Dear Professors Breza, Viviano, and Chandrasekhar,

I am writing to apply to the NBER Workshop on Development Economics and Networks, to be held in Cambridge on December 5, 2026. I am a PhD candidate in economics at the University of Arkansas, currently writing my dissertation and expecting to graduate in May 2027, and I am on the 2026–27 academic job market. My research is in development economics, with a focus on labor markets, gender, and the local effects of infrastructure in India.

The paper I would like to present is my job market paper, "Dams and Rural Labor Markets in India." Large dams deliver irrigation and power to places far from the site, while the land they submerge and the people they displace stay behind. I ask how the villages nearest a dam fare on the labor market, and who among them bears the cost. I geolocate every large dam in India's national register, place every Census village relative to its nearest dam and to that dam's reservoir, and follow the villages across three censuses, comparing those within 20 kilometers of a dam completed between 2002 and 2011 with villages the same distance from a dam not yet completed. In the decade after completion, the share of workers in agricultural wage labor, the lowest-paid category of rural work, rises by 1.8 percentage points, while the share in non-farm work falls by 1.9 points. The geometry of the site separates land loss from irrigation as the mechanism: canal-irrigated area does not increase, and the effect is as large above the reservoir, where the dam's water cannot reach, as below it. The effect is concentrated where land rights were weakest, in villages whose residents held land under customary tenure rather than registered title and who, under the Land Acquisition Act of 1894, received no compensation. The cost of a dam falls on the households that worked its land without owning it, a group India's compensation law first recognized in 2013.

I would welcome the chance to discuss the paper with the workshop's participants.

I have enclosed my curriculum vitae, which lists my completed and current PhD coursework, along with a one-page statement of my motivation for attending and my current research interests, and a copy of the paper.

If I am shortlisted, my dissertation committee chair, Professor Arya Gaduh (AGaduh@walton.uark.edu), would be able to provide a recommendation.

Thank you for your consideration.

Sincerely,

Priyanka Pimple

Contact information:
Priyanka Pimple
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Priyanka Pimple

ppimple@uark.edu

EDUCATION

Ph.D. in Economics — Expected May 2027
University of Arkansas, Fayetteville, Arkansas
Committee: Arya Gaduh (Chair), Kyle Butts, Michael Gechter

M.A. in Economics — 2020
Central University of Tamil Nadu, Tamil Nadu, India

Bachelor of Commerce — 2018
Osmania University, Telangana, India

PH.D. COURSEWORK

Completed

ECON 6133: Mathematics for Economic Analysis	Fall 2022
ECON 6213: Microeconomic Theory I	Fall 2022
ECON 6313: Macroeconomic Theory I	Fall 2022
ECON 6613: Econometrics I	Fall 2022
ECON 643V: Seminar in Economic Theory and Research I	Fall 2022
ECON 6223: Microeconomic Theory II	Spring 2023
ECON 6323: Macroeconomic Theory II	Spring 2023
ECON 6623: Econometrics II	Spring 2023
ECON 644V: Seminar in Economic Theory and Research II	Spring 2023
WCOB 6111: Seminar in Business Administration Teaching I	Spring 2023
ECON 5783: Applied Microeconometrics	Fall 2023
ECON 6633: Econometrics III	Fall 2023
ECON 6913: Experimental Economics	Fall 2023
ECON 636V: Special Problems in Economics	Fall 2023
ECON 643V: Seminar in Economic Theory and Research I	Fall 2023
ECON 6833: International Trade and Development I	Spring 2024
ECON 6843: International Trade and Development II	Spring 2024
ECON 6933: Behavioral Economics	Spring 2024
ECON 644V: Seminar in Economic Theory and Research II	Spring 2024

In Progress

ECON 6430V: Seminar in Economic Theory and Research I	Fall 2026
ECON 7000V: Doctoral Dissertation	Fall 2026

RESEARCH INTERESTS

Development Economics; Applied Microeconomics; Gender Economics; Labor Economics

WORKING PAPERS

Dams and Rural Labor Markets in India (Job Market Paper)

Large dams deliver irrigation and power to places far from the site, while the land they submerge and the people they displace stay behind. I geolocate every large dam in India's national register, place every Census village relative to its nearest dam and to that dam's reservoir, and follow the villages across three censuses, comparing villages within 20 km of a dam completed between 2002 and 2011 with villages the same distance from a dam not yet completed. In the decade after completion, the share of workers in agricultural wage labor rises by 1.8 percentage points and the number of laborers by 11 percent, while the share in non-farm work falls by 1.9 points; per capita consumption shows no effect. Canal-irrigated area does not increase, and the effect is as large above the reservoir as below it, pointing to land loss rather than irrigation as the mechanism. The effect is concentrated in villages whose residents held land under customary tenure and received no compensation under the Land Acquisition Act of 1894: the cost of a dam falls on the households that worked its land without owning it.

Balancing Motherhood and Employment: Analyzing the Consequences of Extended Maternity Leave in India (Working Paper)

Studies the labor market effects of India's 2017 extension of paid maternity leave from 12 to 26 weeks. Using a difference-in-differences design on three rounds of National Family Health Survey data (2005–2021), I find the policy reduced labor force participation among prime-childbearing-age women (20–34) by 2.6 percentage points, with the largest declines among the youngest (20–24) and most educated women, evidence consistent with employer reluctance to hire women of childbearing age as compliance costs rose. Results are robust to placebo and parallel-trends tests.

What Binds Women's Decision-Making? (Working Paper)

Sole-authored structured evidence review synthesizing 89 studies of gender-transformative approaches. Finds that household say, collective voice, and public participation follow distinct production functions, contrary to the conventional cascade theory of change.

PUBLICATION

Pimple, P., & Sumalatha, B. S. (2021). Out-of-pocket Health Spending and Its Impact on Household Well-being in Maharashtra. *Journal of Health Management*. doi:10.1177/09720634211052412

RESEARCH EXPERIENCE

Research Intern June–August 2026
Asian Development Bank (ADB), Manila, Philippines
Economic Research and Development Impact Department (ERDI)
Project: Systematic Review on Gender Transformative Interventions

- Conducted a systematic review of gender-transformative interventions in developing countries, comparing approaches by target population and level of change.
- Participated in EREA's Sustainable Development Goal 5 (Gender Equality) workshop.

Research Associate August 2021–January 2022
Mahindra University

- Managed large-scale datasets and conducted econometric analysis for policy-focused research projects.
- Produced analytical reports and presentations for multidisciplinary teams.

Research Associate May–August 2019
Centre for Economic and Social Studies (CESS), Hyderabad

- Conducted a policy evaluation of the Aarogyasri Health Insurance Scheme.
- Designed the evaluation methodology, collected field data, performed statistical analysis, and authored the final report for government stakeholders.

TEACHING EXPERIENCE

University of Arkansas

Basic Economics: Fall 2023; Spring 2024
Principles of Macroeconomics: Fall 2024; Spring 2025; Fall 2025; Spring 2026; Fall 2026; Spring 2027

SEMINARS AND PRESENTATIONS

Southern Economic Association (SEA) Annual Meeting, Houston (scheduled) November 2026
"Dams and Rural Labor Markets in India"

Asian Development Bank, Economics Brownbag Seminar Series, Manila August 2026
"What Binds Women's Decision-Making?"

Midwest Economics Association (MEA) Annual Meeting, Chicago March 2026
"Dams and Rural Labor Markets in India"

HONORS AND AWARDS

Dr. Yien-I Tu Endowed Scholarship 2026–2027
Sam M. Walton College of Business

Adam Smith Fellowship 2025–2026
Mercatus Center, George Mason University

Carl Menger Fellowship 2024–2025
Mercatus Center, George Mason University

Doctoral Academy Fellowship 2022–2026
University of Arkansas

TECHNICAL SKILLS

Software: Stata (advanced), Python, QGIS

REFERENCES

Arya Gaduh
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Statement of Motivation

Priyanka Pimple, University of Arkansas

My job market paper studies the effects of large dams in India on the villages around them. A dam's benefits, irrigation and electricity, travel through canals and transmission lines to places far from the site, while the land it submerges and the people it displaces stay where they are. I geolocate every large dam in India's national register, place each Census village relative to its nearest dam and reservoir, and follow villages across three censuses. I compare villages within 20 kilometers of a dam completed between 2002 and 2011 with villages the same distance from a dam sanctioned but not yet built. In the decade after completion, employment near a dam moves into agricultural wage labor and out of non-farm work. More broadly, my research interests lie in labor economics, social protection, and gender in developing countries. I am particularly interested in how infrastructure, land institutions, and the informal networks through which rural households absorb shocks shape who is protected by policy and who is not.

The paper has several dimensions that I am still developing. Among them are the land rights that determined who was compensated, the geometry of the site that separates the loss of land from the arrival of irrigation, and the welfare of households who lose land and take up wage labor yet show no fall in consumption a decade later. The dimension this workshop would help me develop is the network one. The villages around a dam share a labor market, so the effect I measure in one village is shaped by what happens in its neighbors. The ties through which customary land was held, caste and kinship, also appear to be the ties that carry households through its loss, as Munshi and Rosenzweig describe for rural India. I would like to explore this angle with the setup and the data I currently have.

The workshop's lectures on empirical network methods address the questions this extension raises. In particular, I would like to learn how to define exposure when villages are linked through a common labor market. I also want to understand how to estimate effects when outcomes depend on the treatment of neighboring villages. A third question is how to recover network structure where the data record no ties directly. I expect that training to inform the paper beyond this dimension as well. I would also value the opportunity to present the paper as work in progress and to receive comments from the organizers and participants at a stage when they can still inform the analysis.

Dams and Rural Labor Markets in India

Priyanka Pimple

University of Arkansas

1 Introduction

The reallocation of labor out of agriculture into industry and services is among the most robust regularities of economic development. As economies grow, the share of employment in agriculture falls, and aggregate productivity rises in part because the sectors that absorb labor are more productive than the one that releases it (Kuznets 1966; Herrendorf, Rogerson, and Valentinyi 2014; Gollin, Lagakos, and Waugh 2014). The regularity has been slow to arrive, and has taken a different route, in the economies that remain poor. Among the 39 economies of the Groningen database, Sen (2019) identifies eight in which agriculture is still the largest employer; seven are in sub-Saharan Africa and the eighth is India.[1] In these economies the agricultural share of employment fell only from 78 percent in 1960–1979 to 66 percent in 2000–2012, and the workers who left moved mostly into low-productivity services rather than into manufacturing (Sen 2019). The pace matters for incomes because non-agricultural workers in developing countries earn two to three times what agricultural workers earn (Gollin, Lagakos, and Waugh 2014; Donovan and Schoellman 2023). It is set, in large part, by frictions that are local in character: the distance between a farm and a factory, the loss of village risk-sharing networks that migration entails, and the search and travel costs that bind hired farm labor to unproductive land (Munshi and Rosenzweig 2016; Morten 2019; Foster and Rosenzweig 2022; Donovan and Schoellman 2023). The village-level evidence for India describes the consequence. Migration responds little to wage gaps because caste-based insurance networks tie households to their villages (Munshi and Rosenzweig 2016), so that local shocks fall on workers who cannot leave (Jayachandran 2006); nominal wages are rigid (Kaur 2019) and work is rationed (Breza, Kaur, and Shamdasani 2021), so that a shock is absorbed through who works and at what. Where these frictions bind, the composition of local work, rather than the wage or the population, is where a shock registers.

Developing countries have tried to accelerate the transformation, and in agrarian economies the instrument has often been investment in agriculture itself. The theoretical case rests on the closed-economy result that productivity growth in agriculture releases labor to the modern sector (Matsuyama 1992; Gollin, Parente, and Rogerson 2002), a channel that a recent survey files under government policy, the driver of structural change on which the literature has been most silent (Chakrabarti, Bhattacharjee, and Sethi 2025). The empirical record is more equivocal. Whether agricultural productivity growth releases labor from agriculture or draws labor into it depends on the factor bias of the technology and on the openness of the local economy (Foster and Rosenzweig 2004, 2008; Bustos, Caprettini,

[1] The classification rests on the Groningen Growth and Development Centre's ten-sector database, which covers 39 economies and, as Sen (2019) notes, few low-income ones; Bangladesh, Nepal, Pakistan, and Sri Lanka are not included.

and Ponticelli 2016), and in the historical record the exit from agriculture has been driven both by this push from within agriculture and by the pull of productivity growth outside it (Alvarez-Cuadrado and Poschke 2011). Transitory gains in India release labor into non-farm work (Emerick 2018). Permanent, land-augmenting gains, irrigation above all, draw labor onto the land and leave local non-farm activity flat, in the American plains and in Indian command areas alike (Hornbeck and Keskin 2015; Asher et al. 2022; Blakeslee et al. 2023; Boudot-Reddy and Butler 2025). The other instrument has been infrastructure. Railroads, roads, and electricity raise incomes and reorganize economic activity where they connect (Donaldson 2018; Donaldson and Hornbeck 2016; Dinkelman 2011; Lipscomb, Mobarak, and Barham 2013; Asher and Novosad 2020); the aggregate returns are less certain than the early macro literature claimed (Straub 2011; Ramey 2021); siting responds to politics (Burgess et al. 2015); and infrastructure moves activity across space at least as much as it creates it, so that connected places can gain while peripheral ones lose (Faber 2014; Redding and Turner 2015). Two features of this literature bear on what follows. Its returns are measured where the service arrives, and its unit of account, whether the country, the district, or the connected town, is coarser than the places that produce the service.

Large dams combine the two instruments, and the technology of a dam separates the places that gain from the places that pay more sharply than either instrument does on its own. India built more large dams in the twentieth century than almost any other country; by 2011 more than 5,000 stood completed and hundreds more were under construction, most of them for irrigation, and successive governments presented them as the means by which rural India would leave subsistence cultivation, the decade studied here opening with a national hydropower initiative that promised "new avenues of employment and economic activity" (Vajpayee 2003). Canals carry the stored water to the command area downstream and the grid carries the power to cities, while the reservoir and the land beneath it, the communities displaced by the water, and the interruption of the river remain at the site. Duflo and Pande (2007) established the resulting asymmetry at the district level: agricultural production rises and poverty falls in the districts downstream of Indian dams, while poverty rises in the district that hosts the dam. Satellite evidence shows the same pattern in cropland productivity and night lights (Strobl and Strobl 2011; Blanc and Strobl 2014; He and Chen 2023; Goodman 2024). The beneficiary side of this asymmetry has since been examined at village resolution, in the command areas of Indian canals, where irrigation draws labor into agriculture and leaves non-farm activity flat (Asher et al. 2022; Blakeslee et al. 2023). The cost side has not, and a district is too coarse a unit to examine it: an Indian district averaged 8,000 square kilometers, 1.5 million people, and more than eight large dams in Duflo and Pande's data, so that a district estimate combines the catchment, the command area, and land the dam does

not touch. How the labor market of the villages beside a dam responds to its completion is not known.

