**British Journal of
Political Science**

ARTICLE

Of 'Welfare Queens' and 'Poor Carinas': Social Constructions, Deservingness Messaging and the Mental Health of Welfare Clients

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Abstract

Politicians engage in, and the media amplifies, social constructions of welfare recipients as undeserving. Such messaging seeks to influence mass public opinion, but what are the effects on the target population receiving welfare benefits? We test if deservingness messaging affects welfare recipients' mental health. To do so, we exploit a quasi-experiment entailing a dramatic shift in deservingness messaging after a welfare recipient in Denmark became the subject of a national debate, utilizing detailed administrative data on the ensuing consumption of antidepressants by other welfare recipients. We find evidence that welfare recipients experienced worse mental health outcomes after being exposed to deservingness messaging, reflected in a 1.2-percentage-point increase in the use of antidepressants in the weeks following the airing of a critical interview. Deservingness messaging particularly affected more vulnerable groups who had a history of mental health problems.

'There's a woman in Chicago. She has 80 names, 30 addresses, 12 Social Security cards and is collecting veterans' benefits on four nonexisting deceased husbands', said Ronald Reagan; 'And she's collecting Social Security on her cards. She's got Medicaid, getting food stamps and she is collecting welfare under each of her names. Her tax-free cash income alone is over $150,000' (*The New York Times*, 1976). Ronald Reagan made the welfare queen a central part of his presidential campaigns and a staple of his stump speech. The phrase 'welfare queen' became shorthand for both the idea of the undeserving poor and the power of political messaging in embedding and exploiting images of the undeserving poor to polarize and dismantle the welfare state (Hancock 2004). Such concerns become ever-more pressing given the return of populist politics, of which a central feature is the demonization of some groups (Thomann and Rapp 2018). Political communication more broadly, and deservingness frames in particular, powerfully shape political views, beliefs and participation (Andersen et al. 2017; Chong and Druckman 2007; Hopmann, Skovsgaard and Elmelund-Præstekær 2017; Jensen and Petersen 2017; Slothuus 2008). However, while there is evidence about how these frames influence what mass publics think and how they behave, including the psychological processes underlying these dynamics, we do not know how they make the subjects of these frames *feel*.

We ask what the effects of deservingness messaging are on the welfare recipients themselves. In particular, we test if conveying negative social constructions to target populations – as lazy and

undeserving – harms their mental health. There is already robust evidence that politics influences health via its impact on policies and institutions (Greer 2004; Greer 2018; Lynch 2020; Schrecker and Bambra 2015). There is also evidence that being stigmatized, as are those who are obese, disabled or in the lesbian, gay, bisexual, trans and queer (LGBTQ) community, negatively impacts mental health (Mak et al. 2007). Administrative burden theory draws from policy feedback and social construction frameworks to extend the pathways of potential effects, claiming that state actions do not just make access to public social welfare programmes more onerous, but also impose psychological costs upon target populations (Herd and Moynihan 2018; Schneider and Ingram 1997). We therefore propose that politics may exert a direct impact on health via messaging featuring negative political frames like deservingness, which stigmatize populations and, in turn, induce psychological costs among a target population construed in public debate as being undeserving of help.

A methodological difficulty in answering our question is that any particular framing will be endogenous to the environment that creates it. For example, Reagan's 'welfare queen' characterization is memorable but occurred in a broader historical characterization of welfare recipients that preceded Reagan, making it difficult to separate out the effects of this specific characterization. Furthermore, tropes about the undeserving poor are so prevalent in many settings that it is difficult to assert if any particular political framing makes much of a difference (Soss et al. 2011). Deservingness messaging often coincides with other changes in the life situations of welfare recipients. For instance, large-scale reforms of welfare benefits not only signal changes in the deservingness of target groups, but may also have a more direct impact on the mental well-being of such groups due to changes in benefit rates or accessibility to benefits.

To separate out the effects of deservingness messaging from other factors requires a setting where deservingness messaging achieves widespread dissemination and legitimacy in a way that is a substantial break from the past and where the messaging does not coincide with changes in welfare caused by, for instance, reduced welfare benefits. We find such an example in the context of the Danish welfare system. In 2011, the case of 'Poor Carina' offered an accelerated Danish version of the 'welfare queen' framing, sparking criticism of welfare recipients who were perceived as receiving excessively generous benefits while not seeking work. A politician at the centre of the debate welcomed the case as a new opportunity to advance a more critical tone against the welfare state: 'for a very long time it has been taboo to talk about the Carinas' (Daley 2013). The 'Poor Carina' case, as it became known, was powerful: in its aftermath, approximately four out of ten Danes changed their opinions on social assistance benefits, generally becoming less supportive (Hedegaard 2014), and the framing of welfare recipients as undeserving became more prevalent in Danish media (Esmark and Schoop 2017).

The second and related value of studying a Danish case is that it represents a hard test of the question about whether demonizing welfare recipients has mental health effects. Denmark features a strong historical consensus of support for a generous welfare state. Compared to settings like the UK, the poor tend to be portrayed positively in Danish media coverage (Larsen and Dejgaard 2013). The third advantage of our setting is that we can observe the effects of deservingness messaging by taking advantage of extraordinarily detailed administrative data not available elsewhere. For obvious ethical reasons, it is undesirable to experimentally impose messaging we expect to generate psychological costs, but administrative data allow us to exploit the natural experiment of the Carina case in great detail.

The case therefore allows us to examine if changes in deservingness messaging are associated with changes in the mental health status of the target population. Specifically, the empirical analysis estimates the use of antidepressants (selective serotonin reuptake inhibitor [SSRI] medication) by recipients of welfare benefits in the aftermath of media coverage of the case. Using a variety of estimation techniques, including an interrupted time-series analysis with panel logistic regression and a difference-in-differences (DiD) design with placebo groups, we observe the same finding: coverage of the Poor Carina case is associated with greater prescription of antidepressants

to the target population of welfare recipients. We observe a 1.2-percentage-point increase in the use of antidepressants in the weeks following a critical media event. The effects hold across a variety of demographic characteristics but are approximately four times higher for welfare recipients with prior mental health diagnoses.

