On the Role of Personality Traits and Social Skills in Adult Economic Attainment

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On the Role of Personality Traits and Social Skills in Adult Economic Attainment

Over persoonlijksheidskenmerken, sociale vaardigheden
en arbeidsmarkt succes

Thesis

to obtain the degree of Doctor from the
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by command of the
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Doctoral Committee

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Chapter 1

Introduction

> "Research on non-cognitive skills is in its infancy. Too little is understood about the formation of these skills or about the separate effects of all of these diverse traits currently subsumed under the rubric of non-cognitive skills. What we currently now, however, suggests that further research on the topic is likely to be very fruitful."
>
> — James J. Heckman & Yona Rubinstein [2001]

A The Research Question

The research reported on these pages analyzes the influence of personality traits and interpersonal skills in adult economic attainment, with particular focus on earnings. The purpose is to bring together ideas from economics, social-psychology, and related disciplines in order to contribute to the growing literature on non-cognitive skills as they relate to adult educational and labor market outcomes. The work at hand offers a peek into the "black box" that economists have come to call "non-cognitive skills", and examines the relevance of such skills for success in the labor market through a collection of self-contained essays.

B Non-Cognitive Skills, Schooling, and Earnings

Scholars in sociology, psychology, and education have long suggested the importance of behavioral and psychological variables in adult socioeconomic attainments, broadly defined. Recently, the idea that there is a dimension of individual characteristics relevant for labor market success *other than* measured cognitive abilities has also gained interest among economists. This is in part because consideration of a host of factors, among them gender, race, years of formal education, labor market experience and parental socioeconomic background has not yet entirely solved the question, why apparently similar individuals earn substantially different wages. In Mincerian earnings equations, more than half of the variation in log hourly wages or annual earnings is conventionally left unexplained by the standard set of regressors referred to above.

There are a number of recent studies which look explicitly at a set of earnings determinants related to individuals' personality traits and social capabilities; leadership skills [Kuhn and Weinberger 2004], soft skills [Duncan and Dunifon 1998], aggression and withdrawal [Osborne 2003], self-esteem [Goldsmith, Veum and Darity 1997], or individual social capital [Glaeser, Laibson and Sacerdote 2002]. This list of examples is by no means exhaustive but gives a flavor of the latest literature and the jumble of factors considered. What all these studies have in common though is the explicit focus on personality and behavioral attributes *as such*, and not the concern over purging estimates of the returns to schooling of potential omitted variables bias.

Within the economics literature these factors have, for right or for wrong, been subsumed as "non-cognitive skills" (see, for example, the work by James Heckman, and coauthors). The intention was to distinguish them notionally as measures of a second skill-dimension, orthogonal to cognitive capabilities conventionally measured by performance on IQ and related aptitude tests. Cognition is a broad term which has been traditionally used to refer to such activities as thinking, conceiving and reasoning [Sternberg 1990]. Most psychologists would be hesitant to term the factors considered in the literature referred to above as truly *non*-cognitive. However, this is the maintained nomenclature within economics and we shall not attempt to change it here.

It is not entirely clear which factor *exactly* it is that underlies this ominous second dimension. Given the diversity of traits subsumed under the heading of non-cognitive skills, it is questionable whether there even exists a non-cognitive behavioral or personality analog to the common g-factor (general intelligence) underlying most measures of cognitive performance [Bowles, Gintis and Osborne 2001]. In this context, it should be noted that also within the cognitive category there is probably more than one type of labor market relevant intelligence [Sternberg 1985]. Caroll [1993], reanalyzing the major data sets of psychometrics, suggests that there are three important sorts of cognitive abilities corresponding to fluid intelligence (ability to solve problems), crystallized intelligence (the ability to draw on old solutions to address new problems), and spatial and mechanical ability. Nevertheless, there is strong evidence of a single *dominant* factor as a determinant of performance on cognitive tests and their effects on economic outcomes [Heckman 1995; Cawley, Heckman, Lochner, and Vytlacil 2001].

The confusion on the empirical side of the issue is in part due to the taciturnity of mainstream economic theory. Human capital, principal-agent and signalling models remain silent on the role of non-cognitive skills in general. One consequence thereof, as several authors have pointed out, is that empirical work on skills and skill formation almost exclusively focuses on measures of cognitive ability: Most discussions of ability bias in the estimated return to schooling treat omitted ability as cognitive ability; most assessments of educational policy interventions stress the gain from reforms in terms of students' ability to perform on some standardized achievement test; and, widespread use of such tests for college admissions and educational evaluation foster the belief that the skills that are tested in this way are essential for success in schooling and work.

It would be inappropriate to deny that cognitive skills play a significant role for adult economic attainments, independent of schooling and parental background. Their quantitative importance, however, has been shown to be rather modest. Almost all empirical studies that focus on cognition and earnings, for example, find that returns to cognitive ability measured by standardized test scores are positive and significant, while the explanatory power is typically not overwhelmingly large. Murnane, Willet and Levy [1995], for instance, find that the addition of IQ test scores to the earnings function improves the R-squared only marginally by 2 to 4 percent; conditioning on schooling, age and family background. The real problem is the monolithic status that measures of cognitive functioning assume in almost any discussion of labor market relevant skills. "Current thinking about human capital and human capital formation policies focuses on cognitive skills *to the exclusion* of non-cognitive skills" [Heckman and Locher 2000; pp.48-49, italics added], ignoring a growing body of evidence indicating the importance of non-cognitive skills for determining success in the labor market.

Lack of guidance from economic theory has had a second consequence. It has left empirical researchers alone with the difficult task of selecting an appropriate subset of non-cognitive skills to study. And, in general, researchers in this field have not been unimaginative in their quest for additional explanatory variables. They did what the available data allowed them to do and studied the effects on earnings of such diverse personality and behavioral measures as participation in high school athletics and social clubs, church attendance, household cleanliness, need for affiliation, self-esteem, degree of trust, and many more. The point here is that the lack of guidance from theory has led to a pragmatic "ad-hoc" approach in empirical research to study whatever measure happens to be within reach. The present work is no exception in this regard. Overall, too many different personality and behavioral traits are lumped into the same category and too few data sets contain measures of personality and behavioral traits thought to be of economic importance, as well as measures of adult educational and labor market outcomes. This last point cannot be overemphasized, the unavailability of reliable and valid measures of personality and behavioral traits is a real issue. Much of the neglect of non-cognitive skills in analyses of earnings, schooling, and other outcomes is due to the relative scarcity of such variables in most of the large-scale survey studies frequently used by economists.

In addition, given the *nature* of available survey studies, there are two econometric problems persistently plaguing the empirical work in this field: measurement error, and the endogeneity of personality or behavioral variables with respect to the outcome. Measurement error can be handled by augmenting the correlation matrix using reliability estimates. The issue is of course potential endogeneity bias in the estimated effects due to simultaneity and selection on unobservables. Most survey studies have only recently added social-psychological factors to their inventories and, in most instances, offer just one cross-section of data with the explanatory and outcome variable measured simultaneously. This makes it very difficult to identify causal effects, which is of course what we are ultimately interested in. Abstracting from the issue of simultaneity, by means of pre-labor market measures for example, it is still difficult to exclude that the estimated effects are not reflecting selection on unobserved factors correlated with both, the explanatory variable and the outcome. I am not aware of a single study in this field that would handle the above issue in a convincing way, similar to (the better) studies on the returns to schooling or the effects of schooling on cognitive performance tests.

It is clear from the above that the challenge of any empirical research in this field lies in the lack of appropriate data. Given that one is still interested to do work on this topic, the only strategy is to do something innovative with (and within the constraints imposed by) the existing data, and not econometric wizardry.

C Sources of Data

The data on which this research is based were collected from a large probability sample of Wisconsin high school seniors who have been followed over their entire life course in order to measure their educational and labor market achievements; the *Wisconsin Longitudinal Study of Social and Psychological Factors in Status Attainment* (WLS). The WLS consists of 10,317 randomly sampled individuals who graduated from Wisconsin high schools in 1957. Together, these individuals constitute approximately one-third of all seniors in Wisconsin high schools in that year. After the initial wave of data collection, primary respondents were re-interviewed in 1975 and 1992. Together with their parents' interview of 1964, these waves provide information on, among others, socio-economic background, mental ability, educational attainment, family formation, and labor market histories. The original sample is broadly representative of white men and women who have completed at least twelve years of schooling. For more detailed information on the WLS be referred to Sewell et al. [2001] and the references therein.

It should be noted that the WLS is very popular among sociologists and psychologists and somewhat "under-researched" by economists.[1]

[1]It was also a sociologist, Prof. Jaap Dronkers, who eventually put me on the right track by making me aware of this data set, for which I again would like to express my gratitude. I still remember the "hunt" for appropriate data in the initial phase of my PhD, months in which I screened a considerable number of European and U.S. studies to finally decide for the WLS.

The WLS has long been viewed as a data-set quite unique in its linkage of personality and social-psychological factors to socioeconomic outcomes at various points in adulthood. One of the strengths of the WLS lies in its assessment of the "Big Five" personality traits, one of the largest personality assessments of midlife adults currently available (for a comparative analysis of the WLS data with a number of alternative surveys, see Kuo et al. 2001). This personality questionnaire is a relatively short test instrument specifically designed to assess trait dimensions of the Five-Factor Model (FFM) of personality structure in surveys. The Extraversion (vs. Social Inhibition, or Introversion) scale captures gregarious, energetic, and expressive features of behavior. The Agreeableness (vs. Antagonism) scale reflects essentially prosocial characteristics, describing the person who is empathic and makes an effort to establish positive relationships with others. The Conscientiousness (vs. Lack of Direction) scale captures the multiple elements of persistence and impulse control in task and achievement settings. The Neuroticism (vs. Emotional Stability) scale reflects multiple elements of negative emotionality, such as nervous tension, fearfulness, and brittleness under stress. The Openness to Experience scale refers to persons who are imaginative, curious creative, and susceptible to absorbing experience.

A further strength of the WLS lies in its "relational" design: it has followed a relatively large sample of high school graduates until their mid-fifties and it has at the same time recorded detailed information on social relationships between graduates and their significant others, such as names of close friends from their senior class. Data on social relations pre-labor market is informative about individuals social behavior and their ability to assume a central position in the group of classmates and, as we shall see, is highly predictive of subsequent economic achievements.

D Organization of the Volume

It has proven remarkably difficult to give an adequate account of the skills that schools produce and to document their reward in the labor market. This is where the two chapters subsequent to this introduction seek to make a contribution. I take the classical sociological literature on socialization for work as my point of departure (Dreeben 1967; Parsons 1959). According to these theories, schools internalize in its pupils important social competencies and behavioral norms, subsequently valued in the labor market. This approach emphasizes the social framework of education, the role of human relations within the school, and the impact of the school system on the behavior of the persons participating in it.

Chapter 2, Friendship Relations in the School Class and Adult Economic Attainment, asks whether we are indeed able to identify characteristics of individuals related to their social behavior that raise earnings. I show that the ability to form interpersonal relationships, and to assume a superior social position in a network of relations, has a significant impact on adult earnings as well as on a number of other outcomes.

Chapter 3, Class Size and Students' Friendships, addresses the important question what role institutional characteristics of schools play in the formation of such relationships? The motivation behind this chapter is to shed some light on which skills (other than cognitive) are acquired at school, and what scope for policy interventions exists.

Chapter 4, Estimating the Effect of Personality on Male-Female Earnings, stands a bit apart from the preceding two and looks at personality traits (largely determined by inheritance) as opposed to acquired behavioral skills. The central theme here is to use the Five-Factor Model of personality structure as a comprehensive organizing framework to analyze the effects of personality traits on earnings. As the title indicates, we shall explicitly consider gender differences in personality structure as well as gender differences in the way the labor market rewards and penalizes the respective traits.

Chapter 5 presents the main conclusions emerging from the previous chapters and reviews a number of issues that remain unresolved with respect to their implications for future research. As mentioned in the introductory paragraph, this volume consists of a collection of self-contained essays grouped around a common theme; without having the intention to offer a complete and exhaustive treatment.

Chapter 2

Friendship Relations in the School Class and Adult Economic Attainment

(joint with A. Galeotti)

"Now in these unequal friendships the benefits that one party receives and is entitled to claim from the other are not the same on either side; ... the better of the two parties, for instance, or the more useful or otherwise superior as the case may be, should receive more affection than he bestows."

— ARISTOTLE, *The Nicomachean Ethics*

I Introduction

In his seminal essay "The School Class as a Social System", T. Parsons [1959] described the school class as an agency of socialization through which individual personalities are trained to be motivationally and technically adequate to the successful performance of their adult roles. When economists study skill formation and the effects of schooling, they often focus on the development of technical skills as measured by scores on reading, writing, and mathematics tests. Important outcomes these may be, the school not only imparts a certain amount of subject knowledge and general problem solving skills. It also internalizes in its pupils the social competencies and behavioral norms that make them function adequately on an interpersonal level. The importance of classmates in this respect should be immediately evident. Classmates constitute *the* primary social system, besides the family, in which any adolescent participates. The focus of this chapter, then, is on analyzing the effect of students' social relationships with classmates on subsequent economic attainment.

The view of the school class as a social system forms the point of departure for our work in two respects: First of all, it draws our attention to the motivational and behavioral outcomes of the schooling process. It stresses the fact that social competencies and norms for interpersonal behavior are not acquired in the abstract. They develop in the relations to others and are the outcomes of a prolonged socialization process stretching from early childhood in the family and elementary school until the end of high school. Secondly, viewing the school class as a social system has an influence on the methodological approach. To us, the most appropriate way of measuring inherently "relational" concepts is to draw on methods from social network analysis.

We employ detailed information on high school friendship relations available from respondents to the Wisconsin Longitudinal Study (WLS). Respondents were asked to report names of up to three best friends from their senior class in high school. We use this information to represent each high school class as a "directed friendship network" where a link from student i to classmate j is established whenever the former claims friendship to the latter. The fact that connections are directed leads to a conceptual distinction: by sponsoring a tie of friendship, a student reveals his affection towards the recipient of the claim, while by receiving a claim of friendship a student is the object of social approval.

Following Burt [1976] and Wasserman and Faust [1994], we treat the position of individuals in their network as a well defined "set of relations" to and from each actor in the system and construct a typology of four positions (or roles) according to the similarity of their ties: *isolates*, *sycophants*, *brokers* and *receivers*. Isolates are individuals who deliberately do not promote ties and who do not receive any social approval either. The position of the sycophant reflects the idea of a person trying to tag along while the attempt to being socially connected is not reciprocated by others. The mirror image of the sycophant is the receiver, also often referred to as occupying a "primary position" in the network of relationships. Receivers represent, somewhat oversimplified, the prototype of socially prestigious actors as they receive social approval without the need to engage in friendship pacts with other classmates. Those who receive and at the same time reciprocate by means of promoting friendship ties are classified as brokers. Brokers are also often referred to as "ordinaries" as they represent the most common position in groups.

These network positions, though highly stylized, relate to a number of individual attributes and behavioral outcomes. Specifically, they appear to be largely orthogonal to measures of cognitive ability, but they do relate to social participation outcomes in a way we would have expected on the base of our discussion in the previous paragraph. To give some examples, those who were socially isolated during high school time are less frequently married, less involved in organizations, have fewer contacts with friends and relatives, and rely less often on informal contacts in their job search, 35 years later!

The main focus of our empirical analysis lies on the estimation of wage differentials across types. We find that differential social standing in adolescence predicts significant and large earnings inequalities over the adult life course. Two results merit special consideration: students who were socially isolated within their school class earn between 43 and 25 percent less than average 35 years later, depending on the set of controls entered. Considering the opposite end of the social spectrum, we find that receiver types earn a wage premium of 33 to 26 percent compared to an average individual, again, conditioning on different sets of covariates. The estimated wage premia and penalties do not appear to be substantially confounded by measures of family and school resources, and materialize largely independent of differences in cognitive abilities, grade rank in class, personality traits or friends' characteristics. We do find, though, that a moderate share of the earnings inequalities is mediated by differential post-secondary human and social capital investment.

We are certainly not the first to recognize that behavioral skills form an important subcomponent of human capital and that schools play a central role in developing such skills [Bowles, Gintis and Osborne 2001]. In fact, there is a growing number of studies on the role of "noncognitive skills" that document how pre-labor market measures of motivation, social adaptability and interpersonal skills help explaining adult socioeconomic outcomes (e.g. Heckman, Stixrud and Urzua [2004]). Another recent strand in the literature prefers to study the social component of human capital under the heading of "individual social capital" [Glaeser, Laibson and Sacerdote 2002]. Whichever generic term one may favor, both serve as a catchall for acquired behavioral skills, socialized norms of conduct, as well as inborn pro-social character traits. In practice, these individual attributes are lumped together as they are empirically indistinguishable.

Our measures capture the outcomes of a differentiation process during secondary school in terms of the "social status" achieved by individuals relative to other individuals within the same class. To be precise, by school class we mean the set of classes participated in by the same grade cohort of students in any given school. This broader interpretation is appropriate here since students tend to specialize in elective subjects towards the end of secondary school. Members of the class in one subject need not be the same as in another. Individuals have been systematically exposed to association with different people across various contexts ranging from mathematics class to organized athletics and extracurricular activities. This implies a considerable reshuffling of friendships in which students have drifted into new and out of old relationships over the years. Since association is a choice, the final position in which an individual is observed is highly informative for identifying different types of personalities and behaviors. Drawing again on Parsons [1959], we would like to spend a few more words on what we think it is that adolescents acquire by social interaction with classmates, and why it should matter for their subsequent economic attainment.

The psychological function of social interaction with classmates is that it provides a testing field for gaining acceptance from age-peers, that is from "status-equals". The degree to which an individual is accepted by his peers is related to the extent to which he is able to make positive personal and social adjustments. During secondary school, a social differentiation process takes hold that gradually breaks up the individual's initial fixation on "generation-superiors" such as parents, the class teacher from elementary school and other significant adults. The new reference system is largely independent of adult supervision and approval. Individuals come to occupy differentiated positions within the group as an immediate consequence of their own interpersonal behavior and of what others consider appropriate conduct. An individual's social status is inevitably a direct function of the position he achieves within the school class and this position enters into the definition of his own identity.[1] Large parts of an individual's role performance when adult, as an employee in a team of co-workers for example, will also be in association with status-equals or near-equals. Therefore, it is social interaction within the group of classmates that provides a bridge to the adult world in terms of acquired social skills and norms for interpersonal behavior.

In summary, using citations as a measure of the interest of citors towards citees, we group individuals into equivalent positions across school classes. The suggested typology is certainly a crude and highly stylized description of individual differences in social capital. And yet, it proves to be a very informative one when it comes to explaining individuals' differential success in the labor market. From a conceptual point of view, we contribute an application of egocentered network methods within conventional labor economic survey research. The remainder of the paper is organized as follows: Section II introduces some elementary concepts of social network analysis, defines our measures and describes the relational data at hand. Section III examines whether in our sample there are significant associations between sociometric position in school class and adult wages. Section IV analyzes the earnings premia and penalties in relation to differential school resources, family background, cognitive ability, grade rank, personality traits and peer characteristics. Moreover, we try to shed some light on possible channels through which the observed earnings inequalities might have evolved, such as social capital accumulation, post-secondary schooling and occupational sorting. Section V concludes with some thoughts on the policy relevance of our findings, a discussion of potential limitations, and directions for future research.

[1] In a study of Illinois high schools, Coleman [1961] finds that students identify themselves as belonging to social categories such as nerds, jocks, leading crowd and others. Students tend to differentiate themselves along two major dimensions: 'cognitive achievement' as measured by grades, and 'social approval' as reflected in leadership roles in extracurricular student activities and participation in high school athletics. See Akerlof and Kranton [2002] on "Identity and Schooling" for a review and economic interpretation of the sociological literature on education.

II Social Network Analysis: Method, Data and Measures

A The Egocentered Network Method

The network approach conceives the social system as a set of individuals (nodes) and patterns of well specified relations (ties) joining individual members. Network analysis may then be conducted on two levels: the individual "actor level" or the overall "structural level". With the latter, interest usually centers on concepts and measures pertaining to the entire network of relationships, such as its density or connectedness. On an actor level, one would typically be interested in quantifying the popularity, influence or sociometric position of an individual within a given network. Depending on the level of analysis, both, the location of individual actors within the network, as well as the structural properties of the whole social system may then be related to economic outcomes of interest. On the structural level, one prominent example would be the work by Granovetter [1974] on job contact networks in which he relates social structures to market performance. On the actor level, the discussion in Akerlof and Kranton [2002] on identity and schooling is exemplary as it is concerned with group dynamics within the school class context.

Clearly, the chosen level of analysis has implications for the kind of network methods that are appropriate as well as for the type of data required for empirical analysis. If we desire to analyze the structural properties of a network, we need to gather "complete network data": information on all ties linking elements of a closed population. This implies that individual units of observation are not sampled by some standard probabilistic method as in conventional survey designs. Gathering full information on relationships among, say, all inhabitants of a small village may still be feasible, with potential limitations on the inferential side though. As the population of interest widens, data collection efforts may become prohibitively complex and expensive for all practical purposes.

However, when interest centers on the individual actors within the net of relationships, full network data might not be required. In many instances, one may resort to "egocentered network data": information on sets of ties surrounding sampled nodes.[2] In the present work, for instance, we consider a random sample from a population of high school seniors and ask them to report who are their close friends in class. Data like this does not allow us to map the full network and analyze its structural properties in great detail. But, the data is still relational in character and we can find out that some have close friends while others have none. Knowing this, we are able to understand something about differences in the actors' position in their (local) social structure and can relate these varying positions to variation in economic outcomes. This is the approach taken in our paper.

B High School Friendship Relations in the WLS

We employ detailed information on high school friendship relations available from respondents to the Wisconsin Longitudinal Study (WLS). The WLS consists of 10,317 randomly sampled individuals who graduated from Wisconsin high schools in 1957. Together, these individuals constitute approximately one-third of all seniors in Wisconsin high schools in that year. After the initial wave of data collection, primary respondents were re-interviewed in 1975 and 1992. Together with their parents' interview of 1964, these waves provide information on, among others, socio-economic background, mental ability, educational attainment, family formation, personality traits and labor market histories. The original sample is broadly representative of white men and women who have completed at least twelve years of schooling. For more detailed information on the WLS be referred to Sewell et al. [2001] and the references therein.

In the 1975 wave, respondents were asked to report names of up to three best friends from their senior class in high school. The survey design of the WLS bounds the set of nodes by school-class membership: claims of friendship can be done only among students who belonged to the same school and class. Relational quantification is based on individual evaluation: student i has a tie with student j if and only if i claims his friendship to j. Two things are worth noting in this context. The first is that we are considering *dichotomous* relations, either a relation exists or it does not, while the strength of the relation is not defined. The sec-

[2]For an exhaustive discussion of sampling methods in a network context and the analysis of survey network data, see Marsden [1990].

ond is that our relations are *directed*, that is, if i claims friendship to j, the reverse is not necessarily true. This leads to a conceptual distinction between a student who receives a claim of friendship and a student who sponsors a claim of friendship. The former is socially approved, while the latter shows general friendliness towards the recipient. There are further three remarks related to measurement of friendship ties in the WLS.

First, the questionnaire is a combination of *free recall*, respondents write down names, and retrospective *fixed choice*, they may nominate at most three friends belonging to their class. The implication of this design for possible measurement error in observed friendship ties is limited. Indeed, the free recall and the retrospective fixed choice design force the responder to remember the identity of his (or her) friends at high school and to select at most three of them. This assures that claims of friendship are towards individuals with whom the respondent experienced stable patterns of interaction.[3] Second, the information gathered is subjective in nature: the social relation under analysis, friendship, is perceived independently by the parties involved. Third, due to random sampling of nodes, we do not have a full description of all relations among students in any given class. We do observe the full set of ties sponsored by sample members towards individuals both, in- and outside the sample. But, claims of friendship coming from classmates who have not participated in the WLS towards sample members are missing. Moreover, individuals were not sampled randomly on a class level, but sampling occurs at the aggregate level. This implies that even if the size of the sampled classes would on average be one-third of the original, there will be classes with higher and lower proportions of students.

