# War and Work : Labor Market impacts of Nepalese Civil War

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**Abstract**

This paper studies the labor market consequences of Nepalese Civil War (1996 -2006 A.D.). Using two rounds of Nepal Labor Force Survey (NLFS) data, the article uses Inverse Probability Weighted (IPW) DiD to assess the effect of conflict intensity (as proxied by conflict deaths) on employment measures. Being in a conflict-intense region had minimal effect on the probability of being employed, although these regions saw reduced work hours. In rural areas, high-afflicted regions had increased likelihood of being self-employed.

## 1 Introduction

Nepal experienced a decade-long Civil War from 1996 to 2006 A.D. which claimed more than 13,000 lives and sustained countless injuries (Lawoti, 2009). While studies in this sphere have looked at the causes of the Insurgency (Nepal et al., 2011; Do and Iyer, 2010) the economic consequences of conflict, apart from some macroeconomic estimates on Tourism and GDP, are understudied and unclear. Given the widespread destruction of infrastructure, conscription of ephebian soldiers by the Maoists, the use of extreme force by the government, and an overall air of violence and uncertainity, there were likely detrimental human capital and employment impacts of the Nepalese Civil War. This paper aims to identify the causal impact of said civil war on long-term employment measures.

The estimand is: How does increased conflict intensity, measured by the number of conflict-related deaths per 10,000 individuals, impact an individual's likelihood of employment? Does experiencing high-intensity of conflict lead to a decrease or an increase in employment rates? Nepal Labor Force Survey I (NLFS I, 1998) and NLFS II, 2008 are used for individual-level employment data, and Uppsala Conflict Data Program (UCDP) is used for measures on conflict causalties. To estimate the causal effect, repeated cross-section Difference-in-Differences is used as the estimator whereby 1998 A.D. serves as the pre-period, and 2008 A.D. as the post-conflict period. The sample is reduced to districts that experienced 0 conflict causalties in 1998 A.D. and a DiD estimator with a continuous treatment design is implemented because by the end of 2008 A.D. there were no untreated units i.e. almost all districts of Nepal had experienced some form of conflict. Inverse Probability Weigthing (IPW) DiD is used alongside DiD to strengthen Strong Parallel Trends (SPT) assumption.

This estimand was decided upon for two reasons: a) To add to the understudied literature of micro-level economic consequences of violent conflict and b) To aid future policy design for better post-conflict recovery. Section 2 provides a brief review of the current literature on conflict causes and consequences, Section 3 sketches a simple theory linking conflict and the labor market, Section 4 describes the data and the variables used, Section 5 provides an extensive discussion on the methodology, its robustness and credibility (and absence thereof), Section 6 displays and summarizes the main results and Section 7 concludes.

## 2 Literature Review

The first studies that looked at the impact of civil war used cross-country measures on prosperity like GDP and GDP per capita to be linked conflict incidence (Blattman and Miguel, 2010).Cross country studies have mainly focused on examining the source of conflict incidence i.e. what factors lead to either presence/absence of conflict or more importantly, more/less conflict causalties. For this, they have relied on various theoretical frameworks from growth theory to information assymetries, informal contracts as well as the nature of ethnic and linguistic fractionalization measures. As for macroeconomic economic legacies, evidence is viewed under two frameworks: one of growth theory (which predicts post-war recovery to follow pre-war steady state) and the other of poverty trap (lasting economic and social legacy). Microeconomic inspection, for either causes or consequences of civil war, are few and far between.

The few within-country microeconomic legacies of conflict literature, which is also the scope of this paper, have used cross-sectional data and DiD to justify their causal identification of post-war legacy. Justino and Verwimp (2013) find that 20 percent of the Rwandan population moved into poverty following the Rwandan genocide. Using panel data on child nutritional measures, Alderman et al. (2006) find that young children who suffered from war-related malnutrition in Zimbabwe are significantly shorter as adults, affecting their labor productivity. Regarding the labor market in Tajikistan, Shemyakina (2011) reports that girls whose homes were destroyed in the civil war were less likely to obtain secondary education, with long-term dominoe effect on later-life wages.

