# How Do People Decide to Allocate Transfers Among Family Members?

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## Abstract

Despite recent advances in data collection and the growing number of empirical studies that examine private intergenerational transfers, there still exist significant gaps in our knowledge. Who transfers what to whom, and why do they it? I argue that some of these gaps could be filled by departing from the standard parent-child framework and concentrating instead on fathers, mothers, sons and daughters in a way that accounts for fundamental—and sometimes obvious—male-female differences in concerns and objectives in family life. Elementary sex differences in reproductive biology constitute the basic building blocks of studies of family behavior in many disciplines, but despite recent progress they get far less attention than they deserve in economic studies of the family. I explore, separately, the implications of three basic biological facts for intergenerational transfer behavior. The first is paternity uncertainty: how does it affect the incentives of fathers, mothers and of various grandparents to invest in children? The second is differing reproductive prospects of sons versus daughters: when are sons a better investment than daughters and vice versa? The third is conflict: How much acrimony might we expect to occur in families, and why? In examining these issues I also explore household survey data from the United States. This preliminary evidence is consistent with non-biological as well as biological explanations of behavior. Nonetheless, the biological focus confers two advantages, by generating falsifiable predictions and by illuminating new avenues for empirical work. There is enormous potential for further micro-data-based empirical work in this area.

# Introduction

How does a parent decide how much to support a child?  Does it matter whether the parent in question is a father or mother?  Or whether the child is a son or daughter?  And how do we know it is the parent who is really doing the deciding?  Might it be that the child has already figured out how much he or she wants, and is now prepared for a lengthy campaign to get the parent to hand it over?

Perhaps surprisingly, the existing empirical literature on intergenerational transfer behavior contains few answers to these basic questions.  Most analyses, for example, are gender-blind, with generic parents and generic children, rather than mothers, fathers, sisters or brothers.  Models that contain husbands and wives usually do not feature anything special about being male or female—they might as well be persons 1 and 2.  Not that these limitations have necessarily impeded this fast-growing literature too much to date.  Advances in data collection and ever expanding empirical interest in the economics of the family have generated significant new knowledge about intergenerational transfer behavior.  Recent progress notwithstanding, I argue that expanding the domain of analysis to recognize separate behavior of mothers and fathers, and sons and daughters, could generate substantial new insight into how families function.

Such knowledge is of considerable policy interest.  The *existence, responsiveness* and *nature* of familial transfers each matter for public policies that redistribute income.  For example: if my grandmother qualifies for Medicaid benefits that pay for her stay in a nursing home, is she the true Medicaid beneficiary?  Or is it my mother, who in absence of Medicaid would have had to care for her?  And if such "crowding out" occurs—the substitution of government financed care for familial care—can it really be characterized as an inconsequential "neutralization"?  Or does it matter what goes on inside the family?  Is it better for elderly women to be cared for by their daughters or by non-relatives?  Would it matter whether my mother and her mother got along well or not?  What if my grandmother had only sons to rely upon rather than daughters?

The *existence* of intergenerational transfers complicates labeling public income redistribution programs "programs for the elderly," or "programs for kids." (What if my grandmother spends part of her social security check to help put my cousin through automotive repair school, for example?) The (as yet) *unknown nature* of intergenerational transfers implies that judgements about crowding out are equally tricky. If the public sector diminishes the role of the family, we need to know how well the family was functioning in the first place.

I argue that one way to improve our understanding of how families operate is to focus more attention on the separate interests and capabilities of male versus female family members by exploring three themes that are prominent in biology. The first is paternity uncertainty: How does it affect fathers' versus mothers' incentives to invest in children? The second is the reproductive and economic prospects of male versus female offspring: Do they create incentives for sex-biased parental investments? The third is parent-child conflict: How could it happen and how prevalent might it be? Along the way I provide crude empirical evidence related to each issue, but keep the investigation deliberately simple, to see if these problems are worthy of further, more discriminating empirical scrutiny. I conclude that they are.

Nothing in this paper should be construed as a claim for the veracity, or even the relevance, of biologically based models of behavior. The descriptive empirical evidence, for example, is consistent with both biological and non-biological explanations. Nonetheless, attention to reproductive biology has two considerable virtues: it generates falsifiable predictions and it guides the discovery of new directions for empirical work. It is important to try to falsify a strictly biologically based approach to assess its predictive power. I believe this is feasible and a worthwhile direction for future empirical research on the economics of the family.

## A.  Men  and  Women.

Over a century ago William James, considered by many to be the father of modern psychology, penned the following tacky rhyme:

> Higamous, hogamous, woman's monogamous;
> Hogamous, higamous, men are polygamous.

James' whimsical assertion is hardly true for everyone, everywhere, but he was nonetheless onto something.  Even if a man and woman have the same objective—say, to produce progeny in sufficient numbers and quality to continue the family line—they differ vastly in their capacity to achieve it.  In her whole life, a typical woman produces only about 400 viable eggs.  In just one day, a typical man produces enough sperm to populate a country the size of Japan.  The fecundity of 18th century Britain's Duchess of Leinster, who had her 21st child at age 46, pales in comparison to17th century Morocco's Moulay Ismail the Bloodthirsty, who sired 888 by his many wives and concubines.  These oddities point up the obvious:  a motivated, resourceful man can literally "go forth and multiply," but a woman can only go forth and add.

Of course, this is just one of many basic sex differences in reproductive biology. Here are three more: Except in the strangest of cases, a woman is always 100 percent certain—and a man never 100 percent certain—that a newborn child is a biological relative; a woman's expected reproductive lifespan is only half that of a man's; and, a fertile man who has secured a mate is a mere spasm's worth of effort away from the prospect of getting his genes into the next generation, while a woman who has just conceived still faces a marathon of exhaustion, sickness, discomfort, pain, and risks to physical and emotional health.

These elementary facts figure prominently in biological studies of family behavior. They also play an important role in analyses of family and kin behavior undertaken by many anthropologists, ethologists, psychologists, ecologists, demographers, and primatologists.  But they are for the most part glaringly absent from most economic studies

of family behavior. Economic models of intergenerational transfers are for the most part unisex models.

Not that they haven't produced substantial insight. Gary Becker's analysis of altruistic parental transfers spawned a whole new sub-discipline in economics, and the family bargaining models pioneered by Marjorie McElroy and others broadened the scope of that sub-discipline. For many of the questions posed in this early work, the recognition of biological differences would have been little more than a distracting nuisance. Despite these early advances, however, and despite the efforts of a small cadre of economists who have adopted and promoted a biological focus, the empirical state of the art of family economics suffers from its chronic inattention to reproductive fundamentals.

In recent years the landscape has begun to change, and increasing numbers of economic analyses have centered on these fundamentals. Theodore Bergstrom has written several papers that use and extend ideas from evolutionary biology and combine them with economic reasoning to study family behavior. An analysis of a longstanding biological theory of preferences of sons versus daughters—the so-called Trivers-Willard conjecture, which I will discuss in this paper—recently appeared in the <u>Journal of Political Economy</u> (Lena Edlund, 1999). Aloysius Siow (1998) has explored the implications of male-female differences in reproductive lifespan for gender roles in the labor market. Robert Willis' (1999) theory of out-of-wedlock childbearing recognizes the distinct problems and incentives faced by men versus women. But there is much to be done, particularly with respect to taking biologically based models to the data. In the sections that follow I concentrate on prominent biological themes, starting with one that is pertinent to fathers and mothers.

## B.  Fathers and Mothers.

Except for having to worry about rare mishaps like babies getting switched in the maternity ward, a woman is always certain that her newborn is a biological relative. But a man

seeking the same certainty would have to do more than watch the maternity ward; he'd have to guard his mate round the clock daily.[1]  In some cultures, past and present, husbands have attempted just that.  But barring extreme "mate guarding"—usually draconian practices ranging from intrusive to downright barbaric—a man would have to trust completely his mate's fidelity or else harbor some flicker of concern, however small, that his putative child is not really a biological relative.  What are the implications of such uncertainty for familial transfers?

Certainty of paternity is a major theme in the biological literature on family behavior and in some branches of anthropology and psychology, but only a minor one in the economics of the family.  Even Gary Becker's monumental *Treatise on the Family,* which helped spur the importation of biological concepts into studies of family economics, makes little mention of it.[2]  Similarly, Jack Hirshleifer's (1977) comprehensive and prescient discussion of the synergies between economics and biology makes no reference to paternity certainty.

But references are beginning to crop up in more recent economic research. Theodore Bergstrom, one of a handful of economists keen on incorporating biological concepts into economic analysis, analyzed uncertain fathers' investment in children in his wide-ranging, inter-disciplinary review of the economics of the family.[3]  Laura Argys and Elizabeth Peters (2001) explore empirically the relationship between the establishment of paternity and fathers' involvement with, and transfers to, children.  But I know of no other empirical economic studies dealing with the implications of paternity certainty on family behavior.  Evidence from other disciplines has frequently identified paternity certainty as a

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[1] Or get a DNA-based paternity test, something I discuss later.
[2] Becker's (1991) discusses briefly the claustration of women in the context of the division of labor within the family, and notes that "Female adultery is a serious offense in traditional societies, mainly because men are reluctant to rear children fathered by others."  But there is not much further reference to the problem of certainty of paternity in his *Treatise*, and no analysis of its implications.  Becker's earlier (1976) article on economics and sociobiology, which has an explicit focus on basic biological concepts such as kin selection, nonetheless makes no mention of paternity certainty.

prominent force; its connection with family transfer behavior is potentially of great importance for the economics of the family.