Obviously, the value of our analysis hinges on whether the rules for including and excluding nodes are sensible in the sense of generating indicators that are not artifacts of those rules [see Marsden 1990]. After having introduced our measures, we will return to this issue and address

[3]We recognize that forcing individuals to make a fixed number of choices distorts some measurements of friendships, but the alternative of allowing each individuals to make *any* number of choices also distorts the measurement of friendships. Given the conceptual ambiguity of the meaning of friends (for the students themselves), the fixed choice design provides information that is at least as reasonable and reliable as any other method. For a discussion see Feld and Elmore [1982]. Also, the retrospective setup has to be evaluated against the alternative of interviewing students while still being at high school. It is not clear that an earlier timing would improve the validity of the choices since students may tend to include unimportant relationships and borderline acquaintances, mention those who at the time of the interview sit close to them in class, or be otherwise influenced in their choice.

in more detail the consequences of sampling portions of a school class network.

C In-degree, Out-degree and Network Positions

We represent each of the school classes in our data as a directed network, g, where a link from student i to student j is established whenever the former claims friendship to the latter. We denote a link from i to j as $g_{i,j} = 1$, while $g_{i,j} = 0$ means that student i does not claim friendship to j. Receiving and sponsoring links may be formalized using two graph-theoretical notions: the *In-degree* and the *Out-degree*. Formally, the Out-degree of student i, denoted as y_i, is the number of claims of friendship he or she sponsors, that is $y_i = \sum_j g_{i,j}$. The In-degree of student i, denoted as x_i, is the number of claims student i receives from others, that is $x_i = \sum_j g_{j,i}$. Thus, each actor i is characterized by a bidimensional social vector $e_i = (x_i, y_i)$ and network positions can be constructed by combining the characteristics of such social vectors across actors. With the relational data at hand, this approach allows us to partition the set of sample members into subgroups of people who have the same position within their respective network.[4] Following Burt [1976] and Wasserman and Faust [1994], we define the position of a student in a given school class in the following way:

A student i in a directed network g is: (i) an Isolate if $x_i = y_i = 0$; (ii) a Sycophant if $x_i = 0$ and $y_i > 0$; (iii) a Receiver if $x_i > 0$ and $y_i = 0$ and (iv) a Broker if $x_i > 0$ and $y_i > 0$.

In Figure 1 we exemplify the network positions of students in a fictitious high school class, with numbered circles representing students and a link from i to j is represented by a line with an arrow pointing to j.

[4] We have experimented with more complex measures of social status such as “Proximity” and “Power-indices”. For an overview and definition of those and alternative measures see Wasserman and Faust [1994]. These indices take into account indirect ties and they may also distinguish between reciprocated and non-reciprocated ties. Our choice to consider the most basic definition of network positions is due to the imperfection of our data. As already noted, we miss information on claims of friendship coming from individuals outside the sample but within the same class. Further, the survey design constrains each respondent to make at most three claims of friendship. The consideration of indirect and reciprocated ties would only amplify the issue of measurement error in our data. By contrast, the network positions we construct allow us to pin down the nature of the misclassification across positions, yet maintaining their relational nature. For an extensive discussion of different “notions of position” and their applicability in several areas of social network analysis, see Borgatti and Everett [1992].

The *isolate* position is occupied by a set of students $\{13,14,15\}$ who are neither promoting nor receiving citations from other individuals in their network.[5] In network terminology, an isolate is "infinitely distant" from other individuals. A student in the *sycophant* position $\{4,11,16,19\}$ reflects the idea of a person trying to tag along while the attempt to be socially connected is not reciprocated by others. The mirror image of the sycophant is the *receiver*, also often referred to as being in "primary position". Receivers $\{3,7,17,18\}$ represent, somewhat oversimplified, the prototype of socially prestigious actors. They are those leader-type of individuals who receive social approval without the need to promote any ties on their own part. Those who receive approval and are reciprocating in the sense of promoting friendship ties on their own are classified as *brokers*.[6] Brokers $\{1,2,5,6,8,9,10,12\}$ are also commonly referred to as "ordinaries" as they represent the most frequently occupied position.

D Measurement

For the construction of the above measures we use information of 9,138 respondents to the 1975 questionnaire who provided names of their best friends in 1957. We use all respondents and the corresponding social relations irrespective of individuals' characteristics such as gender, religion or race. This allows us to exploit all the relational information available in our data.[7] Once the relational measures are constructed, we may treat them as personal attributes and restrict our attention to any subsample for further empirical analysis. In order to abstract from gender and discrimination issues in labor force participation and wage determination, we restrict our attention to 2,514 full-time employed males for whom we have information on adult earnings and control variables.

[5] Following Barry [1998] and Postlewaite and Silverman [2004], we find it worthwhile to distinguish between social isolation and social exclusion. Social isolation refers to voluntary non-participation while social exclusion is due to circumstances that are beyond an individual's control such as ethnicity or race.

[6] Throughout the paper we use the term reciprocation in a very broad sense. Ties need not be directed towards those from whom a claim was received.

[7] In this context, we would like to emphasize that our focus does not lie on explaining "who links with whom" in terms of characteristics as in Marmaros and Sacerdote [2003] or Alesina and La Ferrara [2002]. This is certainly not for lack of interest in the research question. It is the imperfection in our data -the substantial number of missing nodes and ties in each class- that denies us to get a better handle on this issue.

In column 1 of table 1, we report descriptive statistics of all sociometric measures. As mentioned before, we do not observe claims of friendship coming from individuals outside the sample, but within the same school class, towards WLS members. This makes the observed in-degree index, and therefore the network typology we construct, subject to systematic measurement error. Figure 1 illustrates the implications of the sampling scheme for measurement. Students interviewed by the WLS are drawn inside the large hatched circle, and, broken lines and dotted circles stand for the unobserved ties and nodes. It is readily seen that true receivers may be misclassified as isolates and true brokers as sycophants. True isolates and true sycophants will always be correctly classified, that is, observed as such. Since the observed distribution of types is multinomial, measurement errors are functions of the true values and correction methods based on classical errors-in-variables (CEV) models do not apply directly. However, to the extent that we are able to identify the functional relationship between errors and true values, we are in the position to transform the observed distribution of types such as to make it conform to CEV assumptions.

Our correction method is based on a measurement error model for multinomial random variables [Fuller 1987], detailed in the appendix. The measurement errors are represented by a matrix, each element of which defines the probability that a student whose true type is j, is wrongly assigned to category i. We derive these misclassification probabilities in two steps. In the first step, we estimate the (mis)classification that would occur once the links that students within the sample sponsor towards students outside the sample were ignored. In the second step, we show that the misclassification induced by the limitations of our data is symmetric in nature to the one which we estimated in the previous step. This allows us to derive the functional relationship between the mean of the error variable and the mean of the true variable and to impose the correction matrix in estimation of the earnings effects.

Column 2 of table 1 presents summary statistics for the corrected measures. Comparing column 1 with column 2, we note a number of differences between the observed and corrected classifications. First of all, sycophants and isolates were over-represented while broker and receiver categories were under-represented. Moreover, it is reassuring to see that after the correction brokers (ordinaries) constitute the most

TABLE 1

DISTRIBUTION OF OUT-DEGREE, IN-DEGREE, NETWORK POSITIONS AND THEIR RELATION TO CLASS SIZE AND FRACTION OF CLASS SAMPLED

	Observed Proportions	Corrected for Misclassification	Class Size %Δ *to average*	Fraction Sampled %Δ *to average*
		A. Out-degree		
zero	.099	.099	.235	– .030
			(.055)	(.010)
positive	.901	.901	– .026	.003
			(.018)	(.003)
		B. In-degree		
zero	.603	.443	.026	– .010
			(.032)	(.006)
positive	.397	.557	– .021	.008
			(.027)	(.005)
		C. Network Position		
Isolate	.080	.056	.346	– .030
			(.102)	(.019)
Sycophant	.523	.387	– .025	– .006
			(.034)	(.006)
Broker	.377	.514	– .027	.010
			(.028)	(.005)
Receiver	.020	.043	.092	– .031
			(.124)	(.024)

NOTE.— The sample consists of 2,514 full-time employed (white) male workers in the WLS for whom we have information on friendship ties, hourly wages at age 53 and covariates (see Table 2 for descriptive statistics).

frequent type. Isolate and receiver categories are about equal in size and form the two smallest categories.

In column 3 of table 1, we analyze the relation between our corrected sociometric measures and class size. To this end, we calculate mean class size by network position and express it in percentage deviation from the overall mean. Panel A shows that a randomly chosen student with zero out-degree belongs to a school class 23.5 percent larger than the average class. In contrast, the in-degree index does not appear to be related to class size at all (panel B). For the network typology, we find that being isolated corresponds to being in a class 34.6 percent larger than average, while the remaining three categories are not significantly related to size (panel C). These results are interesting for two reasons. Firstly, the relation of social isolation with class size is driven by the out-degree index, which is the one we observe without error. Secondly, finding that the incidence of social isolation is higher in larger classes is in line with existing evidence on social participation and school size. For example, Postlewaite and Silverman [2004] report that the rate of participation in high school athletics is decreasing in the population of the school. Zero out-degree and being isolated are two alternative indicators for a lack of initiative to participate and interact socially. The fact that we reach similar conclusions with alternative measures supports the view that our variables are sensible proxies for individual social skills.[8]

We finally investigate possible relations between our sociometric measures and the fraction of students sampled on a class level. If the measures were mere artifacts of the rules for including and excluding nodes, we should find sizeable correlations with the fraction of a class sampled. Column 4 of table 1 shows that such effects are virtually absent. Moreover, when later estimating the effects of our measures on earnings, we shall always include dummy variables for each school. Since the WLS interviewed students in their final year of high school, that is one class per school, the two notions (school and class) coincide

[8]At first sight, isolation and class size being positively related appears counterintuitive. From a statistical point of view, one would expect the converse: given that individuals match on characteristics, increased class size increases the likelihood that a given student finds another person being sufficiently self-similar. On the other hand, there may be a number of offsetting factors about which we can only speculate: One possible explanation could be that teachers of larger classes adopt pedagogical practices that inhibit social interaction among students to stay in control. Another potential explanation relates to the way students interact among themselves. Larger classes may actually be more "anonymous" due to clique formation and segregation of students into disconnected subgroups [Hallinan and Smith 1989].

and shall be used interchangeably throughout. Controlling for school fixed effects absorbs anything that is constant among classmates but varies across school classes. Therefore, we also automatically account for differences in class size or proportion of students sampled. There is another compelling reason for the inclusion of fixed effects. Our measures refer to the social rank of an individual *within* a particular class. Comparison of such relative measures are meaningless across classes. However, in regressions that include fixed effects, they capture differences in social standing within the same high school class.

E Descriptive Statistics

In this subsection we briefly discuss how our measures of adolescent social standing relate to a number of individual attributes and socioeconomic outcomes (see table 2). Our first observation is that network positions are largely orthogonal to measures of cognitive ability. In contrast, they do relate to measures of adult social participation. For example, students classified as receivers and brokers are those who are most actively involved in formal organizations. Brokers, together with sycophants, are also those who happen to have most frequent contact with their (current) friends and relatives. Interestingly, we also find that social rank in school class is related to marriage decisions as well as the job search method used, 35 years later. Students classified as receivers are more likely to be married when adult, followed by brokers, sycophants and isolates. Similarly, receivers are those who rely more extensively on informal contacts in their job search as compared to brokers, sycophants and isolates. In addition, we note that network positions appear to be related to individuals' personality characteristics. As one might have expected, social standing associates positively with character traits such as extroversion and agreeableness. Overall, these summary statistics support our heuristic interpretation of the network positions as proxies for interpersonal skills and social competencies. In the remainder of the paper we shall focus on the estimation of wage differentials across types.

TABLE 2

SUMMARY STATISTICS OF SELECTED CONTROL VARIABLES

Variable	Mean	(*Std. Dev.*)	Isolate	Sycophant	Broker	Receiver
			by Network Position:			
IQ-score	101.94	(*15.14*)	99.40	100.01	103.82	100.13
No. of memberships in org. (*log*)	.936	(*.715*)	.680	.887	.997	.972
Freq. of contact with friends (*log*)	.992	(*.780*)	.879	.962	1.045	.786
Freq. of contact with relatives (*log*)	.913	(*.742*)	.852	.895	.947	.746
Married	.867	(*.340*)	.801	.848	.884	.917
Job found via informal contact	.408	(*.492*)	.267	.403	.425	.438
Personality traits:						
extroversion	3.779	(*.821*)	3.590	3.750	3.805	3.954
agreeableness	4.633	(*.679*)	4.439	4.639	4.649	4.657

NOTE.— The sample consists of 2,514 full-time employed male workers, WLS, at age 53. Estimated means by network position are corrected for misclassification error.

III Network Position in School Class and Adult Earnings

We start off by examining whether in our data there are significant correlations between social position in school class and wages earned later in life. We are able to measure wages at a relatively advanced age and thus capture the cumulative effects of differences in network position that have materialized over the entire life course.

Table 3 compares deviations from mean log wages by out-degree, in-degree and network position. We consider male workers only and exclude those who are self-employed, work less than 20 hours per week, and earn less than one dollar per hour. We find that having zero out-degree leads to a marginally significant 6.9 percent pay penalty while having positive out-degree is not associated with any significant pay difference compared to the average (col. 1). The results for the in-degree measure are distinctly sharper. Not receiving any social approval from class-mates, as reflected by zero in-degree, leads to a statistically significant 11.4 percent penalty while possessing positive in-degree receives a significant premium of 9.1 percent relative to the mean (col. 2). This amounts to a pay difference of more than 20 percent between individuals who were not mentioned at all and those who received claims of friendship. It is interesting to note that this difference is driven to largely equal extents by the penalty associated with zero in-degree and the reward for having positive in-degree. Overall, we find that receiving (not receiving) has much stronger effects on wages than sending (not sending).

Considering network positions, our primary variables of interest, it is the harsh penalty for the isolate that immediately strikes the eye (col. 3). Social isolation, in the sense of a voluntary choice of not promoting ties and simultaneously not receiving approval from others, is associated with more than 40 percent lower average wages. Of course, the definition of isolation employed here is a highly stylized one and these strong effects apply to a fraction of workers of approximately 6 percent in our sample. And yet, on an individual level, the economic consequences of social isolation during adolescence appear to be substantial. Turning to the other types, we find that the wage of sycophants is 9 percent lower than average. Apparently, individuals who cited others

TABLE 3

OLS ESTIMATES ln(Wage) EQUATION FOR ADULT, MALE WORKERS, WLS, AT AGE 53

Covariate	(1)	(2)	(3)	(4)	(5)
Out-degree 0	– .069				
	(.039)				
Out-degree >0	.008				
	(.012)				
In-degree 0		– .114			
		(.027)			
In-degree >0		.091			
		(.022)			
Network Position					
Isolate			– .426	– .416	– .417
			(.110)	(.109)	(.102)
Sycophant			– .090	– .091	– .089
			(.026)	(.026)	(.028)
Broker			.086	.085	.085
			(.021)	(.021)	(.022)
Receiver			.330	.327	.311
			(.139)	(.138)	(.134)
Observations	2,514	2,514	2,514	2,413	2,364
Adjusted R^2	.043	.063	.077	.077	.070
F-statistic typology			26.31	25.07	21.44

NOTE.— Standard errors are in parentheses. See the note to Table 1. Log hourly wages and controls are in mean-deviation form; the constant term is suppressed. All specifications include a set of dummy variables for each high school class; one reference category omitted. Column (4) excludes individuals in small classes of five students and less. Column (5) excludes all those who were in classes of which less than twenty percent has been sampled.

without having their choices reciprocated fare much better than isolates, but still earn below average. Individuals who acted as brokers in their respective school class earn 8.6 percent more than the average. Individuals who maintained a primary position -receiving unreciprocated ties- in their class network, earn 33 percent more than average 35 years later. As for the isolates, this large premium applies to only a small fraction of workers in our sample; only 4-5 percent are classified as receivers.

In summary, we observe a clear ordering in terms of the wage premia and penalties. The higher the social approval or the more prestigious the position of an actor, the higher the reward. Those in broker and receiver positions receive a wage premium while sycophants and isolates are being penalized by the market. All specifications control for school fixed effects and thereby absorb any differences in class size or fraction of students sampled. Still, to the extent that network positions depend on relational information available on a class level, the estimated mean log wages for the various network positions might be unduly influenced by outlying observations. These concerns relate specifically to subsets of individuals in small classes or classes of which only a tiny fraction is represented in the data. Columns 4 and 5 provide an informal check on model sensitivity to outliers by deleting small portions of our reference sample. In the specification of column 4 we omit individuals in small classes of five sampled students and less. In column 5 we estimate the model omitting those who were in classes of which less than twenty percent of the students were interviewed. Note that our earlier estimates appear to be largely insensitive to the exclusion of these observations. We therefore decide to work with the earlier, and larger, sample of 2,514 observations in all subsequent analyses.

IV Explaining the Earnings Premia and Penalties

A An Effect of Family and School Characteristics?

In this and the following two subsections our primary focus will be on purging the estimates of the influence of possible confounding factors. In particular, we move on to account for a relationship between the social standing of an individual in his school class and aspects of family background. Growing up in families with less human and financial capital may lead to stigmatization by class mates and possibly to social isolation. Similarly, being the son of affluent parents may lead to many claims of friendship received, for reasons other than a genuine capability of gathering a large number of affiliates.

Compared to our first, basic, specification (col. 3 of table 3), we introduce arrays of dummy variables for father's and mother's level of education and type of occupation, as well as a continuous measure of respondents' number of siblings. Parents' education and type of occupation should proxy for family resources like wealth and the sibling count controls for the possibility of intersibling competition for scarce family resources. Adding a vector of controls for differences in family characteristics (col. 1 of table 4; detailed estimates omitted) reduces the coefficients for isolates and receivers by approximately 5 percentage points each. The estimates for the sycophant and broker categories are only marginally affected. In relative terms, about 10 percent of the earnings disparities associated with the various positions appear to work through observable differences in family background. The estimation results show that the sociometric measures impact strongly on wages and operate largely independent of family resources and school characteristics.

One may argue that there are unobserved family resources that we are omitting due to the imperfect quality of our measures. It is clearly impossible to control for such unobservable variables. However, inference may be still be drawn about their effects to the extent that these variables are correlated with choice of school. It is plausible that differences in unobserved family-specific characteristics affect the type of school an individual attends. High social-class parents will decide to reside in certain neighborhoods, and thereby school districts, or may even afford their child private school education. However, these potential effects are accounted for by the inclusion of dummy variables for each school class across all specifications. Indirect evidence therefore suggests that the estimated pay differences should not primarily reflect omitted, unobserved family resources. Note, that we also implicitly control away any systematic differences in (unobserved) measures of school quality such as student-teacher ratio, denominational control, gender composition or racial heterogeneity.

TABLE 4

OLS ESTIMATES ln(Wage) EQUATION FOR ADULT, MALE WORKERS, WLS, AT AGE 53 CONTROLLING FOR FAMILY BACKGROUND, COGNITIVE ABILITIES, BIG FIVE PERSONALITY TRAITS, AND PEER CHARACTERISTICS

Covariate	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Network Position							
Isolate	– .378	– .326	– .318	– .311	– .400	– .354	– .317
	(.110)	(.106)	(.110)	(.107)	(.111)	(.111)	(.109)
Sycophant	– .083	– .068	– .073	– .066	– .088	– .080	– .068
	(.026)	(.025)	(.026)	(.025)	(.026)	(.026)	(.025)
Broker	.079	.061	.066	.058	.085	.074	.061
	(.021)	(.021)	(.021)	(.021)	(.021)	(.021)	(.020)
Receiver	.289	.304	.280	.299	.288	.291	.298
	(.139)	(.134)	(.139)	(.134)	(.138)	(.138)	(.134)
Cognitive abilities							
IQ-score		.164		.142			.129
		(.012)		(.014)			(.015)
Class rank (*log*)			.116	.042			.038
			(.013)	(.015)			(.015)
Personality traits							
extroversion					– .014		.008
					(.012)		(.012)
agreeableness					– .057		– .046
					(.013)		(.013)
Respondent's friends							
planning college						.076	.050
						(.022)	(.021)
getting jobs						.019	.017
						(.022)	(.021)
military service						.007	.019
						(.019)	(.018)
Observations	2,514	2,514	2,514	2,514	2,514	2,514	2,514
Adjusted R^2	.109	.177	.142	.180	.131	.118	.191
F-statistic typology	21.44	17.20	16.48	15.97	24.09	19.52	16.47

NOTE.— Standard errors are in parentheses. Log hourly wages and controls are in mean-deviation form; the constant term is suppressed. All specifications control for high school fixed effects and include sets of indicator variables for parental education and occupation as well as continuous controls for the number of siblings (results omitted). See Table 2 for summary statistics.

B A Proxy for Intelligence or Grade Rank in Class?

Another potential reason for disparities in the average adult outcomes across network positions may be correlation with some productive unobservable related to ability. The WLS has a comprehensive measure of adolescent cognitive ability collected in the initial survey year. Adding standardized scores on the *Henmon-Nelson Test of Mental Ability* to the previous specification as proxy for individual differences in intelligence reduces our estimates further (col. 2 of table 4). The coefficient for isolates drops by another 5 percentage points, while the estimates for the other types are only marginally altered. Despite this reduction, an isolate still faces a significant and very large penalty of 32.6 percent compared to the wage of an average individual. Compared to our first, basic, specification (col. 3 of table 3), this indicates that roughly 80 percent of the adverse effects of adolescent social isolation work independently of family background, school characteristics and cognitive ability.

One may object that while cognitive ability may be accurately observed by the econometrician, it is not by classmates. And, since our relational measures are based on social interaction, conditioning on alternative measures of ability that are readily observable to the individuals under study might be more sensible. Suppose now that our sociometric measures were proxying for a student's relative position in terms of grades within his class. The WLS reports rank in high school graduating class by percentiles. Comparisons of this variable across high schools -much alike our network positions- are meaningless. However, in regressions that include high school fixed effects, it captures differences in academic performance within the same high school class. Accounting for differences in class rank (col. 3 of table 4) has very similar effects to conditioning on IQ-scores. Moreover, introducing both measures simultaneously (col. 4) shows that class rank adds very little to explaining the estimated penalties and premia beyond the specification with only IQ-scores. We interpret these results as to suggest that a substantial portion of the pay differences is not due to selection on cognitive traits. Rather, our results indicate that social rank in school class may have an economically substantial direct influence on later wages.

C An Effect of Personality Traits?

From the point of view of social psychology, it is well-recognized that interpersonal behavior is shaped to a significant degree by the personal dispositions of the individuals involved. Many research studies have shown that persons with low social acceptability among peers are generally characterized as shy, withdrawing individuals or as noisy, rebellious, socially ineffective persons. Our measures of interpersonal associations may therefore simply reflect differences in inborn personality traits.

The personality inventory of the WLS affords us a comprehensive set of variables to address this issue. Specifically, we have measures of respondents' character dispositions based on the Five-Factor Model (FFM) of personality structure [Costa and McCrae 1992, Goldberg 1990]. According to the FFM, five independent categories are sufficient to describe individual personality differences at the broadest level of abstraction: extroversion, agreeableness, conscientiousness, neuroticism and openness to experience. FFM traits are shown to have a statistically significant and economically important impact on wages [Mueller and Plug, 2004]. The major drawback of these measures is that they are assessed at age 53, that is, simultaneously with wages. Personality traits have a strong genetic basis, second only to measured intelligence, and are stable and enduring individual predispositions [e.g. Bouchard and Loehlin 2001, McCrae and Costa 1999]. We therefore use the available measures of adult personality as proxy variables for unobserved childhood personality. Since we are not interested in the effects of personality traits on earnings *per se*, this proxy variable solution may still shed some light on the relative importance of personality traits vis-*à*-vis our measures of social standing. And, if anything, this approach overestimates the role of personal character dispositions as compared to acquired social skills.