This paper measures that response in the villages themselves, over the decade after a dam is completed. I match the National Register of Large Dams, which records the location and completion year of every large dam in India, to the village abstracts of the 1991, 2001, and 2011 Population Censuses, which report for every village the number of workers who are cultivators, agricultural laborers, workers in household industry, and non-farm workers. Because the placement of dams is not random, the comparison is confined to places that were selected for a dam: every village in the sample lies within 20 kilometers of a registered dam site, and villages whose nearest dam was completed between 2002 and 2011 are compared with villages whose nearest dam had not been completed by 2011, after the latter are reweighted to match the former on their 2001 characteristics. The two groups share the terrain that dams require and differ in when the dam was finished, so that identification comes from the timing of completion, and the estimate is the difference between the groups in the change in each outcome over the decade, within state.

The villages close to a completed dam lose on the labor market, and the loss takes a specific form. In the decade after completion, worker participation rises, and the additional workers enter agricultural wage labor, the lowest rung of the rural occupational hierarchy. Relative to reweighted control villages, the share of main workers who are agricultural laborers rises by 1.8 percentage points, and by 0.9 to 1.5 points relative to the control groups whose status is most certain; the number of agricultural laborers rises, while the number of cultivators, which fell throughout rural India over the decade, falls by less in the host villages. The movement into non-farm employment that characterized the decade elsewhere is correspondingly slower in the host villages, and per capita consumption a decade after completion is indistinguishable from that of comparable villages. The effect is confined to the site: it is present within 20 kilometers of the dam and undetectable beyond.

Two mechanisms could produce a rise in agricultural wage labor near a completed dam, and they carry opposite implications for the site. Under the first, the dam's water raises the productivity of nearby land and the additional labor is drawn into agriculture by demand, as in the command areas studied by Asher et al. (2022) and Blakeslee et al. (2023); the site would then share in the benefit. Under the second, the reservoir removes land and the livelihoods attached to it, and households whose claims on that land were not compensated supply labor to a local market that can absorb it only in wage work on the land of others; the site would then bear a cost. Three results discriminate between them. Canal-irrigated area in the host villages does not increase relative to comparable villages over the decade, so no expansion of irrigated cultivation accompanies the rise in farm labor. The effect is present

in villages above the reservoir's surface, which cannot be served by gravity-fed canals from the dam, and it is no smaller there than in villages below the dam. And the land-use record shows the reservoir rather than the canal: the share of village area not available for cultivation rises at the reservoir's rim and not above it, while population is stable and its age and sex composition unchanged, so that the adjustment occurs in the work of a resident population rather than through migration. The incidence of the effect points to the same mechanism. It is concentrated in villages with a high Scheduled Tribe population share, at both distances from the dam, and near irrigation dams, whose reservoirs take cultivated land, and the shortfall in non-farm employment is larger for women than for men. Land acquisition for every dam in the sample was governed by the Land Acquisition Act of 1894, which compensated holders of documented title and no one else, and the communities that settled the valleys chosen for reservoirs held land predominantly under customary tenure. The compositional pattern the paper documents, cultivators retained and laborers added, is the pattern that rule would produce.

These findings bear on three literatures, and the paper's contribution to each can be stated precisely. To the economics of infrastructure and development, it adds a measurement of the cost side of a large project at the resolution where the cost is borne, the village, and in the outcome in which it appears, the composition of work; it does not revisit the aggregate case for infrastructure, but measures what the aggregate accounts leave out. To the literature on agricultural change and structural transformation, it adds a case that fits neither the push nor the pull mechanism as usually stated: a permanent shock that removes land rather than augmenting it, in a labor market whose frictions keep the affected labor local, slows the exit from agriculture at the site. This is the counterpart, on the cost side, of the absorption of labor that the canal studies document on the benefit side of the same technology, and it operates through participation and composition rather than through population. To the economics of dams, it supplies one of the channels beneath the district-level poverty result of Duflo and Pande (2007), who attribute the rise in poverty in dam districts to the displacement of communities in the catchment area but cannot, with district data, identify the channels through which it operates (Section 10). The design follows two established devices: sites selected for treatment but not yet treated serve as controls, as in the planned but unbuilt railway lines of Donaldson (2018) and the proposed regional authorities of Kline and Moretti (2014), and the spatial extent of the effect is estimated rather than assumed (Butts 2023).

The remainder of the paper proceeds as follows. Section 2 describes large dams in India, how they are built and sited, how those who lose land are compensated, and how the Census records rural work. Section 3 describes the data and Section 4 the empirical strategy. Section 5 reports the main results, Section 6 examines the mechanism, Section 7 the hetero-

geneity of the effect, and Section 8 consumption and poverty. Section 9 reports robustness checks, and Section 10 concludes.

2 Background

2.1 Large dams and the geography of their effects

Large dams have been the predominant form of public investment in irrigation in India since independence. Between 1971 and 1999 their number quadrupled, from 882 to 3,364, and dams came to account for 38 percent of the irrigated area; more than 90 percent of them were built for irrigation, alone or in combination with hydropower and flood control (Duflo and Pande 2007). The economic case for them rested on the productivity of irrigated land and on the labor that a more productive agriculture would release, and the political case was made in the same terms, from Nehru's description of the Bhakra-Nangal works as the temples of a new age to the hydropower initiative of the early 2000s (Nehru 1954; Vajpayee 2003).[2]

The technology of a dam allocates its costs and benefits to different places. A wall across a river valley impounds a reservoir that submerges the valley floor and its margins; canals draw water from the reservoir and deliver it by gravity to the command area downstream, which may lie far from the dam; and the power generated at the dam is transmitted to the grid. Land at the reservoir's rim is lost permanently, the flow of the river below the dam is regulated, and land above the reservoir's surface receives no water from the dam. The vicinity of a dam thus absorbs the construction, the submergence, and the interruption of the river, while the returns to the investment accrue in the command area and on the grid (Duflo and Pande 2007; World Commission on Dams 2000). Two features of the timing of these effects matter for the design of this paper. Construction takes years and brings employment and disruption to the site well before completion, so that villages beside a dam under construction are not untouched; Section 4.3 examines such villages directly. And canal networks are completed after the dam is closed, often years after, so that within the decade following completion the command area of a recently finished dam may not yet be served.[3] Whatever labor demand irrigation eventually creates near the dam is therefore attenuated over the horizon studied here.

[2]The phrase "temples of modern India" commonly attributed to Nehru is a later paraphrase. The 1954 address describes "the biggest temple and mosque and gurdwara" as "the place where man works for the good of mankind" (Nehru 1954).

[3]For the Indian canals studied by Asher et al. (2022), the median canal was begun in 1972 and completed in 1980, and He and Chen (2023) find that the downstream gain in cropland productivity from dams in China takes about a decade to appear.

2.2 Construction, placement, and the timing of completion

Dam construction in India is a joint federal and state undertaking. The Planning Commission sets each state's five-year targets for water storage and irrigation; state irrigation departments propose projects against those targets on the basis of topographic surveys of candidate sites; a federal committee appraises each project's economic viability; and the Planning Commission selects among them according to investment priorities, after which construction of the dam and its canal network is the state's responsibility, with federal or international funding where available (Duflo and Pande 2007, Section II.A). A project therefore passes through several rounds of approval and financing before construction begins, and construction itself takes years, so that the year in which a dam is finally completed is governed by funding, clearances, and the acquisition of land rather than by the labor market of the villages around the site. The area upstream that drains into the reservoir, together with the land the reservoir submerges, is the dam's catchment; the area downstream served by its canals is the command area. Farmers within the command area are eligible for the dam's water, whereas upstream of the reservoir the responsible agency often restricts water withdrawal to keep the reservoir full, especially in years of low rainfall (Shiva 2002, as cited in Duflo and Pande 2007). The villages this paper studies lie in and around the catchment.

Within this process, the choice of site is an engineering decision. A dam requires a river flowing at a gradient neither too flat, which yields a shallow and expensive reservoir, nor too steep, which erodes the canals, and a valley that is narrow at the dam and wide behind it (Duflo and Pande 2007). Placement is therefore determined by physical geography rather than by the economic trajectory of nearby villages, and, conditional on a site having been selected, the year of completion is determined by the administrative and financial process just described. The empirical strategy of Section 4 rests on these two facts: it compares dam sites with dam sites, and it identifies from the timing of completion.

2.3 Displacement and compensation

The river valleys and forested uplands that engineers select for reservoirs have historically been settled by Scheduled Tribe communities, whose land tenure rests on customary and communal arrangements rather than on titles registered in revenue records. Official figures for 34 large dams show that Scheduled Tribes, then eight percent of India's population, made up 47 percent of those displaced (World Commission on Dams 2000, as cited in Duflo and Pande 2007), and Fernandes (2001) puts tribal people at more than 40 percent of those displaced or deprived of livelihood by dams and mines. Land acquisition for every dam in this study was governed by the Land Acquisition Act of 1894 (Government of India 1894),

which paid cash compensation to the holders of documented title; as Duflo and Pande (2007) summarize the case-study record, the claims of the landless and of those without formal title were typically not recognized (Thukral 1992), and compensation was usually insufficient to replace lost land in kind (Drèze, Samson, and Singh 1997). The Act was replaced only in 2013, after the study period (Government of India 2013). The institution thus divided the population of a dam site along a single line: households with documented title were paid for their land, whereas households whose livelihood depended on that land as laborers or as untitled occupants received nothing. This distinction is the basis for the heterogeneity examined in Section 7.

2.4 Rural work in the Census

The Census classifies every worker into one of four categories: cultivators, who own or lease the land they work; agricultural laborers, who work for wages on land operated by others; workers in household industry; and other workers, in trade, transport, services, construction, manufacturing, and salaried employment, which I refer to as non-farm work (Office of the Registrar General and Census Commissioner 2011). The categories form a hierarchy. Non-farm workers earn more on average than agricultural workers, and access to non-farm work is conditioned by education, assets, and caste (Lanjouw and Shariff 2004); agricultural wage labor is the least remunerative category, the occupation of the landless and the least educated, and it is concentrated among Scheduled Caste and Scheduled Tribe households (Lanjouw and Murgai 2009). Workers in Indian villages are largely immobile, so that local shocks fall on those who stay (Jayachandran 2006); nominal wages are rigid (Kaur 2019) and work is rationed (Breza, Kaur, and Shamdasani 2021), so that adjustment runs through quantities, through participation, the allocation of family labor, and the sorting of workers into low-wage work, which is why a permanent local shock is most likely to appear in census data in the composition of work rather than in the wage. A rise in the share of workers in agricultural wage labor is a movement down the hierarchy; a rise in the number of agricultural laborers with the number of cultivators retained is an addition at its bottom. The analysis measures both.

3 Data

3.1 Sources

Dam data come from the National Register of Large Dams, maintained by the Central Water Commission of India, which covers every dam at least 15 meters high from the foundation

or between 5 and 15 meters high with a reservoir capacity above 3 million cubic meters (Central Water Commission 2018). For each dam the register records the location, the year of completion, height, length, storage, and the stated purpose. The edition used here lists 5,745 dams, completed and under construction; 587 have no recorded completion year and 102 have a completion year after 2011.[4] Village data come from the Primary Census Abstracts of 1991, 2001, and 2011 as harmonized in the SHRUG (Asher et al. 2021; Development Data Lab 2023), which links villages across census rounds under a common identifier and reports population, households, and workers by category and by sex. The Census distinguishes main workers, who worked for at least six months of the reference year, from marginal workers, who worked for fewer. Each village is matched to the nearest registered dam using the village centroid and the dam's coordinates, over all 5,745 dams irrespective of their status.[5]

Three features of the census data bound what the analysis can claim. The Census is a sequence of cross-sections of villages rather than a panel of individuals, so every result concerns the composition of work in a village and never the transitions of particular workers; the shifts documented below are consistent both with changes among original residents and with compositional change through migration, which the demographic margins can bound but not exclude (Section 7.3). The Census records no wages, so the paper measures how many people work and at what, not what they earn. And a village submerged by a reservoir cannot be followed across rounds and never enters the panel, so the analysis observes the neighbors of such villages and its estimates understate the loss of land at the site; the last round was taken in 2011, so the decade after completion is the horizon of the evidence.[6]

3.2 Sample and cohorts

The sample consists of villages whose nearest registered dam lies within 20 kilometers, a radius fixed before any outcome was examined and applied identically to all villages, excluding villages whose nearest dam was completed in or before 1991, for which the panel offers no pre-period; it contains 99,922 villages. Villages beside those older dams return as a comparison group in Section 4.3. Villages are assigned to cohorts by the completion year

[4]The construction of the dam file is described in Appendix A. One dam identifier is duplicated in the register, and 12 dams carry completion years before 1800; all fall in the group of dams completed in or before 1991, which is excluded from the analysis.

[5]Distances are computed on unprojected coordinates and converted to kilometers at 111.32 kilometers per degree, which overstates east–west distances by roughly 6 to 8 percent at Indian latitudes; the 20-kilometer boundary is correspondingly imprecise by one to two kilometers.

[6]The four-category classification is consistent across 2001 and 2011 but not 1991, in which the SHRUG's recoding undercounts the residual category (the four shares sum on average to 0.913 in 1991 and to exactly 1.000 in 2001 and 2011). Wherever 1991 non-farm employment enters, which is only in the pre-trend checks, I measure it as main workers less cultivators, agricultural laborers, and household-industry workers.

of their nearest dam. Treated villages are those whose nearest dam was completed between 2002 and 2011 (21,921 villages). Control villages are those whose nearest dam had not been completed by 2011: 38,530 beside undated dams and 6,489 beside dams dated 2012 to 2018, 45,019 in all. Villages whose nearest dam was completed between 1992 and 2001 (32,982) are excluded from the main comparison because their dams were under construction during 1991–2001, the only pre-period the panel offers; Section 9 examines them separately. The main comparison therefore contains 66,940 villages, and the headline regression uses the 64,920 of them, 21,308 treated and 43,612 control, for which the composition of work is observed in both 2001 and 2011, in 352 districts.

