Volume Title: The Black Youth Employment Crisis

Volume Author/Editor: Richard B. Freeman and Harry J. Holzer, editors

Volume Publisher: University of Chicago Press

Volume ISBN: 0-226-26164-6

Volume URL: http://www.nber.org/books/free86-1

Publication Date: 1986

Chapter Title: Appendix: NBER-Mathematica Survey of Inner-City Black Youth: An Analysis of the Undercount of Older Youths

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Chapter URL: http://www.nber.org/chapters/c6293

Chapter pages in book: (p. 443 - 459)

Appendix: NBER-Mathematica Survey of Inner-City Black Youth: An Analysis of the Undercount of Older Youths

John Bound

The NBER-Mathematica Survey of Inner-City Black Youth was intended to be a random sample of youths from low-income, predominantly black areas of three cities.[1] Yet older youths are poorly represented in the survey, with half as many in their twenties as in their teens. Comparisons of population counts between the NBER and 1980 census data for targeted areas suggest that this difference is the result of the NBER survey having undercounted older youths. This appendix examines why the undercount may have occurred and evaluates its possible biasing of the results presented in this volume.

The Undercount

Table A.1 reports population counts from the NBER survey and from the 1980 Census for Boston, Chicago, and Philadelphia. The census data include counts for both the central cities and for the particular poverty tracts targeted by NBER. There are noticeable differences between the NBER and the census data on the age distribution, with the NBER having six to seven percentage point greater numbers of younger youths and commensurately fewer older youths. This apparent undercount of older youths naturally raises doubts about the representativeness of the survey, particularly regarding the older youths.

We should not be too surprised that the NBER survey undercounted older youths. Typically, the Census has had the most trouble enumerating working-aged black men. The Census estimates its own undercount of 20- to 24-year-old black men to have been over 12 percent in 1960 and 1970 and over 7 percent in 1980.[2] The Census does not

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Table A.1 **Comparison of Age Distributions of Young Black Men: in the NBER Survey and the 1980 Census, by City and Age**

Age	Chicago			Boston			Philadelphia		
		Census			Census			Census	
	NBER	Poverty Tracts[a]	City[b]	NBER	Poverty Tracts[a]	City[b]	NBER	Poverty Tracts[a]	City[b]
16–17	36.9	30.2	25.6	35.1	25.8	.246	32.5	25.9	35.0
18–19	23.9	23.6	23.3	26.8	24.9	.246	24.0	26.1	19.9
20–21	16.6	21.1	21.8	17.3	20.8	.215	19.6	22.7	18.3
22–24	22.6	25.1	29.3	20.7	28.5	.323	24.0	25.3	26.7
N	800	12,710	105,245	757	4,530	11,873	801	8,740	59,927

[a]Population counts for black men in predominantly black poverty tracts in the city. For Boston, census tracts that had at least 20 percent of families below the poverty line and that were at least 60 percent black in the 1970 Census. For Chicago and Philadelphia, census tracts that had at least 30 percent of families below the poverty line and that were at least 70 percent black in the 1970 Census.

[b]Population counts for black men in various cities. 1980 Census, *General characteristics of the population.*

Sources: 1979 NBER Survey of Inner-City Black Youth Survey and 1980 Census.

report undercount percentages for inner-city youths, but the common presumption is that they are even higher.

The Census traces its undercount of black men to a number of sources. Because the count is based on dwellings, the census surveyors miss people who live in places not recognized as dwelling units, who live alone and are rarely at home, or who tend to move from one place to another. Those who refuse to respond to the surveyor because of their lack of awareness, lack of interest, or distrust are also not counted. Census analysts determine the basic demographic breakdown of the undercount by comparing birth and death statistics; and to obtain a more detailed picture, they use a variety of supplementary surveys designed to identify individuals missed by the Census itself. These supplemental surveys tend to indicate that the undercount is caused by a lack of family attachments, but they otherwise show nothing to suggest that the undercount is not random.[3]

The NBER survey, like the Census, used a dwelling-based sampling scheme. Eligible youths were identified through household screening. Once an eligible youth was identified the NBER interviewer had to find the youth and convince him to be interviewed. The Census, on the other hand, uses one respondent to report on all members of the household. This strategy ensures the highest possible response rate (which is what Census analysts want), but given both the kind and quality of information that the NBER researchers were trying to collect, they decided this strategy was out of the question.

