Migration and Welfare State Spending

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New patterns of international migration have altered the demographic landscape of liberal-democratic countries. New forms of difference have generated new political pressures and sparked debates about traditional conceptions of identity and community, as well as the rights and mutual obligations embedded in citizenship. One sector where the new demography may have changed the rules is the welfare state. Economists often predict that international migration will drive growth in the welfare state. Political scientists and sociologists, along with some economists, predict the opposite. The burden of evidence to date favours the latter prediction, but in truth almost none of this literature goes directly to the impact of international migration. Almost without exception, the data are about ethnic diversity, where the latter is often the residue not of migration but of arbitrary boundary-making or historical patterns of racial domination. Sometimes the evidence is subnational.

The principal exception is Soroka et al. (2006), which finds a negative relationship between international migration and destination-country social spending. This paper builds upon that earlier investigation in two ways. First, we update the general claim and find that the relationship is even more robust than before. Second, we are now able to disaggregate social spending into sub-domains, such as unemployment benefits, pensions, and the like. Where our earlier work barely allowed us to scratch the surface of causal inference, disaggregation enables us to compare domains for their political vulnerability.

Migration and Social Welfare Spending

Where the relationship between migration and welfare state spending is concerned, two strikingly different possibilities appear. One is that a large inflow of migrants will drive up social spending, especially in generous welfare states. Most of the protagonists of this view are economists. This possibility reflects two expectations with both academic support and public resonance. The first is that migrants coming to the developed world from developing countries will be comparatively low-skilled and poorly educated, relative to both their source and host countries (Borjas, 1994). The second is that these low-skilled migrants, well aware of the notable variance in welfare state generosity, will cluster in more generous welfare states. This "welfare magnet theory" (Borjas, 1999) suggests that migrants are more likely than native-born welfare recipients to engage in welfare shopping; this greater propensity toward welfare shopping results from the fact that for an immigrant, the cost of migration is offset by both lower wage dispersion in host countries and the more generous welfare state (whereas for the native-born, the latter factor alone would have to justify the cost of moving).

Both the accuracy and the generalizability of these results have been the subject of considerable debate. On the accuracy question, for instance, Zavodny (1999) argues that Borjas' findings are simply the result of the clustering of migrants in certain states where

immigrant populations are already large, as they attempt to take advantage of pre-existing immigrant networks. On generalizability, the applicability of Borjas' US-focused conclusions to international decisions about migration destination is contested — numerous other factors are at work when migrants choose among destination countries (as opposed to simply choosing among US states). In an international setting, migration policy regimes, family reunification options, and cultural/linguistic similarities can all have considerable roles in directing the flow of migrants, with the result that the effect of welfare generosity may (at least in some instances) disappear (cf. Pedersen et al., 2004; Peridy, 2006). Furthermore, it is also possible that the relationship highlighted by the welfare magnet theory is endogenous, with the composition of immigrant groups and/or policy changes in reaction to immigration impacting unemployment benefit spending (Giulietti et al., 2013).

Existing cross-national research on the matter is divided. Concerns that generous welfare benefits may negatively affect levels of labour market participation of migrants have found some support in research (e.g. Constant and Schultz-Nielsen, 2004). Other researchers find that migrants are – albeit only marginally – more likely than non-migrants to be welfare recipients (Borjas and Hilton, 1996; Brücker et al., 2002). Moreover, the effect of migration on factor prices may in some instances counteract these negative effects (see Razin and Sadka, 2000). In general, the conclusion here suggests that migration in its current form is associated with somewhat smaller increases in welfare state expenditures (Nannestad, 2007).

Most of the critiques just mentioned do not impugn the basic observational pattern; they question the causal ordering or suggest conditions. A more rounded critique argues that the primary effect of international immigration is negative — it leads to decreases in social spending. In this scenario, the impact is political rather than economic: increased migration shifts public attitudes towards a preference for welfare cutbacks. Regardless of whether migrants are actually choosing their destinations on the basis of welfare state generosity or are more welfare state-reliant than native-born populations, concern about migration amongst native-born populations decreases aggregate support for redistributive policies.

It is certainly true that public concern about migration and immigrant reliance on welfare benefits is on the rise throughout the developed world. "Welfare chauvinist" attitudes and political parties have seen a particular increase in Europe. This European phenomenon is in line with work focused on the US as well. The account offered in well-known work by Alesina and colleagues (Alesina et al., 1999; Alesina and Glaeser, 2004), for instance, is that anti-welfare state politicians in the US play upon racial stereotypes in their push to limit welfare benefits.

The political motivations underlying welfare chauvinism may not be purely fiscal in nature. The literature on anti-immigrant attitudes suggests that economic concerns are often secondary to broader anxieties about the changing ethnic makeup of the national community (Sniderman et al., 2000; 2004; 2007). Either way, one consequence is that there

is a link between *increases* in migration and *decreases* in social spending, and in redistributive policy more generally.[1]

Alongside work that finds a negative relationship between migration and welfare spending, however, there is a growing body of work suggesting that the main effects of migration on social spending are limited, or require mediation by other factors. Lipsmeyer and Zhu's (2011) examination of EU states, for instance, suggests that increased migration may increase welfare benefits if left-party strength or union density is high. Taylor-Gooby (2005) similarly finds that the effects of the left's strength can counteract those of diversity. In a study of twenty-one countries over twenty years, Banting et al. (2006) find that the impact of social spending may be weakly conditional on the strength of multicultural policies.

Survey data also suggest that the immigration-redistribution link is nuanced. Mau and Burkhardt (2009) find that migration and ethnic diversity do not have a simple direct effect on public attitudes, but rather are mediated by institutional factors such as "whether inclusion is institutionally organized and whether social benefits schemes have been constructed in such a way that they reinforce or lessen conflicts over redistribution" (226). An examination of survey data in 17 European countries by Burgoon et al. (2012) suggests that working in an occupational sector that has a high percentage of foreign-born employees actually increases support for redistribution, due to increased economic insecurity. Emmenegger and Klemmensen (2013), by contrast, stress the importance of individual motivations other than simple self-interest (such as egalitarianism and humanitarianism) in moderating the perceived relationship between attitudes toward immigration and redistribution.

