

The politics of credit claiming: Rights and recognition in health policy feedback

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Funding information

NORFACE and the European Commission, Grant/Award Numbers: 462-14-070 and 462-14-076

Abstract

Why do governments recognize rights? In this article, we rely on natural experiments and an innovative matching technique to identify a new causal mechanism of policy feedback, which we refer to as the “recognition” effect. We rely on the “hard case” of health care to demonstrate that attitudes towards the health system change in response to government policy change and, indeed, even to rights-based initiatives. During the time when public opinion surveys on public satisfaction with the health system were in the field, governments in both Germany and Sweden introduced a new right: the right to a maximum waiting time for health services. This serendipity allowed us to compare respondents' attitudes both before (control) and after the implementation of the waiting time guarantee (treatment), using coarsened exact matching to account for the imbalances in the treatment and control groups. We find that respondents interviewed after implementation of the new waiting time guarantees (in contrast to those interviewed before the introduction of the guarantees) express higher levels of satisfaction with the health system in general, but do not evaluate their specific medical treatment (including waiting times) more positively. We interpret this finding as evidence that citizens respond to governmental recognition of their rights as a good per se, independent of their personal experience with the particular public service at hand. Thus, we argue that theories of policy feedback need to move beyond their focus on direct material experience with the policies at hand, and to incorporate mechanisms of symbolic action and normative valuations into their causal models.

KEYWORDS

health care, health politics, policy feedback, policy responsiveness, social rights, social solidarity, welfare state attitudes

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1 | INTRODUCTION

Although welfare state politics since the 1990s have often revolved around austerity and retrenchment, governments do not solely cut back social services, but instead also aim to improve them. In the health care area, improvements in patient rights are a growing trend in public policy-making (Fountain, 2001; Segal, 1998; Winblad, Vrangbæk, & Östergren, 2010). Indeed, rights-based policies are common in social policy, such as for example, the right to a day care place, the right to adopt a child, or the right to opt-out of government health and pensions plans. But do citizens notice and give governments credit for these new rights? Or do they wait and see in order to develop judgements about whether these new rights have, indeed, brought about any material improvements in their direct experience with the public service at hand?

The ways in which citizens judge and react to government policies is of central importance not only for social policy and administration, but also for our understanding and appreciation of representative democracy, more generally (Bingham Powell, 2004; Stimson, MacKuen, & Erikson, 1995). Indeed, there is a great deal of scholarly debate on the impact of policies on public opinion. Dynamic representation scholars argue that public opinion reacts very quickly to changes in government policies, and, indeed, that politicians in turn change policies directly after swings in public opinion. Policy feedback scholars argue by contrast that citizens' preferences are endogenous to past policies, and are shaped by long-term policy patterns as well as short-term policy shifts. Despite this division, however, both sides of this debate share a common assumption that citizens evaluate government policies based on their direct experiences with policies. Thus, both policy responsiveness and policy feedback scholars view citizens' evaluation of government policies as a matter of rational calculation and self-interest. By contrast, "deservingness" theory roots differences in support for government policies as resulting from normative, and possibly affective and primordial, processes that govern the development of political and societal attitudes. Consequently, health attitudes should remain relatively invariant.

Efforts to sort out the precise links amongst welfare state attitudes and policy feedbacks are undoubtedly hampered by the very difficult methodological problem of determining the directionality and causality of the relationship between policies and attitudes, particularly in the health area. Fortunately, data from Germany and Sweden provide us with a unique opportunity to contribute to this puzzle by an analysis of opinion change after the implementation of one specific rights-based reform—a waiting time guarantee for health services—in two countries at two different points in time (2005 and 2016). We analyze this natural experiment by relying on targeted surveys and a quasi-experimental design. We are able to show that persons asked about their satisfaction with the health system overall were more positive after the introduction of the waiting time guarantee than before. When asked about their satisfaction with waiting times or medical services, however, they showed no response to the introduction of the waiting time guarantee. From the perspective of instrumental rationality, this is not surprising. The introduction of the waiting time guarantee did not immediately reduce waiting times. Thus, there is no reason to expect higher satisfaction with waiting times or medical services. Despite the lack of increase in satisfaction with *specific* aspects of the health services, respondents nevertheless reported higher levels of satisfaction with the health system in *general*. Given the importance of the concept of generalized diffuse legitimacy in theories of political legitimacy (Easton, 1967), we interpret these findings as evidence of a symbolic or normative effect of policy feedback. When asked about their satisfaction with the health care system in general, individuals seem to base their judgments on substantive rationality; when asked about their satisfaction with specific aspects of the health system, they seem to frame the question in terms of instrumental rationality (Weber, 1978 [1968]). The public appears to appreciate being recognized as sovereign citizens with rights—indeed, without regard for individuals' personal utilization of this new right or any immediate material improvement in public social service delivery. Furthermore, they update their attitudes towards the health system, even though these attitudes are often considered as quite stable. This "recognition¹ effect" is relevant not only for health policy and governance, but also has important implications for the literature on policy feedback and policy responsiveness, as well as the literature on welfare state attitudes.

