

CASTE, CONFLICT, AND THE STRATIFICATION OF WELFARE ENGAGEMENT: EVIDENCE FROM RURAL INDIA

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Abstract

This paper examines how caste-based conflict shapes household engagement with state welfare programmes in rural India. Using the India Human Development Survey-II (2011–12), we find that village-level conflict and locally practiced untouchability significantly reduce a composite index of institutional confidence, with the largest effect among Scheduled Caste households reporting direct experience of untouchability. Eroded confidence does not, however, suppress entry into welfare schemes, particularly the Mahatma Gandhi National Rural Employment Guarantee Scheme. Confidence does predict engagement intensity, measured as NREGA days worked, but the relationship is concentrated among Other Backward Class and Scheduled Tribe households. Scheduled Caste households show no confidence-days relationship, plausibly because their allocated days already approach the statutory ceiling. The caste-stratified pattern suggests that conflict, confidence and welfare engagement do not operate through a single behavioural channel, and that the political economy of NREGA is shaped by which group encounters which margin of the programme.

Keywords: caste, conflict, institutional confidence, NREGA, welfare engagement, India, IHDS

JEL Classification: D74, H53, I38, O12, J15

1. Introduction

About 21 per cent of households in the India Human Development Survey-II (2011–12) live in villages where untouchability is reported as actively practiced, and roughly 23 per cent of Scheduled Caste households indicate that someone in the family has personally experienced untouchability in the preceding five years. These figures, drawn from a nationally representative sample of over 41,000 households, locate caste discrimination as a chronic feature of rural Indian life rather than an episodic one. The conflict thus indexed is not the kind that has occupied much of the development-conflict literature, which has tended to focus on civil war, armed insurgency, or large-scale displacement (Collier et al. 2003; Blattman and Miguel 2010). Caste-based conflict instead persists in the texture of

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everyday interaction, in access to public goods, in labour markets, and in the operation of the very state institutions that are constitutionally tasked with addressing it (Mosse 2018).

A note on terminology is needed at the outset, since the word conflict carries a narrower meaning in parts of the development literature than the phenomena studied here. Much of that literature reserves conflict for episodes of organised violence between armed groups. The caste phenomena recorded in the IHDS-II rarely take that form. What the survey instruments capture is closer to chronic tension and contestation among caste groups, together with the practice of untouchability, which operates against Scheduled Caste households as a form of everyday or soft violence rather than open confrontation (Ambedkar 1936; Mosse 2018). We retain the word conflict, following Tilly (1998, 2005), for whom durable categorical inequalities are themselves institutionalised conflict sustained through systematic exclusion rather than through violent incident. Where the distinction matters in what follows, tension and contestation refer to the graded everyday form and confrontation to the episodic form. The conflict measures used below should accordingly be read as measures of perceived local tension rather than counts of violent events.

Whether this kind of conflict affects how households engage with the state remains an open empirical question. A substantial body of work in political sociology argues that distrust in public institutions suppresses citizen engagement with welfare provision, with downstream consequences for programme reach and effectiveness (Rothstein 2005; Kumlin 2004; Hetherington 2005). The intuition is straightforward enough. Citizens who have been failed or mistreated by state institutions are likely to avoid those institutions where they can, including when those institutions offer transfers and services. Whether the intuition travels to settings of acute rural poverty in India remains less clear. When the alternative to a government workfare scheme is no income at all, the attitude-to-action chain that holds in European welfare states may not operate in the same way.

Three empirical questions follow. Does exposure to caste-based conflict erode institutional confidence among Indian rural households? If it does, does that erosion translate into reduced participation in government welfare schemes? And if participation is largely unaffected, are there other margins of engagement that respond to institutional attitudes? Answering these questions requires data that simultaneously captures conflict exposure, institutional attitudes, and welfare engagement at the household level. The India Human Development Survey-II contains exactly this combination, and to our knowledge has not previously been used to trace the full conflict-confidence-engagement chain in a unified empirical framework.

The findings reported here are mixed in ways that are themselves informative. Conflict at the village level robustly reduces institutional confidence ($\beta = -0.035$, $p < 0.001$), with the effect detectable across most of the four institutional domains measured in the IHDS-II battery. For Scheduled Caste households reporting direct personal experience of untouchability, the confidence penalty is larger by a factor of about 2.5 ($\beta = -0.083$, $p < 0.001$). These results are robust to district fixed effects, alternative confidence indices, and binary conflict indicators. The trust-suppression hypothesis, however, breaks down at the next stage. Institutional confidence has no significant effect on binary NREGA

participation ($\beta = 0.005$, $p = 0.318$) or on a composite welfare uptake index. A small positive association with health insurance receipt is detected ($\beta = 0.009$, $p < 0.05$), but the magnitude is sufficiently modest that it does not substantially alter the broader pattern. Households appear to enter welfare programmes largely regardless of how they regard the state, which is consistent with evidence that workfare programmes provide a binding income floor for the rural poor (Ravi and Engler 2015; Imbert and Papp 2015).

The picture shifts again when programme engagement is measured by NREGA days worked rather than by binary participation. Higher institutional confidence significantly predicts more NREGA days ($\beta = 0.082$, $p = 0.001$), and the effect strengthens under district fixed effects ($\beta = 0.114$, $p < 0.001$). The relationship is not uniform across caste groups. It is largest for Other Backward Class households ($\beta = 0.083$, $p = 0.011$) and Scheduled Tribe households ($\beta = 0.147$, $p = 0.067$). Scheduled Caste households show no statistically detectable relationship between confidence and days worked. One possible explanation is that the days allocated to SC participants already approach the hundred-day statutory ceiling, with SC participants averaging roughly 97.8 days conditional on participation, leaving little room for institutional attitudes to push days higher. The differential is worth noting more than it is worth over-interpreting.

The paper contributes three things to the development-conflict literature. First, it provides nationally representative empirical evidence on how a chronic, structural form of social conflict shapes citizen attitudes toward state institutions in India, a setting where this question has been studied mostly in qualitative or local-quantitative terms. Second, it tests the trust-suppression hypothesis at the margin of welfare programme entry and finds it does not hold for India's flagship workfare scheme, contributing to a small but growing body of work distinguishing programme entry from programme engagement intensity (Niehaus and Sukhtankar 2013; Banerjee et al. 2020). Third, the caste-stratified results suggest that the political economy of welfare engagement in rural India does not operate through a single mechanism uniformly across the caste hierarchy. Different groups encounter different binding constraints, and analyses that aggregate across these constraints may obscure as much as they reveal.