The results offer a number of contributions. This is the first empirical test of the claim that we are aware of that offers causal evidence that negative social constructions created through deservingness messaging might deteriorate the mental health of target groups. In addition, the findings push a variety of literatures that have conceptualized citizen–state interactions, discussed in the following sections, to consider tangible ways that such interactions can generate psychological costs, such as worsened mental health outcomes. In doing so, we direct attention to one important aspect of the broader relationship between politics and health.

After reviewing relevant prior work, we draw on research in public health to establish the basis for a link between exposure to negative social constructions and health. We then summarize the case setting and the data and methods, before presenting and reviewing the results.

Social Constructions of Welfare Clients as Undeserving: Impacts on Public Opinion and Policy Design

Several theories are based on the fundamental idea that certain groups of welfare clients are perceived as undeserving by political actors and the mass public, and that such perceptions are consequential to both how target populations are treated and their own behaviour. The trope of the undeserving poor goes back to the origins of the social welfare state, embedded, for example, in the English Poor Law system (Katz 1989; Somers and Block 2005). In social welfare policy, while older adults and the sick and disabled are generally framed as deserving, other groups in poverty are viewed more negatively, especially if they are viewed as able-bodied enough to resolve their state (Van Oorschot 2006). In short, the poor are blamed for their conditions; their poverty is considered a function of inadequate effort, rather than structural economic conditions (Haney 2002; Somers and Block 2005).

Social construction theory (Schneider and Ingram 1997) argues that constructions in the popular debate of social groups as being either deserving or undeserving are absorbed by citizens and affect their orientations and civic participation. Such constructions help to explain 'why some groups get benefits and others get burdens' (Schneider and Ingram 1997, 3). Some groups are socially constructed as dependants, having low power but receiving empathy. Despite sympathetic statements, they tend to be provided inadequate benefits and may be subject to hidden burdens. Another category, the deviants, both have low power and receive little sympathy, making it difficult for them to claim even meagre benefits. In the social construction literature, deservingness messaging can affect the political orientation and participation of target populations, allowing for policy design to 'feed forward' onto target populations, altering their understanding of self (Ingram, Schneider and deLeon 2007).

The logic of social construction overlaps with policy feedback theory, which suggests that policies reconstruct target populations partly through the provision of resources that provides reason for mobilization, as well as via the civic lessons that policies teach to their subjects: 'Policies convey messages about group characteristics directly to members of a target group and to a broader public audience. Treatment under a given policy can make a group appear powerful or weak, trustworthy or devious, morally virtuous or morally repugnant' (Mettler and Soss 2004, 61). The policy feedback literature has documented how these policies affect people's political participation, views of government and ability to engage as full citizens in a democracy (Bruch, Ferree and Soss 2010; Campbell 2003; Mettler 2005).

Social constructions are dynamic, and so deservingness messaging might succeed in converting dependants into deviants by making claimants appear less sympathetic. To this end, stereotypes such as the welfare queen are powerful: they project the welfare claimant as part of a lazy and

amoral out-group, paving the way for less generous and more conditional welfare supports (Gilens 1999; Hancock 2004). Such changes are possible, argue Schneider and Ingram (2005), in a variety of situations. These include significant events and the skilful manipulation of those events by policy entrepreneurs, such as Reagan's use of the welfare queen or the mobilization of the Poor Carina trope in our case. Media portrayals reflect and reinforce notions of deservingness. For example, while black people are the minority of those in poverty in the US, they occur more frequently in media coverage of the issue and are more likely to be portrayed as non-working or even criminal (Clawson and Trice 2000).

Political communication theory treats such messaging about populations as a form of framing (Chong and Druckman 2007), designed to elicit an automatic and affective response from the public (Jensen and Petersen 2017). Such frames elicit psychological responses, such as anger, which may exert stronger influence than even cultural stereotypes or political values (Jensen and Petersen 2017; Petersen 2012).

Deservingness messaging shapes public support for or opposition to social welfare policy (Applebaum 2001; Kootstra 2020). For example, in the United States, alterations in the wording of survey questions tap into the negative social constructions people have towards welfare recipients. Since 1972, the General Social Survey has asked whether 'too little, too much, or just the right amount' is spent on a range of different social programmes, asking separate questions about spending on 'welfare' and on 'assistance to the poor'. While over time, 40 to 60 per cent of people have agreed that we spend too much on 'welfare', only about 10 per cent have said the same regarding 'assistance to the poor' (GSS Data Explorer 2019). Similar findings apply in other countries.

Public support for distributive programmes also appears to hinge on whether sufficient administrative burdens are imposed on groups seen as underserving (Nicholson-Crotty, Miller and Keiser 2021). Perceptions of deservingness affect street-level bureaucratic discretion when interacting with individuals seeking assistance, further affecting how resources are distributed in ways that limit access for those deemed 'undeserving' (Altreiter and Leibetseder 2015; Bruch, Ferree and Soss 2010; Jilke and Tummers 2018; Schram et al. 2009).

The Psychological Costs of Negative Messaging: Prior Evidence

While prior theories assume or infer that the negative framing of target populations would affect people's psychological orientations, existing research has largely not documented this. Policy feedback research has inferred how target populations feel by documenting how positive frames enhance political participation and negative frames suppress political participation (Bruch, Ferree and Soss 2010). Political communications research has documented how mass publics feel about target populations (Jensen and Petersen 2017). However, we still have limited evidence as to how being framed as undeserving makes the targets of those messages feel.

The question, then, is how and why the mental health of target populations might be affected by social constructions. Negatively constructed groups may accept the constructions and, in the words of Schneider and Ingram (1993, 344), 'buy into the ideas that their problems are not public problems, that the goals that would be most important for them are not the most important for the public interest, and that government and policy are not remedies for them'. In other words, negatively constructed groups will tend to internalize negative beliefs and stereotypes.