Below I explore the basics of female fidelity and its opposite, cuckoldry, i.e., raising unrelated children thought to be one's own.[4]  What is the evidence on paternity and cuckoldry?  What is the evidence on concerns about cuckoldry?  How large would the uncertainty have to be to generate significant behavioral effects?  What are the implications for parenting?  For grandparenting?

I argue that paternity uncertainty merits much more attention, and that thinking about its implications can generate innovative findings and questions for research in economics.  I present evidence of significant differences in maternal versus paternal grandparenting, which suggest that paternity uncertainty might matter for the economics of familial transfers.  Though these are as yet only preliminary findings, which are also consistent with alternative, more straightforward, explanations, they do indicate the value of further theorizing and more exacting empirical work on this issue.  Later I discuss possible future directions for research on paternity.

*A Case Study with Possible Clues: Grandparenting and Public Pensions in South Africa*

A recent paper by Esther Duflo (2000) shows that not all grandparents are equally generous to their grandchildren.  In fact, only one grandparent, the maternal grandmother, appears responsive to incentives to support them.  Duflo's study was not concerned with paternity certainty *per se*, but her results nonetheless shed potentially useful light on this issue.

Duflo studied familial transfers that occurred in an unusual natural experiment. Pressure for racial parity in South Africa's public pensions resulted in huge cash transfers to the country's poor elderly, who by early 1993 were receiving from the government more

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[3] See Bergstrom (1996, pp. 1923-24) for an analysis of a male's decision to support his wife's versus his sister's children as a function of paternity certainty for the Nayars of India, a polyandrous matriarchal society.

[4] This definition differs from what can be found in most dictionaries.  Random House/Webster's College dictionary defines a cuckoldry as being "the husband of an unfaithful wife," a necessary but not sufficient,

money than they had ever dreamed of—twice the median rural per-capita income (Anne Case and Angus Deaton (1998)). Duflo investigated the impact of the expanded pension on young children, motivated by the fact that Black South African elderly often live with their grandchildren in multi-generation or "skip-generation" households where absent parents work elsewhere (Case and Deaton (1998)). She reasoned that these pensioners, many of whom were bringing in more money than anyone else in the household, might be spending some of it on better nourishment for their young grandchildren. Though Duflo's interest was in differences between grandmothers and grandfathers, she also considered whether the grandparent was maternal or paternal.

Only one grandparent's pension eligibility had a statistically significant impact on child well being—that of the maternal grandmother. Her eligibility for a pension was associated with significantly better outcomes for girls' weight-for-height. The maternal grandmother is unique not only economically but also biologically—she is the only grandparent who can be absolutely certain that the grandchild is hers.[5]

Maybe we shouldn't get carried away with this interpretation—after all, how prevalent is cuckoldry likely to be? Aren't the chances trivially small? In the next section I argue that cuckoldry's possible behavioral influences should not be dismissed. It is not certain that cuckoldry is necessarily always rare; in fact, there is hardly any reliable evidence one way or the other. Further, there is some evidence that the *perceived* probability of cuckoldry—which might matter more than the *true* probability—is conceivably quite high. Further, I show that even small probabilities of cuckoldry might generate large behavioral effects.

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condition for cuckoldry as defined here. Instead, I adhere to the definition used, for example, by Daly and Wilson (1992), where cuckoldry refers to the misattribution of parenthood.

[5] Another pattern found by Duflo, which is not directly relevant to paternity but nonetheless important from another biological perspective, is the maternal grandmothers' apparent favoritism toward granddaughters. Only girls benefit from grandmotherly largesse. This is consistent with so-called "Trivers-Willard" effects, which I discuss in the next section.

*Mis-assigned Paternity: X the Unknown*

How many children are sired by someone other than their putative father?  The short answer is that we have no idea.  Some estimates are rather high but difficult to believe.  It could well be that the average probability of cuckoldry in the United States is quite low, though certainly not zero.

While concrete evidence is scarce, there is no shortage of myth, misinformation and speculation about paternity uncertainty.  Further, since beliefs and expectations are presumably what guide behavior, even "urban legends" about paternity could prove relevant.  Both fact and fiction merit scrutiny.

Physiologist Jared Diamond created a minor stir by claiming, in his book The Third Chimpanzee (precursor to his Pulitzer prize winning Guns, Germs and Steel), that "…between about 5 and 30 percent of American and British babies [have been] adulterously conceived"  (p. 86).  Diamond cites an unpublished study by "Dr. X," a "distinguished medical scientist" demanding anonymity, who in the 1940's accidentally uncovered evidence of widespread cuckoldry in the course of investigating heritability of human blood groups.  Diamond reasoned that this early study probably accessed only a few of the many blood-typing procedures now available, so that blood group discrepancies (such as a type-O wife giving birth to a type-A child but having a type-O husband) represented the tip of the cuckoldry iceberg.[6]  Diamond states that Dr. X's results "…were later confirmed by several similar genetic studies whose results did get published" but

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[6] Before the advent of DNA testing, serology was the primary forensic tool for investigating paternity. This method could only reject paternity, and only in cases where offspring characteristics were impossible given characteristics of the potential father.  If, as Diamond argues, only a limited number of blood characteristics were investigated by "Dr. X," such rejections reveal only a subset of the progeny of adulterous unions.  Just to illustrate, suppose that there is a single trait with two phenotypes, $P$ and $p$, determined by two equally prevalent genotypes, dominant $G$ and recessive $g$.  With random mating, only about a quarter of all illegitimate children could be identified by phenotypic discrepancies.  About half would escape detection because the cuckolder and the putative father would share the same genotype.  In half of the remaining cases the mother would carry the dominant gene, making it impossible to reject anyone's paternity.  Only in cases in which (1) the mother carries the recessive gene and (2) the genotypes of the putative father and the cuckolder differ can paternity be excluded.  With equal prevalence of $G$ and $g$ and random mating, this probability is the product of two independent 50-50 events.  Of course, the logic of

unfortunately does not provide any references to them.  A few years later biologists Robin

Baker and Mark Bellis, in their controversial (1995) study of the implications of female

infidelity, made similar claims about the incidence of mis-attributed paternity, this time

summarizing published serology studies.

I tracked down one such blood study, conducted on a sample of parents and

Another biologist, Tim Birkhead, cautions against reading too much into the limited

evidence on paternity uncertainty.  Any unpublished study must be taken with a grain of

salt since it would not subject to routine scientific scrutiny, and published studies are all

based upon serology rather than DNA testing, making results difficult to interpret

(Birkhead (2000), p. 82).

I tracked down one such blood study, conducted on a sample of parents and

children from a small Michigan town and published thirty years ago, and it illustrates some

of the problems Birkhead refers to.[7]  A team of three geneticists and an epidemiologist used

blood samples collected from 9,000 families to identify possible departures from Mendelian

inheritance patterns for 11 different blood groupings.  After dropping observations with no

parental observations, they sought to identify problematic observations, including errors in

data entry, unrecorded adoptions, and the like.  The researchers went back to the

individuals in families with discrepancies and re-collected and re-typed one or more blood

groupings.  These screens left 109 out of 2,507 (4.3 percent) with remaining

discrepancies, observations that were dropped from subsequent analyses because blood

samples were not consistent with parent-child relatedness.  Further, though the use of 11

blood groupings generates a higher incidence of exclusion than the more limited studies

referred to by Diamond, the probability of exclusion is still less than 100 percent.[8]

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paternity rejection and the evidence are distinct issues, and the numerical assumptions in this example are just used to illustrate the inference problems from serology.

[7] C.F. Sing, et. al., (1971) "Studies on Genetic Selection in a Completely Ascertained Caucasian Population.  II.  Family Analyses of 11 Blood Group Systems."  American Journal of Human Genetics, 23 (2):  pp. 164-198.

[8] Information from University of Dundee Department of Forensic Medicine, http://www.dundee.ac.uk/forensicmedicine/llbtesting.htm.

Does this imply a minimum 4.3 percent cuckoldry rate? Not necessarily. The researchers found that first-born children were over-represented among discrepant children, suggesting that many of them might have been stepchildren mistakenly reported as biological children. Despite care with which the authors treated discrepancies, ascertaining the cuckoldry rate proved elusive. Further, it was not a primary aim of the study, and the authors did not pursue it further.

Despite the inferential advantages now available from DNA testing, any direct attempt to study paternity certainty would be fraught with formidable human-subjects concerns and sample-selection bias. It would be obviously unethical, for example, to publish findings about discrepancies in relatedness obtained from DNA studies conducted prior to, say, organ transplants between supposed kin. And disease inheritance studies based on molecular methods to establish relatedness are based on self-selected samples. In Birkhead's words, "On being told that the information they provide might reveal true paternity, many would-be volunteers melt away. Those remaining are hardly a random sample and hence provide no basis for an estimate." (Birkhead, p. 82.)

