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LABOUR MARKET SHOCKS AND THE DEMAND FOR TRADE PROTECTION: EVIDENCE FROM ONLINE SURVEYS*

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We study preferences for government action in response to layoffs resulting from different types of labour-market shocks. We consider: technological change, a demand shift, bad management and three kinds of international outsourcing. Support for government intervention rises sharply in response to shocks and is heavily biased towards trade protection. Trade shocks generate more demand for protectionism and, among trade shocks, outsourcing to a developing country elicits greater demand for protectionism. The 'bad management' shock is the only scenario that induces a desired increase in compensatory transfers. Trump supporters are more protectionist than Clinton supporters, but preferences seem easy to manipulate: Clinton supporters primed with trade shocks are as protectionist as baseline Trump voters. Highlighting labour abuses in the exporting country increases the demand for trade protection by Clinton supporters but not Trump supporters.

Economists have long known that opening up to international trade can have significant labour market consequences.[1] But trade is neither the only, nor even the most important, source of shocks in labour markets. Trade typically accounts for only a minor share of layoffs or involuntary separations and the bulk of labour-market churn has little to do with trade.[2] Manufacturing, where most trade-related job losses occur, is a small part of the economy—less than 10% of non-farm payrolls in the United States—and has been on a shrinking path for decades. Most US workers are employed in services and are shielded from the forces of international competition. And, even within manufacturing, changes in domestic demand and technology (e.g., automation) play a much greater role than import shocks.[3]

Yet we don't see much political opposition to technological change or to shifts in domestic demand patterns. International trade looms much larger in our political debates. Where does the

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The data and codes for this paper are available on the Journal website. They were checked for their ability to replicate the results presented in the paper.

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[1] The well-known Stolper and Samuelson (1941) theorem shows that in a two-good, two-factor economy with perfect factor mobility, one of the two factors must end up worse off with the opening to trade. Even though the original Stolper–Samuelson theorem relies on highly special assumptions, the conclusion that free trade hurts some groups is quite general (Rodrik, 2018). Essentially, it depends only on the assumption of incomplete specialisation. In other words, the home economy produces import-competing goods (see Rodrik, 2018).

[2] See Davis, Haltiwanger and Schuh (1998); Kletzer (2001); Edwards and Lawrence (2013, pp. 47–52). On trade, recent empirical work has documented sharp distributional consequences for the case of the North American Free Trade Agreement (NAFTA) (Hakobyan and McLaren, 2016) and China's entry into the WTO (Autor *et al*., 2013). The rise of protectionist and nativist political movements in the United States and Europe has been linked to the labour market shocks emanating from globalisation (Becker *et al.*, 2016; Autor *et al.*, 2016; Colantone and Stanig, 2017; Guiso *et al.*, 2017).

[3] Acemoglu *et al.* (2016) estimate that the China trade shock accounts for 10% of the job loss in manufacturing during the 2000s (or at most nearly 20% when the indirect effects are taken into account).

anti-trade sentiment come from? It could be that voters exaggerate the relative significance of trade shocks, relative to other labour-market disruptions. Or they could be more easily swayed by demagogues who target foreign nations. Alternatively, they might view trade shocks as inherently different from other kind of shocks, deserving of a stronger government response.

In this article we provide evidence on how individuals' policy preferences are shaped in response to different kinds of labour-market shocks. Specifically, we study how opinions about trade protectionism and compensatory financial transfers change when individuals are presented with six different types of shocks, all of which have the same effect on local labour markets (a plant closure leading to unemployment and lower wages).

Our data come from the United States where we implemented a large-scale online survey in which subjects were exposed to a piece of news formatted as a newspaper article on an impending garment plant closure. Subjects were divided randomly into six treatment groups corresponding to the six different scenarios described in the 'news article': (*i*) a drop in the demand for the good produced by the factory ('demand shock'); (*ii*) disruption in production due to new, labour-saving technology ('technology shock'); (*iii*) mistakes by management ('bad management'); (*iv*) international outsourcing to an advanced country; (*v*) international outsourcing to a developing country; and (*vi*) international outsourcing to a developing country with an emphasis on poor labour standards. In addition to these six treatments, we had a control scenario in which the news article discussed some changes in the factory without mentioning job losses. Our subjects were then asked about their support for various types of government action. They could choose to do nothing, provide government transfers to the displaced workers or impose trade protection.

Economists typically argue that the best way to respond to adverse labour market developments when we care about distributive outcomes is through transfers to workers rather than protectionism. Trade protection is a highly distortionary way of compensating the losers: it (over-)taxes a narrow range of commodities (imports) and encourages inefficient domestic production. The baseline preferences of our sample are consistent with this. Unemployment compensation and training assistance are generally preferred to import protection by a large margin.

Exposure to our news stories about labour-market shocks results in a sharp increase in support for government action. And here the results are sharply at odds with what economists recommend. The *changes* in the desired government response are heavily biased towards trade protection rather than financial assistance. The increase in the demand for government transfers, relative to the control scenario, is small (of 10% or less). Meanwhile the demand for trade protection rises by a magnitude that ranges between 20 and 200%. Trade protection is nearly always the favoured response to labour-market shocks at the margin, even when job losses are due to non-trade factors such as technology and demand shocks. Evidently, when our respondents are primed with specific accounts of factory closure, they perceive trade protection as a more appropriate instrument for helping displaced workers than financial transfers.[4]

An interesting exception is the case of job losses caused by management failures. In this case, it is the demand for compensatory transfers that goes up without much of a rise in the demand for protection. Unlike transfers, trade protection helps employers as well as employees. Our respondents seem unwilling to reward management through import protection when job losses

[4] In a large country like the United States some degree of import protection can be beneficial insofar as it improves the external terms of trade of the country. Even if our respondents internalise this 'optimum tariff' logic, we expect that this would show up in the baseline level of support for import protection. It would not account for the differences between treatment and control or differential responses across treatments.

are due to management failure—and hence the preference for direct transfers to labour in this scenario.

An important takeaway is that people do not treat different types of job loss uniformly. They distinguish among labour market shocks according to what produces them; the elasticity of the response depends on the operative mechanism. Non-trade shocks such as technology and demand shocks do increase the demand for protection, but trade shocks elicit a much more protectionist response by a factor between two and three. Among trade shocks, our respondents exhibit greater sensitivity to trade with a developing nation than to trade with a developed nation. Simply changing the name of the country to which production is outsourced, from France to Cambodia, increases the demand for import protection by six percentage points (which is more than half the baseline level of demand for trade protection).

These results are in line with the last of the hypotheses above, namely that people view trade shocks as being inherently different from other kinds of shocks. More broadly, the evaluative frames that shape our respondents' views on the desirability of government action of some kind (and trade protection in particular) seem to depend not just on prospective outcomes—the job losses—but also on the causal channels. In other words, people seem to have preferences over distributive mechanisms as well.

We find that political ideology plays an important role in shaping protectionist preferences. Trump supporters are on average more protectionist than Clinton supporters and react much more strongly in favour of protectionism when primed with a trade shock. But the impact of the trade treatment on Clinton supporters is still sizable: Clinton supporters primed with trade shocks are as protectionist as baseline Trump voters. This finding attests to the power of even relatively simple vignettes to shape preferences over public policy. It is suggestive of the ease with which political campaigns can manipulate policy attitudes by supplying appropriate narrative frames.

Interestingly, highlighting labour abuses in the exporting country increases the demand for trade protection by 'liberals' (Clinton voters) while decreasing it among Trump voters. We expected priming for labour abuses abroad would increase the salience of concerns about unfair trade and render our respondents more willing to protect workers at home through import tariffs. Our results indicate this is true only for respondents on the liberal end of the political spectrum.

Trade protection is the result of both demand- and supply-side determinants (Rodrik, 1995). On the demand side, we have preferences of individuals and organised interest groups. On the supply side, there are politicians, parties, and the structure of political institutions. These interact to produce a political–economic equilibrium in which tariffs and other trade policies are determined (e.g., Grossman and Helpman, 1994). In this article, our focus is strictly on the demand side: individual preferences. There is an extensive empirical literature on these preferences.[5] Our article extends this literature in two directions. First, we are able to compare how people respond to labour market difficulties linked not only to imports and international outsourcing, but also to other types of shocks. Among trade shocks, we differentiate trade with developed and developing countries. Second, we are able to assess individuals' preferences with respect to alternative policy options: trade protection versus compensatory transfers. This allows us to document the elasticity of demand for protection in a particularly stark way.

[5] Previous research has looked at the role of education, skills, gender, sectoral trade exposure, social status and many other factors (Balistreri, 1997; Scheve and Slaughter, 2001; Beaulieu, 2002; Mayda and Rodrik, 2005; Beaulieu and Napier, 2008; Blonigen 2011; Lu *et al.*, 2012).

