STOCKHOLM SCHOOL OF ECONOMICS
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The Y-name Syndrome: Prisons and Prejudice*

Abstract

The y-name syndrome is a wide-spread conception in Swedish society about men with names that end with a y, such as Ronny, Conny and Jonny. Y-names are taken as signals of low socioeconomic status, including criminality. To the best of our knowledge, the subject has, as of yet, not been examined empirically. First, this thesis explores whether or not the y-name syndrome is evidence-based by examining the relationship between y-names and socioeconomic status in 26 municipalities, and the prevalence of y-names among men who are, or have been in the custody of the Swedish Prison and Probation Service. We find that the y-name syndrome is empirically grounded; men with y-names are more likely to live in municipalities characterized by indicators of low socioeconomic status and are over-represented among criminals. Second, we explore if people with y-names are discriminated against in the labor market. This is investigated by looking at the ceteris paribus effect of a y-name on how 864 Swedish adults rated an employment résumé. The test found no conclusive evidence of discrimination.

Key words: First names, y-names, prison, discrimination, socioeconomic status

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1. Introduction

As a newly graduated teacher, Yvonne, mother to one of the authors, started her first teaching job at an elementary school in central Sweden. Her more experienced co-workers gave her some friendly advice to help her get started. They told her that when preparing for the first day of school, she should quickly scan the students' first names. "Put the boys with y-names in the front of the classroom", they told her. "They usually mean trouble." This episode from the early eighties illustrates a common perception in Sweden about males with first names that end with a y.

Still today, one of the favorite, and seemingly inexhaustible, topics in the name debate is whether - as Yvonne was told - people with so called y-names are trouble makers or perhaps even out right criminals. For the unfamiliar reader, a y-name is basically a first name that ends with a y, usually of Anglo-Saxon origin.[1] They are sometimes described as belonging to a group of names called Pseudo-English names, which became popular in Sweden following the Second World War (Hagström, 2006). Appendix A contains a list of the most common y-names. By investigating data from Statistics Sweden we have concluded that, in total, around one hundred thousand males have y-names as their main first name[2] in Sweden.

Drawing from our own anecdotal evidence as well as from what can be read in online discussions, we infer that the notion that y-name bearers belong to a certain group of people is widely spread in the Swedish society.[3] One typical commentator on a popular forum (Flashback Forum, 2007) expresses himself as follows:[4]

> *Think back about all the old troublemakers you went to school with. Didn't their names surprisingly often end with a y? Roy, Billy, Conny, Eddie(y), Johnny, Jimmy, Bobby, Sonny, Tommy etc etc. I can at*

[1] This thesis will not focus on female first names, as the term y-name usually only refers to male names.
[2] Here, we make use of the opportunity to clarify some of the ambiguity concerning what names are called. In Sweden, and many other Western countries a person may have more than one first name. For example, one of the authors is called Ulf Erik Markus. Of these names Erik is the main first name (for everyday use), while Ulf and Markus are his second and third names. In North America, these names would be called middle names. In other countries, like Sweden, however, the term middle name is only used for names that are originally last names, but not part of the last name of the bearer (for instance a woman can have her maiden name as a middle name). Throughout this thesis we use data on the main first names (Sw. tilltalsnamn).
[3] The Facebook group "My first name ends with a y, but I'm not in jail (yet)" illustrates this rather well (Facebook, 2007).
[4] Freely translated.

least, for my part, list a number of people with these names who are semi-criminals, often they grew up with alcoholic parents.

Y-names have also been discussed by academics and by Sweden's largest and authoritative newspapers. Y-names are, among other things, described as being more prevalent in the working class (Lagerblad, 2010). Charlotte Hagström, ethnologist and author of "Man är vad man heter... Namn och identitet"[5] writes that a Ronny, Sonny or Conny is commonly thought of as never to be found in high status positions with the exception of musicians and sport stars.[6] [7] (Hagström, 2006). Her research concludes that y-names are perceived to be associated with low or working class, low education and with troublesome bearers. These conceptions have become known as the y-name syndrome.

More generally, names have been a part of every culture and a natural part of society as long as it has existed (Deluzai, 1996), and today, names and naming seem to be receiving ever more attention (Alter, 2007). Searching for a good name, many prospective parents resort to the authority of name consultants, name sites and some dozen naming guide books. The largest name site, babynames.com has one and a half million unique visitors every month (BabyNames.com, 2010). Naming is continually among the most discussed subjects at the largest web forum for Swedish parents (Familjeliv, 2010), and the number of name changes in Sweden has greatly increased during the last few years (Swedish Patent & Registration Office, 2010). Some propose that names have become more important since we increasingly interact on the internet without ever meeting face-to-face (Mehrabian, 2001). Others propose that our growing interest in names is reflected in the mounting demands of globalization (names need to work everywhere) and increased room for personal creativity as the use of family names and religious names falls (Alter, 2007).

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[5] In English, "You are what you're called… Name and identity"

[6] This is of course not entirely true. Men with y-names do exist at high positions in society (even though they are not very easy to find). Examples include Billy Gustafsson, Tommy Ternemar and Tommy Waidelich who are all Social Democratic members of the Swedish Riksdag and the newly appointed Justice of the Swedish Supreme Court, Johnny Herre.

[7] A humorous article on the Swedish business news site realtid.se highlights the fact that the exclusive hunting party of King Carl Gustaf XIV had been joined by a new member: a Jimmy. It speculates about the possible implications of welcoming this Jimmy to the hunting party, usually consisting of noblemen and high-level executives. Among other things the author concludes that members with y-names probably are not allowed to attend the concluding hunting dinners as they are inept of using knife and fork (Realtid, 2010).

The overall purpose of this thesis is twofold. First, we aim to investigate the empirical grounds of the y-name syndrome in Sweden: are people with y-names really over-represented in less favored socioeconomic groups? The y-name syndrome has, to the best of our knowledge, not been empirically scrutinized before. Second, we explore whether any such tendency is plausibly explained by the existence of discrimination against people with y-names: are people with y-names regarded less favorably than other people with the same merits? In answering these questions, we hope to contribute to the overall economic discourse on the importance of names.

We will attempt to answer the first research question by investigating whether people with y-names are more likely to live in areas characterized by low education levels, low wealth and ill-health and if they are more likely to be criminals. The second research question will be answered with the help of an experiment, simulating a résumé screening, in order to find out whether or not identical candidates are viewed differently depending on their first names.

The disposition of the paper is as follows: Following the introduction, Section 2 presents relevant literature on the subject. The method is presented in Section 3 and our results are described in Section 4. These results, along with possible sources of errors and implications of our findings, are discussed in Section 5. Section 6 provides a brief conclusion of our findings.

2. Literature

Considering the country specificity of the topic, it is quite understandable that y-names have never been in the limelight of the international academic discourse. There is, however, a considerable amount of research on names and their consequences in general that is of relevance to our study. In this section, we give an overview of select research. As the topic intersects several academic disciplines we will not limit this section to work done in the field of economics, but will also bring up previous work in psychology and sociology. We begin by describing how various names are perceived differently, and continue to elaborate on proposed bases for name selection. We then present different views on the consequences of names, theories on the determinants of name-based discrimination and, finally, empirical evidence for these views and theories.

2.1 Perceptions of names differ

At the most fundamental level, several studies in psychology confirm that individual names are perceived differently. Mehrabian (2002; 2001) conducted extensive studies in which participants were asked to rate hundreds of names in different dimensions. Names such as Steven, James, Thomas and Christopher were generally perceived to belong to the overall most attractive people, whilst Melvin, Waldo, Garee and Igor were associated with the least attractive people. In a Swedish study, following Mehrabian's methodology, junior high school teachers were asked to assess ten different names. It was found that names common to high income earners such as Carl, Ebba and Fredrik were assessed more positively than names such as Kevin, popular among low income parents in Sweden (Malm & Zetterström, 2007).

2.2 Why certain names?

Another branch of the literature discusses how and why people choose certain names for their offspring. This has primarily been researched in sociology and economics. In sociology, Lieberson & Bell (1992), among others, showed that naming is a culturally bound phenomenon, in which naming patterns vary across gender, class and ethnicity. Fryer & Levitt (2004) and Lieberson & Mikelson (1995), for example, concluded that black people in the United States, more often than others, give their children unique names that are not part of the ordinary American name flora. Fryer & Levitt bring up the striking example that, at the peak of the phenomenon in 1980, 60 percent of all

black girls were given unique names. While the findings of Lieberson and others focus on African-Americans in the United States, it is hard to see any reason why naming patterns in Sweden, as in the United States, should not be culturally bound. Indeed, a report by Statistics Sweden, for the Swedish newspaper Expressen, found that there are distinct differences in naming among different socioeconomic groups in Swedish society (Wahlund, 2003). So it is clear that different groups name their children differently, but if parents know that certain names carry negative connotations, why do they still use them?

The rationale behind the naming process has also been studied by economists. In the aforementioned study, Fryer & Levitt (2004) looked at the causes and consequences of giving children distinctively black names. They presented four plausible explanations as to why these names are given. For one, parents may simply be ignorant of the burden they lay on their children. A second explanation is that parents attempt to maximize their children's future utility. They assume that white names work better in white neighborhoods, and, concurrently, that black names work better in black neighborhoods. Parents will choose a name that they think will maximize their children's utility in accordance with the future that they see for them. Thirdly, Fryer & Levitt propose a signaling model. This model includes two types of black people: black blacks and white blacks. The individual's utility depends on the type one belongs to and the community's perception of the individual. While signaling blackness facilitates contacts in the black community, signaling whiteness gives benefits in the job market.[8] Lastly, they propose a closely related identity model, in which the individual derives utility from choosing a name in congruence with what is prescribed by the home culture.

2.3 The consequences of names

In addition to examining the causes of certain names, researchers have tried to investigate what consequences a name has. Several studies propose that a name does in fact have concrete implications for its bearer.

Psychological research on names suggests that names help shape our identity and personality and, in extension, influence our choices and self-concept (Hagström, 2006),

[8] For more info about this model we refer to Fryer and Levitt's article.

(Garwood, 1976). By matching names with results from two widely used tests of self-concept and self-esteem,[9] Garwood also found that children with desirable names positively differed from children with less desirable names in a number of ways, including expectations about future achievements. Findings from name research even challenge views on how choices are made. In an extensive study, following a number of methodical tests, Pelham, Mirenberg, & Jones (2002) concluded that people to a greater extent choose to live in cities that resembles their first or last name (e.g. Philip in Philadelphia) or even choose a profession similar to it (Dennis becomes a dentist).

Apart from influencing our identity and personality, names are also suggested to have consequences for us through how other people perceive them. Mehrabian (2002) fervently claims that names are highly important for the bearers' success in school and at work, just as appearance is; he compares picking an unfamiliar name conveying negative associations with sending a child to school with their hair dyed pink:

> *I am suggesting that the name selected for a child is, by far, the single most important element for a parent to consider in his or her efforts to manage the impressions the child makes on the outside world. The selection of a name is especially crucial in that, unlike the choice of hair style or clothing or nonverbal mannerisms, a name is not altered easily.*

In Figlio's (2005) comprehensive study of how these consequences concretely materialize, he found that teachers expect less from children with names that sound like they were given by parents of low socioeconomic status. By comparing pairs of siblings in 24,298 Florida families with two or more children, and thus controlling for socio-economic factors, he found that teachers treated children differently depending on their first name. These differences, he claims, even translated into large differences in attained test scores. Names that are associated with groups of low socioeconomic status proved to be very important in predicting test scores. Children with these names tended to score lower on their standardized tests, relative to their siblings with less class-identifiable names.

All in all, we see that our names can in fact have consequences for us, both in terms of how we live up to the image that we have of ourselves and in terms of how other people

[9] The Children's Self-Concept of Achievement Test and the Tennessee Self-Concept Scale

treat us because of them. This can have important implications for us, not least in our professional lives.

2.3.1 More on the consequences of names: names in the labor market

Research on the consequences of names has also entered the field of Labor Economics. There are several ways in which researchers have tried to test if people with certain names are discriminated against. Studies thus far have usually focused on investigating whether or not certain names prevent ethnic minorities from finding employment.

In one type of experimental study, a so called correspondence test, researchers send out fictitious résumés to help-wanted ads. Making note of the callback rate for each name, one can draw conclusions as to whether any discriminatory behavior takes place in the early stage of a job application process. In their famous study, Bertrand & Mullainathan (2004) performed this test in Boston and Chicago and found that résumés with white sounding names received 50 % more callbacks than ones with black names. They also found some evidence, though not conclusive, that the lower callback rates were not solely explained by race but also by differences in social background. In a similar study, Bursell (2007) performed a correspondence test in the Swedish labor market. She found proof of ethnic discrimination in almost all occupational categories. The applications with foreign-sounding names received significantly lower callback rates than similar applications with Swedish names.

Name-based discrimination has also been examined using a second type of test: hypothetical hiring exercises. In these studies, participants make hypothetical hiring decisions, and sometimes also rate a job application. Cotton, O'Neill & Griffin (2007) hypothesized that names seen as most unique would also be the least liked ones. They also predicted that African-American names and ethnical names would be perceived as most unique and therefore that applicants with these names would be less likely to be hired. In a first study, in which respondents were asked to make a hypothetical hiring decision based solely on the applicant's first name, they found significant support for their hypotheses. However, their conclusions did not prove to be robust to adding information about the applicant; when respondents also took a résumé into consideration, no evidence of discrimination was found.

A hypothetical hiring exercise was also used by Bart, Hass, Hass Philbrick, Sparks, & Williams (1997), in which subjects rated candidates by using their résumés. They - unlike Cotton et al. - found evidence of discriminatory behavior, as an applicant with a distinctively black name received a significantly lower mean rating than an identical candidate with a white name. They also tested the hypothesis that certain groups would assess the candidates differently depending on the similarities between the applicant and the rater: for example that a black female would rate another black female highly. This last hypothesis was partially supported.

2.3.2 Discrimination – theories, explanations and evidence

Theories on discrimination can help us understand the findings in the studies mentioned above. The two main forms of discrimination relevant to our study are taste-based discrimination and statistical discrimination. According to taste-based discrimination, people have an aversion towards interaction with a certain group of individuals, and as a consequence they are willing to pay a premium in order to avoid it (Becker, 1971). Statistical discrimination explains why even employers who are not prejudice in their nature would still discriminate, as a way to avoid the time-consuming evaluation of CVs and personal letters. According to this theory, employers use skin color, sex, place of residence or other factors, like first names, as proxies for other attributes that are relevant for the job opening (Phelps, 1972). One version of statistical discrimination hypothesizes that employers discriminate on the basis of knowing the average labor productivity in different groups. By only choosing applicants from the group which, on average, is high-performing, an employer's expected rate of success in finding a competent employee increases. However, this method simultaneously puts the employer at risk of foregoing the most qualified applicant which could very well be in another group. If the employer hires regularly, the benefit of being right on average outweighs the cost of not always finding the optimal candidate.

Another theory, that complements the explanation of the rationale behind discriminatory behavior, is the Attraction-Selection-Attrition (ASA) model. Schneider (1987) suggests that people are attracted to, selected by, and stay with organizations that fit their personality. The idea is that organizational culture, and subsequently organizational behavior, is a function of the modal personality in the organization and its behavior. From the prospective employee's perspective, this is what attracts him or

her to the organization – and from the employer's point of view, what sets the standards by which a prospective employee is assessed. The personal fit also determines whether or not an employee will stay. Over time, this cycle results in a more and more homogeneous organization which in turn reinforces the cycle.