NBER interviewers made many follow-up calls, searched local hangouts, and left word with potential respondents of means by which they could contact the interviewer. Despite all these efforts, 15 percent of those originally identified were never successfully interviewed, either because they refused (3.7 percent) or because they were never located (11.5 percent).[4] If, as seems plausible, the 15 percent were concentrated among older youths (since older youths would be less likely to be at home, their parents would be less likely to know their whereabouts, and they would be less likely to find the $5 compensation for the interview sufficient), they could account for the entire discrepancy between the NBER and the census samples.

The Problem: Potential Biases

The real question is not so much why the undercount may have occurred but whether it was random with respect to the variables of interest in the research studies in this volume. On a priori grounds, one can argue that the undercount may have biased the estimated economic and social activities of the older youths in either a positive or negative direction. It is possible, for example, that it was the most troubled youths who were the hardest to find and to interview. If so, we might expect that the NBER survey would, for example, overestimate employment rates and underestimate rates of criminal activity. Alternatively, however, it may have been that the most active youth, those most consistently away from home, were the ones NBER interviewers had the most trouble finding. If this was the case we might expect that the survey underestimated both criminal activity and employment.

It is important to recognize, however, that a biased estimate of the level of a variable does not necessarily imply a biased regression coefficient on the effect of the variable on outcomes. If, for example, the NBER survey tended to miss a certain number of the employed, estimates of employment rates would be biased, but estimates of the impact of a high school diploma on employment would be biased only if the undercount was concentrated among either high school graduates or dropouts. Constant response biases affect estimates of proportions and means but not estimates of slope coefficients. Response biases that affect the variances of the dependent variable affect the magnitudes of coefficients, but they tend to do so proportionately. The gravest problems occur not in sampling the dependent variable but in a sampling that is jointly dependent on the dependent and independent variables.[5]

Analysis of the Undercount

Is the undercount in the NBER survey random with respect to the jointness of the variables? In the remainder of this piece I review the available evidence on the randomness of the NBER undercount. We already know that it was concentrated among older youth. My focus will be on whether sampling was random with respect to various outcomes: enrollment, educational attainment, employment, and wages. These outcomes are certainly central to any assessment of the socioeconomic status of inner-city black youths, but they are also outcomes about which we already have some evidence.[6] I will make three kinds of comparisons. First, for characteristics reported in enough detail in the 1980 Census, I compare the NBER and census tabulations. Second, I use activities reported in the NBER time line to compare youths' reports of their current activities to their reports of those of a year earlier. Lastly, following the techniques popularized by Heckman,[7] I test whether standard sample-selection corrections affect the NBER estimates.

The Census-NBER comparison is straightforward, but here we need to keep in mind the differences in survey instruments. The Census relies primarily on questionnaires filled out by a member of the household and returned by mail. The NBER survey relied on direct interviews with the youths themselves. There is ample evidence that such differences in survey instruments can have substantial effects on the data collected.[8] What is more, the evidence suggests that the discrepancies are wider among young black men than among members of other groups.

