Estimating Family Spillovers: Evidence from a Draft Lottery*

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PRELIMINARY – PLEASE DO NOT CITE

Abstract

Through social interactions, the effects of interventions may spill over to non-treated groups, with feedback to the treated group generating a social multiplier. Given the contextual and correlated effects that groups also share, identifying a social multiplier is difficult. Using the random assignment of Danish men to military service, we overcome this reflection problem. We find that having an older brother inducted into the military increases younger brother's service probability by 8 percent. We find no spillovers from younger to older brothers. Our results provide new and rare evidence on the importance of family networks.

JEL Classification: J24, J38, I38, H56

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I. Introduction

The effect of social interactions on individual behavior has since long been of interest for economists. One strong social influence is believed to come from the family of origin. Given the amount of time during their formative years that most people spend with their family members, this belief is easy to understand. Siblings and parents can provide important information on the costs and benefits of various actions and set behavior norms that are costly to deviate from. Such influences could also mean the existence of important externalities in behavior within families, externalities that could reinforce the effect of various policies imposed on family members. If spillover the effects are strong, the effect of policies can be much greater than expected.

Despite the potential importance of policies, very few studies report causal estimates of sibling spillovers[1] Such effects are notoriously difficult to estimate, because siblings typically share important unobserved characteristics and background factors, making difficult the distinguishing of the influence of a sibling from the influence of other factors.[2]

This paper focuses on sibling spillovers in a situation in which siblings are randomly assigned to serve in a government program but in which there is scope for volunteering. The context is the Danish military service, which uses a lottery to assign males to conscription. By exploiting this lottery, we can credibly estimate the causal effect of having an older brother serving in the army on the younger brother's probability of also doing so. The randomization means that the probabil-

[1] A recent exception is Dahl et al. (2014), who estimate sibling spillovers in the take-up of paternity leave in Norway. Using a birth data eligibility cutoff, they find strong evidence of sibling spillovers in program take-up.

[2] See Manski (1993) for a classic overview of the methodological issues involved in the estimation of peer effects. More recent discussion are found in Manski (2000), Moffitt (2001), Hanushek et al. (2003), and Angrist (2013).

ity of being assigned to military service is unrelated to both observed and unobserved background variables.[3] We are the first to estimate causal spillover effects in conscription.[4]

For many reasons, the Danish context provides an excellent opportunity for estimating causal sibling spillovers. Upon turning 18, all Danish males have to appear at the Armed Forces Day (AFD), where they are subject to a battery of tests and medical check-ups. Those males declared eligible for military service then participate in the conscription lottery, which randomly assigns males to military service. Since the assignment is made after the test and check-ups have taken place,we can use these pre-conscription variables to check the validity of the randomization. Morever, to shed light on possible mechanisms behind the peer effects, we can run analyses by subgroups..

We find strong evidence of sibling spillovers in conscription. The lottery strongly influences participation in conscription, and we find that having a brother serving increases a younger brother's chance of doing so by 3.3 percentage points. This finding corresponds to an 8 percent increase in the probability at the mean. Due to our instrumental variables design, this effect reflects conscription among brothers who are forced to serve because of the lottery outcome but who otherwise would

[3]Our paper also relates to the greater literature that uses random or quasi-random variation in conscription to estimate the effect of conscription on various outcomes among males. Studies on the impact of *wartime* conscription on labor, educational and health outcomes include Hearst et al. (1986), Angrist (1990), Angrist and Krueger (1994), Bedard and Deschenes (2006), Dobkin and Shabani (2009), Angrist et al. (2010), Angrist and Chen (2011), Angrist et al. (2011) and Autor et al. (2011). For evidence on the effect of *peace time* conscription, see Imbens and van der Klaauw (1995), Grenet et al. (2011), Bauer et al. (2012), Galiani et al. (2011), Card and Cardoso (2012), Albæk et al. (2013), and Bingley et al. (2014).