A second literature to which our article contributes is the recent work on the role of ideas and persuasion as determinants of individual preferences.[6] Political economy work by economists has traditionally assumed policy attitudes are driven by material interests. In trade policy, for example, this means that preferences on import protection are determined by an individual's education or sector of employment. To the extent that ideology, or worldview, enters the picture, it is implicitly assumed to be largely immutable. But there is plenty of evidence from psychology and political science that worldviews can be shaped by priming—exposure to news stories with a particular slant, specific cultural references, or simply advertising. Our results not only confirm this, but also indicate the quantitative magnitudes can be significant. Where preferences towards import protection are concerned, a simple news story about job losses due to outsourcing to a developing country can transform a 'Clinton voter' into a 'Trump voter'.

The outline of this article is as follows. In the next two sections we describe the data and the empirical strategy. In Section 3 we report our main results, while in Section 4 we provide additional results on interactions with respondents' ideology and educational level as well as recast our findings in terms of a measure of persuasion introduced by DellaVigna and Kaplan (2007) and DellaVigna and Gentzkow (2010). In Section 5 we offer some concluding comments.

1. Data Description and Representativeness

The data for this project come from a survey that we designed and administered to subjects through Amazon's Mechanical Turk (mTurk), an internet-based market for tasks. One advantage of this approach is that there is no need to physically connect to subjects and compensate them for their travel (this and other aspects of online labour markets are reviewed in Horton *et al.*, 2011). There is a small literature that uses these type of data to study questions in different fields of economics, including the effect of peers' wages on job satisfaction (Card *et al.*, 2012), the effect of inequality on preferences for redistribution (Kuziemko *et al.*, 2015), the relationship between reference points and preferences for redistribution (Charité *et al.*, 2015), the effect of trust in desired tax rate (Di Tella *et al.*, 2017), or views about social preferences (Weinzierl, 2014 and Saez and Stantcheva, 2016), among others.

We invited subjects on the mTurk platform by offering a small reward (1 dollar) for taking a brief survey (less than 10 minutes, approximately) to 'help us learn more about the relationship between the economy and government in America'. We explained that participation was anonymous[7] and restricted access to the survey to individuals who had done a good job in previous tasks. (We set visibility to 'Private' so that only workers with a Human Intelligence Task approval rate equal to or higher than 80% could preview our survey.) We also used a pop-up window that appeared whenever an individual tried to go to the next window before answering all the questions in the current window. The pop-up indicated the number of questions that were not answered and asked whether the respondent wanted to continue without answering all the questions.

The main survey was conducted in two waves, between late June and early July 2018. Table 1a presents a complete list of variable definitions. This first wave included 2,135 observations from New York, Massachusetts, Florida and California. The second wave included 4,220 observations

[6] See Frank (2007); DellaVigna and Kaplan (2007); DellaVigna and Gentzkow (2010); López and Leighton (2012); Blyth (2013); Rodrik (2014); Shiller (2017); Mukand and Rodrik (2018).

[7] We allowed individuals up to 50 minutes to complete the survey. Subjects were paid automatically after 8 hours of completing the survey.

Table 1a. *Variable Definition.*

Variables	Q	Description
Demographics		
Male	1	Dummy equal to 1 if individual is male (0 if female and missing value if neither male/female).
Age	2	Age in years.
White	3	Dummy equal to 1 if indicated 'White'.
Black	3	Dummy equal to 1 if indicated 'Black'.
Hispanic	3	Dummy equal to 1 if indicated 'Hispanic or Latino'.
Asian	3	Dummy equal to 1 if indicated 'Asian'.
Other race	3	Dummy equal to 1 if indicated 'Other'.
Postgraduate degree	5	Dummy equal to 1 if highest level of education is Master's degree/doctoral degree/professional degree (JD, MD, MBA).
Only college degree	5	Dummy equal to 1 if highest level of education is two-year college degree/four-year college degree.
No college degree	5	Dummy equal to 1 if highest level of education is eighth grade or less/some high school/'high school degree/GED' or some college.
Full-time employee	6	Dummy equal to 1 if full-time employee.
Part-time employee	6	Dummy equal to 1 if part-time employee.
Self-employed	6	Dummy equal to 1 if self-employed or small business owner.
Unemployed	6	Dummy equal to 1 if unemployed and looking for work.
Student	6	Dummy equal to 1 if student.
Not in labour force	6	Dummy equal to 1 if not in labour force (for example: retired or full-time parent).
Beliefs and political preferences		
Trust	7	Variable taking 0 if 'Need to be very careful' and 10 if 'Most people can be trusted' (scale: 0–10), when asked whether most people can be trusted.
Poor were unlucky	8	Variable taking 0 if 'They did not make an effort' and 10 if 'They were unlucky' (scale: 0–10), when asked the main reason that poor people are poor.
Rich work hard	9a	Variable that measures the participants' agreement with the statement 'The rich are rich because they worked hard' (scale: 0–100).
Rich were lucky	9b	Variable that measures the participants' agreement with the statement 'The rich are rich because they were lucky' (scale: 0–100).
Rich took advantage	9c	Variable that measures the participants' agreement with the statement 'The rich are rich because they took advantage of others' (scale: 0–100).
Competition	10a	Variable taking 0 if 'Competition is good. It stimulates people to work hard and develop new ideas' and 10 if 'Competition is harmful. It brings out the worst in people' (scale: 0–10), when asked about their point of view about the statements.
People/Gov more responsibilities	10b	Variable taking 0 if 'People should take more responsibility to provide for themselves' and 10 if 'The government should take more responsibility to ensure that everyone is provided for' (scale: 0–10), when asked about their point of view about the statements.
Support Clinton	11	Dummy equal to 1 if supported Clinton.
Centre (leaning Clinton)	11	Dummy equal to 1 if was Centre (leaning Clinton).
Centre (leaning Trump)	11	Dummy equal to 1 if was Centre (leaning Trump).
Support Trump	11	Dummy equal to 1 if was Supported Clinton.

Notes: [a]This column presents the question number in the main survey that was used to construct the variable.

from the rest of the country. The survey was taken by 6,355 individuals from the USA, but only 6,328 individuals finished it (for an attrition rate of less than 1%).

We apply two additional filters to our data. First, there were 37 individuals who admitted not having devoted full attention to the questions, and they thought that we should not use their responses for the study (attention check). Second, we collected data on the time spent by subjects on each of the windows that were presented during the survey. We noted that several subjects took far less time than the minimum amount of time required to read the questions. We only kept responses from individuals whose answers are among the 90% who took more time to finish the

survey.[8] The total number of observations after applying these two filters is 5,685. We refer to this sample as the restricted sample. The mean number of minutes spent answering the survey for the restricted sample is 6.7 minutes.

Table 1a lists the questions used to code the variables from the survey. The first group of questions is used to generate an initial set of demographic controls (gender, age, race, education level, labour status, etc.). This is followed by a small group of questions to capture subjects' values and beliefs.

Table 1b presents the summary statistics for our sample, the basic data for pre-treatment characteristics, and compares them with other samples. Of course, a key feature of our survey is that it is conducted hiring voluntary participants. Thus, those who choose to participate may be different from those that do not (even if they are identical in terms of observables). Still, it is instructive to compare these data with other samples. Columns 2 and 3 split the data according to ideological inclination and reveal reasonable patterns—in particular, that Trump supporters on average across all treatments are almost twice as protectionist as Clinton supporters but significantly less inclined to support transfers.

Table 1b also shows that our sample is, by and large, representative of the US population in terms of demographics (although our sample is younger and more educated, so we expect it to be somewhat less protectionist) and in terms of policy preferences. In column 4 we compare our sample with that of Di Tella *et al.* (2017) and in column 5 with that of Kuziemko *et al.* (2015), who also conduct their studies through Amazon's mTurk. The main conclusion comparing column 1 with 4 and 5 is that the populations are strikingly similar. Kuziemko *et al.* (2015) collected their sample between January 2011 and August 2012 and, at least with respect to observables, it is quite similar to the sample collected by Di Tella *et al.* (2017) in November 2015 and by us in mid-2018.

In terms of beliefs and policy preferences, the dimensions considered show consistent patterns with those in Di Tella *et al.* (2017) and the US sample of the World Value Survey (6th Wave, 2010–4), which has been widely used in politics and economics (column 6). For example, when individuals are asked whether they agree more with 'People should take more responsibility to provide for themselves' or 'The government should take more responsibility to ensure that everyone is provided for', we obtain a mean response of 4.3 while in the World Values Survey (WVS) it is 4.2 and in Di Tella *et al.* (2017) it is 3.9.

