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Narrative based information: is it the facts or their packaging that matters?

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Narrative based information: is it the facts or their packaging that matters?

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Abstract

People typically do not acquire new information about the facts of the economy through consulting official statistics; they read or listen to media-type reports/stories on the economy where the facts are packaged in a story. This paper tests with an experiment whether the explanatory style used in such media-type stories affects individual decision making. We also compare this particular narrative influence with that of the actual facts contained in the story. Our subjects receive a media-type story about the economy before they play a minimum effort game. The media story has either good or bad background facts about the economy and we use the psychological theory of explanatory styles to present these facts in a narrative style designed to engender either optimism or pessimism. We find evidence that the explanatory style matters more than facts in the sense that optimistic styles support higher equilibria than pessimistic ones while the influence of the facts itself is weaker.

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1. Introduction

Publicly available information comes in many forms. There are the unalloyed facts that come from Official Statistics: tables with numbers of deaths, rates of inflation, growth of GDP and the like. Most people, though, rarely consult such sources. Instead, they acquire information on the facts of the economy through media reports or news stories. Unlike Official Statistics, these stories package the facts in some form of a narrative. In this paper, we examine with an experiment whether this is an important difference. In particular, we focus on one aspect of the narrative packaging, its explanatory style, and address the following question. Are individuals who receive information from media reports influenced exclusively by the facts in the story and/or does the explanatory style of the narrative packaging of those facts also influence behaviour?

This is an important question because economics has tended to overlook the possible influence that the narrative wrap-around (the facts) might have on individual behaviour. Most models in economics assume, for instance, that information updating occurs because the facts change. Thus, although few people consult the Official Statistics and most people encounter their facts through media reports that package them in a narrative, the packaging is assumed effectively to be irrelevant. One prominent recent exception to this tendency is Shiller (2017), who argues instead that narratives play an important role in the explanation of economic events. He acknowledges, however, that it is difficult to show that their role is causal: the stories used to interpret the world could be endogenous to the events themselves. This is why we use an experiment here. Experiments are especially well-suited to address this difficulty because, through appropriate design, they identify causal relations. This is our primary contribution: we test in an experiment whether explanatory style of the narrative wrap-around the facts in media-type reports affects behaviour.

One possible reason why the narrative wrap-around has not received much attention in economics is that 'narrative' is a broad and capacious concept and it does not readily yield testable predictions in the manner of Popper (1963). As a result, the empirical support for the claim that narratives influence behaviour in economics largely comes from illustrative case studies (e.g. see Akerlof and Shiller, 2009, Akerlof and Snower, 2016, and Shiller, 2017). It is in this context that we make our other contribution. We focus on a limited aspect of a narrative, its explanatory style,

because this allows us to draw on a psychological theory of explanatory styles to generate testable predictions. Buchanan and Seligman (1995) originally developed their theory of explanatory styles to understand why some people are optimistic and others pessimistic. They distinguish a style along three dimensions and suggest that individuals are different in that they rely on different styles when interpreting an event and this in turn explains why some react optimistically and others pessimistically to the same event. It is an intuitive theory and commands some support (e.g. see Zullow and Seligman, 1991, and Peterson and Vaidya, 2001). We adapt this theory by seeing whether the use of different explanatory styles in the reporting of the same facts in a media-type story influences individual behaviour in ways that would be predicted by this theory of how the styles produce optimism/pessimism. That is, whatever their individual predispositions to optimism/pessimism, we test whether an information prompt for a group of individuals that has an optimistic/pessimistic explanatory style for reporting the same facts produces behaviour in that group that can respectively been associated with optimism/pessimism.