[4]To study peer effects in various contexts, previous research exploiting randomization include Sacerdote (2001), Katz et al. (2001), Ludwig et al. (2001), Duflo and Saez (2003), Cullen et al. (2006), Kling et al. (2007), Kremer and Levy (2008), Lalive and Cattaneo (2009), Bandiera et al. (2009; 2010), Hesselius et al. (2009), Angelucci et al. (2010), Carrell et al. (2011) and Kuhn et al. (2011). A sample of studies on peer effects using other designs to uncover peer effects include Case and Katz (1991), Evans et al. (1992), Glaeser et al. (1996), Hoxby (2000), Gaviria and Raphael (2001), Hanushek et al. (2003), Zimmerman (2003), Munshi (2003), Jacob and Lefgren (2004), Lefgren (2004),Lundborg (2006), Stinebricker & Stinebricker (2006), Maurin and Moschion (2009), Carrell et al. (2008), Mas and Moretti (2009), Carrell et al. (2009), Carrell et al. (2010), Imberman et al. (2012), and Rege et al. (2012).

have chosen not to.

We deal with a number of threats to our design. First, using an extensive set of predetermined variables, we show that none of these can predict assignment by the lottery. Second, we show that the lottery numbers are uncorrelated across brothers. Third, when we use the younger brother's lottery outcome as an instrument for his service, we show no effect runs from a younger brother to an older brother.

To investigate possible channels behind the effects, we look for evidence of information transmission across brothers. A brother serving in the army may provide important information on the opportunity cost of conscription. As we have shown in a previous paper (Bingley et al., 2014) that the costs are highest for high-ability males, we look for differential sibling spillovers across the cognitive test score distribution. We find the effects to be of similar magnitude across the entire distribution, thus casting doubt on the information transmission mechanism hypothesis. Moreover, as we know that earnings and schooling are affected mainly for those at the highest quartile of the cognitive test score distribution, that the transmision mechanism works through earnings and schooling is also unlikely. Instead, our result points to the potential importance of norms by which an older brother who serves sets a powerful within-family norm from which it may be costly for younger brother to deviate. Basically, although impossible to prove, one can think of a mechanism such as "if I am forced to serve, in all fairness you should serve, too!"

The rest of the paper is organized as follows. Section II describes the institutional context in Denmark and the details of the draft lottery. Section III explains the data we use, Section IV describes our empirical approach, while Section V presents the results, and Section VI investigates alternative mechanisms. Section VII concludes.

II. Military Conscription in Denmark

Upon turning 18 years old, all Danish males are called to participate in the "Armed Forces Day" (AFD). Participation in the AFD is mandatory for all men and involves a medical examination and assessment, a psychological evaluation, and an IQ test (Armed Forces Qualification Test, AFQT).[5] Exceptions to AFD participation are given only to males who have severe health problems, or who are currently in jail for at least 30 days or for violent crimes. About 10 to 15 percent of a cohort is typically declared non-eligible in advance because of a doctor's statement documenting serious somatic or psychiatric disorders (Hageman et al. , 2008), and these men are therefore not called to the AFD.

For those males participating in the AFD, the test results and the health assesment form the basis for further selection. About 70 percent of those at the AFD are declared eligible for military service, whith the rest declared ineligible. The eligible group then participates in the conscription lottery, drawing from a lottery drum filled with lottery numbers. Months later, the Danish Armed Forces (DAF) announces the actual cutoffs, depending on the size of the cohort, the number of volunteers, and the needs of the DAF. Although individuals who draw a number above the cutoff do not have to serve in the military, they may volunteer. In the end only about 20-25 percent of a cohort is required to serve (see Teasdale, 2009).

While delaying the start of military service for education reasons is possible, service usually has to be started before the individual turns 32.[6] In our sample, 99.6 percent had completed their military service by age 25. Before 2006 the average length was about eight months, but after 2006 it was shortened to four months.[7]Given that we study the period 1994-2007, the majority of males in our

[5]The test, in use since 1957, is not undermined by lack of motivation or under-performance among the men taking it (Teasdale, 2009; Teasdale et al., 2011). During our study period, it had 78 items.The total test score is the number of correct answers.

[6]Article 25 (paragraph 2) Law of Military Service.

[7]The length is longer for certain types of DAF assignments.

sample faced an eight-month military service period. The service can be accomplished within either the DAF or the civil defense, e.g., fire and rescue services. The conscripts are provided with housing and military clothes free of charge, and as of 2012, to cover costs of food, etc., they receive a monthly allowance of 6,230 DKK (831 EUR). In addition, conscripts receive a monthly taxable salary of 7,421 DKK (990 EUR). There have been minor changes in the amounts over time.

