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ABSTRACT

Reducing family size both reduces the inequality of earnings and raises their average level.

The evidence previously presented in support of this argument has been vulnerable to the charge that the omission of other variables correlated with both the numbers and the later achievements of children might have caused the importance of family size to be overstated. This suspicion can be rejected on the basis of tests that sort out the achievement effects of family size and sibling position more carefully. By examining the relationship of achievement differentials within families as well as differentials between adults raised in different families, this paper finds support for the argument that achievement is significantly retarded both for middle children in families of given size and for each child as the number of siblings rises.

Support is found for the hypothesis that one's sibling position not only matters for later schooling and careers, but also matters in the way one would have predicted by calculating how sibling position should affect the amount of time an individual sibling seems to receive from others. Predictions of the effect of sibling position on the time a child receives were generated from regressions using the 1967-1968 Cornell Time Use Survey. The same regressions yielded important side-results relating to the apparent effects of parents' education and mothers work on the time received by children.

Given that family size and birth order matter to later child achievements, one can calculate the impact of improved contraception on the

average level, and the inequality, of schooling attainment. It appears that more effective birth control somewhat levels, and generally raises, the number of years of schooling. This schooling effect also levels and raises earnings somewhat.

Confirming the micro-economic link between family size and later inequality underlines a potential dilemma for income-maintenance and welfare policies. If the generosity of family support affects fertility, society has reason to scale down the increases in support that are tied to children--yet society also has reason to be generous in supporting the extra children once they are born.

FAMILY INPUTS AND INEQUALITY AMONG CHILDREN

At the family level as well as at the societal level, greater fertility tends both to maintain inequalities and to hold back progress in per capita material living standards. Extra children strain their parents' ability to devote time, energy, and money to each child, giving each child a socioeconomic disadvantage relative to children from smaller families. The disadvantage for children in larger families is particularly relevant to the issue of inequality, since both wanted and unwanted fertility are greater among the poor. In addition, greater fertility can heighten inequalities at the societal level by keeping unskilled wage rates low and buoying up rates of return on property.

To subscribe to the view just stated, one must firmly believe that greater family size really is a socioeconomic disadvantage for individual children. There is an extensive literature on the link between family size and achievement,[1] most of it supporting the belief in a disadvantage of large family size.[2] Attention has also been given to the related argument that the burden of having a large number of siblings is shared unequally by the different birth orders. It has recently been argued that first-borns are at an advantage relative to their younger brothers and sisters,[3] and a similar advantage has been claimed for last-borns.[4] These assertions about birth order, however, have been challenged more frequently than the argument that large families are a net drag on later career attainments.[5]

For all the attention devoted to the relationship of achievement to family structure, the entire literature remains vulnerable to a single line of attack: other important variables have not been held constant, and any correlation between these omitted variables and family structure means that the impact of the latter may have been quite different from the estimates presented. Lacking samples of sufficient detail or size for a conclusive test, researchers in the past have fallen back on simpler displays of averages that fail to dispel doubts about variables not taken into account. Ironically, this problem seems to have been more severe in studies focusing directly on family size and birth order than in the broader literature that simply throws family size into the hopper when analyzing the sources of achievement. The studies concentrating on family size and birth order have been confined to small samples with limited detail. For the most part, they have resorted to displaying two- or three-dimensional group averages. The difficulty is that missing variables can distort the simple profile relating sibling position to achievement. Some studies, for example, try to hold parental attributes constant by dividing the data into middle-class and lower-class categories. Within each of these categories there is considerable variation of parental education, income, and tastes, and such variation is likely to be related to family size as well as to achievement. There is the distinct possibility that the tendency of larger family size to lower average achievement has been misestimated by such studies. For their part, the studies that casually throw in family size as a variable in regressions and path analyses aimed at the larger achievement issues (such as "How much does school quality [or race, or IQ] matter?") fare a little better by at least holding several parental attributes constant. They are able to do so because they typically deal in larger data samples. This procedure, though, is still not sufficient to dispel doubts about omitted variables.

To see more clearly the requirements of a convincing test of the importance of sibling position[6] for achievement, let us briefly note what steps were and were not taken in one of the more careful treatments of this issue to date. Blau and Duncan (1967, pp. 298-330) took advantage of the detail offered in the large 1962 Survey of Occupational Change in a Generation to explore not only the effects of family size but those of birth order as well. After adjusting for the occupational class of the father, they found that larger families were a significant drag on educational attainment, and that last-born children as well as first-born were at an advantage relative to middle children. The last-borns tended to have slightly less schooling than first-borns in small families but slightly more than first-borns in large families. Sibling position had a noteworthy impact on occupational status, though most of this influence disappeared when the interview respondent's education was held constant.

While the Blau-Duncan treatment of sibling position goes well beyond the rest of the research in this topic, their conclusions can be challenged with four variations on the omitted-variable theme. First, like past researchers in this area, they did not hold the respondent's age constant.[7] The demographic and educational history of the twentieth century suggests that this could be an unfortunate omission. The younger the age cohort, the more it benefitted from the rise in average educational attainment and the smaller its average number of siblings (since the postwar baby-boom cohort was largely excluded from the 1962 survey). Educational advantages thus attributed to smaller family size might have been due at least in part to the upward trend in educational opportunities and family income. The apparent advantage of last-born over middle (intermediate-born) siblings could also have resulted from the greater accessibility of education--mainly for these last-borns--during and after World War II. Second, Blau and Duncan were unable to obtain information on the average spacing between siblings, another dimension of sibling position that probably contributes to the explanation of variations in achievement. Third, as the authors acknowledge (p. 310n), distortions can arise from their grouping of siblings into "small family" and "large family" aggregates instead of keeping each family size separate. Finally, several scholars have voiced suspicions about whether parental attributes have really been held constant in any comparison of individuals who are from different families. Even when the father's occupation has been held constant, couples' tastes and abilities still vary in ways that can affect achievement while at the same time being correlated with family size and other independent variables. The case for or against smaller families can therefore be affected by influences that look like consequences of family size when they are not. Suspicions of this sort have led other scholars to remark that only when tests have been run on <u>intra</u>family differences in achievement can we be sure about the importance of sibling position and other family background variables (Hermalin, 1967, pp. 420-421; Bayer, 1967b, pp. 422-423; Smelser and Stewart, 1968, p. 302).

There exists, then, an opportunity for a revisionist attack on the methodology used by past studies to demonstrate a link between sibling position and achievement. Pronatalists could voice any of several suspicions about the use of data in such studies, and advance the unresolved suggestion that large families may not be so bad after all. Steps have already been taken in this revisionist direction (Kunz and Peterson, 1972; Thomas, 1972). Yet the issue need not be left in such an uncertain state.

In the following discussion I shall offer an expanded explanation of just what difference sibling position should make, and shall then subject this reasoning to a test that seems to meet all of the objections raised about empirical work linking sibling position to achievement. More specifically, I shall

(1) lay out a simple proximate explanation of the way in which family size, birth order, and child spacing should influence a child's subsequent attainments by governing the time and other inputs he or she receives;

(2) transform these three dinensions of family structure into a single quantitative family-inputs index;

(3) test the link between sibling position and achievement within families as well as across families; and

(4) present calculations suggesting the order of magnitude of the impact of improved contraception on the subsequent inequality and level of a cohort's educational and career attainments.

It turns out that the link between sibling position and achievement is significant even when age and parents' attributes have been held constant. One reason for the importance of sibling position appears to be the fact that it affects the time inputs into the individual child more strongly than do the ages, status, and labor force participation of the parents. The impact of improved contraception on the distribution of achievements in the next generation turns out to be as important, or possibly more important, than would be inferred from past studies. The family-inputs index which helps to underline these points emerges as a variable which some studies of achievement might profitably use as a more efficient proxy for family-structure effects than the number of siblings.

Time Inputs Into Siblings

There are many hypotheses on why sibling position should affect a child's development.[8] Most of the hypotheses have focused on the intriguing issue of the importance of birth order, though often they also apply to family size and spacing. It has been argued that parents have different expectations for children in different birth orders, just as they have different expectations for boys and for girls. First-borns and only children are often alleged to feel the pressure of higher parental expectations. They also are supposed to have greater dependency on affiliation and conformity with the desires of others, a trait that could raise their probabilities of conventional "success." Last-borns as well as first-borns have been alleged to receive more socioemotional and financial support from their parents than middle children. It has also been argued that children in larger families experience a more external and autocratic form of behavior control from their outnumbered parents, a factor that could retard the development of cognitive and decision-making skills (Elder and Bowerman, 1963). Such a size effect should presumably also affect the pattern of achievement by birth order, since first-born and last-born children spend part of their childhood in the absence of siblings in the home. In addition, a host of supplementary arguments have been advanced about interactions between one's sibling position and the sex pattern of one's siblings, interactions that are supposed to affect other dimensions of behavior (emotional stability, age and stability of marriage, etc.) as well as achievement. These and other arguments compete for recognition as sources of the personality correlates of sibling position.

Though the true origins of the influence of sibling position remain a legitimate subject for debate, it should be noticed that most of the competing hypotheses are in agreement about a <u>proximate</u> cause of whatever importance sibling position may have. The various arguments share the belief that achievements (and certain other personality traits) are influenced by the extent to which a child must share the time, emotional energy, and financial resources of adults with other children of similar age. It is believed, in other words, that a child receives from brothers and sisters less support for the developemnt of career-relevant skills than he loses through their competition for the attention and support of adults.

The argument that sharing adult attention and resources with siblings is a negative influence on later achievement is a simple and straightforward one. Yet, it has several implications, and provides some clues to a clearer formulation of the relevance of sibling position. These can be pursued even without resolving the debate about the underlying psychological mechanisms through which the existence of siblings affects personality development. The first implication, of course, is that being raised in a larger familv is a disadvantage for achievement. A corollary is that first-borns and last-borns should be expected to achieve more than middle-borns for any given family because they grow up with fewer siblings present on the average, the first-born receiving a great deal of attention in his earliest years and the last-born receiving more attention from his elders and more financial support from his parents as a teenager when the older siblings have left the household. Another corollary is that the more closely spaced are a given number of siblings, the lower should be the achievements of the first-born and last-born, since closer spacing denies them years of exclusive parental support. Finally, a child's achievement chances should be retarded more by the existence of a younger sibling than by the existence of an older sibling, since the younger the sibling the less he helps out and the more he has non-negotiable attention needs.

This line of reasoning can be tested at two levels. One alternative is simply to see whether the effects of different sibling positions appear to conform to the qualitative assertions just made. The other testing strategy is to quantify the predicted effect of sibling positions on the amounts of time and money that parents with a given set of attributes would be likely to provide for the development of a child in any given sibling position, and then to test the ability of this quantitative measure of family inputs to match the observed patterns of sibling achievement. This use of a quantitative measure of family inputs is a valuable check on whether the qualitative arguments in the literature stressing the importance of sibling position are right for a wrong reason.[9] Both kinds of tests are presented below.

Quantifying the above arguments about family inputs into a child requires using regression estimates from both time-use surveys and consumption surveys to calculate the expected effect of children in various sibling positions on family expenditures of time and money, given the attributes of the adults in the household. This procedure is complicated, time-consuming, and full of pitfalls, but reasonable estimates can be obtained. In the following discussion I shall focus mainly on the patterns of apparent time inputs into children and shall treat patterns of commodity inputs into children more briefly.

There are several difficulties in trying to use time-use surveys to infer how much time each child receives. One set of difficulties is that it is often hard to decide whether a given activity constitutes caring for or interacting with children. The reader can see the problem easily enough by asking himself whether in each of the following situations the parent is spending time on child care: parent and child are feeding the chickens together and chatting; they are watching a ball game together (on TV, or at the ballpark); Mother is ironing and watching the child across the room;

both parents are fretting in the child's absence about how they should handle the child's misbehavior. The problem of defining child care can be dealt with satisfactorily by limiting the scope of inquiry and by using the most careful survey data available. The arbitrariness of the time-use categories is less of a problem for present purposes than for some other inquiries because the biases built into the definitions of what constitutes child care are likely to be shared proportionally by families with varying ages and numbers of children. The problem of defining child care is also cut down by using a survey that is careful to gather a detailed account of time use over an actual day. This requirement is met by the Cornell time use survey described at greater length in the below appendix. The Cornell team gathered complete 24-hour time accounts for each of two days from 1296 Syracuse families in 1967 and 1968, thereby avoiding some of the sloppiness of response that is likely if respondents are simply asked how much time they spent on such-and-such an activity over a typical day or week. The Cornell team was also careful to give separate measurement to "primary" and to "secondary" time use, the former being the time spent on the activity that was the family member's main focus of attention and the latter being the time spent in more passive supervision or manipulation. This distinction allows one to omit from the calculation of time spent on a child any time spent on other activities while checking periodically, and without personal interaction, to be sure that the child is in no danger. By omitting such secondary child-care time from the calculations below, I have avoided the uninteresting finding that most young children receive <u>some</u> sort of supervision throughout their waking nonschool hours regardless of their sibling position or the nature of their parents.

The other main class of difficulties in interpreting time-use survey data is in allocating to an individual child a part of the total time reported as "child care." It is practically impossible for a survey to disentangle just how much of this time was received by an individual child when more than one child is present in the home. Only by thoroughly disrupting respondents' lives for an entire day could the interviewer see just how the total child-care time was divided up among siblings. In fact, some of it is not divided up at all, as when a parent reads a story to two children at once. In the face of these problems a precise measure of the time inputs received by one child simply cannot be obtained without adding theoretical assumptions. Two key assumptions that seem reasonable are:

(1) the amount of care time that an only child receives (outside of school) is a plausible upper-bound estimate of the amount received by a nononly child of the same age and parental attributes; and

(2) the net amount added to total child-care time by the presence of an extra sibling of given age and parental attributes is a plausible lower-bound estimate of the care time he receives--that is, he is assumed to receive (and to take from other children and from nonchild activities) at least as much time as the family transfers to all child care because of his presence.

These two assumptions have been made en route to the estimates presented below. A further assumption was made, however, to yield a single "best-guess" number for the time inputs into each sibling position. Since the truth lies somewhere in between the upper- and lower-bound estimates, I arbitrarily used the midpoint between these as the estimate of the time received by an individual nononly child. Readers may have hunches suggesting that this mid-range estimate is wrong. In its defense I can say only that it is used in regressions below in a way that (a) makes no difference as long as the lower-bound estimate exerts the same percentage effect (whether or not the fifty percent assumed by the mid-range estimate) on true time inputs for all sibling positions, and (b) fits patterns of sibling achievement quite well.

Using the approach just outlined, one discovers certain patterns linking sibling position to the amount of time spent on child care. Even without the use of assumptions about the allocation of total child-care time among siblings, the regressions explaining the total amounts of child-care time (Appendix, Table A-1) underline the importance of the number and ages of children. The ages of children dominate differences in care time, with infants contributing far more to daily or annual hours of child care than children of school age. The number of children also interacts with their ages, with each extra child in a given age group adding significantly to the child-care load at a decreasing rate. These patterns hold for the use of the mother's time alone as well as for the total time contributed by all persons to caring for the children in one family.

Attributes of the parents prove much less important than family composition as determinants of the amounts of time spent in child care. The mother's age, her education, and the father's socioeconomic status all fail to exert statistically significant influences on the total amount of time spent (by the mother or by all persons) on caring for children of given number and ages. It appears that the only way in which more educated wives as a group manage to devote more time to each child is by having fewer children on the average.[10]

The importance of family composition even eclipses that of the mother's work status. It turns out that for given ages and numbers of children, mothers who work for pay outside of the home spend only slightly less time in child care than nonworking mothers. The differences by work status are only near the border of statistical significane in the Cornell sample, though a much larger sample might make them clearly significant. Furthermore, for given family composition, children of working mothers receive more total time in child care from all persons. That is, the estimated deficit in mother's time is more than made up by extra (non-school) time inputs by the husband and by others.[11] Furthermore, if the numbers of children are not held constant, it turns out that working mothers, having fewer children on the average, spend as much of their own time <u>per child</u> on child care. Each child of a working mother also receives as much of others' time and greater amounts of commodities bought with the mother's pay.

If the number and ages of children are such a dominant influence on the amount of time devoted to child care, there is good reason to look more closely at estimates of how the amount of time going into an individual child is affected by the number and ages of his siblings. To make such estimates, one must make use of assumptions like those introduced above. In addition to deriving such mid-range estimates of the amount of time received by one child from each person each year, one must aggregate over caretakers and over years of the child's life in a way that requires further assumptions. I have opted for the simplest aggregation procedure, and have added up all of the hours of time received by a child (according to the mid-range estimates) from any person in any of the 18 years he spends in his parents' household. The total numbers of hours for a child in each of several sibling positions are given in Table 1, and illustrated in Figure 1. Readers should note, though, that the assumptions implicit in the simple toting of total hours of care received may not square with their own hunches

Table 1. Predicted Child-Care Time Received over 18 Years by Children in Various Sibling Positions, as a Fraction of That Predicted for an Only Child, by Years of Spacing Between Adjacent Children (Based on 1967-68 data)

Child spacing (uniform)	Children ever born	Child's Birth Order: 1st born	2nd born	3rd born	4th born	5th born
1-year	only child	8227 hrs. (1.000)	--	--	--	--
	2	6518 hrs. (.791)	6518 (.791)	--	--	--
	3	5528 (.672)	5393 (.656)	5528 (.672)	--	--
	4	5757 (.700)	5555 (.675)	5664 (.688)	5761 (.700)	--
2-year	2	6662 hrs. (.810)	6662 (.810)	--	--	--
	3	5690 (.692)	5435 (.661)	5698 (.693)	--	--
	4	5799 (.705)	5407 (.657)	5625 (.684)	5819 (.707)	--
3-year	2	6790 hrs. (.825)	6794 (.826)	--	--	--
	3	5853 (.711)	5468 (.665)	5867 (.713)	--	--
	4	5834 (.709)	5256 (.639)	5443 (.662)	6003 (.730)	--
6-year	2	7172 hrs. (.871)	7188 (.874)	--	--	--
	3	6338 (.770)	6049 (.735)	6604 (.803)	--	--
	4	6338 (.770)	5544 (.674)	5794 (.704)	6604 (.803)	--
	5	6338 (.770)	5544 (.674)	4959 (.603)	5794 (.704)	6604 (.803)

Notes:

The figure in parentheses is the ratio of the estimated time received by this child to that received by an only child.