To test these predictions, we therefore need a decision problem where there is scope for optimism and pessimism (i.e. sentiment) to affect individual decisions. We selected the minimum effort game. This game has multiple Nash equilibria that are Pareto rankable. This enables us to associate the selection of a better (worse) equilibrium in this game plausibly with players holding more optimistic (pessimistic) expectations about what other players in the game will do. Thus, in our experiment, the subjects see a media-type story and then they play the minimum effort game. We use four media-type stories. Each story contains facts on job creation, GDP growth and unemployment in the economy and the facts can be packaged in either an 'optimistic' or 'pessimistic' explanatory style in the media report. We have two sets of facts in our stories: the facts can be 'good' or 'bad' in the sense that job creation and growth in one are higher than the other, with a correspondingly lower/higher level of unemployment that is reported. Thus, with two possible explanatory styles and two sets of facts, there are four media-type stories and we can potentially distinguish between the influence of the facts ('good' or 'bad') and the influence of the explanatory style in the media-type story ('optimistic' or 'pessimistic') on behaviour in this game.

It is an important feature of this experiment that the facts in the media-type story do not directly concern individual behaviour and outcomes in the minimum

effort game. This is because we thereby replicate the way that much publicly available information in media-type stories is 'contextual' in this sense. That is, it does not directly concern or relate to the specific details of the decision individuals are making. It may nevertheless have some relevance for those decisions. For example, when a worker or employer in Cleveland, say, hears or reads that the national economy is growing and unemployment is falling, this has some relevance for their assessment of their local economic prospects because the state of the local economy contributes to the state of the national economy; it may also be relevant because such national information could contribute, consciously or unconsciously, to feelings of optimism or pessimism. In this way, our experiment captures two essential features about the information that most people actually receive (i.e. from media reports): it comes packaged in a narrative and it is contextual.[1]

The contextual character of the publicly available information in our experiment is also important in addressing Shiller's general argument. This is because his stories, the spread of which he believes can help explain macroeconomic turning points, are also weakly related to the actual economic decisions he wants to explain, if at all. Thus, in the explanation of the sharp downtown of 1920-21, he identifies, for example, the gruesome story of the murder of Czar Nicholas's family, the horrors of the flu pandemic, the possible exhaustion of oil supplies and stories about profiteering in contributing to the sense of economic uncertainty that might have discouraged discretionary household spending. These are not stories about specific individual economic prospects that directly relate to an individual's decision on whether to spend or not, they are stories about the background environment to those individual decisions. Our media stories have this same background or contextual relation to the actual individual decisions that we study.

While our key contribution is to generate and test predictions regarding the influence of explanatory styles on behaviour in a context that approximates how most people acquire new information, we also address several other debates/discussions in the wider literature. There is, for instance, a large experimental literature on the minimum effort game. This has typically focused on investigating mechanisms related to the game itself, like incentives, repetition or the size of the groups (see for example

1 While the narrative wrap-around the facts has tended to be overlooked in economics, the contextual character of the facts that people receive has not (e.g. see the signal extraction model of Lucas (1972), where firms face a similar problem as they seek to identify whether a change in the price reflects a relative or general price change).

Berninghaus and Ehrhart, 1998, Brandts and Cooper, 2006, Weber, 2006, Feri et al., 2010). Only a few papers deal with external influences, like the one we consider. To our knowledge, only recommendations, social identity and spillovers from previous games have been considered (Devetag, 2005, Chaudhuri and Paichayontvijit, 2010, Chen and Chen, 2011, Chen et al., 2014) and thus we are the first to examine contextual information priming in the form of a news report. Likewise, there is a literature on the political influences of the media. These papers typically use observational data to consider the partisan nature of news stories and find that exposure to partisan news affect political outcomes regardless of the specific media (on television, see DellaVigna and Kaplan, 2007, on newspapers, Gentzkow et al., 2011, on social media, Guriev et al., 2019). A recent strand in this literature has examined more closely whether (fake) narratives support misperceptions in political opinions or correct them (Alesina et al., 2018, Barrera et al., 2020, Eliaz and Spiegler, 2020). In a different but related vein, the influence of media reports on stock markets has been studied by applying a dictionary of emotion charged words to the analysis of media reports (e.g see Tetlock, 2007). Again, to our knowledge we are the first to examine with an experiment one possible complementary casual mechanism for the production of such media effects on behaviour: i.e. the selection of an explanatory styles in reporting events.[2]