While Nepal's Civil War did disrupt regional economic conditions, most literature pertaining to Nepal's Maoist conflict have analyzed it qualitatively, some through the lens of political economy and the evolution of Nepal's political history, while others have used ethnographic small sample evidences to verify their hypotheses (Lawoti and Pahara, 2010). One of the first papers to prioritize empircal evidence to understand the source of Nepal's conflict was Do and Iyer (2010). Their within-country, district level analysis showed that conflict deaths are significantly higher in insurgent favorable geographic areas, such as mountains and forests as well as poorer districts. Following and building on this empirical vein, I find three papers that have used rigorous causal evidence to analyze either the economic causes and consequences of the civil war. Regarding the conflict's source or hypothesized causes, Nepal et al. (2011) find that local jurisdictions in Nepal with higher inequality experienced more killings while poverty did not increase violence (in contrast to (Do and Iyer, 2010)). The second and third papers pertain to the consequences of the civil war: Valente (2014) finds, counterintuitively, that conflict casualties were associated with an increase in female educational attainment while Menon and Van der Meulen Rodgers (2015) find, in a similar vein, that women's likelihood of employment increased as a consequence of the conflict.

# 3 Underlying Theory.

A violent conflict disrupts regional economic activity in a myriad of ways. Destroyed infrastructure, reduced mobility, conscription of able-bodied men and women (both in state forces as well as the opposing guerilla forces), reduced tourism due to insecurity, extortion of local food and resources and so on.

Concerning the labor market, the impact of a violent conflict on regional labor market outcomes may follow two stylized paths:

1. Labor Demand Path : In the conflict decade of 1996-2006 A.D. a) tourist arrival declined in Nepal, b) investment in manufacturing and service industries diminished due to poor law and order, c) existing industries and service sectors suffered faced extortions and closures, and d) many banks were looted and destroyed (Lawoti, 2009). As for infrastructural damages, the maoists had destroyed 1,683 or 43 percent of Village Development Committees(VDC) office buildings as well as many other police posts, airports, electricty stations, bridges and so on (Lawoti, 2009). With decreasing expected returns, increase in risk, and short investment horizons, it is plausible to expect a fall in labor demand from formal and informal firms in conflict afflicted zones.

2. Labor Supply Path: The conscription in civil wars often involve underage school going children, whose educational and career trajectory is tarnished by involvement in conflict. Post war, they may find it harder to match with potential employers increasing unemployment. Labor supply disruption is further exacerbated by the fact that many who fight in the war are either killed or become life-longed disabled, reducing employability. Labor Supply also may be quantitatively smaller in conflict-afflicted areas due to high volumes of emigration, where, an INSEC report from 2008 reports that between 2002 and 2004, 50,365 were displaced in Nepal: 3,837 and 21,320 people by the state and the Maoists respectively, whereas 25,199 persons emigrated from fear and terror.

It is important to note that for both of these paths, an alternative income effect hypotheses is equally plausible. Regarding labor demand, if local firms, in response to lower returns, offer higher wages to attract workers and stimulate economic activity in efforts for survival, conflict-afflicted zones may actually have more employment than their peaceful counterparts. In a similar vein, if displaced men are replaced by women in the workforce, or if higher wages tempt some men to stay put, labor supply, again, may be higher in conflicted afflicted regions.

# 4 Study Area, Data and variables

The research topic requires information both on labor market characteristics as well as conflict incidences and casualties. For information on Nepal's labor market, I use two rounds of Nepal Labor Force Survey: NLFS 1 which was conducted in 1998 A.D. and NLFS 2 from 2008 A.D. The usage of NLFS is amenable to our analysis in two ways:

1. It is an individual-level survey and records important microeconomic employment characteristics such as hours worked in the past 7 days, type of work a person is involved in, reasons for inactivity, any education or training received and other variables pertinent to any labor market analysis.

2. NLFS is a nationally representative survey, with large sample sizes, and obtained through two-stage stratified sampling procedure based on Probability Proportional to Size. This allows users of the survey to make inferences not only within the sample, but also extrapolate to the whole superpopulation of Nepal.