With respect to similarity in demographics, the two exceptions are age and education (our sample being younger and more educated). The mean age in our sample is 37.1 while in the WVS it is 46.5. The fraction of people with a postgraduate degree is slightly higher in our sample (17.7% in our study and 11.5% in the WVS) but there is a large difference in the fraction of people with no college degree (32.6% in our sample and 63.7% in the WVS). Finally, in column 7 we show results from the 2015 American Community Survey, which comprises a well-known large and representative sample of the US population. By comparing it with our sample, we again see that our sample is broadly similar, except for age and education.

The distribution of respondents across US states is similar to those in other surveys, such as the Census Bureau's 2015 American Community Survey. The five states with largest subsamples are the same in both surveys, with very similar percentages (although we seem to have slightly

[8] We took into account the fact that those who chose the option 'The Government should provide financial assistance' mechanically had to take longer because they had an extra question to answer.

Table 1b. *Summary Statistics.*

	All (our sample)	Clinton (our sample)	Trump (our sample)	Di Tella *et al.* (2017)	Kuziemko *et al.* (2015)	WVS 6th wave	ACS 2015
Demographics							
Male	46.4%	42.9%	52.1%	43.8%	42.8%	48.4%	48.6%
Age	37.1	36	39	34.9	35.4	46.5	47.1
White	73.1%	68.4%	81%	80.5%	77.8%	69.8%	72.3%
Black	8.8%	11.3%	4.6%	9.2%	7.6%	10.4%	12.7%
Hispanic	5%	5.7%	4%	6.6%	4.4%	13.4%	–
Asian	6.3%	7.6%	4.2%	6.8%	7.6%	–	5.59%
Other race	6.6%	6.9%	6.2%	2.6%	2.6%	–	–
Postgraduate degree	17.7%	18.8%	15.7%	13.3%	12.6%	11.5%	10.2%
Only college degree	49.8%	50.4%	48.8%	47.4%	40.7%	24.8%	25.7%
No college degree	32.6%	30.9%	35.4%	39.3%	46.7%	63.7%	64.1%
Full-time employee	56%	56.1%	55.8%	46.7%	33.2%	42.7%	43.9%
Part-time employee	11.7%	11.7%	11.7%	12.8%	13.3%	8.8%	16.7%
Self-employed	12.2%	11.8%	12.8%	12.4%	10.5%	5.1%	7.2%
Unemployed	5%	5.2%	4.8%	8.0%	12.4%	9.4%	3.9%
Student	5.5%	6.8%	3.4%	8.7%	15.8%	4.7%	3.8%
Not in labour force	9.6%	8.4%	11.5%	11.5%	14.8%	23.8%	31.7%
Beliefs and political preferences							
Trust	5.1	5.2	5.1	4.9	–	–	–
Poor were unlucky	5.6	6.3	4.5	–	–	–	–
Rich work hard	57.2%	51.1%	67.2%	–	–	–	–
Rich were lucky	59.7%	63.5%	53.5%	–	–	–	–
Rich took advantage	51.1%	54.2%	46%	–	–	–	–
Competiton	3.4	3.7	2.8	2.6	–	2.7	–
People/Gov more responsibilities	4.3	5.2	2.7	3.9	–	4.2	–
Support Clinton	37.5%	60.25%	0%	–	–	–	–
Centre (leaning Clinton)	24.8%	39.75%	0%	–	–	–	–
Centre (leaning Trump)	18.4%	0%	48.83%	–	–	–	–
Support Trump	19.3%	0%	51.17%	–	–	–	–
Democrat	62.3%	100%	0%	68.8%	67.5%	–	–
Outcome variables after treatment (for control group)							
Do nothing	21.3%	14%	33.7%	–	–	–	–
Transfers	68.4%	78.2%	51.6%	–	–	–	–
Protectionism	10.3%	7.8%	14.7%	–	–	–	–
Observations	5,685	3,545	2,140	5,974	3,746	2,138	2,490,616

Notes: Column 1–3: we consider the regression sample, which corresponds to (*i*) the sample of people who belong to the 90% that took more time to finish the survey, separating those who answered financial assistance in the post-treatment question from those who didn't; and (*ii*) people who answered affirmative the attention check. Column 5: we considered the respondents that took any of the omnibus treatment surveys of Kuziemko *et al.* (2015); participants could only choose one ethnicity in this study; variable *Democrat* is actually a variable that takes value 1 if individual answered Clinton or Centre (leaning Clinton) when asked 'In the last election, 'where did you stand politically?'; for the question on outcomes variables we considered the sample corresponding to the control group (sample size 822). Column 6: data source is the 6th wave of the World Value Survey US sample; individuals whose employment status was 'Other' were omitted; variables *Competition_Bad* and Gov more responsibilities were constructed with the same questions that were used in our study (the only difference is that in the WVS answers range from 1 to10 so we rescaled these answers to a 0–10 scale). Column 7: data source is the American Community Survey 2015; we considered individuals with 18 years old or older.

oversampled Florida). The five states with smallest subsamples are the same, with the exception of Vermont which is in our survey instead of South Dakota.

Table 1c presents the data summarised across treatments. The first column focuses on the control group and subsequent columns show summary statistics for each treatment group. All

Table 1c. *Randomisation.*

	Treatment group						
Variables	Control	T1	T2	T3	T4	T5	T6
Demographics							
Male	44.69%	45.81%	43.72%	45.35%	46.40%	52.04%***	46.81%
	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)
Age	36.38	37.85**	37.04	37.54*	37.00	36.72	37.27*
	(12.02)	(12.81)	(12.23)	(12.59)	(12.53)	(11.84)	(12.79)
White	72.87%	76.74%*	73.46%	73.55%	70.63%	72.56%	72.44%
	(0.44)	(0.42)	(0.44)	(0.44)	(0.46)	(0.45)	(0.45)
Black	9.12%	7.21%	7.86%	10.51%	8.92%	8.65%	9.39%
	(0.29)	(0.26)	(0.27)	(0.31)	(0.29)	(0.28)	(0.29)
Hispanic	5.11%	4.98%	5.53%	4.82%	5.08%	4.70%	5.12%
	(0.22)	(0.22)	(0.23)	(0.21)	(0.22)	(0.21)	(0.22)
Asian	5.96%	5.35%	6.14%	6.67%	6.44%	6.55%	7.20%
	(0.24)	(0.23)	(0.24)	(0.25)	(0.25)	(0.25)	(0.26)
Other race	6.93%	5.72%	7.00%	4.45%**	8.92%	7.54%	5.85%
	(0.25)	(0.23)	(0.26)	(0.21)	(0.29)	(0.26)	(0.23)
Postgraduate degree	19.83%	16.04%**	17.81%	16.93%	18.34%	17.31%	17.07%
	(0.40)	(0.37)	(0.38)	(0.38)	(0.39)	(0.38)	(0.38)
Only college degree	51.58%	49.50%	46.81%*	51.79%	48.70%	49.44%	50.73%
	0.50	0.50	0.50	0.50	0.50	0.50	0.50
No college degree	28.59%	34.45%**	35.38%***	31.27%	32.96%*	33.25%**	32.20%
	0.45	0.48	0.48	0.46	0.47	0.47	0.47
Full-time employee	56.45%	57.96%	54.79%	55.25%	54.89%	58.71%	54.02%
	(0.50)	(0.49)	(0.50)	(0.50)	(0.50)	(0.49)	(0.50)
Part-time employee	13.26%	9.95%*	12.41%	11.37%	13.01%	10.75%	10.85%
	(0.34)	(0.30)	(0.33)	(0.32)	(0.34)	(0.31)	(0.31)
Self-employed	13.02%	11.19%	11.92%	14.09%	11.15%	10.26%*	13.41%
	(0.34)	(0.32)	(0.32)	(0.35)	(0.31)	(0.30)	(0.34)
Unemployed	5.23%	3.98%	5.28%	4.45%	5.08%	5.32%	5.98%
	(0.22)	(0.20)	(0.22)	(0.21)	(0.22)	(0.22)	(0.24)
Student	4.50%	5.72%	5.90%	5.81%	5.08%	5.69%	6.10%*
	(0.21)	(0.23)	(0.24)	(0.23)	(0.22)	(0.23)	(0.24)
Not in labour force	7.54%	11.19%**	9.71%*	9.02%	10.78%*	9.27%	9.63%
	(0.26)	(0.32)	(0.30)	(0.29)	(0.31)	(0.29)	(0.30)
Beliefs and political preferences							
Trust	5.17	5.25	5.15	5.10	5.19	5.08	4.99
	(2.46)	(2.34)	(2.41)	(2.43)	(2.39)	(2.43)	(2.42)
Poor were unlucky	5.78	5.63*	5.56	5.61	5.75	5.50**	5.65
	(2.32)	(2.30)	(2.33)	(2.28)	(2.26)	(2.31)	(2.33)
Rich work hard	56.07	58.62**	57.70	56.34	57.06	58.22*	56.22
	(24.18)	(22.69)	(24.22)	(23.19)	(23.32)	(23.35)	(24.23)
Rich were lucky	61.30	60.02	59.30*	59.62	59.22*	59.74	58.93**
	(23.83)	(23.75)	(24.07)	(23.78)	(23.96)	(24.57)	(24.46)
Rich took advantage	52.32	50.14*	51.74	51.42	49.68**	51.17	51.22
	(25.38)	(25.44)	(26.17)	(26.11)	(25.98)	(25.80)	(26.83)
Competition	3.39	3.43	3.42	3.36	3.38	3.27	3.27
	(2.37)	(2.42)	(2.44)	(2.38)	(2.35)	(2.39)	(2.35)
People/Gov more responsibilities	4.40	4.24	4.21	4.45	4.28	4.10**	4.19
	(2.91)	(2.83)	(2.96)	(2.87)	(2.85)	(2.89)	(2.86)
Support Clinton	36.74%	35.70%	36.98%	40.42%	39.28%	36.34%	37.56%
	(0.48)	(0.48)	(0.48)	(0.49)	(0.49)	(0.48)	(0.48)
Support Trump	19.59%	20.15%	20.76%	17.80%	17.60%	20.40%	18.54%
	(0.40)	(0.40)	(0.41)	(0.38)	(0.38)	(0.40)	(0.39)
Observations (regression sample)	822	804	814	809	807	809	820

