



“The Effect of Formal Education on Implicit Social Evaluation of Immigrants”

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Forord

Denne oppgaven ble til delvis av generell interesse for forskjellen mellom det folk selvrappporter og det en kan måle med ulike priming og latenstidsbaserte metoder, delvis av egen opplevelse av at mennesker med høyere utdanning rimelig konsistent uttrykker seg “politisk korrekt” iforhold til en del typisk sensitive temaer i samfunnsdebatten.

Potensialet til implisitte holdningsmålinger for å unngå innflytelsen av slik sosial korrekthet fanget interessen min tidlig i studiet, og at instituttet hadde god veiledningskompetanse på et vanlig brukt instrument for måling av implisitte holdninger (IAT) gjorde valget lett.

Designet med å statistisk sammenligne definerte grupper på en tidligere undersøkt tydelig effekt ble ved siden av direkte interesse for de ulike utslagene på ulike grupper, også valgt fordi det uansett resultater ville si noe om stereotypiske oppfatninger om grupper med ulikt utdanningsnivå. En fristende hypotese var å undersøke direkte stereotypier mellom de ulike utdanningsnivåene, men dette ble forkastet pga. omfanget. At bygningsarbeidere høyst sannsynlig har mer fordommer mot innvandrere enn studenter og akademikere er noe “alle vet”, også forskningen sa det.

Med jevne mellomrom har ulike fagjournaler rapportert studier som har forsøkt å forklare effekten av utdanning på fordommer. Resultatene i denne oppgaven ble interessante i den betydning at de viste at effekten av utdanning slo ulikt ut for ulike målemetoder og en artikkel som rapporterer resultatene er sendt til *European Journal of Social Psychology* (referansenummer: EJSP-09-0153), parallellt med innleveringen av oppgaven til sensur.

Videre ønsker forfatteren å anerkjenne Brian Nosek ved Yale University og Virginia University for å ha delt datasettet som ligger til grunn for Studie 2. Og sist men ikke minst min uhyre dyktige, effektive og presise veileder Frank Siebler ved Universitet i Tromsø.

Sammendrag

En negativ korrelasjon mellom nivå av formell utdanning og fordomsfulle holdninger til innvandrere med annen etnisk opprinnelse, en utdanningseffekt, har blitt vist i en rekke rapporter. Hypotesen om at en slik korrelasjon er resultat av sosial ønskelige responser og *demand characteristics* har tidligere blitt testet ved hjelp av forskjellige metoder for å omgå sosial ønskelighet. Denne tesen er imidlertid underbygget av konseptet *implisitte holdninger*. To studier er rapportert: Studie 1 ($N = 34$) sidestiller holdninger hos en lavt utdannet gruppe av etnisk norske bygningsarbeidere med en høyere utdannet gruppe av etnisk norske studenter, over Bachelorgrads nivå. Begge gruppene blir målt med både eksplisitte og implisitte metoder (spørreskjema versus IAT [responslatens analyser]). Studie 1 replikerer utdanningseffekten i de eksplisitte målingene, men ikke i de implisitte ved et alfanivå på .05. Altså predikerer utdanningsnivå eksplisitte holdninger mot innvandrere, men ikke implisitte. Et Studie 2 ble også utført. Studie 2 undersøker et sekundært datasett bestående av et internett innsamlet metariale ($N = 11.134$). Studie 2 rapporterer grafisk, ved deskriptiv statistikk og effektstørrelser (R^2). Studie 2 støtter det statistiske fraværet av en utdanningseffekt i implisitte målinger. Altså antyder og diskuterer den foreliggende tesen at effekten av formell utdanning på etniske fordommer forsvinner når en måler med implisitte metoder.

Summary

A negative correlation between level of formal education and prejudiced ethnic attitudes, the *education effect*, has been reported in various research reports. Speculations has been made on the causes. The hypothesis that such correlation is due to social desirability or demand characteristics has previously been tested, facilitated by various methods of circumventing such issues. The present thesis is facilitated by the concept of implicit attitudes. Two studies are reported: Study 1 ($N = 34$) juxtaposes a low education group of ethnic Norwegian construction workers to a high education group of ethnic Norwegian graduate students, on measures of both explicit and implicit attitudes. Study 1 replicates the education effect in explicit measures, whereas not in implicit at an alphalevel of .05, i.e. education level predicts explicit attitudes towards immigrants, whereas not implicit attitudes. A Study 2 where conducted, by examining a secondary internet collected dataset ($N = 11.134$). Study 2 reports graphics, descriptive and effect sizes (R^2) that suggest supportive evidence for the statistical lack of education effect in implicit measures from Study 1. Hence, the present studies suggest that the effect of formal education on ethnic prejudice vanishes measured within the implicit attitude framework.

Keywords: Education, Implicit, Attitudes, IAT, Social-Psychology, Prejudice, Immigration.

Imagine that you are entering a campus cafeteria: the walls are painted in soft colours, various works of art are scattered around. The afternoon sunstrokes meet shiny tabletops at a sharp angle, they shimmer. Around a corner table sits someone from the sociology department. By another table, a guest lecturer in psychology sits. She smiles at her peers. She obviously relaxes by her self. Maybe admiring the sundown on the still snowy mountains. The psychology lecturer happens to be a black woman. One of the Norwegian sociologists, a man of Somali origins laughs out loudly. His tablemates giggles of the joke to.

Then imagine the eating barracks on large construction site. The guys have just come in for lunch; the clock closes in on 1200AM, but they have been working hard for five hours straight already. The concrete trucks are ordered in for 1300AM so the guys have not had time to grab a cup of coffee yet. In a long narrow room sits 23 men, all Caucasian. The room smells of sweat and concrete, cigarettes and burnt coffee. A man enters, he shuffles aside the leftovers from yesterday's lunch and puts loafs of bread directly onto the tabletop. He sits down heavily in the brown plastic chair, the tool belt drops to the floor beside him with a bang. On a separate table, the *foreign* guys sits. Probably discussing their minimum wage, as Norwegian, Swedish, Danish and German colleagues are paid by production rate adjusted payment, hence, usually receive double their pay check. In the corner of the *foreigner* table a young Romanian boy, his eyes fixed at the window frame. Last year the company hired a black guy, from temps-rental company. He was doing dirty jobs at the site, and stayed for only three weeks. The word came out that he was completely useless and, obviously, impossible to understand. In an average day you might hear seriously racist expressions, violence jokes, make believe death threats and surely grossly sexist remarks, around the lunch table; the guys like a rough tone says the manager and smiles.

Now ask yourself: in which of the two settings could one expect higher levels of prejudice? Intuitively, I suggest most people answer the latter. Empirically we will see that confirmed. One might list an array of factors, psychological, personal, and social or other contextual traits or characteristics, contrasting a group of white male construction workers to a multicultural, gender blended, group of academics.

However, this thesis will focus on *one* variable that has shown stable predictive properties for social evaluations towards immigrant targets: level of education.

The Education Effect

A robust negative correlation between level of formal education and level of prejudiced social evaluation towards ethnic, racial and immigrant out-groups has been shown by an array of research in Psychology, Sociology, Political Science and Educational Research (e.g. Hello, Scheepers, & Merove, 2002; M. R. Jackman, 1973; M. R. Jackman, 1978; Maykovich, 1975; Ostapczuk, Musch, & Moshagen, 2008; Photiadis, 1962; Rice & White, 1964; Tumin, Barton, & Burrus, 1958; Wagner & Zick, 1995).

Particularly, descriptive European and Norwegian statistics have reported the effect of formal education on attitudes toward immigrant populations (Blom, 2005, 2007; Directorate-General Information, 1988). Therein, among demographic variables like gender, age, urban vs. non-urban residency, immigrant contact-frequency and contact-quality, all yielded for education level as a distinctive variable predicting such attitudes (Blom, 2005, 2007; Directorate-General Information, 1988; Maykovich, 1975; Tumin, et al., 1958), i.e. an empirical rationale for counting education as a robust predictor of attitudes regarding race, ethnicity and immigrant topics, is apparent. Henceforth, the term *education effect* refers to the: *negative correlation between level of formal education and negative social evaluation of immigrants*. The education effect and its definition is an accommodation of various applications of the term (Hello, et al., 2002; Ostapczuk, et al., 2008; Photiadis, 1962; Wagner & Zick, 1995).

Attitude Assessment and the Education Effect

Assessment, by self-report measures, especially of socially sensitive topics, is prone to impression management issues, social desirable responses or demand characteristics (Crowne & Marlow, 1960; Edwards, 1957; Orne, 1962; Ostapczuk, et al., 2008; Pettigrew &

Meertens, 1995; Wagner & Zick, 1995). Presumably, such susceptibility is present for people of all education levels. Regarding demand characteristics, the intuitive notion that higher level of education provides greater ability to understand situations, take different perspectives and positions in general, impression management issues and, prompts the notion that demand characteristics would represent a greater threat to self report response “cleanness” from higher educated people than from lower educated.

Lets revisit the guy from the construction site, heavily armed with hammer and nails, being presented to a questionnaire set. The questionnaire is 10 pages, is counterbalanced in terms of pro and contra items of attitudes toward immigrants, it is compiled of items of affective response modality and cognitive modality and is formulated in an unfamiliar sociolect, superficially casual, but still awkwardly formal. The questionnaire items vary in length and type during the completion of the compilation. Usually, his read and write effort during a week limits itself to reading warning labels of equipment and writing timesheets. In terms of cognitive *abilities* there is nothing obviously distinguishing the construction worker from e.g. an academic. Possibly, the former do more calculations during a day, and take more important decisions in terms of economics and safety during a week, than the average PhD student do in a year or more. Nevertheless, the questionnaire situation is less known, and moreover, the *type* of read, comprehend and write effort is less familiar for him, than it is for the student or academic that spend her days, by own choice, reading, writing and, to various extent, attempting to comprehend concepts and ideas presented to her. Intuitively, I would expect the student or academics susceptibility to demand characteristics as greater than for the worker. Nonetheless, of possible confounding properties and the various issues proposed above, assessments of socially sensitive topics are frequently conducted by pen and paper questionnaires, more or less structured telephone interviews and other self-report methods.

Thus, one apparent question is whether the education effect is a measure of social desirability and demand characteristics, or a true effect from formal education.

Demand Characteristics

Demand characteristics are a concept of reactivity issues in experimental or other research contexts, in addition to clinical contexts. Demand characteristics is defined as features of an experimental situation that encourages certain types of behaviour from the participant that contaminates the results, especially when this behaviour arises from the participants expectations, preconceptions or interpretation of the experimenters expectation (Colman, 2006).

However, in the realm of the present thesis its worth to notice that it do not define demand characteristics as being directed by social desirability in the sense of the broader scope of the topic of attitudes towards immigrations, but merely as a possibly confounding issue of the research context, i.e. demand characteristics is something that develops during the research context, prompting the participant to respond to subjectively acknowledged *demands* in the specific situation, not as *demands* of subjectively acknowledged socially desired, or as societal trends in regards to attitudes towards immigrants.