Labor Market data from both rounds of NLFS is then merged with data on Nepal's Conflict Incidences from Uppsala Conflict Data Program (UCDP), collected and maintained by Uppsala University Sweden. [1]. UCDP has timed records of all casualties

[1] https://ucdp.uu.se/

during the people's war with geo-spatial information on the district of incidence, number of deaths (with lower and upper bound estimates), who died – Maoists, civilians or the police officials, and the dyadic context of the conflict i.e. who initiated the conflict based on the source

Given our main variable of interest relates to work, it is important to be clear and transparent about what is deemed work and what isn't. Work activities in the NLFS are based on ILO standards which are in line with 1993 System of National Accounts (SNA). A major caveat to classifying work activities involves the inclusion of collecting firewood and fetching water. Given that a high proportion of people living in rural areas do have to collect their own firewood or carry water, extending the production boundary to include these activities may skew our estimates given our differnet perception of what counts as work. As such, to account for this, I have generated a new variable with suffx 'out' or 'outside' which doesn't include collecting firewood or fetching water. For eg. Currently Employed Outside (1/0) is not turned on when a person's work exclusively involved collecting firewood or fetching water.

The main variables used in my analysis along with their description is provided in the following table.

Table 1: Variables and Description

| Variable | Description |
|---|---|
| Usually Employed (1/0) | A person is assumed usually employed when they have worked more than 180 days (NLFS I) or more than or equal to 6 full months (NLFS II) |
| Currently Employed (1/0) | There are two situations in which a person can be defined as being currently employed. Either the person is actually working in the reference week (here the last 7 days), or he or she has an attachment to a job or business but did not work during the reference week. |
| Currently Self Employed (1/0) | As a subset of currently employed, a person is currently self-employed if she fulfills working in the categories of work defined unde the self-employment category such as agriculture, handicraft, food processing at home for sale, etc. |
| Work Hours (cont) | The number of hours worked in the last 7 days. |
| Work Hours – Self Emp (cont) | The number of self employment hours in the last 7 days. |
| Conflict Deaths (cont) | From the UCDP dataset, the best estimate of the number of people that died in a conflict between Maoist Insurgents and the Government of Nepal between 1996 and 2006 A.D. (for every district) |
| Conflict Deaths per 10000 population (cont) | Conflict Deaths standardized to population of every district (population metrics from Nepal's 2001 Census) |
| Age (cont) | Age of the people surveyd |
| Ever Attented School (1/0) | A categorical variable indicating whether the person surveyed ever attended school. |
| Year of education (cont) | Total years of completed years of schooling, among those who did attend school i.e. among ever attended school = 1. |
| Years of education for all (cont) | Completed Years of schooling whereby those who never attended school are coded as 0. |
| Hindu (1/0) | If a person surveyed identifies as Hindu. |
| Brahmin/Chhetri (1/0) | If a person surveyed belongs to either Brahmin or Chhetri Caste i.e. the privileged class. |
| Male (1/0) | If a person is male. |
| Married (1/0) | A variable indicating whether a person is currently married. |
| Household Size | The size of the household that a person belongs to. Members from the same household have the same household size. |
| Urban(1/0) | Whether a person's household resides in an urban or a rural area. |

# 5 Empirical Method

This paper uses Difference-in-Difference to gauge the causal impact of Nepal's Maoist Civil War on the likelihood of employment. If Y denotes whether a person is usually employed, and if D denotes treatment(1/0) based on the level of conflict casualties in the given district[2], then canonical DiD setup, under the parallel trends assumption, estimates the Average Treatment Effect on the Treated (ATT) as:

$$ATT = (E[Y_{i,t=2}|D = 1] - E[Y_{i,t=1}|D = 1]) - (E[Y_{i,t=2}|D = 0] - E[Y_{i,t=1}|D = 0])$$

In our context, $t = 2$ represents the post conflict period (2008 NLFS) and $t = 1$ denotes pre-period (1998 NLFS). $D$ refers to treatment assignment and $Y$ refers to the usually employed status (the main outcome variable). While the canonical DiD setup is useful as a tool for preliminary analysis, estimating $ATT$ in this specific empirical context requires much stronger assumptions and correspondingly adequate estimators. The discussion of such constraints along with the proposed solution and estimator is discussed below:

## 5.1 Classification into Treatment and Control

There are two major difficulties into classifying Nepal's districts into conflicted-afflicted (treatment) and not-conflict-afflicted (control) groups :

1. The first concern pertains to the timing of pre-period for which I use 1998 Nepal Labor Force Survey (NLFS 1). The Civil War started in 1996 A.D. and by 1998 A.D. there were some districts that already had sizable conflict casualties.

2. Very few districts (Manang and Mustang) had zero conflict casualties at the end of the insurgency. As such, by the end of 2008 A.D., almost all districts were affected by the conflict, albeit to a varying degree. This is referred to in the literature as *no untreated units* classification.