III. Data

In our analyses, we combine data from different sources. First, we use administrative records from the DAF, which includes information on eligible conscripts from 1994 through 2010. The data includes the results from the ability (AFQT) and physical tests, eligibility status, health profile, occupation, education level (years of schooling), height, and the lottery number and cutoffs for each year since 1994, as well as whether the conscripts had volunteered or not, the conscription year (the AFD year), the starting year for their military service and the unit in which they served. All variables are measured in the AFD year (see table 1 for descriptives).

[Table 1 about here]

Through the Danish civil registration number, we have linked the military records to a variety of registers from which we obtain demographic characteristics, education, social affiliation, health and medical care, death and cause of death, migration, employment, unemployment, income, and criminal records from Statistic Denmark. This information is available from 1980 for conscripts, their parents or those who raised them, their partners, their children or those of their partners, and their siblings.

In our analyses, we require that at least two brothers are observed during our observation window. As we have data on males born 1976-1983, we observe only brothers whose birth spacing does not exceed 7 years. This criterion gives us a sample of 13,124 brother pairs[8]. Importantly, as ineligible males are not included in the military records, we observe only brother pairs in which both are eligible to serve.

Given that the tests at the AFD are performed before the lottery, we can use the test information to assess the randomness of the lottery. Moreover, we can use our extensive information on other background factors for the same purpose. If the assignment is truly random, predicting assignment based on the test results and background factors should not be possible. In Table 2, we show a regression on assignment by the lottery as a function of test results and other background variables. As expected, cognitive test scores, height, being raised in a single-parent family, ethnicity, being placed in out-of-home care, birth weight, parental income, parental education, health factors, and crime do not predict assignment.

In the left column of Table 2, we do not include birth weight, parental income, or parental education, because by including them we lose some observations. We instead include these variables in the right column, and the randomization still works. In our main regression specifications, we check the results both with and without this extended set of controls. The regressions in Table 2 also control for birth year, birth month, and other variables capturing the timing of the AFD lottery.

[Table 2 about here]

IV. Method

We are interested in the effect of having an older brother joining military service on a younger brother's propensity to join, which we model as follows:

$$y_i = \pi_0 + \pi_1 BROTHER\_MILITARY_i + X_i \pi_2 + \upsilon_{it},$$

[8]The two first eligible brothers born within the window 1976-1983.

where y_i is a binary indicator taking the value 1 if an individual i joins military service, $BROTHER\_MILITARY$ is an indicator of whether the older brother joined the army, and X is a set of control variables. A standard OLS estimate of π_1 would be biased because the decision of an older brother to join the army may be correlated with unobserved factors shared by a younger brother, such as family norms and other hard-to-measure background characteristcs. To deal with the non-randomness of the older brother's decision, we exploit the military enlistment lottery and instrument $BROTHER\_MILITARY$ according to:

$$BROTHER\_MILITARY_i = \delta_0 + \delta_1 LOTTERY_i + X_i \delta_2 + \eta_i.$$

where, $LOTTERY_i$ indicates being assigned by the lottery to join military service. As the lottery randomly assigns individuals to military service, we do not need to include control variables other than to gain precision. In our empirical analysis, we will also check for signs of violations of the randomization by comparing estimates of π_1 obtained with different sets of control variables.

The outcome of the lottery will not influence every male's decision to join the DAF: some choose to volunteer for military service, and a few resist serving, irrespective of the lottery outcome. Our IV estimator therefore provides a Local Average Treatment Effect (LATE) that reflects the effect of military service among the group of compliers. In our context, this is the group of males that would serve if they had been randomly assigned to do so but that would otherwise not have served.

The opportunity cost of serving among the group of compliers is greater than that among the group of volunteers. We must keep this in mind when interpreting the effect of having an older brother serving on the younger brother's probability of doing so. Given that the lottery outcome is uncorrelated across brothers, any positive (negative) estimated brother influence should be interpreted as an increased (decreased) likelihood of a younger brother deciding to volunteer when an older brother is forced to serve. This situation is a quite different scenario from those used in other peer effects studies, which often exploit situations in which people are incentivized to take a particular action but in which the decision itself is ultimately voluntary.

The data to which we have access allows us to conduct a number of specification checks and extensions. First, as all brothers in a family are exposed to the lottery, we can run placebo regressions checking for an effect of having a younger brother serving on an older brother's probablity of serving. If our method is valid, there should of course be no such effect in the "wrong" direction. *If* such an "effect" exists, however, it would suggest that the lottery is not truly random and that men in certain families for some reasons have a higher or lower chance of being assigned to serve. Second, we can look for any correlation in the lottery outcome across brothers. With true randomness, the correlation should essentially be zero. Third, we can exploit the test results and background data to examine whether the strength of the sibling spillovers depends on how close in characteristics the brothers are. For example it appears intuitive that the closer the brothers are in characteristcs, the stronger the effects should be.

