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Welfare Nationalism and Popular Support for Raising the Child Allowance:
Evidence from a Norwegian Survey Experiment / Ann-Helén Bay, Henning
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Welfare nationalism, benefit export and support for raising the child allowance. Evidence from a Norwegian survey experiment

Introduction

Like other North-European welfare states Norway has over the past decade received a large inflow of two distinctive types of immigrants – asylum seekers from outside the EU and labour migrants from the new EU member states in Eastern Europe. In this research note we contribute to the growing literature concerning a possible negative effect of immigration on the popular support for redistributive welfare policies, and we do this by presenting a survey experiment designed to investigate how attitudes towards a proposal to raise the Norwegian universal child allowance are affected by cues about immigrants' access to benefits.

A number of studies have shown that a significant proportion of European voters consider immigrants less deserving as recipients of welfare state support than natives (Van Oorschot 2006). We use the term “welfare nationalism” to label this observed reluctance to share welfare benefits with immigrants.¹

Welfare nationalism can be the result of different social dynamics and have different motivational sources. According to a dominant perspective in the literature, welfare nationalism is the result of cultural distance between immigrants and natives, with xenophobia

¹ The term “welfare chauvinism” is widely used in the literature referring both to a general unwillingness to share welfare benefits with immigrants and to a specific preference for a policy of withholding benefits from or lowering benefits to immigrants (Kitchelt 1997; Van der Waal et al. 2010; Hjort 2016; Van der Waal, de Koster & van Oorschot 2013). We prefer the normatively more neutral terms “welfare nationalism” and “welfare dualism”, to refer respectively to the general attitudinal disposition and the specific policy preference (see Bay, Finseraas and Pedersen 2013). The term welfare chauvinism can be used more narrowly to perceptions about a higher propensity to misuse welfare benefits and weaker work ethics among immigrants that might contribute to motivate welfare nationalism and a preference for welfare dualism.

affecting in particular low educated segments of the native population (Sides & Citrin 2007, Van der Waal et al. 2010, Hainmüller & Hopkins 2014). The basic idea is that social solidarity does not easily transcend cultural and ethnic cleavages (Becker 1957), and it follows that the support for redistributive welfare policies will be undermined, especially if the ethnic minorities appear to be overrepresented among welfare benefit recipients (Alesina and Glaser 2004). This hypothesis is particularly relevant in relation to Non-Western asylum immigrants who are clearly ethnically and culturally distinct from the native population.

Another possible mechanism is that immigrants compete for jobs with natives and that immigration negatively affects salaries and working conditions in exposed segments of the labour market. According to this interpretation, negative attitudes towards immigrants are interest based and directed towards those immigrants who can be perceived to pose a direct economic threat to (parts of) the native population (Mayda, 2006; Semyonov et al., 2006; Mewes & Mau 2012; Finseraas et al., forthcoming). Presumably, this competing hypothesis is particularly relevant in relation to labour migrants from other European Economic Area (EEA) countries.

We want to draw attention to the role played by a third possible motivational source (or rationale, if you like) for welfare nationalism: that newly arrived immigrants are perceived (or can be portrayed) as not fulfilling standard criteria for receiving benefits. We should emphasise that we do not assume that this latter mechanism constitutes a competing explanation for welfare nationalism on par with the sociological/economic perspectives discussed above. Rather we believe it might work to legitimize and reinforce welfare nationalism in specific situations.

The most obvious example would be that immigrants are viewed as less deserving because they have not paid prior contributions to the national social insurance system (Oorshot 2006). The issue of a possible violation of standard criteria for receiving benefits might also arise in connection with non-contributory benefits. A concrete example is the possibility for EU labour migrants to export child allowances to other countries. According to EU law, labour migrants from other EU countries are entitled to receive child allowances for the children they support, even if the children are residing in the country of origin (living with the other parent or a relative). Whereas the traditional view in Norway (and the other Nordic countries) is that the child allowance is a direct support to the child, channelled via the mother, EU regulations rest on the view that the child allowance is compensation to the breadwinning parent for the costs of supporting children (Christensen & Malmstedt 2000). It follows logically from the view taken by EU-regulations, that a migrant worker should be entitled to child benefits in the country where he or she works, no matter where the children actually live. From the traditional Nordic perspective, on the other hand, it is likely to appear odd and in conflict with the standard criterion of residence, to pay out child benefits to children living with their mother in another country (Hatland 2015). Thus, this type of deservingness criteria is likely to have appeal beyond groups that support welfare nationalism due to prejudice or anti-immigration sentiments. Although the scope of this kind of benefit export is fairly modest, the issue has received considerable attention in Norwegian news media as well as in official policy documents (NOU 2011:7). It has also been put on the agenda in EU by David Cameron's British Government. In the European Council meeting of 18.-19. February 2016 it was decided to allow member states to adjust benefit rates "to the conditions of the Member State where the child resides" (European Council 2016: 22).

