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Healthcare chauvinism during the COVID-19 pandemic

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ABSTRACT
Social science research has produced evidence of welfare chauvinism whereby citizens turn against social policies that disproportionately benefit immigrants and their descendants. Some policymakers advocate welfare chauvinism as a means to incentivize fast labour market integration and assimilation into the mainstream more generally. These contested arguments about integration incentives can hardly be extended to the case of hospital treatment of an acute COVID-19 infection. On that premise we conducted a pre-registered online survey experiment among a representative sample of the Danish population about healthcare chauvinism against recent immigrants and Muslim minorities during the peak of the COVID-19 pandemic of spring 2020. Our results show no evidence of blatant racism-driven healthcare chauvinism against acute COVID-19 patients with a Muslim name who were born in Denmark. However, we do find evidence of healthcare chauvinism against patients with a Danish/Nordic name who immigrated to Denmark only a year ago. Moreover, healthcare chauvinism against recently-immigrated COVID-19 patients doubles in strength if they have a Muslim name. Our findings thus suggest that there is general reciprocity-motivated welfare chauvinism against recent immigrants who have not contributed to the welfare state for long and that racism against Muslims strongly catalyses this form of welfare chauvinism.

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Introduction

Welfare states protect the health and wellbeing of their citizens. To do so, they depend on their citizens’ mutual trust and solidarity which build the foundation for the economic redistribution that finances any welfare state. Strong welfare states backed by broad public support are a constitutive element of many European and especially of Scandinavian countries. However in a canonical monography, Alesina and Glaeser (2004) predicted that European societies would face difficulties in maintaining their universal welfare systems alongside continuing immigration due to the majority population’s unwillingness to share welfare benefits with immigrants and their descendants. The basic idea is not new. Indeed, Karl Marx (Marx and Engels 1975 [1870]) already noted how the antagonism between English and Irish workers inhibited socialist mobilisation...
at the end of the nineteenth century, and Lipset and Marks (2000) refer to white worker’s racism to explain a similar lack of mobilisation in the US.

However, rather than a general decline in the provision of welfare, others have pointed to the possibility of so-called ‘welfare chauvinism’ whereby majority members remain supportive of a strong welfare state for themselves but turn against social policies that they regard as disproportionately benefitting ethnic minorities (Gilens 2000; Fox 2010). Some policymakers argue in favour of welfare chauvinism as a measure to incentivize fast labour market integration and general assimilation into the mainstream (Daguerre 2007) – although recent social science research on welfare cuts for refugees in Denmark rather suggests the exact opposite effects (Andersen, Dustmann, and Landersø 2019; Jørgensen 2020).

Whether reduced welfare benefits are an incentive to integrate or not, we maintain that similar arguments are not plausible for the case of hospital treatment of acute COVID-19 during the pandemic of 2020. That is, non-treatment of acute COVID-19 can hardly be regarded as an incentive for integration and thus lends itself as a perfect case to provide evidence of healthcare chauvinism proper: the unwillingness to share the benefits of public healthcare with immigrants and their descendants. As we elaborate in more detail below, such unwillingness can be rooted in different motives why immigrants and descendants are less deserving of public healthcare. One reason might be a perceived lack of reciprocity whereby new arrivals who have had little time to economically contribute to the welfare state are regarded as less deserving. Another prominent reason is racism against ethnic minorities.

To test for healthcare chauvinism and whether reciprocity, racism, or a combination of the two drive it, we conducted a pre-registered online survey experiment among a representative sample of 1,208 persons living in Denmark during the peak of the COVID-19 pandemic in April 2020. Using a full factorial vignette design, respondents were asked to evaluate how much an acute COVID-19 patient deserves hospitalisation. To test reciprocity vis-à-vis racism, our experiment randomised whether the fictional patient was born in Denmark or immigrated within the past year and whether the patient had a Danish/Nordic or a Muslim name. We also varied whether the patient was 59 or 83 years old because age is widely known to increase the severity and potential fatality of a COVID-19 infection.

According to our results, the people of Denmark regard acute COVID-19 patients who have immigrated within the past year and have a Muslim name as least deserving of hospital treatment followed by patients with a Danish/Nordic name who immigrated only a year ago. Yet in contradiction to simple blatant form of racism, we do not observe healthcare chauvinism against patients with a Muslim name who were born in Denmark. Our findings thus speak in favour of a theory of general reciprocity-motivated welfare chauvinism that may be catalysed by racism.

**Theoretical background: welfare chauvinism**

The term ‘welfare chauvinism’ describes the antipathy of ethnic majority members to share the benefits of a welfare state with immigrants and their descendants (Andersen and Bjørklund 1990; Kitschelt and McGann 1997). In recent years, European scholars have paid growing attention to the phenomenon, because of expectations that citizens
will increasingly turn towards welfare chauvinism as response to continuing immigration, with right-wing parties mobilising on the topic (Banting 2010; Keskinen, Norocel, and Jørgensen 2016). Public opinion surveys indeed demonstrate that substantial shares of the European population support welfare chauvinism (Bay and Pedersen 2006; van Oorschot 2006; Reesksens and van Oorscht 2012; Cappelen and Midtbø 2016; Ford and Kootstra 2017) and do not support both continued immigration and universal welfare (Kulin, Eger, and Hjerm 2016). Contra often-voiced fears that these results render immigration and universal welfare as incompatible, higher population shares of immigrants and their descendants do not seem to drive welfare chauvinistic attitudes (Mewes and Mau 2012; Reesksens and van Oorscht 2012; Cappelen and Peters 2018). Nevertheless, many welfare states have restricted access to benefits for immigrants and especially refugees over the past decades, among them Canada, Germany, Finland, France, the Netherlands, or Switzerland (OECD 2018; Andersen, Dustmann, and Landersø 2019).

