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Does Maharashtra's Employment Guarantee Scheme Guarantee Employment? Effects of the 1988 Wage Increase*

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I. Introduction

Among developing countries, the Employment Guarantee Scheme (EGS) in the state of Maharashtra in India is probably the most famous and, by many accounts, the most successful direct governmental effort at reducing absolute poverty in rural areas.[1] Since the mid-1970s, the EGS has aimed to offer unskilled rural employment on demand, as embodied in its slogan, "Magel tyala kam" (Whoever desires work will get it). The work creates or maintains rural infrastructure through small-scale irrigation and soil conservation projects, reforestation, and rural road building. The EGS projects are designed to be highly intensive in their use of unskilled labor, which typically accounts for over two-thirds of variable costs. Wages are set in the form of piece rates, stipulating rates of pay for a large number of specific tasks, such as digging, breaking rocks, shifting earth, and transplanting.

The scale of the scheme is impressive; in a typical year, it provides about 100 million person-days of employment, at an average cost of about US$1 per day in the late 1980s. (The state's aggregate rural work force—including cultivators—was about 20 million persons in the mid-1980s.) Given this scale, it is surprising how little we seem to know about the scheme's performance in alleviating income poverty. The little we do know suggests that the scheme is quite well targeted, in that the nonpoor are rarely attracted, and many of the able-bodied rural poor participate at one time or another, and that the net transfer and income stabilization benefits to the poor are likely to be sizable.[2]

To many observers, the employment guarantee built into the Maharashtra scheme has been an important factor in realizing those benefits. The combination of an employment guarantee and a wage rate low enough to balance the budget in an average year is not only fiscally

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sustainable but it may also be socially desirable as the policy which, for any given budget, will have the greatest impact on measures of poverty that give highest weight to the poorest.[3] This will tend to be so if the wages that workers are willing to accept for EGS employment tend to be lowest for the poorest, so that they are the first to be attracted into the scheme.

Until recently, the piece rates on the EGS have been such that a typical EGS worker could earn a daily wage roughly on a par with prevailing agricultural wages. However, in May 1988 the piece rates paid by the EGS were doubled, in line with a doubling in statutory minimum wage rates in agriculture. Serious concerns were expressed at the time about the potential budgetary consequences of this substantial increase in the EGS wage.[4] Not only would workers, at existing employment levels, have to be paid a higher wage but any discrepancy between the EGS wage and agricultural wage was likely to draw more workers to the EGS.

However, EGS records reveal that the average real monthly cost of running the scheme actually fell after the doubling of the wage and, as we will show, this was due primarily to a fall in the average monthly attendance at EGS sites. But this raises new concerns. Did the potential impact on cost create pressures on the government to relax the employment guarantee and ration EGS employment? The provision of higher wages without an accompanying guarantee would have meant that, although the actual participants were made better-off, fewer of the poor were able to become participants. Moreover, the targeting efficiency of the scheme may have been diluted. On the other hand, external factors that influence the availability of alternate work, such as the timing and quantity of the monsoon rains, may have contributed independently to a decline in the demand for EGS employment after the wage increase.

The existing evidence on this issue is inconclusive. The government has always maintained its willingness to accommodate any increases in the demand for EGS employment and has denied that the guarantee was not honored after the wage increase. And the crop-years following the wage increase were among the best in recent times, which may explain equally well the falling attendances.[5] However, some observers have suggested that an unfulfilled demand for EGS work by the poor did emerge as a consequence of the wage increase. There were reports of unseasonal migration into urban areas of Maharashtra in 1988–89, attributed to problems in finding accessible EGS work in rural areas.[6]

This article attempts a careful analysis of the effects of the shift in the EGS wage schedule in mid-1988. Our primary aim is to determine whether, or in what sense, employment under the scheme was guaranteed in the year following the wage increase. The employment guarantee embodied in the legislation creating the scheme states that EGS

work will be made available to any person who desires it within the district in which he or she resides. Given the size of most districts, EGS employment is not necessarily accessible to many who need it. The greater challenge for EGS is to accommodate the demand for locally accessible employment, for which travel costs (pecuniary or nonpecuniary) are not prohibitive. In this article we investigate the extent to which this stronger form of the guarantee was maintained.

We ask, Was the decline in EGS employment in the year following the wage increase the result of budgetary restraint and rationing such that people who needed work could not find it locally? Or did it reflect an actual decline in the demand for (locally accessible) EGS employment? This would be easy to answer if we were able to observe directly the desired levels of EGS employment. However, the only data available are on the actual levels of employment under the EGS, which may or may not equal desired employment. We implement three approaches to getting around this problem and testing for employment rationing on the scheme after the wage increase. While these tests are necessarily indirect, under all three approaches we find evidence consistent with rationing.

Section II describes how the main variables of interest—EGS employment levels, wage rates, and the cost of the scheme to the government—evolved during the period of interest. In Section III, an econometric model of monthly attendance under the EGS from 1975 to 1988 is used to examine the determinants of EGS employment and whether these changed after the wage increase. An estimate is then made of the extent of rationing under the EGS during the 12 months after the wage increase. New data on the scheme for 1987–90 is used in Section IV to address the question, Does the EGS now "guarantee" employment, in the sense that workers can find local employment when they want it? Our conclusions are summarized in Section V.

II. Maharashtra's Employment Guarantee Scheme before and after May 1988

We first shall look closely at the period April 1987 to February 1990, for which we have monthly data on a number of key variables.[7] This will motivate our interest in the more analytical questions addressed in Sections III and IV.

One can usefully divide the period into three subperiods:

Subperiod 1. Pre–May 1988, during which the average wage rate under the EGS was approximately equal to the agricultural wage rate.

Subperiod 2. June 1988 to November 1988, during which there were some significant adjustments in the scheme, as will be discussed below.

Subperiod 3. The period since December 1988 in which, we shall argue, the scheme appears to have settled into a new equilibrium.

The three subperiods are identified in figure 1, which plots the

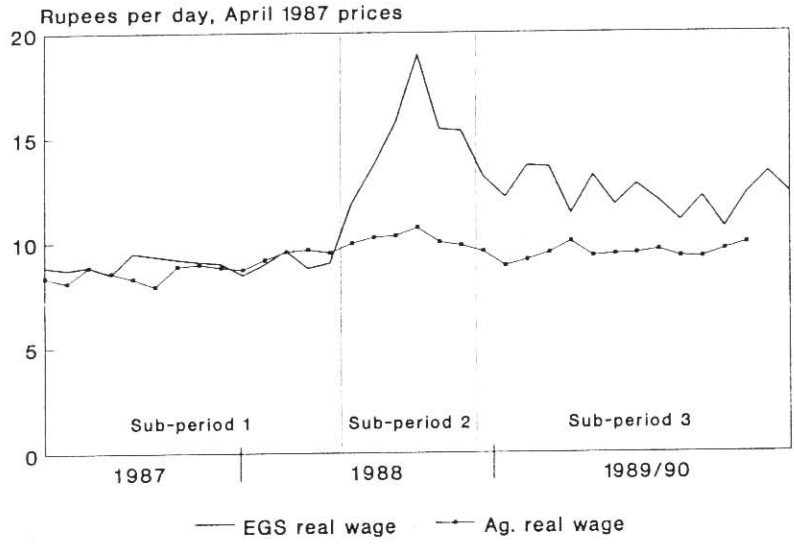

FIG. 1.—Wage rates for the EGS and in agriculture, Maharashtra, 1987–90.

average real wage rate for EGS employees over the period April 1987 to February 1990, as well as our estimate of the average wage rate for agriculture (which we will discuss further in Sec. IV).