Empirical research on names has found varying levels of support for these models of discrimination. Bertrand & Mullainathan (2004) concluded that racial (taste-based) discrimination plays a large role in the US labor market. Fryer & Levitt (2004) on the other hand, pointed to statistical discrimination, i.e. employers use names as signals of productivity after having controlled for information on the résumé. They found that the blackness[10] of a name was correlated with unfavorable birth outcomes (low birth weight, single mother, low-educated parents etc.), which in turn are associated with low labor productivity. In recent research by Aura and Hess (2010) the conclusions of Fryer & Levitt (2004) were supported. Even after controlling for background variables such as race and parents' education they found evidence of correlation between names and life outcomes connected with labor productivity, including class, happiness and education. The authors did not, however, rule out the possibility of names also being basis for taste-based discrimination. In the previously mentioned study by Bart et al. (1997), the predictions based on the ASA-model, i.e., that applicants are more favorable assessed by raters similar to them, was tested, but only partially supported

So, adverse consequences of names in the labor market can be explained in at least three ways: through taste based discrimination, statistical discrimination and through a mechanism that disfavors dissimilar candidates.

2.4 Implications

To conclude this section, we would like to comment on how popular wisdom, as well as academic and non-academic literature, provide us with suggestions as to what answers to our research questions we should expect to find.

Firstly, as Hagström (2006) explains it, the y-name syndrome implies that people with y-names are not likely to hold high status positions, but rather to belong to lower

[10] High probability of a person with that name being black

socioeconomic strata. Accordingly, we should expect y-names to be relatively more common in groups associated with less favorable life outcomes, such as lower wealth, education and ill health. Moreover, the y-name syndrome also specifically suggests that men with y-names are relatively more prone to have a criminal record than the general male population. Thus, if the y-name syndrome holds true we should see that people with y-names are over-represented among criminals.

Secondly, drawing on the conclusions of Malm & Zetterström (2007), Figlio (2005), Bertrand & Mullainathan (2004), Mehrabian (2001) and Bart et al. (1997) we should expect people with names associated with low socioeconomic status to be systematically discriminated against in the labor market. Therefore, given the above-mentioned conceptions about y-names, we should expect people with these names to be assessed more negatively than other people.

Thirdly, the ASA-model, coupled by the indications found by Bart et al. (1997), lead us to suspect that people in general prefer job applicants from their own group. This would mean that a name signaling a certain group affiliation would be regarded more favorably by people from that group and less favorably by other groups. If this holds, we expect respondents from socioeconomic groups in which y-names are more common to hold more positive views of a person with a y-name than would others.

3. Method

The third section of this paper presents the methods used to examine the y-name syndrome. First we describe and evaluate the methods. We then compare our methods with the procedures used in similar studies. Our investigation is divided into three parts. The first two pertain to our investigation of the evidence behind the y-name syndrome. The third, an experiment, is used to investigate the possible existence of discrimination.

3.1 Socioeconomic status

In order to evaluate the empirical validity of the y-name syndrome, we test the hypothesis that people with y-names, to a relatively greater extent than others, are represented among groups with lower socioeconomic status.[11] In a first test, we use three indicators as measures of socioeconomic status: level of education, a measure of ill health and wealth. In a second test we examine an additional indicator, specifically related to the y-name syndrome: the probability of being, or having been incarcerated.

3.1.1 Indicators of socioeconomic status in Stockholm municipalities

In the first test, we analyze the relationship between socioeconomic factors and the frequency of specific names on a municipal level. This degree of resolution was chosen as data on an individual level were not available. We examine how a quotient of the number of people with a particular name per ten thousand inhabitants is related to median wealth (Statistics Sweden, 2010-03), education level[12] (Statistics Sweden, 2010-04) and a commonly used measurement of ill health (Index of Ill Health: IIH)[13] from Försäkringskassan[14] (2010).

Our selection of names consists of the top ten most common y-names and, as a control group, the ten most common names in Sweden. Due to a limitation in the database,

[11] The term socioeconomic status (SES) is rather ambiguous and many definitions exist: "In general terms, however, SES describes an individual's or a family's ranking on a hierarchy according to access to or control over some combination of valued commodities such as wealth, power, and social status" (Mueller & Parcel in Sirin, 2005).
[12] Defined as the percentage of the population having a post-high school education of at least three years.
[13] The Index for Ill Health (ohälsotal) is a measurement of the level of absence due to sickness per capita. Försäkringskassan includes all forms of sick leave in the statistics, from people taking short periods of time off for minor complaints to the Swedes in health-related early retirement.
[14] For the non-Swedish reader, Försäkringskassan is the equivalent of the Swedish Social Insurance Agency.

more uncommon y-names cannot be used in the analysis; for integrity reasons, it does not include data on names with fewer than five bearers in a municipality. The y-names in the bottom of the list[15] are relatively uncommon and therefore, predominantly in small municipalities, too many observations are missing. Therefore, we limit the analysis to the ten most common y-names.

The sample is limited to the county of Stockholm (roughly around 20% of the country's population) and its 26 municipalities. Stockholm County is used because it is comprised of a rather large number of municipalities in a confined geographic area, whereby any geographically related variation should be minimized. At the same time, variation in the three SES variables is great across municipalities - especially in median wealth. While the data on names, education level and ill health are all up to date, we find one possible cause for concern; data on median wealth are two years older than the data on number of name-bearers.[16] It is, however, unlikely that there have been any large redistributions of wealth between the various municipalities in this very short period, since there was not a large change in the previous 6 years (Statistics Sweden, 2010-05). Since the study is restricted to male names, median wealth, IIH and level of education for the male population.

When performing the statistical analysis we use an OLS regression specified for median wealth as:

Regression 1: Frequency of a name over median wealth

$$\text{frequency}_{\text{Name}} = \beta_0 + \beta_1 \text{median wealth} + \varepsilon$$

where:

$$frequency_{Name} = \frac{No.of\ Name\ in\ municipality}{No.of\ inhabitants\ in\ municipality} \times 10000$$

The regressions for IIH and level of education follow the same pattern.

[15] The list of y-names can be found in Appendix A.
[16] As of the abolishment of the Swedish tax on wealth in January 2007, data on wealth is no longer collected.

3.1.2 The Prison and Probation Service

Having presented the method used in our first test, we now proceed to present the one used in the second. The Swedish Prison and Probation Service assisted us with data on the first names and years of birth of all the living persons who are or have been in their custody[17] with certain names in the ages 20-69 years. This was complemented by purchasing data from Statistics Sweden for the number of people with the same names in the particular age group. The data is used to see if y-names are good predictors of being, or having been, in contact with the penal system, i.e. whether men with y-names are more common among criminals than are other men. In analyzing the data, we calculate a quotient, representing the proportion of men with a certain name that is or has been a client of the Prison and Probation Service. We define it as:

$$\text{quotient}_{\text{Name}} = \frac{No, of\ present\ and\ former\ clients\ 20-69\ years\ old\ with\ Name}{No. of\ males\ 20-69\ years\ old\ with\ Name}$$

Our selection comprises the twenty most common male y-names[18] and, as control group, the twenty most common Swedish male names (Statistics Sweden, 2010-01). The control group was chosen simply as it, per definition, represents a large proportion of the Swedish male population. To determine if y-names are over-represented, we look at the quotients for the names in aggregate as well as for each name individually.

Concerns could be raised that the data on past and present clients is not entirely up to date. The database is cleared from deceased interns every 10 years so it is possible that some deceased former interns are included. However, even though the clients of the Prison and Probation Service can be suspected to have a somewhat lower life expectancy with greater volatility than the population in general, the sample only includes past and present clients up to the age of 69, which is far from the average life expectancy. The number of included dead former clients should thus be low. We consider it safe to assume that the misrepresentation arising from including a small

[17] In this category prison inmates are included, but also persons sentenced to intensive supervision (with the aid of a transmitter attached to the ankle), probation, probation with community service and probation with contract treatment. (The Prison and Probation Service, 2009). From here on, to make things easier, we will refer to them as clients, just as the Prison and Probation Service does.
[18] When compiling this list, we used the list of the most common y-names, which can be found in Appendix A, but excluded Torgny as it is never defined as a typical y-name in any discussions online or in Hagström's (2006) book.

quantity of deceased persons in the client data is very small and that it does not affect our results noticeably.

3.1.2 Limitations to our method

The primary limitation to both the aforementioned tests is that they are coarse estimates. It could be argued that more variables should be investigated before concluding anything about the y-name syndrome's empirical grounds. Examples of such variables could include: parents' level of education, age of parents at birth, income etc. Even though the scope of the thesis has forced us to limit our analysis, we believe that the chosen measures cover much of what is referred to as socioeconomic status. Also, the chosen measures – the probability of having unpleasant encounters with the penal system and living in socioeconomically less favored areas – are perceived by us as some of the most frequently occurring themes in the y-name debate on various online fora.

3.2 Discrimination

In the next step, using a hypothetical hiring exercise, we experimentally investigate whether people with y-names are discriminated against on the basis of their name. More specifically, we investigate whether the name of a person affects the perception of him even after the respondents considered other information about the fictitious applicant. Below we describe our experimental design and the execution of our test, which is followed by a discussion of our sample and a comparison with other studies.

3.2.1 Experimental design

A survey was distributed randomly to 843 Swedish adults (aged 15-80) in the Stockholm area during the spring of 2010. Respondents participated voluntarily and were not compensated for their effort. Participants were asked to read a synthetic résumé and to imagine themselves taking on the role of a recruiter. Their task was to try to envision the person behind the résumé and to rate eight different personal attributes of the hypothetical job applicant: drive, intelligence, reliability, social competence, power of initiative, planning ability, common sense and creativity, on a scale 1 to 10. Participants were also asked to estimate how much the person earned per month.

In half of the cases an identical résumé was assigned with the name Carl Andersson and in the other half with the name Ronny Andersson. Andersson is one of Sweden's most common surnames and was chosen to be as neutral as possible to isolate the effect of the first name. The names were clearly printed on the top of the résumés, yet not with too large a font, in order to diminish the risk of suspicion. Ronny and Carl were chosen to represent a typical y-name and a typical traditional Swedish name respectively. Ronny was chosen because it is one of the most common y-names and because it is often mentioned as a typical example of a y-name in discussions on the internet and in descriptions of the y-name syndrome (Hagström, 2006). Carl was chosen as a contrast to Ronny partly because of its popularity among high income earners and partly, as it has been found that the name Carl carries positive connotations (Malm & Zetterström, 2007).

The eight attributes used in the evaluation were chosen because we perceive them as being important attributes to have in order to succeed in a professional career. They are, furthermore, not explicitly stated in the résumés, but rather to be inferred from them. Also included in the questionnaire were anonymous background questions about the participant. These questions were included to investigate any relation between ratings and the background characteristics of the participants. It also makes it possible to correct for any potential shortfall of the randomization.

One may suspect that any effect of the name on the rating would vary depending on how strong a candidate's résumé is. To account for this possibility we used two different résumés: one with a higher attained educational level and more qualified professional experience and one with lower education and less qualified employment. In all other regards the résumés were identical. The résumés were based on real résumés found online and included information about education, professional experience and hobbies.[19]In designing the résumé rather neutral experiences were deliberately chosen in order to leave some room for interpretation.

3.2.2 Conducting of the survey

The résumés were distributed randomly to the respondents in order to equalize factors that are not accounted for in the experimental design, but which could still affect the

[19] The questionnaire and the résumés can be found in the Appendix B.

assessment. Ideally, every other résumé should have been a Carl, and every other a Ronny. Such a randomization would, however, entail additional concerns as many of the respondents were filling out the forms close to each other. Seeing identical résumés with different names would add suspicion about our purpose, potentially jeopardizing the validity of the experiment. Instead, the questionnaires were randomized at intervals of 20 résumés. This could affect the degree of randomization, and subsequently bias our results. This in mind, we have conducted a large scale experiment, to increase the precision of our estimates. One may also object that additional precaution should have been taken to assure that respondents were not influenced by their neighbors who were also filling out the questionnaire. However, we believe that the influence of sneak-peaking on the neighbors' questionnaires should have a minimal effect on the results as people were explicitly told to fill out the questionnaires individually.

The questionnaires were manually distributed by us to students at two Stockholm universities (the Stockholm School of Economics and Stockholm University) and to the general public at Stockholm Central Station, Sweden's largest hub for railroad traffic. The two universities were primarily chosen because of the availability of respondents. As argued by Bart et al. (1997), students are also thought to have a personal interest in résumés and the process of job applications. Stockholm Central Station was, it too, chosen with the ease of collecting data in mind, as well as the great variation in age, professions, socioeconomic groups and parts of the country represented among the travelers.

3.2.3 Sample

At this point we content ourselves with presenting some general issues that deserve more immediate attention. The composition itself will be presented and discussed in more detail in section 4 and 5.

Ideally it would be interesting to see how attitudes towards people with y-names manifest themselves both among people in the labor market that have an influence on recruitment processes – as these individuals have a large impact on the success of a candidate in his professional life – and in the general public – as manifestations of discriminations could obviously have ramifications outside the labor market. It is, however, difficult to construct a sample that is representative of both groups.

There are some issues that adversely affect our ability to generalize our conclusions. Firstly, to some extent, the relatively large proportion of respondents that either studies, or has studied, Business and Economics can be argued to give some indication of attitudes in the labor market. But a sample that is representative for the labor market specifically, is quite obviously less representative for the general public. In this case, it for example results in a disproportionately large proportion of younger respondents. Secondly, by sampling at Stockholm Central Station, our sample consists of relatively more travelers than does the general population. Thirdly, collecting data at Stockholm Universities, our sample is quite naturally biased towards people living in Stockholm. While it is not entirely straightforward to pin down the effects of these three issues on our results, one should still recognize their existence. What is clear is that these issues do restrict the extent to which our conclusions can be generalized.

While small, there is also one more concern with our sampling method that should be raised, as it could lead to erroneous conclusions. If respondents have unwillingly accepted to participate, one may be worried that they did not take the survey seriously. If such a phenomenon, albeit unlikely, were to be particularly pervasive across the résumés, it could lead to a type II error: failing to reject a false null-hypothesis.

3.2.4 Benefits and drawback in relation to other studies

As could be seen in Section 2, there were a number of different ways available for us to investigate the effects of a name on its bearer, and ultimately, how this affects a person's success in the labor market. Below, we comment on the choices we made and the trade-offs we had to consider.

The most basic studies have several shortcomings when it comes to our purpose, and thus we chose not to use them. Connotation studies merely ask respondents to rate a name in a number of dimensions. Equally simplistic, some hypothetical hiring exercises only ask participants to decide whether or not they would hire the person solely on the basis of the candidate's name. Firstly, it is our view that the leap of faith often taken in these studies is big. From the fact that names are perceived differently, it does not necessarily follow that these perceptions affect the bearers' life outcomes. Furthermore, it is not particularly realistic not to allow for additional information to affect any name-induced bias; job applicants are seldom judged purely on basis of their name.

Performing such a study on y-names might still have been useful in determining how these names are perceived. Nevertheless, supported by the non-academic literature about y-names we dare to say that no connotation study is necessary to further establish the common attitudes towards y-names.

A different test, which we also refrained from using, is the previously explained correspondence test. However, correspondence tests still have several benefits. First of all, they take into account other information about the candidate than just the name. As pointed out by Bursell (2007), this type of test also has several other benefits in relation to other methods. The most important point to make is that what employers say and what employers do are likely two very different things. If asked straight out, prejudiced people will probably not be open about any of their discriminatory attitudes. Correspondence tests are a good reflection of a real environment in which the participants are unknowing and therefore have all the incentives to make realistic and honest decisions.

While there are certainly merits to correspondence tests, we still consider our design to be better suited for our purposes. In relation to correspondence tests our design has several advantages. First – correspondence tests only give researchers feedback on whether the résumé generated a callback or not. A more comprehensive survey opens up for more extensive conclusions to be drawn, for example, about what dimensions of personal traits that are perceived differently because of the name. Also, a correspondence test would not allow for any information about the recruiters, even though some attempts have been made to infer the type of person assessing the résumé by sending them to firms in different locations[20] (Bertrand & Mullainathan, 2004). Our method is in this regard more precise in its data. Finally, correspondence tests are unfortunately very extensive and labor intensive. In light of the economic crisis and tough labor market conditions, such a study would have been even more cumbersome as we would expect a very low overall response rate. We concluded that performing such a test would have been outside the scope of this thesis.