Matching villages to the nearest dam irrespective of its status is what produces the control group. Had the match been restricted to dams completed by 2011, a village beside a dam under construction would have been indistinguishable from a village with no dam nearby, and the control group would have carried the imprint of construction.[7] Classifying undated dams as not completed is a judgment the register forces, since it records no status; its cost runs in one direction, because an undated dam that had in fact been completed places villages that have already experienced a completion among the controls and biases the estimates toward zero. Section 4.3 examines the composition of the control group directly.

3.3 Position relative to the reservoir

Where the nearest dam has been completed, the match also records the village's position relative to the reservoir. Using elevations from the HydroSHEDS void-filled digital elevation model for Asia at 3 arc-second (roughly 90-meter) resolution (Lehner, Verdin, and Jarvis 2008) and the level-6 river basins of HydroBASINS (Lehner and Grill 2013), I place each treated village in one of four cells: below the dam, in the same basin and below the dam site, where a canal fed by the dam would run; at the rim of the reservoir, in the same basin at an elevation between the dam site and the crest of the dam; above the reservoir, in the same basin above the crest, where a gravity-fed canal from the dam cannot reach; and in a different basin. Of the 21,308 treated villages in the estimation sample, 5,465 lie below the dam, 3,511 at the rim, 7,061 above the reservoir, and 2,900 in a different basin; 2,371 lack a dam elevation. Elevation is available only for completed dams, so control villages are never divided by direction.

[7] A version of the match restricted to completed dams produced exactly this: a significant differential pre-trend in an event-time specification on the three rounds, which disappears when the match is made over all registered dams.

3.4 Other outcomes

Three further sources describe what reached the site over the decade and the living standards of its residents at the end of it. The Village Directories of the 2001 and 2011 censuses report the area under each land use and the area irrigated by each source, from which I construct, as shares of village area, canal-irrigated area, well and tubewell irrigation, total irrigated area, cultivated area, area not available for cultivation, forest, and culturable waste, along with indicators for village amenities.[8] Consumption and poverty come from the 2012 Socio-Economic and Caste Census as processed in the SHRUG: village per capita consumption estimated by small-area methods, and the poverty rate at the SHRUG line and the Tendulkar line (Asher et al. 2021). Because these are levels observed once, after treatment, they are analyzed as 2012 outcomes conditional on the 2001 characteristics used for balancing; the SECC matches 63,524 villages in the estimation sample. Finally, the Mahatma Gandhi National Rural Employment Guarantee Act was rolled out by district in three phases between 2006 and 2008, the districts of the first two phases selected on an index of agricultural wages, output per agricultural worker, and the Scheduled Caste and Scheduled Tribe share of the population, criteria that overlap the characteristics of dam sites (Planning Commission 2003; Imbert and Papp 2015); I attach each district's phase and use it in Section 9.

Table 1 reports descriptive statistics by census round for treated villages, control villages, and, as a benchmark, all villages in the SHRUG. Dam sites are a particular kind of place: relative to all villages, both groups have a larger Scheduled Tribe share (24 against 19 percent in 2001), higher worker participation, a larger cultivator share, and about half as much canal irrigation. Within the sample, the two groups were alike in 2001 in the composition of work, with agricultural-labor shares of 22.4 and 21.9 percent, and differed in the composition of their populations: treated villages have a higher Scheduled Tribe share (27.9 against 21.6 percent), a lower Scheduled Caste share, higher literacy, and smaller households, differences the estimator addresses by reweighting. Over the decade the composition of work moved in the same direction everywhere: the cultivator share fell by nine to ten percentage points, and the agricultural-labor share rose, from 22.4 to 29.4 percent in treated villages, from 21.9 to 27.4 in control villages, and from 21.7 to 26.9 in all villages. The question is whether the host villages moved as their comparison villages did.

[8] A share above 1.05 is treated as a recording error and set to missing; shares between 1 and 1.05 are capped at 1. In 2001 canal irrigation is the sum of government and private canals; in 2011 the Directory reports a single canal category. Coverage of the Directories is 96.6 percent of treated and 93.7 percent of control villages in 2001 and 99 percent of both in 2011, with no differential coverage by treatment status ($p = 0.81$).

Table 1: Descriptive statistics by census round: villages within 20 kilometers of a dam site

	Treated	Control	Sample	All villages
Villages in group	21,921	45,019	66,940	605,910
Panel A. 1991				
Population per village: mean	1,270	1,443	1,387	1,434
Population per village: median	562	546	551	627
Households per village	243	276	266	259
Household size (persons)	5.43 (1.23)	5.59 (1.43)	5.54 (1.37)	5.79 (1.72)
Main workers per village	496	553	535	488
Worker participation rate	0.490 (0.116)	0.473 (0.128)	0.479 (0.125)	0.424 (0.135)
Cultivator share of main workers	0.592 (0.279)	0.597 (0.279)	0.595 (0.279)	0.585 (0.275)
Agricultural-labor share	0.262 (0.244)	0.265 (0.247)	0.264 (0.246)	0.260 (0.235)
Household-industry share	0.016 (0.047)	0.017 (0.049)	0.017 (0.049)	0.018 (0.054)
Non-farm share (residual)	0.129 (0.180)	0.118 (0.163)	0.121 (0.169)	0.136 (0.178)
Scheduled Tribe share	0.283 (0.379)	0.221 (0.353)	0.241 (0.363)	0.186 (0.332)
Scheduled Caste share	0.129 (0.172)	0.160 (0.193)	0.150 (0.187)	0.173 (0.202)
Literacy rate	0.321 (0.179)	0.308 (0.177)	0.312 (0.178)	0.324 (0.175)
Villages observed	20,531	42,233	62,764	572,217
Panel B. 2001				
Population per village: mean	1,510	1,732	1,659	1,724
Population per village: median	656	639	644	739
Households per village	302	342	329	324
Household size (persons)	5.21 (1.05)	5.38 (1.20)	5.33 (1.15)	5.56 (1.22)
Main workers per village	524	587	566	525
Worker participation rate	0.504 (0.114)	0.480 (0.131)	0.488 (0.126)	0.443 (0.134)
Cultivator share of main workers	0.549 (0.284)	0.542 (0.289)	0.544 (0.287)	0.524 (0.284)
Agricultural-labor share	0.224 (0.234)	0.219 (0.233)	0.220 (0.233)	0.217 (0.224)
Household-industry share	0.026 (0.066)	0.030 (0.075)	0.029 (0.072)	0.031 (0.073)
Non-farm share	0.202 (0.236)	0.210 (0.241)	0.207 (0.239)	0.228 (0.243)
Scheduled Tribe share	0.279 (0.375)	0.216 (0.348)	0.237 (0.358)	0.186 (0.329)
Scheduled Caste share	0.126 (0.174)	0.156 (0.194)	0.146 (0.188)	0.172 (0.203)
Literacy rate	0.476 (0.177)	0.452 (0.180)	0.460 (0.180)	0.466 (0.173)
Villages observed	21,544	44,259	65,803	593,795
Panel C. 2011				
Population per village: mean	1,729	2,032	1,933	2,030
Population per village: median	749	725	734	861
Households per village	378	437	418	418
Household size (persons)	4.72 (1.00)	4.96 (1.16)	4.88 (1.12)	5.06 (1.27)
Main workers per village	616	692	667	608

continued on next page

Table 1, continued

	Treated	Control	Sample	All villages
Worker participation rate	0.508 (0.121)	0.483 (0.139)	0.491 (0.134)	0.445 (0.137)
Cultivator share of main workers	0.460 (0.299)	0.439 (0.300)	0.446 (0.300)	0.432 (0.290)
Agricultural-labor share	0.294 (0.279)	0.274 (0.271)	0.281 (0.274)	0.269 (0.258)
Household-industry share	0.019 (0.053)	0.026 (0.070)	0.024 (0.065)	0.027 (0.067)
Non-farm share	0.227 (0.264)	0.261 (0.282)	0.250 (0.277)	0.272 (0.271)
Scheduled Tribe share	0.293 (0.381)	0.226 (0.352)	0.248 (0.363)	0.192 (0.332)
Scheduled Caste share	0.132 (0.178)	0.159 (0.195)	0.150 (0.190)	0.176 (0.206)
Literacy rate	0.569 (0.159)	0.561 (0.156)	0.564 (0.157)	0.573 (0.150)
Villages observed	21,921	45,019	66,940	596,393
Panel D. Land, water, and consumption				
Canal-irrigated share of village area, 2001	0.039	0.061	0.054	0.097
Canal-irrigated share, 2011	0.036	0.055	0.049	0.088
Irrigated share of village area, 2001	0.154	0.194	0.181	0.287
Irrigated share, 2011	0.257	0.280	0.273	0.341
Cultivated share of village area, 2001	0.592	0.598	0.596	0.641
Cultivated share, 2011	0.555	0.572	0.567	0.615
Log consumption per capita, 2012	9.691	9.720	9.710	9.692
Poverty rate, SHRUG line, 2012	0.363	0.326	0.338	0.355
Poverty rate, Tendulkar line, 2012	0.280	0.242	0.254	0.267

Notes: Unweighted means over villages observed in the round, with standard deviations in parentheses for shares. Treated: nearest dam completed 2002–2011. Control: nearest dam not completed by 2011. Sample: the two together. All villages: every village in the SHRUG, whatever its distance from a dam. Composition shares are of main workers; the Scheduled Tribe, Scheduled Caste, and literacy shares are of population; the worker participation rate is total workers (main and marginal) over population. Households are population divided by household size. In 1991 the non-farm share is main workers less cultivators, agricultural laborers, and household-industry workers (Section 3.1). Composition shares are observed for villages with at least one main worker: 62,708 sample villages in 1991, 65,465 in 2001, and 66,282 in 2011. Panel D: shares of village area from the Village Directories (Section 3.4), observed for 60,304 sample villages in 2001 and 63,917 in 2011 (canal); consumption and poverty from the 2012 SECC, observed for 63,524 sample villages.

4 Empirical strategy

4.1 Identification

A comparison of villages near dams with villages elsewhere would conflate the effect of the dam with the characteristics of the places chosen for dams. The design therefore compares dam sites with dam sites. Every village in the sample lies within 20 kilometers of a registered dam, and the comparison is between sites at which the dam was completed in 2002–2011 and

sites at which it had not been completed by 2011. The two groups share what selection for a dam implies: a river at a workable gradient, a valley of the appropriate shape, the terrain in which dams are sited, and the tenure conditions that make land at the site inexpensive to acquire. What differs between them is when the dam was finished. The identifying assumption is that, conditional on the 2001 characteristics on which the estimator balances and on the state, treated and control villages would have followed the same path over 2001–2011 had no dam been completed. Sites selected for a treatment but not yet treated have served as controls before, in the runner-up counties of Greenstone, Hornbeck, and Moretti (2010), the proposed regional authorities of Kline and Moretti (2014), and the mines and dam sites that open later in Kotsadam and Tolonen (2016) and Goodman (2024); the present design tests the status of its control sites directly (Section 4.3).

4.2 Estimator

The primary estimate is a first difference over 2001–2011 with control villages reweighted by entropy balancing. For village v, let $\Delta y_v = y_{v,2011} - y_{v,2001}$ denote the change in an outcome over the decade and T_v an indicator for a nearest dam completed in 2002–2011. Control villages are first reweighted by entropy balancing (Hainmueller 2012) so that the weighted means of nine 2001 characteristics in the control group equal their means in the treated group: the shares of main workers who were agricultural laborers, cultivators, and non-farm workers; the worker participation rate; the Scheduled Tribe and Scheduled Caste shares of population; the literacy rate; log population; and household size. Table 2 reports the balance before and after reweighting; after reweighting the control means match the treated means to the third decimal. I then estimate

$$\Delta y_v = \alpha_{s(v)} + \beta\, T_v + \varepsilon_v, \tag{1}$$

by weighted least squares with state fixed effects $\alpha_s(v)$ and standard errors clustered by district (352 clusters). The coefficient β is the difference between the average 2001–2011 change in treated villages and the weighted average change in control villages in the same state. The first difference removes time-invariant village characteristics, the reweighting removes the differential trends that observed baseline differences would otherwise induce, and the state effects absorb state policy and the state-specific pace of the exit from cultivation.[9]

[9] A two-way fixed effects regression on the three rounds would rest in part on the 1991 round, in which non-farm employment is not measured consistently, and would leave no pre-period against which to check the design; with the 1992–2001 cohort excluded, the staggered comparisons that make such regressions uninformative under heterogeneous effects (Goodman-Bacon 2021; de Chaisemartin and D'Haultfœuille 2020) are avoided. The Callaway and Sant'Anna (2021) estimator on the same panel, with not-yet-treated villages

Table 2: Balance on the 2001 characteristics before and after entropy balancing

	Treated	Control, unweighted	Control, reweighted
Agricultural-labor share of main workers	0.224 (0.234)	0.219 (0.232)	0.224 (0.234)
Cultivator share	0.549 (0.284)	0.542 (0.289)	0.549 (0.287)
Non-farm share	0.202 (0.236)	0.210 (0.240)	0.202 (0.236)
Worker participation rate	0.504 (0.113)	0.481 (0.130)	0.504 (0.127)
Scheduled Tribe share	0.278 (0.374)	0.215 (0.347)	0.278 (0.381)
Scheduled Caste share	0.126 (0.173)	0.156 (0.193)	0.126 (0.163)
Literacy rate	0.476 (0.177)	0.453 (0.179)	0.476 (0.180)
Log population	6.404 (1.281)	6.382 (1.276)	6.404 (1.287)
Household size (persons)	5.216 (1.053)	5.382 (1.188)	5.216 (1.087)
Villages	21,462	44,003	44,003

Notes: Means, with standard deviations in parentheses, of the nine balancing characteristics in 2001 for treated villages (nearest dam completed 2002–2011) and control villages (nearest dam not completed by 2011) with all nine characteristics observed; the last column weights control villages by the entropy-balancing weights, which equate the nine means to those of the treated villages (Hainmueller 2012). The headline regression uses the 64,920 of these villages for which the 2011 composition of work is also observed.