Each estimate was derived by the following steps: (1) Regressions were run on the Cornell time use survey data, relating child care time by all persons to the

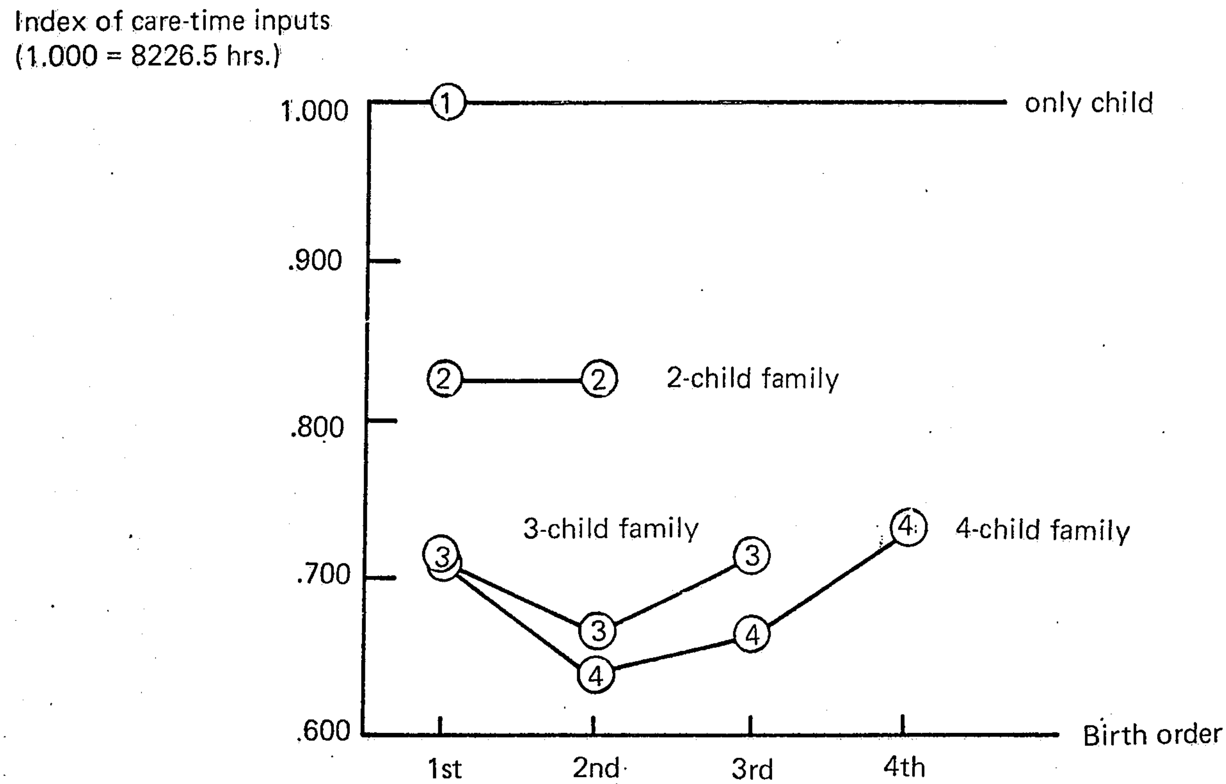
Notes to Table 1 (cont.).

ages and numbers of children in the household and other variables. These regressions are reported in Appendix A. (2) For each child age-and-number variable, the coefficients from regressions for different parts of the year (nonsummer weekdays, summer weekdays, and weekends) were weighted by their shares of a year and summed to get an annual coeffícient. (3) The net impact of each child on total child care time in a given year was computed by adding and subtracting the coefficients just mentioned. For example, the net impact of a five-year-old with a one-year-old sibling would equal the coefficient for (2-5 year age group, two children in the home) plus that for (one-year-old, two children in the home) minus that for (one-year-old, the only one in the home). (4) These net impacts are then summed over the 18 years that each child spends in the household. (5) The 18-year net impact total is corrected by subtracting from that sibling's total impact on family child care the number of hours he or she spent taking care of younger siblings. This correction is necessary to keep the extra child care supplied by an older sibling from being counted as time spent on him by others. The estimated amounts of such care for younger siblings by older are given in Table A-3 of Appendix A. (6) As explained in the text, the estimated net impact of a given child on total care of family members is a lower-bound estimate of the hours of care time from which that child received some benefit. The upper-bound estimate is the hours of care time he would receive as an only child. As noted in the text, the next step is to calculate the mid-point between these two bounds. This "mid-range" estimate is what is presented in the table above. (To get the prior net-impact figure, double the mid-range estimate and subtract 8226 hours, the total care time for an only child.)

For families with five or more children, direct estimates could not be made because too few such families were represented in the sample. In order to include large families in the regressions using an index of time inputs below, it was necessary to extrapolate values based on those for the largest families represented in this table. The rule for extrapolating was as follows: for all first-borns in larger families use the index for the first-born in the largest family size represented here; for all last-borns use the last-born's index for the largest family here; similarly extrapolate for those of penultimate birth order; and for all others extrapolate the index for the second-born of the same average child spacing. The same rule could be used for the case of six-year spacing, but it is unlikely that such spacing would occur in families with six or more children.

Figure 1

Predicted Child-care Time Received over 18 years by Children in Various Sibling Positions, as a Fraction of that Predicted for an Only Child: The Case of Three-year Child Spacing



Source: Table 1 and Appendix A

about the importance of different kinds of care. Some may think that interaction with adults is so much more important in the first few years of a child's life than later that one should give a greater weight even to each hour of care time for these earlier years. Others may consider parental time so much more important than time spent by others caring for the child that each hour of the parents' time should be given a greater weight. These possibilities are hard to explore, since we have no data on the achievement consequences of each hour of child care from various persons at various times. The only way of dealing with the fact that simply adding up the hours may be inappropriate is to check the predictions of input measures like those in Table 1 against actual achievement patterns. For example, if it looks as though the input measures seem to underexplain the achievement of first-borns, then it would appear that each hour of time input in the earliest years (where the first-borns get the greatest advantage over others) deserves greater weight than it is given in Table 1. We shall return to this issue when empirical results are presented below.

Several patterns emerge from estimates in Table 1 of the time received by children in different sibling positions. In general these patterns correspond to the simple formula mentioned at the start of this section: the presence of an extra sibling of similar or lower age reduces the amount of time (and attention) that a child receives. The appearance of a younger sibling lowers time inputs for a child in most sibling positions. It is interesting to note, though, that the estimates find this effect stronger for the arrival of the second and third child in the family than for the arrival of the fourth. It may be that a first-born or second-born suffers no further loss of attention from the appearance of the fourth and later siblings. Being the youngest of four or more siblings may similarly be no worse in this respect than being the latter of two or the last of three. Indeed, the estimates imply that the youngest of four receives more time inputs than the youngest of three, in part because the first-born is often old enough to help with the care of the fourth child. The table also asserts that middle-born children receive fewer hours of care than the eldest and youngest in all cases, primarily because they lack the opportunity to be the only child in the household at any time in their lives. The estimates also imply that wider child spacing provides more hours of attention for the eldest and youngest but has an ambiguous effect for the middle children. Finally, the estimates also imply that a last-born receives at least as much care time as a first-born in families of the same size, a pattern not mentioned in the past literature and not strongly supported by the empirical results below.

A slightly transformed version of the indices in Table 1 will end up being used below in regressions designed to explain differences in achievement within and between families, as a means of testing the hypothesis that sibling position affects achievement by affecting family inputs into the child. The slight transformation involved is the introduction of a new variable, SIBLOSS, which equals one minus the appropriate index shown in parentheses for each sibling position in Table 1. This transformation is intended to cover against uniform percentage errors in the deviation of the time inputs received by each nononly child from those received by an only child. SIBLOSS, in other words, is assumed to equal the fraction of input reduction from the only-child norm times an unknown constant. As long as this assumption is correct, the value of the unknown constant is irrelevant to the ability of sibling position to help in explaining achievement differentials. The validity of this assumption about SIBLOSS is one of the issues at stake in the tests presented below. If it turns out that achievement differentials fail to show the expected relationships to sibling positions or to the SIBLOSS index, then the entire line of reasoning about sibling position, family inputs, and achievement is to be rejected. If the tests show a clear relationship of achievement to sibling position but none to SIBLOSS, then the present reasoning about *why* sibling position matters is incorrect. If the tests show a clear relationship both to sibling position and to SIBLOSS, then the present hypotheses are sustained.

Commodity Inputs Into Siblings

If the presence of siblings affects the amount of time a child receives, it is likely to affect the amount of goods and services purchased on his behalf by his parents and others. A child who must share adults attention in any given year must also share their financial resources with his siblings. The greater the number of children present, the harder it is for parents to give each one extra medical care, extra lessons, and a room of his own. As with time inputs, closer child spacing and middle-child position should be a disadvantage for the child--the more so, the harder it is for the parents to spend on his behalf out of prior savings or out of borrowings against earnings that will come after the child has left home. It seems reasonable, then, to make calculations exactly analogous to those just summarized for time inputs, calculations yielding an index of the commodity inputs received for each sibling position as a fraction of those for an only child from parents of the same attributes.

There are two steps to the calculation of a sibling commodity input index, the same two steps taken for time inputs in the previous section. The first is to run regressions quantifying the impact of the number and ages of children, along with other variables, on family consumption patterns. The second is to impose *a priori* assumptions about how the expenditures predicted for different family compositions are shared among individual children and adults within the family. The resulting index for the commodity inputs predicted for each sibling position could then be combined with the index for time inputs by some appropriate choice of weights, yielding an overall index of family inputs into each sibling, to be compared to that for an only child in a similar family.

The first step is easy. Elsewhere, I have estimated the impacts of numbers and ages of children on total family expenditures and on expenditures on 11 commodity classes, using the 1960-1961 Survey of Consumer Expenditures.[12] The second step is also easy in principle, but extremely tedious to carry out. As with time inputs, the unobservability of the amounts of inputs received by an individual child can be overcome if certain plausible assumptions are allowed. One can roughly assume, for example, that bedroom space and utilities are divided so as to give each child the same amount of space, heat, etc. One can approximate the expenditures on recreation and education for each child by dividing the total increment of expenditures for given family composition over that for a childless couple by the number of children. Expenditures on children's clothing could be allocated across the children in the family according to the ratios of only-child clothing expenditures for the different age groups. Finally, the USDA has made its own rough guesses as to the relationship of family size to the food and medical expenses of individual children. Plausible guesses like these can then be summed up to get a total value of commodity inputs for each sibling position. Unfortunately, that involves summing over ten commodity categories, over 18 years of childhood, and over the 42 sibling positions represented in Table 1. The number of calculations involved proved more than patience and research budget could bear, and for related research I made such calculations only for an only child and for the last of three children spaced at three-year intervals.

Clues to the likely patterns of commodity inputs can be had, however, by noting some likely effects of family composition to the ratio of home time to commodities. First, it appears likely that children from families of different size differ slightly more in commodity inputs than in time inputs. The larger the family, the more time-intensive and the less commodity-intensive its home life. This follows from the fact that having an extra child tends to reduce the working hours and earnings of mothers by slightly more than it raises the average hours and earnings of husbands through moonlighting. Couples with more children tend to spend less money and more of their own (and older siblings') time in the home. What is true about the ratio of time to commodities in all home activities is also probably true of the raising of each child. One inference about commodities and child rearing is thus that children in larger families may experience a greater percentage reduction in commodity inputs than in time inputs relative to an only child. This likely pattern is not equally shared by the different birth orders, however. Parents apparently spend slightly more on last-born children than on first-borns, and much more on the last-borns than on middle children.[13] If this is true, the main reason for it is probably the fact that by the time the last-born is growing up, his parents are older and earn more, allowing them to spend more on him (even if he does not go to college at their expense). There is no convenient way to adjust the SIBLOSS index for this likely relationship of birth order to commodity-intensity of child rearing, and the tests below must be examined to see if in fact the achievements of last-borns are underexplained by the use of the SIBLOSS variable, which must be based on time-input calculations only.

The New Jersey Sibling Sample

To test whether and why sibling position matters to achievement, it is important to have a sample that can meet the requirements for a convincing test. As mentioned in the introduction above, the sample must allow tests on intrafamily achievement differentials. It should also allow one to make adjustments for age to take account of the fact that different cohorts of growing children experienced different access to schooling. A sample that meets these requirements is a sample of 1,087 siblings collected in 1963, when a Cornell Medical School team interviewed 312 senior male employees of a New Jersey utility company in search of information about the incidence of heart disease.[14] The 312 interview respondents were asked, among other things, for the age, sex, educational attainment, and most recent occupation of each of their siblings, living or dead. The respondents themselves ranged in age from 55 to 61, and the ages of their siblings ranged from 31 to 81. The sibling sample thus consists of persons old enough that their formal schooling has been completed. Furthermore, only a dozen siblings were under 40 in 1963, so that the boom in incomes and educational opportunity after Pearl Harbor (e.g., the GI bill) must have had no effect on the schooling or first jobs of almost all of the sample.[15]

The nature of the New Jersey sibling sample does complicate the present inquiry in one respect, however. The interview respondents were clustered into the higher educational and occupational classes, being all long-term employees of a company with a high average skill level. This high average status need not be a problem in itself, but two other facts make it a problem. First, the respondents had a higher average schooling and career achievement than the average levels they reported for their own siblings. Second, the likelihood that any given sibling in the sample was himself the interview respondent is closely related to his sibling position, and in particular to his family size. In the absence of any other information about an individual in the sample, we know that the probability of his being one of the high-achievement respondent group is 1/N, where N is the number of siblings in his family. Thus all only children were interview respondents, as were half the siblings from two-child families, one-third of the siblings from three-child families, and so forth. Thus in this sample there is an unavoidable relationship between family size and respondent status, and another relationship between respondent status and predicted achievement.

The selection bias involved in the sample design can be handled in various ways. The range of options can be seen more clearly by referring to the causal relationships portrayed in Figure 2. The first option is the "reduced-form" strategy of arguing that the probability that an individual is the interview respondent for his family is itself simply one channel through which the true importance of sibling position for achievement shows up in this particular sample. Following this strategy, one would simply ignore who the respondents were, and run regressions explaining achievements in terms of the variables on the left in Part A of Figure 2: family background, sibling position, and sex. This procedure, of course, is likely to overestimate the importance of sibling position and sex. The interview respondents were not a random sample of males, nor were the only-child respondents a random sample of only children, or the two-sibling respondents a random sample of persons from two-child families, and so forth. Such a strategy will be tried out below, but with the expectation that it overstates the importance of sibling position.

Two other testing options are likely to attribute less, and probably too little, importance to sibling position as a determinant of achievement. One is the device of adding a dummy variable for respondent status to the regressions explaining educational and career attainments. This is likely to understate both the unit impact and the statistical significance of sibling position. By mixing the respondents into the sample, this procedure is biased against sibling position by implicit

tion was an influence in the likelihood of one's being a respondent in this survey, and by including a subsample (the respondents) selected by occupational class, within which any independent variable has diminished explanatory power. A better procedure is to run tests on the subsample of nonrespondents. This can keep the nature of the respondents from biasing the estimates of the impact of sibling position. Still, tests on a sample of siblings who are not respondents is a stern one for the sibling-position hypotheses, since it cuts down the representation of the small families over which family size was supposed to matter most. Tests from a sample of nonrespondents are thus likely to underestimate the significance (t-statistics) of sibling position variables.

The best way of dealing with the special nature of the sample, however, is to run the kind of test that is also best for meeting the suspicions about omitted family variables raised by the past literature: examine <u>intrafamily</u> differentials in achievement. Part B of Figure 2 sketches a set of influences that could be tested by redefining all variables as differentials between the first-born and a later-born, to be employed over a sample consisting of all of the later-borns (non-first-borns) in the New Jersey sibling sample. Explaining intrafamily differences in achievement should give the most accurate indication of the importance of sibling position. The special nature of the group of interview respondents is less of a problem in an intrafamily test. Since family size is the same for each first-born as for each of his own siblings, a first-born and each later-born in the same family have the same likelihood of being the interview respondent working for the New Jersey utility (except to the extent that birth order truly affects achievement). Therefore regressions following the scheme of Part B in Figure 2 should reveal the role of sibling position with or without dummy variables identifying the interview respondents. These intrafamily tests, like the other testing options just mentioned, will be probed next.

Achievement Patterns: Regression Results

When all of the different ways of correcting for the special nature of the New Jersey sibling sample have been tried, it turns out that sibling position is at least as significant an influence on achievement as past literature implied, and for the simple reason that sharing family time and money with siblings is a drag on achievement. This conclusion emerges from results like those given in Tables 2 through 4. To survey these results, let us examine the three tables in order.

Whenever the schooling levels of individuals in different families are analyzed, as in Table 2, certain background variables prove unmistakably significant. Schooling levels are higher, the higher are the parents' schooling levels and occupational status. A broken-home background significantly reduces one's likely educational attainment. Sex and age, on the other hand, were not significant influences on schooling in this sample. Both sex and age looked significant only in those regressions where they were allowed to act as proxies for interview respondent status, the respondents being all male and all over 55 (cf. Regressions [1] and [4] in Table 2).

Table 2

Patterns of Individual Educational Attainment:
Regression Results from the New Jersey Sibling Sample.

Definitions of Variables Used

Dependent variables:

ED = a one-digit schooling code, ranging in values from 1, for less than an 8th-grade education, to 8, for 5 or more years of college. (An alternative scaling was also tried: the ED code was transformed into a logarithm of the earning potential predicted over a lifetime (capitalized at 5 percent back to age 14) on the basis of this schooling code alone. The figures for earning potential are based on 1959 data for northern white males, taken from Giora Hanoch, "Personal Earnings and Investment in Schooling," unpublished Ph.D. dissertation, University of Chicago, 1965, Table 8. This alternative scaling yielded results essentially identical to those for the regressions reported here for ED.)

SEI= a two-digit index of socio-economic status, based on the individual's most recent occupation. The index number assignments were made by Professor Albert I. Hermalin.

Independent variables:

MALE = 1 if the individual is a male, 0 if female.
EDMA = the mother's educational attainment = ED for the mother.
EDPA = the father's educational attainment = ED for the father.
SEIPA = the socio-economic status of the father's final occupation = SEI for the father.
BROKEN= dummy variable for a broken home during the individual's childhood = 1 if the parents were separated or divorced or if the interview respondent spent time in a foster home or orphanage.
AGE = the individual's age in 1963, in years.
AGESQ = AGE squared.
RESP = 1 if this individual is the interview respondent, 0 if this is one of his siblings
SIBLOSS = index of reduction in time inputs received by the child because of his having siblings = one minus the index given for the appropriate sibling position in Table 1. The indices calculated for 1-year spacing in Table 1 were used whenever average child spacing was less than 1.5 years; the 2-year indices were used for spacing from 1.5 years to 2.5; the three-year figures for spacing from 2.5 to 4.5; and the 6-year figures for spacing over 4.5 years.
ONLY = 1 if the individual is an only child, 0 otherwise.
FIRST2, FIRST3, FIRST45, FIRST613 each = 1 when the individual is the first-born of two, three, four or five, and six through thirteen children, respectively.
MID3, MID45CLO, MID45WID = 1 when the individual is an intermediate-born child of three children, of four or five children spaced more closely than two years apart, or of four or five children spaced two years or more apart, respectively.
LATTER, LAST3, LAST45, LAST613 = 1 when the individual is the last born of two, three, four-or-five, or six or more children, respectively.
TWO, THREE, FOUR, FIVE = 1 when the number of siblings (including the respondent) is two, three, four, or five, respectively.
N = number of siblings (including the respondent).
NSQ = N squared.