From this perspective, deservingness framings like the welfare queen may have stigmatizing effects on target populations, instigating negative effects on mental health. Administrative burden theory proposes that citizens can experience psychological costs as they interact with the state (Herd and Moynihan 2018). Psychological costs include the stigma of being associated with negative programmes, experiences of loss of autonomy via disempowering processes and stresses arising from the experience of administrative processes or the potential loss of benefits or rights. Stigma more broadly can be defined as 'the co-occurrence of labeling, stereotyping, separation,

status loss, and discrimination in a context in which power is exercised' (Hatzenbuehler, Phelan and Link 2013, 813). This definition corresponds with the concept of negative social constructions, as negative social constructions depend upon processes of labelling, stereotyping and status loss.

The experience of deservingness messaging may generate a variety of effects within target populations: it might discourage claims and access to benefits (Moffitt 1983), and undermine political self-efficacy (Bruch, Ferree and Soss 2010), but we know little about its health effects. Such psychological costs might undermine health by both discouraging access to health-supportive welfare programmes and directly undermining mental and other health conditions (Herd and Moynihan 2020). There is almost no evidence examining the latter causal pathway.

Public health evidence, however, provides a strong basis for hypothesizing that stigma related to welfare use and deservingness might affect mental health outcomes (Hatzenbuehler, Phelan and Link 2013; Link and Phelan 2006; Mak et al. 2007). A key assumption is that stigma is 'internalized', subsequently affecting mental health (Corrigan and Watson 2002; Goffman 1963; Link and Phelan 2001). There is robust evidence documenting these relationships. For example, studies have shown that being HIV positive is stigmatizing and that this stigmatization, in turn, increases the risk of depression and psychological distress (Grov et al. 2010; Li et al. 2009). Obesity has also been well documented as a stigmatizing condition, especially for women. Stigma appears to increase rates of depression and psychological distress (Emmer, Bosnjak and Mata 2020; Fettich and Chen 2012; Myers and Rosen 1999; Tronieri et al. 2017). In sum, meta-analyses across stigmatizing conditions, ranging from disability and obesity to gender and race, show that stigma can negatively affect mental health (see Benner et al. 2018; Mak et al. 2007; on race, see also Pieterse et al. 2012). To date, however, there is no empirical research testing how stigmatizing messages linked to poverty and welfare benefit receipt, or deservingness more broadly, might impact mental health, effectively as a form of policy feedback.

There is also not uniform agreement about the nature and heterogeneity of the effects of deservingness messaging on target populations. One strain of social welfare literature argues that target populations may simultaneously resist and concur with such frames, accepting that welfare recipients are undeserving but regarding themselves as 'exceptions' to the rule (Bullock 1999; Rank 1994). Such defensive reactions might offer a protective psychological mechanism that allows people to participate in welfare programmes but avoid a sense of personal stigma. If so, deservingness messaging should have limited psychological consequences. On the other hand, administrative burden theories emphasize how human capital shocks, such as illness, make some people more vulnerable to burdens imposed by the state (Christensen et al. 2020). According to this logic, people who experience some sort of human capital disadvantage, such as prior mental health challenges, will be more responsive to messaging that portrays them negatively.

In sum, while a number of literatures emphasize the importance of deservingness messaging, there is little evidence on whether target populations internalize it to the point that this stigmatizing messaging creates observable psychological costs. Public health research gives reason to believe that negative framing can be internalized, but social welfare literature suggests that recipients might employ protective psychological mechanisms to avoid internalization. There is, therefore, a need for research in which causal conclusions can be drawn from real-life settings.

The Case Setting: Poor Carina

Beliefs about target populations are conveyed by using stereotyped individuals to establish and exploit heuristics about a broader group (whether or not the stereotype of the individual is more broadly representative). In the context of welfare, the media and politicians can exploit a deservingness heuristic – widely agreed-upon beliefs that certain attributes make one more or less deserving of welfare support (Jensen and Petersen 2017). In the context of unemployment

policies, frames typically associated with constructing an undeserving population are a lack of motivation and amoral behaviour (Esmark and Schoop 2017). The criteria for deciding whether target groups are deserving recipients of welfare includes the extent to which people are seen as responsible for their own situation (the control criterion), their need (the need criterion), their proximity to the population at large (the identity criterion), their gratefulness for support (the attitude criterion) and the extent to which they have earned support (the reciprocity criterion) (Van Oorschot 2000, 36; Van Oorschot and Roosma 2015).

To test our proposition that deservingness messaging may affect the mental health of target group members, we need a case where a significant change in messaging about the target group occurs and where this change does not coincide with other important changes in living conditions of the encompassed target groups. We therefore turn to a media incident, known in Denmark as the 'Poor Carina' case. The case centres on the deservingness of unemployed recipients of Danish social assistance benefits, based on an exemplar who challenged many of the criteria for deservingness.

On 28 November 2011, a 36-year-old single mother given the pseudonym 'Carina' was visited by two members of the Danish parliament. A representative of the Socialist People's Party, at that time, part of the government coalition in Denmark, had sought out Carina as an example of someone who was on benefits but still faced real needs. Her counterpart from the Liberal Alliance opposition party was part of a movement that pushed for a new formal definition of poverty and had argued that no one on welfare was truly poor. As details of Carina's case were debated between the two politicians on national television, it became harder to sustain that Carina was poor in an absolute sense. She was earning about US$2,700 per month after tax, including subsidized housing, which was far above the official Organisation for Economic Co-operation and Development (OECD) poverty limit.

Carina's stated lack of desire to engage in employed work, ungratefulness for benefits and higher disposable income than some groups of people in low-income jobs touched upon some key criteria by which deservingness is established. The backlash picked on such details as her flat-screen TV, her use of cigarettes and her reluctance to ask her family for support. Discussions of Carina in the media contrasted her situation with full-time workers with lower disposable incomes. On welfare from 16 years of age and hoping to be awarded disability benefits because of anxiety, Carina was compared unfavourably to the working poor.