The test we chose to perform, a résumé based hypothetical hiring exercise, relates to previous such studies, but has been modified to accommodate for our purpose. It also - to the extent it is possible - addresses some of the shortcomings of earlier research. In a

[20] More specifically, résumés have been sent to firms in areas with varying levels of black inhabitants, to get an idea of the probability of the person reading the résumé being black.

couple of regards, our test differs from previous similar tests, such as the ones by Bart et al. (1997) and Cotton et al. (2007), who examined discrimination of black names. Most importantly it differs from the study by Cotton et al.. Firstly – we address their concern that respondents spend too much time assessing the résumé. In this regard our survey is more representative of an actual screening process; respondents spent about five minutes completing the questionnaire compared to around 20 minutes in the study by Cotton et al.. Also, although not mentioned as a weakness by the authors themselves, Cotton et al. used a quite small sample which moreover only included students. We believe that the sheer number of respondents in our study, as well as its more heterogeneous composition, give a more representative sample of the actual population. It also allows us to test for discriminatory behavior among different subgroups of respondents. Our method is more similar to the one used by Bart et al. – but there are still some additional differences. Most noticeably, we isolate for a first name effect, while they investigate the effect of both first and last names together. Furthermore, as in relation to Cotton et al., we use a larger and more heterogeneous sample. Lastly, we examine potential name-based discrimination for men and Bart et al. for women. The distinction is important to make since earlier studies have found that discriminatory behavior has varied across gender.[21]

There is however a few more issues, more general in their nature, left to be raised. One concern is the impossibility of making the questionnaire incentive compatible. Since we made use of synthetic résumés, we had no true picture that could serve as a benchmark in rewarding people for being correct. It is also difficult to objectively quantify a true value of largely subjective attributes such as social competence and creativity. Still, participants expressed enthusiasm over participating. Thus, in spite not having any incentives to do so, we believe that the survey was taken seriously and that respondents made an effort to give a well thought-through opinion. Finally, as with correspondence tests, the conclusions that can be drawn from the experiment are limited to the initial stage of a hiring process.

[21] An example of this is Bertrand & Mullainathan (2004).

3.2.5 Statistical analysis

Regression 2: Attribute over dummy for Ronny's résumé (and dummy for highly qualified résumé)

$$attribute = \beta_0 + \beta_1 Ronny\ (+\beta_2 Resume) + \varepsilon$$

The example above shows the base regression for how respondents have assessed the attribute intelligence. All the other attributes are regressed on a dummy for the Ronny résumé in a similar way. If Ronny is discriminated against, we should expect to see the coefficient for the variable Ronny to be negative.

In the analysis we start by regressing each of the attributes that the respondents were asked to assess, on a dummy for Ronny – once for each type of résumé. Initially no control variables are included, as, with a random sample, we should expect the background characteristics of the respondents to be evenly distributed across the two names. We run the regressions for each version of the résumé separately. We then proceed to pool the two versions. By pooling the observations and including an extra dummy variable (Resume) that takes on a value of 1 for the highly qualified résumé and 0 for the less qualified résumé, the sample size grows whereby smaller differences could be picked up. To control for any shortfall in the randomization, we run a regression of the attributes on a dummy variable for Ronny and a series of control variables for background characteristics of the respondents. A specification of this regression can be found in Appendix E.

Finally – as a supplementary analysis – we run the same regressions for some select subgroups to see if any potential differences in assessments between the two candidates are somehow dependent on the background of the respondent.

4. Results

This section presents the results of each of the performed analyses along with comments on their reliability. To facilitate the reading we have chosen to include only the most relevant figures and statistical tests. A more detailed account can be found in the appendices.

4.1 Socioeconomic status

We find that there are statistically significant relationships between y-names and all the tested indicators of low socioeconomic status, including representation among past and present clients of the Prison and Probation Service. While some of these investigations yield coarse estimates they still show that the y-name syndrome holds true. For both the performed tests, we present the results for the group of y-names as well as for each name separately.

4.1.1 Indicators of socioeconomic status in Stockholm County

When examining the sample we immediately see that the frequency of different y-names greatly varies across municipalities. For example, the number of Jimmies per 10 000 inhabitants peaks at 41.18 in Nykvarn and reaches its lowest point in Danderyd at 3.21). We also notice large differences across municipalities when looking at our three measurements of socioeconomic status. In particular, there is a hefty spread in median wealth. Danderyd tops the list at 522 thousand Swedish Kronor in median wealth for men, while Botkyrka trails far behind at 11 thousand. The spreads in level of education and the index of ill health are not as large, but differences are still apparent.[22]

In the first case we regress the frequency of the most common y-names[23] on each of the three chosen socioeconomic indicators. In each and every regression we find a significant relationship between y-names and the three indicators. In municipalities with a high frequency of y-names we can expect median wealth and level of education to be

[22] Further details can be found in Appendix D.

[23] By frequency of y-names we here refer to the aggregated number of people with any of the ten most common y-names per ten thousand inhabitants. The frequency of common names is the equivalent measure for the ten most common male names.

low, while average sick-days to be high. The opposite is true in municipalities with a low frequency of y-names. The results can be seen below in Table 1.

Table 1: Relationship between frequency of names and socioeconomic indicators in Stockholm municipalities (aggregated)

	(1) **Median Wealth♦**	**t**	(2) **Ill Health♦♦**	**t**	**(3) Level of Education♦♦♦**	**t**
Y-names	-0.171**	(-2.89)	4.535**	(3.13)	-4.261**	(-9.16)
_cons	166.4**	(15.98)	43.28	(1.29)	227.3**	(23.14)
N	26		26		26	
R-sq	0.258		0.290		0.778	
Common Names	1.090**	(3.27)	-30.02**	(-3.77)	6.506	(1.16)
_cons	1629.9**	(27.70)	2440.2**	(13.20)	1636.9**	(13.81)
N	26		26		26	
R-sq	0.308		0.371		0.053	

t statistics in parentheses
* p<0.05 ** p<0.01
♦ In K SEK
♦♦ In days of sick absence
♦♦♦ As a percentage of the male population with a university education longer than three years

The coefficients show us the relationship between the type of name and each of the tested indicators. For example, as the average sickness absence days among men increases by one day in a municipality, we should expect the number of persons with the ten most common y-names per ten thousand inhabitants to be 4.535 higher. For y-names, all the coefficients have the expected signs and are significant on the 1% level. When running the same regressions for our control group, consisting of the ten most common male names in Sweden, we find the coefficients to be of the opposite signs. The coefficient for education level was, however, not significant.

It is interesting to note that when running the same regressions for every name separately - with one exception - all the coefficients for the y-names have the expected signs and almost all are significant. Our results, for example, suggest that we should expect to see one less Jimmy per ten thousand inhabitants for every increase of about thirty thousand crowns in median wealth and similarly, we anticipate an increase of one Tommy per ten thousand inhabitants for every extra average sickness absence day. The results for the individual names can be found in Table 2. Harry is the only name that has no significant relationship with the tested indicators and that has a coefficient with the unexpected sign (median wealth). One explanation for this could be that Harry, being an English name that arrived in Sweden already in the 19th century (Svenskanamn.se,

2009), does not really belong to the y-name category, as most of the other names grew in popularity in the fifties (Hagström, 2006).

Table 2: Relationship between frequency of names and socioeconomic indicators in Stockholm municipalities (for each name)

			(1) Median Wealth♦	t	(2) Ill Health♦♦	t	(3) Level of Education♦♦♦	t	N
Common Names	(1)	**Lars**	0.106*	(2.12)	-3.462	(-2.02)	0.0169	(0.02)	26
	(2)	**Karl**	0.0994*	(2.12)	-1.964	(-1.01)	-0.0446	(-0.07)	26
	(3)	**Anders**	0.161**	(4.46)	-3.997**	(-3.06)	1.525**	(3.00)	26
	(4)	**Mikael**	-0.0574	(-2.04)	0.895	(0.87)	-1.982**	(-6.34)	26
	(5)	**Johan**	0.254**	(4.86)	-6.602**	(-4.61)	3.227**	(5.66)	26
	(6)	**Per**	0.140**	(4.55)	-3.988**	(-3.66)	1.348*	(2.59)	26
	(7)	**Erik**	0.280**	(4.09)	-6.616**	(-2.90)	2.172*	(2.25)	26
	(8)	**Jan**	0.0443*	(2.15)	-1.422	(-1.90)	0.0150	(0.05)	26
	(9)	**Peter**	0.0510*	(2.40)	-2.036**	(-3.27)	0.381	(1.45)	26
	(10)	**Thomas**	0.0121	(1.12)	-0.780	(-1.89)	-0.168	(-1.01)	26
Y-names	(1)	**Jonny**	-0.00767*	(-2.21)	0.151	(1.15)	-0.272**	(-5.31)	25
	(2)	**Harry**	0.00116	(0.18)	0.0719	(0.31)	-0.120	(-1.39)	26
	(3)	**Jerry**	-0.00516**	(-2.83)	0.0769	(1.46)	-0.111**	(-5.36)	25
	(4)	**Tommy**	-0.0403**	(-3.07)	1.245*	(2.40)	-1.184**	(-7.48)	26
	(5)	**Jimmy**	-0.0327**	(-6.15)	0.748*	(2.76)	-0.728**	(-6.36)	26
	(6)	**Tony**	-0.0257**	(-5.08)	0.659**	(4.41)	-0.474**	(-10.03)	26
	(7)	**Conny**	-0.0131*	(-2.58)	0.393	(1.90)	-0.424**	(-7.10)	26
	(8)	**Johnny**	-0.0172**	(-4.92)	0.446**	(3.40)	-0.393**	(-7.69)	26
	(9)	**Ronny**	-0.0116**	(-3.93)	2.826*	(2.17)	1.440	(0.58)	25
	(10)	**Benny**	-0.0118**	(-4.01)	0.362**	(3.12)	-0.272**	(-5.29)	26

t statistics in parentheses
* p<0.05 ** p<0.01
♦ In K SEK
♦♦ In days of sick absence
♦♦♦ As a percentage of the male population with a university education longer than three years.

In the control group, the relationships are mostly the opposite. For example, the number of Johans per ten thousand inhabitants expected to be found in a municipality is estimated to rise with about 2.5 for every ten thousand in increased median wealth. Among the most common names, only one of the coefficients has the opposite sign and is significant (level of education for Mikael). In addition a few names have coefficients that are not significant.

In summary, when investigating how the frequency of y-names in a municipality relates to low socioeconomic status, we find that they are positively correlated. This

relationship is even clearer when considering the negative correlations to low socioeconomic status that common names have.

4.1.2 The Prison and Probation Service

The data supplied by the Swedish Prison and Probation service included first names of a selection of past and present clients. The selection was based on the twenty most common y-names[24] and, as a control group, the twenty most common male names overall. The sample included year of birth, given names and – in the cases it had been recorded – also the main first name. The entries without this last specification were excluded , as in the other cases it is uncertain which name the client actually uses on a daily basis, whereby the total sample size finally amounted to 23 536 present and former clients. Thus, the sample is not complete, which adds a level of uncertainty to the size of the quotients. However, we find no reason to suspect any systematic tendency in the omissions of main first names that should affect the comparisons between y-names and the control group. In Table 3 below, we list quotients representing the proportion of the total population with a certain name that is, or has been, in the custody of the Swedish Prison and Probation Service.

Looking at the quotients, it is immediately apparent that there is a big difference in the level of representation of the y-names and the most common names. While the quotient is 0.020 for the most common names in aggregate, the same quotient is almost 2.5 times larger for the most common y-names (0.0492). Just as we did in the first test, we also investigated each name individually. The highest quotient is that for Timmy (0.101) followed by the quotient for Danny (0.0895). It is interesting to note that out of the most common names, not a single one has a higher quotient than the average for the y-names. (One of the authors reluctantly admits that Mikael is close at 0.0427.)

We used a t-test to see if we could statistically reject that y-names had equal quotients as the control group. This null-hypothesis was rejected at a very high significance level (t=-5.619). Considering that we do not know the underlying distribution of our sample, we also used a non-parametrical Wilcoxon rank-sum test that does not rely on any such assumptions. (Newbold, Carlson, & Thorne, 2009) This test rejected the null-hypothesis

[24] Information for one of the common names, Thomas, was unfortunately not available in the data provided by the Prison and Probation Service.

at a very high significance level as well (z=-4.433). The results support the theory that people with y-names are more prone to crime (or at least to getting caught).

Table 3: Proportion of the total population with a certain name that is, or has been, in the custody of the Swedish Prison and Probation Service

Name	Quota	Name	Quota
Benny	0.0434	**Anders**	0.0192
Billy	0.0618	**Andreas**	0.0406
Conny	0.0405	**Bengt**	0.0107
Danny	0.0895	**Bo**	0.0129
Freddy	0.0495	**Daniel**	0.0459
Harry	0.0329	**Erik**	0.0154
Jerry	0.0573	**Fredrik**	0.0322
Jimmy	0.0767	**Hans**	0.0124
Johnny	0.0510	**Jan**	0.0181
Jonny	0.0455	**Johan**	0.0266
Ricky	0.0729	**Karl**	0.0060
Ronny	0.0359	**Lars**	0.0138
Sonny	0.0678	**Mats**	0.0184
Thommy	0.0314	**Mikael**	0.0427
Timmy	0.1009	**Nils**	0.0051
Tommy	0.0410	**Per**	0.0139
Tonny	0.0498	**Peter**	0.0326
Tony	0.0564	**Stefan**	0.0319
Villy	0.0213	**Sven**	0.0076
Willy	0.0261		
Average y-names	0.0540	**Average common names**	0.0214
Y-names in aggregate	0.0492	**Common names in aggregate**	0.0202

4.2 Discrimination

In our experimental section, we let respondents rate a hypothetical job candidate's personal attributes with the help of a résumé. We find no evidence of systematic discrimination against people with y-names. If any, the results in some instances point to a more positive view of the tested y-name, Ronny, than our tested control name Carl.

4.2.1 Data on the respondents

In total, the survey was answered by 843 people. The distribution of questionnaire answers across the four different résumés is shown in Figure 1. The questionnaires belonging to the highly qualified and the less qualified résumés were distributed on different days and at different locations respectively. Therefore, even though we have an approximately equal number of questionnaires pertaining to each of the tested names, the number of questionnaire answers belonging to each version of the résumés, is not

equal. This is not problematic, since the purpose of this study is to investigate any differences in how people with y-names are perceived compared to others, and not how less qualified people are perceived compared to more qualified people.

Figure 1: Number of respondents answering questionnaires pertaining to each résumé[25]

	Highly qualified (H)	***Less qualified (L)***
Carl (C)	*259*	*157*
Ronny (R)	*274*	*153*

A comparison of the demographics in the different subgroups within the two different versions of the résumé (H and L) shows that they have very similar demographics.[26] The similarities in the demographics are confirmed by running t-tests on all relevant variables: none are proved to be statistically different in between people reading Ronny's CV and those reading Carl's. There are, however, differences in demographic composition between the group that assessed the more qualified résumé and the one that assessed the less qualified résumé. The latter is characterized by a higher average age and a smaller proportion of students.

To see how representative our sample is of the general population in Sweden it is useful to look at some key data. The age distribution of the respondents compared to the age distribution for the Swedish population (15-80 years old) is found in Appendix F. It is apparent that people in their early twenties are over-represented in the sample. This is not very surprising considering the locations at which the experiment was conducted. When comparing with the general population, we can also see that well educated respondents are over-represented (Statistics Sweden, 2010-04).

4.2.2 Regression results

The random distribution of the résumés means that unidentified factors that could influence the assessments of the candidates have been properly equalized. This allows us to regress the assessments in the different categories on a dummy variable for the résumé with Ronny's name without including any control variables. This was repeated

[25] Distributing the résumés twenty at the time we should have an equal number of questionnaire answers to HC as HR and an equal number answers to LC as LR. The apparent difference is caused by a small number of participants failing to fill out the form properly or just leaving it blank.