Whatever the motivational sources might be, two rather different implications of welfare nationalism have been discussed in the literature. One possibility is that the support for redistributive social policies will be undermined, and that voters will be more inclined to accept general welfare state retrenchment (Alesina and Glaser 2004, Banting et al.2005). Another possibility is that voters will demand direct or indirect discrimination against immigrants in the provision of social welfare (Van der Waal et al. 2010; Emmenegger and Careja 2012; Bay, Finseraas & Pedersen 2013). A third, intermediate possibility is that voters will be more inclined to support redistributive policies if they are combined with some kind of discrimination against immigrants.

It is primarily the last possibility we explore here with reference to the Norwegian child allowance. The child allowance could play an important role in supporting families with children and keeping income poverty among this population group at bay, but it has in recent years been subject to a significant decline in relative terms as well as in absolute purchasing power; a trend that has contributed to increasing poverty rates among children (NOU 2009:11).

We ask whether welfare nationalism can be invoked to weaken the support for raising the child allowance, whether the support can be increased if made conditional upon discrimination against immigrants, and last but not least we are interested to study if the prospect of benefit export (and an associated perception about a violation of standard criteria for receiving benefits) plays a distinctive role in affecting the support for raising the child allowance.

The Survey Experiment

Our survey experiment is of the counter-argument type (e.g. Sniderman 2003: 109). In the first step, respondents are asked to evaluate a proposal about raising the child benefit:

“There has recently been an increase in the number of children living in poverty in Norway, which has led to a discussion on whether the child benefit should be increased. What is your view on a proposal to increase the child benefit? Should the child benefit increase? Yes, No”

In the second step respondents who initially support the proposal to increase the child benefit, are reminded about immigrant’s access to benefits, and they are then asked if they uphold their support for the proposal. Respondents who initially oppose the proposal are instead asked if they would change their mind if immigrants’ access to benefits was curtailed. The procedure allows us to estimate the effect of invoking the issue of immigrants’ access to benefits on initial supporters as well as the effect of opening up for discrimination on initial opponents (see Bay and Pedersen 2006 for a similar set-up with respect to a proposal to introduce a basic income).

The persuasion experiment in the second step is done in different versions on two randomized subsamples, Group A and Group B. For respondents in Group A, the persuasion cue refers to “newly arrived immigrant families”. For respondents in Group B it refers to “labour immigrants from an EEA country” with the additional information that these labour migrants are entitled to benefits (also) for children living in the country of origin. The initial supporters in both groups are then asked if they will uphold their support. Since initial supporters are randomized into groups A or B, a comparison of these groups is a classic randomized experiment. The discrimination option suggested to initial opponents in the two subsamples differs accordingly, and constitutes its own randomized experiment. For members of Group A it is suggested to limit eligibility to Norwegian citizens, while for members in Group B it is

suggested to limit access to children actually living in Norway. The exact question wordings for all groups are shown in the Online Appendix.

This set-up allows us to investigate differences in the response to different types of immigrants/situations. The expression “newly arrived immigrant families” used in the cue offered to Group A is likely to be interpreted at least partly as referring to Non-Western asylum migrants, while Group B is exposed to cues that specifically refer to labour migrants from an EEA country (most likely Eastern European) with the additional information that the right to benefits also applies to children that might be living in the country of origin.

If welfare nationalism increases with the degree of cultural differences, we would (in the absence of competing forces) expect that the responses are stronger in Group A where the cue refers at least in part to Non-Western immigrants. If it instead turns out that responses are stronger to the stimuli provided in Group B, we take this as an indication of the relative importance in this particular case of one or both of the two alternative motivational sources discussed above: welfare nationalism fuelled by economic competition or by ideas about the standard criteria for receiving benefits – in particular that the child allowance is meant as a support to the mother and child and hence should be granted only if the true beneficiaries actually live in Norway. Our experiment does not allow us to directly distinguish between the two latter mechanisms, but we provide indirect evidence about their relative influence by investigating the social and ideological profile of initial supporters who are persuaded to change their opinion.

