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EXPLAINING THE RISE IN ANTEBELLUM
PAUPERISM: NEW EVIDENCE

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ABSTRACT

The 1850s witnessed one of the earliest “welfare explosions” in American history. During the decade the “pauper rate” -- the proportion of individuals receiving public assistance -- increased from 5.8 in 1850 to 10.2 in 1860, an increase of 76 percent. Previous attempts to explain the increase in antebellum pauperism have been hampered by the available published data, which are too aggregated to be of much use.

This paper explores the determinants of antebellum pauperism using previously unexploited archival data drawn from the manuscript censuses of social statistics. These records provided detailed evidence on the incidence of pauperism at the county level. We find that about half of the increase in pauperism can be attributed to falling real wages during the decade. Contributing factors were increased immigration and urbanization.

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1. Introduction

The 1850s were a decade of aggregate economic growth in the United States. According to Robert Gallman's unpublished estimates, real per capita income increased by 24.5 percent between 1850 and 1860.¹ Frontier states grew rapidly as population migrated westward in pursuit of economic gain. Railroad mileage increased by 360 percent over the decade, facilitating an enormous expansion in internal trade (U.S. Secretary of the Interior 1866, p. 331; Taylor 1964).

Despite evidence of aggregate growth, certain population groups evidently experienced economic distress during the 1850s. Among these were certain non-farm workers, particularly artisans, located primarily in urban areas in the Northeast (Fogel 1989; Ferrie 1996). Recent estimates suggest that real wages of non-farm workers were stagnant in the 1850s, and may have even declined (Williamson and Lindert 1980; Margo 1992). Fueled in part by rising immigration, rapid urbanization fostered a host of attendant ills; nutritional status, morbidity, and mortality also may have worsened over the decade (Margo and Steckel 1983; Komlos 1987).

Yet another sign of economic distress was an increase in the incidence of public "pauperism". The 1850 census was the first to report the total number of persons receiving poor relief from public funds during the census year, as well as the number so supported on June 1. According to the census, the total pauper

rate (the total number receiving public poor relief during the census year per thousand persons) was 5.8 in 1850. By 1860 the total pauper rate had increased to 10.2, or by 76 percent.²

This paper uses previously untapped archival data to study the determinants of public pauperism during the late antebellum period. The data are derived from the manuscript censuses of social statistics of 1850 and 1860, which have been little used by economic historians. These data allow us to extend previous work on antebellum poor relief in two ways. First, because the data are disaggregated below the state level (unlike the published census data), they allow analysis of geographic variation in pauperism.³ By analyzing geographic variation we can pinpoint, to a greater extent possible than with published data, where the increase in pauperism took place during the 1850s.

Second, we explore the cross-sectional determinants of pauperism. Although the results are preliminary, they suggest the evolution of real wages and pauperism over the decade were not independent dynamics. Areas that experienced real wage declines suffered from disproportionate increases in pauperism. Immigration contributed to the increase in overall pauperism (because the foreign born were more likely to receive assistance) and there is some evidence that native pauper rates in urban areas increased with the influx of foreign-born.

2. Institutional Background

Antebellum poor relief was a diverse affair. This diversity reflects the local nature of the English poor law, upon which American poor relief was based. In England, churches and monastic orders had played large roles in relief provision; the administration of English poor relief in the nineteenth century, which functioned on the parish level, was one reflection of this influence (Trattner 1994, pp. 4-6). The growing civil bureaucracy of the Tudor and Elizabethan periods absorbed the church's role in poor relief, resulting in the 43rd Elizabeth (1601), the legislative foundation for English, and later colonial and American, public poor relief.

As in England, localities administered American poor relief. American colonial statutes preserved the family responsibility for the needy, with public relief available for those unable to care for themselves, or whose families were unable to do so. Virginia's legislation dates from 1646, Massachusetts' from 1692.⁴ In the colonial period, the most common forms of relief were to have the impoverished live with each family in town for a portion of the year, and to abate taxes or tithing.

By the nineteenth century provision of relief included the poorhouse ("indoor" relief), and cash or in-kind transfers ("outdoor" relief). Public and charity officials favored poorhouses, on the belief that outdoor relief had more severe disincentive effects on work effort. Trattner (1994, p. 58)

highlights the incentive problem, as well as the moral tone in which observers cast the relief debate:

Outdoor aid, especially public relief, only aggravated the problem. "The more paupers you support, the more you will have to support," claimed one citizen . . . By encouraging the poor to rely upon the public dole rather than upon their own energies, and by removing the dread of want, considered by many to be the prime mover of the needy, the poor laws destroyed the incentive to work, causing the poor to become even more idle and improvident.