[26] More specific information can be found in Appendix F.

for the eight attributes rated by the participants and the salary estimation to see whether the ratings differed across résumés. The results are shown in tables 4 to 6 below. We recall that a significant coefficient for one of the attributes means that the assessment of this attribute statistically differs between Ronny and Carl. Having a number of statistically significant negative coefficients would suggest that Ronny is systematically discriminated against in comparison to Carl. The base-case OLS regression was run separately for the less qualified résumé with low education and the more highly qualified résumé.

Table 4: Assessments of attributes and wage regressed on dummy variable for Ronny's résumé (highly qualified résumé)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Drive	**Intelligence**	**Power of Initiative**	**Social Competence**	**Reliability**	**Ability to Plan**	**Creativity**	**Common Sense**	**Salary**
Ronny	0.0459	0.0660	0.214	0.103	-0.101	0.125	0.365*	-0.180	0.524
	(0.29)	(0.55)	(1.27)	(0.68)	(-0.70)	(0.83)	(2.15)	(-1.26)	(1.08)
_cons	5.965**	6.295**	5.767**	6.319**	7.043**	6.353**	4.581**	6.582**	28.23**
	(51.94)	(70.84)	(47.98)	(59.84)	(66.20)	(60.51)	(36.29)	(62.15)	(91.32)
N	530	527	530	528	530	531	529	527	493
R-sq	0.000	0.001	0.003	0.001	0.001	0.001	0.009	0.003	0.002
F	0.0839	0.301	1.624	0.457	0.495	0.689	4.601	1.586	1.171

t statistics in parentheses
* p<0.05 ** p<0.01

Table 5: Assessments of attributes and wage regressed on dummy variable for Ronny's résumé (less qualified résumé)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Drive	**Intelligence**	**Power of Initiative**	**Social Competence**	**Reliability**	**Ability to Plan**	**Creativity**	**Common Sense**	**Salary**
Ronny	0.235	0.165	0.336	0.384*	0.271	0.101	0.412	0.354	0.964
	(1.04)	(0.92)	(1.50)	(2.06)	(1.42)	(0.52)	(1.79)	(1.89)	(1.27)
_cons	6.039**	5.783**	5.824**	6.954**	6.248**	5.822**	4.951**	6.178**	25.39**
	(36.94)	(45.68)	(35.27)	(51.74)	(42.51)	(39.68)	(30.16)	(43.92)	(52.61)
N	310	307	310	309	309	309	310	308	285
R-sq	0.004	0.003	0.007	0.014	0.007	0.001	0.010	0.012	0.006
F	1.082	0.847	2.257	4.243	2.022	0.273	3.208	3.559	1.609

t statistics in parentheses
* p<0.05 ** p<0.01

Table 6: Assessments of attributes and wage regressed on dummy variable for Ronny's résumé (both résumés pooled)

	(1) Drive	(2) Intelligence	(3) Power of Initiative	(4) Social Competence	(5) Reliability	(6) Ability to Plan	(7) Creativity	(8) Common Sense	(9) Salary
Ronny	0.116	0.103	0.259	0.207	0.0357	0.116	0.382**	0.0173	0.685
	(0.89)	(1.02)	(1.93)	(1.74)	(0.31)	(0.98)	(2.80)	(0.15)	(1.65)
Résumé	-0.170	0.461**	-0.118	-0.778**	0.605**	0.543**	-0.393**	0.132	2.617**
	(-1.24)	(4.27)	(-0.85)	(-6.45)	(5.07)	(4.43)	(-2.75)	(1.12)	(5.80)
_cons	6.100**	5.815**	5.862**	7.044**	6.367**	5.815**	4.966**	6.348**	25.53**
	(46.27)	(56.62)	(43.96)	(63.11)	(54.49)	(49.34)	(36.97)	(55.87)	(62.57)
N	840	834	840	837	839	840	839	835	778
R-sq	0.003	0.024	0.005	0.049	0.030	0.024	0.018	0.002	0.049
F	1.162	9.832	2.276	22.88	12.87	9.990	7.416	0.637	18.58

t statistics in parentheses
* p<0.05 ** p<0.01

None of the regressions shows that Ronny systematically receives a lower assessment than Carl or that estimated to have a lower salary. Rather, most of the coefficients for Ronny are actually positive but not statistically significant. The only significant result, at the five percent significance level, in the first regression, shown in Table 4, is a higher assessment of creativity for Ronny. In the regression for the less qualified résumé, presented in Table 5, the only significant difference was that Ronny was perceived to be more socially competent than Carl. When pooling the two groups (and adding a dummy variable to isolate the effect of the different résumés), only the positive coefficient for creativity is significant, as can be seen in Table 6. Considering the fact that the study has multiple tests, one should at least mention the possibility that the significant results produced by the regression may be a coincidence. It is generally so that when a set of statistical inferences are considered simultaneously, the likelihood of committing a type I error (rejecting a true null-hypothesis) increases (Miller, 1981).

In light of these results, we find no support for the claim that people with y-names (here proxied by Ronny) are viewed more negatively compared to others (proxied by Carl) in a job-seeking situation. We continue to investigate whether there is evidence of discriminatory behavior among any subgroups of respondents. From the background information that respondents provided about themselves, they were divided into various subgroups based on sex, education level, age and social class (proxied by the median wealth of the municipality that the respondent grew up in and the education level of their parents). The results are the same, also when we run the regressions in various subsamples. We do, however find some small, sporadic, but seldom significant differences across the subgroups. It is, for example, worth mentioning that when

women assessed the résumé with low education, Ronny received statistically significant higher ratings than Carl in a few variables. However, none of the groups display signs of discriminatory behavior towards Ronny. There are also some very weak tendencies which indicate that people that have grown up in municipalities characterized by low wealth assess Ronny more favorably than Carl. Overall though, our results do not, in contrast to what was indicated by Bart et al., support the hypothesis that raters similar to the applicants would evaluate the candidate more favorably.

4.2.3 Sensitivity analysis

We have used a number of measures to evaluate the robustness of our results. To correct for any incomplete randomization, we added a series of control variables based on the background characteristics of the participants.[27] This did not influence the results noticeably. As a precautionary measure all regressions were run using robust standard errors, which only had minor effects on the results of the regressions.[28] It can be noted that the R-squared of the base regressions described in Table 4 - 6 above is generally very low. The low explanatory power is not very surprising, considering the general lack of significant results.

Furthermore, we tested the robustness of our results to excluding problematic observations. In separate regressions, we removed: outliers, questionnaires suspected not to be seriously answered, answers from participants who grew up abroad and answers from the youngest participants. Outliers were identified using studentized residuals and were defined as answers corresponding to more than three standard deviations away from the predicted assessment of the eight attributes and salary. (Woolridge, 2009) This limit reflects our cautious attitude to excluding observations that may still be part of the true picture. To correct for including questionnaires from respondents who did not give the task enough attention, we removed all the observations which had equal assessments for all eight attributes, as this could be a signal of just trying to get the questionnaire over with. Participants that grew up abroad were excluded as one could suspect them not to be as well-versed in Swedish names as

[27] A description of the different control variables as well as the complete regressions can be found in Appendix E.

[28] The regressions were tested for heteroskedasticity. Using a Breusch-Pagan test, no evidence of heteroskedasticity was found in any of the regressions run on the more highly qualified version of the résumé. Some indications of heteroskedasticity was found in the regressions for planning ability and reliability for the less qualified version of the résumé.

are native Swedes. Lastly, although our aim was to include only participants older than 18, a small number of younger respondents participated. It could very well be that such young people have very limited experience of résumés. Running the regression without including these different groups of observations did not change our conclusions. With one exception, running the regression without including these different groups of observations did not significantly change our results.[29]

4.2.4 Summary of findings

In our first test, regressing the frequency of y-names, we found that they are positively correlated to indicators of low socioeconomic status. In our second test we investigated the representation of men with y-names among clients of the Swedish Prison and Probation Service, and found that it was significantly higher than that the most common male names. However, in our experimental section – contrary to what was expected – we did not find any evidence of discrimination among our respondents: neither in the total sample, nor when examining different subgroups. In the next section we continue to discuss the implications and validity of our results.

[29] When removing outliers for résumés LR2 and LC2, the higher relative assessment of Ronny's social competence was no longer statistically significant.

5. Discussion

The purpose of the thesis has been to investigate a number of questions relating to the y-name syndrome. In this section, the results pertaining to each of our questions are discussed. We conclude by commenting on the implications of our results on earlier findings and suggest possible avenues for further research.

5.1 Socioeconomic status: analysis of the results

First, we investigated the empirical basis of the common syndrome about y-names in Sweden. We concluded that the y-name syndrome is evidence based, as we found strong positive correlations between the rate of y-names per ten thousand inhabitants and all the three tested indicators of low socioeconomic status. Furthermore, we found that y-names on an individual level are over-represented in the population who are, or have been, clients of the Swedish Prison and Probation Service.

It should, however, be stated that the method used in investigating the correlation between y-names and socioeconomic factors in municipalities yield a coarse estimate as we have no data on individuals, only on the places they live in. The relationships found do not necessarily mean that people with y-names have lower socioeconomic status *per se*, only that they, more so than others, tend to live in areas where people on average have lower education, lower median wealth and are less healthy. Still, if you live in municipalities characterized by low socioeconomic status, you are in fact more likely to have low socioeconomic status yourself. To be sure about the investigated group's wealth, health and education compared to the rest of the population, we would need to examine individual data on the chosen indicators, which unfortunately proved to be outside the scope of the thesis. We are also aware of the fact that a number of other life outcomes, such as professional status, social class and life expectancy, should be examined to make sure that our results about people with y-names being members of less privileged groups hold.

If we believe that people with y-names in general have lower social economic status, it is reasonable to discuss the possible causes of such a relationship. There are essentially three plausible explanations. It may first of all be a result of selection, i.e., that parents

who live under socially less favorable circumstances may be more likely to give their child a y-name than other people. Thereby they are also more likely to have a child that in turn will end up living under less fortunate circumstances. Whilst not conclusive, there seem to be large differences as to which names are chosen in different classes - even in such a homogenous population as the Swedish. Thus, it is reasonable to assume that selection is part of the explanation of the y-name syndrome. However, as we have no evidence of this, further research is needed to confirm this notion. Why these names are selected could for example, as Fryer & Levitt (2004) suggested for black names, be explained by ignorance, utility maximization or signaling.

It may also be that names have a causal effect on its bearer. This would mean that a person with a certain name- in this case a y-name – runs a greater risk of ending up with lower socioeconomic status, irrespective of the context he was born into. Two different mechanisms may come into play. One is that other people restrict the possibilities for y-name people solely because of their names. That is, people with y-names may be discriminated against. Another conceivable mechanism through which a name can have a causal effect is, as suggested by research on self-concept, that a particular name may lead its bearer to embody the image he and other people have of that name. If the beliefs about the y-name syndrome are wide-spread, those called Ronny, Conny or another y-name may subconsciously try to live up to the expectations they have and that they perceive their surrounding world have of them. In other words, they themselves may restrict their own potential in life.

5.2 Discrimination: analysis of the results

In our field experiment, one of the mechanisms behind a causal relationship was tested. We did not find any evidence to support the hypothesis that people are discriminated against for having a y-name. Thus, it does not seem to be any disadvantage to have a name that is widely perceived as belonging to trouble-makers and working class, and that - as proven by our tests - actually is more likely to belong to a criminal or a person living in a municipality with low socioeconomic status than people with other names. Nor did we, apart from sporadic occurrences, find any evidence of discrimination being systematically contingent on the socioeconomic group that the respondents belong to. We found no conclusive evidence that people in socially dissimilar groups to the y-name bearers were more negative in their assessments than others.

Surprisingly, some subgroups, notably women assessing the less qualified résumé, were actually more positive to Ronny than they were to Carl. It is difficult to see why this might be. As previously mentioned, the more sporadic results possibly reflect the fact that we performed multiple tests; what is perceived as being significant may in fact not be. Women's more positive appraisals of Ronny in almost all the dimensions are less easily dismissed as a statistical peculiarity. In general though, more positive appraisal for Ronny may also be a result of suspicion. More specifically, people may – perhaps to a greater extent than we initially thought – be aware of correspondence tests and name studies in general. If respondents given Carl's résumé, had suspicions about our purpose and mistakenly thought that we were investigating the prevalence of racism, they may have reacted to Carl Andersson being a typically Swedish name. Subsequently, not wanting to be perceived as racist or prejudice they may – consciously or unconsciously have lowered their rating of Carl. This would result in higher appraisals for Ronny relative to Carl. But again – we have nothing to suggest that women would be more prone to this behavior.

5.2.1 Sources of error

Despite all the measures taken to assure ourselves of performing an effective and precise experiment, and to correct for any weaknesses *ex post*, reasons to question the validity of our results remain. To begin with, for practical purposes, we chose to test only one y-name (Ronny) against one example of a common Swedish name (Carl). Therefore, it relates the perceptions about Ronny's CV relative to Carl's, and not relative to the population in general. We could conceivably be wrong in our conception that Ronny is representative for y-names, and equally mistakenly in our assumptions that Carl is associated with high socioeconomic status. If this would be the case and we had altered the names, it is possible that the same study would have resulted in other conclusions. However, from the tests on indicators of socioeconomic status, it seems like Ronny is an average y-name. It may be that a y-name with worse outcomes, like Danny, would have given the expected results. However, in such a case – knowing what we know about the experiment using an average name as Ronny – the results would be hard to generalize for the whole group of y-names.

Furthermore, it should be noted - and this is a recognized problem with hypothetical hiring exercises - that people realize that they are performing an exercise and not acting

in a real life situation. This could result in the respondents putting less effort into the task, and also, despite being anonymous, that they suppress potential prejudice. As previously mentioned, although not very likely, this could lead to a type II error. It could also be that people in real life would discriminate against a person named Ronny based on prejudice, but, in the absence of any perceived cost of not acting on their prejudice, they feel that they can afford to be more objective. For example, let us assume that there is a perceived benefit for a prejudice person in discriminating against people with y-names (it could be that they are perceived as having lower labor productivity). At the same time there may a cost to being perceived as prejudiced by others. If this is the case, since there are no costs to not being prejudiced in our experiment, only benefits in terms of being perceived as objective, respondents are less prone to discriminate.

Lastly, as our sample is not representative for the whole population, we should be careful generalizing our results. However, since no subgroups were shown to discriminate Ronny, our conclusions should not have to be restricted only to our sample either.

5.2.2 Theory and empirics in light of our results

Overall, our findings indicate that the tested names do not influence the evaluation of our hypothetical applicant. This is in contrast with the views expressed in previous papers, most notably those in name connotation studies. According to our findings, there seems to be little ground to the conclusions made about the far-reaching implications of bearing a certain unattractive name. In fact, the lack of effect in the experiment points to another possibility. It could be that even if some names have negative associations when seen by themselves without any other information, these associations do not translate into negative appraisal of a person when presented with additional information. Probably, when we assess the attractiveness of a name, we envision a person using all our previous experience (or preconceived ideas) of people with that name. If most Connys we have met are working class, we probably envision a working class person when hearing the name. When our initial perceptions are juxtaposed with new information (like a résumé of Conny as university graduate), we may, in the case at hand, abandon our former perceptions. As previously mentioned, the results from Cotton et al.'s connotation study and following hypothetical hiring exercise suggest that this may be the case.

One reason why the results are not as obvious as one may have been led to expect by similar studies is that our study does not really examine the same phenomenon. Bart et al., along with most other researchers, investigate racial discrimination. We, for our part, look at possible intra-racial discrimination based on social class. More precisely, it may be that our diverging conclusions stem from the fact that differences between groups of African-American and white people in the United States are much larger than differences between people with y-names and the rest of the male population in Sweden. A huge racial divide still exists in the US, exemplified in large differences in life expectancy (Harper, Lynch, Burris, & Davey Smith, 2007), wealth (Altonji & Doraszelski, 2005) and criminality (Harrison, Minton, & Sabol, 2007). Although, we have found differences between men with y-names and men with common names, we see it as unlikely that they would be as large as those between African-Americans and white people.