The special cases

Specifically, the hypothesis that the education effect is a measure of susceptibility to demand characteristic, or social desirability issues is previously tested. Two studies applying various means for circumventing the social desirability issues, have both replicated the education effect (Ostapczuk, et al., 2008; Wagner & Zick, 1995) and hence, concluded that it is *not* an artefact of such issues.

The present thesis reports from these two studies. The rationale for this is partly to illustrate previous efforts to assess the education effect, partly to illustrate the theoretical different approach taken in the present thesis by submitting the question to tests performed on implicit responses, investigating the possibility that an education effect will replicate in explicit measures of attitudes, whereas not in implicit. As we will see that is the pattern that emerges.

Background

Circumventing Social Desirability

Generally people tend to control their responses and expressions such as to impress on others a wanted image of them selves. Such impression management strategies (Goffman, 1959) may be underpinned by several tactical elements. In the realm of psychometric assessment the notion of demand characteristics (Orne, 1962) has been a prominent contribution to the field. Social desirable responses, or demand characteristics, should be expected in self-report surveys when assessing social sensitive topics (Crowne & Marlow, 1960; Edwards, 1957; A. G. Greenwald, Poehlman, Uehmann, & Banaji, 2008; Orne, 1962). Social evaluation of immigrants is a socially sensitive topic (Directorate-General Information, 1988; A. G. Greenwald, et al., 2008; Ostapczuk, et al., 2008; Pettigrew & Meertens, 1995; Wagner & Zick, 1995). The following paragraphs will give a brief description of some methods for bypassing social desirability. Including some special cases applied to the proposition: that the education effect is an artefact of social desirability.

Subsequent paragraphs will then define more thoroughly the concept of implicit attitudes and measurement of to underpin the choice of the IAT as a preferred tool of data

collection circumventing social desirability issues (Cunningham, Preacher, & Banaji, 2001; A. G. Greenwald, et al., 2008; Hofmann, Gawronski, Gschwender, Lee, & Schmitt, 2005; Nosek, Greenwald, & Banaji, 2005) for testing the present hypotheses.

Social Desirability Scales

One strategy for correcting social desirable biased questionnaire responses, is the Marlowe-Crowne social desirability scale (SDS) (Crowne & Marlow, 1960). Alternatively, one of its multiple versions and derivatives (H. J. Greenwald & Satow, 1970; Loo & Loewen, 2004) may be applied.

The SDS is a questionnaire measured scale that rates non-pathological participants tendency to answer in a social desirably way (Crowne & Marlow, 1960). Its rationale is to adjust for individual social desirability pattern in the responses to the primary topic assessed. However, the full version SDS, and most derivatives, have low reliability and poor psychometric properties (Loo & Loewen, 2004). One version, nonetheless, showed acceptable psychometric properties (Ballard, Crino, & Rubenfeld, 1988; Loo & Loewen, 2004) for general purposes. For further discussion see *The Rationale of the Present Study*, paragraph below.

Special case 1- Subtle and Blatant Prejudice Scales

Using *Subtle and blatant prejudice* subscales of the Eurobarometer 30 (Directorate-General Information, 1988) relevant to immigration, Wagner and Zick (1995) tested the hypothesis that the education effect do not reflect true differences in prejudice, rather stronger response tendency towards socially desirable answers (Wagner & Zick, 1995).

The *subtle and blatant prejudice* concepts (Pettigrew & Meertens, 1995), aims at revealing prejudicial attitudes from subjects that openly express compliance to the socially desirable, presumably non-prejudice, cultural paradigm of the western world (Pettigrew & Meertens, 1995). Subtle prejudice is described as *cold, distant and indirect*, whereas blatant prejudice is described as *hot, close and direct* (Pettigrew & Meertens, 1995). Thus, subtle prejudice concept describes a “hidden” kind of prejudice. A definition similar to *latent prejudice* (Bergman & Erb, 1986) and, moreover, it is related to the *modern racism* concept (McConahay, 1983; Pettigrew, 1989), whereas blatant prejudice is the concept of prejudice one usually refers to in everyday language (Pettigrew & Meertens, 1995). Taking the more subtle and less open aspects into account may thus, in principle, remove differences in the degree of expressed prejudice between education levels. Empirically, however, even with the Subtle prejudice items of the Eurobarometer 30 (Directorate-General Information, 1988) education effect was replicated (Wagner & Zick, 1995).

Special Case 2- Bogus Pipeline Procedure

Moreover, Wagner & Zick (1995) intended to bypass social desirability by applying a Bogus Pipeline Procedure (Jones & Sigall, 1971). The principle of bogus pipeline is an experimental facilitation of deceit. Particularly, participants were: *connected via electrodes to an impressive electromyograph and associated computer equipment* (Wagner & Zick, 1995). The rationale of the bogus pipeline procedure is to deceive the participant to believe that the machine is a sophisticated “lie-detector” which can measure their true attitudes. Hence, the Bogus Pipeline Procedure, aims at getting participant to answer more truthfully, and less biased by social desirability, as they believe the experimenter will access their real attitude anyway.

Wagner and Zick's studies (1995) replicated the education effect in all conditions. Consequently, they concluded that the effect is *not* an artefact of demand characteristics (Orne, 1962; Wagner & Zick, 1995). However, their study accounted for some of it by controlling for other social psychological variables such as self perceived and reported social strata (Wagner & Zick, 1995). The latter could, nevertheless, have an expected correlation with, and supposedly be causally subsequent of, education level itself.

Special Case 3- Randomized Response Technique

Ostapczuk, Musch and Moshagen (2008) tested the hypothesis that highly educated populations are not truly less xenophobic, but simply more prone to give socially desirable, xenophile answers (Ostapczuk, et al., 2008) i.e. congruent to the present studies, and Wagner and Zick (1995). Ostapczuk et.al. (2008), deployed a *Randomized Response Technique* (RRT) (Rittenhouse, 1996), for circumvention of the social desirability issue.

Conversely, to the bogus pipeline procedure, RRT elaborately reassures the participant of complete informed and actual, personal anonymity. Information of the randomization algorithm expose the blinding of the experimenter, regarding whether participant answers truthfully or just according to experimental prompts in their responses. Paradoxically, the same probability structure of the method that guarantee no true subject-responses connection, actually makes the statistical derivation of true answer numbers possible (Ostapczuk, et al., 2008; Rittenhouse, 1996). They measured responses to a Bogardus Social Distance Scale (Bogardus, 1933) and collected from low/high educated groups. The conditions contrasted, were the RRT (Rittenhouse, 1996) condition modified by an sophisticated cheat detection extension of the method (Ostapczuk, et al., 2008), and a direct questionnaire condition.

However, despite application of sophisticated methods, Ostapczuk et.al. (2008), replicated the education effect in all conditions. Hence, they concluded that a true education effect exists (Ostapczuk, et al., 2008).

Attitudes

Attitudes were early noted as the most indispensable concept of Social Psychology (Allport, 1935). Attitudes are frequently defined as *an enduring pattern of evaluative responses toward a person, object or issue* (Colman, 2006). Moreover, such responses can be divided into an affective, a cognitive and a behavioural component (Martin, Carlson, & Buskist, 2007), i.e. an attitudinal response is suggested to be somewhat predictive of behaviour, not only an informational checkmark in a questionnaire. However, the three response modalities are not easily separable (Bohner & Wänke, 2002), and are presented in the literature in an array of different definitions, conceptualisations and working theories.

Thus, for the present purpose, wherein we do not investigate the inner structure of attitudes, neither the attitude-behaviour consistency, but rather a special case of group differences in attitudes, towards specified targets, a simplified pinpointed definition is appropriate.

Hence, herein the definition: *an attitude is a psychological tendency that is expressed by evaluating a particular entity with some degree of favour or disfavour* (Eagly & Chaiken, 1993).

The distinction of explicit and implicit attitudes

Suppose you are white, you work at the desk of a lurid and vulgar tabloid news paper, persistently reporting foreign ethnic origins of violent offenders or other criminals.

Nonetheless, as a college educated journalist you consider yourself as a tolerant, reflected and enlightened person. The last thing you expect from yourself, is prejudiced attitudes toward immigrants. If someone asks you directly you would also deny it promptly.

Now, suppose you are white. You still work at the newspaper. You live in a big city and sit on a subway on your way home from a late night cinema with a friend. You pull your shiny new Blackberry cellular from your pocket preparing a phone call to your girlfriend. The subway arrives at a station and just before departing again a group of boys enters the otherwise empty train. The boys are black, probably of Somali origin. You cancel the phone call, and discretely puts the phone back into the pocket whilst pressing the silent profile button. Crudely the former example represents your explicit *attitude you are both willing and able to report*, the latter an implicit *attitude you are either unable or unwilling to report*.

Thus, two subjective outcomes in the latter case is obvious. *Firstly*, you could continue your way home feeling ashamed of the notion that you, the non-prejudiced journalist reacted like that, contemplating on whether you would react the same way if they were white, however still explicitly presenting and perceiving yourself as a tolerant and enlightened person . *Secondly*, you might not even have noticed.

In recent years, researchers have emphasized the need to distinguish two kinds of cognition, and also two kinds of attitudes. Explicit attitudes are attitudes that people are aware of, and that they can communicate in self-report measures such as questionnaires. In contrast, implicit attitudes are defined as: *the introspectively unidentified (or inaccurately identified) traces of past experience that mediate favourable or unfavourable feeling, thought or action toward social objects* (A. G. Greenwald & Banaji, 1995). i.e. the definition underpinning the present thesis, as it proposes the possibility of subjectively unknown evaluative tendencies, attitudes.

A.G. Greenwald and Banaji's (1995) initial definition refers to implicit attitudes as attitudes that we are unaware of. More recently, the Implicit Association Test (IAT) was developed as a research instrument that measures attitudes people *cannot* or *do not want* to report (A. G. Greenwald, McGhee, & Schwartz, 1998). The notion of not being aware of, i.e. cannot report an attitude explicitly, versus the possibility of measuring attitudes that people are aware of having, but reluctant to report explicitly is obviously fundamentally different regarding the social desirability issue. In the former situation the issue of social desirability is far fetched as social desirability presupposes subjective awareness of the real attitude, whereas the latter do not. The distinction of unawareness and unwillingness is of course theoretically very important. For the purpose of the present studies, however, I do not yet take that distinction into account. Instead, I use the IAT as a research instrument that allows the measurement of attitudes that people would normally *not* report. Whether this is because of unwillingness or unawareness of the attitude, should be clarified in future research. For further explanations on this issue see the *Rationale for the Present Studies* below.

Measuring Implicit Attitudes

The experimental use of methods assessing implicit attitudes, through analyzing latency time differences in categorization of social stimuli has been widespread the last decade. A surge of applications of such methods, mostly the IAT, have been seen (A. G. Greenwald, Nosek, & Banaji, 2003; A. G. Greenwald, et al., 2008; Hofmann, et al., 2005). From the introduction of the standard IAT (A. G. Greenwald, et al., 1998), and its variations, to the Sorting Paired features (Bar-Anan, Nosek, & Michelangelo, 2009) the measurement of automatic social evaluations through computer based assessment of response latencies has

established itself as a methodological paradigm in social psychological research. The present studies are facilitated by an standard IAT (A. G. Greenwald, et al., 1998).

