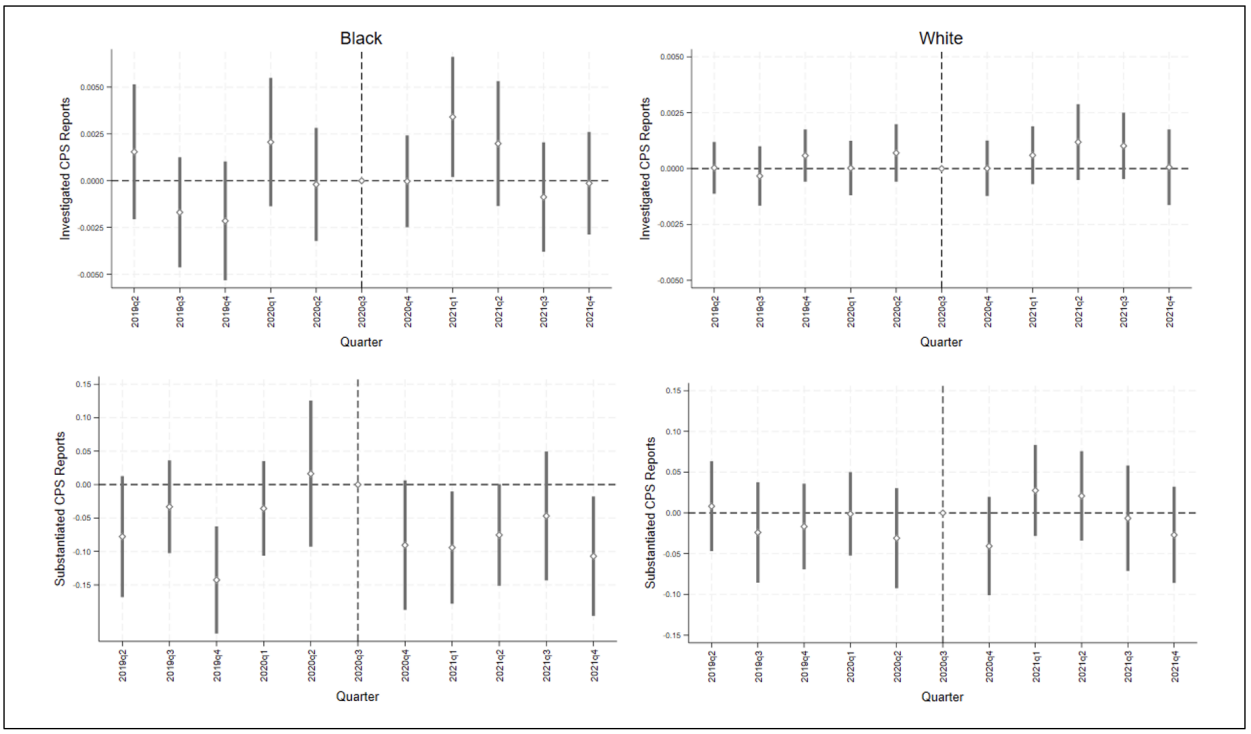
Note: WADC 2019 Q2 – 2021 Q4. This figure presents event study estimates assessing the impact of in-person schooling on child protective services (CPS) reports. The coefficient estimates are displayed separately for all reporters and education-type reporters across four dependent variables: investigated reports, substantiated reports, investigated reports from education-type reporters, and substantiated reports from education-type reporters. Substantiated reports are measured only within the subsample of investigated cases and should not be directly compared with outcomes derived from the full sample. Investigated reports refer to any CPS referral that was formally investigated, while substantiated reports reflect referrals where maltreatment was confirmed. Education-type reporters include daycare center staff, home daycare providers, school administrators, school counselors, teachers/other, licensed/certified daycare providers, speech therapists/audiologists, occupational therapists, and social workers. The x-axis represents calendar quarters from 2019 Q2 to 2021 Q4, with 2020 Q3 as the reference period, indicated by the vertical dashed line. The y-axis shows point estimates (regression coefficients) from the event study specification, with 95% confidence intervals. Covariates included in the estimation are listed in Table 1 and include district and year-quarter fixed effects. Standard errors are clustered at the district level.