The adjustment period saw the EGS wage roughly double initially, in line with the increase in the statutory minimum wage rate. The wage rate then fell almost as dramatically, to end up at about one-third higher (in real terms) than it had been prior to May 1988 (fig. 1).[8]

Inflation contributed to this decline, but there are other factors to consider. Wage rates under the EGS are fixed according to the type of work done and its output; for example, rock breaking is paid at a different rate per cubic foot than loading soil on trucks. The whole piece rate schedule doubled after May 1988. The allocation of available work across these activities—which we will refer to as the composition of EGS work—is determined by the EGS authorities, through their project choices. Under such conditions it may be quite sensible for the authorities to adjust the latter in favor of activities with lower piece rates, in response to the increase in the wage schedule.[9]

Given that such adjustments are not instantaneous, a better indication of how the scheme responded to the change in wage schedule can be obtained by comparing outcomes across periods 1 and 3. Table 1 gives some data of interest for all three subperiods, calculated from unpublished monthly EGS records.

By the third subperiod, the impact of doubling the piece rate

TABLE 1

SUMMARY DATA ON EGS BEFORE AND AFTER THE WAGE INCREASE

	SUBPERIOD			
	1 April 1987– May 1988	2 June 1988– November 1988	3 December 1988– February 1990	PERCENTAGE CHANGE, 1 TO 3
Total cost (Rs × 10^6/month, April 1987 prices)	132.6	123.6	105.3	−21
Of which:				
Wage cost	88.7	91.0	76.0	−14
Nonwage cost	43.9	32.6	29.2	−33
Employment (person-days × 10^6/month)	9.9	6.5	6.1	−38
Average wage rate (Rs/day, April 1987 prices)	9.0	15.1	12.4	37
Average nonwage cost per worker (Rs/day, April 1987 prices)	4.6	5.9	4.9	7

schedule amounted to about a one-third increase in the average real EGS wage rate; roughly two-thirds of the initial wage increase was absorbed through inflation and changes in work composition. But the more striking observation is that, despite the net real wage increase, employment actually fell, and by a proportion similar to the increase in the average wage.[10] Comparing subperiods 1 and 3, the unit nonwage cost remained fairly constant in real terms. On balance, the average real monthly cost of running the scheme fell by about one-fifth after the doubling of piece rates in May 1988.

A decomposition formula can be used to throw further light on the relative importance of these various factors—wage rates, nonwage costs, and employment—in determining the scheme's operating cost. Let C_t denote the total operating cost of EGS in month t, which can be written as $C_t = V_t L_t$, where V_t is unit real cost per worker and L_t is the number of workers employed in month t. Unit cost can be written as $V_t = W_t + X_t$, where W_t denotes average real wage cost per worker and X_t denotes average real nonwage cost. The value of W_t depends on the current purchasing power of the stipulated nominal piece rate schedule and on the allocation of available work across the various piece rate categories at date t. The piece rate schedule is tied to the statutory minimum wage rate for agricultural labor, m_t (in nominal units), for some predetermined allocation of work across piece rate categories. If that is also the allocation at date t, then $W_t = m_t/p_t$, where p_t denotes the price deflator for date t. More generally, $W_t = k_t m_t/p_t$, and a change in k will be referred to as a change in work composition (relative to the date when the piece rate schedule was fixed). We consider two dates, $t = 1$ (subperiod 1) and $t = 3$ (subperiod 3), and we know that $m_3 = 2m_1$. The change in monthly operating cost between subperiods 1 and 3 and its decomposition is then given in table 2.

Table 2 shows that both inflation and (more important) the change in work composition were important in holding down real costs after the increase in the minimum wage. However, the overwhelming term in the decomposition is the cost saving due to the decrease in employment; if nothing else had changed, the saving due to the drop in employment would have eliminated about 60% of the initial impact of the doubling of the minimum wage rate (table 2). In the following sections we will attempt to explain this fall in employment.

III. Did the Process of Employment Determination Change Significantly?

Now we shall look as far back into the history of the scheme as data permit, to better understand the determinants of employment. First we present a model of EGS attendances prior to mid-1988, which we then use to perform statistical tests for drift in EGS employment after

TABLE 2

A Decomposition of the Change in Operating Cost

	Change in Average Monthly Cost Due to	
$(m_3 - m_1)k_1L_1/p_1$	Increase in minimum nominal wage rate	89.0
$+ (p_3 - p_1)m_1k_1L_1/p^2_1$	Increase in price level	−17.8
$+ (k_3 - k_1)m_1L_1/p_1$	Change in work composition	−37.9
$+(X_3 - X_1)L_1$	Change in real unit nonwage cost	2.6
$+(L_3 - L_1)V_1$	Change in employment	−51.8
	Residual (interaction effects)	−11.5
$= C_3 - C_1$	Change in real cost (Rs × 10^6/month, April 1987 prices)	−27.3

the wage increase. After that we use the model to estimate the extent of employment rationing in the 12 months following the wage increase.

A Model of Monthly Attendances under the EGS, 1975–88

The data on monthly person-days of employment under the EGS used in the previous section are only available since April 1987. We need to go much further back in time to test convincingly for a structural change in the model determining employment after mid-1988. Fortunately, while a long time series of employment in person-days is not available, the government of Maharashtra has kept a regular attendance count at EGS project sites since July 1975. This appears to be a good proxy for recorded employment when both are observed.[11] However, it has been claimed by some observers that attendance counts are artificially inflated by some local officials. If this is true, then our results may be biased. Assuming that the wage increase enhanced the incentive to inflate the attendance count, our results will tend to underestimate the extent of rationing after the wage increase.

We assume that monthly EGS attendances are determined by the current EGS wage rate, food grain output in the current year, the level and pattern of rainfall, seasonal dummy variables, and a time trend, included to allow for any time-dependent omitted variables. We also allow the possibility that EGS attendances in a given month do not adjust instantaneously to the current values of these variables. Serial correlation in attendances can arise in a number of ways. For example, changes in agricultural output may have lagged effects on income of the poor and, hence, their demand for relief work. Under certain conditions (notably that the lags can be smoothed exponentially), simply adding lagged attendances to the model will adequately capture such lagged effects of the explanatory variables. The fortnightly payment period used by the EGS also may generate some "stickiness" in the adjustment of employment to changing conditions. It also can be con-

jectured that the attendance count will tend to be more serially correlated than person-days of employment.