Table 1c. *Continued*

	Treatment group						
Variables	Control	T1	T2	T3	T4	T5	T6
Observations (unrestricted)	899	897	901	902	897	896	901

Notes: Mean value of the variable is presented in the first row; standard deviation is presented in parentheses. *, **, *** indicate statistical significance at the 10%, 5% and 1% levels, respectively, for the mean differences. Regression sample corresponds to (*i*) the sample of people who belong to the 90% who took more time to finish the survey, separating those who answered financial assistance in the post-treatment question from those who didn't; and (*ii*) people who answered affirmative the attention check. An omnibus test of joint orthogonality shows that the variables are unrelated to treatment status (p-value = 0.21).

summary statistics are computed using the restricted sample. Consistent with a successful randomisation, the data on observables appears to be balanced across treatments. Of course, when a large number of variables are being considered, we expect some of them to be different across groups purely by chance. An omnibus test of joint orthogonality shows that the variables are unrelated to treatment status (p-value = 0.21).[9]

2. Empirical Strategy

We study the effects of six treatments that result in an identical labour market effect: a plant closure leading to unemployment and lower wages. Each treatment takes the form of a news article with a different type of labour market shock introducing unemployment. In all cases, we describe the outcome in the following way at the outset: 'Nine hundred jobs are at risk at the YGF garment plant in Creekstown, which is facing closure.' This is followed by a second part that discusses the nature of the underlying shock. A YGF spokesman is quoted as describing a specific source, depending on the treatment:

1. Investment in automation and other new technologies (T1 = 'technology shock');
2. Reduced demand for the type of products made by the plant (T2 = 'demand shock');
3. Management failures in the plant (T3 = 'bad management');
4. Global competition and the outsourcing of production to an advanced country, France (T4 = 'advanced nation');
5. Global competition and the outsourcing of production to a developing country, Cambodia (T5 = 'developing nation');
6. Global competition and outsourcing of production to a developing country, Cambodia, with an emphasis on poor labour standards (T6 = 'developing nation PLS').

The news article then explains that these changes require the plant to be phased out and operations to be consolidated in other plants in the country. The vignette ends with a quote from an employee about the 'devastating consequences' for the workers: 'Many will become unemployed and the rest will have to accept lower-paying jobs.'

[9] We can also investigate whether there are particular imbalances in variables that might be correlated with the demand for protectionism. There aren't any obvious patterns, in particular because of the presence of multiple treatment arms. For example, there is an imbalance in the proportion of males in T5, so one can use the patterns in the other treatments (in particular in T6) to estimate its influence. Using the criteria outlined in Imbens and Rubin (2015), the size of these differences, normalised, is very small.

We note that T4 and T5 are identical except for a single word, which is the name of the country to which the plant's production is being outsourced (France in T4 versus Cambodia in T5). T6, by contrast, differs from T4 in that, in addition to naming a different country, it includes a description of poor labour standards—'labour abuses such as unsafe working conditions and use of child labour'—that 'can be common in developing countries'.

There is also a control group, which is not exposed to factory closure or job losses. Individuals in the control group were presented with a news article stating: 'The YGF garment plant in Creekstown, employing nine hundred workers, announced its plans for the year.' Then it mentions efforts made by the plant to improve on all dimensions included in treatments. In Online Appendix A we describe the scenarios in detail.

Following the treatment/control scenario, participants are then asked a question on what they think the response of the federal government should be. There are three options:[10]

1. 'do nothing';
2. 'provide some financial assistance to workers who lose their jobs (e.g., unemployment compensation or training assistance'; or
3. 'restrict imports of garments from overseas, by placing import tariffs on such imports for example'.

Accordingly, we code three separate binary indicator variables, which take the value of one if the participant says the government should 'do nothing' ('do nothing'), 'provide some financial assistance to workers who lose their jobs' ('transfers'), and 'restrict imports of garments from overseas' ('protectionism'), respectively, and zero otherwise.

Our preferred specifications are fitted with the logistic link function.[11] The reasons for using this link function are twofold. First, we are interested not only in marginal effects but also in predicted probabilities, and logits provide bounded predicted probabilities. Second, we can easily interpret our main results as odds-ratios to give an intuitive reading (in addition to displaying marginal effects from the control group). We include a set of demographic and political preferences covariates and perform interactions. We also group between treatments to address different hypotheses.

As in all studies of this kind, we need to be concerned about 'experimenter demand effects'—the possibility that respondents change their behaviour (in this case their stated views) since they know they are subjects in an experiment. A recent paper by de Quidt *et al.* (2018) has tried to bound these effects in a series of common tasks. It finds that potential biases 'are probably modest'. In our case, even if experimenter effects are present on average, it is unclear how they would affect what we are most interested in, namely the differences across treatments. Neither is it clear that respondents would know how to shade their answers, given the ambiguity about the expected experimental effects in this case. We are also comforted by the fact that our results on the effects of the demographic covariates on the demand for protection are consistent with previous work based on surveys (as discussed below). Finally, we note that our results are very

[10] Although we are interested in the demand for protectionism, we include three different options in an effort to avoid potential biases caused by experimenter demand effects. See Zizzo (2010), and Kagel and Roth (2016). Experimenter demand effects are 'changes in behaviour by experimental subjects due to cues about what constitutes appropriate behavior (behaviour 'demanded' from them)'. While it cannot be entirely ruled out, there exist several strategies from the design point of view to minimise it.

[11] Our results are unchanged when we use different estimation approaches, such as multinomial logits, linear probability models or probits.

similar when we restrict our sample to men.[12] Since de Quidt *et al.* (2018) find that women respond more strongly than men to 'experimenter demand effects', this is consistent with the absence of experimenter demands in our survey.

3. Main Results

Table 2 provides a first pass at the data by exploring the general effect of our labour market shocks by grouping all the treatments together. Thus, 'Labour shock' is a dummy equal to 1 if the subject was shown any of the six treatments, and equal to zero for the control group. We run the same specification for each our three binary indicators separately: 'Do nothing' (columns 1 and 2), 'Transfers' (columns 3 and 4), and 'Protectionism' (columns 5 and 6). Estimates in columns 1, 3 and 5 are predicted probabilities from logistic regressions. Estimates in column 2, 4 and 6 can be interpreted as marginal changes relative to the base category in percentage points or probability units. All regressions include covariates capturing demographic characteristics and political preferences. The estimated coefficients for the full set of covariates are shown in this initial table; we will skip their presentation in subsequent tables to focus exclusively on the treatments. Standard errors are clustered at the state level.

The estimated coefficients on the covariates in the Protectionism regression display patterns that are consistent with previous work: women, less-educated individuals and those who support/lean towards Trump tend to be more protectionist (column 6). There are no detectable differences in attitudes towards protectionism across different categories of race or employment status. The patterns displayed by the coefficients of the covariates are reasonable and encouraging insofar as they enhance the credibility of the responses that we get to the treatments we are really interested in.

Next, we look at the results for the control group across different types of government action (first line in Table 2). These give us the distribution of baseline preferences for government behaviour. We note that the baseline level of preference for 'Transfers' is quite high: the probability of thinking that the government should provide financial assistance is 0.70 (column 3). The baseline levels of 'Do nothing' and 'Protectionism' are comparatively low (0.19 and 0.09, respectively). Thus, our control group heavily favours government intervention in the form of unemployment or training assistance, but shows little support for import protection.