Figure B2: Event Study, CPS outcomes using Medicaid Sample



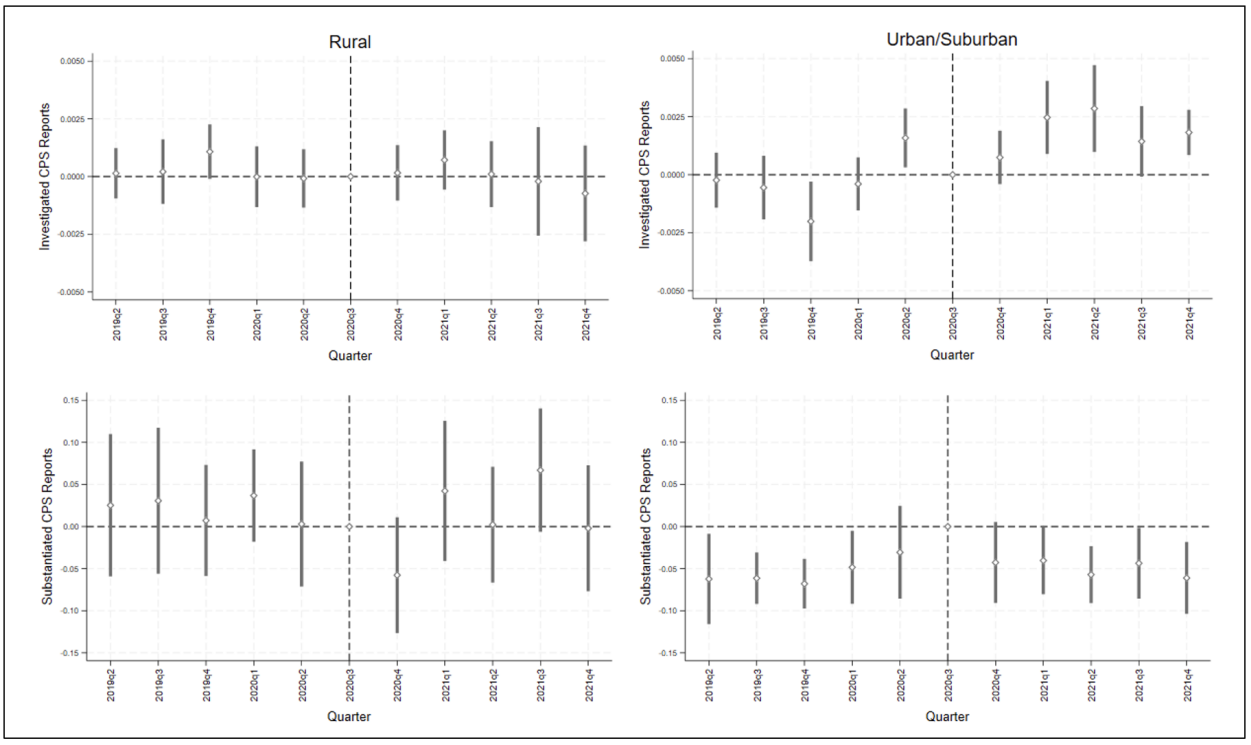
Notes: WADC 2019 Q2 – 2021 Q4. This figure presents event study estimates assessing the impact of in-person schooling on child protective services (CPS) reports. The estimates are displayed separately for all reporters and education-type reporters across four dependent variables: investigated reports , substantiated reports , investigated reports from education-type reporters , and substantiated reports from education-type reporters. Substantiated reports are measured only within the subsample of investigated cases and should not be directly compared with outcomes derived from the full Medicaid sample. Investigated reports refer to any CPS referral that was formally investigated, while substantiated reports reflect referrals where maltreatment was confirmed. Education-type reporters include daycare center staff, home daycare providers, school administrators, school counselors, teachers/other, licensed/certified daycare providers, speech therapists/audiologists, occupational therapists, and social workers. The x-axis represents calendar quarters from 2019 Q2 to 2021 Q4, with 2020 Q3 as the reference period, indicated by the vertical dashed line. The y-axis shows the estimated coefficients from the event study specification, with 95% confidence intervals. Covariates included in the estimation are listed in Table 1 and also include district and year-quarter fixed effects. Standard errors are clustered at the district level.