In short, the relationship between migration and welfare state spending is complex, mediated by a number of factors, and may also be changing over time. We address this last possibility first by seeing if the relationship changes with the addition of ten new years of data. We also explore one facet of conditionality, by examining effects in nine social welfare sub-domains. We do so with the expectation that if migration affects social expenditures, it will affect different types of welfare programmes in different ways.

Broadly speaking we see two possibilities. First, increased migration may reduce spending across all social programs, regardless of how heavily they may be used by immigrants or how exposed they may be to moral hazard (*Hypothesis 1*). Perhaps political backlash

[1] Note that these aggregate findings are in line with individual-level survey research on public attitudes about welfare and redistributive policy as well. Gilens (1999), for instance, argues that much of the American public's hostility to the welfare state is derived from an erroneous belief by white Americans that the majority of welfare recipients are African-American, and that African-Americans on the whole lack a sufficient work ethic – thereby rendering them generally underserving of benefits. Luttmer (2001) finds that racial group loyalty plays an important role in individual support for welfare spending, with individuals increasingly likely to support welfare spending as the proportion of local welfare recipients from their racial group increases (and vice versa). See also, e.g., Razin et al. (2002); Van Oorschot (2011).

against increasing migration pushes voters and governments to the right, and this ideological shift reduces support for social welfare programs of all kinds. As a result, the negative effects of increasing migration on social welfare spending would be equally evident across all social welfare policies. A second possibility is that increased migration reduces commitment to specific programs that are – or are perceived to be – more open to use by migrants (*Hypothesis 2*). In this instance, the effects of increasing migration on social welfare spending are policy-specific, and evidenced in certain types of programs. In particular, we see pensions and labour-market related programs as most vulnerable. Other programs may be thought of as not implicated in changing demography or has having strong countervailing political claims — health care comes to mind. Lastly, an intermediate pattern is possible. Migration-driven retrenchment may be ubiquitous but vary in its strength along the lines in the preceding paragraph; that is, migration may matter, but to quite varying degrees across both countries and domains (*Hypothesis 3*).

Data

For social spending we draw on OECD SOCX data. The most recent version includes total social spending alongside spending in nine subdomains: old age, survivors, incapacity-related, health, family, active labour market policies, unemployment, housing, and "other." Spending variables, whether as levels or changes, are represented as percentage or percentage points of GDP. Expressing spending as a percentage of GDP has both advantages and disadvantages. A negative relationship between migration and social spending could reflect decreases in the numerator, as anti-immigrant politics reduces outlays, precisely the mechanism of interest. But it could also reflect increases in the denominator, reflecting a boost to GDP that comes with immigration, either as cause or effect. In the end normalizing to GDP makes spending comparable across countries and over long periods. This decision, and our modelling more generally, follow directly on past work — in particular, Soroka et al. (2006), which drew in turn on on a vast and valuable literature on time-series cross-sectional (TSCS) modelling of social spending (e.g., Swank, 2002; Huber and Stephens, 2001; Hicks, 1999). The OECD is also the source for other demographic data, also reported annually.

To extend the analysis back to 1970, we link the current SOCX dataset to an older OECD dataset dating back, in most countries, in the 1960s. We combine the two using backward interpolation, where percentage changes in the new series are estimated backwards, year-by-year, based on the percentage changes in the old series. The method is not perfect, as the series have slightly different definitions; that said, there are relatively small differences between the new and old series, and this is the only means by which to get a spending series that runs for 47 years across multiple countries. This spending dataset has annual values.

Migration data have a different source and a different time frame. Here the source is the United Nations, *Trends in International Migrant Stock: The 2008 Revision*. These data are

noisier than the various OECD series. In particular, they are not annual data but rather are reported in five-year intervals, roughly tracking national censes. But the timing of the census differs from country, so the series has temporal noise. Migration statistics, levels or changes, are expressed as percentages of the total population.

We focus on the 17 OECD countries that are advanced capitalist economics with long-standing democratic systems. The exclusions are (a) Greece, Spain and Portugal, each of which had dictatorships for the early period and has a late-developing welfare state, and (b) Switzerland, which presents an impenetrable challenge because of a society-wide change in social welfare spending, some of which seems to be a shift in accounting methods, in the mid 1990s (see Soroka et al. 2006).

Analysis

We proceed by stage. First up is a highly aggregated, simple cross-sectional analysis that reproduces the logic of Soroka et al. (2006). We then extend the analysis to a time series-cross section setup with a five-year frequency. This enables controls for competing hypotheses and to represent the "workhorse" elements in the standard models of welfare-state growth. Then we move down the ladder of policy aggregation, to look at the nine spending domains separately. Again, we start with the simple cross-section and then move to the five-year frequency.

Total Social Spending

First consider the basic bivariate cross-sectional relationship. Figure 1 shows the 1970 -2007 link between change in the foreign-born share of the population and change in social spending as a percent of GDP. The emphasis on change is important. *Level* of migration (as measured by the proportion of the population that is foreign-born) is a standard variable in the literature; but our past work suggests that it is not overall levels of migration that matter so much as changes in migration flows. Countries with high but stable foreign-born populations seem to have less difficulty in sustaining their historic welfare commitments than countries with smaller but rapidly growing migrant populations (Soroka et al. 2006). Similarly, the size of the welfare state is set by historic patterns, reflecting coalitions in place before the great post-1970 expansion in international migration. Also, the logic of the argument seems to us to pertain to the rate of growth in the welfare state, not to static levels. And nowhere does social spending actually decrease over this period, as Figure 1 shows. The scatterplot in the figure is accompanied by the bivariate OLS regression line and the 95% confidence interval for point estimation. The figure reveals a long-term relationship between immigration and social spending that is strong, linear, and negative.[2]

[2] We also tested for the possibility that effects are nonlinear, by using changes in foreign born in its quadratic form, but this only weakens the estimated relationship. The linearity in Figure 1 seems clear. Also clear is that the linear relationship has strengthened relative to the 1970-98 pattern indicated in Soroka et al. (2006), Figure 10.1 and Table 10.2.