While poorhouses existed as early as 1664, they were not strongly advocated until the 1820s, when local authorities in New England and the Mid-Atlantic states perceived an increase in pauperism and expressed optimism about the recent American experience with almshouses (Katz 1986, p. 16).

As westward expansion took place, the "problem" of relief went with it. As in New England, localities were responsible for providing relief. Whereas in New England, relief tended to be provided at the township level, county governments were generally charged with the responsibility in other states, on the belief that difficulties in determining "settlement" (that is, which locality was responsible for providing relief, on the basis of residence) were lessened at the county level.⁵

3. Data

The data analyzed in this paper derive principally from the 1850 and 1860 manuscript censuses of social statistics. As part of the enumeration effort in both years, census marshals were

instructed to collect information for minor civil divisions on the total number of recipients of poor relief (foreign and native); the number receiving poor relief on June 1 (native and foreign); and total expenditures on poor relief. At the time the census was compiled, the data were aggregated to the state level, and state averages were reported in the published volumes of the 1850 and 1860 censuses. Economic historians have relied on these state averages in previous work (for example, Lebergott 1976). In addition to the data on poor relief, the social statistics censuses also report estimates of wages for various occupations, including common labor without board, which we use in our analysis of the determinants of pauperism and of relief spending (see below).

The original manuscripts of the social statistics survive for some states and microfilm copies for some of these are available at the National Archives or from various state archives. In this paper we make use of microfilms from eight states, two from each census region: Pennsylvania and Massachusetts (Northeast); Michigan and Iowa (Midwest); North Carolina and Virginia (South Atlantic); and Kentucky and Tennessee (South Central). The selection of states was dictated by the availability of microfilm copies of the census manuscripts, as well as a desire to have a geographically dispersed sample of counties.

The instructions to census enumerators specified that social statistics were to be collected for each minor civil division

(MCD) of every county, or townships (in New England) "as far as practicable" (DeBow 1854, pp. xxiv). Inspection of the microfilms suggest that this goal was broadly achieved, in the sense that virtually all counties reported data for at least one MCD. Sometimes the reported MCD names correspond to known geographic entities (for example, wards in Philadelphia) but in other cases they do not (for example, a census marshal might refer to an MCD as "my division"). Because of this problem (along with illegibility of some MCD names) we have chosen to aggregate all data to the county level.

Other than commenting that information was "not to be ascertained entirely by personal inquiry of individuals, but in part from public records and reports, and public offices" the census was silent on the exact procedures marshals were to follow in collecting the information on poor relief (Debow 1854, p. xxiv).⁶ Unfortunately, none of the microfilms contain marginalia on data collection procedures. However, "public records" probably provided most or all of the relevant figures, on the assumption that individuals chosen randomly by the census enumerators would presumably be poorly informed about the number of persons on relief or the amount spent on relief.

The problems with the data are numerous. No information is available on the type of relief (outdoor versus indoor). As noted in section 2 (see also Hannon, 1996) use of the poorhouse tended to restrict pauperism -- the poor preferred outdoor to indoor relief. Direct information on the average duration of

relief (for example, in days or months) was not reported by the census. Lebergott (1976), however, suggested that the number of recipients on June 1 was a proxy for the "long-term" relief population. The implication of Lebergott's assertion is that the remaining recipients (Total recipients - June 1 recipients) can be thought of as "short-term" and, therefore, an increase in the proportion of "short-term" recipients is an indication that the average duration of relief was falling (vice versa for a decrease in the proportion of short term recipients). The appendix develops a (crude) methodology to infer the average duration of relief from the available evidence, and our estimates of average duration (see the appendix) are consistent with Lebergott's interpretation.

Second, many MCD's, particularly in frontier areas (for example, Iowa in 1850) simply left the relevant columns blank, which we have interpreted as true zeros. Upon aggregating to the county level, non-reporting is reduced but not eliminated. However, once the data are weighted by appropriate county-level population counts, the economic significance of "zeros" is greatly reduced. Consequently, all the numerical estimates reported in this paper are weighted by the relevant county-level population.⁷

Third, it is possible that the non-reporting MCDs (or counties) are not true zeros, or more generally, that the census enumerators under-reported the number of relief recipients (particularly of the "short-term" variety, as defined above).

Hannon (1984a), for example, argued that the census underenumerated relief recipients in antebellum New York (see also DeBow 1854). While a detailed analysis of under-reporting is beyond the scope of this paper, if under-reporting were random, it should not affect our econometric analysis of the determinants of pauper rates, except in reducing fit of the regressions.