Labor attendance data are available on a monthly basis for the 14 years spanning July 1975 through June 1989.[12] However, only annual data on the EGS time wage are available for this period; the time wage is measured as the average wage per person-day for the EGS financial year of April through March. Thus we have had to use the same value of the wage variable for the months April through March. The nominal wage rates were deflated by the Consumer Price Index for Agricultural Laborers for Maharashtra to obtain real EGS wages at 1975–76 prices.

Data on output of food grains pertain to the agricultural year of July to June, and (as for the EGS real wage) the food grain output variable takes on the same value over these months.[13] Food grain output is, of course, observed only at the end of the agricultural year, and it may appear odd to use it to explain EGS attendances for the earlier months (of the agricultural year). However, it seems plausible that, insofar as EGS attendances in the earlier months depend on the level of agricultural activity in those months, the conditions determining the latter (e.g., adequate and timely rainfall in the monsoon period) would also be reflected in the agricultural output for the entire year.[14]

Monthly data on rainfall for four meteorological subdivisions in Maharashtra were collated from various issues of the *Statistical Abstract of India* and the *Agricultural Situation in India*. Rainfall for Maharashtra is derived as a weighted average of the rainfall in the four subdivisions, the weights being proportional to the average net sown areas in the subdivisions during 1978–79 to 1980–81.[15] The effect of rainfall on monthly EGS labor attendance, although potentially significant, need not be straightforward. The rainfall effect presumably would be seasonally differentiated, depending on whether there is deficit or excess rainfall and on the amount of deficit or excess. We thus use a fairly flexible approach in introducing the rainfall variables, by defining the following "rainfall excess" (*RE*) and "rainfall deficit" (*RD*) variables: $RE_j \equiv M_j \cdot DE \cdot (R - NR)/NR$ and $RD_j \equiv M_j \cdot (1 - DE) \cdot (NR - R)/NR$ ($j = 1, \ldots, 12$), where M_j is a dummy variable for month j, DE is a dummy variable for excess rainfall (equals one if there is excess rain, zero otherwise), and R and NR are the actual and normal rainfall, respectively. The normal rainfall for any month is defined as the average rainfall for that month over the entire 13-year period 1975–76 to 1988–89.

The wage rate in agriculture is probably the most important omitted variable in our model. There are two sources of agricultural wage data, namely, the data used in figure 1 (which we have obtained from unpublished sources, as described in Sec. IV) and the data published in *Agricultural Wages in India* (*AWI*). The former source is not available prior to 1986. The *AWI* data currently are available only up to

1986–87, so if we used that data as an independent variable in the model we would have missing values to deal with for the last 2 years, and we would have to forecast the series for 1988–89 prior to forecasting attendances. This creates complications, and it is probably better to give our model a reduced-form interpretation, in which an equation for the agricultural wage rate has been "solved out." The agricultural wage data for Maharashtra over the last few years used in figure 1 do not indicate that the drop in EGS attendances after mid-1988 could plausibly be attributed to an increase in the agricultural wage rate, which remained fairly constant in real terms (an interesting observation, which we will discuss further below). This suggests that our later inferences based on the postsample forecasts are likely to be robust to the omission of agricultural wages from our model.

Our model of EGS labor attendances can be written as

$$L_t = \alpha_0 + \alpha_1 L_{t-1} + \alpha_2 W_t + \alpha_3 Q_t + \alpha_4 t + \sum_{j=1}^{11} \beta_j M_{jt} + \sum_{j=1}^{12} (\pi_j RE_{jt} + \delta_j RD_{jt}) + \epsilon_t, \quad (1)$$

where L_t is the recorded labor attendance on the EGS for month t, W_t is the real EGS wage rate for the year including month t, Q_t is the output of food grains for that crop year, M_js are the monthly dummy variables for $j = 1, \ldots, 11$ ($j = 1$ represents July), t is a time trend, RE_j and RD_j are the month-specific excess and deficit rainfall variables (as defined above), respectively, for $j = 1, \ldots, 12$, and ϵ_t is an independent and identically distributed (i.i.d.) error term. The variables L_t, W_t, and Q_t are measured in natural logarithms.

The above model was estimated by ordinary least squares over the period July 1975 to June 1988.[16] After pruning the model to eliminate parameters with absolute t-ratios less than unity (one variable at a time), the final estimates presented in table 3 were obtained. The within-sample predictive performance of the model is good; the standard error of estimate is less than 1% of the mean log attendance, and fitted values track actual values well (fig. 2). The model passed all diagnostic tests performed (table 3).

The parameter estimates indicate significant short-run elasticities of EGS attendances with respect to food grain production and the real EGS wage rate of -0.28 and 0.20, respectively. The dynamic process of adjustment in attendances to changing conditions is quite slow, as indicated by the coefficient on lagged attendances of 0.8. Thus, the long-run elasticities for output and the real wage are considerably higher than for the short run; the long-run elasticities are -1.4 and 1.0, respectively. A negative time trend in attendances is indicated,

TABLE 3

PARAMETER ESTIMATES FOR THE EGS MONTHLY ATTENDANCE MODEL, 1975–88

Explanatory Variable			Parameter Estimate (Absolute t-ratio)
Intercept			4.907
			(4.51)
L_{-1}			.805
			(22.25)
W			.196
			(2.30)
Q			−.284
			(2.93)
t			−.001
			(1.60)
	M_j	RE_j	RD_j
July	−.116	−.758	−.572
	(1.74)	(1.86)	(1.41)
August	−.087	−.406	. . .
	(2.06)	(2.06)	
September	. . .	−.239	. . .
		(3.05)	
October	−.163	. . .	. . .
	(4.33)		
November	. . .	. . .	−.161
			(2.64)
December	.376	−.075	−.182
	(5.39)	(1.57)	(2.01)
January	.106	.069	.085
	(1.77)	(1.13)	(1.02)
February	. . .	.063	.226
		(2.01)	(3.96)
March	.275	. . .	−.206
	(4.82)		(1.94)
April	.071	. . .	. . .
	(1.82)		
May	.113	−.090	. . .
	(2.59)	(1.65)	
June	. . .	−.301	. . .
		(1.61)	

NOTE.—R^2 = 0.923; SEE = 0.1096; mean d.v. = 13.1286; N = 155; Durbin h = 0.445; LM tests: autocorrelation (1) = 0.0479; autocorrelation (12) = 0.6761; functional form (1) = 0.7574; normality (2) = 0.6949; heteroscedasticity (1) = 2.638. Postsample forecasting: χ^2_1 (12) = 30.59; χ^2_2 (12) = 67.33.