2 | LITERATURE AND THEORY

As argued in all of the articles in this regional issue, public support is a key issue for the political legitimacy and fiscal sustainability of welfare states, but scholars remain divided about the causes and consequences of public opinion on welfare state policies. Policy responsiveness theory (Brooks & Manza, 2006a, 2006b; Erikson, MacKuen, & Stimson, 2002; Stimson et al., 1995) argues that changes in public opinion have a direct effect on public policies. As politicians are aware that they risk losing votes in the next election if they do not adjust their policies accordingly (e.g., by increasing social spending), these theories posit that public policy will follow public opinion very closely. However, once politicians have re-adjusted these policies, the public is thought to “reset” its opinion. If social expenditures have just been increased, the public will reduce its support for further increases. If, instead, cutbacks have been undertaken, the public should prefer social expenditure increases. This adjustment of public opinion to changes in government policies has been termed the “thermostatic” effect (Bartle, Dellepiane-Avellaneda, & Stimson, 2011; Soroka & Wlezien, 2010; Wlezien, 1995).

Policy feedback theorists, on the other hand, criticize policy responsiveness theory precisely for assuming such a smooth and seamless dynamic equilibrium. Given all of the intricacies and contingencies of public policy-making, policy feedback scholars argue that it is unreasonable to assume that politicians will instantly adjust their policies in response to changes in public opinion (Cammatt, Lynch, & Bilev, 2015). Furthermore, policy feedback scholars argue that pre-existing policies—many of which have been relatively stable for a long period of time, shape citizens' policy preferences—and even their attitudes and behavior (Campbell, 2012; Shore, 2014). Indeed, this is precisely the original claim of welfare regime theory, which argues that the patterns of solidarity embodied in different welfare regimes—results of historic class compromises and political mobilization—become self-reinforcing, a process sometimes termed “institutional stickiness” or “path dependence” (Esping-Andersen, 1990; Esping-Andersen & Korpi, 1984; Pierson, 1994; Rothstein, 1998; Svallfors, 2012).

Deservingness theorists argue, instead, that welfare state attitudes have cultural roots, which are not necessarily related to the particularities of welfare state regimes or policy feedback, and these scholars emphasize differences between policy areas rather than welfare state regimes. Van Oorschot argues that “Europeans share a common and fundamental deservingness culture” of support for the sick and disabled because they are viewed throughout Europe as being particularly deserving, as their illness is largely interpreted as being beyond their control (van Oorschot, 2006, p. 23). Jensen and Petersen (2017) go even further, arguing that the deservingness heuristic is a biologically-rooted response that shapes attitudes towards the sick and explains the widespread support for governmental responsibility to care for the sick and high levels of government spending for health care. Consequently, health care is generally speaking a valence issue in politics, as opposed to other policy areas, such as unemployment. Thus, the deservingness literature suggests that health attitudes should be very resistant to change, and does not really address cross-national or inter-temporal variation on health care attitudes. Larsen proposes a synthesis by pointing out that the type of welfare state regime may be tapping into how social programs distinguish between the deserving and the undeserving, thus changing how the public views the legitimacy of welfare state recipients (Larsen, 2008).

Consequently, the great difficulty in sorting out the relationship between welfare state institutions and public attitudes is precisely this endogeneity of welfare institutions and attitudes. Welfare state regimes may be shaping welfare state attitudes, while differences in attitudes may be the cause of differences in welfare state regimes or differences across policy areas. Hence, in order to pinpoint how exactly policies affect the attitudes of individuals, current cutting-edge research has increasingly turned to inter-temporal research designs. By examining changes in public programs, and the differential response of individuals directly targeted versus those who remain unaffected by policy change, scholars hope to tease out the mechanisms of policy feedback. To date, they have discovered two key mechanisms: “visibility” and “proximity”. Visibility entails that individuals will react to policies only when they have access to information about these policies, and find them salient. Proximity means that individuals react to policies at higher rates when they (“direct users”) or their family members (“secondary users”) personally experience these policies.