An alternative source of evidence is animal studies, which are free from human subjects and sample selection problems. Recent studies point to widespread female infidelity even among species once thought to be almost exclusively monogamous. Even prior to the advent of DNA testing, biologists were beginning to overturn conventional wisdom concerning parentage in avian species thought to be paragons of monogamy. For example, the indigo bunting, a species of bird once thought to be almost exclusively monogamous, was found to have an estimated cuckoldry rate of at least 35 percent. Findings like these were uncovered time and again for one species after another, once DNA fingerprinting techniques were discovered (Birkhead, pp. 37-38.)

One might think it strange to refer to animal studies in the context of economic analysis of families. After all, what does the parentage of indigo bunting or baboons have to do with our behavior? Economists should be careful not to dismiss such findings out of

hand or to treat them as fanciful curiosities. In addition to being largely exempt from ethical concerns and selection bias, their culture-free settings create better laboratories for learning about biologically based behavior. (I have yet to uncover any concerns being voiced about moral decay or crumbling family values in indigo bunting.) Further, evolutionary models imply that all living things face the same basic problems of surviving and reproducing. Other disciplines, such as anthropology and psychology, routinely sift through cross-species evidence when studying family behavior.[9]

Why might cuckoldry occur? Another way to phrase the question is "Why would a woman seek sex with more than one man?" If biology is at the root of infidelity, isn't just the male the one who stands to gain, in terms of extra progeny, from stepping out on his mate? This argument fails once it is recognized that people care about child quality in addition to quantity. Quality/quantity tradeoffs, first emphasized in economics by Gary Becker, create a conflict of interest between men and women, even if both sexes value these attributes identically. The price of child quantity relative to quality differs dramatically between men women. A man can pursue extra-marital liaisons to raise the quantity of his progeny, whereas a woman obviously cannot (except in cases of male fertility problems). But she can pursue such liaisons to raise the *quality* of her progeny. And, if some of these high-quality progeny happen to be sons, they may afford her an opportunity to "go forth and multiply" vicariously.

A woman's concerns about child quality is a central theme in anthropological, biological and psychological theories about female infidelity. The theories most interesting from an economic perspective are those concerned with material resources—anthropological models of how women provision their children. Donald Symons (1979) argued that a woman might engage in short-term sexual liaisons in order to secure resources for her existing or

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[9] Becker (1991) argues that some of the uncanny similarities in much of the behavior of non-human and human families are not necessarily manifestations of "biological determinism," but could instead result from the ubiquity of economic problems faced by non-humans and humans alike.

future progeny. Sarah Hrdy (1981) reasoned that a woman might seek to create confusion about paternity in order to secure resources from more than one man.[10]

Arguments like these point to female infidelity as the quintessential "inferior good." In Symons' framework, women are driven to risk violence and sexually transmitted diseases working as *de facto* or actual prostitutes.  In Hrdy's framework, two putative fathers might be preferred to a single certain one because men are too poor to invest enough by themselves.

Evidence supporting Hrdy's argument comes from patterns in culturally sanctioned, rather than surreptitious, multiple husbands.  This rare practice, polyandry, almost always occurs in environments so rough that households with only one man have difficulty in making a go of it.  For instance, among poor Yak herders of Tibet a man might permit his brother to share his wife if having the extra worker insures the viability of the household.[11]

*How Much Could Paternity Uncertainty Possibly Matter?*

Applying strictly biological considerations to the problem of paternity uncertainty and familial transfers is bound to be misleading.  Economic considerations are essential too. For example, consider the most important insight into evolutionary biology since Darwin's time—the kin selection model of William Hamilton (1964).  Imagine (since no such thing has ever been found) a "helping gene," that is, a portion of the genome governing altruistic behavior.  Hamilton argued that altruistic behavior between kin was determined by the following implicit calculation "I'll help someone else if expected benefits in terms of 'inclusive fitness' exceed expected costs in terms of inclusive fitness."  "Fitness," in turn, is usually defined as the expected number of progeny, and "inclusive fitness" is my own

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[10] Other explanations for female infidelity are similarly founded upon motives to enhance child quality.  For example, there is "sexy sons" hypothesis, attributed to statistician R. A. Fisher (1958).  Like Flaubert's Madame Bovary, a woman consorts with handsome but irresponsible "Fast Freddie" while married to wealthy but unexciting "Steady Eddie."  With Freddie's chiseled features and Eddie's money, her son inherits the wherewithal to perpetuate the family line by producing several high quality grandchildren.

[11] Besides poverty, another hallmark of polyandrous unions is the tendency for husbands to be related (see also Becker (1991)).  This behavior likely reflects paternity concerns.  If I can't be sure that my child shares half my genes, then at least I can be assured that he shares at least a quarter of them.

fitness plus a weighted sum my relatives' fitness. The weights, in turn, are the "coefficients of relatedness," defined as the probability (over and above pure chance) that I and my beneficiary share the same helping gene. For example, because of the genetic shuffling from sexual reproduction, there is a 50-50 chance that my daughter inherited my helping gene, so my coefficient of relatedness to her would be 0.5.

In a nutshell, Hamilton's rule says to provide help if $c < rb$, where $c$ denotes fitness costs, $b$ denotes fitness benefits, and $r$ is the coefficient of relatedness. Certainty of paternity figures into this rule in a straightforward way. Suppose I'm only 95 percent sure that I'm really the father of my daughter. Then my coefficient of relatedness would be only 0.475, compared to my wife's 0.5. When my daughter wants to go to the zoo, or to college, my wife will ask, "Is $c < 0.5b$?" while I will ask, "Is $c < 0.475b$?"

Surely things aren't as bad as this cold-blooded Hamiltonianism suggests? Actually they are a good deal worse. If maternal devotion is the gold standard of altruism, paternal devotion garners far less metallurgical merit. Consider the Aka pygmies of central Africa, touted as the most devoted fathers on earth, as measured by the time they spend holding infants (Hewlitt (1991), Buss (1999), pp. 212-213). But Aka fathers' daily average of 57 minutes pales compared to Aka mothers' 490 minutes. Surely there is more to the story than just paternity certainty and Hamilton's rule.

Of course, there is much more. Heading the list of determinants of relative parental investment is the phenomenon of husband-wife division of labor. Decades ago Gary Becker pointed out that specialization can magnify small differences in underlying preferences and/or technology, resulting in large differences in behavior. Sex differences in the certainty of paternity could figure in just as other biological differences that Becker (1991) lists in his *Treatise*.

In addition to the division of labor, however, there are other economic considerations that can be appended to Hamilton's rule. For example, paternity uncertainty creates a public goods problem. The flip side of paternal doubt is the possibility that there

is another father out there somewhere, and perhaps a devoted one at that. Such prospects create externalities that, at least in principle, further dilute incentives for male investment.

Lastly, there is the consideration of limited time budgets. Departures from strict monogamy create obvious opportunities and tradeoffs that divert a philanderer's attention from his children. A husband who arrives home too late to take his kids to the ballgame because a dinner with his attractive new systems analyst went on too long is implicitly sacrificing quality of existing offspring for quantity of future offspring.[12]

*Grandparenting and Paternity Certainty*

Separating the effects of paternity uncertainty from these distinct though related crosscurrents is a potentially daunting task. One way to help control for (though not completely eliminate) the effects of sex-related division of labor in provisioning to children is to back up a generation and focus on grandparenting. Contrasting maternal versus paternal grand*mothers*, for example, holds constant the sex of the provider while allowing things like relatedness to vary.

