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FAMILY CHARACTERISTICS AND ACHIEVEMENT: EFFECTS OF BIRTH ORDER AND FAMILY SIZE IN THE KALAMAZOO BROTHERS SAMPLE

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ABSTRACT

Research on the effects of birth order on cognitive ability and other outcomes often fails to control relevant variables related to family background, and does not usually investigate the effects of birth order among members of the same families. Consequently, apparently significant birth order effects may in fact be spurious. This study uses a sample of brothers to investigate the effects of birth order within families on sixth grade test scores, educational attainment, adult occupational status, and earnings.

Despite a detailed search for both linear and nonlinear effects of birth order, our analyses suggest few statistically significant, large, or consistent effects. Allowing the effects of birth order to vary by age-spacing does not alter this finding.

Unlike the effects of birth order, the effects of family size are significant. The effects of increased family size diminish as family size grows larger. The nonlinear effects of family size are reduced appreciably when other measures of socioeconomic status are controlled. The effects of family size on educational attainment once socioeconomic background and test scores are controlled are small but significant. The effects on occupational status and earnings are insignificant with background, test scores, and education controlled. These results suggest that the most important independent consequence of larger families for the attainment process is lowered cognitive skill. This apparent effect may, however, itself be spurious.

Findings on sibling resemblance suggest that brothers from smaller families may share more common backgrounds than do brothers from larger families. The tendency of sibling correlations to be larger for men from small families than for men from large families suggests the role of environmental as against genetic factors in the determination of sibling resemblance.

Family Characteristics and Achievement:
Effects of Birth Order and Family Size in
the Kalamazoo Brothers Sample

1. INTRODUCTION

Men from different families differ in their likelihood of educational and economic success or failure. This is true even of men whose measured socioeconomic background is similar (Olneck 1976; forthcoming). However, the mechanisms by which family background affects life chances remain obscure despite considerable effort by social scientists to measure the extent and channels of influence. Speculation about the sources of family influences above and beyond the influence of socioeconomic status is at best untested conjecture (Corcoran, Jencks, and Olneck 1976). Measurement of variables tapping the processes by which socioeconomic effects arise is rare, and inevitably depends upon imprecise measures (Sewell and Hauser 1975). Efforts to quantitatively assess psychological mechanisms in the process of intergenerational status transmission are confined to small and atypical samples (Elder 1968). More wholistic approaches in anthropology and psychology, often falling under the rubric of the "culture of poverty," are unable to distinguish observed behavior due to opportunity structures from persistent values and preferences (Moynihan 1969; Leacock 1971; Allen 1970).

Two possible explanations for the paucity of survey research adequately explaining rather than simply measuring the effects of family background come to mind. First, such research is exceedingly difficult to conceive and conduct. Since it is likely that parental characteristics affect a son's life chances principally by affecting a son's own characteristics, even early in the life cycle, research explaining the impact of family background would have to measure relevant characteristics of men when they were young. Not only is it difficult to enumerate and measure relevant characteristics in any population, it is virtually impossible to measure the personal characteristics at youth of a sample of adults.

Moreover, even if the relevant characteristics intervening between family background and measures of later success were identified and measured, researchers who confined themselves to such efforts would not necessarily be able to account for the systemic sources of newly discovered empirical relationships. It is difficult enough to establish what matters, and to what extent; but exclusive attention to individual level data can never reveal the processes by which particular characteristics come to matter. Thus, few researchers have explored the nexus between the institutional structures of school experience and individual test scores. Test scores are simply labelled "ability" and are used to explain school achievement or persistence. Economists assessing the impact of education on earnings generally assume that schooling reflects productivity, but few direct tests of this assumption are available.[1] While researchers analyzing individual level data may hold to theories explaining the sources of the relationships they measure, their work--and ours is unfortunately no exception--inherently cannot provide the data necessary to evaluate those theories. This is true even if theories can be rejected or not rejected on the basis of their consistency with observed relationships. This is because explanation for social phenomena cannot stop at establishing, however accurately, the "rules of the game." We believe good explanation must also give an account of the reasons why one set of rules and not another prevails. This we can never hope to accomplish by confining our attention to individual level data.

The second reason we suspect that researchers have not provided good explanations for the effects they have measured is that their research questions and results generally derive from and accord with common sense expectations about individual differences in life chances. The study of the effects of family background is rooted in an historical tradition of research and social policy that accounts for and responds to individual outcomes in terms of earlier individual and family configurations. The variables whose effects we assess are often those commonly believed to account for observed patterns of advantage and disadvantage. Thus, the lower school achievement of poor children is seen, in part, as a consequence of large and fractioned families. There is an intimate and long-standing connection between the empirical research social scientists conduct about these issues, the everyday language used to account for patterns of individual difference, and the social policy measures attempted to remedy the recurrence of "deviant" outcomes.

This is not the appropriate place to develop a detailed view of the connections between research paradigms and broader social ideology. We note our concern with the question here only because we believe that it is important for researchers to remind themselves of the sources of their own inquiries and the conditions shaping the form of those inquiries. We have been struck by the extent to which research into the effects of family background is content to establish magnitudes of relationships without investigating sources of relationships. Besides the technical difficulties we outlined above, we believe that this is due to the derivative nature of the research. If we are correct that research of this kind is shaped by a broad tradition that implicitly assumes individual outcomes are best understood as the self-explanatory projections of individual origins, then quantifying the connections rather than explicating them--or even ignoring them in favor of other kinds of questions--is an explicable activity of social scientists. We are disappointed that we can merely raise this issue without offering an example of how to proceed differently. The form in which our data exist permit us to do little but replicate and extend earlier inquiries. We cannot here, however, redefine the terms of inquiry.

While we know that men from different families differ in their life chances, we also know that men from the same family are quite likely to occupy very different positions in the occupational and earnings hierarchies. Indeed, our data suggest that the average difference between the occupational statuses of two brothers is 83% as large as the average difference between the occupational statuses of two randomly chosen men. They suggest that the average difference between the earnings of two brothers is 87% as large as the average difference between the earnings of two randomly chosen men.[2]

One reason brothers differ in economic success is that they often differ on measurable characteristics that affect success. While sibling resemblance on early characteristics such as aptitude test scores and educational attainment is greater than sibling resemblance on later outcomes, it is by no means perfect. In the Kalamazoo sample, brothers differ by almost 12 points, or over three-quarters of a standard deviation, on aptitude tested in sixth grade, and they differ by 1.78 years on eventual educational attainment. But these differences, and remaining differences in occupational status and earnings not attributable to earlier measured differences, are not readily explained.

While a traditional explanation for otherwise unexplained individual differences is available--i.e., genetics--our data are in no way suited to explore its influence. Moreover, we are skeptical of the assumptions necessary to estimate models of genetic influence on individual outcomes. The sensitivity of the conclusions suggested by such models to the requisite assumptions disturbs us further.[3]

A potential source of systematic difference between brothers is birth order. Since our reading of several reviews of the literature had convinced us that birth order effects were weak and inconsistent, we did not approach our task of assessing its effects with great optimism. Moreover, we were unsympathetic to the apparently ad hoc or post hoc approach to explaining anomalous results.[4] While we were not prepared to reject the importance of birth order out of hand, we imagined that its import might vary from family to family in ways that would tend to cancel out the measurement of its effects. The possibility of what amounts to a family by birth order interaction vitiates any wholesale rejection of the importance of birth order on the basis of negative results.

Despite our reservations, and despite a sample in which the effects of age-spacing cannot be adequately explored and the effects of sex composition cannot be explored at all, we felt that our data warranted analysis along these lines. The possibility that birth order contributed to unexplained intrafamily differences could not be ignored, and previous analyses of these data had not been sufficiently extensive (Olneck 1976). Despite its limitations, by virtue of being a sibling sample our data is useful in remedying a defect of most earlier work on birth order. Few studies adequately control aspects of family background that can bias the measure of birth order effects.[5] The most serious variable to omit is no doubt family size. Sibling data is obviously not required to control for family size. Other, unmeasured aspects of family background might remain, however, that affect the measurement of birth order effects. While most concern with the bias issue relates to overestimation of effects, we wanted to test the possibility that birth order effects could be <u>larger</u> within families than across unrelated individuals.[6]

Our work has convinced us that our initial reservations were sound, and that we can echo Schooler's (1972) conclusion "Birth Order Effects: Not Here, Not Now!" with "Not Here, At Any Rate."

The remainder of this paper is devoted to describing our sample, discussing the measurement and specification of our variables, and reporting the results of our analyses of the effects of birth order, and of family size.

2. THE KALAMAZOO BROTHERS SAMPLE

During the summer of 1973, Olneck selected a sample of males from the records of sixth grade aptitude scores on tests taken by students in the Kalamazoo, Michigan, public schools in the years 1928 to 1950. He then used school census and enrollment records to determine siblingships. This procedure resulted in a potential sample of 2782 individuals from 1224 sets of brothers.

Olneck traced 1612 of the original 2782 individuals in the sample. Of these, 1243 completed a follow-up telephone interview during the period

September 1973 to May 1974; 152 were dead, 52 were never directly contacted, and 165 refused to be interviewed.*

Olneck (1976; forthcoming) analyzed data for 692 individual respondents or 346 pairs of brothers for whom test scores, background data, and self-reported data on educational attainment, occupational status, and earnings were available. Differences between the means, standard deviations, and correlations for the 1243 men interviewed and the 692 men comprising his complete pair sample were negligible. The present analyses draw on a subsample of the complete pairs, and on the sample of 1243 interviewees.