[Figure 1 about here]

Parameters for the relationship in Figure 1 appear in the first column of Table 1. They suggest that each percentage point increase in foreign born is associated with, on average, a 0.77 percentage-point reduction in social spending growth. The second model in Table 1 tests for the possibility that the bivariate relationship is an artifact of the starting points in either spending or immigration. Higher levels of spending in the early years of our analysis may constrain potential upward changes in spending over the time period, and the same may be true for the initial size of the foreign-born population. As it turns out, including the 1970 levels makes little difference; neither is statistically significant, and the estimated effect of migration change barely changes.[3]

[Table 1 about here]

The evidence in Figure 1 and Table 1 is spartan, to say the least. It will be more persuasive if the basic relationships hold as we move to a more elaborate suite of controls and higher temporal frequencies. Controls also enable us to gauge if any of the effect of immigration is indirect, through shifts in the party system for instance. In constructing the estimations, we respect the frequency of the migration data, so five years is the minimum frequency and the only one we report in tables. To get to annual estimations would require interpolation for the immigration data, which would build in serially correlated errors as an artifact. Equally to the point, migration requires some time to pass for its effects to be felt in policy. This will be visible in the tables that follow. Instead of interpolation for missing years on the dependent variable, we aggregate independent variables across years to reflect the unfolding of policy change. For instance, the dependent variable is the difference between current five-year-averaged spending and lagged five-year averaged spending (1995-1999 minus 1990-1994, and so on). The same is true for other variables that appear as changes. Only the change in the percentage foreign-born is based solely on data spaced five years apart.

Our choice of controls mirrors the welfare state literature; the justification for each is given in some detail in Soroka et al. (2006). The variables are: (1) population under 15 years, (2) population over 64 years, (3) trade union density, (4) female labour force, (5) unemployment, (6) inflation, (7) percent right-wing cabinet posts, and (8) percent left-wing cabinet posts. These variables capture the main demographic, economic and political

[3] Adding the other control variables to the cross-sectional models in Table 1 also makes little difference to the estimated impact of migration. The controls are not statistically significant either — there are after all only 17 cases in these models. Results are available upon request.

drivers of total social spending.[4] Political factors are lagged one year before collapsing to the five-year average, given that expenditures in the current year are the consequence of budgetary policy in the previous year. This means that the partisanship of government is included at *t-1,* and so is immigration. All other variables appear as changes to the current year.[5]

Table 2 starts with current changes in spending regressed only on lagged changes in the percent foreign born; the second adds lagged levels of spending, on the possibility that system has autoregressive tendencies; the third adds both the remaining political effects, that is, the measures of partisanship of government in lagged levels, alongside all the economic variables in concurrent changes. Results appear for both random- and fixed-effects estimations of each model, to set upper and power bounds on coefficients. We do not have a compelling case to prefer one model over the other. Random effects setups use information more efficiently and yield more stable estimates. But the estimates may be biased because of clustering of observations in countries. Fixed effects setups remove the source of bias, but much else besides, such that they can be quite inefficient.

[Table 2 about here]

The critical coefficients, for changes in the percentage foreign born, are in the top row. Consider first the random-effects setup. Immigration is significant across all of the random-effects estimations. Controlling for previous spending reduces the current effect somewhat; part of what the bivariate estimation models is a regression artifact. Entering the full suite of controls reduces the direct effect of migration, suggesting that some of its total impact is through other factors. We will discuss those factors when we turn to the fixed-effects estimations. Suffice it to say here that immigration retains a robust effect even with all the rivals in the setup. Coefficients are smaller here than in the cross-sectional setup, but comes as no surprise.

The fixed-effects setup is harsher on the effect of immigration. In the bivariate case, immigration still comes through with a powerful effect, not much smaller than the estimate in the random-effects setup. And consider what this means: *none* of the effect of

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[4] GDP per capita has been included in past models, in our work and others', but the longest existing series is still missing for roughly one seventh of the cases in our final model. Adding it, once both unemployment and inflation, seems to make little difference to the estimated effects of other variables, however, so we exclude it for the models below. There may also be other controls that are important once we move from total social spending to by-domain models. For the time being, however, we do not consider these additional controls.

[5] We might view trade union density as both a political and an economic variable. The economic effect operates through the impact on wages and employer-based protections; as a result, union density might actually reduce the level of government social spending, since it involves building other forms of protection for workers. The political effect sees strong unions in alliance with strong social democratic parties provide the political underpinnings of the universalist welfare state. Since trade unionization is in this case just a control variable, we let the data decide: having tried models with both current changes and lagged levels of trade unionization, the former were consistently statistically significant and the latter were not. We thus include trade unionization in current changes.

immigration here reflects differences across countries, in contrast to the random-effects estimate which combines variance across countries with that over time. The coefficient in column 4 thus reflects the over-time impact of immigration on spending, within countries. Once other variables enter, the immigration effect shrinks, a bit more in absolute terms at each stage than in the random-effects sequence. Much more weight is carried by the lagged spending term. And some of the story comes out in the demographic/economic coefficients on the under-15s, unemployment, and female labour force participation.

Exploring the relationship between migration and the other independent variables in the fixed-effects may be telling, in that it speaks to the possibility that migration matters for social spending not just directly, but indirectly (through demographics, for instance.) It is accordingly worth mulling over some diagnostics. Which demographic/economic variables might be capturing the impact of migration on social spending? Intuition (and past work) suggests the potential importance of three variables in particular, each of which has a significant impact on social spending in Table 2: (1) unemployment, which may be positively related to migration, (2) under-15s, also positively related to migration, and (3) female labour force participation, negatively related to migration.