The nickname 'Poor Carina' was intended to be ironic, conveying the perceived lack of deservingness. Politicians from both opposition and government argued that Carina was proof that it did not pay to work (Stanners 2011). Even those who argued for the welfare state, including the politician who had chosen Carina as an example, conceded that she was not truly poor and that her case reflected systemic problems in the welfare system. The Minister of Social Affairs at the time said:

> Many Danes need to seriously re-address their own value system and again take a pride in managing in their own lives. We have to stop considering society as a 'smorgasbord' that we don't need to contribute to, and that's a debate I'd like to see high on the social welfare agenda. (Chebotareva and Young 2011)

The effects of news events on individual opinion and behaviour are diluted by public attention to the news and the specific topic, and, for our study, the degree to which individuals identified with Carina. It is therefore helpful to understand the degree of public attention to the topic. One analysis found that the case tripled media attention to social assistance programmes (Hedegaard 2014).[1] Carina led to media coverage of other cases, generating unprecedented public discussion

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[1] While no evidence exists of the extent to which the specific target group followed the Carina story in the media, media consumption analyses suggest that national television and other media, such as the social media where the incident was also discussed, is used by around 90 per cent of adult Danes, and that news and social media consumption of Danes outside the labour market and low-income groups is similar to that of other groups (Jensen et al. 2019, 26).

of the deservingness of welfare claimants (Daley 2013).[2] The incidents were certainly not the sole basis for public discussion about welfare. Indeed, the whole point of the Carina visit was to inform a broader debate about poverty measures. However, Carina transformed and personalized the framing of welfare recipients in a way that dry discussions about absolute and relative poverty measures or changing demographics could not. She became a stereotype that people could recall when considering welfare, even if she was not representative of the actual welfare population.

Poor Carina and ensuing cases changed how Danes viewed the welfare state (Scrutton and Jensen 2013). Prior to the cases, 23 per cent of respondents in a two-wave panel study said that the Danish welfare state spends too much on social assistance, while after the cases, this number increased to 29 per cent. There was also a four-point increase in the number of people who expressed uncertainty about the right approach. While the drop in support was small in the aggregate, it facilitated polarization on the topic, with people interpreting the case to fit with their pre-existing biases. People with more anti-egalitarian values developed stronger opposition to the welfare system (Hedegaard 2014). There was also a clear spike in media discussions of social assistance after the cases became public (Hedegaard 2014).

Not only was there more attention to the topic, but the tone of the coverage also changed, reflecting the growing narrative of undeserving claimants. Traditionally, the poor and welfare recipients enjoyed more sympathetic media coverage in Denmark than in such settings as the UK (Larsen and Dejgaard 2013). A cross-time analysis of Danish media coverage of welfare issues (which itself directly reflects political attitudes and statements) found markedly higher reliance on tropes of undeservingness in 2013 relative to a previous discussion of welfare reform in 2005. In 2005, there was about equal use of language to convey deservingness and undeservingness, but in 2013, undeservingness language was twice as prevalent (Esmark and Schoop 2017). Thus, the Carina case might be seen as either a shock to a pre-existing welfarist consensus or a turning point where a conflicted policy began to shift towards a more sceptical perspective typical of more neoliberal settings. Deservingness framings would increasingly come to shape policy-maker perspectives. For example, a survey experiment showed that Danish policy makers on both the Left and the Right were similarly willing to impose administrative burdens on welfare recipients framed as undeserving (Baekgaard, Moynihan and Thomsen 2021).

None of this tells us much about how welfare recipients experienced the deservingness messaging around Carina, with the exception of Hedegaard's (2014) panel study, in which he finds that those who were likely to be more dependent on welfare were less likely to have their attitudes towards the generosity of the welfare state affected by the Poor Carina case. In this respect, the target population resisted deservingness messaging, but as Hedegaard points out, it is in their interest to do so; therefore, we cannot know whether they internalized the negative framing of their status and suffered any psychological costs.

Data and Methods[3]

Data Source

We use individual-level register data collected by Statistics Denmark on Danish social assistance recipients in the year 2011. In consecutive analyses, we compare this trend to that of other groups

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[2]The most notable other case was that of Robert Nielsen, who became known as 'Lazy Robert' for his objection to taking undesirable jobs. We did not include the Robert case in our analysis for two reasons. First, the Carina case preceded the Lazy Robert case. The scale and novelty of the coverage compelled welfare recipients to update their beliefs about how they were viewed by politicians, the media and society more broadly. For the Lazy Robert case, coming less than a year later, there was an obvious precedent in the Carina case and less reason to update beliefs because the Carina case had already demonstrated widespread criticism to a somewhat more sympathetic figure. Second, the Lazy Robert case coincided with the passing of legislation that had a direct impact on the size of social assistance benefits, thus making it impossible to isolate the impact of messaging associated with Robert from changes in resources available to recipients.

[3]While we did not formally pre-register, our hypotheses and design were explicitly proposed in a funding proposal to the Horizon 2020 programme (POAB 802244: The Psychology of Administrative Burden) before the analysis was undertaken.

of comparable individuals, as well as the trend in the outcome variable for the same individuals in previous years. Permission to use the data for this study was granted by Statistics Denmark, the Danish Data Protection Agency and the Danish Health Data Agency. The anonymized data can only be accessed on a password-protected server managed by Statistics Denmark.

Outcome Variable

Given our interest in the consequences of negative social constructions on mental health and prior evidence that stigma can induce stress (Hatzenbuehler, Phelan and Link 2013), our analysis estimates whether social assistance recipients were more likely to receive antidepressant medication in the aftermath of the Carina TV interview. Specifically, we focus on whether recipients were more likely to receive antidepressants of the types N05 and N06 in the Anatomical Therapeutic Chemical Classification (ATC) system. These drugs include SSRI drugs, which are widely used to combat symptoms of stress, anxiety and depression. In Denmark, this kind of medication is prescribed by general practitioners based on medical assessments. Access to general practitioners is generally free in Denmark. Each Dane is attached to a general practitioner and can book time for services in the consultation hours of the general practitioner.