Denmark, the country we study in this article, might be regarded as another case in point. Denmark is traditionally seen as an example of what Esping-Andersen (1990) termed the social-democratic (i.e. Nordic) welfare regime. Of course, no two countries are the same and so grouping Norway, Sweden, Finland, and Denmark together is inevitably a simplification. Yet, it is a simplification that attempts to highlight essential similarities in how the provision of welfare is organised in the Nordic countries: a strongly intervening state regulates its labour market via public bargaining between unions and employer associations, provides universal and generous welfare not only to the poor but also to members of the middle class, and finances this regime via comparatively high redistributive taxes and a programmatic emphasis on full employment – among all genders; high levels of social trust and solidarity are frequently added to the mix as a social lubricant that helps to explain why citizens are willing to pay the high taxes that finance the costly welfare provisions (Greve 2004; Veggeland 2016; Schröder 2019).

Yet, by 2001 Denmark spent 0.83% of its GDP and 3.4% of its total public spending on welfare transfers to non-EU immigrants (Matthiessen 2009). In response, welfare chauvinism made its way into Danish public opinion and politics (Bay, Finseraas, and Pedersen 2013; Jørgensen and Thomsen 2016), contesting some of the essential characteristics of its Nordic welfare regime: universal welfare backed especially by social democrats. In 2002, the right-leaning government of the time cut welfare benefits specifically for refugees and recent immigrants with the declared goal to incentivize fast employment and self-sufficiency. And while these reforms were revoked in 2012, a new right-leaning government reinstated them only three years later when millions of refugees sought rescue throughout Europe (Andersen, Dustmann, and Landersø 2019). In 2018, the same government introduced the ‘Ghetto Pakken’, which entails welfare sanctions for anyone moving into a housing block designated as ‘Ghetto’ (Silver and Danielowski 2019). This policy may be regarded as welfare chauvinistic, in so far as it ties welfare sanctions to a benefit claimant’s place of residence, which in turn is defined as ‘Ghetto’ by, among other things, its share of non-EU immigrant residents. Beyond the right-wing, the Danish Social Democratic Party (SD) partly owes its recent electoral success to their tightened stance on immigration and asylum, too (Hjorth and Larsen 2019). In fact, the SD’s party manifesto somewhat paraphrases the idea of the incompatibility of universal welfare and open borders: ‘Our policy must balance the desire to help people in need
against the social cohesion and welfare that characterise Denmark. We cannot take in more foreigners than we can integrate effectively’ (Socialdemokratiet 2017:21).

Muslim immigrants and refugees stand centre stage in the discussions surrounding these policies (Nielsen 2012; Rytter and Pedersen 2014). Contested debates over Muslim immigrants are a general European trend (Helbling 2013). Also in Denmark, with its strong emphasis on free speech and gender equality, the conflict following the Muhammad caricatures of Jyllands-Posten (Sniderman et al. 2014), the low employment rates among Muslim women (Caswell, Kleif, and Jensen 2008), or recent reports of Sharia courts discriminating women (Birk 2020) have continuously fuelled a controversial discourse – the pinnacle of which may be seen in the burning of Korans by extremist Rasmus Paludan and members of his party ‘Stram Kurs’ (Meinecke 2019). Beyond public discourse, some welfare sanctions may even be regarded as having been implicitly targeted at Muslim families. For instance, the non-EU immigrants, whose high shares define a housing block as ‘Ghetto’, are by and large Muslims. Another example is the 225 hour rule, which demanded each partner in a union to have worked for at least 225 hours over the past twelve months so that both can receive welfare benefits (M. B. Jørgensen and Emerek 2014). This policy affected particularly Muslim families, because of the low labour force participation rates among Muslim women.

**Explaining welfare chauvinistic public opinion: the theory of deservingness**

Social science research has established different explanations of welfare chauvinistic attitudes. Initially one could be inclined to simply extend classic arguments from research on general support for social policies and redistribution. Along these lines, welfare chauvinism could be seen as a result of mere self-interest, because majority members frequently believe that welfare disproportionally benefits ethnic minorities (Gilens 2000; Fox 2010). Additionally, political orientation and ideology, typically resulting from early and teenage socialisation, predict general welfare attitudes (Jæger 2006) and welfare chauvinism in particular (van der Waal et al. 2010). And like most types of anti-ethnic-minority attitudes (e.g. racism, islamophobia, or anti-immigration attitudes), welfare chauvinism has a strong educational gradient with better educated populations typically being less chauvinistic (van der Waal et al. 2010).