Table 3 presents some diagnostic analyses for each of these three variables. The first row shows the coefficient for change in migrant stock at (*t-1*) in a TSCS fixed-effects estimation where each of the three potential mediating variables (in changes at *t*) is the dependent variable.[6] These bivariate fixed-effect regressions are the simplest route to identifying the within-country correlation between changes in migrant stock and changes in each of unemployment, under-15s, and female labour force participation. The second row of Table 3 then shows the estimated coefficient for changes in migrant stock in the fully-specified estimation in Table 2 — except in this case we drop each of the potential mediators in turn. Here we can see the extent to which each mediating variable captures the indirect impact of migration on social spending.

[Table 3 about here]

Results in Table 3 are telling. To start, there is no impact of migration through unemployment. Taking both cross-sectional and longitudinal variation into account, the overall within-sample correlation between changes unemployment and lagged changes in migration is just -0.08 (p=0.34), and that correlation is not markedly stronger when we look at within-country variation only. (In fact, just 7 of our countries show positive correlations between these unemployment and migration variables, based on the 5-year TSCS data.) The fixed-effects model regressing changes in unemployment on lagged changes in migration, in the first column of Table 3, reveals no significant relationship between the two variables. (This fits with a growing body of work suggesting that migration is at the

[6] Note that the timing of migration (at t-1) and other demographic varaibles (at t) corresponds to what we have used in the regressions shown in Table 2. Results here thus speak directly to issues of collinearity in the models of social spending.

most only very weakly connected to unemployment rates. For a recent review, see, e.g., Brücker 2012.) And dropping unemployment from the fully-specified model makes no real difference to the — still insignificant — migration coefficient (also in the first column of Table 3).

Population changes are not the culprit either, although in this case the correlation with migration is much stronger. Changes in the percent of the population under 15 are positively correlated with lagged changes in migration at 0.19 (p=0.02) overall. Within-country correlations are positive in all but four cases; and the fixed-effects model regressing population change on lagged migration change reveals a statistically significant coefficient. That said, as shown in the second column of Table 3, dropping the population variable from the model on social spending leads to no change in the coefficient for migration. Migration matters for the size of the young population, to be sure; and based on results in Table 2, under-15s clearly increase social spending. But it is not the impact of under-15s that reduces the coefficient for migration.

Dropping female labour force participation does, however. The overall bivariate correlation between changes in female labour force participation and lagged changes in migration is weak and insignificant (-0.08, p=0.33); but a fixed effects model regressing the former on the latter shows a powerful negative impact of migration on female labour force participation (third column, Table 3). That impact is not equal across countries — just 10 countries show a clearly negative relationship between the two variables. But existing work does signal a negative relationship (in the Canadian case, see Kustec 2012); and taken on average, female labour force participation clearly matters for the estimate of migration effects here. As shown in Table 3, dropping changes in female labour force from the final fixed-effects model produces a coefficient for migration (-0.265) that is nearly identical to that in the model including only the lagged spending control only (-0.286).

What, in sum, do results in Tables 2 and 3 reveal about the connection between spending and migration? There quite clearly is a negative relationship between the two: if the bivariate random-effects model captures the total effect, a one percentage-point increase in foreign-born reduces growth in social spending by 0.57 percentage points. In the final, fully-controlled, random-effects model, the impact would be, on average, a 0.36-percentage point reduction.[7] And there is evidence of a (somewhat smaller) an impact in the more constraining fixed-effects model as well — though the within-country impact of migration is captured in part indirectly, through the negative relationship between changes in migration and changes in female labour force participation.

[7] Note that, given that we are using five-year averages, our changes include a period of up to ten years — from the first year of the last period to the last year of the first period.

By-Domain Social Spending

Are there differences when we shift to by-domain results? Table 4 shows the critical coefficients from a re-estimation of our models, this time separately for each domain. The first set of coefficients is based on the same cross-sectional model as in Table 1. By-domain spending is not available for the entire 1970-2007 period, so the estimations in this case rely on 1980-2007 changes. This is true for most cases, at least — there actually are several countries in which spending in certain domains is not available until mid-way through the 1980s, so the Table 4 models are based on slightly varying time periods, beginning in the first year in which spending is available in each country, in each domain. Diagnostic data are included in the Appendix (in Appendix Tables 1 and 2). So too are the complete regression models (in Appendix Table 4). For economy of presentation, we include just the coefficient for changes in migration in Table 4.

The same is true for the by-domain TSCS models. Again, we repeat the analyses above, this time for each of nine sub-domains. We use simplified versions of the model in Table 2, however — including just changes in migrant stock alongside lagged levels of spending.[8] Full results are in Appendix Table 5; in Table 4, we show just the coefficients for changes in migrant stock.

[Table 4 about here]

Listing the TSCS estimates alongside cross-sectional estimates has the advantage of making a comparison across modeling strategies especially easy. Taken together, Table 4 results make clear that the impact of immigration is not felt equally across all domains. Indeed, although most of the nine coefficients in the cross-sectional models are negative, only a few are statistically significant. The same is true for the TSCS estimates. The domains that stand out in the first column are Pensions and Unemployment. This variation in the effect of migration across domains points to the possibility that the impact of migration is greatest for programs on which migrants might be especially dependent (*Hypothesis 2*) — or, perhaps more to the point, programs on which migrants are *seen* to be particularly dependent. With this in mind, the relatively strong effect of migration on spending on Unemployment may not come as much of a surprise.

TSCS estimates alsoconfirm the impact of migration on unemployment spending. This is likely not because migration lowers unemployment — recall that our diagnostics above suggest no significant relationship between the two variables either way. Rather, the implication is that the effect of migration on unemployment spending is political rather than economic. And a similar effect is evident in Active Labour Market Programs (ALMP),

[8] The appropriate control variables will vary across sub-domains, of course — where age will matter for old-age spending, unemployment will matter for unemployment spending, and so on. Adding the most relevant variables in each case makes for less comparable models, however, so we opt for this more parsimonious strategy here.

nearly in the random-effects model and quite clearly in the fixed-effects model. The impact of migration on ALMP makes less sense than unemployment, insofar as one would expect such programs to increase the labour market participation of migrants. But again it may be politics that matters, not economics — it may be that perceived use by migrants is what matters to public support, and to thus government spending.