Olneck (1976; 1977) made some attempt to assess the influence of nonresponse on his sample characteristics. He concluded that interviewees were only somewhat more likely to have high test scores than nonrespondents, and that sibling resemblance was not significantly exaggerated in the complete pair sample. The lack of substantial distortion in the complete pair sample, despite an extraordinarily high nonresponse rate (1 - 692/2782 = 75.1%!), may be attributed to the failure to find successful men who left Kalamazoo counterbalancing the usual lower response rate of less successful men.

While the sample we analyze may be quite representative of the original target population, it is discrepant from nationally representative data in some important respects; levels of parental status and respon-

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dents' attainments are higher in the Kalamazoo data than in data for men of comparable age (i.e., 35 to 59) in the 1973 Occupational Changes in a Generation (OCG) replication data.[7] This is due to the specific characteristics of Kalamazoo and to a likely modest success bias in the sample. The impact of fathers' education and occupational status on respondents' current occupational status and earnings is less in the Kalamazoo data than in OCG-II, but the correlations among measures of respondents' education, occupation, and earnings are comparable in the two data sets. What is most reassuring, given our interest in birth order effects, is that none of the correlations involving family size differ significantly between the two data sets.

The principal virtues of the Kalamazoo Brothers Sample are (1) it is the only American data set that has both measures of early ability and socioeconomic background for a reasonably large number of men who are middle-aged; and (2) it allows us to control and assess the importance of family background more adequately than if we had to rely solely on traditional socioeconomic measures.

The principal deficiencies of the data for our present analyses are the following. (1) Sample attrition is severe once we impose certain restrictions, and analyze only 274 pairs of brothers (548 individuals) in our sibling analyses. Fortunately, the individual level effects of birth order in this subsample are rarely significantly different from the effects in a larger subsample of 979 similarly restricted interviewees. The differences that do exist indicate stronger birth order effects in our complete pair subsample, so we are not likely to err on the side of underestimation. (2) We do not know the sex composition of the respondents' families of origin. (3) We can only assess the importance of age-spacing for pairs of adjacent brothers since we do not know the ages of siblings who are not in our sample. (4) The sample does not include only children. Wright (1976) reports that for education, which was the only variable for which she found any significant birth order effects in the 1962 OCG-I data, the mean of only children is virtually identical to the mean for individuals from two-child families. This suggests that our estimates for the effects of first-born status will not be distorted by having only men from families with two or more children. Zajonc (1976) reports, however, an only-child decrement on test scores in several data sets. This should temper our confidence that the omission of only children is trivial.

3. VARIABLES

In all our analyses of birth order effects, we have ignored the effects of measured parental status. Instead, we have controlled family background by eliminating all between-family differences and looking at the effects of birth order differences within families. We did this by defining sibling difference variables on all our measures of interest. We did, however, control total number of siblings, and age in our individual level analyses.

Our measure of tested ability comes from tests administered to Kalamazoo sixth graders during the period 1928 to 1950. From 1928 to 1943, the Kalamazoo school system administered the Terman group test; after 1943, students were given the Otis test (see Buros 1975). The two tests measure similar skills, and in our data correlate almost identically with the overall quotient from the Metropolitan Achievement test, which was available for some respondents. Other correlations involving the two tests are also quite similar. Nor is there evidence in the literature that the variances or reliabilities of the two tests differ significantly (Flemming 1925; Cattell 1930; Ratcliff 1934; Buros 1965).

The Otis test, however, has historically been scaled to a lower mean than the Terman (Ratcliff 1934). In the original Kalamozoo sample, individuals who had taken the Terman test (N = 2075) scored 4 points higher than students who had taken the Otis (N = 707). Because the difference in average scores was expected, and because the tests were otherwise comparable, Olneck (1976) did not separate men who had taken different tests. Rather, after taking into account the effects of secular changes in parental education, father's occupation, and family size, he adjusted the scores of men who had taken the Otis test (see Olneck 1976; 1977).

Our measures of attainment include years of schooling completed, Duncan occupational scores for the respondent's first full-time job after completing all of his schooling and for his current job, and the natural logarithm of 1973 expected earnings. Men who reported expected earnings of $25,000 and over were originally coded $34,000. Moreover, few respondents reported earnings below $8,000. These restrictions, and the fact that the income data were originally recorded in grouped intervals, account for the rather low standard deviation of ln earnings (see Table 1).

We specified birth order in three distinct ways for our regression analyses. First, we employed a simple linear variable. Second, we employed seven dummy variables representing the effects of birth orders 2 through 8. (We did not analyze men from families larger than eight in our sibling analyses.) These dummies are <u>not</u> ordinary dummies, which are coded 0,1 to represent exclusive membership in a category, and which measure the adjusted difference between each category for which a dummy is specified and an omitted category. Rather, our dummies measure and test the significance of <u>differences</u> between <u>adjacent</u> birth orders. This is accomplished by assigning the value of 1 to all dummies that represent the birth orders equal to and lower than the respondent's own position. Thus, a respondent who was sixth on birth order would be assigned a 1 on dummies 2 through 6, and a 0 on dummies 7 and 8. A respondent who was second in order would receive a 1 only for dummy 2. First-borns are the ommitted category. The average difference between a particular birth order and first-borns may be calculated by summing the coefficients of dummy 2 through the dummy representing the particular birth order.

Finally, we created dummies that represent first-borns and last-borns. When employed separately, these measure the difference between those represented by the dummy and all others. When used together, they represent the differences of first-borns and last-borns from middle-borns. In an effort to distinguish the effects of being last-born from the effects of later birth order, we also employed our last-born dummy in analyses that used our nonlinear dummies. The variable was never significant in those analyses, and the results are neither reported nor discussed below.

Table 1

Means and Standard Deviations for Selected Variables
for Subsample of Kalamazoo Brothers Sample
(N = 548 or 274 weighted pairs)

Variable	Mean	Standard Deviation
AGE	45.971	6.040
TEST	102.527	15.495
ED	13.539	2.727
SIBS	2.989	1.586
YNGOC	41.811	24.500
DUNC	51.873	22.926
LNEARN	9.641	.458
BRTHORD	2.544	1.417
BRTHORD2	.743	.437
BRTHORD3	.432	.496
BRTHORD4	.211	.409
BRTHORD5	.093	.291
BRTHORD6	.040	.196
BRTHORD7	.019	.137
BRTHORD8	.006	.074
FIRST	.257	.437
LAST	.287	.453

Table 1 gives the means and standard deviations for the variables used in our analyses of a subsample of complete pairs of brothers who reported having the same number of siblings, and who reported no more than seven siblings. We eliminated men from larger families because the Ns were too small to allow fruitful within-pair analyses. We did not group men from later birth orders because that would eliminate within-family variability. (We did, however, include men from larger families in our two-way breakdowns of outcomes by birth order by family size for the full sample of interviewees. See Tables 2, 7, 10, 13, and 16 in text.)

Our discussion of our results and analyses is organized around separate outcomes.

4. RESULTS

Test Scores

We first looked at test scores for 1216 men for whom we had both a test score and a report of family size. Table 2 shows the mean scores for men from different family size-birth order combinations, and by birth order and family size alone. While inspection of mean scores by birth order alone suggests a trend (nonuniform) toward lower scores at later birth orders, inspection of scores within family sizes shows considerable inconsistency. No interpretable pattern obtains.

Had we expected first-borns to show a marked advantage, we might have been tempted to explain the rather low scores for first-borns from families of six or more as the result of higher attrition due to the deaths of older men, and as the result of the omission of females. If families with clever first-born girls have a preference for males, later-born males from such families might show an advantage over first-born males from equally large families. First-born males would tend to come from families with lower average scores. But since we did not expect any such advantage, and since the Ns involved are small, we do not believe such explanations necessarily apply to our data. Others working with single-sex, age-variable samples should probably consider such possibilities.

In Tables 3 and 4, we report individual-level and within-family regression results for 548 individuals or 274 pairs who had complete data on the variables of interest, came from families of eight or less, and who reported the same number of siblings. Equation (1) in Table 3 shows that a unit increment in birth order is associated with a 2.2 point decrement in test scores. Controlling number of siblings and age reduces this effect by 1 point, but it remains significant. This suggests that while our tabular analyses (Table 2) evidence fluctuation, there is nevertheless a general though small impact of increasing birth order that persists within family sizes.

When we turn, however, from individual-level analysis and ask about the effects of birth order within families, our results are different. Equation (1) in Table 4 shows that earlier birth order within a family is associated with only a 0.4 point test score advantage, and the effect is statistically insignificant. Controlling age differences raises the effect to 2.2 points, the sample zero-order effect, but it is statistically insignificant.