Table 2 (cont.). Patterns of Individual Educational Attainment: Regression Results from the New Jersey Sibling Sample

Independent variable	Regression number (dependent variable is ED): (1)		(2)		(3)	
	coeff.	std.err.	coeff.	std.err.	coeff.	std.err.
constant term	-1.815	(2.268)	1.198	(2.351)	1.233	(2.343)
MALE	.298**	(.109)	.076	(.116)	.078	(.118)
EDMA	.249**	(.052)	.226**	(.060)	.251**	(.052)
EDPA	.220**	(.046)	.260**	(.053)	.223**	(.046)
SEIPA	.018**	(.003)	.018**	(.003)	.018**	(.003)
BROKEN	-.505**	(.172)	-.582**	(.201)	-.526**	(.170)
AGE	.138	(.078)	.035	(.081)	.033	(.081)
AGESQ	-.0014*	(.0007)	-.0005	(.0007)	-.0005	(.0007)
RESP	---	---	---	---	.636**	(.140)
SIBLOSS	---	---	---	---	---	---
ONLY	1.071**	(.294)	---	---	.658*	(.305)
FIRST2	.742**	(.216)	.718*	(.316)	.512*	(.219)
FIRST3	.531*	(.238)	.533	(.283)	.410	(.237)
FIRST45	.209	(.217)	.248	(.232)	.178	(.215)
FIRST613	.638*	(.269)	.625*	(.287)	.589*	(.267)
MID3	.613*	(.236)	.644*	(.263)	.551*	(.234)
MID45CLO	.086	(.250)	.007	(.264)	.111	(.248)
MID45WID	.250	(.176)	.151	(.193)	.194	(.175)
LATTER	.454	(.215)	.365	(.252)	.369	(.214)
LAST3	.436	(.238)	.346	(.282)	.329	(.237)
LAST45	.480	(.221)	.489	(.248)	.387	(.220)
LAST613	.455	(.276)	.300	(.306)	.315	(.275)
$R^2_{adj.}$/s.e.e.	.3074/1.5866		.3144/1.5334		.3210/1.5709	
degrees of freedom:	988		700		987	
Sample:	1008 persons		719 non-respondents		1008 persons	

* = significant at the 5 percent level.
** = significant at the 1 percent level.

Note: the coefficients on sibling-position binary variables show the impacts of these positions relative either to middle children in families of six or more [in Regressions (1)-(3)] or to all children in families of six or more children [in Regression (7)].

Table 2 (Cont.). Patterns of Individual Educational Attainment: Regression Results from the New Jersey Sibling Sample

Independent variable	Regression number (dependent variable is ED): (4) coeff.	(4) std.err.	(5) coeff.	(5) std.err.	(6) coeff.	(6) std.err.
constant term	-1.252	(2.134)	1.919	(2.152)	1.770	(2.196)
MALE	.302**	(.108)	.058	(.115)	.063	(.117)
EDMA	.250**	(.052)	.229**	(.060)	.253**	(.052)
EDPA	.223**	(.046)	.261**	(.053)	.224**	(.045)
SEIPA	.018**	(.003)	.018**	(.003)	.018**	(.003)
BROKEN	-.482**	(.170)	-.572**	(.199)	-.505**	(.168)
AGE	.152*	(.074)	.039	(.075)	.034	(.077)
AGESQ	-.0015*	(.0007)	-.0005	(.0006)	-.0004	(.0007)
RESP	---	---	---	---	.680**	(.137)
SIBLOSS	-2.393**	(.611)	-2.236*	(.968)	-1.373*	(.638)
R^2_{adj} /s.e.e.	.3054/1.5889		.3128/1.5351		.3214/1.5705	
degrees of freedom:	999		710		998	
Sample:	1008 persons		719 non-respondents		1008 persons	

Independent variable	Regression number (dependent variable is ED): (4) coeff.	(4) std.err.	(5) coeff.	(5) std.err.	(6) coeff.	(6) std.err.
constant term	-.387	(2.181)	.121	(2.250)	.596	(2.253)
MALE	.300**	(.109)	.333**	(.108)	.303**	(.108)
EDMA	.251**	(.052)	.248**	(.052)	.251**	(.052)
EDPA	.218**	(.046)	.228**	(.046)	.216**	(.046)
SEIPA	.018**	(.003)	.017**	(.003)	.018**	(.003)
BROKEN	-.511**	(.172)	-.489**	(.171)	-.492**	(.171)
AGE	.093	(.076)	.095	(.077)	.092	(.077)
AGESQ	-.0010	(.0007)	-.0010	(.0007)	-.0010	(.0007)
RESP	---	---	---	---	---	---
SIBLOSS	---	---	---	---	---	---
ONLY	.945**	(.291)	---	---	---	---
TWO	.464**	(.165)	---	---	---	---
THREE	.389*	(.154)	---	---	---	---
FOUR	.137	(.156)	---	---	---	---
FIVE	.109	(.155)	---	---	---	---
N	---	---	-.073**	(.022)	-.245**	(.075)
NSQ	---	---	---	---	.014*	(.006)
R^2_{adj}/s.e.e.	.3045/1.5899		.3025/1.5922		.3058/1.5884	
degrees of freedom:	995		999		998	
Sample:	1008 persons		1008 persons		1008 persons	

* = significant at the 5 percent level.
** = significant at the 1 percent level.

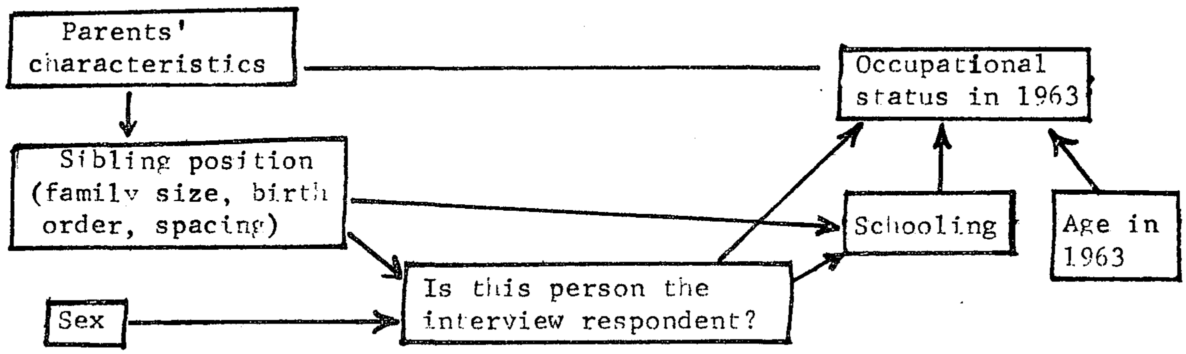
Any of several specifications predicts that greater family size reduces the schooling expected for each sibling. The first three regressions also suggest that closer child spacing (for four- and five-child families) may be a disadvantage for educational attainment, but the spacing pattern was not statistically significant. As for birth order (in Regressions [1] - [3]), the hypothesized disadvantage of being a middle child shows up among families with more than three children, but fails to pass significance tests.

Recasting the sibling position variables into the single SIBLOSS index (in Regressions [4] through [6]) yields results suggesting that the reason why sibling position matters is primarily because extra siblings reduce the amount of time and money spent on each child. This suggestion comes from the ability of the SIBLOSS index to contribute almost as much to explaining achievement variations as a dozen sibling-position dummy variables. The SIBLOSS index even remains significant at the five percent level when forced to compete with the respondent-status dummy with which it is so highly (negatively) correlated. Its predictive power is all the more remarkable when one remembers that the children in the Syracuse survey whose care underlies the SIBLOSS estimates are on the average half a century younger than the adults whose schooling levels SIBLOSS helps to explain. Table 2, then, tends to support all of the sibling-position hypotheses.

All family background variables tend to explain schooling levels better than they explain peak lifetime occupational achievements. The reason is that other events intervene between the completion of schooling and the height of one's career. Accordingly, Table 3 shows that all background variables are less significant, and account for a smaller share of

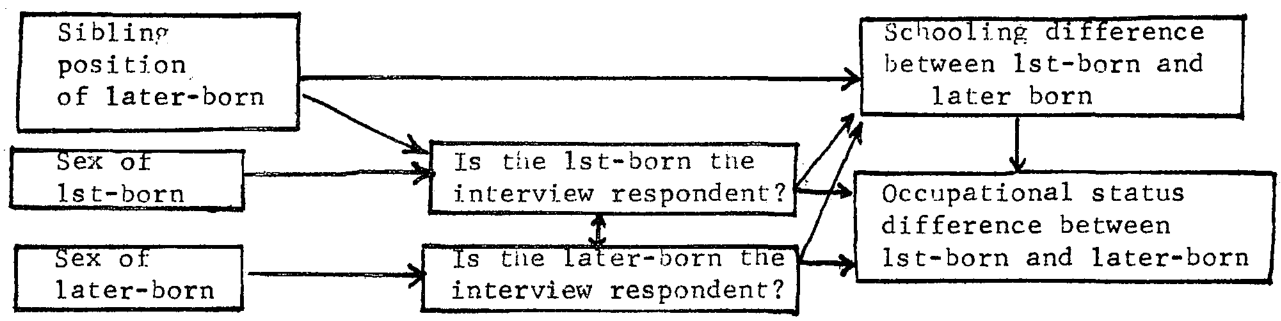
Figure 2. Influences on Predicted Achievement in the New Jersey Sibling Sample

A. Achievements by Individuals in Different Families.



(The other possible arrows from left to right have been omitted because they failed to prove statistically significant.)

B. Achievement Differentials within Families.



total variation, when it comes to explaining career status rather than schooling. Not surprisingly, age has much more to do with occupational status than with one's schooling. Respondent status is strongly significant as well, since the respondents were drawn from a fairly homogeneous occupational group. Sibling position, like the other background variables,[16] is not statistically significant as a job status determinant (in Regressions [2] and [4] of Table 3), though its coefficients are still generally of the "right" sign.

The best tests for resolving suspicions about omitted variables and sample selection bias are the intrafamily regressions reported in Table 4. It is well to recall why regressions on intrafamily achievement differentials seemed necessary. First, the past literature was vulnerable to the suspicion that each set of parents may have had unmeasured attributes that made them produce higher-achieving children, and fewer children, than other couples with similar measurable attributes. If so, a larger number of siblings would seem to be a greater drag on achievement than it really is, given <u>all</u> attributes (including motivation to control births) of the parents. Second, as argued in the previous section, the nature of the New Jersey sample suggests that intrafamily tests can best deal with the problem of sample selection bias.

One might expect the intrafamily tests satisfying these requirements would be difficult ones for the sibling-position hypothesis to pass. When the dependent variable is a differential in the schooling of two siblings raised together, R^2 is going to be low. The dependent variable is a single digit, and some of the most powerful determinants of achievement across families--parents' education and occupational status, a broken-home history, and even completed family size--are of no help in accounting for intrafamily

Table 3. Patterns of Individual Career Status Attainment: Regression Results from the New Jersey Sibling Sample

Independent variable	Regression number (dependent variable is SEI): (1) coeff.	(1) std.err.	(2) coeff.	(2) std.err.	(3) coeff.	(3) std.err.	(4) coeff.	(4) std.err.
constant term	-190.66**	(39.11)	-90.16*	(40.26)	-179.26**	(37.68)	-81.72*	(38.57)
EDPA	1.78*	(0.74)	1.91**	(0.71)	1.80*	(0.73)	1.86**	(0.70)
EDMA	0.83	(0.84)	0.85	(0.81)	0.79*	(0.84)	0.84	(0.81)
SEIPA	0.22**	(0.04)	0.22**	(0.04)	0.22**	(0.04)	0.22**	(0.04)
BROKEN	-7.12*	(2.88)	-7.40**	(2.77)	-6.96*	(2.84)	-7.54**	(2.73)
AGE	8.11**	(1.35)	4.47**	(1.40)	8.09**	(1.29)	4.25**	(1.34)
AGESQ	-0.07**	(0.01)	-0.04**	(0.01)	-0.07**	(0.01)	-0.04**	(0.01)
RESP	---	---	13.35**	(1.88)	---	---	13.59**	(1.84)
SIBLOSS	---	---	---	---	-36.82**	(8.94)	-14.18	(9.13)
ONLY	13.08**	(4.03)	4.21	(4.08)	---	---	---	---
FIRST2	11.45**	(3.27)	5.38	(3.27)	---	---	---	---
FIRST3	3.94	(3.92)	0.35	(3.81)	---	---	---	---
FIRST45	4.78	(3.78)	3.65	(3.64)	---	---	---	---
FIRST613	10.01*	(4.63)	8.42	(4.47)	---	---	---	---
MID3	7.35	(4.04)	4.91	(3.90)	---	---	---	---
MID45CLO	-3.14	(4.20)	-2.52	(4.05)	---	---	---	---
MID45WID	1.24	(2.89)	-0.68	(2.80)	---	---	---	---
LATTER	4.58	(3.39)	1.84	(3.29)	---	---	---	---
LAST3	2.39	(3.87)	-0.87	(3.75)	---	---	---	---
LAST45	0.86	(3.68)	-1.91	(3.56)	---	---	---	---
LAST613	8.09	(5.03)	3.60	(4.89)	---	---	---	---
R^2_{adj}/s.e.e.	.2125/20.97		.2688/20.20		.2112/20.98		.2708/20.17	
degrees of freedom	640		639		651		650	

Sample: 659 males.

* = significant at the 5 percent level.
** = significant at the 1 percent level.

(For definitions of variables, see Table 2.)

differentials. In view of the fact that family size was a more important determinant of predicted inputs into each child than were birth order and spacing, it would have been no surprise if Table 4 had reported only an insignificant influence of differences in family inputs on differences in schooling.

It is thus striking to find that sibling position matters so much within families. Within large families, last-borns have little or no schooling disadvantage with respect to their own first-born siblings, while middle-borns have some disadvantage.[17] When all of the later-born sibling positions are transformed into a single difference-in-family-inputs index, this index proved highly significant and was unaffected by taking account of the respondent dummy variables.[18] Furthermore, the coefficient on this index--DIFINPUT, or the difference between SIBLOSS for the later-born and SIBLOSS for the first-born--actually proved much larger than the impact attributed to differences in siblings' family inputs in Tables 2 and 3.

The fact that the family input index had greater unit impact in explaining intrafamily achievement differences than it seems to have had in explaining patterns of individual achievement across families requires further attention. The higher intrafamily impact (of DIFINPUT in Regression [3] of Table 4), when combined with Table 1's input coefficients, implies that a two-child family will produce an average level of schooling 1.49 years greater than the average predicted for a five-child family with the same parental characteristics. The lower interfamily coefficient (for SIBLOSS in Regression [5] of Table 2) implies that a two-child family will produce 0.67 years less schooling on the average than a comparable five-child family. In both comparsions, almost all of the impact of family size was predicted to fall in the gap between a two-child and a three-child

Table 4

Intra-Family Differences in Educational Attainment: Regression Results from the New Jersey Sibling Sample

Definitions of Variables Used

The Dependent Variable:

EDDIF = the first-born's educational advantage = the difference between the schooling level attained by the first-born sibling and that attained by one later-born sibling in the same family. The schooling level in this case is a one-digit code with values from 1, for less than an eighth-grade education, to 8, for five or more years of college.

The Independent Variables:

MALE1 = 1 if the first-born is a male, 0 if female.

MALEH = 1 if the later-born to whom the observation applies is a male, 0 if female.

RESP1 = 1 if the first-born is the (male) interview respondent working for the utility company in 1963, 0 otherwise. The function of this and the next variable is to quantify whatever distorting influences may have arisen from the facts that the interview respondents are a specially-selected group of employees and that they may have systematically misreported their siblings' schooling.

RESPH = 1 if the later born to whom the observation applies is the interview respondent, 0 otherwise.

AGEDIFF = the difference in age (years) between the first-born and the later-born to whom the observation applies.

AGESQDIF = the difference between the square of the first-born's age and the square of the later-born's age.

DIFINPUT = the difference between the index of time inputs that the first-born child is predicted to have received and the same index for the later-born child, where the input indices are those given in Table 1. The figures calculated for one-year spacing were used whenever average child spacing was less than 1.5 years; the two-year figures were used for spacing from 1.5 years to 2.5; the three-year figures for spacing from 2.5 to 4.5; and the six-year figures for spacing over 4.5 years.

LATTER = 1 if the later-born to which the observation applies is the second of two children.

LAST3 = 1 if the later-born is the last of three children, 0 otherwise.

LAST45 = 1 if the later-born is the last of four or the last of five, 0 otherwise.

LAST613 = 1 if the later-born is the last of from 6 to 13 children (no families had more than 13 children), 0 otherwise.

MID3 = 1 if the later-born is the second of three children, 0 otherwise.

MID45CLO = 1 if the later-born is a middle (intermediate-born) child in a family of four or five children and the number of years in age to the next oldest sibling plus the number of years to the next youngest sibling is less than 4, 0 otherwise.

MID45WID = 1 if the later born is a middle child in a family of four of five children and the number of years separating the next oldest sibling and the next youngest sibling is greater than or equal to 4, 0 otherwise.

SPACE1N = the average spacing between siblings in the same family = the years separating the oldest and the youngest in the family divided by (the number of siblings minus one).

Table 4 (Cont.). Intra-Family Differences in Educational Attainment: Regression Results from the New Jersey Sibling Sample

Independent Variable	Regression number (Dependent variable is EDDIF): (1) coeff.	(1) std.err.	(2) coeff.	(2) std.err.	(3) coeff.	(3) std.err.
constant term	.440 *	(.200)	-.198	(.152)	-.208	(.152)
MALE1	.253	(.146)	.275	(.145)	.528 **	(.133)
MALEH	-.155	(.145)	-.144	(.145)	-.429 **	(.134)
RESP1	.479 *	(.191)	.521 **	(.177)	---	---
RESPH	-.634 **	(.180)	-.657 **	(.178)	---	---
DIFINPUT	---	---	4.758 **	(1.819)	4.983 **	(1.813)
LATTER	-.112	(.261)	---	---	---	---
LAST3	-.347	(.260)	---	---	---	---
LAST45	-.719 **	(.232)	---	---	---	---
LAST613	-.428	(.280)	---	---	---	---
MID3	-.355	(.259)	---	---	---	---
MID45CLO	-.340	(.267)	---	---	---	---
MID45WID	-.304	(.195)	---	---	---	---
SPACE1N	-.065	(.043)	---	---	---	---
R^2_{adj}/s.e.e.	.0785/1.7491		.0752/1.7522		.0396/1.7856	
degrees of freedom	754		761		763	

* = significant at the 5 percent level.
** = significant at the 1 percent level.

The Sample:

767 later-born (not first-born) siblings from 269 families, each compared in achievement with the first-born from the same family.

(Note: the sibling-position binary variables in Regression (1) yield coefficients comparing the impact of assuming each later-born position relative to being a middle child in a family with six or more children. Thus, for example, the coefficient on LAST613 indicates the extent to which the schooling advantage of first-borns over last-borns in these large families differs from (in this case, is less than) the advantage of first-borns over middle children in families of six or more children.)

family, since the average family input index changes little after the third child. The smaller estimate of the difference in schooling between two- and five-child families essentially matches Beverly Duncan's estimate (1969, pp. 648-649) that average schooling is 0.6 years higher in the two-child family than in the five-child family when age and six other variables are held constant. Her estimates do not confirm that the influence of family size on schooling is concentrated into the difference between three-child families and smaller families, as is implied by the index in Table 1 above and roughly confirmed by the results in Tables 2 and 3. Instead, her regressions found no clear pattern of rising or diminishing importance of an extra sibling as family size rose.