I investigated empirically a variety of measures of grandparenting, focusing mainly on maternal/paternal differences among grandmothers. Evidence from three household surveys—the Health and Retirement Study (HRS), the National Survey of Families and Households (NSFH), and the Panel Study of Income Dynamics (PSID)—points to large differences in behavior by maternal/paternal status. Most of these differences are consistent with the qualitative predictions of Hamilton's rule. But they are also consistent with more prosaic considerations—much is left uncontrolled for. Many of these problems can and will be remedied in future work. For now, the results indicate little more than a green light for further study. And, while one might hesitate to ever accept the idea that concerns about paternity are really a driving force behind intergenerational transfers in the United States, the model has two virtues: it makes predictions that can be empirically falsified, and it illuminates new directions for empirical work.
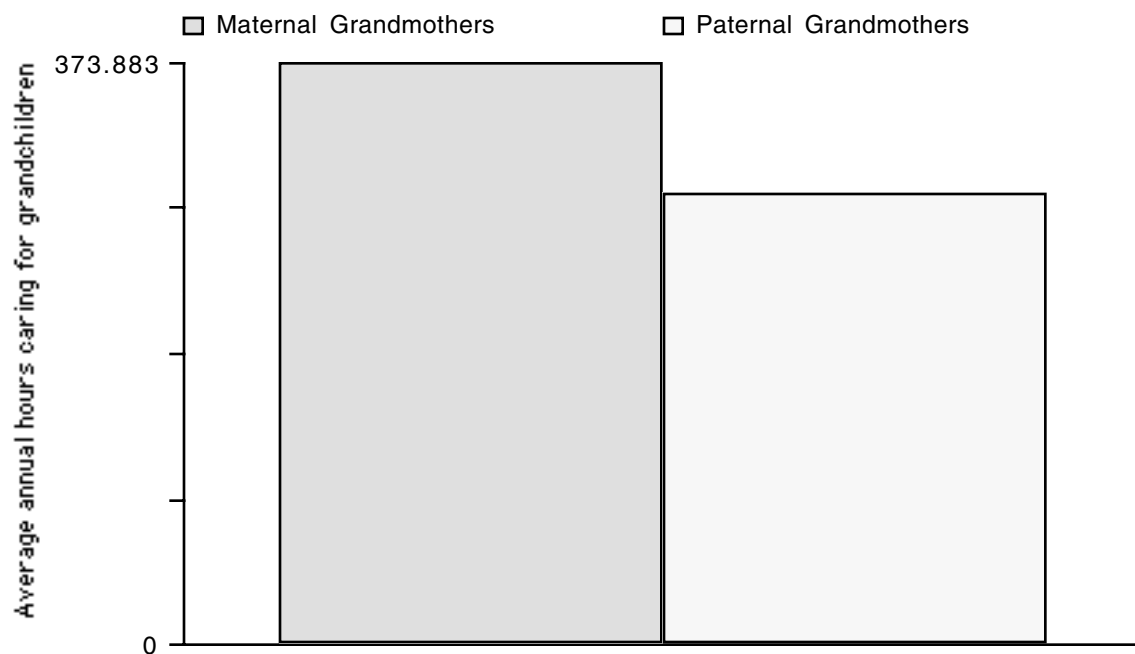
The first finding, from the HRS, is that grandmothers who are exclusively maternal (i.e., have grandchildren only by their daughters) provide more hours of childcare than grandmothers who are exclusively paternal (have grandchildren only by their sons). To avoid picking up the effects of grandmothers helping out daughters who are single mothers, I looked only at grandmothers with married children. In the first wave of the HRS, respondents were asked if during the preceding 12 months they spent 100 hours or more caring for their grandchildren. Those responding "yes" were then asked to give the amount of time, which was coded as the annual number of hours. Exclusively maternal grandmothers provided 374 hours compared to 290 hours provided by exclusively paternal grandmothers, a difference of 29 percent (Figure B-1).

This finding proves little about paternity certainty *per se*, and there are far more commonsensical explanations for it. Suppose my wife and I need a babysitter for my daughter, but we divide our labor in such a way that finding one is "her department." Though both our mothers might be available, she feels more comfortable imposing on hers. Indirectly, then, the maternal/paternal differences in grandmother care come from how my wife and I divide our labor.

Maternal/paternal differences, however, extend to other forms of grandmother/grandchild relationships besides hours of care. The second wave of the NSFH included a special module that gathered information about contact and affection between grandparents and grandchildren. As with the HRS, I concentrated on grandmothers who were exclusively maternal or exclusively paternal and whose own children were not single parents. The NSFH grandparenting module measured grandparent-grandchild contact (visits separate from letters-plus-phone calls) and the number of times in the past year a grandchild spent the night at her house ("sleepovers"). Each measure indicates more contact with maternal grandmothers than paternal ones. (See the first three pairs of

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[12] See Robert Willis (1999) for extensive analyses of these problems and related issues.

bars in Figure B-2.) Maternal grandmothers had 35 percent more sleepovers and 22



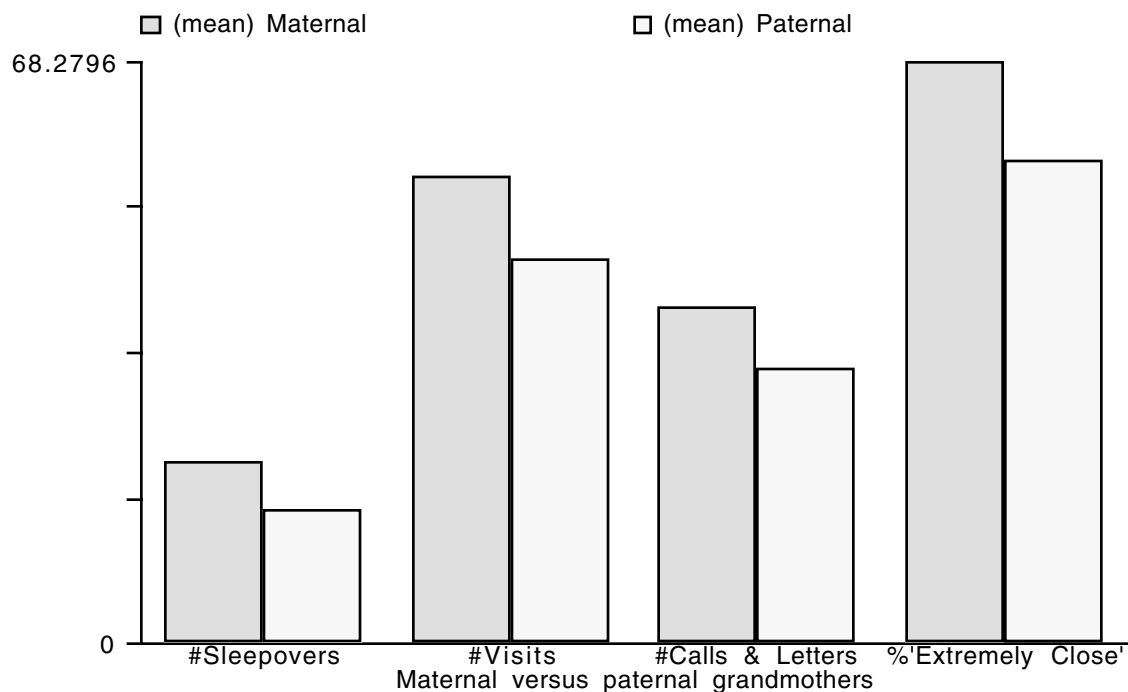Figure B-1. Average hours spent caring for grandchildren



Figure B-2. Grandmothers' involvement with grandchildren

percent more visits and calls-plus-letters.

The NSFH also asked grandparents to report their feelings of affection for grandchildren:

> On a scale from 0 to 10, where 0 is "not at all close" and 10 is "extremely close," how would you describe your relationship with your grandchild(ren)?

Most respondents chose "extremely close," so for simplicity I created a dichotomous variable indicating that choice. The results are displayed in the rightmost bars in Figure B-2. As with the other indicators, this self-reported closeness measure shows the same maternal/paternal differential—68 percent of the exclusively maternal grandparents chose the extreme portion of the closeness scale compared to 56 percent of their paternal counterparts.

But these additional results still do not overturn the idea that more straightforward explanations exist. Closeness, for example, is likely to be nurtured by repeated contact and care; feelings and actions are not separate.[13] So the results from Figure B-2, while consistent with the idea that paternity uncertainty matters, are hardly conclusive. Instead, they only indicate that further scrutiny is warranted.

I provide a bit more scrutiny in this section's final piece of evidence, which explores the relationship between incomes of adult children and grandparenting behavior. Recall from the discussion above that there are reasons to believe that paternity uncertainty could be the outcome of a rational strategy of a woman who seeks to provision her children in the face of poverty. Sarah Hrdy (1999) refers to this problem as the choice of "the optimal number of fathers" (p. 135). Hrdy focuses on cultures with quasi-polyandrous fathering arrangements, but a related argument can be made with respect to *expected* paternal support.

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[13] Though altruism is mostly treated as an unwavering, exogenous parameter of the utility function, economists have begun to recognize that such preferences are themselves forged by parental choices. For example, see the recent work of Casey Mulligan (1997) on the formation of altruistic preferences.

For example, imagine a situation with a less-than-certain probability, $p$, of a potential father's ability and willingness to provide support, $S$, to a putative child. Faced with this prospect, a woman might consort with more than one man, so as to have more than one potential father waiting in the wings, in the hopes of minimizing the chances that her child goes fatherless. This strategy has risks of its own, however, because fears of cuckoldry dilute a man's willingness to provide support.

To illustrate, imagine that a woman must choose between being strictly monogamous versus consorting with two men. (It is not necessary to make any special assumptions about the number of partners the men themselves associate with; female monogamy, for example, could well be a one-way street.) Suppose that if she is monogamous she faces a probability of $q_0$ that her mate will fail to provide support. Alternatively, if she allies herself with two men, each one's chances of failing is assumed to be $q_0 + a$, where $a > 0$. The boost in the probability of paternal failure is the cost of non-monogamous behavior. For simplicity (and realism too) I rule out explicit polyandry; at most one man provides support. I also assume for simplicity that probabilities of paternal failure are independent. The woman chooses monogamy versus non-monogamy by comparing $q_0$ with $(q_0 + a)^2$ and opting for the strategy with the lower joint probability of paternal failure. For $a < 0.25$ non-monogamy can dominate monogamy for certain values of $q_0$. As $q_0$ increases from 0, non-monogamy becomes increasingly attractive, and it can be preferable even at low values of $q_0$, if $a$ is small enough.[14]

One could imagine a slightly more complex setting in which the values of $q_0$ and $a$ are endogenously determined though history, giving rise to vicious circles of interaction between the labor market, social and family life. Diminished employment prospects raise the value of $q_0$, reducing the value of monogamous behavior for women and increasing $q_0$ further still. Meanwhile, the marginal value of $a$ could fall if women encounter increasing

numbers of male partners who presume from the start that they are non-monogamous.
Elijah Anderson's (1993) ethnography of sexual mores among inner-city youth, for
example, describes a situation of mutual suspicion between the sexes regarding infidelity.
He continues with a description of concerns about paternity uncertainty among members of
the putative father's extended family:

> In a number of cases of doubtful paternity, the boy's mother, sister, aunt,
> or other female relatives or close family friends may form informal visiting
> committees, charged with going to see the baby…. [T]he object is always
> the same: to see if the baby "belongs" to the boy it is said to. …In the
> community, the identity of the baby's father becomes a hot topic of
> conversation. The viewpoints have much to do with who the girl is,
> whether she is a "good girl" or "bad girl" or whether she has been accepted
> and taken in by the boy's family. (pp. 88-89)

These considerations suggest that paternity uncertainty looms larger among the poor than
among the non-poor. I return to the earlier topic of differences in the familial support
provided by paternal versus maternal grandparents, this time with a focus on the incomes
of parent (middle) generation. The Relatives' and Friends' Help Module of the PSID
contains information about help in the form of childcare and money received by adult
children from their parents.