Data and Empirical Strategy

The survey experiment was part of the third round of the Norwegian Citizenship Panel Study (NCPS, see uib.no/en/citizen). The NCPS is a web-based survey of a representative sample of the Norwegian population, who are contacted about twice a year to answer various questions about social and political issues. Researchers can propose question items and survey experiments, and this is how our survey experiment was included in NCPS. 1667 respondents were asked the initial question about the child benefit. 109 respondents who did not answer are excluded, leaving us with a sample of 1558 respondents. The online appendix includes descriptive statistics, which reveals that people with high education is overrepresented in the sample.

The empirical strategy is simple. First we compare the distribution of answers in the different treatment groups and discuss whether they are substantively and statistically different. Next we use a multinomial regression approach to examine the ideological profile of those who change view in the two groups.

Empirical Results

Due to randomization of treatment, we should expect no differences between the treatment groups on background characteristics. Results in the Online Appendix (Table A2) confirm that a set of background characteristics which are typically important for welfare state preferences do not predict whether the respondent receives the “EEA” treatment rather than the “newly arrived immigrant” treatment.

Figure 1. Results from the survey experiment. Stepwise distribution of responses to a proposal to increase the child allowance.

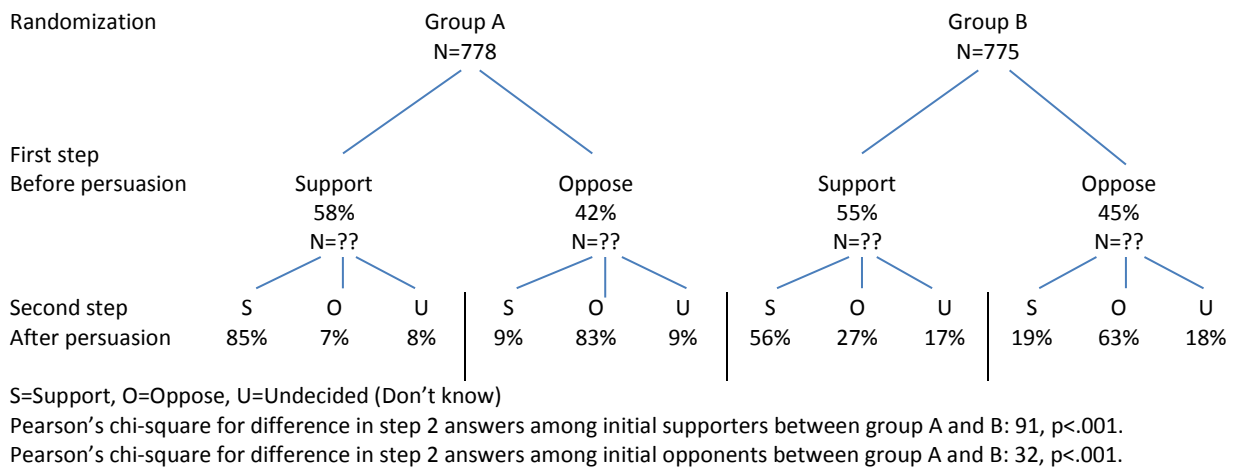


Figure 1 presents the main results of the experiment. In the first step, before the persuasion stimulus, the proposal to increase the child benefit in order to reduce child poverty receives support from a small majority of the respondents. Among the entire sample, 56 percent initially support an increase in the child benefit while 44 percent are opposed. Since the respondents are randomly assigned to Groups A and B and since the different cues are only introduced in the second step, we would expect the two treatment groups to have a similar distribution of responses to the initial question. It turns out, however, that there is a slight difference with 58 percent initially supporting the increase in Group A and 55 doing the same in Group B. This difference is not significant (see also Column 1, Table A2 in the appendix). In light of the insignificant differences between Groups A and B on the background variables, we rest assured that the small difference in the initial responses is entirely accidental.

More importantly, the persuasion argument offered to Group A has a relatively modest effect on the respondents. Among those who initially support increasing the child benefit, 85 percent uphold their support after being informed/reminded that newly arrived immigrant families are entitled to receive benefits. Only 7 percent change their mind to oppose an increase, and 8

