Labor Rationing

Online Appendices

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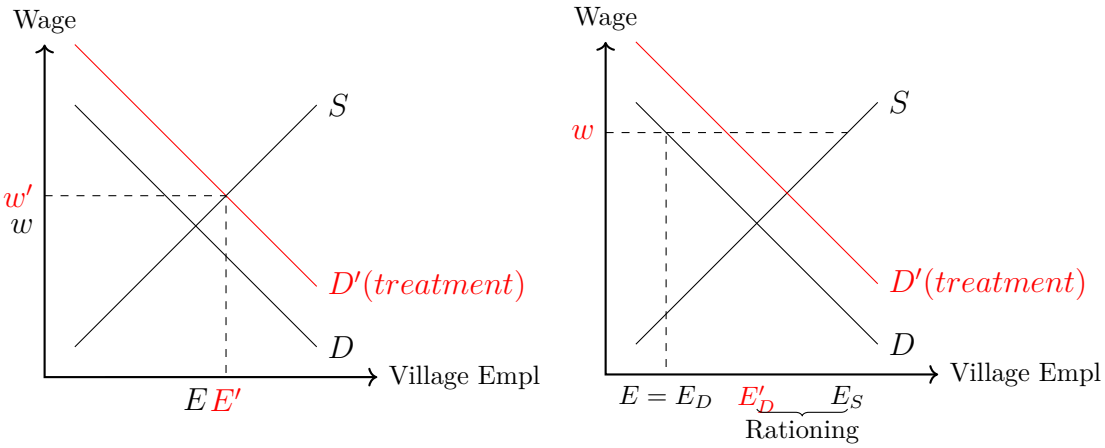
Appendix A. Appendix Figures



(A) H_0 : No Rationing ($E_D \geq E_S$)

(B) H_1 : Rationing ($E_D < E_S$)

Figure A.1. Effects of a Negative Labor Demand Shock

Note: Figure shows the effects of a negative demand shock on employment and wages under no rationing ($E_D \geq E_S$) in panel A, and under rationing ($E_D < E_S$) in panel B.

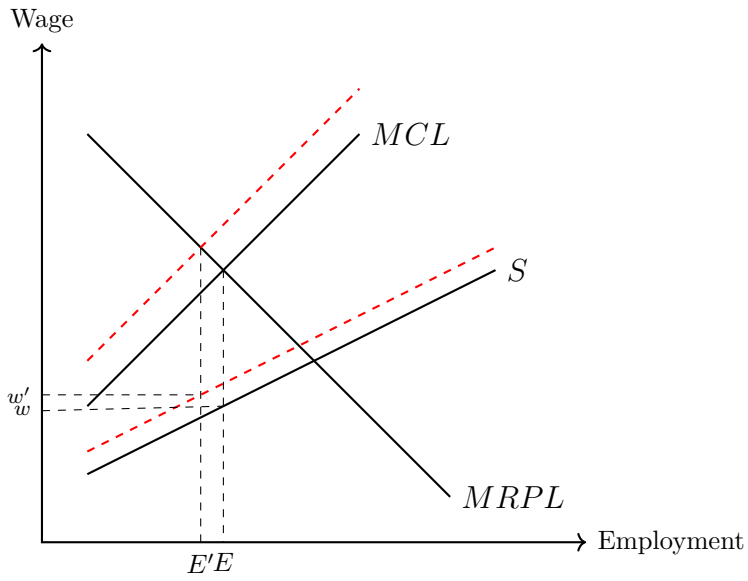


FIGURE A.2. Effects of a Negative Labor Supply Shock Under Monopsony

Note: Figure shows the effects of a negative supply shock on employment and wages under monopsony. A parallel shift in labor supply leads wages to rise and employment to fall, the same qualitative predictions as in the benchmark, market clearing model. Because the marginal cost of labor curve is steeper than the labor supply curve, the wage increases by less and employment decreases by more under monopsony, relative to market clearing.

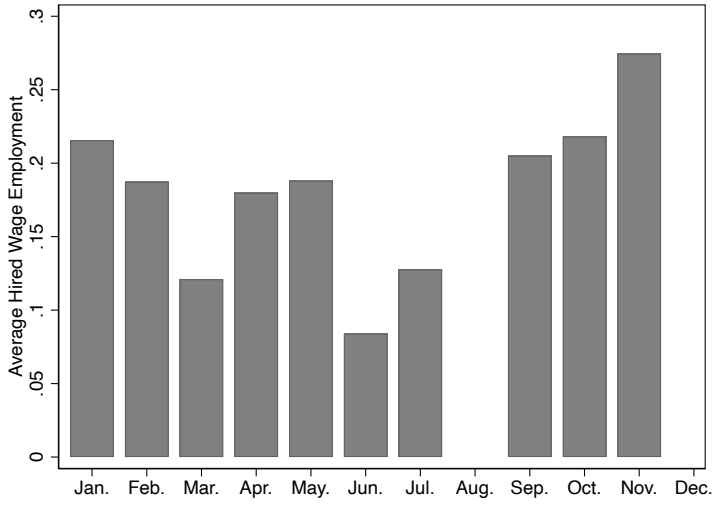


FIGURE A.3. Average Hired Wage Employment Rate by Month

Note: Figure plots average wage employment rates in control villages by calendar month. The median employment rate is 0.19.