What follows is organised in the conventional way. Section 2 reviews relevant literature on trust, welfare engagement, and caste. Section 3 describes the data and the construction of key variables. Section 4 sets out the empirical strategy. Section 5 presents results. Section 6 discusses interpretation and limitations, and Section 7 concludes with some implications for programme design.

2. Related Literature

2.1. *Institutional Trust and Welfare Engagement*

Whether citizens trust the institutions that administer welfare programmes has been argued to matter for whether and how those citizens engage with the programmes in question. The European welfare-state tradition has examined this link extensively. Rothstein's analysis of "social traps" emphasises that low trust in public bureaucracies

can generate self-reinforcing cycles of non-cooperation, with citizens withdrawing from collective welfare arrangements when they perceive those arrangements as unreliable or unfair (Rothstein 2005). Kumlin's work on Scandinavian welfare states traces how direct experience with welfare bureaucracies feeds back into broader political attitudes, including trust in government (Kumlin 2004). Hetherington documents related dynamics for the American case, arguing that low political trust constrains support for redistributive policy (Hetherington 2005). A comparative synthesis of this work appears in the *Handbook on Political Trust* (Kumlin and Haugsgjerd 2017), which integrates the various national findings into a broader theoretical frame.

Whether the same mechanism operates in lower-income countries with incomplete welfare systems is a separate empirical question. The literature is thinner here, and the available evidence less unified. Cross-national survey work suggests that institutional trust is correlated with political participation in developing democracies, but the link to welfare uptake specifically is rarely examined (Krishna 2002). For India, Kruks-Wisner's work on rural Rajasthan documents how citizens engage in "claim-making" against state institutions and how this engagement is conditioned by social networks and prior experience (Kruks-Wisner 2018). Auerbach's study of urban Indian slums shows that informal political brokerage often substitutes for direct institutional engagement among marginalised populations (Auerbach 2020).

The trust-suppression hypothesis, as it travels to the Indian rural context, carries with it some assumptions that may not survive contact with the data. One is that welfare programme participation is meaningfully voluntary, in the sense that households choose whether to participate based partly on their attitudes toward the providing institutions. This assumption is plausible in middle-income welfare states where the marginal participant is selecting between several income sources. It is harder to sustain in rural India, where the relevant comparison for many NREGA participants is between government wage labour and no wage labour at all. Whether the trust-engagement chain operates differently under economic compulsion is something the existing literature has not directly examined, at least to our knowledge.

What an expression of institutional confidence contains deserves the same care as what conflict contains. In the European studies cited above, trust in the welfare state is largely a judgment about fairness, about whether bureaucracies treat claimants impartially (Rothstein 2005; Kumlin 2004). In the Indian setting a prior question intervenes, namely whether the state has the capacity to deliver anything at all. The IHDS-II items are phrased functionally. Respondents report confidence in the police to enforce the law, in the state government to look after the people, in government hospitals and doctors to provide good treatment, and in the courts to deliver justice. Each item therefore bundles an assessment of competence with an assessment of fairness into a single evaluation of whether the institution performs its stated function for people like the respondent. A household with very little agency may report low confidence in this functional sense even where it perceives no active bias, simply because extracting what is due from the state, what Kruks-Wisner (2018) calls claim-making, requires effort, standing, and follow-through that the household

cannot supply. Section 3 records the exact question wordings, and Section 6 returns to the interpretive consequences of this bundling.

2.2. Caste, Discrimination, and Welfare Programmes in India

Caste requires some framing for readers outside the Indian literature, because it differs from class and from race in ways that bear directly on the analysis. Ambedkar's classical account describes the system as graded inequality, an ordering in which each stratum holds rank over those below while accepting subordination to those above, reproduced through endogamy and the ritual idiom of purity and pollution (Ambedkar 1917, 1936). Untouchability sits at the bottom of this gradation and operates less through episodic assault than through everyday denial, of entry, of touch, of shared food and water, a soft violence that leaves few marks the criminal law can see. Two features distinguish this configuration from the settings in which the trust and welfare literature developed. Discrimination here is hereditary and ritually sanctioned rather than only economic, and it persists inside modern markets rather than at their margins (Deshpande 2011). And the Indian state has taken the removal of caste discrimination as an explicit constitutional object, abolishing untouchability under Article 17 and legislating against its practice through the Protection of Civil Rights Act of 1955 and the Scheduled Castes and Scheduled Tribes (Prevention of Atrocities) Act of 1989. The state is therefore at once the arena in which discrimination is experienced and the instrument through which its removal is promised. Confidence held by Scheduled Caste households in state institutions carries a weight under this duality that it would not carry elsewhere.

The economic documentation of caste is substantial. Thorat and Newman's edited volume gathers evidence that caste-based discrimination operates in labour markets, in credit allocation, and in the delivery of public services (Thorat and Newman 2010). Anderson, Francois, and Kotwal find that upper-caste village dominance reduces the wages of lower-caste workers through clientelistic mechanisms that operate even where the state is nominally present (Anderson, Francois, and Kotwal 2015). Pande's earlier work showed that mandated political reservation for Scheduled Castes and Scheduled Tribes shifts redistributive policy outcomes in their favour, indicating that representation matters even in contexts where the state is supposed to be impartial (Pande 2003). Hoff and Pandey demonstrate experimentally that the salience of caste identity itself affects cognitive performance and risk-taking, suggesting that the attitudinal consequences of caste discrimination operate at a psychological level as well as a structural one (Hoff and Pandey 2006; Akerlof and Kranton 2000).

Untouchability remains a particularly difficult phenomenon to study, partly because it is constitutionally prohibited and partly because its practice is often concealed or normalised. Dasgupta and Pal provide one of the more careful empirical treatments, using IHDS data to examine how village-level community power shapes the practice of untouchability among upper and backward caste Hindus (Dasgupta and Pal 2021). Their analysis emphasises the role of group contestation and caste power, both of which are relevant to the present paper. Mosse documents that caste functions as a dynamic institution in modern Indian markets,

with discrimination persisting in labour markets and business networks despite extensive constitutional and policy prohibitions (Mosse 2018).