I attend to the concerns in the following ways[3]:

1. I filter the sample to only those districts that had 0 conflict casualties up until the end of 1998 A.D.

[2]The mechanism behind treatment assignment is explained in the following section

[3]The following subsection is highly influenced by (Callaway et al., 2024). Please refer to it for further exposition and explanation

2. Given that conflict casualties per population is continuous, the canonical DiD non-parametric estimator for $ATT$ must be changed to:

$$E[\Delta Y|D = d] - E[\Delta Y|D = d_L] = ATT(d|d) - ATT(d_L|d_L)$$

   (a) Here, the specification is comparison of dose group $d$ with dose group $d_L$, the lowest possible amount of treatment. This is done because there are no untreated units in our empirical context. However, as is evident from the left-hand side of the equation, this estimator estimates the average causal response of moving from $d_L$ to $d$ with a selection bias term. Because we donot have any untreated units, the DiD is not able to use the outcome path of untreated units but rather uses the outcome evolution of units treated with $d_L$, and assumes this to be the untreated potential outcome for the treated group. This is a much stronger assumption and requires invoking the assumption of Strong Parallel Trends (SPT) (Callaway et al., 2024). Under strong parallel trends we have[4]:

$$E[\Delta Y|D = d] - E[\Delta Y|D = d_L] = E[Y_{t=2}(d) - Y_{t=2}(d_L)|D > 0]$$

   (b) The SPT assumption implies assuming lower dose groups are valid counterfactuals for higher dose groups. While this is a strong assumption, strong parallel trends can be more plausible if we condition on some observed covariates $X$.

If these arguments are summarized and constellated, we can estimate Average Treatment Effect on the treated in our empirical context as:

$$ATT_x(d) = E[Y_{t=2}(d) - Y_{t=2}(d_L)|X = x, D > 0]$$

As noted earlier, my classification entails, what is referred to in the DiD literature as, *no untreated units* specification. This means I am comparing units that are *"more treated" (d)* to units that are *"less treated"*$(d_L)$. This is evidenced from table 1, where differences in the dose of treatment (here, conflict-related deaths per 10000 population) is shown. Districts that went to have high death tolls are classified as *more treated* (treatment) and districts with significantly fewer deaths tolls as control. For this purpose, districts whose conflict casualties per 10000 population higher than the 75th percentile are classified as treatment while districts whose casualties are lower

[4]I follow the conventional potential outcomes framework as outlines in (Baker et al., 2025) and use similar notation. For notational reference, please check this paper.

than 25th percentile are classified as control.[5]
Table 1 shows that districts classified as treatment experienced 1.6 standard deviations higher conflict casualties per 10000 people than control districts. I argue, thus, that such classification into treatment and control districts in a high-dose/low-dose dynamic is justified.

Table 2: Conflict Deaths per 10,000 population regressed on Treatment Status

| | *Dependent variable:* |
|---|---|
| | Conflict Deaths per 10,000 population (std) |
| Treatment (1/0) | 1.632 |
| | (0.247) |
| Constant | −0.855 |
| | (0.179) |
| Observations | 21 |
| $R^2$ | 0.697 |
| Adjusted $R^2$ | 0.681 |
| *Note:* | (std) represents standardized variables |

## 5.2 Strong parallel trends and DiD with covariates

In the previous section, I mentioned that estimating causal effect of conflict intensity (as proxied by conflict deaths) on employment requires strengthening the parallel trends assumption. This implies that districts that experienced the low-dose become valid counterfactuals for districts in the high-dose. However, if more afflicted and less afflicted districts differ in important observable covariates, it is not plausible to assume that the trend of $Y$ – employment in control districts represent the untreated potential outcomes for treated districts. For eg: let's suppose that during 1998 A.D. (the pre-period), a person's years of education or ethnic background was strongly associated with their employment status. If this relationship would've held in the absence of conflict, and if highly-afflicted and low-afflicted districts differ in these demographic and economic characteristics, then parallel trends assumption may fail to hold. Therefore, as noted by Baker et al. (2025), checking balance in observable

[5]Conflict casualties per 10,000 population is used instead of raw conflict casualties information to relativize the magnitude of the impact of conflict. This is done because high death toll in a highly populated district and high death toll in a low population area do not signify the same impact.

determinants of $Y_i, t(d_L)$ is a sensible way to evaluate parallel trends (in this case strong parallel trends).