Implicit Association Test

An Implicit Association Test (IAT) measures the strength of associations between concepts, by observing response latencies in a computer administered categorization task (A. G. Greenwald, et al., 2008). The aim of an IAT is to measure relative preferences, that is, differences in evaluations. In the present Study 1 differences in the evaluation of the target concepts Norwegians and non-Norwegians was assessed, whereas in Study 2 used black persons versus white persons as target concepts.

E.g. the present IAT in Study 1 measures relatively slower response latencies to pleasant words paired with non-Norwegian names compared to unpleasant words paired with non-Norwegian names. Vice versa, i.e. faster, for pleasant words paired with Norwegian names compared to unpleasant paired with Norwegian names.

In reduced terms, an IAT is a chronometrically measured sorting task. It typically consists of five main steps (A. G. Greenwald, et al., 1998). Each of these steps is a sorting task with different instructions, e.g. 1. Non-Norwegian names to the left, Norwegian to the right, 2. Unpleasant words to the left, pleasant to the right, 3. Norwegian names to the left non-Norwegian to the right, 4. Pairs of non-Norwegian names and unpleasant words to the left versus Norwegian and pleasant pairs to the right, 5. Norwegian names paired with unpleasant words to the left versus pleasant and non-Norwegian pairs to the right. Step 4 pairs are referred to as the compatible pairs, whereas step 5 as incompatible, i.e. compatible to the notion of the typical stereotype.

The present studies uses a structure with seven blocks as step three and five is repeated in one practice block and one test block. The tasks of each stimulus block are to sort screen-presented stimuli using the assigned left and right keyboard buttons, adhering to guideline labels at the left and top of the screen. The sorting tasks measures sorting performance in millisecond latencies. Latency differences for stereotype compatible vs. incompatible combinations of target/attribute stimuli is the raw-scores of the implicit effect size coefficient D . Coefficient D is in text referred to as the *IAT effect*.

Particularly, in the present study 1, sorting time latencies is generally lesser for stereotype *compatible* pairs like Amir and Bomb, compared with *incompatible* combinations, as the traditional Norwegian name Jarle paired with Bomb. For an elaborate description of stimulus material and computation of the IAT-effect, see the Implicit Measures, *Contents and Administration* paragraph in the Study 1 section below.

Rationale of the Present Studies

To sum up, the education effect has proven stable in several studies that tested whether it is perhaps “only” an effect of a lesser tendency in lowly educated people to express their prejudice. Note, however, that all of these studies have used *self-report measures*. *Firstly*, as the Marlowe-Crowne type scales, measure tendency of faking “good” (Crowne & Marlow, 1960; Meehl & Hathaway, 1946), it relies on self-reported, explicit attitudes. *Secondly*, the concept of subtle prejudice (Pettigrew & Meertens, 1995) is merely a facet of explicit attitudes, hence, relying on self-report. *Thirdly*, the application of a bogus pipeline procedure assumes a subjectively known true attitude. If socially undesirable participants would under other circumstances *fake good* (Edwards, 1957), whereas during the bogus pipeline procedure participants are compelled to answer more truthfully (Jones &

Sigall, 1971). *Finally*, the RRT produce truthful responses from otherwise socially desirable ones by absolute assurance of anonymity.

Different from these previously applied methods the IAT does *not* rely on self-reported attitudes, but uses participants' response times instead. Hence, testing the education effect with an indirect measure such as the IAT will represent a different methodological approach (Cunningham, et al., 2001; A. G. Greenwald, et al., 2008; Hofmann, et al., 2005). Furthermore, using the IAT will also open a new and different *theoretical* perspective on the education effect, namely by tapping into social evaluation tendencies participants might not be aware of having (Graf & Schacter, 1985; A. G. Greenwald & Banaji, 1995; A. G. Greenwald, et al., 1998). The potential of the implicit attitude concept for circumventing the social desirability issues is apparent, and have previously been noticed (Cunningham, et al., 2001; Hofmann, et al., 2005; Nosek, et al., 2005). Hence, one goal of the present studies is to circumvent social desirability issues, regarding the education effect by assessing group differences in two methodologically different attitude measurement tools. Moreover, a rationale of the present studies use of implicit measurement methods is derived from the theory and definitions underpinning the implicit attitude concept, i.e. the proposal that implicit constructs might be outside subjective awareness (Graf & Schacter, 1985; A. G. Greenwald & Banaji, 1995; A. G. Greenwald, et al., 1998). Subjective unawareness of mental constructs is an established concept in social psychology (Aronson, Wilson, & Akert, 2005; Bohnet & Wänke, 2002; Graf & Schacter, 1985; Nisbett & Wilson, 1977).

Revisiting Issues of Concept Dissociation

Both at the methodological and theoretical level, the literature has raised several questions concerning the appropriate interpretation of attitude scores, whether acquired by

the researcher from participants' self-report or otherwise. These questions are particularly important if more than one attitude can be collected from the same participant. For example, interpretative ambiguity concerns whether socially desirable answers to certain topics should be considered as a) faking good (Crowne & Marlow, 1960; Edwards, 1957), b) more or less rational and healthy degrees of susceptibility to demand characteristics (Orne, 1962), c) an expression of cultural, political or societal variations (Arkes & Tetlock, 2004; Hello, et al., 2002; McConahay, 1983), d) different expressions of the same underlying concept (Bergman & Erb, 1986; Pettigrew & Meertens, 1995), e) possibly dissociated dual attitude modalities (Pratkanis, Breckler, & Greenwald, 1989; Wilson, Lindsey, & Schooler, 2000), f) predictors of different kinds of behavior (Asendorpf, Banse, & Mücke, 2002), or g) psychometric or philosophical artefact (Arkes & Tetlock, 2004; Brendl, Messner, & Markman, 2001). These theoretically important questions will not be answered in the present thesis. Instead, the present thesis neutrally assesses the the absence or presence of an education effect in two samples. The method is to apply two different methods of measurement, one relying on self-report, and the other relying on response latency. The major goal is to find out whether results are the same, or are different between the two assessment methods. In other words, I will try to replicate the well-established education effect both with traditional (self-report questionnaire) as well as new (IAT) techniques, to gain insight into the stability of the education effect across different measurement techniques.

Method

The present Study 1 assesses two groups regarding the education effect, from locally collected primary data ($N=33$). One group with less than 13 years formal education, another of more than 16 years formal education. *Firstly*, by a standard questionnaire compilation identical to Statistics Norway's official survey reports (Blom, 2005, 2007). *Secondly*, by an Implicit Association Test (IAT) (A. G. Greenwald, et al., 1998).

Study 2, aims at confirming the non predictive properties of education level regarding implicit attitudes suggested in Study 1. Study 2 is based on a dataset ($N=11.134$) kindly shared by Brian Nosek at Virginia University (Nosek, 2009). The data is extracted from a Race-IAT administered on the Project Implicit web site between Jan 20, 2009 and Jan 29, 2009 (IAT-Corp, 2008).

Study 1 reports primary data. Separately for explicitly and implicitly measured attitude scores, difference from neutral preference are t-tested. Moreover, two general linear models (GLM) are produced separately for the selected explicit measure versus the IAT effect. Finally, a repeated measures analysis of variance is conducted where both attitude scores (explicit, and implicit) were used as dependent variables simultaneously.

With this setup, I expected to replicate the education effect. That is, to find significantly more positive attitudes towards Non-Norwegians in academics than in workers. The question of interest for my research was whether a successful replication of the education effect would occur for *both* assessment methods (explicit, and implicit), or for only *one of them* (namely the explicit assessment of attitudes via self-report).

Study 2, reports secondary data from a race-IAT (IAT-Corp, 2008; Nosek, 2009). Due to the large sample size ($N = 11.134$) inferential analysis produce statistical significance even from tiny effect sizes. Thus, graphics and effect sizes (R^2) are reported to explain the data: a)

from a full sample, elementary school through doctoral level education, and b) a dichotomized subsample closely mimicking the groups in Study 1.

Study 1

Participants

One low-educated group ($N = 14$) henceforth: Workers, were partly convenience sampled ($N = 10$), partly snowballed ($N = 4$), from all male ethnic Norwegian construction workers. The Workers sample had a mean age of 27.79 years (range: 20, 42 [$SD = 6.23$]). Moreover, a high-educated group ($N = 19$) henceforth: Academics, with a mean age of 27.95 years (range: 23, 36 [$SD = 3.73$]). Academics were convenience sampled from graduate level psychology students ($N = 10$), and graduate level law students ($N = 9$), distributed as 7 female and 3 male, and 4 female and 5 male respectively. The completion rate was 33/33. Hence, none were initially excluded from overall performance or completion criteria. Overall mean age for both groups ($N = 35$) were 27.88 years (range: 20, 42; [$SD = 4.86$]). Moreover, the workers all reported below 13 years formal education, whereas the academics reported more than 16 years. See Table 1 for descriptives.

Table 1. Age and Gender Descriptives

Group	Gender	N	Min	Max	Mean	SD
Workers	Male	Age 14	20.00	42.00	27.79	6.23
Academics	Male	Age 8	23.00	36.00	28.13	4.67
	Female	Age 11	24.00	35.00	27.83	3.12

Note. Workers reported below 13 years formal education.
Academics reported beyond 16 years of formal education

Materials, Administration and Procedure

A computer based questionnaire was compiled from Norwegian Statistics official surveys on attitude towards immigrants (Blom, 2005, 2007). The questionnaire was combined seamlessly in a package with a seven block standard IAT (A. G. Greenwald, et al., 1998). The stimulus content of the IAT was compiled and pre-tested for the purpose of the study. Henceforth, the questionnaire part will be referred to as the *explicit measure*, whereas the IAT as the *implicit measure*, whereas the combination as the *package*. The package was administered to one *low education* group, all ethnic Norwegians, and one *high education* group, all ethnic Norwegians.

A mini-laptop (Asus Exec 901; screen size: 8.9 inch) with a Windows XP OS (Support, 2008), was used for the administration. The software package integrated both explicit and implicit measures. Nonetheless, two entry choices, *implicit first* or *explicit first*, were available, facilitating the counterbalancing of implicit/explicit order. Significant order effects have been seen in some reports (Hofmann, et al., 2005). Whereas others have reported non-significant order effects (Nosek, et al., 2005). The latter's recommendation, is nevertheless, to consider counterbalancing until consistent evidence is accumulated. Thus, measurement order were decided at random, by the experimenter, priority of administration.

Due to human error the ratio did not reach exactly 50/50. After counterbalancing implicit/explicit ratio was 9/5 for the workers and 11/8 for the academics.

Furthermore, the graphic interface was designed as a clean and simple black letters on light-grey background. Apart from the sorting tasks, participants had to manually shift to the next IAT block by mouse clicking ok in a pop-up. Conveyance to the next main part, explicit or implicit respectively, demanded mouse click to a rectangular button at the bottom part of the screen. The workers, were tested on site, in a semi-shielded, but familiar environment.

The academics were tested on University of Tromsø campus, in semi-shielded, but familiar environment. Particular administration and procedure for the respective measures follows below.