Yet, a better-fitting approach to explain welfare chauvinism is the theory of deservingness, according to which people assess the degree to which a person or a group deserves welfare support as a question of social justice and consider five criteria: their level of need, their responsibility for this hardship, reciprocity with respect to the welfare system, the degree to which they are perceived as good citizens, and their ethnic background; to the degree that accurate information on these criteria is not available, people rely on group stereotypes (Feather 1999; van Oorschot 2000). For instance, the elderly are an example of a group broadly considered as very deserving. As van Oorschot (2006) explains, they are stereotypically seen as being in need of pension without being personally responsible for the fact they cannot earn their living anymore, they have contributed to the welfare state throughout their adult life, are perceived as good citizens and as co-ethnics. Immigrants, by contrast, tend to be ranked by Europeans among the least deserving (van Oorschot 2006; van Oorschot and Uunk 2007; van Oorschot 2008).
While much of the early literature on deservingness of welfare recipients compared immigrants to groups such as the elderly or the unemployed, this is a biased comparison, as Kootstra (2016) explains, because being an immigrant logically implies no situation of need or responsibility for it and thus also no deservingness. Recently, scholars have therefore moved towards using survey experiments in which respondents are presented with fictional vignettes of individuals who are comparable in terms of need and responsibility (e.g. unemployed) but differ with respect to their ethnic background and length of residence. These experimental studies consistently show that majority members regard ethnic minorities and recent immigrants as less deserving of welfare (Gilens 1996; Ford 2016; Hjorth 2016; Kootstra 2016; Reeskens and van der Meer 2019). A related line of survey experimental work demonstrates that majority members support for universal welfare policies declines if typical benefits claimants are portrayed as immigrants (Bay and Pedersen 2006; Bay, Finseraas, and Pedersen 2016; Goerres, Karlsen, and Kumlin 2020). By experimentally disentangling length of residence and ethnic background, experimental studies on welfare deservingness demonstrate that immigrants are regarded as less deserving because of both reciprocity concerns and racism – although the racism uncovered by these studies does not disentangle stereotypes about ethnic minorities being bad citizens (e.g. widespread Islamic fundamentalist values, low labour force participation among women) from pure antagonism against them (see our discussion in the conclusion). However, only two of these studies investigated whether reciprocity-motivated and racism-driven welfare chauvinism act in concert in a joint interaction effect. Ford (2016) concludes that reciprocity and racism exert complementary additive effects in the UK. But according to Kootstra (2016) these dimensions only work as a joint effect, so that minorities in the UK and the Netherlands are penalised more for a lack of reciprocity – with the exception of native welfare claimants of Pakistani or Moroccan origin who are penalised despite being born domestically.

**Healthcare chauvinism during the COVID-19 pandemic**

Survey experiments that compare vignettes of persons or groups in similar situations of need and personal responsibility for it are indeed an important step forward. We maintain, however, that they still fall short of testing welfare chauvinism proper. Beyond the five criteria of deservingness, policymakers frequently justify welfare chauvinism as a form of integration policy, that is, as an incentive for fast labour market integration and self-sufficiency, both of which tend to further general assimilation into the mainstream. In fact, Jørgensen and Thomsen (2016) who have conducted a comprehensive critical analysis of welfare chauvinistic legislation in Denmark, list employment and general integration as the most frequent goal or rationale of these policies (see also Andersen, Dustmann, and Landersø 2019). Thus, the governments of the time did not justify welfare cuts in reference to any of the above-discussed five dimensions of deservingness, such as lacking prior contributions to the welfare state (reciprocity) or the immigrants’ ethnic background. Instead, they justified their legislation as a means to further immigrants’ integration, which needs to be understood against the more general observation that large shares of citizens believe that immigrants have an obligation to integrate. That is, as newcomers to the polity, immigrants face especially high expectations to live up to the norms of what constitutes a good citizen. This is also reflected in the
naturalisation laws of most European countries; in Denmark, for instance, applicants need to demonstrate economic self-sufficiency, Danish language skills, and a passed citizenship test (MIPEX 2020) – requirements that native citizens do not need to pass. In consequence, the justification typically given for welfare chauvinistic policies – irrespective of whether one regards them as lip service – is markedly different from an unwillingness to share public resources with ethnic minorities. By contrast the underlying argumentation may in fact even serve to justify increased public spending, if this furthers the integration of ethnic minorities into the mainstream. The controversial ‘Ghetto Pakken’ may serve as an example for it entails increased funds for public schools located close to deprived residential housing blocks.

Against this background we criticise that current research overlooks the possibility that welfare chauvinism may also be justified on the grounds of incentivizing labour market integration and assimilation more generally. To overcome this shortfall, we need to study a policy area that is not subject to this confound and thus allows us to identify welfare chauvinism proper. We argue that hospital treatment of acute COVID-19 during the spring 2020 pandemic in Denmark lends itself as the perfect test case to overcome these problems. Generally, acute COVID-19 patients (of advanced age) are in an unequivocal situation of need and individually not responsible for their hardship. Moreover, the fear that the limits of the hospital capacities might be reached was a realistic and widely debated threat among the populations of many (European) countries in spring 2020. In Denmark the first case of COVID-19 was confirmed late February 2020 with the country introducing lockdown and social distancing measures in an effort to flatten the curve on March 11. The fact that the number of hospital beds, and especially intensive care beds, per Capita is one of the lowest in the European Union caused serious concerns among medical professionals and the public (OECD 2017; Jensen 2020). This made the cost-considerations of whether or not to share healthcare resources much more immediate than usual concerns about tax or debt burdens of welfare spending (but see comment on timing of data collection from April 17–30 below). Denmark further represents the Scandinavian welfare state that scholars like Alesina and Glaeser (2004) typically have in mind. But most importantly, we claim that non-treatment of acute COVID-19 cannot be justified as incentivizing labour market integration or assimilation into the mainstream more generally.