Respondents were asked to report in-kind transfers in the form of child care and
money transfers received from parents. I restricted the sample to married households with
children (aged 18 or younger) and all four grandparents. I first calculated, for the whole
sample, average hours of care received from maternal versus paternal grandparents, and the
results are similar to using the HRS data above (first two bars of Figure B-3). The next
pair of bars, again calculated for the whole sample, shows a reversal previous
results—paternal grandparents are more generous with transfers of money.

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[14] For instance, if $a = 0.125$, $q_0$ need only exceed 0.0215 to make non-monogamy the preferred option.
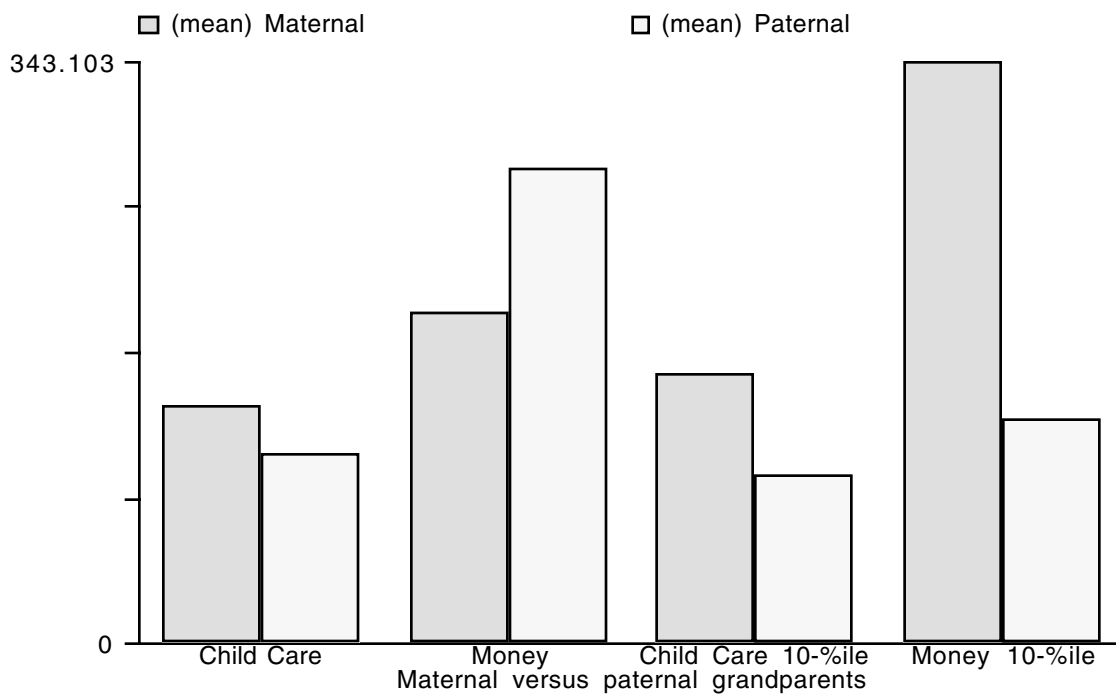
Figure B-3.  Parental Transfers--All incomes and lowest decile

One likely explanation for this result is related to earlier, commonsensical arguments:  A wife calls upon own mother more often than her mother-in-law to baby-sit. Her mother-in-law makes up for the inequity by giving more money.  But focusing just on the households in the lowest decile reverses this paternal advantage in money transfers (rightmost pair of bars, Figure B-3).  Further, the maternal advantage in hours of childcare increases.

Unlike the earlier differences, however, which save for one (sleepovers) were all significant at the 1 percent level, this difference in differences—between paternal and maternal money transfers for the poorest 10 percent of households versus the others—is significant only at the 10 percent level.  Further, the difference in differences for childcare is not significant except at the 25 percent level.  The sample sizes are small and select—recall that only married households are included.[15]

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[15] Relaxing the requirement that all four grandparents be living, and replacing it with requiring both living grandmothers doubles the sample size.  Using this sample, the difference in differences for the maternal advantage in child care is significant at the .02 level, but that of money transfers, while still positive, is not statistically significant.

There are other concerns as well. Part of the maternal/paternal differences in behavior could come from the effects of different ages, income, location and so on. The simple tabulations only suggest that there might be something to be explained. The focus on certainty of paternity also suggests connections between cultural practices in marriage markets and the attentiveness of paternal grandparents. Might their generosity depend on prevailing sexual mores? What impact might the Pill have had on the relative incentives of paternal and maternal grandparents?

The technology of paternity determination has undergone enormous change in recent years. For $250 one can obtain definitive, DNA-based evidence about paternity. How will this technology affect the family? Might it bring fathers closer to their children? If it became inexpensive, might it have an impact on families in the developing world? Would it help diminish the misogynistic practices that arise from paternity uncertainty?

One potentially distressing implication of the prospect of DNA-based paternity testing is that it could weaken the family rather than strengthen it. Consider the implications of "mate guarding" a prominent theme in evolutionary approaches to marriage (e.g., Wilson and Daly (1992)). Mate guarding is the distinctively unromantic notion that marriage is an institution to facilitate the monitoring of mutually suspicious spouses. Concealed ovulation requires guarding males stick around just about all the time, and, in the process, they are available to help invest in children. If the incentive to guard evaporates, paternal contact with children might evaporate along with it. But all this depends upon whether paternity uncertainty really matters, and more empirical research is needed in this area.[16]

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[16] The differing incentives of paternal versus maternal grandparents, and their relation to uncertainty of paternity, has attracted recent attention among psychologists with an evolutionary bent. For example, Harald Euler and Barbara Weitzel query German grandparents about their solicitude-cum-attachment (in German, *"gekummert"*) and found a positive relationship between *gekummert* and certainty of relatedness as proxied by grandparent type (e.g., maternal grandmothers versus paternal grandmothers, etc.).

## C.  Sons and Daughters.

*The Trivers-Willard Hypothesis*

Consider again Esther Duflo's (2000) South African evidence, but this time concentrate on the *recipients* of grandparental largesse—who happened to be the granddaughters.  Is it a coincidence that the benefits of pension expansion accrued to them rather than their brothers?  Perhaps, but consider: a daughter from a poor family might stand a better chance of escaping poverty—by marrying "up" the status scale—than a son who lacks the resources to marry at all.  Conversely, a son from a rich family might well be in a position to "go forth and multiply," perhaps not as successfully as Ismail Moulay, but perhaps well enough to attract more than one mate, either serially or concurrently.

Such is the logic of the so-called "Trivers-Willard" effect, an idea formulated in 1973 by renowned evolutionary biologist Robert Trivers and his fellow student at the time, mathematician Dan Willard.  Trivers and Willard were not concerned with human behavior *per se,* but instead were interested mainly in whether animals of various species might somehow control the production of female versus male offspring to take advantage of propitious circumstances for one or the other sex.  Trivers-Willard effects have been uncovered in both field and laboratory tests for several species.  But so far there is little evidence of biased sex ratios for humans.[17]

But people have other ways of controlling the sex ratios of their progeny, and many of them exact an enormous toll in human suffering: sex-specific pregnancy termination, infanticide and neglect all play a role.  Amartya Sen's (2001) recent estimates point to a catastrophe of unimaginable proportions—as many as 100 million females in the developing world lost from efforts to control the sex of offspring.

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[17] One fact that occasionally arises in discussions of sex ratios is the effect of being a United States president, a natural proxy for high status and wealth, and the propensity to sire sons versus daughters. Starting with Barbara and  Jenna Bush and going back in time, Presidential families have produced just 63 daughters compared to 90 sons, a 59 percent proportion of sons, which teeters on the brink of conventional levels of statistical significance.  Does this evidence demonstrate the value of Trivers-Willard theory for

Parents have considerable latitude for making differential investments in sons versus daughters, even if they choose to stop short of extreme measures. Edlund (1999) cites evidence that points to Trivers-Willard effects in infant and toddler care among North American and German mothers, for example.

What about parental investments in older and grown children? In addition to schooling investments, parents can make inter-vivos transfers to children and bequests at death.