Implicit Association Test

Consistent with empirical findings regarding number of target and attribute stimuli suggestedly statistically sufficient in an IAT (Hofmann, et al., 2005; Nosek, et al., 2005), eight names, four non-Norwegian (Amir, Jamal, Mahdi, Rafi), and four typically Norwegian names (Arnt, Jarle, Margot, Rolf), were selected as target stimuli. Likewise, eight valence words, four positive (Blomst, Frihet, Rolig, Supert) and four negative words (Bombe, Fattig, Robbe, Sykdom) was selected as attribute stimuli. The selection of stimulus words and names was made on the basis of pretest results. The implicit measure part of the computer program was a standard IAT as described above (A. G. Greenwald, et al., 1998).

Pre-test

18 feminine and masculine Arabic names (Al-Sudairy, 1999, 1999-2008), and 18 Norwegian names for target stimuli was pretested. Attribute stimuli candidates were 36 words, a mix of translated items from Greenwald & Banaji (1998), and supposedly distinctively unpleasant and pleasant words listed for the purpose. Both the first names and the attribute stimuli, were tested on a *non-Norwegianness-Norwegianness* dimension as well as on the *unpleasantness-pleasantness* dimension. A convenience sample ($N = 18$) from undergraduate and graduate psychology students, was used to score the target and attribute dimensions of both names and words.

Particularly, each first name and valence word had to be rated on two semantic differentials by the participants in a paper-and-pencil pre-test. The differentials ranged from -3 to 3, with scale midpoint in zero. The differentials were respectively anchored in unpleasant versus pleasant for attribute scoring, and non-Norwegian versus Norwegian for target scoring. The differentials were stacked vertically, immediately following under each item, consistently with the attribute differential on top. The administration were counterbalanced, i.e. turning the semantic differential anchoring direction for half of the sample (see Appendix A for the complete pre-test questionnaire).

The four non-Norwegian target names were initially selected by two criteria. *Firstly*, not statistically significant ($p < .05$) deviation from zero on the attribute dimension (see Table B2). *Secondly*, significantly differing from zero ($p > .05$) on the target differential (Table B4), latter criteria in the non-Norwegian direction (Table B3). In other words, to be eligible for inclusion as a non-Norwegian name in the main study, a first name had to be significantly non-Norwegian, but had to be rated neutrally on the attribute dimension. The first criteria were applied similarly, to select Norwegian names, whereas the second were applied in the opposite direction. Following a converse rationale, eight attribute words were chosen, i.e. target words should differ significantly from zero on the semantic differential regarding unpleasantness/pleasantness, but, at the same time, not on the Norwegian/Non-Norwegian semantic differential. This rationale was adopted to choose eight valence words, four in the unpleasant direction, four in the pleasant (see tables B5 through B8).

Thirdly, all stimuli material were submitted to three additional super criteria. 1. Pairwisely equal initial letter 2. Pairwisely equal number of syllables. 3. Pairwisely equal number of syllables. The latter criteria were applied to avoid confounding effects of perceptual artefacts.

However, the tests of attribute stimuli all was significantly deviating from zero also on the target dimension (Table B8). This anomaly was consistently in the same direction, i.e. all valence words were scored as more typically Norwegian. The latter is most likely an artefact of the fact that all words in fact was expressed in Norwegian language. Hence, a misunderstanding of the semantic purpose of the scoring task might have occurred.

Therefore, a selection were conducted by ranking t-values for picking out four of each initially, presumably valence directed, words that at the same time adhered to the super criterias above, and subsequently submitted to a pairwise t-test. The pairwise t-tests confirmed the preliminary selection to be statistically different on the attribute dimension, pleasant/unpleasant ($p < .001$) (see Table B10), whereas not on the target dimension, Norwegian/Non-Norwegian ($p = .08$) (Table B12). Hence, they were finally accepted on the rationale that the standard IAT aim at measuring relative association differences.

Administration

In line with the usual IAT procedure (Greenwald et al., 1998), the present Implicit Association Test used several blocks of trials where participants responded to single words that appeared on the computer screen, by pressing one of two keys on the computer keyboard. Which key to press for what kind of word, was indicated by category labels at the top of the computer screen.

In an initial target-concept discrimination block, participants distinguished stimuli from the target dimension, by sorting non-Norwegian names to the left, but Norwegian names to the right, using the respective keyboard buttons D and K. In a second attribute discrimination block, they distinguished valence words by sorting unpleasant words to the

left, and pleasant words to the right. Both of these blocks served exclusively to familiarize participants with the IAT task; these data will not be analyzed.

The first blocks of interest are the third and fourth blocks. Here, for the first time, stimuli from both categories were shown, and participants had to take both dimensions (valence, and target dimension) into account simultaneously. Specifically, participants were asked to sort a stimulus to the left if it was a non-Norwegian name or an unpleasant word, but to sort a stimulus to the right if it was a Norwegian name or a pleasant word. These two blocks are commonly called the *stereotype-compatible* blocks, because the mapping of response keys to stimulus dimensions coincides with cultural (here: pro-Norwegian) stereotypes.

The fifth block merely served to practice a reversal of the assignment of response keys to stimulus categories. Specifically, whereas participants had previously always sorted Norwegian names to the right and Non-Norwegian names to the left, they now sorted Norwegian names to the left and non-Norwegian names to the right.

The sixth and seventh block are then again of interest. Here, participants were asked to sort a stimulus to the left if it was a Norwegian name or an unpleasant word, but to sort a stimulus to the right if it was a Non-Norwegian name or a pleasant word. These two blocks are commonly called the *stereotype-incompatible* blocks, because the mapping of response keys to stimulus dimensions is exactly counter to cultural stereotypes.

The next paragraph elaborates in detail how the implicit measures results were cleaned and reduced. However, a general overview of how to interpret the resulting scores shall be given here. Generally speaking, a participant's implicit attitude is computed from the *difference* in average response speed between blocks six and seven on the one hand, and blocks three and four on the other (stereotype-incompatible minus stereotype-compatible). If

a participant has no implicit association between “Norwegian” and “pleasant”, or between “non-Norwegian” and “unpleasant,” then he or she should respond with approximately the same speed in all four blocks, and the resulting difference score would be close to zero. This would indicate a neutral implicit attitude. However, some participants may find it easier to respond when “Norwegian” shares a response key with “pleasant” (blocks three and four) rather than “unpleasant” (blocks six and seven). If so, then those participants could respond faster in the stereotype-compatible blocks than in the stereotype-incompatible blocks. The difference score would then become greater than zero, indicating a more positive implicit attitude towards Norwegians than towards Non-Norwegians. And finally, other participants may find easier to respond when “Non-Norwegian” shares a response key with “pleasant” (blocks six and seven) rather than “unpleasant” (blocks three and four). In that case, the difference score would become smaller than zero, indicating a more negative implicit attitude towards Norwegians than towards Non-Norwegians.

Data reduction and transformation-IAT

Consistent with Greenwald et.al (2003) improved scoring algorithm the following reduction steps are performed: 1. Both practice blocks (blocks three and six) and test blocks (blocks four and seven) are included in the derivation of an overall score. 2. Slow *trials* (> 10000 ms latencies) were eliminated. 3. Fast *cases* (> 10% fast trials [< 300 ms latencies]) were excluded. 4. Blockwise means were computed from subsequently remaining latencies. 5. Standard deviations were computed for both practise blocks (compatible and non-compatible), and for both test blocks. 6. Latencies from trials with wrong responses were replaced with the mean from the respective block, plus a constant “penalty” of 600ms

The overall IAT score is reported as the coefficient D (A. G. Greenwald, et al., 2003). D is computed from the above described (step 4) means differences from non-compatible – compatible pairs latencies, divided by the pooled standard deviation of the two associated blocks, i.e. from the practice blocks and the test blocks respectively. The interpretation of these D scores is the same as described above: scores close to zero indicate a neutral implicit attitude, scores above zero indicate a pro-Norwegian implicit attitude, and scores below zero indicate a pro-Non-Norwegian implicit attitude.

Explicit Measures

The contents of the explicit measures were identical to a section in official Norwegian Statistics reports, regarding attitudes towards immigrants by education level (Blom, 2005, 2007) (see Appendix C for the full material).

Consisting of six statements, of the type: *most immigrants are a source of unsafety in the society?* (translated from Norwegian, italics added). All statements prompted mouse click ratings on a vertical five point semantic differential. Furthermore, three Bogardus type items (Bogardus, 1933) e.g. *would you feel it awkward if you had a son or daughter who wanted to marry an immigrant?* (translated from Norwegian, italics added), prompted answers *yes* or *no* represented by mouse clicks in vertically stacked tick boxes. Moreover, one political question and three contact related questions were included (see Table C for the full materials).

Selection rationale, data reduction and transformation.

Affective measures, e.g. Bogardus items, are usually reported to have higher correlations with IAT effects compared to more cognitive measures as the present statements. Theoretically this is due to more similar modalities, as implicit measures is usually viewed as

measuring the affective dimension of attitudes (Hofmann, et al., 2005), i.e. thought to tap into a *less* controllable dimension of attitudes.

Hence, Bogardus items would represent a conservative approach to comparing the differences in education effect across implicit-explicit measures. Nonetheless, statement measures were chosen as the critical *explicit* attitude measures in the analyses below, from the following rationale.

Internal scale reliability of the statements suggest good ($\alpha = .82$) reliability properties (Cronbach, 1951), across all six items, whereas the Bogardus items yielded low ($\alpha = .57$) reliability (see Table B13). Furthermore, the “single items deleted” Cronbach Alpha ranges for Statements were both higher and narrower ($\alpha = .77$, $\alpha = .83$) (Table B14) compared to Bogardus items ($\alpha = .20$, $\alpha = .57$) (Table B15). Thus, the self-report scores based on statements were the more reliable measure of explicit attitudes. Hence, statements were chosen as explicit measure for the further analysis and thus statements 1-6 were collapsed into a common variable. The summary score ranged from 1, ‘very unfavourable’, to 7, ‘very favourable’.

Correlations: neither statements, nor Bogardus items correlate significantly with IAT effect ($\alpha = .05$). Nonetheless the intercorrelation ($r = .73$) between explicit measures were clearly significant ($p < .001$), i.e. both measures are useful for contrasting to the implicit measures.

Moreover, variables related to the quantity (How *many* and How *often*) and quality (What is your *personal experience*) of contact with Non-Norwegians were part of the test questionnaire. These variables were included into the study to apply the full immigrant section of the Norwegian statistics questionnaire compilation referred to in the introduction of this thesis. Nevertheless, these contact variables are excluded from the further analysis on the

rationale that thoroughly examining such typically moderating or mediating variables requires complex regression analysis that is not justified in the present sample size.

Analysis, Results and Discussion

Preliminary analysis. As the Workers sample solely consisted of males, whereas the Academics were gender blended acknowledging the possible confounding effect from gender is appropriate. Hence, independent-samples t-tests comparing gender within the Academics were conducted. These tests revealed no significant difference between men and women for either Statements or IAT scores ($p > .50$). For the Bogardus items, the means were very similar (men: 1.88; women: 2.00); the small difference could not be meaningfully interpreted for statistical significance because the women's scores had zero variability. Overall, these results indicate that male and female academics' answers were the same (see Tables B16 and B17). Moreover, expectedly, weak explicit/implicit correlations for both workers and academics ($r = .075$; $r = -.14$). Supposedly due to the socially sensitive nature of the topic (Cunningham, et al., 2001; Hofmann, et al., 2005) (Table B18).