Indeed, if individuals do not directly experience policies but hear about them only in the media, they may react to policies very differently from the direct and secondary users (Bendz, 2017; Hedegaard, 2014; Kumlin & Rothstein, 2005; Kumlin & Stadelmann-Steffen, 2014; Mettler & Soss, 2004; Soss & Schram, 2007).

While this precision in locating mechanisms of policy feedback marks a decided improvement in the study of welfare state attitudes, the field has gone into quite a rationalist and utilitarian direction, given its original concerns about explaining differences in norms and values. Furthermore, it has neglected the process by which direct experiences with welfare state programs generate broader political support for the welfare state. Indeed, the literature on political legitimacy emphasizes that legitimacy is not necessarily based on specific policies or actions of government, but on a more general trust in government that is distinct from “short-run evaluations of individual incumbents and their policies” (Muller, Jukam, & Seligson, 1982, p. 244). We posit that there may be an intermediate level of generalized diffuse legitimacy—one that pertains to specific policy domains such as the health system. General diffuse support for the health system may be a key stepping-stone to developing and reinforcing trust in government as a whole, and it could be of importance in explaining support for the welfare state. Policy proximity alone does not explain how a utilitarian appreciation of a policy would spill over into a general moral commitment to social solidarity more broadly. Further, given that welfare states entail a relationship between program beneficiaries and the wider public, not just users but taxpayers need to support the welfare state; thus, non-users must eventually come to support social solidarity. Lastly, in thinking about politicians' motivations for introducing reforms, the electoral pay-off for policy changes that affect the diffuse support of broad sections of the electorate would surely be more effective than ones that affect only the direct users. On all three counts, generalized diffuse support provides a mechanism by which recognition of rights might increase broad support for the welfare state. This does not preclude the possibility that persons who support the introduction of social rights might not think they might at some point directly benefit from the new right. But we are suggesting that this is a broader, more diffuse and more normative attitude that extends beyond personal self-interest to include support for the claims of others to these rights, and suggests approval of the way in which government interprets its mandate. As such, this fits Weber's definition of substantive rationality as opposed to instrumental rationality.

2.1 | Hypotheses

In contrast to theories that focus on policy proximity and policy visibility as prerequisites for policy feedback effects, we argue that citizens might well respond favorably to governmental recognition of their rights as a matter of principle. Access to health care is generally seen as a universal citizen right, and at the same time access is the Achilles heel of many health care systems—amongst them the Swedish and the German. By launching waiting time guarantees, this lack of fulfillment of a citizen right is recognized, and acted upon by the government. In line with the understanding that the recognition of need or injustice is conceived as something intrinsically good, we hypothesize that this will lead to more positive evaluations and higher satisfaction amongst the general public. Thus, we expect that individuals would respond positively to the introduction of this new right, without necessarily having visited a medical facility or experienced shorter waiting times. In addition, we expect that respondents can distinguish between issues of principle versus issues of utility; waiting time guarantees appeal to their sense of substantive justice, shorter waiting times and better service to their instrumental self-interest.

These theoretical conjectures result in the following empirical expectations that are tested. First, we hypothesize that respondents surveyed after the implementation of the waiting time guarantees will respond based on their normative beliefs, and will evaluate the health system as a whole more favorably than those surveyed prior to the implementation date. Second, when asked about *specific* aspects about the health system, however, respondents will answer realistically, and will not respond positively to guarantees alone. Thus, we expect that individuals are able to distinguish between their agreement with the principles expressed by government versus their evaluation of the concrete public service provision at hand.

3 | RESEARCH DESIGN, DATA, AND ESTIMATION STRATEGY

This article makes use of two natural experiments. Waiting time guarantees were implemented in Sweden and Germany during the period when two public opinion surveys were in the field. In Germany, Parliament passed the law that introduced the waiting time guarantee on June 11, 2015, the law went into legal force on July 25, 2015, and the hotline procedure for obtaining an appointment with a specialist was advertised and opened—i.e., actually implemented—on January 25, 2016. The German Socio-Economic Panel Study Innovation Sample² (which included a panel of questions on satisfaction with the health system of our own design) was in the field between September 8, 2015 and March 4, 2016. Out of the total 2,500 respondents, 95% answered our questions of interest before the implementation date, and the remaining 5% afterwards. In Sweden, the waiting time guarantee was introduced through an agreement between the Swedish Association of County Councils and the Social Democratic Government. It was implemented on November 1, 2005, in the midst of field canvassing for the Swedish Society Opinion Media (SOM)³ survey of the Västra Götaland Region (which includes Gothenburg and the surrounding area) between September 19, 2005 and February 22, 2006. In contrast to Germany, all Swedish respondents received the survey questions on the same date in September; 82% responded before the waiting time implementation date; the remaining 18% after the date.