Table 1 reports sample statistics, which we use to perform a decomposition analysis of the change in aggregate (in the sample) pauper rates between 1850 and 1860. Before performing the analysis we divide the sample counties into two groups, "urbanized" and "non-urbanized". Urbanized counties are those that contained at least one town or city of population greater than 10,000. The remainder are designated non-urbanized.

Consistent with the published census aggregates, the overall pauper rate increased between 1850 and 1860 from 4.6/1000 to 7.5/1000, although the levels of pauperism in the sample are below (in both years) the aggregate levels reported in the published census. The impact of 1850s immigration is visible in the increase in the proportion of foreign-born between 1850 and 1860. Although the rural foreign-born increased their population share during the decade, overall the proportion of the population living in urbanized counties rose over the decade, by 7.3 percentage points. Short-term relief appears to have increased during the decade, suggesting that while pauperism rose overall, the duration of relief declined.⁸

The table also documents several stylized facts of antebellum pauperism. Pauperism was much higher, in general, among the foreign born (Axinn and Levin 1982). However, the duration of relief appears to have been shorter, on average, among foreign-born recipients, as indicated by the higher proportions of short term recipients. Pauperism was much higher in urbanized counties than in non-urbanized counties. The gaps in pauperism between the native and foreign born were larger in absolute value in urbanized counties, suggesting that the urbanized foreign-born may have been more prone to the sort of distress that led to reliance on public assistance.

Finally, the table presents evidence on relief expenditures per capita (of the total population). These have been deflated (in each county) by the weekly cost of board, and so should be thought of as index numbers, rather than absolute dollar amounts. Relative to the cost of board, urbanized counties spent more per capita than non-urbanized counties, a reflection of the higher incidence of pauperism in urban areas. Overall, per capita spending rose by about 29 percent [= $1 - (0.081/0.063)$] between 1850 and 1860, a consequence of rising expenditures on average in both urbanized and non-urbanized counties, and a shift in population towards urbanized counties.

We use the figures in Table 1 to compute a decomposition of the change in the average pauper rate (2.9 percentage points = $7.5/1000 - 4.6/1000$) between 1850 and 1860.⁹ The decomposition reveals that about 28 percent of the increase in pauperism can be

attributed directly to increases in urbanization and the foreign-born share during the 1850s. Thus, had there been no change in pauper rates among the native and foreign born, urban or non-urban, during the 1850s, the aggregate pauper rate in 1860 would have increased from 4.6 (its 1850 value) to 5.4, simply because of shifts in composition.

The bulk of the increase in pauperism in the 1850s, however, can be attributed to a greater incidence of pauperism within groups. By itself, the increase in incidence within groups would have raised the aggregate pauper rate to 6.2/1000 in 1860. Although in absolute terms the increase in foreign pauper rates in urbanized counties was the largest, the increase in urban native pauper rates accounted for the greatest share (28 percent) of the increase in aggregate pauperism, simply because the urban native share of the total population was much larger (for example, six times larger in 1860) than the urban foreign share.

4. Econometric Analysis

In this section we report on a preliminary econometric analysis of the data discussed in the previous section. In particular, we report estimates of weighted least square regressions of pauper rates, the proportion of short-term recipients, and (deflated) per capita expenditures on relief.

The pauper rate and short-term relief regressions can be thought of as reduced forms of a model of the "demand" for

relief, and its "supply" (Hannon 1996). Both demand and supply are determined at the local government level, the unit primarily responsible for the administration of public relief during the antebellum period.¹⁰ We think of "supply" as the outcome of an maximization process in which antebellum communities (via representative government) levy local taxes to fund a socially optimal level of public relief. The exact motives for supplying relief are left unspecified, although at some level we presume that taxpayers are altruistic towards the poor (public relief is a normal good) and that the level of relief available from private sources, while positive, may not be socially optimal.

Taxpayer altruism has limits, however. First, holding constant the average duration of relief in (say) days, the generosity of taxpayers (measured by the amount spent per recipient per day) varies inversely with the number of relief recipients. Second, holding constant the number of recipients, the greater the average duration, the less generous is relief per day. These two tradeoffs will arise in any model in which spending on the poor has diminishing marginal utility (to taxpayers, if taxpayers are altruistic) and taxable resources are limited.¹¹

The "demand" for public relief has two components, the number seeking relief and the average duration. In general, the more generous is relief per day, the greater the number seeking relief and (possibly) the average duration.¹² The demand for public relief also depends (negatively) on the availability of

private relief (see Kiesling 1996), on demographic structure (for example, the proportion foreign-born), and on various characteristics of the local economy. For example, if the demand for low-wage labor fell -- for example, during an general economic downturn -- we would expect the demand for relief to increase.