Difference from the neutral scale midpoint. As reported in Table 2, in their *explicit* attitudes, Workers do not ($p = .49$) differ from explicit measures scale neutral (3), Academics do ($p < .001$), i.e. Workers explicitly express neither negative nor positive attitudes towards immigrants, whereas Academics explicitly express positive attitudes ($mean = 4.18$ [$SD = .6$]) toward immigrants, whereas. Furthermore, the *implicit* measures have a significantly ($p < .001$) positive mean ($Workers = .82$; $Academics = .92$ [$SD = .62$; $.34$]) for both groups, i.e. both groups express relatively stronger pro-Norwegian implicit attitudes.

Table 2: T-tests of Groups Difference from neutral

		t	df	Sig. (2-tailed)
Workers	Statements	.71	13	.49
Academics	Statements	8.56	18	.000
Workers	IAT-effect	4.9	13	.000
Academics	IAT-effect	11.91	18	.000

Note. Statements test value = 3, i.e. scale neutral. IAT-effects test value = 0, i.e. no effect.

Difference between groups. Table 3 reports t-test of group differences in responses to the explicit measures versus the implicit measure. As seen in Table 3 statement variances according to Levenes test ($p = .084$), are homogenous from both groups. Thus, ordinary parametric group mean t-tests were conducted regarding statements. Hence, Table 3 show a significant ($p < .001$) group difference in statement responses. Given the scoring of variables (workers = 1; academics = 2), the direction of the significant difference is such that academics shows *less* prejudice on the explicit (statement) measure than workers do. In other words, when looking at the self-report measure, the present data replicate the well-established education effect.

A significant Levenes test ($p = .016$), for IAT-responses suggest heterogeneous variance in IAT scores, hence, due to dissimilar sample sizes t-testing the equality of medians is appropriately applied (Foster, Barkus, & Yavorsky, 2006). This non-parametric t-test, show no difference between Workers and Academics in the implicit measure ($p = .57$). Thus, when looking at the same participants' implicit scores, the present data do *not* replicate the well-established education effect.

Table 3: t-tests Group Difference Between Explicit and Implicit Measures

		t	df	Sig. (2-tailed)
Statements	Equal variances assumed	-4.16	31	.000
IAT-effect	Equal Variances NOT assumed	-.58	18.64	.57

Note. Levenes test for homogeneity of variances suggested homogenous Statements variances ($p = .84$), whereas heterogeneous for IAT-effects ($p = .16$), hence, The choice of median comparison for IAT effects.

General Linear Models. To safeguard against possible effects of the order of applying the explicit and implicit measures, participants' attitude scores were submitted to analyses of variance with group membership (Worker versus Academic) and order of experimental tasks (first measure: implicit versus explicit) as the factors. That was done separately for implicit and explicit attitudes.

According to Table 4a, and consistent with the t-tests above, only group membership contributes significantly to the prediction of self-report (statement) attitude scores ($p < .001$). In contrast, the GLM indicates neither a main effect of sequential order of measurement ($p = .26$) nor an interaction of order with group ($p = .16$).

Table 4a: General Linear Interaction Model of Explicit Measures

Dependent Variable:

Statements

Source	df	Mean Square	F	Sig.
Measurement order	1	.63	1.32	.26
Group	1	9.68	20.21	.000
Order * Group	1	1.02	2.13	.16

According to Table 4b, and again consistent with the t-tests above, no effect of group membership (or task order, or the interaction of both factors) is observed for implicit (IAT) attitudes, all $ps > .50$.

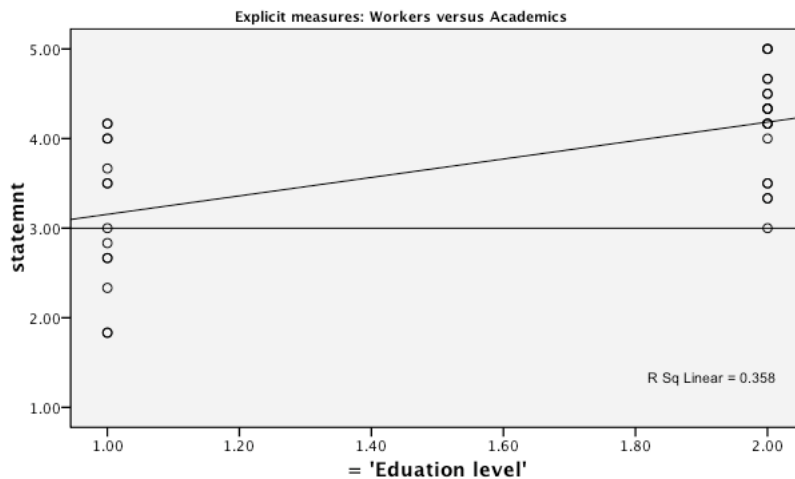
Table 4b: General Linear Model of the Implicit Measure

Dependent Variable: IAT-

Source	df	Mean Square	F	Sig.
Measurement order	1	.05	.21	.65
Group	1	.16	.67	.42
Measurement order * group	1	.17	.70	.41

Graphical illustration. Figure 1 presents visually the replicated education effect in Explicit measures. The horizontal line represents the neutral scale midpoint (3) of the explicit measure Statements. Responses scored above neutral indicates pro-immigrants explicit attitudes, whereas scores below neutral indicates negative explicit attitudes towards immigrants. Moreover, a linear regression line is added, i.e. the slope of the line is equivalent to the slope tested in the GLM above and reported significant ($p < .001$) in the Group row of Table 4a.

Figure 1

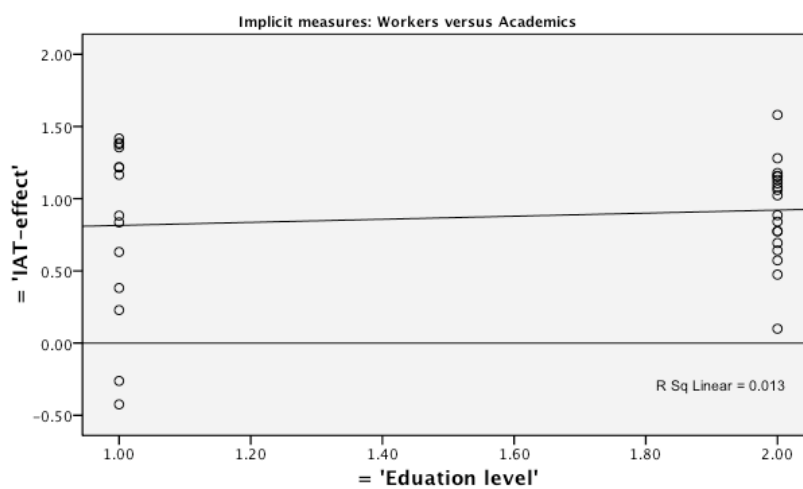


Note. Straight line visually suggest a relatively flat slope. R-square (.013) confirms the notion. Horizontal line (Y = 3) indicates scale neutral.

Figure 2 present visually the IAT-effect between Workers and Academics, i.e. the education effect is not replicated in the implicit measure. IAT.-Scores above zero at the Y-axis indicate

pro-Norwegians implicit attitudes. The regression line is, similarly as in Figure 1, added for illustrative purposes, and the slope ($p = .42$ [see Table 4b]) indicates a non-predictive property of education level to implicit responses. The neutral midpoint is indicated by the horizontal line at Y-zero. However, the variance were tested to be unequal, and visual examination of Figure 2 indicates a somewhat wider dispersion of IAT-responses in the Workers group compared to the Academics. Moreover, two possible outliers can be observed, i.e. two participants yields negative IAT-effects. Negative IAT-effects indicate that the respondent has pro-immigrant implicit attitudes. This represent a slight anomaly as the usually reported pattern show general implicit preference towards one's ingroup. Nonetheless, such responses emerge now and then and also the first publication of the IAT reported some (see Greenwald et al., 1998, p. 1473, and p. 1476). A variety of interpretations might be applied, but the general notion is that the respective participant has in his or her closest social proximity someone belonging to the target outgroup. E.g. marriage to an immigrant.

Figure 2



Note. Straight line visually suggest a relatively steep slope. R-square (.36) confirms the notion. Horizontal line (Y = 0) indicates neutral effectsize

Repeated-measures GLM. Finally, to fully use the within-subjects design of the present study, a repeated-measures analysis of variance was performed on the *Z*-transformed explicit and implicit attitude scores. Again, task order (implicit first versus explicit first) was used as a between-subjects factor, to control for possible effects of this variable. Repeated measures were on assessment method (explicit versus implicit). *Z*-transformed scores were used to compensate for the different units of measurement of the raw scores (explicit measure: scale units from 1 to 7; implicit measure: effect size coefficient *D*). Table 5a presents within subjects effects, whereas Table 5b presents pairwise comparisons of the groups.

Table 5a show no significant order effect. However, it does show a significant interaction ($p < .05$) between measurement type and group membership. In other words, it confirms the notion that the measures are not answered in the same way by Workers and Academics.

A significant interaction can in principle be due to contributions from both of the factors that are involved. Table 5b rules this possibility out. Similar to the preliminary analysis of t-tests (Table 3), Table 5b reports further evidence for an education effect exclusively in the explicit attitudes ($p < .001$), but not in the implicit attitudes ($p = .42$).

Table 5a: Repeated Measures of Within Subjects Effects

Source	df	Sig.
Explicit Implicit Order	1	.699
Explicit Implicit * Group	1	.048

Table 5b: Between Subjects Pair wise Comparison of Groups

Explicit Implicit	(I) Participant group	(J) Participant group	Sig. ^a
Explicit	Workers	Academics	.000
Implicit	Workers	Academics	.421

Study 1-Summary

Study 1, expectedly replicated the education effect in explicit, supposedly cognitive mode, self-report items. However, assessment of implicit IAT-effects did not. Study 1 showed this using *dichotomous* groups of low (Workers) and high (Academics) formal education level i.e. below 13 years versus beyond 16 years respectively. *Firstly*, t-tests revealed that the Workers did not differ from scale neutral in explicit measures of evaluation of Non-Norwegians, whereas high education group show positive evaluation (Table 2). *Secondly*, groupwise (Workers vs. Academics) comparison of central tendencies by t-tests, showed that groups did differ in explicit responses, whereas not in implicit, i.e. Study 1 replicated an education effect in explicit measures which vanishes when measuring the same sample implicitly (Table 3). *Thirdly*, the explicit/implicit dissociation was suggestively confirmed from submitting explicit and implicit measures to separate general linear models which, moreover, disconfirmed interaction effect from the order of explicit/implicit administration (Table 4a and 4b). *Finally*, a repeated measures analysis of variance established the dissociation in explicit and implicit measures regarding the education effect (Table 5a and 5b).

Study 2

A Study 2 were deployed consequently of the non-existing, implicit education effect in Study 1. The rationale is to assess the effect of formal education on implicit racial attitudes, in an independent and larger dataset. The purpose is to suggest support for the results from Study 1, alternatively propose disconfirmation.