Why was the importance of family size shown in the regressions on intrafamily differentials not matched by its impact in the kinds of tests run in Table 2 as well as in the past literature? And why did family size seem to matter most over the smaller-family range here when tests on another survey failed to yield this result? The answers to these questions are not clear. I shall suggest that the resolution of both issues may lie in differing patterns of bias imparted by omitting certain parental characteristics that cannot be measured. The failure of the studies of individual achievement to yield as high an impact for sibling position as did the regressions on intrafamily differentials may be due to a pattern of omitted characteristics that is the opposite of what was considered at one point in the introduction above. There the possibility was mentioned that the usual tests omitted characteristics of individual married couples that tend to produce larger families in correlation with lower achievers than result from other families with the same education and occupational status. The contrast in family-input coefficients between Table 4 and Table 2 now suggests that the most important omitted characteristic may be child-orientation. That is, it may be that within each educational and occupational class individual couples having more children may also be more effective at producing highly schooled, high-status children because their aspirations and/or talents center more on child-rearing. If so, this would explain why the tests comparing individuals from different families attached a lower impact to sibling position: holding parents' education and status constant, being from a larger family appeared to have only moderate influence on achievement when in fact its larger influence was partly offset by a tendency of certain parents with larger families to have other qualities raising achievement.

The omitted-variable bias suspected for the New Jersey sibling sample, however, may not exist for other samples. Here lies a possible resolution of the issue whether the effect of family size tapers off after the third child. The New Jersey sibling sample consists of higher-status individuals with high-status parents, on the average, than are picked up in such samples as the 1962 Survey of Occupational Change in a Generation used by Beverly Duncan. It could well be that the larger families in the OCG sample differ from each other in a way that is the opposite of the pattern just hypothesized for the New Jersey sibling sample. The larger families of the OCG sample are lower in average status and presumably characterized by greater variation in the effectiveness of birth control and foresightedness. Thus it seems more likely for the OCG sample than for the New Jersey sample that parents of eight or nine children differed from parents of four children in unmeasured qualities correlated positively with the number of children and negatively with average achievement of the children included in the sample. In the higher-status New Jersey sample, on the other hand, it seems more reasonable to hypothesize that the extra children in large families were planned, as were future child resources, and that the main unobserved parental trait was a degree of child-orientation. This reasoning might reconcile the tendency of extra siblings to reduce average achievement more over the large-family ranges of the OCG sample than in the large families of the New Jersey sample.

The main outcome of the tests, then, is that sibling position is indeed a significant influence on schooling, and when other things have been held as constant as possible, its influence is in fact greater than ordinary testing methods would suggest. Being from a larger family and being a middle sibling are important achievement disadvantages on the average.

Birth Control and Unequal Schooling Attainment

If sibling position matters to schooling and career attainments, the decline in American birth rates must have made some contribution to reducing inequalities of schooling, status, and income. To know how great a contribution it has made, one must decide on an appropriate hypothetical experiment. The experiment involves imagining how a given cohort would have been distributed over different completed family sizes if births were hypothetically restricted. That in turn requires knowledge of how both achievements and the incidence of prevented births are spread over different sibling positions.

To get a rough idea of the degree of impact of birth restriction on inequality in schooling, let us ask: How does the variability of schooling among the 1008 individuals in the New Jersey sibling sample seem to compare with the variability of schooling in the hypothetical subset of those individuals out of the 1008 who would have been born under conditions of more effective birth control? To imagine the situation with more effective birth control, let us first divide the sample of 1008 individuals into each birth order and each of three classes based on the mother's schooling: less than high school graduation, high school grad, and some college. For each birth-order-and-mother's-schooling group, let us assume that the percentage of births prevented is the percentage of such births between 1960 and 1965 found to have been unwanted in the 1965 National Fertility Survey (Bumpass and Westoff, 1970, p. 1179). This is presumably an underestimate of the incidence of unwanted births among those surviving in the New Jersey sample, since contraception was less effective in the prewar cohorts represented in the sample than in the early pill years investigated in the 1965 survey. The hypothetically prevented unwanted births turn out to be about 21 percent of the total (23 percent for mothers in the lowest-schooling group, and 14 percent for mothers with some college). Let us next regroup the 798 wanted births into their new family size groups. Finally, we compare the standard deviations and means of the schooling of these 798 with the original 1008, under the assumption that the means and variances within each family-size-and-mother's education group is unchanged. The result is a measure of how the average level and variability of schooling would be affected if nothing changed but the numbers of persons falling in each group.

The impact of the hypothetical birth restriction is unimpressive according to some measures but more noteworthy according to others. The mean schooling index of the sample of 798 wanted births was 3.57 percent, or roughly 0.32 years, higher than that of the sample of all births.[19] The standard deviations hardly differed at all in absolute terms, being only 0.3 percent lower with the improved birth control. At first glance it appears that birth reduction could have raised average schooling noticeably while leaving the inequality of schooling unaffected. Yet for several reasons this appearance understates the contribution of birth reduction to the historic leveling of American educational attainment. One reason is that the level of the standard deviation is raised by the higher average schooling of the wanted-birth group. When the standard deviation is expressed as a share of mean years of schooling, it proves to be 3.74 percent lower with the smaller-family group. That represents a minor but noteworthy contribution to the total reduction in schooling inequality over successive twentieth-century cohorts, especially if one compares the New Jersey respondents' birth cohort (1905-1911) with the cohort of 30 years later, which had a fertility rate that was about 37 percent lower, instead of the 21 percent used here.[20]

The impact of birth reduction on schooling inequality would also appear larger in a perspective spanning two or more generations. The higher average schooling, and slightly less variable schooling, of a generation born into smaller families would not only transmit itself to the following generation for constant family size, but would also be a force reducing fertility further in the next generation, thereby making the cycle of fewer births and higher and more equal schooling become cumulative.

It should also be remembered that this estimation of the impact of birth restriction takes as fixed a reward structure that birth restriction apparently changes. A drop in the birth rate will slow down the growth of the labor force, thereby raising unskilled wages relative to top salaries and profit rates. For this reason, too, birth restriction tends to reduce inequalities in income and schooling.

There is, then, sufficient reason to conclude that fertility rates have a noteworthy effect on the levels of inequality of schooling (and incomes) in America. The desire to promote greater equality of income is thus one more justification for antinatalist policies. This is not to say that the glacial progress toward equality through birth restriction is fast enough. To take great comfort in the equalizing effect of birth rate decline over the generations, one would have to attach less urgency to the goal of more equal schooling and incomes than is professed by most Americans.

Notes

1. Throughout this paper, as in most of the literature on this subject, the term "achievement" is used as a shorthand for educational and career attainments. The measure of educational attainment is simply the number of years of schooling received by some adult age, or some index monotonically related to the number of years of schooling. The measure of career attainment available in the relevant data samples is an index of the socioeconomic status of the person's occupation. Lifetime potential income would also be a reasonable measure of career attainment for present purposes if it were available. The term "achievement" is <u>not</u> meant to apply, however, to IQ, grades in school, emotional stability, or happiness.

2. Some recent studies focusing at least in part on the influence of family on subsequent achievement are: Adams and Meidam (1968); Bayer (1966, 1967a, 1967b); Blau and Duncan (1967); and Wray (1971, pp. 425-452). Studies briefly noting family size as one of many influences on educational and/or career attainments are: Bowles (1970); Bowles (1972); Beverly Duncan (1965); Beverly Duncan (1967); Duncan, Featherman, and Duncan (1972); Johnson and Stafford (1973); and Leibowitz (1974).

3. See Alexander (1968); Bayer (1966, 1967a, 1967b); Blau and Duncan (1967); Schacter (1963); and Sutton-Smith and Rosenberg (1970, Ch. 5); and the sources cited there.

4. See Bayer (1966, 1967a, 1967b), and Blau and Duncan (1967).

5. See, for example, Adams and Meidam (1968); Alexander (1968), the exchange between Hermalin (1967) and Bayer (1967b), Hermalin (1969), Smelser and Stewart (1968), and Sutton-Smith and Rosenberg (1970, Ch. 5).

6. In what follows, I shall use the term "sibling position" as a phrase encompassing family size, birth order, and spacing between siblings.

7. In her handling of the same survey data, Beverly Duncan (1967) did examine each five-year age group of respondents separately, again finding a consistently negative predicted effect of number of siblings on educational attainment. She did not present data on the statistical significance of this effect, however, and confined her attention to the number of siblings rather than exploring the effects of sibling position and child spacing.

8. For a convenient review of the relevant literature, see Sutton-Smith and Rosenberg (1970).

9. A natural objection at this point is that the quality of adult-child interaction may be a more important influence on achievement than the amounts of time and money spent. That is possible, but the drift of the recent literature is that the hypothesized effects <u>of sibling position</u> on both the quantity and the quality of family support for the child are quite similar. The existence of extra siblings in the home is supposed to lower

the quality of parent-child interaction roughly in proportion to its reduction of the amount of time and commodities received by the child. Thus, to test the more quantifiable part of the hypotheses about sibling position is to test the entire line of reasoning.

This does not mean that the quality and quantity of support for a child's development are proportional in all profiles. If the subject of investigation were parental attributes rather than sibling position, more direct attention would have to be given to the quality of interaction than is given here. More educated parents, for example, may well be more "productive" at combining their time and financial resources to produce high-education and high-status children than are less educated parents with the same incomes. This is very possible even though they apparently do not spend significantly more time interacting with their children than do less educated parents.

10. This finding contradicts the conclusion of other authors (e.g., Hill and Stafford [1972], Leibowitz [1974]) that more educated mothers spend more time with their children. My reasons for concluding that they have based this finding on inappropriately measured variables are given in Appendix C.

11. For an examination of the patterns of child-care time by mother's work status, see Appendix C. A calculation based on the case of a two-child family with three-year spacing suggests that the extra amount of care time received by the children of a working mother from persons other than her is half again as large as the loss in time inputs from her because she works outside the home. This implies that the children of the working mother are receiving less quality-adjusted time inputs in all only if one could somehow show that an hour of the mother's time contributes more to the child's development even at the margin than 1.5 hours of the time of others caring for her children. It seems unlikely that a strong case could be made for so great an inequality in achievement contributions at the margin.

12. Appendix D of Fertility and Scarcity in America. By 1977 it should be possible to make more refined calculations of consumption effects of children, by drawing on the greater detail for expenditure and (it is hoped) on the ages of children to be made available in the 1972-73 Consumer Expenditure Survey.

13. One shred of additional evidence that tends to support this conjecture is a set of calculations I made of a proxy for the relationship between sibling position and the individual child's commodity inputs. I calculated the net impact of a sibling in each position on total family consumption over the 18 years of his childhood (note again: this is not what the child received, but just his impact on family expenditures). Last-borns tend to affect total family consumption more strongly than do first-born or (especially) middle-born children. It seems likely that the true amount of commodities being received by a last-born is also larger than that received by each of his older siblings. The proxy was also enough higher for the last-born to suggest that his input advantage over earlier siblings was greater for commodity inputs than it was for time inputs. It thus appears that within each family size the last-borns are raised in a more commodity-intensive fashion than their older siblings.

14. For a more detailed description of the sample and for extensive analysis of the homogeneity of the siblings in the sample, see Albert I. Hermalin (1969).

The elimination of observations with incomplete information narrowed the sample from 1087 siblings, including the 312 respondents, to 1008 siblings, including 289 respondents.

Larger samples may soon be available which meet the requirement of having data on the age, sex, education, and occupation of each adult sibling. One possibility, being investigated by Professor Hermalin, is that the original interview sheets of the 1965 Productive Americans survey will yield these data on all the grown children of several hundred elderly respondents. Another source of such data, apparently, will be the 1970 National Fertility Survey. These samples could be employed as a cross-check on the results presented here, but were not available at the time of writing.

15. Unreported regressions showed that removing the 12 siblings under 40 had no effect on intrafamily patterns of educational attainment.

16. Two additional regressions tried adding Catholic parentage to the variables in Table 3 as a determinant of occupational status. The Catholicism variable showed a negative sign, but was not significant. The sample lacked enough Jews to test whether their religion showed up as a separate influence.

17. In two-child and three-child families, all three tables show an advantage of the first-born over the last-born, though the difference is never significant at the five percent level when respondent status is taken into account. This result leaves unresolved a conflict between the predictions of the SIBLOSS index and the opinions of some past authors. Some have implied that in families of two or three children, the first-born should have significantly greater achievement than the last-born, yet the SIBLOSS index predicts the same amount of achievement for both. Only tests on other samples can resolve this conflict.

18. Adding the age differential (and the differential in the squares of ages) does reduce the magnitude and significance of the DIFINPUT index. In a sample restricted to later-borns, the age difference between each sibling and the first-born is highly correlated with the DIFINPUT index, which predicts higher inputs for a later-born child the farther his birth lags behind the first birth. Thus additional regressions revealed that neither age nor the DIFINPUT index was significant when both were run together. This result is interpreted as showing that age differentials themselves do not belong in the model used for intrafamily testing. Since age proved insignificant in the comparison of individuals from different families while SIBLOSS was significant (Table 2), it is viewed as an unimportant determinant of achievement differentials within families, and as a variable that just happens to be correlated with DIFINPUT in a sample of later-borns.

Other unreported regressions tested the hypothesis by Smelser and Stewart (1968) that the schooling advantage of the first-born is greater in families with children of both sexes than in families where all were of the same sex. Regressions were run with the variable ALLMALE added to sets of variables like those included in Table 4. The ALLMALE variable did have the negative coefficient that Smelser and Stewart would predict, suggesting that when boys are raised without sisters, the first-born has less advantage than when both sexes are represented. But the coefficient for ALLMALE was never statistically significant. This leaves the issue unresolved. This interaction of sex with sibling position deserves further exploration, especially if the development of sex preselection technology leads to a shift toward first-born males followed by second-born females, as predicted by Westoff and Rindfuss (1974). If Smelser and Stewart are right, sex preselection could be a minor force tending to widen male-female differentials in occupational status through an effect of sibling positions on schooling.

19. The calculations were performed on the index of the mother's schooling, which assigned such values as 1 to 0-4 years of schooling, 3 to an eighth-grade education, 5 to a high school finisher, and 7 to a college graduate who received no post-graduate schooling. The results in terms of this index were:

	Mean	Standard Deviation
All 1008 births	4.6605	1.9103
798 wanted births	4.5000	1.9161

If one starts from equating the index value of 5.0 with 12 years of schooling and equates each point of the index with two years of schooling, then it could be said that the respective means were 11.321 years and 11.000 years, while the standard deviations were 3.8322 years and 3.8206 years.

20. Average schooling rose from 9.6 years for the 1905-1909 birth cohort to 12.1 years for the 1935-1939 cohort (Beverly Duncan [1968], p. 611). The most comparable pair of figures available on standard deviations is the estimation of Chiswick and Mincer that the standard deviation among adults fell from 3.70 years for 1949 to 3.04 years for 1970) Chiswick and Mincer [1972], p. S43).

APPENDIX

TIME INPUTS INTO SIBLINGS, 1967-68

The task of measuring family time inputs into children is basic to any study of the costs of children and the relationship between the inputs they receive and their subsequent achievements. The necessary measurements must be based on time-budget studies that peer into the allocation of time within the home. A child takes time away from many things--from care of other children, from other household work, and from paid work. The amount of time involved cannot be determined at all just by measuring the effect of the child on either paid work time or total household work time. Only a detailed survey of the allocation of time within the home will do as a basis for measuring time inputs into children.

The Basic Identification Problem

There is an unavoidable difficulty involved in any attempt to measure what one child receives or takes away from other things when more than one child is present in the home: all that we can reasonably measure is the time spent on the care of all children or all family members. Regressions therefore can show us only the net effect of an extra child on total care time. We cannot directly observe the time inputs into one child unless he is an only child. The problem is not just that children divide up total child care time in unknown proportions. The problem is also that some of that time they do not divide up at all. Often parental attention is a joint good shared by more than one sibling, with its enjoyment by one sibling detracting not at all from its enjoyment from the other(s). Thus even before confronting any data on time use one must realize that only a leap of faith, only _a priori_ assumptions, can lead to quantitative estimates of the time that a non-only child receives or the time he takes away from other activities.

There are, however, two plausible assumptions that allow us to establish upper and lower bounds on the child care time received by a sibling:

(a) It can be assumed that a sibling does not receive more care time than would be received by an only child of the same age and parental attributes. This assumption might be violated if somehow the presence of an only child in the home placed demands on adult time that fell below some threshold necessary to divert the adults away from other activities (e.g., paid work) while the presence of more children would force a major shift toward care of each child. That seems unlikely, however, for large samples.

(b) It can be assumed that a sibling does not receive less care time, or take less care time from other siblings, than he adds to total child care time. It seems likely that he takes more from others, and receives more, than his _net_ addition to _total_ child care time. Therefore regression estimates of the effects of a non-only child on total child care time can be used to establish a lower bound on the child care time put into a sibling.

These two assumptions are made here, and yield upper and lower bounds on the time inputs into a child when other children are present in the home. The true magnitudes lie somewhere between these two bounds. To create specific estimates of the child care time inputs, I shall arbitrarily assume the midpoint between the two bounds to be the best measure of the care time received by the individual sibling. That is, I shall assume that he receives something midway between his contribution to total child care time (the lower bound) and the time he would have received as an only child (the upper bound). These estimates are used in the text above in ways that do not seem to require that this mid-range estimate be accurate.

The effects of an extra child on time use, however, go beyond the child care time he receives. The child forces others, usually his mother, to engage in extra household chores just to keep his presence from damaging other household pursuits, such as cleanliness and disease prevention. The time spent on these extra chores is truly part of the time cost of the extra child, even though he does not personally receive them as inputs relevant to his later development. These chore time burdens can be estimated by regressions explaining total chore time in terms of the presence of children and other variables. That procedure is followed here. Part of the estimated time cost of an extra child is the net addition to the mother's time at meal preparation, meal cleanup, and washing prompted by his presence in the home. These three tasks are ones that regressions show take more time with each extra child. (It turns out, not surprisingly, that the extra chore burden, like most home burdens, falls almost entirely on Mother.) Note that the chore burden has to be measured as the net addition to time spent on these specific chores by persons other than the extra child. It is practicably impossible to observe gross chore burdens for an extra child. We would need unrealistically detailed survey data to find out, for example, to what extent cleaning up the baby's house mess made a wife not only spend more total time on house care but also cut back on her cleaning up after herself and her husband. Selected measures of net chore increases will have to do, even though these underestimated the gross time cost of an extra child.

As an offset to this net chore effect, the child also helps out with chores that the family would have to perform in any case. He also, as a teenager, works for pay outside the household while still a member of it. His house work contribution can be estimated directly from time-use surveys. His paid work can be measured from census and labor market data. These contributions of time, like the net effect on family chore loads, are relevant to the time cost of a child, but not the time inputs into his development.