Based on our argument and the above-discussed theory of deservingness we therefore deduce the following three hypotheses. First, to the degree that concerns about reciprocity (i.e. low contributions to the welfare system) drive welfare chauvinism, acute COVID-19 patients who have recently immigrated are seen as less deserving of hospital treatment than patients who were born domestically (H1). Second, if welfare chauvinism is motivated by racist concerns about ethnic identity, acute COVID-19 patients with a typical Muslim name are seen as less deserving of hospital treatment than patients with a typical Nordic name (H2). Third, if concerns about reciprocity and racism reinforce each other in a joint effect, acute COVID-19 patients with a Muslim name who have recently immigrated are seen as least deserving of hospital treatment (H3). We have pre-registered these hypotheses (and the research design discussed below) on April 20, 2020 before we had access to any of the data. Our publicly available and time-stamped pre-registration can be accessed via the Open Science Framework (Larsen and Schaeffer 2020).
Note that the pre-registration formulates four more hypotheses, because we also varied the fictional patient’s age as an indicator of their situation of need; in spring 2020 it was widely known among the population that the severity and fatality of COVID-19 increases with age. Against our hypotheses, this cue about the severity of hardship does not alter the effect of a Muslim name and of recent immigration. For reasons of brevity, we only mention these Null findings here in the main article. Full results can be found in Appendix C of the online supplement.

**Data and methods**

To test our arguments about healthcare chauvinism during the spring 2020 COVID-19 pandemic in Denmark, we followed the example of the recent literature on welfare chauvinism and designed a factorial survey experiment (Auspurg and Hinz 2014). We pre-registered our experimental design on April 20, 2020 ten days before data collection by YouGov was finalised and before we gained access to the data (AUTHOR YEAR)1. Pre-registration strengthens the validity and reliability of confirmatory (experimental) research by constraining p-hacking and the selective reporting of results (Nosek et al. 2015). Below we first introduce our experimental design, then elaborate on how the data collection was implemented, detail how we analyse the data, and finally explain how this study design operationalises the theory of deservingness. The Online Supplement to this article contains the raw data as delivered by YouGov, the R code that replicates our findings, and additional results.

**Experimental design**

In light of using the COVID-19 pandemic in Denmark as a case to study welfare chauvinism proper, we devised a survey experiment which starts by introducing potential participants to the following background information:

The outbreak of Coronavirus has placed an immense pressure on healthcare systems, and in several countries, we are witnessing a lack of hospital beds, materials, and personnel. A lot is being done in Danish society to avoid a similar scenario. Yet, Denmark is one of the countries in Europe with the lowest number of hospital beds per citizen. This fact is considered a bottleneck if the number of COVID-19 patients in need of hospitalization continue to increase.

After this introduction, our experiment confronts participants with the following vignette, which randomly varies three pieces of information (italicised in squared brackets) and ends with an assessment of deservingness:

In the following you are asked to take a stand on the case of [name] who has lived in Denmark [length of residence]. [Name] has been tested positive for COVID-19 (Corona virus) and later developed severe symptoms such as fever, fatigue, dry coughs, breathing difficulties, and chest pains. He is [age]. The doctor who examined [name], assessed that he needs treatment. However, given the increased pressure the healthcare system is experiencing, [name] might be admitted a hospital bed at the expense of someone else.

Based on the above scenario, to what degree do you feel this person should be prioritized a hospital bed?
Our assessment of deservingness does not directly ask whether ‘[…] this person [deserves] a hospital bed’, but instead even increases the gravity of the decision by asking about potentially prioritising the patient over someone else; it measures whether the introduced patient deserves a hospital bed more than someone else. To answer this question, we offered an eight-point Likert scale running from ‘(1) To a very low degree’ to ‘(8) To a very high degree’.

The three characteristics of the fictional COVID-19 patient that we randomised each indicate one dimension of the theory of deservingness and thereby serve to test our hypotheses about reciprocity- and racism-driven welfare chauvinism. Specifically, they are meant to tap into the deservingness dimensions of racism, reciprocity, and need. Overall, our factorial survey experiment has \((3 \times 2 \times 2 =) 12\) treatment conditions.

First, we took inspiration from Dahl and Krog’s (2018) correspondence experiment and randomised the patient’s name in one treatment and two control conditions as a subtle way to test racism-driven welfare chauvinism. Note that in each of the conditions we used a pool of three (again randomised) names to avoid estimating a name-specific rather than a general racism effect. Using Dahl and Krog’s (2018) list of popular Muslim names in Denmark, we chose the following three Muslim names as signalling an ethnic minority background in the treatment condition: Mustafa, Ibrahim, and Ahmad. From a methodological point of view, Muslim names have the advantage that they are an unequivocal ethnic marker in Denmark. Taken from the angle of relevance, Muslim names identify an ethnic minority that makes close to 9% of the Danish population and forms the vast majority among immigrants from non-EU countries (Statistics Denmark 2019). Moreover, much of the Danish public debate about immigration, and about welfare chauvinistic policies in particular, revolves around Muslim immigrants and their descendants (see theory section above). This is not to say that the notion of racism-driven welfare chauvinism only applies to Muslims in Denmark. But studying Vietnamese (i.e. largest non-EU sending country without a Muslim majority population) or Polish (i.e. largest EU sending country) names additionally would have implied an additional four treatment conditions and thus a considerable increase in sample-size demands and therefore costs. To select names for the first control group, we used Denmark’s public name register and chose the three most frequent Danish names for the birth cohorts of 1937 and 1961 (reflecting the patient’s two potential ages, see below): Jørgen, Hans, and Erik. However, in Denmark Muslim names may also be regarded as signalling a low socio-economic status, which would bias the comparison to typical middle-class Danish names (Dahl and Krog 2018; Wenz and Hoenig 2020). In a second control condition, we thus used three frequent US-inspired names found in Denmark’s public name register for the cohorts of 1937 and 1961: Sonny, Freddy and Benny – such US-inspired names are typically held by working class Danes (Kisbye 1988). Note that we combine all six names into one Nordic-name treatment in the main analysis, because we actually do not find any differences between the middle- and working-class names (see section ‘Robustness tests’). We only use male names, because the sample-size demands for sufficient statistical power increase with the number of experimental treatments and randomising gender would have doubled the number of treatments. Yet, focusing on male rather than female names may be justified against the fact that the majority of COVID-19 cases and fatalities were male patients. Moreover, Muslim men face harsher labour market discrimination than Muslim women in
Denmark (Dahl and Krog 2018); by focusing on men we thus chose the potentially stronger treatment.