The estimated effect of the grouped 'Labour shock' treatment is shown on the second line in Table 2. In column 2 we show that participants primed with 'Labour shock' reduce by nine percentage points their preference for the government to do nothing as opposed to providing financial assistance or import protection. This amounts to a reduction of almost 50% in their preference for the government to do nothing after being primed with a shock that causes unemployment. The estimates in columns 3 to 6 indicate that the movement is almost exclusively towards protectionism. The demand for financial assistance to workers who lose their jobs barely changes (columns 3 and 4). Instead, participants react by increasing their demand for protectionism. The probability of thinking that the government should restrict imports from overseas (as opposed to doing nothing or providing financial assistance to workers who lose their jobs) is on average 0.09 in the control (column 5). In column 6 we show that participants primed with unemployment increase their preference for the government to restrict imports from overseas by 11 percentage points on average. One in five subjects thinks that the government should restrict imports from

[12] These results are not shown, but are available on request.

Table 2. *Unemployment and Government Intervention.*

	(1)	(2)	(3)	(4)	(5)	(6)
	Do nothing		Transfers		Protectionism	
	Mean	Marginal change	Mean	Marginal change	Mean	Marginal change
Control group (822)	0.19***		0.70***		0.09***	
	(0.011)		(0.013)		(0.01)	
Labour shock (4,863)	0.10***	0.09***	0.69***	−0.02	0.20***	0.11***
	(0.004)	(0.012)	(0.007)	(0.015)	(0.005)	(0.012)
Gender						
No female (2,630)	0.13***		0.69***		0.16***	
	(0.006)		(0.011)		(0.006)	
Female (3,055)	0.09***	−0.04***	0.69***	0	0.20***	0.05***
	(0.005)	(0.009)	(0.008)	(0.014)	(0.006)	(0.008)
Age						
Young: under 40 (1,785)	0.12***		0.69***		0.18***	
	(0.005)		(0.007)		(0.005)	
Old: over 40 (3,900)	0.10***	−0.02**	0.69***	0.01	0.19***	0.01
	(0.006)	(0.009)	(0.011)	(0.013)	(0.009)	(0.010)
Race						
White (4,160)	0.11***		0.68***		0.18***	
	(0.005)		(0.007)		(0.006)	
Black (501)	0.08***	−0.03**	0.72***	0.04*	0.17***	−0.01
	(0.011)	(0.013)	(0.022)	(0.021)	(0.017)	(0.017)
Hispanic or Latino (287)	0.11***	−0.01	0.66***	−0.03	0.21***	0.03*
	(0.021)	(0.024)	(0.034)	(0.038)	(0.016)	(0.019)
Asian (360)	0.11***	−0.01	0.68***	0	0.18***	0
	(0.015)	(0.016)	(0.023)	(0.026)	(0.018)	(0.017)
Other (377)	0.09***	−0.03*	0.72***	0.04*	0.17***	−0.01
	(0.014)	(0.014)	(0.026)	(0.023)	(0.016)	(0.016)
Education level						
Low education—No college	0.09***		0.69***		0.20***	
(1,852)	(0.005)		(0.01)		(0.009)	
Medium education—College	0.12***	0.03***	0.68***	−0.01	0.18***	−0.03**
(2,831)	(0.006)	(0.009)	(0.01)	(0.014)	(0.006)	(0.011)
High education—Post-college	0.12***	0.03**	0.71***	0.01	0.15***	−0.05***
(1,002)	(0.012)	(0.014)	(0.017)	(0.021)	(0.012)	(0.015)
Employment status						
Full-time (3,184)	0.11***		0.68***		0.18***	
	(0.005)		(0.009)		(0.006)	
Part-time (663)	0.09***	−0.02**	0.71***	0.03	0.18***	0
	(0.01)	(0.011)	(0.017)	(0.018)	(0.017)	(0.019)
Self-employed (691)	0.10***	−0.01	0.72***	0.04*	0.16***	−0.02
	(0.012)	(0.013)	(0.019)	(0.023)	(0.012)	(0.014)
Student (315)	0.11***	−0.01	0.67***	−0.01	0.20***	0.01
	(0.021)	(0.021)	(0.037)	(0.038)	(0.027)	(0.027)
Unemployed (287)	0.09***	−0.02	0.74***	0.06**	0.16***	−0.03
	(0.015)	(0.015)	(0.028)	(0.03)	(0.02)	(0.021)
Not in labour force (545)	0.12***	0.01	0.68***	0	0.18***	−0.01
	(0.01)	(0.011)	(0.017)	(0.016)	(0.016)	(0.015)
Supported past election						
Clinton (2,136)	0.06***		0.82***		0.11***	
	(0.006)		(0.007)		(0.006)	
Centre—leaning Clinton (1,409)	0.11***	0.05***	0.72***	−0.11***	0.16***	0.05***
	(0.008)	(0.01)	(0.012)	(0.015)	(0.008)	(0.011)

Table 2. *Continued*

	(1)	(2)	(3)	(4)	(5)	(6)
	Do nothing		Transfers		Protectionism	
	Mean	Marginal change	Mean	Marginal change	Mean	Marginal change
Centre—leaning Trump (1,045)	0.18***	0.12***	0.53***	−0.29***	0.27***	0.17***
	(0.012)	(0.015)	(0.016)	(0.016)	(0.014)	(0.015)
Trump (1,095)	0.19***	0.13***	0.45***	−0.38***	0.35***	0.24***
	(0.013)	(0.016)	(0.011)	(0.014)	(0.015)	(0.018)
Observations	5,685	5,685	5,685	5,685	5,685	5,685

Notes: Estimates come from a logistic regression. Standard errors clustered at state level are in parentheses. A constant term is included in every regression. In columns 1 and 2, dependent variable is a dummy variable that takes the value one if the participant thinks the government should do nothing ('Do nothing'), and zero otherwise ('Transfers' or 'Protectionism'). In columns 3 and 4, dependent variable is a dummy variable that takes the value one if the participant thinks the government should 'provide some financial assistance to workers who lose their jobs' ('Transfers'), and zero otherwise ('Do nothing' or 'Protectionism'). In columns 5 and 6, dependent variable is a dummy variable that takes the value one if the participant thinks the government should 'restrict imports from the contractor overseas' ('Protectionism') and zero otherwise ('Do nothing' or 'Transfers'). Labour shock is a dummy variable that takes the value one for shocks priming participants with unemployment: T1 = technology shock, T2 = demand shock, T3 = bad management shock, T4 = trade shock from advanced nation, T5 = trade shock from developing nation and T6 = trade shock from developing nation with poor labour standards, and zero otherwise. Estimates in columns 1, 3 and 5 are predicted probabilities. Estimates in columns 2, 4 and 6 should be interpreted as marginal changes relative to the base category in percentage points or probability units. All regressions include covariates regarding demographic and political preferences. Regression sample corresponds to (*i*) the sample of people who belong to the 90% who took more time to finish the survey, separating those who answered financial assistance in the post-treatment question from those who didn't; and (*ii*) people who answered in the affirmative the attention check. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

overseas after being exposed to unemployment shocks. In the control group, only one in ten participants says the same. Furthermore, this effect seems fairly homogenous across different categories of subjects. Political preferences are one notable characteristic showing heterogeneous treatment effects. We will explore this phenomenon later.

We next look at individual treatments and the differences among them. Figure 1 and Table 3, Panels A–C explore desired government interventions by type of unemployment shock for each outcome as defined above ('Do nothing', 'Transfers' and 'Protectionism'). The probability levels for the control group are the same as in Table 2. Figure 1 provides a visual description of the impact of individual treatments, while the three panels of Table 3 display the detailed results.

We can see from Table 3, Panel A that there are some differences across treatments for 'Do nothing', but they are not sizable. The preference for 'Do nothing' drops (statistically significantly) with each treatment, with the size of the drop varying from six percentage points ('bad management') to 13 points ('developing nation'). In Panel B, on the other hand, we find that there are some important differences for 'Transfers' across treatments. Interestingly, trade shocks (T4, T5 and T6) significantly reduce participants' preferences for transfers from control group levels. Non-trade shocks tend to have the opposite effect, but more weakly. The probability of thinking that the government should provide financial assistance (as opposed to doing nothing or providing import protection) increases from three to seven percentage points on average for non-trade shocks (column 4). But 'Bad management' (T3) is the only shock that produces a statistically significant increase in demand for transfers. 'Technology shock' (T2) and 'Demand shock' (T1) have the same sign pattern as 'Bad management', and 'Technology shock' is borderline significant in the specification with the full set of covariates (column 4).