The implementation of NREGA has been studied from multiple angles, and a recurring theme is that programme administration is not caste-neutral. Niehaus and Sukhtankar provide some of the most careful evidence on this, documenting that local officials systematically skim wages from NREGA workers, with embezzlement rates that respond to changes in official wage rates (Niehaus and Sukhtankar 2013). Banerjee and colleagues demonstrate experimentally that e-governance reforms reduce leakage substantially, suggesting that administrative discretion at the local level is a major source of programme inefficiency (Banerjee et al. 2020). Olken's earlier work on Indonesian rural infrastructure programmes established that monitoring intensity affects local corruption in predictable ways, providing a useful comparative anchor for the Indian findings (Olken 2007).

Despite these implementation problems, the literature broadly agrees that NREGA has had positive effects on rural welfare. Imbert and Papp find that the programme raised private-sector wages and reduced poverty in early implementation phases (Imbert and Papp 2015). Ravi and Engler document substantial consumption gains for participating households despite the discrimination documented elsewhere in the literature (Ravi and Engler 2015). The puzzle is how a programme with documented administrative bias nonetheless reaches the most marginalised groups in absolute terms. One way to read the puzzle is to recognise that programme entry and programme engagement intensity may be governed by different mechanisms. Marginalised households may participate because participation is the only available alternative to severe income deficit, even when they distrust the institutions running the programme. What happens once they are inside the programme may then depend on factors beyond mere willingness to participate, including the discretion exercised by local officials and the household's capacity to push back against under-allocation. Whether this distinction operates empirically is what the present paper examines.

2.3. Conflict, Confidence, and Indian Institutions

The development-conflict literature has examined many forms of conflict, but caste-based discrimination has not figured prominently in this body of work. Collier's influential framework conceptualises conflict primarily as armed violence between organised groups (Collier et al. 2003). The civil war literature reviewed by Blattman and Miguel similarly focuses on episodic, large-scale violence (Blattman and Miguel 2010). These framings are appropriate for many of the conflicts studied in the development literature but leave little analytical space for chronic, structural forms of social conflict like caste discrimination.

A more accommodating frame is provided by Tilly's work on trust networks and durable inequalities, which conceptualises caste-like categorical distinctions as institutionalised forms of social conflict that operate through systematic exclusion rather than through episodic violence (Tilly 2005; Tilly 1998). Under this framing, the relevant outcome is not the count of violent incidents but the patterned distribution of trust, opportunity, and access across categorical groupings. Adapting this frame to the Indian case is what permits

the present analysis, which treats caste conflict as a structural condition with measurable attitudinal consequences. Hardin's work on the analytical foundations of trust complements this framing, distinguishing the conditions under which trust functions as a useful social heuristic from those under which it breaks down (Hardin 2002).

Institutional confidence in India has been examined in several studies, though typically not in connection with caste conflict directly. Chhibber and Verma show that political attitudes in India are organised by ideological cleavages, particularly around statism and the recognition of marginalised groups, that have implications for how citizens regard public institutions (Chhibber and Verma 2018). Mansuri and Rao review the broader evidence on participatory development and document substantial heterogeneity in citizen engagement with state institutions across different social and political contexts (Mansuri and Rao 2013). The literature has not, as far as we can tell, traced confidence forward to actual welfare engagement outcomes within a single empirical specification.

The gap addressed by this paper is therefore relatively well defined. We ask how a chronic, caste-based form of social conflict affects institutional confidence, and how that confidence in turn shapes welfare engagement at multiple margins. We allow the relationships to vary across caste groups, which the existing literature suggests they should. The argument is not that we resolve the long-running debate about caste and welfare in India, but that we offer a unified empirical treatment of a chain that has previously been studied in pieces. Putnam's earlier work on civic engagement provides a useful comparative reference for thinking about how institutional trust relates to development outcomes, though the Indian context differs sufficiently that direct transposition of his findings would be inappropriate (Putnam 1993).

3. Data and Measurement

The empirical analysis draws on the India Human Development Survey-II (IHDS-II), a nationally representative panel survey conducted in 2011–12 by the University of Maryland and the National Council of Applied Economic Research (Desai and Vanneman 2015). The survey covers 42,152 households across all major Indian states and union territories, with a complex multistage sampling design that includes primary sampling units (PSUs) at the village and urban block level. After applying listwise deletion on key variables, the main analytical sample comprises 41,545 households. A focused subsample of 8,199 Scheduled Caste households is used for analyses that exploit a direct discrimination question asked only of SC respondents.

Table 1 lays out every construct used in the analysis, the IHDS-II item or items from which it is built, the item wording as recorded in the survey instrument, the admissible

responses, and the construction applied in this paper. Researchers working with the IHDS-II should be able to reproduce each variable from this table alone.

Table 1. Variable Definitions, IHDS-II Sources, and Question Wording

Construct	IHDS-II item	Item wording in IHDS-II	Response categories	Construction in this paper
Village conflict intensity	TR1 (HQ 19.1)	“Conflict in village”. Amount of conflict in the village or urban neighbourhood.	1 Lot of conflict, 2 Some conflict, 3 No or not much conflict	Reversed as 4 – TR1 so higher values indicate more conflict (range 1–3)
Inter-caste conflict intensity	TR3 (HQ 19.3)	“Conflict among communities/jatis in village”.	Same as TR1	Reversed as 4 – TR3 (range 1–3)
Untouchability practiced (village)	TR4A (HQ 19.4a)	“Practice untouchability”. Whether some people in the village practise untouchability.	0 No, 1 Yes	Binary indicator
Untouchability experienced (household)	TR4C (HQ 19.4c)	“Household members experienced untouchability last 5 years”.	0 No, 1 Yes	Binary indicator. Asked only of Scheduled Caste households, used in the SC subsample
Confidence in police	CI3 (HQ 21.3)	“Confidence: Police, to enforce the law”.	1 A great deal of confidence, 2 Only some confidence, 3 Hardly any confidence at all	Reversed as 4 – CI3 so higher values indicate more confidence (range 1–3)
Confidence in state government	CI4 (HQ 21.4)	“Confidence: State government, to look after the people”.	Same as CI3	Reversed as 4 – CI4
Confidence in government hospitals	CI9 (HQ 21.9)	“Confidence: Government hospitals and doctors, to provide good treatment”.	Same as CI3	Reversed as 4 – CI9
Confidence in courts	CI11 (HQ 21.11)	“Confidence: Courts, to deliver justice”.	Same as CI3	Reversed as 4 – CI11
Institutional confidence index	CI3, CI4, CI9, CI11	Composite of the four items above.	1–3 continuous	Mean of the four reversed items. Missing if any component is missing
NREGA participation	NWKNREGA (HQ 7.3)	“Number of workers (at least 240 hours): NREGA”.	Count of household members	1 if NWKNREGA > 0, else 0
NREGA days worked (household)	IN21, individual file (HQ 9.21)	“NREGA: number of days worked” per member.	Days, 0–100 per member	Summed across household members, then $\ln(1 + \text{days})$
Government health insurance	IN15B1 (HQ 9.15b)	“Health insurance: government” received in last five years.	0 No, 1 Yes	Binary indicator
Government latrine or toilet	IN15D1 (HQ 9.15d)	“Latrines, toilets: government” received in last five years.	0 No, 1 Yes	Binary indicator

(Contd.)