To secondly test reciprocity-driven welfare chauvinism, we mimicked earlier survey experiments on welfare chauvinism and randomised whether the fictional COVID-19 patient has lived in Denmark for one year (treatment) or was born in Denmark (control). A patient who lives in Denmark for one year only, clearly has not had the time to reciprocate to the welfare state to the same extend as a person who was born in Denmark. Yet, randomising the length of residence in Denmark means that half of the patients with one of the six frequent Danish names are recent immigrants. It is thus important to note that these names are not untypical among the populations of Denmark’s neighbouring countries (i.e. Norway, Sweden, and Germany) and can well be regarded as typical Nordic names. Third and finally, we tried to vary the deservingness dimension of need to test for potential interaction effects with racism and reciprocity. Because the severity and fatality of a COVID-19 infection increases with age, we randomised whether the fictional patient was 83 (treatment), or 59 years old (control).

Because all \((3 \times 2 \times 2 =) 12\) treatment conditions are theoretically plausible, we decided to implement the vignette study as a full factorial design, allowing us to also study the interaction (i.e. joint) effects of the treatment conditions. To ensure a sufficient sample size to test this experimental design, we conducted a statistical power analysis, which suggests that we need a sample size of \(n = 966\) to detect small effects sizes of our treatments and their interaction effects, where a small effect size indicates that the additional consideration of a treatment or interaction effect increases \(R^2\) (i.e. explained variance) by \(f^2 = 0.01\). Based on these analytical considerations, we initially considered on a sample size of \(n = 1,000\). However, out of concerns over an effectively smaller sample resulting from potential non-response, we decided for a sample of \(n = 1,200\).

By default, one would distribute the number of cases equally across all 12 treatment conditions. Yet, we regard the distinction between middle- and working-class Nordic names simply as enabling us to conduct an additional robustness test and therefore treat them as one control group with respect to the distribution of cases across treatments. This results in effectively \((2 \times 2 \times 2 =) 8\) cells and a relative cell frequency of \((100/8 =) 12.5\%\) or \(n = 150\) to be able to detect even small treatment effects with a statistical power of 0.8.

**Sample**

We commissioned YouGov to implement our survey experiment among the respondents of their ongoing online panel of the Danish population. YouGov recruits panelists who are at least 18 years of age based on a stratified random sample drawn from the Danish registers. Panelists receive points for gift cards as an incentive to complete surveys. Our final sample consists of \(n = 1,208\) participants who are stratified by education, gender, age, and region to match the Danish population in these characteristics. The data also contains post-stratification weights to further ensure that the sample is representative of the population living in Denmark. The full data set as delivered by YouGov can be publicly accessed as part of our supplementary replication materials.

Participants completed the survey between April 17 and 30, 2020. Yet, the Danish government started to lift some lockdown restrictions on April 15 already. One might thus
question how current and realistic the threat of hospital capacity shortages was during fieldwork. That said, the survey was conducted only five weeks after the lockdown of Danish society on March 11. And while kindergartens and primary schools were opened again, it was very unclear at this point in time whether the eased restrictions would lead to a resurgence of the COVID-19 infections. Moreover, we actually asked participants after the introduction to our experiment but before the vignette: ‘How concerned are you about a lack of resources at the hospitals (e.g., lack of hospital beds and/or medical staff)?’, with potential answers ranging from ‘(1) Not concerned at all’ and ‘(8) Very concerned’. The average of value of 4.13 shows a fair degree of concern among the respondents. Most importantly however, this average concern does not decline over time, suggesting that hospital capacity shortages were indeed a current, realistic and stable concern during the study period (see Appendix B of the supplementary RMarkdown replication file).

Estimation strategy

We use OLS regression with robust standard errors to analyse the experimental data. Reporting (heteroscedasticity-)robust standard errors is necessary, because all models contain the post-stratification weights delivered by YouGov and weighting introduces heteroscedasticity (Winship and Radbill 1994). We z-standardize the outcome to easily assess effect sizes in terms of standard deviations. In line with standard practice, we code the treatment conditions as three dummy variables indicating the fictional COVID-19 patient’s name (Muslim as compared to Danish), length of residence (One year in Denmark versus born in Denmark), and age (83 versus 59).