The idea behind the time-line comparison is also quite simple. Retrospective responses by older youths give us another reading on youths a year younger. The discrepancies in responses suggest sampling biases. Here again, alternative explanations of any particular discrepancy will always be possible. If, for example, the retrospective responses are systematically biased with respect to the current responses or if the economic environment is not stable, we may find discrepancies that have little to do with the sampling. At the same time, with sampling biases of the same magnitude on both cross-sectional and longitudinal data, differences will not be evident.[9]

Consider the following explication of an outcome that varies with age and year:[10]

$$Y_{ijt} = \alpha_i + \beta_i T + \epsilon_{ijt},$$

where i indexes individuals; j, time periods; and t, years in the labor force; and Y is the outcome; T measures length of time in the labor force; and α and β are individual-specific constants and growth rates. For simplicity's sake consider only two ages ($t = 0{,}1$) and two time periods ($j = 0{,}1$).[11]

As long as the sample is random it will yield unbiased estimates of $\bar{\alpha}$ and $\bar{\beta}$, using cross-sections from either year, and of β, using longitudinal comparisons. The different estimates should agree as to sampling error. Disparities will suggest sampling biases. Thus, for example, if the NBER had tended to miss those worse off, only $\bar{\alpha}$ might be biased; but if attrition also rose with age, cross-sectional biases on $\bar{\beta}$ might also be the case. In either case, however, comparisons over time would correctly estimate $\bar{\beta}$, since selection is on the permanent component that is differenced out of the equation. Alternatively, suppose that those missed by the survey were missed partly because they were away from home. This would suggest that the NBER survey tended to miss those currently employed, in legal or illegal activity. Here, cross-sectional comparisons will tend to underestimate $\bar{\alpha}$. How estimates of $\bar{\beta}$ are affected depends on how sampling varies with age. The longitudinal estimates $\bar{\beta}$ will potentially be the most seriously affected, since the sampling is based on current rather than past status.

More formally, imagine that inclusion in the survey depends on this selection rule: A member of the target population is sampled only if a unit-variance variable, Z,[12] is below a threshold level, $Z < \bar{Z}_t$. Z is a function of the components of Y, such that:

$$Z_{ijt} = \lambda_1\alpha_i + \lambda_2\beta_i T + \lambda_3\epsilon_{ijt} + \nu_{ijt}.$$

Selection into the sample depends on age directly through Z and in-directly through Z's dependence on $\beta_i T$. Now, in the population as a whole:

$$E(Y \mid j,t) = \alpha_i + \beta_i T = Y \cdot jt,$$

but in the sampled population:[13]

$$E(Y \mid j,t,Z < \bar{Z}_t) = Y{\cdot}jt + \sigma_{yz} \times E(Z/Z < \bar{Z}_t) =$$

$$Y{\cdot}jt + \kappa \{\lambda_l \sigma_\alpha^2 + \lambda_2 \sigma_\beta T + (\lambda_1 + \lambda_2)\, \sigma_{\alpha\beta} T + \lambda_3 \sigma_\epsilon^2\}.$$

A cross-sectional estimate of β would simply be $Y{\cdot}j1 - Y{\cdot}j0$ with a bias.

$$(\lambda_1 \sigma_\alpha^2 + \lambda_3 \sigma_\epsilon^2)\Delta\kappa + \{\lambda_2 \sigma_\beta^2\, (\lambda_1 + \lambda_2)\sigma_{\alpha\beta}\}\kappa.$$

A longitudinal estimate of β would be Y.11 − Y.00 with a bias.

$$\sigma_\epsilon^2 \kappa + \{_2\, \sigma_\beta^2 + (\lambda_1 + \lambda_2)\sigma_{\alpha\beta}\}\kappa.$$

We see that, in general, the biases will not be the same in the two estimates. If selection is primarily on the permanent component, that is, if λ_1 dominates Z, then the cross-sectional estimates will be more biased. On the other hand, if selection depends only weakly on age and depends strongly on the transitory components of outcomes, we would expect biases to be larger for the longitudinal comparisons.