Construct	IHDS-II item	Item wording in IHDS-II	Response categories	Construction in this paper
Kisan Credit Card	IN15E1 (HQ 9.15e)	Kisan Credit Card received from government.	0 No, 1 Yes	Binary indicator
Welfare uptake index	IN15B1, IN15D1, IN15E1	Composite of the three benefit indicators above.	0–3 count	Count of benefits received, missing components treated as zero
Per capita income	INCOMEPC	Total annual household income divided by household size.	Indian rupees per year, 2011–12 prices	ln of max(INCOMEPC, 1). See Section 3 and the robustness checks in Section 5.6
Household education	HHEDUC (HQ 11.6)	“Highest adult education” in the household.	Years, 0–16	Used as reported
Urban residence	URBAN2011	Urban residence from Census 2011 classification.	0 Rural, 1 Urban	Binary indicator
Panchayat member in household	ME14 (HQ 18.14)	“Panchayat member/official in household”.	0 No, 1 Yes	Binary indicator
Caste category	ID13	Caste or community category of the household.	1 Brahmin, 2 Forward, 3 OBC, 4 SC, 5 ST, 6 Other	Five categories used as subsamples. “Other” excluded from heterogeneity analysis

Note: Item wordings and response categories are transcribed from the ICPSR 36151 codebook for IHDS-II (Desai and Vanneman 2015). HQ references give the household questionnaire section and item number. IN21 is drawn from the individual-level file and aggregated to the household.

Conflict exposure is measured at the village level using two ordinal variables and one binary variable. The first ordinal measure (TR1) records respondents’ assessment of general conflict intensity in the village. The second (TR3) records conflict among communities and jatis on the same scale. Both variables are re-coded so that higher values indicate more conflict. The binary variable (TR4A) indicates whether untouchability is practiced in the village, with about 21 per cent of households reporting that it is. For Scheduled Caste households, an additional binary variable (TR4C) records whether any household member has personally experienced untouchability in the preceding five years, with about 23 per cent of SC respondents acknowledging such experience.

Institutional confidence is constructed as a composite index of reported confidence in four state institutions, namely the police (CI3), the state government (CI4), government hospitals (CI9), and the courts (CI11). Each item names the function against which the institution is to be judged, and each is measured on a three-point scale re-coded so that higher values indicate greater confidence. The composite index is the mean of the four items, with mean value of 2.29 (SD 0.43) on a 1–3 scale in the analytical sample. Dropping any single component does not substantively change the results, which is worth noting in connection with robustness.

Welfare scheme engagement is measured along two dimensions. The first is binary participation, coded as 1 if the household has at least one member working in NREGA

(NWKNREGA > 0), and similar 0/1 indicators for receipt of three other welfare benefits (government health insurance, government-provided toilet, and Kisan Credit Card). A welfare uptake index is constructed as the sum of the three non-NREGA receipt indicators. The second engagement dimension is intensity, measured as the log-transformed sum of NREGA days worked across all household members (constructed from individual-level IN21 reports). About 7.9 per cent of sample households have at least one NREGA worker, and among participants the average household days worked exceeds 90 days, close to the hundred-day statutory ceiling.

Caste category is recorded in the IHDS-II using the standard five-group classification of Brahmin, Forward Caste other than Brahmin, Other Backward Class (OBC), Scheduled Caste (SC), and Scheduled Tribe (ST). The composition of the analytical sample is approximately 5.2 per cent Brahmin, 23.1 per cent Forward Caste, 40.6 per cent OBC, 21.0 per cent SC, and 8.7 per cent ST, with the remainder coded as other. The heterogeneity analyses use these five categories as separate subsamples.

Standard controls include the natural logarithm of annual per capita household income. Income is the IHDS-II total household income per member (INCOMEPC), measured in Indian rupees per year at current 2011–12 prices. A small number of households report zero or negative annual income, so values below one rupee are set to one rupee before the logarithm is taken. The transform is therefore a member of the $\ln(x + c)$ family with c fixed at the smallest meaningful unit of the currency, and Section 5.6 reports a robustness check that replaces it with the inverse hyperbolic sine, which admits zero values without an arbitrary floor (Bellemare and Wichman 2020). Remaining controls are maximum adult education in the household measured in years (range 0 to 16), a rural or urban indicator based on the Census 2011 classification (urban share roughly 34.5 per cent), and an indicator for whether any household member is or has been a panchayat official (about 4.1 per cent of households). Summary statistics, units, and the share of zero values for every variable are reported in Table 2, with construction cross-referenced to Table 1.

Table 2. Summary Statistics

Variable	Units	Mean	SD	Min	Max	Share = 0 (%)
Village conflict intensity	1–3 scale	1.54	0.71	1.00	3.00	0.0
Inter-caste conflict intensity	1–3 scale	1.50	0.66	1.00	3.00	0.0
Untouchability practiced (village)	0/1	0.21	0.41	0	1	79.0
Confidence: police	1–3 scale	2.02	0.69	1.00	3.00	0.0
Confidence: state government	1–3 scale	2.09	0.70	1.00	3.00	0.0
Confidence: government hospital	1–3 scale	2.43	0.68	1.00	3.00	0.0
Confidence: courts	1–3 scale	2.60	0.62	1.00	3.00	0.0
Confidence index (composite)	1–3 scale	2.29	0.43	1.00	3.00	0.0
Any NREGA worker in HH	0/1	0.08	0.27	0	1	92.1
$\ln(1 + \text{NREGA days worked})$	log days	0.66	1.51	0.00	6.40	92.1
Government health insurance	0/1	0.09	0.29	0	1	91.0
Government latrine or toilet	0/1	0.05	0.22	0	1	95.0
Kisan Credit Card	0/1	0.05	0.22	0	1	95.0
Welfare uptake index	0–3 count	0.19	0.44	0	3	82.6
Log per capita income ^c	$\ln(\text{Rs/year})$	9.62	1.55	0.00	15.24	1.4