Adding FFM personality traits to the baseline regression model (col. 2 of table 4) leaves the estimated earnings premia and penalties for sociometric position largely unaffected. We condition on all five traits, but report coefficients only for those traits that comprise facets of social behavior, the extroversion-introversion and agreeableness-antagonism dimension (see also table 2). Our interpretation of the evidence is that there are valuable skills acquired through interaction with classmates and that subsequent earnings disparities do not primarily reflect selection on predetermined personality traits.

D An Effect of Friends' Characteristics?

Controlling for high school fixed effects absorbs anything that is constant among classmates but varies across school classes. We thereby purge our estimates of the possible confounding influences of peer characteristics at a fairly general contextual level. This means that we implicitly account for variables such as peers' average IQ, the percentage of classmates that is planning college, and peers' household characteristics such as average parental income and education. Having said this, one may argue that we are ignoring a contextual level that is more influential than classmates. This second level of context refers to characteristics of the subset of classmates who are being considered friends by the individuals in our sample. The most natural approach would of course be to take averages over characteristics of those classmates who were mentioned as friends by our primary respondents. Due to the missing data issue we opt for a different route.

The WLS allows us to partial out these effects to some extent through detailed information on an alternative question asking "what most of respondent's friends in 1957 were going to do: attending college, getting jobs, going into military service, or doing something else". Column 6 of table 4 presents results from a regression in which we add to our current reference specification (col. 1) a set of indicator variables for what respondent's friends were doing. The coefficients should be interpreted as estimated payoffs relative to an omitted reference category in which we pooled all those who were doing something else or had missing information. We find that friends' characteristics, mainly through the decision to attend college, have an independent positive impact on respondents' wages. However, compared to column 1, the effect of controlling for friends' characteristics on the estimated pay differences is negligible. This finding is consistent with the fact that social ties tend to occur among persons with similar attributes. Conditioning on respondents' own attributes is likely to capture most of these potentially confounding influences.

Column 7 table 4 summarizes the first part of our analysis. By adding simultaneously to the baseline specification all control variables for family background, cognitive abilities, personality traits and peer characteristics, our results indicate that the estimated wage differentials across types are not sensitive to holding constant these observables.

E Does it Work through Social Capital Accumulation?

So far, we did not control for variables such as postsecondary schooling, marital status, or occupation which are choice variables and therefore endogenous. Instead, we looked at reduced-form wage equations that conditioned on variables determined before post-secondary education and pre-labor market: school resources, family background, cognitive ability, grade rank and friends' characteristics.

Here, we depart from that route and start investigating the channels through which position in school class influences later wages. A channel that is likely to be of great importance is the accumulation of social capital in the course of one's labor market career. Joining a social network may be one of the most common forms of social capital investment. These networks could be anything ranging from labor unions, political clubs and hobby groups to broad classes of individuals with a common social characteristic such as the same nationality. In all cases, organizational participation diminishes social distance between the individual and some social group. This leads to information flows, which usually serve both the investor and the other members of the network.

The empirical work on social capital often uses survey responses about the number of organization or group memberships and the frequency of contact with friends and family members as proxies for social capital [Glaeser et al. 2002, Durkin 2000]. The WLS asked respondents about the extent of their social participation in a variety of different groups, associations, clubs and organizations. The data set also contains detailed information on frequency of contact with friends and relatives. We are therefore in the fortunate position to be able to disentangle how much of the effects of adolescent social capital on market outcomes work through *current* social participation.

First, we construct a variable representing the total number of group memberships. This variable is based on a simple count of memberships, ignoring the intensity of participation. In forming this measure, we exclude the subset of organizations with a strong consumption component.[9] This ensures that our measure properly reflects the current stock of an individual's social capital investments. Second, we derive an alternative measure that tries to capture variation in the extent to which people are active in the various groups. We simply weight the number of memberships by the intensity of participation.[10] As mentioned earlier, another very common set of proxy variables for social capital is related to the frequency of contact with friends and family members. The WLS collected information on how many times, if at all, during the past four weeks respondents have gotten together socially with friends and relatives, respectively.

Table 5 presents results from regressions in which we add to our set of explanatory variables various measures proxying for the current stock of individuals' social capital. All specifications control for school fixed effects, differences in family background and IQ, that is, those factors that have been shown to be of relevance in earlier specifications. Detailed results of this baseline model are presented again in column 1 for ease of comparison (c.f. col. 2 of table 4). Adding the log number of memberships (col. 2) reduces the coefficient for the isolates by 3.7 percentage points and the premia for receivers by 2.3 percentage points. The remaining two categories are only weakly affected, the estimates being reduced by less than half a percentage point. Using the alternative membership measure that weights participation by the intensity of involvement (col. 3) leaves this picture virtually unchanged. Our estimations also indicate that the wage disparity among types is certainly not due to current frequency of contacts with friends and relatives (cols. 4 and 5). The estimated effects are almost identical to what they were

[9]Our measures include any participation in church, church-connected groups, labor unions, veterans' organizations, business or civic groups, parent-teacher associations, community centers, organizations of people of same nationality, youth groups, professional groups, political clubs, neighborhood improvement organizations, charity and welfare organizations. We follow Glaeser et al. [2002] and exclude fraternal organizations and lodges, sport teams, country clubs, and hobby groups. If there was some missing data and some valid data on participation items, the missing data was counted as not being involved in that organization. If the entire social participation section had missing data codes, individuals were treated as not being involved. However, we included a flag for these observations in our regressions in order to distinguish them from non-participating respondents. This and all subsequent log transformations of participation counts are done as follows: $\log(n + 1)$. Of course, this is not a fully accurate treatment of non-participation, but it is an approximation that may be sufficient for our purposes.

[10]For intensity weighted participation, the WLS coded the degree of activity in the following way: no involvement (0), very little (1), some (2), quite a bit (3), a great deal of involvement (4).

TABLE 5

OLS ESTIMATES ln(Wage) EQUATION FOR ADULT, MALE WORKERS, WLS CONTROLLING FOR MEASURES OF SOCIAL PARTICIPATION AND CONTACTS AT AGE 53

Covariate	(1)	(2)	(3)	(4)	(5)	(6)
Network Position						
Isolate	– .326	– .289	– .289	– .325	– .325	– .289
	(.106)	(.107)	(.107)	(.106)	(.106)	(.107)
Sycophant	– .068	– .065	– .065	– .067	– .067	– .065
	(.025)	(.025)	(.025)	(.025)	(.025)	(.025)
Broker	.061	.057	.056	.058	.060	.056
	(.021)	(.020)	(.020)	(.020)	(.020)	(.020)
Receiver	.304	.281	.283	.322	.304	.282
	(.134)	(.134)	(.134)	(.134)	(.134)	(.135)
No. of memberships in org. (*log*)		.109				.101
		(.020)				(.021)
No. of memberships in org.‡(*log*)			.086			
			(.015)			
Freq. of contact with friends (*log*)				.060		.056
				(.017)		(.018)
Freq. of contact with relatives (*log*)					– .043	– .065
					(.018)	(.019)
Observations	2,514	2,514	2,514	2,514	2,514	2,514
Adjusted R^2	.177	.190	.191	.182	.180	.195
F-statistic typology	17.20	14.55	14.53	17.50	17.06	14.52

NOTE.— Standard errors are in parentheses. Log hourly wages and controls are in mean-deviation form; the constant term is suppressed. All specifications control for high school fixed effects and include indicator variables for parental education and occupation as well as continuous controls for the number of siblings and iq-scores (see Table 2 for descriptives).

‡ This variable weights the number of membership in organizations by the intensity of respondents' involvement.

in the absence of conditioning on contacts. In sum, this suggests that about 10 percent of the pay differences for receivers and isolates are mediated through their current stock/lack of social capital (col. 6).[11]

F Other Channels

The model underlying our empirical results views the network position youths maintain by their late teens as a predetermined initial condition that shapes the future path of human and social capital accumulation and, hence, wages. In this last subsection we look at an array of alternative channels like post-secondary schooling, marriage, job finding and type of occupation through which the earnings gaps might develop. Detailed regression results are presented in table 6. As before, all specifications control for school fixed effects, differences in family background and IQ; in column 1 we again provide the results of the baseline model for ease of comparison (c.f. col. 2 of table 4).

Post-Secondary Schooling.— The positive relationship between social capital and human capital variables is one of the most robust empirical regularities in the social capital literature; see for example, Helliwell and Putnam [1999]. One explanation for this connection is that schooling plays a central role in developing such skills [Bowles and Gintis, 2002]. Another possible explanation is that the marginal value of social participation is increasing in the level of individual human capital. Since our sample is based on a cohort of equal age individuals who all completed high school, differences in human capital accumulation are identified by differences in post-secondary educational attainment. Including the years of schooling completed as a control variable in our regression (col. 2 of table 6) shows that there are some complementarities between initial levels of social capital and subsequent human capital accumulation. Overall, the nature and magnitude of the effects is very similar to what we found when conditioning on social participation variables.

[11] Receivers, along with brokers, are those individuals who are most frequently involved in organizations. Interestingly though, receivers appear to have much less contact with friends and relatives than any other type (see also table 2). One reason might be that receivers -earning the highest wages- have a high opportunity cost of time and substitute away from family and friendship ties towards organizational memberships.

TABLE 6

OLS ESTIMATES ln(Wage) EQUATION FOR ADULT, MALE WORKERS, WLS CONTROLLING FOR OTHER OUTCOME MEASURES, AT AGE 53

Covariate	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Network Position							
Isolate	– .326	– .287	– .310	– .329	– .313	– .264	– .247
	(.106)	(.104)	(.106)	(.106)	(.105)	(.104)	(.105)
Sycophant	– .068	– .055	– .064	– .073	– .060	– .050	– .049
	(.025)	(.025)	(.025)	(.027)	(.025)	(.024)	(.024)
Broker	.061	.050	.057	.055	.053	.044	.042
	(.021)	(.020)	(.020)	(.023)	(.020)	(.020)	(.020)
Receiver	.304	.268	.294	.298	.304	.260	.255
	(.134)	(.130)	(.133)	(.135)	(.131)	(.129)	(.131)
Yrs. of schooling completed		.152				.129	.123
		(.014)				(.015)	(.015)
Married			.053			.054	.057
			(.011)			(.011)	(.011)
Job found via informal contact				.013		.018	.012
				(.024)		(.011)	(.011)
Dict. of Ocp. Titles people-score (*log*)					.108	.056	.050
					(.012)	(.013)	(.013)
Social participation controls							yes
Observations	2,514	2,514	2,514	2,514	2,514	2,514	2,514
Adjusted R^2	.177	.224	.185	.177	.205	.240	.24 9
F-statistic typology	17.20	13.42	15.67	17.01	15.81	11.52	10.57

NOTE.— Standard errors are in parentheses. Log hourly wages and controls are in mean-deviation form; the constant term is suppressed. All specifications control for high school fixed effects and include indicator variables for parental education and occupation as well as continuous controls for the number of siblings and iq-scores (see Table 2 for descriptives).

Marriage.— Finding a partner for life and getting married is "relational" in the most literal sense. Virtually all cross-sectional wage studies find that currently married men earn a premium in the labor market, holding other characteristics constant [e.g. Korenman and Neumark, 1991]. This finding is relevant for the present work to the extent that our measures of adolescent social capital impact on later marriage decisions. The results presented in column 3, conditioning on being currently married, show only insignificant changes in the estimates. The largest change occurs for the isolate with a 1.6 percentage point reduction compared to the reference specification. The direction of the effect indicates that those who were isolates during high school tend to be married less frequently at later ages. However, the extent to which this mediates the wage gap compared to other types is very moderate.

Job Finding.— It is well recognized that the social ties of an individual may play an important role for the kind of search methods that are used by job seekers [e.g. Montgomery, 1991; Granovetter, 1974]. Many workers make use of informal contacts to former co-workers and acquaintances in their job search, instead of relying on employment agencies or direct application. Montgomery [1991], to quote one example, reports that approximately 50 percent of all workers currently employed found their jobs through informal channels, with the frequency of alternative job-finding methods varying somewhat by gender and occupation. Based on a question about how WLS respondents' got to know that their current job was available, we constructed an indicator variable for their job-finding method. Responses are classified into two categories; informal contacts like friends or acquaintances, former co-workers, teachers, clergypersons, relatives, and formal channels like employment agencies, newspapers and professional meetings or conferences. We note that the job-finding method does not appear to impact on wages itself and is also not mediating much of the wage disparities across types (col. 4).

Occupation Choice.— To investigate the relation between our pre-labor market measures of interpersonal skills and the distribution of workers across occupations, we adopt a similar approach as Hamermesh and Biddle (1994) based on Worker Function Codes available from the *Dictionary of Occupational Titles* (DOT). We combine this information with WLS data on respondents' job holding in terms of three digit occupational codes, based on the Bureau of the Census classification system, to rank occupations according to average scores on the DOT measure of "the job's relationship to people".[12] The occupation averages can be thought of as proxying for skill requirements of the respective occupation in terms of interpersonal interaction. Our estimations (cols. 5 and 6) show that working in an occupation with high requirements in terms of interpersonal skills associates with substantially higher wages; 6 to 11 percent depending on the set of controls. To our surprise, we find that the earnings differentials among types are not mediated to a significant extent by this measure. This lends little empirical support to a productivity model in which more socially skilled workers are observed in greater proportions in occupations where such skills are rewarded. Our measures appear to proxy some social attribute that is generally valued in the labor market, irrespective of an individual's occupation.[13]

Column 7 of table 6 concludes this section by adding again measures of social participation to the previous specification to simultaneously account for all variables that we have considered in this paper. We find earnings disparities across types in the same rank order as in the baseline regression. Further, the magnitudes of the effects are still very high for isolates, who earn wages roughly 25 percent lower than the mean, and receivers, who earn about 26 percent more than the average man.

V Discussion

This study was motivated by a growing interest in the behavioral outcomes of the schooling process and their relevance for individuals' subsequent success in the labor market. We adopted the view of the school class as a social system in which classmates assume important socialization functions. They provide an environment of status-equals in the ab-

[12] The averages are based on the fifth digit of the DOT code, which can take nine different values according to whether the job involves "mentoring" (highest), "negotiating", "instructing", "supervising", "diverting", "persuading", "speaking-signalling", "serving", or "taking instructions, helping" (lowest).

[13] We have also estimated specifications in which we condition directly on single-digit and two-digit Census occupation codes and find essentially the same: the earnings differentials among types do not appear to be complementary with any particular vocational path. Conditioning on three-digit codes, in combination with the school-fixed effects imposed in estimation, is infeasible due to the resulting loss in degrees of freedom.

sence of direct adult supervision and control. Social interaction and voluntary informal association with classmates may be viewed as a kind of early on-the-job training in the business of interpersonal behavior. This contrasts sharply with top-down instructions from generation superiors in the familial context but also through teachers in formal class room situations. Even though the school remains largely adult-controlled and teachers play a role of outstanding importance, it is the dimension of social interaction among equals that we consider fundamental to the development of behavioral skills valued in the labor market.

Based on detailed information on adolescents' friendship relations, our network approach allowed us to utilize a number of sociometric measures that position individuals within the social system of their high school class. We presented evidence that differential social standing in adolescence associates with large and persistent earnings disparities over the entire life course. The estimated wage premia and penalties do not appear to be substantially confounded by measures of family and school resources, nor are they proxying for differences in cognitive abilities, grade rank in class, personality traits or peer characteristics. Our results indicate, though, that a moderate fraction of the earnings inequalities is mediated through post-secondary human and social capital accumulation.

Interestingly, the structure of the effects we find corresponds very well with different levels of "youth-culture" that Parsons [1959] identified in his essay: There is a middle level without clear status differentiation in which individuals are characterized by "being a good fellow" in the sense of general friendliness and readiness to take responsibility in the informal group when something needs to be done. In our view, individuals in sycophant and broker positions correspond to this middle level. Above this, there is a level of "outstanding popularity" characterized by persons with qualities of "leadership" who are turned to where unusual responsibilities are required. Clearly, these are those who occupy primary positions within the network of relationships, that is, receiver-type of individuals. Below the middle level are those with behavioral patterns bordering on delinquency, withdrawal, and generally unacceptable behavior –the socially isolated in our study. This last level is the one that is clearly "dysfunctional" compared to expectations of appropriate behavior.

The results for the socially isolated deserve special attention. The magnitude of the wage differential, relative to an average person, is enormous: between 43 and 25 percent, depending on the set of controls entered (col. 3 of table 3 and col. 7 of table 6). Much more importantly though, the case of social isolation may be key to understanding what kind of valuable skills are acquired through social interaction with classmates. The graph theoretical notion of an isolate being “infinitely distant” is a lucid illustration. Isolates lack the interpersonal skills and social adaptability that others have acquired from working in groups, participating in student clubs and athletics, all of which foster informal association with classmates. These are also the broad conclusions that other authors have drawn before us, using different data and measures. What we contribute to the literature is a parallel set of findings based on a decidedly micro-sociological approach, both in terms of perspective and method.

Related Literature.— There are many studies that emphasize the economic importance of participation in social activities. One very recent and related example is Postlewaite and Silverman [2004], who examine social isolation in terms of voluntary nonparticipation in high school athletics.They conclude that the observed earnings differences are not primarily due to selection on predetermined characteristics, but reflect valuable skills acquired through social interaction. Earlier work on the effects of high school athletic participation and education and labor market outcomes includes Barron et al. [2000], and Maloney and McCormick [1993].

We have already mentioned in the introduction that there is a growing literature on behavioral and psychological variables (e.g. Carneiro and Heckman [2004]) and labor market outcomes. This line of work draws a number of important conclusions in relation to our analysis. First of all, despite some differences in naming, it points out the relevance of noncognitive abilities in shaping socioeconomic success. Secondly, it stresses that human capital accumulation (including the social capital components) is a dynamic life cycle process. Behavioral skills, broadly defined, develop in the relations to others and are the outcomes of a prolonged process of socialization stretching from early childhood in the family, through the entire schooling phase, and quite possibly continuing “on-the-job”.

Limitations.— There are limitations that our work shares with virtually all studies in the literature. To begin with, what kind of valuable social skills are being identified? The fact that we are still struggling with generic terms like noncognitive skills or individual social capital is indicative of the state of the field. A great variety of individual behavioral and personality attributes are lumped together in practice, for lack of anything better. No single factor has yet assumed a comparable role among the noncognitive abilities as the common g-factor on the cognitive side, and we agree with Heckman and Rubinstein [2001, p.145] that “it is unlikely that one will ever be found”. Comparing GED recipients with other high school dropouts and comparing isolates with average students are two alternative routes to identifying valuable interpersonal skills. But, it is not entirely clear whether a GED recipient, for instance, is different from other dropouts because this person lacks persistence, discipline and motivation, or because of something else. GEDs might also be those cross-pressured individuals who could potentially be upwardly mobile due to their cognitive skills, but who would need to “burn their bridges” with family and status peers to do so. Social disapproval may pressure them to behave in a regressive manner and to show indifference to their school performance. The upshot of our discussion here is that explanations based on psychological factors are not the whole story and that much of the existing evidence is equally consistent with a micro-sociological interpretation. Both approaches leave it to future studies to examine which factors in particular are being captured.

Some Thoughts on Human Capital Policy.— When economists study skill formation and analyze treatment effects of policy programs, they often focus on the development of cognitive skills as measured by performance on ACT or SAT college entrance exams (e.g. Krueger and Whitmore [2001]). In contrast, our analysis emphasizes the importance of the social component in human capital formation and thereby adds another dimension to the outcome space along which interventions have to be evaluated. This point has been made earlier by Carneiro and Heckman [2004]. What we want to add to the discussion is a conceptual consideration that follows directly from our micro-sociological stance.

Let us take the example of a policy that seeks to increase the quality of schooling by means of a class size reduction. The underlying mechanism is of course that a given amount of school resources is shared by a smaller number of pupils, a reduced student-teacher ratio being one example. Whatever the precise setup of the experiment, by focussing on the consequences for performance on cognitive ability tests, one important outcome is left out of consideration: the group structure of the school class is affected. There exists an extensive literature in sociology of education which examines the effects that classroom characteristics have an impact on the formation of friendships among classmates (see e.g. Hallinan [1979], and Hallinan and Smith [1989]). This work provides strong empirical evidence suggesting that both, pedagogical practices and class size, play a crucial role in the determination of the structural properties of school class social networks. For example, an empirical regularity is that social networks of larger classes exhibit a higher number of disconnected groups, which implies a higher level of segregation within the class.

When discussing the relation between our measures of position and class size we found a positive relation between being socially isolated and being in a bigger class. Such a relation was absent for the other types. It appears that student networks in smaller classes exhibit a higher degree of connectedness with fewer nodes being infinitely distant. Reducing class size means reducing social distance among members and, on an individual level, implies a lower incidence of social isolation. This would avoid its adverse consequences in terms of individuals' earnings and quite possibly many other important life outcomes. Nevertheless, our discussion strongly suggests that there may be significant "side effects" affecting the formation of the social component in human capital. It is important to recognize that there are multiple channels of influence and a number of outcome dimensions when designing educational policies and evaluating them in terms of their effects, costs and benefits.

Closing Remarks.— With our paper, we sought to contribute a compelling example of the potential that lies in egocentered network methods for survey-based economic research. Our simple application provided a methodological preview on the wide applicability of empirical network analysis and economic relevance of sociological concepts. A number of areas for further research open-up from here. One could extend our analysis and relate individuals' social status to other economically relevant choices and outcomes. Table 2 is suggestive of the fact that social capital investment and marriage decisions or job search channels are likely to be interesting candidates. From this perspective, the use of egocentered network methods may complement the existing literature on individual social capital in vein of Glaeser et al. [2002] by providing a rich set of alternative measures.

In addition, network analysis could prove highly instrumental for the emerging field on the importance of sociological concepts within economics (recently summarized by Gibbons [2004]). It equips the economist with a rigorous toolbox to approach many inherently relational concepts of interest, such as trust, identity and social capital. The most problematic aspect is the availability of accurate relational data and -at the same time- measures of relevant economic outcomes. In light of what has been said earlier, we note that complete network data collection and conventional survey designs are not always incompatible. If the social system of interest is bounded to a reasonable size, like the school classes in our study, one could easily collect information on the full set of ties among students. Sampling could then occur on the class instead of the individual level.

Finally, longitudinal data containing repeated observations of social associations among the same set of individuals would allow us to study the formation and evolution of friendship patterns during school time. In our view, increased efforts to integrate the collection of egocentered network data into conventional survey designs are likely to bear great potential for future work.

VI Appendix

A Misclassification Error in the Role Typology

The observation process consists of assigning each of the n students in our sample to one of four mutually exclusive and exhaustive categories: isolate, sycophant, broker and receiver. We denote each observation as a vector $\mathbf{A_t}$. If the t-th sample element is placed in the first category ('isolate') of the classification, we write $\mathbf{A_t} = (1, 0, 0, 0)$; if the t-th sample element is placed in the second category ('sycophant'), we write $\mathbf{A_t} = (0, 1, 0, 0)$; and similarly for the remaining two categories. The j-th element of the $\mathbf{A_t}$ vector, denoted by A_{tj} is a binomial random variable that takes the values zero and one. It follows that the observed distribution of $\mathbf{A_t}$ obtained by making a single determination on each student in our random sample is multinomial.

We formalize the measurement error process according to the *right-wrong* model for multinomial variables as outlined by Fuller (1987). According to this model, every student truly belongs to one and only one of the four categories. The measurement error is characterized by a set of misclassification probabilities κ_{Aij}, where κ_{Aij} is the probability that a student whose true category is j is (wrongly) assigned to category i. To give an example, the first column of the $\boldsymbol{\kappa_A}$ matrix contains the misclassification probabilities for the isolate. That is, conditional on truly being an isolate, it contains the probability that a student actually is classified as an isolate, or misclassified as a sycophant, a receiver or a broker. It is assumed that every element in true category j has the same vector of misclassification probabilities, that is, we ignore individual fixed effects in measurement error.

B Correcting the OLS Estimates

It is clear that with multinomial variables, the expected value and the variance of the measurement error are functions of the true values. Therefore, classical errors-in-variables (CEV) models that are useful when continuous variables are measured with additive noise do not apply in this situation. However, if the matrix $\boldsymbol{\kappa_A}$ of misclassification probabilities is known, one can transform the observations $\mathbf{A_t}$ as to make the model conform to CEV assumptions.