I investigated the Trivers-Willard hypothesis in a somewhat narrow setting by comparing education levels of male versus female children for the sample of HRS households with just two biological children from an intact marriage. I focussed on how female and male education levels compared with varying parental wealth.
The results are presented in Figure C-1. They are somewhat surprisingly (to me, at least) consistent with the Trivers-Willard hypothesis. Figure C-1 plots non-parametric regressions of the incidence of having a daughter who is better educated than a son and *vice versa*.[18] Relative investment in sons versus daughters appears strongly related to the rank of parental net worth. Favoritism (if it can be called that) toward daughters prevails in the lower ranks, with favoritism toward sons not emerging until well into the top quartile for parental net worth.

As before, however, there are many other explanations. The predominant, and compelling, view in the human capital literature is that schooling is an investment, chosen to maximize expected lifetime earnings. A obvious corollary is that, with well functioning capital markets, schooling choices are optimal, which implies that no person contributing his or her information to Figure C-1 needed any more or less schooling than he or she got.

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predicting sex ratios in human births? Or does it reinforce the value of statistical reasoning for preventing Type I errors? See Lena Edlund (1999) for more discussion of sex-ratio evidence.

[18] Figure C-1 plots locally weighted smoothing of the binary variable indicating that the female is better educated (1 if yes, 0 if no) on rank of parental net worth. It also plots for comparison purposes the binary variable indicating that the male is better educated. I also ran ordered probit on the ordinal variable (2=male better educated, 1=male-female education equal, 0=female better educated) on rank of parental net worth (estimated coefficient, 0.48; asymptotic t-value, 3.30).

Perhaps daughters appear "favored" because labor market prospects for male high-school dropouts are relatively better than female dropouts, and that both tend to be concentrated among poorer families.

No doubt there are several other possible explanations for the figure that have little to do with Trivers-Willard effects. Nonetheless, whether or not there is any credence to Trivers-Willard effects in the United States, the theory has two virtues referred to earlier: it takes a stand with respect to predictions and is therefore falsifiable, and it points the way to new and different directions for empirical work. Despite the select sample and crude measures, Figure C-1 is interesting, and I wouldn't have thought of doing the estimation had I not been pondering the Trivers-Willard hypothesis.

In fact, another measure of transfers to children available in the HRS, intentions to bequeath, indicate no Trivers-Willard effects at all. I took all HRS respondents with only biological children and grouped them according to whether they had only male children, only female children or both.[19] The desire to leave a substantial bequest was regressed on the rank of parental net worth as before, and the results are shown in Figure C-2. There are no discernable differences in wealth effects by sex composition of the family.[20]

Kathleen McGarry's (1999) recent work comparing inter-vivos transfers and bequests indicates that the former are much more responsive of economic and demographic circumstances. It may therefore be worthwhile to using information on inter-vivos transfers to test for Trivers-Willard effects.

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[19] Many HRS respondents had stepchildren, and the problem of giving to step versus biological children merits separate, detailed attention. Recall, for example, the earlier discussion of Hamilton's rule. I defer the important issue of transfers to stepchildren to another time.

[20] These findings are consistent with Paul Menchik's (1980) finding that the majority of bequests to male-female sibling pairs are shared equally.
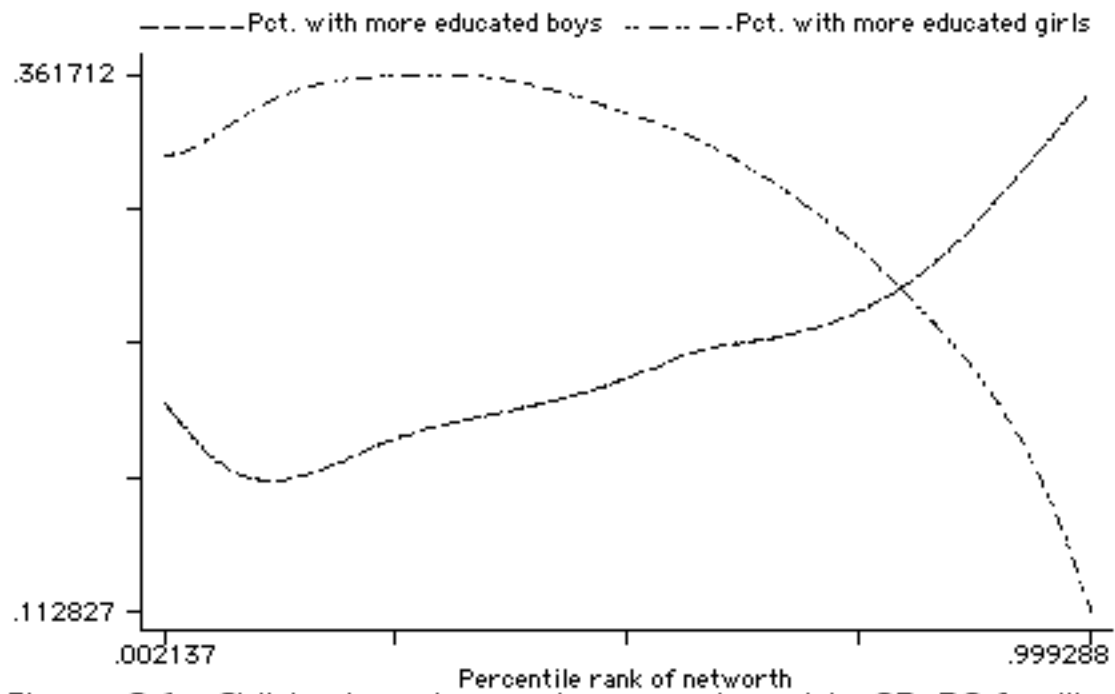
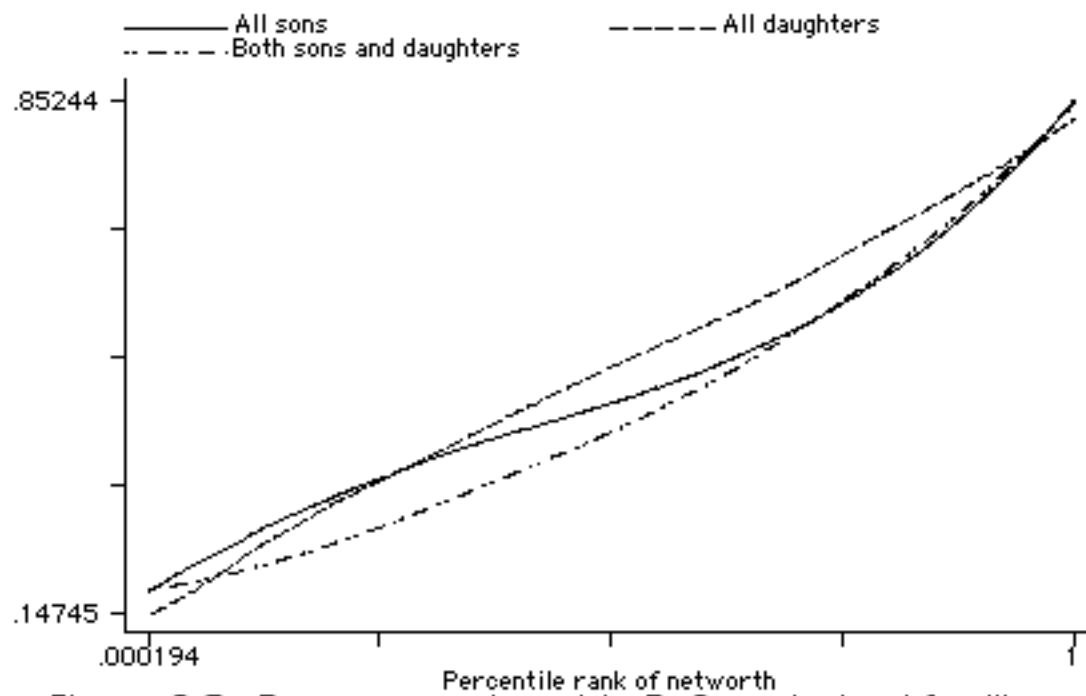Figure C-1. Child education and parental wealth: GB, BG families



Figure C-2. Bequests and wealth: B, G, and mixed families

*2. Old-age Support*

Most of the world's elderly in need of financial support receive it in the form of assistance from kin rather than public pensions. Jeffrey Nugent's (1985) survey cites individual studies of 24 developing countries on 4 continents (and even a study for Greenland) that provide evidence for the value of children for old-age security. Nugent provides a systematic listing of background conditions that make old-age security an important motive for having children, and prominent institutional factors include the lack of developed capital markets and the lack of old-age pension and disability programs.

But would parents desiring support favor sons or daughters? There are no easy answers to this question, but a case can be made for favoring sons in many instances. For example, Mead Cain (1977) estimates that Bangladeshi sons can start pulling their weight as net producers as early as age 10, but that daughters, despite also starting work very young, leave home before having a chance to repay parental investments. But even female exogamy with dowry payments can confer benefits to parents in the form of risk sharing by forging ties with in-laws living far away (Rosenzweig and Stark (1989)).