Variable	Units	Mean	SD	Min	Max	Share = 0 (%)
HH max education ^c	years	8.31	5.09	0	16	16.9
Urban residence ^c	0/1	0.34	0.48	0	1	66.0
Panchayat member in HH ^c	0/1	0.04	0.20	0	1	96.0
Scheduled Caste HH	0/1	0.21	0.41	0	1	79.0
Scheduled Tribe HH	0/1	0.09	0.28	0	1	91.0

Note: Source: authors' computations from IHDS-II (Desai and Vanneman 2015). Variable construction and question wordings are given in Table 1. $N = 41,545$ for all variables except panchayat membership ($N = 41,382$) and the three benefit indicators (N between 41,372 and 41,405). Superscript ^c marks control variables entering every regression. For binary variables the share of zeros equals one hundred minus the reported mean in percentage points. Ordinal conflict and confidence scales are bounded below at one, so their share of zeros is zero by construction. Estimation throughout is by ordinary least squares with state fixed effects and standard errors clustered by primary sampling unit, as recorded in each results table.

4. Empirical Strategy

The estimation strategy is presented in three stages corresponding to the three empirical questions posed in the introduction. The first stage estimates the effect of conflict exposure on institutional confidence using a linear model of the form

$$C_{hs} = \alpha + \beta_1 V_{hs} + \beta_2 IC_{hs} + \beta_3 U_{hs} + X'_{hs} \gamma + \delta_s + \varepsilon_{hs} \quad (1)$$

where C_{hs} is the institutional confidence index for household h in state s , V_{hs} is village-level conflict intensity, IC_{hs} is inter-caste conflict intensity, U_{hs} is the village untouchability indicator, X_{hs} is the vector of household controls listed in Table 1, δ_s are state fixed effects, and ε_{hs} is the error term. Standard errors are clustered at the PSU level throughout, which addresses correlated errors among households in the same sampling cluster.

The second stage replaces the confidence outcome with welfare scheme engagement and adds the confidence index as a predictor

$$E_{hs} = \alpha + \theta C_{hs} + \beta_1 V_{hs} + \beta_2 IC_{hs} + \beta_3 U_{hs} + X'_{hs} \gamma + \delta_s + \varepsilon_{hs} \quad (2)$$

where E_{hs} is one of several engagement outcomes, including binary NREGA participation, log NREGA days worked, and the welfare uptake index. The coefficient θ captures the partial association between institutional confidence and the relevant engagement margin, conditional on direct conflict exposure and household characteristics.

The third stage runs both specifications separately for each of the five caste subsamples (Brahmin, Forward Caste, OBC, SC, ST), allowing the coefficient structure to vary across the caste hierarchy. A separate analysis is conducted on the SC subsample using the direct untouchability experience variable (TR4C) as an additional regressor, which is not available for non-SC respondents.

The identification strategy is observational throughout. The paper does not claim causal identification of conflict on confidence, or of confidence on engagement. State fixed effects absorb time-invariant unobservables that operate at the state level, including baseline political culture and aggregate institutional quality. PSU-clustered standard errors

address within-village correlation in unobservables. The estimates are best interpreted as conditional associations that indicate the partial relationships of interest. Several robustness checks, presented in Section 5.6, examine sensitivity to alternative specifications.

5. Results

5.1. Conflict and Institutional Confidence

Conflict exposure is significantly and negatively associated with institutional confidence, though the effects vary considerably across the four institutional domains. In the full sample, a one-unit increase in village conflict intensity (on the 1–3 scale) reduces the composite confidence index by 0.035 points ($p < 0.001$). The effect is largest for confidence in government hospitals ($\beta = -0.055$, $p < 0.001$) and is detectable for state government and courts (both at $\beta = -0.039$, $p < 0.001$). Confidence in the police, somewhat surprisingly, shows no significant association with village conflict ($\beta = -0.006$, $p > 0.10$). One reading is that police confidence in rural India is already low to begin with (mean 2.02 on the 1–3 scale), leaving little attitudinal room for further erosion. Another is that village conflict, as captured in the IHDS-II measure, may not typically involve police intervention, which would weaken the link between conflict exposure and police-specific confidence.

Untouchability at the village level produces a weaker and more domain-specific pattern. The composite-index coefficient is small and non-significant ($\beta = -0.008$), but disaggregating reveals significant negative effects on confidence in courts ($\beta = -0.030$, $p < 0.05$) and on government hospitals ($\beta = -0.024$, $p < 0.10$). These are precisely the institutions that are formally tasked with addressing caste-based discrimination, and the patterning of the effect across institutions is at least suggestive that respondents distinguish between institutional roles when reporting confidence.

Table 3: Conflict and Institutional Confidence

Dependent variable	(1) Composite index	(2) Police	(3) State govt	(4) Hospital	(5) Courts
Village conflict (intensity)	−0.035*** (0.006)	−0.006 (0.010)	−0.039*** (0.009)	−0.055*** (0.009)	−0.039*** (0.009)
Inter-caste conflict (intensity)	0.011 (0.007)	0.003 (0.010)	−0.008 (0.010)	0.035*** (0.010)	0.016 (0.010)
Untouchability practiced (village)	−0.008 (0.009)	0.009 (0.012)	0.014 (0.013)	−0.024* (0.013)	−0.030** (0.013)
Mean of dependent variable	2.29	2.02	2.09	2.43	2.60
Controls	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes
Estimation	OLS	OLS	OLS	OLS	OLS
N	41,382	41,382	41,382	41,382	41,382
R ²	0.070	0.098	0.060	0.069	0.071

Note: Each column reports an OLS regression of the confidence measure named in the column head on the conflict variables shown and the full control set (log per capita income, household maximum education, urban residence, panchayat membership), with state fixed effects. Standard errors clustered by primary sampling unit in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The direct experience of untouchability among Scheduled Caste households produces a substantially larger confidence penalty. In the SC subsample analysis, households reporting that a member had personally experienced untouchability in the preceding five years show a composite confidence deficit of 0.083 points ($p < 0.001$), which is roughly two and a half times the magnitude of the village conflict effect in the full sample. This finding is consistent with the broader argument that direct experience of discrimination has stronger attitudinal effects than exposure to general conflict, and it adds an Indian-specific empirical data point to a literature that has often relied on Western survey contexts.

