

**Testing Theories about Ethnic Markers:
Ingroup Accent Facilitates Coordination, Not Cooperation**

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Abstract

In recent years, evolutionary psychologists and anthropologists have debated whether ethnic markers have evolved to solve adaptive problems related to interpersonal coordination or to interpersonal cooperation. In the present study, we add to this debate by exploring how individuals living in a modern society utilize the accents of unfamiliar individuals to make social decisions in hypothetical economic games that measure interpersonal trust, generosity, and coordination. A total of 4,603 Danish participants completed a verbal-guise study administered over the Internet. Participants listened to four speakers (two local and two non-local) and played a hypothetical Dictator Game, Trust Game, and Coordination Game with each of them. The results showed that participants had greater faith in coordinating successfully with local speakers than with non-local speakers. The coordination effect was strong for individuals living in the same city as the particular speakers and weakened as the geographical distance between the participants and the speakers grew. Conversely, the results showed that participants were not more generous toward or more trusting of local speakers compared to non-local speakers. Taken together, the results suggest that humans utilize ethnic markers of unfamiliar individuals to coordinate behavior rather than to cooperate.

Keywords: coordination, trust, cooperation, accent, ethnic marker

1. Introduction

Despite widespread geographical mobility and exposure to different beliefs and behavioral norms, humans remain stratified into ethnic groups with distinct social norms and with commonly recognized boundaries between groups (Boyd and Richerson 1987; LeVine and Campbell 1972). As evidenced by decades of research within social psychology, this group-based structure is maintained by spontaneous social categorization processes and parochialism (i.e., preferences for assorting with ingroup members; see Bernhard et al. 2006; Tajfel et al. 1971). Ethnic markers are phenotypic tags, such as standardized body ornaments, hairstyles, manners, or clothing, that are shared by the members of a group, and they facilitate social categorization processes by communicating social identity (Barth 1969). Archaeological records indicate that prehistoric humans used ethnic markers as far back as 100,000 years ago in the African Middle Stone Age (Kuhn and Stiner 2007; Marwick 2003; 2005; McBrearty and Brooks 2000). During this time period, it is generally believed that early hominids lived in large mobile bands embedded in multi-band exchange networks with some level of exogamy and migration (Apicella et al. 2012; Aiello and Dunbar 1993; Chapais 2010; Hill et al. 2011). Under such socio-ecological conditions, prehistoric humans would often interact face-to-face with unfamiliar ingroup as well as outgroup individuals, and ethnic markers could have provided prehistoric humans with a competitive advantage, by allowing selective assortment with similarly marked individuals (McElreath et al. 2003; Riolo et al. 2001).

There is, however, a lack of scientific consensus about the core function of ethnic markers once selective assortment is enabled. A common view (Van den Berghe 1981; Nettle and Dunbar 1997; see also Cohen 2012) holds that social networks with commonly recognizable ethnic markers enable individuals to benefit from tag-based cooperative exchanges. Because cooperation norms are widespread within groups but not necessarily between groups (see Bernhard et al. 2006a; Bernhard et al. 2006b; Shinada et al. 2004), ethnic markers could enable individuals to benefit from reciprocal exchanges by selectively directing (and receiving) altruistic and cooperative efforts toward ingroup members. This theory is supported by three findings in the literature. First, several modeling studies in evolutionary biology have shown that a strategy of cooperating with similarly marked individuals can emerge and sustain among different strategies given specific socio-ecological conditions (Antal et al. 2009; Masuda and Ohtsuki 2007; Riolo et al. 2001). Second, several economic studies have found a negative association between ethnic diversity and generalized trust and public cooperation in various societies (Alesina and La Ferrara 2002; Esteban

et al. 2012; Hero 2003; Putnam 2007; Stolle et al. 2008; Warring 2012). Third, experimental studies conducted in the lab and on the internet using the minimal group paradigm show that humans are usually more cooperative and generous toward unfamiliar individuals with whom they share social categories, even when the shared social category is trivial and temporary as well as when exchange partners remain anonymous to each other (Ahmed 2007; Chen and Li 2009; De Cremer and Van Vugt 1999; Hewstone et al. 2002; Koopmans and Veit 2009; Simpson 2006; Summerville and Chartier 2013; Tajfel et al. 1971).

At the same time, however, each of these findings faces challenges. First, modeling studies are limited by their reliance on certain parameters and conditions (e.g. low mutation rates of shared tags, obligatory cooperation with similarly tagged individuals - Antal et al. 2009; Roberts and Sherratt 2002). Also, many modeling studies of tag-based cooperation systems rest, to different degrees, on greenbeard theory, a theory that has been criticized for being vulnerable to exploitation by cheaters—i.e., individuals possessing the tag but not the associated behavioral disposition (Dawkins 1976; McElreath et al. 2003). Second, several studies have failed to find a significant association between ethnic diversity and generalized trust, in particular in European countries (Gesthuizen et al. 2009; Hooghe et al. 2009; Letki 2008; but see Koopmans and Veit 2014). Different explanations for this divergence have been offered. According to Hooghe et al. (2009), socioeconomic status might be a confounding factor in many existing North American studies, because the ethnically diverse communities sampled in these studies are typically economically disadvantaged and being economically disadvantaged has, in turn, been linked to low levels of generalized trust (e.g. Holland, Silva and Mace 2012; Nettle et al. 2014). Third, the extent to which findings of ingroup favoritism might generalize from minimal group settings to natural groups has been questioned by several researchers (see Ferraro and Cummings 2007; Ferstman and Gneezy 2001; Koopmans and Veit 2014; Yamagishi et al. 2005; for exceptions, see Bernhard et al. 2006; Falk and Zehner 2013). As argued by Leyens et al. (1994), groups may be defined by minimal criteria in the minimal group approach, but the salience of group membership is maximized. Hence, a number of studies suggests that ingroup favoritism is reduced substantially in more realistic settings. For example, including more than two social categories as well as cross-cutting social categories weakens ingroup favoritism substantially (Brown and Turner 1979; DeChamps and Doise 1978; Hartstone and Augoustinos 1995); including meaningful group markers (i.e. shared political and religious orientations) eliminates ingroup favoritism towards meaningless ingroups (i.e. shared birth season) (Koopmans and Rebers 2009); providing subjects with individuating

information about others in addition to cues about their social identity, eliminates ingroup favoritism (Tanis and Postmes 2005); and, finally, studies have shown that ingroup favoritism in minimal groups vary between cultures, and that it is less evident whether it exists in eastern cultures at all (see Falk, Heine and Takemura 2014).

The theoretical criticism and mixed empirical findings have created space for an alternative take on the social role of ethnic markers and ethnic categorization. Hence, some researchers (Gil-White 2001; McElreath et al. 2003; see also Moya 2013) have argued that ethnic markers, rather than facilitating problem-solving directly related to judgments about the cooperativeness of others, enable individuals to solve coordination problems (i.e., ambiguity related to predicting and synchronizing perceptions, beliefs, attitudes, and actions). Deriving benefits from successful coordination between individuals relies on accurate inferences of each others' knowledge states, preferences, and behavioral habits, and one way to boost inference accuracy when facing unfamiliar individuals is to adhere to group-based norms as local focal points (see Schelling 1960; Sugden 1995). Accordingly, the spontaneous categorization of unfamiliar individuals based upon arbitrary ethnic markers serves the function of determining whether one shares social norms with them. Importantly, this form of tag-based coordination is not vulnerable to exploitation, because cheaters do not gain anything from unsuccessful coordination (McElreath et al. 2003).

1.1. The Case for Human Accent as an Ethnic Marker

In this paper, we test whether ethnic markers affect decisions related to interpersonal cooperation and interpersonal coordination. We test these two hypotheses in the context of a particularly important ethnic marker: accent. Decades of sociolinguistic research across the world have shown that human accent (i.e., phonetic-based variation in speaking across regions) is a universal feature of human language (Edwards 2009). Potential causes of accent-based linguistic variations include cultural isolation and drift, imperfect learning, and social selection (Chambers et al. 2002; Nettle 1999). Because prehistoric humans living in fission/fusion networked bands, they would continually be exposed to accent-based linguistic differences (Nettle 1999), and because human accents play a key role in social categorization (Edwards 2009; Gluszek and Dovidio 2010), several researchers have claimed that accents could have evolved as a prototypical ethnic marker (Bernhard et al. 2006; McElreath et al. 2003; Nettle and Dunbar 1997). Consistent with this, Pietraszewski and Schwartz (2014a; 2014b) have recently found evidence that suggest accent is a privileged dimension of social categorization (along with sex and age) and Moya (2013) has shown

that already children possess priors for ethnolinguistic categorization.