Beside every estimate, the results tables report the raw 2001 and 2011 means in the two groups.

4.3 The control group

The control group pools two kinds of site: villages beside dams under construction in 2011 (completion years 2012–2018) and villages beside undated dams. Construction raises non-farm employment and lowers farm employment at a site, so the first group biases the estimated rise in agricultural labor upward; an undated dam that was in fact completed long ago biases every estimate toward zero. Table 3 examines the two components separately and adds, as a third comparison group whose status is certain, villages beside dams completed in or before 1990. Villages beside construction sites show the pattern that construction produces: in 2001 their non-farm share of main workers was already 29 percent against 20 in treated villages, and over the decade it rose 8.9 points against 2.4 while their number of main workers fell 5 percent against a rise of 7. Villages beside undated dams resemble villages beside old dams: balanced on 2001 characteristics, their decade changes differ by 0.14 points (0.42) in the non-farm share and 0.03 (0.34) in participation (Appendix Table A3).

Against each group in turn, the rise in the agricultural-labor share is 0.90 points (0.43) relative to old-dam villages, 1.10 (0.64) relative to construction sites, 1.47 (0.50) relative

as controls and no covariates, gives 1.62 percentage points (0.63) for the 2002–2011 cohort against 1.78 (0.47) from (1) (Section 9).

Table 3: Treated villages and three control groups of known status, 20-kilometer ring

	Treated	Old dams ($\leq$ 1990)	Construction sites (2012–18)	Undated dams	Pooled pipeline
Villages	21,921	190,797	6,489	38,530	45,019
2001 levels					
Agricultural-labor share	0.224	0.239	0.153	0.229	0.219
Non-farm share	0.202	0.201	0.293	0.196	0.210
Literacy rate	0.476	0.461	0.537	0.438	0.452
Scheduled Tribe share	0.279	0.270	0.168	0.224	0.216
Log population	6.395	6.480	6.020	6.424	6.367
Raw change 2001–2011					
Agricultural-labor share (pp)	7.17	6.30	3.17	5.94	
Non-farm share (pp)	2.40	3.80	8.88	4.38	
Main workers (%)	7.1	3.1	−5.0	1.1	
Estimate from (1), treated relative to the group					
Agricultural-labor share (pp)		0.90** (0.43)	1.10* (0.64)	1.47*** (0.50)	1.78*** (0.47)
Non-farm share (pp)		−0.88** (0.40)	−4.45*** (1.22)	−0.64 (0.54)	−1.91*** (0.61)
Worker participation rate (pp)		1.29*** (0.29)	0.19 (0.58)	1.24*** (0.39)	0.99** (0.41)
Consumption per capita, 2012 (%)		−1.7** (0.85)	−2.8* (1.5)	−0.65 (1.3)	−0.83 (1.21)
Agricultural-labor share, 1991→2001 placebo (pp)		1.29*** (0.44)	0.12 (0.57)	0.65 (0.57)	0.13 (0.52)
Agricultural-labor share, 0–10 km (pp)		1.35*** (0.42)	0.42 (0.79)	0.83 (0.51)	0.92* (0.47)

Notes: Old dams: nearest dam completed in or before 1990. Construction sites: nearest dam with a completion year of 2012 to 2018. Undated: nearest dam with no recorded completion year. Pooled pipeline: the last two together, the control group of the main estimates. Each estimate is from (1) with entropy balancing of that group to the treated villages on the nine 2001 characteristics, state fixed effects, and standard errors clustered by district (in parentheses). Consumption is the 2012 level conditional on the nine 2001 characteristics (Section 3.4). Levels for the pooled group are the control means of Table 1. Significance at the 10, 5, and 1 percent levels is marked *, **, and ***.

to undated-dam villages, and 1.78 (0.47) relative to the pooled control group. The pooled estimate is the largest because the construction villages, whose farm-labor share fell as construction employment rose, are among its controls; removing them gives 1.47. Old-dam villages cannot serve as the main control group because they fail the 1990s placebo in the ring (1.29, 0.44): villages beside dams completed decades earlier were on a trajectory of their own. I therefore retain the register's own definition of the control group and report the headline as a range, 0.9 to 1.5 percentage points against the groups whose status is certain and 1.8 against the pooled group. The non-farm estimate is more sensitive: −1.91 (0.61) against the pooled group, −0.64 (0.54) against undated-dam villages, and −0.88 (0.40) against old-dam villages, so that the shortfall in non-farm growth is 0.6 to 0.9 points against the clean groups; within 10 kilometers of the dam it is significant against every group (−0.92, 0.44, against undated-dam villages; −1.24, 0.34, against old-dam villages; −4.07, 1.12, against construction sites).

4.4 Pre-trends

The 1991 round provides a pre-period in which no dam in the treatment group had been completed, and over 1991–2001 treated and control villages moved together. In the Callaway and Sant'Anna 1991–2001 placebo cells for the 2002–2011 cohort, 18 of the 19 outcomes and characteristics show no differential change at the 5 percent level; the exception is literacy, a balancing characteristic whose 2001 level is held fixed and whose 2001–2011 change does not differ between the groups (Appendix Table A2). The first-difference placebo for the agricultural-labor share is 0.24 percentage points (0.61) at 0–10 kilometers and 0.33 (0.64) at 10–20, and fails in every band beyond 20 kilometers, so the causal claims of the paper are confined to the ring (Appendix Table A1). Villages more than 50 kilometers from any dam fail the same test against treated and control villages alike, by 5 to 6 points, which is the empirical basis for comparing dam sites only with dam sites. Two further checks are reported in the appendix: among treated villages the year of completion is unrelated to the 2001 characteristics of the surrounding villages, so there is no indication that sites cheaper to displace were finished first (Appendix Table A6), and the household margin, which does show a pre-trend, is reported as a descriptive pattern rather than as an effect (Section 7.3).

4.5 Distance and direction

Two extensions of the design test what the 20-kilometer radius assumes, one across distance and one across the geometry of the site. The distance computation is cut into 10-kilometer bands out to 50 kilometers, and within each band treated villages are compared with control villages of the same band, with the balancing repeated within the band, so that the spatial extent of the effect is estimated rather than assumed (Butts 2023). Within the ring, treated villages are divided by their position relative to the reservoir (Section 3.3), and the four cells enter equation (1) together, each compared with the same reweighted control group. If the rise in farm labor reflected irrigation-driven demand, it would be largest below the dam and absent above the reservoir; if it reflects a cost of the site, it need not be.[10]

5 Main results

This section reports what the completion of a dam did to work in the villages around it. Every estimate is from equation (1): the change over 2001–2011 in treated villages relative

[10] An alternative is to reweight the control group separately to each cell. The two constructions give the same estimate for the treated villages as a whole but rank the cells differently, because in 2001 the agricultural-labor share was 27 percent below the dam and 18 percent above the reservoir, against 22 percent in control villages, and villages with low initial shares rose more over the decade. Section 6.2 reports both.

Table 4: Main estimates: the composition of work and the number of workers, 2001–2011

	Treated 2001	Treated 2011	Control 2001	Control 2011	Estimate (SE)
Panel A. Shares (percentage points)					
Agricultural-labor share	22.4	29.6	21.9	27.4	1.78*** (0.47)
Cultivator share	54.8	45.9	54.1	43.9	0.68 (0.68)
Household-industry share	2.6	1.9	3.0	2.6	−0.54*** (0.21)
Non-farm share	20.2	22.6	21.0	26.0	−1.91*** (0.61)
Worker participation rate	50.4	50.8	48.0	48.3	0.99** (0.41)
Main-worker share of population	36.5	36.3	34.4	32.9	1.31*** (0.41)
Marginal-worker share of population	13.9	14.5	13.7	15.4	−0.31 (0.50)
Panel B. Headcounts per village (log change × 100)					
Total workers	673	795	744	901	3.6*** (1.1)
Main workers	530	630	595	712	6.3*** (1.8)
Agricultural laborers	129	187	118	168	10.9*** (3.2)
Cultivators	193	189	177	163	6.5** (2.8)
Non-farm workers	189	237	276	358	−2.6 (2.1)
Household-industry workers	19	17	24	24	−4.9 (3.2)
Marginal workers	143	166	149	189	−2.7 (3.7)
Population	1,525	1,767	1,756	2,089	1.5* (0.8)
Households	305	387	347	450	2.2*** (0.8)

Notes: Treated villages: nearest dam completed 2002–2011. Control villages: nearest dam not completed by 2011. Means are unweighted means over the villages in the headline regression sample with the outcome observed in both rounds; shares in Panel A are in percent, headcounts in Panel B are counts per village. Estimates are from equation (1): the 2001–2011 change in treated villages relative to control villages reweighted by entropy balancing on nine 2001 characteristics, with state fixed effects and standard errors clustered by district (in parentheses). Panel A estimates are in percentage points; Panel B estimates are 100 times the coefficient on a log change, read as percent. Villages with no workers in a category in either year are absent from that category's log-change regression (Appendix Table A5 reports the inverse hyperbolic sine versions). Significance at the 10, 5, and 1 percent levels is marked *, **, and ***.

to control villages reweighted on the nine 2001 characteristics, with state fixed effects and standard errors clustered by district. Table 4 gives the estimates for the 20-kilometer ring, first for the composition of work and the participation margins, then for the number of workers in each category; beside each estimate it reports the raw 2001 and 2011 means in the two groups, so that each effect can be read against the decade's own movement. Table 5 traces the estimates with distance from the dam.

5.1 The composition of work

The headline is the first row of Table 4. The share of main workers in agricultural wage labor rose by 1.78 percentage points (0.47) more in treated villages than in reweighted control villages over 2001–2011, an estimate significant at the 1 percent level whose 95 percent confidence interval runs from 0.9 to 2.7 points. The raw means show what the estimate adds to. The decade raised the agricultural-labor share everywhere: from 21.9 to 27.4 percent in control villages, a rise of 5.5 points, and from 22.4 to 29.6 percent in treated villages, a rise of

7.2 points. The estimate is the gap between these paths after reweighting, about a third of the decade's own rise added on top of it, or, against the 2001 base of 22.4 percent, an increase of 8 percent. In the villages beside a completed dam, roughly two more of every hundred main workers were agricultural laborers in 2011 than the path of comparable villages implies.

The other three categories show where, in the accounting sense that the four shares sum to one, those workers came from. The cultivator share is the largest category and the one that moved most over the decade, falling by nine to ten percentage points in both groups; it fell by the same amount in treated and control villages (0.68, 0.68), so the dam did not accelerate the exit from cultivation. The household-industry share, small to begin with at 2.6 percent, fell in both groups and fell 0.54 points (0.21) further in treated villages, from 2.6 to 1.9 percent against 3.0 to 2.6 in control villages. The non-farm share carries the counterpart of the headline. It rose in both groups, but by 2.4 points in treated villages against 5.0 in control villages, and the estimate, −1.91 points (0.61), is a shortfall in growth rather than a decline: relative to comparable villages, the host villages moved into non-farm work at less than half the pace. Taken together, the four estimates describe one reallocation. The part of the workforce that elsewhere left cultivation for trade, transport, services, and construction left it, in the host villages, for wage work on the land of others.[11]

The lower rows of Panel A show that the reallocation came with more work, not only different work. The worker participation rate, workers of either kind over population, rose 0.99 points (0.41) more in treated villages: it rose 0.4 points in treated villages over the decade while, among control villages reweighted to resemble them, it fell by about half a point. The main-worker share of the population, those who worked at least six months of the year, rose 1.31 points (0.41) more, and here the raw means are stark: it held at about 36 percent in treated villages (36.5 to 36.3) while it fell from 34.4 to 32.9 percent in control villages. The marginal-worker share, those who worked fewer than six months, did not differ (−0.31, 0.50). More residents of the host villages were at work after completion, and the additional work was main work rather than seasonal work.

5.2 The number of workers

Panel B converts the shares into people. A share can rise because its numerator grows or because other categories shrink, and the headcounts, in log changes per village, settle which. The number of agricultural laborers per village rose 10.9 percent (3.2) more in treated villages, from 129 to 187 against 118 to 168 in control villages. The number of cultivators, which fell in both groups as the decade's exit from cultivation ran its course, fell

[11]The four estimates sum to 0.01 percentage points: $1.78 + 0.68 - 0.54 - 1.91$. Each is estimated on the villages for which that share is observed in both rounds.

by 6.5 percent (2.8) less in treated villages, from 193 to 189 against 177 to 163: the host villages retained cultivators that comparable villages lost. The number of non-farm workers rose in both groups, and the difference is small and not significant (−2.6 percent, 2.1), as is the log estimate for household-industry workers (−4.9, 3.2), which becomes significant only when villages with no such workers are retained through the inverse hyperbolic sine (−8.4, 2.7; Appendix Table A5). The rise in the agricultural-labor share is therefore an addition of laborers to a workforce whose other components grew as they grew elsewhere or fell less. It is not a relabeling of cultivators as laborers, since cultivators were retained, and it is not a collapse of non-farm employment, since non-farm workers were not lost; it is a growth of the bottom of the occupational hierarchy.

The totals confirm the reading. Main workers per village rose 6.3 percent (1.8) more in treated villages and total workers 3.6 percent (1.1) more, against a rise in population of 1.5 percent (0.8) and in households of 2.2 percent (0.8). Workers grew faster than the people they are drawn from, and the marginal-worker count did not differ (−2.7, 3.7), so the growth was in year-round work. A temporary construction boom would raise marginal employment and non-farm employment at the site; what the data show is the opposite pair, an addition of main workers in agricultural wage labor, and Section 9 shows that it grows with time since completion. Section 7.3 examines the demographic margins directly: the age and sex structure of the population did not change, and adding the demographic changes as controls leaves the share estimates intact and reduces the headcount estimates by a tenth to a quarter.