In both countries, this constitutes a natural experiment set-up, as a policy was implemented during the course of surveys that were being carried out with no relationship to the policy. However, unlike a true experiment, there is no experimental control over the treatment and control conditions, and respondents are not randomly distributed between the two groups by the experimenter. In Germany, there is no a priori reason to believe that those questioned before and after the treatment differed systematically; but owing to random chance, this might nevertheless be the case. In Sweden, we can assume it likely that people who returned their questionnaires at the last minute differed from those who returned them promptly. Thus, in both cases, these are not true experimental situations, but quasi-experiments, and we need to take measures to balance the distribution in the treatment and control groups, which is described in more detail below.

Critically, we have evidence that these respondents were exposed to government information about the implementation of the waiting time guarantee, and hence have evidence of its visibility. Indeed, both governments relied on new strategies of political communication. Despite both countries' long tradition of corporatist interest intermediation, these governments reached out directly to voters, publicizing this policy change through YouTube videos, press releases, and flyers delivered directly to individual households. The implementation of these guarantees was reported in television news broadcasts and in national as well as local newspapers. Indeed, in both countries, much effort was invested in disseminating information on the very day the reform was brought into force.⁴ In the Västra Götaland region, for example, all households received information about the reform in the Regional government's magazine *Regionmagasinet*, which was distributed to the households on or about the day the waiting time was brought into force in Sweden. Perhaps even more importantly, the waiting time guarantee was featured in several articles in printed media in Sweden on the day of implementation, not least in an editorial in the most important newspaper in the region, signed by politicians from three different parties in the regional executive committee.⁵

Not only did the respective governments rely on visible and direct appeals to the public, but they also bypassed established interests. Despite the criticism of health policy stakeholders, such as doctors, nurses, and local government associations, both Christian Democratic Minister of Health Hermann Grohe in a grand-coalition government and Social Democratic Minister of Care and the Elderly Ylva Johansson in a Social Democratic minority government pressed ahead with their reforms, making these a political priority. The German guarantee set up a hotline, such that patients could be guaranteed an appointment with a specialist within six weeks. The Swedish reform guaranteed hospital and/or specialist treatment within three months. Hence, logically speaking, the reductions in waiting time themselves could only be directly experienced at least six weeks or three months (respectively) after the implementation date. Furthermore, subsequent surveys of waiting times demonstrated a lack of improvement in waiting times for specialist appointments in Germany,⁶ and an improvement of waiting times for hospital treatments in Västra Götaland,

initially in 2007.⁷ Thus, the implementation of a new right is the treatment, and not the reduction of waiting times, which might follow months after the survey period was over.

3.1 | Dependent variables

In the German SOEP-IS 2015 study, we use indicators of satisfaction with the health care system developed by the authors. Respondents are asked to indicate how satisfied or dissatisfied they were with the German health care system as regards: (i) the health system in Germany in general; (ii) the availability of GPs; (iii) the availability of specialists; (iv) the waiting time for an appointment with a physician; and (v) the waiting time for hospital treatment. For each of these survey items, respondents were told to indicate their level of satisfaction on a 0–10 scale, where 0 was totally dissatisfied and 10 was totally satisfied.

In the Swedish case, we make use of three survey items asking respondents to evaluate: (i) services provided by the Västra Götaland Region; (ii) hospital care; and (iii) primary health care. Respondents could choose amongst five categories: “very good”, “good”, “neither good nor bad”, “bad”, and “very bad”. Due to the small proportion of respondents who said that the systems are “very good” or “very bad”, we grouped those together with answers of “good” and “bad”, accordingly. Thus, the dependent variable is measured as a trichotomous variable indicating whether people are dissatisfied, neither satisfied or dissatisfied, and satisfied. We consider the survey item measuring satisfaction with the service provided by the Västra Götaland Region a good proxy of overall satisfaction with the health system because 80% of its budget goes to health care, and, in the eyes of citizens it is very much perceived as an organization responsible for health care, although it is also responsible to some extent for regional development and public transportation. The questions on primary and hospital care, by contrast, ask specifically about the satisfaction with the care provided in your municipality, and are closer to the German survey’s questions about satisfaction with specific aspects of the health care system, such as primary care and hospital care.⁸

3.2 | Treatment variable

To assess the effect of the waiting time guarantee on satisfaction, we build a simple dichotomous variable indicating whether people answered the survey questions before or after the day the waiting time guarantee was implemented. We code the variable 1 for those who answered after this day and, as such, have been “treated”. Correspondingly, we code 0 for those who have indicated their satisfaction before the implementation date and as such are in the “control” group.