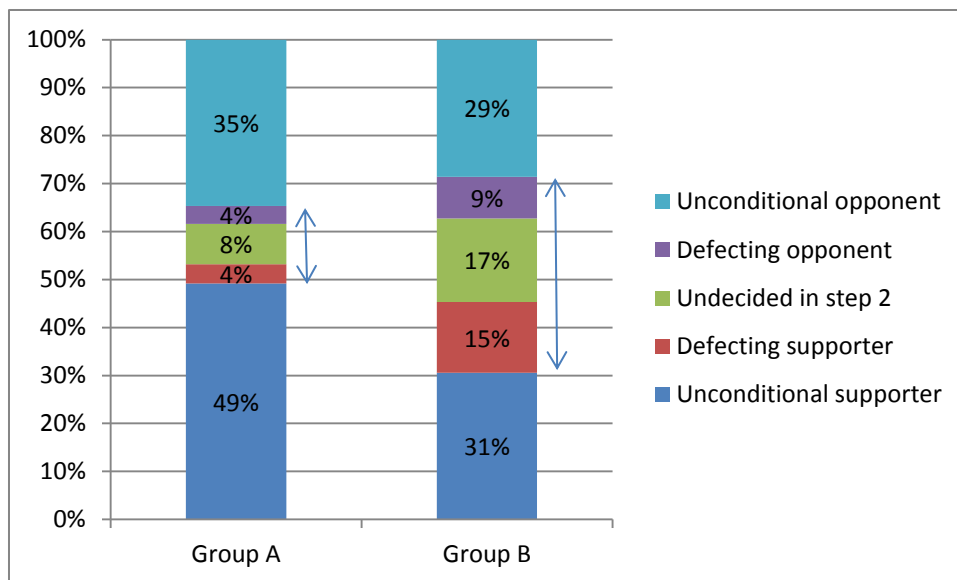
percent opt for saying “don’t know” in step 2. Among those who initially oppose increasing the child allowance, 83 percent remain faithful to their initial rejection, 9 percent are willing to support an increase in the child allowance if entitlement can be restricted to Norwegian citizens, and another 9 percent are now undecided. Thus, the “newly arrived immigrant” treatment does not move many respondents away from their initial position.

The alternative (EEA) treatment given to Group B respondents, however, has rather strong implications for the support for increasing the child allowance. Among initial supporters who were given this treatment, 27 percent switch to opposing an increase, and another 17 percent now report to be undecided. Only 56 percent continue to express support for the increase after being exposed to persuasion. Among initial opponents, 19 percent say that they would change their mind and support a benefit increase if the allowance was restricted to children living in Norway, and another 18 percent declare themselves undecided. 63 percent of the initial opponents insist on their original response to reject an increase in the child allowance.

Figure 2 summarises the distribution of answers in the two-step experiment, for Group A and Group B respectively. It shows very clearly that references to the benefit access of EEA labour migrants with children living outside of Norway has a stronger persuasive power than references to the access of newly arrived immigrants in general. Unconditional supporters are respondents who agree to the proposal of increasing the child allowance both before and after the persuasion stimulus. They constitute 49 percent of Group A and only 31 percent of Group B. When reference has been made to the fact that labour migrants can receive benefits for children living in another EEA country the share who consistently favour an increase in benefits shrinks to less than one third of the respondents.

Unconditional opponents uphold their opposition from the first step even when offered the opportunity to limit the access to benefits, while defecting opponents change towards support in the second step. Among respondents in Group A, 35 percent are unconditional opponents while 29 percent of Group B insist on opposing an increase in the child allowance when offered the opportunity to exclude children living outside Norway. Defecting supporters who withdraw their initial support in the second step constitute only 4 percent of the respondents in Group A, but 15 percent in Group B.

Figure 2. The distribution of response patterns in the two experimental groups.



As indicated by the vertical arrows, a total of 16 percent of the respondents in Group A change their views on the child allowance as a result of the immigrant persuasion treatment. The corresponding share in Group B is 41 percent, implying that their views on the child allowance are conditional on the inclusion/exclusion of EEA labour migrants who can export benefits to children living in another EEA country.

These results clearly show that European labour migrants' access to the child allowance (also for children living in the country of origin) is viewed as more controversial and problematic than the access of newly arrived immigrant families, even if the latter group is, on average, of a more distant cultural background.

As we have already suggested, a possible explanation is that the child allowance is considered to be a benefit to the child via the mother and that the possibility to export the benefit is therefore perceived to violate the basic residency criterion. An alternative explanation is that EEA labour migrants are viewed with stronger suspicion than migrants in general, possibly due to direct competition with segments of the native population in the labour market.

Finally we follow up on the first of these ideas by investigating whether the propensity to change view is driven by the respondents' attitudes towards immigrants in both the two treatment groups. We expect the respondents' ideological position on immigration to be strongly related to the propensity to change view in the immigrant treatment (Group A). If this is the case to a smaller degree among respondent who are exposed to the EEA treatment (Group B), and this persuasion cue has a broader appeal also to immigration liberals, we take this as an indication that general beliefs about standard criteria for receiving benefits have been invoked and play a significant role on this particular issue.