5.3 Implications for further research

In our tests we found empirical evidence confirming popular belief about people with y-names, but no statistical evidence of discrimination against males with y-names. It would be natural for further research to examine the other two determinants of the y-name syndrome.

Firstly, as one of the possible determinants, selection could be investigated by collecting data on births, especially in the decades where most people holding y-names were born, to examine if parents choosing these names differ from other parents in important parameters (class, education, wealth, age etc). We think that social status among parents is likely correlated with a preference for y-names.

Secondly – and more closely related to our thesis – further research should continue to investigate the causality of names on life outcomes. One possible way of conducting such a study is to use sets of identical twins in which one twin has a y-name and the other has not. Significantly deviating life outcomes would then support a casual relationship. It would, however, be difficult to distinguish between two effects. For one, it could be due the individuals own behavior as influenced by how his name affects his perceived image of the self: self-concept (Garwood, 1976). It could also be due to name-based discrimination. If, hypothetically, discrimination could be ruled out, effects on

self-concept seem to be a likely explanation. The practical interpretation of this would be that a Conny or Jimmy, due to his name, consciously or subconsciously makes choices that generate life outcomes for him that coincide with what others associate with y-names.

Furthermore, the results of this thesis still do not enable us to entirely rule out the possibility of name-based discrimination with absolute certainty. To be absolutely positive that people with y-names are not discriminated against, further research would be needed. Correspondence tests and hypothetical hiring exercises could also be performed with other names than the ones we used. Furthermore, it should also be mentioned that even if there is no discrimination against people with y-names in the labor market, discrimination in other institutions may still exist. One possible way of examining discrimination in the education system is to replicate the study by Herbert & McDavid (1973) in which was shown that teachers' grading of children's school work differed depending on the name of the student assessed.

Looking ahead, as a final possible subject for future research, one could look at the new-generation of y-names. Y-names are continually losing popularity, but there are clear indications that other names are taking their place. As proposed by Hagström (2006) and in several discussions about names online, new names are more frequently taken as indicators of low class and trouble. Many different names are mentioned as being the new y-names. Among the most frequent suggestions, male names such as Robin, Kevin and Liam are found. Whether this popular conception will be true as well will have to be determined in the future.

6. Summary of conclusions

This thesis investigated the empirical grounds of the y-name syndrome: the wide-spread conception in Swedish society that men with names that end with a y are likely to have low socioeconomic status and to be overrepresented among criminals. We found the y-name syndrome to be evidence based. It is in fact true that people with y-names are more likely to have low socioeconomic status, including being more prone to crime. Secondly, we explored if people with y-names are discriminated against in the labor market. We found no systematic evidence of discriminatory behavior against y-name bearers. Our results indicate that the conclusions in more basic name studies about the far-reaching implications of bearing a certain unattractive name are premature. Further research is needed to verify our conclusions and investigate other possible determinants of the y-name syndrome.

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Appendix A: The most common y-names

Table A 1: The 45-most common y-names in Sweden (used as main first names)*

Rank	Name	Frequency	Rank	Name	Frequency	Rank	Name	Frequency
1	Tommy	26715	**16**	Willy	1237	**31**	Lenny	320
2	Jimmy	13632	**17**	Freddy	856	**32**	Gerry	308
3	Tony	9832	**18**	Tonny	834	**33**	Stanley	304
4	Conny	8860	**19**	Thommy	737	**34**	Hardy	215
5	Johnny	7087	**20**	Villy	700	**35**	Perry	215
6	Ronny	6986	**21**	Danny	628	**36**	Konny	211
7	Benny	6714	**22**	Ricky	544	**37**	Jhonny	149
8	Jonny	6459	**23**	Eddy	515	**38**	Donny	106
9	Harry	4522	**24**	Denny	514	**39**	Terry	92
10	Henry	4456	**25**	Teddy	503	**40**	Geoffrey	90
11	Jerry	3717	**26**	Sammy	449	**41**	Garry	67
12	Sonny	2021	**27**	Andy	426	**42**	Cenny	55
13	Torgny	1997	**28**	Gary	397	**43**	Fredy	50
14	Billy	1521	**29**	Larry	359	**44**	Mickey	49
15	Timmy	1366	**30**	Bobby	328	**45**	Kelly	46

*Source: Manual searches in Statistics Sweden's database for names ((Statistics Sweden, 2010-01)

Appendix B: The questionnaires

BEDÖMNINGSFORMULÄR

1. Bedömning

Börja med att läsa igenom CV:t.

1.1. Försök att skapa dig en bild av personen bakom CV:t och betygsätt personen enligt kriterierna nedan. Föreställ dig att du är en rekryterare som ska bedöma en person inför en eventuell anställning.

Använd skalan 1 till 10, där 1 motsvarar *lägsta* betyg och 10 *högsta* betyg.

Driv	☐	Pålitlighet	☐
Intelligens	☐	Planeringsförmåga	☐
Initiativtagande	☐	Kreativitet	☐
Social kompetens	☐	Sunt förnuft	☐

.2. Uppskatta personens nuvarande månadsinkomst före skatt i SEK: _____

Fyll i information om dig själv nedan

2. Din ålder:_____

3. Kön: ☐ Man ☐ Kvinna

4. a) Huvudsaklig sysselsättning: ☐ Arbete ☐ Studier

4. b) *Om du valde arbete*, vilken är din senast uppnådda examen?

☐ grundskoleexamen ☐ gymnasieexamen ☐ eftergymnasial examen

4. c) Vilken utbildning läser du/läste du?_____

4. d) Vilket lärosäte läser du/har du läst vid?_____

5. Uppväxtort (kommun)_____

6. Föräldrars utbildning:

Mamma:	☐ grundskoleexamen	☐ gymnasieexamen	☐ eftergymnasial examen
Pappa:	☐ grundskoleexamen	☐ gymnasieexamen	☐ eftergymnasial examen

Appendix C: The résumés

Curriculum Vitae

Carl Andersson

Född: 24 augusti 1975 i Västerås
Familj: Sambo och ett barn, fött 2006
Intressen: Innebandy och film
E-post: carl.andersson@gmail.com

Utbildning

1993-94	Allmän värnplikt, Hundförare vid F7 Såtenäs
1990-93	Tekniskt program, Westerlundska Gymnasiet Enköping

Arbetslivserfarenhet

2006 -	Säljare, Telia Sonera AB
2003-06	Säljare, Ensto Contact AB
1999-01	Försäljningsassistent, Wendler AB
1995-98	Installatör, Manpower
1994-95	Lagerarbete, ICA, Västerås
1992	Sommarjobb, Västerås hamn
1991	Sommarjobb, Johanssons gummiverkstad

Språk

Svenska (modersmål)
Engelska (mycket bra i både tal och skrift)

Övrigt

Körkort B
Aktiv i korplaget ”Alla på bollen” (innebandyspelare och kassör)
Van att använda Windows XP, Excel etc

Curriculum Vitae

Carl Andersson

Född: 24 augusti 1975 i Västerås
Familj: Sambo och ett barn, fött 2006
Intressen: Innebandy och film
E-post: carl.andersson@gmail.com

Utbildning

1995-99	Stockholms Universitet, Företagsekonomi, Ekonomie kandidat.
1993-94	Allmän värnplikt, Hundförare vid F7 Såtenäs
1990-93	Samhällsvetenskapligt program, Westerlundska Gymnasiet Enköping

Arbetslivserfarenhet

2006 -	Biträdande controller, Jernhusen AB
2003-06	Fakturering och projektadministration i affärssystemet SAP R/3, Poolia
2001-03	Databehandling och distribution av marknadskartläggningar, InfoOne
1999-01	Ekonomiassistent, Adecco Uppsala
1994-95	Lagerarbete, ICA, Västerås
1992	Sommarjobb, Västerås hamn
1991	Sommarjobb, Johanssons gummiverkstad

Språk

Svenska (modersmål)
Engelska (mycket bra i både tal och skrift)
Tyska (gymnasienivå)

Övrigt

Körkort B
Aktiv i korplaget ”Alla på bollen” (innebandyspelare och kassör)Van att använda SAP R/3, Windows XP, Excel etc Appendix

Appendix D: Statistics on names in municipalities in Stockholm

Table D 1: Frequency of y-names per ten thousand inhabitants in Stockholm municipalities together with measurements of median wealth, ill health and education level

Municipality	Ill health*	Education level**	Median wealth***	Population	Tommy	Jimmy	Tony	Conny	Johnny	Ronny	Benny	Jonny	Harry	Jerry	Aggregated†
Botkyrka	32	12%	11	81195	35	15	15	10	12	7	8	6	11	4	123
Danderyd	12	45%	522	31150	16	3	4	4	3	3	4		13		51
Ekerö	19	19%	147	25095	39	15	13	20	10	14	6	7	24	5	152
Haninge	28	10%	44	76237	56	27	22	19	14	11	12	7	18	7	192
Huddinge	24	16%	46	95798	41	16	18	15	10	9	9	9	16	5	148
Järfälla	25	17%	99	65295	42	15	11	10	11	8	6	6	15	5	128
Lidingö	17	35%	227	43445	23	9	7	6	4	6	5	5	17	2	83
Nacka	17	27%	163	88085	29	11	10	9	7	7	8	5	18	3	108
Norrtälje	32	9%	150	55927	72	31	17	23	19	11	14	14	30	5	234
Nykvarn	19	10%	64	9227	57	41	17	17	22	14	12	13	13	5	212
Nynäshamn	29	9%	47	25781	63	26	22	23	17	20	19	12	25	5	232
Salem	19	16%	52	15313	46	20	18	12	13	7	11	13	19	8	167
Sigtuna	28	13%	29	39219	38	21	14	15	11	9	9	8	15	5	147
Sollentuna	20	26%	106	63347	29	10	10	8	9	7	6	5	18	2	105
Solna	21	30%	64	66909	29	12	10	8	10	7	7	6	15	3	107
Stockholm	24	28%	47	829417	29	11	11	9	7	6	6	5	15	3	102
Sundbyberg	26	20%	15	37722	35	16	17	14	11	7	10	6	18	3	138
Södertälje	28	12%	16	85270	37	21	19	13	14	8	9	11	14	5	150
Tyresö	21	15%	82	42602	44	23	17	16	13	10	7	13	16	4	163
Täby	16	31%	519	63014	29	6	8	10	7	5	6	6	17	3	95
Upplands Väsb	28	13%	35	38641	44	19	21	12	13	8	12	7	20	4	161
Upplands-Bro	26	12%	39	23202	57	19	24	11	13	9	12	11	17	3	176
Vallentuna	18	16%	145	29361	48	19	11	21	10	11	10	9	23	3	165
Vaxholm	19	25%	178	11001	42	8	10	9	8		5	5	25	5	118
Värmdö	19	14%	147	37756	51	16	16	17	12	11	10	8	21	4	166
Österåker	21	16%	130	39173	52	21	15	16	14	7	10	8	21	5	169
Total Stockholm				2019182	36	14	13	11	9	7	8	7	17	4	126

*** Level of absence due to sickness per capita (days) for men.**

**** Percentage of male population (older than 16) with three years or more of post-high school education**

***** Median wealth for male population (unit: ten thousand crowns)**

† Aggregated data shows the frequency of the ten most common y-names per ten thousand inhabitants

Table D 2: Frequency of common names per ten thousand inhabitants in Stockholm municipalities together with measurements of median wealth, ill health and education level

Municipality	Ill health*	Education level**	Median wealth***	Population	Lars	Karl	Anders	Mikael	Johan	Per	Erik	Jan	Peter	Thomas
Botkyrka	32	12%	11	81195	142	116	103	106	99	91	194	86	75	42
Danderyd	12	45%	522	31150	249	210	229	97	287	210	394	137	133	68
Ekerö	19	19%	147	25095	294	221	195	130	226	200	385	148	142	75
Haninge	28	10%	44	76237	217	177	158	158	140	129	275	124	117	77
Huddinge	24	16%	46	95798	217	159	155	129	152	131	271	117	112	61
Järfälla	25	17%	99	65295	217	185	161	135	151	160	303	114	108	65
Lidingö	17	35%	227	43445	243	197	208	102	243	183	356	137	132	61
Nacka	17	27%	163	88085	233	202	190	118	210	180	350	128	127	66
Norrtälje	32	9%	159	55927	283	292	219	165	212	184	391	141	126	66
Nykvarn	19	10%	64	9227	303	232	190	194	208	221	359	148	154	78
Nynäshamn	29	9%	47	25781	266	228	191	164	155	166	327	163	123	76
Salem	19	16%	52	15313	251	202	192	161	187	161	297	148	117	76
Sigtuna	28	13%	29	39219	209	191	153	142	162	157	280	125	114	60
Sollentuna	20	26%	106	63347	221	201	199	114	197	189	350	126	114	62
Solna	21	30%	64	66909	207	182	172	113	192	155	276	117	123	61
Stockholm	24	28%	47	829417	207	178	177	112	194	164	290	114	116	61
Sundbyberg	26	20%	15	37722	200	161	157	123	148	142	269	116	114	66
Södertälje	28	12%	16	85270	168	158	132	130	135	112	222	110	96	51
Tyresö	21	15%	82	42602	236	188	190	164	185	169	311	139	131	80
Täby	16	31%	519	63014	253	215	225	126	242	203	392	142	130	69
Upplands Väst	28	13%	35	38641	219	189	168	153	171	152	307	133	113	66
Upplands-Bro	26	12%	39	23202	240	188	163	149	148	152	310	126	119	65
Vallentuna	18	16%	145	29361	275	245	206	169	216	174	369	146	145	88
Vaxholm	19	25%	178	11001	275	241	224	135	259	173	397	155	138	65
Värmdö	19	14%	147	37756	275	223	207	160	205	193	360	137	133	81
Österåker	21	16%	130	39173	264	235	214	153	197	164	375	153	138	78
Total Stockholm				2019182	218	186	177	125	186	161	302	121	117	63

*** Level of absence due to sickness per capita (days).**

**** Percentage of male population (older than 16) with three years or more of post-high school education**

***** Median wealth for male population (unit: ten thousand crowns)**

† Aggregated data shows the frequency of the ten most common names per ten thousand inhabitants

Appendix E: Results from statistical analysis

Regression 3: Attribute over dummy for variable Ronny and a number of control variables

$$attribute = \beta_0 + \beta_1 \text{Ronny} (+\beta_2 \text{Résumé}) + \beta_3 \text{Age 18} + \beta_4 \text{ Ages 18-20} + \beta_5 \text{ Age 30- 40} + \beta_6 \text{ Age 40} - 50 + \beta_7 \text{ Age 50} - 60 + \beta_8 \text{ Age 60} + \beta_9 \text{ Man} + \beta_{10} \text{ Student} + \beta_{11} \text{ High School Diploma} + \beta_{12} \text{ University Degree} + \beta_{13} \text{ Mother University} + \beta_{14} \text{ Father High School} + \beta_{15} \text{ Father University} + \beta_{16} \text{ Above median income} + \varepsilon$$

Table E 1: Description of control variables

Variable name	Description
Ronny	Dummy variable that takes the value 1, if the résumé assessed Ronny's and zero if the résumé assessed had Carl's name on it.
Résumé	Dummy variable that takes the value 1, if the résumé assessed had high education and zero if the résumé assessed had low education.
Age 18	Dummy variable that takes the value 1, if the respondent was aged under 18 and zero otherwise
Ages 18-20	Dummy variable that takes the value 1, if the respondent was aged 18-20 and zero otherwise
Age 30-40	Dummy variable that takes the value 1, if the respondent was aged 30-40 and zero otherwise
Age 40-50	Dummy variable that takes the value 1, if the respondent was aged 40-50 and zero otherwise
Age 50-60	Dummy variable that takes the value 1, if the respondent was aged 50-60 and zero otherwise
Age 60	Dummy variable that takes the value 1, if the respondent was aged over 60 and zero otherwise
Man	Dummy variable that takes the value 1, if the respondent was a man zero if the respondent was a women
Student	Dummy variable that takes the value 1, if the respondent was a student and zero if the respondent was working.
High School Diploma	Dummy variable that takes the value 1, if the respondent was working and last attained education was high school and zero otherwise.
University Degree	Dummy variable that takes the value 1, if the respondent was working and last attained education was a university degree
Mother High School	Dummy variable that takes the value 1, if the respondent's mother's education level was high school and zero otherwise.
Mother University	Dummy variable that takes the value 1, if the respondent's mother's education level was university and zero otherwise.
Father High School	Dummy variable that takes the value 1, if the respondent's father's education level was high school and zero otherwise.
Father University	Dummy variable that takes the value 1, if the respondent's father's education level was university and zero otherwise.
Above median income	Dummy variable that takes the value 1, if the respondent grew up in a municipality with median income being higher than the median income in Sweden and zero if the median income was lower.