In principle, the equilibrium of such a model would yield structural equations for the number of recipients (normalized by the relevant population, for example, the native or foreign born), the average duration of relief, and generosity (the amount spent per recipient per day). Even if we could identify the structural equations, we cannot estimate them because the census did not report the amount spent per recipient per day.¹³ Thus, we imagine solving the structural equations for the reduced forms. Following the discussion in the previous section (see also the appendix), we proxy duration by the proportion of short term recipients. We also report regressions of per capita relief expenditures, although these are not, strictly speaking, the true reduced forms.¹⁴

At present, the independent variables included in the regressions are: CHPC, the number of churches per capita; PFOR, the percent foreign born in the county; RWPC, per capita assessed wealth, deflated by the weekly cost of board; RWAGE, the full-time annual wage of common labor without board, again deflated by the weekly cost of board; and, in the non-urbanized regressions, a dummy variable, TOWN, indicating the presence of a town of

population 2,500 to 10,000.¹⁵ Also included (but not reported) are a full set of state dummies and a dummy for 1860 observations.¹⁶ This list of independent variables is incomplete, and so the results should be viewed as tentative.¹⁷

We find little evidence that the presence of churches had significant effects on pauperism or expenditures (except possibly in non-urbanized counties) and, with the exception of the foreign born in non-urbanized counties, on duration. A priori, we expected that the presence of churches was a proxy for the availability of private relief. Recent research on nineteenth century poor relief suggests that the poor viewed public relief as a last resort, and that public and private relief were substitutes (Kiesling 1996; Ziliak 1995). If the "last resort-cum-substitute" effects are dominant, we would expect to see negative relationships between CHPC and pauperism, and between CHPC and expenditures. On the other hand, the presence of a large number of churches per capita may indicate a greater willingness to assist the poor in general, which might produce positive coefficients in the pauper and expenditure regression, and also might be responsible for the negative effect on short-term relief in the non-urbanized foreign regression (recall that a decline in short-term relief is equivalent to an increase in duration). The fact that the majority of the coefficients are insignificant suggests that these two opposing forces mostly cancelled each other out.

We find weak evidence that increases in the share of foreign

born in the local population were associated with higher native pauper rates in urbanized counties, and stronger evidence that a higher foreign born share led to rising expenditures in non-urbanized counties. The positive effect of the foreign born share on urban native pauperism is consistent with recent research suggesting that immigration in the late 1840s and throughout the 1850s hurt some native born workers (Fogel 1989; Ferrie 1996). Immigrants who moved to rural areas may have exhausted their savings in doing so and, lacking the private support network found in large cities, may have been more prone to request public assistance.

In general, increases in real per capita wealth were positively associated with pauper rates and per capita real expenditures; and, in urbanized counties, with significant increases in short-term relief. A plausible explanation is that public relief was a normal good and thus more readily available in wealthier counties. The positive effect on short-term relief in urbanized counties may be due to a greater tendency to use outdoor relief, which is known to have been favored by the poor (compared with the poorhouse), particularly in situations of short-term distress (Hannon 1984b).

Except in the case of the foreign born in non-urbanized counties, an increase in real unskilled wages produced decreases in pauperism. The inverse link between pauperism and real unskilled wages is important, because it was low-wage labor that was presumably at greatest risk of falling into pauperism, and

because the 1850s was a decade of stagnant or perhaps even falling, real wages, particularly in urban areas (Williamson and Lindert 1980; Margo 1992). Among the sample urbanized counties, real wages fell between 1850 and 1860, and this decline is of sufficient size to explain 47 percent of the rise in foreign pauperism and 32 percent of the rise in native pauperism in urbanized counties.¹⁸

Controlling for other factors, real wages were positively associated with per capita expenditures in both urbanized and non-urbanized counties (although the link was insignificant in urbanized counties). These positive coefficients appear consistent with Lebergott's (1976) observation that communities judged the adequacy of their relief efforts in light of average unskilled labor income.

Finally, we find that, after controlling for other factors, pauperism and expenditures were lower in completely rural counties -- that is, the presence of even a small town was associated with higher pauper rates and per capita relief. A possible explanation is that, within such counties, a town was the natural location for the county poorhouse, or more generally, a "central place" location may have facilitated the administration of local relief.

4. Conclusions

Our preliminary analysis of data on antebellum pauperism in

the 1850s reveals that pauper rates increased dramatically in the 1850s, particularly in urban areas, and partly as a consequence of economic distress as reflected in stagnant (or falling) real wages. Increased immigration and urbanization also played a role in the surge in pauperism. Although the overall incidence of pauperism was greater in urbanized than in non-urbanized counties, the average duration of relief was shorter in urbanized counties. Average duration appears to have fallen in the 1850s, softening somewhat the impact of rising pauper rates. Future work will concentrate on expanding the list of independent variables in the regressions, in order to better pinpoint the causes of antebellum pauperism.