Importantly, for our purposes, human accents possess critical features that make them suitable for tag-based cooperation (for a thorough review of these features, see Cohen 2012). First, countering the argument that systems of tag-based cooperation are vulnerable to cheaters, human accents are rather hard to fake, because the process of accent acquisition piggybacks on general human language-learning capabilities (i.e., accents are acquired quickly and easily during early development but slowly during adulthood; Baker 2001; Herschenshon 2007; Kuhl et al. 2006). Consequently, a speaker's accent is a fairly reliable cue to his or her social identity.¹ Second, human accents are relatively dynamic, which is another feature that is important for protecting against invasion by cheaters (Cohen 2012). As a consequence, whereas many other types of phenotypic markers (i.e., particular clothing items or hairstyles) can be easily dismissed as candidates for the evolution of tag-based cooperation because of their vulnerability to cheating, human accents constitute an interesting case. Sociolinguistic research corroborates this. People across the world tend to evaluate ingroup (local) speakers more positively than outgroup speakers, especially on items related to integrity and social solidarity (Edwards 2009; Giles and Powesland 1975; Trudgill 1974).² Furthermore, recent studies have shown that accent guides infants' and children's social interactions with other individuals more than other social categories such as race (Kinzler et al. 2009). Infants prefer to reach for objects and food that are offered by native speakers rather than by foreign-accented speakers (Kinzler et al. 2007; 2012; Shutts et al. 2009). Pre-school children selectively learn from native speakers and prefer native speakers as friends and as recipients of gifts to foreign-accented speakers (Kinzler et al. 2009; 2011; 2012). Recently, Cohen and Haun (2013) also found that accent guided five to ten- year-old Brazilian children's friendships and resource preferences, but only in towns with accent heterogeneity.

¹ As a means of effectively discriminating between ingroup and outgroup individuals, accents strike a functional balance between allowing detection of cheaters (a given accent can typically not be faked in a fleeting encounter) and allowing the accommodation of new individuals into the ethnic group over time (an accent can typically be acquired through extensive linguistic exposure to it). See Cohen 2012.

² Experimental studies using behavioral responses to adult speakers' accents are rare. Using natural experiments with cinema audiences, two studies have, however, shown that behavioral compliance to public solicitations (i.e., to fill out questionnaires) made over the cinema's loudspeakers were affected by the solicitor's language and accent. In Wales, Bourhis and Giles (1976) found significantly greater compliance to the solicitations when they were made by ingroup speakers as compared to outgroup speakers. Kristiansen and Giles (1992) replicated this finding in a Danish sample, but only with adult audiences. Young audiences returned more questionnaires when the solicitations were made by a non-local (Standard) speaker.

1.2. The Present Study: Predictions

In the present study, we explore how individuals living in a modern society utilize unfamiliar individuals' accents to make social decisions in hypothetical economic games. Given the different potential functions of ethnic markers for social interaction, we test two distinct hypotheses:

1. To the extent that ethnic markers such as accent serve to facilitate tag-based cooperation, individuals should place more trust in and show greater generosity toward local than toward non-local speakers in situations in which they can harvest cooperative surpluses (cf. Nettle and Dunbar 1997; Cohen and Haun 2013).
2. To the extent that ethnic markers such as accent serve to facilitate tag-based coordination, individuals should be more motivated to coordinate with local than with non-local speakers in situations in which they can harvest surpluses from successful exchanges (cf. McElreath et al. 2003).

2. Method

We tested these predictions using a very large sample from Denmark, a Western developed democracy. The small Danish population (around 5.5 million) is characterized by relatively high residential mobility and ethnic homogeneity with low levels of social inequality³. At the same time, accents vary from local area to local area, with distinct and recognizable accents tied to distinct geographical areas (Ejstrup 2011). In tandem, these characteristics make Denmark an ideal site for testing the psychological underpinnings of ethnic markers. In many countries with more heterogeneous populations, differences in accent correlate with differences in value orientation or socioeconomic status. The homogenous nature of the Danish population implies that many such confounding variables are controlled and, by implication, any effects that we observe of ingroup accent on behavior are more likely to actually reflect an effect of accent rather than potential confounds. Furthermore, to increase the ecological validity of our test, we make use of a subtle and realistic priming approach. Ethnic identity is primed without any explicit or overt references to the ethnic identity of the speakers. Instead, we use speech recordings, which, in addition to social identity information, provide a lot of idiosyncratic information about a given speaker (e.g., traits,

³ In 2013, Denmark ranked as no. 9 (out of 187 countries) on UNDP's Human Development Index adjusted for inequality, and Denmark has a Gini coefficient of about .25 - one of the smallest in the world (see OECD 2013; UNDP 2013).

emotional states, physical dominance, and physical attractiveness; Hughes et al. 2002; Puts et al. 2006; Scherer 1972; 1986; Sell et al. 2010; Zuckerman and Driver 1989).

2.1. Participants

In total, 4,603 Danish participants (2,082 male, 2,521 female; age: $M = 37.20$, $SD = 14.74$) completed the study, which was accessed through public hyperlinks posted on various Danish websites. The percentage of the participants who had completed two years or more of education beyond high school was 63.5.

2.2. Design

The study used a mixed design, with each participant listening to four different speakers. Two of the speakers were approximated local speakers (one male and one female), and the other two speakers were non-local speakers (one male and one female). To predetermine which speakers would serve as local and non-local speakers, respectively, for a specific subject, we utilized the participant's region of origin (operationalized as the zip code in which the participant had spent the majority of his life up to 18 years of age). The particular local and non-local speakers to whom each participant listened were selected by random assignment from the sets of local and non-local speaker categories (more on this in the procedure section).

2.3. Materials and Measures

2.3.1. Independent variable: Accent as ethnic marker.

We used 30-second audio recordings of 12 Danish native speakers (six male) as speaker stimuli. The speakers came from three different Danish speech communities⁴ (i.e., four speakers from each community). The speakers were selected from a pool of standardized studio-recorded interviews with local Danish speakers recorded in 2003 (Ejstrup and Hansen 2004). All speakers were similarly aged (25-35) and belonged to the Danish middle class. From each speaker-interview, we extracted a 30-second clip, in which the speaker described what he or she saw in a cartoon drawing of children playing in a schoolyard. Naturally, the speakers do not make the same observations and consequently the same verbal descriptions, but the descriptions are within

⁴ In essence, this meant that only a fraction of the respondents would listen to locally accented speakers in the most narrow sociolectual linguistic sense. Most participants would instead listen to two evidently non-local speakers (i.e., speakers living more than 150 km from the participant's region of origin) and to two speakers with accents that approximated the accent spoken by the participant (i.e., speakers living less than 150 km from the participant's region of origin).

acceptable levels of comparability, and, importantly, based on non-scripted spontaneous speech.⁵ This type of accent sampling is standard within modern language studies (see Kristiansen 2009), and it allows speakers to make full use of their natural community-based way of speaking, unconstrained and unbiased by scripted text. As shown by several studies, having subjects read from a text results in their conforming their accents to the Standard national accent (see Giles and Bourhis 1976).

As mentioned, we used four speakers (two male), from each of three different Danish speech-communities: Copenhagen, Naestved, and Herning. Copenhagen is the capital of Denmark with a population of about 1.2 million, and it is located on the eastside of the large island Zealand. Naestved is a commercial town located on the south-side of Zealand, some 80 km south of Copenhagen. With a population of about 42,000, it is considered a regional center on the island (Kristiansen and Giles 1992), and the accent was included in the study as a potential ingroup accent for a large part of the Zealand population who did not identify with the Copenhagen accent. Herning is a commercial town located in the center of the peninsula Jutland, about 250 km west of Copenhagen, and has a population of about 47,000. This accent was included as a potential ingroup accent for a large part of the population in the center of Jutland. In order not to draw explicit attention to the speaker's accent, all speakers included in the study spoke with a non-conspicuous version of the local accents.