A more straightforward approach to the question might be to rely on the method of "revealed preference." China's 1990 census counted 90 girls for every 100 boys (Hrdy (1999), p. 319). For every 100 males in Egypt and Iran there are 97 females; in Bangladesh and Turkey, 95; in India and Pakistan, 93 (Sen (2001)).[21]

Country averages can mask substantial within-country variance, however. Sen finds, for example, significant variation in sex ratios and sex-specific child mortality across individual Indian regions and states. He expresses bewilderment at the heterogeneity: "The pattern of contrast does not have any obvious economic explanation. The states with anti-female bias include the rich states…as well as poor states…" (p. 40). But such a

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[21] Do proverbs constitute admissible evidence? Consider these: "More sons, more happiness and prosperity" (China); "Eighteen goddess-like daughters are not equal to one son with a hump." (India); "Daughters are no better than crows. Their parents feed them and when they get their wings, they fly away." (quoted in Hrdy, pp. 320-324.)

pattern could be conceivably be explained as an outcome of conflicting forces of Trivers-Willard effects and the need for old-age support.

Sen also concludes that the worldwide problem of "missing women" is too complicated to be addressed by economic reasoning alone. While he is certainly right to call attention to broader cultural and social issues, I'm not sure that simpler approaches are altogether out of the question.

For example, consider the common theme of crowding out referred to earlier. Full crowding out implies that public pensions just cause an equal reduction in private support. Sometimes economists assert that crowding out renders public income redistribution useless, or even counterproductive, if, for example, it raises transactions costs. But a completely different perspective is that the crowding out from public pensions might hold the key to dramatic improvements in the well being of women. If male-biased sex preferences are in large part determined by the desire for private old-age support, then shifting that support to the public sector could mitigate these biased preferences. Of course, this is just a conjecture; substantiating it requires a lot more evidence than we currently have. I have been working with Emmanuel Jimenez and Emanuela Galasso to gather evidence on crowding out. The task is daunting because it requires variation in the size of public pensions, which our 11-country household survey microdata provides. We contrast the prevalence of private transfers from adult children to their parents for countries whose public pensions range from generous (e.g., Bulgaria) to almost non-existent (Nepal). We are finding evidence for crowding out—public pensions and private support appear to be strongly inversely related.

If our initial estimates are substantiated by further empirical scrutiny, one implication, in light of this section's discussion, is that instituting public pensions might affect the desire for, and treatment of, daughters. One could even imagine preferences shifting from a bias toward sons to a bias toward daughters. Consider the retired elderly in the United States, for example. Their income sources include social security, private

pensions, dividends, and the like, but hardly any financial support from children. Instead, adult children provide support in-kind: a drive to the optometrist, for example, or advice with investments. Companionship, emotional support, assistance with problems in daily living—these, not money, constitute familial old-age security in the United States. And there is overwhelming evidence that daughters provide much more help than sons.[22]

The logic of crowding out suggests that instituting public pensions could tilt parental preferences toward daughters. This conjecture is testable. For example, it might be interesting to compare measures of the desire for, and treatment of, daughters versus sons in the United States before and after social security.

## D. Conflict.

In the course of exploring some biological roots of family economics I have referred to some of the nastier aspects of family life: the oppression of wives and neglect of daughters. In this section I explore further dimensions of family distress. Some of these emanate from a different fundamental theme from biology—Trivers' theory of parent-child conflict. Still others come from extensions of this theory of conflict.

To see why understanding familial conflict can have potential use within the economics of the family, consider the "case of the yelling parent." In a recent study, Frank Sloan, Gabriel Picone and Thomas Hoerger (1997) used data from the United States National Long-Term Care Survey (NLTCS) to study the provision of care by adult children to their disabled elderly parents. Part of their aim was to explain variation in hours of help provided by adult children. They regressed hours of informal care provided by children to their parents on a variety of mostly economic and demographic variables, including parent and child wealth, the child's wage rate, sex, marital status and the like. In addition, though, they entered the dummy variable "parent yells when upset." About a third of the

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[22] Eleanor Stoller's (1983) study of hours of elder care provided by adult children, for example, showed that daughters provided twice as many hours of help as did sons. See related evidence from Kotlikoff and Morris

parents in their sample were so classified, and yelling turned out to be an important covariate in the regression for informal care. It was associated with an extra 13 weekly hours of care, a lot by any standard, and large in relation to the average of 29 hours received from the primary caregiving child.

It is not clear that yelling doesn't belong on the left-hand side of some other regression equation. Be that as it may, what is clear is that something interesting seems to be happening here. For one thing, yelling falls outside the typical "altruism/exchange" dichotomy so common in empirical studies of intergenerational transfer behavior.[23]

The altruism/exchange dichotomy has caught on in empirical literature because it seems to cover the bases: I give you something either because I care about you or because I want something in return, or both. This ignores a third possibility: Maybe I just want you to stop bugging me.

At first blush, this alternative looks like exchange, but it's not. My disabled father yells; I change his blankets. Wouldn't we have both been better off if he had asked nicely, or at least gave a less noisome distress signal? Yelling is not Pareto optimal; neither is wheedling, nagging, cajoling, or any of the other seven deadly sins of family conflict. Such episodes are wasteful, like strikes or wars. We usually prefer to skip them and proceed straight to an agreement.

Such waste makes conflict difficult to analyze in economic terms. John Kennan's (1986) thoughtful analysis of strike behavior illuminates the problem, which he calls the "Hicks' paradox." To paraphrase Kennan's explanation, consider the following situation. You and I argue constantly, though when the dust settles we always come to an agreement of some sort. We go to therapy, in search of a theory that predicts: when an argument will occur, how long it will last, and what the outcome will be. The therapist supplies us with

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(1989). More recent findings from the HRS and Assets and Health Dynamics of the Elderly (AHEAD) data accord with this evidence. See, for example, Soldo and Hill (1995).

[23] In fact, however, Sloan, et. al. were primarily interested in testing the implications of the "strategic bequests" model of B. Douglas Bernheim, Andre Shleifer and Lawrence Summers (1985).

one.  We then use it to circumvent the *sturm und drang* and go straight to the outcome.  But this makes our therapist's theory cease to hold!

So strong is the economist's affinity for Pareto optimal solutions that when he first encountered Robert Trivers' conjecture about parent-child conflict, Gary Becker was inclined to discount it.  The theory, which first occurred to Trivers while he was watching pigeons, and which applies more to yelling toddlers than to yelling parents, is summarized in his own words below:

> The parent has been selected to invest in its offspring in such a way as to maximize the number eventually surviving.  From the parent's standpoint we can dissolve parental investment into associated benefit and cost.  The benefit is the degree to which the investment increases the survival of the offspring at hand, while the cost is the degree to which the investment decreases the parent's ability to invest in other offspring (including those still unborn).  Put this way, the parent is naturally selected to avoid any investment in the offspring for which the cost is greater than the benefit, since such investment would decrease the total number of its offspring surviving.
>
> By contrast, the offspring is selected to devalue the cost it inflicts compared to the benefit it receives.  This is because the offspring is identically related to itself but only partly related to its siblings….
>
> Because the offspring is selected to devalue the cost of parental investment, it will always tend to favor a longer period of parental investment than the parent is selected to give.  (Trivers <u>Social Evolution</u> (1985, p. 148).

Trivers reasoned that such differences in perspective, where mother likes "ego" and "sis" equally, but ego likes himself more, would lead him to temper tantrums and other behaviors aimed at steering extra maternal resources his way.  His recounting of a description of pelican chicks reads like an avian version of the "terrible twos":

> Young, ten or more days old, often begged vigorously for their food.  Usually a young pelican sat very upright in front of its parent, with neck stretched high and wings beating, until it was admitted to the pouch.  Sometimes, however, a young bird ran to an adult, threw itself on the ground, and beat its wings wildly, all the while swinging its head from side to side.  Occasionally the young lay on its side, beat one wing, suddenly jumped up, ran at and pecked several young in the vicinity, driving them away, only to continue begging.  It also grabbed, shook and bit its own wing with the bill as it turned its body around and around, growling all the time.  (Observations of George Schaller, quoted in Trivers, pp. 156-157.)

One half expects these birds to start holding their breath till they turn blue in the face, and Trivers explicitly uses the term "temper tantrums" to describe the behavior.

Becker (1976) discounted the importance of tantrums, appealing to the logic of his "rotten-kid" theorem.[24] For example: my sister, altruist mother, and I live under the same roof. Conflict, from wherever it may arise, saps shared family resources, and all are made to suffer, even the perpetrator, who therefore thinks twice about causing trouble. This is the same logic that might prevent a pet from biting the hand that feeds it or a virus from killing its host.

Bergstrom (1989) showed that Becker's reasoning had more limited applicability than he had supposed. While Becker's theorem arguably qualifies as a behavioral benchmark, it need not *necessarily* be true. Bergstrom shows that it is logically possible for a child—equipped, for example, with powerful lungs and first-strike capability against a parent with no means of precommitment—to grab more than he was supposed to get. Bergstrom and Carl Bergstrom (1996) exploit this possibility to further explore the biological implications of Trivers' approach to parent-child conflict.