This formulation clearly shows that nonrandom sampling is likely to bias cross-sectional and longitudinal estimates differently. Still, without some prior interpretation, we cannot judge the precise difference between the two. Moreover, in many cases the biases will run in the same direction and will be quantitatively close, so that we will not notice any significant discrepancies.[14]

Finally, the fact that the undercount seems to have been more severe in Boston than in Philadelphia or Chicago suggests another simple check on sampling biases: We would expect any biases to be larger in Boston. Thus, differences across cities might suggest sampling biases. We do not want to exclude the possibility that employment or wage rates might have been higher in one city than in another, but we may be more willing to expect regression coefficients to be comparable.

In essence, examining the sample-selection techniques, at least as a specification test, is simply one version of this kind of comparison. One looks for exogenous influences on the probability of being sampled that do not affect the outcome in question. If the estimates vary systematically with the probability of being sampled, the discrepancies can be interpreted as evidence of nonrandom sampling. In the case of the NBER sample, we can use the apparent city differences in sampling, and also the apparent undercount of those living on their own,[15] to estimate the true underlying age by the household status distributions of the targeted populations. From this we can impute selection rules to use in testing for selection biases. Because we do not want to exclude either the city or family status effects from the primary equations, we will have to capture these sampling effects from the interaction terms and the nonlinearity implicit in the selection correction.

The problems with the sample-selection procedure are well known. In particular, the technique is sensitive to misspecification in either the original or the auxiliary equation.

Each of these three comparisons—between the NBER and the census data, between the current and retrospective data, and among the data collected in the three cities—carries its own problems of interpretation. Differences in the data may be the result of sampling biases but could also arise for many other reasons. The focus here will therefore be on a search for common patterns.

Census-NBER Comparisons

Table A.2 compares the census to the NBER tabulations on enrollment, employment, and educational attainment for each of the two cities for which census data were available at the time of writing. The samples are large enough that differences of even a few percentage points are significant. The first thing to notice is the broad agreement between the two tabulations. Both report proportionately more youths in school, in school past the twelfth grade, and employed in Boston than in Chicago. A more detailed comparison shows some differences, however. The Census reports a higher proportion of youths in school, a higher proportion with more education than a high school diploma, and in three out of four cases, a lower proportion employed than the NBER reports.

Taking the Census numbers at face value, we would conclude that the NBER survey undersampled the enrolled-in-school and the unemployed youths. Yet this pattern fits no simple selection explanation. The NBER survey does not seem to have missed the more active or the most disadvantaged youths. Of course, we can imagine other sampling schemes that would have given these contrasts. Still, there is another explanation. We know that there is some tendency for self-reported surveys to show higher employment and lower school enrollment rates than do surveys that rely on proxies. For example, Freeman and Medoff (1982) reported discrepancies for black youths of around 2 percent for enrollment rates and from 5 to 10 percent for employment rates between the National Longitudinal Surveys (self-reported) and the Current Population Survey (proxies). The closeness with which these differences match the discrepancies in table A.2 is striking.

Table A.2 **Comparison of School Enrollment and Employment Rates in the NBER Survey and the 1980 Census, by City and Age**

	School Enrollment Rates			Employment Rates		
	Census			Census		
Age	City	Poverty Tracts	NBER	City	Poverty Tracts	NBER
			Boston			
16–17	.898	.937	.857	.285	.271	.373
18–19	.656	.651	.567			
20–21	.407	.350	.229	.620	.619	.723
22–24	.268	.173	.166			
Proportion with Fewer Than 12 Years of Education				Proportion with More Than 12 Years of Education		
18–24	.357	.456	.511	.276	.180	.121
			Chicago			
16–17	.863	.845	.831	.184	.060	.204
18–19	.504	.463	.393			
20–21	.230	.203	.158	.478	.365	.322
22–24	.146	.132	.066			
Proportion with Fewer Than 12 Years of Education				Proportion with More Than 12 Years of Education		
18–24	.449	.556	552	.209	.147	.109

Note: Universe defined as in table A.1. For the Census, samples are one of six out of the population. For the NBER Survey, they are the same as in table A.1.