We restrict the analysis to those who initially supported the increase, since we have the largest sample size for this group. Results point in the same direction if we instead study the initial opponents (see online appendix), but due to the smaller sample size the estimates are less precise. We analyse the propensity to change view in a multinomial regression model where the dependent variable is response in the second round (support increase, oppose increase, and

don't know). We choose a multinomial model since we do not want to exclude those who shift to a do not know answer from the analysis. The key independent variables are a measure of ideological position on immigration issues (see below) and its interaction with the treatment.

We measure restrictive ideology on immigration/integration by the respondents' position on a welfare dualism question, namely a question on their view on whether "refugees should have the same rights to social assistance as Norwegians, even if they are not Norwegian citizens". Respondents are asked to state their view on a scale from 1 to 7 where 1=Strongly agrees and 7=Strongly disagrees. We recode the scale to go from 0 to 1 to ease interpretation of coefficients. The ideology items were asked prior to the survey experiment which means that they are not polluted by the experiment. We consistently include controls for age, education and gender to pick up correlated interest-based divisions that the ideology measure might pick up. In the online appendix we show that the results are robust to further controls for economic interest-based controls (income and current employment situation).

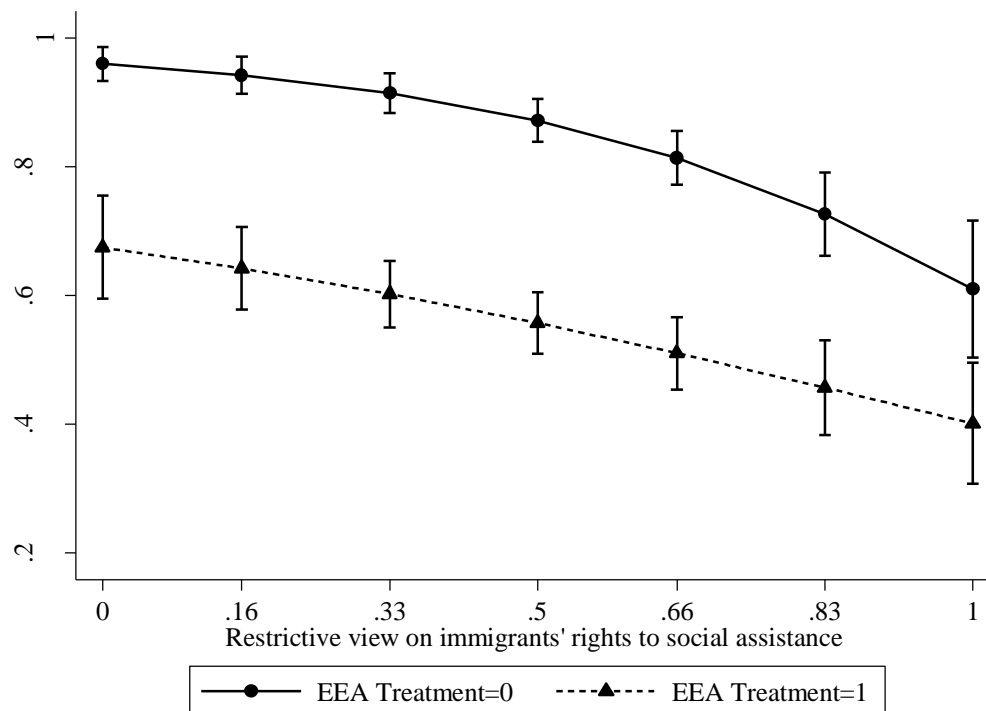
Table 1: Should the child benefit increase? The dependent variable is response after treatment. The sample is restricted to those who initially supported an increase in the benefit. N=863.

	Oppose vs support	Don't know versus support
Restrictive X EEA treatment	-2.32*** (.88)	-1.71** (.77)
Restrictive	4.11*** (.79)	1.98*** (.62)
EEA treatment	3.38*** (.65)	2.19*** (.47)
High school education	.58 (.39)	-.17 (.36)
University/college education	.06 (.39)	-.60 (.37)
Missing education	.53 (.59)	.55 (.53)
Age: 30-59	-.70** (.28)	-1.03** (.29)
Age: Above 59	.04 (.30)	-.01 (.30)
Constant	-4.91***(.72)	-2.64***(.54)

Note: Standard errors in parentheses. Reference categories: Compulsory education, Age: 18-30. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

The results provide strong support for the expectation that the response to the EEA treatment is less ideologically driven than the response to the immigrant treatment. The coefficient for *restrictive* captures the difference between being restrictive and liberal for members of Group A (EEA Treatment = 0) who have received the immigrant treatment. The large and positive coefficient tells us that those with a restrictive view are significantly more likely to change view than liberals as a result of the immigrant treatment. The negative and significant interaction term tells us, however, that the effect of the liberal-restrictive distinction is significantly smaller in Group B (EEA Treatment = 1).