3.3 | Control variables

To rule out the potential influence of confounding from exogenous socio-economic characteristics and possible endogenous determinants of health satisfaction, we make use of a number of controls. Regarding socio-economic characteristics, we control for age, income, gender, marital status, number of children, education, and position on the labor market. Among the possible endogenous determinants, we consider experience with the health system, subjective health status, private insurance (only Germany), and political ideology (in Sweden) or political partisanship (in Germany). The controls used in the models are selected because they are potentially linked to both satisfaction with health services and with potential variance in the composition of respondents in the treatment and control groups (Kohl & Wendt, 2004; Missinne, Meuleman, & Bracke, 2013; Wendt, Kohl, Mischke, & Pfeifer, 2010).

3.4 | Estimation strategy

As discussed above, our data analysis strategy assumes an experimental research design, with all respondents randomly assigned into the treatment or control groups. However, we do not have control over the randomization procedure (thus the quasi-experimental nature of the analysis), and as such need to correct for any differences in the

treatment and control groups that might be driving differences in their satisfaction. An initial estimate of the imbalance index shows significant differences in the socio-economic characteristics between the control and treatment groups in both German and Swedish samples (L-index estimates in Tables 1 and 3). We adjust for these differences in two different ways: (i) including the control variables in the regression models; and (ii) matching the control and treatment samples before estimating the regression models. While a regression model does adjust for background variables, it can bias the results in case there is no sufficient overlap between the treatment and control groups (Stuart, 2010, p. 2). As such, we also estimate the average treatment effect after coarsened exact matching. The matching strategy can, nevertheless, suffer from bias due to incomplete (failure to match all treated units) or inexact (failure to obtain exact matches) matching (Rosenbaum & Rubin, 1985). As such, we present the results from the regression models both before and after matching for comparison and cross-validation (Rubin, 1974).

In the first part of the analysis, we estimate regression models of satisfaction variables as function of the treatment variable and all control variables. In the analysis of the German data, we treat the 11-point item as continuous and implement OLS models to estimate how satisfaction differs across our independent variables. The coefficient of the treatment variable thus indicates, on average, how satisfied are those in the treatment group compared to those in the control group. For the analysis of the Swedish data, the satisfaction variables are categorical and thus we estimate ordered probit instead of OLS models. Thus, we estimate the differences in the probability of being satisfied among different groups. The coefficient of the treatment variable indicates in this case how likely or unlikely people in the treatment group are to be satisfied compared to those in the control group.

In the second part of the analysis, we estimate the effect of treatment variable after matching the treatment and control samples based on a set of socio-economic background characteristics.⁹ We use coarsened exact matching, which requires fewer assumptions and is more easily automated than other existing matching strategies. The matching is done in three steps: (i) first the data is coarsened into substantively meaningful groups; (ii) then the treatment and control groups are exactly matched based on these groups; and (iii) the analysis is done on the original (uncoarsened) values of the matched data (Iacus, King, & Porro, 2009; Iacus, King, Porro, & Katz, 2012).¹⁰ As expected, the coarsened exact matching technique reduces significantly the imbalance in the sample. In the German data, the imbalance index is reduced from around 0.91–0.95 to 0.49–0.56, while in Sweden it is reduced from 0.67–0.61 to around 0.30–0.24 after machining (see L-index in Tables 1–4). This means that although the control and treatment groups are more similar than before, there are still dissimilarities after matching. We correct those by including the socio-economic characteristics together with the treatment variable in the new regression models (Blackwell, Iacus, King, & Porro, 2010).

4 | RESULTS

Sections 4.1 and 4.2 present the results from the empirical investigation of the German and Swedish data, respectively. We report results first from the initial regression model (Tables 1 and 3) and second from the regression analysis after the coarsened exact matching (Tables 2 and 4).

4.1 | Germany

Table 1 includes the results from regression models of satisfaction with the German health system in general (models 1a and 1b), versus satisfaction with specific aspects of the health system, notably waiting time for an appointment with a physician (models 4a and 4b). As we clearly see, only the satisfaction with the overall health system responds to the introduction of the waiting time guarantee. The effect of the treatment on the other aspects of health care satisfaction are small and statistically insignificant. The “a” series of models are regression models with all control variables, while the “b” series are the models including only the socio-demographic control variables used in the coarsened exact matching. We present both models to indicate that excluding the endogenous variables, for example, self-reported ideological position, does not alter the results substantially.