While birth order may, on the average, exercise trivial effects, it is of course true that the effect of birth order may be systematically nonlinear. However appealing, our data only weakly support this possibility. Equation (3) in Table 3 shows that test scores fall from birth orders 2 through 5, rise in birth orders 6 and 7, and fall again among eighth-borns. The magnitudes of declines within birth orders 2 through 5 fluctuate substantially. Only the differences between third- and second-borns and fifth- and fourth-borns are statistically significant, and only the difference between fifth- and fourth-borns remains significant after controlling for age and number of siblings (Table 3, equation (4)).

Within families, once age differences between brothers are controlled, birth order shows a somewhat more consistent, though always insignificant, relationship to test scores. Every birth order except the sixth shows a decline in scores relative to the preceding position (Table 4, equation (4)). Men who are sixth-born score almost 5.5 points more than their fifth-born brothers, controlling for age differences. If the anomalous result for sixth-borns is ignored, there is some suggestion that the effects of birth order are more pronounced among later positions. We are, however, hesitant to offer any theories about the cognitive strengths of sixth-borns relative to fifth-borns, or the marked disadvantage of eighth-borns relative to seventh-borns, and view our results on the nonlinear effects of birth order as, at best, suggestive.

First-borns scored 3.7 points higher than all others (Table 3, equation (5)). This advantage is largely due to first-borns coming from smaller families. Controlling number of siblings and age reduces the advantage to reduce the first-born effect to insignificance. First-

Table 2

Average Test Score By Birth Order and Family Size

Family Size	Birth Order 1	2	3	4	5	6	7	8 or more*	Total
2	104.84 (82)	105.80 (90)							105.34 (172)
3	104.03 (94)	101.71 (115)	103.35 (99)						102.95 (308)
4	105.26 (54)	99.42 (59)	103.72 (69)	101.65 (63)					102.49 (245)
5	102.00 (21)	96.77 (30)	93.40 (40)	99.33 (27)	95.27 (26)				96.81 (144)
6	87.89 (9)	95.62 (21)	96.45 (22)	100.40 (25)	95.27 (26)	95.07 (14)			96.06 (117)
7	96.40 (5)	93.80 (10)	95.60 (10)	91.00 (12)	94.75 (12)	95.57 (7)	101.93 (15)		95.82 (71)
8 or more*	93.44 (9)	102.31 (13)	96.46 (13)	92.94 (17)	86.35 (23)	96.65 (17)	92.14 (21)	92.59 (46)	93.26 (159)
Total	103.34 (274)	101.37 (338)	100.62 (253)	99.08 (144)	92.84 (87)	95.87 (38)	96.22 (36)	92.59 (46)	100.12 (1216)

*Run ungrouped.

(N)

Table 3
Individual Level Regressions of Test Score
(N = 548)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
BRTHORD	-2.173 (.458)	-1.178 (.571)								
BRTHORD2			[-.510] (1.728)	[-.942] (1.704)						
BRTHORD3			-3.765 (1.802)	[-2.693] (1.804)						
BRTHORD4			[-.819] (2.335)	[.662] (2.298)						
BRTHORD5			-8.541 (3.390)	-6.833 (3.320)						
BRTHORD6			[4.655] (5.289)	[6.409] (5.165)						
BRTHORD7			[4.429] (7.123)	[5.263] (6.924)						
BRTHORD8			[-10.067] (10.367)	[-8.606] (10.069)						
FIRST					3.709 (1.507)	[2.135] (1.565)			4.938 (1.619)	[1.212] (1.769)
LAST							[1.413] (1.463)	[-2.396] (1.474)	3.189 (1.564)	[-1.862] (1.668)
SIBS		-3.948 (1.671)		[-2.966] (1.819)		-4.055 (1.682)		-4.901 (1.676)		-4.572 (1.769)

Table 3—Continued.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
SIBSSQ		[.292] (.224)		[.152] (.248)		[.243] (.226)		[.314] (.226)		[.288] (.229)
AGE		−.467 (.107)		−.479 (.108)		−.452 (.107)		−.458 (.109)		−.469 (.108)
C	108.056	135.457	105.284	133.745	101.575	132.097	102.123	135.339	100.346	134.666
$\bar{R}^2$	.038	.102	.040	.104	.009	.099	−.000	.100	.015	.099
Standard Deviation of Residuals	15.200	14.680	15.181	14.664	15.424	14.712	15.496	14.701	15.379	14.708

Note: Standard error of regression coefficients shown in parentheses.
Bracketed coefficients less than 1.96 times their standard errors.

Table 4

Regressions of Test Score Controlling Brothers' Common Family Background
(N = 274 pairs)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
BRTHORD	[-.408]	[-.210]								
	(.593)	(1.279)								
BRTHORD2			[.253]	[-1.457]						
			(1.453)	(1.834)						
BRTHORD3			[-.880]	[-2.683]						
			(1.696)	(2.065)						
BRTHORD4			[-.780]	[-2.532]						
			(2.242)	(2.515)						
BRTHORD5			[-2.867]	[-4.394]						
			(3.412)	(3.548)						
BRTHORD6			[7.202]	[5.475]						
			(5.376)	(5.481)						
BRTHORD7			[-1.226]	[-3.906]						
			(7.425)	(7.613)						
BRTHORD8			[-5.925]	[-7.067]						
			(9.607)	(9.613)						
FIRST					[.171]	[.385]			[.269]	[-.026]
					(1.352)	(1.635)			(1.594)	(1.823)
LAST							[.041]	[-.125]	[.177]	[.380]
							(1.292)	(1.767)	(1.523)	(1.688)
AGE		[-.630]		[-.611]		[-.052]		[-.035]		[-.054]
		(.397)		(.401)		(.223)		(.252)		(.267)

Table 4—Continued.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
$\bar{R}^2$	.468	.471	.462	.465	.467	.465	.467	.465	.465	.463
Standard Deviation of Residuals*	11.302	11.270	11.361	11.329	11.311	11.331	11.312	11.332	11.332	11.352

Note: Standard error of regression coefficient shown in parentheses.
Bracketed coefficients less than 1.96 times their standard errors.

* Equivalent to standard deviation of residuals for individuals controlling family background dummy variables. Calculated by multiplying the ovserved standard deviation of residuals for sibling differences by 0.5 to yield a within-pair standard deviation, and dividing by 1.414 to correct for degrees of freedom.

borns have only an insignificant and negligible 0.2 point advantage over their own brothers (Table 4, equation (5)).

Last-borns have scores that are slightly and insignificantly higher than those of others (Table 3, equation (7)) because they are more likely to come from smaller families. Controlling age and number of siblings reverses the apparent advantage of last-borns, although the resulting disadvantage is small and insignificant (Table 3, equation (8)). The scores of last-borns are virtually equal to the scores of their own brothers (Table 4, equation (7)).

Compared to the scores of middle-borns, the scores of first- and last-borns are significantly higher (Table 3, equation (9)), but this is because both first- and last-borns are more likely to come from smaller families. Once number of siblings and age are controlled, the differences between the scores of first-, last- and middle-borns are insignificant (Table 3, equation (10)), and first- and last-borns are insignificantly different from their middle-born brothers (Table 4, equation (9)).

There are a number of reasons why our data might indicate the absence of significant birth order effects on test scores even if such effects were present in the general population. The omission of females could bias our results if there are birth order by sex interactions, or if the effects of birth order for males depend upon the sex composition of the sibling group. If the first is true, we would simply caution that our results should not be generalized to females. We would also speculate that any birth order-sex composition interaction favoring early-born males would be insignificant if sex were controlled. We have no evidence on these possibilities, and offer them merely as suggestions for other researchers working with richer data bases.

Another possibility is that birth order effects are significant only among same sex siblings. If birth order in our sample was defined as position in the sequence of males, significant effects might be found.[8] Since men of differing birth orders can hold the same position within the sequence of males, such effects would be obscured in our analyses. While this possibility has some appeal for explaining later attainments such as education or occupational status, we cannot see its theoretical applicability for sixth grade test scores.

Finally, Zajonc (1976) has argued that birth order effects depend upon age-spacing. Zajonc's argument derives from a model that posits reciprocal effects of family members' mental levels on one another's mental growth. Since we were skeptical of a model that weights each family member equally, we did not expect our analysis of age-spacing to alter our earlier findings. Contrary to Zajonc's findings, we did not find that the average test score for pairs of adjacent siblings rose as age-spacing rose (Table 5). Nor did we find that the average difference between the scores of elder and adjacent younger brothers varied systematically by age-spacing (Table 6). In individual-level regressions for men from pairs of adjacent brothers, the coefficient for absolute age difference was negligible and insignificant (not shown); nor was a multiplicative birth order by absolute age difference interaction significant among such individuals. Within pairs of adjacent brothers, a birth order by signed age difference interaction was insignificant.