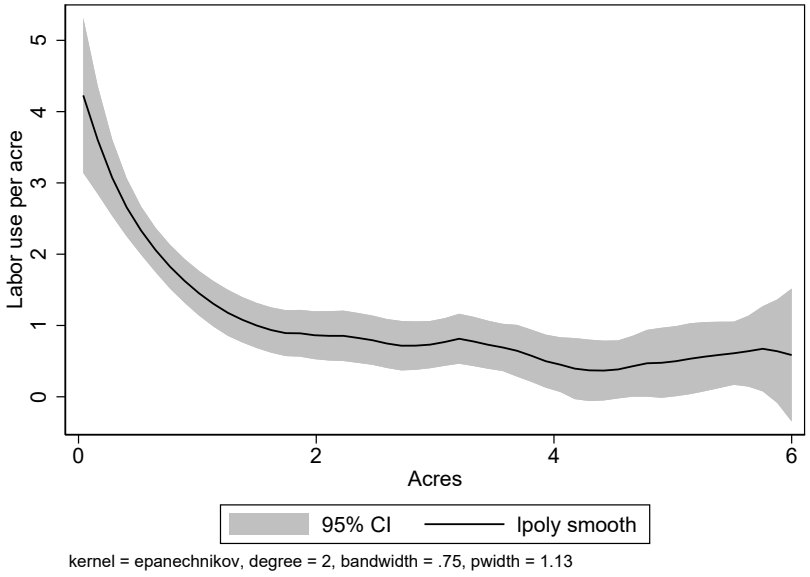


FIGURE A.4. Inverse Farm-Size Relationship

Note: Figure plots intensity of labor use against farm size. The y-axis measures total labor inputs per acre on the farm in the past week (own family labor + hired labor).

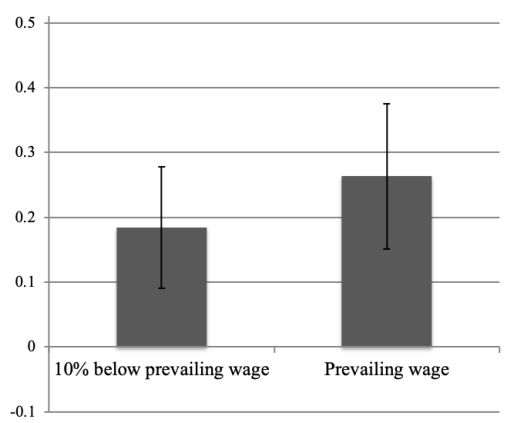
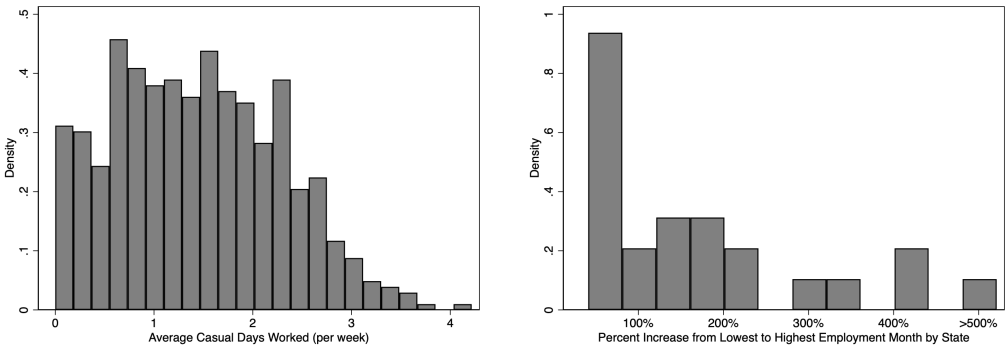


FIGURE A.5. Job take-up

Note: Figure illustrates take up of a job offer made in private at different wage rates among casual workers in villages similar to our study sample, in the same districts of Odisha, India. This data comes from a labor supply estimation exercise conducted by Breza et al. (2019).



(A) Average Casual Employment Across Districts

(B) Percent Increase in Casual Employment from Lowest to Highest Month Across States

FIGURE A.6. India-wide Variation in Employment by District and Month

Note: Data is from from the rural sample of Round 61 of the National Sample Survey, pre-NREGA roll-out. Panel A shows the distribution of weekly average casual employment across districts in India. Panel B shows the distribution of the percent difference between the months with the highest and lowest rate of casual employment across states. For both panels, the sample is restricted to individuals whose primary or secondary occupation is casual labor or agricultural self employment.

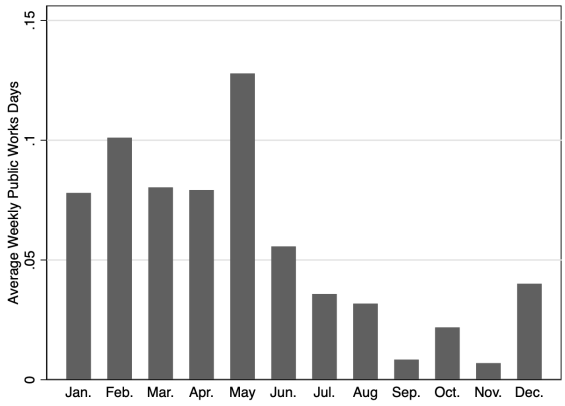


FIGURE A.7. Average Weekly Days in Public Works by Month

Note: Figure plots weekly average public works employment in each month using data from Round 64 of the National Sample Survey (post-NREGA roll-out). The sample is restricted to districts that received the NREGA program by Round 64.