5.3 Effects by distance from the dam

Table 5 traces the effect with distance, comparing treated villages in each 10-kilometer band with control villages of the same band, and Figure 1 plots the agricultural-labor estimates and their placebos by band. The rise in the agricultural-labor share is present in the two bands of the ring and in no band beyond it: 0.92 points (0.47) at 0–10 kilometers, 2.55 (0.66) at 10–20, and then 0.84 (0.66), 0.04 (0.52), and −0.70 (0.68). The raw changes in the last row make the same point: the agricultural-labor share of treated villages rose 7.6 points against 6.5 in control villages at 0–10 kilometers and 6.5 against 4.7 at 10–20, while beyond 20 kilometers the two groups' changes are within a point of each other. The non-farm shortfall is largest and most precisely estimated in the innermost band, −2.01 (0.56). Participation and main workers rise in both bands of the ring, and the number of agricultural laborers rises most at 10–20 kilometers, 18.4 percent (4.5). The estimates beyond 20 kilometers are reported for completeness and are not interpreted, because the 1991–2001 placebo in the

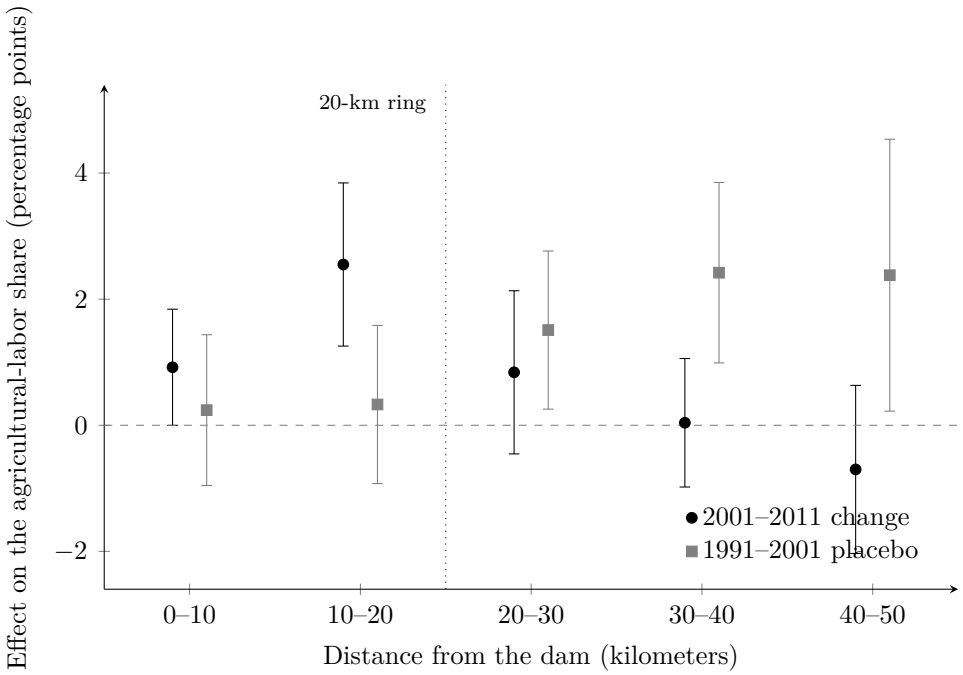


Figure 1: The effect by distance from the dam and its placebo

Notes: Point estimates and 95 percent confidence intervals from Table 5. Each estimate compares treated villages in the band with control villages of the same band, with entropy balancing on the nine 2001 characteristics repeated within the band, state fixed effects, and standard errors clustered by district. The placebo applies the same specification to the 1991–2001 change.

sixth column fails there. The effect is a property of the site, present where the reservoir and its rim are located and absent 20 kilometers away.[12]

6 Mechanism

The rise in agricultural wage labor near a completed dam is consistent with two mechanisms that carry opposite implications for the site. Under a labor-demand mechanism, the dam's water expands cultivation around it and draws workers onto the land, as it does in command areas; the site would then share in the benefit, and the additional labor would be its gain. Under a land-loss mechanism, the dam submerges land and disrupts the livelihoods attached to it, and displaced households supply labor to a market that absorbs it, if at all, in agricultural wage labor; the site would then bear a cost, and the additional labor would be its

[12]The effect is larger at 10–20 kilometers than at 0–10. Section 9 shows that beside the largest reservoirs it is present in both bands at similar magnitude. Appendix Table A1 also compares each group with villages more than 50 kilometers from any dam over 2001–2011: against far villages, treated and control villages alike show a larger rise within 10 kilometers of the site and a smaller rise beyond 20, so a design that used far villages as the counterfactual would attribute to the dam a pattern that the control villages share.

Table 5: The effect by distance from the dam

Distance band (km)	Agricultural-labor share	Non-farm share	Participation	Main workers (%)	Agricultural laborers (%)	1991–2001 placebo, ag.-labor share	Treated / pipeline villages
0–10	0.92* (0.47)	−2.01*** (0.56)	0.79* (0.42)	5.5*** (1.9)	4.3 (3.4)	0.24 (0.61)	12,726 / 21,129
10–20	2.55*** (0.66)	−1.37 (0.98)	1.12* (0.64)	5.8** (2.6)	18.4*** (4.5)	0.33 (0.64)	8,582 / 22,483
20–30	0.84 (0.66)	−1.48 (1.08)	2.16 (1.48)	4.6 (4.4)	6.4 (5.8)	1.51** (0.64)	4,876 / 18,577
30–40	0.04 (0.52)	−2.80 (1.94)	2.96** (1.15)	17.8*** (4.6)	2.6 (6.4)	2.42*** (0.73)	3,201 / 13,743
40–50	−0.70 (0.68)	−2.88 (2.06)	1.31 (0.99)	14.8*** (4.1)	4.4 (7.9)	2.38** (1.10)	2,204 / 10,775
Raw change in the agricultural-labor share, treated / pipeline	7.6 / 6.5	6.5 / 4.7	3.4 / 3.3	2.6 / 3.8	3.0 / 3.9		

Notes: Each row compares treated villages in the band with control villages in the same band, with entropy balancing on the nine 2001 characteristics repeated within the band, state fixed effects, and standard errors clustered by district (in parentheses). Shares in percentage points; main workers and agricultural laborers are 100 times the log change. The placebo column applies the same specification to the 1991–2001 change in the agricultural-labor share. The raw-change row is the unweighted change in the agricultural-labor share, in percentage points, for treated and control villages of the band, listed by band from 0–10 to 40–50 kilometers. Significance at the 10, 5, and 1 percent levels is marked *, **, and ***.

Table 6: Mechanisms: irrigation, land use, and position relative to the reservoir

	Treated 2001	Treated 2011	Control 2001	Control 2011	Estimate (SE)
Panel A. Irrigation and land use, shares of village area, 20-km ring (percentage points)					
Canal-irrigated	3.9	3.7	6.0	5.2	−0.06 (0.31)
Well- and tubewell-irrigated	3.7	10.0	6.9	10.8	0.79 (0.55)
Irrigated, all sources	15.3	27.2	19.4	27.7	−1.23 (1.57)
Unirrigated cultivated	44.0	29.7	40.3	30.2	0.67 (1.32)
Cultivated	59.1	56.1	59.8	56.2	0.64 (0.85)
Not available for cultivation	14.6	14.0	15.1	14.5	0.78 (0.58)

	Below the dam	Reservoir rim	Above the reservoir	Different basin	p, above = below
Treated villages	5,465	3,511	7,061	2,900	
Panel B. Joint specification, main weights (percentage points)					
Agricultural-labor share	1.12 (0.73)	1.86*** (0.61)	2.27*** (0.63)	1.69* (0.93)	0.11
Non-farm share	−1.41*** (0.53)	−1.95*** (0.54)	−1.86** (0.88)	−0.79 (1.51)	0.53
Worker participation rate	1.24*** (0.47)	1.13*** (0.43)	1.74*** (0.49)	−1.09 (0.95)	0.27
Cultivator share	1.29 (0.88)	0.55 (0.67)	−0.06 (0.99)	−0.78 (1.56)	0.18
Agricultural-labor share, raw 2001 → 2011	26.9 → 34.3	26.2 → 35.0	18.0 → 25.3	17.0 → 22.1	
Canal-irrigated share of area	0.65 (0.48)	−0.58 (0.63)	−0.21 (0.35)	−0.04 (0.42)	0.09
Panel C. Control group reweighted to each cell					
Agricultural-labor share (pp)	3.29*** (0.75)	3.27*** (0.67)	0.54 (0.62)	−0.07 (0.82)	
Non-farm share (pp)	−2.51*** (0.51)	−3.06*** (0.53)	−1.68* (0.87)	1.65 (1.12)	
Worker participation rate (pp)	0.93** (0.47)	1.26*** (0.44)	2.25*** (0.52)	−1.33* (0.77)	
Area not available for cultivation (pp)	1.04 (0.66)	2.18*** (0.57)	0.39 (0.83)	0.19 (0.70)	
Households (%)	2.1** (1.0)	1.1 (1.0)	2.8*** (1.0)	2.4** (1.2)	
Population (%)	2.3** (1.0)	0.7 (1.1)	2.3** (1.0)	1.3 (1.4)	
Household size (persons)	0.001 (0.026)	−0.034 (0.029)	−0.031 (0.034)	−0.066* (0.034)	
Main workers (%)	9.5*** (2.3)	8.9*** (2.3)	7.8*** (2.3)	−1.9 (3.7)	
Cultivators (%)	7.2** (3.5)	2.3 (3.5)	8.2** (3.7)	4.9 (3.9)	
Agricultural laborers (%, asinh)	25.1*** (5.6)	23.7*** (5.1)	8.3** (4.2)	3.4 (7.1)	
Non-farm workers (%, asinh)	−2.3 (3.0)	−7.7** (3.0)	−1.0 (2.9)	10.7** (5.1)	

Notes: Panel A: shares of village area in percent from the Village Directories of 2001 and 2011, for treated and control villages in the 20-kilometer ring; estimates from equation (1) in percentage points. Panels B and C: treated villages of the ring divided by position relative to the reservoir (Section 3.3); 2,371 treated villages lack a dam elevation and are not classified. Panel B: the four cells enter equation (1) together with the control group reweighted as in the main specification; the last column tests equality of the above-the-reservoir and below-the-dam coefficients; the canal row is the same specification applied to the canal-irrigated share of village area. Panel C: each cell compared with the control group reweighted to that cell's 2001 characteristics, one regression per cell and outcome; headcounts are 100 times the log change, except agricultural laborers and non-farm workers, which use the inverse hyperbolic sine so that villages with no such workers in a year are retained. Standard errors clustered by district in parentheses; significance at the 10, 5, and 1 percent levels is marked *, **, and ***.

measure. Table 6 distinguishes the two by asking whether water arrived, whether the effect depends on the village's position relative to the reservoir, and what happened to the land.

6.1 Irrigation and land use

Panel A of Table 6 reports irrigation and land use in the 20-kilometer ring, as shares of village area in 2001 and 2011. Canal-irrigated area did not increase in treated villages relative to control villages: the estimate is −0.06 percentage points (0.31) on a 2001 base of 3.9 percent of village area, and the canal share fell slightly in both groups. Well and tubewell irrigation grew in both groups, from 3.7 to 10.0 percent of area in treated villages and from 6.9 to 10.8 in control villages, with no significant difference between them (0.79, 0.55), and total irrigated area and cultivated area moved together in the two groups. Band by band, the only significant canal estimate in the ring is for villages below the dam at 10–20 kilometers, 1.92 points (0.76), the signature of a canal head reaching the first villages of a command area; it amounts to two percent of village area and is confined to one cell, and the labor effect is not confined to it (Appendix Table A8). Village amenities, from roads to banks, spread over the decade in both groups, and none differs between them. Villages beside a dam that was completed during the decade were no more irrigated at its end than villages beside a dam that was not.

6.2 Position relative to the reservoir

Panel B divides the treated villages by their position relative to the reservoir. The rise in the agricultural-labor share is present in every cell of the dam's own basin: 1.12 points (0.73) below the dam, 1.86 (0.61) at the rim, and 2.27 (0.63) above the reservoir, with 1.69 (0.93) in the different-basin cell. The estimate above the reservoir is at least as large as the estimate below the dam, and the two are not statistically distinguishable ($p = 0.11$). The non-farm shortfall and the rise in participation are likewise present in all three same-basin cells, and the raw changes show the same pattern before any adjustment: the agricultural-labor share rose 7.4 points below the dam, 8.8 at the rim, and 7.3 above the reservoir, against 5.5 in control villages. Canal-irrigated area did not rise in any cell. Under the labor-demand mechanism the additional farm labor would be largest below the dam and absent above the reservoir; it is instead present above the reservoir, in villages that received no water from the dam and could receive none, and it is no smaller there than below the dam. The labor-demand mechanism is rejected for the site.

Panel C reports the alternative construction, in which the control group is reweighted to each cell separately. The two constructions rank the cells differently because the cells start from different baselines: in 2001 the agricultural-labor share was 27 percent below the dam and 26 at the rim but 18 above the reservoir, and villages with low initial shares rose more over the decade in both groups. Reweighted to each cell, the rise in the agricultural-

labor share is 3.29 points (0.75) below the dam and 3.27 (0.67) at the rim, and 0.54 (0.62) above the reservoir, not significant, while participation rises most above the reservoir under both constructions (2.25 points, 0.52). The two constructions agree that the effect is present throughout the dam's own basin and that no canal water reached any part of it; they disagree about whether the shift into wage labor is concentrated at the water or spread across the basin. The joint specification is the paper's main statement.

6.3 Land and population by cell

The lower rows of Panel C report what happened to the land and to the population in each cell. The share of village area not available for cultivation rose 2.18 percentage points (0.57) more at the reservoir's rim than in reweighted control villages, did not rise above the reservoir (0.39, 0.83), and rose imprecisely below the dam (1.04, 0.66); this is the reservoir in the land-use record, and land left cultivation where the water is.[13] Population moved in the opposite direction. Households rose 2.8 percent (1.0) above the reservoir and 2.1 percent (1.0) below the dam, and population by 2.3 percent in each, while at the rim neither households nor population differ from control villages. Main workers rose in all three same-basin cells, by 7.8 to 9.5 percent; cultivators were retained above the reservoir (8.2 percent, 3.7) and below the dam (7.2 percent, 3.5); and agricultural laborers rose most where the land was lost, 25.1 percent (5.6) below the dam and 23.7 percent (5.1) at the rim. The Census does not follow households, so the analysis cannot establish that the households added above the reservoir are those whose land at the rim was lost. It does establish that the site was not depopulated: land was lost at the water, and the population and its work were reorganized around the land that remained.[14]

7 Heterogeneity

7.1 Tribal population share and dam purpose

If the cost of the site is the cost of land lost without compensation, it should be concentrated where land was held without title. Table 7 splits the treated villages at the median 2001 Scheduled Tribe population share (1.3 percent) and by the stated purpose of the nearest

[13]The Directory reports land use for villages that exist; a village submerged entirely leaves the panel, so the estimate understates the loss of land. At 0–10 kilometers the rise in area not available for cultivation is 2.39 points (0.59) at the rim and 1.52 (0.62) below the dam.