TABLE 1 OLS models of satisfaction with the health system and its features before matching using the German SOEP-IS data

Model	Overall health system		Availability of GPs		Availability of specialists		Waiting time for appointment with a physician		Waiting time for hospital treatment	
	1a	1b	2a	2b	3a	3b	4a	4b	5a	5b
Treatment	0.513* (0.239)	0.466* (0.234)	0.095 (0.227)	0.073 (0.223)	0.032 (0.269)	0.002 (0.265)	-0.162 (0.277)	-0.127 (0.275)	0.455 (0.361)	0.474 (0.358)
Observations	2570	2713	2560	2703	2529	2671	2538	2682	1413	1489
L-index		0.911		0.914		0.919		0.913		0.948
R ²	0.060	0.030	0.063	0.029	0.096	0.051	0.126	0.069	0.104	0.072
C_obs	2606			2597		2570		2577		1439
T_obs	107			106		101		105		50
Control	All	SD	All	SD	All	SD	All	SD	All	SD

Notes. Standard errors in parentheses.

p* < 0.05, *p* < 0.01, ****p* < 0.001.

SD: socio-demographic control variables used also in matching.

Tables with the estimates of the control variables are presented in the Supporting Information.

TABLE 2 OLS models of satisfaction with the health system and its features, after matching using the German SOEP-IS data

Model	Overall health system		Availability of GPs		Availability of specialists		Waiting time for appointment with a physician		Waiting time for hospital treatment	
	1a	1b	2a	2b	3a	3b	4a	4b	5a	5b
Treatment	0.642*	0.642*	-0.061	-0.061	0.056	0.058	0.027	0.027	0.338	0.328
	(0.260)	(0.252)	(0.253)	(0.246)	(0.319)	(0.301)	(0.331)	(0.306)	(0.451)	(0.388)
Observations	572	572	543	543	492	492	538	538	206	206
L-index	0.514		0.519		0.556		0.556		0.308	0.489
R ²	0.093	0.093	0.076	0.076	0.135	0.135	0.163	0.163	0.308	0.308
C_obs	485		457		411		452		169	
T_obs	87		86		82		86		37	
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

Notes. Standard errors in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Tables with the estimates of the control variables are presented in the Supporting Information.

TABLE 3 Ordered logit models of satisfaction with the services provided by the VGR, primary care, and hospital care before matching using the Swedish Western SOM survey data

Model	Services provided by the VGR		Primary care		Hospital care	
	1a	1b	2a	2b	3a	3b
Treatment	0.439** (0.141)	0.394** (0.131)	0.158 (0.121)	0.045 (0.111)	0.120 (0.132)	0.053 (0.121)
Observations	1548	1692	2308	2531	2100	2298
L-index		0.674		0.612		0.627
Pseudo R ²	0.006	0.021	0.026	0.047	0.054	0.036
C_obs		1418		2136		1927
T_obs		274		395		371
Controls	All	SD	All	SD	All	SD

Notes. Standard errors in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

VGR: Västra Götland Region, SD: socio-demographic control variables used also in matching.

Tables with the estimates of the control variables are presented in the Supporting Information.

TABLE 4 Ordered logit models of satisfaction with the services provided by the VGR, primary care, and hospital care after matching using the Swedish Western SOM survey data

Model	Services provided by the VGR		Primary care		Hospital care	
	1a	1b	2a	2b	3a	3b
Treatment	0.307* (0.153)	0.331* (0.154)	-0.037 (0.121)	-0.040 (0.122)	-0.085 (0.134)	-0.106 (0.136)
Observations	816	816	1395	1395	1212	1212
L1		0.296		0.250		0.241
Pseudo R ²	0.003	0.02	0.014	0.0001	0.0002	0.037
C_obs		603		1064		907
T_obs		213		331		305
Controls	No	Yes	No	Yes	No	Yes

Notes. Standard errors in parentheses.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

VGR = Västra Götland Region.

Tables with the estimates of the control variables are presented in the Supporting Information.