Figure 3. Probability of support for increasing the child benefit by view on immigrants' right to social assistance in the two treatment groups.



The result is illustrated in Figure 3, which shows the probability of support for an increase of the child benefit at the different levels of support for restricting immigrants' rights to social assistance. The support decreases with restrictive view in both groups, but does so more rapidly in the “newly arrived immigrant” treatment group. The propensity to withdraw support is more strongly concentrated among those with a restrictive view in Group A compared to Group B. There is an ideological divide also among those receiving the EEA treatment (which we can also see in Table 1 since the sum of the restrictive coefficient and the interaction term is positive), but our key point is that position switching is substantively and significantly less ideological. Also many immigration liberals have second thoughts about raising the child allowance when they are reminded that benefits can be exported to other EEA countries.

In a robustness check (see online appendix) we interact the treatment indicator with the education dummies to make sure that the interaction between treatment and view on immigrants' rights are not purely picking up treatment heterogeneity from education-based divisions. This robustness check produces the same results as above, since the interaction between ideology and treatment barely moves when adding these controls. The same is true if we instead control for heterogeneity on (other) economic variables like employment or income (see online appendix).

Conclusion

Our survey experiment on support for an increase in the child allowance produces three key findings. First, we find that the persuasion cue about newly arrived immigrants' access to the benefit has only a fairly modest negative impact on the support for increasing the child allowance, and the suggestion to discriminate against immigrants does not persuade many initial opponents to change their mind. Welfare nationalism understood as unwillingness to share benefits with immigrant families in general does not appear to have serious negative implications for the support for raising the child allowance. The degree of sensitivity to persuasion with reference to «newly arrived immigrants» observed here, is much smaller than that found in Bay and Pedersen (2006) with respect to support for a proposal to introduce a basic income. Compared to introducing a basic income, raising the child allowance as an anti-poverty measure is of course much less radical and less vulnerable to suspicions that it will impact on immigration flows and undermine work incentives.

The second finding is that the cue about labor migrants' access to benefits (also) for children living in another EEA-country has a very large negative impact on the support for increasing the child allowance, and quite a few of those who initially oppose raising the benefit change

their mind when offered the opportunity to withhold benefits from the children living in the country of origin. Thus, the impact of the persuasion cue which refers to labor migrants and highlights a potential violation of the ideology behind the Norwegian child allowance (the residency of mother/child criterion) has a significantly stronger impact than the cue which refers to immigrant families in general – both in a statistical and substantive sense.

The third finding is that the EEA-treatment not only affects respondents who are generally skeptical towards immigrants and their access to welfare benefits (for cultural and/or economic reasons), but it also moves respondents who otherwise hold pro-immigrant views away from supporting an increase in the child benefit. We believe that this provides indirect support for our expectation that the reaction to the EEA treatment is partly motivated by a perceived violation of the residency criterion and not just driven by particularly negative attitudes towards labor migrants from Eastern Europe. In any case it shows that the EEA cue has a more broad ideological appeal, and its political clout is therefore potentially much larger.

Our results on this point are consistent with results from a recent survey experiment on Swedish respondents reported in Hjort (2016). The primary conclusion of this study is that information cues about labour migrants – whether they come from a more culturally distant EU country (Bulgaria relative to the Netherlands) and how many children they have – affect the propensity to support discriminating against this group in the access to the Swedish child allowance. However, Hjort's (2016) results further indicate a strong reaction in favour of discrimination triggered by information that the labour migrant receives benefits for children living in the home country. This latter result is directly parallel to our results.

Pointing to labour immigrants' right to export the child benefit in the political debate appears to be a potentially very effective strategy for politicians opposed to an increase in the benefit. And conversely, to restrict the possibility of exporting the child benefit appears as a promising strategy for politicians who would like to see an increase in the child benefit. This latter policy option has however so far been blocked by existing EU regulations. During the European Council meeting in February 2016 the British Government managed, with support from the Nordic member countries, to negotiate a change in the coordination rules allowing affluent, high cost member states to pay child benefits at a lower rate if they are exported to poorer, low cost member states (European Council 2016). The more radical idea of changing the underlying principle of coordination with respect to this particular benefit type, from "Lex Loci Laboris" to "Lex Loci Domicilii" (Christensen and Malmstedt 2000), does not appear to have been on the agenda.