[14]The remaining land-use categories show no degradation at the site (Appendix Table A7): forest area did not change in any cell, and current fallows in 2011 were lower at the rim and above the reservoir, by 1.4 and 1.7 points on a control mean of 5.6, while net sown area was higher below the dam and at the rim.

dam, comparing each subsample with the same reweighted control group.[15] In villages below the median tribal share the effect on the agricultural-labor share is 0.61 percentage points (0.44) and not distinguishable from zero; in villages above the median it is 2.70 points (0.60), with the non-farm shortfall (-2.24 against -1.33) and the rise in participation (1.45 against 0.76) following the same pattern. The interaction with distance rules out proximity as the explanation. In high-tribal-share villages the effect is present at both distances, 2.66 points at 0–10 kilometers and 3.24 at 10–20; in low-tribal-share villages it is absent at both. Under irrigation-driven labor demand the effect would appear in villages close to the dam irrespective of their population; instead it appears wherever tribal communities live within the ring, and nowhere else.

The split by the stated purpose of the nearest dam points in the same direction. Where the purpose includes irrigation, the agricultural-labor share rises 2.09 points (0.52); where it does not, 0.38 (0.65), not significant. Irrigation dams are built where cultivable land is, and their reservoirs take cultivated land, whereas a hydropower dam in a gorge takes less. The bottom rows of Table 7 show that the marginal-worker share of the population does not respond, overall or in either tribal group: the reallocation operates entirely through main workers, employed six months or more, which is the signature of a persistent change rather than of a seasonal demand shock. These results describe where the cost falls; they are statements about exposure rather than about the individuals exposed, since the Census does not observe which residents of a high-share village became laborers.

7.2 Women and men

Table 8 estimates the main outcomes separately for women and men and tests the difference directly. The shift into agricultural wage labor falls on both: the agricultural-labor share rises 1.93 points (0.56) among female main workers and 1.59 (0.44) among male, and the number of female and male agricultural laborers rises by about the same proportion, 9.6 and 9.4 percent. The shortfall in non-farm employment is larger for women. The non-farm share of female main workers grows 2.75 points (0.78) less in treated villages than in control villages, against 1.62 (0.55) less for men, and the difference, 1.23 points (0.48), is significant at the 5 percent level; participation rises for both sexes, by 1.21 points (0.58) for women and 0.70 (0.31) for men. The headcounts complete the picture: the number of female main workers rises 8.7 percent (3.2) while the number of female non-farm workers is unchanged

[15] The median of the 2001 Scheduled Tribe share is 1.3 percent while its mean is 24 percent, so the split separates villages with almost no Scheduled Tribe population from villages with a substantial one. The control group is reweighted once on eight of the nine characteristics of Section 4.2 (household size omitted), and for the tribal split both treated and control villages are divided at the median. Reweighting the control group separately to each subsample on all nine characteristics gives larger contrasts in the same directions.

Table 7: Heterogeneity by tribal population share and dam purpose

Subsample of treated villages	Agricultural-labor share	Non-farm share	Participation	Treated / control villages
Scheduled Tribe share below the 2001 median	0.61 (0.44)	−1.33 (0.96)	0.76 (0.72)	9,196 / 23,292
Scheduled Tribe share above the median	2.70*** (0.60)	−2.24*** (0.77)	1.45*** (0.39)	12,112 / 20,320
Low ST share, 0–10 km	−0.23 (0.47)	−1.01 (0.73)	0.46 (0.51)	5,083 / 43,612
Low ST share, 10–20 km	0.80 (0.56)	−1.81 (1.27)	0.17 (0.88)	4,113 / 43,612
High ST share, 0–10 km	2.66*** (0.54)	−2.11*** (0.58)	1.34*** (0.44)	7,643 / 43,612
High ST share, 10–20 km	3.24*** (0.84)	−2.58** (1.10)	1.79*** (0.50)	4,469 / 43,612
Dam purpose includes irrigation	2.09*** (0.52)	−0.87* (0.49)	0.99*** (0.34)	18,507 / 43,612
Dam purpose excludes irrigation	0.38 (0.65)	−7.49*** (2.61)	1.61 (1.93)	2,732 / 43,612
Dam purpose includes hydropower	1.41*** (0.53)	−2.58 (1.66)	1.75* (0.90)	3,776 / 43,612
Dam purpose excludes hydropower	2.04*** (0.56)	−1.84*** (0.58)	0.80* (0.42)	17,463 / 43,612
Marginal-worker share of population (pp)				
All treated villages	−0.25 (0.50)			21,308 / 43,612
Low ST share	−0.50 (0.87)			9,196 / 23,292
High ST share	−0.29 (0.58)			12,112 / 20,320

Notes: Each row compares the stated subsample of treated villages with the control group reweighted once by entropy balancing on eight 2001 characteristics (the nine of Section 4.2 without household size), with state fixed effects and standard errors clustered by district (in parentheses). The Scheduled Tribe rows divide both treated and control villages at the sample median of the 2001 Scheduled Tribe share (1.3 percent); the distance-by-share rows and the purpose rows restrict treated villages only. The marginal-worker rows use the same weights; with the main nine-characteristic weights the full-sample estimate is −0.31 (0.50) (Table 4). Percentage points; significance at the 10, 5, and 1 percent levels is marked *, **, and ***.

(0.6 percent, 2.4), so that the women who entered the workforce at the site entered as agricultural laborers. In 2001, 17 percent of female main workers were in non-farm work against 23 percent of male main workers; women had the least access to the work that the site's adjustment foreclosed, and they absorbed the larger share of the shortfall.

7.3 Demographic margins

The changes documented above are changes in the work done at the site, and Table 9 asks whether the population itself changed. Population in the ring rose 1.5 percent (0.8) more in treated villages over 2001–2011, a difference significant only at the 10 percent level, and the number of households 2.2 percent (0.8) more; the share of the population aged zero to six, the sex ratio, and the literacy rate did not move differently. An inflow of construction workers or a departure of families would alter the age and sex structure of a village, and neither is

Table 8: Estimates by sex

	Women	Men	Women − men	2001 level, women / men
Agricultural-labor share (pp)	1.93*** (0.56)	1.59*** (0.44)	0.37 (0.42)	28.9 / 19.8
Cultivator share (pp)	1.47* (0.80)	0.48 (0.63)	1.07* (0.59)	50.2 / 55.0
Non-farm share (pp)	−2.75*** (0.78)	−1.62*** (0.55)	−1.23** (0.48)	17.2 / 23.0
Worker participation rate (pp)	1.21** (0.58)	0.70** (0.31)	0.57 (0.40)	45.8 / 54.7
Main workers (%)	8.7*** (3.2)	5.0*** (1.5)	3.9 (2.6)	
Agricultural laborers (%)	9.6*** (3.4)	9.4*** (2.8)	0.5 (2.0)	
Non-farm workers (%)	0.6 (2.4)	−3.0 (2.0)	2.0 (1.9)	

Notes: Female and male outcomes are the shares of female and male main workers in each category, the female and male worker participation rates (workers over population of that sex), and the log counts of female and male workers; each is estimated with equation (1) in the 20-kilometer ring. The third column regresses the difference between the female and male changes on treatment in the same specification. The last column reports the 2001 levels of the shares, in percent, among treated villages. Standard errors clustered by district in parentheses; significance at the 10, 5, and 1 percent levels is marked *, **, and ***.

visible. Re-estimating the headline equation with the changes in population, households, the 0–6 share, and the sex ratio added, a decomposition rather than a preferred estimate since these are post-treatment variables, leaves the share results unchanged (the agricultural-labor estimate moves from 1.78 to 1.73) and reduces the headcount results by a tenth to a quarter (main workers from 6.3 to 4.8 percent). The composition of work at the site changed because the work of its residents changed.[16]

8 Consumption and poverty

If the additional labor at the site were the absorption of a benefit, it would be visible in what households consume. Table 10 reports the 2012 level of per capita consumption and of two poverty rates from the SECC for treated villages relative to reweighted control villages. Consumption is not measurably different: the estimate is −0.83 percent (1.21), with a 95 percent confidence interval from −3.2 to +1.5 percent, and the poverty rate is not different at either line, 0.85 points (0.84) at the SHRUG line and 0.61 (0.74) at the Tendulkar line. No estimate differs from zero at the 5 percent level within 10 kilometers of the dam, in any band to 50 kilometers, or in any direction cell; the estimates that come closest are lower consumption and higher poverty at 20–30 kilometers, outside the ring, and above the reservoir, where the SHRUG-line poverty rate is 1.7 points (0.9) higher ($p = 0.07$). Against

[16]The household margin is the one margin with a pre-trend: over 1991–2001 the number of households grew 1.6 percent (0.8) less in treated villages and household size 0.07 persons (0.02) more, and over 2001–2011 both signs reverse (Table 9, Panel B). Households at dam sites consolidated in the decade before completion and divided in the decade after, which is consistent with anticipation of acquisition under a rule that compensated the household holding title; the paper reports the pattern and claims no effect on household formation.

Table 9: Demographic margins

	20-km ring	0–10 km
Panel A. Demographic margins, 2001–2011		
Households (%)	2.2*** (0.8)	1.5* (0.9)
Population (%)	1.5* (0.8)	1.0 (1.0)
Household size (persons)	−0.050** (0.023)	−0.034 (0.024)
Share of population aged 0–6 (pp)	0.03 (0.11)	−0.04 (0.11)
Sex ratio	−0.0008 (0.0020)	−0.0013 (0.0022)
Literacy rate (pp)	0.34 (0.36)	0.38 (0.37)
Total workers (%)	3.6*** (1.1)	2.6** (1.3)
Main workers (%)	6.3*** (1.8)	5.5*** (1.9)
Panel B. The same margins, 1991–2001		
Households (%)	−1.6** (0.8)	−1.6** (0.8)
Population (%)	−0.1 (0.8)	−0.3 (0.8)
Household size (persons)	0.070*** (0.021)	0.056*** (0.020)
Share of population aged 0–6 (pp)	0.04 (0.13)	0.05 (0.15)
Sex ratio	−0.0007 (0.0018)	−0.0003 (0.0023)
Panel C. Headline estimates with and without post-treatment demographic controls (ring)		
	No controls	With demographic controls
Agricultural-labor share (pp)	1.78*** (0.47)	1.73*** (0.47)
Non-farm share (pp)	−1.91*** (0.61)	−2.02*** (0.61)
Worker participation rate (pp)	0.99** (0.41)	0.97** (0.40)
Agricultural laborers (%)	10.9*** (3.2)	9.7*** (3.1)
Main workers (%)	6.3*** (1.8)	4.8*** (1.6)

Notes: Panels A and B: estimates from equation (1) for the demographic margins over 2001–2011 and over 1991–2001, in the 20-kilometer ring with the main weights and within 10 kilometers of the dam with the balancing repeated for the band. Percentages are 100 times the log change; the 0–6 share and literacy are in percentage points; household size is in persons; the sex ratio is females per male. Panel C: the headline estimates with and without the 2001–2011 changes in log population, log households, the 0–6 share, and the sex ratio added as regressors; the added variables are post-treatment and the exercise is a decomposition. Standard errors clustered by district in parentheses; significance at the 10, 5, and 1 percent levels is marked *, **, and ***.

old-dam villages, consumption is 1.68 percent (0.85) lower and the poverty rate 1.40 points (0.59) higher, and against construction-site villages 2.75 percent (1.49) lower and 2.07 points (0.92) higher; against undated-dam villages the differences are small and not significant. No comparison indicates a gain. A decade after completion, the villages that host a dam had more of their residents at work, more of them in agricultural wage labor, and no more to consume than comparable villages whose dam had not been finished.

9 Robustness

Table 11 reports the three headline estimates across fourteen specifications that vary the control group, the fixed effects, the clustering, the sample, the cohort, and the estimator.

Table 10: Consumption and poverty, 2012

	Log consumption per capita (%)	Poverty rate, SHRUG line (pp)	Poverty rate, Tendulkar line (pp)	Villages
20-km ring	−0.83 (1.21)	0.85 (0.84)	0.61 (0.74)	62,189
Raw 2012 levels, treated / control (log consumption; poverty rates in percent)	9.691 / 9.720	36.3 / 32.6	28.0 / 24.2	
By distance band, treated vs. pipeline villages of the band				
0–10 km	−0.31 (1.14)	0.53 (0.83)	0.47 (0.77)	12,396 / 20,721
10–20 km	−1.53 (1.49)	1.23 (1.03)	0.78 (0.86)	8,456 / 21,951
20–30 km	−2.46* (1.32)	1.69* (0.92)	1.47* (0.83)	5,021 / 18,148
30–40 km	−1.09 (1.64)	0.34 (0.91)	−0.05 (0.87)	3,233 / 13,460
40–50 km	−0.06 (1.54)	0.98 (1.14)	0.78 (1.18)	2,258 / 10,557
By direction cell, control group reweighted to each cell (ring)				
Below the dam	−0.02 (1.65)	0.37 (1.18)	0.42 (1.08)	
Reservoir rim	0.87 (1.20)	−1.09 (0.87)	−1.15 (0.78)	
Above the reservoir	−2.16 (1.38)	1.71* (0.93)	1.17 (0.75)	
Different basin	0.25 (1.82)	0.35 (1.28)	0.72 (1.24)	
By control group (ring)				
Old-dam villages	−1.68** (0.85)	1.40** (0.59)		
Construction-site villages	−2.75* (1.49)	2.07** (0.92)		
Undated-dam villages	−0.65 (1.28)	0.72 (0.89)		

Notes: Consumption per capita and poverty rates are 2012 levels from the SECC as distributed in the SHRUG (Section 3.4); estimates are for treated villages relative to control villages reweighted on the nine 2001 characteristics, with state fixed effects and standard errors clustered by district (in parentheses). Consumption estimates are 100 times the difference in logs; poverty estimates are in percentage points. The band rows compare treated with control villages of the band with the balancing repeated within the band; the cell rows reweight the control group to each cell; the control-group rows reweight each group of Section 4.3 to the treated villages. Significance at the 10, 5, and 1 percent levels is marked *, **, and ***.