Now, consider a simple regression equation where we insert dummy variables based on our multinomial data as explanatory variables. Assume that the vector of true values $\boldsymbol{x_t}$ satisfies the following linear model

$$Y_t = \boldsymbol{x_t}\boldsymbol{\beta} + e_t, \qquad \boldsymbol{A_t} = \boldsymbol{x_t} + \boldsymbol{\xi_t}, \tag{2.1}$$

where $\boldsymbol{x_t}$ is a 4-dimensional vector with a one in the jth position and zeros elsewhere when student t is in the jth category. The equation error e_t is assumed to be independent of $\boldsymbol{x_t}$ and independent of the error $\boldsymbol{\xi_t}$ made in determining $\boldsymbol{x_t}$. Then, if we apply the following transformation to the observed vector

$$\boldsymbol{X_t'} = \boldsymbol{\kappa}_{\boldsymbol{A}}^{-1}\boldsymbol{A_t'}, \tag{2.2}$$

we obtain $\boldsymbol{X_t} = \boldsymbol{x_t} + \boldsymbol{u_t}$, with $E\{\boldsymbol{u_t}\} = \mathbf{0}$ and $E\{\boldsymbol{X_i} \mid i = t\} = \boldsymbol{x_t}$ for all t. Therefore, the vectors $\boldsymbol{X_t}$, $\boldsymbol{x_t}$, $\boldsymbol{u_t}$ are conform to CEV assumptions of zero mean errors that are uncorrelated with the true values.

Due to the transformation applied, we can now write

$$\boldsymbol{\Sigma}_{XX} = \boldsymbol{\Sigma}_{xx} + \boldsymbol{\Sigma}_{uu}, \tag{2.3}$$

with

$$\begin{aligned} \boldsymbol{\Sigma}_{uu} &= \boldsymbol{\kappa}_A^{-1}\boldsymbol{\Sigma}_{AA}\boldsymbol{\kappa}_A^{-1\,\prime} - \boldsymbol{\Sigma}_{xx}, \\ \boldsymbol{\Sigma}_{AA} &= diag(\mu_{A1}, \mu_{A2}, \mu_{A3}, \mu_{A4}) - \boldsymbol{\mu}_A'\boldsymbol{\mu}_A, \\ \boldsymbol{\Sigma}_{xx} &= diag(\mu_{x1}, \mu_{x2}, \mu_{x3}, \mu_{x4}) - \boldsymbol{\mu}_x'\boldsymbol{\mu}_x. \end{aligned} \tag{2.4}$$

This is the classical errors-in-variables decomposition where the variance of the observed values $\boldsymbol{\Sigma}_{XX}$ is modelled as the sum of a true variance ('signal') component $\boldsymbol{\Sigma}_{xx}$ and an error variance ('noise') component $\boldsymbol{\Sigma}_{uu}$. The observed sample proportions and the true population proportions of isolates, sycophants, brokers and receivers are denoted by $\boldsymbol{\mu_A} = (\mu_{A1}, \mu_{A2}, \mu_{A3}, \mu_{A4})$ and $\boldsymbol{\mu_x} = (\mu_{x1}, \mu_{x2}, \mu_{x3}, \mu_{x4})$ respectively.[14] For detailed derivations and proofs see Fuller (1987). Then, a consistent estimator of $\boldsymbol{\beta}$ is

$$\hat{\boldsymbol{\beta}} = (\boldsymbol{X}'\boldsymbol{X} - \hat{\boldsymbol{\Sigma}}_{uu})^{-1}\boldsymbol{X}'\boldsymbol{Y} \tag{2.5}$$

[14]Knowing $\boldsymbol{\kappa}_A$, an estimator of the vector of true proportions is given by $\hat{\boldsymbol{\mu}}_{\boldsymbol{x}}'\boldsymbol{\kappa}_A^{-1}\boldsymbol{\mu}_{\boldsymbol{A}}'$. The fact that $\boldsymbol{\mu}_x$ must be estimated, introduces some error into $\hat{\boldsymbol{\Sigma}}_{uu}$ (and therefore into the variance of the estimator of $\boldsymbol{\beta}$) which we have to ignore here.

and the variance covariance matrix of the estimator is obtained as

$$\mathrm{Var}\{\hat{\boldsymbol{\beta}}\} = s^2(\boldsymbol{X}'\boldsymbol{X} - \hat{\Sigma}_{uu})^{-1}\boldsymbol{X}'\boldsymbol{X}(\boldsymbol{X}'\boldsymbol{X} - \hat{\Sigma}_{uu})^{-1}, \tag{2.6}$$

where

$$s^2 = (\boldsymbol{y}'\boldsymbol{y} - \hat{\boldsymbol{\beta}}(\boldsymbol{X}'\boldsymbol{X} - \hat{\Sigma}_{uu})\hat{\boldsymbol{\beta}}')/(n-p), \tag{2.7}$$

is the root mean square error and p the number of estimated parameters.

C Derivation of $\boldsymbol{\kappa}_A$

We have shown that if the functional relationship between the mean of the error variable and the mean of the true variable is known, a transformation can be applied to the original observations such as to obtain errors conform to CEV assumptions. The question is of course how to obtain an estimate of $\boldsymbol{\kappa_A}$. We again denote the mean vector for the observed proportions as

$$\boldsymbol{\mu}'_{\boldsymbol{A}} = \boldsymbol{\kappa_A}\boldsymbol{\mu}'_{\boldsymbol{x}} \tag{2.8}$$

where $\boldsymbol{\mu}'_{\boldsymbol{x}}$ is the vector of true proportions and $\boldsymbol{\kappa_A}$ is the matrix containing the misclassification probabilities conditional on the type. Consider the following two thought experiments.

First, suppose we started from an ideal situation in which we observed all ties, incoming and outgoing. In this case individuals would be classified correctly and there would be no difference between observed and true proportions $\boldsymbol{\mu}'_{\boldsymbol{x}}$. Now, let us remove all those links that students within the sample receive from students outside the sample and denote the observed proportions by $\boldsymbol{\mu}'_{\boldsymbol{A_1}}$. What can we say about the relation between $\boldsymbol{\mu}'_{\boldsymbol{A_1}}$ and $\boldsymbol{\mu}'_{\boldsymbol{x}}$, in other words, what is the nature of the misclassification error that occurs if some of the *incoming* links are ignored? Obviously, true isolates and true sycophants will never be misclassified, that is, they are always observed as such. But, a true receiver could be misclassified as an isolate and a true broker could be misclassified as a sycophant. This allows us to define the matrix of misclassification probabilities as

$$\boldsymbol{\kappa_{A_1}} = \begin{bmatrix} 1 & 0 & 0 & \beta \\ 0 & 1 & \alpha & 0 \\ 0 & 0 & 1-\alpha & 0 \\ 0 & 0 & 0 & 1-\beta \end{bmatrix} \tag{2.9}$$

with $\alpha, \beta \in (0,1)$. In particular, α (resp. β) denotes the probability that a true broker (resp. receiver) is misclassified as a sycophant (resp. isolate).

Second, suppose again to start from the ideal situation with in-degree and out-degree fully observed, $\boldsymbol{\mu}'_{\boldsymbol{x}}$. However, now we remove the links that individuals within the sample sponsor towards individuals outside the sample and denote the observed proportions by $\boldsymbol{\mu}'_{\boldsymbol{A_2}}$. That is, what can we say about the relation between $\boldsymbol{\mu}'_{\boldsymbol{A_2}}$ and $\boldsymbol{\mu}'_{\boldsymbol{x}}$ when some of the *outgoing* links are ignored? Here, a true sycophant may be misclassified as an isolate and a true broker as a receiver, while true isolates and true receivers will always be observed as such. Thus,

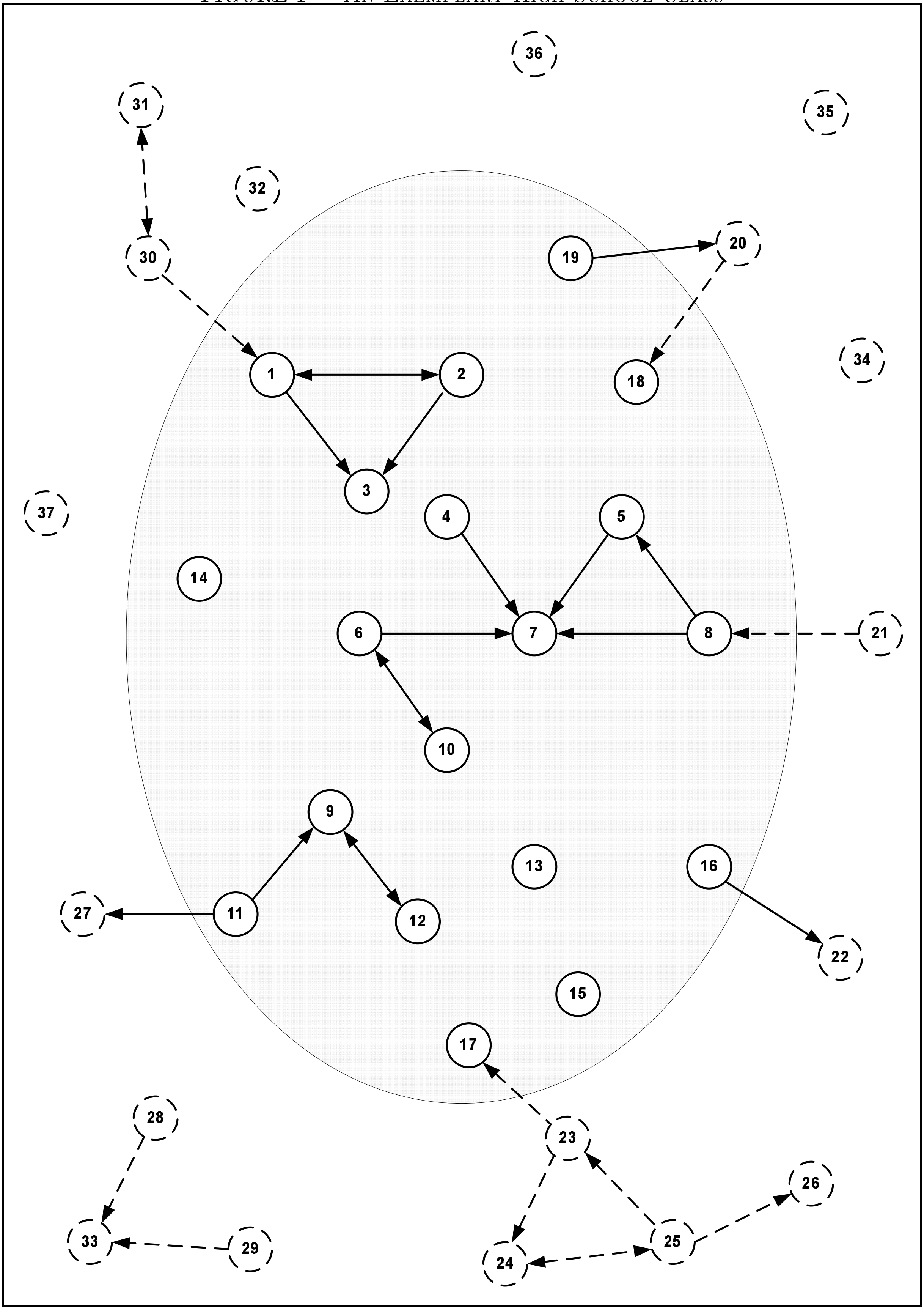
$$\boldsymbol{\kappa}_{\boldsymbol{A_2}} = \begin{bmatrix} 1 & \gamma & 0 & 0 \\ 0 & 1-\gamma & 0 & 0 \\ 0 & 0 & 1-\delta & 0 \\ 0 & 0 & \delta & 1 \end{bmatrix} \tag{2.10}$$

with $\gamma, \delta \in (0,1)$. Here, γ (resp. δ) denotes the probability that a true sycophant (resp. broker) is misclassified as an isolate (resp. receiver).

Let us compare the two experiments. Under both schemes, the deletion of links occurs randomly since individuals have been sampled randomly. This implies a clear symmetry in terms of the effects on the observed proportions. Symmetry means that a fraction α of true brokers is misclassified as sycophants after performing the first experiment, a fraction δ of true brokers is misclassified as receivers after the second experiment and $\alpha = \delta$. The same argument can be made for β and γ. Compared to the ideal situation, a fraction β of true receivers will be misclassified as isolates due to deletion of incoming links; a fraction γ of true sycophants will be misclassified as isolates due to deletion of outgoing links. Since deletion occurs randomly, we will have symmetric effects in terms of misclassification probabilities, that is $\beta = \gamma$.

We are now ready to derive the correction matrix $\boldsymbol{\kappa}_{\boldsymbol{A}}$ in the following way. First, using all available information in our data we observe $\boldsymbol{\mu}'_{\boldsymbol{A_1}} = [0.080, 0.523, 0.377, 0.020]$; see also column 1 of table 1. Then, we ignore all ties sponsored by sample members to non-WLS classmates and re-calculate the mean proportions which leads to $\boldsymbol{\mu}'_{\boldsymbol{A_3}} = [0.372, 0.231, 0.277, 0.120]$. We observe outflows from the sycophant to the isolate category and from the broker to the receiver category. This comes as no surprise since the relation between $\boldsymbol{\mu}'_{\boldsymbol{A_1}}$ and $\boldsymbol{\mu}'_{\boldsymbol{A_3}}$ in terms of misclassification probabilities is identical to the one between $\boldsymbol{\mu}'_{\boldsymbol{x}}$ and $\boldsymbol{\mu}'_{\boldsymbol{A_2}}$. Therefore, solving $\boldsymbol{\mu}'_{\boldsymbol{A_3}} = \boldsymbol{\kappa}_{\boldsymbol{A_2}}\boldsymbol{\mu}'_{\boldsymbol{A_1}}$ allows us to determine $\gamma = 0.558$ and $\delta = 0.265$. As we have shown above, these probabilities equal β and α respectively. This yields the desired correction matrix $\boldsymbol{\kappa}_{\boldsymbol{A_1}}$. The estimates for the unobserved true proportions $\boldsymbol{\mu}'_{\boldsymbol{x}}$ are provided in column 2 of table 1. The correction of the observed in-degree classification (zero versus positive) follows the same procedure, except that here we simply have two categories instead of four.

FIGURE 1 — An Exemplary High School Class



Chapter 3

Class Size and Students' Friendships

"Cognitive skills, the chief measure of educational quality, may not be the only, let alone the most important, outcome of schooling in determining individuals' future success."

— Eric A. Hanushek [1979]

"Evidence that it is difficult to boost IQ in no way undermines programs that intervene to improve non-cognitive skills."

— James J. Heckman & Lance Lochner [2000]

I Introduction

In the previous chapter, we have argued that social skills form an important subcomponent of human capital and that schools play a central role in the development of such skills. We also noted that social skills are not acquired by means of top-down instructions from teachers in the way mathematics, reading or writing skills are. Alluding again to Parsons' view of the school class as a social system, social skills are thought to develop in a more indirect way, through social interaction among classmates. This raises the question whether organizational characteristics of schools and school classes do have an impact on schooling outcomes along the "social" dimension of human capital; as much as they have along the cognitive dimension.

One naturally interesting candidate in this respect is the size of a school class; subject of many policy debates and controversies among academic economists. This chapter contributes by estimating probit models of friendship choice to determine how much class size affects the probability that a friendship is formed between an arbitrary pair of classmates. Given our data, class size refers to the size of the entire grade cohort of final year students in a given school, rather than the number of students in any particular teaching unit. This contrasts with most of the literature on class size and scholastic achievement [Hoxby 2000, Angrist and Lavy 1999, Krueger 1999] which usually considers measures of the size of a "classroom" or proxies thereof, such as pupil-teacher ratios.

Class size and grade are organizational features that define the pool of peers with whom a student most frequently interacts and the age range of that pool. Characteristics of a school or a classroom may therefore play a significant role for the formation of social relations among students [Hallinan 1982; Newcomb 1961; Coleman 1961]. In particular, the pedagogical practice of assigning students to groups for instruction subdivides the class (grade cohort) into smaller well-defined groups and can be expected to have important effects on social interaction among group members. The number of instructional groups into which the class is divided represents a partition of the class that affects interaction patterns and constrains activities. This has consequences for students' relationships. By imposing proximity among certain students, it provides the students with opportunities for interaction. Exposure and shared experiences of two students who happen to be assigned to the same group increases the probability that they will become friends. At the same time, it separates other students, decreasing the frequency of their potential interactions.

In general, there is little research on the effects of class size reductions on outcomes *other than* test scores of cognitive skills. Some argue that cognitive skills may not be the only outcome of schooling in determining individuals' future success [e.g. Hanushek 1979]. Given our strong findings in terms of adult earnings inequalities (and other socioeconomic variables), it is therefore immediate to ask which are the institutional determinants of social interaction and relationship formation. One argument conventionally put forward to justify the focus on cognitive achievement as the primary schooling outcome, relates to the continuation of education. Subject knowledge, general problem solving skills, grades, test scores are all highly predictive of the probability that individuals take up college education after high school. But similar may hold for social relations, if we think of peer effects among classmates, or friends, in the college attendance decision. The point here is that cognitive skills are highly relevant, but that it is important to recognize that there are a number of alternative outcomes of the schooling process.

II Theories of Friendship Choice

The social psychological literature on interpersonal attraction suggests a number of causes for the development of social ties such as friendship relations among classmates [e.g. Homans 1974; Newcomb 1961]. At the individual level, it is primarily "homophilic selection", that is, the tendency of people to form social ties with others who are similar to themselves in terms of attributes; a phenomenon known to economists as assortative mating. Besides attribute similarity among persons, there are other factors affecting the development of interpersonal relations.

A necessary condition for the emergence of any social relation is that persons are available to each other so that some kind of interaction may take place. This is the core idea behind "contact theory" [Blau 1977; Feld 1981]. The key factor for friendship formation among classmates is therefore simple mixing opportunity: the greater the opportunity for people to meet, the greater the likelihood that relationships form. Of course, mere exposure to another individual does not necessarily lead to repeated social interaction and the formation of a friendship relation. It may in fact lead to neutrality or even dislike, contingent on other bases of interpersonal attraction. Nevertheless, exposure –or "propinquity" as it is called in the social psychology literature– is consistently found to be a strong predictor of friendship choices (see e.g, Hallinan and Teixeira [1987]; Schutte and Light [1978]).

What direction of the effect of class size on the likelihood of friendship formation should we expect? At first sight, one would tend to think that class size is positively related to the probability that a friendship relation is formed between an arbitrary pair of students. This is, in line with contact theory, because in large classes students have more opportunities to identify peers who possess characteristics they find attractive.

It is not a logical necessity, however, that class size has a positive effect on friendship formation. It could be that as the number of students in a class increases, social interaction among classmates actually decreases due to the way schools are organized. As grade cohort size increases, more class sections will be added to keep individual class sizes manageable. In larger schools, a student will not share all of his or her classes with the same set of students. Overall larger student populations may actually be more "anonymous" due to clique formation. There are many empirical studies in sociology documenting that social networks of larger classes in fact exhibit a higher number of disconnected sub-groups, which implies a higher level of segregation within the class (see e.g., Hallinan [1979]; Hallinan and Smith [1989]). The working hypothesis is therefore that the larger the class (in the sense of grade cohort), the less the chance that a student will interact with any particular other, and thus the less likely that a student will choose that other as a friend.

III Method

A Dyadic Level Analysis

As in the preceding chapter, organizing the available data on student friendships is conceptually not straightforward. It again depends on the research question which method and level of analysis is appropriate. So far, we have distinguished between two levels of network analysis; the structural and the actor level. The outcome of interest in the present chapter is the probability that a friendship relation is formed between two pupils in the same class. Therefore, the empirical analysis has to be carried out on yet another, the "dyadic" level, in which the unit of observation becomes the relation itself. This also implies that if we want to study who links with whom, it is not sufficient to only look at the pairs of students in which one side has named the other as a friend. It is strictly necessary to take into account those pairs of individuals in which friendships do *not* exist. Only this allows us to estimate how much more likely friendships are to be formed between two students when placed in a smaller class, everything else constant.

A dyad is a *pair of actors* and it specifies the ties that exist between the two actors in the pair [Wasserman and Faust 1994]. With dichotomous relations, three states for dyads can be distinguished. First, the mutual relationship between actors i and j, which exists when $i \rightarrow j$ *and* $j \rightarrow i$, for $i \neq j$. The second state is the asymmetric dyad which can occur in two ways. Either $i \rightarrow j$ or $j \rightarrow i$, but not both. Since the labeling of actors is arbitrary, studying asymmetric dyads in terms of the one or the other direction is perfectly equivalent. This brings us to the third type, the null dyad, in which neither actor has a tie with the other. In our data, the actors are individuals and the relations reflect positive sentiment.

Using the friendship pair as the unit of analysis is common practice in studies of friendship choices in the sociology literature [e.g., Hallinan and Williams 1987]. The same strategy is also employed in a recent economic study by Marmaros and Sacerdote [2004]. The dyadic approach makes it possible to define the dependent variable as a dummy indicating whether or not a friendship relation exists between two randomly chosen classmates. This approach allows us to determine how much measures of school or class size affect the likelihood that (a) some individual i chooses j as a friend, and (b) that a reciprocal friendship exists between i and j. These are the two primary outcomes of interest.

Besides the conceptual considerations leading to such a strategy, the dyadic approach offers a further advantage. As explained earlier, due to the survey design we do only observe sociometric choices made by a one-third subset of all students in any given class. In the previous chapter, the random deletion of nodes was a real concern as a source of measurement error in the explanatory variables. Moving from the individual-level to a dyadic-level of analysis resolves this issue. The reason is that if student i chooses friend j, and j happens to be outside the sample, this dyad will simply be discarded. Since students are sampled randomly, dyads will be missing randomly. *Within* the remaining subset of students we do observe perfectly whether i chose j, and whether that choice was reciprocated or not. The dependent variables are therefore left unaffected.

B Friendship Relations

In forming all possible student pairs we again use information of the 9,138 students who responded to the 1975 questionnaire and who provided names of their friends in high school senior class. Half of the students name three others as their close friends, while 28 percent name only two, 13 percent name one, and the remaining 9 percent do not mention any classmate at all. A total of 15,457 claims of friendship are made, of which about a third is retained in the data due to the sampling scheme. In order to determine the state of all dyads in the sample, each respondent is matched with every other respondent who was a member of the same senior class. It is then recorded whether i chose j as a friend, and whether that choice was reciprocated. This procedure is repeated class-by-class for the entire set of school classes, yielding information about 426,104 pairs of students and the state of their relation.

Based on this data two binary dependent variables are created: (a) for the broadest definition of friendship, taking on the value one if $i \to j$ and zero otherwise; and (b), for reciprocal friendships, being equal to one if $i \to j$ conditional on $j \to i$ and zero otherwise. After removing cases with missing data on some of the independent variables, we are left with a final sample of 421,328 dyads which corresponds to 9,040 pupils. This implies that we have data on almost 90 percent of all students initially interviewed, limiting the potential for nonresponse bias in later estimations.

In table 1, number and types of friendship relations are (cross-) tabulated by rank order of choice. We start off with discussing columns 1-3 and the top three rows. These nine cells contain all reciprocal dyads in the sample, summing to 2,670. The first row shows in how many cases i chose j as first best friend while j reciprocated that choice by naming i in first, second or third position respectively. The following two rows show the same for i's second and third choice. We see the larger number of choices concentrated in the top left corner which indicates that reciprocity tends to occur more frequently among first as compared to second and third best alternatives. Column 4 shows in how many instances j did *not* choose i although i named j as first, second or third best friend. Summing over all rows in this column yields 2,934 asymmetric dyads. Column 5 counts all active dyads no matter of the

TABLE 1

NUMBER AND TYPES OF FRIENDSHIP RELATIONS

	(1)	(2)	(3)	(4)
	Reciprocal			
	$j \xrightarrow{1^{st}\ Choice} i$	$j \xrightarrow{2^{nd}\ Choice} i$	$j \xrightarrow{3^{rd}\ Choice} i$	$j \not\rightarrow i$
$i \xrightarrow{1^{st}\ Choice} j$	706	361	217	884
$i \xrightarrow{2^{nd}\ Choice} j$	361	310	182	1,019
$i \xrightarrow{3^{rd}\ Choice} j$	217	182	134	1,031
Sum by Type		2,670		2,934

NOTE.— The total number of friendship relations included in the sample is 5,604. For friendship rates, see table A1.

direction and leaves us with a total of 5,604 relations in which either side mentioned the other.