Figure B3: Event Study, CPS outcomes by mother's race (Black/White Non-Hispanic)



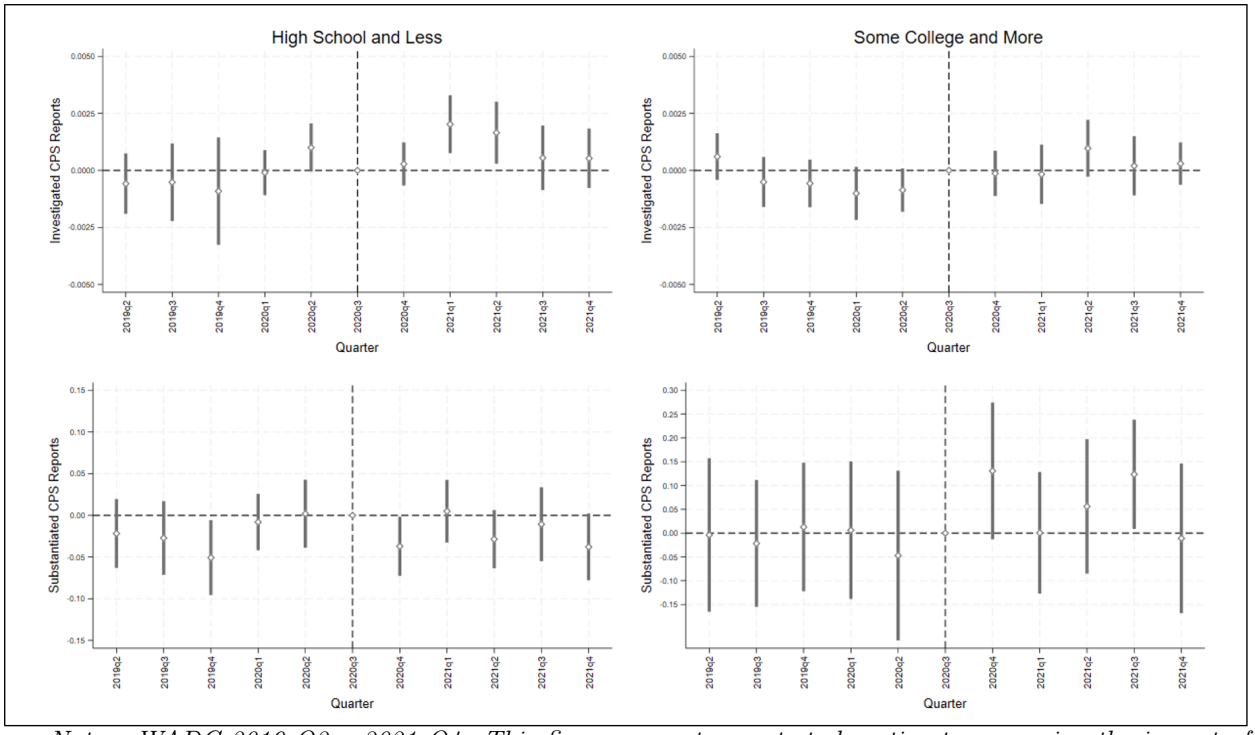
Notes: WADC 2019 Q2 – 2021 Q4. This figure presents event study estimates assessing the impact of in-person schooling on child protective services (CPS) outcomes, stratified by mother's race (Black/White Non-Hispanic). The four panels separately display estimates for investigated CPS reports and substantiated CPS reports, with stratification by whether the mother race is Black Non-Hispanic or White Non-Hispanic. Investigated reports refer to any CPS case that was formally investigated, while substantiated reports reflect cases where maltreatment was confirmed. The x-axis represents calendar quarters from 2019 Q2 to 2021 Q4, with 2020 Q3 as the reference period, indicated by the vertical dashed line. The y-axis shows the estimated coefficients from the event study specification, with 95% confidence intervals. Covariates included in the estimation are listed in Table 1 and include district and year fixed effects. Standard errors are clustered at the district level.