Appendix B. Appendix Tables

Table B.1. Baseline Characteristics: Sign-Ups and Non Sign-Ups

	Lean			Semi-Peak		
	(1) Sign-Ups	(2) Difference	(3) Pval	(4) Sign-Ups	(5) Difference	(6) Pval
Ever participate in casual labor market	0.939	-0.117	0.129	0.946	-0.186	0.00809
	(0.239)	(0.072)		(0.226)	(0.062)	
Days would like to work in labor market (in next 30 days)	16.76	-4.803	0.0205	19.10	-8.461	0.000000399
	(8.900)	(1.776)		(8.068)	(1.031)	
Total wage (Rs.)	254.6	31.27	0.0135	251.2	4.425	0.550
	(67.723)	(10.202)		(56.468)	(7.241)	
Weekly wage earnings	675.4	23.96	0.804	637.7	-20.73	0.787
	(514.302)	(94.285)		(545.994)	(75.464)	
Any activity	0.403	0.111	0.249	0.387	0.0215	0.414
	(0.370)	(0.091)		(0.356)	(0.026)	
Employment rate: Hired wage employment	0.179	-0.0564	0.395	0.176	-0.0530	0.0489
	(0.282)	(0.064)		(0.288)	(0.025)	
Public works employment	0.0124	0	1	0.000366	-0.000347	0.340
	(0.093)	(0.000)		(0.007)	(0.000)	
Has HH business	0.851	0.00460	0.939	0.876	0.0347	0.162
	(0.357)	(0.059)		(0.330)	(0.024)	
Self employment	0.160	0.0597	0.176	0.0852	0.0416	0.000631
	(0.276)	(0.041)		(0.211)	(0.010)	
Landless	0.498	-0.110	0.0130	0.290	-0.0288	0.365
	(0.501)	(0.037)		(0.454)	(0.031)	
HH members (age 12+)	3.988	-0.143	0.486	3.952	-0.175	0.189
	(1.696)	(0.198)		(1.422)	(0.128)	

Notes: Cols. (1) and (4) present baseline means and standard deviations of worker-level characteristics in control villages for workers who sign up for external jobs in lean and semi-peak months respectively. Cols. (2) and (5) report coefficients from regressing the covariate in each row on a dummy for non sign-ups in lean and semi-peak months respectively, with round (strata) fixed effects and standard errors clustered at the village level. Cols. (3) and (6) report corresponding p-values for the regression coefficients presented in Cols. (2) and (5) respectively.

TABLE B.2. Survey Completion in Spillover Sample

	(1) Baseline Survey Completion	(2) Endline Survey Completion
Hiring shock	0.0160	0.0125
	(0.019)	(0.014)
Sample	Spillover	Spillover
Dep Var Mean	0.934	0.896
N (workers)	1094	1108

Notes: Observations are from the spillover sample (workers who signed up for external jobs but were not offered employment). The dependent variable is a binary indicator for whether the survey respondent completed the survey. Col. (1) examines baseline survey responses and Col. (2) examines endline survey responses. Regressions include round (strata) FEs. The regression in Col. (2) includes a control for baseline survey completion. Standard errors are clustered at the village level in parentheses.

TABLE B.3. Sample Sizes & Survey Completion Rates

	Lean		Semi-Peak		Hiring shock
	(1)	(2)	(3)	(4)	(5)
	No hiring shock	Difference	No hiring shock	Difference	Lean vs Semi-Peak
Total Sign-Ups	28.15	1.538	33.53	3.353	1.814
	(13.53)	(2.675)	(12.08)	(3.923)	(4.749)
EL Survey Completion: Spillover	0.877	0.0260	0.901	0.00409	-0.0219
	(0.329)	(0.0254)	(0.298)	(0.0157)	(0.0297)
Total Non Sign-Ups	15	-1.500	15.43	-0.0714	1.429
	(4.721)	(0.512)	(7.703)	(0.898)	(1.034)
EL Survey Completion: Non Sign-Ups	0.770	0.0336	0.898	-0.0304	-0.0640
	(0.0877)	(0.0548)	(0.102)	(0.0415)	(0.0687)
Share of Non Sign-Ups Surveyed	0.177	-0.0219	0.169	-0.0133	0.00862
	(0.0451)	(0.0116)	(0.0418)	(0.0133)	(0.0177)

Notes: Cols. (1) and (3) present means and standard deviations of village-level outcomes in control villages in lean and semi-peak months respectively. Col. (2) reports differences in outcomes by treatment status in lean months, Col. (4) reports differences in outcomes by treatment status in semi-peak months, and Col. (5) reports differences in outcomes across lean and semi-peak months, for treated villages. These differences are coefficients from a regression of the covariates in each row on dummies for treatment and treatment interacted with semi-peak, with round (strata) fixed effects and robust standard errors. Total sign-ups = number of workers who sign up for the external job. EL survey completion: spillover = 1{spillover worker completed the endline survey}. Total non sign-ups = number of non sign-ups selected for the endline survey. EL survey completion: non sign-ups = 1{non sign-up worker completed the endline survey}. Share of non sign-ups surveyed = share of total non sign-ups selected for the endline survey.

TABLE B.4. Employer Characteristics

	Lean			Semi-Peak		
	(1)	(2)	(3)	(4)	(5)	(6)
	No hiring shock	Difference	Pval	No hiring shock	Difference	Pval
Landholdings (total)	2.634	0.308	0.0947	2.631	-0.0506	0.645
	(1.696)	(0.177)		(1.796)	(0.109)	
HH members (age 12+)	4.199	0.178	0.167	4.254	0.265	0.0258
	(1.508)	(0.124)		(1.771)	(0.114)	
Fraction of randomly selected workers given a rating	0.725	-0.0151	0.702	0.791	0.0366	0.209
	(0.250)	(0.039)		(0.229)	(0.029)	
Fraction of randomly selected workers ever hired	0.257	0.00727	0.817	0.216	0.0116	0.473
	(0.221)	(0.031)		(0.201)	(0.016)	
Ever participate in casual labor market	0.569	-0.0488	0.266	0.495	-0.000666	0.985
	(0.496)	(0.043)		(0.501)	(0.035)	
Occupation: Self-emp (agri)	0.611	-0.0208	0.123	0.620	-0.0176	0.00702
	(0.489)	(0.013)		(0.486)	(0.006)	
Occupation: Self-emp (non-agri)	0.0616	-0.0191	0.316	0.0693	-0.00483	0.718
	(0.241)	(0.019)		(0.254)	(0.013)	
Occupation: Laborer	0.303	-0.0556	0.192	0.274	-0.0197	0.463
	(0.461)	(0.041)		(0.447)	(0.026)	

Notes: Observations are from the employer survey. Cols. (1) and (4) present means and standard deviations of employer-level characteristics in control villages in lean and semi-peak months respectively. Cols. (2) and (5) report coefficients from regressing the covariate in each row on a dummy for treatment in lean and semi-peak months respectively, with round (strata) fixed effects and standard errors clustered at the village level. Cols. (3) and (6) report corresponding p-values for the regression coefficients presented in Cols. (2) and (5) respectively.