The Cornell Syracuse Survey, 1967-68

Gathering believable survey data on how time is allocated within the home is not easy. To be useful, the survey data must represent an annoying bother and invasion of privacy to many respondents. The researcher must go to great pains to keep respondents from putting down just any old thing to get the interviewer to go away. Respondents may give answers that are less than candid. Some may exaggerate the amount of time they spend on socially laudable things like reading to their children or fixing dinner as opposed to watching television. Nor do the surveys offer any positive evidence that people go to the bathroom or have intercourse, although these activities presumably get discreetly buried under "other personal activities." To maximize its credibility a time-use survey must have respondents fill out a time clock. Only by recording or reconstructuring a 24-hour stretch will respondents make the effort to unravel just what it was they did with their time. If the interviewer asks cheap quick questions like "How much time did you spend on house work (or taking care of the children) yesterday?" he will get cheap quick answers. Another valuable precaution is to be prepared for the fact that respondents often do two things at once. A mother who is ironing while watching the kids is engaged in a primary activity (ironing) and a secondary activity (supervising the children). Any study pursuing what time is taken away from some tasks for others must allow for this distinction. (The present study will focus on the "primary" time use only.)

The only survey currently available that has clearly taken all the necessary precautions is the survey of time use in 1,296 Syracuse families in 1967-68.* Each family had to account for the activities of each family

*A time-use survey currently underway at the University of Michigan may prove to meet the same standards. Past Michigan surveys, however, have not been of sufficient detail and care regarding home time use to support the kind of child care estimates sought here.

member six and older on two dates. The sample was a stratified random sample evenly over family sizes, days of the week, and months of the year. It turned out that the sample was somewhat younger and more educated than would have been true for Syracuse as a whole in that year.

The survey gathered very detailed data on time use itself plus extensive information the family's endowment of home equipment, right down to barbecue grills. Three lacunae stand out: nothing was recorded about religion, race, or income. That last omission was offset by the availability of information on the educational attainment, occupation, and age of both husband and wife.

To use the Cornell survey to determine the effects of children on home time use, one should begin by dividing the sample into different days of the week and time of the year. The effects (coefficients) of a child on patterns of time use are likely to vary over the calendar. A school-age child takes more attention, and helps out more as well, on weekends than on weekdays, and more in the summer than over the rest of the year. Accordingly, I divided the 1592 interview observations (2 interviews times 1296 families) into three mutually exclusive samples:

Sample 1: nonsummer weekdays
Sample 2: weekend days (any month of the year)
Sample 3: summer weekdays (June, July, August)

Doing so raised the number of observations to twice the number of families. On the other hand, some of the observations were discarded from the set of three samples. This happened whenever both interviews with the same family occurred in the same sample (e.g., on nonsummer weekdays). It was felt that including both observations would raise unresolvable issues about correlation among individual error terms. The second interview for the family was thus discarded. The net results of this sample design procedure were a sample of 925 nonsummer weekdays, a sample of 734 weekend days, and a sample of 362 summer weekdays.

Since the focus of this study is on the changes in behavior associated with extra children, it is important to specify the family composition variables carefully. Children must be broken down into different age groups, since a newborn requires much more time inputs than a teenager. It is also likely (as the results below confirm) that a child of given age has less effect on the parents' use of time the more siblings there are in the home. There is thus a need for presence-of-children variables that count the number of children in a particular age group within families of a given total number of children, e.g., there should be a variable counting the number of children aged 6-11 within families having four children in the home. This precaution is taken in the regression models below, and the results confirm that a 6-11 year old child has less effect on family time use, and appears to receive less attention, in a family of four children than when he is an only child of the same age.

Time Use Regression Results

Table A-1 displays selected regressions showing the effects of children in the home and other variables on the amounts of time given to various home tasks. Long as the list of independent variables may be, some variables are not displayed here. Table A-1 leaves out the NKIDS coefficients and standard errors for families having more than four children (NKIDS50 through NKIDS74). These were included in the regressions, but are omitted here because some of them proved less reliable than the NKIDS coefficients for families with four or fewer children. Specifically, it turned out that the

Definition of Variables Used

Characteristics of Husband and Wife

PREDWAGE = predicted hourly wage rate of the husband in 1969, in dollars per hour. The wage rate is a transformation on the educational attainment, occupational class, and exact age of the husband, using the detailed table of predicted values given in Boone A. Turchi, "The Demand for Children: An Economic Analysis of Fertility in the United States," unpublished doctoral dissertation, University of Michigan, Ann Arbor, 1973, Table VII-1.
COLLEGEW = 1 if wife has received a college degree, zero otherwise.
HSW = 1 if wife graduated from high school but does not hold a college degree, zero otherwise.
AGEWIFE1 = 1 if wife is under 35, zero otherwise.
AGEWIFE2 = 1 if wife is 35 but less than 45, zero otherwise.
AGEWIFE3 = 1 if wife is 45 but less than 55, zero otherwise.
DISABLEW = 1 if wife is ill or handicapped on the day of the interview data, otherwise = 0.

Date of Interview

SUMMER = 1 if the interview data refer to a date in June, July, or August, zero otherwise.
SUNHOL = 1 if interview refers to a Sunday, zero otherwise. Due to a coding error, about half of the Sunday observations were incorrectly recorded as non-Sunday observations. There is no known relationship between the incidence of this error and other independent variables, so that its only effect appears to be to cast doubt on the SUNHOL coefficient itself within the samples confined to weekend observations.

Time Spent on Various Kinds of Household Work (all variables in min./da.)

DRUDGEW = time spent by wife on all household work.
DRUDGEH = " " " husband on all household work.
DRUDGE27 = " " " children 12-17 on all household work.
DRUDGE61 = " " " children 6-11 on " " " .
DRUDGES = total time spent on all household work by all persons.
CARE1W = time spent on physical care of family members by the wife.
CARE1H = " " " " " " " " husband.
CARE1S = " " " " " " " " all persons.
CARE2W = " " other " " " " " the wife.
CARE2H = " " " " " " " " the husband.
CARE2S = " " " " " " " " all persons.

Numbers and Ages of Children in the Home

NUMKIDS = number of children in the home.
KIDS0 = " " " " under 1 year in age.
KIDS1 = " " " " 1 year old.
KIDS2 = " " " " 2-5 years old.
KIDS3 = " " " " 6-11 years old.
KIDS4 = " " " " 12-17 years old. (None in the sample were 18 or older.)

NKIDSij: the number of children in a family with i children who are in the jth age group. That is, the NKIDSij variables are a transformation on the total number of children (NUMKIDS) and the number in each age group (KIDSj) as follows:

Value of NKIDSij = Value of:	Total number of children (NUMKIDS): 1	2	3	4	5	6	7
KIDS0 :	NKIDS10	NKIDS20	..	..	..	..	NKIDS70
KIDS1 :	NKIDS11						..
KIDS2 :	NKIDS12						..
KIDS3 :	NKIDS13						..
KIDS4 :	NKIDS14	..	..	..	..	..	NKIDS74

(Thus, for example, a family of four children aged 7, 5, 3, and 1 would have NKIDS43 = 1 (the 7-year-old), NKIDS42 = 2 (the two middle children), NKIDS41 = 1, and all other NKIDSij variables = 0.)

TABLE A-1

Selected Home Time Use Regressions, Syracuse Survey, 1967-68
(Dependent variables in minutes per day)
(Standard errors of regression coefficients in parentheses)

Independent Variables:	Dependent Variable: (1) DRUDGEW: wife's total home work time	(2) DRUDGEW: wife's total home work time	(3) DRUDGEW: wife's total home work time	(4) DRUDGES: total home work time by all persons
CONSTANT	247.6	228.4	258.8	368.8
	(37.1)	(87.1)	(47.5)	(51.0)
COLLEGEW	-17.6	.058	23.7	.0
	(22.6)	(38.5)	(27.1)	(31.1)
HSW	22.6	14.1	-15.6	13.6
	(18.0)	(27.9)	(21.2)	(24.7)
PREDWAGE	10.0	5.5	6.9	5.5
	(4.9)	(10.3)	(6.2)	(6.8)
AGEWIFE1	-37.0	71.0	-7.7	-98.4
	(32.1)	(81.2)	(41.5)	(44.2)
AGEWIFE2	-19.5	57.9	-6.7	-76.6
	(32.9)	(82.3)	(42.4)	(45.2)
AGEWIFE3	6.9	104.7	25.7	-42.2
	(32.0)	(83.2)	(42.1)	(44.0)
DISABLEW	-20.1	226.8	-8.4	63.9
	(42.0)	(79.4)	(58.7)	(57.9)
NKIDS10	287.9	182.6	150.3	349.9
	(32.7)	(56.1)	(38.6)	(45.0)
NKIDS11	241.9	148.9	97.5	296.4
	(31.8)	(62.4)	(38.7)	(43.7)
NKIDS12	101.2	-34.8	59.9	172.8
	(32.3)	(58.8)	(38.5)	(44.4)
NKIDS13	108.4	116.6	58.2	178.8
	(31.3)	(63.9)	(38.6)	(43.1)
NKIDS14	57.7	6.1	68.8	166.4
	(32.3)	(60.8)	(38.3)	(44.5)
NKIDS20	184.7	181.8	133.0	217.6
	(22.7)	(37.3)	(27.0)	(31.2)
NKIDS21	161.4	145.0	109.8	203.6
	(22.7)	(34.7)	(26.0)	(31.3)
NKIDS22	122.1	83.6	82.7	166.6
	(13.9)	(24.4)	(16.6)	(19.1)
NKIDS23	92.5	59.6	58.0	148.9
	(13.7)	(26.4)	(16.6)	(18.8)
NKIDS24	36.1	13.2	45.2	96.1
	(14.7)	(28.5)	(18.2)	(20.3)
NKIDS30	106.9	223.5	177.7	125.6
	(31.4)	(56.0)	(42.2)	(43.3)
NKIDS31	151.5	140.4	93.6	140.8
	(27.6)	(44.0)	(30.0)	(38.0)

TABLE A-1 (cont.)

Independent Variables:	Dependent Variable: (1) DRUDGEW: wife's total home work time	(2) DRUDGEW: wife's total home work time	(3) DRUDGEW: wife's total home work time	(4) DRUDGES: total home work time by all persons
NKIDS32	101.9	65.5	46.1	123.3
	(13.1)	(22.2)	(15.4)	(18.1)
NKIDS33	68.9	46.0	53.5	121.1
	(9.4)	(15.4)	(10.9)	(12.9)
NKIDS34	20.3	5.6	16.9	83.1
	(9.4)	(17.5)	(11.8)	(13.6)
NKIDS40	144.1	68.4	107.3	243.1
	(31.9)	(51.8)	(36.4)	(43.9)
NKIDS41	123.8	93.1	35.1	136.7
	(33.7)	(50.7)	(33.3)	(46.4)
NKIDS42	86.1	53.5	62.2	91.6
	(11.5)	(17.9)	(13.5)	(15.8)
NKIDS43	55.4	51.1	30.4	106.4
	(7.6)	(14.2)	(9.3)	(10.4)
NKIDS44	12.5	10.6	24.9	53.9
	(11.2)	(15.8)	(11.4)	(15.4)
SUMMER	---	---	-24.5	---
	---	---	(13.7)	---
SUNHOL	---	---	-87.0	---
	---	---	(13.9)	---
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Sample constraint:	nonsummer weekdays	summer weekdays	weekend days	nonsummer weekdays
$R^2_{adj.}$/s.e.e.	.3240/152.44	.2248/164.70	.1713/160.15	.2978/209.84
no. of obs./ d.f.	925/882	362/320	734/689	925/882

Note: In all regressions involving the set of NKIDS variables, the coefficients for variables NKIDS50 through NKIDS74 have been omitted here, for reasons mentioned in the text of this Appendix.

TABLE A-1 (cont.)

Independent Variable:	(5) DRUDGES: total home work time, all persons	(6) DRUDGES: total home work time, all persons	(7) CARE1W: wife's physical care of family members	(8) CARE1W: wife's physical care of family members
	Dependent Variable:			
CONSTANT	315.2	267.0	-7.9	-1.9
	(135.2)	(89.8)	(11.7)	(25.9)
COLLEGEW	-114.3	20.0	10.0	9.2
	(59.8)	(51.2)	(7.1)	(11.4)
HSW	0.5	-48.2	6.8	1.3
	(43.4)	(40.1)	(5.7)	(8.3)
PREDWAGE	15.5	26.6	.4	-0.4
	(15.9)	(11.7)	(1.6)	(3.1)
AGEWIFE1	57.1	41.7	.0	0.1
	(126.1)	(78.4)	(10.2)	(24.2)
AGEWIFE2	81.0	19.1	0.5	-12.6
	(127.8)	(80.1)	(10.4)	(24.5)
AGEWIFE3	130.1	43.1	4.5	-5.7
	(129.3)	(79.6)	(10.1)	(24.8)
DISABLEW	231.6	-33.4	-6.9	-0.7
	(123.4)	(110.8)	(13.3)	(23.6)
NKIDS10	182.3	220.7	145.6	123.3
	(87.1)	(72.8)	(10.3)	(16.7)
NKIDS11	165.9	125.0	89.0	73.4
	(97.0)	(73.2)	(10.1)	(18.6)
NKIDS12	21.1	141.7	32.6	30.1
	(91.4)	(72.8)	(10.2)	(17.5)
NKIDS13	80.4	94.8	7.1	16.7
	(99.2)	(72.8)	(9.9)	(19.0)
NKIDS14	87.5	263.0	-2.0	17.9
	(94.5)	(72.4)	(10.2)	(18.1)
NKIDS20	188.3	159.3	130.6	162.3
	(58.0)	(51.0)	(7.2)	(11.1)
NKIDS21	221.2	203.5	77.9	67.0
	(53.8)	(49.1)	(7.2)	(10.3)
NKIDS22	72.2	114.2	29.2	24.3
	(37.9)	(31.3)	(4.4)	(7.3)
NKIDS23	87.0	122.6	14.3	12.1
	(41.0)	(31.3)	(4.3)	(7.8)
NKIDS24	74.4	177.4	4.1	12.7
	(44.3)	(34.3)	(4.7)	(8.5)
NKIDS30	160.6	267.2	117.6	128.4
	(87.0)	(79.7)	(9.9)	(16.7)
NKIDS31	95.8	136.2	65.2	71.7
	(68.3)	(56.6)	(8.7)	(13.1)
NKIDS32	68.1	74.9	26.5	39.5
	(34.5)	(29.0)	(4.1)	(6.6)

TABLE A-1 (cont.)

Independent Variable:	Dependent Variable: (5) DRUDGES: total home work time, all persons	(6) DRUDGES: total home work time, all persons	(7) CARE1W: wife's physical care of family members	(8) CARE1W: wife's physical care of family members
NKIDS33	116.7	121.4	5.3	5.7
	(23.9)	(20.6)	(3.0)	(4.6)
NKIDS34	52.4	102.8	2.2	7.0
	(27.2)	(22.2)	(3.1)	(5.2)
NKIDS40	185.8	221.9	123.7	65.5
	(80.5)	(68.8)	(10.1)	(15.4)
NKIDS41	113.1	75.7	66.8	66.0
	(78.8)	(62.9)	(10.7)	(15.1)
NKIDS42	46.7	74.5	23.8	24.4
	(27.8)	(25.4)	(3.6)	(5.3)
NKIDS43	71.2	89.8	6.2	7.6
	(22.1)	(17.6)	(2.4)	(4.2)
NKIDS44	53.5	105.3	1.9	6.2
	(24.6)	(21.6)	(3.5)	(4.7)
SUMMER	---	-29.0	---	---
	---	(25.9)	---	---
SUNHOL	---	-129.9	---	---
	---	(26.2)	---	---
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Sample constraint:	summer weekdays	weekends	nonsummer weekdays	summer weekdays
$R^2_{adj.}$/s.e.e.	.1854/255.87	.2135/302.58	.6173/48.23	.5998/49.02
no. of obs./ d.f.	362/320	734/689	925/882	362/320

TABLE A-1 (cont.)

Independent Variable:	Dependent Variable: (9) CARE1W: wife's physical of family members	(10) CARE1H: husband's physical care of family members	(11) CARE1H: husband's physical care of family members	(12) CARE1H: husband's physical care of family members
CONSTANT	-2.2	-1.4	2.8	-4.4
	(13.2)	(4.2)	(10.1)	(6.0)
COLLEGEW	4.7	-4.1	5.2	4.9
	(7.5)	(2.5)	(4.6)	(3.4)
HSW	-3.9	-2.4	1.1	-0.3
	(5.9)	(2.0)	(3.4)	(2.7)
PREDWAGE	1.4	0.7	-1.0	1.2
	(1.7)	(.6)	(1.2)	(0.8)
AGEWIFE1	-2.8	1.5	-2.4	-1.8
	(11.5)	(3.6)	(9.8)	(5.3)
AGEWIFE2	0.5	2.3	0.9	-2.7
	(11.8)	(3.7)	(9.9)	(5.4)
AGEWIFE3	2.8	0.7	-0.4	.0
	(11.7)	(3.6)	(10.0)	(5.4)
DISABLEW	-10.2	-0.3	4.7	1.7
	(16.3)	(4.7)	(9.6)	(7.5)
NKIDS10	131.1	13.9	30.3	9.3
	(10.7)	(3.7)	(6.8)	(4.9)
NKIDS11	78.0	6.0	9.3	2.7
	(10.7)	(3.6)	(7.5)	(4.9)
NKIDS12	34.6	6.1	3.1	6.0
	(10.7)	(3.6)	(7.1)	(4.9)
NKIDS13	4.2	-1.0	2.4	-0.5
	(10.7)	(3.5)	(7.7)	(4.9)
NKIDS14	2.2	0.5	1.1	-0.8
	(10.6)	(3.6)	(7.3)	(4.9)
NKIDS20	123.3	8.8	4.7	12.0
	(7.5)	(2.5)	(4.5)	(3.4)
NKIDS21	63.4	1.4	12.6	11.6
	(7.2)	(2.5)	(4.2)	(3.3)
NKIDS22	31.4	3.5	2.5	7.1
	(4.6)	(1.6)	(2.9)	(2.1)
NKIDS23	10.8	0.8	1.0	-0.0
	(4.6)	(1.5)	(3.2)	(2.1)
NKIDS24	3.4	-0.3	1.1	-0.9
	(5.0)	(1.6)	(.34)	(2.3)
NKIDS30	129.1	14.9	16.4	11.3
	(11.7)	(3.5)	(6.8)	(5.4)
NKIDS31	70.5	-0.0	2.5	7.4
	(8.3)	(3.1)	(5.3)	(3.8)
NKIDS32	20.5	1.3	8.8	6.5
	(4.3)	(1.5)	(2.7)	(2.0)

TABLE A-1 (cont.)