I check for the balance of the following variables: a person' age, years of education, hindu, brahmin chhetri, sex, marital status, household size, urban/rural status, and if the person ever attended school. Table 2 shows the averages of these variables by groups in 1998 A.D. along with the normalized difference[6] (Imbens and Rubin, 2015).

| | Norm. Diff | Treated Mean | Control Mean |
|---|---|---|---|
| Age | 0.20 | 40.56 | 37.42 |
| Years of Education (All) | -0.75 | 1.80 | 5.16 |
| Years of Education (If attended school) | -0.80 | 6.27 | 9.12 |
| Hindu (1/0) | 0.27 | 0.92 | 0.83 |
| Brahmin/Chhetri(1/0) | 0.69 | 0.57 | 0.25 |
| Male | -0.16 | 0.42 | 0.50 |
| Married | 0.03 | 0.80 | 0.79 |
| Household Size | -0.16 | 5.38 | 5.78 |
| Urban | -0.96 | 0.25 | 0.69 |
| Ever Attended School | -0.59 | 0.29 | 0.57 |

Table 3: Covariate Balance Statistics

There are meaningful imbalances in several baseline (pre-period) variables. Imbens and Rubin (2015) provide a general heuristic of normalized difference more than |0.25| as an indication of meaningful imbalance in treatment and control groups. As per the heuristic, highly afflicted and less afflicted regions differed in completed years of education, if they ever attended school, composition of Hindus and Brahmin Chhetris, as well as urban/rural residency. Such imbalance makes us congnizant of two important things:

1. First, conflict incidence is clearly not exogenous or random but rather is a consequence of the pre-conflict observable economic and demographic characteristics like education, ethnic composition and rurality.

2. Such imbalance will likely violate parallel trends if areas starting out with different education level or ethnic composition would have different employment trends in the absence of conflict itself.

Another measurement heuristic that help us gauge imblance in high and low conflic afflicted regions is the average changes in these characteristics from 1998 to 2008. Table 3 shows the 1998-2008 trend for some observable covariates.

[6]Norm Diff = $\frac{\bar{X}_T - \bar{X}_C}{\sqrt{0.5(SD_T^2 + SD_C^2)}}$

| | Control trend | Treatment trend |
|---|---|---|
| Age | 0.52 | 0.59 |
| Years of Education (All) | 1.20 | 1.37 |
| Years of Education (If attended school) | 0.67 | 1.25 |
| Hindu (1/0) | 0.00 | -0.04 |
| Brahmin/Chhetri(1/0) | 0.01 | -0.05 |
| Male | -0.02 | 0.01 |
| Married | -0.02 | 0.03 |
| Household Size | -0.24 | 0.41 |
| Urban | 0.00 | -0.06 |
| Ever Attended School | 0.09 | 0.15 |

Table 4: Covariate Balance Statistics for trends/differences (2008 - 1998)

As with pre-period covariates, there are some meaningful differences in treatment and control trends for some variables. Counterintuitively, high afflicted regions experienced higher increase in complete years of schooling, as well as school attendance. Similarly, while control districts experienced decrease in household size, treatment districts had a considerable increase. Observing covaraite imbalance in trends is useful if differences are meaningful contributors to changes in less treated potential outcomes $\Delta Y_{i,t}(d_L)$. For example, it is plausible that increase in completed years of schooling may have meaningful impact on a person's probability of employment. As such, using such a variable as a covariate seems sensible. However, a problem arises when the covariate itself may have been a consequence of the conflict. It is also highly plausible that the increased/decreased years of schooling is itself caused by treatment, and in such a case, it is not exogenous to the less-treated potential outcomes and may bias our estimate if we include it as a control. In summary, while exogenous covariate changes may indicate a violation of parallel trends, change in endogenous covariates may reflect a consequence of the treatment itself, and as such may not be appropriate as a control.

## 5.3 Inverse Probability Weighting (IPW)

The previous section showed that there were meaningful imbalances in various observable determinants of $\Delta Y_{i,t}(d_L)$. A way to estimate the Average Treatment Effect on the Treated (ATT) conditional on covariates is to use Inverese probability weighted (IPW) DiD Procedure (Abadie, 2005). The aim of the procedure is to improve the comparability of the comparison group – here the group that was less afflicted by conflict. According to Baker et al. (2025),

> The logic of IPW builds on ... if imbalance in covariates is the source of parallel trends violations, then adjusting the comparison group to be balanced on covariates can address that bias.