TABLE B.5. Wage Effects using Non-Winsorized Wages

	(1) Log cash wage	(2) Log total wage	(3) Log total wage	(4) Total wage	(5) Log total wage	(6) Total wage
Hiring shock	-0.0202	-0.0113	-0.0183	-5.632	-0.0620	-19.24
	(0.021)	(0.022)	(0.019)	(3.925)	(0.050)	(11.425)
Hiring shock * Semi-peak	0.0740	0.0676	0.0684	18.57		
	(0.031)	(0.032)	(0.029)	(7.595)		
Hiring Shock * Empl. Level					0.457	133.3
					(0.240)	(57.182)
Sample	Spillover	Spillover	Spillover	Spillover	Spillover	Spillover
Baseline controls	No	No	Yes	Yes	Yes	Yes
Pval: Shock + Shock*Semi-peak	0.0232	0.0227	0.0256	0.0472	.	.
SE: Shock + Shock*Semi-peak	0.0231	0.0241	0.0219	6.379	.	.
Control mean: lean	5.458	5.500	5.500	253.8	5.500	253.8
Control mean: semi-peak	5.428	5.504	5.504	251.6	5.504	251.6
N (worker-days)	1543	1544	1544	1545	1544	1545

Notes: Observations are from the spillover sample (workers who signed up for external jobs but were not offered employment). Total wage = cash + in-kind wages. Controls include worker-level mean employment and wage levels at baseline. Regressions include round (strata) FEs. Standard errors are clustered at the village level in parentheses.

TABLE B.6. Treatment Effects Restricting to 7-day Recall Window

	(1) Hired wage empl.	(2) Log total wage	(3) Self empl.	(4) Hired wage empl.	(5) Log total wage	(6) Self empl.
Hiring shock	0.0518	-0.00613	-0.0534	0.00530	0.00616	-0.00282
	(0.023)	(0.015)	(0.018)	(0.031)	(0.036)	(0.017)
Hiring shock * Semi-peak	-0.0855	0.0450	0.0260	-0.0658	0.0247	-0.0236
	(0.032)	(0.028)	(0.025)	(0.038)	(0.044)	(0.025)
Sample	Spillover	Spillover	Spillover	Spillover	Spillover	Spillover
Baseline controls	Yes	Yes	Yes	Yes	Yes	Yes
Pval: Shock + Shock*Semi-peak	0.140	0.0948	0.150	0.00764	0.230	0.159
SE: Shock + Shock*Semi-peak	0.0226	0.0229	0.0188	0.0218	0.0254	0.0185
Control mean: lean	0.156	5.526	0.171	0.188	5.543	0.174
Control mean: semi-peak	0.232	5.502	0.107	0.222	5.529	0.130
N (worker-days)	6944	1274	6545	6196	1131	6196

Notes: Observations are from the spillover sample (workers who signed up for external jobs but were not offered employment). Hired wage employment = 1{worker hired that day and paid a wage}. Total wage = cash + in-kind wages. Self employment = 1{worker self-employed that day}. Cols. (4) to (6) examine impacts two weeks after the end of the hiring shock. Regressions include round (strata) FEs. Standard errors are clustered at the village level in parentheses.

TABLE B.7. Self-Employment Hours

	(1) Self emp	(2) Self emp	(3) Self: non-agri	(4) Self: agri	(5) Self: agri	(6) Self emp	(7) Self emp
Hiring shock	-0.164	-0.158	-0.122	-0.145	-0.322	0.0498	-0.000103
	(0.091)	(0.105)	(0.098)	(0.117)	(0.146)	(0.112)	(0.100)
Hiring shock * Above Median Land Per Capita					0.313		
					(0.227)		
Hiring shock * Semi-peak	0.0237	-0.0169	-0.109	0.119	0.486	-0.277	-0.202
	(0.127)	(0.131)	(0.145)	(0.126)	(0.177)	(0.155)	(0.138)
Hiring shock * Semi-peak * Above Median Land Per Capita					-0.692		
					(0.292)		
Sample	Spillover	Spillover	Spillover	Spillover	Spillover	Spillover	Spillover
Baseline controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Recall window (days)	5	7	7	7	7	5	7
Pval: Shock + Shock*Semi-peak	0.143	0.0449	0.0438	0.595	0.120	0.0403	0.0407
SE: Shock + Shock*Semi-peak	0.0946	0.0853	0.110	0.0490	0.104	0.108	0.0961
Control mean: lean	0.723	0.711	0.243	0.744	0.744	0.711	0.723
Control mean: semi-peak	0.522	0.506	0.281	0.349	0.349	0.841	0.787
N	4674	6544	3899	5837	5837	4251	5951
Level of observations	worker-days	worker-days	worker-days	worker-days	worker-days	worker-days	worker-days

Notes: Observations are from the spillover sample (workers who signed up for external jobs but were not offered employment). Self employment = hours worked in self-employment that day. Self employment: non-agri = hours worked in non-agricultural self-employment that day. Self employment: agri = hours worked in agricultural self-employment that day. We restrict the sample to experimental rounds with non-zero self-employment in control villages at endline in Cols. (1), (2), (6) and (7), non-zero non-agricultural self-employment in control villages at endline in Col. (3), and non-zero agricultural self-employment in control villages at endline in Cols. (4)-(5). Cols. (6) and (7) examine impacts two weeks after the end of the hiring shock. Controls include worker-level indicators for any self-employment activities at baseline. Regressions include round (strata) FE. Standard errors clustered at the village level in parentheses.