The Study 2 was based on a secondary dataset, acquired for the purpose (Nosek, 2009). The set consisted of a complete temporal series ($N = 11.134$), extracted from a race-

IAT at the Project Implicit web survey (IAT-Corp, 2008), of nine subsequent days after January 20th 2009. Study 2 includes no inferential analysis, as the sample size produce statistical significance already from very small effect sizes. Hence, the results and discussion paragraph is reporting descriptives and effect sizes combined with graphic illustrations.

Participants and Procedure

Presumably, all participants voluntarily and self-selected logged onto the Project Implicit demo site (IAT-Corp, 2008; Nosek, Banaji, & Greenwald, 2002) and downloaded an IAT applet. Collecting data that at completion automatically uploaded to the Project Implicit database.

Data Reduction/Exclusion Criteria

Firstly, cases with missing values for education level were excluded, leaving $N = 7478$. *Secondly*, at acquiring the dataset, a variable adhering to the improved scoring algorithm (A. G. Greenwald, et al., 2003) was already included (Nosek, 2009). This feature facilitated data reduction greatly. Cases with a non-valid value in this variable were excluded, i.e. only cases with acceptable IAT data were included in the dataset ($N = 6315$). The algorithm underpinning the second exclusion criteria is described in detail in the *Data reduction, transformations and analysis* paragraph of the Study 1 section above. *Thirdly*, a conservative reduction approach, regarding previously completed IAT's, was adopted excluding participant reporting more than 1 previously performed IAT (Nosek, et al., 2002). The third exclusion criterion further removed 621 cases. After completion of the data reduction regimen the remaining sample were $N = 5694$. Henceforth, referred to as the *full sample*.

Furthermore, a *subsample* were extracted. Recoding education levels into one collapsed and dichotomized variable. This procedure were performed in order to match the education groups in Study 1. Particularly, two separated education ranges were recoded into a dummy, i.e. high range: *Elementary School through High School Graduate*, and *Bachelors Degree through Doctoral level* respectively. Hence, excluding the values for *some college*, *associates degree* and *bachelors degree*. Producing a Study 2 *subsample* ($N = 3181$). See Appendix D for an overview of the education categories.

Full sample: Descriptives, Results and Discussion

The Study 2 fullsample ($N = 5694$) IAT effect descriptives are reported in Table 6. Positive IAT-effect, D-values, indicate a relative pro-whites implicit preference, whereas negative indicates a relative pro-blacks preference. Furthermore, Table 6 reports the overall range of IAT-effects ranging from $D = -1.37$ to $D = 1.69$ ($SD = .44$), i.e. a somewhat wide dispersion around neutral (0). Moreover, Table 6 suggests a slight overall pro-whites preference across all education levels ($Mean = .33$).

Table 6: Overall Descriptives for the *Full sample* of Study 2

	N	Minimum	Maximum	Mean	Std. Deviation
IAT-effect	5694	-1.37	1.69	.33	.44

Note. Negative values suggest relative preference for blacks, whereas positive values suggest relative preference for whites

Figure 3 visually illustrate the distribution of IAT scores for the full sample. Noticing that the straight line running from left to right suggests a slightly stronger pro-whites preference among the Higher educated participants as contrasted to the Lower. However, the broken interpolation line indicates category means. The interpolation line are visually close to coinciding, and suggest homogenous dispersion along the regressed straight line.

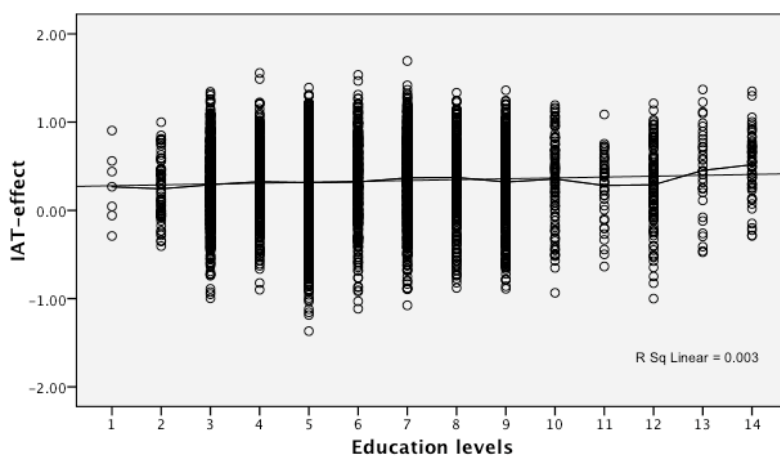
Nonetheless, with a few exceptions: a slight drop in whites preference in categories 11 and 12 (Doctoral level: PhD and J.D.). Moreover an slight increase in category 13 trough 14 (MD. and MBA).

For the purpose of the visual examination of the overall pattern in the full sample, the anomaly of the MBA level placed at the high end of the category array is acceptable, and actually give interesting information in terms of the discrepancy between the straight line and the interpolation line. Nonetheless, it confounds the validity of the R^2 reported, as the underpinning statistical calculations treats the array as scale. However, exclusion of the MBA category lowers R^2 only slightly ($R^2_{\text{-excluded}} = .002$) compared to the full sample ($R^2 = .003_{\text{Fullsample}}$) (see Figure E). Another peculiarity is the increase of white preference in category 13 (MD.) relative to the other doctoral level participants.

R^2 values suggest only a *per mil* implicit effect of education, however, in the whites preferred direction. Hence, in terms of effect sizes it validates the evidence from Study 1 that an education effect in implicit attitudes is void.

Figure 3

Education Range: Elementary school (1) --> Doctoral level (10 through 13) 14 = MBA



Note. Straight line visually suggest a slightly stronger white preference in the High education group. However, R square (.003) indicates that appr. 0.3% of variation are explained by education category. Interpolation line breaks at Y-mean.

Subsample-Descriptives, Results and Discussion

Table 7 also reveals a non-considerable, but slightly narrower range and smaller standard deviance for IAT-scores in the Low education group compared to the High education group (-1.00 , 1.56 [*SD* = .40] ; -1.08 , 1.69 [*SD* = .45]). Calculated from the subsample.

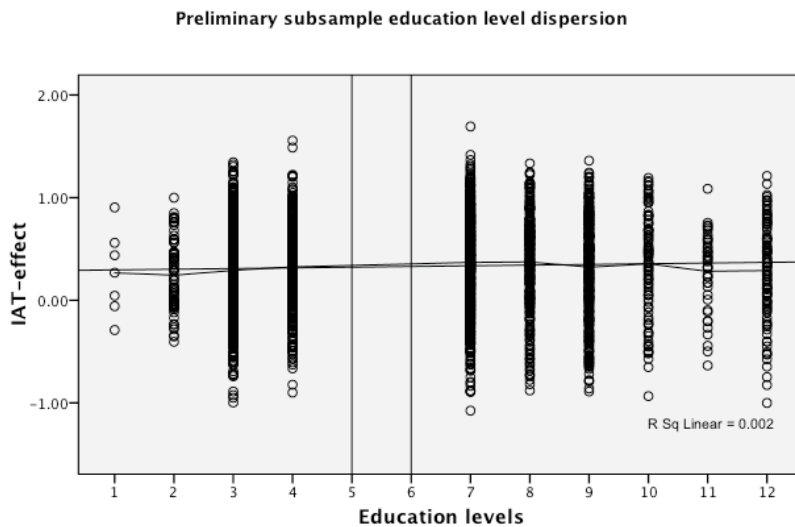
Table 7: Descriptive for the Dichotomized Subsample of Study 2

Two level education		N	Minimum	Maximum	Std. Deviation
Low education	IAT-effect	1321	-1.00	1.56	.40
High education	IAT-effect	1860	-1.08	1.69	.45

Note. Negative values suggest relative preference for blacks, whereas positive values suggest relative preference for whites

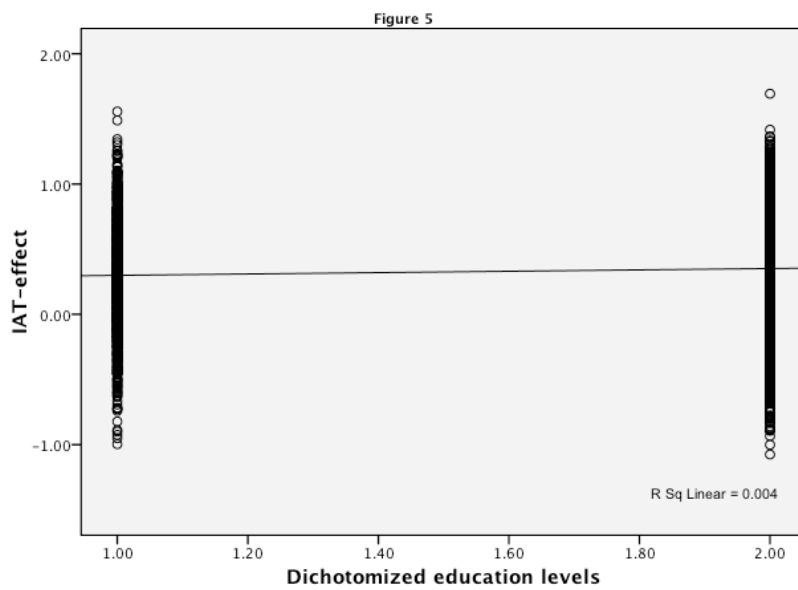
Figure 4 preliminary juxtapose the internal dispersion of the selected subsample category ranges. The figure 2 array yields an R^2 of .002. The interpolation line breaks at category means and, hence, coincide with the interpolation line in the full sample (Figure 1).

Figure 4



Note. Straight line visually suggest slightly stronger white preference in the High education group. However, R-square (.002) indicates that appr. 0.2% of variation are explained by education ranges. Interpolation line breaks at category Y-means. Vertical lines in place of excluded education categories.

Figure 5 illustrates the collapsed variable dichotomizing education groups on the left and right of the vertical lines in Figure 2 (Categories 1 through 4 and 7 through 12). The pattern of the slightly higher pro-whites preference in the higher education groups are replicated. Yielding a R^2 of .004. Although a 100% increase compared to Figure 2, this is still a negligible effect in terms of explained variance.



Note. Straight line visually suggest a slightly stronger whites preference for High education group. However, R-square (.004) indicates that appr. 0.4% of variation is explained by education group

General Summary and Discussion

Summary

Previous research had reported a robust significant negative correlation between level of formal education and negative social evaluation, i.e. attitudes, toward immigrant groups get more positive with increasing education. However, data underpinning the notion of an robust education effect relied exclusively on self-reported measures. Self reported responses are known to be affected by social desirability issues, nevertheless the summed up in the *Special case* sections above, all fundamentally relies on self report in their effort to circumvent social desirability. However, the present Study 1 juxtaposes measured attitudes relying on the implicit attitude paradigm to traditional self report measures, whereas the present Study 2 confirms the vanishing of the education effect when applying implicit measurement methods.