Given the geographical and social mobility of Danish society and the fact that new accents can be acquired through extensive exposure to them, we utilize two distinct strategies for measuring the ingroup status of a specific accent. The first privileges the idea that group identities are rigid and utilizes the subject's place of origin as the basis for the determination of whether an accent is ingroup or outgroup. The second privileges the idea that group identities are flexible and uses the subject's current living place as the basis. For each strategy, we calculate three different measures of ingroup accent (see Fig. 1). These measures of ingroup categories utilize increasingly narrow definitions of what counts as ingroup in terms of the geographical distance between the subject and the geographical area of the speaker's accent (defined as city-level, county-level, and region-level). The most narrow ingroup category (city-level) included only subjects from the same zip code as a particular speaker (up to a 20 km radius), whereas the broadest ingroup category (region-level) included subjects from a much larger radius (up to 150 km). Based on these defined ingroup categories, 919 subjects lived in the three city-level ingroups (20.0% of the total sample), 2,870

⁵ The most common verbal descriptions made by the speakers include descriptions of children playing on a seesaw, children playing with a ball, children climbing a tree, and the time indicated by a large clock.

subjects lived in the three county-level ingroups (62.4%), and 3,985 subjects lived in the three region-level ingroups (86.6%). Given that each subject listened to four different speakers, this translates into 3,676, 11,480, and 15,940 observations made in the respective ingroup categories. A breakdown of each of these observation numbers over the three speech communities is given in Figure 1.

- FIGURE 1 ABOUT HERE -

2.3.2. Independent variable: Speaker evaluations.

To control for the possibility that online participation generated responses that were not comparable to responses generated in laboratory settings, we gathered impressions of the speakers' traits and analyzed how these traits predicted variation on the dependent variables. Impressions were gathered by having subjects rate the speakers on nine items (a modified version of the speaker evaluation guidelines presented in Kristiansen 2009). The subjects were asked to rate the speakers on scales with the following end-labels: *goal-oriented vs. sluggish*, *trustworthy vs. untrustworthy*, *serious vs. indifferent*, *exciting vs. boring*, *confident vs. insecure*, *smart vs. stupid*, *nice vs. unsympathetic*, *beautiful vs. ugly*, and, finally, *dominant vs. submissive*. Ratings were measured on 7-point scales with maximum and minimum ratings labeled "To a large degree" and the middle rating labeled "Neither ... nor."

We conducted a factor analysis of the perceptions of the speakers according to these nine traits. A two-factor structure emerged from the analyses, with one factor relating to the trustworthiness of the speaker and the other factor relating to the dominance of the speaker. This factor structure is fully consistent with previous studies on impression formation from minimal cues such as facial features (Oosterhof and Todorov 2008). For each subject's judgment of each of the four speakers, we saved the factor scores from this analysis. We include these judgments in some of the analyses below.

2.3.3. Dependent variables: Hypothetical economic games.

To measure the effects of speaker accent on social behavior, we utilize three one-shot economic games. These games provide paradigmatic situations related to (a) coordination, (b) reciprocal altruism, and (c) unilateral altruism. Using a hypothetical setup, each subject played the games with each of the four speakers. Participants were instructed to imagine themselves playing

the particular games with the speakers to whom they listened as if the speakers were making simultaneous decisions. Because the games were hypothetical, participants (knowingly) were not playing with real money, and they were consequently unable to win any money for participating.

2.3.4. Coordination: Coordination Game.

To investigate whether accent influences tag-based coordination behavior, we measure participants' expectations of being able to coordinate actions with each of the speakers in a hypothetical assignment, from which they can benefit from successful exchange. The assignment was designed as a pure Coordination Game (equal preferences and symmetrical payoff), where the subject had to decide between two options (Colman 1997). The assignment entailed the following. The participant and the speaker, both being on holiday in a foreign town, were given an opportunity to receive a free 1,000 kr (\$175 U.S.) dinner, if they were able to meet up at the same restaurant at a particular time without being able to make an explicit agreement. The participant and the speaker were given two restaurants from which to choose. They could either choose "Odysseus, a restaurant in the southern part of town" or "Orfeus, a restaurant in the northern part of town." If they chose different restaurants, both would have to pay full price for their dinner. Importantly, the labels representing each of the two options contained no intended focal points for neither of the accent groups (Bacharach and Bernasconi 1997; Mehta et al. 1994; Schelling 1960). Compared to examples of real-life coordination, pure Coordination Games without clear focal points, appear abstract, and participants typically base judgments on intuition rather than reasoning (Colman 1997; 2003; Sugden 1995). After the assignment presentation, participants were asked three questions. First, they were asked to evaluate the likelihood, as they saw it, that they would be able to decide on the same restaurant as the speaker (measured on a 5-point Likert scale⁶). Second, they were asked whether they would prefer to take on the assignment or not to take the risk. Given that decisions in pure Coordination Games rely more on intuition than reason (Colman 1997; 2003; Sugden 1995), these two questions were designed to measure individuals' intuitive expectations and motivation to coordinate with the speakers. Irrespective that there is no immediate reason for successful coordination above chance level, we would expect individuals' computational mechanisms designed to regulate coordination behavior to be triggered by cues to shared knowledge. Finally, to control for the possibility that the constructed choice labels did in fact contain different focal points for different accent groups (i.e. that individuals were able to utilize shared knowledge in making their

⁶ Answer labels: 1) No, I don't think we would; 2) That would probably be completely random; 3) Well, I guess we might; 4) Yes, I'm pretty sure we would; 5) Yes, definitely.

choice), the participants were asked to choose one of the restaurants (irrespective of whether they would take on the assignment or not).

2.3.5. Cooperation: Trust Game and Dictator Game.

To assess how accent influences tag-based cooperation behavior, we first used the Trust Game. The Trust Game provides a direct measure of the expected benefits from engaging in a direct reciprocal exchange with a specific other individual (Berg et al. 1995). Participants played a round of the game as the truster with each of the four speakers. Specifically, participants were asked to decide to either entrust the particular speaker with an investment of 1,000 kr (about \$175 U.S.) or not. If the participant chose to trust a given speaker, the invested money would be tripled, and the speaker would receive 3,000 kr (about \$350 U.S.). The speaker would then be presented with the decision to either send half of the money back (1,500 kr) to the participant or to keep all the money for himself. Before making the decision to trust or not to trust, participants were asked to evaluate the likelihood, as they saw it, that the speakers would send half the money back to the participant if the participant did in fact choose to trust the speaker. This evaluation was measured on a 7-point Likert scale.⁷

Second, we used another standard economic game: the Dictator Game (Forsythe et al. 1994). In the Dictator Game, one individual is assigned the role of dictator, and another is assigned the role of recipient. The dictator is endowed with a sum of money that he must divide between himself and the recipient, as he sees fit. Hence, whereas the Trust Game mimics a dyadic cooperative situation, in which the benefits accruing one individual (the truster) depends on the decision made by another individual (the trustee), the Dictator Game mimics a unilateral cooperative situation, in which the benefits accruing one individual (the dictator) are solely determined by the individual's own decision making. Including this game as a cooperation measure enables the study to assess whether accent affect subjects' intuitive expectations related to indirect reciprocity (cf. Yamagishi et al. 1999). Each subject played a round of the Dictator Game with each of the four speakers, with the subject assigned the role of dictator. The endowments given in the respective rounds were 1,000, 200, 500, and 800 kr (about \$175, \$35, \$85, and \$140 U.S. respectfully), a variation utilized in order to avoid consistency bias. Subjects were also asked to evaluate how much money they imagined the particular speaker would send to them in the event that the roles were reversed.

⁷ Answer labels: 1) Definitely not; 2) Most likely not; 3) Probably not; 4) Neutral; 5) Probably yes; 6) Most likely yes; 7) Definitely yes.