Figure B4: Event Study, CPS outcomes by mother's urbanicity



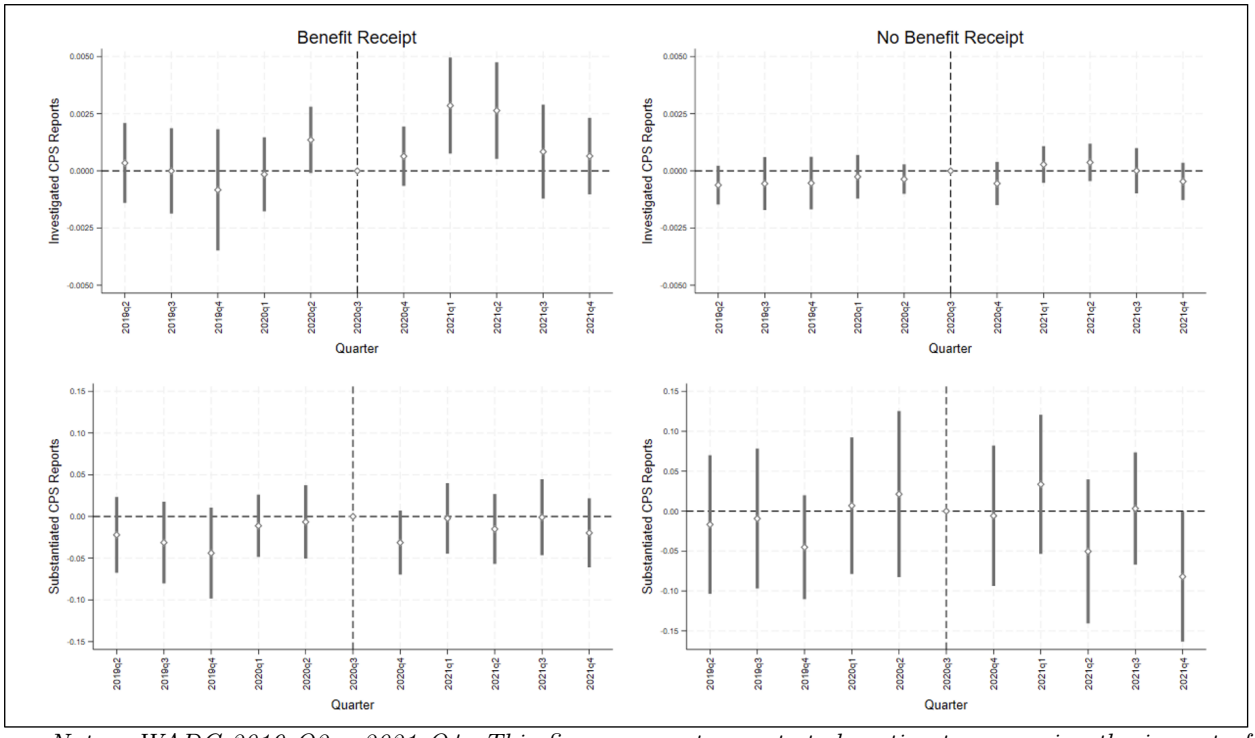
Notes: WADC 2019 Q2 – 2021 Q4. This figure presents event study estimates assessing the impact of in-person schooling on child protective services (CPS) outcomes, stratified by mother's region of residence. The four panels separately display estimates for investigated CPS reports and substantiated CPS reports, with stratification by whether the mother resides in a rural or urban/suburban area. Investigated reports refer to any CPS case that was formally investigated, while substantiated reports reflect cases where maltreatment was confirmed. The x-axis represents calendar quarters from 2019 Q2 to 2021 Q4, with 2020 Q3 as the reference period, indicated by the vertical dashed line. The y-axis shows the estimated coefficients from the event study specification, with 95% confidence intervals. Covariates included in the estimation are listed in Table 1 and include district and year fixed effects.Standard errors are clustered at the district level.