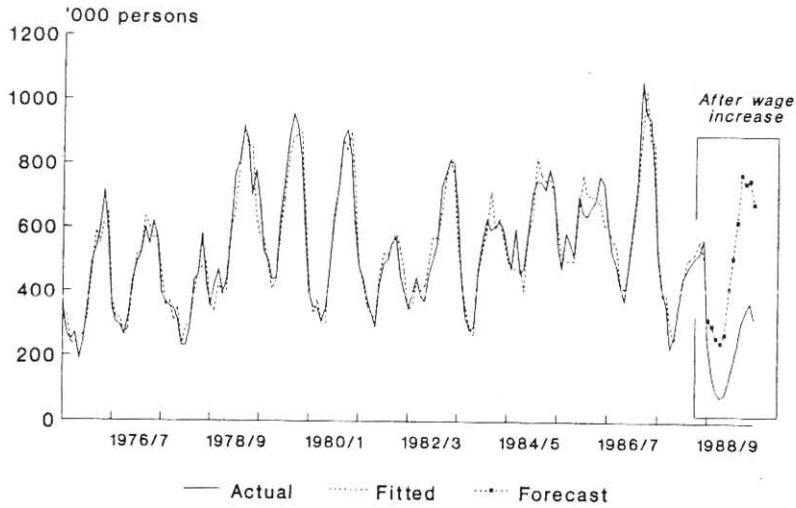

FIG. 2.—EGS attendances by month, Maharashtra, 1975–89

although it is quantitatively small, implying a monthly rate of decline of 0.07% (the latter cumulated over 13 years would result in a fall by 10%). Attendances also follow a highly seasonal pattern, induced in part by the seasonality of rainfall (table 3).

Tests for a Structural Break in the Model after the Wage Increase

The results in table 3 suggest that, ceteris paribus, labor attendances under the EGS would have increased in response to the higher wage after mid-1988. However, attendances actually declined sharply during the agricultural year 1988–89 (fig. 2). The average monthly attendance during 1988–89 was barely half of what it was during the previous year, which itself was not, by a long shot, a peak EGS year. An explanation may be sought in the higher than normal rainfall during 1988–89 (also reflected in the food grains production for that year), resulting in greater availability of agricultural employment. The critical issue here is whether this explanation is sufficient to account for the observed levels of attendance under the EGS for this period. Does the above model adequately explain the decline in attendances after the wage increase, taking account of the prevailing conditions?

We performed two statistical tests of the accuracy of the model's predictions in the postsample period of July 1988 to June 1989.[17] The first involves estimating a new model for the entire (sample and postsample) period, where the original model is augmented with a set of dummy variables, one for each observation in the postsample pe-

riod, and then testing for the joint significance of the dummy variables.[18] The second test uses the ratio of the sum of squares of the one-step-ahead forecast errors to the variance of the regression error of the model as the test statistic, which is distributed as χ^2, with degrees of freedom given by the length of the forecast period.[19]

The results of the two tests are reported at the bottom of table 3. They both indicate that the null hypothesis of parameter stability is strongly rejected for the postsample period of July 1988 to June 1989. The attendance count for the latter period cannot plausibly have been generated by the model prevailing prior to the wage increase.[20]

An Estimate of the Extent of Rationing after the Wage Increase

The drift in the attendance model after the wage increase is statistically significant. We shall now offer an assessment of its quantitative importance. We assume that the model we have estimated for 1975–88 satisfactorily represents the supply function for EGS work. This could be a strong assumption, for there may well have been some rationing in the earlier period as well.[21] To the extent that this assumption does not hold, we will underestimate the extent of rationing in recent years. However, the results we have obtained for 1975–88 are at least consistent with what we would expect to find for a labor supply function (and quite unlike those we have obtained for recent years, as we discuss later).

Under this assumption, we can use the 1975–88 model to estimate the latent desired EGS employment in the 12 months after the wage increase. This can then be compared to actual employment, to estimate the extent of rationing after the wage increase. Figure 2 gives the forecasted attendance counts for the postsample period. Note that these are dynamic forecasts using the lagged forecasts (rather than lagged actual attendances).[22] That is appropriate, since we want to know what attendances we would have expected if there had been no rationing in the postsample period.

The forecasts indicate a sharp fall in attendances immediately after June 1988, but not as large as the fall in attendances actually observed. Substantially higher attendances are predicted by the model, and the deviation between forecast and actual attendance continues to increase well into the crop year. Table 4 gives the estimated employment rationing by month.

The mean predicted attendance for the postsample period is 491,269 per month. The mean actual attendance was 212,840. We thus estimate that almost 280,000 persons per month—about 3.3 million for the whole year—desired EGS employment in 1988–89 but could not get it. The amount of rationing increased steadily after the wage increase, peaking in March 1989 (table 4).

How much of the observed decline in attendances after the wage

TABLE 4

ESTIMATED EMPLOYMENT RATIONING AFTER THE EGS WAGE INCREASE (in Thousands of Persons)

	Actual Attendances	Unfulfilled Desired Local Employment
July 1988	248.1	70.6
August 1988	147.7	151.7
September 1988	97.5	163.7
October 1988	79.3	168.4
November 1988	85.1	188.2
December 1988	130.1	283.2
January 1989	182.1	327.2
February 1989	238.9	385.0
March 1989	308.9	461.3
April 1989	342.5	401.6
May 1989	371.3	382.7
June 1989	322.5	357.7
Mean 1988–89	212.8	278.4

increase is attributable to this rationing rather than other factors such as the good monsoon of that year? The mean monthly attendance under the EGS in the sample period, 1975–88, was 534,974 persons. The decrease in 1988–89 was thus 322,134, of which 86% is attributable to rationing.

IV. Does the EGS Now Guarantee Local Employment?

The above results suggest that the higher minimum wage was associated with a significant change in the way employment under the EGS is determined. We now return to the 1987–90 period, to see whether the recent data are consistent with the existence of rationing. Two tests will be performed; the first uses information on works in progress, while the second uses information on agricultural wage rates. For both, we have a data set of a little over 33 months, though 14 months are in subperiod 1, prior to the wage increase. Rather than drop this period, we have retained the full 33 observations. Since rationing is less likely in subperiod 1, its inclusion will probably bias our tests toward accepting the null hypothesis of no rationing.

The Effect of the Number of EGS Projects in Progress on Employment

The motivation for this test can best be understood by noting that the number of EGS projects should not affect the desired level of local EGS work, ceteris paribus. If the scheme is successful in providing guaranteed local employment, then the number of projects, and atten-

dances at the given projects, will adjust flexibly to accommodate desired work. An extra project, for example, would simply displace employment elsewhere; the aggregate would remain unchanged, unless other conditions facing workers have changed.

The null hypothesis of guaranteed local employment thus can be interpreted as the hypothesis that the observed monthly employment under the EGS (denoted L) equals the unobserved desired level of EGS work at minimal travel costs. It then follows that, under the null, L should not be influenced by the number of EGS projects in progress during the same month (denoted P).

The simplest testing procedure is as follows: Regress L_t on P_t and on other variables which, a priori, might be expected to influence L_t. A significant estimate of the coefficient on P_t implies rejection of the null, subject to the usual caveat that the estimate be consistent.