Dividing the total number of active dyads by the overall number yields a friendship rate of 1.3 percent, roughly half of which are mutual friendships. When interpreting this number we have to bear in mind that (close) friendship is by definition a very infrequent event. The reader should try to imagine the following situation: out of some 100 people that would be randomly introduced to him or her, 1 or 2 would plausibly qualify as friends in the broad sense; while he or she would need to meet at least 200 people –on average– to find one reciprocal friend. Reasonable estimates, intuitively. In addition, these numbers are well in line with what other studies have found, using different data sets and friendship measures. Hallinan and Williams [1989], based on the High School and Beyond survey with roughly 450,000 sophomore dyads, report a friendship rate of 1.3 percent. Marmaros and Sacerdote [2004], drawing on information about more than 5.3 million possible friendship relations among students from Dartmouth College, finds that 1.4 percent of all dyads are active.

C Measuring Class Size

The WLS data offers a measure of the original size of the graduating class which participated in the 1957 survey. As mentioned earlier, this notion of a class relates to the size of the entire grade cohort of final year students in a given school, rather than the average number of students in any particular teaching unit. It is this latter notion of a classroom which underlies most of the literature on class size and scholastic achievement [Hoxby 2000, Angrist and Lavy 1999, Krueger 1999]. For lack of more detailed information in the original WLS, on the number of teachers for example, it is impossible to relate the size of the grade cohort to actual class size, and derive proxies such as pupil-teacher ratios. Recent work by Olson and Ackerman [2002] has supplemented the WLS in this regard. They have matched to each respondent information on the characteristics of his or her high school obtained from the Wisconsin Department of Public Instruction (WDPI). The augmented data set contains variables such as pupil-teacher ratio, teachers' salaries and experience, length of the school term and many others; but is not publicly available.

Table A1 presents summary statistics both at the individual and the dyadic level (see appendix). We denote the size of the high school senior cohort by CLSIZE. The mean CLSIZE in the estimation sample is 172 students, varying from 7 to 482 students. It is common to use the natural logarithm of the size [see e.g. Hoxby 2000] to take account of the fact that a student reduction is proportionately larger from a base of 30 students, say, than from a base of 100. At the dyadic level, the higher sample means are reflecting the fact that student pairs from larger classes constitute a larger part of the sample. The number of dyads in a class increases geometrically, rather than arithmetically, with size. In a senior class with 50 students, there are 2,450 cross combinations, while in a senior class with 150 students, there are 22,350 pairs of students, more than 9 times as many. Reweighting each dyad by the inverse of the class size minus 1 (because students cannot have a tie with themselves), produces weighted sample moments on a pair level that are almost identical to those calculated on an individual level. For variables not related to CLSIZE, there should be little difference in means calculated at the individual versus pair level, since students have been sampled randomly. This is also what we generally see when comparing sample moments for selected control variables (to be discussed later) across columns.

IV The Effects of Class Size on Students' Friendships

A Probit Models of Friendship Choice

In this section we examine the quantitative importance of class size in determining friendship formation by estimating a set of probit models. We begin with the broadest definition of friendship and define an indicator variable $Y_i = 1$ if student i chose j as a friend, and let $Y_i = 0$ otherwise. It is assumed that these one-way ties indicate the existence of some sort of informal relationship, even if it is not a mutual friendship. We will later examine the robustness of the findings using a second definition of friendship choice, one that involves reciprocation.

The probability that a friendship is formed between two students is defined as

$$\begin{aligned} \text{prob}[Y_i = 1] &= \text{prob}[\beta \text{log}(\text{clsize}_i) + X_i\gamma + \epsilon_i > 0] \\ &= \Phi[\beta \text{log}(\text{clsize}_i) + X_i\gamma] \end{aligned} \quad (1)$$

where log(clsize$_i$) is the natural logarithm of the number of students in high school senior class to which a dyad belongs, X_i is a vector of institutional and individual characteristics to be discussed below, and ϵ_i is a normally distributed random error with zero mean and unit variance. $\Phi[\cdot]$ is the evaluation of the standard normal cdf.

Table 2 summarizes the estimation results and is structured as follows. Column (i) indicates the definition of the dependent variable used in the respective model; either friendship ($i \rightarrow j$) or reciprocal friendship ($i \rightarrow j | j \rightarrow i$). Column (ii) lists the independent variables that are included in addition to log(clsize) across a number of alternative model specifications. The parameters reported in column (iii) are marginal effects $\partial \text{prob}[Y_i = 1]/\partial \text{log}(\text{clsize}_i)$ evaluated at the sample means of the covariates. The effects therefore apply to a fictitious reference pair of students with "average" characteristics. Robust standard errors are provided in brackets below, and are adjusted for clustering on the individual level. Column (iv) reports z-values to test for statistical significance while column (v) provides a measure of model fit. In column (vi), the marginal effect is transformed and scaled such that it represents the proportionate change in the probability of friendship choice due to a 10 percent reduction in class size.[1] Additionally, estimates are provided of the "average partial effect" (APE) induced by a 10 percent class size reduction (column vii). That is, we evaluate and average the marginal effects across the entire class size distribution and scale them again by the friendship rate. The APE is computed as the average difference between the probability that a student pair would be friends if they attended a 10 percent smaller school and the probability that the same pair would be friends in the current school, *ceteris paribus*. Thus, if n is the sample size, the total number of dyads, and β and γ are the estimates of the parameters in equation (1), then the average partial effect equals

$$N^{-1} \sum_{i=1}^{N} \Phi[\beta(\text{log}(\text{clsize}_i) + .10) + X_i\gamma] - \Phi[\beta \text{log}(\text{hssize}_i) + X_i\gamma], \quad (2)$$

which is a very convenient summary measure of the "treatment effect" of a 10 percent size reduction on the distribution of the outcome. In most of the discussion we will therefore focus on this effect.

[1]The marginal effect is simply multiplied by .10 and then divided by the overall friendship rate.

Estimates for model 1, the baseline specification without controls, are presented in the first row of Table 2. We find that students in smaller classes have a significantly higher probability of interacting with each other. Our reference pair of student's probability of being associated as friends would be close to .12 percentage points higher if they attended a 10 percent smaller class (column iii). This is a fairly large effect as it has to be evaluated against an overall friendship rate of 1.3 percent (column vi); the interpretation being that the probability of interaction would be increased by 8.75 percent in response to a 10 percent size reduction.[2] Averaging the marginal effects across the entire distribution, we find that reducing the senior class population by 10 percent is associated with a 10 percent increase in the probability of social interaction among a randomly drawn pair of students (column vii).[3]

The qualitative finding that class size is significantly negatively related to friendship choice is consistent with the literature in this field (e.g. Kubitschek and Hallinan [1998], Hallinan and Williams [1989, 1987]), although none of the cited works investigates the role of class size as such. Class size is mostly treated as a control variable when examining other determinants of friendship formation; no marginal effects are reported, sometimes probit coefficients instead. This makes it very difficult to draw any quantitative comparison with our findings. The central question we are going to address in the remainder of this chapter is whether we can give this correlation a causal interpretation.

[2]Estimation of a logistic regression model yields virtually identical results. In most instances with reasonable sample sizes, a simple linear probability model would do equally well. In our case, however, the fact that frequencies of friendship relations are very sparse renders the linear probability model inappropriate; more than 35 percent of the predicted values were smaller than zero. Since modern software packages like STATA routinely offer commands for probit (*dprobit*) and even instrumental variable probit models (*divprob*), computational considerations are not relevant anymore in deciding which model to choose.

[3]The fact that the marginal effect for an "average" pair of students is smaller than the APE, indicates that the marginal effects may look different over regions of the support away from the mean. In the class size literature it is often found that changes in size have smaller effects on student achievement over the mid-range. I use linear splines to determine if there are certain ranges for which changes in class size have little impact on friendship choice. The class size variable is split into three equal sample-size groups based on percentiles, that is the bottom third of the size distribution, the mid-range, and the top third. I thereby approximate the nonlinearities in the relationship between size and the outcome by a piecewise linear function and test whether the size effect is the same over the three intervals. I indeed find that the marginal effect is weakest over the mid range and strongest in the tails, yielding the kind of U-shaped relation documented in class size literature. The differences are not statistically significant though.

TABLE 2

THE EFFECT OF CLASS SIZE ON FRIENDSHIP CHOICE

Model	Dependent variable	Additional Control variables	Marginal effect of log(clsize)	$\|z\|$	Pseudo R-sq	%Δ in Pr(Y=1) due to a 10% size reduction	APE (in %) due to a 10% size reduction
	(i)	(ii)	(iii)	(iv)	(v)	(vi)	(vii)
			A. PROBIT ESTIMATES				
(1)	$i \rightarrow j$		– .0116 (.0002)	74.21	.077	– 8.75***	– 10.22***
(2)	$i \rightarrow j$	network density	– .0110 (.0003)	35.15	.077	– 8.25***	– 9.62***
(3)	$i \rightarrow j$	individual, parental background and school characteristics	– .0092 (.0003)	31.67	.107	– 6.90***	– 9.21***
(4)	$i \rightarrow j \| j \rightarrow i$	all of the variables in model (3)	– .0042 (.0001)	35.31	.102	– 6.64***	– 9.51***
			B. IV-PROBIT ESTIMATES				
(3*)	$i \rightarrow j$	all of the variables in model (3)	– .0090 (.0033)	2.70	.094	– 6.74***	– 8.99***
(4*)	$i \rightarrow j \| j \rightarrow i$	all of the variables in model (3)	– .0044 (.0003)	12.50	.079	– 6.94***	– 10.02***

NOTE.— Robust standard errors in parentheses. Standard errors are adjusted for clustering on the sender level. The number of observations is 421,328.

B An Artifact of the Questionnaire Design?

The first issue that arises in this regard is to exclude that the positive association between smaller senior classes and student friendships is a mere artifact of the survey design. The primary concern relates to the fixed-choice nature of the questionnaire in which individuals were asked to name *three same-sex best friends only*. Leaving the gender issue aside for a moment, it is mainly the following argument: Since all students were limited to three friendship choices, the larger the senior class in high school, the less likely it is that any particular pair of individuals will be friends. To illustrate, a student who has 10 classmates and names three of them is friends with 30 percent, while somebody who has 100 classmates is friends with only 3 percent. Denoting the number of choices each individual has by d, and the number of students in any particular senior class by n, the theoretical friendship rate will be $(d/n-1)$ since students may not name themselves. Thus, the friendship rate that is "theoretically" possible varies across schools, and it varies in direct proportion to the number of students n, since the number of choices allowed d is the same for all. This implies that even if there were no "true" behavioral differences among students in small as compared to large classes one would still find that students are less likely to be friends in larger classes.

Although, at first sight, the problem may appear nonstandard, we are actually in the quite common situation in which two variables are found to be statistically related but may not in fact be causally linked because the statistical relation is caused by a third variable. We therefore have to construct a control variable that equals the theoretical friendship rate (or network density), as defined above, for each school in our sample.When adding this measure to the baseline probit (model 2 in table 2) the effects of this third variable are partialed out. If the class size effect was a mere artifact of differing theoretical friendship rates induced by the survey design, it should not survive the inclusion of the control. This is not the case as we can see, although the estimated effect of log class size is somewhat reduced. The (theoretical) network density is statistically highly significant (at the 1 percent level, detailed coefficients omitted) which implies that the fixed-choice design does indeed have an impact on the probability of observing a friendship choice. It is however, and this was the primary concern, by no means driving the results.

Strictly speaking, the questionnaire design imposes a second restriction. Students may name friends of the same gender only. I have tested for gender differences by estimating a fully interacted version of model 2. Besides the fact that women appear to be somewhat more social, a level effect, there is no evidence of gender-differences in the way class size and the theoretical friendship rate affect the probability to form a friendship, which makes the pooling restriction acceptable. There is another reason to believe that the same-sex restriction is actually quite innocuous. Most studies of friendship formation in school contexts find a very strong segregation of choices along gender and race dimensions. Based on data that *does not* impose the same sex restriction in naming, Hallinan and Williams [1989, 1987] find that between 90 and 93 percent of all friendship choices occur among students of the same gender anyway.

C Potential for Omitted Variables Bias

In this subsection I examine in greater detail whether my basic results are robust to specification. There may be omitted measurable characteristics of the student pairs that are correlated with the class size variable and that, as a consequence, I have overstated the relevance of class size for friendship formation. I ask whether including measures of students' academic abilities, family background, chosen curriculum, and an array of other variables would change the basic finding that the size of the senior class in secondary school has an impact on the probability of friendship formation among pairs of students.

Since the unit of analysis is a pair of students, controls have to be defined separately for characteristics of the chooser i, the one being chosen j, and the pair (i, j) [see also Marmaros and Sacerdote 2004]. To give an example, for an indication of a student's status in terms of academic ability, I use (the logarithm of) test scores on the Henmon-Nelson test of mental ability. When running the probits of friendship choice, the following control variables are added: i's LOG(IQ-), j's LOG(IQ), as well as the absolute value of the difference between i's and j's log iq-score $|\Delta|$ LOG(IQ) as a measure of (dis-)similarity.[4]

[4]Please see Table A1 for summary statistics on the individual and dyadic level. I show sample means calculated over all choosing students i and all pairs. For the current discussion, only the dyadic level sample moments in the two rightmost columns are relevant. Since all students are both at the

In a similar vein, I include (the logarithm of) a continuous composite measure for students' socioeconomic status LOG(SES), for i and j separately, as a proxy for their respective family background. This status variable is a factor-weighted combination of parent's education, occupation and average parental income. I then create a variable denoted by $|\Delta|$ LOG(SES) that takes on the absolute value of the difference between i's and j's log social status, proxying for the (dis-)similarity of the dyad members in terms of social class of origin.

I further account for the religious preference of a respondent's family when he or she was a high school senior. A set of dummy variables is created for whether parents were CATHOLIC, belonged to one of the various PROTESTANT groups, or held beliefs OTHER than the two above, a category that mainly comprises agnostic or atheist people. A dummy variable called SAME RELIGION indicates whether the two schoolmates in any given dyad share the same religious background.

Controls are added for respondents' AGE, defined in number of years, separately for the chooser i and j. Students are between 17 and 20 years old, with 78 percent of age 18, 16 percent of age 19 years, and the remainder distributed over the two smallest categories. In the late teens, a difference of one or two years in age can be expected to have a sizeable impact on the decision to interact with one another. I again define a similarity indicator taking on the value one if the two dyad members are of equal age and zero otherwise.

A dummy variable MALE is created controlling for the gender of the chooser i. Since friendship choices are by design confined to same sex relations, the MALE dummy is perfectly collinear with a variable for j's gender, or for whether a pair of students is of equal sex or not.

It has to be noted that the WLS generally provides only limited information on the characteristics of the schools that the respondents attended. One important institutional characteristic is the denominational control of the respective high school. If denominational control is somehow related to size and at the same time affecting the way students form friendships (for instance through greater homogeneity in terms of religious affiliation), part of the class size effect could be due to the way public, Catholic and other private schools are organized. CATHOLIC

same time 'choosing' and 'being chosen', it does not matter whether the pair level sample means are calculated over all i or j.

is a dichotomous variable indicating whether a friendship dyad belongs to a Catholic private school, and PUBLIC is the corresponding variable for public schools. PRIVATE refers to a small number of other schools under private control.

Another important institutional feature of secondary schools in the U.S. is that of "tracking", that is the placement of students into tracks based on academic performance (and interests). High schools typically have three broad track levels: academic, general and vocational. I was unable to locate a variable in the WLS that would classify pupils into tracks. In order to (partially) control for differences and similarities in curricula, I rely on detailed information about the coursework done by students over the last eight semesters prior to the interview. In particular, I include the # of semesters of algebra, geometry, trigonometry, biology, chemistry, physics, english history, social studies and foreign languages as well as the absolute value of the difference between i's and j's number of semesters of respective coursework.

In model 3 of table 2 the full set of controls just discussed is added to the previous specification. While the overall magnitude of the result is somewhat weakened, the marginal and average partial effects in model 3 are still very close to that in model 2. Overall, including a large array of control variables reduces the raw average partial effect of a 10 percent class size reduction by 1 percentage point, from about 10 to 9 percent.

D An Alternative Definition of Friendship

As a final sensitivity test in this section, I examine the robustness of my findings to an alternative definition of friendship, that of a reciprocal friendship. I have reestimated models 1-3 of table 2 using a sharper measure of the outcome variable, now defining Y_i to equal 1 if i named j, conditional on j reciprocating that choice. Besides offering a second definition, the advantage of considering reciprocal friendships is that the fixed choice design is likely to have much less of a "bite". It is reasonable to believe that students name their closest friends first, and are named by them first, or second rather than in any other position. It should be extremely rare to find that two students who were *truly* close and mutual friends in high school, named each other in position 4, 5, and so forth. This conjecture receives strong empirical support from table 1, as discussed earlier.

Estimates for the full specification (equivalent to model 3) but using the reciprocal friendship measure instead are presented as model 4. The results are virtually identical to those I found using the broader friendship definition: on average, a 10 percent reduction in class size is associated with a 10 percent increase in the probability that a randomly selected pair of students is friends.[5] Using this alternative outcome measure has very little impact on the basic conclusion: class size matters for friendship formation, it is not an artifact of the way the data were collected, and operates independently of a large number of institutional and individual background characteristics.

Finally, it is important to keep the unit of analysis in mind when interpreting these results. All of the above results are established on a dyadic level as opposed to a student level. As explained earlier, the main implication of the chosen level of analysis is that the number of dyads in a senior class increases geometrically, rather than arithmetically, with size. Recall the example that compared a class with 50 students (2,450 cross combinations) with a class of 150 students(22,350 pairs of students). Dyads from larger classes thus have a higher weight in the estimations. We also saw that reweighting each dyad by the inverse of the class size minus 1, produces weighted sample moments on a pair level that are identical to those calculated on a student level. Imposing these weights in estimation of probit models 1-4, I find that all estimates are robust to reweighting. Of course, the point estimates for marginal effects (col. iii) change, but relatively –that is in terms of elasticities and average partial effects– there is virtually no difference. The reason is of course that as the smaller classes receive a larger weight due to the reweighting, the (weighted) mean friendship rate increases as well which implies few changes in relative terms. All results established at the dyadic level, therefore carry over to the individual level.

[5]In order to control for the implications of the survey design, the calculations of the probability of mutual choice have to adjusted. If d is again the number of fixed choices made by each of the n students in a class, then the probability of a mutual choice between any arbitrary pair of students is $d^2/(n-1)^2$, assuming that choices are made at random and independently.

E Instrumental Variable Estimates

As opposed to using experimental or randomized data, this study draws on one cross-section of conventional survey data. A valid concern is, therefore, that student allocation to different schools is not a random process. Selection into schools may be based on observed and unobserved (to the econometrician) characteristics correlated with the outcome of interest. Depending upon the precise mechanism by which students sort into schools of varying sizes, selection effects may therefore bias the estimates in favor of size reductions.

As is well-known, selectivity bias is potentially the most serious problem in the literature on the effects of class size reductions. The relevance and severity of the selection problem, however, depends on the context and the structure of the model estimated. To illustrate, in most of the existing studies, the outcome variable is academic achievement, measured by scores on standardized aptitude tests. In this context, parents' choosing schools by choosing residences is conventionally thought to be the single largest source of variation in school inputs [e.g. Hoxby 2000]. The size of bias induced by such self-selection into neighborhoods (and thereby school districts) is related both to the strength of the selection on the outcome and the correlation with other variables included in the model. If selection was unrelated to the outcome measure and other variables in the model, there would be no bias of the coefficients estimated. When the outcome is academic achievement we know that selection into neighborhoods occurs in ways related to the outcome; students' academic ability is strongly correlated with family background.

The case of friendships, I shall argue, has a different quality. Parents select into neighborhoods, but they do not select on something that is (strongly) correlated with their child's propensity to form a friendship. As Jencks and Mayer [1990, p.117] note, "[...] a neighborhood's social composition may not have much effect on individual behavior. Most people prefer friends like themselves. So long as neighborhoods and schools are moderately heterogeneous, most young people can indulge this preference." This is to say that social background and cognitive ability play a role in determining who is friend with whom, in the sense of similar people choosing one another. But, there is little reason to believe that students of lower academic ability or lower social background form fewer friendships. They may have different friends, maybe the "wrong" friends, but they do socialize. This view is supported by the probit models estimated earlier. I did not find evidence that a student's ability or family background had an impact on the probability of friendship choice. What I did find was, as expected, that similar people choose each other as friends more frequently.[6] The point is that social skills are largely orthogonal to cognitive skills or academic ability (see also table 1 in chapter 1).

In sum, I am arguing that while variation in class size is certainly endogenous to *student achievement* in terms of aptitude tests, it is not clear whether it is endogenous to *friendship formation* in a similar way and to a similar extent. Potential biases arising from non-random selection into schools may of course not be excluded a-priori, but, the scope for such effects seems likely to be much smaller in the present context. In order to test whether the size of the senior class in high school may indeed plausibly be treated as exogenous to friendship, the solution entails instrumenting class size.

Instrumental variable estimation requires a (set of) variable(s) that are exogenous determinants of class size but that are not determinants of the probability of forming a friendship. I use variation in population size as a source of exogenous variation in class size. In particular, I instrument log class size with the log number of inhabitants in the town, city, or metropolitan area the respective school is located in. The size of school districts and of the schools within them normally depends on the number of school-age children within district boundaries, which in turn is related to the number of inhabitants. Of course, living in a town, city, or metropolitan area is a decision. Note however, that by instrumenting class size by the size of the entire town, city or metropolitan area the school belongs to, I decide for a fairly high level of aggregation. The implicit assumption is that parents take their choice of town, city, or metropolitan area as given when choosing a school for their daughter or son, but treat their location in terms of school districts (that is, *within* town, city, metropolitan area) as the decision variable.[7]

[6]Since the primary focus lies on analyzing the role of class size for friendship formation, the estimates for the other variables are omitted from table 2. Detailed results are available upon request.

[7]I would argue that this is a sensible assumption given that intracounty mobility is much more common than intercounty, intermetropolitan, or interstate mobility. The residential mobility of the population in the U.S. is such that about twenty percent moved houses in the year 1956-57, and of those families who moved, more than two-thirds moved *within the same county*. These are U.S. averages,

For the log number of inhabitants to be a valid instrument, it (i) must be a determinant of the number of students in a senior class, but (ii) must not be a determinant of the propensity to form a friendship; that is, it must not be correlated with the error term ϵ. Since the population size variable is defined at a fairly aggregated level, it may be a weak instrument for class size; that is, assumption (i) not satisfied. This is, however, something that can explicitly be tested. Table A2 reports results of first-stage regressions of log class size on the instrument alone (model a). Log population size explains 22.3 percent of the variance in class size. In a regression of log class size on the full vector of independent variables plus the instrument (model b), the log number of inhabitants is still highly significant (z-value 7.64). I therefore conclude that the instrument is not weak.

Thus, the credibility of the instrument turns on assumption (ii), an assumption that is inherently untestable, given that I only have one candidate instrument.[8] It must hold that an arbitrary pair of students in one school is no more likely to become friends than an otherwise identical pair of students attending an identical school, with the only difference being that this other school is located in a city of a different size. When I include log population size as an additional regressor in the probits (model c and d, table A2), I find that its estimated coefficients are statistically insignificant; being always larger than the associated standard error. The fact that the instrument is redundant in both equations does not prove exogeneity of course, but it does offer an informal check of the validity of the instrument. Finding the opposite would cast serious doubt on population size being a credible candidate.