Table 5

Average Test Score by Spacing for Pairs of Adjacent Brothers

Years Apart	Test Score	N of Pairs
1	102.9	33
2	104.3	75
3	101.3	50
4 or more	103.1	89
Total	103.1	248
$Eta^2 = .0060$		

Table 6

Average Test Score Difference Between Elder and Younger Brothers Among Adjacent Pairs by Spacing

Years Apart	Elders' Score Minus Youngers' Score	N of Pairs
1	-2.75	33
2	3.53	75
3	-1.92	50
4	-2.88	27
5	0.45	22
6	3.56	14
7	-1.03	12
8 or more	-7.18	14
Total	-0.21	248

$Eta^2 = .0619$

Education

Educational attainment is substantially correlated with test scores. (In our sibling subsample analyzed here, $r_{ED,TEST} = 0.543$.) Therefore, we might expect our results for education to be quite similar to those for test scores. On the other hand, we know that family background exercises a substantial effect on educational attainment that is not mediated by test scores (Olneck, 1976). From this we might reason that differences in educational attainment within families might not be explained solely by differences on test scores, and that within-family processes unrelated to test scores need to be taken into account. Indeed, the correlation between sibling differences on education and on test scores is only 0.349 in our present subsample. We therefore suspected that the pattern of birth order effects on education might well differ from the pattern of effects on test scores, particularly since education may depend on parental finances and preferences in ways that intellectual development might not. Whatever the theoretical possibilities, however, we are struck by the similarity between our findings for education and test scores. The signs of some variables do differ, but the principal finding is replicated: no substantial or significant effects of birth order.

Table 7 shows that while there is a general tendency for attainment to fall with birth order, the pattern within family sizes is inconsistent. Because educational attainment rose over time, we would expect the educational disadvantage of later-borns in a given family size to be minimized because of their younger age. In our regression analyses, we take into account age as well as family size.

Among individuals in our subsample of brothers, an increase in birth order is associated with a third-of-a-year decline in educational attainment. But controlling age and family size reduces the coefficient of birth order to an insignificant -0.102. Controlling test scores as well reduces the coefficient to virtually zero (Table 8, equations (1-3)).

Within families, the linear relationship between birth order and education is also virtually zero. Controlling brothers' age differences raises the effect to about a third-of-a-year, but the coefficient is insignificant. It is reduced to an insignificant -0.185 when sibling test score differences are controlled (Table 9, equations (1-3)). These results suggest that the average effects of birth order on educational attainment are negligible, and largely mediated by cognitive skills.

None of our nonlinear terms is significant in either our individual or within-pair equations (Table 8, equations (4-6); Table 9, equations (4-6)). With age differences between brothers controlled, the signs of all but one of the nonlinear coefficients are negative as expected, but the magnitudes of the coefficients fluctuate inconsistently. We cannot identify any interpretable pattern of the effects of specific birth orders on education within families.

First-borns in our sample have an average of two-thirds of a year more schooling than others. But controlling number of siblings and age reduces this effect to an insignificant advantage of 0.4 years (Table 8, equations (1), (2)). Controlling age differences, first-borns have only an insignificant and negligible 0.04 year educational advantage over their own brothers (Table 9, equation (8)).

Table 7

Average Education by Birth Order and Family Size

Family Size	1	2	3	4	5	6	7	8 or more*	Total
2	13.83 (83)	13.95 (88)							13.89 (171)
3	13.66 (94)	13.57 (115)	13.87 (99)						13.69 (308)
4	13.54 (54)	13.31 (58)	13.91 (69)	13.35 (63)					13.54 (244)
5	12.57 (21)	12.35 (31)	12.18 (40)	12.85 (27)	12.77 (26)				12.50 (145)
6	11.10 (10)	12.19 (21)	11.95 (22)	12.23 (26)	12.19 (26)	11.50 (14)			11.98 (119)
7	11.00 (5)	10.90 (10)	11.90 (10)	11.92 (12)	11.75 (12)	12.71 (7)	12.20 (15)		11.82 (71)
8 or more*	11.8 (10)	8.31 (13)	11.36 (14)	11.41 (17)	10.57 (23)	12.35 (17)	10.95 (21)	11.37 (46)	11.43 (161)
Total	13.40 (277)	13.31 (336)	13.23 (254)	12.71 (145)	11.87 (87)	12.11 (38)	11.47 (36)	11.37 (46)	12.97 (1219)

*Run ungrouped.

(N)

Table 8

Individual Level Regressions of Educational Attainment

(N = 548)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
BRTHORD	-.332 (.081)	[-.102] (.100)	[-.002] (.083)				
BRTHORD2				[-.309] (.307)	[-.381] (.298)	[-.301] (.261)	
BRTHORD3						[.183] (.277)	
BRTHORD4						[-.256] (.352)	
BRTHORD5						[.284] (.511)	
BRTHORD6						[.111] (.783)	
BRTHORD7						[.435] (1.063)	
BRTHORD8						[-.012] (1.545)	
FIRST							.660 (.265)
LAST							
SIBS		[-.438] (.292)	[-.103] (.256)		[-.270] (.319)	[-.019] (.280)	
SIBSSQ		[.004] (.039)	[-.021] (.034)		[-.021] (.043)	[-.034] (.038)	
AGE		-.092 (.019)	-.053 (.017)		-.097 (.019)	-.056 (.017)	
TEST			.085 (.007)				
C	14.385	19.308	7.811	14.030	19.360	8.020	13.369
$\bar{R}^2$	.028	.117	.324	.021	.113	.320	.009
Standard Deviation of Residuals	2.689	2.563	2.242	2.699	2.568	2.249	2.714

Table 8--Continued.

	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
BR								
2								
3								
4								
5								
6								
7								
8								
FIRST	[.426] (.272)	[.246] (.239)				.950 (.284)	[.366] (.308)	[.264] (.270)
LAST			[.410] (.257)	[-.283] (.257)	[-.080] (.225)	.752 (.274)	[-.122] (.291)	[.036] (.255)
SIBS	[-.401] (.293)	[-.059] (.257)		-.535 (.252)	[-.119] (.258)		[-.435] (.304)	[-.049] (.268)
SIBSSQ	[-.004] (.039)	[-.024] (.034)		.007 (.039)	[-.020] (.035)		[-.001] (.040)	[-.025] (.035)
AGE	-.095 (.019)	-.057 (.017)		-.093 (.019)	-.054 (.017)		-.096 (.019)	-.056 (.017)
TEST		.084 (.007)			.085 (.007)			.085 (.007)
C	19.033	7.871	13.421	19.404	7.937	13.080	19.201	7.816
$\bar{R}^2$	.119	.325	.003	.117	.324	.201	.117	.324
St. D of Res.	2.560	2.240	2.723	2.563	2.242	2.698	2.562	2.242

Note: Standard error of regression coefficients shown in parentheses.
Bracketed coefficients less than 1.96 times their standard errors.

Table 9
Regressions of Education Controlling Brothers' Common Family Background
(N = 274 pairs)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
BRTHORD	[.020] (.100)	[-.313] (.216)	[-.185] (.204)				
BRTHORD2				[.113] (.245)	[-.224] (.308)	[-.141] (.291)	
BRTHORD3				[.049] (.286)	[-.307] (.347)	[-.155] (.328)	
BRTHORD4				[-.391] (.377)	[-.737] (.423)	[-.593] (.399)	
BRTHORD5				[.181] (.575)	[-.121] (.596)	[.130] (.564)	
BRTHORD6				[1.432] (.905)	[1.091] (.921)	[.780] (.870)	
BRTHORD7				[-1.203] (1.250)	[-1.732] (1.280)	[-1.510] (1.207)	
BRTHORD8				[-.599] (1.618)	[-.825] (1.616)	[-.423] (1.525)	
FIRST							[-.095] (.228)
LAST							
AGE		[-.116] (.067)	[-.080] (.063)		[-.121] (.067)	[-.086] (.064)	
TEST			.058 (.010)			.057 (.010)	
$\bar{R}^2$	.511	.515	.570	.508	.615	.566	.511
Standard Deviation of Residuals*	1.907	1.900	1.788	1.913	1.693	1.796	1.907

* See Table 4.

Table 9--Continued.

	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
BR								
2								
3								
4								
5								
6								
7								
8								
FIRST	[.044] (.275)	[.022] (.258)				[-.159] (.269)	[-.033] (.284)	[-.055] (.266)
LAST			[-.036] (.218)	[-.338] (2.97)	[-.331] (.278)	[-.116] (.257)	[-.347] (.306)	[-.345] (.287)
AGE	[-.034] (.038)	[-.031] (.035)		[-.063] (.042)	[-.061] (.040)		[-.062] (.045)	[-.058] (.042)
TEST		.059 (.010)			.059 (.010)			.059 (.010)
$\bar{R}^2$	.511	.569	.511	.513	.571	.510	.511	.570
St. D. of Res.*	1.908	1.791	1.908	1.903	1.786	1.910	1.907	1.789

Note: Standard error of regression coefficients shown in parentheses. Bracketed coefficients less than 1.96 times their standard errors.

* See Table 4.

Last-borns do not differ significantly from their brothers, and first- and last-borns do not differ significantly from their middle-born brothers (Table 9, equations (10-14)).

We conclude that differences between brothers in educational attainment cannot be explained by either direct or indirect effects of birth order.

Occupational Status and Earnings

We investigated the effects of birth order on the occupational status of the jobs men held when they first completed their schooling and of their current jobs, and on the natural logarithm of their current earnings. We could identify no appreciable or significant effects which persisted when family size and age, or family background common to brothers were controlled. Changes in already insignificant coefficients due to entering intervening variables (e.g., education and test scores) generally suggest that even if larger samples were to evidence significant birth order effects on adult economic attainment, such effects would be mediated by education and cognitive skills. Tables 10 through 18 give the results of our analyses.