Table E 2: Assessments of attributes and wage regressed on dummy variable for Ronny's résumé with control variables added (highly qualified résumé)[30]

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Drive	**Intelligence**	**Power of Initiative**	**Social Competence**	**Reliability**	**Ability to Plan**	**Creativity**	**Common Sense**	**Salary**
Ronny	0.0422	0.0581	0.216	0.0790	-0.0880	0.123	0.403*	-0.160	0.583
	(0.26)	(0.50)	(1.28)	(0.52)	(-0.60)	(0.82)	(2.38)	(-1.10)	(1.25)
Age <18	0.565	0.987**	0.381	1.334**	0.514	0.622	0.199	1.068*	-1.540
	(0.82)	(2.77)	(0.48)	(2.64)	(0.90)	(1.24)	(0.22)	(2.11)	(-1.01)
Age 18-20	0.0504	0.475*	0.272	0.523	0.0196	0.174	-0.0866	-0.297	1.502
	(0.15)	(2.21)	(0.73)	(1.64)	(0.07)	(0.58)	(-0.28)	(-1.10)	(1.64)
Age 30-40	0.448	0.418	0.105	0.122	0.0646	0.139	0.209	0.0966	0.894
	(1.11)	(1.66)	(0.24)	(0.31)	(0.23)	(0.35)	(0.44)	(0.27)	(0.93)
Age 40-50	0.344	-0.144	0.426	0.128	-0.0784	1.035*	-0.254	-0.00560	-1.609
	(0.69)	(-0.38)	(0.64)	(0.20)	(-0.14)	(1.99)	(-0.36)	(-0.01)	(-0.90)
Age 50-60	0.709	0.685	0.822	0.841*	0.899**	0.478	0.827	0.395	3.432
	(1.44)	(1.89)	(1.69)	(2.17)	(2.76)	(1.41)	(1.72)	(0.98)	(1.89)
Age >60	0.412	0.492	0.573	0.852	0.567	0.653	0.887	0.475	3.031
	(1.10)	(1.82)	(1.34)	(1.51)	(0.82)	(1.67)	(1.55)	(0.82)	(1.76)
Man	-0.311*	-0.390**	-0.299	-0.152	-0.0269	-0.161	0.00722	-0.183	0.281
	(-1.97)	(-3.37)	(-1.79)	(-0.99)	(-0.19)	(-1.09)	(0.04)	(-1.30)	(0.59)
Student	0.507	-0.175	0.535	-0.0550	-0.375	0.251	0.327	0.241	-1.656
	(0.69)	(-0.47)	(0.87)	(-0.08)	(-0.67)	(0.48)	(0.45)	(0.34)	(-1.00)
High School Diploma	0.157	0.703	0.879	0.796	0.485	1.649**	1.425	1.472	1.279
	(0.19)	(1.57)	(1.18)	(1.04)	(0.79)	(3.09)	(1.63)	(1.92)	(0.57)
University Degree	0.563	0.231	0.774	0.112	-0.0741	0.560	0.599	0.304	-0.0720
	(0.77)	(0.62)	(1.25)	(0.15)	(-0.13)	(1.10)	(0.80)	(0.42)	(-0.04)
Mother High School	-0.231	0.142	-0.372	0.221	0.144	-0.309	-0.316	0.178	-1.482
	(-0.75)	(0.60)	(-1.23)	(0.73)	(0.51)	(-1.13)	(-0.91)	(0.60)	(-1.47)
Mother University	-0.408	0.0585	-0.400	-0.153	0.165	-0.153	-0.689*	0.360	-1.209
	(-1.53)	(0.26)	(-1.40)	(-0.54)	(0.58)	(-0.58)	(-2.14)	(1.43)	(-1.29)
Father High School	0.166	0.178	-0.403	-0.221	0.250	-0.176	-0.106	-0.0225	-0.462
	(0.64)	(0.95)	(-1.57)	(-0.81)	(0.96)	(-0.70)	(-0.35)	(-0.08)	(-0.55)
Father University	-0.0765	0.0346	-0.198	0.147	0.273	0.0892	0.0217	-0.0575	0.0846
	(-0.31)	(0.20)	(-0.78)	(0.56)	(1.08)	(0.39)	(0.07)	(-0.24)	(0.10)
Above median income	-0.121	-0.0421	-0.0237	-0.147	-0.0189	0.0862	-0.0257	-0.0655	0.637
	(-0.77)	(-0.36)	(-0.14)	(-0.96)	(-0.13)	(0.57)	(-0.15)	(-0.46)	(1.36)
_cons	5.874**	6.308**	5.763**	6.335**	6.930**	6.091**	4.589**	6.151**	29.82**
	(8.10)	(15.82)	(9.48)	(8.77)	(11.56)	(11.73)	(6.18)	(8.90)	(17.40)
N	527	524	527	525	527	528	526	524	490
R-sq	0.048	0.095	0.062	0.056	0.034	0.066	0.078	0.037	0.099
adj. R-sq	0.019	0.066	0.032	0.027	0.004	0.037	0.049	0.007	0.069
F	2.065	3.667	2.421	2.486	1.628	4.566	3.012	1.800	2.846

t statistics in parentheses

* p<0.05 ** p<0.01

[30] Indications of heteroskedasticity were found in the regressions for reliability and planning. When running the regressions with robust standard errors, the results did not change noticeably.

Table E 3: Assessments of attributes and wage regressed on dummy variable for Ronny's résumé with control variables added (less qualified résumé)[31]

Attributes, Less Educated Résumé with Controls

	(1) Drive	(2) Intelligence	(3) Power of Initiative	(4) Social Competence	(5) Reliability	(6) Ability to Plan	(7) Creativity	(8) Common Sense	(9) Salary
Ronny	0.353	0.203	0.360	0.463*	0.280	0.127	0.463*	0.352	0.887
	(1.51)	(1.14)	(1.58)	(2.33)	(1.41)	(0.65)	(2.11)	(1.76)	(1.21)
Age <18	1.584**	1.595**	1.227**	0.411	0.361	0.849*	0.825	0.810*	6.750*
	(3.05)	(3.89)	(2.71)	(0.94)	(0.95)	(2.38)	(1.62)	(2.11)	(2.26)
Age 18-20	0.611	0.286	-0.00414	0.107	0.286	0.218	-0.113	-0.117	-1.863
	(1.11)	(0.84)	(-0.01)	(0.26)	(0.70)	(0.44)	(-0.22)	(-0.24)	(-1.39)
Age 30-40	0.104	-0.171	0.544	-0.181	-0.636	0.538	0.408	-0.0573	-2.869**
	(0.19)	(-0.51)	(1.13)	(-0.38)	(-1.34)	(1.26)	(0.71)	(-0.18)	(-2.66)
Age 40-50	-0.699	0.321	-1.203	-0.920	-0.232	-0.329	-0.791	-0.744	2.000
	(-1.10)	(0.81)	(-1.90)	(-1.65)	(-0.45)	(-0.68)	(-1.24)	(-1.88)	(1.42)
Age 50-60	-0.0559	0.404	0.0926	-0.378	0.480	0.624	0.519	0.151	1.011
	(-0.15)	(1.41)	(0.25)	(-1.24)	(1.54)	(1.81)	(1.42)	(0.45)	(0.95)
Age >60	0.758	0.646	0.969	-0.704	1.077**	1.173**	1.310*	0.643	3.213*
	(1.54)	(1.43)	(1.75)	(-1.65)	(2.67)	(2.63)	(2.56)	(1.55)	(2.07)
Man	-0.108	-0.513**	-0.239	-0.229	-0.421*	-0.329	-0.216	-0.128	1.119
	(-0.46)	(-3.03)	(-1.06)	(-1.18)	(-2.19)	(-1.66)	(-0.98)	(-0.65)	(1.71)
Student	-0.747*	-0.550	-0.612	-0.232	-0.489	-0.457	-0.585	-0.579	-3.019**
	(-2.27)	(-1.70)	(-1.72)	(-0.60)	(-1.43)	(-1.44)	(-1.72)	(-1.72)	(-2.60)
High School Diploma	-0.0921	0.279	-0.144	0.575	0.458	-0.0213	0.646	0.250	-2.600
	(-0.20)	(0.73)	(-0.30)	(1.22)	(1.14)	(-0.05)	(1.53)	(0.59)	(-1.83)
University Degree	-0.258	-0.436	-0.697	0.207	-0.275	-0.602	-0.397	-0.358	-0.724
	(-0.60)	(-1.21)	(-1.65)	(0.47)	(-0.73)	(-1.59)	(-1.01)	(-0.87)	(-0.55)
Mother High School	0.0417	-0.127	0.0373	0.188	0.670*	0.105	0.312	0.383	0.947
	(0.11)	(-0.45)	(0.10)	(0.54)	(2.18)	(0.36)	(0.84)	(1.28)	(0.75)
Mother University	-0.436	-0.215	-0.694	-0.650	0.461	-0.134	-0.620	-0.0286	-0.212
	(-0.98)	(-0.70)	(-1.73)	(-1.73)	(1.43)	(-0.40)	(-1.61)	(-0.09)	(-0.16)
Father High School	-0.00202	-0.207	-0.142	-0.105	-0.357	-0.128	-0.596	-0.145	-0.796
	(-0.01)	(-0.77)	(-0.40)	(-0.33)	(-1.20)	(-0.45)	(-1.62)	(-0.50)	(-0.68)
Father University	-0.221	-0.478	-0.0287	0.451	-0.429	-0.209	-0.282	-0.184	1.407
	(-0.55)	(-1.79)	(-0.08)	(1.42)	(-1.46)	(-0.72)	(-0.90)	(-0.63)	(1.09)
Above median income	0.188	-0.0485	0.0212	0.222	-0.101	0.0804	0.0920	0.00500	0.111
	(0.78)	(-0.26)	(0.09)	(1.12)	(-0.52)	(0.40)	(0.40)	(0.02)	(0.15)
_cons	6.394**	6.443**	6.592**	7.042**	6.480**	6.164**	5.425**	6.499**	25.70**
	(12.61)	(16.23)	(12.62)	(13.53)	(15.42)	(13.55)	(11.47)	(14.39)	(18.99)
N	292	289	292	291	291	291	292	290	270
R-sq	0.103	0.196	0.120	0.098	0.129	0.098	0.196	0.100	0.163
adj. R-sq	0.050	0.149	0.069	0.046	0.078	0.045	0.149	0.048	0.110
F	2.037	4.066	2.641	2.323	3.512	2.264	4.805	2.498	2.745

t statistics in parentheses

* p<0.05 ** p<0.01

[31] No evidence of heteroskedasticity

Table E 4: Assessments of attributes and wage regressed on dummy variable for Ronny's résumé with control variables added (both résumés pooled).[32]

Attributes, Pooled Résumés with Controls

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Drive	**Intelligence**	**Power of Initiative**	**Social Competence**	**Reliability**	**Ability to Plan**	**Creativity**	**Common Sense**	**Salary**
Résumé	0.193	0.925**	0.291	-0.476**	0.912**	0.960**	0.178	0.410**	3.902**
	(1.20)	(7.92)	(1.83)	(-3.50)	(6.76)	(7.01)	(1.12)	(3.02)	(8.13)
Ronny	0.147	0.112	0.276*	0.193	0.0284	0.121	0.414**	-0.00639	0.675
	(1.12)	(1.14)	(2.05)	(1.61)	(0.24)	(1.01)	(3.08)	(-0.06)	(1.68)
Age <18	1.182**	1.471**	1.047**	0.882**	0.525	0.871**	0.685	0.915**	4.751*
	(3.02)	(5.23)	(2.69)	(2.71)	(1.79)	(3.09)	(1.56)	(3.20)	(2.14)
Age 18-20	0.183	0.395*	0.183	0.403	0.0545	0.196	-0.128	-0.274	0.433
	(0.64)	(2.23)	(0.61)	(1.63)	(0.24)	(0.76)	(-0.50)	(-1.15)	(0.56)
Age 30-40	0.346	0.233	0.265	0.0374	-0.153	0.297	0.298	0.0793	-0.701
	(1.09)	(1.15)	(0.83)	(0.12)	(-0.61)	(1.03)	(0.81)	(0.32)	(-0.95)
Age 40-50	-0.293	0.0190	-0.405	-0.477	-0.267	0.296	-0.433	-0.424	0.0738
	(-0.71)	(0.07)	(-0.87)	(-1.16)	(-0.76)	(0.82)	(-0.95)	(-1.19)	(0.07)
Age 50-60	0.267	0.613**	0.364	0.159	0.709**	0.568*	0.645*	0.320	1.840
	(0.91)	(2.80)	(1.24)	(0.65)	(3.10)	(2.35)	(2.20)	(1.22)	(1.90)
Age >60	0.669	0.738*	0.797*	-0.116	1.032**	0.976**	1.162**	0.688*	3.075*
	(1.90)	(2.42)	(2.06)	(-0.35)	(2.95)	(3.19)	(3.08)	(2.12)	(2.58)
Man	-0.251	-0.443**	-0.302*	-0.177	-0.175	-0.233*	-0.0957	-0.161	0.498
	(-1.92)	(-4.62)	(-2.28)	(-1.50)	(-1.53)	(-1.97)	(-0.73)	(-1.42)	(1.28)
Student	-0.353	-0.469	-0.374	-0.242	-0.422	-0.319	-0.364	-0.321	-2.810**
	(-1.15)	(-1.86)	(-1.24)	(-0.74)	(-1.44)	(-1.20)	(-1.22)	(-1.04)	(-3.06)
High School Diploma	-0.0946	0.437	0.161	0.646	0.527	0.468	0.822*	0.594	-1.694
	(-0.24)	(1.47)	(0.42)	(1.66)	(1.53)	(1.41)	(2.17)	(1.62)	(-1.41)
University Degree	-0.182	-0.210	-0.209	0.129	-0.188	-0.167	-0.131	-0.213	-0.595
	(-0.53)	(-0.79)	(-0.63)	(0.36)	(-0.60)	(-0.59)	(-0.39)	(-0.63)	(-0.60)
Mother High School	-0.169	0.0428	-0.226	0.177	0.395	-0.115	-0.0122	0.259	-0.212
	(-0.70)	(0.23)	(-0.93)	(0.77)	(1.89)	(-0.57)	(-0.05)	(1.23)	(-0.25)
Mother University	-0.387	-0.00415	-0.440	-0.313	0.365	-0.0904	-0.553*	0.276	-0.480
	(-1.66)	(-0.02)	(-1.84)	(-1.39)	(1.70)	(-0.44)	(-2.29)	(1.45)	(-0.58)
Father High School	0.138	-0.00261	-0.270	-0.103	-0.0141	-0.189	-0.335	-0.0703	-0.761
	(0.64)	(-0.02)	(-1.25)	(-0.50)	(-0.07)	(-1.02)	(-1.44)	(-0.36)	(-1.04)
Father University	-0.124	-0.198	-0.144	0.273	-0.0511	-0.0654	-0.172	-0.143	0.442
	(-0.57)	(-1.33)	(-0.69)	(1.38)	(-0.27)	(-0.37)	(-0.80)	(-0.80)	(0.58)
Above median income	-0.0286	-0.0391	-0.00824	-0.0372	-0.0449	0.103	0.0202	-0.0471	0.363
	(-0.22)	(-0.40)	(-0.06)	(-0.31)	(-0.38)	(0.86)	(0.15)	(-0.41)	(0.89)
_cons	6.390**	5.939**	6.296**	6.959**	6.187**	5.775**	5.131**	6.304**	26.21**
	(16.55)	(19.84)	(16.53)	(17.79)	(17.68)	(17.38)	(13.78)	(17.89)	(23.07)
N	819	813	819	816	818	819	818	814	760
R-sq	0.051	0.143	0.062	0.090	0.080	0.083	0.121	0.046	0.123
adj. R-sq	0.031	0.125	0.042	0.071	0.060	0.063	0.102	0.026	0.103
F	2.606	7.778	3.298	5.020	4.884	4.856	7.072	2.763	7.195