Inter-caste conflict, measured separately from general village conflict, does not produce a comparable negative effect on the composite confidence index. The coefficient is positive but non-significant ($\beta = 0.011$, $p = 0.117$). For confidence in government hospitals, inter-caste conflict actually shows a positive and significant effect ($\beta = 0.035$, $p < 0.01$), which is the opposite of the expected direction. One reading is that the village conflict variable already captures most of what inter-caste conflict measures, given the correlation of 0.65 between the two variables. Another reading is that the inter-caste variable picks up something distinct, perhaps related to the political mobilisation that accompanies inter-caste contestation, which may strengthen rather than weaken some forms of institutional engagement. Whether this is the right interpretation cannot be determined with the available data.

5.2. Confidence and Welfare Scheme Participation

Institutional confidence has essentially no effect on whether households participate in welfare schemes, with one minor exception. The coefficient on the composite confidence index in the binary NREGA participation regression is 0.005 ($p = 0.318$). For the welfare uptake index, which sums receipt across three non-NREGA welfare benefits, the coefficient is 0.002 ($p = 0.786$). For government health insurance specifically, the coefficient is 0.009 ($p < 0.05$), small but statistically detectable. The substantive size of the health-insurance effect is modest, corresponding to roughly a 0.9 percentage point increase in coverage for each one-unit increase in confidence on the 1–3 scale. The null results on NREGA participation and the welfare uptake index are precisely estimated and survive a range of specification changes, including binary conflict measures, district fixed effects, and the use of alternative confidence indices that drop one institutional domain at a time.

Table 4. Institutional Confidence and Welfare Scheme Engagement

Dependent variable	(1) NREGA participation (0/1)	(2) ln (1 + NREGA days)	(3) Welfare index (0–3)	(4) Health insurance (0/1)
Institutional confidence index	0.005 (0.005)	0.082*** (0.026)	0.002 (0.006)	0.009** (0.004)
Village conflict (intensity)	0.002 (0.003)	0.019 (0.019)	–0.011** (0.006)	–0.009*** (0.003)
Inter-caste conflict (intensity)	0.004	0.033*	–0.001	0.003

(Contd.)

Dependent variable	(1) NREGA participation (0/1)	(2) ln (1 + NREGA days)	(3) Welfare index (0–3)	(4) Health insurance (0/1)
Untouchability practiced (village)	(0.003) –0.002 (0.005)	(0.020) –0.018 (0.028)	(0.006) –0.004 (0.008)	(0.004) –0.009* (0.005)
Mean of dependent variable	0.08	0.66	0.19	0.09
Controls	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes
Estimation	OLS	OLS	OLS	OLS
N	41,382	41,382	41,382	41,242
R ²	0.122	0.204	0.099	0.083

Note: Each column reports an OLS regression of the engagement outcome named in the column head on the confidence index, the conflict variables, and the full control set, with state fixed effects. Column 1 is a linear probability model. Standard errors clustered by primary sampling unit in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

That distrustful households participate in welfare programmes at very similar rates to trusting households is consistent with the broader argument that economic compulsion is the binding constraint on programme entry for the rural poor. NREGA participation rates in our sample average about 7.9 per cent of households, with substantially higher rates among Scheduled Caste (11.6 per cent) and Scheduled Tribe (14.7 per cent) households. These are the same groups that show the largest confidence deficits, yet they show the highest participation rates, which would be difficult to reconcile with a simple trust-suppression account of welfare engagement.

The word compulsion in this argument deserves some explanation. Rural households at the bottom of the income distribution face no unemployment insurance, thin and expensive credit, and a lean agricultural season in which private wage work largely disappears. NREGA's design leans on exactly this position through self-targeting, since the requirement of manual labour at the statutory wage attracts only those whose outside options are worse (Besley and Coate 1992; Imbert and Papp 2015). Participation under these conditions is a decision in only a formal sense. A second difference from the European settings concerns political voice. Kumlin (2004) and Rothstein (2005) describe electorates in which the very poor possess no dedicated partisan vehicle, so that quiet withdrawal from welfare arrangements is among the few responses to institutional failure open to them. Rural India presents nearly the reverse configuration. Marginalised castes have been the object of sustained electoral mobilisation, through reserved constituencies and through parties organised substantially around lower caste assertion (Jaffrelot 2003; Chhibber and Verma 2018). A household can therefore keep drawing material benefit from a distrusted state while exercising voice through caste-based electoral channels, an option largely closed to the European poor. Exit and voice separate here in a way the trust-suppression account does not anticipate, and the coexistence of the highest NREGA participation with the deepest confidence deficits among Scheduled Caste households looks less anomalous once that separation is recognised.

The conflict variables themselves also fail to predict scheme entry in any consistent direction. Village conflict shows a marginally significant negative association with the welfare uptake index ($\beta = -0.011, p < 0.05$) and a small but statistically significant negative association with health insurance ($\beta = -0.009, p < 0.01$). These effects are practically small but suggest that conflict exposure does have some bearing on health-related welfare uptake, perhaps because health insurance enrolment requires repeated interaction with state agencies that conflict-affected households may avoid. Whether this reflects a behavioural response by households or administrative friction operating against conflict-affected villages cannot be determined from the cross-sectional data.

5.3. Confidence and Engagement Intensity

The pattern changes when programme engagement is measured by NREGA days worked rather than by participation. Higher institutional confidence significantly predicts more days worked, with a coefficient of 0.082 in the log-days specification ($p = 0.001$). Translated into approximate levels, a one-standard-deviation increase in confidence corresponds to roughly a 3.5 per cent increase in NREGA days worked at the household level. The effect is robust to district fixed effects, where it actually grows ($\beta = 0.114, p < 0.001$), suggesting that within-district variation in confidence is more strongly associated with days than the between-district variation captured by state fixed effects.

The income-quintile decomposition of this effect is informative. Interacting the confidence index with quintiles of log per capita income reveals that the relationship between confidence and days is largest in the poorest quintile ($\beta = 0.146$) and attenuates progressively toward the higher quintiles. By the fifth quintile, the partial relationship has essentially disappeared. This pattern is broadly consistent with the argument that NREGA is most economically meaningful to households with few outside options, and that within this group the intensity of engagement responds to institutional attitudes in a way that does not show up for households with greater income diversification.

5.4. Caste Heterogeneity

The caste-stratified analyses produce results that complicate any single-channel interpretation. Table 5 reports the key coefficient, institutional confidence on log NREGA days, separately for each of the five caste subsamples.