V. Results

We start by reporting first stage results. Table 3 shows the estimates, in which military service of the older brother is regressed on his lottery cutoff. The sample consists of all cases in which for each family we observe two brothers, born between 1976-1983, who were eligible for military service. In column 1, we report estimates of the lottery cutoff, with the basic covariates, including birth year, birth month and dummies indicating the timing of the enlistment. We then add various sets of covariates in columns 2 and 3. If the lottery is truly random, we do not expect to see any important changes. In addition, the covariates serve to increase the precision of our estimates.

[Table 3 about here]

In the first column, we see that the impact of drawing a lottery number below the threshold increases the probability of military service by 52.3 percentage points. The coefficient of the cutoff is highly significant, and the F-statistic is well above the rule of thumb of 10. In column 2, adding our first set of control variables, including cognitive test scores, height (and the square of test score and height) barely changes the coefficient of the cutoff (52.3 percentage points). We finally add an extended set of controls in column 3, including being a native Dane, growing up in a single-parent family, and being placed in out-of-home care, and having been convicted of a crime, as well as birth weight, parental income, parental schooling, and health indicators . Again, adding these covariates has very little impact on the estimated effect of the cutoff. Indeed,these results are expected, as with proper randomization these controls should not matter other than to increase the precision of the estimates.

[Table 4 about here]

We next focus on our main IV estimates. In Table 4, we report these estimates, using the same specifications as in Table 3. The first column shows a positive and significant effect of having a brother serving. The magnitude of the estimate suggests that having a brother serving increases the probability that a younger brother serves by 3.3 percentage points—about an 8 percent increase in the probability at the mean. The brother effect measures volunteering by the younger brother as a function of having an older brother being forced to serve. The estimated effects remain much the same in columns 2 and 3, even when we add the various sets of covariates. In Table 5, we show results for OLS and reduced forms. In panel A, the service coefficient is remarkably stable across specifications, showing that having a older brother serving increase the younger brother probability of service by 9 percentage points. Panels B shows reduced form coefficients on an indicator for older brother draft status. The draft coefficient shows that having a older brother drafted increases the younger brother probability of service by 2 percentage points.

[Table 5 about here]

Our estimates suggest a strong influence of having an older brother serving. A powerful placebo-like test in this context is to look for an effect of having a younger brother serving on the probability that an older brother serves. Such a test would address one of Manski's (2000) threats in estimations of peer effects: the "reflection problem", i.e., while I affect my peer, my peer also affects me. Table 6 shows the results from this test, where we run our main analysis but now the other way around. We use the same specifications as before but now instrument the younger brother's military service with his lottery outcome. This specification appears to rule out the reflection problem: the estimates are small and insignificant across the line. Given that the older brother attends his AFD earlier than the younger brother—i.e., having the military outcome of the older brother precede that of the younger brother—this result is expected.[9]

[Table 6 about here]

Our estimates thus far are average effects. However, the strength of sibling spillovers may depend on several factors. If one mechanism behind the sibling effects is the provision of information, we expect stronger effects when conscription

[9] In principle, some scope exists for a younger brother to affect an older brother's decision. An older brother who was not assigned to serve by the lottery may later decide to volunteer, in principle possibly a function of the younger brother's conscription. However, the scope for such effects is small . The average birth spacing is three years, and most males serve at age 20. As almost 100 percent of the males have served by age 25, t only a small window exists for the an older brother to act on the younger brother's conscription.

entails large opportunity costs. Moreover, we expect that the closer the brothers are in characteristics, the stonger the interaction and the greater the influence on one another. We now explore these issues in more details.

VI. Extensions

An often-mentioned mechanism behind peer effects is information transmission. In our context, having an older brother serving means that important information about both the consequences and the content of conscription, is easily accessible. One particularly important source of information concerns the opportunity cost of conscription, as males spend on average eight months in the DAF—time that could have been spent studying or at work. In Bingley, Lundborg and Lyk-Jensen (2014) we show that Danish conscripts face substantial opportunity costs: on average, those being forced to serve face a 2.5 percentage earnings penalty. This finding hides important heterogeneity, i.e., males scoring high on the AFQT face much higher costs, amounting to about a 7 percent earnings penalty. If this information is more accessible from having a brother serving, we expect sibling spillovers to be different for this subgroup, perhaps even negative. We investigate this hypothesis by running our main analysis by quartiles of the ability distribution, as indicated by the results on the AFQT.