2.4. Procedure

Subjects entered the study through a public link posted on seven Danish media websites. All but one of these websites are quite large, with 100,000 to 1 million different users per month, and they all specialize in conveying news online.⁸ This approach aimed at reaching a large sample with key demographic variables, such as geographic residency, age, educational level, and income, representative of the Danish population. Data was collected from December 2011 to February 2012. In order not to prime participants extensively with the social identities of the speakers, we used a verbal guise approach (Cooper and Fishman 1974). This approach (standard within language attitude studies) entails that introductory information makes no explicit reference to the different accents or regional origins of the given speakers. Instead, participants were informed that people typically make spontaneous firsthand impressions of other people based on their voices (i.e., their tone, speed, rhythm, intonations) and that some of these impressions can be quite accurate. Participants were then invited to participate in a test of their personal impressions of different Danish speakers. Having entered the website, participants were informed that they would be listening to four different speakers several times during the study, and they were encouraged to use a set of headphones and check audio quality before starting the audio clips. Subsequently, participants filled out demographics (age, gender, educational level, zip code born and raised, and current zip code). The entered zip code, born and raised, would then, unknowingly to participants, activate a script that assigned two local speakers (one male) and two non-local speakers (one male) to the participants.⁹ On the next four pages, participants were presented with the task of evaluating the traits of the four assigned speakers. The evaluation task acted to familiarize the participant with the assigned speakers. In each task, a speaker was represented with a small gender-matched face silhouette with a small speaker icon next to it (see Figure A1 in the appendix). The audio clip played automatically once a new webpage had loaded, and, in addition, the participant was able to replay the clip whenever he wanted to by clicking on the speaker icon. After evaluating each

⁸ Two of the sites (www.bt.dk and www.politiken.dk) convey general news (both sites have nearly one million unique users a month each); one site (www.tdc.dk) conveys IT-support and news (700,000 users); one site (www.videnskab.dk) conveys science news (100,000 users); two sites (www.oestrogen.dk and www.femina.dk) specialize in fashion and lifestyles for women (100,000 users each); and one smaller site (www.studerende.au.dk) conveys news for students at the local university (<10,000 users).

⁹ More specifically, if participants were born and raised in Jutland, they were assigned two Herning speakers (ingroup) and two Copenhagen speakers (outgroup). Assignments of the particular speakers from each speaker category were random. Similarly, if participants were born and raised in the larger Copenhagen region, they were assigned two Copenhagen speakers and two Herning speakers (outgroup). Finally, if participants were born and raised in Zealand but outside the larger Copenhagen region, they were assigned two Naestved speakers (ingroup) and two Herning speakers (outgroup). Participants born and raised outside the study regions ($N = 618$) could not be assigned local speakers. They were assigned two Herning speakers and two Copenhagen speakers.

speaker, the participant played a Dictator Game with each of the four assigned speakers, then the participant played a Trust Game, with each of the four assigned speakers, and finally the participant played a Coordination Game with each of the four assigned speakers. Within each block of the four tasks (i.e. evaluations, Dictator Games, Trust Games, Coordination Games) the order of ingroup vs. outgroup speakers as well as gender of speakers was randomized. Finally, participants answered some additional background information and were thanked for their participation.

3. Results

3.1. Analyses

The initial step in the analyses was to restructure the data. Each of the 4,603 subjects played the games against four speakers and, hence, for each subject and game, we have four separate observations. We restructured the data such that the basic units of analysis are these separate observations. This provides us with a total of 18,412 observations and, hence, a dataset with a tremendous amount of statistical power. To correct for errors for within-subject autocorrelation, we report cluster robust standard errors with the subject ID as the cluster variable.

As described above, we created six different measures of ingroup accent. To maximize the possibilities of causal identification (and, hence, avoid omitted variable bias), we control each of these measures for two potential confounds. First, we control all analyses for geographical region of origin, which was used as a basis for the random assignment of specific speakers (see above). Second, we control all analyses for the geographical location measure that was used to construct the measure of ingroup accent in the specific analyses. So, for example, when analyzing the effects of whether the subject current lives in the city associated with the accent or not, we control for the specific city in which the subjects live. The reason for both controls is that we need to take into account that some geographical areas are naturally overrepresented in the ingroup accent measure (see Fig. 1). For example, much more people live in the capital of Denmark than in the other two cities, and, hence, people living in the capital will constitute a larger share of those perceiving the speakers as ingroup members on the basis of their accent. Hence, without these controls, our measure of ingroup accent will be confounded with individual differences relating to living place and/or place of origin. Through our control strategy, however, we can partial out all explained variance that is related to living in and being raised in specific areas and isolate the specific effect of living in the *same* area as the speaker.

Main analyses are performed using either binary logistic regression or OLS regression analysis. Dependent variables in the Trust Game and one of the variables in Coordination Game (whether the subject chooses to play the game or not) are binary variables and, hence, need to be analyzed using logistical regression. As an effect size measure, we here report odds ratios (θ). For the other two dependent variables, behavior in the Dictator Game and perceived likelihood of successful coordination, we use OLS regression. The reported effect sizes are unstandardized regression coefficients on z-scored dependent variables (i.e., showing the difference in standard deviations on the variable between those for whom the accent signifies an ingroup and outgroup member, respectively).¹⁰

3.2. Testing Prediction 1: Does Ingroup Accent Facilitate Cooperation?

We begin by investigating the theory that ethnic markers such as ingroup accent facilitate tag-based cooperation; that is, that individuals preferentially cooperate with those whose accent signals a shared ingroup. As described above, we utilize two distinct strategies for measuring the ingroup status of a specific accent. The first privileges the idea that group identities are rigid and utilizes the subject's place of origin as the basis for the determination of whether an accent is ingroup or outgroup. The second privileges the idea that group identities are flexible and uses the subject's current living place as the basis. For each strategy, we calculate three different measures of ingroup accent. These measures utilize increasingly narrow definitions of what counts as an ingroup in terms of the geographical distance between the subject and the prototypical geographical area of the speaker's accent.

Overall, we find that 51 percent of the subjects chose to trust the speaker in the Trust Game and that on average subjects awarded 44 % of their endowment to the speaker in the Dictator Game. Figure 2 shows how these decisions are affected by whether the speaker exhibited an ingroup or outgroup accent. As evident, we find no effect of ingroup accent on cooperative motivations in the Trust Game or the Dictator Game; not even a single coefficient is significantly different from zero. Neither if the accent signals a shared place of origin nor if it signals a shared current living place do we find that ingroup accent facilitates cooperation in either of the two games. Investments in the Trust Game and offers in the Dictator Game are unaffected by the accent of the speaker.

- FIGURE 2 ABOUT HERE -

¹⁰ We cannot report standardized regression coefficients, i.e., betas, as we also use cluster robust standard errors.

Given that these analyses utilize more than 18,000 observations, these null effects are clearly not a matter of statistical power. Yet, another possibility resides. Although we have taken extreme care in crafting the descriptions of the games in an accessible form, the subjects in our study were not average college undergraduates but a diverse group from all parts of society, most of whom likely have never heard about game theory and economic experiments. Hence, it could be the case that people simply did not understand the games, which could explain the observed lack of effects. To test for this possibility, we can investigate how other factors influence game behavior. In particular, it is relevant to note that the argument that ingroup accent facilitates tag-based cooperation is an extension of reciprocity-based models of the evolution of cooperation (such as Axelrod and Hamilton 1981; Trivers 1971). Consistent with these broader models, previous studies have found that game behavior in the Trust Game and the Dictator Game is driven by a logic of conditional and reciprocal cooperation (see Mifune et al. 2010; Tanis and Postmes 2005). Hence, if the subjects in the current study understand the dynamics of the games, we should find that they behave on the basis of this very logic. That is, subjects should tend to make cooperative moves to the extent that they perceive the speaker to be cooperatively motivated.

We can perform two tests on whether subjects do indeed act according to a broader logic of reciprocity. First, initial tests can be performed by looking at whether there is a correlation between game behavior and perceptions of whether the speaker would send back money and share money in the Trust and Dictator Games, respectively. In each game, the relevant measure correlates significantly and strongly with game behavior (Trust Game: $r = .61, p < .001$; Dictator Game: $r = .53, p < .001$). Second, we can look at how more general trait perceptions influence game behavior. As described above, we assessed each subject's perception of the trustworthiness and the dominance of each speaker. To the extent subjects follow a conditional cooperation strategy, they should tie their behavior closely to these perceptions and, in particular, to their perception of the trustworthiness of the speaker (as an index of the speaker's likelihood of reciprocation). In line with this, we find that behavior in the Trust Game correlates positively with perceptions of dominance ($r = .11, p < .001$) and, in particular, with perceptions of trustworthiness ($r = .30, p < .001$). For the Dictator Game, we find the same pattern ($r_{\text{dominance}} = .14, p < .001$; $r_{\text{trustworthiness}} = .24, p < .001$).¹¹ Overall, these findings show that subjects tend to make cooperative moves in these games when they perceive the speaker to be cooperatively motivated. Hence, providing confidence that the

¹¹ Additional analyses show that in both games, the effect of perceptions of trustworthiness is significantly stronger than the effect of dominance ($p < .001$).

subjects understand the setup of the games, their behavior conforms to a logic of reciprocal cooperation. As evidenced by Figure 2, the speaker's accent is simply not factored into this logic.