Trivers' approach has recently received some rather intriguing support from recent, and widely cited, findings by evolutionary biologist David Haig (1993). In the first few days in which an embryo's cells begin to divide, it must accomplish several things to thwart imminent doom: send signals to shut down the menstruation process, establish a connection to the mother's blood supply, and duck attacks from the mother's immune system (Elison (2001)). Thus begins the cat-and-mouse game that is pregnancy. For example, the placenta secretes a hormone that blocks the sugar-reducing effects of insulin. The mother's system can respond by upping insulin levels, which is why obstetricians sometimes observe pregnant women with sky-high insulin levels and normal blood sugar. Sometimes these effects do not cancel, and the mother contracts gestational diabetes—while

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[24] Becker (1976) wrote "My analysis denies that such a conflict exists when parents are altruistic because children have an incentive to act as altruistically toward each other as their parents want them to, even if children are really egotistical." (p. 825)

her baby gets ever fatter. Haig's evidence reads like an *in utero* version of the grabby kid in the supermarket candy aisle.

Meanwhile, on the theoretical front, Becker (1993) opened the door to the study of family conflict still further with his elegant model of "preference formation," or what I prefer to call "taking behavior." A "taker" is someone too poor to provide compensation for doing his bidding, so he manipulates people instead. He does this with some ambivalence, since he is also altruistic toward his victims, but not enough to justify giving transfers because of his limited means. Instead, he "turns up the heat," with behavior that could be construed as threats, wheedling or guilt-tripping, to get his way. Vijayendra Rao, in presenting his work with Frances Bloch on domestic violence in India (Bloch and Rao (1997)) recounted an interesting example. A husband who couldn't afford a down payment for a motorcycle threatened his wife with violence in order to get her to ask her parents for the money. A necessary condition for this "taking behavior" to work, as Becker shows, is for the wife's marginal utility of capitulation to increase with her husband's threats.

It is puzzling that Becker's approach has not caught on among those who study family behavior. In stark contrast to the avalanche of applied research stimulated by models of altruism, there has only been a trickle of applied work emanating from models of conflict. Part of the problem might be an obvious question that Becker ignores: "Who in their right mind would tether themselves to some manipulative ogre?" There is, I think, a simple answer: financial or psychological burdens from breaking up create fixed costs that deter easy escape. It is possible to add these to the model, however, and doing so can add both realism and additional insight.[25]

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[25] For example, as with most rural Indian wives, those in Bloch and Rao's data set lived far from their parents and faced virtually infinite fixed costs of terminating the relationship. The more money they had access to, the worse their situation could become, if, say, parental riches present a target of opportunity for bullying husbands. Not necessarily so in, say, the United States, where a having a well-paying job and a credible escape plan could help keep a potential bully in check.

To summarize so far: conflict is a natural addition to the list of intergenerational transfer motives, and it is amenable to economic analysis. But is there any further evidence besides the case of the yelling parent, or the grabby fetus?

Evidence about family conflict based on survey data is mostly limited to studies of domestic violence.[26] There are fewer sources of household survey information concerning other forms of conflict. One recent source of new evidence, however, is now available from a special module of the 2000 wave of the HRS. This module asked a number of "point blank" questions, designed to probe for some of the more subtle motivations for intergenerational transfer behavior. The questions were directed to a sub-sample of survey respondents, and they included two related to family conflict. Respondents were asked if they agreed, disagreed, or were neutral about the following statements:

> "My immediate family sometimes pressures me to do more than I
> want to do for them,"

> "I sometimes have to ask over and over again to get my immediate
> family to help me."

Call those who agree with the first statement "pressured," and those who agree with the second "pressurers." The responses are given in Table D-1.

The rightmost column indicates that 18 percent of the respondents agreed that they felt pressured by family members, with another 6 percent giving a neutral response, which I presume indicates "maybe." The flip side of feeling pressure is applying it. It would be silly however, to ask someone whether they would describe themselves as a pest. Couching the problem as one of repeated requests for help is a more diplomatic way to broach the issue. Twelve percent of respondents agreed that they had been frequent askers, and another 7 percent were neutral. Sixty-nine percent disagreed with each of the two statements, which leaves a remaining 31 percent who perhaps cannot be certified "pressure free." Note also that 7 percent agreed to both feeling and applying pressure.

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[26] See for example, the informative papers by Helen Tauchen, Ann Witte and Sharon Long (1991) and Amy Farmer and Jill Tiefenthaler (1996).

These preliminary findings, along with the theoretical considerations discussed earlier, indicate that door is open to further inquiry about family conflict. Further research in this area would be of potential policy and intellectual interest. The policy interest stems from the potential for crowding out. If for example, private income transfers are crowded out by public income transfer programs, does this mean that some familial strife would get crowded out too? Such possibilities could change the way we think about crowding out.

Table D-1.  HRS respondents who reported feeling pressure to help family members and/or reported having to ask repeatedly for help from family members.

| Feels Pressured | Asks Repeatedly | | | Total |
|---|---|---|---|---|
| | Yes | Maybe | No | |
| Yes | 77 | 20 | 97 | 194 |
| | 39.69 | 10.31 | 50.00 | 100.00 |
| | 57.89 | 27.78 | 11.12 | 18.01 |
| | 7.15 | 1.86 | 9.01 | 18.01 |
| Maybe | 10 | 21 | 36 | 67 |
| | 14.93 | 31.34 | 53.73 | 100.00 |
| | 7.52 | 29.17 | 4.13 | 6.22 |
| | 0.93 | 1.95 | 3.34 | 6.22 |
| No | 46 | 31 | 739 | 816 |
| | 5.64 | 3.80 | 90.56 | 100.00 |
| | 34.59 | 43.06 | 84.75 | 75.77 |
| | 4.27 | 2.88 | 68.62 | 75.77 |
| Total | 133 | 72 | 872 | 1077 |
| | 12.35 | 6.69 | 80.97 | 100.00 |
| | 100.00 | 100.00 | 100.00 | 100.00 |
| | 12.35 | 6.69 | 80.97 | 100.00 |

## Conclusion

The economic approach to intergenerational transfers often invokes the assumption that men and women have identical interests and capabilities in the reproductive realm. Often this assumption is harmless and convenient, but can sometimes stifle fruitful inquiry into family behavior. I have tried to show how relaxing this assumption illuminates potentially valuable directions for research.

Another standard economic perspective on the family is that it is a place of harmony and agreement, forged either altruistic ties (Becker 1974) or the possibility of Pareto-improving trades (e.g., Cox (1987)). But more recent theoretical work, and fragmentary evidence as well, indicates that conflict, and the "taking behavior" that goes along with it, might occupy a significant niche in the familial landscape.

Some of these ideas are amenable to testing with existing household survey microdata. New sources of such information on families are becoming available all the time. But some of these theories of behavior are quite nuanced, and they broach sensitive areas that might be difficult to investigate with survey evidence alone. What interviewee would be comfortable being asked about their spouse's fidelity, for example? And who would admit to causing their relatives pain?

These limitations suggest that economists should become more open to new ways of gathering evidence. Earlier I suggested that animal studies be scrutinized and taken seriously. Here is another suggestion for gathering evidence: look at material in the "self-help" literature targeted to those seeking advice concerning problems connected to intergenerational transfer behavior.

For example, when I began this paper, I considered the predicament of an actual testator who might be concerned about problems connected with bequests and other transfers, including, perhaps, family harmony and his or her own mental health. I went to the personal finance

section of the Harvard bookstore and amazon.com to find books marketed to people who are worrying about such things.[27]

Here's an example of such a book:  <u>Beyond the Grave:  The Right Way and the Wrong Way of Leaving Money to Your Children (and Others)</u>.[28]  Written by two lawyers specializing in estate planning, it contains intriguing material pertinent to "taking behavior" and the potential family conflict.  One chapter is titled "Protecting the Surviving Parent from 'Grasping Children,' or 'When Mom Dies—I'll Be Too Old to Enjoy My Inheritance'".  Here is how it begins:

> In the vast majority of families, Dad will die first, leaving Mom with ownership and control of the family money.  When Mom dies, everything will go to the children.
>
> Most children wait patiently for their inheritance…no matter how old Mom may get.  However, I will occasionally hear children tell me without any sense of embarrassment that Mom is taking too long to die.  As one child said, "Mom is eighty-five and just keeps on going.  I'm sixty-two.  If Mom dies at ninety-five, I'll be too damn old to enjoy my inheritance."
>
> If a child is intent on an early inheritance, that child may be very persuasive when trying to convince Mom to part with ownership and control of her home and other assets.  This chapter deals with the most common scenarios in which a child attempts to advance the time he or she can grasp all or part of the family money.

One can only hope stories like these are apocryphal or at least rare.  (My first reaction was, "Have a heart, 'child'!")  But we do not know, and there is certainly no scientific way to evaluate such quirky shards of evidence.  Still, they should probably not be ignored.  Economists interested in why parents make transfers to children might do well to read magazines and books targeted to people who must struggle with these decisions.  This "self-help" angle just might offer clues about motives for intergenerational transfers, and perhaps even help guide the design of household surveys.

Theoretical breakthroughs and advances in data collection make this an especially exciting time for economists interested in family behavior.  Further progress will require harnessing insights from biology and using new ways of looking for evidence.  There is much to be done.

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[27] The most frequent apparent concern was with taxes, something beyond the scope of this paper.
[28] Written by Gerald and Jeffrey Condon, New York: HarperCollins, 2001.

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