5. THE EFFECTS OF FAMILY SIZE

We attributed a large portion of the apparent effects of birth order to the fact that men from later birth orders tend to come from larger families. Among men who share sibling position, however, there are persistent effects of family size.[9] Men from larger families tend to have lower test scores, less education, lower occupational statuses, and lower earnings than men from smaller families. This is a common finding in socioeconomic survey data (Duncan, Featherman, and Duncan 1972), and our findings are no discovery.

Nevertheless, we thought it worthwhile to explore in detail several questions about the effects of family size that are not usually given close attention in the sociological literature. We wanted to know if having additional siblings[10] exercises uniform or nonuniform effects. We wanted to test the likelihood that parental IQ or socioeconomic status accounts for the apparent impact of family size. We wanted to know for which outcomes the disadvantages of coming from a larger family persist. And, finally, we wanted to know if the backgrounds shared by brothers from large families were less similar than the backgrounds shared by brothers from smaller families.

Time did not permit us to explore the nonuniform effects of increased family size as fully as we would have liked. We would have preferred to construct dummy variables representing the effects of each additional sibling. Instead, we relied on a specification employing linear, and orthogonal square terms.

The positive significant coefficients for the Sibling^2 terms indicate that, on the average, the negative impact of having additional siblings diminishes as family size grows (Table 18). For example, we would predict that adding a second sibling would reduce a respondent's test score by 3.7 points, but that adding a fourth sibling would reduce a respondent's test score by only 2.8 points. Similarly, we would predict that adding a second sibling would reduce a respondent's educational attainment by 0.65 years, but that adding a fourth sibling would reduce educational attainment by only 0.52 years.[11]

Table 10.

Average Occupational Status of First Job

by Birth Order and Family Size

Family Size	Birth Order 1	2	3	4	5	6	7	8 or more*	Total
2	42.46 (83)	45.77 (88)							44.16 (171)
3	44.37 (93)	42.27 (114)	43.49 (96)						43.30 (303)
4	41.15 (54)	42.19 (57)	45.08 (66)	42.58 (62)					42.85 (239)
5	35.38 (21)	32.00 (31)	27.67 (39)	34.50 (26)	38.04 (26)				32.87 (143)
6	34.20 (10)	31.90 (20)	33.73 (22)	34.92 (24)	32.27 (26)	23.00 (14)			32.08 (116)
7	23.25 (4)	22.11 (9)	26.80 (10)	28.58 (12)	29.58 (12)	30.29 (7)	31.00 (15)		28.04 (69)
8 or more*	32.20 (10)	30.50 (12)	27.92 (12)	28.80 (15)	21.05 (22)	34.50 (16)	27.80 (20)	27.41 (46)	27.65 (153)
Total	41.35 (275)	40.63 (331)	39.08 (245)	37.05 (139)	30.77 (86)	29.35 (37)	27.54 (35)	27.41 (46)	38.11 (1194)

*Run ungrouped.

(N)

Table 11
Individual Level Regressions of Early Occupational Status
(N = 548)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
BRTHORD	-2.765 (.730)	[-.812] (.915)	[-.069] (.845)				
BRTHORD2				[-3.141] (2.762)	[-3.380] (2.733)	[-.921] (1.931)	
BRTHORD3				[-1.859] (2.881)	[-.570] (2.894)	[.002] (2.046)	
BRTHORD4				[-3.981] (3.732)	[-1.357] (3.685)	[-.193] (2.601)	
BRTHORD5				[-4.314] (5.421)	[-.678] (5.325)	[1.896] (3.772)	
BRTHORD6				[-8.015] (8.456)	[-3.730] (8.284)	[-8.476] (5.851)	
BRTHORD7				[11.510] (11.389)	[14.556] (11.106)	[8.534] (7.839)	
BRTHORD8				[4.533] (16.576)	[8.982] (16.151)		
FIRST							5.996 (2.382)
LAST							
SIBS		[-3.179] (2.674)	[-.687] (2.492)		[-1.411] (2.918)	[.584] (2.063)	
SIBSSQ		[-.022] (.359)	[-.207] (.331)		[-.283] (.398)	[-.168] (.281)	
AGE		-.595 (.171)	[-.300] (.160)		-.635 (.173)	[.014] (.126)	
TEST			[.606] (.055)			.109 (.056)	
ED			6.195 (.317)			6.179 (.318)	
C	48.846	80.968	-52.939	46.267	81.380	-52.865	40.272
$\bar{R}^2$	.024	.080	.542	.019	.078	.541	.010
Standard Deviation of Residuals	24.207	23.502	16.580	24.272	23.520	16.590	24.382

Table 11—Continued.

	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
BR								
2								
3								
4								
5								
6								
7								
8								
FIRST	[3.780] (2.495)	[.918] (1.767)				8.134 (2.557)	[3.261] (2.825)	[.870] (1.998)
LAST			[2.622] (2.312)	[-2.483] (2.356)	[-.479] (1.666)	5.548 (2.471)	[-1.046] (2.664)	[-.098] (1.884)
SIBS	[-2.813] (2.683)	[.097] (1.905)		[-3.989] (2.679)	[-.163] (1.905)		[-3.104] (2.785)	[.069] (1.980)
SIBSSQ	[-.088] (.360)	[-.090] (.255)		[.007] (.361)	[-.069] (.255)		[-.063] (.366)	[-.087] (.259)
AGE	-.621 (.171)	[.013] (.124)		-.603 (.171)	[.020] (.124)		-.630 (.173)	[.012] (.126)
TEST		[.105] (.055)			[.105] (.055)			[.105] (.055)
ED		6.188 (.318)			6.194 (.317)			6.188 (.318)
C	78.805	-52.840	41.060		-52.336	38.134	80.248	-52.690
$\bar{R}^2$	.542	.542	.001		.542	.017	.081	.541
St. D. of Res.	16.576	16.576	24.494		16.579	24.292	23.488	16.591

Note: Standard error of regression coefficients shown in parentheses.
Bracketed coefficients less than 1.96 times their standard errors.

Table 12
Regression of Early Occupational Status Controlling Brothers' Common Family Background
(N = 274 pairs)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
BRTHORD	[-.181] (1.016)	[-1.903] (2.198)	[.064] (1.776)				
BRTHORD2				[-.054] (2.496)	[-1.657] (3.150)	[-.288] (2.525)	
BRTHORD3				[-1.115] (2.913)	[-2.805] (3.558)	[-.823] (2.849)	
BRTHORD4				[-1.043] (3.851)	[-2.685] (4.333)	[1.931] (3.478)	
BRTHORD5				[.565] (5.861)	[-.867] (6.114)	[.027] (4.893)	
BRTHORD6				[2.993] (9.234)	[1.374] (9.444)	[-5.525] (7.561)	
BRTHORD7				[8.099] (12.753)	[5.587] (13.117)	[16.367] (10.502)	
BRTHORD8				[7.300] (16.501)	[6.230] (16.562)	[11.546] (13.231)	
FIRST							[.517] (2.314)
LAST							
AGE		[-.603] (.682)	[-.124] (.551)		[-.572] (.691)	[.190] (.556)	
TEST			[.034] (.089)			[.035] (.090)	
ED			6.050 (.529)			6.144 (.534)	
$\bar{R}^2$	.376	.375	.598	.366	.365	.596	.376
Standard Deviation of Residuals*	19.360	19.368	15.541	19.514	19.525	15.580	19.359

Note: Standard error of regression coefficients shown in parentheses. Bracketed coefficients less than 1.96 times their standard errors.

* See Table 4.

Table 12--Continued.

	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
BR								
2								
3								
4								
5								
6								
7								
8								
FIRST	[1.229] (2.798)	[.947] (2.242)				[-.606] (2.725)	[.376] (2.880)	[.560] (2.313)
LAST			[-1.731] (2.209)	[-3.929] (3.014)	[-1.888] (2.427)	[-2.036] (2.604)	[-3.831] (3.111)	[-1.742] (2.504)
AGE	[-.173] (.381)	[.034] (.306)		[-.460] (.430)	[-.078] (.346)		[-.479] (.454)	[-.106] (.367)
TEST		[.034] (.089)			[.035] (.089)			[.035] (.089)
ED		6.048 (.528)			6.019 (.529)			6.021 (.530)
$\bar{R}^2$	.374	.607	.377	.377	.599	.375	.375	.597
St. D. of Res.*	19.388	15.36	19.339	19.334	15.523	19.373	19.369	15.551

* See Table 4.

Table 13

Average Occupational Status of Current Job by Birth Order and Family Size

Family Size	Birth Order 1	2	3	4	5	6	7	8 or more*	Total
2	55.87 (83)	56.94 (90)							56.43 (173)
3	54.12 (93)	54.16 (114)	53.10 (97)						53.81 (304)
4	53.21 (52)	49.02 (57)	54.52 (67)	51.52 (63)					52.13 (239)
5	49.67 (21)	45.20 (30)	37.35 (40)	41.67 (27)	50.31 (26)				43.93 (144)
6	41.20 (10)	44.10 (21)	47.32 (22)	44.27 (26)	44.81 (26)	41.57 (14)			44.34 (119)
7	30.75 (4)	40.90 (10)	42.20 (10)	35.64 (11)	30.17 (12)	50.29 (7)	41.40 (15)		38.86 (69)
8 or more*	42.90 (10)	44.92 (13)	45.92 (13)	35.06 (16)	40.81 (21)	50.47 (17)	42.26 (19)	40.93 (46)	42.40 (155)
Total	52.91 (273)	51.84 (335)	49.63 (249)	45.28 (143)	43.44 (85)	47.16 (38)	41.88 (34)	40.93 (46)	49.41 (1203)

*Run ungrouped.