3.3. Testing Prediction 2: Does Ingroup Accent Facilitate Coordination?

We now turn to the second theory of how the accent of others influences social behavior. According to this theory, individuals use ethnic markers such as accent to assess the likelihood of attaining coordination with others. Because shared ethnic markers reveal that the self and other share social norms and cultural worldviews, people should be more likely to engage in tasks requiring a synchrony of knowledge, values, and idiosyncrasies with individuals with ingroup ethnic markers.

To test this prediction, we have two measures available related to coordination: first, whether the subject chose to play a coordination game with the speaker (involving potential gains and losses) or chose to shun the game; and second, how likely the subject perceived a successful coordination outcome to be with the speaker. Overall, we find that 48 percent of the subjects decided to engage in the Coordination Game with the speaker and that 20 percent of the subjects perceived successful coordination as "pretty certain" or "definite". Figure 3 show how these decisions are modulated by the different operationalizations of what constitutes the subject's ingroup. The first column of Figure 3 shows how being raised in the area of the speaker (defined as same region, county and city, respectively) influences coordination behavior and perceptions. Overall, we find only weak and no consistent evidence that ingroup accent thus defined facilitates coordination. There are no significant effects on coordination behavior (i.e., choosing to play the Coordination Game). With regards to perceived likelihood of coordination success, there are, however, small but significant effects of being raised in the overall geographical region associated with the accent ($b = .03, p < .05$), and small significant effects of being raised in the county associated with the accent ($b = .05, p < .05$). However, being raised in the specific city associated with the accent has no effect ($b = -.01, p > .05$).

- FIGURE 3 ABOUT HERE -

However, turning to the second column of Figure 3, we find significant, consistent, and non-trivial evidence that *currently* living in or near areas associated with a speaker's accent do facilitate coordination. Overall, there is a consistent effect such that the closer the subject lives to the specific

area associated with the accent of the speaker, the more the subject tends to choose to play the Coordination Game with the speaker and the more likely the subject perceives coordination success to be. The only exception to this pattern is that living in the broader county associated with the accent of a specific speaker does not have a significant effect on the choice to play the Coordination Game ($\theta = 1.06, p > .05$). In contrast, living in the overall geographical region ($\theta = 1.08, p < .05$) and, particularly, living in the specific city ($\theta = 1.25, p < .001$) associated with the speaker's accent increase the likelihood of choosing to play the Coordination Game. For the perceived likelihood of successfully coordinating with the speaker, we find a consistently increasing effect of the speaker's accent as geographical distance decreases between the subject's current living place and the area associated with the accent ($b_{\text{region}} = .06, p < .001$; $b_{\text{county}} = .08, p < .001$; $b_{\text{city}} = .17, p < .001$).¹² In sum, these results show that (1) individuals use accent cues to regulate coordination behavior such that they engage more in coordination tasks when the accent of an individual is associated with an ingroup, (2) individuals represent their ingroup on the basis of where they live currently, and (3) substantially important effects only emerge for individuals living in the specific city associated with the accent.

3.4. A Focused Re-Test: Effects of Ingroup Accent at the City Level

The above analyses summarize massive amounts of information and, as consequence, it can be difficult to gauge the substantial size and character of the effects of ingroup accent. In this section, we conduct a more focused - and stronger test - to demonstrate the effects. Specifically, we focus on the 919 subjects that live in one of the three focal cities and 489 subjects that were raised in one of the cities. Among these subjects, the design approximates a true experimental design as all of them have been exposed to a randomly selected set of *both* local (i.e., from the same city) and non-local speakers (i.e., from one of the other two cities). We continue to analyze the data at the level of decisions rather than subjects, yielding 3,676 and 1,956 observations, respectively. Importantly, the focused samples allow us to bypass concerns about the inclusion of control variables and, instead, estimate effects using hierarchical linear modeling including fixed effects for each subject. Accordingly, the estimated effects are within-subject effects, showing the average

¹² Analyses of the actual choices in the Coordination Game, however, show that the assumption that it is easier to coordinate with ingroup members is false. In the overall sample, the restaurant "Odysseus" constitutes a focal point, with 68% of the sample choosing this restaurant. However, individuals living in the same city as the speaker are slightly more uncoordinated in their choices, with 65% choosing "Odysseus" This difference is marginally significant ($p < .10$). The disparity between assumptions and actual coordination success underlines that this bias is driven by intuition rather than reason.

effect on cooperation and coordination decisions when the target changes from having an outgroup to an ingroup accent. In terms of internal validity, this is a superior analytical strategy. Because this focused analysis is less information dense, we report predicted values (rather than regression coefficients) from the fixed effects models, which is superior in terms of gauging effect sizes.

- FIGURE 4 ABOUT HERE -

Figure 4 shows the predicted values for how city of origin and current city influence game behavior and perceptions across both our cooperation and coordination tasks. This stronger test fully replicates the findings above. There are systematic positive and significant effects of currently living in the city associated with the speaker's accent on the two coordination measures such that subjects are more willing to engage in a coordination task and perceive the likelihood of successful coordination as higher when the speaker is associated with the current city. When speaker accent is associated with the subject's current city, 55 percent engage in the coordination compared to only 47 percent when speaker accent is associated with a different city. Similarly, when the speaker accent is associated with the subject's current city, people perceive the likelihood of success as .40 on a 0-1 scale compared to .35 when speaker accent is associated with a different city. Given the subtle nature of the ingroup cue, these effects cannot be considered trivial. In contrast - confirming the empirical patterns established above - the tendency to cooperatively transfer money in both the Trust Game and in the Dictator Game is constant across the conditions with an ingroup and outgroup speaker. Also, again in line with the above effects, all differences across conditions are associated with current city and not the city of origin. In sum, using a stronger, more directly experimental test, we are able to confirm all key conclusions from the above analyses and demonstrate the non-triviality of the effects in a more accessible way.

4. Discussion

Utilizing a verbal guise approach in a large socially diverse sample in Denmark, the present study found that sharing accent did not facilitate cooperation - neither unilateral altruism in a hypothetical Dictator Game nor interpersonal trust in a hypothetical Trust Game. Amongst subjects presently living in proximity to the speakers, the results did, however, reveal that sharing accent both facilitated expectations about coordinating successfully with the speakers in a hypothetical

Coordination Game and facilitated subjects' willingness to attempt to coordinate with the speakers. In accordance with previous studies, these findings suggest that individuals regulate social exchange behavior on the basis of ethnic markers such as accent, but the study goes on to suggest that the specific function of this effect is more tied to achieving coordination rather than cooperation. Hence, the results suggest natural selection favored cognitive mechanisms that monitor certain environmental cues that predict coordination success rates with unfamiliar individuals.

The effect of sharing an accent on coordination expectations adds to recent comparative and neurocognitive research that suggests that the capacities for flexible interpersonal coordination rely on complex human-specific, socio-cognitive abilities, such as theory-of-mind, neural mirroring, shared intentionality, motivations for sharing emotional states, private experiences, and activities with other individuals (see Guionnet et al. 2012; Hari and Kujala 2009; Tomasello et al. 2005). Many different species do manifest remarkable coordinated action sequences (e.g., cooperative hunting in lions and killer whales, work-specialization in ants, formation-swimming and antipredator maneuvers in fish species; see Fish, 1995; Kay and Rissing 2005; Lopez and Lopez 1985; Marras et al. 2011; Stander 1992), but, unlike human coordination, these coordinated actions are typically relatively non-flexible and confined to solving a particular adaptive problem. Just as in other species, human capacities for coordinating actions confer important group-level advantages in the form of large game hunting, intergroup warring, and work-specialization. However, in addition to these benefits, the human ability to effectively synchronize perceptions, judgments, and expectations is thought to have been a key driver in enabling human groups to form social norms and cultural institutions, which could, in turn, have enabled cultural selection processes (Boyd and Richerson 2005). Relatedly, DeScioli and Kurzban (2013) have suggested that human capacities for efficiently coordinating decisions and actions around social norms might have been a key evolutionary driver of unique human moral capacities (i.e., moral impartiality and third-party interventions) as well as human social organization (i.e., relative egalitarianism). In tying moral condemnation and punitive sanctions to transgressions of particular social norms, rather than to particular individuals or groups of individuals, our prehistoric ancestors were able to engage in efficient collective third-party interventions. Coordination is key to efficiency, because it both reduces the costs of sanctioning for individual third parties (less risk of retaliation if third parties sanction together) and it reduces the costs to the group (less large-scale symmetric contests if individuals sanction actions rather than individuals; see Boehm 2000; Boyd et al. 2010). Given that capacities for efficient coordination could have provided our ancestors with substantial evolutionary

advantages and that attempted, but unsuccessful, coordination could be quite costly (e.g., siding with the wrong individuals in intra-group conflicts, attacking rival groups at the wrong time and place), it makes sense that natural selection favored cognitive mechanisms that monitor environmental cues to predict coordination success rates with unfamiliar individuals.