Establishing the Time Series

We focus on people who received benefits both prior to and after the media incident. Since the Poor Carina incident took place on 28 November 2011, and since new rules on social assistance benefits (BEK No. 190 2012) came into effect on 1 January 2012, we limit our analysis to the time period before 1 January 2012 to obtain an estimate that is unaffected by the implementation of the new rules. Furthermore, to reduce the risk that prior events affect our estimates, we also limit the time span leading up to the incident. For individuals in our main specification, we therefore limit the analysis to include only those who received social assistance benefits ±four weeks from the incident. This amounts to a total of 101,328 individuals who are observed over eight weeks, amounting to more than 800,000 observations. Our strategy thus allows us to study the short-term effects of the media interview and immediate post-interview discussions. In placebo tests of our interrupted times series, we extend the timeline to cover a total of fourteen weeks (ten weeks prior to the incident and four weeks after).

We generated a time-series coding on a weekly basis for each individual in our dataset as to whether antidepressants were prescribed to them. Thus, each observation contains information on the number of prescriptions of antidepressants in a given week for each recipient of social assistance benefits. Since we are interested in whether antidepressants were prescribed rather than the number of times this happened, we recode the outcome variables to dummy variables taking the value of 1 if the medication was prescribed and 0 if not.[4] Since 28 November was a Monday, the time series was organized such that each week in the dataset begins on a Tuesday, ensuring that the first post-interview week begins the day after the airing of the Poor Carina interview.

Analysis

Interrupted Time-Series Analysis

Our main specification relies on an interrupted time-series analysis, but we also subject this to a series of sensitivity analyses in the following sections. Our time-series analysis includes only those

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[4]The number of times medication has been prescribed is unlikely to provide useful variation in the dependent variable, as only a very minor share of the sample had such medication prescribed more than once per week in the period of interest. More importantly, prescriptions may contain different doses or reflect that recipients did not react well to the first drug prescribed and therefore received another type instead.

Table 1. Descriptive statistics for the main sample

Variable	Mean	SD	N
Recipience of antidepressants			
– Weekly share of respondents receiving	0.044	0.106	101,328
– Recipience at least once over eight-week period	0.209	0.407	101,328
Visits to medical practitioner			
– Weekly share of respondents visiting	0.162	0.189	101,328
– Visiting at least once over eight-week period	0.592	0.492	101,328
Previously diagnosed with stress, anxiety or depression	0.231	0.421	101,328
Female	0.524	0.499	100,303
Age (years)	36.049	11.885	100,303
Education (more than secondary school)	0.348	0.476	96,406
First-generation immigrant	0.235	0.424	100,303
Second-generation immigrant	0.024	0.152	100,303
Single mother with one or more kids living at home	0.049	0.216	100,303
Other single-person households	0.596	0.491	100,303
No. of consecutive days on social assistance benefits (truncated at five years)	199	274	101,227

individuals who received social assistance benefits in all eight weeks in our time frame and who – like Carina – were not part of any education or job training programme during this period. Table 1 provides descriptive statistics for the main sample. On average, 4.4 per cent of the sample received a prescription for antidepressants, while 16.2 per cent visited a medical practitioner in any of the eight weeks covered. In total, 20.9 per cent received a prescription in at least one of the eight weeks, while almost 60 per cent visited a medical practitioner at least once. These relatively high numbers reflect a correlation between ill-health and unemployment: almost one quarter of the respondents had been diagnosed with stress, anxiety or depression before the Poor Carina incident.

Women and men are represented to an almost equal extent in the sample. The age of the sample is in the interval of eighteen to sixty-four years, with an average of around thirty-six years. More than one third of the sample had not finished a post-secondary education. Around one quarter of the sample consists of immigrants. Two thirds of the sample are single-person households, and around 5 per cent are single mothers with children living at home. In our analysis, we include the sociodemographic indicators as covariates to improve the precision of our estimates. We use random-effects logistic regression with individual-level cluster robust standard errors to account for correlated error terms.[5]

If the Poor Carina TV interview had the expected effect, we should observe an increase in medical prescriptions among target group members in its aftermath. Models 1 and 2 in Table 2 (using a fixed-effects and a random-effects specification, respectively) show that this is indeed the case, as the dummy variable that distinguishes between observations before and after the interview is positive and statistically significant ($p < 0.001$ in both models). The estimate in Model 1 is based on an individual fixed-effects regression and involves a reduced sample size because the dependent variable is constant over time for many individuals, who are thus excluded from this analysis. The estimate in this analysis amounts to a 0.21-percentage-point increase in the reception of antidepressants. Model 2 is based on a random-effects specification, thus allowing us to use the full sample size. Here, the estimate is significant and equivalent to an increase in the likelihood of receiving antidepressants of 0.3 percentage points. Since this is measured on a weekly basis, the total difference over the course of the full period amounts to 1.2 percentage points. With a sample containing around 100,000 individuals, this, in turn, is equivalent to

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[5]In robustness tests, we use individual-level fixed-effects regressions instead. While this strategy effectively controls for individual-level differences, a huge share of the sample is excluded because of constant outcomes across all eight weeks. In the analysis reported in Model 1 in Table 2, for instance, the sample is reduced to 21,156 individuals if specified using fixed effects. The results using fixed effects do replicate well, as shown in Model 1 in Table 3.

Table 2. Interrupted time-series analysis: panel logistic regression

	1	2	3	4	5
			Placebo: cut-off two weeks earlier	Placebo: cut-off four weeks earlier	Placebo: cut-off six weeks earlier
	Main test Fixed effects	Main test Random effects	Random effects	Random effects	Random effects
After interview/cut-off	0.084*** (0.011)	0.082*** (0.012)	0.022* (0.012)	−0.007 (0.011)	−0.013 (0.011)
Female		0.254*** (0.020)	0.254*** (0.0201)	0.253*** (0.020)	0.261*** (0.020)
Age		−0.001 (0.001)	−0.001 (0.001)	−0.002* (0.001)	−0.001 (0.001)
Education		0.184*** (0.021)	0.188*** (0.021)	0.191*** (0.021)	0.191*** (0.021)
First-generation immigrant		−0.585*** (0.025)	−0.591*** (0.025)	−0.567*** (0.025)	−0.597*** (0.025)
Second-generation immigrant		−1.038*** (0.082)	−0.950*** (0.079)	−0.918*** (0.078)	−0.960*** (0.079)
Other single-person households		0.083*** (0.021)	0.087*** (0.021)	0.077*** (0.021)	0.049* (0.021)
Single mother with one or more kids living at home		0.188*** (0.044)	0.175*** (0.044)	0.185*** (0.044)	0.176*** (0.044)
Days on social assistance benefits		0.001*** (0.000)	0.001*** (0.000)	0.001*** (0.000)	0.001*** (0.000)
Intercept		−4.560*** (0.039)	−4.541*** (0.039)	−4.514*** (0.039)	−4.492*** (0.039)
N (observations/individuals)	169,248/21,156	764,336/95,542	764,336/95,542	764,336/95,542	764,336/95,542
χ2	50.86***	1992.47***	1922.86***	1826.76***	1881.12***