TABLE B.8. Self-Employment: Household Level

	(1) HH self empl	(2) HH self empl	(3) HH self emp
Hiring shock	-1.386	-1.390	-1.341
	(0.482)	(0.475)	(0.482)
Hiring shock * Semi-peak	1.328	1.265	1.214
	(0.583)	(0.583)	(0.590)
Sample	Spillover	Spillover	Spillover
Baseline controls	Yes	Yes	Yes
Pval: Shock + Shock*Semi-peak	0.867	0.724	0.722
SE: Shock + Shock*Semi-peak	0.344	0.353	0.356
Control mean: lean	4.048	4.048	4.048
Control mean: semi-peak	1.663	1.663	1.663
N	816	816	816
Level of observations	households	households	households

Notes: Observations are from the spillover sample (workers who signed up for external jobs but were not offered employment). Household self employment = total number of worker-days in self-employment across all household members in the past week. We restrict the sample to experimental rounds with non-zero self-employment in control villages at endline. Controls include worker-level indicators for any self-employment activities at baseline. Cols. (2) and (3) also include controls for the number of household members above the age of 12, and Col (3) further controls for landholdings at baseline. Regressions include round (strata) FE. Standard errors clustered at the village level in parentheses.

TABLE B.9. Impacts on Employers

Panel A: No Controls	(1) Act Done As Many Times As I Like	(2) Act Done As Early As I Like	(3) Hire From Outside Vill	(4) Trouble Hiring (Days)	(5) Worker Rating
Hiring shock	0.0316	0.00824	0.00789	-0.0386	-0.0105
	(0.013)	(0.023)	(0.027)	(0.070)	(0.082)
Hiring shock * Semi-peak	-0.0497	-0.0548	0.0568	0.0444	0.173
	(0.017)	(0.039)	(0.031)	(0.083)	(0.115)
Pval: Shock + Shock*Semi-peak	0.101	0.145	0.000142	0.898	0.0472
SE: Shock + Shock*Semi-peak	0.0108	0.0314	0.0159	0.0446	0.0801
Control mean: lean	0.972	0.972	0.0758	0.190	3.639
Control mean: semi-peak	0.987	0.962	0.0363	0.239	3.266
N	678	678	1029	1029	1264
Level of observations	emp-activity	emp-activity	employer	employer	worker-day
Panel B: With Controls					
Hiring shock	0.0247	-0.0131	0.00203	-0.0610	
	(0.012)	(0.036)	(0.027)	(0.070)	
Hiring shock * Semi-peak	-0.0487	-0.0427	0.0635	0.0854	
	(0.017)	(0.048)	(0.031)	(0.084)	
Pval: Shock + Shock*Semi-peak	0.0599	0.0819	0.000128	0.606	
SE: Shock + Shock*Semi-peak	0.0124	0.0313	0.0159	0.0469	
Control mean: lean	0.972	0.972	0.0758	0.190	
Control mean: semi-peak	0.987	0.962	0.0363	0.239	
N	678	678	1029	1029	
Level of observations	emp-activity	emp-activity	employer	employer	

Notes: Observations in Cols. (1)-(4) are from a sample of employers surveyed in the village. Observations in Col. (5) are from the spillover sample (workers who signed up for external jobs but were not offered employment). Activity done as many times as I like = 1{employer reports being able to complete a particular activity as frequently as he liked}. Activity done as early as I like = 1{employer reports being able to complete a particular activity as early as he liked}. Hire from outside village = 1{employer hired from outside the village for any activity}. Trouble hiring = number of days the employer reports having trouble hiring workers, in the past 10 days. Worker rating = ability rating of the worker, averaged across all surveyed employers in the village. Panel A presents regression results with no controls, while Panel B presents regression results with employer-level controls for which there is imbalance. Regressions include round (strata) FEs. Standard errors are clustered at the village level in parentheses.

TABLE B.10. Employment Spillovers: Heterogeneous Impacts by Ability

	Dependent variable: Hired wage employment		
	High ability proxy		
	(1) Worker Rating	(2) Employment Rate	(3) Wages
Hiring shock	0.0847	0.0526	0.0628
	(0.022)	(0.018)	(0.018)
Hiring shock * Semi-peak	-0.103	-0.0662	-0.0780
	(0.040)	(0.029)	(0.031)
High ability proxy	0.0246	0.0752	0.0521
	(0.018)	(0.018)	(0.013)
Hiring Shock* High ability proxy	-0.00450	-0.0441	-0.00906
	(0.030)	(0.030)	(0.025)
Semi-peak* High ability proxy	-0.0139	0.0470	0.0644
	(0.023)	(0.024)	(0.024)
Hiring shock* Semi-peak* High ability proxy	0.0309	0.0495	0.00975
	(0.035)	(0.041)	(0.039)
Sample	Spillover	Spillover	Spillover
Baseline controls	No	No	No
Pval: Shock + Shock*Semi-peak	0.586	0.540	0.548
SE: Shock + Shock*Semi-peak	0.0334	0.0221	0.0253
Control mean: lean	0.134	0.145	0.145
Control mean: semi-peak	0.228	0.216	0.216
N (worker-days)	6405	8906	8906

Notes: Observations are from the spillover sample (workers who signed up for external jobs but were not offered employment). We use three different worker-level proxies for high ability: standardized ability rating of the worker, averaged across all surveyed employers in the village in Col.(1), standardized mean employment levels at baseline in Col. (2), and standardized mean wage levels at baseline in Col. (3). Regressions include round (strata) FEs. Standard errors are clustered at the village level in parentheses.