The undercount was more severe in Boston than in Chicago by 30 percent, suggesting that if the NBER differences can be explained in terms of sampling biases in the data, we should see the larger differences in Boston. There is little evidence of this effect for either school enrollment or educational attainment. Employment presents a bit more complicated pattern. Among older youths the discrepancy in employment rates is larger for Boston. Among teens the discrepancy is largest for Chicago, but here the census number is almost unbelievably low. A possible interpretation of these disparate numbers is that there is, in fact, a sampling bias for the older youths in Boston, but for teens, the undercount of whom is not severe, the differences caused by differences in the two survey instruments represent the crucial element. The plausibility of this interpretation awaits other corroborating evidence.

Time-Line Comparisons

Table A.3 tabulates currently and retrospectively reported school enrollment rates from the time line of the NBER survey and compares them to similar data for black youths from the National Longitudinal Surveys (NLS). In the NBER data, the currently reported enrollment rates are consistently higher than the retrospectively reported ones. No similar pattern is evident in the NLS data. Retrospective 17-year-old enrollment rates represent the proportion of 18-year-olds who reported that they were enrolled in school the previous year. Serving as summary statistics are the weighted average differences between the two cross-sections and chi-squared tests on the homogeneity of the aging effect across the two years. The differences between the current and retrospective data are clearly significant, with a slight indication that the pattern of aging differs between the two cross-sections.

Does the contrast between the current and retrospective responses suggest sampling biases? Perhaps the NBER tended to miss youths currently out of school. The problem with this interpretation is that the sampling bias runs in the opposite direction to the one found in the NBER-Census comparisons. An alternative interpretation is to explain the discrepancy in terms of differences between what we can expect from current and retrospective reports. We can, in fact, easily imagine a variety of reasons why currently reported enrollment rates would be

Table A.3 Comparison of Current and Retrospective School Enrollment Rates in the NLS and NBER Survey, by Age

	Total		Chicago		Boston		Philadelphia		NLS	
Age	lag	cur	lag	cur	lag	cur	lag	cur	1979	1980
16	81.1	92.8	73.9	92.1	84.3	90.7	84.6	96.1	94.6	93.4
17	64.0	74.7	50.9	69.8	81.7	80.3	57.6	73.9	90.7	92.9
18	39.2	52.1	34.6	45.5	62.7	67.5	20.9	41.5	51.0	81.4
19	26.6	28.0	24.7	30.9	36.8	40.9	19.8	12.8	44.8	24.5
20	16.8	16.9	17.3	17.3	21.8	23.7	12.1	11.0	16.2	27.6
21	14.4	14.5	10.0	13.5	20.8	21.8	13.7	9.1	12.1	13.5
22	10.9	14.9	3.8	13.1	22.0	16.7	5.7	15.1	14.3	9.1
23	4.9	12.1	4.4	5.7	4.0	22.0	6.1	7.6		
N	1,913	2,173	624	732	618	707	671	734	254	308
Weighted Mean Differential		6.9		8.5		4.4		5.7		−7.4
Standard Error		(1.2)		(2.0)		(2.1)		(1.8)		(1.9)
χ^2 Test of Homogeneity		18.71		9.54		7.94		18.07		15.02
Degrees of Freedom		7		7		7		7		6

higher than the retrospectively reported ones. For example, youths who were only marginally attached to school a year ago might be less likely to report themselves as having been enrolled.

Table A.4 compares current and retrospective employment rates in the NBER and NLS data. We see virtually no indication of any differences between the two cross-sections. It is customary to report employment rates separately for those enrolled in school, but for our purposes this would be a mistake. The enrolled are a self-selected population whose composition changes over time. If we were to make employment status conditional on enrollment status, we would confound these compositional effects with the sampling biases we are looking for.