That substantial ingroup coordination effects are confined to subjects presently living in the specific city associated with a given accent suggests that identification with a particular Danish accent is restricted to a relatively narrow geographical area (less than a 20 km radius), which is in accordance with recent sociolinguistic perception research conducted in Denmark (Ejstrup 2011). Moreover, it shows that subjects identify more with the sociolinguistic group among which they presently live rather than with the sociolinguistic group among which they were born and raised. This finding corroborates previous studies that show that humans use phenotype cues to categorize individuals in social groups in a flexible rather than rigid manner (Castillo and Petrie 2010; Kurzban et al. 2001; Pietraszewski, Cosmides and Tooby 2014). This flexibility is adaptive because it tunes individuals' cognitive and motivational mechanisms to their present context by committing to appropriate social norms shared with the group with which they presently live. Furthermore, a flexible social categorization detection system facilitates exogamy, which is a common practice amongst human cultures and tribes (Chapias 2010; Thornhill 1993).

The lack of support for hypothesis 1, i.e., that shared ethnic markers such as accent facilitate interpersonal cooperation, also needs attention. As previously argued, one theoretical explanation for this result is that tag-based cooperation systems are too vulnerable to exploitation by cheaters and that even the unique hard-to-fake properties of human accents are insufficient to protect systems of tag-based cooperation from invasion by cheaters. Nonetheless, the results presented here remain surprising given the extensive body of studies reporting ingroup favoritism, in particular those using the minimal group approach.

One explanation for the divergence between the present and previous findings could be that studies finding ingroup favoritism in minimal groups maximize the salience of a particular social identity of other individuals (Leyens et al. 1994). This might have two consequences. First, because humans base their trustworthiness assessments of others on multiple cues, the lack of trustworthiness cues offered to subjects in minimal group scenarios might increase the relative emphasis put on a particular social identity to a degree that might not generalize to more real-life-like scenarios. In real-life encounters one would be given multiple cues to others' trustworthiness as well as be attentive to multiple ways of sorting others' in to context-relevant social categories (Tanis

and Postmes 2005; see also Tooby and Cosmides 2010). Second, maximizing the salience of social identity might facilitate reputation-driven cooperation (rather than trust-driven cooperation), just as subtle social cues in general has been found to do (Bateson et al. 2006; Hoffman et al. 1996; Oda et al. 2011). As argued by Yamagishi and Mifune (2008), a heightened salience of social identity activates and boosts a group heuristic in humans, a heuristic that is designed to protect and increase personal reputation within the group (i.e., by cooperating and being generous toward ingroup members). The robust results generated by Yamagishi's and others' experiments suggest that individuals do not necessarily direct more altruism and cooperative efforts toward similarly marked individuals because they increasingly trust or favor them, but because want to avoid being perceived as selfish, unfair, or a free-rider (Mifune et al. 2010; Yamagishi et al. 1999; Yamagishi and Mifune 2008; see also Koopmans and Rebers 2009). As argued by Yamagishi et al. (1999), the relatively high levels of altruism and cooperation commonly observed within human groups are enabled by generalized social exchange systems governed by indirect reciprocity rather than by direct reciprocity. In conclusion, whereas a particular salient social category could facilitate ingroup cooperation by different proximate mechanisms, it is less evident whether non-salient social categories would. With the verbal guise approach utilized in the present study (i.e. usage of natural spontaneous speech, no explicit references made to speakers' accent or region of origin), we argue that ethnolinguistic identity is primed in a more ecologically valid way and, in particular, in a way that does not maximize the salience of a speaker's particular social identity.

Another explanation could be that there is something special about accent relative to other ethnic or group markers. Recent findings suggest that ethnolinguistic markers are used as a privileged dimension of social categorization and processed and represented differently than other group markers (i.e. race, clothing, etc. - Pietraszewski and Schwartz 2014a; 2014b; see also Moya 2013). Pietraszewski and Schwartz' results show that subjects continue to categorize target individuals by accent when the targets' respective accents are crossed with orthogonal group membership cues. This stands in contrast to other researched powerful group markers, such as race, which are abandoned as base for social categorization when crossed with orthogonal group markers (Kurzban et al. 2001; Pietraszewski, Cosmides and Tooby 2014). According to interpretations offered in these recent studies, ethnolinguistic cues might specifically be utilized for purposes that are more aligned with coordination goals than with cooperation goals. In particular, Pietraszewski and Schwartz (2014a) suggest that ethnolinguistic markers are utilized by a computational mechanism that evolved to predict the beliefs and behavioral repertoires of unfamiliar individuals

(important for coordination), rather than to predict their coalitional affiliations (see also Moya 2013). Given the fission/fusion dynamics of prehistoric human groups as well as prevalent exogamy practices, ethnolinguistic markers were not particularly well suited for predicting coalitional relationships (Pietraszewski and Schwartz 2014a). Social coalitions are far more dynamic, intertwined, and context-dependent than static linguistic group boundaries (Tooby and Cosmides 2010).

Finally, as cooperative exchanges often entail degrees of coordination (e.g., social conventions regarding how and when to cooperate or reciprocate), certain manifestations of ingroup favoritism might be derived from ingroup coordination effects. Importantly, the proximate reason for this bias is not based on cost-benefit differentials related to trust, but on cost-benefit differentials related to coordination efforts. The coexistence of group-based coordination and cooperation effects is supported by computer simulations run by Efferson et al. (2008). Here, they find that coordination *and* cooperation between similarly marked individuals typically survive as an evolutionary strategy, implying the coevolution of both functions (see also Bowles and Gintis 2004; Smith 2010).

4.1. Limitations and Implications for Further Research

A significant limitation of the present study is the use of hypothetical incentives in the games. Some studies have found that individuals are somewhat more prosocial and trusting with hypothetical incentives as compared with actual incentives (e.g. Forsythe et al. 1994; see Amir et al. 2012 for mixed findings), but other studies do not find significant differences (e.g. Ben-Ner et al. 2008; Ben-Ner et al. 2009; Gillis and Hettler 2007). Nonetheless, it would be important for future studies to replicate the findings using real incentives. A second limitation relates to the used coordination measures. The study focused on investigating implicit inferences regarding interpersonal coordination (i.e. estimation of coordination success and willingness to attempt to coordinate), but it would also be important for future studies to measure actual coordination decisions. Such measures could include the Stag Hunt game, but they could also include measures where subjects' particular and shared beliefs and social norms could actually be utilized in coordination tasks. Another limitation of the study is that we do not know whether subjects' different accent-based stereotypes might have influenced their decisions in the games. Future studies should include variables that measure such stereotypes on comparable dimensions, because existing stereotypes might act as a moderator in relation to social identity effects on evaluations and

behavior. An additional limitation of the study relates to the decision to only expose subjects to speakers with mild versions of the three different accents. As argued already, this decision was made in order not to explicitly prime subjects with the social identity of the speakers. However, as argued by sociolinguists, accents can end up not being “local enough” to some individuals in order to be subconsciously recognized as local speakers (Kristiansen and Giles 1992, p.175). Including increasingly local accents should, in theory, increase the observed effects of shared accents on interpersonal coordination and perhaps even facilitate interpersonal cooperation as well. Other parts of the methodology utilized in the study are novel, and, as such, it could benefit from several tweaks. One limitation is that the study was conducted over the Internet, which meant that we had no control over the acoustic environment in which subjects listened to the audio recordings. Some might have used high-quality headphones, while others might have used poor-quality PC speakers. The online sampling approach is also novel for this type of research. Given that we found significant effects of shared accent only in the city-level ingroups, future studies should focus on recruiting subjects from smaller geographical areas. In extension, it would be interesting to investigate whether subjects living in peripheral border regions within a well-defined ethnic category manifest different levels of intergroup bias in comparison to subjects living in centered regions. As argued by several researchers, intergroup categorization and intergroup discrimination should be more pronounced in border regions (McElreath et al. 2003).