TABLE B.11. Impacts on Male Household Members

	(1) Work in casual labor market	(2) Work in casual labor market	(3) Work on own land	(4) Work in HH business	(5) Self employment
Hiring shock	0.0188	0.0287	-0.0333	-0.00516	-0.0416
	(0.013)	(0.012)	(0.016)	(0.004)	(0.018)
Hiring shock * Semi-peak	-0.0301	-0.0487	0.0401	0.00282	0.0474
	(0.023)	(0.028)	(0.017)	(0.007)	(0.020)
Sample	Spillover	Spillover	Spillover	Spillover	Spillover
Baseline controls	Yes	Yes	Yes	Yes	Yes
Pval: Shock + Shock*Semi-peak	0.544	0.423	0.233	0.736	0.569
SE: Shock + Shock*Semi-peak	0.0184	0.0247	0.00558	0.00689	0.0100
Control mean: lean	0.119	0.146	0.0980	0.0137	0.111
Control mean: semi-peak	0.169	0.218	0.0312	0.0151	0.0442
N (workers)	1660	1296	1690	1750	1752

Notes: Observations are from male household members (above the age of 12) of workers in the spillover sample (workers who signed up for external jobs but were not offered employment). Controls in Cols. (1)-(2) include worker-level mean employment and wage levels at baseline. Controls in Cols. (3)-(5) include indicators for any self-employment activities at baseline. In Col. (2), we restrict the sample to household members who report participating in the casual labor market. Self-employment = 1{work on own land or in household business}. Regressions include round (strata) FEs. Standard errors are clustered at the village level in parentheses.

TABLE B.12. Differential Impacts of NREGA by Labor Market Slack

	(1) Public Works Days	(2) Casual Wage	(3) Casual Days Worked	(4) Casual Agri. Days	(5) Self-Empl. Days
NREGA	0.0213	-0.618	0.0678	-0.00650	-0.206
	(0.010)	(1.009)	(0.054)	(0.047)	(0.071)
NREGA x High Casual Employment	0.00906	2.594	-0.0980	-0.121	0.0923
	(0.013)	(1.052)	(0.059)	(0.061)	(0.080)
Control Mean	0.0121	72.00	1.071	0.652	4.738
Test: NREGA + NREGA x High Cas. Empl.	0.0132	0.0554	0.609	0.0308	0.152
Observations	353180	64634	353180	353180	329177

Notes: Observations are from prime-age individuals surveyed in rounds 61 and 64 of Schedule 10 of the National Sample Survey (NSS), with sampling weights. Regressions include district and month-by-year fixed effects, as well as a vector of controls including worker characteristics by round (gender, education, age), baseline agricultural yields by round, and district-by-month baseline means of casual employment, wages, and self-employment. Standard errors are clustered at the district level in parentheses.

TABLE B.13. Treatment Effects Using All Experimental Rounds

	(1) Log total wage	(2) Hired wage employment	(3) Hired wage employment	(4) Self employment	(5) Invol unempl (traditional)	(6) Invol unempl (alternate)
Hiring shock	-0.0181	0.0540	0.0136	-0.0331	-0.0339	-0.0625
	(0.019)	(0.018)	(0.020)	(0.019)	(0.027)	(0.026)
Hiring shock * Semi-peak	0.0673	-0.0681	-0.0570	-0.00737	0.0529	0.0793
	(0.028)	(0.028)	(0.024)	(0.026)	(0.037)	(0.037)
Sample	Spillover	Spillover	Full Village Sample	Spillover	Spillover	Spillover
Baseline controls	Yes	Yes	Yes	Yes	Yes	Yes
Pval: Shock + Shock*Semi-peak	0.0189	0.526	0.00154	0.0263	0.453	0.523
SE: Shock + Shock*Semi-peak	0.0204	0.0221	0.0131	0.0178	0.0252	0.0262
Control mean: lean	5.500	0.145	0.135	0.139	0.482	0.580
Control mean: semi-peak	5.502	0.209	0.190	0.112	0.413	0.553
N (worker-days)	1603	9466	22188	8941	9466	9466

Notes: This table presents regression results including two experimental rounds that were excluded from the main analysis because we were not able to complete survey work (see Appendix C). Observations in Cols. (1)-(2) and (4)-(6) are from the spillover sample (workers who signed up for external jobs but were not offered employment). Observations in Col. (3) are from all potential workers in the village with appropriate weights. Controls in Cols. (1)-(2) and (5)-(6) include worker-level mean employment and wage levels at baseline. Controls in Col. (3) include worker-level and village-level mean employment and wage levels at baseline. Controls in Col. (4) include indicators for any self-employment activities at baseline. Regressions include round (strata) FEs. Standard errors clustered at the village level in parentheses.

Appendix C. Implementation Protocols

Recruitment of study participants. Several days prior to the first day of work, the external jobs are advertised in villages through flyers, village meetings, and door-to-door visits. First, flyers containing basic information about the job opportunity (nature of work, duration, hours per day, location, daily wage etc.) are distributed to all households. Next, a meeting is held in a central location within the village. During this meeting, more information is given to interested workers. For example, they are informed that the job opportunity is one-time only and employment is not guaranteed — the number of workers hired would depend on the needs of local contractors, and job offers would be randomly assigned among those who sign up using a lottery.[1] Lastly, door-to-door visits are made to ensure that all households in the village have received information about the external job opportunity. Male workers who are interested in the job are encouraged to sign up at the village meeting or during door-to-door visits. Workers who are under the age of 18 or above the age of 65 are ineligible.

We use a computer to randomize assignment of eligible sign-ups to one of three groups: (1) workers who will be offered a job; (2) workers who might be offered a job (waitlist); and (3) workers who will never be offered a job. We draw from the group 2 roster in the event that a worker from group 1 opts out of the external job. Workers in group 3 constitute our spillover sample.

Description of external jobs. The external jobs take place in factory workshops within 15 kilometers of our study villages. Workers are employed full-time (5 days a week) for 2-4 weeks, and they engage in low-skill manufacturing of products such as ropes, brooms, disposable plates and floor mats. All output produced by workers is sold by partnering contractors, who set production standards. Workers are paid a flat daily wage for attendance, in accordance with the typical pay structure in the area. There are no strict production minimums imposed on workers, though workers can be fired for excessive absences (more than three days in a row) or disruptive behavior.