Table A.5 compares current and retrospective wage rates in the two samples. These data are more difficult to interpret for a number of reasons. Perforce, we must make wages conditional on employment both at the time of the survey and a year earlier. Moreover, the NBER Survey reports only one wage per job, thereby capturing only between-job not within-job wage growth. The table shows that wages were higher in the NBER current data, but even more so in the NLS data, in which average hourly compensation rose over the period by about 10 percent per year. This fully accounts for the differences between the two NLS series. The NBER numbers actually show less wage growth than we would expect given the growth in compensation, but this could easily be due to the lack of within-job wage growth. The linear age terms summarize the cross-sectional aging effect for each survey. No great differences stand out.

Finally, although the NBER survey did not ask retrospective questions about educational attainment, the time line shows whether the rising attainment is consistent with the enrollment rates. Table A.6 reports current attainment by age and the predicted future attainment based on current enrollment and educational attainment. If there was no sampling bias, the predicted rates should be slightly higher than the actual rates. Not everyone finished the year and not everyone was promoted. The table fits exactly this pattern.

To summarize, we have found little indication of sampling biases. The exception was with enrollment data, but here the pattern went in the opposite direction from that found in the Census-NBER comparison. It is therefore easier to explain both the discrepancies in terms of the differences in survey instruments rather than in terms of sampling biases.

Cross-City Comparisons

Wage and employment equations with age, education, and enrollment status as explanatory variables were run separately for each

Table A.4 Comparison of Current and Restrospective Employment Rates in the NLS and NBER Survey, by Age

	NLS		Total		Chicago		Boston		Philadelphia	
Age	1979	1980	lag	cur	lag	cur	lag	cur	lag	cur
16	19.6	27.9	19.4	21.6	16.0	20.5	31.5	29.5	10.8	14.7
17	48.8	25.0	31.3	32.5	26.4	23.5	45.8	48.0	19.8	25.4
18	42.9	58.1	37.6	39.0	37.0	37.3	48.2	51.7	27.9	26.4
19	65.5	59.2	45.6	45.2	34.6	37.0	63.2	60.2	40.7	38.4
20	67.6	72.4	55.5	49.6	51.9	35.8	65.5	61.8	50.0	51.7
21	84.9	67.6	56.4	50.3	55.0	38.5	68.8	70.9	49.3	42.4
22	71.4	84.9	56.4	55.3	54.7	41.7	66.1	83.3	47.2	48.0
23			57.1	57.6	50.0	50.9	80.0	66.1	47.0	54.7
N	254	308	1,913	2,173	624	732	618	707	671	734
Weighted Mean Differential		−2.0		−.10		−2.2		0.3		2.0
Standard Error		(3.9)		(1.5)		(2.5)		(2.6)		(2.3)
χ^2 Test of Homogeneity		12.83 (.05)		3.56		8.05		7.36		3.17
Degrees of Freedom		6		7		7		7		7

Table A.5 **Comparison of Current and Retrospective Average Wage Rates in NLS and NBER Survey, by Age and City**

	NLS		Total		Chicago		Boston		Philadelphia	
Age	1979	1980	lag	cur	lag	cur	lag	cur	lag	cur
16	3.13		3.09	3.08		3.16		3.14	2.76	2.68
17	3.09	3.75	3.28	3.14	3.27	3.18	3.33	3.21	3.14	2.81
18	3.28	3.77	3.47	3.44	3.32	3.79	3.51	3.35	3.59	3.14
19	3.81	4.37	3.62	3.85	3.64	3.94	5.57	3.66	3.46	4.29
20	3.88	4.22	3.70	4.13	3.97	4.23	3.82	4.35	3.38	3.82
21	4.42	4.88	3.97	3.84	4.20	4.13	3.97	4.04	3.78	3.44
22	3.73	4.91	4.41	4.27	4.60	4.54	4.07	4.42	4.72	3.91
23		4.44	4.11	4.75	4.56	5.09	3.88	4.44	3.97	4.81
24				4.33	3.06	4.90	2.44	4.13		4.02
N	110	103	866	919	263	279	376	406	227	234
Weighted Mean Differential	.108		.024		.048		.020		−.001	
Standard Error	(.031)		(.010)		(.023)		(.013)		(.087)	
Linear Age	.068 (.016)	.083 (.016)	.047 (.005)	.050 (.005)	.064 (.009)	.058 (.009)	.033 (.006)	.047 (.006)	.055 (.010)	.049 (.010)
χ^2 on Equality of Age Coefficients	1.0395		.6331		.464		7.947		1.1937	