The bottom part of table 2 shows estimates from instrumental variable probits of the previous model specifications 3 and 4. The IV estimates are statistically significant and very close to the estimated class size effects coming from the simple probit models. This finding is well in line with the theoretical arguments put forward earlier. To determine whether the differences between probit and ivprobit are indeed insignifi-

I was unable to find figures for the state of Wisconsin separately. However, people in the North-East region (that includes Wisconsin) are generally less mobile than the national average; in addition, the group of married persons aged 35-45 with children (individuals similar to the parents of our respondents) had the lowest proportions of movers, mobility in urban areas is generally higher than in rural areas, but the largest proportion of urban movers are "short-distance" movers. For more detailed figures see the Current Population Reports Series P-20, No82. Bureau of the Census.

[8]With multiple instruments I could test the validity of overidentifying restrictions. The population size variable is, unfortunately, the only (defendable) candidate at my disposal.

cant, I conduct a variant of the familiar Hausman exogeneity test [Hausman 1978]. I run an OLS regression of log class size on all explanatory variables plus the instrument and save the residuals. Then, I reestimate the probits (model 3 and 4, table 2) with the residuals included as an additional regressor. A nice feature of this procedure is that the usual probit z-statistic on the residual is a valid test of the null hypothesis that log class size is exogenous [Wooldridge 2002]. The z-statistic on the residuals is only 0.07 (.058), which is weak evidence against class size being exogenous with respect to friendship formation.

V Discussion

In summary, we find that students in smaller classes have a significantly higher probability of interacting with each other. Our reference pair of student's probability of being associated as friends would be increased by 7-9 percent in response to a 10 percent class size reduction; conditional on the model specification. Averaging the marginal effects across the entire distribution, we find that reducing the senior class population by 10 percent is associated with a 9-10 percent increase in the probability of social interaction among a randomly drawn pair of students. These results are robust: experimenting with a variety of specifications, different outcome measures, and estimation techniques, shows that the basic findings are not sensitive to these experiments.

In order to get an idea about the economic significance of these findings, the following example might help. The WLS interviewed a senior cohort of secondary school students with a total of 15,457 friendships among them. If the above estimates of the APE are correct, it implies that about 1,500 new relations would be formed among students if all classes were to be reduced in size by 10 percent. It appears that keeping schools and school classes relatively small might be more efficacious with respect to a number of dimensions. Schooling, and institutional characteristics of schools malleable by policy, play a central role in the formation of both, cognitive as well as social skills.

Overall, this implies that by focussing on cognitive achievement one may in fact be overlooking important side effects of policy interventions. Looking solely at the role of class size, the evidence presented in this chapter indicates that the effects tend to work in the same direction, in favor of small classes. This hints at significant complementarities in educational production of cognitive and non-cognitive skills; complementarities which have to be taken into account when designing educational policies and evaluating them in terms of their effects.

One drawback of the otherwise rich WLS data is the lack of detailed measures of school characteristics. This precludes a more thorough analysis of what it exactly is about larger classes that induces less social interaction among group members. It is clear that reduced-form estimates, such as those presented above, do not indicate the process by which an exogenous variable affects an outcome and may be misleading if one undertakes policies that change the underlying structural relationships. This shortcoming is something that would need to be addressed in future research with new data, if available.

What, then, could explain an inverse relationship between class size and student relations? There are empirical studies that provide some idea about where to start looking for explanations. I refer again to Postlewaite and Silverman [2004] who reported decreasing student participation in athletics and other school activities in larger schools. Extracurricular activities such as clubs, sports, and student service organizations provide informal and often enjoyable mixing opportunities in settings of relative equality (everyone is the member of the same team) that foster friendship. Besides mere exposure (contact theory), such activities foster the development of social relations and norms for interpersonal behavior through "cooperative interdependence" [Bossert 1989; Johnson and Johnson 1992]. The most effective groups are those organized around a common goal that cannot be achieved independently [Schofield 1995]. Team sports, music and drama, newspapers, or student government are all examples of school activities that involve students working for collective ends. Small schools may be friendlier institutions, capable of involving staff and students in their educational purposes.

To conclude, this study adds to a very large number of previous studies that have examined the relationship between schooling outcomes and organizational characteristics of schools such as class size, expenditure per pupil, teachers' characteristics and so forth. Unlike the present study, none of the preceding studies considered the social outcomes of the schooling process. What we have established in this and the previous chapter is that social competencies are an outcome of schooling and important in determining individuals future success. The data at hand does not allow us to identify the precise mechanisms by which schooling affects the formation of such skills. Still, given the difficulty in establishing a clear link between variables such as class size and schooling outcomes, and the novelty of the approach, the findings presented here deserve some attention.

TABLE A1
SUMMARY STATISTICS OF SELECTED CONTROL VARIABLES

Variable Name	Individual level $N = 9,040$ Mean	Individual level Std. Dev.	Dyadic level $N = 421,328$ Mean [weighted]	Dyadic level Std. Dev.
CLSIZE/100	1.721	*1.341*	2.779 [1.735]	*1.334*
LOG(CLSIZE)	4.782	*.924*	5.454 [4.797]	*.671*
i's LOG(IQ-SCORE)	4.602	*.150*	4.614 [.460]	*.147*
$\|\Delta\|$ LOG(IQ-SCORE)			.160 [.163]	*.124*
i's LOG(SES)	2.525	*.763*	2.655 [2.524]	*.729*
$\|\Delta\|$ LOG(SES)			.745 [.770]	*.606*
i is CATHOLIC	.413	*.492*	.412 [.416]	*.492*
i is PROTESTANT	.472	*.499*	.459 [.470]	*.498*
i is OTHER	.115	*.325*	.129 [.114]	*.342*
i, j SAME RELIGION			.529 [.546]	*.499*
i's AGE	18.150	*.496*	18.158 [18.150]	*.496*
i, j SAME AGE			.639 [.640]	*.480*
i is MALE	.473	*.499*	.467 [.472]	*.499*
PUBLIC HS	.866	*.340*	.868 [.863]	*.338*
PRIVATE HS	.015	*.121*	.010 [.015]	*.098*
CATHOLIC HS	.119	*.323*	.122 [.122]	*.327*

NOTE.— Dyad level weighted means are provided in square brackets; each observation is weighted by the inverse of the class size minus one.

TABLE A2

FIRST-STAGE REGRESSIONS

Model	Dependent variable	Independent variable	Control variables	Coefficient	(std. err.)	$\|t\|$	R-sq
(a)	log(clsize)	log(popsize)		.590	(.030)	19.45	.223
(b)	log(clsize)	log(popsize)	full set	.102	(.013)	7.64	.399
(c)	$i \rightarrow j$	log(popsize)	full set	.001	(.014)	.07	—
(d)	$i \rightarrow j \| j \rightarrow i$	log(popsize)	full set	.012	(.020)	.58	—

NOTE.— Robust standard errors in parentheses. Standard errors are adjusted for clustering on the sender level. The number of observations is 421,328.

Chapter 4

Estimating the Effect of Personality on Male-Female Earnings

(joint with E. Plug)

I Introduction

It is clear that individuals' cognitive abilities play a vital role in generating labor market success. Almost all empirical studies that focus on cognition and earnings find that returns to cognitive ability, measured by standardized test scores, are positive and significant. But we know little about the role of noncognitive traits. Empirical studies that focus on noncognitive traits and earnings are relatively scarce, and those studies that are around focus on traits that are very different; Machiavellianism [Turner and Martinez 1977], self-esteem [Goldsmith, Veum and Darity 1997], aggression-withdrawal [Osborne 2003], to name just a few. Given the diversity of traits studied, their measures and corresponding returns, it is difficult to identify a consistent pattern and make any generalization about the role of noncognitive traits in the labor market.

This paper focuses on personality and earnings, and contributes by incorporating traits from the Five-Factor Model (FFM) of personality structure [Digman 1990; Goldberg 1990] into models of wage determination using data from the Wisconsin Longitudinal Study (WLS). The five personality traits composing the FFM are extroversion, agreeableness, conscientiousness, neuroticism and openness to experience. In addition to estimating how yet another five different personality traits affect earnings, this paper offers three advantages over previous studies.

The first advantage concerns the comprehensiveness in which we model and estimate the role of personality. Psychologists argue that virtually any personality construct can be mapped onto the FFM. Therefore, the five-factor taxonomy may also be of interest to an audience of economists. It may serve as an organizing framework for integrating the existing body of evidence, as well as for structuring future research efforts. The second advantage is our application of the FFM to the gender wage gap. We explicitly allow for gender differences, both in terms of personality traits and corresponding premia/penalties. This enables us to examine to what extent gender differences in earnings are due to differences in masculine and feminine personality traits, as opposed to differences in estimated returns to such traits. The third advantage is the direct comparison that our data allows us to draw between returns to the five personality traits and those to cognitive ability.

Having stressed the advantages of our approach, we do not want to shy away from mentioning the main limitation of our empirical work –a limitation that it shares with most other studies in the field and that is related to availability of reliable and credibly exogenous measures of personality. Reviewing the existing literature on the importance of noncognitive traits, Carneiro and Heckman [2004] express our concern. They note that most personality determinants of earnings studied so far are self-reported ex-post assessments and are likely to be both, causes and consequences of labor market outcomes. However, they also emphasize the value of such studies, shedding new light on the importance of personality traits. Given that research on personality traits is still in its infancy, there is ample room for explorative studies of the kind presented here.

II Five-Factor Model of Personality Structure

According to the Five-Factor Model (FFM), five independent categories are sufficient to describe individual personality differences at the broadest level of abstraction [Costa and McCrae 1992; Goldberg 1990]. The dimensions of the FFM are labelled extroversion, agreeableness, conscientiousness, neuroticism and openness to experience. This categorization does not imply that all personality attributes can be fully reduced to five traits. Rather, the "big five" should be viewed as broad factors underlying a number of related personality facets and sets of even more specific attributes. To provide a better idea, Table 1 lists a number of characteristics related to each one of the five personality dimensions.

The five factor categorization of personality traits is all-pervasive in the current personality and social psychology literature. The FFM was first suggested by studies that tried to organize trait adjectives commonly used to describe people, available from dictionaries of a natural language, into a taxonomic structure [Allport and Odbert 1936; Norman 1963; Tupes and Christal 1961]. The factorial structure has since been retrieved in a large number of studies, cross-validated using a variety of questionnaire scales, and found to generalize across languages and cultures (e.g. McCrae and Costa [1997]). For comprehensive reviews of the historical roots of the FFM, as well as the more recent developments, we refer to Digman [1989, 1990], John and Srivastava [1999], McCrae and Costa [1999], McCrae and John [1992].

In this paper, we adopt the standard economic viewpoint of personality as a bundle of productive attributes valued in the labor market. Earnings follow, as usual, from the kind and amount of traits possessed, and the return that each trait receives in the market. We thereby implicitly assume that personality impacts behavior. This view closely corresponds to that of trait theorists who believe that personality traits constitute basic determining tendencies (e.g. McCrae and Costa [1999]). Determining tendencies are psychological dispositions that evoke "recurrent patterns of acting and reacting that simultaneously characterize individuals and differentiate them from others" (op.cit.; p.140). This interpretation does not imply that traits predispose an individual to behave in exactly the same way, irrespective of the situation. It merely holds that traits make certain behaviors more likely to occur, that is, more frequently observed across a multitude of situations.[1]

Using the FFM as a comprehensive framework to organize traits, there have been multiple studies by organizational and industrial psychologists that examine how the big five personality dimensions relate

[1] The trait perspective, like every theory, is not without its critics. For brevity, we refer to Funder [2001] who reviews all of the major research paradigms in personality psychology such as the behaviorist and social-cognitive paradigms, most critical of trait theory, but also psychoanalytic, biological and evolutionary perspectives.

TABLE 1
THE BIG FIVE PERSONALITY TRAITS

Dimension	Facet (and correlated trait adjective)
Extraversion vs introversion	Gregariousness (sociable) Assertiveness (forceful) Activity (energetic) Excitement-seeking (adventurous) Positive emotions (enthusiastic) Warmth (outgoing)
Agreeableness vs antagonism	Trust (forgiving) Straightforwardness (not demanding) Altruism (warm) Compliance (not stubborn) Modesty (not-show-off) Tender-mindedness (sympathetic)
Conscientiousness vs lack of direction	Competence (efficient) Order (organized) Dutifulness (not careless) Achievement striving (thorough) Self-discipline (not lazy) Deliberation (not impulsive)
Neuroticism vs emotional stability	Anxiety (tense) Angry hostility (irritable) Depression (not contented) Self-consciousness (shy) Impulsiveness (moody) Vulnerability (not self-confident)
Openness vs closedness to experience	Ideas (curious) Fantasy (imaginative) Aesthetics (artistic) Actions (wide interest) Feelings (excitable) Values (unconventional)

NOTE.— This table is adapted from John and Srivastava [1999] and shows Costa and McCrae's [1992] NEO-PI-R Facets.

to labor market outcomes, including job performance [Barrick and Mount 1991; Tett, Jackson and Rothstein 1991], job satisfaction [Judge, Heller and Mount 2002], firm performance [Welbourne, Cavanaugh and Judge 1998] and, most closely related to ours, executive career success [Boudreau, Boswell and Judge 2001], and occupational attainment across the life span [Judge, Higgins, Thoresen and Barrick 1999].

To round-off this section, we would like to stress that the FFM is certainly not all there is to be said about personality, and that researchers should not, "seduced by convenience and seeming consensus, act as if they can obtain a complete portrait of personality by grabbing five quick ratings" [Funder 2001; p.201]. Nevertheless, it is certainly fair to say that the FFM is the most comprehensive categorization of personality traits available to date. And, for the time being, the FFM may turn out to be valuable to economists for the same reason it proved useful to psychologists: by providing a common denominator to integrate findings for a variety of traits studied in isolation.

III Personality and Earnings Differentials

In this section we briefly discuss how personality may affect earnings. We follow a very simple framework outlined in most economic studies on earnings differentials and distinguish three alternative sources to explain differences in pay, being (a) differences in skills; (b) differences in preferences; and (c) a discriminating labor market. Within this framework, we will then draw on a much bigger literature in psychology, especially the empirical part of it, to understand which and in which way particular personality traits matter for earnings.

Differences in Skills.— Human capital theory features prominently in the analysis of wage differentials [Becker 1975; Mincer 1958]. In this framework, systematic variation in earnings arises from differences in productive skills. Productive skills are individual human capital attributes providing a direct input into the production process, and may be anything ranging from innate abilities, to skills that an individual has developed through investments in education, training and work experience. Individuals sell their bundle of skills to firms against an equilibrium market price per unit of skill. Therefore, an individual's overall compensation depends on the kind and amount of skills possessed, and the return that each subcomponent of the skill vector earns in a given occupation. In this vein, one may think of personality as part of an individuals' set of productive traits, valued in the market, with both exogenous (innate) and endogenous (developed over time) components. Of course, this does not mean that personality traits are equally productive across all occupations. If some traits are valued in certain occupations but not in others, we expect to find occupational sorting based on personality, assuming that workers choose those occupations that offer the highest rewards for their traits.

There have been a number of studies in occupational psychology in which personality traits are linked to job performance [Barrick and Mount 1991; Tett, Jackson and Rothstein 1991]. Since job performance is closely related to the economist's notion of productive output, we may associate these personality effects directly with higher earnings. Tett et al. [1991] find that neuroticism and job performance were negatively related across all occupations. Barrick and Mount [1991] report a robust positive correlation between conscientiousness and job performance across all occupation groups. Extroversion predicted job performance within management and sales occupations, that is for jobs involving a strong interpersonal performance component. Thus, the traits that increase performance, and thereby wages, depend on the requirements of the job. This evidence is well in line with sorting theories suggesting that some of the five personality dimensions may predict "extrinsic career success" (e.g. salary) if personality traits fit the psychological requirements of the job [Bretz and Judge 1994; Holland 1997; Tharenou 1997].

Any group differences in personality traits between men and women will translate into gender differences in earnings either directly, through productivity differences, or indirectly, through occupational segregation [e.g. Polacheck 1981]. In this regard, we expect the agreeableness and neuroticism dimensions to be of importance. In a recent literature review, Bouchard and Loehlin [2001] came to the conclusion that agreeableness and neuroticism are the two traits most consistently showing the largest gender differences.

There are alternative theories of earnings determination according to which certain traits/skills still receive a return in a competitive market, although they are not productive skills in the sense defined above.

One example, which we shall explore later on in this paper, is the wage bargaining model. In *Women Don't Ask: Negotiation and the Gender Divide* Babcock and Laschever (2003) argue that personality differences between men and women may lead to differences in pay: women shy away from negotiations, and if they do start negotiating, women ask for less in their opening offer, and tend to concede too quickly in the end.[2]

Differences in Preferences.— In addition to differences in skills, individuals may as well posses different preferences or tastes that are work-related. If these differences are somehow related to someone's personality, it is possible that personality affects earnings indirectly through occupational choice processes. In particular, Tokar, Fischer, and Subich [1998] report results that indicate some overlap between FFM personality traits and vocational preferences. Significant positive associations were generally found of openness with artistic and investigative interests and of extroversion with enterprising and social interests. These findings generalized across genders. Judge and Cable [1997], based on a sample of college graduates applying for jobs with various employers, find that agreeableness relates positively to preferences for supportive, team-oriented organizational cultures, and negatively to aggressive, decisive, and outcome-oriented cultures. Conscientiousness related positively to preferences for detail and outcome oriented, and negatively to innovative cultures.

Labor Market Discrimination.— In light of the large part of the gender wage gap left unexplained by productive traits and vocational preferences, it has been argued that differences in occupational structure and pay may also be a result of labor market discrimination (e.g. England [1982]). In fact, one may even conceive of discrimination against women that starts pre-labor market. This is because subject choice within schools plays a major role in determining subsequent occupational choices and thereby earnings. Females may be discouraged to enter gender-nontraditional fields of study such a engineering, physics and mathematics. Such gender stereotyping may later confine them to traditional service oriented female-type occupations with, generally, lower wages. Although discrimination certainly plays an important role as a

[2]These hypotheses are related to recent experimental work on behavioral differences between men and women which finds that women try to avoid competitive environments, and that women perform worse within competitive environments [Gneezy, Niederle and Rustichini 2003; Niederle and Vesterlund 2005].

determinant of the gender pay gap, it is difficult to separate out empirically the differences in pay that are due to discrimination from differences in unobserved preferences and productive traits (e.g. Bertrand and Hallock [2001]).

With this in mind, the main focus of our empirical work is to determine whether standard earnings equations yield evidence of a pay difference based on personality, and to what extent these differences in pay relate to labor-market sorting. We explicitly focus on gender differentials and ask whether women are treated differently because they are different and posses, on average, more feminine and less masculine personality traits, or because they face different rewards and penalties for their prototypical traits.

IV Method

A Data and Sample

Our analysis again employs the Wisconsin Longitudinal Study (WLS) of 10,317 randomly sampled graduates from Wisconsin high schools in 1957. Unlike other large longitudinal studies of school-based samples, the WLS contains personality measures together with information on respondents' labor market careers. This allows us to work with a much larger sample than comparable studies do in the psychological literature. We use data on personality traits from the 1992 mail questionnaire sent to 8,493 members of the original survey. This questionnaire collects information on respondents' personality traits based on the Big Five Inventory (BFI) developed by John, Donahue and Kentle [1991]. The BFI was specifically designed to facilitate the collection of personality data in surveys using a short test instrument that allows efficient assessment of the five dimensions when there is no need for a more differentiated measurement of individual facets [John and Srivastava 1999].

Of the initial 10,317 random sample of high school graduates, 8,493 received the 1992 mail questionnaire; 6,875 individuals responded, and 6,692 individuals gave at least two complete answers to the separate items that correspond to each personality trait. Thus, nonresponse is a potential threat to the validity of our analysis although, compared to other studies covering a similarly long time span, the response rate is fairly high. The population under study is then restricted to men and women who are in employment in 1992, which reduces our sample to 6,062 observations. We further exclude all workers who are self-employed, work less than 20 hours per week, earn less than one dollar per hour, and all those for whom data on the various control variables are unavailable. In the end, we are left with a sample of 5,025 observations. Descriptive statistics for both, the male (N=2,424) and female (N=2,601) subsamples are provided in Table A1.

B Measurement

The personality test instrument used in the WLS assesses the various dimensions by means of self-ratings on 29 questionnaire items. It is an abbreviated version of the original 44-item BFI [John et. al., 1991]. Each dimension is assessed by 6 items, except for neuroticism-emotional stability which is assessed by 5 items. Items are statements such as “I see myself as someone who is talkative” or “I see myself as someone who is easily distracted”. Individuals are asked to rate to what extent these statements apply to themselves on a 6-point scale ranging from “agree strongly (1)” to “disagree strongly (6)”; see appendix A for a detailed description of all questionnaire items. The single item responses are then coded into average scores.

Any research based on measurement must confront the reliability of its measures. We quantify the size of the measurement error by calculating reliability coefficients for the BFI scales, often referred to as Cronbach’s alpha reliabilities [Cronbach 1951]. Derivations are provided in Appendix B. We found some notable differences: extroversion .76, agreeableness .71, conscientiousness .66, neuroticism .77, and openness .60. The reliabilities of the abbreviated scales averaged .70, which suggest that a considerable fraction of the variability in the reported traits is due to measurement error.

It is possible to compare these numbers with previous estimates of reliability ratios. John and Srivastava [1999] report that reliabilities of the original 44-item BFI scales typically lie between .75 and .90 and are on average above .80. These estimates indicate that the internal consistency of the BFI scales is on average high, and about ten percentage points higher than those we find. This does not necessarily mean that the original BFI scales are better. Reliability ratios increase with the number of items. The abbreviated BFI scales we use include only five to six items. If we had eight to ten items, as the original scales do, the estimated reliability ratios would range from .69 to .83, average out at .78, and thus be very similar to the ones previously estimated.[3]

To measure general intelligence, we use test scores on the *Henmon-Nelson Test of Mental Ability* that respondents took in 1957 while attending high school. Unfortunately, we do not have access to individual test items which precludes the option of estimating reliability coefficients ourselves. But, in contrast to the BFI, the Henmon-Nelson test was implemented as originally designed. We can therefore safely rely on estimates available in the literature. The psychometric properties of the test are well established with reliability ratios ranging from .87 to .94 [Buros 1959].[4] In addition to that, potential measurement error due to time and cohort effects are ruled out by the very nature of our data.

C Estimation

We first estimate a standard log-linear earnings equation separately for men and women in the form

$$Y_{im} = X'_{im} b_m + \epsilon_{im}, \quad Y_{if} = X'_{if} b_f + \epsilon_{if} \tag{4.1}$$

where i, m and f subscripts individual and gender groups, Y denotes the logarithm of hourly earnings, X is a vector of covariates including the five personality measures and control variables assumed to affect

[3]The reliability ratio R_0 of any given personality measure is defined by

$$R_0 = \frac{k_0 \rho}{1 + (k_0 - 1)\rho}$$

where k_0 and ρ represent the number of items and the average inter-item correlation, respectively. Let R_1 be the reliability ratio of the same personality measure but measured with Δk additional items. With ρ fixed, we can express R_1 in terms of k_0, R_0 and Δk as follows

$$R_1 = R_0 \left(\frac{k_0 + \Delta k}{k_0 + R_0 \Delta k} \right)$$

It is easy to see that reliability ratios increase with the number of items. For example, if a reliability ratio of .71 is obtained using 9 instead of only 6 items, as is the case for our original agreeableness measure, the ratio would rise to .79.

[4]Zax and Rees [2002; p.603] report that, based on publicly unavailable WLS data, Robert Hauser estimated the reliability ratio to be between .92 and .95. In our later calculations we will impose the average of .935.

earnings, and ϵ is the remaining error. The parameter vector b contains the estimates of how the labor market would value different characteristics.