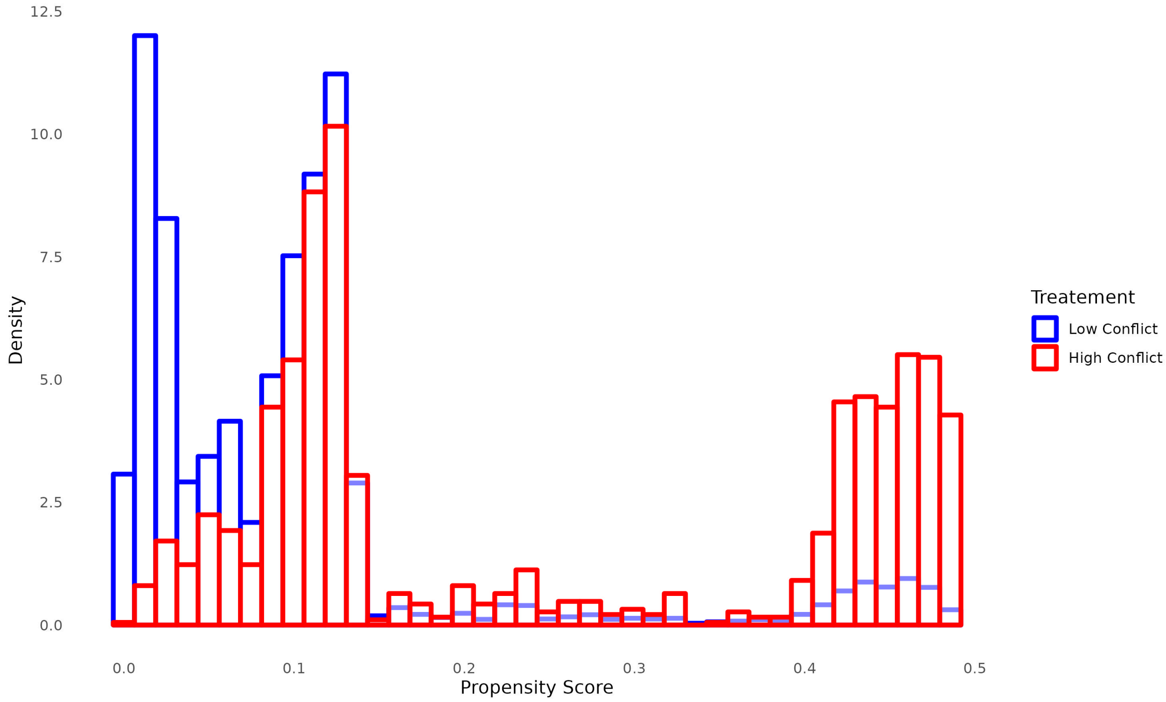
As the name suggests, IPW DiD procedure reweights the observed changes in employment status for low afflicted regions to ensure that high and low afflicted regions are similar on covariates. To construct these balancing weights, we need to model $p(X_i) = P(D_i = d|X_i)$ i.e. the propensity score of belonging to the high-afflicted group give the covariates. Using logistic model to estimate the parameters using maximum likelihood, the coefficients for the propensity score model are presented in Table 4.

The propensity score regression in table 4 shows that pre-period observable covariates like ethnicity and years of schooling do predict treatment assignment. If pre-period differences in treatment and control go on to influence untreated potential outcomes, then the IPW based DiD estimation must show different estimates than the conventional DiD. However, if the IPW DiD estimator is to be made more credible, it is important that there is strong overlap in propensity scores between high-afflicted and low-afflicted areas. Figure 1 shows that there is considerable overlap in our predicted propensity scores, and as such, the IPW DiD estimator is able to estimate ATT conditional on these covariates in a meaningful manner.

Table 5: Propensity Score Regression on observed covariates (1998 NLFS)

| | *Dependent variable:* |
|---|---|
| | Treatment (high-conflict) |
| Age | −0.006 |
| | (0.003) |
| Hindu | 0.260 |
| | (0.505) |
| Brahmin/Chhetri | 1.815 |
| | (0.259) |
| Years of Education | −0.219 |
| | (0.021) |
| Constant | −2.014 |
| | (0.473) |
| Observations | 12,607 |
| Log Likelihood | −3,814.769 |
| Akaike Inf. Crit. | 7,639.538 |
| *Note:* | Standard errors are psu-clustered |

Figure 1: Distribution of Propensity Scores




After such due diligence, we can finally gauge our ATT estimates and see the results.