Using two groups: one of below 13 years formal education ($N = 14$), one beyond 16 years of formal education ($N = 19$), Study 1, expectedly, replicated the education effect in explicit measures. Statements were post selected, but also Bogardus items clearly replicated the education effect. As hypothesised, however, no education effect were seen in implicit measures, i.e. in t-tests, general linear models and in repeated measures analysis considering the order of explicit implicit administration as well.

Study 2 was facilitated by a secondary dataset (IAT-Corp, 2008; Nosek, 2009), suggested confirmation the proposition corollary of Study 1 that education level is not predictive for implicit race attitudes.

Various Issues

The possible confounding effect from gender in Study 1, as the Workers group solely consisted of males, whereas Academics were gender blended, were initially dealt with by t-testing scores between gender within the gender blended group. These tests indicated no confounding from gender.

Interestingly, investigating the education effect in explicit measures, revealed that the low educated groups mean was approximately tangent to scale neutral, not as intuitively expected, biased to the prejudiced side. Notice, however, that no formal analysis of skewness or kurtosis were performed. However, homogeneity of variance between groups were concluded from Levene tests ($p > .05$) for all the explicit measures, and hence, suggested overall underlying normality of the scores.

Moreover, the implicit measure produced a statistically non-significant response difference between low and high education. However, it suggest an general pro-Norwegian preference across education groups. The latter is an expected effect as preference for ones own group is generally seen. Even more importantly, the significant implicit pro-Norwegian attitudes that were found for both workers and academics rule out a trivial alternative explanation of the present results. Specifically, it might be argued that the IAT in this study may have been inadequate somehow, and may therefore have failed to detect differences between the attitudes of workers and academics. This is however clearly not the case. The IAT was sufficiently sensitive to identify existing, pro-Norwegian attitudes in both groups. It would therefore also have been sufficiently sensitive to detect differences between the groups – had there been any. Subsequently of the results from Study 1, a study 2 were deployed. Using a temporal series from a web survey race-IAT (IAT-Corp, 2008) ($N = 11.134$). A similar inferential analysis regime as in Study 1 are obviously mandated in Study 2.

However, the application of inferential analysis techniques to a dataset of close to 12,000 participants produce significance from extremely small effect sizes. Presently on a per mil explanation level. Hence, such analysis were not conducted.

Due to the sample size the decision to examine it in terms of effect sizes and visual study of graphics were taken, i.e. Study 2 data were treated, consistent with some recommendations for working with large web collected samples (Nosek, et al., 2002).

Visual examination and effect sizes of the Study 2 data, revealed no apparent education effect. However, a slight increase in pro-whites preference is visually suggested in Figures 1 through 3, i.e. in the opposite direction of self report prediction. Examining effect size expressed as R^2 suggest that this increase is on a per mil level (R^2 range: [.002 , .004]) and, thus, can hardly be considered equivalent or even close to the magnitude of self reported education effects (Blom, 2005, 2007; Directorate-General Information, 1988; M. R. Jackman, 1978). Hence, Study 2 suggest confirmation to the lack of an education effect in implicit measures in Study 1.

Moreover, Study 2 data were divided into 14 education categories. One could argue that the comparison of IAT-effects from IAT's differently administered, and with different contents are a far fetched comparison. However, the IAT-effects are resulting from the same scoring algorithm and, hence, it is directly comparable to the primary data in Study 1. Nonetheless, the results should be treated with caution, as web collected data on a socially and politically sensitive topic possibly cause considerable restriction of range due to self selection.

Future research on the present topic should *firstly* replicate the present results in stringently designed studies as in Study 1 with larger samples. *Secondly*, future studies should

be designed also regarding the dissociation question, i.e. whether the implicit attitude concept taps into completely different underlying attitude modality.

As previously discussed if subjective unawareness of the implicit attitude is the case, then the non-existing education effect in such measures is *not* a contradiction to the explicit measures education effect, merely a complementary. Alternatively, if unwillingness to report is the case, the non-existing education effect in implicit measures suggest higher educated peoples greater susceptibility to demand characteristics. Which would be the suggestion if adopting a definition of attitudes as one single underlying concept.

Other suggested regards to take in future research is that the measurement of implicit attitudes might be confounded by the familiarity of the stimulus items as well as the internal context of the IAT might change the subjective response criterion as the test goes along (Brendl, et al., 2001). The present Study 1 attempted to avoid such low level confounders by applying perceptually clean stimulus, nevertheless, as the present pre-test illustrates, Norwegian spelled words are judged as Norwegian regardless of their semantic properties, Hence, careful analysis and selection of such contents are necessary.

Finally, poor correlation between explicit and implicit measures on socially sensitive topics, as the present, is expected from some reports (Hofmann, et al., 2005). However, others argue by considering latent correlations, that dissociation is not true (Cunningham, et al., 2001). The former would predict the pattern seen in the present thesis, whereas the latter would validate it as evidence and a contribution to the debate of single or dual attitudes.

Regardless of the theoretical stance to the issues presented in this thesis, the results are, to the knowledge of the author, a unique contribution for examining the effect of formal education on prejudiced attitudes.

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Appendix A

Selection of Contents

Pre-Selection of Implicit Stimuli

Pre-test questionnaire

Formålet med dette spørreskjemaet er som forstudie til en masteroppgave ved Universitetet i Tromsø.

Hensikten med spørsmålene er å kartlegge hvorvidt ord og navn er assosiert svakere eller sterkere til dimensjonen *norsk-unorsk*, og i tillegg *om* de samme ordene og navnene, har *behagelig* eller *ubehagelig* valør.

Dine svar vil være fullstendig anonyme, og vil utelukkende inngå i et materiale med formål å velge best mulig grunnlag for de senere undersøkelsene.

Vi ber derfor om at du svarer raskt og intuitivt.

På den første skalaen i del 1, under hvert navn, skal du svare på om du reagerer på navnene med behag eller ubehag ved å sette en ring rundt tallet du velger.

På den andre skalaen i del 1, under hvert navn, skal du svare på hvor typisk norske eventuelt innvandrere typiske navnene/ordene er.

I del 2 gjør du det samme med ordene.

Et eksempel:

Henkie

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Da starter vi:

Del 1 Navn

Rafi

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Rolf

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Amir

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Arnt

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Emir

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Erik

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Luja

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Lena

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Ayat

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Aina

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Saja

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Silje

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Mahdi

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Magne

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Jamal

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Jarle

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Gamal

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Gaute

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Budur

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Berit

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Dahab

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Dagny

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Tarub

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Tiril

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Tawfiq

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Tarjei

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Sayyar

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Sondre

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Halvor

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Hajjaj

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Durrah

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Dagmar

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Musnah

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Margot

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Sundus

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Sigrid

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Del 2 Ord

Fred

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Feig

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Venn

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Vold

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Rett

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Redd

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Sann

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Sorg

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Bror

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Bitt

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Smile

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Smuss

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Rolig

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Robbe

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Bake

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Bombe

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Snill

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Stank

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Helse

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Hate

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Supert

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Sykdom

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Vakker

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Voldta

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Frihet

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Fattig

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Blomst

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Blodig

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Varsom

Behagelig 3 2 1 0 1 2 3 Ubehagelig

Unorsk 3 2 1 0 1 2 3 Norsk

Hvor gammel er du?

20-25

25-30

35-40

40-45

45-50

55-60

60-65

Hvilket kjønn?

Mann/Kvinne

Har du over tre år utdannelse etter videregående?

Ja/Nei

Appendix B

*Pre-test Analysis**Target Stimuli*

Table 1. Target Descriptives on Attribute Dimension

	N	Mean	Std. Deviation	Std. Error Mean
Amir	17	.12	1.54	.37
Arnt	17	.12	1.58	.38
Jamal	17	.06	1.59	.39
Jarle	17	.29	1.53	.37
Mahdi	17	.49	1.62	.39
Margot	17	.12	1.87	.45
Rafi	17	.06	1.56	.38
Rolf	17	.29	1.53	.37

Note. Positive mean suggest pleasantness

Table 2. Comparison of Deviance from Zero for Target Stimuli on Attribute Dimension
t-test Names unpleasant and pleasant

Test Value = 0						
95% Confidence Interval of the Difference						
	t	df	Sig. (2-tailed)	Mean Difference	Lower	Upper
Amir	.32	16	.76	.12	-.67	.91
Arnt	.31	16	.76	.12	-.69	.93
Jamal	.15	16	.88	.06	-.76	.88
Jarle	.79	16	.44	.29	-.49	1.08
Mahdi	1.05	16	.31	.41	-.42	1.25
Margot	.26	16	.80	.12	-.84	1.08
Rafi	.16	16	.88	.06	-.74	.86
rolf	.79	16	.44	.29	-.49	1.08

Note. Non-significant t-test suggest neutral score on attribute differential ($\alpha = .05$)

Table 3. Target Descriptives on Target Dimension
Names statistics Non-Norwegian vs. Norwegian

	N	Mean	Std. Deviation	Std. Error Mean
Amir	17	-2.88	.33	.08
Arnt	17	2.35	1.11	.27
Jamal	17	-2.65	.70	.17
Jarle	17	2.76	.44	.11
Mahdi	17	-2.29	1.49	.36
Margot	17	1.76	1.88	.46
Rafi	17	-2.59	.71	.17
Rolf	17	2.47	.62	.15

Note. Negative mean suggest non-Norwegianness. Positive mean suggest Norwegianness

Table 4. Comparison of Deviance from Zero for Target Stimuli on Target Dimension
t-test names non-Norwegian and Norwegian

Test Value = 0						
95% Confidence Interval of the Difference						
	t	df	Sig. (2-tailed)	Mean Difference	Lower	Upper
Amir	-35.78	16	.000	-2.88	-3.05	-2.71
Arnt	8.70	16	.000	2.35	1.78	2.93
Jamal	-15.55	16	.000	-2.65	-3.01	-2.28
Jarle	26.07	16	.000	2.76	2.54	2.99
Mahdi	-6.35	16	.000	-2.29	-3.06	-1.52
Margot	3.85	16	.001	1.76	.79	2.74
Rafi	-14.98	16	.000	-2.59	-2.95	-2.22
Rolf	16.32	16	.000	2.47	2.15	2.79

Note. Significant t-test suggest non-neutral score on Norwgianness differential. See descriptives table for directions.

Attribute stimuli

Table 5. Attribute Descriptives on Attribute Dimension
Words statistics unpleasant vs. pleasant

	N	Mean	Std. Deviation	Std. Error Mean
Blomst	17	2.24	.90	.22
Bombe	17	-1.94	1.25	.30
Fattig	17	-2.18	1.13	.27
Frihet	17	2.88	.33	.08
Robbe	17	-1.64	1.32	.32
Rolig	17	2.65	.61	.15
Supert	17	2.00	1.70	.41
Sykdom	17	-2.06	1.43	.35

Note. Negative mean suggest unpleasantness. Positive mean suggest pleasantness.