6. Appendix: Estimating the Average Duration of Relief

In this appendix we develop a methodology for using the census data to estimate the duration of relief. Only very limited information has been available on the duration of relief in the nineteenth century. Using a similar method, Hannon (1984, 1996) provides estimate of the average number of weeks on relief in New York state beginning in the early 1820s, while Ziliak (1995) has used archival data to estimate the length of complete spells of relief in Indianapolis in the late nineteenth century. To our knowledge, however, there are no estimates for the mid-nineteenth century that cover as broad a geographic range as our sample.

Our estimates are derived from the following equation:

$$e = e^d * D_{LTN} * SLTN + e^d * D_{STN} * SSTN + e^d * D_{LTF} * SLTF + e^d * D_{STF} * SSTF \quad [1]$$

where:

e^d = expenditures per recipient per day

D_{ij} = average duration of relief, in days, $i = LT$ (long-term), ST (short term), $j = N$ (ative), F (foreign)

S_{ij} = share of relief type in total recipients, $i = LT, ST$, $j = N, F$

The data contain no direct information on e^d . In place of direct information, we make the assumption that:

$$e^d = \beta * (\text{weekly cost of board} / 7) \quad [2]$$

That is, the generosity of relief is a constant fraction of the daily cost of food. Because the data on board refer to the cost of food to "laboring men" (as the census phrased it), setting $\beta = 1$ implies that relief per day was sufficient to feed one adult male recipient per day.

It is likely that $\beta = 1$ is too high. While adult males certainly were recipients of relief during the antebellum period, it is unlikely that they accounted for more than 25-30 percent of total recipients, if data for New York State are any indication (see Hannon 1984b, p. 1015).¹⁹ In the case of almshouse

recipients, an informed estimate of β is possible for New York state ca. 1850: $\beta = 0.42$.²⁰ However, as Hannon (1984) notes, the bulk of antebellum relief was outdoor relief, and it is plausible that outdoor relief was more costly than indoor relief.

In any case, the social statistics do not distinguish between outdoor and indoor relief, nor do they give information on the age and sex structure of relief recipients. If almshouse recipients accounted for 20 percent of total recipients (for whom $\beta = 0.4$), while the remainder were mostly adults and their offspring (for whom we assume $\beta = 0.9$, on average), a reasonable (aggregate) estimate of β is 0.8.²¹

Setting $\beta = 0.8$ in [2], substituting [2] into [1] and rearranging terms (because, for example, $SLTN = 1 - SSTN - SSTF - SLTF$), we estimate [1] using weighted least squares (the weights are the county population totals; also note that the equation does not have an intercept term), pooling the data across the two years. Separate regressions are estimated for urbanized and non-urbanized counties. The coefficients from these regressions are the estimates of average duration in days, which we convert to months for facilitate comparisons with previous research.

The limitations of this methodology are severe. Because our estimated durations cannot (by definition) exceed 12 months, they are biased downward relative to estimates based on complete spells (see Hannon 1984b for a similar argument). If e^d varied between short and long-term recipients (as we have defined them) or between the native and foreign born, our assumption that e^d is

the same for all groups within a county will bias the estimates of duration. The assumption that β is the same for all counties is almost certainly inconsistent with the optimization model discussed in Section 3. It is possible to relax this assumption by introducing interaction terms (that is, letting β be a function of covariates) but we have yet to do so in a systematic way.²² The estimates of duration appear in Appendix Table 1.

On average, the duration of relief in urbanized counties was 4.1 months, while average duration in non-urbanized counties was 8.0 months; the overall average was 6.8 months. The differences in duration between urbanized and non-urbanized counties were primarily due to the greater incidence of short-term relief in urban areas, as well as somewhat shorter duration among long-term native-born recipients. In the aggregate, the duration of relief among foreign born recipients was considerably shorter (3.8 months) than among native-born recipients (7 months), because the foreign born were more likely to live in urbanized counties, and the relative incidence of short-term relief (that is, compared with long-term relief) was greater among the foreign born than the native born.