# 6 Main Results

Table 5 reports two separate estimators for ATT. The first column reports the conventional 2 X 2 DiD design estimate, while the second column reports estimates from the IPW DiD procedure.

Conditional on covarites, people in high-afflicted districts were marginally (given high standard error) less likely to be employed for 6 months or more. On the other hand, we see positive estimates for self employment in the past 7 days. Conditional on covariates, people in high conflict afflicted region are more likely to be self-employed (exlucding collecting firewood and carrying water), albeit, again with moderately high standard errors.

The story is different when we look at work hours and self-employment hours. In both these dimensions, the IPW DiD estimate shows that people in high conflict afflicted regions worked fewer hours.

With little to no difference in the probability of being employed, it would be naive to

Table 6: DiD estimates with covariates.

| | Normal DiD | IPW |
|---|---|---|
| Usually Employed (1/0) | -0.0048 | -0.0201 |
| | (0.0121) | (0.0141) |
| Currently Employed | 0.0217 | 0.0081 |
| | (0.0112) | (0.0133) |
| Currently Employed | 0.0143 | 0.0011 |
| Outside | (0.0117) | (0.0137) |
| Currently Self Employed | 0.0253 | 0.0218 |
| Outside | (0.0135) | (0.0155) |
| Work Hours | -4.403 | -4.994 |
| Outside | (0.776) | (0.863) |
| Self Employed Hours | -4.172 | -4.41 |
| Outside | (0.844) | (0.885) |

Standard Errors in Paranetheses
Estimates calculated using *drdid R* package from (SantâAnna and Zhao, 2020)

assume that such a huge difference in work hours was, in its entirety, caused by the destruction and uncertainity of conflict. Differences in work hours maybe a function of differentially evolving industry composition in two areas, differentially evolving labor productivity, or any other violation of strong parallel trends. The covariates which helped laid the basis for conditional parallel trends in Table 5 estimates were age, Hindu, Brahmin/Chhetri and completed years of schooling. While conditioning on industrial composition and labor productivity is beyond the scope of this paper, a good misspecification test is to divide the sample into urban/rural and male/female counterparts to examine the validity of strong parallel trends. This amounts to assuming that the treatment effect maybe heterogenous and is line with Baker et al. (2025)'s advice on Heterogeneity Analysis, whereby a dataset can be paritioned into subsets and ATT identified within those subsets.

## 6.1 Misspecification/Heterogeneity Analysis

### 6.1.1 Rural Only Sample.

Table 7 reports the propensity score of being assigned into treatment (high conflict intensity) when the sample is reduced to only the rural areas. The four covariates that are used : age, Hindu, Brahmin/Chhetri and Years of education do predict treatment assignment quite well, even when standard errors are clustered at the psu level. Also, the propensity score overlap of figure 2 allows us to make inferences based on IPW (DiD design). The results for DiD estimates for the rural only sample are presented in table 8. The results for the first three variables: usually employed, currently employed and currently employed outside status, mimic that of the full sample where we see only moderate effects of conflict intensity (with moderately high standard errors). In contrast to the full sample, among rural areas, conflict does seem to have positive effect on the status of being self employed, especially under the IPW DiD. However, the stark negative results for work hours and self-employment hours in the full sample is not corroborated in the Rural Sample, with conflict having little to no effect on labor supply of rural citizens.

The results from the rural sample allow us to theorize that the negative effect we saw in Table 6 on work hours and self-employed hours were largely driven by the urban sample. This allows us to contextualize the negative findings in a different labor-market theory sense, whereby one could hypothesize that the urban areas affected by conflict had different evolution of industry or labor productivity compared to rural areas. It is also possible that given urban areas are generally the center of economic activities, an intense conflict may impact it more than rural areas, where economic activity is already subdued in the first place.

Table 7: Propensity Score Regression on observed covariates (1998 NLFS) for the Rural only Sample

| | *Dependent variable:* |
|---|---|
| | Treatment (high-conflict) |
| Age | −0.003 |
| | (0.004) |
| Hindu | 0.027 |
| | (0.664) |
| Brahmin/Chhetri | 2.312 |
| | (0.367) |
| Years of Education | −0.169 |
| | (0.026) |
| Constant | −1.545 |
| | (0.600) |
| Observations | 4,582 |
| Log Likelihood | −2,097.166 |
| Akaike Inf. Crit. | 4,204.333 |
| *Note:* | Standard errors are psu-clustered |

Figure 2: Distribution of Propensity Scores for Rural only Sample.