Table 5. Institutional Confidence and NREGA Days by Caste Group

Subsample	(1)	(2)	(3)	(4)	(5)
Institutional confidence index	Brahmin	Forward	OBC	SC	ST
	0.047	0.011	0.083**	0.024	0.147*
	(0.047)	(0.036)	(0.032)	(0.052)	(0.080)
Mean ln(1 + days)	0.27	0.37	0.58	0.98	1.34
NREGA participation (%)	3.5	4.8	6.9	11.6	14.7
Mean days among participants	85.6	96.4	94.5	97.8	108.0
Controls	Yes	Yes	Yes	Yes	Yes
State fixed effects	Yes	Yes	Yes	Yes	Yes

(Contd.)

Subsample	(1)	(2)	(3)	(4)	(5)
Institutional confidence index	Brahmin	Forward	OBC	SC	ST
Estimation	OLS	OLS	OLS	OLS	OLS
N	2,177	9,558	16,790	8,706	3,593
R ²	0.184	0.190	0.182	0.240	0.241

Note: Each column reports a separate OLS regression of $\ln(1 + \text{household NREGA days})$ on the institutional confidence index for the caste subsample named in the column head, with the full control set and state fixed effects. Standard errors clustered by primary sampling unit in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The confidence-days relationship in the full sample is driven primarily by Other Backward Class households ($\beta = 0.083$, $p = 0.011$) and Scheduled Tribe households ($\beta = 0.147$, $p = 0.067$). Brahmin and Forward Caste households show small and non-significant effects. Scheduled Caste households show essentially no relationship between confidence and days worked ($\beta = 0.024$, $p = 0.652$), despite showing the largest confidence deficit in response to conflict.

A separate analysis of the conflict-to-confidence link by caste group adds further texture to the pattern. The conflict-confidence effect is strongest for Scheduled Caste households ($\beta = -0.049$, $p < 0.001$) and Forward Caste households ($\beta = -0.043$, $p < 0.001$). Both groups show clear and precisely estimated reductions in confidence in response to village conflict, but they sit at very different ends of the caste hierarchy, which suggests that the conflict-confidence link does not operate purely through discrimination experience. Forward Caste households may experience village conflict differently from Scheduled Caste households, but the attitudinal response is comparable. Brahmin households show no significant conflict-confidence relationship, possibly because the Brahmin sample is small ($N = 2,177$) and the variance in their conflict exposure is limited.

One way to read the SC null on the confidence-days margin is to look at the unconditional NREGA engagement pattern in the lower panel of Table 5. Scheduled Caste participants average 97.8 days of NREGA work conditional on participation, which is essentially at the hundred-day statutory ceiling. Scheduled Tribe participants average 108 days (the apparent ceiling violation comes from household-level aggregation of individual-level work days). With SC participants already working close to the maximum entitlement, there is limited room for confidence to push days higher, regardless of how the underlying behavioural relationship operates. The SC null may therefore reflect ceiling-induced compression rather than the absence of a behavioural mechanism, but distinguishing these two interpretations would require either supply-side data on allocation decisions or a different identification strategy.

5.5. Untouchability, Confidence, and NREGA Days

The interaction between untouchability experience and institutional confidence on NREGA days is suggestive but does not reach statistical significance at conventional thresholds. In the SC subsample, the marginal effect of confidence on log NREGA days is essentially zero ($\beta = 0.001$) for households that did not experience untouchability, and substantially larger ($\beta = 0.137$) for households that did experience it. The standard error on the interaction

term is too large for this difference to be statistically distinguishable from zero, but the direction of the pattern is at least consistent with the reading that institutional attitudes matter more for those who have actually encountered discriminatory treatment within the local institutional environment. We report the result for completeness but do not treat it as a primary finding.

5.6. Robustness

Six sets of robustness checks address potential concerns about the main results. First, binary high-conflict indicators replace the continuous conflict intensity variables. The binary versions reduce the precision of the conflict-confidence coefficient considerably (the binary village conflict coefficient is close to zero with a large standard error), suggesting that the continuous specification captures variation that the binary version cannot. The confidence-days effect, however, remains strong ($\beta = 0.079, p = 0.002$) under this alternative specification.

Second, district fixed effects replace state fixed effects in a specification that absorbs much more spatial heterogeneity at the cost of effective sample size. The conflict-confidence relationship strengthens under this specification ($\beta = -0.043$ for village conflict, $p < 0.001$), and the confidence-days relationship strengthens similarly ($\beta = 0.114, p < 0.001$). Whatever bias is present in the state-FE specification appears to be biasing the estimates toward zero rather than away from it.

Third, alternative confidence indices that drop one of the four institutional domains at a time produce coefficients between 0.062 and 0.077 on the days outcome, all significant at the 1 per cent level. The composite-index result is therefore not driven by any single institutional domain. Fourth, the addition of caste fixed effects to the conflict-confidence specification reduces the village conflict coefficient slightly but leaves the qualitative result intact. Fifth, running the engagement-intensity regression separately by income quintile confirms that the confidence-days relationship is strongest among the poorest households and fades at higher income levels, which is consistent with the bivariate descriptive pattern.

Sixth, the income control enters the baseline as the logarithm of per capita income floored at one rupee. Re-estimating the core specifications with the inverse hyperbolic sine of per capita income, which accommodates zero and negative values without a floor (Bellemare and Wichman 2020), leaves the coefficients of interest materially unchanged. Under this alternative the confidence coefficient in the days regression is $\beta = 0.082, p = 0.001$, against 0.082 in the baseline, and the village conflict coefficient in the confidence regression is $\beta = -0.035$, essentially identical to the baseline value.

The pattern that survives all of these robustness checks is clear enough. Conflict erodes institutional confidence, particularly among Scheduled Caste households experiencing direct discrimination. Confidence does not predict scheme entry in any substantive way. Confidence does predict engagement intensity, but with substantial heterogeneity across caste groups and with the SC null plausibly attributable to ceiling saturation rather than to behavioural absence.

6. Discussion

The empirical pattern is not as clean as the trust-suppression hypothesis would have predicted, but it is also not consistent with the opposite null. Conflict matters for institutional attitudes, particularly for groups that experience discrimination directly. Institutional attitudes do not, however, govern whether households enter welfare programmes in any substantial way. The mechanism through which conflict feeds into welfare outcomes therefore cannot be the simple trust-mediated channel proposed in some of the European welfare-state literature. What the data suggest instead is a more disaggregated story in which different caste groups encounter different binding margins, and in which different state institutions register the effects of conflict differently from one another.