In Panel C we present results for import protection. In short, all shocks except for 'Bad management' significantly increase participants' preferences for protectionism. 'Technology'

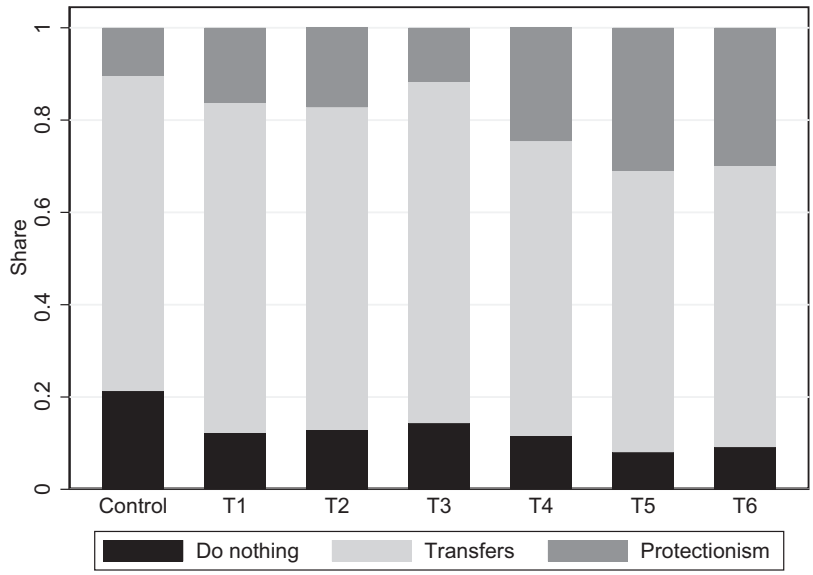

Fig. 1. *Distribution of Answers Over Treatment Status.*

Table 3. *Panel C. Unemployment and Government Intervention by Shock.*

	(1) Pr[Protectionism]	(2) Marginal change	(3) Pr[Protectionism]	(4) Marginal change
Control group	0.10***		0.09***	
	(0.011)		(0.010)	
T1: technology shock	0.16***	0.06***	0.14***	0.05***
	(0.012)	(0.014)	(0.011)	(0.012)
T2: demand shock	0.17***	0.07***	0.15***	0.06***
	(0.013)	(0.017)	(0.011)	(0.014)
T3: bad management shock	0.12***	0.01	0.10***	0.01
	(0.012)	(0.016)	(0.011)	(0.014)
T4: advanced nation	0.25***	0.14***	0.23***	0.14***
	(0.019)	(0.024)	(0.017)	(0.022)
T5: developing nation	0.31***	0.21***	0.29***	0.20***
	(0.014)	(0.017)	(0.017)	(0.020)
T6: developing nation	0.30***	0.20***	0.29***	0.20***
(poor labour standards)	(0.015)	(0.021)	(0.014)	(0.020)
Observations	5,685	5,685	5,685	5,685
Controls	No	No	Yes	Yes

Notes: Estimates come from a logistic regression. Standard errors clustered at state level are in parentheses. A constant term is included in every regression. Dependent variable is a dummy variable that takes the value one if the participant thinks the government should 'restrict imports from the contractor overseas' ('Protectionism') and zero otherwise ('Do nothing' or 'Transfers'). T1 to T6 stand for shocks priming participants with unemployment form different sources: T1 = technology shock, T2 = demand shock, T3 = bad management shock, T4 = trade shock from advanced nation, T5 = trade shock from developing nation and T6 = trade shock from developing nation with poor labour standards, and zero otherwise. Estimates in columns 1 and 3 are predicted probabilities. Estimates in columns 2 and 4 should be interpreted as marginal changes relative to the control group in percentage points or probability units. Bonferroni's method adjusts *p*-values for multiple comparisons. Controls refer to a set of demographic and political preferences covariates. Regression sample corresponds to (*i*) the sample of people who belong to the 90% who took more time to finish the survey, separating those who answered financial assistance in the post treatment question from those who didn't; and (*ii*) people who answered in the affirmative the attention check. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

Table 4. *People Demand More Protectionism When Trade Competition is with Developing Nations.*

	(1) Pr[Protectionism]	(2) Marginal change	(3) Pr[Protectionism]	(4) Marginal change
Control group	0.10***		0.09***	
	(0.011)		(0.010)	
Non-trade shock	0.15***	0.05***	0.13***	0.04***
	(0.008)	(0.012)	(0.007)	(0.011)
Trade shock (advanced nation)	0.25***	0.14***	0.23***	0.14***
	(0.019)	(0.024)	(0.017)	(0.023)
Trade shock (developing nation)	0.30***	0.20***	0.29***	0.20***
	(0.009)	(0.016)	(0.011)	(0.016)
Observations	5,685	5,685	5,685	5,685
Controls	No	No	Yes	Yes
p-value		0.003		0.005

Notes: All estimates come from a logistic regression. Standard errors clustered at state level are in parentheses. A constant term is included in every regression. Dependent variable is a dummy variable that takes the value one if the participant thinks the government should 'restrict imports from the contractor overseas' ('Protectionism') and zero otherwise ('Do nothing' or 'Transfers') after being primed with information. Treatment status has four categories and differentiate between control group, treatments associated with non-trade shocks (T1 = technology shock, T2 = demand shock and T3 = bad management shock), treatments associated with trade shocks from an advanced nation (T4 = trade shock from advanced nation), and treatments associated with trade shock from a developing nation (T5 = trade shock from developing nation and T6 = trade shock from developing nation with poor labour standards). Estimates in columns 1 and 3 are predicted probabilities. Estimates in columns 2 and 4 should be interpreted as marginal changes relative to the control group in percentage points or probability units. Controls refer to a set of demographic and political preferences covariates. In columns 2 and 4, *p*-value corresponds to a Wald test of equality of marginal effects between Trade shock (advanced nation) and Trade shock (developing nation). Regression sample corresponds to (*i*) the sample of people who belong to the 90% who took more time to finish the survey, separating those who answered financial assistance in the post-treatment question from those who didn't; and (*ii*) people who answered in the affirmative the attention check. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

and 'Demand' shocks have quantitatively similar effects. And, perhaps unsurprisingly, participants primed with trade shocks provide the strongest protectionist response. It is interesting to note that the demand for protectionism from participants primed with the 'Bad management' shock is not statistically different from the control group's preferences. One way to interpret this result is through theories that assign a large emotional cost to policies that benefit people who have been deemed 'unworthy' (see Rotemberg, 2003 and Di Tella and MacCulloch, 2009).

In scrutinising the results further, it is practical to group the results into three sub-groups: non-trade shocks, trade shocks with advanced nations, and trade shocks with developing nations. In Table 4 we show predicted probabilities for the demand for protectionism in columns 1 and 3, and marginal changes from the control group in columns 2 and 4. The predicted probability of thinking that government should restrict imports from overseas is on average 0.09 in the control group, 0.13 for participants primed with a non-trade shock, 0.23 for those exposed to a trade shock form an advanced nation, and 0.29 for those exposed to a trade shock form a developing nation. The marginal changes from the control group are statistically different from zero in all cases.

Hence, while adverse labour market shocks bolster the demand for protectionism in general, the quantitative magnitudes differ depending on the shock in question. Non-trade shocks have the smallest effect, though these are still statistically significant (except for the case of management mistakes). Trade shocks have stronger effects, and among trade shocks it is trade with developing

countries that elicits the greatest demand for protection when it is associated with job losses. We can reject at 1% level a Wald test of equality between coefficients for 'Trade shock (advanced nation)' and 'Trade shock (developing nation)' in columns 2 and 4.

Remember that we have two distinct treatments that involve trade with a developing nation: T5 and T6. These yield very similar results, although T6 includes a specific mention to poor labour standards in developing nations. The jump in the desired level of protection when we go from 'Developed' to 'Developing' (of six percentage points) occurs between treatments T5 and T4, with T6 adding no further boost to protectionist demand. Recall that the only difference between T5 and T4 is that we switch a single word, the name of the country (from France to Cambodia). It is remarkable that changing the identity of a country makes such a large difference to the preferences for protection. Job losses that arise from trade with a less-developed country seem to be regarded in a more negative light than those from trade with a rich nation.

It is perhaps also surprising that highlighting the issue of poor labour standards in developing nations (T6) seems to make little difference to the results. T6 differs from T5 in that it adds the following sentence: 'Labour abuses such as unsafe working conditions and use of child labour can be common in developing countries.' The fact that we did not get any leverage out of labour-standards priming may be due to the fact naming the country was enough to suggest the presence of poor labour standards, with the additional priming not adding much. In other words, respondents may have already assumed that poor labour standards are endemic to developing countries. In this case the difference in responses to the France–Cambodia cases could have been driven in part by such considerations. Or it could be that the subjects did not particularly care about the labour situation in the exporting country (and the differential response to France versus Cambodia is explained by differences between the two countries that are not related to the treatment of workers).