Notes: Entries are panel logistic regression coefficients; cluster robust standard errors in parentheses. * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$.

around 1,200 individuals receiving prescriptions who would not otherwise have received such prescriptions. As shown in Tables S1–S3 in the Online Appendix, this result is robust to including covariates, including a control for visits to medical practitioners (see Table S1), an interaction between the week-count variable and the cut-off dummy to account for differences in trends before and after the time of the cut-off (Bernal, Cummins and Gasparrini 2017) (see Table S2), and interactions between the cut-off dummy and higher-order polynomials of the week-count variable (see Table S3).

If it was indeed the interview that caused the increase, we should expect less significant results if other cut-offs were used instead. To explore if this is the case, we add additional data from the six weeks before the eight weeks included in the main analysis. We then estimate models over the course of eight weeks but where the cut-off point is moved two weeks ahead in Model 3 (meaning that the cut-off dummy compares antidepressant usage six to two weeks prior to the interview with the usage two weeks before to two weeks after the incident), four weeks ahead in Model 4 and six weeks ahead in Model 5. While the estimate is still statistically significant at the 0.05 level in Model 3, the coefficient is only around a quarter of the size of the estimate in Model 1. Bearing in mind that the two weeks right after the incident are still contained in the cut-off dummy in this analysis, the model lends some support to the proposition that the interview led to an increase in antidepressant usage. This is further supported by the analyses in Models 3 and 4, in which we do not identify any significant effects of the cut-off.

Placebo Analysis

Our identification strategy in Table 2 exploits variation in outcomes before and after the media incident, treating the incident as an exogenous event that has the potential to alter how welfare recipients see themselves. To put the findings in Table 2 to a harder test, we use a DiD design with placebo groups. We compare the difference in outcomes before and after the media incident for the treatment group (that is, those who received benefits) with the difference in placebo groups to examine whether possible changes in the outcome variable differ between recipients of benefits and the placebo group as we would expect. The difference in trends between the treatment and placebo groups can then be ascribed to the media incident if: (1) the media incident did not coincide with other major events of relevance to either the treatment or placebo group that could plausibly lead to mental health deterioration; (2) the placebo group was not plausibly affected by the media incident; (3) the treatment and placebo groups exhibit parallel trends in the outcome variable prior to the media incident; and (4) trends would have been the same in the treatment and placebo groups in the absence of the incident (Angrist and Pischke 2009, 230; Jaeger, Joyce and Kaestner 2020, 319).

We rely on four different strategies to create placebo groups that live up to these criteria, assuming an immediate impact of the interview. In these analyses, we use the same approximately 95,000 individuals as in the interrupted time-series analysis as our treatment group. Thus, the treatment group alone contains more than 760,000 observations. First, we compare the trend over the eight-week period in the treatment group to the trend in the same eight weeks in placebo groups consisting of the exact same individuals as in the treatment group but just observed in each of the years 2007–10, thereby increasing the number of observations by a factor of five. This strategy builds on the fact that the placebo group could not have been affected by the Poor Carina interview in previous years, and it enables us to reduce noise considerably by exploiting a fully balanced panel.

It could be that, for some reason, those receiving social assistance benefits have different seasonal trends in medication use than others or use medication to different extents than others. Indeed, while we find that pre-interview trends are parallel in the treatment and control groups in the first placebo analysis, it is also evident that the very same people in the treatment group receive considerably more antidepressants when on social assistance benefits (see Figure S1 in

the Online Appendix). A likely cause of this pattern is that long-term unemployment in itself impairs mental health (Paul and Moser 2009) and thus the use of antidepressants has increased over time for the treatment group, creating a difference in the level of usage over time when they are compared with themselves in previous years, as in placebo analysis 1. As argued by Kahn-Lang and Lang (2020), this could be a real challenge to analyses relying on DiD designs. In our second placebo analysis, we therefore compare the change in medication use in the treatment group with that of those people who received social assistance benefits at the same time of the year in any of the years 2007–10. This analysis thus draws on a substantially higher number of individuals but not on more observations, as we observe medication usage for each year for those individuals who received social assistance benefits in the eight-week window that we are interested in and who were not part of any education or job training programme. Thus, as in the first placebo analysis, we expect that medication use will significantly increase in the treatment group as compared to the placebo group. As demonstrated in Figure S1 in the Online Appendix, pre-treatment trends and levels of the dependent variable are very similar in the treatment and placebo groups in this analysis.

A disadvantage of the first two placebo strategies is that the weather or other circumstances may influence trends in the placebo group and create differences between 2011 and previous years that are unrelated to the media incident of interest. If this is the case, previous years may not be proper placebo groups. Therefore, we supplement our analysis with a third placebo analysis in which we focus on the year 2011 only and compare the treatment group with a placebo group of non-recipients of social assistance benefits. A solid match on background characteristics is unlikely in this case. Instead, to increase comparability between the treatment and placebo groups, and to ensure parallel trends in outcomes prior to the incident, the third placebo analysis focuses on a group of individuals who received social assistance benefits at any point in the first nine months of 2011 but not at all in the last three months. The people in this placebo group consist of both individuals who received other unemployment-related benefits than social assistance benefits at least part of the time in the last three months of 2011 and individuals who did not receive any unemployment-related benefits at all. It is possible that people who received some kind of benefits were still affected by the interview and consequent discussion. To explore if this is the case, we reduce the placebo group even further in our fourth test to those individuals who: (1) did receive social assistance benefits at some point the first nine months of 2011; (2) did not receive any such benefits in the last three months of 2011; and (3) received other unemployment-related benefits in at least part of the last three months in 2011. In both the third and fourth placebo analyses, there are some differences between treatment and placebo groups on background characteristics (see Table S10 in the Online Appendix). These analyses should therefore be considered a supplement to the prior placebo analyses.