Note: Universe for NLS is black youths employed during the survey week in both years, living in SMSAs, and not in the military. Universe for NBER is black youths employed during the survey week and one year earlier. All computations are in logs and converted back to actual-dollar wage ratios for ease of comprehension.

Table A.6 Actual versus Predicted Portion with a High School Diploma, NBER

	Total		Chicago		Boston		Philadelphia	
Age	Actual	Predicted	Actual	Predicted	Actual	Predicted	Actual	Predicted
16	.45		1.14		0.0		0.0	
17	1.86	5.86	.00	6.82	.79	2.88	4.62	7.75
18	21.19	30.05	17.27	20.00	20.83	16.54	25.71	41.54
19	43.60	53.13	43.21	39.93	45.58	65.00	43.02	47.62
20	53.44	57.60	45.68	45.45	63.16	66.27	52.22	51.16
21	61.27	57.89	57.69	55.56	74.55	71.05	53.03	54.44
22	60.22	61.27	60.00	48.38	60.42	74.55	60.27	53.03
23	66.67	60.22	58.49	57.69	81.36	60.42	58.49	60.27
24	54.89	60.67	57.35	60.00	64.00	81.36	45.45	58.49

Note: Predicted high school completion rates calculated using youths one year younger than the stated age and assuming all those enrolled in school would complete their current grade.

city. The chi-squared statistics for the test for pooling across the cities were 10.7 and 8.5 for the wage and employment equations, respectively. With six degrees of freedom, these show no sign of a systematic difference across the cities in terms of wage or employment responsiveness to the three explanatory variables.[16] Comparing Boston with each of the other two cities yields chi-squared statistics of 4.7 and 3.2 for the Boston-Chicago comparisons and 4.9 and 3.6 for the Boston-Philadelphia comparisons, with coefficients never differing by more than 10 percent. There is no evidence that any of the regression parameters differs across cities or that Boston is different from the other two cities.

We can address the same issue by using the sample-selection technology. An auxiliary equation can predict who will be in the survey conditional on exogenous variables. Using the proportion of family heads reported in the 1976 Survey of Income and Education for black men in the 16–24 age range and imposing the census age distributions, we can compute a selection equation and then reestimate the wage and employment equations conditional on these probabilities. For the wage equation, the inverse Mills ratio can be used as an explanatory variable. The two-step procedure is preferable to the maximum-likelihood technique because it depends less heavily on distributional assumptions.[17] For the employment equation such a two-step procedure is not available, but it is still possible to write the probit likelihood conditional on a supplementary selection rule and on a cross-equation correlation. The cross-equation correlation captures the nonrandomness of the selection, as does the Heckman term in the linear regression. As long as this correlation is not zero, estimates of the original equation that do not take account of the sampling will be biased.

For wages the estimated cross-equation correlation implied by the inverse Mills ratio is .05, with a t-ratio of .8. For employment the estimated correlation is a trivial .001, but with a standard error of 1.009. Coefficients in the wage and employment equations are negligibly affected and so are not reported here. Thus, there is no evidence of selection biases, but the relevant selection parameters, particularly the one for the employment equation, are poorly estimated. The tests therefore have negligible explanatory power against plausible alternatives.