We thus estimate the following test equation:

$$L_t = b_0 + b_1 L_{t-1} + b_2 W_t + b_3 WA_t + M_t b_4' + b_5 P_t + u_t, \qquad (2)$$

where L_t is now measured by observed person-days of employment under the EGS for month t; W_t is the average daily wage paid to laborers on EGS sites in that month; WA_t is the reported daily wage in agriculture for month t; M_t is a set of monthly dummies; P_t is the number of projects in progress in month t; and u_t is an i.i.d. disturbance term.[23] All variables except the monthly dummies are in natural logs. The lagged dependent variable was included to preclude a spurious estimate of b_5 as a result of serial correlation in both the L and P series. One would generally expect that the demand for EGS employment depends positively on W. Since agricultural employment is an alternative to EGS employment, we would expect L to depend negatively on WA under the null hypothesis. And the monthly dummies are included to capture the seasonal nature of agricultural activity.

Since April 1987, EGS authorities have been keeping more detailed monthly records of employment, wages paid, and projects in progress. Our employment data after April 1987 will be estimates of actual person-days of employment, rather than attendances, as were used in the previous section. At the time this study began, these data were available for April 1987 to February 1990. For the period up to December 1989, monthly data on agricultural wage rates were also available from the Directorate of Economics and Statistics, government of Maharashtra.[24]

Ordinary least squares estimates of the parameters of interest are given in column 1 of table 5. The estimated coefficient on P is positive and significant. The estimate of the coefficient on W is significant but does not have the sign one would expect of the supply function for

TABLE 5

FURTHER TESTS FOR EMPLOYMENT RATIONING, 1987–89

ELASTICITY OF EGS EMPLOYMENT WITH REGARD TO		ESTIMATION METHOD		
		1 (OLS)	2 (Instrumental Variables)	3 (Instrumental Variables)
Number of work projects in progress (P)	b_5	1.094	1.243	1.136
		(3.83)	(3.08)	(4.85)
Average EGS wage rate (W)	b_2	−.514	−.523	−.479
		(2.30)	(2.32)	(2.92)
Average agricultural wage rate (WA)	b_3	−.011	.102	N.A.
		(.017)	(.144)	

NOTE.—Absolute t-ratios in parentheses, 31 monthly observations, R^2 = .97 for 1, 2, and 3. Regressions 1 and 2 include 11 monthly dummy variables and lagged dependent variable. Regression 3 excludes highly insignificant variables (including agricultural wage rate). All regressions passed LM tests for serial correlation, functional form, normality, and heteroscedasticity. See text for further details on the model.

EGS work. The elasticity of employment with respect to the agricultural wage rate is not significantly different from zero.

While these results are suggestive of rationing, they do not warrant an immediate rejection of the null hypothesis. There may be sources of bias in the OLS estimates that would invalidate any inference based on them.

A necessary condition for the consistency of OLS estimates is that all the regressors be exogenous. This includes P. Under the null hypothesis, exogeneity of P would necessarily imply that the costs of access to EGS projects are negligible. Only then would a full accommodation of the demand for employment in all periods be consistent with an exogenously given number of projects. This seems unlikely. A more plausible assumption is that P is responsive to the demand for EGS work in the current period and hence, under the null hypothesis, to L. Thus, there is reason to suspect the presence of simultaneity bias in the OLS estimates.

We correct for the possible bias by using instrumental variables (IV) for P_t. The natural instrument is P_{t-1}. Possible additional instruments are the lagged values of L, W, and WA.[25] With these instruments for P_t, we obtained the Instrumental Variables (IV) estimates given in column 2 of table 5. Column 3 gives the slightly more precise estimates obtainable by deleting strongly insignificant variables from equation (2). The estimates of b_5 are again highly significant, and the aforementioned comments on the two wage elasticities, b_2 and b_3, continue to

hold. Alternative assumptions about the partial adjustment process governing P might suggest somewhat different instruments. While the use of those instruments would affect the efficiency of the estimates, it should not affect their consistency.

The assumptions we make about the processes generating W and WA also will be important. They determine the status of the regressors in equation (2), which in turn dictates the appropriate estimation procedure. The null hypothesis does not, a priori, imply any restrictions on these processes. So far we have implicitly assumed that W and WA are exogenous. Relaxing this assumption, we estimated equation (2) treating W_t and WA_t as endogenous, as well as P_t; that is, these variables were dropped from the set of instruments, and a dummy variable to capture the doubling of the EGS wage was added. Again, the IV estimate of the coefficient on P_t was highly significant (with similar results in table 5), and the wage responses remained inconsistent with what one would expect of a supply function.

The other potential source of bias is the possibility of omitted variables. The problem in this instance is somewhat more serious than in most, for the following reason. Even if the omitted variables are uncorrelated with the remaining included variables, given the endogeneity of P_t, they will be correlated with this variable. A significant estimate of the coefficient of P_t may therefore simply reflect the influence of these omitted variables. We have no way of distinguishing this effect from that which would arise from rationing.

Very little can be done about this problem. We attempted to mitigate its effects by adding to equation (2) various potential proximate influences on the demand for EGS employment. Among the variables we included were year dummies, the deviations in rainfall from the mean, monsoon dummies, and various interaction terms. In each case, the instrumental variables estimate of the coefficient on P_t was highly significant.

We cannot eliminate the possibility of omitted variable bias, and so we may be incorrectly rejecting the null hypothesis. Nevertheless, different variants of the test equation failed to yield either an insignificant estimate of b_5 or signs on the wage variables in accord with our intuition about the determinants of the demand for EGS employment.

Thus, it is hard to believe that we are observing the supply function to the EGS. The opening and closing of EGS project sites gives the authorities an effective instrument for influencing EGS employment and, hence, budgetary outlays, independent of the need for EGS employment by the poor.

Unfortunately, we cannot repeat this test for any reasonable length of time prior to May 1988, as the necessary data do not exist. However, given that we found strong evidence in Section III of a structural break in the model determining EGS attendances after that

date, our combined results are consistent with the view that rationing was introduced into the EGS in response to the increase in the minimum wage rate. The revealed negative wage elasticity of employment in the above results, in contrast to the expected positive response found in the previous section, is also consistent with this interpretation.

Note that workers do not have to be turned away from existing and ongoing sites to achieve employment rationing under the EGS. The process of opening and closing projects allows the scheme to influence employment, irrespective of demand at a given wage rate. For example, by not opening new sites and by insisting that new workers are accommodated at existing sites, the authorities generally will raise the (pecuniary and nonpecuniary) cost that workers incur in participating in the EGS. The way in which employment is rationed across workers may thus be quite complex, involving the various factors that influence effective access to EGS sites, including, for example, the demands on a worker's time at home.

From our informal discussions with EGS administrators (after this article had been substantially written), it seems that the main way in which the reduction in employment was achieved was through restrictions on the opening of new projects after the wage increase. Unlike previous practice, after the wage increase the authorities appear to have insisted that existing works in progress should be completed before new projects are opened. For many actual or potential workers, this made EGS sites less accessible or less well known.[26]

The Effect on Agricultural Wages

A second testable implication of the existence of an effective employment guarantee is that one would expect to see a relationship between the EGS wage rate and the agricultural wage rate. The strength of that relationship will depend on specific labor market conditions. Many EGS workers are also regular participants in the agricultural labor market and vice versa. Furthermore, the work involved appears to be quite similar, as are the hours worked. In these circumstances, with EGS and agricultural wages roughly equal initially, higher EGS wages with guaranteed employment would almost certainly result in higher agricultural wages.