Figure B5: Event Study, CPS outcomes by mother's education



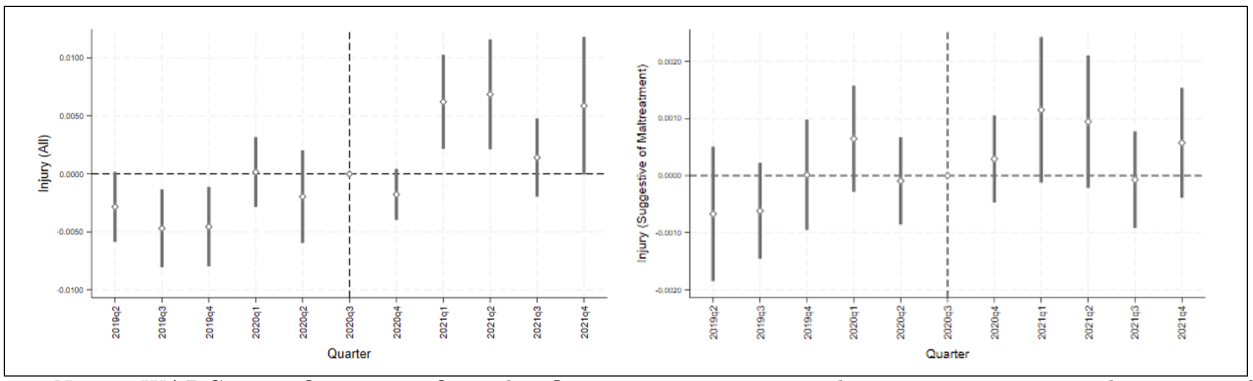
Notes: WADC 2019 Q2 – 2021 Q4. This figure presents event study estimates assessing the impact of in-person schooling on child protective services (CPS) outcomes, stratified by mother's education level. The four panels separately display estimates for investigated CPS reports and substantiated CPS reports, with stratification by whether the mother has a high school education or less versus some college education or more. Investigated reports refer to any CPS case that was formally investigated, while substantiated reports reflect cases where maltreatment was confirmed. The x-axis represents calendar quarters from 2019 Q2 to 2021 Q4, with 2020 Q3 as the reference period, indicated by the vertical dashed line. The y-axis shows the estimated coefficients from the event study specification, with 95% confidence intervals. Covariates included in the estimation are listed in Table 1 and include district and year-quarter fixed effects. Standard errors are clustered at the district level.

Figure B6: Event Study, CPS outcomes by mother's benefit receipt



Notes: WADC 2019 Q2 – 2021 Q4. This figure presents event study estimates assessing the impact of in-person schooling on child protective services (CPS) outcomes, stratified by mother's benefits receipt. The four panels separately display estimates for investigated CPS reports and substantiated CPS reports, with stratification by whether the mother received benefits (SNAP, TANF, Medicaid, or SSI) or did not receive benefits. Investigated reports refer to any CPS case that was formally investigated, while substantiated reports reflect cases where maltreatment was confirmed. The x-axis represents calendar quarters from 2019 Q2 to 2021 Q4, with 2020 Q3 as the reference period, indicated by the vertical dashed line. The y-axis shows the estimated coefficients from the event study specification, with 95% confidence intervals. Covariates included in the estimation are listed in Table 1 and include district and year-quarter fixed effects. Standard errors are clustered at the district level.

Figure B7: Event Study, Child Injuries – All injuries and Injuries Suggestive of Maltreatment



Notes: WADC 2019 Q2 – 2021 Q4. This figure presents event study estimates assessing the impact of in-person schooling on child injury outcomes, using a sample of Medicaid-enrolled children. The dependent variables include all injuries, and injuries suggestive of maltreatment. Injury outcomes are based on Medicaid claims. The subset of injuries suggestive of maltreatment includes drowning, fire and flame injuries, firearm-related injuries, poisoning, suffocation, and hot object or scald injuries—categories that are commonly associated with supervisory neglect or abuse. The x-axis represents calendar quarters from 2019 Q2 to 2021 Q4, with 2020 Q3 as the reference period, indicated by the vertical dashed line. The y-axis shows the estimated coefficients from the event study specification, with 95% confidence intervals. Covariates included in the estimation are listed in Table 1 and include district and year-quarter fixed effects Detailed ICD codes used to define injury categories are provided in Appendix Table A1. Standard errors are clustered at the district level.