For all placebo analyses, we use an interaction term between the treatment variable and the cut-off dummy denoting whether the week in question is before or after the date of the Poor Carina interview. Positive and statistically significant interaction coefficients are evidence that coverage of the incident increased the use of antidepressants among welfare recipients. We specify analyses both without (see Panel 1 of Table 3) and with (see Panel 2 of Table 3) week fixed effects. Table 3 provides a summary of the results, while the full regression tables can be found in the Online Appendix. As shown in Table 3, our placebo analyses lend support to the proposition that the media incident led to a short-term increase in reception of antidepressants among social assistance recipients, though it should be noted that the estimated weekly effects are substantially weaker than in the interrupted time-series specification. As is evident from Tables S4–S9 in the Online Appendix, these estimates are very stable across specifications, regardless of whether sociodemographic variables are included as covariates, possibly endogenous variables like the duration on social assistance benefits or visits to the medical practitioner are controlled for, or week fixed effects are included.

Across all specifications (interrupted time series and placebo), we are left with estimates suggesting a weekly effect varying between 0.04 and 0.30 percentage points. Effects are stronger in

Table 3. Results from placebo specifications

	Placebo 1	Placebo 2	Placebo 3	Placebo 4
Panel 1: without week fixed effects				
DiD estimate	0.038**	0.052***	0.055*	0.055*
	(0.012)	(0.012)	(0.024)	(0.026)
Weekly effect (percentage-point increase)	0.17	0.19	0.21	0.24
Panel 2: week fixed effects included				
DiD estimate	0.039***	0.052***	0.055*	0.055*
	(0.012)	(0.012)	(0.024)	(0.026)
Weekly effect (percentage-point increase)	0.04	0.04	0.13	0.15
Placebo group composition	Treatment group in previous four years	Recipients of social assistance benefits in previous four years	Recipients of social assistance benefits 1/1–30/9 2011 but not 1/10–31/12 2011	Recipients of social assistance benefits 1/1–30/9 2011 and of other unemployment-related benefits 1/10–31/12 2011
Years included in analysis	2007–2011	2007–2011	2011	2011
N (observations/individuals)	3,741,440/95,638	3,293,992/178,827	1,152,984/144,123	998,136/124,767

Note: The placebo analyses are based on random effects specifications and use the same set of control variables listed in Table S1, model 3. * $p < 0.05$; ** $p < 0.01$; *** $p < 0.001$.

specifications without week fixed effects, which can be explained by the fixed effects absorbing much of the weekly variation. From a substantive point of view, the effect is relatively weak in the fixed-effects estimations in particular. This may be unsurprising given that our dependent variable of antidepressant use is a relatively extreme outcome that arguably does not capture milder symptoms and that the population studied already has relatively high consumption of antidepressants. Also, it should be kept in mind that this is measured on a weekly basis; thus, with more than 100,000 people in the specified target groups, the total difference over the full four weeks amounts to an additional 160–1,200 recipients receiving antidepressants due to the incident.

Another important caveat is that the case occurs in a setting where the population has relatively easy access to healthcare and where many were already visiting doctors on a regular basis. In settings where the stigmatized group has more limited access to healthcare, measures of antidepressant consumption are less likely to change, even if the target population experience equivalent psychological costs to those we study. In terms of the magnitude of the effects, prior work shows that seemingly distant shocks – such as changes in daylight savings time (Berk et al. 2008; Hansen et al. 2017; Kuehnle and Wunder 2016) or terrorist events in other countries (Hansen et al. 2016; Sønderskov et al. 2021) – can have large negative mental health effects. This further makes the detection and size of effects seen in our case plausible.

Heterogeneous Effects

In this section, we consider if the effects of the messaging are heterogeneous and fall harder on some groups than others. Generally, we find little evidence of such effects, with one exception: people who have already shown prior evidence of mental health problems are more vulnerable to the deservingness messaging.

Table S11 in the Online Appendix returns to the interrupted time-series specification to explore if estimates are heterogeneous across various groups of social assistance recipients. Model 1 tests if long-term recipients of social assistance benefits react differently than short-term recipients. One possibility is that people who have reason to more closely identify with Carina are more susceptible to the critical messaging. Models 2 and 3 explore if identification with Carina matters by testing if single mothers with kids (see Model 2) or women in general (see Model 3) are differently affected than others. Since all interaction terms are far from being statistically significant, we find no evidence supporting these propositions. Model 4 examines if the increase in medication recipience is driven by clients or by medical practitioners. Indeed, the behaviour of medical practitioners is an alternative source of variation in our dependent variable, as their view of social assistance recipients may have also been affected by the media incident. If this was the case, we should find a statistically significant interaction between the cut-off point and visits to medical practitioners. Again, however, we end up with a statistically insignificant result, suggesting that the result is mainly driven by clients.[6]

The administrative burden framework also predicts that some people, including those with human capital challenges, such as illnesses, are more vulnerable to negative experiences, including psychological costs (Christensen et al. 2020). We therefore investigate if those who already have demonstrated mental health challenges – as indicated by a prior stress-related diagnosis – are more vulnerable to the Poor Carina coverage. Model 5 therefore explores if effects are stronger among those social assistance recipients who had at some point prior to the Poor Carina incident been diagnosed with stress, anxiety or depression-related disorders. Specifically, we focus on four diagnoses that are all characterized by potential vulnerability towards negative events (F32, F33, F41 and F43 in the International Classification of Diseases 10th Revision [ICD-10]) and code people as diagnosed with these disorders if they have received any such diagnosis from 1995