Table 6. Comparison of Deviance from Zero for Attribute Stimuli on Attribute Dimension
t-test words unpleasant and pleasant

Test Value = 0						
95% Confidence Interval of the Difference						
	t	df	Sig. (2-tailed)	Mean Difference	Lower	Upper
Blomst	10.20	16	.000	2.24	1.77	2.70
Bombe	-6.41	16	.000	-1.94	-2.58	-1.30
Fattig	-7.93	16	.000	-2.18	-2.76	-1.59
Frihet	35.79	16	.000	2.88	2.71	3.05
Robbe	-5.14	16	.000	-1.65	-2.33	-.97
Rolig	18.00	16	.000	2.65	2.33	2.96
Supert	4.86	16	.000	2.00	1.12	2.87
Sykdom	-5.95	16	.000	-2.06	-2.80	-1.32

Note. Significant t-test suggest non-neutral score on Pleasantness differential. See descriptives for directions.

Table 7. Attribute Descriptives on Target
Words statistics non-Norwegian vs. Norwegian

	N	Mean	Std. Deviation	Std. Error Mean
Blomst	17	2.00	1.37	.33
Bombe	17	1.41	1.73	.42
Fattig	17	1.47	2.27	.55
Frihet	17	2.29	1.05	.25
Robbe	17	1.06	1.71	.42
Rolig	17	1.76	1.52	.37
Supert	17	1.76	1.20	.29
Sykdom	17	1.70	1.36	.33

Note. Positive mean suggest Norwegianness.

Table 8. Comparison of Deviance from Zero for Attribute Stimuli on Target Dimension
t-test words non-Norwegian and Norwegian

Test Value = 0						
t	df	Sig. (2-tailed)	Mean Difference	95% Confidence Interval of the Difference		
				Lower	Upper	
Blomst	6.02	16	.000	2.00	1.29	2.70
Bombe	3.36	16	.004	1.41	.52	2.30
Fattig	2.68	16	.02	1.47	.31	2.64
Frihet	9.04	16	.000	2.29	1.76	2.83
Robbe	2.55	16	.02	1.06	.18	1.94
Rolig	4.78	16	.000	1.76	.98	2.55
Supert	6.06	16	.000	1.76	1.15	2.38
Sykdom	5.18	16	.000	1.71	1.01	2.40

Note. Significant t-tests suggest non-neutral scores on Norwegianness differential. See Pair wise t-test below for fatherly specific selection rationale.

Pair wise comparison of selected attribute stimuli

Table 9. Attribute Descriptives Attribute Dimension

	Mean	N	Std. Deviation
Pair 1 Unpleasant words	-1.96	17	1.03
Pleasant words	2.44	17	.68

Table 10. Pair wise t-test of Selected Attribute Stimuli. Testing Means Across Attribute Dimension

Paired Differences of Means									
				95% Confidence Interval of the Difference		t	df	Sig. (2-tailed)	
Mean	Std. Deviation	Std. Error Mean	Lower	Upper					
Pair 1 Unpleasant words versus Pleasant words	-4.39	1.41	.34	-5.12	-3.67	-12.87	16	.000	

Table 11. Attribute Descriptives Target Dimension

		Mean	N	Std. Deviation
Pair 1	Unpleasant words	1.41	17	1.55
	Pleasant words	1.81	17	1.10

Table 12. Pair wise t-test of Selected Attribute Stimuli. Testing Means Across Target Dimension

		Paired Differences of Means					t	df	Sig. (2-tailed)
		Mean	Std. Deviation	Std. Error Mean	95% Confidence Interval of the Difference				
					Lower	Upper			
Pair 1	Unpleasant words versus Pleasant words	-.39	.87	.21	-.84	.05	-1.89	16	.08

Post-Selection of Explicit Measures

Reliability statistics and correlation matrixes explicit/implicit measures

Table 13. Reliability Explicit

	Cronach's Alpha
Statements	.82
Bogardus	.57

Table 14. Single Item Contribution Statements

	Cronbach's Alpha if Item Deleted
Statement 1	.78
Statement2	.77
Statement3	.80
Statement4	.77
Statement5	.83
Statement6	.77

Table 15. Item-total contribution Bogardus

	Cronbach's Alpha if Item Deleted
Bogardus 1	.52
Bogardus 2	.57
Bogardus 3	.20

Preliminary Analysis of Confounding Effects from Gender

Table 16. Gender Descriptives Within Academics

	Participant gender	N	Mean	Std. Deviation
IAT	Male	8	.93	.37
	Female	11	.92	.33
Statements	Male	8	4.08	.45
	Female	11	4.26	.71
Bogardus	Male	8	1.88	.17
	Female	11	2.00	.00
Political	Male	8	2.00	.93
	Female	11	2.36	.81

Table 17. T-test Gender Within Academics on Relevant Scores

		t	df	Sig. (2-tailed)
IAT	Equal variances assumed	.05	17	.96
Statements	Equal variances assumed	-.61	17	.55
Political	Equal variances assumed	-.91	17	.38

Note. Gender t-test of Bogardus scores is not conducted as Female scores yielded Zero variance.

Table 18: Explicit Implicit Correlations by Group

Participant group		Statements	IAT
Workers	Statements	Pearson Correlation	1 .075
		Sig. (2-tailed)	.80
IAT	IAT	Pearson Correlation	.075 1
		Sig. (2-tailed)	.80
Academics	Statements	Pearson Correlation	1 -.14
		Sig. (2-tailed)	.57
IAT	IAT	Pearson Correlation	-.14 1
		Sig. (2-tailed)	.57

Appendix C

Study 1 Computer Package Content

Start Window

Hei, takk for at du deltar i dette studiet:

"I de neste oppgavene vil du måtte sortere ord ved å trykke på knappene "D" og "K". Hvert ord som kommer opp på skjermen skal sorteres enten til høyre ("K" knappen), eller venstre side ("D" knappen). Oppgaven er enkel. Ikke vær redd for å gjøre feil. Feil retter du opp underveis. Vær så snill å trykk så raskt du kan.

Part 1:

-->Del 1

-->Fortsett

The "Obs! Popup"

-->Følg med etikettene oppe på skjermen. Bruk knappene «D» og «K». TRYKK SÅ RASKT DU KAN. Feil (rødt X) må rettes opp.

Contents IF explicit part 1:

Intro to the statement type items

Nå følger noen spørsmål om innvandring og innvandrere. Først noen påstander. Vil du for hver av de følgende påstandene si om du er helt enig,

nokså enig, nokså uenig eller helt uenig?

Intro to the Bogardus type items:

I de følgende tre spørsmålene forutsetter vi at innvandreren behersker norsk. Svaralternativene er Ja eller Nei

Table C

	5 point Likertscaled	Bogardus items	Political	Personal experience
	<p>1. Innvandrere flest gjør en nyttig innsats i norsk arbeidsliv.</p> <p>2. Innvandrere flest misbruker de sosiale velferdsordningene.</p> <p>3. Innvandrere flest beriker det kulturelle livet i Norge.</p> <p>4. Innvandrere flest er en kilde til utrygghet i samfunnet.</p>	<p>1. Ville du synes det var ubehagelig hvis du eller noen i din nærmeste familie fikk en hjemmehjelp som var innvandrer?</p> <p>2. Ville du synes det var ubehagelig dersom du fikk en innvandrer som ny nabo?</p> <p>3. Ville du</p>	<p>1. Så et spørsmål om flyktningers adgang til Norge. Sammenliknet med i dag, bør det bli lettere for flyktninger og asylsøkere å få opphold i Norge, bør det bli vanskeligere, eller bør adgangen til å få opphold være som i dag?</p>	<p>1. Hvor mange innvandrere har du kontakt med? Er det:</p> <p>2. Hvor ofte har du vanligvis kontakt med innvandrere? Er det</p> <p>3. Hvordan er din personlige</p>

	<p>5. Alle innvandrere i Norge bør ha samme mulighet til arbeid som nordmenn.</p> <p>6. Innvandrere i Norge bør bestrebe seg på å bli så like nordmenn som mulig</p>	<p>synes det var ubehagelig dersom du hadde en sønn eller datter som ville gifte seg med en innvandrere?</p>		<p>erfarin g med denne kontakt en? Er den:</p>
--	--	--	--	--

"Part 2"

--> Del 2

-->Fortsett

Contents IF Implicit part 2:

Demographics page

Alder? (År:[])

Kjønn? (Mann [] ; kvinne:[])

Utdanningsnivå? (Grunnskole:[] ;

Videregående:[] ;

Høgskole/Universitet inntil 3 år: ;

Høgskole/Universitet over 3 år

Norsk morsmål? (Ja: ; Nei)

"That`s all screen"

Det var alt. Takk for deltakelsen

Lukk programmet

IAT content

	Unpleasant	Pleasant	Non-norwegian	Norwegian
Words	Bombe Fattig Robbe Sykdom	Blomst Frihet Rolig Supert		
Names			Amir Jamal Mahdi Rafi	Arnt Jarle Margot Rolf

Appendix D

Education level coding Study 2

1 >elementary school

2 >junior high

3 >some high school

4 >high school graduate

5 >some college

6 >associate's degree

7 >bachelor's degree

8 >some graduate school

9 >master's degree

10>J.D.

11>M.D.

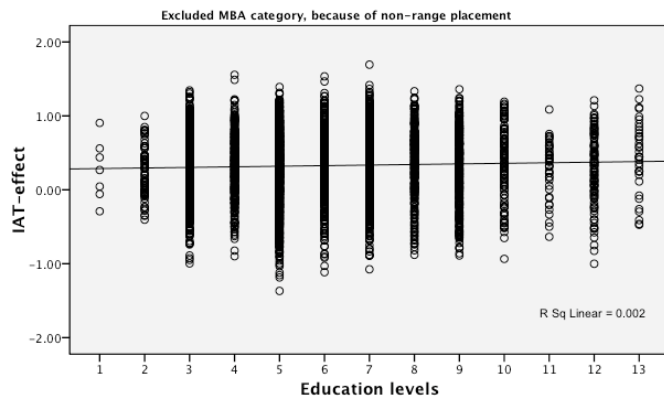
12>Ph.D.

13>other advanced degree

14>M.B.A.

Appendix E

Figure A



Note. Straight line visually suggest a slightly stronger white preference in the High education group. However, R square (.003) indicates that appr. 0.3% of variation are explained by education category

Appendix F

Statement of informed consent:



Education effect of implicit social evaluations study.

Vær snill å lese følgende informasjon nøye før deltakelse, **still gjerne spørsmål dersom det er noe som er uklart:**

Undersøkelsen går ut på å klarlegge forholdet mellom utdanning/yrke og enkelte typer holdninger. Det består av en ren spørreundersøkelse og i tillegg en del der du skal trykke på to knapper på tastaturet etter enkle regler. Programmet gir instruksjoner underveis.

Prosedyren tar rundt 15 minutter og innebærer ikke noe som vanligvis vil oppfattes som ubehagelig, støtende eller vanskelig.

Jeg _____ forstår hva min deltakelse i undersøkelsen innebærer, at min deltakelse er frivillig og at jeg på ethvert tidspunkt kan avbryte uten noen konsekvenser.

Jeg er også oppmerksom på at mine personlige opplysninger holdes konfidensielle, ikke blir, og heller ikke kan koples sammen med mine resultater i selve undersøkelsen.

Signatur: _____ Dato: _____ (deltaker)

Signatur: _____ Dato: _____ (eksperimentator)