We find that the explanatory style of the media-type story that our subjects read does affect their subsequent behaviour in the minimum effort game; and it is in the direction predicted by the theory of explanatory styles. An optimistic style of reporting produces a higher level of output in the minimum effort game than a pessimistic explanatory style. We find this both in our between-subject and our within-subject tests. Furthermore, this explanatory style effect on behaviour is much stronger than the influence of the facts. Whether the facts are 'good' or 'bad' in the

2 There is also a large literature that has examined how sentiment or animal spirits might explain the business cycle. For example, Blanchard et al. (2013) test models of aggregate fluctuations in which consumers, using some noisy sources of information, form expectations about future events that can affect short-run output. Using US data, on the one hand, they separate fluctuations due to changes in fundamentals (news) and on the other hand, due to temporary errorsin agent's expectations (noise). Results point towards noise explaining a sizeable fraction of short-run consumption fluctuations. Beadry and Portier (2014) and Miyamoto and Nguyen (2019) discuss how the arrival of news may cause fluctuations in the business cycles due to agents' expectational changes. There is also a large empirical literature has also examined how 'confidence' as measured in consumer and business surveys influences consumption and investment decisions (e.g. see Carroll *et al*, 1994, and Ludvigson, 2004). In this context, our experiment might also be thought to develop one possible explanation of how animal spirits or sentiment changes: i.e. through changes in the explanatory style of media reports.

media report has a statistically significant effect on behaviour less often and when it does it is quantitatively smaller than that of the difference in the explanatory style.

The rest of the paper is organized as follows. In the next section we develop the theory of explanatory styles and the hypotheses that we wish to test. Section 3 gives the experimental design and Section 4 has the results. In section 5, we discuss the results and conclude the paper with some observations on how our results relate to policy.

2. Theory of explanatory styles and hypotheses

2.1 Explanatory styles

The psychological theory of explanatory styles was originally developed to understand why some people were optimistic and others pessimistic. The key to the likelihood of being optimistic or pessimistic on this account turns on how events are attributed or explained by the person. A bad event will be attributed to an external source by an optimist, while a pessimist will locate the source internally. The attribution switches over for a good event because the pessimist will not locate an internal source for something good, whereas the optimist will. The internal-external binary relates to personal responsibility and is one dimension for possible attributions. There are two others in the theory. One refers to the permanence of the event (stable-unstable) and the other to its pervasiveness (global-specific). Optimists/pessimists will treat bad events as likely short/long lived and small/large in their effect; and the attributions switch over for a good event for the same reason as above (e.g. an optimist will think a good event is long lived). The mapping along each attribution dimension for optimists and pessimists by event type is set out in Figure 1.[3]

	Good event	Bad event
Optimist	*Internal*	*External*
	Global	*Specific*
	Stable	*Unstable*
Pessimist	*External*	*Internal*

3 We have used Buchanan and Seligman (1995) terms but there are clear connections to more familiar distinctions in economics: e.g. internal/external = endogenous/exogenous; global/specific = macro/micro; and stable/unstable = structural/random.

	Specific *Unstable*	*Global* *Stable*

Figure 1: Explanatory styles for optimism/pessimism depending on the event

We use these insights with respect to the influence of explanatory styles to construct news stories that engender optimism/pessimism among the subjects reading the news story: i.e. by varying how the facts are narrated using different explanatory styles. The idea is that, while individuals will differ in their natural optimism/pessimism, by priming all individuals with one or the other explanatory style in the reporting of an event, we will shift their optimism/pessimism predictably in one direction or the other. We illustrate this use of the explanatory styles theory in news reporting below with the bad event (the electronic Appendix A has the versions of the good event). In addition, in the construction of these news stories, we have checked, to ensure that we do not inadvertently use words with sentimental associations that pull against the explanatory style, with the Loughran and MacDonald (2011) sentiment word lists from the Master Dictionary. This is the dictionary that has been used in the analysis of sentiment in financial markets (e.g. Tetlock, 2007 and Garcia, 2013) and it classifies approximately 80.000 words into positive, negative and uncertain sentiments.[4]

PESSIMISTIC VARIANT

Job creation slows

Job creation slowed last quarter as output grew by only 0.2%. The increase in jobs was 140k, down from 220k in the previous quarter. The dwindling effect of the one-shot fiscal stimulation package that the government put together with some difficulty two years ago is one cause of the slower growth as is the fall in export growth. Predictions for output growth next quarter may be revised down.