Independent Variable:	Dependent Variable: (9) CARE1W: wife's physical of family members	(10) CARE1H: husband's physical care of family members	(11) CARE1H: husband's physical care of family members	(12) CARE1H: husband's physical care of family members
NKIDS33	7.2	1.8	0.3	1.8
	(3.0)	(1.1)	(1.9)	(1.4)
NKIDS34	1.7	-0.9	0.2	-0.6
	(3.3)	(1.1)	(2.1)	(1.5)
NKIDS40	97.9	11.3	2.6	11.3
	(10.1)	(3.6)	(6.3)	(4.6)
NKIDS41	35.4	2.6	10.4	9.1
	(9.2)	(3.8)	(6.1)	(4.2)
NKIDS42	26.7	5.2	0.3	7.3
	(3.7)	(1.3)	(2.2)	(1.7)
NKIDS43	4.8	-0.3	0.4	1.6
	(2.6)	(0.8)	(1.7)	(1.2)
NKIDS44	0.5	0.1	1.3	-0.6
	(3.2)	(1.3)	(1.9)	(1.5)
SUMMER	-2.8	---	---	-0.6
	(3.8)	---	---	(1.7)
SUNHOL	-6.3	---	---	0.3
	(3.9)	---	---	(1.8)
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Sample constraint:	weekend days	nonsummer weekdays	summer weekdays	weekend days
$R^2_{adj.}$/s.e.e.	.5941/44.42	.0807/17.07	.0403/19.91	.1492/20.37
No. of obs./ d.f.	734/689	925/882	362/320	734/689

Table A-1 (Cont.)

Independent Variables:	(13) CARE1S: phys. care of family, by all persons		(14) CARE1s		(15) CARE1S		(16) CARE2W: other care of family members, by wife	
	Dependent Variable:							
CONSTANT	-11.2	(12.8)	5.0	(32.0)	-4.8	(15.2)	-9.5	(11.7)
COLLEGEW	7.3	(7.8)	17.9	(14.2)	10.0	(8.7)	5.8	(7.1)
HSW	5.8	(6.2)	3.0	(10.2)	-3.3	(6.8)	2.5	(5.7)
PREDWAGE	1.3	(1.7)	-2.7	(3.8)	2.1	(2.0)	2.3	(1.6)
AGEWIFE1	2.2	(11.1)	-2.8	(30.0)	-5.1	(13.3)	2.1	(10.1)
AGEWIFE2	2.8	(11.4)	-1.4	(30.3)	-4.9	(13.6)	1.6	(10.4)
AGEWIFE3	4.6	(11.1)	-5.0	(30.6)	1.5	(13.5)	5.7	(10.1)
DISABLEW	-3.1	(14.6)	7.5	(29.2)	-8.7	(18.8)	-2.4	(13.3)
NKIDS10	161.3	(11.3)	153.8	(20.6)	146.8	(12.3)	64.5	(10.3)
NKIDS11	94.4	(11.0)	112.7	(23.0)	80.2	(12.4)	62.2	(10.0)
NKIDS12	39.1	(11.2)	34.2	(21.6)	40.3	(12.3)	36.9	(10.2)
NKIDS13	6.0	(10.8)	25.8	(23.5)	4.8	(12.3)	27.5	(9.9)
NKIDS14	-0.9	(11.2)	20.8	(22.4)	2.3	(12.3)	16.9	(10.2)
NKIDS20	139.7	(7.8)	172.4	(13.7)	135.3	(8.6)	26.9	(7.2)
NKIDS21	82.2	(7.9)	90.5	(12.8)	80.8	(8.3)	31.2	(7.2)
NKIDS22	32.2	(4.8)	26.5	(9.0)	37.9	(5.3)	19.7	(4.4)
NKIDS23	15.5	(4.7)	15.4	(9.7)	14.3	(5.3)	24.8	(4.3)
NKIDS24	5.1	(5.1)	18.1	(10.5)	4.6	(5.8)	9.1	(4.7)
NKIDS30	134.2	(10.9)	146.4	(20.6)	159.3	(13.5)	16.2	(9.9)
NKIDS31	65.7	(9.6)	73.8	(16.2)	79.9	(9.6)	32.3	(8.7)
NKIDS32	27.8	(4.5)	48.4	(81.7)	27.3	(4.9)	20.1	(4.1)
NKIDS33	7.0	(3.2)	7.7	(56.6)	8.9	(3.5)	20.3	(3.0)
NKIDS34	1.8	(3.4)	8.8	(64.4)	3.1	(3.8)	3.3	(3.1)
NKIDS40	134.6	(11.0)	72.6	(19.1)	108.1	(11.6)	18.3	(10.1)
NKIDS41	74.2	(11.7)	76.3	(18.7)	47.4	(10.6)	35.2	(10.7)
NKIDS42	29.6	(4.0)	25.3	(65.8)	34.1	(4.3)	22.4	(3.6)
NKIDS43	5.9	(2.6)	9.4	(52.3)	7.2	(3.0)	13.2	(2.4)
NKIDS44	2.4	(3.9)	7.6	(58.2)	.2	(3.7)	6.0	(3.5)
SUMMER	--	--	--	--	--	--	--	--
SUNHOL	--	--	--	--	--	--	--	--
Sample Constraint:	nonsummer weekdays		summer weekdays		weekend days		nonsummer weekdays	
R^2/s.e.e.	.6261/52.76		.5512/60.62		.6044/51.21		.1846/48.17	
no. of/ obs./ d.f.	925/882		362/320		734/689		925/882	

Table A-1 (Cont.)

Independent Variables:	Dependent Variable: (17) CARE2W		(18) CARE2W		(19) CARE2H: other care of family members, by husband		(20) CARE2H	
CONSTANT	24.3	(27.5)	-1.5	(11.8)	-.6	(7.9)	-2.5	(15.3)
COLLEGEW	13.9	(12.1)	3.8	(6.7)	4.8	(4.8)	-3.2	(6.7)
HSW	7.3	(8.1)	2.3	(5.3)	2.3	(3.8)	3.1	(4.9)
PREDWAGE	-2.0	(3.2)	.2	(1.5)	.9	(1.0)	1.9	(1.8)
AGEWIFE1	-23.2	(25.6)	-.2	(10.3)	-4.0	(6.8)	-1.2	(14.2)
AGEWIFE2	-12.1	(26.0)	7.6	(10.5)	-4.5	(7.0)	-2.2	(14.4)
AGEWIFE3	-17.3	(26.3)	3.8	(10.5)	.7	(6.8)	6.1	(14.6)
DISABLEW	-4.9	(25.1)	6.4	(14.6)	1.5	(8.9)	0.8	(13.9)
NKIDS10	28.2	(17.7)	31.0	(9.6)	29.8	(6.9)	10.7	(9.8)
NKIDS11	30.7	(19.7)	30.7	(9.6)	20.6	(6.7)	11.4	(11.0)
NKIDS12	37.1	(18.6)	20.5	(9.6)	25.1	(6.9)	-2.9	(10.3)
NKIDS13	13.4	(20.2)	20.8	(9.6)	12.8	(6.6)	5.6	(11.2)
NKIDS14	6.1	(19.2)	8.1	(9.5)	6.1	(6.9)	-9.3	(10.7)
NKIDS20	11.4	(11.8)	27.7	(6.7)	6.7	(4.8)	7.2	(6.5)
NKIDS21	59.7	(10.9)	14.3	(6.5)	9.4	(4.8)	18.6	(6.1)
NKIDS22	35.1	(7.7)	11.2	(4.1)	8.0	(2.9)	4.8	(4.3)
NKIDS23	15.2	(8.3)	11.7	(4.1)	8.0	(2.9)	6.5	(4.6)
NKIDS24	-0.7	(9.0)	4.8	(4.5)	2.5	(3.1)	-6.1	(5.0)
NKIDS30	27.9	(17.7)	-9.8	(10.5)	-2.0	(6.7)	-5.1	(9.8)
NKIDS31	42.2	(13.9)	20.2	(7.4)	-.6	(5.9)	0.3	(7.7)
NKIDS32	5.1	(7.0)	12.7	(3.8)	10.4	(2.8)	-2.0	(3.9)
NKIDS33	9.8	(4.9)	11.1	(2.7)	4.2	(2.0)	1.5	(2.7)
NKIDS34	5.4	(5.5)	.8	(2.9)	1.2	(2.1)	-2.2	(3.1)
NKIDS40	-8.9	(16.4)	4.2	(9.1)	23.5	(6.8)	25.8	(9.1)
NKIDS41	-2.2	(16.0)	-4.5	(8.3)	-9.0	(7.2)	9.8	(8.9)
NKIDS42	18.9	(5.6)	13.8	(3.3)	5.3	(2.4)	-2.0	(3.1)
NKIDS43	11.7	(4.5)	7.7	(2.3)	1.7	(1.6)	0.1	(2.5)
NKIDS44	4.0	(5.0)	5.6	(2.8)	2.2	(2.4)	-1.9	(2.8)
SUMMER	--	--	--	--	--	--	--	--
SUNHOL	--	--	--	--	--	--	--	--
Sample Constraint	summer weekdays		weekend days		nonsummer weekdays		summer weekdays	
R^2/s.e.e.	.0989/51.98		.0774/57.18		.0700/32.38		.0407/28.89	
no. of obs./d.f.	362/320		734/689		925/882		362/320	

Table A-1 (Cont.)

Independent Variables:	Dependent Variable: (21) CARE2H		(22) CARE2S: other care of family, by all persons		(23) CARE2S		(24) CARE2S	
CONSTANT	-16.5	(13.5)	-14.7	(19.3)	0.7	(56.3)	-19.0	(31.1)
COLLEGEW	4.0	(7.7)	18.7	(11.7)	16.8	(24.9)	26.4	(17.7)
HSW	2.4	(6.0)	10.6	(9.4)	17.8	(18.1)	11.2	(13.9)
PREDWAGE	3.3	(1.8)	2.9	(2.6)	2.4	(6.6)	2.4	(4.0)
AGEWIFE1	0.9	(11.8)	-4.2	(16.7)	-22.7	(52.5)	-1.6	(27.2)
AGEWIFE2	-.3	(12.1)	-5.3	(17.1)	-4.6	(53.3)	2.5	(27.8)
AGEWIFE3	4.1	(12.0)	7.9	(16.6)	-4.9	(53.8)	7.6	(27.6)
DISABLEW	-2.1	(16.7)	17.8	(21.9)	17.1	(51.4)	7.9	(38.4)
NKIDS10	29.8	(11.0)	96.2	(17.0)	54.2	(36.3)	119.5	(25.2)
NKIDS11	21.0	(11.0)	87.8	(16.5)	67.7	(40.4)	59.4	(25.3)
NKIDS12	8.4	(11.0)	85.4	(16.8)	93.8	(38.1)	46.1	(25.2)
NKIDS13	18.4	(11.0)	60.1	(16.3)	20.1	(41.4)	47.5	(25.2)
NKIDS14	1.1	(10.9)	25.1	(16.8)	-4.8	(39.4)	13.7	(25.1)
NKIDS20	19.0	(7.7)	48.4	(11.8)	36.0	(24.2)	75.4	(17.7)
NKIDS21	20.7	(7.4)	61.2	(11.8)	105.0	(22.4)	68.8	(17.0)
NKIDS22	12.7	(4.7)	38.5	(7.2)	63.7	(15.8)	29.4	(10.8)
NKIDS23	9.4	(4.7)	44.2	(7.1)	18.6	(17.1)	37.6	(10.8)
NKIDS24	7.0	(5.2)	14.4	(7.7)	-8.2	(18.5)	16.2	(11.9)
NKIDS30	7.2	(12.0)	36.0	(16.3)	-2.2	(36.2)	10.1	(27.6)
NKIDS31	35.9	(8.5)	32.6	(14.4)	26.3	(24.5)	80.5	(19.6)
NKIDS32	9.6	(4.4)	33.4	(6.9)	14.8	(14.4)	35.6	(10.1)
NKIDS33	4.9	(3.1)	30.4	(4.9)	29.6	(10.0)	28.1	(7.1)
NKIDS34	0.1	(3.3)	8.9	(5.1)	8.7	(11.3)	3.7	(7.7)
NKIDS40	16.6	(10.4)	56.7	(16.6)	74.7	(33.6)	47.5	(23.8)
NKIDS41	5.8	(9.6)	34.9	(17.5)	11.2	(32.8)	9.4	(21.8)
NKIDS42	5.7	(3.8)	30.8	(6.0)	34.7	(11.6)	29.4	(8.8)
NKIDS43	4.6	(2.6)	18.9	(3.9)	20.9	(9.2)	20.9	(6.1)
NKIDS44	4.5	(3.2)	10.9	(5.8)	0.8	(10.2)	13.8	(7.5)
SUMMER	--	--	--	--	--	--	-10.5	(9.0)
SUNHOL	--	--	--	--	--	--		
Sample Constraint	weekend days		nonsummer weekdays		summer weekdays		weekend days	
R^2adj/s.e.e.	.0967/45.54		.1566/79.31		.0833/106.62		.1602/104.70	
no. of/ obs./d.f.	734/689		925/882		362/320		734/689	

Table A-1 (Cont.)

Independent Variables:	Dependent Variable: (25)DRUDGE61: home chore time by children 6-11		(26) DRUDGE61		(27) DRUDGE61		(28)DRUDGE27: home chore time by children 12-17	
CONSTANT	-17.5	(20.8)	-24.5	(44.9)	4.0	(29.9)	-32.3	(29.2)
COLLEGEW	22.8	(18.3)	5.8	(34.0)	6.0	(24.8)	33.5	(25.5)
HSW	9.9	(13.9)	- 6.4	(21.5)	-17.1	(18.3)	23.9	(18.5)
PREDWAGE	- 2.9	(3.8)	2.1	(8.9)	0.5	(5.3)	- 0.2	(5.2)
KIDSO	11.7	(13.3)	39.7	(31.3)	41.2	(20.9)	30.9	(28.0)
KIDS1	11.6	(11.1)	- 3.4	(21.3)	12.8	(14.0)	17.9	(22.5)
KIDS2	- 3.3	(5.4)	- 4.7	(13.1)	- 1.1	(7.6)	- 7.1	(10.8)
KIDS3	37.6	(3.8)	50.7	(8.7)	38.4	(5.3)	8.3	(5.6)
KIDS4	7.2	(4.3)	6.1	(9.2)	12.5	(6.0)	51.9	(6.5)
SUMMER	---	---	---	---	-12.1	(11.9)	---	---
SUNHOL	---	---	---	---	-14.1	(12.2)	---	---
Sample Constraint:	at least 1 child, 6-11; nonsummer weekdays		at least 1 child,6-11; summer weekdays		at least 1 child, 6-11; weekend days		at least 1 child, 12-17; nonsummer weekdays	
R^2_{adj}/s.e.e.	.1867/79.56		.1664/102.44		.1411/95.87		.1978/95.83	
no. of abs./d.f.:	419/410		172/164		335/324		282/273	

Independent Variables:	Dependent Variable: (29)DRUDGE27		(30)DRUDGE27	
CONSTANT	-60.1	(62.5)	45.6	(57.9)
COLLEGEW	14.8	(49.4)	-72.1	(46.0)
HSW	-16.5	(27.6)	-73.1	(31.2)
PREDWAGE	15.1	(12.3)	7.8	(10.4)
KIDSO	110.7	(63.2)	111.9	(58.9)
KIDS1	- 6.1	(43.6)	40.6	(37.0)
KIDS2	21.3	(20.9)	-11.6	(19.6)
KIDS3	9.2	(11.3)	6.7	(10.5)
KIDS4	67.5	(12.0)	78.8	(12.1)
SUMMER	---	---	- 9.9	(23.3)
SUNHOL	---	---	-44.8	(24.2)
Sample contraint:	at least 1 child, 12-17; summer weekdays		at least 1 child, 12-17; weekend days	
R^2_{adj}/s.e.e.	.2136/121.43		.1791/159.81	
No. of obs./d.f.:	130/121		236/225	

Table A-1 (Cont.)

Means of Selected Variables for the Three Main Samples:

Variable:	Nonsummer weekday sample	Summer weekday sample	Weekend day sample
COLLEGEW	.187	.182	.183
HSW	.717	.678	.718
AGEWIFE1	.548	.611	.569
AGEWIFE2	.272	.227	.252
AGEWIFE3	.145	.149	.151
DISABLEW	.015	0.014	0.011
SUNHOL	.145	.138	.251
DRUDGEW	468.010	476.560	378.280
DRUDGEH	75.319	84.807	141.380
DRUDGE27	27.458	50.069	51.308
DRUDGE61	24.643	37.251	32.016
DRUDGEF	15.086	25.925	23.426
DRUDGEM	3.611	7.320	4.401
DRUDGES	614.120	681.930	630.810
MEALW	78.989	79.116	72.568
MEALH	3.978	6.671	8.740
DISHESW	45.059	42.459	46.124
DISHESH	2.389	1.519	2.882
HOUSEW	71.465	70.981	51.792
HOUSEH	1.111	.746	3.018
WASHW	31.719	35.649	23.849
IRONW	.784	.331	.552
CARE1W	58.168	56.561	50.484
CARE1H	5.460	5.428	7.970
CARE127	.741	.345	.313
CARE161	.443	.925	.456
CARE1F	.584	2.141	1.614
CARE1M	.000	.221	.000
CARE1S	65.395	65.622	60.838
CARE2W	48.697	36.188	27.234
CARE2H	15.827	11.340	23.072
CARE227	2.005	4.599	3.161
CARE261	2.151	3.232	2.227
CARE2F	8.2486	15.166	16.594
CARE2M	.843	2.320	2.057
CARE28	77.773	72.845	74.387
SUMMER	.000	1.000	.278
PREDWAGE	4.763	4.614	4.718
NUMKIDS	2.304	2.384	2.315
KIDS0	.166	.155	.155
KIDS1	.183	.199	.202
KIDS2	.554	.530	.544
KIDS3	.851	.865	.838
KIDS4	.550	.635	.576

Table A-1 (cont.)

	Means of Selected Variables for the Three Main Samples:		
Variable:	Nonsummer weekday sample	Summer weekday sample	Weekend day sample
NKIDS10	.032	.033	.033
NKIDS11	.036	.025	.033
NKIDS12	.034	.030	.033
NKIDS13	.036	.025	.033
NKIDS14	.032	.033	.033
NKIDS20	.060	.064	.060
NKIDS21	.069	.086	.076
NKIDS22	.158	.166	.158
NKIDS23	.174	.144	.163
NKIDS24	.128	.133	.128
NKIDS30	.028	.028	.023
NKIDS31	.042	.050	.050
NKIDS32	.149	.160	.155
NKIDS33	.234	.354	.266
NKIDS34	.179	.229	.192
NKIDS40	.032	.025	.030
NKIDS41	.024	.033	.034
NKIDS42	.146	.122	.142
NKIDS43	.230	.193	.207
NKIDS44	.104	.135	.127
NKIDS50	.001	.003	.003
NKIDS51	.003	.003	.003
NKIDS52	.013	.019	.011
NKIDS53	.057	.080	.076
NKIDS54	.034	.047	.037
NKIDS60	.009	.003	.005
NKIDS61	.004	.003	.003
NKIDS62	.020	.033	.022
NKIDS63	.043	.069	.040
NKIDS64	.020	.058	.020
NKIDS70	.004	.000	.001
NKIDS71	.004	.000	.003
NKIDS72	.034	.000	.023
NKIDS73	.077	.000	.053
NKIDS74	.053	.000	.040
Number of observations	925	362	734

(As noted at the start of this table, a coding error mismeasured the SUNHOL variable. About half of the Sunday observations were given SUNHOL=0 in the weekend sample, and a similar number of observations has SUNHOL=1 in the weekday samples, even though all observations in the weekend sample truly refer to weekend observations and no weekend observations were included in the weekday sample. The only effect of the coding error was to throw some suspicion on the SUNHOL coefficient and standard error for each model run on the weekend sample.)

samples had too few children under two years of age within families of five or more children. Rather than report coefficients for age-and-parity classes represented by only four or fewer positive observations, I dropped all of the larger-family coefficients from Table A-1.