The estimates of duration are broadly consistent with general descriptions of antebellum relief and previous studies. Perhaps the most important implication is that the distinction between short term and long-term recipients, made by Lebergott (1976) and accepted by us, appears to be reasonable. That is, one could characterize antebellum paupers as comprising two groups--

a "permanent" relief population that needed care for relatively lengthy periods of time, and another group that needed occasional relief. The duration of short-term relief in non-urbanized counties -- between 3 and 5 months -- suggests that seasonality in labor demand may have played a recurring role. The much briefer duration of short-term relief in urban areas, however, suggests a population widely at risk of income shortfall, generally able to take care of their subsistence needs, but with a fairly high probability of needing temporary assistance at least once a year.

Although the paucity of estimates makes comparisons hazardous, the durations in Appendix Table 1 seem consistent numerically with previous studies of the antebellum period. Hannon (1984b) that the average duration of relief in New York City in the early 1820s was about 5 months, while outside New York the duration was 9 months; in the state as a whole, the average duration was 7 months. Ziliak's (1995) recent study of relief in late nineteenth century Indianapolis pegs the average length of a complete spell at 8 months, but this may be a better estimate of duration among long-term recipients than the general relief population.²³ Ziliak argues that the average duration of relief has not changed very much over the course of the nineteenth and twentieth centuries, and our estimates are consistent with his interpretation.

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Notes

1. Gallman, personal correspondence. We divided Gallman's GNP estimates (in constant 1860 dollars) for 1849 and 1859 by the federal census count of population in both years to derive the growth rate in the text.
2. The number of paupers was computed from DeBow (1854, p. 163) and Secretary of the Interior (1866, p. 512). All population figures are from U.S. Department of Commerce (1976).
3. Hannon (1984a, 1984b) examined geographic variation in pauperism but only for a single state, New York.
4. See Trattner (1994, pp. 4-6). New York (then New Amsterdam) provides a marked contrast to the evidently secular foundations of poor relief in the English colonies. New Amsterdam established an explicitly ecclesiastical relief system, with officers of the Dutch Reformed Church assuming responsibility for soliciting voluntary contributions.
5. For example, Pennsylvania adopted a county system, which then became the basis for poor relief in states carved out of the Northwest Territories.
6. Specific instructions for collecting the wage data amounted to the statement that the "information called for in the six columns is so simple and so plainly set forth in the headings that it is deemed unnecessary to add thereto" (DeBow 1854, pp. xxv).
7. Thus, for example, in computing the mean foreign pauper rate, we weight by county level foreign-born population; analogously, in the case of native-born.

8. Using the estimates of average duration in the appendix, and the weights (urban and foreign born shares) in Table 1, the mean duration of relief fell from 7.2 months to 6.3 months between 1850 and 1860.

9. By definition (using the symbols in Table 1):

$$p = \alpha_{FU}p_{FU} + \alpha_{FR}p_{FR} + \alpha_{NU}p_{NU} + \alpha_{NR}p_{NR}$$

Thus the change in the pauper rate (dp) between 1850 and 1860 can be decomposed into two components. The first component holds constant the group-specific pauper rates (for example, p_{FU}) at their 1850 values and allows the α 's to change. We refer to this first component as a "compositional" shift, since it explains the proportion of the change in p that is due to shifts in distribution of the population over time. The remainder of the change in p reflects changes in the group-specific pauper rates (for example, p_{FR}).

10. We assume in our econometric work that decisions are made at the county level although, strictly speaking, this is incorrect for Massachusetts, where decisions were made at the township level. We have yet to collect township level data for Massachusetts.

11. As Lebergott (1976) and many others have noted, one essential reason for diminishing marginal utility is the desire to limit shirking on the part of the working poor. Thus, according to Lebergott, throughout American history expenditures per recipient

have always been set at a relatively low fraction of earnings of low-wage labor. In particular, Lebergott defined the "generosity" of poor relief to be the average annual amount spent per recipient as a fraction of the full-time annual wage of unskilled labor (= 311 x average daily wage of common labor without board in the antebellum case). According to Lebergott, generosity so defined has remained roughly constant over time (at about 0.3, or 30 percent). See, however, Hannon (1984a) for an argument that generosity declined in New York State between 1820 and 1860.

12. It is debatable on a priori grounds whether the demand for relief was endogenous in the sense implied by the text. The typical antebellum county, for example, placed severe limits on the ability of the poor to migrate from one county to the next in search of more generous relief, as well as upper limits on the amount of outdoor relief that could be dispensed (Hannon 1996). On the other hand, nothing in the usual residency requirements would seem to preclude residents from seeking relief if it became more generous.

13. In the methodology for estimating duration in the appendix, we assume that the amount spent per day was a constant fraction of the daily cost of board. As noted in the appendix, this assumption is, strictly speaking, inconsistent with the optimization model discussed in the text (although the assumption can be relaxed; see the appendix). While we believe the appendix results to be sufficiently robust to support Lebergott's (1976) contention that June 1 recipients were long-term and the other

recipients were (on average) short term, the methodology is not designed to produce estimates of expenditures per day to be used in the analysis of a full-blown structural model.