Labour force participation, the proportion of the population in work or looking for work, remains stubbornly low compared with other countries even though unemployment fell slightly to 6%. Some blame the low participation rate on the relatively generous disability benefits in this country, others point to the increasing numbers retiring as baby boomers stop working.

OPTIMISTIC VARIANT

Job creation slows

Output grew by 0.2% last quarter and job creation slowed: the increase in jobs was 140k, down from 220k in the

4 For more details, see https://sraf.nd.edu

> previous quarter. The dwindling effect of the fiscal stimulation package is one cause of the slower growth as is a natural fluctuation in exports.
>
> Unemployment fell to 6%, but the labour force participation rate, the proportion of the population in work or looking for work, remains surprisingly low. Some blame the low participation rate on the level of disability benefits and, although public expenditure is under control, these benefits may be reviewed. Others point to a temporary demographic change and the increasing numbers retiring as baby boomers stop working.

The facts in terms of growth figures and the possible causes are the same in both versions but the explanatory style differs. The 'pessimistic' one emphasizes the 'internal' source by referring to the '*government*' as the origin of the fiscal problem, by comparing the situation to other economies (i.e. '*compared to other countries*'), and by highlighting the situation '*in this country*'. In comparison, the 'optimistic' version dispenses with these 'internal' references and instead identifies 'external' or natural causes: the '*natural fluctuations in exports'* and the '*demographic change*'. The 'pessimistic version also hints at the stability of the reported facts through the mention of the '*stubbornly*' low participation rate and how predictions for future growth '*may be revised down*'; and to the global/pervasive nature of the gloom because no upside possibility is mentioned. In contrast, the 'optimistic' variant contains reference to a possible counter-veiling upside by suggesting how public expenditure is *'under control*' and how disability benefits '*may be reviewed*' and with no similar reference to the negative effect on the future growth predictions, the facts have been spun to be 'specific' rather than part of a stable pattern of slower growth. Further, it highlights the unusual (i.e. unstable character of the reported facts): the '*temporary demographic change*' and the '*surprisingly low*' participation rate.

Although explanatory styles is an established theory, we decided to pilot the 4 versions of the news stories on MTurk to check whether they induced more or less optimism/pessimism. Subjects read one report, answered comprehension control questions and finally, on a Likart scale, reported how pessimistic or optimistic they felt about the state of the economy. After a sample of around 20 for each news story, we reviewed the results. The optimistic spin had a higher Likart reading for optimism than the pessimism spin for both good and bad facts news stories and this was already significant at 10 percent for both events in individual regressions (see electronic

Appendix B). In short, after a relatively small number of observations, the pilot had pointed to the efficacy of the news stories as a priming device and we moved on to the main experiment.

1.2 Hypotheses

The media stories are given to the subjects before they play the minimum effort game. The minimum effort game has several Pareto rankable Nash equilibria in pure strategies and, in this version, the higher the smallest number chosen the more efficient is the outcome. There is evidence that priming or framing of this sort (i.e. the introduction of what is strictly extraneous information) can affect equilibrium selection in other games with multiple Nash equilibria (e.g. see Mehta *et al*, 1994, and Chen *et al*, 2014). Thus, we have reason to expect that the extraneous information provided by the media report might influence the equilibrium selection in the minimum effort game. Our media reports vary along two dimensions: 'good/bad facts' and 'optimistic/pessimistic explanatory styles'; and our first hypothesis about such a media report influence picks up on the latter. It is a test of the specific explanatory style version of Shiller's more general claim that narratives affect behaviour.

> *H1. An 'optimistic' explanatory style in the report yields a higher smallest number in the minimum effort game than a 'pessimistic' style when applied to the same facts.*

Our second hypotheses addresses the difference in the contextual facts in the reports. Individuals will likely vary in the extent to which they extract information from such national news reports about the likely choice of effort levels by other players in the minimum effort game. They may simply project from the national to the local using some version of a signal extraction model or they may more or less consciously use the extraneous information of the report as a focal point in this game. Whichever is the case, we expect that 'good' news will lead to a higher expectation of the 'co-players' chosen effort level than when there is 'bad' news. Indeed there is some evidence from the minimum effort game itself that good news has such a beneficial effect in the sense that good news about how the game has been played in the past makes better outcomes more likely in the future (see Devetag, 2005).