t statistics in parentheses

* p<0.05 ** p<0.01

[32] No evidence of heteroskedasticity

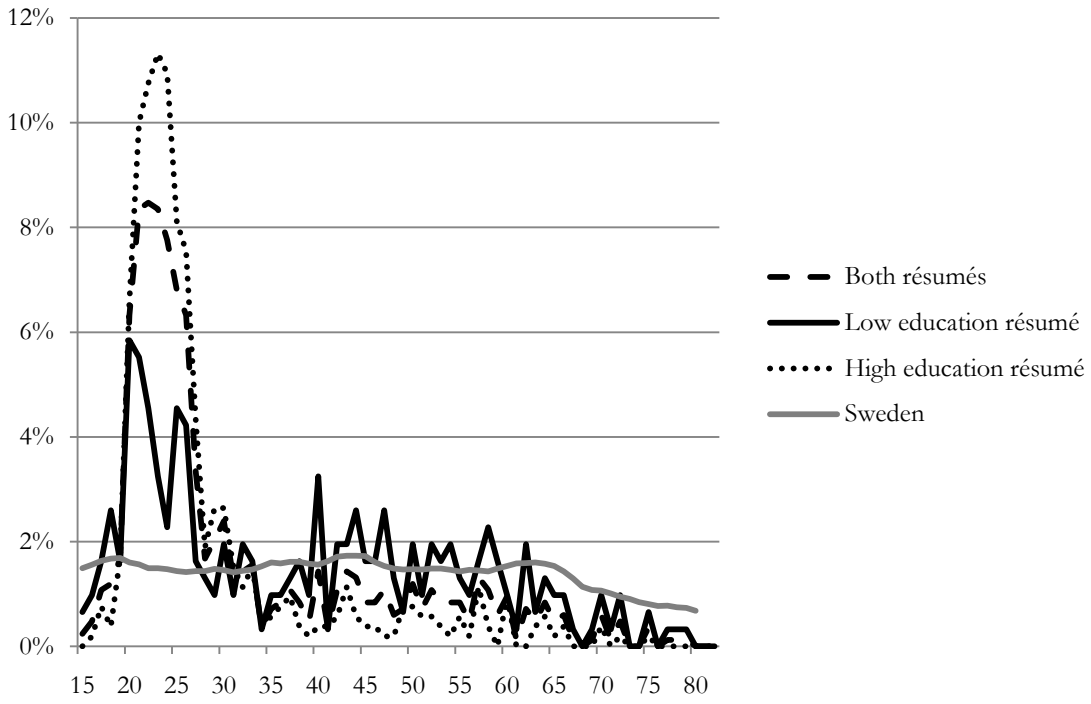
Appendix F: Descriptive statistics of respondents' characteristics

Table F 1: Descriptive statistics of control variables

Mean estimation

	Mean	Std. Err		Mean	Std. Err
Age			**Mother's education: nine years compulsory school**		
Low education CV, Carl	37.8175	1.43853	Low education CV, Carl	0.39416	0.0419
Low education CV, Ronny	36.9511	1.34465	Low education CV, Ronny	0.3007	0.03848
High education CV, Carl	28.0502	0.68488	High education CV, Carl	0.18828	0.02534
High education CV, Ronny	27.6314	0.62216	High education CV, Ronny	0.15294	0.02258
Man			**Mother education: High school**		
Low education CV, Carl	0.46715	0.04402	Low education CV, Carl	0.24818	0.03704
Low education CV, Ronny	0.47552	0.04191	Low education CV, Ronny	0.23776	0.03573
High education CV, Carl	0.50628	0.03241	High education CV, Carl	0.16736	0.0242
High education CV, Ronny	0.46667	0.0313	High education CV, Ronny	0.24706	0.02706
Student			**Mother's education: University**		
Low education CV, Carl	0.32847	0.04027	Low education CV, Carl	0.35037	0.04091
Low education CV, Ronny	0.37762	0.04634	Low education CV, Ronny	0.44056	0.04166
High education CV, Carl	0.73222	0.0287	High education CV, Carl	0.6318	0.03126
High education CV, Ronny	0.7451	0.02734	High education CV, Ronny	0.58431	0.03092
Median wealth of home municipalities (100=median for Sweden)			**Father's education: Nine years compulsory school**		
Low education CV, Carl	112.51	9.51749	Low education CV, Carl	0.37226	0.04145
Low education CV, Ronny	111.415	9.31363	Low education CV, Ronny	0.33566	0.03963
High education CV, Carl	163.721	13.379	High education CV, Carl	0.20084	0.02597
High education CV, Ronny	166.166	12.6095	High education CV, Ronny	0.18824	0.02453
Nine year compulsory school			**Father's education: High school**		
Low education CV, Carl	0.0146	0.01028	Low education CV, Carl	0.21168	0.03503
Low education CV, Ronny	0.03497	0.01542	Low education CV, Ronny	0.22378	0.03497
High education CV, Carl	0.00418	0.00418	High education CV, Carl	0.21757	0.02674
High education CV, Ronny	0.00784	0.00554	High education CV, Ronny	0.20392	0.02528
High school			**Father's education: University**		
Low education CV, Carl	0.14599	0.03028	Low education CV, Carl	0.39416	0.0419
Low education CV, Ronny	0.23776	0.03573	Low education CV, Ronny	0.40559	0.0412
High education CV, Carl	0.04184	0.01298	High education CV, Carl	0.56904	0.0321
High education CV, Ronny	0.02353	0.00951	High education CV, Ronny	0.59216	0.03084
University					
Low education CV, Carl	0.50365	0.04287			
Low education CV, Ronny	0.3986	0.04109			
High education CV, Carl	0.20084	0.02597			
High education CV, Ronny	0.21569	0.02581			

Figure F 1: Age distribution in experiment



Source for age distribution in Sweden is Statistics Sweden **(2010-05)**.

Appendix G: Sensitivity analyses

Table G 1: Assessments of attributes and wage regressed on a dummy variable for Ronny's résumé without outliers

Drive

	Highly Qualified		Less Qualified		Pooled	
	w. outliers	**no outliers**	**w. outliers**	**no outliers**	**w. outliers**	**no outliers**
Ronny	0.0459	0.0459	0.235	0.165	0.116	0.116
	(0.29)	(0.29)	(1.04)	(0.92)	(0.89)	(0.89)
Résumé					-0.170	-0.170
					(-1.26)	(-1.26)
N	530	530	310	307	840	840

Intelligence

	Highly Qualified		Less Qualified		Pooled	
	w. outliers	**no outliers**	**w. outliers**	**no outliers**	**w. outliers**	**no outliers**
Ronny	0.0660	0.0316	0.165	0.165	0.103	0.102
	(0.55)	(0.27)	(0.92)	(0.92)	(1.02)	(1.03)
Résumé					0.461**	0.482**
					(4.42)	(4.68)
N	527	523	307	307	834	832

Power of Initiative

	Highly Qualified		Less Qualified		Pooled	
	w. outliers	**no outliers**	**w. outliers**	**no outliers**	**w. outliers**	**no outliers**
Ronny	0.214	0.214	0.336	0.336	0.259	0.259
	(1.27)	(1.27)	(1.50)	(1.50)	(1.93)	(1.93)
Résumé					-0.118	-0.118
					(-0.85)	(-0.85)
N	530	530	310	310	840	840

Social Competence

	Highly Qualified		Less Qualified		Pooled	
	w. outliers	**no outliers**	**w. outliers**	**no outliers**	**w. outliers**	**no outliers**
Ronny	0.103	0.219	0.384*	0.311	0.207	0.192
	(0.67)	(1.30)	(2.06)	(1.72)	(1.74)	(1.64)
Résumé					-0.778**	-0.777**
					(-6.32)	(-6.42)
N	528	524	309	307		

t statistics in parentheses

* p<0.05 ** p<0.01

Reliability

	Highly Qualified		Less Qualified		Pooled	
	w. outliers	no outliers	w. outliers	no outliers	w. outliers	no outliers
Ronny	-0.101	-0.0978	0.271	0.201	0.0357	0.0134
	(-0.70)	(-0.75)	(1.42)	(1.09)	(0.31)	(0.13)
Résumé					0.605**	0.684**
					(5.08)	(6.20)
N	530	519	309	307	839	826

Planning Ability

	Highly Qualified		Less Qualified		Pooled	
	w. outliers	no outliers	w. outliers	no outliers	w. outliers	no outliers
Ronny	0.125	0.166	0.101	0.0626	0.116	0.128
	(0.83)	(1.11)	(0.52)	(0.33)	(0.98)	(1.09)
Résumé					0.543**	0.544**
					(4.40)	(4.48)
N	531	529	309	308	840	837

Creativity

	Highly Qualified		Less Qualified		Pooled	
	w. outliers	no outliers	w. outliers	no outliers	w. outliers	no outliers
Ronny	0.365*	0.365*	0.412	0.412	0.382**	0.382**
	(2.15)	(2.15)	(1.79)	(1.79)	(2.80)	(2.80)
Résumé					-0.393**	-0.393**
					(-2.78)	(-2.78)
N	529	529	310	310	839	839

Common Sense

	Highly Qualified		Less Qualified		Pooled	
	w. outliers	no outliers	w. outliers	no outliers	w. outliers	no outliers
Ronny	-0.180	-0.188	0.354	0.320	0.0173	0.000302
	(-1.26)	(-1.37)	(1.89)	(1.73)	(0.15)	(0.00)
Résumé					0.132	0.159
					(1.12)	(1.39)
N	527	523	308	307	835	830

t statistics in parentheses

* p<0.05 ** p<0.01

Salary

	Higly Qualified		**Less Qualified**		**Pooled**	
	w. outliers	**no outliers**	**w. outliers**	**no outliers**	**w. outliers**	**no outliers**
Ronny	**0.524**	**0.271**	**0.964**	**0.582**	**0.685**	**0.331**
	(1.08)	(0.63)	(1.27)	(0.92)	(1.65)	(0.92)
Résumé					**2.617****	**2.982****
					(6.07)	(7.98)
N	**493**	**487**	**285**	**281**	**778**	**769**

t statistics in parentheses

* p<0.05 ** p<0.01

Table G 2: Assessments of attributes and wage regressed on a dummy variable for Ronny's résumé without outlier when removing answers from respondents not born in Sweden

Highly Qualified Résumé	(1) Drive	(2) Intelligence	(3) Power of Initiative	(4) Social Competence	(5) Reliability	(6) Ability to Plan	(7) Creativity	(8) Common Sense	(9) Salary
Ronny	0.104	0.0512	0.239	0.0787	-0.146	0.153	0.347*	-0.151	0.277
	(0.64)	(0.42)	(1.40)	(0.51)	(-0.99)	(1.01)	(2.04)	(-1.05)	(0.60)
N	507	503	506	504	506	507	505	504	470
Less Qualified Résumé	**Drive**	**Intelligence**	**Power of Initiative**	**Social Competence**	**Reliability**	**Ability to Plan**	**Creativity**	**Common Sense**	**Salary**
Ronny	0.248	0.184	0.339	0.329	0.253	0.137	0.431	0.366	0.771
	(1.09)	(1.01)	(1.50)	(1.76)	(1.30)	(0.70)	(1.85)	(1.93)	(1.00)
N	300	297	300	299	299	299	300	298	276
Pooled Résumés	**Drive**	**Intelligence**	**Power of Initiative**	**Social Competence**	**Reliability**	**Ability to Plan**	**Creativity**	**Common Sense**	**Salary**
Ronny	0.157	0.101	0.276*	0.172	0.00250	0.147	0.378**	0.0411	0.460
	(1.19)	(0.99)	(2.03)	(1.44)	(0.02)	(1.23)	(2.75)	(0.36)	(1.12)
N	807	800	806	803	805	806	805	802	746

t statistics in parentheses
* p<0.05 ** p<0.01

Table G 3: Assessments of attributes and wage regressed on a dummy variable for Ronny's résumé without outlier when removing answers from very young respondents

Highly Qualified Résumé	(1) Drive	(2) Intelligence	(3) Power of Initiative	(4) Social Competence	(5) Reliability	(6) Ability to Plan	(7) Creativity	(8) Common Sense	(9) Salary
Ronny	0.0468	0.0695	0.209	0.127	-0.118	0.117	0.355*	-0.183	0.512
	(0.29)	(0.57)	(1.24)	(0.83)	(-0.81)	(0.77)	(2.09)	(-1.28)	(1.06)
N	525	522	525	523	525	526	524	522	491
Less Qualified Résumé	**Drive**	**Intelligence**	**Power of Initiative**	**Social Competence**	**Reliability**	**Ability to Plan**	**Creativity**	**Common Sense**	
Ronny	0.247	0.142	0.341	0.408*	0.309	0.0826	0.411	0.348	0.348
	(1.07)	(0.78)	(1.50)	(2.14)	(1.58)	(0.42)	(1.75)	(1.82)	(1.82)
N	299	296	299	298	298	298	299	297	297
Pooled Résumés	**Drive**	**Intelligence**	**Power of Initiative**	**Social Competence**	**Reliability**	**Ability to Plan**	**Creativity**	**Common Sense**	
Résumé	-0.164	0.486**	-0.0874	-0.787**	0.625**	0.570**	-0.375**	0.159	0.491
	(-1.17)	(4.44)	(-0.62)	(-6.44)	(5.14)	(4.57)	(-2.59)	(1.33)	(1.01)
N	824	818	824	821	823	824	823	819	488

t statistics in parentheses
* p<0.05 ** p<0.01

Table G 4: Assessments of attributes and wage regressed on a dummy variable for Ronny's résumé without outlier when removing answers suspected to not be serious

Highly Qualified Résumé	(1) Drive	(2) Intelligence	(3) Power of Initiative	(4) Social Competence	(5) Reliability	(6) Ability to Plan	(7) Creativity	(8) Common Sense	(9) Salary
Ronny	0.0682	0.0912	0.236	0.129	-0.0723	0.151	0.380*	-0.155	0.512
	(0.43)	(0.77)	(1.41)	(0.85)	(-0.51)	(1.01)	(2.23)	(-1.09)	(1.06)
N	528	525	528	526	528	529	527	525	491
Less Qualified Résumé	**Drive**	**Intelligence**	**Power of Initiative**	**Social Competence**	**Reliability**	**Ability to Plan**	**Creativity**	**Common Sense**	**Salary**
Ronny	0.300	0.221	0.400	0.480*	0.344	0.155	0.457	0.430*	1.015
	(1.30)	(1.22)	(1.76)	(2.56)	(1.78)	(0.79)	(1.96)	(2.26)	(1.31)
N	300	297	300	299	299	299	300	298	277
Pooled Résumés	**Drive**	**Intelligence**	**Power of Initiative**	**Social Competence**	**Reliability**	**Ability to Plan**	**Creativity**	**Common Sense**	**Salary**
Résumé	-0.152	0.491**	-0.0951	-0.791**	0.620**	0.572**	-0.347*	0.146	0.693
	(-1.09)	(4.52)	(-0.67)	(-6.55)	(5.17)	(4.64)	(-2.41)	(1.23)	(1.66)
N	828	822	828	825	827	828	827	823	768

t statistics in parentheses
* p<0.05 ** p<0.01

Table G 5: Assessments of attributes and salary regressed on a dummy variable for Ronny's in different subgroups (highly qualified résumé)