While these equations determine whether personality traits matter for generating earnings, and whether they affect earnings differently for men and women, it does not tell us how large a role these differences play in explaining the gender gap in earnings. To address that, we decompose the gender gap into two components; one that can be attributed to differences in observable personality traits between men and women, and a second, that can be attributed to differences in trait permia/penalties between men and women. To calculate the latter component, we must choose which set of coefficients to use as the standard of comparison (male or female). We follow Neumark [1988] and Oaxaca and Ransom [1994] and use the common coefficients estimated from a pooled regression of men and women ($\widehat{b}_p$). If we express the difference in earnings between men (m) and women (f) in terms of averages

$$\bar{Y}_m - \bar{Y}_f = \bar{X}_m' \widehat{b}_m - \bar{X}_f' \widehat{b}_f, \tag{4.2}$$

where $\widehat{b}_m$ and $\widehat{b}_f$ are the estimates from (4.1), the earnings differential can be separated into two components

$$\bar{Y}_m - \bar{Y}_f = [\bar{X}_m - \bar{X}_f]' \widehat{b}_p + [\bar{X}_m' (\widehat{b}_m - \widehat{b}_p) - \bar{X}_f' (\widehat{b}_f - \widehat{b}_p)] \tag{4.3}$$

The first term can be interpreted as the part of the earnings differential that is due to differences in observed characteristics, and the second, as the part due to differences in estimated parameters. Decompositions like these have a long tradition in studies of wage differentials beginning with the work of Oaxaca [1973], who interpreted the differences in returns between men and women with similar characteristics as a measure of labor market discrimination. We do not want to adhere to this particular interpretation. As noted earlier, different parameters may just as well come from (unobserved) differences in preferences and productive skills.

V Results

A Personality and Male-Female Earnings

Table 2 reports OLS estimates of the relationship between our measures of the five personality traits and the log of hourly earnings, separately for men and women. For reasons of comparability, we have standardized each trait scale on the full estimation sample to have zero mean and unit variance. The same transformation is applied to IQ-scores. Panel A and B of Table 2 show results for samples of employed men and women, respectively. Each panel contains four OLS estimates, with varying sets of covariates.

We begin by discussing the personality effects on earnings of men. In column (i) we estimate a baseline specification in which the five personality traits are the only right-hand-side variables. We find that antagonistic, emotionally stable and open men enjoy significant and substantial earnings advantages. Of all five personality traits, openness to experience seems to be the most rewarding whereas extroversion and conscientiousness generate no returns at all. In column (ii) we add the childhood IQ test scores to the regression to somehow control for the respondents' cognitive ability. With this IQ measure added, the returns to being antagonistic, emotionally stable and open fall, but remain positive and statistically significant.[5] In column (iii) we add several other covariates including years of schooling, work experience and tenure, region, and other individual and family characteristics. Including these variables reduces the estimated coefficients for non-agreeableness, emotional stability and openness effects, yet results remain qualitatively similar.

It is not clear whether respondents' occupation (and industry) affiliation are appropriate variables to include in our wage regressions. If we believe that workers are selected into certain jobs on the basis of specific personality profiles, we would probably not want to control for

[5]Based on psychometric and experimental studies, psychologists argue that there is no meaningful relation between personality and intelligence. However, there is evidence that actual performance on IQ tests is related to some dimensions of personality. It has been found, for example, that introverts show more vigilance and less fatigue during extended tests. Also, feelings of anxiety (a facet of neuroticism) are known to affect test performance if the test subjects the individuals to considerable stress (e.g. time pressure). Our proxy variable for intelligence might be picking up this performance effect to some extent. For an exhaustive treatment of the relation between personality and intelligence, see Sternberg and Ruzgis [1994]

TABLE 2
THE EFFECTS OF PERSONALITY ON MALE-FEMALE EARNINGS

	(i)		(ii)		(iii)		(iv)	
	A. Males, log hourly earnings ($N = 2,424$)							
Personality traits:								
extroversion	– 0.002	*0.012*	0.019	*0.012*	0.014	*0.011*	0.009	*0.010*
agreeableness	– 0.064	*0.012****	– 0.047	*0.012****	– 0.036	*0.011****	– 0.037	*0.010****
conscientiousness	– 0.006	*0.012*	0.009	*0.012*	0.003	*0.011*	– 0.002	*0.010*
neuroticism	– 0.050	*0.013****	– 0.032	*0.012***	– 0.022	*0.011***	– 0.020	*0.011**
openness	0.104	*0.012****	0.058	*0.012****	0.033	*0.011****	0.024	*0.011***
IQ-scores	—		0.179	*0.011****	0.098	*0.011****	0.065	*0.011****
adjusted R^2	0.05		0.14		0.29		0.45	
F-test personality traits	24.39		11.35		5.37		4.39	
	B. Females, log hourly earnings ($N = 2,601$)							
Personality traits:								
extroversion	– 0.034	*0.011****	– 0.022	*0.011***	– 0.004	*0.010*	0.005	*0.009*
agreeableness	– 0.031	*0.012****	– 0.023	*0.011***	– 0.005	*0.010*	– 0.008	*0.009*
conscientiousness	0.030	*0.011****	0.028	*0.011***	0.025	*0.010****	0.023	*0.009****
neuroticism	– 0.035	*0.012****	– 0.017	*0.011*	– 0.018	*0.010**	– 0.006	*0.009*
openness	0.122	*0.011****	0.092	*0.011****	0.043	*0.010****	0.027	*0.010****
IQ-scores	—		0.127	*0.011****	0.066	*0.010****	0.051	*0.010****
adjusted R^2	0.06		0.11		0.31		0.40	
F-test personality traits	36.14		18.76		7.88		4.52	
Controls:								
Individual, human-capital, region	—		—		×		×	
Occupation, industry, job characteristics	—		—		—		×	

NOTE.— Standard errors in italics; ***significant at 1% level; **significant at 5% level; *significant at 10% level. F-tests indicate whether estimated coefficients for the big five personality traits are jointly significant.

occupations while estimating the effect of personality on earnings. When controlled for, we expect job-holding to partially mediate the significant effects of personality on earnings. In column (iv), where we added 8 one-digit industry and occupation dummies to the previous specification, returns to non-agreeableness and emotional stability remain virtually identical. Returns to openness fall some, but remain statistically significant.[6] It appears that antagonistic and open men always earn more. Across all specifications, the agreeableness-antagonism dimension has the most persistent effect on earnings. One standard deviation increase raises the hourly earnings for antagonistic male workers on average by 4 to 6 percent.

For measured cognitive ability, we find strong positive effects which are reduced substantially when we add human capital characteristics in the third column. The magnitude is of the order 7 to 17 percent due to a one standard deviation change in IQ-scores, conditional on the particular set of covariates entered. It appears much larger than any of the trait premia/penalties viewed in isolation, which range from 0 to 6 percent. However, a favorable personality profile potentially leads to equally strong earnings effects as cognitive ability if personality is viewed as bundle of traits. Nonetheless, personality does not predict earnings as well as our cognitive ability measure. In isolation, the five personality measures explain about 5 percent of the variance in earnings. The addition of IQ test scores improves the R-squared by almost 10 percentage points.

And what about women? In column (i) of Table 2, where we estimate the earnings specification without controls, we find that all five personality traits associate significantly with earnings. Three personality estimates are very similar to those found for men. Antagonistic, emotionally stable and open women earn higher wages. Two personality estimates are different. Unlike men, women appear to be penalized for being extrovert while they receive a premium for being conscientious. If we include IQ test scores in column (ii), we find that all partial personality effects decrease only marginally. When we start adding

[6]For information on individuals' job-holding we rely on the standard classification system of the U.S. Bureau of the Census, using 1990 occupation and industry definitions at the one-digit level. Of course, it is possible that personality results would change if we used more fine-tuned information on industry or occupation characteristics. As a simple test, we replaced the one-digit level dummies with sets of occupation and industry indicators defined at the two-digit and three-digit level. We find that our parameter estimates are robust to the inclusion of more detailed indicators of respondents' job-holding.

more controls, however, results appear to be sensitive. In columns (iii) and (iv) only returns to openness and conscientiousness are consistently significant and positive for women.[7] A one standard deviation increase in either of the two traits is associated with a 2 to 3 percent increase in hourly earnings. The combined premia of openness and conscientiousness compare in magnitude to the earnings effect of measured cognitive ability. With the full set of controls entered, a one standard deviation increase in IQ raises hourly earnings by about 5 percent. Comparison of column (i) and (ii) further shows that personality and cognitive ability are roughly equally important in explaining the variance in earnings. The five personality measures together account for approximately 6 percent of the earnings variation; adding IQ test scores to the baseline specification in column (ii) improves the R-squared by 5 percentage points.

Overall, these results suggest that personality predicts earnings for both men and women. A favorable personality profile –a distinct bundle of traits rewarded in the market– appears to have an impact on earnings that is comparable to that of cognitive ability. Of course, we are not arguing to have identified causal effects of personality traits on earnings. We merely show that personality adds explanatory power to our model. All our F-tests indicate that the big five traits are jointly statistically significant. In terms of the magnitude of additional variance explained, the contribution is similar to that of cognitive ability; significant, but modest.[8]

There are, to our knowledge, two studies in the psychological literature that also investigate the relationship between the big five traits and earnings [Boudreau, Boswell and Judge 2001; Judge, Higgins, Thoresen and Barrick 1999]. Both studies employ American data and are therefore most valuable for comparative purposes.[9] Boudreau, Boswell

[7] As for men, results are insensitive to replacing the one-digit occupation and industry dummies with indicators defined at the more detailed two or three-digit level.

[8] The magnitudes are, for example, much smaller than those conventionally found for education. According to the specification in column (iii), the traditional "return to education", as measured by the earnings effect of an additional year of schooling, is estimated to be .064 for men and .066 for women. When we replace the years of schooling variable with its standardized equivalent, we find point estimates of .149 [13.73] and .153 [13.18] for men and women, respectively; with t-ratios shown in brackets.

[9] After the first draft of this paper was written, it came to our attention that Nyhus and Pons [2005] were analyzing the effect of big five personality traits on earnings using a sample of about 800 male and female workers in The Netherlands. In a specification comparable to our model in column (iii), they mainly find that emotional stability is positively associated with wages of both women and men.

and Judge [2001] study the effects of personality on intrinsic and extrinsic career success based on samples of American and European executives. For the American sample, consisting primarily of white males in their late forties, they find that agreeableness and neuroticism relate negatively to remuneration, with extroversion and conscientiousness having little or no impact and openness to experience associating positively. The highly selective nature of the sample places limits on possible generalizations, most importantly with respect to the effects one should expect for women. Judge et al. [1999] also find that agreeableness and neuroticism have a negative effect on earnings. Extroversion and conscientiousness associate positively with earnings, but the positive effect of the openness dimension disappears with the full set of conditioning variables entered. Some caution is again in order when generalizing these findings to broader populations, as they are based on a sample only slightly in excess of one hundred observations.

B Limitations

While our results are comparable to those obtained by previous studies, we should treat our findings with care. The parameter estimates presented in Table 2 may be subject to a number of sources of bias: measurement error in the BFI scales, selective non-response, misspecification, and simultaneity between wages and personality traits.

Measurement Error.— Our first concern relates to the possible attenuating effects of measurement error. If personality effects seem only modestly important, it is quite possible that our personality traits are measured with error. After all, random error will bias any estimated effect to zero. One way to correct for such error is to adjust the parameter estimates and standard errors by imposing reliability ratios in estimation (see Appendix C). Panel A of Table 3 presents parameter estimates that are adjusted for the effects of measurement error.[10] The estimated effects remain qualitatively very similar, except that they are almost all larger than the corresponding point estimates in Table 2. The increase is substantial and often significant. Assuming that there is no serial

They do not analyze a gender wage decomposition.

[10]We only allow for unreliability in the measurement of the five personality traits and the *Henmon-Nelson* IQ-scores. Note further that in Table 2 it was useful to see how coefficients changed as additional covariates were added. This is not as important when we test for the effects of measurement error. We therefore show only two specifications that correspond to columns (ii) and (iii) of Table 2.

correlation among the measurement errors across the five personality scales, our results suggest that unreliability in trait measurement indeed leads to a considerable underestimation of the corresponding premia and penalties.[11]

Selective non-response.— A related concern derives from the fact that respondents are kept in the sample if they provided at least two complete answers to the question sets that correspond to each personality trait. It is possible that selective non-response introduces inconsistencies when we estimate our regression models. In order to see how sensitive our results are, we calculate reliability ratios and run OLS regressions on a sample of workers who responded to all items. As Panel B of Table 3 shows, this restriction induces a loss of 651 observations but does not affect the results substantially. This means that the trait coefficients, as well as the estimated reliability ratios, are not sensitive to item non-response.

Nonlinearities.— The third issue relates to whether or not the relationship between the big five traits and hourly earnings is nonlinear. When it comes to personality, it is not a priori clear that more is necessarily better. If, for example, the labor market values people who are only moderately extrovert and punishes those who are too introvert or too extrovert, it is possible that the linear specification is pushing estimated average returns to zero. In Panel C of Table 3, we test for nonlinear personality effects by replacing the reported trait scores with sets of trait level dummies. For each personality trait, we transform the average reported scores into quartiles, and create three corresponding dummy variables for whether or not the personality scores are in the top or bottom 25 percent of the distribution. The middle 50 percent of the scores is the omitted category. With personality traits measured in levels, we observe that not all of the individual dummy coefficients are significantly different from zero. However, for those personality traits which mattered in the linear specifications, we find that many individual dummy variables are significant and show a consistent monotonic pattern. These results suggest that for the traits that mattered previously,

[11] We have skirted around the more subtle issue of subjectivity in self-reported data. Bertrand and Mullainathan [2001] discuss how cognitive factors and the social nature of the survey procedure may affect the way people answer questions, and how subjectivity may be treated in a measurement-error framework. We cannot explore this issue empirically with the data at hand, but refer to Costa and McCrae [1988] who present evidence that the convergence between self and peer or expert-ratings is, on average, between .80 and .90.

TABLE 3
SENSITIVITY ANALYSES

	Males				Females			
Personality traits:	(i)		(ii)		(i)		(ii)	
	A. Effects of Personality on Earnings Corrected for Measurement Error							
extroversion	0.009	*0.019*	0.009	*0.018*	– 0.067	*0.017****	– 0.028	*0.016*
agreeableness	– 0.085	*0.021****	– 0.067	*0.019****	– 0.035	*0.020**	– 0.014	*0.018*
conscientiousness	0.023	*0.023*	0.011	*0.021*	0.055	*0.021****	0.045	*0.018***
neuroticism	– 0.042	*0.020***	– 0.033	*0.018**	0.005	*0.018*	– 0.007	*0.017*
openness	0.103	*0.025****	0.063	*0.025***	0.189	*0.025****	0.100	*0.026****
IQ-scores	0.179	*0.013****	0.102	*0.013****	0.111	*0.013****	0.063	*0.011****
R^2	0.16		0.30		0.15		0.33	
F-test personality traits	10.81		5.28		18.17		7.72	
	B. Effects of Personality on Earnings using the Full-Response Sample							
extroversion	0.006	*0.020*	0.008	*0.019*	– 0.054	*0.020****	– 0.023	*0.018*
agreeableness	– 0.087	*0.022****	– 0.067	*0.021****	– 0.042	*0.022**	– 0.021	*0.020*
conscientiousness	0.021	*0.024*	0.011	*0.022*	0.057	*0.022***	0.051	*0.020***
neuroticism	– 0.043	*0.021***	– 0.036	*0.019**	0.003	*0.020*	– 0.009	*0.018*
openness	0.108	*0.027****	0.065	*0.026***	0.177	*0.028****	0.090	*0.030****
IQ-scores	0.180	*0.014****	0.105	*0.013****	0.111	*0.014****	0.066	*0.012****
R^2	0.16		0.30		0.13		0.31	
F-test personality traits	9.66		4.86		14.17		6.29	
N	2,149		2,149		2,225		2,225	

— continued on next page —

Personality traits:	Males (i)		Males (ii)		Females (i)		Females (ii)	
	C. Testing for Nonlinear Effects of Personality on Earnings							
extroversion								
bottom 25 percent	– 0.016	*0.026*	– 0.012	*0.024*	0.015	*0.025*	– 0.004	*0.023*
top 25 percent	0.050	*0.028**	0.023	*0.026*	– 0.026	*0.025*	– 0.010	*0.022*
agreeableness								
bottom 25 percent	0.067	*0.024****	0.060	*0.026****	0.049	*0.025**	0.000	*0.022*
top 25 percent	– 0.050	*0.033*	– 0.029	*0.030*	0.008	*0.025*	0.009	*0.022*
conscientiousness								
bottom 25 percent	– 0.004	*0.027*	– 0.000	*0.024*	– 0.043	*0.025**	– 0.049	*0.022***
top 25 percent	0.023	*0.028*	0.021	*0.025*	0.014	*0.025*	0.003	*0.022*
neuroticism								
bottom 25 percent	0.058	*0.036***	0.036	*0.024*	0.007	*0.026*	0.006	*0.023*
top 25 percent	– 0.050	*0.026**	– 0.038	*0.027*	– 0.047	*0.025**	– 0.045	*0.022***
openness								
bottom 25 percent	– 0.070	*0.026****	– 0.033	*0.023*	– 0.112	*0.025****	– 0.074	*0.022****
top 25 percent	0.082	*0.028****	0.049	*0.027**	0.147	*0.026****	0.055	*0.024***
IQ-scores								
bottom 25 percent	– 0.217	*0.028****	– 0.131	*0.024****	– 0.158	*0.024****	– 0.081	*0.022****
top 25 percent	0.268	*0.028****	0.119	*0.026****	0.175	*0.025****	0.083	*0.023****
adjusted R^2	0.12		0.29		0.10		0.31	
F-test personality traits	5.93		2.72		25.07		7.83	
Controls	—		×		—		×	

NOTE.— Standard errors in italics; ***significant at 1% level; **significant at 5% level; *significant at 10% level. In Panel (1) reliability ratios imposed in estimation: extroversion .76, agreeableness .68, conscientiousness .63, neuroticism .77, openness to experience .60; *Henmon-Nelson* iq-scores .94; In Panel B the sample is restricted to workers who respond to all personality items. Corresponding reliability ratios imposed in estimation: extroversion .77, agreeableness .69, conscientiousness .64, neuroticism .77, openness to experience .60; In Panel C the (omitted) reference categories are the 2^{nd} and 3^{rd} quartile of the respective trait distribution; F-tests indicate whether estimated coefficients for the big five personality traits are jointly significant. The set of controls includes all variables on individual, human-capital and region characteristics as detailed in Table A1.

a linear representation is a pretty accurate approximation of the overall relationship. For the traits that did not affect earnings in previous specifications, the fluctuations we observe are difficult to reconcile with any consistent pattern.

Reversed Causation.— The fourth and our biggest concern is that of endogenous personality measures. Since in our data personality traits were assessed at the same time as hourly earnings, we do not know whether personality is the cause or the consequence of earnings. To the extent that trait measures are endogenous, our parameter estimates will be upward biased because they capture both effect and result. In what follows, we will argue that the extent of the bias may not be as severe as it appears at first sight.

Personality traits are both inherited and formed. Reviewing a large number of twin studies, Bouchard and Loehlin [2001] find that 40 to 60 percent of the variation in personality is generally attributable to genetic differences between individuals. The inherited part, which is substantial, can be treated as predetermined with respect to earnings. The concern about endogeneity bias derives, of course, from the fact that part of one's personality is developed over time and shaped by labor market experiences.

The current state of knowledge indicates that the formation of personality occurs primarily during early childhood and adolescence, that personality is largely set by age 30, and that it remains fairly stable thereafter (see the reviews by Caspi and Roberts [1999]; Costa and McCrae [1994, 1997]; Digman [1989]). The evidence on stability of mean levels in big five traits, 'absolute continuity', is pervasive. Moreover, measures of personality traits are found to exhibit strong 'differential continuity', meaning that individuals tend to preserve their relative position within the respective trait distribution as they age (e.g. Costa and McCrae [1988]). Overall, the big five personality traits are heritable and enduring individual predispositions, second in stability only to measures of cognitive ability [Conley 1984].

Our study is based on a single cohort of equal age individuals in their early fifties. It therefore offers the clear advantage that sample members are homogenous in terms of age and timing of personality measurement. We referred to evidence saying that mean trait levels change only imperceptibly over time and that individuals generally maintain their own rank order within the group. This implies that, even if personality changed as people age, it is unlikely that the corresponding estimates are driven much by the simultaneity between wages and our personality regressors. We are aware that these arguments do not prove that endogeneity bias is absent. With the data at hand, it is impossible to remove this bias. It is appropriate, though, to interpret our estimates as upper bounds of true personality effects.

To summarize, we find that our estimated personality returns (a) increase substantially when adjusted for measurement error; (b) are not caused by selective non-response; (c) are not an artifact of the linear specification; but (d) remain upper bounds of actual personality effects due to the endogeneity of our personality regressors. The sensitivity tests we performed reinforce our earlier conclusion. The impact of personality on earnings is significant and comparable to the impact of differences in cognitive ability, and though it is not large, it is not trivial either.

C Discussion

How do our findings in terms of FFM traits relate to other personality variables studied in the economic literature? A number of recent studies investigate whether personality traits account for differences in labor market success [Bowles, Gintis and Osborne 2001; Duncan and Dunifon 1998; Dunifon, Duncan, and Brooks-Gunn 2001; Goldsmith, Veum and Darity 1997; Osborne 2003], building on earlier work by Andrisanni [1978], Filer [1981, 1986], Jencks [1979], and Turner and Martinez [1977]. In this section we briefly discuss our own findings in relation to some of the preceding studies.

We start-off with one of the earliest contributions. Based on a survey conducted by the National Opinion Research Center (NORC), Turner and Martinez [1977] examine the influence of the “Machiavellian” personality on socioeconomic attainment. Machiavellianism, that is being cool, distant and treating people as objects to be manipulated, can be related to the negative pole of the agreeableness dimension [Paulhus and Williams 2002]. For men with above average education, Turner and Martinez find that Machiavellianism is associated positively with income. Inverting our estimates for the agreeableness scale, we observe a similar relationship.

Dunifon et al. [2001] examine the role of organization and efficiency in affecting earnings for fathers and their children. Based on the Panel Study of Income Dynamics (PSID), they use a measure of household cleanliness to proxy these traits, and find for both fathers and children (including daughters) that living (being raised) in a clean and organized house is positively related to hourly earnings obtained 25 years later. The authors do not report results for mothers, nor for sons and daughters, separately. Treating conscientiousness as a direct measure of being organized and efficient, we find statistically significant effects for women, but not for men.

Based on data from the National Longitudinal Survey of the Youth (NLSY), Goldsmith, Veum and Darity [1997] find that self-esteem is positively and significantly associated with higher wages. Self-esteem, or the lack thereof, is clearly a facet of the neuroticism dimension. Unfortunately, Goldsmith et al. pool their male and female subsamples and do not provide estimates of gender differences in returns. In an analysis not shown in this paper, we estimate our earnings models on a pooled sample of men and women and, like Goldsmith et al., find a statistically significant negative effect for being neurotic. On the basis of our separate equations for men and women, we find that high levels of neuroticism (low self-esteem) are associated with lower earnings for men, but not for women.

A very recent example, and one that explicitly takes gender differences into account, comes from Osborne [2003]. Using U.K. data from the National Child Development Study (NCDS), she examines whether measures of aggression and withdrawal are differentially rewarded across genders. Aggression has a dominance and lack of impulse control component and can therefore be viewed as a combination of extroversion and neuroticism facets. Withdrawal is highly related to compliance, that is agreeableness, but may also reflect aspects of introversion. According to Osborne, women appear to face significantly larger penalties for aggression, while men are more heavily penalized for withdrawal. We find that men are penalized for agreeableness across all specifications while introversion is found to have virtually no effect. For women, extroversion and neuroticism are both penalized in specifications without controls, however, as further controls are added these effects disappear.

Of course, those above are selected examples, but they are suggestive of the potential that lies in the FFM as an organizing framework. We agree with Bowles, Gintis and Osborne [2001], who conclude that we are unlikely to find a noncognitive personality analogue to the common g-factor *underlying* most measures of cognitive performance (in a factor-analytic sense). But, the FFM may assume a similar function by providing a common denominator, enabling comparisons of a multitude of variables that have been studied in isolation. It might bring the necessary structure to our inquiry into the role of personality traits in the labor market.

D Decomposing the Gender Gap

In the tradition of most empirical work on wage differentials, we focus on two major mechanisms explaining differences in pay: (a) differences in characteristics, and (b) differences in the corresponding premia and penalties.