[Table 7 about here]

Table 7 shows the results by quartiles. To maximize the number of observations, we use the specification from column 2 of Table 4 . Two results stand out. First, precision is lower when we slice the sample across the ability distribution, and none of the estimates are statistically significant. Second, the point estimates are remarkable similar across the distribution, ranging from a 3 percentage point increase to a 3.7 percentage point increase at the third quartile. We thus find no evidence that the group facing larger opportunity costs from conscription react any differently than other groups. This result suggests that the estimates are not primarily reflecting information about the costs on conscription, as would be expected if information on the opportunity cost is well known or easily accessible.

VII. Conclusions

Social interaction effects are of great interest to economists but at the same notoriously difficult to estimate. For family interactions, the key challenge is to separate within-family member similarities caused by shared but unobserved factors and family members influencing one another. Consequently, very few studies provide causal estimates of family spillovers. We contribute to this literature by exploiting a unique situation wherein family members are randomized into conscription.

Our results suggest that social influences within families are strong. Having an older brother serving in the army increases a younger brother's probability of serving by about 8 percent. This estimate reflects a decision of the younger brother to volunteer as a function of having an older brother being forced into conscription. Our findings are of importance for policy-makers, because they show that policies that change behaviours for some family members may run further through the family.

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VIII. Tables and Figures

TABLE 1—SUMMARY STATISTICS

	Full sample mean	Served mean	Not served mean
Height (cm)	180.42	180.42	180.43
	(6.57)	(6.49)	(6.64)
AFQT	45.25	45.10	45.37
	(8.28)	(8.03)	(8.47)
Birth year	1979.36	1979.30	1979.41
	(2.20)	(2.28)	(2.14)
Birth month	6.38	6.41	6.35
	(3.35)	(3.30)	(3.39)
Raised in single-parent family	0.15	0.16	0.15
	(0.36)	(0.37)	(0.36)
Placed in out-of-home care	0.03	0.04	0.03
	(0.18)	(0.20)	(0.16)
Dane	0.96	0.96	0.95
	(0.20)	(0.19)	(0.21)
Birth weight (gr)	3420	3407	3430
	(637)	(651)	(627)
Household income at age 15 (DKK)	130715	129177	131893
	(57119)	(57945)	(56453)
Mother's years of schooling	11.83	11.73	11.91
	(2.88)	(2.85)	(2.90)
Father's years of schooling	12.24	12.12	12.33
	(3.12)	(3.09)	(3.14)
Criminal conviction before age 18	0.04	0.04	0.03
	(0.19)	(0.19)	(0.18)
Psychiatric diagnosis before age 18	0.01	0.01	0.01
	(0.08)	(0.08)	(0.07)
Mental health medication before age 18	0.00	0.00	0.00
	(0.05)	(0.06)	(0.04)
Treatment for addiction before age 18	0.00	0.00	0.00
	(0.01)	(0.01)	(0.01)
Hospital admissions before age 18	0.78	0.77	0.78
	(0.42)	(0.42)	(0.41)
Bank balance before age 18	9435	8588	10083
	(26709)	(25225)	(27774)
Unemployment before age 18	0.00	0.00	0.00
	(0.01)	(0.00)	(0.01)
Military service	0.43	1.00	0.00
	(0.50)	(0.00)	(0.00)
Observations	26248	11377	14871

Note: For the variables birth weight, household income at age 15, mother's and father's years of schooling, the sample size is 25,259.