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Online appendix

The experiment

In the first step, respondents are asked a question tapping the support for increases in the child benefit: “Det har i den senere tid vært ført en diskusjon om barnetrygden bør økes. Hvordan stiller du deg til et forslag om å øke barnetrygden? Bør barnetrygden økes? Ja, nei” [There has recently been an increase in the number of children living in poverty in Norway, which has led to a discussion on whether support for the child benefit should be increased. What is your view on a proposal to increase the child benefit? Should the child benefit increase? Yes, No]

In the second step the respondents are randomized into two groups, group A and group B, where the question wording depends on the answer to the question in the first step. Those supporting an increase are randomized to groups A1 and B1, while those opposing the increase are randomized to groups A2 and B2:

Group A:

Intro: “Nå er det jo slik at nyankomne innvandrerfamilier har samme rett til barnetrygd som norske familier.” [Today, newly arrived immigrant families have the same right to the child benefit as Norwegian families.]

Group A1: “Mener du likevel at barnetrygden bør økes? Ja, nei, vet ikke” [Do you nonetheless support an increase in the child benefit? Yes, No, Don’t know]

Group A2: “Ville du støttet forslaget om å øke barnetrygden, hvis ordningen bare gjaldt barn av norske statsborgere? Ja, nei, vet ikke” [Would you support an increase in the child benefit if eligibility was restricted to children of Norwegian citizens? Yes, No, Don’t know]

Group B:

Intro: “Nå er det jo slik at arbeidsinnvandrere fra EØS-land har samme rett til barnetrygd som norske familier selv om de har barn som bor i hjemlandet” [Today, labor immigrants from EEC countries have the same right to the child benefit as Norwegian families even if their children live in the home country.]

Group B1: “Mener du likevel at barnetrygden bør økes? Ja, nei, vet ikke” [Do you nonetheless support an increase in the child benefit? Yes, No, Don't know]

Group B2: “Ville du støttet forslaget om å øke barnetrygden, hvis ordningen bare gjaldt barn som bor i Norge? Ja, nei, vet ikke” [Would you support an increase in the child benefit if eligibility was restricted to children of Norwegian citizens? Yes, No, Don't know]

Table A1: Descriptive statistics. N=1558

	Mean
Male	0.49
Compulsory education	0.08
High school education	0.33
University/college education	0.54
Missing education	0.05
Age: 18-30	0.15
Age: 30-59	0.54
Age: Above 59	0.31

Table A2: OLS Regression. Test of balance between Group A (=0) and B(=1).

	(1) Group B	(2) Group B	(3) Group B	(4) Group B	(5) Group B
Support increase	-0.037 (-1.430)				-0.029 (-1.115)
Male		0.019 (0.760)			0.016 (0.615)
High school			-0.052 (-1.105)		-0.046 (-0.957)
Uni/college			-0.034 (-0.753)		-0.032 (-0.707)
Missing edu			-0.064 (-0.908)		-0.062 (-0.864)
Age: 30-59				0.040 (1.074)	0.032 (0.848)
Age: Above 59				0.055 (1.382)	0.041 (0.996)
Constant	0.521*** (27.156)	0.491*** (27.615)	0.538*** (12.976)	0.462*** (14.149)	0.514*** (9.113)
Observations	1,558	1,558	1,558	1,558	1,558
R-squared	0.001	0.000	0.001	0.001	0.003
F-test			0.501	0.961	0.695
Prob > F			0.682	0.383	0.676

Note: Robust t-statistics in parentheses. Reference categories: Compulsory education, Age: 18-30.*** p<0.01, ** p<0.05, * p<0.1.

Table A3: Should the child benefit increase? The dependent variable is response after treatment. The sample is restricted to those who initially opposed an increase in the benefit.

N=676.

	Support vs oppose	Don't know versus oppose
Restrictive X EU/Child treatment	-1.51 (1.03)	-1.41** (.94)
Restrictive	3.73*** (.88)	2.83*** (.78)
EU/Child treatment	2.15*** (.79)	1.90*** (.66)
High school education	.11 (.39)	-.78 (.41)
University/college education	-.71 (.39)	-.45 (.36)
Missing education	.15 (.58)	.09 (.56)
Age: 30-59	-.50 (.42)	-.91** (.41)
Age: Above 59	.44 (.44)	-.39 (.41)
Constant	-3.89***(.86)	-2.89***(.74)

Note: Standard errors in parentheses. Reference categories: Compulsory education, Age: 18-30. *** p<0.01, ** p<0.05, * p<0.1.