A visual inspection of figure 1 does not suggest that the agricultural wage rate responded much to changes in the EGS wage rate during this period. Granted, the simple correlation coefficient between the two wage rates is high ($r = .74$). But this is largely a spurious correlation, reflecting the serial correlation of both series; the correlation coefficient is only .32 between the innovations in each series around its first-order autoregression.[27] The following dynamic regres-

sion performs fairly well in reproducing the agricultural wage series in figure 1 (all variables in linear form):

$$WA_t = \underset{(1.95)}{2.607} + \underset{(3.66)}{.675}WA_{t-1} + \underset{(2.33)}{.134}W_t - \underset{(1.99)}{.105}W_{t-1} + \text{monthly dummy variables and time trend} \quad (3)$$

$R^2 = .851$; SEE = .347; mean d.v. = 9.34; $N = 31$; LM tests: autocorrelation (1) = .41, functional form = 1.57, normality = 1.19, heteroscedasticity = 2.22.

Within a current month, the impact on the agricultural wage rate of a, say, Rs 10 increase in the EGS wage rate is Rs 1.3.

While the short-run agricultural wage response is small, one could still conjecture that there is some sort of long-run equilibrium relationship between the two wage rates. Neither figure 1 nor the above regression offers much support for that conjecture. The long-run response of the agricultural wage rate to the EGS wage rate implied by the above regression is very much less than unity; an Rs 10 increase in the EGS wage rate would result in less than an Rs 1 increase in the agricultural wage in the long run. The existence of a long-run equilibrium wage differential also can be assessed by testing for cointegration, using the methodology proposed by R. F. Engle and C. W. J. Granger.[28] The two wage series in figure 1 fail the cointegration test; the null hypothesis of no cointegration cannot be rejected convincingly.[29] There is little sign in these data of anything more than a slight impact of changes in the EGS wage on agricultural wages in either the short run or the long run.

While suggestive, these results fall short of being conclusive evidence of rationing on EGS. The number of available time-series observations is modest and may be inadequate for detecting the true long-run response; the power of these tests is questionable in small samples. And, as long as there is at least some positive response from agricultural wages, one cannot rule out the possibility that the EGS does in fact provide a secure fallback position (or "threat point") in wage bargaining, implying that it is really the low bargaining power of agricultural workers that underlies our empirical result.[30]

Nonetheless, while indirect transfer benefits to the poor from the second-round effects of public employment on agricultural labor markets are potentially large, these data do not suggest that the potential has been realized by the EGS, at least over recent years.[31]

V. Conclusion

Increasing the statutory minimum wage rate can seem an attractive policy for politicians keen to enhance their popularity among the poor.

Political pressure for an increase in the minimum agricultural wage rate in Maharashtra led to its doubling in mid-1988. Subsequently, there has been concern in government circles and the development community about the consequences for the state's famous Employment Guarantee Scheme. The view one takes of the Maharashtra experience also has implications for the design of other schemes of this sort, such as the recently proposed national EGS in India and the many actual or proposed rural public-works schemes in other countries.

Historically, statutory minimum wage rates in rural areas have been virtually impossible to enforce, and the agricultural wage rate often has been below the minimum wage rate. In theory at least, a scheme such as the EGS gives the government a policy instrument for enforcing a minimum wage rate, as the guarantee should be a credible threat in unskilled labor markets. However, unless the politicians are also willing to foot the bill, the need for budgetary restraint will create pressure for a relaxation of the guarantee. If so, the poverty alleviation impact of an increase in the minimum wage rate may be dissipated quickly; some will get higher wages but others will have to go without.

Our comparison of the budgetary aggregates and related statistics on the EGS reveals that average monthly expenditures fell after the increase in the statutory minimum wage rate. This is attributable to two main factors: (i) After an initial adjustment period of a few months, we find that the real average EGS wage rate increased by only one-third, despite the initial doubling of all nominal piece rates. While inflation helped, the more important factor was that EGS work shifted toward activities that are paid at lower piece rates. (ii) There was a sharp fall in EGS employment after the increase in wage rates. Employment fell by about one-third.

However, this alone does not imply that the authorities introduced some form of rationing. There are other variables to consider. The two crop-years after the wage increase were good for agriculture, thanks to excellent and timely monsoons, particularly in 1988–89. Only by careful modeling of the determination of employment under the EGS can we find out whether the imposition of higher minimum wage rates led to a significant and substantial change in the scheme, such that an unfulfilled demand for relief work emerged on a large scale.

Our econometric investigations suggest that employment rationing did occur after the wage increase. We have presented three empirical observations to support that conclusion:

i) A model of EGS employment (measured by an attendance count) was estimated on monthly data over 13 years prior to the doubling of the minimum wage rate in mid-1988. The model predicts substantially higher EGS attendances than those actually observed after the increase in the minimum wage rate and allowing for the fact that

1988–89 was a good agricultural year. Our results suggest a structural break in the process of employment determination, consistent with rationing. Assuming that our model adequately captures the supply function for local EGS work, we estimate that the EGS met only 43% of desired local employment in the 12 months after the wage increase. Over 80% of the difference between mean employment per month in 1988–89 and mean employment for the preceding 13 years was due to rationing; otherwise the expected attendance figure in 1988–89 would have been some 3.3 million persons higher.

ii) From further tests for a 3-year period up to early 1990 (for which we have better employment data and a monthly series on the number of EGS projects in progress) we find little evidence to suggest that the observed series of EGS employment can be interpreted as the workers' desired local employment in the scheme. The number of projects in progress has a sizable and significant effect on EGS employment, which is not what one would expect if the scheme was simply accommodating desired local EGS employment. Also, unlike the period prior to the increase in minimum wage rates, we find a seemingly perverse response of employment to EGS wage rates, and agricultural wage rates do not appear to have influenced employment on the EGS in recent years.

iii) Finally, we investigated the effect on agricultural labor markets. If the EGS really was absorbing the excess supply of labor to alternative work, then one would expect to see a substantial impact on wages for that work stemming from shifts in EGS wages. However, we find no sign of an equilibrium relationship between the two wage rates. Little more than 10% of an increase in the EGS wage was passed on in the agricultural wage rate in either the short run or the long run. This is consistent with the view that rationing of EGS employment dampened the expected second-round income effects arising through agricultural labor markets.

These results suggest that the doubling of the EGS wage rate in May 1988 did not go hand in hand with a commitment to the extra budgetary resources needed to avoid substantial subsequent rationing of EGS employment. EGS administrators, upon being asked to explain our findings, acknowledged that after the wage increase, directives were issued stipulating that existing projects be completed before new projects were opened. From our results, it appears that this significantly restricted the number of projects in progress, and this appears to have been the main mechanism through which rationing was achieved. Thus EGS employment was probably rationed across desired participants according to their accessibility to existing project sites. While those with easy access to EGS work gained, others went without.