The impact of migration on pensions disappears in the TSCS estimations. The issue here, we believe, is the frequency domain itself. Pensions, especially non-contributory ones, *are* a flashpoint in high-immigration societies. But pension benefits are not like those in the rest of the welfare state. Most persons ultimately entitled to a pension are not currently receiving one. The clientele is broad, however, and dynamics in spending are sluggish. Structural change usually requires mobilization of political will and is rare. It strikes us as eminently reasonable that pensions respond to multi-decade changes in the foreign-born share of the population, but not to five-year ones.

There is one other change from cross-sectional models to TSCS estimates as well: a significant and negative impact in the "other" social policy category. The contents of this residual spending category varies somewhat one country to the next, but the SOCX definition is illuminating: "other" includes programs that do not fit into the other categories, including social expenditure related to immigrants and indigenous people, and more importantly (in terms of spending levels), income support and social assistance payments (OECD 2007). This other category thus makes up a negligible proportion of social welfare spending in some countries (such as Italy and France), though it makes up roughly 2.7% of total social spending on average. The major outlier is Canada, where "other" makes up a full 15% of social spending (See Appendix Table 2 for data on the distribution of total social spending across the nine subdomains.) "Other" spending clearly matters, then; it is made up of policies for which we expect that migration may have negative (political) effects on support; and this negative relationship is evident in the TSCS models.

That said, and just as importantly, there is no discernible impact of migration on spending in the remaining subdomains. Clearly, some areas of social spending are affected by migration, but others — and many of the largest in terms of levels of expenditure — are not. In sum, migration matters to social welfare expenditure; the effect appears to be more political than economic; and the impact is felt only in certain subdomains.

Discussion and Conclusions

The main objectives of this paper are to (a) examine the relationship between migration and social spending over an extended period, and (b) explore differences in the relationship between migration and social spending across subdomains. At a minimum, results suggest that there are some advances to be made by disaggregating social spending. But are there more specific lessons to be drawn from the preceding analyses?

We wish to highlight two points emerging in our earlier work which remain relevant here. First is the importance of focusing on changes in migration rather than levels of migration. Existing work often focuses on the latter, which presents several difficulties. Most importantly, it tends to emphasize cross-national variance rather than (within-country) variance over time — so analyses draw their power from variance *across* countries, even as hypotheses are based on political effects occurring *within* countries. In short, the current political effect of migration cannot be adequately observed without moving to an estimation that focuses on changes in immigrant flows. Second, the effect of migration takes place over the medium-term. Annual estimations are thus not the most appropriate way to capture the impact of migration; indeed, while the impact is clear in our five-year models, it is totally absent from annual models (see Appendix Table 3). This makes good sense. Public reactions to increasing migration are bound to take some time — there needs to be migration, the public needs to notice that migration and change their preferences accordingly, and then those preferences have to find their way into political and budgetary processes.

Results in this paper also point to a fruitful area for further work, as there is evidence that disaggregating spending has advantages. There is considerable variation in the impact of migration on spending across subdomains. In some domains – indeed, in most of them – there is only weak evidence of a relationship between migration and spending. There are, however, some domains in which the impact is clear, and the variation across domains is telling. The impact of migration is most pronounced in the Unemployment domain — one in which immigrant use may be perceived to be particularly high. ALMP and "other" spending show similar trends, and are similarly open to perceptions of immigrant use.

That said, we should not overstate the magnitude of the relationship between changes in migration and changes in social welfare spending. Results in Table 2, for instance, suggest that a one-percentage point increase in foreign born is related to a roughly 0.15-percentage point decrease in spending on Unemployment and a 0.09-percentage point decrease in Active Labor Market Programs, *ceteris paribus*. These effects do cumulate, both over time and across domains. As our estimations of total spending suggest: a one-percentage point increase in foreign born over 37 years is associated with a nearly 0.8-percentage point decrease in spending. Moreover, there is certainly a good deal of measurement error in our models, the likely result of which is underestimation of effects. Nevertheless, our estimations suggest that, although the relationship between migration and the welfare state is of real salience politically, the empirical effects may be both mediated and modest — not to mention constrained to a limited number of subdomains.

There clearly is more to do. Our analyses assume that the immigration-welfare state link is the same everywhere, such that different outcomes are solely the result of different values for component variables. But Figure 1 also strongly suggests that there is considerable scope for variation in response to immigration pressures. The pattern in the figure is clearly heteroscedastic: residuals get bigger as immigration changes get bigger. Note, for instance,

that the vertical gap between the Netherlands and Germany, with essentially identical (high) growth in the percentage foreign-born, is as large as that between Canada and France, with markedly different (low to moderate) migration histories. One possibility, probed in Banting et al. (2006), is the conditioning role of multicultural policy. Such policies may reshape the discourse of immigrant reception and buy insulation; or they may do the opposite. Another possible conditioning factor may be shape of the *ex ante* welfare state. Perhaps universal systems are more resilient in the face of immigration pressure than conservative or liberal ones; then again, perhaps not. Multiculturalism and pre-existing welfare regimes may interact. Alternatively, the simplest possibility that may be buried in Figure 1 is a distinction between refugee and non-refugee flows.

There is a strong debate about the potential tensions between migration and the welfare state. Existing work may both *under*- and *over*-state the impact of migration on welfare state spending. Our reading of that literature suggests that the negative impact of migration on welfare state spending, where it exists, can be mediated by various political and policy institutions. It follows that future work should take heterogeneity — in both spending domains and institutions — into account.

Appendix

Variable Sources

Social Welfare Spending: all measures are drawn from the OECD SOCX databases.

Migrants, % Population: data drawn from the UN, *Trends in International Migrant Stock: The 2008 Revision*; as data are reported in five year intervals, we use linear interpolation to fill in missing years.

Population under 15yrs: annual data from OECDStat.

Population over 64yrs: same as above.

Unemployment: same as above.

Female Labour Force: same as above.

Trade Union Density: same as above.