Gender Differences in Personality Traits.— In the first column of Table 4 we test for average gender differences in personality characteristics using standardized trait scales to make the gender differences more readily visible. We find that women are significantly more agreeable, neurotic, extrovert and open, with differences in the first two traits being the largest. Gender means for neuroticism and agreeableness lie 20 to 40 percent of a standard deviation apart. These findings are consistent with evidence from the psychological literature (e.g. Bouchard and Loehlin [2001]).

Gender Differences in the Earnings Premia and Penalties.— The next three columns of Table 4 present gender differences in personality returns based on error-corrected estimates.[12] According to the baseline model in column (ii) we find that women receive higher returns for the traits introversion, agreeableness, conscientiousness, neuroticism and openness. As we include more controls (columns iii and iv), agree-

[12]Since it is interesting to see what happens to these estimated differences when we take into account other sources of variation, we compute differences in parameter estimates based on the specifications shown in columns (ii)-(iv) of Table 2.

TABLE 4

MALE-FEMALE DIFFERENCES IN PERSONALITY TRAITS AND COEFFICIENTS

	Characteristics ($\bar{X}_m - \bar{X}_f$)		Coefficients ($\hat{b}_m - \hat{b}_f$)					
	(i)		(ii)		(iii)		(iv)	
Personality traits:								
extroversion	– 0.120	*0.028****	0.075	*0.025****	0.037	*0.024*	0.012	*0.023*
agreeableness	– 0.396	*0.027****	– 0.050	*0.029**	– 0.052	*0.027***	– 0.049	*0.025**
conscientiousness	– 0.043	*0.028*	– 0.033	*0.030*	– 0.034	*0.028*	– 0.041	*0.027*
neuroticism	– 0.202	*0.028****	– 0.047	*0.027**	– 0.025	*0.025*	– 0.036	*0.024*
openness	– 0.062	*0.028***	– 0.086	*0.035***	– 0.037	*0.036*	– 0.016	*0.036*
IQ-scores	– 0.030	*0.028*	0.068	*0.018****	0.039	*0.017***	0.016	*0.016*
Controls:								
Individual, human-capital, region			—		×		×	
Occupation, industry, job characteristics			—		—		×	

NOTE.— Standard errors in italics; ***significant at 1% level; **significant at 5% level; *significant at 10% level.

ableness remains the only dimension that shows statistically significant differences in labor market valuation across genders; with the estimated penalties for men driving these differences. When it comes to measured cognitive abilities, we find that differences in returns to IQ-scores are insignificant. Since the magnitude and direction of the effects happen to be very similar across genders, the difference is virtually zero.

It is further interesting to note that, except for extroversion, the penalties (returns) to males tend to be larger for those personality traits for which males have the lower (higher) means; the converse holding for women.[13] This can easily be verified by checking that the following interaction term $(\bar{X}_m - \bar{X}_f)'(\widehat{b}_m - \widehat{b}_f)$ is strictly positive. Apparently, it is not universally better to be masculine (absolute advantage), but that individuals with masculine traits have a comparative advantage under a male wage; and those with feminine traits under a female one.

Decomposition Results.— In Table 5 we report earnings decompositions based on error-corrected parameter estimates from male and female earnings equations for the same three specifications as before. The overall differential, that is the difference in logarithms of hourly wages between men and women, amounts to .58. The magnitude of the gap is large, but is not unusual for the particular generation of men and women under study.

In rows 2 and 3 we start decomposing the wage gap into the share that is attributable to differences in characteristics versus differences in coefficients. It is clear that the part of the wage gap explained increases as more regressors are added. Our primary focus lies of course on the decomposition results for personality traits in the fourth and fifth row. Based on the first specification (column i), we find that 16 percent of the gender gap can be attributed to differences in mean personality traits, and about 13 to differences in labor market rewards/penalties. When additional variables are introduced in column (ii) and (iii), these numbers fall to 10 and 8 percent, and 7 and 5 percent, respectively. Note that the effects work in opposite directions such that the overall differential is only moderately affected. Overall, only 3 to 4 percent of the gender gap is explained by differences in personality including differences in traits and trait returns. This number is about the same in every column.

[13]Finding women slightly more extrovert than men appears odd, at first sight. However, extroversion has both dominance and sociability facets, and our abbreviated test instrument may be picking up the sociability component to a larger extent, therefore women scoring higher on this trait scale.

TABLE 5
DECOMPOSITION RESULTS FOR PERSONALITY TRAITS

	(i)		(ii)		(iii)	
Difference:						
(1) log hourly earnings	0.587					
Differences due to:						
(2) characteristics	0.091	*15.6%*	0.307	*52.3%*	0.403	*68.7%*
(3) coefficients	0.495	*84.4%*	0.280	*47.7%*	0.184	*31.3%*
Differences due to:						
(4) personality characteristics	0.095	*16.2%*	0.060	*10.3%*	0.043	*7.3%*
(5) personality coefficients	– 0.074	*12.7%*	– 0.045	*7.7%*	– 0.027	*4.5%*
Differences due to characteristics:						
(6) extroversion	0.006	*1.0%*	0.002	*0.3%*	– 0.000	*0.1%*
(7) agreeableness	0.076	*12.9%*	0.048	*8.1%*	0.035	*5.9%*
(8) conscientiousness	– 0.003	*0.5%*	– 0.002	*0.3%*	– 0.001	*0.2%*
(9) neuroticism	0.023	*4.0%*	0.016	*2.8%*	0.011	*1.9%*
(10) openness	– 0.007	*1.2%*	– 0.003	*0.6%*	– 0.001	*0.2%*
(11) IQ-scores	– 0.004	*0.7%*	– 0.002	*0.3%*	– 0.002	*0.3%*
(12) other characteristics			0.248	*42.2%*	0.361	*61.7%*
Differences due to coefficients:						
(13) extroversion	– 0.003	*0.4%*	– 0.001	*0.1%*	0.000	*0.1%*
(14) agreeableness	– 0.052	*8.8%*	– 0.031	*5.3%*	– 0.017	*2.9%*
(15) conscientiousness	0.001	*0.2%*	0.001	*0.1%*	0.000	*0.0%*
(16) neuroticism	– 0.019	*3.3%*	– 0.012	*2.1%*	– 0.008	*1.4%*
(17) openness	– 0.002	*0.3%*	– 0.002	*0.3%*	– 0.002	*0.4%*
(18) IQ-scores	– 0.000	*0.1%*	0.001	*0.1%*	– 0.000	*0.0%*
(19) other characteristics			0.139	*23.6%*	0.242	*41.3%*
(20) intercept	0.570	*97.2%*	0.187	*31.9%*	– 0.032	*5.4%*
Controls:						
Individual, human-capital, region	—		×		×	
Occupation, industry, job characteristics	—		—		×	

NOTE.— Earnings effects as a proportion (percentage share) of the gross differential in italics.

In the remaining rows we report in more detail how much of the total difference is attributable to each of the five personality traits separately. We find that the decomposition results for personality are primarily driven by one single dimension; agreeableness-antagonism. Rows 6 to 10 indicate that most of the share explained by differences in personality characteristics, comes from mean differences in agreeableness. Rows 13 to 17 describe a similar pattern, showing that most of the gender differences in personality returns are due to the differences in returns to non-agreeableness.

Interpreting the Decomposition Results.— Our findings thus suggest that, among the five personality traits, differences in agreeableness are the most important factor in explaining differences in male-female earnings. Two channels are responsible for this result: (a) men are much less agreeable than women; and (b) men are those who receive a reward for being less agreeable. The first channel does not require an economic explanation, while the second one does. In what follows, we consider labor market sorting, discrimination and bargaining as possible sources for why the market rewards agreeableness differently for men and women.

The sorting argument is one of the standard arguments to explain particular differences in pay. With our focus on agreeableness, sorting implies that less agreeable workers (primarily men) select into occupations where being less agreeable is required and rewarded as a productive trait. In regression models that do not control for occupation choice, it is possible that the return estimates for the agreeableness trait are picking up sorting effects. A simple test for sorting would therefore be to add variables to our models that measure characteristics of occupations in which non-agreeableness is possibly productive. As it is difficult to identify such occupations a priori, we revert to the equivalent procedure of conditioning directly on occupation codes at the one, two and three-digit level. Consistent with our earlier findings, we conclude that there is little evidence supporting the view that returns to agreeableness are so different for men and women because of sorting.

A discrimination argument that is partly consistent with our findings comes from Badget and Fobre [2003]. Their argument goes as follows. In the presence of societal expectations about gender-appropriate traits and behavior, it is possible that the market rewards men and women who comply to traditional gender roles, but punishes those who deviate. With this type of discrimination, we expect that agreeable men and non-agreeable women are punished for being perceived as too feminine and too masculine, respectively. Mens' returns to agreeableness behave as the model predicts; the market punishes those who are too considerate and cooperative, by male standards. In case of women, our evidence does not square with the model predictions. Womens' returns to agreeableness are either negative or close to zero, but never positive.

An alternative argument which received little attention in the empirical literature on gender wage differentials is the bargaining argument. Most of the work on gender differences in bargaining has been done by psychologists, who find that women are more cooperative in bargaining than men [Walters, Stuhlmacher and Meyer 1998]. Since we observe the extent to which men and women are cooperative in terms of our agreeableness measures, our data corroborate the findings that women are, on average, more cooperative when bargaining over their wages. So far, this argument does not explain the gender differences in returns, that is, why only men benefit from being uncooperative. If, however, anticipating employers start to offer lower wages to women, returns for uncooperative behavior will be higher for men than for women. Note that it is not necessary for men and women to be actually any different: it is sufficient for employers to think they are and to set their wage offers accordingly.

VI Discussion

In this paper, we estimate the effect of personality on male-female earnings using the Five-Factor Model of personality structure as a comprehensive organizing framework. The personality traits we examine are extroversion, agreeableness, conscientiousness, neuroticism and openness to experience. Our results indicate that (a) men, who are antagonistic, open and, to a lesser extent, emotionally stable enjoy earnings advantages over otherwise similar men; (b) women receive a premium for being more conscientious and open; (c) returns to non-agreeableness are very different for men and women; but (d) that the positive returns to openness are very similar across gender, suggesting that being creative, unconventional and artistic is equally important for men and women.

We would like to emphasize once more that our empirical findings require careful interpretation. The main reason is that causality may be reversed. We assume that personality affects earnings, but we cannot rule out the possibility that earnings may also impact personality. Despite well-founded endogeneity concerns, we still believe that an exploratory study like the one at hand makes a contribution in terms of organizing our thoughts about the role of personality in the labor market.

Last but not least, we should stress that our results are specific to a highly educated group of mainly white men and women, raised in the state of Wisconsin, and who were in their early fifties about a decade ago. Traits that are important for this particular group are not necessarily relevant for current generations in the labor market.

Having said this, let us take one step back and evaluate what we have found. Our results indicate that personality matters and that the impact of personality on earnings is comparable to that of cognitive ability. Its contribution in explaining the variance in observed hourly earnings, much alike measured intelligence, is rather modest. Our analyses shows that the joint influence of the FFM trait variables is considerably weaker than that of education, for example. This holds even though we have made considerable allowance for unreliability in the measurement of personality and despite the fact that our estimates are likely to be upper bounds of true effects. Nonetheless, our results do indicate that personality has earnings effects comparable to those of cognitive ability, which has a recognized place in the literature.

VII Appendix

A The Big Five Inventory (BFI)

Here are a number of characteristics that may or may not apply to you. For example, do you agree that you are somebody who *likes to spend time with others*? Please write a number next to each statement to indicate the extent to which you agree or disagree with that statement (1. agree strongly, 2. agree moderately, 3. agree slightly, 4. disagree slightly, 5. disagree moderately, 6. disagree strongly).

EXTRAVERSION ITEMS:
I see myself as someone who ...
... is talkative.
... is reserved.
... is full of energy.
... tends to be quiet.
... is sometimes shy, inhibited.
... generates a lot of enthusiasm.

AGREEABLENESS ITEMS:
I see myself as someone who ...
... tends to find fault with others.
... is sometimes rude to others.
... is generally trusting.
... can be cold and aloof.
... is considerate to almost everyone.
... likes to cooperate with others.

CONSCIENTIOUSNESS ITEMS:
I see myself as someone who ...
... does a thorough job.
... is a reliable worker.
... tends to be disorganized.
... is lazy at times.
... does things efficiently.
... is easily distracted.

NEUROTICISM ITEMS:
I see myself as someone who ...
... can be tense.
... is emotionally stable, not easily upset.
... worries a lot.
... remains calm in tense situations.
... gets nervous easily.

OPENNESS ITEMS:
I see myself as someone who ...
... prefers the conventional, traditional.
... prefers work that is routine and simple.
... values artistic, aesthetic experiences.
... has an active imagination.
... wants things to be simple and clear-cut.
... is sophisticated in art, music, or literature.

B Measuring Unobserved Traits and Classical Measurement Error

$A_1, \dots, A_k$ are observed scores on k items, all designed to measure the same but unobserved trait A. The following relationship is used to link these observed variables to the unobserved trait

$$A_i = A + e_i \text{ for } i = 1, \dots, k$$

The observed measure is decomposed into its true value A and a classical measurement error e_i that is uncorrelated with A and with each other. If Var(e) is the variance of the measurement error, assumed identical for all i, and if Var(A) represents the variance of the true trait, the covariance matrix can be written down as

$$\begin{array}{cccc} & A_1 & \dots & A_k \\ A_1 & \text{Var}(A) + \text{var}(e) & & \text{Var}(A) \\ \vdots & & \ddots & \\ A_k & \text{Var}(A) & & \text{Var}(A) + \text{Var}(e) \end{array}$$

The reliability ratio of any available measure of A represents the fraction of the variance in the observed measure of A that is due to the true variation in A

$$\frac{\text{Var}(A)}{\text{Var}(A_i)} = \frac{\text{Var}(A)}{\text{Var}(A) + \text{Var}(e)}$$

which, in this model, is identical to the correlation between any two measures

$$\rho_{ij} = \frac{\text{Var}(A)}{\text{Var}(A) + \text{Var}(e)} = \rho$$

The reliability ratio of the average score $\bar{A} = (A_1 + \cdots + A_k)/k$ is defined by

$$\frac{\text{Var}(A)}{\text{Var}(\bar{A})} = \frac{\text{Var}(A)}{\text{Var}(A) + (\text{Var}(e)/k)} = \frac{k\rho}{1 + (k-1)\rho}$$

It is easy to see that the impact of measurement error is reduced when we use not one but all available measures of A. If we could consistently estimate ρ, we also obtain a consistent estimate of the reliability ratio by simply substituting the estimated ρ in previous equation.

C Correcting the OLS Estimates

Having said this, consider the following simple model

$$Y = \beta A + \epsilon$$

where Y represents a measure for earnings, β measures the effect of A on earnings, and ϵ is an error independent of A. For simplicity we ignore other covariates and suppress all subscripts that indicate that variables are measured for individuals. We are interested in parameter estimation when A is an unobserved variable. We observe $\bar{A}$ instead. The effect of regressing outcome Y on $\bar{A}$ rather than on A

$$Y = \beta \bar{A} + \epsilon$$

provides the following least square estimator

$$\widehat{\beta}_{OLS} = \frac{\text{Cov}(Y, \bar{A})}{\text{Var}(\bar{A})} = \beta_{OLS} \frac{\text{Var}(A)}{\text{Var}(\bar{A})}$$

which is inconsistent. The least squares regression coefficient is attenuated by an amount equal to the reliability ratio. We already mentioned that data on all observed measures $A_1, \ldots, A_k$ allows us to measure the reliability ratio and therefore to identify the effect of A on earnings.

TABLE A1

SUMMARY STATISTICS

	Males ($N = 2,424$)		Females ($N = 2,601$)	
	Mean	Std. Dev.	Mean	Std. Dev.
Labor market outcomes:				
log hourly wages	2.886	*0.569*	2.299	*0.542*
hourly wages	21.891	*21.642*	11.827	*11.521*
Personality traits:				
extroversion	3.751	*0.878*	3.857	*0.898*
agreeableness	4.597	*0.737*	4.887	*0.701*
conscientiousness	4.875	*0.674*	4.904	*0.670*
neuroticism	3.081	*0.956*	3.277	*0.981*
openness	3.626	*0.770*	3.675	*0.807*
Individual characteristics, human capital and region:				
Henmon-Nelson IQ-scores	102.225	*14.870*	102.666	*14.332*
married	0.861		0.774	
no. of children	2.483	*1.490*	2.660	*1.609*
years of education	14.076	*2.507*	13.474	*2.089*
experience	17.773	*2.293*	15.372	*4.358*
tenure	17.723	*10.972*	10.912	*8.602*
state of residence Wisconsin	0.679		0.688	
Occupations:				
professional and technical	0.230		0.256	
executive and managerial	0.180		0.085	
sales and trade	0.098		0.092	
clerical	0.062		0.368	
production and crafts	0.176		0.015	
operatives	0.164		0.060	
service	0.056		0.106	
laborers	0.033		0.017	
other	0.001		0.001	
Industries:				
agriculture and mining	0.014		0.006	
construction	0.064		0.008	
manufacturing	0.371		0.139	
transportation	0.097		0.042	
wholesale and retail trade	0.106		0.166	
finance	0.048		0.088	
services	0.221		0.504	
administration	0.076		0.046	
other	0.003		0.001	
Public sector:	0.242		0.277	
Part-time:	0.019		0.226	

Chapter 5

Conclusions

"Why ruin a good story with the truth?"
— Woody Allen

It should have become clear that the study of behavioral and personality traits as determinants of labor market success is still in its infancy, and that much remains to be clarified concerning the relationship among personal characteristics, education and earnings. While I have made explicit the limitations of the research presented above, the following three broad conclusions can be supported.

First, a more inclusive view of the individual traits affecting earnings is warranted. When economists talk about the importance of abilities, they usually refer to unobserved abilities that may bias the estimated return to schooling, or discuss measures of cognitive ability and their effects on outcomes like schooling and earnings. This is obviously too restrictive. Measures of personality traits and behavioral attributes are highly predictive of labor market success and are interesting in their own right.

Second, while less definite than my findings concerning the pricing of such skills, I also presented some results concerning the production of the "social" components of human capital. My conclusion is that measures of cognitive performance are not sufficient indicators of the effectiveness of schools in promoting student labor-market success. We need broader indicators of school success, including measures of success based on the contribution of schooling to the behavioral and personality traits that we have termed non-cognitive skills.

Third, given such new measures, the evaluation of policy interventions in terms of their effectiveness in altering non-cognitive skills and improving socioeconomic attainment should assume a central place in the literature on educational production. So far, assessments of the efficacy of the educational system and the productivity of certain school resources have centered on cognitive achievement scores, rather than on a more comprehensive set of social outcomes of schooling.

Finally, a personal remark: Self-subversion is a vital part of the process of scientific progress. This is to say that I have to draw some conclusions from my work which cast some doubt on the relevance of non-cognitive skills. First, differences in such skills are found to make only a moderate contribution to the explanation of earnings, not only in the work presented here, but also in all other studies in the field. Their explanatory power is comparable to that of cognitive abilities, but this in itself does not resolve the puzzle we started out with: what is it, then, that accounts for individual differences in earnings, net of intelligence, education, parental background, gender and many other variables? In addition, the statistical explanation that these variables accomplish, although significant, is rather uninformative from a causal standpoint: we still have very little understanding of why these traits make a difference.

One interesting line of thought in this regard is that the payoffs to certain traits may depend on the nature of the job. Distinct job categories may require distinct skills and personality traits, that is, the traits predictive of success for a top manager are different from those of a piece rate worker. If this is true, we must go beyond standard linear regression analysis on a few continuous variables and explore the interactions between job requirements and personal capacities more closely. My view is that such an approach, however appealing from an analytic point of view, requires very detailed data on job descriptions if to be implemented successfully. Classification systems such as the three-digit occupation and industry codes of the Bureau of the Census for example, usually available in most large-scale survey studies in the U.S., are by far too aggregated to allow any meaningful analysis. An integrated research (and data collection!) effort with industrial psychologists would certainly open-up new opportunities for empirical work in the field. Psychologists have developed numerous test-scales for personality traits as well as detailed measures of the psychological requirements of jobs, but tend to apply them to small samples and without regard to economic outcomes. New data would potentially allow to include explicitly the context-dependent nature of the relationship between personality and behavioral traits in the analysis of earnings and other measures of labor market success.

Many issues remain unresolved, in particular questions relating to the "production" of non-cognitive skills. Are personality traits and behavioral skills genetically inherited, transmitted by family socialization or learned while being at school? Essentially, what are the relative contributions of nature and nurture and what is taught in schools? From a social policy perspective this would help identifying possible stages for intervention. These are important questions deserving separate consideration in future research.

Chapter 6

Summary

Chapter 1 — Introduction. The research reported on these pages analyzes the influence of personality traits and interpersonal relations in adult economic attainment, with particular focus on earnings. The main idea is to dare a peek into the "black box" of non-cognitive skills by drawing on new data and measures not considered in prior research, in order to investigate their role in educational and labor market outcomes.

Chapter 2 — Friendship Relations in the School Class and Adult Economic Attainment. This chapter analyzes the impact of adolescents' friendship relations in their final-year class of high school on subsequent labor market success. Based on a typology of network positions each student is located within the social system of the school class as either: an 'isolate', a 'sycophant', a 'broker' or a 'receiver'. These positions identify individuals' social standing within the group of classmates and proxy for their interpersonal behavior and social competencies. I offer empirical evidence that differential social standing in adolescence predicts large and persistent earnings disparities over the entire life course. The estimated wage premia and penalties do not appear to be substantially confounded by measures of family and school resources, and materialize largely independent of differences in cognitive abilities, grade rank in class, personality traits or friends' characteristics. A moderate share of the earnings inequalities is mediated by differential post-secondary human and social capital investment.

Chapter 3 — Class Size and Students' Friendships. This chapter starts out on the premise that social skills form an important subcomponent of human capital and that schools play a central role in developing such skills. Following up on the previous chapter, I argue that social competencies and norms for interpersonal behavior are not acquired in the abstract, but develop in the relations to others. I examine whether organizational characteristics of schools, such as the size of a class, have an impact on social interaction among classmates and therefore on the formation of skills along the "social" dimension of human capital. I estimate probit models of friendship choice to determine how much class size affects the probability that a friendship is formed between an arbitrary pair of classmates. In summary, I find that students in smaller classes have a significantly higher probability of interacting with one another. It appears that keeping school classes relatively small might be more efficacious with respect to a number of dimensions.

Chapter 4 — Estimating the Effect of Personality on Male-Female Earnings. This chapter uses the Five-Factor Model of personality structure as a comprehensive organizing framework to analyze the effects of personality on earnings. I find that extroversion, agreeableness, conscientiousness, neuroticism and openness to experience are rewarded / penalized significantly and differentially across genders. Antagonistic, emotionally stable and open men enjoy substantial earnings advantages over otherwise similar individuals. In case of women, the labor market appears to value conscientiousness and openness to experience. I also find evidence of significant gender differences in personality traits. Decomposition of personality-based earnings differentials into trait and parameter effects suggests that, of the five traits, differences in agreeableness are the most important factor in explaining differences in male-female earnings.

Chapter 5 — Conclusions. The study of behavioral and personality traits as determinants of labor market success is still in its infancy, and much remains to be clarified concerning the relationship among personal characteristics, education and earnings. Despite some limitations, the research presented here supports the following three broad conclusions: First, a more inclusive view of the individual traits affecting earnings is warranted. When economists talk about the importance of abilities, they usually refer to unobserved abilities that may bias the estimated return to schooling. This is obviously too restrictive. Measures of personality traits and behavioral attributes are highly predictive of labor market success and are interesting in their own right. Second, measures of cognitive performance are not sufficient indicators of the effectiveness of schools in promoting student labor-market success. We need broader indicators of school success, including measures of success based on the contribution of schooling to the behavioral and personality traits that we have termed non-cognitive skills. Third, given such new measures, the evaluation of policy interventions in terms of their effectiveness in altering non-cognitive skills and improving socioeconomic attainment should assume a central place in the literature on educational production. So far, assessments of the efficacy of the educational system and the productivity of certain school resources have centered on cognitive achievement scores, rather than on a more comprehensive set of social outcomes of schooling.

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