	(1) Drive	(2) Intelligence	(3) Power of Initiative	(4) Social Competency	(5) Reliability	(6) Ability to Plan	(7) Creativity	(8) Common Sense	(9) Salary
Men	0.00960	-0.00400	0.319	-0.0455	-0.370	0.118	0.338	0.125	-0.182
	(0.04)	(-0.02)	(1.37)	(-0.22)	(-1.78)	(0.57)	(1.50)	(0.60)	(-0.27)
N	250	249	251	249	249	250	249	248	229
Women	0.0514	0.0973	0.0946	0.225	0.136	0.116	0.384	-0.467*	1.145
	(0.23)	(0.59)	(0.40)	(1.01)	(0.67)	(0.54)	(1.52)	(-2.43)	(1.65)
N	280	278	279	279	281	281	280	279	264
Income above Median	0.171	0.137	0.176	-0.0311	-0.284	0.0347	0.416	-0.0270	2.036**
	(0.71)	(0.74)	(0.68)	(-0.14)	(-1.27)	(0.15)	(1.63)	(-0.13)	(2.81)
N	217	216	216	217	217	217	216	217	203
Income below Median	-0.0316	0.0196	0.242	0.205	0.0256	0.182	0.331	-0.284	-0.557
	(-0.15)	(0.12)	(1.08)	(0.99)	(0.14)	(0.90)	(1.45)	(-1.46)	(-0.87)
N	313	311	314	311	313	314	313	310	290
Income above Q2	0.496	0.285	0.367	0.0873	-0.447	-0.121	0.638	0.0200	1.966*
	(1.61)	(1.15)	(1.07)	(0.29)	(-1.52)	(-0.40)	(1.97)	(0.08)	(2.55)
N	116	116	116	116	116	116	115	116	112
Business and Economics Degree	0.132	0.0358	0.0858	0.0325	-0.370	0.107	0.150	-0.255	-0.274
	(0.59)	(0.21)	(0.36)	(0.14)	(-1.76)	(0.50)	(0.68)	(-1.29)	(-0.46)

N	266	263	265	264	266	265	264	264	249
High School Diploma	-1.500	-0.300	-0.267	-0.867	-0.933	-0.500	0.0667	-0.733	5.833
	(-1.49)	(-0.56)	(-0.25)	(-1.62)	(-1.65)	(-1.26)	(0.07)	(-1.21)	(1.30)
N	16	16	16	16	16	16	16	16	16
University Degree	0.0788	0.346	0.443	0.648*	0.273	-0.0490	0.406	-0.244	-0.140
	(0.22)	(1.31)	(1.17)	(1.99)	(0.88)	(-0.15)	(1.04)	(-0.80)	(-0.15)
N	110	111	110	110	110	111	111	110	99
Age 18-20	0.303	0.695	1.680*	0.228	-0.213	0.535	0.920	0.375	-0.0918
	(0.45)	(1.74)	(2.38)	(0.38)	(-0.41)	(0.91)	(1.58)	(0.70)	(-0.05)
N	44	43	44	44	44	44	44	43	41
Age 20-30	0.0704	-0.0975	0.00349	0.0173	-0.198	-0.0148	0.226	-0.315	0.477
	(0.39)	(-0.69)	(0.02)	(0.10)	(-1.14)	(-0.08)	(1.21)	(-1.88)	(0.88)
N	371	368	371	370	371	371	369	370	349
Age 30-50	0.0833	0.531	0.247	0.375	0.299	0.356	0.578	-0.172	0.584
	(0.19)	(1.66)	(0.54)	(0.94)	(0.79)	(0.90)	(1.07)	(-0.43)	(0.50)
N	68	69	68	68	68	69	69	67	64
Age >50	-0.273	0.125	0.426	0.815	0.222	0.295	0.557	0.534	3.055
	(-0.43)	(0.28)	(0.63)	(1.48)	(0.44)	(0.64)	(0.87)	(0.98)	(1.13)
N	38	38	38	37	38	38	38	38	31
Well Educated Parents	-0.161	-0.102	-0.0464	0.106	-0.187	0.191	0.255	-0.226	0.737
	(-0.70)	(-0.60)	(-0.19)	(0.49)	(-0.97)	(0.92)	(1.11)	(-1.14)	(1.17)
N	254	254	255	255	255	255	255	254	240
Less Educated Parents	0.281	0.240	0.439	0.126	-0.0230	0.0544	0.453	-0.144	0.190
	(1.30)	(1.42)	(1.90)	(0.58)	(-0.11)	(0.24)	(1.82)	(-0.69)	(0.26)
N	269	266	268	267	268	269	267	266	249
Students	0.0501	-0.0259	0.125	-0.00280	-0.164	0.142	0.370	-0.173	0.497
	(0.27)	(-0.19)	(0.65)	(-0.02)	(-0.96)	(0.81)	(1.93)	(-1.03)	(0.91)
N	392	388	392	391	392	392	390	389	366
Workers	0.0931	0.353	0.509	0.373	0.0641	0.0956	0.404	-0.217	0.593
	(0.29)	(1.50)	(1.51)	(1.25)	(0.24)	(0.33)	(1.15)	(-0.78)	(0.63)
N	135	136	135	134	135	136	136	135	124

t statistics in parentheses
* p<0.05 ** p<0.01

Table G 6: Assessments of attributes and salary regressed on a dummy variable for Ronny's in different subgroups (less qualified résumé)

	(1) Drive	(2) Intelligence	(3) Power of Initiative	(4) Social Competency	(5) Reliability	(6) Ability to Plan	(7) Creativity	(8) Common Sense	(9) Salary
Men	0.0211	0.0978	-0.0469	0.0147	0.278	-0.00676	0.0395	0.127	-0.0284
	(0.06)	(0.35)	(-0.14)	(0.06)	(0.91)	(-0.02)	(0.11)	(0.42)	(-0.03)
N	142	141	142	142	142	142	142	142	128
Women	0.386	0.235	0.648*	0.711**	0.266	0.181	0.709*	0.514*	1.680
	(1.31)	(1.03)	(2.17)	(2.75)	(1.12)	(0.75)	(2.34)	(2.21)	(1.53)
N	167	165	167	166	166	166	167	165	156
Income above Median	0.0410	0.0120	0.169	0.359	-0.160	-0.307	-0.228	0.611	-0.528
	(0.11)	(0.04)	(0.45)	(1.19)	(-0.51)	(-0.99)	(-0.58)	(1.79)	(-0.46)
N	106	105	106	106	105	106	106	106	99
Income below Median	0.400	0.339	0.485	0.408	0.546*	0.402	0.823**	0.282	1.285
	(1.34)	(1.46)	(1.65)	(1.60)	(2.12)	(1.52)	(2.78)	(1.18)	(1.26)
N	189	187	189	188	189	188	189	187	173
Income above Q2	1.311*	0.748	0.673	0.428	0.130	0.213	0.965	1.143*	-0.519
	(2.29)	(1.65)	(1.24)	(0.90)	(0.27)	(0.46)	(1.58)	(2.47)	(-0.21)
N	43	43	43	43	42	43	43	43	39
Business and Economics Degree	0.0534	-0.191	-0.425	0.226	0.605	0.401	0.202	-0.0554	1.575
	(0.11)	(-0.51)	(-0.88)	(0.63)	(1.38)	(0.90)	(0.38)	(-0.13)	(1.05)
N	63	63	63	63	63	63	63	63	60
High School Diploma	0.318	0.0909	0.290	0.871	0.687	-0.187	0.230	0.639	-1.985
	(0.60)	(0.25)	(0.52)	(1.63)	(1.58)	(-0.39)	(0.45)	(1.37)	(-1.59)
N	58	58	58	58	58	58	58	58	51
University Degree	0.348	0.219	0.517	0.352	0.195	0.0702	0.474	0.264	0.0559
	(1.09)	(0.86)	(1.59)	(1.35)	(0.70)	(0.25)	(1.41)	(1.01)	(0.05)
N	143	141	143	142	143	142	143	141	129
Age 18-20	0.442	-0.608	0.283	1.083	0.617	0.917	0.775	-0.0583	0.383
	(0.42)	(-0.82)	(0.28)	(1.58)	(0.93)	(1.14)	(0.85)	(-0.06)	(0.15)
N	23	23	23	23	23	23	23	23	23
Age 20-30	0.166	0.158	0.0587	0.0689	0.478	0.0217	0.697	0.218	1.190
	(0.39)	(0.47)	(0.15)	(0.20)	(1.33)	(0.06)	(1.89)	(0.68)	(1.14)
N	93	93	93	93	93	92	93	93	89
Age 30-50	0.107	0.582*	0.765	0.594	0.606	0.0575	0.242	0.401	0.180
	(0.27)	(2.02)	(1.85)	(1.82)	(1.85)	(0.18)	(0.59)	(1.25)	(0.18)
N	94	92	94	93	93	94	94	94	86
Age >50	0.200	-0.360	0.150	0.275	-0.0500	-0.275	0.0500	0.359	-0.232
	(0.50)	(-1.04)	(0.35)	(0.73)	(-0.13)	(-0.74)	(0.12)	(0.89)	(-0.16)
N	80	79	80	80	80	80	80	78	67

Well Educated Parents	0.493	-0.0221	0.0794	0.357	0.0765	0.119	0.415	-0.0804	0.918
	(1.16)	(-0.07)	(0.19)	(1.21)	(0.21)	(0.31)	(0.98)	(-0.23)	(0.63)
N	91	91	91	91	91	91	91	91	91
Less Educated Parents	0.190	0.283	0.489	0.425	0.375	0.133	0.508	0.604**	1.064
	(0.72)	(1.38)	(1.89)	(1.83)	(1.65)	(0.60)	(1.87)	(2.72)	(1.19)
N	213	210	213	212	212	212	213	212	191
Students	-0.130	-0.0486	-0.177	0.154	0.0317	0.179	0.183	0.0308	3.436*
	(-0.30)	(-0.13)	(-0.43)	(0.49)	(0.09)	(0.50)	(0.45)	(0.09)	(2.24)
N	94	93	94	94	94	94	94	94	91
Workers	0.490	0.301	0.587*	0.607**	0.410	0.126	0.548*	0.537*	0.0361
	(1.85)	(1.48)	(2.18)	(2.63)	(1.76)	(0.54)	(1.98)	(2.41)	(0.04)
N	208	206	208	207	207	207	208	207	189

t statistics in parentheses
* p<0.05 ** p<0.01

Table G 7: Attributes, Pooled Résumés with control variables

	(1) Drive	(2) Intelligence	(3) Power of Initiative	(4) Social Competency	(5) Reliability	(6) Ability to Plan	(7) Creativity	(8) Common Sense	(9) Salary
Men	0.0138	0.0328	0.187	-0.0237	-0.135	0.0728	0.230	0.126	-0.127
	(0.07)	(0.22)	(0.97)	(-0.15)	(-0.78)	(0.42)	(1.19)	(0.73)	(-0.23)
N	392	390	393	391	391	392	391	390	357
Women	0.177	0.149	0.302	0.407*	0.185	0.140	0.505**	-0.101	1.344*
	(1.00)	(1.11)	(1.62)	(2.39)	(1.19)	(0.86)	(2.61)	(-0.67)	(2.25)
N	447	443	446	445	447	447	447	444	420
Income above Median	0.128	0.0963	0.174	0.0971	-0.243	-0.0776	0.204	0.183	1.194
	(0.63)	(0.60)	(0.82)	(0.54)	(-1.33)	(-0.42)	(0.95)	(1.01)	(1.93)
N	323	321	322	323	322	323	322	323	302
Income below Median	0.130	0.139	0.333	0.281	0.221	0.264	0.516**	-0.0716	0.130
	(0.75)	(1.06)	(1.87)	(1.75)	(1.45)	(1.65)	(2.85)	(-0.47)	(0.23)
N	502	498	503	499	502	502	502	497	463
Income above Q2	0.717**	0.411	0.450	0.180	-0.292	-0.0301	0.728*	0.325	1.325
	(2.62)	(1.88)	(1.56)	(0.71)	(-1.16)	(-0.12)	(2.53)	(1.43)	(1.55)
N	159	159	159	159	158	159	158	159	151
Business and Economics Degree	0.117	-0.00840	-0.0136	0.0702	-0.181	0.165	0.160	-0.216	0.0897
	(0.57)	(-0.05)	(-0.06)	(0.36)	(-0.95)	(0.86)	(0.78)	(-1.21)	(0.16)
N	329	326	328	327	329	328	327	327	309
High School Diploma	-0.0736	0.00669	0.170	0.497	0.338	-0.254	0.195	0.343	-0.114
	(-0.15)	(0.02)	(0.35)	(1.12)	(0.91)	(-0.66)	(0.43)	(0.87)	(-0.08)
N	74	74	74	74	74	74	74	74	67
Univeristy	0.231	0.275	0.485*	0.481*	0.229	0.0178	0.444	0.0409	-

Degree									0.0296
	(0.97)	(1.49)	(1.97)	(2.36)	(1.11)	(0.08)	(1.74)	(0.21)	(-0.04)
N	253	252	253	252	253	253	254	251	228
Age 18-20	0.348	0.265	1.225*	0.507	0.0576	0.659	0.873	0.232	0.0701
	(0.62)	(0.72)	(2.09)	(1.10)	(0.14)	(1.39)	(1.79)	(0.50)	(0.05)
N	67	66	67	67	67	67	67	66	64
Age 20-30	0.0896	-0.0459	0.0146	0.0276	-0.0621	-0.00752	0.321	-0.208	0.622
	(0.53)	(-0.35)	(0.09)	(0.18)	(-0.40)	(-0.05)	(1.92)	(-1.40)	(1.29)
N	464	461	464	463	464	463	462	463	438
Age 30-50	0.0969	0.560**	0.547	0.501*	0.476	0.184	0.384	0.163	0.353
	(0.33)	(2.62)	(1.78)	(1.98)	(1.93)	(0.73)	(1.17)	(0.65)	(0.46)
N	162	161	162	161	161	163	163	161	150
Age >50	0.0504	-0.205	0.237	0.444	0.0360	-0.0944	0.210	0.415	0.802
	(0.15)	(-0.75)	(0.66)	(1.42)	(0.12)	(-0.32)	(0.59)	(1.28)	(0.62)
N	118	117	118	117	118	118	118	116	
Well Educated Parents	0.00972	-0.0808	-0.0137	0.172	-0.118	0.172	0.297	-0.188	0.786
	(0.05)	(-0.53)	(-0.06)	(0.96)	(-0.69)	(0.95)	(1.47)	(-1.09)	(1.30)
N	345	345	346	346	346	346	346	345	331
Less Educated Parents	0.240	0.259*	0.461**	0.258	0.153	0.0891	0.477**	0.187	0.569
	(1.44)	(1.98)	(2.68)	(1.62)	(0.97)	(0.56)	(2.60)	(1.22)	(1.00)
N	482	476	481	479	480	481	480	478	440
Students	0.0151	-0.0303	0.0664	0.0276	-0.126	0.149	0.334	-0.133	1.083*
	(0.09)	(-0.23)	(0.38)	(0.18)	(-0.83)	(0.95)	(1.92)	(-0.88)	(2.02)
N	486	481	486	485	486	486	484	483	457
Workers	0.334	0.322*	0.556**	0.515**	0.274	0.114	0.491*	0.239	0.257
	(1.63)	(2.09)	(2.65)	(2.82)	(1.56)	(0.63)	(2.26)	(1.37)	(0.40)
N	343	342	343	341	342	343	344	342	313

t statistics in parentheses
* p<0.05 ** p<0.01