Right, % Cabinet Posts: data from Soroka et al. 2006 updated using Armingeon et al., Comparative Political Data Set III, 1990-2008.

Left, % Cabinet Posts: same as above.

[Appendix Tables 1 though 5 about here]

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Figure 1. Changes in Foreign-Born Population and Changes in Social Spending, 17-country OECD sample, 1970-2007

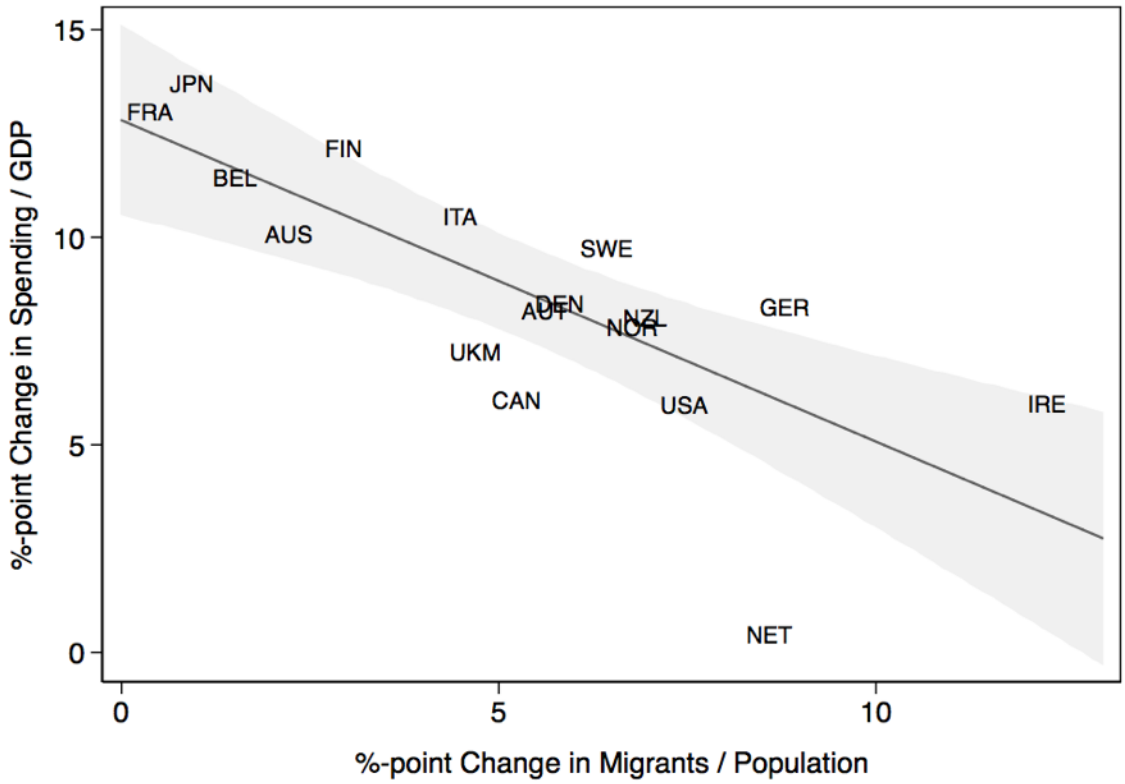

Table 1. Cross-Sectional Models, Changes in Total Social Spending, 1970 to 2007, 17-country OECD sample

	DV: Δ Spending, % GDP $_t$	
Δ Foreign Born, % Population$_t$	-0.774***	-0.764***
	(0.172)	(0.181)
Spending, % GDP$_{t=1970}$		-0.139
		(0.141)
Foreign Born, % Population$_{t=1970}$		-0.111
		(0.106)
Constant	12.815***	15.336***
	(1.060)	(2.311)
N	17	
Rsq	0.574	0.621

* p < .10; ** p < .05; *** p < .01. Cells contain coefficients from an OLS regression with standard errors in parentheses.

Table 2. Total Social Spending, 5-Year TSCS Analysis, 1970-2007

	DV: Δ Spending, % GDP $_t$					
	Random-effects			Fixed effects		
Δ Foreign Born, % Pop$_{t-1}$	-0.568**	-0.414*	-0.362**	-0.458*	-0.286	-0.188
	(0.232)	(0.219)	(0.169)	(0.274)	(0.219)	(0.182)
Spending, % GDP$_{t-1}$		-0.116***	-0.094***		-0.285***	-0.244***
		(0.026)	(0.025)		(0.035)	(0.044)
Right, % Cabinet Posts$_{t-1}$			-0.125			0.037
			(0.469)			(0.616)
Left, % Cabinet Posts$_{t-1}$			0.512			0.117
			(0.474)			(0.643)
Δ Population under 15yrs$_t$			0.432***			0.395***
			(0.133)			(0.131)
Δ Population over 64yrs$_t$			0.287			0.198
			(0.194)			(0.249)
Δ Unemployment$_t$			0.461***			0.443***
			(0.061)			(0.060)
12-month Inflation Rate$_t$			0.024			-0.073
			(0.045)			(0.051)
Δ Female Labor Force$_t$			0.241**			0.375***
			(0.111)			(0.139)
Δ Trade Union Density$_t$			0.073**			0.023
			(0.032)			(0.037)
Constant	1.520***	3.648***	2.776***	1.458***	6.811***	5.867***
	(0.211)	(0.514)	(0.780)	(0.231)	(0.685)	(1.134)
N	130					
N (panels)	17					
Rsq (overall)	0.045	0.175	0.570	0.045	0.164	0.415
Rsq (between)	0.440	0.049	0.508	0.440	0.013	0.010
Rsq (within)	0.024	0.344	0.578	0.024	0.387	0.639

* p < .10; ** p < .05; ** p < .01. Cells contain coefficients (with standard errors in parentheses) from a TSCS model, using a GLS estimation.