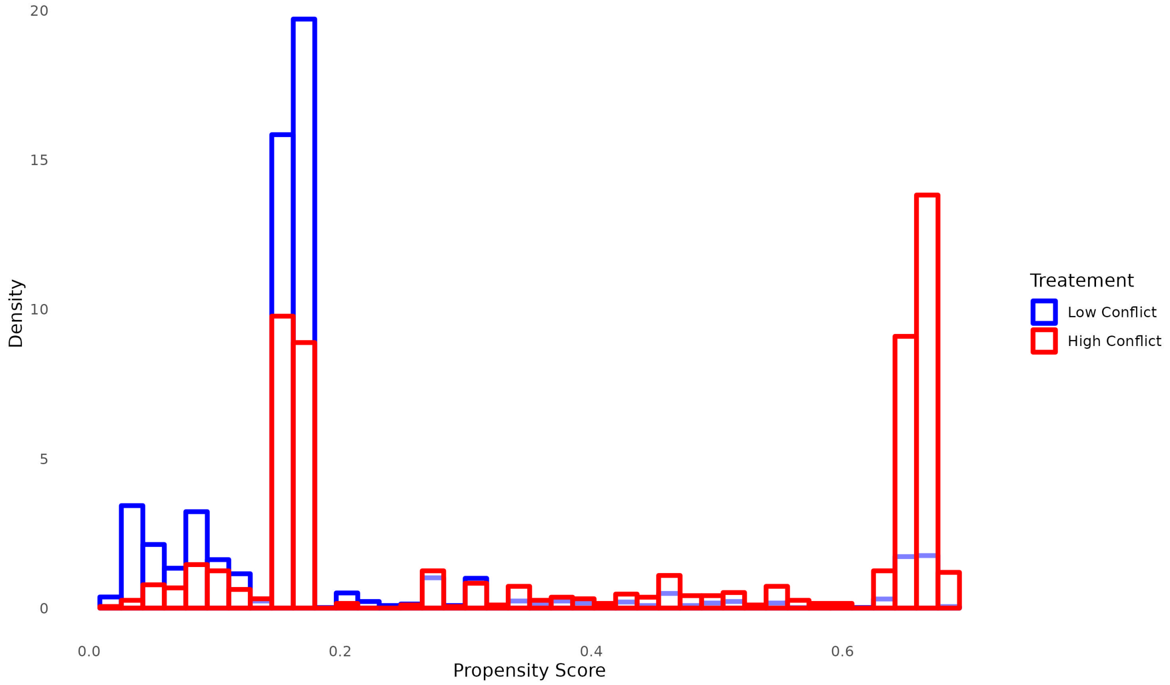


Table 8: DiD Estimates with covariates (Rural Only Sample)

| | Normal DiD | IPW |
|---|---|---|
| Usually Employed (1/0) | -0.0096 | -0.0122 |
| | (0.0142) | (0.0184) |
| Currently Employed | 0.0227 | 0.0215 |
| | (0.0135) | (0.0177) |
| Currently Employed | 0.0151 | 0.0177 |
| Outside | (0.014) | (0.0182) |
| Currently Self Employed | 0.039 | 0.0501 |
| Outside | (0.0165) | (0.0203) |
| Work Hours | -2.588 | -1.417 |
| Outside | (0.948) | (1.096) |
| Self Employed Hours | -2.414 | 0.4909 |
| Outside | (0.9867) | (1.128) |

Standard Errors in Paranetheses

# Conclusion

This paper investigates the labor market consequences of the Nepalese Civil War using repeated cross-section data from the 1998 and 2008 Nepal Labor Force Surveys, combined with conflict casualty data from UCDP. By leveraging a Difference-in-Differences framework and using Inverse Probability Weighted estimates to establish strong parallel trends, the study estimates the causal impact of conflict intensity on employment outcomes.

The results suggest that living in a conflict-affected district did not significantly reduce the likelihood of being employed, though individuals in highly afflicted areas tended to work fewer hours. In rural areas, however, conflict intensity was associated with a higher probability of self-employment.

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