(N)

Table 14
Individual Level Regressions of Current
Occupational Status
(N = 548)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
BRTHORD	-2.954	[-.941]	[-.197]				
	(.680)	(.853)	(.672)				
BRTHORD2				[-1.965]	[-1.911]	[.156]	
				(2.577)	(2.551)	(2.008)	
BRTHORD3				[-3.759]	[-2.273]	[-1.378]	
				(2.688]	(2.701)	(2.127)	
BRTHORD4				[-5.318]	[-2.618]	[-1.921]	
				(3.482)	(3.440)	(2.704)	
BRTHORD5				[-2.617]	[.921]	[3.403]	
				(5.056)	(4.971)	(3.922)	
BRTHORD6				[.956]	[4.921]	[2.926]	
				(7.887)	(7.733)	(6.095)	
BRTHORD7				[2.235]	[5.063]	[-2.324]	
				(10.623)	(10.367)	(8.158)	
BRTHORD8				[-4.467]	[-.509]	[.732]	
				(15.462)	(15.077)	(11.869)	
FIRST							5.912
							(2.228)
LAST							
SIBS		[-4.063]	[-1.213]		[-1.994]	[-.268]	
		(2.494)	(1.966)		(2.724)	(2.145)	
SIBSSQ		[.100]	[.029]		[-.204]	[-.110]	
		(.335)	(.263)		(.371)	(.292)	
AGE		-4.789	[.015]		-.516	[.002]	
		(.160)	(.129)		(.162)	(.131)	
TEST			.233			.233	
			(.058)			(.058)	
ED			2.377			2.359	
			(.429)			(.432)	
YNGOC			.280			.280	
			(.045)			(.045)	
C	59.389	87.281	-12.784	56.267	86.449	-13.248	50.356
$\bar{R}^2$	.032	.086	.438	.025	.083	.434	.011
Standard Deviation of Residuals	22.561	22.919	17.189	22.641	21.956	17.247	22.800

Note: Standard error of regression coefficients shown in parentheses.
Bracketed coefficients less than 1.96 times their standard errors.

Table 14--Continued.

	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
BR								
2								
3								
4								
5								
6								
7								
8								
FIRST	[3.106] (2.329)	[.539] (1.833)				8.186 (2.388)	[2.815] (2.637)	[.752] (2.072)
LAST			[2.956] (2.162)	[-1.826] (2.199)	[.102] (1.728)	5.900 (2.307)	[-.586] (2.487)	[.431] (1.953)
SIBS	[-3.881] (2.504)	[-1.195] (1.976)		[-4.808] (2.500)	[-1.274 (1.975)		[-4.044] (2.600)	[-1.073] (2.053)
SIBSSQ	[.041] (.336)	[.018] (.264)		[.116] (.337)	[.024] (.265)		[.055] (.342)	[.0078] (.269)
AGE	-.489 (.160)	[.015] (.129)		-.470 (.160)	[.026] (.129)		-.494 (.162)	[.019] (.130)
TEST		.234 (.057)			.234 (.058)			.234 (.058)
ED		2.375 (.429)			2.377 (.429)			2.374 (.430)
YNGOC		.279 (.045)			.280 (.045)			.279 (.045)
C	84.690	-28.133	51.026	87.063	-13.710		85.499	-14.031
$\bar{R}^2$	.087	.398	.002	.085	.438		.085	.437
St. D. of Res.	21.908	17.786	22.907	21.929	17.190		21.927	17.204

Table 15

Regression of Current Occupational Status Controlling Brothers' Common Family Background (N = 274 pairs)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
BRTHORD	[-.042] (.988)	[-2.262] (2.136)	[-.550] (1.878)				
BRTHORD2				[1.007] (2.419)	[-1.222] (3.058)	[.027] (2.684)	
BRTHORD3				[-1.340] (2.823)	[-3.689] (3.443)	[-1.685] (3.029)	
BRTHORD4				[-3.068] (3.732)	[-5.350] (4.193)	[-2.461] (3.899)	
BRTHORD5				[2.636] (5.680)	[.646] (5.917)	[2.165] (5.200)	
BRTHORD6				[11.656] (8.949)	[9.405] (9.114)	[5.358] (8.044)	
BRTHORD7				[-3.998] (12.360)	[-7.491] (12.694)	[-4.113] (11.213)	
BRTHORD8				[1.333] (15.992)	[-.156] (16.029)	[1.807] (14.082)	
FIRST							[-.310] (2.251)
LAST							
AGE		[-.776] (.663)	[-.211] (.583)		[-.796] (.669)	[-.244] (.591)	
TEST			.240 (.094)			.237 (.095)	
ED			2.292 (.682)			2.211 (.696)	
YNGOC			.244 (.064)			.246 (.066)	
$\bar{R}^2$	.325	.326	.487	.320	.321	.478	.327
Standard Deviation of Residuals*	18.831	18.818	16.428	18.911	18.896	16.558	18.803

Note: Standard error of regression coefficients shown in parentheses. Bracketed coefficients less than 1.96 times their standard errors.

* See Table 4.

Table 15--Continued.

	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
BR								
2								
3								
4								
5								
6								
7								
8								
FIRST	[.474] (2.721)	[-.021] (2.372)				[-.131] (2.654)	[.351] (2.809)	[.243] (2.447)
LAST			[.391] (2.151)	[-.646] (2.940)	[1.130] (2.570)	[.325] (2.536)	[-.555] (3.034)	[1.193] (2.653)
AGE	[-.191] (.371)	[-.058] (.323)		[-.217] (.419)	[.051] (.367)		[-.235] (.444)	[.038] (.388)
TEST		.242 (.094)			.241 (.094)			.241 (.094)
ED		2.301 (.681)			2.311 (.681)			2.312 (.683)
YNGOC		.244 (.064)			.245 (.065)			.245 (.065)
$\bar{R}^2$	.324	.486	.325	.324	.487	.323	.321	.485
St. D. of Res.*	18.856	16.431	18.830	18.855	16.425	18.865	18.890	16.455

* See Table 4.

Table 16

Average Natural Logarithm of Earnings by Birth Order and Family Size

Family Size	Birth Order 1	2	3	4	5	6	7	8 or more*	Total
2	9.70 (66)	9.69 (80)							9.69 (146)
3	9.68 (89)	9.69 (105)	9.70 (89)						9.69 (283)
4	9.62 (46)	9.49 (50)	9.69 (63)	9.64 (60)					9.62 (219)
5	9.55 (19)	9.49 (29)	9.56 (36)	9.42 (26)	9.70 (82)				9.54 (132)
6	9.72 (10)	9.52 (21)	9.67 (20)	9.63 (25)	9.56 (25)	9.53 (13)			9.60 (114)
7	9.29 (5)	9.44 (10)	9.19 (9)	9.39 (10)	9.33 (10)	9.62 (7)	9.46 (14)		9.39 (65)
8 or more*	9.63 (8)	9.66 (13)	9.56 (11)	9.45 (16)	9.50 (22)	9.42 (13)	9.54 (17)	9.52 (44)	9.52 (144)
Total	9.66 (243)	9.62 (308)	9.65 (228)	9.56 (137)	9.55 (79)	9.51 (33)	9.50 (31)	9.52 (44)	9.61 (1103)

*Run ungrouped.

(N)

Table 17
Individual Level Regressions of Natural Logarithm of Earnings
(N = 548)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
BRTHORD	-.041 (.014)	[-.013] (.018)	[-.000] (.015)				
BRTHORD2				[-.016] (.052)	[-.014] (.052)	[.010] (.046)	
BRTHORD3				[.005] (.054)	[.035] (.055)	[.062] (.049)	
BRTHORD4				[-.132] (.070)	[-.095] (.071)	[-.074] (.062)	
BRTHORD5				[-.007] (.102)	[.032] (.102)	[.055] (.090)	
BRTHORD6				[-.222] (.159)	[-.187] (.159)	[-.259] (.139)	
BRTHORD7				[.200] (.214)	[.219] (.213)	[.147] (.186)	
BRTHORD8				[.069] (.311)	[.093] (.309)	[.141] (.271)	
FIRST							[.058] (.045)
LAST							
SIBS		[-.076] (.051)	[-.025] (.045)		[-.075] (.056)	[-.044] (.049)	
SIBSSQ		[.004] (.007)	[.002] (.006)		[.004] (.008)	[.005] (.007)	
AGE		[-.006] (.003)	[.001] (.003)		[-.006] (.003)	[.001] (.003)	
TEST			.003 (.001)			.004 (.001)	
ED			.017 (.008)			.017 (.009)	
OC			.007 (.001)			.007 (.001)	
C	9.747	10.137	8.694	9.684	10.111	8.653	9.626
$\bar{R}^2$	.015	.038	.261	.013	.034	.262	.001
Standard Deviation of Residuals	.455	.450	.394	.456	.451	.394	.458

Note: Standard error of regression coefficients shown in parentheses.
Bracketed coefficients less than 1.96 times their standard errors.