Table 3. Relationships Between Changes in Migrant Stock and Three Mediating Variables

	Y		
	Δ Unemployment$_t$	Δ Population under 15yrs$_t$	Δ Female Labor Force$_t$
Impact of Δ Foreign Born$_{t-1}$ on *Y* [a]	-.312 (.287)	.261** (.129)	-.437*** (.147)
Impact of Δ Foreign Born$_{t-1}$ on Δ Spending$_t$ when *Y* is removed [b]	-.185 (.223)	-.162 (.189)	-.265 (.184)

* p < .10; ** p < .05; *** p < .01. Cells contain coefficients (with standard errors in parentheses) from a TSCS model, using a fixed-effects GLS estimation.

[a] Coefficients are drawn from TSCS fixed-effects models in which each Y is regressed on Δ Foreign Born$_{t-1}$ (with no additional controls).

[b] Based on the fully-saturated models of social spending, as in Table 2, but where each *Y* is dropped from the estimation (holding the sample constant).

Table 4. Subdomain Social Spending, Various Models, 1980-2007

Domain	Cross-Sectional Models [a]	TSCS Models [b]	
		Random Effects	Fixed Effects
Old Age	**-.514*** (.118)**	-0.074 (0.109)	0.036 (0.110)
Survivors	-.028 (.077)	-0.073 (0.050)	-0.025 (0.051)
Incapacity	.026 (.090)	-0.060 (0.067)	0.004 (0.069)
Health	-.083 (.063)	0.026 (0.072)	0.109 (0.085)
Family	.012 (.062)	-0.053 (0.052)	0.024 (0.057)
ALMP	-.017 (.029)	-0.055 (0.038)	**-0.093** (0.039)**
Unemp't	**-.140** (.058)**	**-0.154** (0.073)**	**-0.155** (0.077)**
Housing	-.001 (.042)	-0.013 (0.023)	0.007 (0.020)
Other	-.023 (.026)	**-0.076** (0.034)**	**-0.070** (0.031)**

* p < .10; ** p < .05; ** p < .01. [a] Cells contain coefficients from an OLS regression with standard errors in parentheses. Complete results are shown in Appendix Table 3. [b] Cells contain coefficients (with standard errors in parentheses) from TSCS modelS, using fixed-effects GLS estimations. Complete results are shown in Appendix Table 4.

Appendix Table 1. Basic Descriptive Data for Countries Included in TSCS Analyses

Country	1970-2007 % change in Migrant Stock	1970-2007 % change in Spending	# Years in TSCS Models	Average Migrants (% of population)	Average Refugees (% of population)
Australia	2.216	10.094	43	20.197	0.535
Austria	5.613	8.239	39	10.958	0.407
Belgium	1.507	11.437	43	8.452	0.266
Canada	5.237	6.084	46	16.363	0.318
Denmark	5.809	8.420	37	4.842	0.574
Finland	2.944	12.161	46	1.453	0.078
France	0.378	13.041	46	10.348	0.289
Germany	8.792	8.325	46	7.899	0.643
Ireland	12.269	5.995	46	7.147	0.030
Italy	4.493	10.520	46	2.580	0.038
Japan	0.932	13.720	46	0.927	0.002
Netherlands	8.587	0.443	32	7.385	0.376
New Zealand	6.940	8.070	37	16.450	0.132
Norway	6.772	7.853	46	4.151	0.432
Sweden	6.433	9.751	44	8.639	0.951
UK	4.689	7.248	35	7.056	0.255
USA	7.457	5.977	46	8.585	0.216

Appendix Table 2. Descriptive Data SOCX Data, by subdomain

	Total Spending, % of GDP	Subdomain spending, % of Total								
		Old Age	Survivors	Incapacity	Health	Family	ALMP	Unemp	Housing	Other
Australia	13.1	25.6	2.4	12.3	33.5	13.6	2.5	7.3	1.8	1.4
Austria	24.0	39.2	8.4	10.5	23.0	10.9	1.7	4.1	0.4	1.2
Belgium	24.3	26.3	9.8	11.6	25.1	9.9	4.4	12.0	0.3	1.6
Canada	16.3	21.7	2.3	6.1	36.8	4.7	2.6	7.8	3.5	15.0
Denmark	24.8	28.0	0.1	14.2	20.2	12.4	5.3	15.1	2.4	3.8
Finland	23.2	30.0	4.0	16.4	23.1	11.9	3.9	7.8	1.1	1.8
France	24.3	36.5	6.7	8.7	25.7	10.6	3.5	5.3	2.9	0.7
Germany	23.9	36.3	7.0	8.3	30.0	7.7	4.1	5.1	1.1	1.1
Ireland	15.6	21.2	6.2	10.7	32.6	11.1	6.2	10.1	3.3	1.6
Italy	20.7	44.9	10.3	8.9	26.6	4.4	2.0	3.5	0.0	0.1
Japan	12.5	38.1	8.1	5.0	38.7	3.9	1.9	3.4		1.5
Netherlands	23.3	24.7	2.6	21.0	23.8	7.5	5.4	9.8	1.6	3.7
New Zealand	17.6	31.0	1.1	12.4	30.0	13.0	3.4	5.5	2.5	1.2
Norway	19.9	30.8	1.7	20.8	17.6	13.5	3.4	3.1	0.8	3.3
Sweden	28.2	30.3	2.2	17.0	23.8	12.6	6.2	4.2	2.5	2.3
UK	18.1	26.8	3.9	11.1	28.7	12.9	2.6	5.1	6.6	2.4
USA	14.1	36.8	6.4	7.7	37.3	4.3	1.3	3.1		3.3

Note: Cells contain mean values, based on annual data. Total spending is based on all years from 1970 forward; subdomain spending is based on all available data (from 1980 forward). Total spending is an annual average of spending/GDP. Subdomain spending is, for diagnostic purposes, shown as an annual average of the value as a percent of total spending — though note that analyses use subdomain spending as a percent of GDP.