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[6]While the results support this interpretation, it should be noted that our data do not allow us to distinguish between different causes of visits to medical practitioners. The results are therefore, at best, an indication – not a proof – that the results are driven mainly by clients.

onwards. Those with a prior diagnosis to a substantially higher extent than those without received antidepressants prior to the Poor Carina incident (8.5 per cent versus 3.1 per cent on average per week), confirming that diagnosis is indeed an important predictor of antidepressant usage. As demonstrated in Model 5, the estimate for the interaction between the cut-off dummy and the dummy for prior diagnosis is clearly statistically significant and positive. Thus, the weekly effect of the Poor Carina incident is more than four times higher among those with a prior diagnosis (an estimated weekly effect of 0.7 percentage points; or a total effect of 2.8 percentage points over the course of four weeks) as compared to those without a prior diagnosis (weekly effect = 0.17 percentage points).

We further probe this finding by conducting split-sample analyses of the first four placebo groups. As shown in Table S12 in the Online Appendix, overall, these additional tests support the proposition that effects were stronger among those with a prior diagnosis (weekly effect sizes are larger in all cases), though the estimated effects for those with a prior diagnosis are statistically insignificant in the analysis of the second and third placebo groups.

Conclusion

A variety of studies have posited that negative social constructions can hurt citizens in several ways – via less generous programmes, more burdensome procedures and discouraging political participation. This study offers direct evidence, with objective indicators, that deservingness messaging not only limits access to political and economic resources, but also exerts negative psychological costs. Stigmatizing messages worsen people's mental health. Specifically, in the four weeks after the Poor Carina coverage, welfare recipients became more likely to increase their use of stress/anxiety-reducing medication. The effects were concentrated among those with existing mental health problems. Relative to subjective self-reports of mental health, the findings provide objective indicators that recipients felt sufficiently motivated to seek out medical help, and that medical professionals felt that their condition warranted additional prescription medication.

We note some limitations of our analysis. First, the substantive effect sizes are small. This is unsurprising bearing in mind the limited variation on the dependent variable. It is also the case that we examine an extreme indicator of psychological costs: the consumption of antidepressants. We do not know how many more experienced milder negative psychological effects. That said, the concentration of these effects among those with existing mental health problems is reassuring from an analytical perspective. There is robust evidence that individuals with existing mental health problems are more sensitive to environmental triggers that would negatively affect their mental health than are those without an existing issue (Domingue et al. 2017; Zhao et al. 2018).

Second, we study a single case in one country, reflecting the difficulty of finding quasi-experimental designs that are realistic while also being ethical. Our particular findings may depend on case-specific factors, such as the norm-breaking nature of the case and the access target populations have to mental health services. On the other hand, the public health literature warns about the cumulative effect of negative experiences on health. In other words, the underlying mechanism of deservingness messaging generating mental health outcomes appears generalizable, and research points to the relevance of such frames as deservingness in very different settings (Aarøe and Petersen 2014). In either case, the ubiquity, durability and power of tropes like the welfare queen or Poor Carina demands that we better understand their effect on the people towards whom the stereotypes are directed.

Third, the field-based nature of our case means that we cannot separate the effect of media and political messaging. We are therefore studying the compound effect of both, reflecting the reality of such messaging. Politicians depend on media for a platform for their views, even when those views are being contested, and media is influenced by politicians' framing of events. For example, the term 'Poor Carina' was coined by a politician but became the dominant frame through which the media conveyed the case and the broader policy issue. Future work, such as survey experiments, could try to separate the relative influence of different sources of stigmatizing information.

Overall, our results emphasize how politics influences people beyond their political views and political engagement. For research on politics and health, it points to deservingness messaging as one vehicle by which political actors can directly affect the health of citizens. This research has pushed the public health literature to connect downstream health consequences with upstream politics (Greer 2004; Greer 2018; Schrecker and Bambra 2015). Most of this research, however, has emphasized mediating mechanisms, ranging from policies to institutions, but little research has examined direct effects. Future work could look at other forms of negative messaging to isolate their effects on target populations. While our results centre on welfare recipients, the rise of populism speaks to the need for broader attention to negative political messaging. Populist messages are defined by demarcating in-groups and out-groups, and directing negative messaging to out-groups, who could include immigrants, minority ethnic groups and, in some settings, public employees (Thomann and Rapp 2018).

Further, while prominent theories, particularly social construction, policy feedback and administrative burden, assume or infer that the politics of the negative framing of target populations would affect people's images of themselves or their psychological orientations, existing research had largely not documented this. Policy feedback research inferred how target populations feel by documenting how positive frames enhance political participation and negative frames suppress political participation (Bruch, Ferree and Soss 2010; Mettler and Soss 2004), and political communications research documented how mass publics feel about target populations. However, our research documents that being framed as undeserving makes the targets of those messages feel the psychological consequences of that framing. One additional implication for policy feedback work to consider is that depression is itself associated with lower political participation and voting (Landwehr and Ojeda 2021; Ojeda 2015). In other words, negative messaging that makes target populations experience higher psychological cost may discourage their political participation and reduce their ability to protect the public supports they depend upon.

While much of the focus of social constructions of deservingness has been on their impacts on the mass public, there is much to learn about their effects on target populations. In our case, we find that the process of internalizing deservingness messaging did not vary by subgroup within the target population for such reasons as personal identification with the recipient. Rather, it seemed that deservingness messaging seemed to have a broad-based effect on people on welfare. This suggests that fellow welfare recipients who were of a different gender or family situation did not use those characteristics to distinguish themselves from Poor Carina, but internalized the related messaging to be about them. In situations where deservingness or other types of negative messaging invokes sociodemographic factors by, for example, using racialized terms, the results may be different. However, our findings also point to the potential of looking beyond such variables. The finding that deservingness messaging had much stronger effects on those who were already vulnerable to mental health shocks alerts future research to consider variation in human capital as a basis for better understanding the heterogeneous effects of such messaging.

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