Table 2 includes the results from regression models estimated after matching. The “a” model series comprises bivariate regression models with only the treatment variable included, while the “b” series models include, additionally, the socio-demographic control variables used in the coarsened exact matching. Again, we see a clear treatment effect of the introduction of the waiting time guarantee on satisfaction with the health system in general, but no effect on other aspects of the health system. As in the initial regression model, the treatment coefficients from the coarsened data are statistically significant only in models 1a and 1b, but not in the others. In the initial model, the increase in overall satisfaction is estimated at almost 0.5 points, and after matching this is slightly higher. With a mean level of satisfaction of 6.4 (6.7 after matching) and a standard deviation of 2.4 (2.1 after matching), the increase in satisfaction is thus of substantive magnitude. Based on the results in model 1a in Table 2, the average predicted level of satisfaction is of 6.30 in the control group and 6.94 in the treatment group. That means that after the reform, satisfaction increased on average by 10%. The substantive effect is comparable to the difference in the satisfaction level of

Germany (5.92 points) and Estonia (5.27 points) on a 0–10-point scale, respectively, in 2014, according to our own calculations based on the European Social Survey.

4.2 | Sweden

Table 3 presents the results from ordered logit models of satisfaction in Sweden before matching. As in the presentation of the German data, the “a” series models include all control variables, while the “b” series include only the socio-demographics used in matching. The results show a significant effect of the treatment on the satisfaction with the services provided by the Västra Götaland Region under both conditions (1a and 1b). However, as in the German case, when asked about specific features of the health system, such as primary health care (2a/b) or hospital (3a/b) services, respondents did not indicate increased satisfaction after introduction of the waiting time guarantee. Thus, respondents report being more satisfied with the regional government after the introduction of the waiting time guarantee, but they do not indicate being more satisfied with health service delivery.

Table 4 includes the results from regression models after matching, the “a” series only with the treatment variable, and the “b” series including also the socio-demographic control variables used in the coarsened matching. As in the previous sets of models and as in the German case, respondents report significantly higher levels of general satisfaction after the introduction of the waiting time guarantee (models 1a/b), but report no increased satisfaction regarding specifically primary (2a/b) or hospital care (3a/b). Based on the results in model 1a in Table 4, the probability of being satisfied is of 38% in the control group and of 46% in the treatment group.¹¹ The magnitude of the effect is estimated as the average marginal change in the probability of being satisfied for people in the treatment and control groups. We find that there is an increase of around 8% in the probability of being satisfied with the health system after the reform is brought into force. The difference in satisfaction is comparable to the drop in overall satisfaction in Sweden between 2012 (6.30 points) and 2014 (5.77 points), or between overall satisfaction in Sweden (5.77 points) and Lithuania (5.06 points) on an 11-point scale, respectively, in 2014, based on our own calculations with the European Social Survey.

5 | DISCUSSION AND CONCLUSION

The objective of this article was to study the impact of introduction of right-based health care policy reforms on public opinion, in this case, the impact of the German and Swedish waiting time guarantees on satisfaction with the health system. Making use of a quasi-experimental research design with unique survey data from both Germany and Sweden, we are able to identify the causal effect of the waiting time guarantees on satisfaction in a rigorous way. By comparing service satisfaction before (control group) and after (treatment group) the day the reforms were actually implemented, and adjusting for imbalances in the composition of the groups, we show striking similarities in outcomes in the German and Swedish context. Despite being implemented in two rather different health care systems and welfare states—and with more than a decade between the reforms—the waiting time guarantee reforms resulted in an immediate increase in the overall satisfaction with the health system in both Sweden and Germany. Furthermore, this positive effect is in evidence even though waiting times were not shorter at the time of the surveys. Indeed, when asked specifically about waiting times for a doctor's appointment (the aim of the German waiting time guarantee) or for satisfaction with hospital treatment (the aim of the Swedish waiting time guarantee), respondents in both countries failed to note any increased satisfaction. Thus, we argue that this jump in overall satisfaction constitutes a normative effect: citizens approve of the way in which government is treating its commitment to provide health access to its citizens, and appreciate this, even though they have not yet felt any improvement in service. It is, of course, theoretically possible that there was some other aspect of health service delivery that caused increased satisfaction that was not included in our surveys, but it is highly improbable that this “omitted” variable went into effect on the same day as the waiting time guarantees, and that this happened in two different countries, ruled by different governments,

and a decade apart. Furthermore, if the introduction of new rights never brings any improvement in service, there could be a backlash. But, at least at the time of the introduction of these waiting time guarantees, we can show that the public responded favorably to these new rights.