Our results do not constitute a case against Maharashtra's EGS

or similar schemes elsewhere. The concept of assured employment does have a number of potentially attractive features from the point of view of poverty alleviation: it often allows scarce resources to go to the poorest first (at least to those able to work), it enhances the insurance benefits to the poor, and it helps undermine some of the possibilities for their corruption on such schemes and for their exploitation in labor markets and tenancy contracts. However, achieving these benefits must entail a wage rate consistent with budgetary resources in a typical year. Finding those resources will not be as easy as raising the statutory minimum wage rate, and there can be no presumption that higher wage rates under fiscal restraint will be in the interests of the poor.

Notes

* We are grateful to the government of Maharashtra for providing generous access to its records on the Employment Guarantee Scheme and related data. For comments on this article and other forms of help in the research, our thanks also go to Seema Arora, John Echeverri-Gent, Felipe Jaramillo, Samuel Lieberman, Kiran Moghe, Roger Slade, Dominique van de Walle, and the journal's referees. This research has been supported by the World Bank's Research Committee under RPO 675-96. However, the views expressed here are our own and should not be attributed to any other person or organization, including the World Bank.

1. There is a large literature on the scheme, including K. Dandekar and M. Sathe, "Employment Guarantee Scheme and Food for Work Programme," *Economic and Political Weekly* 15 (April 1980): 707–13; K. Basu, "Food for Work Programmes: Beyond Roads that Get Washed Away," *Economic and Political Weekly* 16 (1981): 37–40; R. J. Herring and R. M. Edwards, "Guaranteeing Employment to the Rural Poor: Social Functions and Class Interests in Employment Guarantee Scheme in Western India," *World Development* 11 (1983): 575–92; Kumudini Dandekar, *Employment Guarantee Scheme: An Employment Opportunity for Women*, Gokhale Institute Studies, no. 67 (Bombay: Orient Longman, 1983); S. Lieberman, "Field-level Perspectives on Maharashtra's Employment Guarantee Scheme," *Public Administration and Development* 5 (1985): 109–27; J. Echeverri-Gent, "Guaranteed Employment in an Indian State: The Maharashtra Experience," *Asian Survey* 28 (1988): 1294–1310; S. Acharya and V. G. Panwalkar, "The Maharashtra Employment Guarantee Scheme: Impacts on Male and Female Labour" (paper prepared for the Population Council Program on Women's Roles and Gender Differences, New York, 1988); J. Dreze, "Famine Prevention in India," in *The Political Economy of Hunger*, vol. 2, *Famine Prevention*, ed. J. Dreze and A. Sen (Oxford: Oxford University Press, 1991); M. J. Bhende, T. S. Walker, S. S. Lieberman, and J. V. Venkataram, "The EGS and the Poor: Evidence from Longitudinal Village Studies" (International Crops Research Institute for the Semi-Arid Tropics, Hyderabad, 1990, mimeographed); World Bank, *World Development Report, 1990: Poverty* (Washington, D.C.: World Bank, 1990), chap. 6; and M. Ravallion, "Reaching the Rural Poor through Public Employment: Arguments, Lessons, and Evidence from South Asia," *World Bank Research Observer* 6 (June 1991): 153–76.

2. The existing evidence is more conclusive on performance in screening poor from nonpoor than on transfer and stabilization benefits, recognizing that

the latter are net of participants' forgone incomes, which are difficult to measure. See Ravallion, "Reaching the Rural Poor through Public Employment," for a recent survey of the evidence on the scheme's performance in poverty alleviation.

3. See M. Ravallion, "On the Coverage of Public Employment Schemes for Poverty Alleviation," *Journal of Development Economics* 34 (1990): 57–79. Exceptions can arise if nonwage cost per worker is sufficiently high or if a high value is placed on participants crossing the poverty line (rather than raising the lowest incomes).

4. See K. Subbarao, "Interventions to Combat Household-level Food Insecurity: A Review of India's Experience" (World Bank, Washington, D.C., 1989, mimeographed); and World Bank, *India: Poverty, Employment and Social Services*, World Bank Country Study (Washington, D.C.: World Bank, 1989).

5. During a brief visit to Pune district in Maharashtra in May 1990, M. Ravallion and G. Datt asked a number of workers at three EGS project sites whether they had experienced unusual difficulties in obtaining EGS work recently. The most common answer was no. But one should be cautious here. Ideally, one should be talking to people who were unable to obtain desired EGS employment rather than those currently employed (though, of course, the latter are far more easy to find). It also seems that workers in the areas we visited were quite well organized politically; possibly we would have received different answers elsewhere. Furthermore, inflation had eroded much of the impact of the 1988 wage increase by the time of our interviews, and 2 years is a long recall period. State government officials administering the scheme, when asked to explain the decline in EGS employment, generally attributed it to the good monsoons in the 2 years following the wage increase.

6. A *Times of India News Service* report from Pune (October 18, 1988) stated that "Slums mushrooming on the periphery of several towns in Maharashtra's sugar belt indicate the large scale migration of landless laborers from the drought-hit areas in search of livelihood. The migration does not appear to be seasonal like the earlier ones, with the result that the slums may well develop into permanent settlements." The report identifies failure to find EGS work as the main cause.

7. Only limited data are available on a monthly basis for the time prior to this period. From April 1987 on, computerized records have been kept of person-days of employment and other variables, such as projects in progress. Prior to this date, only an attendance count was kept on a monthly basis. There were clear errors in a few observations, usually underreported employment evident as extreme average wage rates; 15 of the 875 district/month observations in the data set (25 districts by 35 months) had to be deleted for this reason. Our aggregate series on the EGS were built up from the district data, deleting these obvious errors.

8. The monthly Consumer Price Index for Agricultural Laborers for Maharashtra is used as the deflator in fig. 1.

9. For example, suppose (to simplify the exposition) that there are just two activities, "digging" and "shifting," with the rate for digging set above that for shifting. The relative wage across activities is fixed (it did not change before and after May 1988), and we will assume that the administrator cannot influence total employment. The value of output is $f(s, 1 - s)$ per unit of total employment, where s denotes the proportion of labor allocated to shifting, which is paid at the rate W. (The production function is quasi-concave and homogeneous of degree one.) The surplus per unit of employment is then $f(s,$

$1 - s) - [s + (1 - s)r]W$, where $r > 1$ denotes the wage relativity (digging/shifting). The surplus is maximized when the difference in marginal social products between shifting and digging is equal to the difference in wage rates, $f_s - f_{1-s} = (1 - r)W$, and it is readily verified that the surplus maximizing value of s is a strictly increasing function of W.

10. This is also observed between subperiods 1 and 2, though this may partly reflect seasonality; we model seasonality explicitly in the next section.

11. From April 1987 to February 1990 the two variables are very highly correlated ($r = .996$ in levels, .989 in logs).

12. Data on labor attendance under the EGS and the EGS time wage were obtained from the Planning Department, government of Maharashtra.

13. Food grain output data disaggregated by *kharif* and *rabi* seasons are available only up to 1987–88. Since we are particularly interested in seeing how the model predicts labor attendances after mid-1988, we are unable to use the seasonally disaggregated data on food grain output.