Scheduled Caste households face the most severe confidence deficit in response to caste conflict. Their participation in NREGA is highest in absolute terms. The days they work conditional on participation already approach the statutory ceiling. Whatever effect institutional confidence might exert on their engagement intensity is therefore difficult to detect, because the entitlement maximum binds before behavioural responsiveness can become observable. Other Backward Class households, who participate at lower rates and work fewer days when they do participate, show a clearer confidence-days relationship, plausibly because their position in the engagement distribution leaves more room for attitudinal factors to operate. Scheduled Tribe households show the largest confidence-days coefficient point estimate, though with considerable noise.

Reading the caste-stratified pattern against the caste and untouchability literature adds interpretive weight that the internal estimates cannot supply on their own. Ambedkar's description of caste as graded inequality implies that no two strata face the state from the same position, which is close to what the estimates show, with the binding margin shifting from entitlement design for Scheduled Castes to attitudinal responsiveness for Other Backward Classes and to imprecision in the small tribal sample (Ambedkar 1936). Thorat and Newman's (2010) documentation of discrimination in service delivery, Mosse's (2018) account of caste as a live institution in modern markets, and Dasgupta and Pal's (2021) evidence on local caste power and untouchability all point toward mechanisms on the supply side of NREGA administration, in the discretion officials exercise over work allocation, which household survey data can register only as reduced days. Deshpande's (2011) argument that caste disadvantage is reproduced through contemporary economic institutions rather than surviving as a residue of the past sits comfortably beside the finding that confidence deficits are largest exactly where discrimination is directly experienced. The pattern also bears on Kruks-Wisner's (2018) account of claim-making, since converting a statutory entitlement of one hundred days into days actually worked is itself an exercise of agency that the caste hierarchy distributes unevenly. None of this, although, settles the mechanism. Nonetheless, it does locate the estimates inside a literature that has long insisted caste operates through the routine functioning of institutions rather than outside them.

A reading that ties these patterns together would emphasise that the political economy of NREGA is not a single behavioural function applied uniformly across households. It

is a stratified set of relationships in which caste, income, and institutional design interact. The hundred-day ceiling is a feature of programme design instead of household behaviour, and yet it shapes which margins are observable in the data. For groups working at or near the ceiling, institutional attitudes can move days only so much. For groups working further from the ceiling, the relationship between attitudes and days is observable in the ways the European welfare-state literature would predict.

The institutional-domain heterogeneity within the conflict-confidence relationship also deserves attention. Conflict exposure most strongly reduces confidence in government hospitals and the courts, while leaving police confidence largely unaffected. One reading of this patterning is that respondents already discount the police heavily, making the marginal effect of additional conflict exposure on police confidence difficult to detect. Another is that conflict events as experienced in rural villages most often affect access to health services and judicial recourse, which would explain the domain-specific concentration. The bundling of competence and fairness inside the confidence items, noted in Section 2, is relevant here as well. Hospitals and courts are the institutions whose function respondents most plausibly evaluate through direct or vicarious service experience, while evaluations of the police may rest on a settled prior that additional conflict exposure does little to move. The data cannot distinguish these readings cleanly, but the patterning itself is worth flagging for future qualitative work.

What this paper does not establish should be made plain. We do not identify the precise behavioural mechanism through which confidence shapes NREGA days. We hypothesise that low-trust households may accept under-allocation by local officials without pushing back, while higher-trust households claim their full entitlement, but this remains a conjecture rather than a demonstrated finding. The IHDS grievance variables do not measure complaints about welfare programme administration specifically, which prevents direct testing of the conjecture. Testing the mechanism more directly would require a worksite-level survey or qualitative work that asks households about their interactions with NREGA administrators.

The structural conflict framing also deserves some scrutiny. Caste-based conflict in rural India differs from the kinds of conflict that occupy much of the development-conflict literature, and the attitudinal effects we document may not generalise to other conflict settings. Whether episodic violence produces comparable confidence deficits, and whether those deficits translate into similar engagement-intensity patterns, are open questions. The Indian case contributes one observation to a comparative empirical project that remains incomplete.

7. Conclusion

This paper has examined how caste-based conflict shapes household engagement with the Indian state, using a unified empirical framework that connects conflict exposure, institutional confidence, and welfare scheme engagement. Three findings emerge with

reasonable robustness. Conflict erodes confidence in state institutions, with the effect particularly pronounced for Scheduled Caste households experiencing direct untouchability and concentrated in confidence in government hospitals and the courts rather than in confidence in the police. Eroded confidence does not, however, suppress entry into welfare programmes in any substantial way, with households participating at similar rates regardless of their institutional attitudes. Confidence does predict the intensity of engagement once households are inside a programme, particularly for Other Backward Class and Scheduled Tribe households, with the Scheduled Caste null plausibly attributable to ceiling saturation rather than behavioural absence.

The policy implications follow from the heterogeneity rather than from any single coefficient. Welfare programmes that target marginalised groups face different binding constraints depending on which group is being served. For Scheduled Caste households, the programme reach appears to be primarily limited by the entitlement design itself, since participants already approach the hundred-day maximum. Reforms that raise the ceiling or that ease the administrative friction around day allocation may matter more for this group than reforms aimed at restoring institutional trust, though restoring trust matters for its own sake. For Other Backward Class households, who participate at lower rates and work fewer days, attention to institutional quality and to the conditions under which households decide to engage may yield larger gains in effective programme reach. For Scheduled Tribe households the picture is suggestive, but the sample is too small to support strong policy conclusions.

The chronic caste-based conflict that this paper has examined operates at the margin of what the development-conflict literature has traditionally studied. The constitutional promise that Ambedkar attached to the annihilation of caste runs through the programmes studied here, and the estimates suggest that the promise is redeemed unevenly across the very hierarchy it was meant to dissolve. Whether the framework developed here travels to other settings of chronic, structural conflict is an empirical question that future work will have to address. The Indian case provides suggestive evidence that conventional trust-suppression accounts of the conflict-welfare nexus do not map cleanly onto contexts of acute poverty, but the alternative we have proposed (a caste-stratified political economy of engagement) is more a useful frame than a fully specified theory. Whether households really do under-claim entitlements in proportion to their distrust, and whether targeted administrative reforms can repair the engagement deficit without first addressing the underlying conflict, are questions worth pursuing with primary data and worksite-level designs.

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