An alternative, intriguing possibility is that the response to poor labour standards abroad may depend on respondents' ideology and that the heterogeneity is masked by aggregation. There is some suggestive evidence in our data that Clinton and Trump supporters moved in opposite directions when we primed them specifically for labour standards. These results are shown in Table 5. When we divide respondents into groups that reflect political orientation, we find that that mentioning labour abuses increases the demand for trade protection by 'liberals' (Clinton voters) while decreasing it among Trump voters. For respondents who identify themselves as pro-Clinton or leaning towards Clinton, there is a 2–3 point increase in the demand for protection as we go from T5 to T6. For respondents who identify with Trump, there is a 4–6 point decrease. These differences are not statistically significant, though a two-way split in the sample between the two political camps comes close to borderline significance at the 10% level (p-value 0.114). These two effects in the opposite direction cancel out in the aggregate, which is why we see no difference between T5 and T6 in Table 4.

Our prior at the outset of this study was that priming for labour abuses in the exporting country would increase the salience of concerns about unfair trade (see discussion in Rodrik, 2019). Hence, we expected our respondents to become more willing to protect workers at home through import tariffs. The findings with respondents on the liberal end of the political spectrum are consistent with this expectation. The results for individuals at the other end of the spectrum are more puzzling: highlighting labour abuses abroad seem to make Trump supporters less sympathetic toward domestic workers.

Table 5. *Demand for Protection and Political Ideology, by Shock.*

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Clinton		Centre (leaning Clinton)		Centre (leaning Trump)		Trump	
	Prediction	Marginal change	Prediction	Marginal change	Prediction	Marginal change	Prediction	Marginal change
Control group	0.07***		0.09***		0.12***		0.17***	
	(0.016)		(0.016)		(0.030)		(0.023)	
T1: technology shock	0.08***	0.00	0.16***	0.07***	0.22***	0.09**	0.25***	0.08**
	(0.016)	(0.025)	(0.019)	(0.026)	(0.033)	(0.041)	(0.029)	(0.036)
T2: demand shock	0.09***	0.01	0.14***	0.05*	0.24***	0.11**	0.29***	0.13***
	(0.012)	(0.020)	(0.024)	(0.028)	(0.034)	(0.044)	(0.026)	(0.037)
T3: bad management shock	0.05***	−0.02	0.11***	0.02	0.17***	0.05	0.22***	0.05
	(0.013)	(0.020)	(0.021)	(0.024)	(0.023)	(0.042)	(0.028)	(0.040)
T4: advanced nation	0.14***	0.07**	0.16***	0.08***	0.39***	0.27***	0.45***	0.28***
	(0.026)	(0.031)	(0.024)	(0.026)	(0.040)	(0.050)	(0.047)	(0.059)
T5: developing nation	0.16***	0.09***	0.24***	0.15***	0.42***	0.30***	0.56***	0.39***
	(0.020)	(0.023)	(0.030)	(0.035)	(0.032)	(0.047)	(0.041)	(0.045)
T6: developing nation (poor labour standards)	0.19***	0.12***	0.26***	0.18***	0.38***	0.25***	0.50***	0.34***
	(0.020)	(0.025)	(0.030)	(0.036)	(0.042)	(0.053)	(0.050)	(0.056)
Observations	2,136	2,136	1,409	1,409	1,045	1,045	1,095	1,095
p-value partial: [T6−T5][(2)] = [T6−T5][(8)]	0.261							
p-value: [T6−T5][(2)+(4)] = [T6−T5][(6)+(8)]	0.094							

Notes: All estimates come from a logistic regression with the regression sample (5,685 observations). Standard errors clustered at state level are in parentheses. A constant term is included in every regression. Dependent variable is a dummy variable that takes the value one if the participant thinks the government should 'restrict imports from the contractor overseas' ('Protectionism') and zero otherwise ('Do nothing' or 'Transfers') after being primed with information. T1 to T6 stand for shocks priming participants with unemployment form different sources: T1 = technology shock, T2 = demand shock, T3 = bad management shock, T4 = trade shock from advanced nation, T5 = trade shock from developing nation and T6 = trade shock from developing nation with poor labour standards, and zero otherwise. Estimates in columns 1, 3, 5 and 7 are predicted probabilities. Estimates in columns 2, 4, 6 and 8 should be interpreted as marginal changes relative to the control group in percentage points or probability units. Controls refer to a set of demographic and political preferences covariates. In column 1, '*p*-value partial' corresponds to a Wald test of equality of marginal effects between a trade shock in a developing nation with and without poor labour standards (T6 and T5 respectively), for Clinton supporters versus Trump supporters. '*p*-value' corresponds to the same test but also using 'centre' supporters leaning towards either Clinton or Trump. Regression sample corresponds to (*i*) the sample of people who belong to the 90% who took more time to finish the survey, separating those who answered financial assistance in the post-treatment question from those who didn't; and (*ii*) people who answered in the affirmative the attention check. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$.

4. Ideology, Education and Persuasion

The treatment effects that we have discussed seem to operate fairly uniformly across many demographic and other categories (age, race, employment status, attitudes towards inequality, confidence in various institutions, etc.). In other words, there are not many strong interaction effects with the covariates. But political preferences constitute one important exception to this generalisation.

We show this in Figure 2, where we explore the relationship between the demand for protectionism and political preferences. Subjects are grouped according to whether they were pro-Clinton, Centre (but leaning towards Clinton), Centre (but leaning towards Trump), or pro-Trump. We graph predicted probabilities for import protection for, respectively, the control group, participants

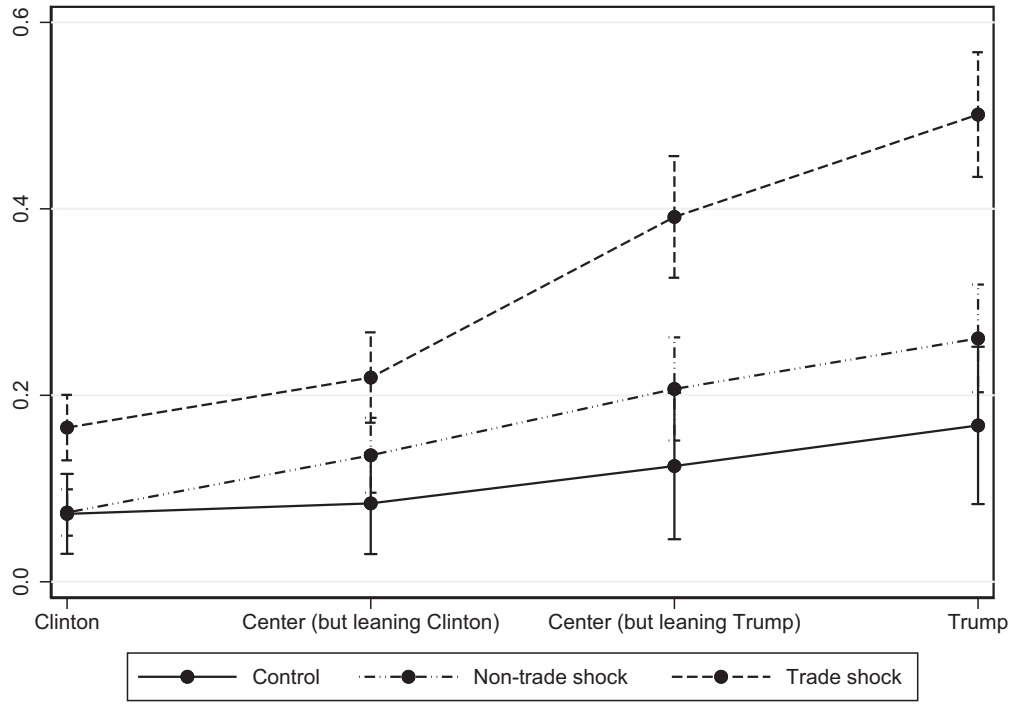

Fig. 2. *Predicted Probabilities for Protectionism Over Presidential Support.*
Notes: The figure is drawn with all covariates held at their mean value within categories.

exposed to non-trade shocks, and participants exposed to trade shocks. (Note that differential treatment effects for Trump or Clinton supporters are shown with other covariates—including education levels—held constant at their mean values.) We can see from Figure 2 that the base level of demand for import protection differs across Trump and Clinton supporters in the expected manner. Beyond this, we find two interesting facts to note from Figure 2. First, respondents who voted for Trump exhibit much more elastic demand for trade protection than those who voted for Clinton. Secondly, however, even Clinton voters' preferences appear to be quite malleable: we find that a trade-shock primed Clinton voter is as protectionist as a baseline Trump voter.