H2. With the same explanatory style, a 'good' fact in the news leads to a higher smallest number in minimum effort game than a 'bad' fact.

2. Experimental design

2.1. Overview: 3 stages and 3 sources of evidence

We have a Baseline, where subjects play the minimum effort game with no media reports, and information treatments where the subjects read a media report before they play the minimum effort game. In all cases, they play the minimum effort game in groups of three. They do this 10 times (rounds) in each of 3 stages with stranger matching in each round and they know the result after each round.

In the Baseline, the minimum effort game played is the same in each of the 3 stages. In the information treatments, the news reports varies across the Stages. In Stage 1, they receive one of 4 media reports in what is a 2x2 design with variation in the fact and the explanatory style along the two dimension, as given by Figure 2.

	Optimistic style	Pessimistic style
Good facts	GO	GP
Bad facts	BO	BP

Figure 2: 2x2 Design

The horizontal and vertical comparisons of the information treatments in Stage 1 are the basis of one test for H1 and H2. Thus, we test whether facts make a difference by comparing vertically GO with BO and GP with BP (i.e. in each case we hold explanatory style constant). Likewise, for the influence of explanatory style, we compare horizontally GO with GP and BO with BP by holding in each case the facts constant. This is a comparative static, cross treatment, between subject test. Further in Stage 1, by comparing each information treatment with the Baseline, we can also determine whether, when there is a difference say between GO and GP, it arises because only one information treatment had an effect on behaviour, or both did but one was more powerful in its effect on the minimum effort chosen than the other.

In Stage 2 of the information treatments, the media report changes but only along one dimension: that is, either the fact changes or the explanatory style changes. This allows two further tests of the hypotheses. First, we can make the same cross treatment comparisons with the Stage 2 data as we did in Stage 1: i.e. GO(2) with

BO(2), GP(2) with BP(2), GO(2) with BO(2) and GP(2) with BP(2), where the number in parenthesis now refers to the stage. The difference as compared with the Stage 1 comparisons is that each [fact, explanatory style] pair now has a history. This is important because the contrast, that now becomes possible with the [fact, explanatory style] of stage 1, may make the stage 2 [fact, explanatory style] more perspicuous. Notice, however that [fact, explanatory style] pair like GO(2) could arise from two different histories: GP(1) or BO(1). This is because either the explanatory style or the facts could have changed from Stage 1. As a result, when comparing GO(2) with GP(2), say, to test for the influence of a different explanatory style, we use only the GO(2) that was preceded by GP(1) and the GP(2) that was preceded by GO(1). In this way, the facts remain the same and we can plausibly assume that if the history makes a difference, it will make the optimism/pessimism in GO(2)/GP(2) more salient because each current explanatory style in Stage 2 was preceded in Stage 1 by its opposite. For the same reasons, when addressing the cross treatment differences in facts like GO(2) cf. BO(2), we only use the GO(2) that was preceded by BO(1) so that only the facts change. Likewise, for BO(2), we use the BO(2) that was preceded by GO(1). Since we expect the change in fact/style in Stage 2 will make the actual fact/style in Stage 2 more perspicuous, we call this Stage 2 cross treatment comparison an enhanced comparative static test. Like the Stage 1 cross treatment comparison, it is a between-subject test.

Our final source of evidence is the historical or time series one that arises from the change between Stage 1 and Stage 2. It is within-subject evidence. For example, in the treatment with GO(1) where the fact changes to B in stage 2 (i.e. = BO(2)), we examine whether the smallest number in BO(2) is different to GO(1). That is, did the change in fact between stage 1 and stage 2 make a difference? Of course, there is a possible confound of mere accumulating experience in the contrast between Stage 2 and Stage 1 and we control for this. Similar time series tests for *H1* can be done by comparing GO(1) with GP(2) for the treatment that started with GO(1) and where O changed to P in stage 2; and so on.