The agricultural-labor estimate is positive in every one, between 0.8 and 2.2 percentage points, and significant at the 5 percent level in twelve. It is stable to the specification of equation (1): 1.67 (0.61) without state fixed effects, 1.78 (0.47) with them, and 1.76 (0.47) with a linear control for distance to the dam. Adding the district's employment-guarantee phase moves it to 1.81; the program reached control districts first (50.4 percent of control villages and 44.9 percent of treated villages lie in the 200 first-phase districts), so it works against the result. Clustering by state instead of district leaves the standard error unchanged. Excluding Maharashtra, which contributes 12,277 of the 64,920 villages, raises the estimate to 1.97 (0.57). Replacing state with district fixed effects confines the comparison to the 159 districts that contain both treated and control villages and halves the estimate to 0.90 (0.50; $p = 0.07$); the same villages give 1.78 (0.54) with state effects, so the reduction reflects within-district shocks or spillovers onto control villages in the same district rather than sample composition, and the within-district estimate serves as a conservative lower

Table 11: Robustness: the headline estimates across specifications

Specification	Agricultural-labor share	Non-farm share	Participation	Villages	Clusters
Main specification: pooled pipeline controls, state effects, district clusters	1.78*** (0.47)	−1.91*** (0.61)	0.99** (0.41)	64,920	352
Undated-dam villages as controls	1.47*** (0.50)	−0.64 (0.54)	1.24*** (0.39)	58,761	338
Old-dam villages as controls	0.90** (0.43)	−0.88** (0.40)	1.29*** (0.29)	207,912	409
District MGNREGA phase added	1.81*** (0.47)	−1.93*** (0.61)	1.00** (0.40)	64,917	350
Mixed districts only, state effects	1.78*** (0.54)	−1.98*** (0.67)	0.74 (0.47)	41,480	159
District effects	0.90* (0.50)	−0.95 (0.64)	0.87* (0.44)	64,906	338
Treated 2007–2011 vs. undated controls	0.79 (0.59)	0.49 (0.70)	1.04** (0.44)	46,313	296
Treated 2002–2006 vs. all controls	2.16*** (0.61)	−2.53*** (0.73)	1.28*** (0.46)	56,059	332
Treated 2007–2011 vs. all controls	1.12** (0.54)	−0.94 (0.69)	0.58 (0.50)	52,473	316
Maharashtra excluded	1.97*** (0.57)	−2.07** (0.81)	1.34** (0.52)	52,643	318
Callaway–Sant'Anna, 2001–2011 group-time effect	1.62*** (0.63)	−2.61*** (0.87)	0.18 (0.47)	—	—
Standard errors clustered by state	1.78*** (0.46)	−1.91** (0.82)	0.99** (0.36)	64,920	26
No state effects	1.67*** (0.61)	−2.20*** (0.81)	1.03** (0.44)	64,920	352
Distance to the dam added	1.76*** (0.47)	−1.93*** (0.62)	0.99** (0.41)	64,920	352

Notes: Each row re-estimates the three headline outcomes under the stated variation of equation (1); unless stated, the control group is the pooled control group reweighted on the nine 2001 characteristics, with state fixed effects and standard errors clustered by district (in parentheses). "Mixed districts" are the 159 districts containing both treated and control villages. The Callaway and Sant'Anna row is the 2001–2011 group-time effect for the 2002–2011 cohort with not-yet-treated controls and no covariates. Percentage points; significance at the 10, 5, and 1 percent levels is marked *, **, and ***.

bound. Villages beside dams completed in 2002–2006, with five to nine years of exposure by 2011, show a larger effect (2.16, 0.61) than villages beside dams completed in 2007–2011 (1.12, 0.54): the effect grows with time since completion, as a persistent change would and a construction-period disturbance would not. The Callaway and Sant'Anna (2021) estimator, with not-yet-treated villages as controls and no covariates, gives 1.62 (0.63) for the 2002–2011 cohort (Appendix Table A2 reports all nineteen series).[17]

[17]Three further checks. On the 37,120 villages with no second, older dam within 20 kilometers, the estimate is 1.61 percentage points, significant at the 1 percent level, from an earlier run of the same specification whose standard error was not recorded. The 1992–2001 cohort, excluded from the main comparison, shows no rise over the decade in which its dams were built and finished (−0.33, 0.58, in the Callaway and Sant'Anna 1991–2001 cell), consistent with an interval that mixes construction, when non-farm employment rises, with the years after completion, when farm labor rises. And the effect is found beside large reservoirs: within the largest tercile of storage the rise in the agricultural-labor share is 1.73 (0.72) at 0–10 kilometers and 1.55 (0.79) at 10–20 with null placebos in both bands, while the two smaller terciles show no effect in the innermost band (Appendix Table A4).

10 Conclusion

This paper has asked what a large dam does to the labor market of the villages around it. In the decade after completion, the villages within 20 kilometers of a dam have more of their residents at work, and the additional work is agricultural wage labor: the share of main workers who are agricultural laborers rises by about two percentage points relative to comparable villages, the number of laborers rises by about a tenth, and the movement into non-farm work that the rest of rural India made over the decade is slower there. No canal water reaches these villages within the decade, the effect is no smaller above the reservoir than below the dam, and consumption in 2012 is no different from that of comparable villages. The effect is confined to the site and grows with time since completion.

Duflo and Pande (2007) found that poverty rises in the districts that host dams, and district data could not show through which channels. The village labor market is one of them, and the results describe how it works. Land leaves cultivation at the reservoir's rim, but the number of cultivators falls less than in comparable villages, not more, so the loss of land does not show up as an exit from cultivation; what grows is the number of laborers. The Census cannot follow households, but this is the pattern one would expect from the way land was acquired. The Land Acquisition Act of 1894 paid the holders of documented title. The people who worked that land without holding title, as tenants, sharecroppers, and laborers, received nothing, and when the land went under water the livelihood it supported went with it; the work they could find was wage labor on other farms, the least remunerative category of rural work. The concentration of the effect in villages with large Scheduled Tribe populations, where land was held under customary tenure rather than registered title, points the same way. The losers from a dam are not only, and perhaps not mainly, those who lost land they owned; they are those who lost the use of land they never owned.

The rule has since changed. The 2013 Act counts as affected families the agricultural laborers, tenants, sharecroppers, and forest-dependent households of the acquired area (Government of India 2013, Section 3(c)), which is the group this pattern points to. The dams built under it, and the villages around them, will be observed in the next census, and whether the new rule changes who bears the cost of a dam is a question that census can answer.

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A Data construction and additional tables

A.1 The dam file

The National Register of Large Dams (Central Water Commission 2018) lists 5,745 dams with their state, district, river, nearest town, coordinates, year of completion, height, length, reservoir area, gross and effective storage, spillway capacity, and stated purpose. The state sheets of the register were stacked, the completion year was parsed from the register's text field, and coordinates recorded in degrees, minutes, and seconds were converted to decimal degrees. Known coordinate errors were corrected, among them the transposed latitude and longitude of the Manzira dam. Dam characteristics used in the analysis are the logs of height, length, volume, storage, reservoir area, effective storage, and spillway capacity, and indicator variables for irrigation, hydropower, and flood control parsed from the purpose string. One dam identifier is duplicated in the register, and 12 dams carry completion years before 1800; all fall in the group of dams completed in or before 1991, which is excluded from the analysis. Of the 5,745 dams, 587 have no completion year and 102 have a completion year after 2011.

A.2 The village panel and the match

Village census abstracts for 1991, 2001, and 2011 come from the SHRUG (Development Data Lab 2023), keyed on the shrid2 identifier, with village centroids from the SHRUG spatial keys. Each village is matched to the nearest registered dam, over all dams in the register regardless of completion status, and its distance to that dam is computed on unprojected coordinates at 111.32 kilometers per degree. The nearest-dam computation was run over all registered dams because an earlier version that matched villages to completed dams only produced controls contaminated by villages beside dams under construction and a differential pre-trend that disappeared when the match was made over all dams (Section 3.2). Direction cells are built from HydroBASINS level-6 basins (Lehner and Grill 2013) and from the elevation of each village relative to the dam site, both taken from the HydroSHEDS digital elevation model (Lehner, Verdin, and Jarvis 2008), the latter available for completed dams only: below the dam, same basin and below the dam's elevation; rim, same basin and at or below the dam's elevation plus its height; above the reservoir, same basin and above that height; and different basin. In the headline sample, 2,371 treated villages lack a dam elevation and are unclassified.

The non-farm share in 1991 is main workers less cultivators, agricultural laborers, and household-industry workers, because the 1991 abstracts do not report the "other workers" category on the same basis as the later rounds; the four shares sum on average to 0.913 in 1991 and to exactly 1.000 in 2001 and 2011 (Section 3.2). Land-use shares from the Village

Table A1: The 1991–2001 placebo by distance band, twelve outcomes

1991–2001 change in	0–10 km	10–20 km	20–30 km	30–40 km	40–50 km
Agricultural-labor share	0.24 (0.61)	0.33 (0.64)	1.51** (0.64)	2.42*** (0.73)	2.38** (1.10)
Cultivator share	−0.48 (0.76)	1.18 (1.09)	−1.56 (2.22)	−1.89 (1.86)	−3.47* (2.02)
Non-farm share (residual)	−0.02 (0.55)	−1.46 (0.99)	0.56 (2.16)	−0.79 (1.82)	0.91 (2.42)
Household-industry share	0.26* (0.16)	0.05 (0.17)	−0.05 (0.28)	0.37* (0.20)	0.17 (0.13)
Worker participation rate	−0.36 (0.31)	−0.84 (0.63)	−2.52* (1.28)	−4.04*** (1.05)	−3.77*** (1.29)
Marginal-worker share	−0.44 (0.40)	0.61 (0.68)	−0.41 (1.41)	−3.21*** (1.06)	−1.33 (1.36)
Scheduled Tribe share	0.24 (0.27)	−0.05 (0.30)	−0.10 (0.30)	−0.18 (0.37)	0.01 (1.05)
Scheduled Caste share	−0.02 (0.16)	−0.21 (0.14)	−0.26 (0.23)	−0.43** (0.20)	0.63*** (0.22)
Literacy rate	−0.26 (0.36)	−0.47 (0.47)	−1.48*** (0.50)	−1.84** (0.79)	−2.59*** (0.74)
Log population (× 100)	−0.31 (0.79)	0.27 (1.01)	1.80 (1.35)	1.46 (2.02)	5.14* (2.91)
Household size (persons)	0.056*** (0.020)	0.080*** (0.029)	0.139*** (0.038)	0.121** (0.059)	0.027 (0.084)
Failures at 5 percent, of 12	1	1	3	6	4
Each group against villages more than 50 kilometers from any dam, agricultural-labor share					
Treated villages vs. far villages	−5.13*** (1.00)	−1.60* (0.90)	2.26* (1.15)	2.52*** (0.67)	2.03** (0.85)
Pipeline villages vs. far villages	−6.14*** (1.01)	−3.03*** (0.92)	−2.09** (0.87)	−0.62 (0.73)	−1.47* (0.79)
Each group against far villages, 2001–2011 change in the agricultural-labor share					
Treated villages vs. far villages	3.27*** (0.60)	1.09 (0.67)	−1.75** (0.83)	−1.16* (0.70)	−1.98*** (0.61)
Pipeline villages vs. far villages	2.31* (1.24)	−1.03 (0.88)	−3.24*** (0.59)	−2.75*** (0.61)	−2.87*** (0.53)

Notes: Estimator (1) applied to the 1991–2001 change, treated villages against pipeline villages of the same band with the balancing repeated within the band. Percentage points except log population (100 times the log change) and household size (persons). The failure row counts the outcomes whose placebo differs from zero at the 5 percent level. The last four rows compare treated villages, and control (pipeline) villages, of the band with villages more than 50 kilometers from any registered dam, over 1991–2001 and over 2001–2011. Standard errors clustered by district in parentheses; significance at the 10, 5, and 1 percent levels is marked *, **, and ***.

Directories are computed as shares of total village area; a share above 1.05 is set to missing and shares between 1 and 1.05 are capped at 1. In 2001 canal irrigation is the sum of government and private canals; in 2011 the Directory reports a single canal category.

A.3 Additional tables

Table A2: Callaway and Sant'Anna group-time estimates, all series

Outcome	1991→2001 placebo, 2002–11 cohort	2001→2011 effect, 2002–11 cohort	1991→2001, 1992–2001 cohort	Observations
Agricultural-labor share	0.66 (0.62)	1.62*** (0.63)	−0.33 (0.58)	286,400
Cultivator share	0.79 (1.07)	1.27* (0.76)	0.85 (0.80)	286,401
Non-farm share (residual)	−0.67 (0.81)	−2.61*** (0.87)	0.10 (0.57)	277,887
Non-farm share (recorded)	−0.58 (1.09)	−2.61*** (0.87)	1.06 (0.94)	286,146
Household-industry share	−0.25 (0.16)	−0.27 (0.20)	−0.13 (0.17)	277,887
Worker participation rate	0.26 (0.47)	0.18 (0.47)	0.12 (0.36)	285,230
Marginal-worker share	0.09 (0.51)	−1.01* (0.61)	−0.30 (0.41)	285,230
Landless ratio	0.21 (0.70)	0.47 (0.61)	−0.40 (0.65)	280,744
Scheduled Tribe share	0.18 (0.22)	0.14 (0.28)	−0.03 (0.13)	288,101
Scheduled Caste share	−0.24* (0.14)	0.31* (0.18)	0.08 (0.09)	288,101
Literacy rate	1.12** (0.53)	−1.70*** (0.44)	0.30 (0.47)	288,101
Log population (× 100)	−1.11 (0.97)	0.08 (1.37)	1.06 (0.82)	288,101
Household size (persons)	0.019 (0.031)	−0.079** (0.040)	−0.033 (0.040)	288,101
Agricultural-labor share, women	1.47 (1.05)	2.71*** (0.72)	−1.58* (0.87)	264,012
Agricultural-labor share, men	0.64 (0.51)	1.39** (0.60)	0.09 (0.54)	286,064
Cultivator share, women	−0.60 (1.11)	1.20 (0.99)	1.79* (0.96)	264,014
Cultivator share, men	0.94 (1.05)	1.35* (0.73)	0.61 (0.78)	286,065
Non-farm share, women	−0.39 (0.79)	−3.71*** (1.16)	1.65* (0.85)	263,831
Non-farm share, men	−0.50 (1.32)	−2.53*** (0.80)	0.52 (1.03)	285,813

Notes: Group-time estimates with not-yet-treated villages as controls, no covariates, and standard errors clustered by district, on the three-round panel of all villages within 20 kilometers of a registered dam. The first column is the 1991–2001 cell for the 2002–2011 cohort (the placebo of Section 4.4); the second is its 2001–2011 cell (the effect); the third is the 1991–2001 cell for the 1992–2001 cohort. Percentage points except log population and household size; the two non-farm rows use the 1991 residual definition and the recorded category respectively. Significance at the 10, 5, and 1 percent levels is marked *, **, and ***.