With regard to the first point, Figure 2 shows that Trump supporters are particularly sensitive to trade shocks. When primed with unemployment caused by a trade shock, Trump supporters increase their preference for protectionism by 33 percentage points on average, while Clinton supporters increase their preference for protectionism only by nine percentage points on average. The effect is three times as large among Trump voters as among Clinton voters. We strongly reject the null hypothesis of equality in the magnitude of the effect with a Wald test (p-value less than 1%). Thus, although trade shocks increase demand for protectionism across the board, the effect is amplified with Trump supporters.

Nevertheless, the impact of trade shocks on Clinton supporters is still quite large relative to the baseline levels of demand for trade protection. In fact, Clinton supporters exposed to the

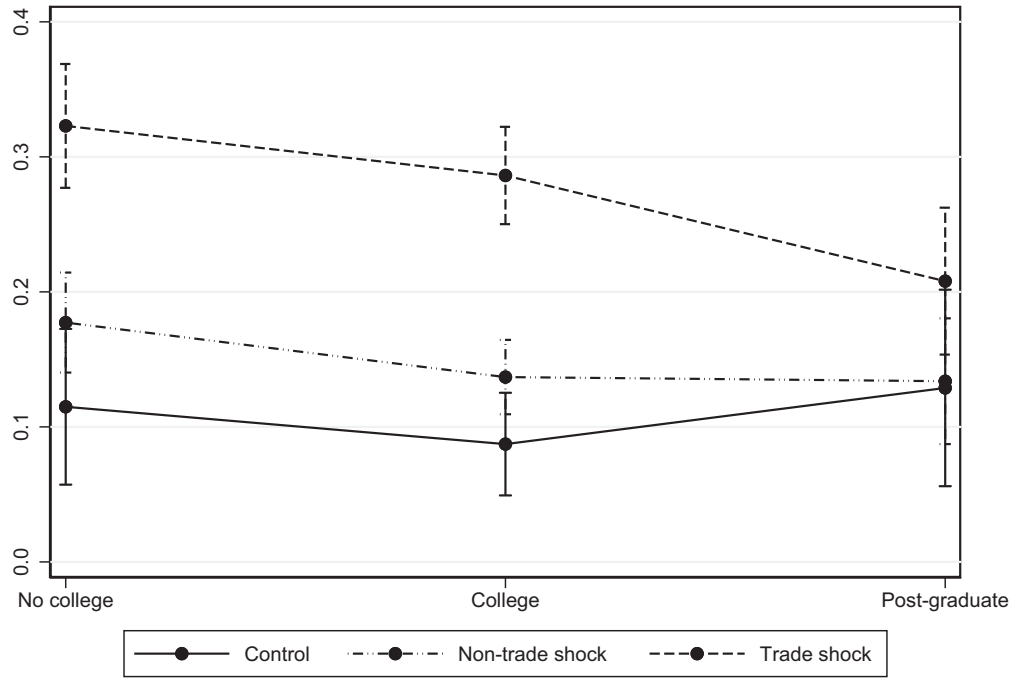

Fig. 3. *Predicted Probabilities for Protectionism Over Education Level.*
Notes: The figure is drawn with all covariates held at their mean value within categories.

trade shock demand virtually identical levels of protectionism as Trump supporters in the control group. We cannot reject the hypothesis of equality of coefficients with a Wald test (p-value is 0.942). This finding speaks to the power of narratives in shaping preferences over policy choices. Whatever the underlying proclivities based on material interests or ideologies, attitudes towards specific policies can be manipulated relatively easily by exposing individuals to simple vignettes.[13]

We also find some heterogeneity with respect to education. The protectionist response elasticity is broadly similar for less- and medium-educated groups, but it is half the magnitude for the most educated ones. This is shown in Figure 3, which is analogous to Figure 2 (but with educational attainment on the horizontal axis instead of political preferences). We see that protectionism levels are broadly the same in the control group on average. The trade shocks push up the demand for trade protection by roughly 20 percentage points for less- and medium-educated groups. A Wald test cannot reject the hypothesis that these shifts are identical (p-value is 0.781). For the most educated groups, the increase in the demand for trade protection is only eight percentage points, and the change is not statistically different from zero with 95 confidence intervals. We can strongly reject the hypotheses that the shift is the same for the most educated and for the less- or medium-educated (p-values are 0.0022 and 0.0015 respectively).

Education tends to correlate negatively with protectionist preferences due to one or both of two reasons. First, in a country like the United States, more educated individuals are more likely to be

[13] We note that these results on heterogeneity are robust to including interactions between treatments and the full set of covariates (available on request).

Table 6. *Persuasion Rates.*

	Do nothing	Transfers	Protectionism
T1: technology shock	−11%	10%	7%
T2: demand shock	−11%	5%	8%
T3: bad management shock	−9%	18%	1%
T4: advanced nation	−12%	−14%	16%
T5: developing nation	−17%	−23%	23%
T6: developing nation (poor labour standards)	−15%	−23%	22%
Non-trade shock	−10%	11%	5%
Trade shock	−15%	−20%	20%

Notes: See text for explanation.

beneficiaries of free trade. Second, more educated individuals are likely to be more familiar with the economic costs of trade barriers. Our finding that education does moderate the protectionist response to trade gives support to these explanations, although this is true only for post-graduate education (only 17.7% of our sample has the highest educational attainment). It suggests that the evaluative frames that people use in response to job losses are affected by educational attainment.

Finally, we summarise the impact of our treatments on respondents' policy preferences, using a measure of persuasion due to DellaVigna and Kaplan (2007) and DellaVigna and Gentzkow (2010). The 'persuasion rate' is defined as the percentage of recipients of a message that change their behaviour among those that received the message and were not already persuaded. In our case, the message is the news story about job losses, and persuasion refers to expressed demand for a policy response (either transfers or import protection). The baseline levels of persuasion for the two policies are reflected in the control group's preferences.

Given a binary behavioural outcome (persuaded or not), and denoting treatment and control groups as T and C, respectively, DellaVigna and Gentzkow (2010) express the persuasion rate f as follows:

$$f = 100 \times \frac{y_T - y_C}{e_T - e_C} \times \frac{1}{1 - y_0}, \tag{1}$$

where e_i is the share of group i receiving the message, y_i is the share of group i adopting the behaviour of interest, and y_0 is the share that would adopt if there were no message. In their words: 'The persuasion rate captures the effect of the persuasion treatment on the relevant behaviour $(y_T - y_C)$, adjusting for exposure to the message $(e_T - e_C)$ and for the size of the population left to be convinced $(1 - y_0)$.' Since we have a survey experiment with perfect compliance, we can set $e_T = 1$ and $e_C = 0$. Also, we can rely on the control group to estimate the size of the population left to be convinced, so $(1 - y_0) = (1 - y_C)$. With these simplifications, the expression for the persuasion rate becomes:

$$f = 100 \times \frac{y_T - y_C}{1 - y_C}. \tag{2}$$

We present the persuasion rates for transfers and protectionism in our various treatments in Table 6. Persuasion rates are quite high on average: 11% in favour of transfers in the case of non-trade shocks and 20% in favour of import protection in the case of trade shocks. A 20% persuasion rate is at the high end of the estimates reported by DellaVigna and Gentzkow (2010) from a wide range of studies on consumer and political marketing.

While not as impressive, the persuasion rate for transfers in the case of non-trade shocks (11%) is notable as well. Indeed, it may seem surprisingly large in light of the modest econometric effects that we reported previously. The apparent discrepancy is resolved by considering the baseline level of support for transfers, which the persuasion rate takes into account: the proportion of respondents not favouring transfers is small to begin with (see Figure 1), reducing the denominator of the equation above. Finally, the persuasion rate for transfers in the case of the trade treatment is negative, reflecting the decline in proportion of respondents who favour transfers in the trade scenarios, as we noted previously.

5. Concluding Remarks

Our results demonstrate the ease with which protectionism can be stoked by presenting the public with concrete stories about factory closures and job losses. Even though the base level of demand for import protection is low—a large majority of our respondents prefer financial transfers to import barriers—the elasticity of demand for trade barriers in response to such stories is quite high. People evidently desire a larger government role when they are exposed to accounts of adverse labour market shocks. In such circumstances trade policy presents a fertile terrain for political demagoguery.

Our study leaves some obvious questions for further research. Might the protectionist response be dampened by highlighting the efficiency costs of import barriers? Why do imports from developing countries elicit greater protectionism than imports from advanced countries? Why do liberals and conservatives respond differently to stories of labour abuse in the developing world? Does exposure to the type of stories that we used here have a longer-term impact on policy preferences as well? It would be especially useful to unpack further the procedural fairness frames through which people evaluate different labour market shocks and policy options. That would provide direct insight into why people respond so differently to labour market outcomes that are essentially identical.

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Additional Supporting Information may be found in the online version of this article:

Online Appendix A. Main survey questionnaire.

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