In conclusion, we suggest that we have identified an additional mechanism of policy feedback that we refer to as the "recognition effect". As we found this mechanism in health policy, an area of relatively stable attitudes, we expect that this mechanism will be relevant for other areas of policy where new rights are recognized, and perhaps also in cases where old grievances are acknowledged, such as in public apologies. We pointed out that the German and Swedish governments made a notable effort to inform the public about these guarantees, but would claim that press conferences, public relations, and even social media campaigns are becoming ever more common, such that one would expect to find many additional instances of public awareness of new rights. Indeed, discussions of welfare state attitudes have long theorized about the importance of norms as well as self-interest (Gevers, Gelissen, Arts, & Muffels, 2000). However, as current research hones its methods to move beyond cross-sectional models in order to focus squarely on causal identification, the emphasis in the literature has become increasingly on mechanisms based on self-interest rather than norms. Here, we suggest that generalized diffuse support for welfare states may be a crucial component of the political legitimacy of welfare states, and of governance in an age of austerity.

ACKNOWLEDGEMENTS

The authors acknowledge the support for the research for this article by NORFACE (New Opportunities for Research Funding Agency Co-operation in Europe) and the European Commission (ERA-Net Plus funding, grant agreement number 618106) under grants 462-14-070 and 462-14-076 for the project "Healthdox: The Paradox of Health Care Futures" as part of the Welfare State Futures program. Burlacu and Immergut are grateful for the opportunity of including our questionnaire "Private or Public Health Care? Evaluation, Attitudes, and Social Solidarity" in SOEP-IS in 2015 and 2017, and would like to thank Dr David Richter for his help and guidance during the submission process. All authors thank the editors of this regional issue, two anonymous reviewers, and Femke Roosma for their helpful comments on our earlier drafts.

CONFLICT OF INTEREST

None declared.

ENDNOTES

- ¹ The term "recognition" is inspired first by the attention paid by political philosophers to the politics of recognition as being distinct from the politics of socio-economic distribution, for example, Fraser (1995); Fraser and Honneth (2003); Taylor (1997). For the welfare state area, the key work is undoubtedly Banting and Kymlicka (2006). Second, the discovery of the recognition heuristic may indicate that human beings respond very strongly to recognition. We are not saying the recognition in the first sense—recognized as being sovereign—is necessarily buttressed by the cognitive effect, but it is a possibility. See Gigerenzer (2001); Goldstein and Gigerenzer (2002).
- ² More information about SOEP and the SOEP Innovation Sample can be found in Richter and Schupp (2015); Wagner, Frick, and Schupp (2007); and <https://www.diw.de/soep> (accessed July 15, 2017).
- ³ The SOM Institute is an independent survey research organization at the University of Gothenburg, more information can be found at http://som.gu.se/som_institute.
- ⁴ <https://www.youtube.com/watch?v=3knmeqoPwTw> and <https://www.youtube.com/watch?v=v-KAwii9XaE> (accessed June 12, 2017).
- ⁵ "Så ska vi klara vårdgarantin i Västra Götaland" ("This is how we will fulfill the waiting time guarantee in Västra Götaland") Göteborgs-Posten 2005-11-01.
- ⁶ <http://gesundheitsdaten.kbv.de/cms/html/24045.php>, and http://www.kbv.de/html/418_26468.php (accessed August 17, 2017).
- ⁷ See Landsting (2007).
- ⁸ A question on waiting time was not available until subsequent surveys.

- ⁹ We do not coarsen based on attitudes, preferences, and values (e.g., ideological left–right placement), which could be endogenous to satisfaction with the health system. First, we assume that the socio-economic characteristics, which are predictors of political attitudes and preferences, account for most of the variation in the endogenous variables of interest. Second, including these variables in our matching strategy reduces significantly the number of matched treated units, which as mentioned above, can lead to biased estimates.
- ¹⁰ For both the Swedish and German data, we use the default Sturge's rule approach, which coarsen the continuous variables into a fixed number of bins, calculated using the formula: " $\lceil \log_2 n \rceil + 1$ ", where n is the number of data points in the sample. The only exception is the income variable in the German data, which is skewed to the right, and for this reason we coarsen it according to different percentiles: 1, 10, 25, 50, 75, and 90.
- ¹¹ The probability of being dissatisfied is 12% in the control group and 9% in the treatment group. The probability of saying that they are neither satisfied nor dissatisfied is 50% in the control group and 46% in the treatment group.

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How to cite this article: Burlacu D, Immergut EM, Oskarson M, Rönnerstrand B. The politics of credit claiming: Rights and recognition in health policy feedback. *Soc Policy Admin.* 2018;52:880–894. <https://doi.org/10.1111/spol.12403>