At any point in time, a total of approximately 30 workers are recruited into a worksite — this comprises of workers from both treatment and control villages within an experimental round. In experimental rounds where the number of recruited workers across both treatment and control villages is less than 30, we hire additional workers from nearby villages until we reach the capacity limit at the factory workshop.

More details on the external jobs can be found in Breza et al. (2018).

[1] We advertise the jobs as "training contracts," indicating that workers will receive one-time training to produce a low-skill manufacturing good during their employment period. This concept is familiar in this context, and is viewed as a one-time opportunity since one would not receive training more than once. See Breza et al. (2018) for further details.

Worker surveys. We conduct surveys with all eligible workers who sign up. We also conduct surveys with a random sample of potential workers in the village who do not sign up for the job. For this sample, the enumerators follow a left-hand rule — they start at the first household on the left as they enter the village, and approach every three households thereafter. The survey is conducted with a prime-age male household member who is working in any capacity — this includes work in the casual labor market, self-employment or salaried work. If a member of the household is part of the spillover sample, the household is skipped.

In the last 11 experimental rounds, we conduct a census of all households in the study villages prior to advertising the external job. For these rounds, we use the census data to identify a random sample of potential workers in the village. We generate a roster consisting of prime-age males village residents who do not sign up for the job. We do not impose any condition on work status, as we did in earlier rounds.

Timeline of worker surveys. We conduct three waves of surveys: (1) baseline surveys at the start of the hiring shock; (2) endline surveys during the final two weeks of the hiring shock; and (3) post-intervention surveys two weeks after the end of the hiring shock.[2] Surveys are conducted privately with workers at their residence. To compensate workers for their time as well as to minimize survey attrition, workers are given a small gift upon completion of the post-intervention survey.

A subset of baseline surveys were conducted immediately after the external jobs began. Recall that in each of the surveys, workers are asked to describe their employment activity separately for each day for the past 10 days. As such, part of the employment recall in those surveys included days during which the external job had already commenced. We restrict baseline data used in regressions to worker-days that occurred before the external jobs began. As we document in the paper, our wage and employment effects are similar with or without baseline controls.

Changes to worker survey instruments. In the first 9 experimental rounds, we collected recall data for the past 14 days. We shortened this to 10 days for the next 9 rounds, and further shortened this to 7 days for the remaining rounds.

As mentioned above, we began conducting the post-intervention worker survey in experimental round 3. For the first six rounds thereafter, the survey was conducted 1-2 months after the end of the hiring shock. We shortened this to two weeks for the remaining experimental rounds.

[2]We added the post-intervention survey from experimental round 3 onwards.

Employer surveys. In addition to worker surveys, we survey a subset of agricultural employers in the village at endline (i.e. during the final two weeks of the hiring shock). Our enumerators first consult a village resident to obtain a (partial) list of employers who hire agricultural workers in each village. Using this listing, we generate a roster with a randomized ordering of employers. Enumerators then conduct employer surveys following the order as stated in the roster, with a stopping rule of 20 employer surveys per village. In some cases, there are fewer than 20 listed employers per village, so the total number of surveys completed is less than 20. The mean number of employer surveys completed per village is 18.

In the 11 experimental rounds where we conduct a census of all households, we use census data to identify a sample of agricultural employers in the village. We generate a roster consisting of the five largest employers in the village (based on the number of worker-days they report hiring for a paid wage in the most recent harvest season), and a random sampling of the remaining employers in the village.

The employer survey includes a detailed hiring grid, in which employers describe each activity for which they required labor in the past 30 days. This includes data on labor use (number of workers, days worked, hiring location), cash wages and in-kind payments made to hired workers for each activity, as well as whether the activity was completed as early and as frequently as they would have liked. All regressions using the employer sample are unweighted.

Implementation issues. We faced issues conducting end line worker surveys in two experimental rounds and were only able to survey a small subset of intended participants. One village previously had a negative experience with outsiders who promised gifts if the village residents converted to a particular religion. As such, residents in this village got suspicious (our standard survey protocol was to offer respondents a small gift upon completion of the surveys) and refused to let our enumerators into the village to conduct surveys. In another village, several village residents who were not part of our study sample did not want surveys to be conducted in their village. While the respondents themselves did not have any issues participating in the survey, the hostility from other residents in the village made it difficult and potentially unsafe for our enumerators to enter the village. As such, we decided to halt data collection in this village.

In Appendix Table B.13, we present regression results including the two experimental rounds discussed here that were excluded from the main analysis because we were not able to complete survey work. Our results are robust to the inclusion of these two rounds.

Appendix D. Construction of Census Variables

We use village-level data on demographic and economic indicators from the 2011 Indian Population Census. This includes data on village size (population and number of households), caste composition, literacy, as well as worker counts.

As described in the 2011 Census meta data,[3] workers are defined as "all persons (irrespective of age and sex) who participated in any economically productive activity for any length of time during the reference period." The reference period corresponds to the one year preceding the date of survey enumeration. Workers who "worked for more than 6 months in the reference period" are referred to as main workers. Workers are further classified into four categories:

(1) agricultural laborers — this refers to individuals who "work on another person's land for wages in money or kind or share. She or he has no risk in the cultivation, but merely works in another person's land for wages"

(2) cultivators — this refers to individuals who are "engaged in cultivation of land owned or held from Government or held from private persons or institutions for payments in money, kind or share"

(3) workers in household industry — household industry is defined as "an industry conducted by one or more members of the household at home or within the village in rural areas and only within the precincts of the house where the household lives in urban areas"

(4) other workers — this refers to workers other than those in categories (1)-(3). Examples include work in the public sector, manufacturing, construction etc.