Conclusion

The foregoing analysis searched for consistent patterns that would suggest sampling biases in the NBER survey data. Although some discrepancies between the NBER and census data and between the current and retrospective data were uncovered, the discrepancies are more easily attributable to differences in the survey instruments than to sampling bias.

Perhaps the strongest evidence for sampling bias lies in Census-NBER comparisons of employment in Boston. Yet Boston showed no discrepancy in any of the other comparisons. Moreover, even if we were to take the census data on Boston as correct, the conclusion we would reach using the NBER data would still be valid. We would be correct in concluding that it was much easier for a young black man to find a job in Boston than in Chicago and in concluding that employment rose with age.

The NBER data are unique; no other survey of inner-city youth of comparable scale or scope exists. The apparent undercount of older youths raises legitimate doubts about possible sampling biases. Yet there is no consistent evidence that the undercount seriously biased the analysis of the key variables studied in this volume.

Notes

1. The NBER survey targeted census tracts in three cities that had at least 30 percent of the population below the poverty line and 70 percent of the population black in 1979. (In Boston the proportions were 20 percent and 60 percent, respectively.) For details of the survey design, see Jackson and McDonald (1981).

2. These percentages are based on birth, death, and net immigration statistics. The assumption is made that there is no illegal immigration, which for blacks may be close to the truth. Details on the estimates can be found in various census publications (see, in particular, U.S. Bureau of the Census [1974; 1982]).

3. Introductions to the work the Census has done in this area can be found in Klein (1970), Johnston and Wetzel (1969), and U.S. Department of Labor (1968). The most serious conceptual problem with these census and Bureau of Labor Statistics surveys is that they may themselves under- or overrepresent a characteristic in the undercount, and so there is no guarantee that they will even be correct in suggesting the right sign on an undercount bias. For example, suppose the nonemployed are those who tend to be missed in the original enumeration. If this is just as true in the postenumeration survey, we will be apt to believe that there is no bias, when, in fact, there is.

4. All details regarding the NBER survey design and implementation can be found in Jackson and McDonald (1981).

5. For amplification on this point see Goldberger (1981), Maddala (1983), and Manski and McFadden (1982).

6. The probable underreporting of criminal activity is discussed in Viscusi (in this volume).

7. Heckman (1979) is the classic reference.

8. See Freeman and Medoff (1982) and Borus, Mott, and Nestel (1978).

9. The comparison I am suggesting is the within versus between comparison, familiar to users of panel data, though not commonly used to test for sampling bias. See Chamberlain (1984) and Hausman (1978).

10. In conception and notation, the equation follows the wage-dynamic literature. Examples are Lillard and Weiss (1979) and Ashenfelter and Card (1984). That literature typically specifies some time-series pattern on ε, with some form of fixed time effects (vintage). The time-series pattern is omitted here for notational convenience, but the

fixed time effects are a necessary restriction, as will become clear below. The sampling and time effects are indistinguishable without information from outside the sample.

11. For notational convenience, the subscripts will be dropped below except where doing so would create confusion.

12. The units of Z are arbitrary. Unit variance is a notationally convenient normalization.

13. $\kappa_t = E(Z|Z < Z_t)$, $\Delta\kappa = \kappa_1 - \kappa_0$.

14. This is the familiar problem that omnibus tests tend to have nebulous explanatory power.

15. The number of older youths heading their own household is strikingly low in the NBER survey. Only 28 percent of 22- to 24-year-olds in the NBER survey headed their own household, whereas the comparable percentage for blacks of the same ages in the 1976 Survey of Income and Education was 53 percent, and for blacks of those ages in the poverty areas of Boston, Chicago, and Philadelphia in the 1970 Census, it was 64 percent.

16. The procedure yields two restrictions each for age, education, and enrollment status. Age is entered as a set of nine dummy variables, but education is still entered linearly. The chi-squared values are again close to their expected values: 25.9 and 19.02, respectively, for wages and employment, for statistics with 20 degrees of freedom.

17. See Olsen (1982).

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