In Stage 3 half the group is randomly assigned to read the media report from Stage 1 and the other half read the report they had just read in Stage 2 again. Stage 3 is not relevant for our two hypotheses and so we say not more about it here. [5]

5 We introduced it to see whether pluralism of 'facts' or 'explanatory styles' made a difference. It does not. This is what would be expected given the results that we report from stage 1 and stage 2, where

2.2 Details

In each of the 10 rounds of the minimum effort game, each participant *i* has to choose a number from 1 to 7. The payoff for each round is given by the formula below where x_i is the number chosen by the subject, and *min(x)* is the smallest number chosen by members of the group under stranger matching:

$$\pi_i = a + b * min\left(x_i, x_{-i}\right) - c * x_i$$

We chose $a = 60$, $b = 20$, $c = 10$ and a number of group members equal to three because we assumed that, given previous experimental results, this is unlikely naturally to produce coordination on a high payoff (Goeree and Holt, 2005). The following table reproduces the formula for all possible combinations of numbers and the smallest numbers in the group (the subjects saw both the formula and this table):

	Smallest Number in your Group						
Your Number	7	6	5	4	3	2	1
7	130	110	90	70	50	30	10
6	-	120	100	80	60	40	20
5	-	-	110	90	70	50	30
4	-	-	-	100	80	60	40
3	-	-	-	-	90	70	50
2	-	-	-	-	-	80	60
1	-	-	-	-	-	-	70

At the end of each round, subjects see the smallest number chosen in each round. They have no information on the other numbers chosen by players in their group. We chose stranger matching to avoid reputation and leadership effects that have previously been found in the minimum effort game (see Brandts and Cooper, 2006).

At the end of the three stages, the subjects complete a Revised LOT questionnaire (Scheier *et al*, 1994). This questionnaire is designed to capture their individual dispositional optimism or pessimism.[6] Then, they answer questions on demographics,

there is only one news report (i.e. these results suggest that plural reports would likely mean that pluralism sets up opposing changes, which therefore tend to cancel out).

6 We use this as a control (along with other demographic variables) in individual level regressions. It is interesting that while it is a significant determinant of behaviour in the 1st round of the Baseline, it is

on how much they know about economics and on how many news reports they usually read.

The experiment was held at the CBESS lab at the University of East Anglia. It was all computerized, using Z-Tree (Fischbacher, 2007). We carried out 18 sessions with a range of subjects from 12 to 18 for a total of 270 subjects (141 females, 129 males). The experimental sessions lasted around 45 minutes. Subjects were paid all parts of the experiment. The exchange rate between tokens and pounds was 100 tokens = £0.40. The average payment was £13.49, including a show up fee of £3.

3. Results

We begin with the cross section, between subject, evidence from Stage 1. Figure 4 plots the smallest number in each period of stage 1 in the Baseline and by Treatment. It is apparent that BO is notably different to the other Treatments and the Baseline. This is borne out by the statistical comparisons. Table 1 gives Wilcoxon pairwise comparisons of the smallest number in the 1st and all rounds of the various Treatments and the Baseline. We pick out the 1st round (as well as all rounds) because the 1st round captures the immediate effect of the different news stories; whereas, thereafter, there is the additional influence that comes from the experience within each session of playing the game.

The smallest number in BO is weakly significantly different from the Baseline (10 percent) in the 1st round and over all rounds it is significant at 1 percent level. BO also has a significantly higher smallest number than GO both in the 1st round and in all rounds (5 percent and 1 percent, respectively). Thus, the initial 1st round differences between BO and the Baseline and GO and BO seemed to have persisted. BO also has a weakly significantly (10 percent) higher smallest number over all rounds than BP.

not significant in the Treatments with news stories (see electronic Appendix C). This suggests that dispositional optimism is important but that the individual personality sources of such dispositions cease to be important when subjects share extraneous information that is designed to trigger optimism or pessimism. In this sense, our Treatment effects appear to be successful: they override the other individual based sources of optimism/pessimism.