14. Per capita expenditures on relief are the product of expenditures per recipient per day, the average duration in days, and recipients per capita. That is, the dependent variable in the expenditure regression is a non-linear function of the endogenous variables, and thus the regression is not a true reduced form (in terms of the model).

15. By "full-time" we mean the daily wage of common labor, without board multiplied by 311 days (see Lebergott 1976).

16. The reason for including state dummies rather than county-level dummies (as in a fixed effects regression) is that the panel structure is unbalanced; that is, there are counties in 1860 not present in 1850. Going to a fixed effects regression would mean dropping additional observations and also raises the problem of modelling county formation.

17. In particular, we plan to add more demographic variables (for example, the age structure) as well as indicators of the extent of market activity (for example, per capita home manufactures), following Hannon (1984b).

18. In the units that we measure them, urban real wages fell, on average, by 5.94 between 1850 and 1860. Multiplying 5.94 by the relevant coefficients from the pauper regressions in Table 2, and expressing the result in per thousand's, produces predicted increases of 5.7/1000 in the urban foreign pauper rate and

1.4/1000 in the urban native pauper rate. According to Table 1, the urban foreign pauper rate increased by 12.1/1000 and the urban native pauper rate increased by 4.4/1000, between 1850 and 1860. Thus the fall in real wages can explain approximately 47 percent ($= 5.7/12.1$) of the increase in urban foreign pauperism, and approximately 32 percent ($= 1.4/4.4$) of the increase in urban native pauperism.

19. Even assuming that the cost of board for adult women was the same as for adult men, the share of able-bodied adult recipients in New York was between 60-72 percent in the 1850s (Hannon 1984b, p. 1015). Adding in the elderly and disabled would raise the adult percentage to approximately 80 percent.

20. Hannon (1984, p. 1017) provides estimates of the weekly cost of almshouse relief in New York state (excluding New York City) for 1845-49 (68 cents per week) and 1850-54 (89 cents per week). These estimates are in 1860 dollars. Averaging the two gives 78.5 cents per week, which we assume applies to 1850. We obtained a microfilm copy of the 1850 social statistics manuscripts for New York state (most of the 1860 manuscripts, unfortunately, are missing). Excluding New York city, the average weekly cost of board (in 1860 dollars) was \$1.88, which implies a β of 0.42 ($= 0.785/1.88$).

21. It should be noted that the estimates of D_{ij} are an implicit check on the reasonableness of the choice of β . As pointed out in the text, the D 's cannot exceed 12 months (assuming the census data accurately reflect a full year's expenditures, and no more).

Setting β to a lower number than 0.8 will produce longer durations; in particular, setting $\beta = 0.6$ produces an estimated duration exceeding 12 months for long-term foreign born recipients in urbanized counties.

22. We have, however, tested whether the average durations of long-term and short-term relief changed between 1850 and 1860, but found no evidence that such changes were economically or statistically significant.

23. Ziliak argues that his data oversample seasonal relief cases, since he begins tracing individuals who entered the Indianapolis relief rolls during the first quarter of 1881. However, the proportion of short-term recipients among total recipients over the course of a year (the relevant period to compare with the census data) may be understated in Ziliak's sample, since persons entering the rolls in, say, September, but leaving in October would be missed. Thus Ziliak's estimate may better represent the mean duration of an (approximate) cross-section of relief recipients, which could be biased upwards relative to duration among all recipients. On the other hand, our estimates of aggregate duration are biased downward since our method, by definition, truncates the duration of long-term relief at a maximum of 12 months.

Table 1: Sample Statistics

| | 1850 | 1860 |
|------------------|-------|-------|
| α_{FU} | 0.046 | 0.064 |
| α_{FR} | 0.030 | 0.039 |
| α_{NU} | 0.186 | 0.240 |
| α_{NR} | 0.739 | 0.657 |
| P_{FU} (/1000) | 32.8 | 44.9 |
| P_{FR} | 11.2 | 14.6 |
| P_{NU} | 6.1 | 10.5 |
| P_{NR} | 2.2 | 2.3 |
| $\%ST_{FU}$ | 0.842 | 0.851 |
| $\%ST_{FR}$ | 0.541 | 0.651 |
| $\%ST_{NU}$ | 0.554 | 0.714 |
| $\%ST_{NR}$ | 0.218 | 0.358 |
| p | 4.6 | 7.5 |
| $\%ST$ | 0.319 | 0.486 |
| Exp_U | 0.110 | 0.133 |
| Exp_R | 0.053 | 0.056 |
| Exp | 0.063 | 0.081 |

α = sample proportion, F=foreign, N=Native, U=Urbanized, R=non-urbanized

p = sample pauper rate

$\%ST$ = (Total recipients - June 1 Recipients)/Total Recipients

Exp = [(Expenditures on Poor Relief)/(Population)]/Weekly Cost of Board

Urbanized counties contain at least one town or city of population greater than 10,000.