We construct several outcome variables using this data:

- Male population share: male population/village population
- Literacy rate: literate population/village population
- Worker share: all workers/village population
- Male worker share: male workers/all workers
- Main worker share: main workers/all workers
- Agricultural labor share: agricultural laborers/all workers
- Cultivator share: cultivators/all workers
- Non-farm self-employment share: workers in household industry/all workers
- Other workers share: other workers/all workers

[3]The 2011 Census meta data is available at https://www.censusindia.gov.in/2011census/HLO/Metadata_Census_2011.pdf.

Appendix E. Implied Aggregate Demand and Supply Elasticities

The nature of our shock allows us to back out both aggregate demand and supply elasticities during semi-peak periods, if we are willing to assume competitive equilibrium. While this is a strong assumption — and one that is not required in our main analysis — there are few opportunities in the literature to measure these implied elasticities, so we include these estimates here for completeness. Of course, as with all such analyses, the credibility of these estimates relies on the extent to which one believes in the underlying assumption of competitive equilibrium in semi-peak times.

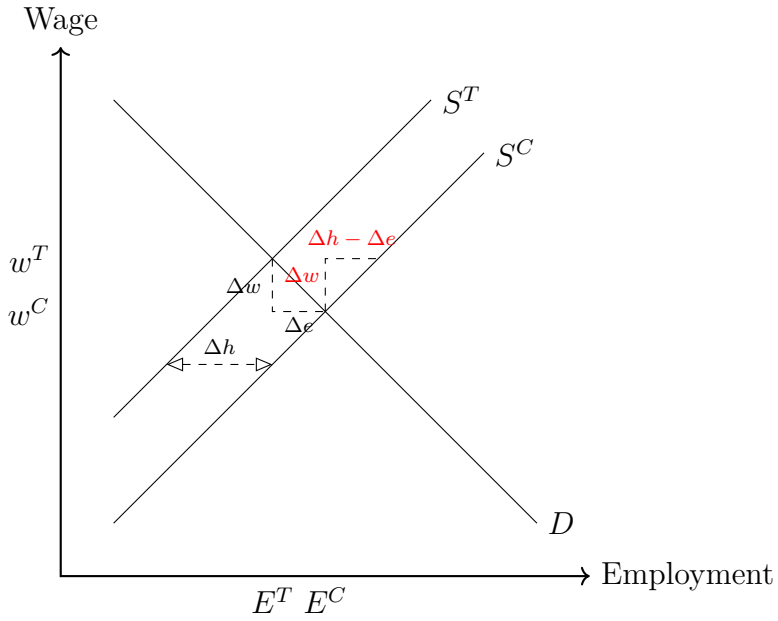


Figure A.8. Aggregate Elasticities

Note: Figure illustrates how we can identify aggregate demand and supply elasticities during semi-peak months, under the assumption of competitive equilibrium. Δw and Δe denote the observed change in wages and employment respectively as a result of the hiring shock. The shock traces out the inverse slope of the demand curve, $\frac{\Delta e}{\Delta w}$. Δh denotes the size of the shift in aggregate supply. Given Δh, a simple geometric argument shows that the slope of the supply curve is $\frac{\Delta w}{\Delta h - \Delta e}$.

Appendix Figure A.8 shows how we can identify both the aggregate demand and supply elasticities under this strong assumptions. We denote the observed change in wages between treatment and control as Δw and the change in aggregate employment as Δe. Given that our treatment shifted the supply curve inward, the shock traces out the (inverse) slope of the demand curve $\frac{\Delta e}{\Delta w}$. That supply shocks identify demand elasticities is well known. However, using arguments similar to Zoutman et al. (2018), we can also back out an implied supply elasticity as well. Because we know the exact shock size—how many workers were

employed in our external jobs and those workers' counterfactual employment rates—we can directly measure the size of the shift in aggregate supply Δh.[4] Given Δh, a simple geometric argument shows that the inverse slope of the supply curve is $\frac{\Delta w}{\Delta h - \Delta e}$.

Incorporating the impacts of our experimental shocks into this framework, the implied aggregate supply elasticity is 0.03, though clearly there is substantial noise around this estimate. This very small supply elasticity is consistent with the development literature.[5] The implied aggregate demand elasticity is -4.16, a very large magnitude relative to the literature.[6] If we believe that the magnitude of this implied demand elasticity is "too large," then one of our assumptions is likely unreasonable. First, under partially segmented markets, in semi-peak periods, employers may substitute toward workers outside of the village, who typically command higher wages. This would bias our measure of the change in employment in semi-peak times, leading to an overesimtate of the demand elasticity. Second, relaxing the assumption of market clearing could also help to explain these large demand elasticity magnitudes. One departure from market clearing that would yield an unbiased supply elasticity but distort the implied demand elasticity is monopsony.[7]

In addition to the caveats about underlying assumptions, our estimates are measured with considerable noise. We consequently view them as suggestive. More generally, we intend this stylized exercise to be illustrative—demonstrating how knowing the exact shock size enables one to recover both elasticities with one source of variation.

[4]Recall that our experimental job paid workers a premium. Therefore Δh is not equivalent to the fraction of laborers who took our job in treatment versus control, but instead the fraction of days they would have worked at the market wage in absence of the external job.

[5]For example, Goldberg (2016) measures a supply elasticity of 0.17 in rural Malawi.

[6]The demand elasticity of Impert and Pap (2020), obtained from a structural model of the migration response to NREGA, is an imprecisely estimated -0.22. Kaur (2019)'s estimate is -1.17.

[7]Under monopsony, employment levels are pinned down by the aggregate supply curve facing the employer. However, changes in employment and wages do not trace out the demand curve. In fact, wages fall by less and employment falls by more under monopsony versus market clearing (Appendix Figure A.2), leading to an implied demand elasticity that is too large. This is consistent with the view of Muralidharan et al. (2020).