14. Ideally, we also would like to include variables for the non–food grain output. We are, however, constrained by the nonavailability of data for the last 2 years of our sample period. Food grains nevertheless still account for about 70% of the gross cropped area in Maharashtra. See Government of Maharashtra, *Selected Indicators for Districts in Maharashtra and States of India, 1984–85* (Bombay: Directorate of Economics and Statistics, 1987), and *Selected Indicators for Districts in Maharashtra and States of India, 1986–87* (Bombay: Directorate of Economics and Statistics, 1989).

15. See Government of Maharashtra, *Report of the Fact Finding Committee on Regional Imbalance in Maharashtra* (Bombay: Planning Department, 1984).

16. The fact that we do not have a monthly series of the EGS wage rate means that there is measurement error in this variable, which will bias OLS estimates. We also estimated a generalized instrumental variables estimator, in which the EGS wage was not used as its own instrument. The instrumental variables included the average EGS wage rate of the previous year and the rank of the current year's EGS wage, as well as all other right-hand-side variables. The instrumental variables estimator gave very similar results in table 3, though with a slightly lower wage elasticity (0.15 rather than 0.20). In view of this, and the fact that we gain 11 degrees of freedom using the OLS estimator, we decided to stay with the latter. The model parameters also contain a dummy variable for December 1977. We believe there is an error in the data for this month. Excluding that variable had little effect on other parameters, though it did yield significant nonnormality of residuals, as indicated by the Lagrange Multiplier test.

17. As evident in fig. 1, the actual increase in the wage rate was staggered over June to September 1988, so starting the postsample forecasting in July (rather than June) is reasonable. This also coincides with the beginning of the new crop-year. While 1988 was not the first instance when piece rates of the EGS increased, that year had the largest increase. However, in the earlier cases we do not find any evidence of structural instability, as indicated by the evolution of recursive residuals since December 1978. Both the cumulative sum (CUMSUM) and cumulative sum of squares (CUMSUMSQ) statistics are well within the 95% confidence intervals; see R. L. Brown, J. Durbin, and J. M. Evans, "Techniques for Testing the Constancy of Regression Relations over Time," *Journal of the Royal Statistical Society B* 37 (1975): 149–92.

18. See M. H. Pesaran and B. Pesaran, *Microfit: An Interactive Econometric Software Package* (Oxford: Oxford University Press, 1987).

19. See C. W. J. Granger and P. Newbold, *Forecasting Economic Time Series,* 2d ed. (San Diego, Calif.: Academic Press, 1986).

20. To focus on any possible changes in the attendance-wage relationship in the postsample period we also estimated the following model over the entire period:

$$L_t = \underset{(4.458)}{4.73} + \underset{(22.14)}{0.8101}L_{t-1} - \underset{(2.786)}{0.2715}Q_t + \underset{(2.178)}{0.1866}(1 - DPS_t)W_t + \underset{(0.973)}{0.0816}\mathrm{DPS}_t \cdot W_t$$
$$- \underset{(1.476)}{0.00066}t + \text{other terms as in eq. (1)},$$

$$R^2 = 0.95;\ \mathrm{SEE} = 0.1115;\ \text{mean d.v.} = 13.0571;\ N = 167;\ \text{LM tests: autocorrelation (12)} = 0.704;\ \text{functional form (1)} = 5.66;\ \text{heteroscedasticity (1)} = 2.84;\ \text{normality (2)} = 1.273,$$

where *DPS* is a dummy variable taking the value one for the postsample period, and zero otherwise. Thus, a significant positive relationship between attendances and the EGS wage tends to vanish in the postsample period. This disappearance of the attendance-wage relation for a period when the wage itself increased appreciably is suggestive of there being some rationing of EGS employment.

21. For example, it has been said that even in normal times the scheme is less effective in reaching poor tribals in some districts, such as Thane.

22. Two types of forecasts can be made: (i) static ("one-step") forecasts, which are made 1 month ahead, and so are based on the actual values of the previous month's attendance count as well as the observed current values of all other variables in the postsample period, and (ii) dynamic forecasts, which do not assume that any attendance counts are known for the postsample period; unlike static forecasts, dynamic forecasts use the forecasted values of the previous months' attendances. The forecasts in fig. 2 are dynamic forecasts. These are our best estimates of the time series of attendances in the postsample period had there not been any change in the model that determines employment. Unlike static forecasts, dynamic forecasts do not build in the effects of any employment rationing since the increase in the minimum wage rate.

23. The wages are in real terms. The Consumer Price Index for Agricultural Laborers was used to deflate the nominal wages. The base date is April 1987.

24. These data are unpublished, though they are available from the Directorate of Economics and Statistics, government of Maharashtra, in the form of a fortnightly series of daily wages by gender for various agricultural operations for each of 75 monitoring points spread over rural Maharashtra. For our purposes, we have used the simple monthly mean of all observations of the wage rate for an agricultural operation as the estimate of the average agricultural wage rate for Maharashtra in each month.

25. The choice of these instruments can be justified within a partial adjustment framework (or error correction model) expressing the actual number of projects as a suitable function of its own lagged value and the current and lagged values of the desired number of projects, and where the latter is a function of the expected and unexpected components of desired EGS employment.

26. This particular policy response may well have increased the returns to EGS assets, insofar as, by restricting new projects, a higher rate of comple-

tion of existing works could be achieved. To the extent that the poor benefited from those assets, this effect would have mitigated any adverse effects on poverty.

27. The Durbin-Watson test on the OLS residuals from the static regression of real agricultural wage rate against real EGS wage rate is .86.

28. See R. F. Engle and C. W. J. Granger, "Co-integration and Error Correction: Representation, Estimation and Testing," *Econometrica* 55 (1987): 251–76.

29. The Engle-Granger methodology involves first testing for unit roots in each series and then (if unit roots are indicated) testing for stationarity in the OLS residuals from the static regression in the levels of the two series. We used the augmented Dickey-Fuller tests proposed by Engle and Granger. Unit roots were indicated in both the EGS and agricultural wage series. The first difference of the residuals from the static regression was then regressed against its own lagged values (first and second order) and the lagged level of the residual. The *t*-ratio on the latter was 1.06, which is well below the critical value (for larger sample sizes) given in Engle and Granger. The test was repeated using nominal wage rates and with both real and nominal wage rates in log form. In no case could the null hypothesis of no cointegration be rejected (*t*-ratios were all similar to the above figure). While it is recognized that these tests can lack power in small samples, the rejection of cointegration would still seem convincing.

30. Agricultural workers in these regions of India are widely thought to have little bargaining power in agricultural wage determination, and there is supportive evidence from estimates of the asymmetric Nash bargaining coefficient. See Gaurav Datt, "Wage and Employment Determination in Agricultural Labour Markets in India" (doctoral thesis, Australian National University, Canberra, 1989).

31. See M. Ravallion, "Market Responses to Anti-Hunger Policies: Effects on Wages, Prices and Employment," in *The Political Economy of Hunger*, vol. 2, *Famine Prevention*, ed. J. Dreze and A. Sen (Oxford: Oxford University Press, 1990).