Conflict of interest

The authors declare that they have no conflict of interest.

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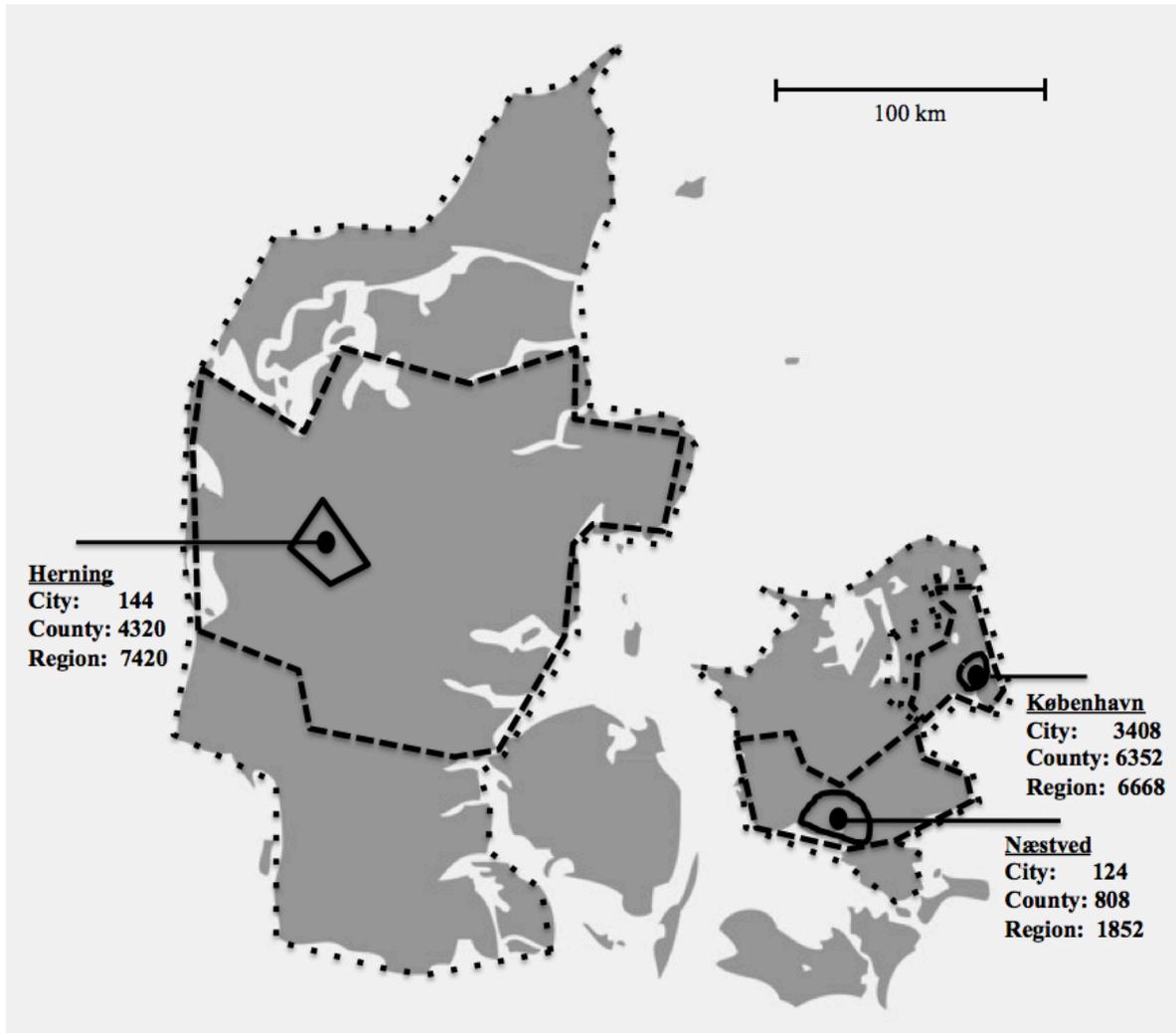


Fig. 1. Geographical location of the three accents and demarcations of ingroup categories. Numbers designate total observations for each ingroup category level.

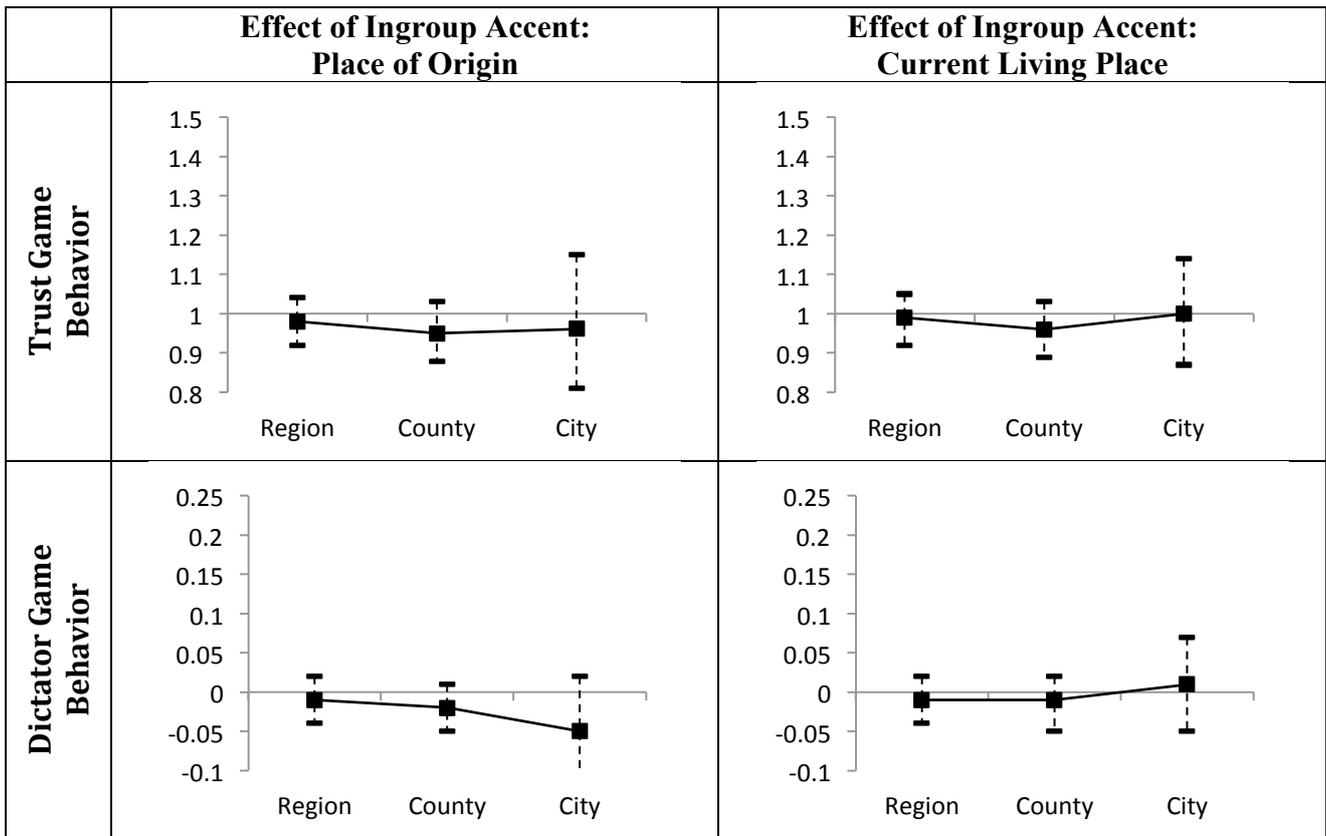


Fig. 2. Effect of Ingroup Accent on Cooperation Behavior.