Overall, we run four regression models. A baseline specification tests all three hypotheses simultaneously, by considering the three explanatory variables and an interaction term between the name and length of residency variables. For reasons of brevity, we do not include any interaction term between the age dummy and any of the other variables, because none of these turn out as significant (see Appendix C of the Online Supplement for these results).

In another specification we additionally consider a set of control variables. Because our treatments are randomised there is no need to adjust for potential confounders. However, considering control variables can increase the precision of the estimation and thus lower the probability of falsely retaining the Null-hypothesis of no treatment effect (Angrist and Pischke 2009). We add the following seven control variables to the second model: gender, five age groups, education (low, medium, high), employment status, a continuous variable indicating the degree of urbanisation, a categorical variable indicating Denmark’s five regions, a dummy indicating whether respondents are cohabitating with partner, and finally a dummy indicating whether they have children at home. Further details on the coding of these variables and a table with descriptive statistics can be found in the RMarkdown replication file which is part of the Online Supplement. The data delivered by YouGov contains three further variables, which we do not include in the models: left-right orientation, political party preference, and income. Because each of these variables contains at least 15% missing values, it is not worth adjusting for them at the cost of losing observations.

Finally, we estimate both specifications mentioned above using casewise deletion and multiple imputation, because 205 (i.e. 17%) respondents did not provide a valid answer.
We multiply impute the 205 missing values using predictive mean matching and chained equations (Buuren and Groothuis-Oudshoorn 2011; Buuren 2012). We generate 50 imputations and consider all treatment and control variables. To acknowledge potential interaction effects, we separately impute by each of the 12 experimental conditions.

**Study design and the theory of deservingness**

Before discussing our results, we would like to come full circle and briefly summarise how any potential results of the above-discussed OLS models relate to the theory of deservingness. Deservingness itself is the outcome variable of our models, that is, the degree to which a respondent regards the specific patient introduced to her/him as more deserving of a hospital bed than someone else. The dummy variables capturing the characteristics of the fictitious patients identify three of the five dimensions of deservingness, with the other two dimensions (implicitly) held constant. The dimension of need is generally high since the vignette explicitly states that ‘The doctor who examined [name], assessed that he needs treatment’. Yet beyond that, we further varied the dimension of need as the patient’s age, because it was early and widely known that the severity and fatality of a COVID-19 infection strongly increases with age. The responsibility dimension is (implicitly) held constant by the experiment, because anyone faces the risk of being infected with COVID-19 and particularly during the early stage of the pandemic, in April 2020, no groups were publicly identified as being particularly responsible for spreading the disease. Another dimension that our experiment (implicitly) holds constant is the question to which the patient is (stereotypically) regarded to be a good citizen; the respondents receive no information about how virtuous the patients live their life and it is not apparent why this would matter for the decision whether they should receive treatment. Yet, in the conclusion we discuss the limitation of this aspect of our experimental design. The time which the patient has lived in Denmark (i.e. one year or all his life) identifies reciprocity to the welfare state, especially given the patient’s potential age of either 59 or 83 years. Finally, the patient’s name captures the ethnic background dimension. In summary, all five dimensions of the theory are either purposefully held constant or are explicitly varied by our experimental design to predict healthcare deservingness among potential COVID-19 patients. All other variables considered in the models simply serve to increase precision of the statistical estimates but serve no substantial purpose.

**Results**

Is there evidence of healthcare chauvinism among the Danish population? And if so, is this healthcare chauvinism driven by concerns over a lack of reciprocity to the welfare state, racism against Muslim minorities, or a combination of both? To answer these questions, we asked survey participants whether a fictional COVID-19 patient with randomised characteristics should be prioritised a hospital bed given hospital capacity shortages. Table 1 shows the results of our regression models. Note that we exclusively report two-tailed p-values and confidence intervals, apart from two cases in which we explicitly state otherwise.

The two central variables of interest, patient’s name and length of residence, are conditional on each other because the regression models contain their interaction effect. This
renders their interpretation a bit more complex. To ease interpretation, Figure 1 visualises the results. The x-axis displays the treatment effects as the average difference in hospital bed prioritisation to a COVID-19 patient with a Danish/Nordic name who was born in Denmark. On the y-axis we see three types of patients who might face discrimination as compared to a patient with a Danish/Nordic name who was born in Denmark. For each of the three types of patients Figure 1 displays four estimated treatment effects along with their 90 and 95% confidence intervals in thick black and thin grey respectively. The four estimates derive from different model specifications, but it is assuring to see that