Table 17--Continued.

	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
BR								
2								
3								
4								
5								
6								
7								
8								
FIRST	[.011] (.048)	[-.027] (.042)				[.092] (.048)	[.006] (.054)	[-.025] (.047)
LAST			[.053] (.043)	[-.012] (.045)	[.015] (.040)	[.086] (.046)	[-.009] (.051)	[.004] (.045)
SIBS	[-.080] (.051)	[-.030] (.045)		[-.084] (.051)	[-.022] (.045)		[-.082] (.053)	[-.029] (.047)
SIBSSQ	[.004] (.007)	[.003] (.006)		[.004] (.007)	[.002] (.006)		[.004] (.007)	[.002] (.006)
AGE	[-.006] (.003)	[.001] (.003)		[-.006] (.003)	[.001] (.003)		[.006] (.003)	[.001] (.003)
TEST		.003 (.001)			.003 (.001)			.003 (.001)
ED		.017 (.009)			.017 (.008)			.017 (.009)
OC		.007 (.001)			.007 (.001)			.007 (.001)
C	10.099	8.684	9.626	10.115	8.668	9.593	10.111	8.679
$\bar{R}^2$	.037	.262	.001	.037	.261	.006	.035	.260
St. D. of Res.	.450	.394	.458	.450	.394	.457	.450	.394

Table 18
Regression of Natural Logarithm of Earnings Controlling
Brothers' Common Family Background
(N = 278 pairs)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
BRTHORD	[.012] (.021)	[-.070] (.046)	[-.039] (.042)				
BRTHORD2				[.025] (.052)	[-.054] (.065)	[-.035] (.060)	
BRTHORD3				[.005] (.060)	[-.078] (.073)	[-.034] (.067)	
BRTHORD4				[-.076] (.080)	[-.157] (.089)	[-.103] (.082)	
BRTHORD5				[.110] (.122)	[.039] (.126)	[.067] (.116)	
BRTHORD6				[.015] (.192)	[-.065] (.195)	[-.169] (.179)	
BRTHORD7				[.254] (.265)	[.130] (.271)	[.210] (.248)	
BRTHORD8				[.322] (.343)	[.269] (.342)	[.323] (.312)	
FIRST							[-.021] (.048)
LAST							
AGE		-.029 (.014)	[-.019] (.013)		-.028 (.014)	[-.018] (.013)	
TEST			.007 (.002)			.007 (.002)	
ED			[-.000] (.013)			[.001] (.014)	
OC			.007 (.001)			.007 (.001)	
$\bar{R}^2$	.223	.231	.359	.219	.227	.356	.223
Standard Deviation or Residuals*	.404	.402	.367	.405	.403	.368	.404

Note: Standard error of regression coefficients shown in parentheses.
Bracketed coefficients less than 1.96 times their standard errors.

* See Table 4.

Table 18--Continued.

	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
BR								
2								
3								
4								
5								
6								
7								
8								
FIRST	[.025] (.058)	[.019] (.053)				[-.004] (.057)	[.022] (.060)	[.016] (.055)
LAST			[.034] (.046)	[-.020] (.063)	[-.014] (.057)	[.032] (.054)	[-.014] (.065)	[-.010] (.059)
AGE	[-.011] (.008)	[-.009] (.007)		[-.011] (.009)	[-.009] (.008)		[-.012] (.009)	[-.010] (.009)
TEST		.007 (.002)			.007 (.002)			.007 (.002)
ED		[.001] (.013)			[.000] (.013)			[.000] (.014)
OC		.007 (.001)			.007 (.001)			.007 (.001)
$\bar{R}^2$	.227	.359	.223	.227	.359	.223	.223	.356
St. D. of Res.*	.403	.367	.404	.403	.367	.404	.404	.368

* See Table 4.

Unfortunately, inspection of Tables 2 and 7 indicate that these predictions are not accurate for the pool of interviewees from which the subsample of complete pairs of brothers on which the regression analyses were conducted was drawn. The drop in test scores between families of four and five is 5.7 points (Table 2). The decrease in educational attainment between families of two and three is 0.20 years, while the decrease between families of four and five is 1.04 years. We have not constructed tables analogous to Tables 2 and 7 for our subsample of brothers, but we suspect that the predictions from the regression analyses would still be discrepant if we did. This is because our estimate of the diminishing impact of family size probably derives from the relative equality of outcomes among men from families of five and more, and not from a consistent nonlinear effect along the entire distribution of family sizes. Jackson (1977), however, reports more consistent, though smaller nonlinear effects in the 1962 OCG-I data than are evident in our data. We put more faith in Jackson's results than in our own, and conclude only that researchers analyzing the effects of family size should be careful to take into account nonlinearities.

We have no ready way of distinguishing psychological from economic explanations for the diminishing impact of family size as family size grows. We would argue, however, that such explanations may to some extent be superfluous. This is because the differences in family size among smaller families may be more strongly related to socioeconomic differences than are differences in family size among larger families. We have not yet investigated this possibility directly, but we did find that the nonlinear sibling term is reduced to insignificance when background measures

Table 19
Regression of Respondents' Characteristics on Number of Siblings
(N = 692)

Dependent Variable		Siblings	$Sibling^2$ (Orthogonal)	R^2	Other Variables Controlled
Test Score	1.	-1.675 (.222)		.076	None
	2.	-1.671 (.220)	.225 (.061)	.094	None
	3.	-1.162 (.229)	.154 (.060)	.162	POPED, POPOC, POPWHCOL, NOMALE, POPNAT, MOMED, AGE
Education	1.	- .354 (.039)		.107	None
	2.	- .354 (.039)	.032 (.011)	.118	None
	3.	- .203 (.037)	[.016] (.010)	.333	POPED, POPOC, POPWHCOL, NOMALE, POPNAT, MOMED, AGE
	4.	- .118 (.033)	[.005] (.009)	.488	POPED, POPOC, POPWHCOL, NOMALE, POPNAT, MOMED, AGE, SES interactions, $POPED^2$, TEST
Early Occupation	1.	-2.414 (.347)		.066	None
	2.	-2.414 (.345)	.267 (.096)	.076	None
	3.	-1.238 (.340)	[.141] (.089)	.242	POPED, POPOC, POPWHCOL, NOMALE, POPNAT, MOMED, $POPED^2$, SES interactions, AGE
	4.	[-.045] (.267)	[-.020] (.069)	.578	As in Equation 3, plus TEST, EDUC, square and interaction terms.
Current Occupation	1.	-2.021 (.341)		.049	None
	2.	-2.021 (.338)	.278 (.094)	.060	None

Table 19--Continued.

Dependent Variable	Siblings	$Sibling^2$ (Orthogonal)	R^2	Other Variables Controlled
Current Occupation	3. -1.199 (.356)	.199 (.093)	.125	POPED, POPOC, POPWHCOL, NOMALE, POPNAT, MOMED, $POPED^2$, AGE, SES interactions
	4. [- .061] (.309)	[.101] (.079)	.377	As in Equation 3, plus TEST, EDUC
Earnings	1. -467 (113)		.024	None
	2. -467 (113)	82 (32)	.033	None
	3. -292 (119)	66 (31)	.090	POPED, POPOC, POPWHCOL, NOMALE, POPNAT, MOMED, AGE, SES interactions
	4. [-9] (113)	[38] (29)	.23	As in Equation 3, plus TEST, EDUC, $TEST^2$

Note: POPED - Father's Education

POPOC - Father's Occupational Status

POPWHCOL - Father White Collar

POPNAT - Father U.S. Born

MOMED - Mother's Education

AGE - Age

TEST - Test Score

EDUC - Years Schooling Completed

NOMALE - Female Headed Family

All individuals from Kalamazoo Brothers complete data sample.

are controlled in the education and early occupation equations, and it is reduced appreciably in the equations for other outcomes (Table 19).

Smaller families tend to be of higher socioeconomic status. Since higher socioeconomic status is positively related to all of our achievement and attainment measures, we anticipated that the apparent effects of family size might disappear when we controlled socioeconomic background. They did not. Even among men whose measurable family characteristics are similar, larger families result in significant disadvantages.[12]

For example, the linear effect of additional siblings on test scores is barely reduced when measures of father's education, mother's education, father's occupational status, father's occupational grouping, father's nativity, and family composition are controlled. If parents with lower cognitive ability had more children while ability and socioeconomic status were only modestly related, we would expect this result since there is a substantial correlation between the test scores of parents and children (Jencks, et al. 1972). We have no direct evidence on this point, but we do know that the correlation between our respondents' test scores and the number of children they have is virtually zero.[13] Unless there have been significant changes in the relationship between cognitive ability and fertility, this evidence suggests that parental ability is not the explanation for the relationship between family size and test scores among our respondents.