Notes. N=18,412. Y-axes report effect sizes estimated from Tables A1 and A2 in the appendix. For ‘Trust Game Behavior’ effect sizes are odds ratios from logistic regression models. For ‘Dictator Game Behavior’ effect sizes are unstandardized OLS regression coefficients for a z-scored dependent variable. Effect sizes controlled for living place (see methods discussion). Error bars are 95 % confidence intervals calculated on the basis of cluster robust standard errors with subject ID as cluster variable.

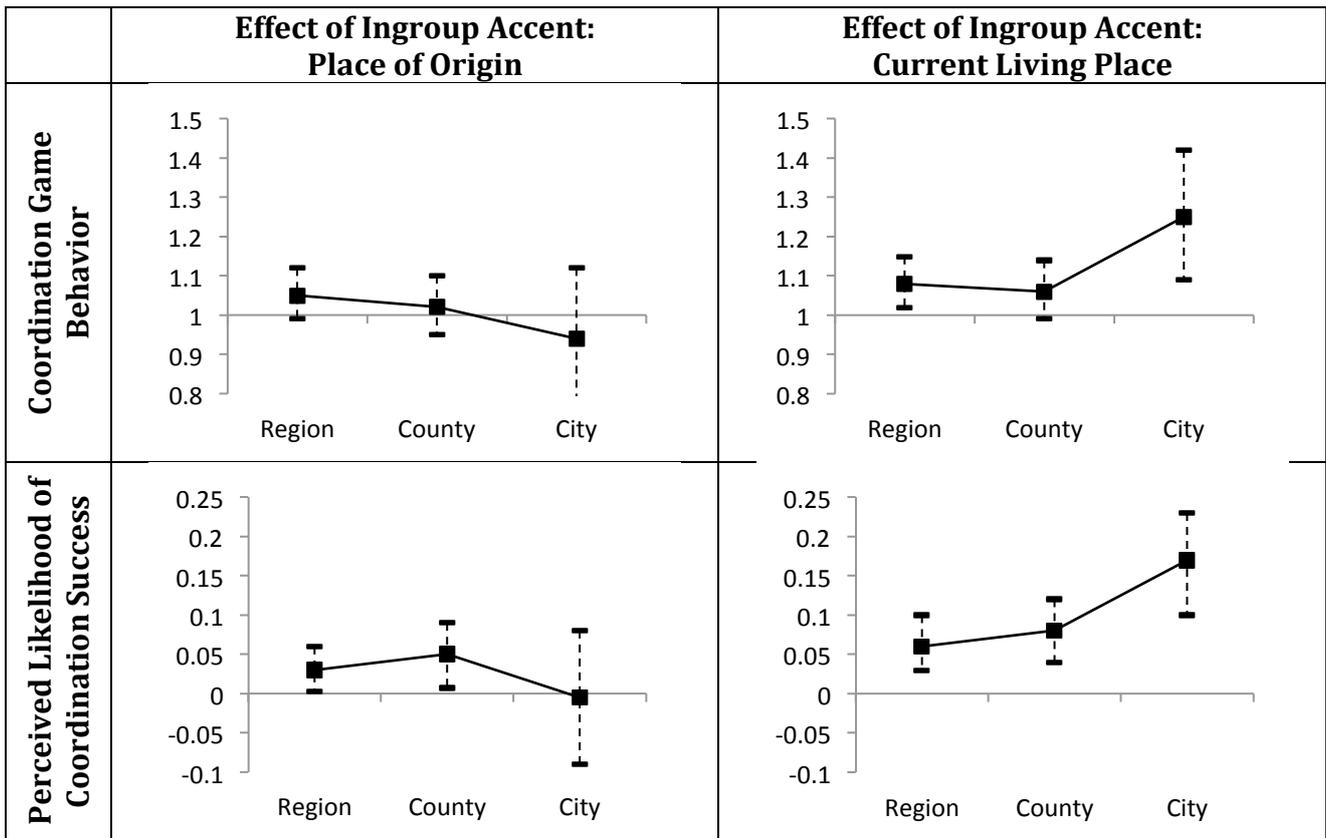


Fig. 3. Effect of Ingroup Accent on Coordination Behavior.

Notes. N=18,412. Y-axes report effect sizes estimated from Tables A3 and A4 in the appendix. For ‘Coordination Game Behavior’ effect sizes are odds ratios from logistic regression models. For ‘Perceived Likelihood of Coordination Success’ effect sizes are unstandardized OLS regression coefficients for a z-scored dependent variable. Effect sizes controlled for living place (see methods discussion). Error bars are 95 % confidence intervals calculated on the basis of cluster robust standard errors with subject ID as cluster variable.

Running head: INGROUP ACCENT FACILITATES COORDINATION, NOT COOPERATION

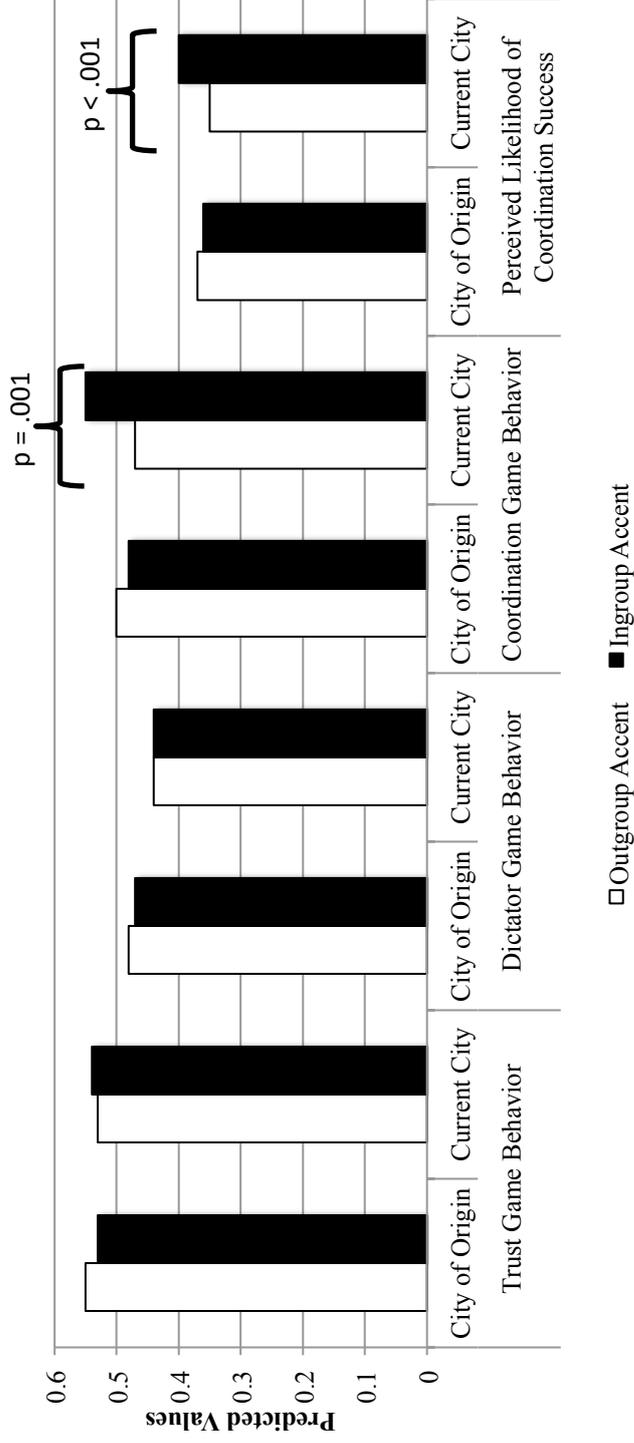


Fig. 4. Effect of City-Level Ingroup Accent on Cooperation and Coordination Behavior.

Notes. N=1460-2792. The Y-axis reports predicted values estimated from Table A5 in the appendix. All models are multilevel models with fixed effects for subject. For ‘Trust Game Behavior’ predicted values are the percentage of subjects choosing to send their endowment to the speaker. For ‘Dictator Game Behavior’ predicted values are the mean percentage of the endowment sent to the speaker. For ‘Coordination Game Behavior’ predicted values are the percentage of subjects choosing to engage in the coordination task with the speaker. For ‘Perceived Likelihood of Coordination Success’ predicted values are mean perceived likelihood measured on a 0-1 scale