Source: eight-state sample of manuscript censuses of 1850 and 1860 censuses of social statistics; see text

Table 2: Weighted Least Square Regressions

A. Pauper Rates

| | Urbanized | | Non-Urbanized | |
|------------------------------|-------------------|-------------------|-------------------|-------------------|
| | Foreign | Native | Foreign | Native |
| Constant | 0.044 (0.671) | 0.005 (0.377) | -0.003 (0.340) | 0.003 (5.759) |
| TOWN | | | 0.008 (3.448) | 0.001 (4.836) |
| CHPC | 26.075 (1.446) | 5.234 (1.330) | 0.451 (0.281) | -0.110 (0.947) |
| PFOR | 0.015 (0.173) | 0.031 (1.627) | -0.014 (1.401) | -0.002 (1.057) |
| RWPC x 10 ⁻³ | 0.590 (7.889) | 0.128 (7.431) | 0.021 (1.513) | 0.004 (4.476) |
| RWAGE x 10 ⁻³ | -0.953 (2.874) | -0.242 (3.795) | 0.091 (2.188) | -0.008 (3.009) |
| Sample mean | | | | |
| dep. var. x 10 ⁻¹ | 0.406 | 0.089 | 0.140 | 0.023 |
| R ² | 0.688 | 0.642 | 0.066 | 0.232 |

B. Percent Short Term

| | | | | |
|------------------------------|-------------------|-------------------|-------------------|-------------------|
| Constant | 0.725 1.608) | 0.496 (1.085) | 0.216 (1.314) | 0.364 (5.947) |
| TOWN | | | -0.018 (0.335) | 0.010 (0.408) |
| CHPC x 10 ² | 0.058 (0.054) | 0.719 (0.578) | -1.170 (2.860) | -0.183 (1.162) |
| PFOR | -0.199 (0.375) | 0.924 (1.397) | 0.308 (1.255) | 0.280 (1.225) |
| RWPC x 10 ⁻³ | 0.848 (3.141) | 1.268 (3.762) | -0.027 (0.087) | -0.054 (0.601) |
| RWAGE x 10 ⁻² | -0.064 (0.305) | -0.239 (1.096) | 0.374 (4.608) | -0.006 (0.198) |
| Sample mean | | | | |
| dep. var. x 10 ⁻¹ | 0.850 | 0.674 | 0.650 | 0.314 |
| R ² | 0.596 | 0.580 | 0.136 | 0.215 |

C. Real expenditures per capita

| | Urbanized | Non-Urbanized |
|----------|--------------------|------------------|
| Constant | 0.030 (0.384) | 0.034 (4.723) |
| TOWN | | 0.012 (4.190) |
| CHCP | -26.165 (1.082) | 2.672 (1.612) |
| PFOR | 0.093 (0.808) | 0.057 (2.495) |

Table 2 (continued)

| | | |
|--------------------------|------------------|------------------|
| RWPC x 10 ⁻⁴ | 1.380 (1.340) | 0.839 (6.850) |
| RWAGE x 10 ⁻³ | 0.225 (0.562) | 0.065 (1.826) |
| Sample mean dep. var. | 0.124 | 0.055 |
| R ² | 0.608 | 0.353 |

Weights are year-specific counts of the relevant population; for example, the weight for the urban foreign pauper regression is the foreign born population in the county. Regressions also include state dummies and a dummy for 1860 observations. Absolute value of t-statistics in parentheses. CHCP: churches per capita; PFOR = foreign-born population/total population; RWPC = (per capita assessed wealth)/weekly cost of board; RWAGE = (311 * average daily wage of common labor with board)/weekly cost of board; TOWN = 1 if county contains town with population between 2,500 and 10,000, 0 otherwise.
Source: see text

Appendix Table 1: Average Duration of Antebellum Relief, in Months

| | Urbanized | Non-Urbanized |
|---------------------|-----------|---------------|
| Long-term, Native | 6.9 | 9.0 |
| Short-term, Native | 2.3 | 5.3 |
| Long-term, Foreign | 11.6 | 11.3 |
| Short-term, Foreign | 0.4 | 3.3 |
| Aggregate | 4.1 | 8.0 |

Source: see text