Also omitted from the list of independent variables, even in the full regressions, were variables relating to the labor force participation of the wife. It might seem natural to include them, in order to be able to ask to what extent a working wife gives less time to each specific household chore than a non-working wife. The influence of the mother's work status on child care is taken up later in this appendix. For present purposes, however, this is an inappropriate specification. The issues of key importance here are the influences of the presence of children and the education of the couple on their time use in and out of the home. Children and the couple's education affect all their time use patterns simultaneously. To quantify their impact on, say, child care time, it is a lot simpler <u>not</u> to include the wife's work status as a competing variable, in order to avoid having to ask, after the regression, to what extent this work status itself is shifted by the presence of children, thereby further shifting the amount of child care time given by the wife. Similar reasoning led me to exclude the wife's preference ratings for different home tasks from the list of independent variables: her stated preferences, like her labor force participation, are simultaneously determined along with her home time use by the couple's background, and including the preference variables would only lead to the further task of asking how responsive these were to the couple's background.

Some of the patterns revealed by the regressions are not surprising. It turns out that virtually all of the housework and virtually all child care are done by the wife. Husbands, older children, and nonfamily females (primarily babysitters) do help out, especially on weekends and summer weekdays. Yet over half of any indoor task gets done by the wife. This pattern is consistent enough that most of the regressions for time inputs by the husband or hired females yielded such low R^2s, low average time inputs, and low significance of coefficients that they were not worth reporting here.

Some of the patterns regarding the home's less rewarding chores also square with intuition. Meal preparation, meal cleanup, and washing are performed mainly by the wife, and her burden in these areas is raised by pre-teenage children. Teenagers help out enough so that her total time on these chores is raised only 0.6 hours per week as a year-round average by the presence of each teenager. Teenagers in turn put in more home chore time than they seem to add to the mother's total chores--though one can wonder whether their rate of home production per hour matches hers. Regressions were also run for other chores--chauffering, ironing, and regular house care--but too many zeroes were recorded in these time use categories for interesting regression results to show up.

Several clear patterns in child care time emerge from Table A-1. The impact of a child on total care time is much greater for an infant than for an older child. A newborn requires tremendous physical care, a one-year-old requires large amounts of both physical and other (interactive) care, and the demands drop off for each older age group. A glance at the coefficients further suggests that the impact of a child of given age on total care time tends to be lower the more children there are. One of two children in the home seems to affect total care time less than does an only child, and one of three less than one of two. The pattern does not continue, however, from three-child families to four-child families. Each child in a four-child family seems to have the same or greater impact on total care time as each child of the same age in a three-child family. There is no obvious explanation for this result. The unreported coefficients for larger families do suggest that the net impact of a child of any age above one year (i.e., for any age group well represented in the large-family observations) is lower for five- and six-child families than for four-child families. The prevailing pattern appears, at a glance, to be one of declining impact of one child on total care time the more siblings he or she has. This pattern is examined more carefully below.

Class Differences in Child Care Time?

Past authors have suggested that a wife with more schooling and socio-economic status will tend to put more hours of time into the care of each child. This conclusion was reached, with varying degrees of qualification, by the U.S. Bureau of Human Nutrition and Home Economics (1944), Leibowitz (1972; 1974), Hill and Stafford (1974), Vanek (1973), and Szalai and others (1972). The issue is one that might raise class sentiments, and deserves a closer look.

The literature to date has advanced two related hypotheses about class differences in child care time. The first is that a higher-status wife tends to put more hours into child care per child. The second is that she tends to devote more time to each child than would a lower-status wife having the same number of children. The first hypothesis is correct. The second is incorrect.

The regressions in Table A-1 demonstrate that when one has held the number and ages of children constant, the education of the wife and the status (predicted wage rate) of the husband have a small and insignificant effect on total time put into care of family members. The signs of the coefficients for extra education and status are usually positive, as past authors implied, but the difference between the wife's having a college degree and her having dropped out of high school has less predicted impact on child care time per child than, say, the difference between having two and having three children for given parental characteristics. The unimpressiveness of the effects of the couple's education and status on child care time shows up consistently. It shows up for all times of the week and year. It shows up both for the wife's child care time and for child care time by all persons. It shows up when the current regressions are cross-checked by (unreported) regressions yielding predicted care time impacts of children in samples restricted to high- and then to low-education wives. The Cornell survey data clearly imply, then, that the tendency of higher-status wives to put in more child care time per child is primarily or even entirely due to the fact that they have fewer children on the average. Here, as elsewhere in this book, we encounter the conclusion that the mechanisms governing family size are prime determinants of the inputs that one generation gives to each member of the next.

This finding can be reconciled with the cited findings to the contrary by other authors. Some of the studies noting class differences in child care time were simply based on raw averages too crude for reliable conclusions about class differences. This is true of the studies by Guilbert and others (1967) and by the international team of Szalai and others (1972, pp. 249-263). Authors who took enough care to use regressions based on large samples often masked the present finding by their choices of variables. Professors Hill and Stafford, for example, used total household work time as a dependent variable, since the Michigan survey they used did not yield any breakdown between child care and other tasks. The apparent extra impact of a child of a more educated and higher-income couple might reflect not differences in time inputs into a child but a greater tendency of low-education wives to find time for the child at the expense of other home chores instead of leisure or paid work. This is especially true since the independent variables used by these authors fail to identify parity effects properly. By using the same variable for the impact of a child of given age whatever the number of his siblings, Hill and Stafford seem to have passed over the point that lower-status wives, having more children and less leisure on the average, might find the time for an extra child more at the expense of other household work, thereby showing a lower impact of the extra child on total household work even if the extra child received the same amount of care time as a higher-status child of the same parity.

Professor Leibowitz, using the better Cornell survey data, chose more appropriate dependent variables than Hill and Stafford had at their disposal. She divided the Syracuse sample into subsamples consisting of families with high-education wives and families with low-education wives. It turned out that the predicted values of care time were higher for the high-education group, though not always significantly so. This result hinged on the fact that the high-education sample had a greater intercept value (care time with no children), even though they had lower apparent impacts of each child on total care time than did the low-education group. In Leibowitz' formulation, as in that of Hill and Stafford, children are presented by the numbers of them within each age group, with no recognition of the fact that the impact of an extra child on each age group depends strongly on how many siblings are present. The likelihood that this simplification of the independent variables affected Leibowitz' conclusions is underlined by another property of the Syracuse sample. In that sample it turns out that the families having no children have less schooling on the average than the sample as a whole, while those having one child had higher than average schooling. This explains why the regressions for care time among higher-education families had higher intercepts and higher predicted values for small (and perhaps mean) family sizes: the intercepts and predicted values were bouyed up by the relative absence of childless couples and the greater representation of one-child couples. This twisting of the regression line is avoided by making all the presence-of-children variables specific to the total number in the family.

Another study purporting to show marked class differences in child care time is that of the U.S. Bureau of Human Nutrition and Home Economics (1944). That study found that in the late 1920s and early 1930s rural households spent quite a bit less time on child care for each age of the youngest child in the family than did urban alumnae of six prestigious eastern women's colleges. This result is hard to interpret, given the nature of the sample and the impossibility of inspecting the original data behind these averages. I strongly suspect that the differences do not relate at all to education, but only to rural-urban differences in the reporting of time used to survey takers. Rural wives may report the large amounts of time spent both supervising the children and working about the house and farm as primarily time spent in chores other than child care, while the urban wives record such multiple-use time mainly as child care. Or it might be that there are true rural-urban differences in the devoting of time and energy to child care. It is hard to tell, but it does appear that the differences observed in this study hinged more on the rural-urban split than on the split by educational class.

Though past studies have underemphasized the extent to which differences in child care time hinge on differences in fertility, the present results uphold other patterns asserted by others. It is still true that a wife with more schooling will tend to put more care into each child if numbers and ages of children are <u>not</u> held constant across classes. And the present regressions allow one to reject the hypothesis that children of more schooled parents receive markedly <u>less</u> time than children of less-schooled parents. In addition, nothing in Table A-1 denies (or confirms) the plausible argument that a more schooled wife is more productive in developing a child's achievement potential with each hour and each bundle of commodities she spends on him or her. That issue is hard to test, given the multicollinearity between her unobserved productivity and such influences as the amount of purchased inputs given to the child.

Working Mothers and Child Care

To determine how child care time is influenced by family composition and by the age and education of the parents, it has proved convenient to omit the mother's work status from the list of independent variables, as noted above. Yet it is reasonable to wonder how a mother's working for pay outside the home affects the amount of time spent on child care by herself and by others. The movement of mothers into the labor force has been one of the most conspicuous changes in work habits in this century, and the relative pay prospects for women remain bright enough to make it likely that the trend toward having careers and children at the same time will continue, even without any further exogenous shifts in attitudes.* This trend raises curiosity about the effects of mothers' careers on child development, a curiosity that is reinforced by the absence of evidence that husbands have begun to devote a greater share of their lives to child care and other home tasks than in the past.

The Cornell time use data cannot directly appraise the effects of mothers' careers on a child's later development. Indeed, few bodies of data are up to this task, since it requires having information on the mother's work history, other family attributes, and the child's own achievements or emotional history in much later years. Only if one settles for school grades or test scores as early indicators of child development can one expect to get all the necessary information on family history into one sample. The time use data can, however, give useful clues on the extent to which the time inputs into children of given numbers and ages are affected by the mother's labor force participation. If her working seems to reduce time spent by herself on child care by more than the extra time that others devote to the same set of children because she has a job, there is good reason to suspect that her work may be depriving the children of attention needed for development. If her work takes away from the children less of her time than the extra care time provided by others, the case for suspecting a net deprivation seems weakened. If this latter result holds, children might still suffer on balance from a mother's working outside the home, but to show it one would have to show that the greater number of others' hours plus the extra commodities bought with the mother's pay were not enough to offset the reduction in contact with the mother.

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*See Victor R. Fuchs, "Recent Trends and Long-Run Prospects for Female Earnings," American Economic Review 64 (1974): 236-42.

To estimate the effects of a mother's work status on child care time, I divided the observations of the Cornell survey into four subsamples:

(a) 421 weekday observations, families with working* wives and 0-4 children;

(b) 789 weekday observations, families with nonworking wives and 0-4 children;

(c) 254 weekend observations, families with working wives and 0-4 children; and

(d) 438 weekend observations, families with nonworking wives and 0-4 children.

For each subsample regressions were run on the amounts of physical care and of other care given to family members by the wife and by all persons. These regressions use the same independent variables as those used for Models (1)-(24) in Table A-1. The coefficients for the effects of various numbers and ages of children, converted into total annual hours, are shown in Table A-2. The implications of these coefficients for the inputs of time in children over their entire childhood is illustrated in Figure A-1, which follows the coefficients from Table A-2 over the childhood of two siblings born three years apart.

The estimates bring out several contrasts in the time devoted to child care in families differing by the mother's work status. First, the underlying averages show that working mothers have fewer children, and older children, than nonworking mothers, as one would expect. A prime mode of reconciling home time demands with a mother's job is thus the simple option of having fewer children. By having fewer, working mothers are able to provide the same amount of their own time and of others' time (and more commodities) for

*Wives were defined as working if they had worked for pay outside the home in the seven days preceding the date of the first interview.

Table A-2. Hours of Child-Care Time per Year Associated with Various Numbers and Ages of Children Present in the Home, Working versus Non-Working Mothers (Based on Data from the Cornell Time Use Survey, 1967-68)

A. On Weekdays, Families with Working Mothers

(Figures in parentheses are standard errors of regression coefficients)

		Physical Care by: Mother	Physical Care by: All Persons	Other Care by: Mother	Other Care by: All Persons	All Care by: Mother	All Care by: All Persons
For the only child present, aged:	<1:	a	a	a	a	a	a
	1:	a	a	a	a	a	a
	2-5:	139.4 (33.5)	170.3 (43.2)	132.4 (41.0)	404.5 (93.1)	271.8	574.8
	6-11:	43.4 (33.9)	58.4 (43.8)	88.6 (41.6)	231.8 (94.5)	132.0	290.2
	12-17:	-10.7 (35.7)	8.8 (46.0)	40.3 (43.7)	75.4 (99.3)	29.6	84.2
With 2 children present, for each child aged	<1:	516.5 (35.9)	508.3 (46.3)	68.7 (44.0)	96.4 (100.0)	585.2	604.7
	1:	248.8 (33.6)	371.2 (43.3)	177.6 (41.2)	409.8 (93.5)	426.4	781.0
	2-5:	89.9 (17.8)	94.3 (23.0)	95.0 (21.8)	263.6 (49.6)	184.9	357.9
	6-11:	30.7 (16.9)	59.7 (21.8)	105.6 (20.6)	194.4 (47.0)	136.3	254.1
	12-17:	8.9 (16.7)	26.4 (21.6)	10.0 (20.5)	4.1 (46.6)	18.9	30.5
With 3 children present, for each child aged	<1:	a	a	a	a	a	a
	1:	a	a	a	a	a	a
	2-5:	131.6 (19.2)	154.4 (24.7)	50.0 (23.5)	77.4 (53.4)	181.6	231.8
	6-11:	34.6 (11.2)	45.2 (14.5)	63.8 (13.8)	160.0 (31.3)	98.4	205.2
	12-17:	7.9 (10.8)	9.8 (14.0)	11.5 (13.2)	22.2 (30.0)	19.4	32.0
With 4 children present, for each child aged	<1:	a	a	a	a	a	a
	1:	a	a	a	a	a	a
	2-5:	21.5 (37.5)	64.3 (48.4)	158.8 (46.0)	170.3 (104.4)	180.3	234.6
	6-11:	19.5 (9.6)	20.9 (12.4)	53.9 (11.7)	69.1 (26.6)	73.4	90.0
	12-17:	15.8 (11.7)	23.3 (14.8)	29.3 (14.1)	29.7 (32.0)	45.1	53.0

Table A-2 (Cont.). Hours of Child-Care Time per Year Associated with Various Numbers and Ages of Children Present in the Home, Working versus Non-working Mothers (based on data from the Cornell Time Use Survey, 1967-68)

B. On Weekdays, Families with Non-working Mothers

(Figures in parentheses are standard errors of regression coefficients)

	Physical Care by: Mother	Physical Care by: All Persons	Other Care by: Mother	Other Care by: All Persons	All Care by: Mother	All Care by: All Persons
For the only child present, aged < 1:	597.0	677.2	247.0	387.0	844.0	1064.1
	(52.3)	(58.4)	(51.8)	(86.1)		
1:	356.0	389.9	240.1	382.0	596.0	771.9
	(55.0)	(61.3)	(54.4)	(87.2)		
2-5:	110.0	131.1	190.2	339.7	300.2	470.8
	(64.8)	(72.2)	(64.1)	(102.7)		
6-11:	26.6	19.2	110.7	182.0	137.3	201.2
	(64.0)	(71.4)	(63.4)	(101.5)		
12-17:	26.1	32.3	71.7	75.6	97.8	107.9
	(62.6)	(69.8)	(62.0)	(99.3)		
With 2 children present, for each child aged < 1:	615.6	669.2	99.7	222.4	715.3	891.6
	(35.5)	(39.6)	(35.2)	(56.3)		
1:	316.4	340.4	177.2	316.5	493.6	656.9
	(35.1)	(39.1)	(34.7)	(55.6)		
2-5:	114.2	127.8	103.7	173.6	217.9	301.4
	(24.6)	(27.4)	(24.3)	(39.0)		
6-11:	61.5	55.5	86.7	138.1	148.2	193.6
	(24.7)	(27.8)	(24.4)	(39.1)		
12-17:	49.0	51.1	40.5	53.8	89.5	104.9
	(27.9)	(31.1)	(27.7)	(44.3)		
With 3 children present, for each child aged < 1:	499.2	548.7	127.5	146.3	626.7	695.0
	(48.3)	(53.7)	(47.7)	(76.4)		
1:	280.8	289.0	132.2	122.1	412.4	411.0
	(39.2)	(43.7)	(38.8)	(62.1)		
2-5:	116.3	126.7	76.3	139.0	192.6	265.7
	(21.4)	(23.9)	(21.2)	(34.0)		
6-11:	8.3	12.5	73.8	115.7	82.1	128.2
	(16.3)	(18.1)	(16.1)	(25.8)		
12-17:	16.4	13.4	17.3	41.0	33.7	54.4
	(18.6)	(20.7)	(18.3)	(29.4)		
With 4 children present, for each child aged < 1:	446.5	500.0	43.2	251.7	489.7	751.7
	(42.7)	(47.6)	(42.3)	(67.7)		
1:	264.4	294.1	66.0	121.3	330.4	415.4
	(45.1)	(50.3)	(44.7)	(71.6)		
2-5:	105.4	122.8	91.1	133.5	196.5	256.3
	(16.4)	(18.3)	(16.3)	(26.1)		
6-11:	24.9	24.7	53.9	86.8	78.8	111.5
	(13.3)	(14.9)	(13.2)	(21.2)		
12-17:	13.3	15.7	12.4	21.0	25.7	36.7
	(19.1)	(21.3)	(19.0)	(30.3)		

Table A-2 (Cont.). Hours of Child-Care Time per Year Associated with Various Numbers and Ages of Children Present in the Home, Working versus Non-working Mothers (based on data from the Cornell Time Use Survey, 1967-68)

C. On Weekend Days, Families with Working Mothers

(Figures in parentheses are standard errors of regression coefficients)

	Physical Care by: Mother	Physical Care by: All Persons	Other Care by: Mother	Other Care by: All Persons	All Care by: Mother	All Care by: All Persons
For the only child present, aged < 1:	a	a	a	a	a	a
1:	a	a	a	a	a	a
2-5:	58.6 (16.6)	64.3 (23.4)	21.6 (18.7)	74.2 (51.1)	80.2	138.5
6-11:	13.8 (16.4)	21.0 (23.1)	24.9 (18.4)	64.6 (50.4)	38.7	85.6
12-17:	9.2 (18.0)	15.8 (25.3)	13.2 (20.2)	20.5 (55.3)	22.4	36.1
With 2 children present, for each child aged <1:	227.3 (16.6) b	251.2 (23.4) b	-3.8 (18.7) b	227.2 (51.1) b	225.1b	478.4b
1:	86.1 (15.7) c	148.0 (22.1) c	40.0 (17.6) c	225.3 (48.3)	126.1c	373.3c
2-5:	50.3 (8.9)	52.5 (12.4)	22.6 (9.9)	24.8 (27.2)	72.9	77.3
6-11:	10.4 (9.1)	27.9 (12.8)	15.0 (10.2)	101.7 (28.1)	25.4	129.6
12-17:	7.5 (8.5)	13.6 (12.0)	6.6 (9.6)	9.7 (26.2)	14.1	23.3
With 3 children present, for each child aged <1:	a	a	a	a	a	a
1:	a	a	a	a	a	a
2-5:	25.0 (9.3)	31.6 (13.0)	27.8 (10.4)	115.1 (28.5)	52.8	146.7
6-11:	12.9 (6.0)	15.4 (8.4)	6.0 (6.7)	16.9 (18.4)	18.9	32.3
12-17:	8.0 (5.9)	11.6 (8.3)	1.4 (6.6)	0.9 (18.2)	9.4	12.5
With 4 children present, for each child aged <1:	a	a	a	a	a	a
1:	a	a	a	a	a	a
2-5:	23.7 (15.8)	64.5 (22.2)	-14.3 (17.7)	67.6 (48.4)	9.4	132.1
6-11:	12.1 (4.9)	13.4 (6.9)	12.2 (5.5)	14.1 (15.0)	24.3	27.5
12-17:	1.4 (5.2)	3.9 (7.3)	3.6 (5.9)	21.4 (16.1)	5.0	29.2