Appendix Table 3. Annual TSCS Analysis, Total Social Spending 1970-2007

	DV: Δ Spending, % GDP $_t$			
Δ Foreign Born, % Population$_{t-1}$	-0.174	-0.043	-0.042	0.195
	(0.228)	(0.221)	(0.219)	(0.184)
Spending, % GDP$_{t-1}$		-0.022***	-0.027***	-0.024***
		(0.008)	(0.008)	(0.006)
Right, % Cabinet Posts$_{t-1}$			-0.061	-0.013
			(0.090)	(0.078)
Left, % Cabinet Posts$_{t-1}$			0.202**	0.164**
			(0.096)	(0.082)
Δ Population under 15yrs$_t$				0.426***
				(0.110)
Δ Population over 64yrs$_t$				0.284
				(0.175)
Δ Unemployment$_t$				0.397***
				(0.041)
12-month Inflation Rate$_t$				0.019**
				(0.009)
Δ Female Labor Force$_t$				0.181***
				(0.066)
Δ Trade Union Density$_t$				0.065***
				(0.024)
Constant	0.260***	0.667***	0.710***	0.521***
	(0.069)	(0.141)	(0.179)	(0.142)
N		710		
Rsq	0.001	0.031	0.044	0.332

* p < .10; ** p < .05; ** p < .01. Cells contain coefficients from a random-effects TSCS regression with panel-corrected standard errors (PCSE) in parentheses.

Appendix Table 4. Subdomain Social Spending, Full Cross-Sectional Models, 1970-2007

	DV: Δ Spending, % GDP $_t$				
	Old Age	Survivors	Incapacity	Health	Family
Δ Foreign Born, % Pop$_t$	-.514***	-.028	.026	-.083	.012
	(.118)	(.077)	(.090)	(.063)	(.062)
Spending, % GDP$_{t=1970}$	-.187	-.454	-.492***	-.833***	-.333*
	(.151)	(.301)	(.161)	(.161)	(.173)
Foreign Born, % Pop$_{t=1970}$	-.192***	-.018	.002	.028	.014
	(.061)	(.036)	(.049)	(.031)	(.032)
Constant	6.131***	.645	1.039	5.886***	.854
	(1.172)	(.716)	(.763)	(.902)	(.542)
N	17	17	17	17	17
Rsq	.711	.161	.455	.754	.237

	DV: Δ Spending, % GDP $_t$			
	ALMP	Unemployment	Housing	Other
Δ Foreign Born, % Pop$_t$	-.017	-.140**	-.001	-.023
	(.029)	(.058)	(.042)	(.026)
Spending, % GDP$_{t=1970}$	-.500**	-.616***	-1.018**	.052
	(.179)	(.128)	(.373)	(.143)
Foreign Born, % Pop$_{t=1970}$	-.017	-.010	.006	.003
	(.015)	(.029)	(.021)	(.014)
Constant	.533**	1.183***	.403	.142
	(.224)	(.392)	(.293)	(.172)
N	17	17	15	17
Rsq	.423	.746	.431	.062

* p < .10; ** p < .05; *** p < .01. Cells contain coefficients from an OLS regression with standard errors in parentheses.

Appendix Table 5. Subdomain Social Spending, Full 5-Year TSCS Model

RANDOM EFFECTS	DV: Δ Spending, % GDP $_t$				
	Old Age	Survivors	Incapacity	Health	Family
Δ Foreign Born, % Pop$_{t-1}$	-0.074	-0.073	-0.060	0.026	-0.053
	(0.109)	(0.050)	(0.067)	(0.072)	(0.052)
Spending, % GDP$_{t-1}$	-0.032	-0.110**	-0.111***	-0.238***	-0.072*
	(0.040)	(0.049)	(0.043)	(0.049)	(0.037)
Constant	0.436	0.139*	0.327**	1.618***	0.255***
	(0.293)	(0.073)	(0.127)	(0.280)	(0.092)
N	85	85	85	85	85
N (panels)	17	17	17	17	17
Rsq (overall)	0.009	0.048	0.082	0.226	0.057
	ALMP	Unemp't	Housing	Other	
Δ Foreign Born, % Pop$_{t-1}$	-0.055	-0.154**	-0.013	-0.076**	
	(0.038)	(0.073)	(0.023)	(0.034)	
Spending, % GDP$_{t-1}$	-0.123**	-0.217***	-0.173***	-0.048	
	(0.060)	(0.053)	(0.061)	(0.040)	
Constant	0.146**	0.338***	0.105***	0.083**	
	(0.061)	(0.107)	(0.039)	(0.037)	
N	70	84	71	85	
N (panels)	17	17	15	17	
Rsq (overall)	0.098	0.202	0.022	0.083	
FIXED EFFECTS	DV: Δ Spending, % GDP $_t$				
	Old Age	Survivors	Incapacity	Health	Family
Δ Foreign Born, % Pop$_{t-1}$	0.036	-0.025	0.004	0.109	0.024
	(0.110)	(0.051)	(0.069)	(0.085)	(0.057)
Spending, % GDP$_{t-1}$	-0.346***	-0.496***	-0.455***	-0.345***	-0.518***
	(0.081)	(0.084)	(0.089)	(0.074)	(0.091)
Constant	2.475***	0.512***	1.208***	2.179***	1.155***
	(0.556)	(0.090)	(0.234)	(0.410)	(0.191)
N	85	85	85	85	85
N (panels)	17	17	17	17	17
Rsq (overall)	0.219	0.374	0.298	0.249	0.335
	ALMP	Unemp't	Housing	Other	
Δ Foreign Born, % Pop$_{t-1}$	-0.093**	-0.155**	0.007	-0.070**	
	(0.039)	(0.077)	(0.020)	(0.031)	
Spending, % GDP$_{t-1}$	-0.680***	-0.567***	-0.559***	-0.806***	
	(0.114)	(0.091)	(0.079)	(0.097)	
Constant	0.642***	0.868***	0.269***	0.494***	
	(0.101)	(0.153)	(0.039)	(0.058)	
N	70	84	71	85	
N (panels)	17	17	15	17	
Rsq (overall)	0.464	0.394	0.483	0.541	

* p < .10; ** p < .05; ** p < .01. Cells contain coefficients (with standard errors in parentheses) from a TSCS model, using GLS estimations.