Table A3: Pairwise comparisons among the three control groups, 20-kilometer ring

Difference in 2001–2011 change (or 2012 level)	Undated − old dams	Undated − construction sites	Construction sites − old dams
Agricultural-labor share (pp)	−0.67* (0.38)	0.14 (0.98)	−2.77*** (0.63)
Non-farm share (pp)	−0.14 (0.42)	−3.29*** (1.01)	4.01*** (0.96)
Worker participation rate (pp)	0.03 (0.34)	−0.34 (0.61)	1.41*** (0.53)
Cultivator share (pp)	0.44 (0.41)	2.59** (1.09)	−0.58 (1.06)
Household-industry share (pp)	0.37*** (0.14)	0.55* (0.33)	−0.66*** (0.21)
Main workers (%)	−2.6 (1.7)	2.6 (3.7)	−9.3** (3.6)
Agricultural laborers (%)	−6.1** (2.5)	1.3 (5.7)	−17.8*** (3.7)
Non-farm workers (%)	−1.7 (1.7)	−6.2* (3.4)	2.0 (3.1)
Population (%)	−2.0*** (0.5)	−4.0** (1.6)	−1.5 (1.2)
Log consumption per capita, 2012 (%)	−0.91 (0.74)	−1.90 (1.62)	0.37 (1.52)
Poverty rate, 2012 (pp)	0.36 (0.55)	0.43 (0.89)	−0.28 (0.94)
1991–2001: agricultural-labor share (pp)	0.89** (0.38)	0.81 (0.67)	3.22*** (0.69)
1991–2001: non-farm share (pp)	−0.30 (0.41)	2.29*** (0.86)	−3.88*** (0.81)
1991–2001: participation (pp)	0.18 (0.28)	0.56 (0.57)	−1.28** (0.62)
1991–2001: cultivator share (pp)	−0.82 (0.56)	−2.91*** (1.09)	0.19 (0.93)

Notes: Each column compares two of the control groups of Section 4.3 after entropy balancing of the second-named group to the first on the nine 2001 characteristics, with state fixed effects and standard errors clustered by district (in parentheses). Rows are 2001–2011 changes unless stated; consumption and poverty are 2012 levels; the last four rows are 1991–2001 changes. Percentage points except headcounts (100 times the log change). Significance at the 10, 5, and 1 percent levels is marked *, **, and ***.

Table A4: The distance profile by tercile of reservoir storage

Distance band (km)	Agricultural-labor share	Non-farm share	Canal-irrigated share	Log consumption (%)	1991–2001 placebo	Treated / pipeline villages
Large reservoirs (storage ≥ 7.93 million)						
0–10	1.73** (0.72)	−3.12*** (0.67)	−0.28 (0.53)	2.02 (1.59)	−0.80 (0.72)	4,997 / 9,899
10–20	1.55* (0.79)	−1.21 (1.22)	0.17 (0.82)	0.92 (2.08)	1.34 (0.84)	4,599 / 13,174
20–30	0.95 (1.03)	−0.49 (1.23)	−0.35 (0.91)	−2.02 (1.84)	1.36* (0.78)	2,897 / 11,479
30–40	1.49* (0.85)	0.60 (1.68)	0.34 (1.47)	−1.72 (2.51)	0.19 (0.87)	1,922 / 8,138
40–50	1.92 (1.52)	−1.92 (2.23)	3.45 (2.70)	−1.63 (3.80)	1.81 (1.66)	892 / 5,802
Medium reservoirs						
0–10	−0.52 (0.85)	−0.11 (1.23)	0.12 (0.26)	−1.71 (1.50)	3.29*** (1.08)	4,069 / 5,155
10–20	−1.42 (1.56)	2.93 (1.89)	−0.11 (0.53)	−2.74 (2.44)	2.43* (1.41)	2,353 / 3,072
20–30	−3.06 (2.23)	5.44* (3.01)	7.45*** (2.32)	−8.50** (3.73)	1.43 (0.93)	1,240 / 1,225
30–40	−8.31*** (2.69)	−0.06 (4.32)	−7.95** (3.08)	−4.30* (2.15)	1.75 (1.78)	769 / 799
40–50	−5.99*** (0.43)	−4.48 (10.28)	−0.90 (2.62)	−18.27** (7.52)	−1.36 (3.26)	750 / 728
Small reservoirs (≤ 2.07 million)						
0–10	0.18 (0.98)	−1.02 (0.66)	−0.86** (0.41)	−1.80 (1.65)	−0.05 (1.20)	3,470 / 5,503
10–20	9.87*** (1.80)	−3.95*** (1.17)	−0.11 (0.47)	−0.66 (2.97)	−8.82** (3.64)	1,400 / 5,334
20–30	12.49*** (2.59)	−11.00** (4.33)	3.86* (1.97)	4.56 (5.00)	−13.15** (6.25)	348 / 4,692
30–40	4.04 (4.89)	−14.31*** (4.04)	2.17 (5.84)	−8.01 (6.58)	−11.30*** (3.35)	61 / 3,651
40–50	—	—	—	—	—	16 / 3,391

Notes: Terciles of the storage of the nearest dam as recorded in the register, cut over the treated villages of the profile sample. Within each tercile and band, treated villages are compared with pipeline villages of the band with the balancing repeated within the cell. Percentage points except consumption (100 times the log difference). Cells with fewer than 50 treated villages, or in which the balancing weights do not converge, are not estimated. Standard errors clustered by district in parentheses; significance at the 10, 5, and 1 percent levels is marked *, **, and ***.

Table A5: Headcounts: log change and inverse hyperbolic sine change

Headcount	Log change (zeros drop)	Villages	Asinh change (zeros kept)	Zero count in 2001, treated / control (%)
Total workers	3.6*** (1.1)	65,439	3.7*** (1.1)	0.0 / 0.0
Main workers	6.3*** (1.8)	64,920	6.1*** (2.0)	0.0 / 0.0
Marginal workers	−2.7 (3.7)	57,467	−5.2 (5.0)	7.1 / 7.8
Non-workers	−0.7 (1.4)	65,288	−0.7 (1.4)	0.2 / 0.1
Agricultural laborers	10.9*** (3.2)	47,900	13.9*** (3.7)	19.4 / 19.8
Cultivators	6.5** (2.8)	60,021	7.8** (3.8)	2.5 / 3.0
Household-industry workers	−4.9 (3.2)	28,855	−8.4*** (2.7)	39.9 / 39.5
Non-farm workers	−2.6 (2.1)	57,851	−2.5 (2.3)	7.6 / 8.4
Population	1.5* (0.8)	65,465	1.5* (0.8)	0.0 / 0.0
Households	2.2*** (0.8)	65,465	2.2*** (0.8)	0.0 / 0.0
Agricultural laborers, men	9.4*** (2.8)	46,055	12.9*** (3.3)	21.6 / 21.8
Agricultural laborers, women	9.6*** (3.4)	39,200	12.7*** (3.4)	29.1 / 32.1
Non-farm workers, men	−3.0 (2.0)	56,474	−2.8 (2.2)	8.9 / 9.5
Non-farm workers, women	0.6 (2.4)	44,604	−0.8 (2.8)	23.7 / 28.5
Main workers, men	5.0*** (1.5)	64,740	5.0*** (1.8)	0.1 / 0.1
Main workers, women	8.7*** (3.2)	60,147	9.7*** (3.6)	4.0 / 5.8

Notes: The log-change regressions omit villages with no workers in the category in either year; the inverse hyperbolic sine regressions retain them. Both are from equation (1) in the 20-kilometer ring; estimates are 100 times the coefficient. The last column gives the share of treated and control villages with a zero count in 2001. Standard errors clustered by district in parentheses; significance at the 10, 5, and 1 percent levels is marked *, **, and ***.

Table A6: Timing of completion and the 2001 characteristics of treated villages

2001 characteristic	Slope per year of completion	Mean, completed 2002–06	Mean, completed 2007–11	Late − early, within state
Scheduled Tribe share (pp)	0.17 (0.67)	28.59	27.02	1.54 (3.19)
Scheduled Caste share (pp)	0.07 (0.21)	12.63	12.61	0.44 (1.18)
Literacy rate (pp)	0.46* (0.24)	45.81	50.00	0.50 (1.17)
Cultivator share (pp)	0.00 (0.32)	55.01	54.61	−0.25 (1.98)
Agricultural-labor share (pp)	0.04 (0.22)	23.57	20.75	0.43 (1.36)
Non-farm share (pp)	0.01 (0.23)	18.82	22.17	0.16 (1.31)
Worker participation rate (pp)	−0.03 (0.10)	50.34	50.47	0.33 (0.49)
Log population (× 100)	−1.4 (1.5)	639.6	639.5	−9.1 (8.0)
Household size (persons)	−0.012 (0.009)	5.236	5.184	−0.082 (0.050)

Notes: Treated villages only. The first column regresses each 2001 characteristic on the year of completion of the nearest dam, within state, with standard errors clustered by district; the last column is the difference between villages beside dams completed in 2007–2011 and in 2002–2006, within state. Percentage points except log population and household size. Significance at the 10, 5, and 1 percent levels is marked *, **, and ***.

Table A7: Land use from the Village Directory, ring and by direction cell

Share of village area (pp)	20-km ring	Below the dam	Reservoir rim	Above the reservoir	Different basin
Forest, change 2001–2011	0.03 (0.36)	0.47 (0.32)	−0.02 (0.30)	−0.43 (0.44)	0.31 (0.66)
Not available for cultivation, change	0.78 (0.58)	1.04 (0.66)	2.18*** (0.57)	0.39 (0.83)	0.19 (0.70)
Culturable waste, change	1.44* (0.77)	1.42** (0.65)	0.95* (0.58)	2.12** (1.08)	2.88** (1.13)
Pasture and grazing, 2011 level	−0.10 (0.40)	−0.42 (0.34)	−0.18 (0.35)	−0.09 (0.68)	−0.80* (0.47)
Net sown area, 2011 level	1.50 (1.48)	4.43*** (1.69)	3.70** (1.50)	2.30 (1.84)	1.55 (2.23)
Culturable waste, 2011 level	0.61 (0.62)	−0.72 (0.52)	−0.06 (0.46)	0.23 (0.87)	1.53 (0.94)
Forest, 2011 level	−1.21 (1.13)	−2.69** (1.33)	−2.15** (0.90)	0.97 (1.48)	−1.49 (1.94)
Cultivated, 2011 level	0.19 (1.34)	3.30** (1.58)	2.06 (1.34)	0.48 (1.66)	1.47 (2.10)
Current fallow, 2011 level	−0.80* (0.41)	−0.34 (0.52)	−1.44*** (0.39)	−1.73*** (0.49)	−0.41 (0.53)

Notes: Shares of village area in percentage points. Rows labeled "change" are 2001–2011 first differences from equation (1); rows labeled "2011 level" are 2011 levels conditional on the nine 2001 characteristics, because the 2001 Directory does not report the category. The ring column uses the main weights; the cell columns reweight the control group to each cell. Standard errors clustered by district in parentheses; significance at the 10, 5, and 1 percent levels is marked *, **, and ***.

Table A8: Canal irrigation by distance band and direction, and village amenities

	Treated 2001	Treated 2011	Control 2001	Control 2011	Estimate (SE)
Panel A. Canal-irrigated share of village area by distance band (treated vs. control villages of the band; percentage points)					
0–10 km: all treated					−0.26 (0.25)
0–10 km: below the dam / above the reservoir					0.11 (0.43) / −0.41 (0.28)
10–20 km: all treated					0.22 (0.54)
10–20 km: below the dam / above the reservoir					1.92** (0.76) / 0.44 (0.62)
20–30 km: all treated					1.22 (1.47)
20–30 km: below the dam / above the reservoir					0.99 (1.07) / 2.80* (1.60)
30–40 km: all treated					0.39 (1.17)
30–40 km: below the dam / above the reservoir					0.05 (1.03) / −0.55 (1.68)
40–50 km: all treated					−0.04 (1.22)
40–50 km: below the dam / above the reservoir					−2.34 (1.70) / 2.26 (1.38)
Panel B. Amenities, share of villages with the facility (percentage points)					
Paved approach road	55.1	66.1	53.1	67.4	1.21 (1.59)
Tap water	61.7	62.6	56.0	56.3	−1.50 (2.19)
Primary school	85.3	87.5	77.3	81.6	0.01 (0.90)
Health center	3.9	25.5	3.3	21.1	2.80 (1.82)
Bank	4.4	8.7	2.6	7.3	−0.40 (0.43)

Notes: Panel A: canal-irrigated share, treated villages of each band against control villages of the same band with the balancing repeated within the band; the below-the-dam and above-the-reservoir estimates come from the same specification with the treated villages of the band divided by the direction cells of Section 3.3. Panel B: share of villages reporting the facility, in percent, 20-kilometer ring; estimates from equation (1) in percentage points. The Directory's amenity variables are indicators for the presence of a facility in the village. Standard errors clustered by district in parentheses; significance at the 10, 5, and 1 percent levels is marked *, **, and ***.