Table A-2 (Cont.). Hours of Child-Care Time per Year Associated with Various Numbers and Ages of Children Present in the Home, Working versus Non-working Mothers (based on data from the Cornell Time Use Survey, 1967-68)

D. On Weekend Days, Families with Nonworking Mothers

(Figures in parentheses are standard errors of regression coefficients)

	Physical Care by:		Other Care by:		All Care by:	
	Mother	All Persons	Mother	All Persons	Mother	All Persons
For the only child present, aged <1:	236.2	271.1	64.8	249.8	301.0	520.9
	(26.6)	(29.1)	(21.7)	(53.4)		
1:	146.1	155.4	62.4	121.8	208.5	277.2
	(27.7)	(30.4)	(22.6)	(55.7)		
2-5:	54.5 [b]	73.5 [b]	66.4 [b]	74.6 [b]	120.9b	148.1b
	(33.6)	(36.8)	(27.4)	(67.5)		
6-11:	a	a	a	a	a	a
12-17:	3.6 [d]	2.5 [d]	8.5 [d]	7.9 [d]	12.1d	10.4d
	(30.6)	(33.5)	(24.9)	(61.4)		
With 2 children present, for each child aged <1:	210.4	234.9	68.4	110.8	278.8	345.7
	(18.8)	(20.6)	(15.3)	(37.7)		
1:	117.2	138.0	19.9	81.9	137.1	219.9
	(18.1)	(19.8)	(14.7)	(36.3)		
2-5:	54.6	71.1	20.8	61.4	75.4	132.5
	(12.6)	(13.8)	(10.3)	(25.4)		
6-11:	20.8	22.6	23.6	47.1	44.4	69.7
	(12.4)	(13.6)	(10.1)	(24.9)		
12-17:	6.4	5.5	3.5	27.8	9.9	33.3
	(14.6)	(16.0)	(11.9)	(29.4)		
With 3 children present, for each child aged <1:	224.1	290.9	19.9	30.0	244.0	323.9
	(27.7)	(30.3)	(22.6)	(55.7)		
1:	122.1 [d]	135.9 [d]	40.7 [d]	138.3 [d]	162.8d	274.2d
	(19.0)	(20.9)	(15.5)	(38.3)		
2-5:	38.4	53.9	21.0	47.8	59.4	101.7
	(11.1)	(12.1)	(9.0)	(22.3)		
6-11:	10.7	14.4	25.2	60.1	35.9	74.5
	(8.2)	(9.0)	(6.7)	(16.5)		
12-17:	0.1	2.8	2.5	7.5	2.6	10.3
	(9.1)	(9.9)	(7.4)	(18.2)		
With 4 children present, for each child aged <1:	166.6	173.6	11.8	107.6	178.4	281.2
	(21.5)	(23.6)	(17.6)	(43.3)		
1:	59.7	90.0	-13.1	-8.3	46.6	81.7
	(19.9)	(21.8)	(16.2)	(39.9)		
2-5:	48.1	58.0	29.2	55.9	77.3	113.9
	(8.7)	(9.5)	(7.1)	(17.5)		
6-11:	5.4	10.9	14.3	47.1	19.7	58.0
	(7.0)	(7.6)	(5.7)	(14.0)		
12-17:	1.8	-1.6	13.6	21.2	15.4	19.6
	(9.8)	(10.7)	(8.0)	(19.7)		

Table A-2 (Cont.). Hours of Child-Care Time per Year Associated with Various Numbers and Ages of Children Present in the Home, Working versus Non-working Mothers (based on data from the Cornell Time Use Survey, 1967-68)

E. Total Year-Round Child Care Time		Families with Working Mothers		Families with Non-Working Mothers	
		By Mother	By All Persons	By Mother	By All Persons
For the only child present, aged	<1:	a	a	1145.0	1585.0
	1:	a	a	804.6	1049.1
	2-5:	352.0	713.3	421.1	618.9
	6-11:	170.7	375.8	a	a
	12-17:	52.0	120.3	109.9	118.3
With 2 children present, for each child aged	<1:	810.3	1083.1	994.1	1237.3
	1:	552.8	1154.3	630.7	876.8
	2-5:	257.8	435.2	293.3	433.9
	6-11:	161.7	383.7	192.6	263.3
	12-17:	33.0	53.7	99.4	138.2
With 3 children present, for each child aged	<1:	a	a	870.4	1018.9
	1:	a	a	575.2	685.2
	2-5:	234.4	387.5	252.0	367.4
	6-11:	117.3	237.5	118.0	202.7
	12-17:	28.8	44.5	36.3	64.7
With 4 children present, for each child aged	<1:	a	a	668.1	1032.9
	1:	a	a	377.0	497.1
	2-5:	189.7	366.7	273.8	370.2
	6-11:	97.7	117.5	98.5	169.5
	12-17:	50.1	82.2	41.1	56.3

Notes:

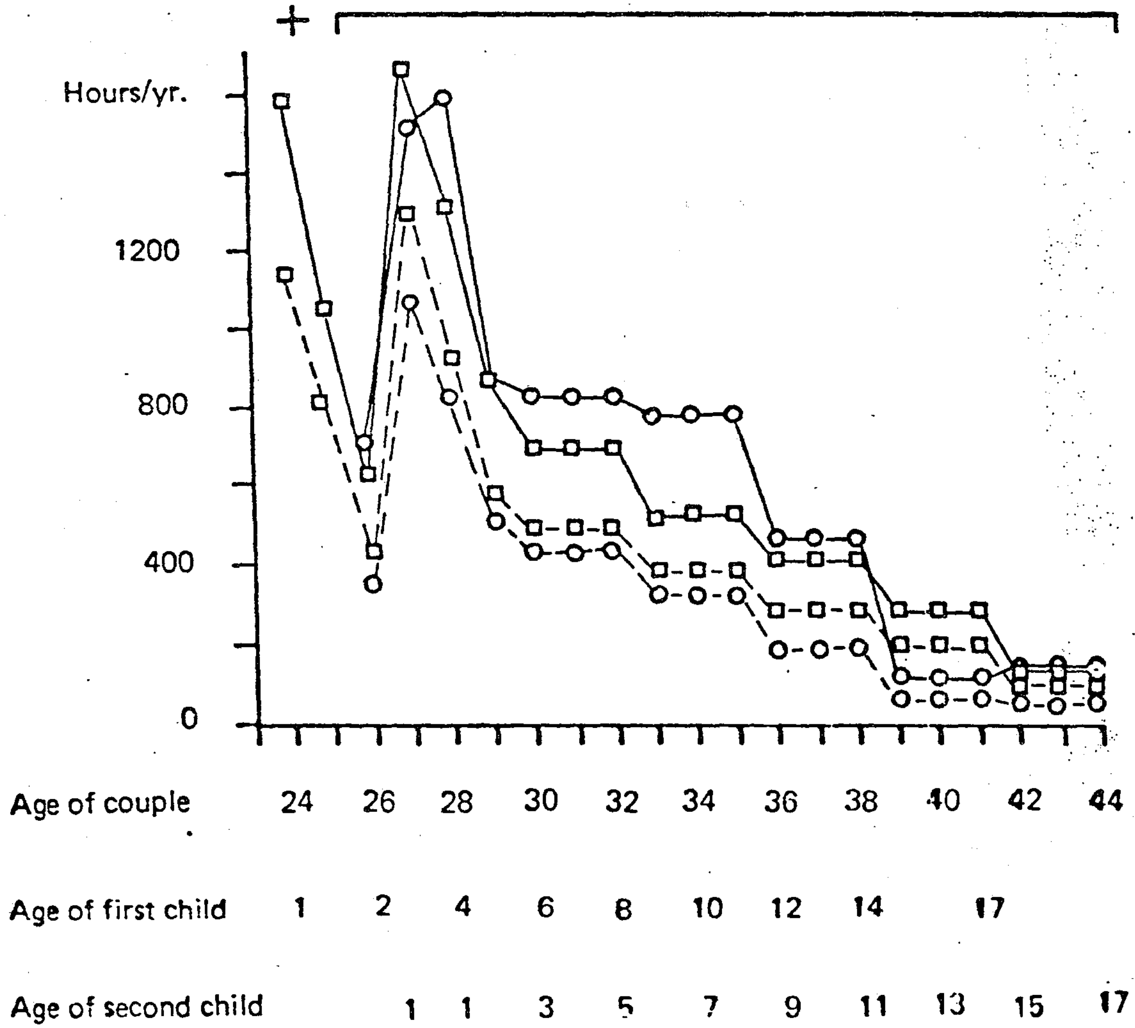
a: figures omitted, since the underlying regression coefficients were based on fewer than 10 children of the appropriate age, family size, and interview day of the week.
b: based on only 10 children in the appropriate category.
c: based on only 14 children in the appropriate category.
d: based on only 12 children in the appropriate category.

The figures are calculated from unreported regressions using the independent variables shown in Table A-1. Each weekday coefficient in those models was multiplied by 4.348 to convert from minutes per weekday to weekday hours per year. Each weekend coefficient was correspondingly multiplied by 1.739. Note that each coefficient is not the same thing as the child-care impact of that one child, as explained in the text of this appendix. Rather each is the contribution of one child that age to the total effect of having that many children (versus having none) in the home. (This distinction is unimportant, of course, for only children.)

Figure A-1 Child Care Time in Hypothetical Two-Child Families, Working versus Nonworking Mother (3-year child spacing)

	Total hours of child care, over the 19 yrs. in parentheses*
care time by all persons, mother works	11,446.6 (11.6 hrs./work.)
care time by all persons, mother does not work	10,528.6 (10.6 hrs./wk.)
care time by nonworking mothers	7,634.5 (7.7 hrs./wk.)
care time by working mothers	5,913.1 (6.0 hrs./wk.)

*(Figures for the first 2 years in the life of the first child could not be compared, since the sample for working mothers included almost no only children under 2 years in age.)



each individual child, as do non-working mothers. Another means of adjusting to the job is the tendency of working mothers to spend less time at nonchild home tasks, since the effect on child care time is much smaller than the average number of hours worked for pay (25.5 hours a week). Yet it remains true, even when the numbers and ages of children are held constant, that working mothers spend less of their own time on child care. The difference is on the border of statistical significance, though a mammoth sample would probably show clear significance.

The striking result in Table A-2 and Figure A-1, however, is that the reduction in the mother's child care time appears to be more than matched by an increase in the time spent by the husband and others caring for her children. In the two-child case illustrated in Figure A-1 it appears that each hour of contact the children lose with her is matched with about an hour and a half of care from others. This outcome can be interpreted in either of two ways. It could be the result of a reporting bias, as would occur if every single minute of babysitters' time were reported as child care while only part of the time spent by a nonworking mother at home with the children was reported as child care. Alternatively, it may actually be that working mothers spend more of their nonworking hours of the week to child care than nonworking mothers do, in an attempt to avoid shortchanging the children. Either interpretation, though, tends to leave little confirmation of the suspicion that children are deprived by a mother's work. If the reporting bias were responsible for the greater total care hours for children of working mothers, one must recognize that underlying this reporting bias is the fact that much of the time spent by the nonworking mother at home when her children are at home involves no more contact with them than a passive type of babysitting. If, on the other hand, working mothers actually concentrate more of their off-hour attention to their children, the result is the same: we find little hint of a net overall reduction in total child-care time, even when holding the numbers and ages of children constant. The hypothesis of deprivation from a mother's career is not rejected, but it must find evidence not implying a serious reduction in total adult attention.

Estimating Time Inputs Into Siblings

The comments made thus far about the relationship of child care time to numbers and ages of children could be made on the basis of casual inspection of the coefficients in Table A-1. Yet a proper quantification of the child care time received by individual children, and the time costs of individual children, requires a more careful processing of the regression estimates. The coefficients for minutes per day of time spent on different days of the week and seasons of the year first need to be aggregated to yield hours of time use per year. That involves multiplying each figure on minutes per nonsummer weekday by 3.261 [=(365.25/60)x(3/4 of the yr.)x(5/7 of the week)], each figure on minutes per summer weekday by 1.087, and each figure on minutes per weekend day by 1.739. Next, the spacing of births determines which coefficients are relevant for each year of a child's life in appraising his impact on time use. For example, the first of two siblings born two years apart is an only child for his first two years, while the other is an only child for his last two years in the home, assuming they both leave home at the same age. The coefficients for a two-child family should therefore be used only for the years in which they are both present in the home. Another complication to be introduced in processing the estimates of time inputs into children is that the regression coefficient for a given age group and number of children is not exactly the impact of a child that age on time use in a family achieving that number of children with his presence. Were he not present, that family would be smaller and a different set of child coefficients would be relevant. An example should help clarify this subtlety:

> The net impact on total care time caused in a given year by a three-year-old who is the second of two children is not just the coefficient NKIDS22, which is associated with a three-year-old in a family with two children present. If the other child is, say six years old, the net impact of the three-year-old is the difference between the predicted value of care time inputs for the family with this pair of children and the predicted value that would obtain if the six-year-old were an only child. This difference, the net impact of the younger child on care time, equals NKIDS22 + NKIDS23 - NKIDS13, which does not exactly equal NKIDS22. Correspondingly, the net impact of the older child on care time in the same family equals NKIDS22 + NKIDS23 - NKIDS12, which is not exactly equal to NKIDS23.

The impact of children on various birth orders on total child care time (physical care plus other care) was calculated for different spacing intervals in accordance with these guidelines. The estimates are given in Table 1 in the text above. An examination of the figures there confirms that a child tends to make a greater difference to total care time the fewer in number are his siblings. First-born and last-born children also appear to have greater impacts on total child care time than do middle children, because the first-born and last-born spend part of their childhood as the only child in the household. Again, as with the raw regression coefficients, there appears to be little difference between the care-time use impact of a third versus a fourth child.

Note that the figures in Table 1 refer to the estimated impact of the child on total care time during his presence in the home, and not to the time inputs he himself receives. The net impacts are thus low numbers where three or four children are present. In some cases, in fact, the values are negative for individual years (unreported here), implying that the presence of the extra child for such years lowers total care time. The negative values are counter to intuition. They stem from the inability of a less-than-mammoth sample of families to generate coefficients so finely tuned that they imply positive net impacts for all children in all spacing and parity combinations in all years. None of the negative predicted impacts, at any rate, are as negative as one standard error of estimate.

To convert these impacts on total care time into estimates of the inputs of care time recieved by an individual sibling, it is necessary to employ the assumptions made above about upper and lower bounds on the inputs of time into a child. The upper bound on the care time he receives is the time he would receive as an only child with similar parents. The lower bound is his impact in Table 1. The preferred estimate of the care time he receives is the average of these two extreme estimates. For each sibling position the average can be computed as the average of his column in Table 1 and the only-child column. The individual-year averages are omitted here. At the bottom of the table are presented the raw 18-year sums of the hours put into each child's care according to these mid-range estimates.

The estimates for total care time inputs into children in different family settings suggest that a child receives more care time by having fewer siblings, by having greater age gaps between siblings, and by being first or last. The ratios shown in Table 1 confirm that an only child receives much more time than a sibling. Either child in a two-child family also receives a good deal more than each child in larger families. Middle children receive less time than first or last children, especially in wider-spaced families, where the first and last each spend several years as the only child in the household. And the wider the average spacing, the more each child receives. These patterns are discussed at greater length in the text where they are compared with the apparent effects of sibling position on adult achievement.

The estimates in Table 1 imply that a last-born receives more time inputs than a first-born when the average spacing between siblings is three years or wider. This result stems from the ability of the oldest siblings to care for the youngest when the age gaps are wide. What needs to be clarified is how the extent of this care of younger siblings by older siblings was calculated and incorporated into the overall estimates of time inputs. The complicated estimation procedure described above was based on regression estimates of the effects of the presence of each child on the total child care time logged by <u>all persons</u> (in Regressions [13]-[15] and [22]-[24] of Table A-1). The use of the all-persons regressions was dictated by the necessity of cutting down on the number of calculations. The more logical procedure of working up separate estimates of the hours of care time by the wife, the husband, older siblings, and all others was just too time-consuming to be practical. The decision to use the all-persons regression results means that part of the net impact of older siblings on child care time by all persons is not really related to the care <u>of</u> him, but instead reflects care <u>by</u> him. It is thus necessary to subtract out estimates of the care of younger siblings by older siblings in order to get a more accurate picture of what time the older siblings are receiving over their childhood. This subtraction was done. Table A-3 reports my estimates of the total amounts of child care

Table A-3. Predicted Care of Younger Siblings by Older Siblings, Various Numbers and Spacing of Children: (hours of care over the 18 years spent in the household by the older sibling)

Child Spacing (Uniform)	Children ever born	Care of all Younger Siblings by 1st born	2nd born	3rd born	4th born
1-year	4	8.22	0	0	0
	5	n.c.	n.c.	n.c.	n.c.
2-year	3	16.44	0	0	--
	4	41.10	8.22	0	0
	5	271.89	24.66	8.22	0
3-year	2	8.22	0	--	--
	3	28.77	8.22	0	--
	4	323.62	28.77	8.22	0
	5	733.99	225.48	28.77	8.22
6-year	2	32.88	0	--	--
	3	533.17	32.88	0	--
	4	533.17	533.17	32.88	0
	5	533.17	533.17	533.17	32.88

Notes:

n.c. = not calculated, because the the Cornell time-use sample did not include families with such numbers and spacing of children.

The estimates are based on unreported regressions relating sibling care time to the numbers of siblings in each of five age groups and to other variables. It was found that the number of children in each older age group (6-11, 12-17) had no significant effect on the total time spent by such age groups in caring for family members. The only significant influences on care time by older children were the numbers of children in each of the preschool age brackets (<1, 1, 2-5). Thus for each of the later years of the older sibling's life in the household, I summed the appropriate pre-school coefficients to find how much sibling-care burden to assign to each older age group. The total burden for the year was then divided among the siblings in that age-range. (In the case of the 6-11 age range, I arbitrarily attributed all of the care of preschool siblings to those who were 8-11 years old.) Each sibling's care time was then summed over the years he lived in his parents' household (here ages 8-17).

supplied by older siblings. These estimates come from unreported regressions covering subsamples consisting of those families in the Cornell survey having at least one child in the relevant older age group (6-11 or 12-17). The figures in Table A-3 are sums of the amounts of care time supplied by each older sibling over the years from his eighth birthday to his eighteenth birthday.

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