| Table 1. OLS regression of hospital-treatment prioritisation on COVID-19 patient characteristics |
|-----------------------------------------------|-----|-----|-----|-----|
|                                               | Model 1 | Model 2 | Model 3 | Model 4 |
| (Intercept)                                   | 0.190**| 0.215  | 0.196***| 0.230*  |
|                                              | (0.065) | (0.156) | (0.056) | (0.133) |
| Treatments                                    |       |       |       |       |
| 83 years old                                  | −0.192**| −0.205**| −0.191**| −0.207***|
|                                              | (0.068) | (0.068) | (0.058) | (0.057) |
| Muslim Name                                   | 0.097  | 0.119  | 0.076  | 0.098  |
|                                              | (0.088) | (0.086) | (0.075) | (0.073) |
| Lives in DK since 1 year                      | −0.162*| −0.154*| −0.180*| −0.180*|
|                                              | (0.088) | (0.087) | (0.074) | (0.073) |
| Muslim Name*Lives in DK since 1 year          | −0.253*| −0.282*| −0.207*| −0.233*|
|                                              | (0.137) | (0.134) | (0.115) | (0.113) |
| Male                                          | −0.297***| −0.295***|       |       |
|                                              | (0.068) | (0.058) |       |       |
| Age (reference: 18-29)                        |       |       |       |       |
| 0–39                                          | −0.103 | −0.111 |       |       |
|                                              | (0.122) | (0.100) |       |       |
| 40–49                                         | −0.037 | −0.040 |       |       |
|                                              | (0.120) | (0.105) |       |       |
| 50–59                                         | 0.152  | 0.145  |       |       |
|                                              | (0.121) | (0.103) |       |       |
| 60+                                           | 0.351**| 0.347***|       |       |
|                                              | (0.107) | (0.091) |       |       |
| Education (reference: Low)                    |       |       |       |       |
| Middle                                        | 0.002  | 0.007  |       |       |
|                                              | (0.097) | (0.082) |       |       |
| High                                          | 0.162  | 0.159* |       |       |
|                                              | (0.101) | (0.085) |       |       |
| Inactive                                      | −0.004 | −0.004 |       |       |
|                                              | (0.080) | (0.069) |       |       |
| Urban                                         | −0.030 | −0.031 |       |       |
|                                              | (0.027) | (0.023) |       |       |
| Region (reference: Hovedstaden)               |       |       |       |       |
| Sjælland                                      | −0.017 | −0.016 |       |       |
|                                              | (0.129) | (0.111) |       |       |
| Syddanmark                                    | 0.093  | 0.086  |       |       |
|                                              | (0.110) | (0.096) |       |       |
| Midtjylland                                   | 0.066  | 0.062  |       |       |
|                                              | (0.103) | (0.087) |       |       |
| Nordjylland                                   | −0.013 | −0.004 |       |       |
|                                              | (0.144) | (0.120) |       |       |
| Single                                        | 0.196**| 0.194***|       |       |
|                                              | (0.073) | (0.062) |       |       |
| No children at home                           | −0.075 | −0.073 |       |       |
|                                              | (0.091) | (0.078) |       |       |
| Adj. $R^2$                                    | 0.026  | 0.062  | 0.029  | 0.077  |
| Num. obs.                                     | 1003  | 1003  | 1208  | 1208  |

Note: Results of OLS regression with robust standard errors in parentheses; two-tailed p-values are indicated by $^*$ $p < 0.10$, $^*$ $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. 

renders their interpretation a bit more complex. To ease interpretation, Figure 1 visualises the results. The x-axis displays the treatment effects as the average difference in hospital bed prioritisation to a COVID-19 patient with a Danish/Nordic name who was born in Denmark. On the y-axis we see three types of patients who might face discrimination as compared to a patient with a Danish/Nordic name who was born in Denmark. For each of the three types of patients Figure 1 displays four estimated treatment effects along with their 90 and 95% confidence intervals in thick black and thin grey respectively. The four estimates derive from different model specifications, but it is assuring to see that
these model specifications basically result in similar point estimates. The only difference is that the estimates based on multiply imputed data are somewhat more precise (i.e. smaller standard errors and confidence intervals) and therefore more trustworthy under the assumption that our imputation model is accurate.

Starting from the top, the first result of Figure 1 is that we find no statistically significant evidence of blatant racism-driven welfare chauvinism. Note that we here use the term ‘blatant racism’ to describe situations where racism is the unequivocal and single driver of healthcare chauvinism, that is, ceteris paribus which here includes being born in Denmark. COVID-19 patients with a Muslim name who were born in Denmark are regarded as similarly deserving of hospital treatment as ethnic majority patients. Yet, our survey experiment does provide evidence of reciprocity-driven healthcare chauvinism. On average, the Danish population thinks that patients with a Danish/Nordic name who immigrated to Denmark only a year ago should be prioritised less when it comes to the distribution of scarce hospital beds. Across the four models, the difference is ca. 16% of a standard deviation, which is a medium but noteworthy effect size in the social sciences. That said, we should acknowledge that two of the estimates regarding this finding are only marginally significant (p_{Model 1} = 0.0651 and p_{Model 2} = 0.0771). But given that this is a pre-registered and thus confirmatory analysis, it is fair to apply the logic of a one-tailed test of statistical significance. Doing so renders the estimates from all model specifications as statistically significant (one-tailed Wald tests: \( p_{Model 1} = 0.0325 \) and \( p_{Model 2} = 0.0385 \)).

The third and final result displayed in Figure 1 suggests that while there is no blatant racism-driven healthcare chauvinism against Muslims in Denmark, such racism strongly catalyses reciprocity-driven healthcare chauvinism. That is, similarly-recent immigrant patients with a Muslim name are regarded as least deserving of hospital treatment. In fact, having a Muslim name doubles the reciprocity-driven healthcare chauvinism that immigrants with a Danish/Nordic name face. The resulting difference of ca. 40% of a standard deviation is a considerable effect size in the social sciences. Figure 1 displays
the statistically highly significant difference to ethnic majority patients. But Table 1 further shows a statistically significant interaction effect of having a Muslim name and being a recent immigrant. This suggests that the difference between recent Muslim and Nordic immigrants (ca. 20% of a standard deviation) is statistically significant, too. Again, the interaction effect is only marginally significant in two of the four specifications ($p_{\text{Model 1}} = 0.0651$ and $p_{\text{Model 3}} = 0.0730$), but passes conventional levels if the logic of a one-tailed test is applied (one-tailed Wald tests: $p_{\text{Model 1}} = 0.0322$ and $p_{\text{Model 3}} = 0.0365$). As before, we believe that the logic of a one-tailed test is justified given that we have pre-registered to test this interaction effect.