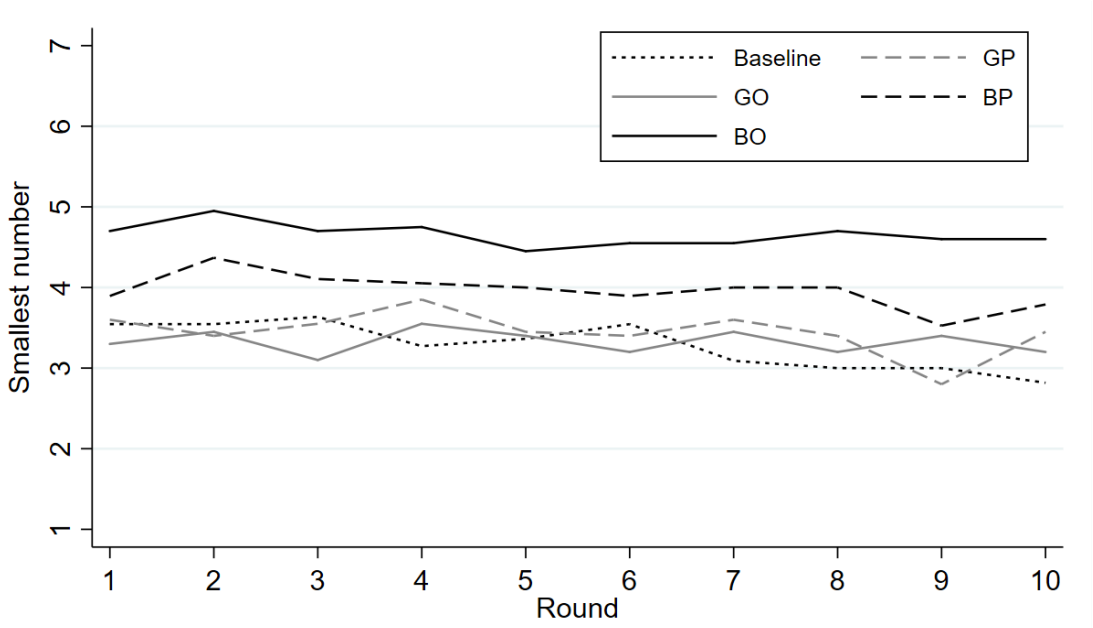

Figure 4: Average Smallest number for each period, 1st stage

		1st round	All rounds
Comparison of style	BO vs. BP	z = 1.57; p = 0.117	z = 1.74; p = 0.081*
	GO vs. GP	z = -0.72; p = 0.471	z = -0.98; p = 0.329
Comparison of facts	BO vs. GO	z = 2.45; p = 0.014**	z = 4.35; p = 0.000***
	BP vs. GP	z = 0.39; p = 0.698	z = 1.04; p = 0.298
Comparison of treatments to the baseline	BO vs. baseline	z = 1.86; p = 0.063*	z = 3.35; p = 0.001***
	BP vs. baseline	z = 0.66; p = 0.507	z = 1.16; p = 0.245
	GO vs. baseline	z = -0.19; p = 0.848	z = 0.08; p = 0.934
	GP vs. baseline	z = 0.38; p = 0.704	z = 0.48; p = 0.634

*Note: Observations on smallest number are at group level. *** p<0.01 ** p<0.05 * p<0.10*

Table 1: Wilcoxon ranksum tests on smallest numbers, 1st stage

Table 2 gives regressions on the smallest number in round 1 (column 1) and in all rounds by Treatment (column 3) where the omitted category is the Baseline. Columns 2 and 4 do the same on the Treatments alone, with GO as the omitted category. As subjects interact with each other within the same session, for regressions over all rounds we use two-level mixed-effects ordered probit models that allow for random effects at session level. In this parametric test, there is additional strong evidence in column 1 (1 percent) that BO has a higher smallest number in 1st round than the Baseline and a Wald test also confirms that this is higher than BP (p = 0.0278), which

is itself weakly (10 percent) higher than the Baseline. In column 2, BO has a significantly higher 1st round (1 percent) than GO, BP is also significantly higher than GO and BO is significantly higher than BP.