Since the effects of family size are not reduced when birth order is controlled, they must arise for reasons that affect members of given family sizes similarly. Two possibilities are consistent with this constraint. One is that parents, who for reasons unrelated to parental test scores choose to have more children, socialize them in ways antithetical to high test performance. Under this interpretation, larger families and lower test scores reflect a common, unmeasured characteristic. Alternatively, lower test scores may reflect the lower per capita availability of tangible resources in larger families. If parental income rose to keep pace with additional children, the per capita resources available to children in a given family might remain constant despite growing family size. We find either of these possibilities preferable to explanations that stress the consequences of variations in time inputs or other finite psychological resources between larger and smaller families. Such explanations would also seem to imply substantial birth order effects within families, and we found none.[14]

The effects of family size on outcomes other than test scores (i.e., education), early and current occupational status, and earnings are reduced appreciably when socioeconomic background is controlled, but they remain significant nevertheless. We have not yet checked the extent to which controlling background reduces the effects of family size because background itself is related to the factors intervening between family size and outcomes. We do not yet know, for example, whether controlling background after controlling test scores would further reduce the effect of family size on educational attainment.

Men from larger families with socioeconomic backgrounds and test scores similar to those of men from smaller families get significantly less education, but the differential is small--about one-tenth of a year per additional sibling. For men with similar backgrounds, test scores, and educational attainments, there are no significant effects of family size on measures of adult economic success. These results suggest that the most important independent consequence of larger families for the attainment process is lowered cognitive skill. The test score decrement is associated with lower educational attainment, and together these explain virtually all the continuing effects of family size on occupational status and earnings.[15]

Throughout our discussion, we have been concerned with the effects of family size on mean levels of achievement or attainment. Family size may, however, also have consequences for the salience of family background. Men from larger families might "go it alone" more often than men from smaller families. The presence of a large number of children might accentuate differences among siblings more than is the case in small families. In either event, men from larger families would have backgrounds less in common with their siblings than would men from smaller families.

To test this possibility we compared sibling correlations for men from larger and smaller families. Since we were interested only in how alike men were to their siblings relative to variability among men from similar family sizes in general, we did not compare absolute differences between brothers from larger and smaller families. Regardless of absolute differences or point estimates of within-family variability, we defined higher sibling correlations as evidence of greater common background.

One obvious source of greater dissimilarity among brothers from larger families is the higher average age spread than obtains among brothers from smaller families. Arbitrarily dividing our sample into men with three or less siblings and men with four or more siblings, we found the correlations between brothers' ages to be 0.658 in the first group and only 0.460 in

Table 20
Sibling Correlations Between Errors Net of the Effects of Age for Men From Smaller and Larger Families

Outcome	Three or Less Siblings (N=219 pairs)	Four or More Siblings (N=140 pairs)
Test Scores	.454	.319
Education	.502	.440
Early Occupation	.366	.260
Current Occupation	.266	.207
Earnings	.192	.251

the second group. While the consequences of age differences are "real" and could be one mechanism by which larger families differentiate their children more than do small families, we wanted to test the consequences of family background factors that could in principle be similar for men from larger and smaller families. To do this, we eliminated the effects of age resemblance on sibling correlations among outcomes, and compared the sibling correlations between errors net of the effects of age.[16] (Because the effects of age are small, the results are not appreciably different from those we would get by simply comparing overall sibling correlations among outcomes.) Table 20 gives the results of these calculations.

For all outcomes except earnings, the sibling correlations among men from smaller families are noticeably larger than the correlations among men from larger families. None of the differences are statistically significant, however. Still, the pattern is pronounced. We conclude that brothers from smaller families may tend generally to share more common backgrounds than do brothers from larger families, and urge researchers working with larger samples to explore this issue more fully. Since brothers from larger families can be expected to share the same proportion of genes as brothers from smaller families, we interpret our results as indicative of the importance of environmental influences in accounting for the effects of family background. Proponents of genetic interpretations will, of course, note the anomalous result for earnings (see Behrman, Taubman, and Wales, forthcoming) and will argue that only the differences between sibling correlations for men from large and small families may be suggestive of environmental factors. Since we find it difficult to believe that genes account for the total sibling correlations among men from smaller families, we would reject that interpretation. We cannot, however, bring any direct evidence to bear on the question.

6. CONCLUSION

Our findings suggest to us that birth order effects on cognitive development are weak, inconsistent, and statistically insignificant. They suggest that birth order effects on other outcomes are even weaker and less consistent. We are aware that partisans of birth order effects might interpret our evidence differently, and downplay the lack of statistical significance as a consequence of our small sample size. We readily concede complicity in the practice of dismissing unpalatable results which are insignificant, but emphasizing the "suggestive" character of insignificant findings that we favor. (For example, see our discussion of sibling resemblance and family size.)

But our sample is not so small that meaningful results fail to gain statistical significance. For example, differences in siblings' test scores are significantly related to differences in educational attainment. Moreover, even if a unit increase in birth order within families were associated with an average 2.2 point decrement in test scores, the practical consequences of such a result would be small. For example, a 2.2 point difference in test scores between brothers is associated with only a 2.2 (.057) = 0.125 year difference in years of education. Birth order partisans will have to turn elsewhere for convincing evidence of statistically or socially meaningful results

NOTES

[1]For an effort to explain the relationships among family background, schooling, test scores, and economic success in terms of systemic imperatives, see Bowles and Gintis (1976) and Collins (1974). For a critique of marginal productivity explanations of the schooling-income relationship, see Thurow (1975). For a general critique of acontextual analyses of individual level relationships, see Michelson (1973).

[2]The observed sibling correlations on the Duncan scores of current occupational status and on earnings in the Kalamazoo Brothers Sample are 0.309 and 0.237, respectively. The residual standard deviations of Duncan scores and earnings as a proportion of the total standard deviations, after eliminating the effects of family background, are $[1 - .309]^{\frac{1}{2}} = 0.831$ and $[1 - .237]^{\frac{1}{2}} = 0.873$. Corrections for measurement error do not appreciably alter these results (Olneck 1976).

[3]For attempts to assess the genetic contribution to schooling and earnings among twins see Behrman, Taubman, and Wales (forthcoming). For critiques of the twin methodology see Jencks and Brown (forthcoming) and Goldberger (forthcoming).

[4]For reviews of the literature, we have relied on Adams (1972), Hermalin (1969), Schooler (1972), and Wright (1976). For recent empirical work, we have examined Zajonc and Markus (1975), Zajonc (1976), Lindert (1974), and Wright (1976).

[5]Lindert (1974) is an exception.

[6]Lindert (1974) reports that the effect on education of his variable proxying family time inputs that vary by birth order is larger within families than across individuals in his reanalysis of Hermalin's (1968) sibling data.

[7] See Olneck (forthcoming) for these comparisons. We are grateful to Robert M. Hauser for permitting Olneck access to the OCG-II data. See Featherman and Hauser (1975) for discussion of the OCG replication.

[8] Barbara Wolfe suggested this possibility to us.

[9] Controlling birth order in regression analyses does not significantly or noticeably change the estimates of the effects of family size. Nor does the effect of having additional siblings vary significantly by birth order once the nonlinear effects of family size are already controlled.

[10] The vast majority of our respondents grew up in homes with two natural parents. We do not have information on live-in boarders or relatives. We have assumed that number of siblings and family size may be used interchangeably.

[11] The coefficient for X at X_o is $B_1 - B_2(B_{X^2,X} - 2X_o)$, where B_1 is the coefficient of the linear term, B_2 is the coefficient of the orthogonal square term, $B_{X^2,X}$ is the regression coefficient of <u>non</u>orthogonal X^2 or X (Mueser n.d.).

[12] We could not control parental income, but unpublished analyses of the Sewell-Hauser Wisconsin sample show a significant effect of number of siblings on education, net of test scores, and measured background, including parental income (Hauser personal communication). Men from larger families with similar earnings would still be at a per capita financial resource disadvantage compared to men from smaller families.

[13] $r = 0.022$ ($N = 1207$). This finding is consistent with the findings of other researchers who have investigated the relationship between parental test scores and number of children in completed families (Anastasi 1956). Since our respondents are 35 to 59 years old, we assume their families of procreation are generally complete, though for younger men with young wives this would not be true. This could bias the correlations downward.

[14]Our reliance on sixth grade test scores may, of course, obscure effects evident on earlier or later measures of performance. John Conlisk pointed this out in a seminar critique of our paper.

[15]If we are right that the apparent effects of family size on test scores may be spurious, and that some families both choose to have more children and do not stress high cognitive performance, concern for the effects of family size on individuals may be misplaced. The emotional and psychological benefits of being a member of a large family may outweigh the "benefits" of higher test scores, more education, and greater economic "success."

[16]For example, the correlation between brothers' ages for men with four or more siblings is 0.658. The correlation between brothers' test scores in this group is 0.458, and the correlation between age and test scores is -0.148. Assuming no interbrother effects, we can posit the model shown in the following figure.

$$
\begin{tikzcd}
\text{AGE} \arrow[r, "-.148"] \arrow[d, leftrightarrow, bend right, ".658"'] & \text{TEST} & e_T \arrow[l, "a"'] \arrow[d, leftrightarrow, bend left, "b"] \\
\text{AGE} \arrow[r, "-.148"] & \text{TEST} & e_T \arrow[l, "a"]
\end{tikzcd}
$$

(1) $a = [1 - .148^2]^{\frac{1}{2}} = .989$

(2) $r_{TEST,TEST'} = .458 = .148^2(.658) + .989^2 b$

$.454 = b$

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