Table A4: Should the child benefit increase? The dependent variable is response after treatment. The sample is restricted to those who initially supported an increase in the benefit. N=863.

	Oppose vs support	Don't know versus support
Restrictive X EEA treatment	-2.34*** (.89)	-1.70** (.78)
Restrictive	4.18*** (.80)	2.04*** (.63)
EEA treatment	3.41*** (.66)	2.20*** (.48)
High school education	.57 (.39)	-.10 (.37)
University/college education	.09 (.40)	-.41 (.38)
Missing education	.36 (.60)	.40 (.55)
Age: 30-59	-.70** (.29)	-.90** (.31)
Age: Above 59	.03 (.31)	.14 (.31)
Employed	-.05 (.26)	.33 (.27)
Income below median	-.06 (.26)	.64** (.29)
Missing income	.66* (.40)	.91** (.44)
Constant	-4.96***(.79)	-3.52***(.65)

Note: Standard errors in parentheses. Reference categories: Compulsory education, Age: 18-30. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A5: Should the child benefit increase? The dependent variable is response after treatment. The sample is restricted to those who initially supported an increase in the benefit.

N=863.

	Oppose vs support	Don't know versus support
Restrictive X EEA treatment	-2.19** (.89)	-1.82** (.78)
Restrictive	4.01*** (.80)	2.04*** (.63)
High school education X EEA treatment	1.42* (.77)	-.26 (.77)
University/college education X EEA treatment	1.30 (.79)	-.48 (.77)
Missing education X EEA treatment	.69 (1.22)	-1.43 (1.07)
EEA treatment	2.12** (.95)	2.63*** (.83)
High school education	-.30 (.57)	.05 (.62)
University/college education	-.75 (.59)	-.26 (.63)
Missing education	.09 (.95)	1.36 (.79)
Age: 30-59	-.70** (.28)	-1.00** (.29)
Age: Above 59	.02 (.30)	-.01 (.30)
Constant	-4.12*** (.80)	-2.97*** (.71)

Note: Standard errors in parentheses. Reference categories: Compulsory education, Age: 18-30. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table A6: Should the child benefit increase? The dependent variable is response after treatment. The sample is restricted to those who initially supported an increase in the benefit.

N=863.

	Oppose vs support	Don't know versus support
Restrictive X EEA treatment	-2.307*** (0.884)	-1.741** (0.775)
Restrictive	4.100*** (0.792)	2.016*** (0.628)
Employed X EEA treatment	-0.083 (0.468)	0.188 (0.468)
Employed	0.020 (0.412)	-0.017 (0.384)
EEA treatment	3.422*** (0.699)	2.122*** (0.536)
High school education	0.577 (0.390)	-0.176 (0.367)
University/college education	0.063 (0.397)	-0.617* (0.370)
Missing education	0.490 (0.595)	0.403 (0.541)
Age: 30-59	-0.684** (0.284)	-1.069*** (0.302)
Age: Above 59	0.021 (0.305)	-0.031 (0.299)
Constant	-4.911*** (0.745)	-2.637*** (0.556)

Note: Standard errors in parentheses. Reference categories: Compulsory education, Age: 18-

30. *** p<0.01, ** p<0.05, * p<0.1.

Table A7: Should the child benefit increase? The dependent variable is response after treatment. The sample is restricted to those who initially supported an increase in the benefit. N=863.

	Oppose vs support	Don't know versus support
Restrictive X EEA treatment	-2.428*** (0.902)	-1.736** (0.773)
Restrictive	4.261*** (0.813)	2.053*** (0.626)
Below median income X EEA treatment	0.079 (0.494)	-0.200 (0.530)
Missing income X EEA treatment	-0.610 (0.787)	-0.471 (0.827)
Below median income	1.074* (0.644)	1.152* (0.648)
Missing income	-0.093 (0.433)	0.649 (0.444)
EEA treatment	3.497*** (0.728)	2.386*** (0.612)
High school education	0.581 (0.394)	-0.079 (0.368)
University/college education	0.102 (0.406)	-0.399 (0.378)
Missing education	0.373 (0.603)	0.524 (0.535)
Age: 30-59	-0.689** (0.292)	-0.841*** (0.303)
Age: Above 59	0.056 (0.304)	0.092 (0.300)
Constant	-5.096*** (0.819)	-3.401*** (0.680)

Note: Standard errors in parentheses. Reference categories: Compulsory education, Age: 18-

30. *** p<0.01, ** p<0.05, * p<0.1.