TABLE 2—RANDOMIZATION BALANCE CHECK: EFFECT OF PRE-DETERMINED CHARACTERISTICS ON THE PROBABILITY OF DRAWING A LOTTERY NUMBER ABOVE THE CUTOFF

	(1)	(2)
Height (cm)	-0.00050	-0.00074*
	(0.00043)	(0.00045)
AFQT	0.00053	0.00037
	(0.00035)	(0.00037)
Dane	0.01486	0.00608
	(0.01418)	(0.01998)
Raised in single-parent family	-0.00196	-0.00114
	(0.00770)	(0.00791)
Placed in out-of-home care	0.01617	0.01795
	(0.01588)	(0.01636)
Criminal conviction before age 18	0.01480	0.01377
	(0.01474)	(0.01524)
Psychiatric diagnosis before age 18	-0.04073	-0.04359
	(0.03478)	(0.03568)
Mental health medication before age 18	0.03525	0.03742
	(0.05648)	(0.05750)
Treatment for addiction before age 18	0.06843	0.08474
	(0.12201)	(0.12667)
Hospital admissions before age 18	0.00172	0.00339
	(0.00802)	(0.00836)
Bank balance before age 18	-0.00000	-0.00000
	(0.00000)	(0.00000)
Unemployment before age 18	0.08108	-0.01811
	(0.26280)	(0.17541)
Birth weight (gr)		0.00000
		(0.00000)
Household income at age 15 (DKK)		0.00000
		(0.00000)
Mother's years of schooling		0.00155
		(0.00112)
Father's years of schooling		-0.00061
		(0.00103)
Observations	26248	25259
F-test of covariates	1.07	1.00

Note: Standard errors in parentheses.* p<0.10, ** p<0.05, *** p<0.010.

TABLE 3—FIRST-STAGE REGRESSIONS. THE EFFECT OF THE OLDER BROTHER LOTTERY CUTOFF ON THE PROBABILITY THAT HE PARTICIPATES IN MILITARY

	(1)	(2)	(3)
Older brother cutoff	0.523***	0.523***	0.520***
	(0.00759)	(0.00759)	(0.00775)
Basic controls	Y	Y	Y
Extended controls I	N	Y	Y
Extended controls II	N	N	Y
Extended control III	N	N	Y
Observations	13124	13124	12644
Adjusted R^2	0.274	0.275	0.274
Angrist Pischke F-stat	4736.4	4736.7	4498.5

Note: Standard errors in parentheses.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$.

TABLE 4—IV ESTIMATES OF THE EFFECT OF THE OLDER BROTHER DOING MILITARY ON A YOUNGER BROTHER'S PROBABILITY OF DOING MILITARY SERVICE.

	(1)	(2)	(3)
Older brother service	0.0329**	0.0336**	0.0299**
	(0.0143)	(0.0143)	(0.0147)
Basic controls	Y	Y	Y
Extended controls I	N	Y	Y
Extended controls II	N	N	Y
Extended control III	N	N	Y
Observations	13124	13124	12644

Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$.

TABLE 5—OLS AND REDUCED FORM.

	(1)	(2)	(3)
A. OLS regression: outcome younger brother service			
Older brother service	0.0910***	0.0907***	0.0857***
	(0.00840)	(0.00840)	(0.00856)
Adjusted R^2	0.080	0.081	0.082
B.Reduced form regression: outcome younger brother service			
Older brother cutoff	0.0198**	0.0202**	0.0174**
	(0.00846)	(0.00845)	(0.00860)
Adjusted R^2	0.072	0.073	0.075
Basic controls	Y	Y	Y
Extended controls I	N	Y	Y
Extended controls II	N	N	Y
Extended control III	N	N	Y
Observations	13124	13124	12644

Note: Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$.

TABLE 6—IV PLACEBO ESTIMATES OF THE EFFECT OF THE YOUNGER BROTHER DOING MILITARY ON AN OLDER BROTHER'S PROBABILITY OF DOING MILITARY SERVICE.

	(1)	(2)	(3)
Younger brother service	0.00228	0.00216	-0.00128
	(0.0144)	(0.0144)	(0.0148)
Basic controls	Y	Y	Y
Extended controls I	N	Y	Y
Extended controls II	N	N	Y
Extended control III	N	N	Y
Observations	13124	13124	12571

Note: Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$.

TABLE 7—EFFECT ON AN OLDER BROTHER'S MILITARY SERVICE ON THE PROBABILITY THAT A YOUNGER BROTHER DOES MILITARY SERVICE. ESTIMATES ACROSS QUARTILES OF THE ABILITY DISTRIBUTION OF THE YOUNGER BROTHER.

	1st quartile	2nd quartile	3rd quartile	4th quartile
Older brother service	0.0333	0.0300	0.0374	0.0324
	(0.0290)	(0.0286)	(0.0288)	(0.0279)
Observations	3434	3392	2948	3350
First stage F-test	1138.8	1202.2	1243.5	1250.0

Standard errors in parentheses.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$.