The Online Supplement contains further results that testify the robustness of these results. First, the fictional COVID-19 patient was either 59 or 83 years old. Table 1 shows a highly significant age effect suggesting that people think that older patients should generally be prioritised less. Appendix C in the Online Supplement shows however that patient age does not significantly alter any of the results reported here. Secondly, one might be concerned that the results discussed here conceal classism against working class patients. Yet, results discussed in Appendix D in the Online Supplement show that respondents do not distinguish between patients with middle- or working-class Danish/Nordic names and that both types of ethnic majority patients enjoy the same advantage over recent immigrant patients with a Muslim name.

**Conclusion**

Social science research has produced evidence of welfare chauvinism whereby citizens turn against social policies that disproportionately benefit immigrants and their descendants. This welfare chauvinism is typically explained by the theory of deservingness, suggesting that we tend to evaluate whether a person or a group deserves welfare benefits based on their need for assistance, responsibility for their hardship, earlier reciprocity to the welfare system, image of being a good citizen, and their ethnic background. Initial research on the topic established that immigrants are typically regarded as less deserving of welfare than groups such as the elderly or the unemployed. Yet, such comparisons are flawed by the fact that immigration or being an ethnic minority does not logically imply a situation of need that calls for assistance and solidarity. More recently, scholars have started to use survey experiments to compare immigrants and ethnic minority members to ethnic majority members in similar situations of need and responsibility for it. These studies provide evidence of both reciprocity-motivated welfare chauvinism against recent immigrants and racism-driven welfare chauvinism against native ethnic minorities (i.e. born in the country of assessment).

But while these recent survey experimental studies are an important step forward, they potentially suffer from another incommensurability: Welfare chauvinism may be motivated as incentivizing labour market integration and assimilation into the mainstream – an expectation that ethnic majority members do not face. To test welfare chauvinism proper, we thus have to identify policy domains to which this idea of incentivizing integration does not apply.

We argued that hospital treatment of an acute COVID-19 infection is such a case. On that premise we conducted a pre-registered survey experiment among 1,208 respondents who are representative of the Danish population during the COVID-19 spring pandemic.
of 2020. In contrast to earlier research, our results show no blatant racism-driven healthcare chauvinism against patients with a Muslim name who were born in Denmark. However, we generally find evidence of reciprocity-motivated healthcare chauvinism against recent immigrants, which doubles in strength if the recent immigrant patient has a Muslim rather than a Danish/Nordic name. In our study, racism therefore acts as a catalyst of reciprocity-motivated welfare chauvinism and not as a complimentary additional mechanism.

Based on these findings we draw the following three conclusions about directions for future research. First, we should pay more attention to the ways in which the different dimensions of need are working in concert rather than as complementary additive mechanisms. Kootstra’s (2016) research is inspiring in these regards. But two empirical studies from three different countries are not enough to properly understand these complexities, particularly since a third study does not find any such joint effects (Ford 2016). Second, we have demonstrated that recent survey experimental work on welfare chauvinism might fall short of overlooking further deservingness dimensions beyond the five proposed by psychologists several decades ago. These five dimensions were originally not intended to assess welfare chauvinism against immigrants and their descendants, and so the field needs further theorisation of the dimensions on which citizens judge the deservingness of new arrivals and their descendants. Our article suggests that the degree to which people regard welfare assistance as (de-)incentivizing integration and assimilation into the mainstream has the potential to be one of such additional dimensions. Third and finally, all survey experimental research, including our own, falls short of disentangling the identity/racism dimension from the dimension of whether ethnic minority members are generally seen as a good citizens. To understand this shortfall, consider the initially-mentioned case of the elderly, who are typically regarded as very deserving: they are in a situation of need, not responsible for it, have contributed to the welfare state throughout their life, are regarded as good citizens, and finally as part of one’s in-group. Immigrants, by contrast, are not necessarily in a situation of need, may or may not be responsible if they find themselves in a situation of need, have typically not contributed to the welfare state throughout their working life, are often regarded as questionable citizens and portrayed as such in public media (see discussion about Muslims in Denmark in the theory section), and typically belong to an ethnic minority. Survey experiments have successfully managed to hold the first two dimensions (i.e. need and responsibility) constant, and to isolate the effect of the reciprocity dimension by comparing recently-immigrated to native-born co-ethnics or ethnic minorities. Yet, comparing presumably co-ethnic immigrants with a native (here Nordic) name against those with a foreign (here Muslim) name still conflates the dimension of blatant racism with the fact that ethnic minority members are stereotypically regarded as questionable citizens. That is, a true racist does not want to share welfare benefits with hard-working and well-integrated ethnic minority members either. This shortfall is arguably not grave, since one may regard these stereotypes are themselves racist too – particularly if they serve to frame Muslim immigrants as less deserving of COVID-19 treatment. But analytically, existing survey experiments, including the current one, still blur two distinct dimensions of the theory of deservingness; hopefully a new comprehensive survey experimental design will fill this gap eventually.
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