In columns 3 and 4, we see that BO remains weakly different from the Baseline (10 percent) and strongly significantly different from GO (1 percent) over all rounds, respectively. The differences between BO and BP are no longer significant over all rounds.

	(1) **1st round**	(2) **1st round**	(3) **All rounds**	(4) **All rounds**
GO	-0.126		0.0191	
	(0.159)		*(0.495)*	
GP	0.102	0.227	-0.115	-0.135
	(0.181)	*(0.197)*	*(0.616)*	*(0.439)*
BO	0.801***	0.941***	0.962*	0.939***
	(0.177)	*(0.227)*	*(0.524)*	*(0.314)*
BP	0.278*	0.408**	0.470	0.450
	(0.150)	*(0.176)*	*(0.633)*	*(0.494)*
November sessions	-0.0184	-0.0254	-0.00309	-0.00482
	(0.417)	*(0.425)*	*(1.128)*	*(1.130)*
Sessions with 12 subjects	-0.0950	-0.0914	0.126	0.124
	(0.289)	*(0.294)*	*(0.331)*	*(0.332)*
Observations	90	79	900	790

*Note: Regressions (1) and (2) are ordered probit. Regressions (3) and (4) are mixed-models ordered probit. These are with random intercepts at the session level. Period dummies are included (but not reported). Errors clustered at session level in parentheses. *** p<0.01 ** p<0.05 * p<0.10*

Table 2: Regressions on the Smallest Number, 1st Stage

We draw two results, 1(1) and 2(1) below, that address respectively *H1* and *H2* on the basis of the cross treatment comparisons from Stage 1, they are indexed by (1) to indicate that they come from the first source of evidence: the Stage 1 cross treatment comparisons.

Result 1(1) (partial support for *H1*): There is evidence from cross treatment comparisons in Stage 1 that BO has a higher smallest number (is more efficient) than BP (BO>BP) in the 1st round; and that the BO and BP 1st round differences are significantly different from the Baseline (5 percent and 10 percent, respectively).

Result 2(1) (against *H2*): There is evidence from cross treatment comparisons in Stage 1 that BO has a higher smallest number in the 1st round and all rounds (is

more efficient) than GO (BO>GO). Only BO is significantly different than the Baseline.

We turn now to Stage 2 between subject evidence. We dispense with the visual representation because the figure would become rather congested. Tables 3 and 4 give the results of comparing the coefficients from analogous regressions on Stage 2 for the 1st round and all rounds as those in column 1 and 3 of Table 2 (see Appendix D for the regression results). For the reasons sketched earlier, we distinguish between whether a Stage 2 [fact, explanatory style] pair arose (a) from a change in explanatory style when we test for a possible cross treatment difference in explanatory styles in Table 3 and (b) from a change in the facts when we test for a possible cross treatment difference in the facts in Table 4.

	1st round	**All rounds**
BO vs. BP	z = -0.28; p = 0.777	z = 2.02; p = 0.044**
GO vs. GP	z = 3.36; p = 0.001***	z = 2.48; p = 0.013**
BO vs. baseline	z = 0.97; p = 0.331	z = 1.02; p = 0.307
BP vs. baseline	z = 1.09; p = 0.276	z = 0.03; p = 0.973
GO vs. baseline	z = 0.84; p = 0.402	z = 0.69; p = 0.493
GP vs. baseline	z = -0.54; p = 0.587	z = -0.19; p = 0.851

*Note: These are post-estimation Wald tests between treatments. Regressions are mixed-models ordered probit. These are with random intercepts at the session level. Period dummies are included (but not reported). Errors are clustered at session level. *** p<0.01 ** p<0.05 * p<0.10*

Table 3: Post-estimation pairwise comparisons of smallest numbers – change in spin

	1st round	**All rounds**
BO vs. GO	z = -2.32; p = 0.021**	z = -2.11; p = 0.035**
BP vs. GP	z = -1.97; p = 0.049**	z = -1.33; p = 0.185
BO vs. baseline	z = 0.34; p = 0.734	z = 0.09; p = 0.928
BP vs. baseline	z = -0.45; p = 0.650	z = -0.61; p = 0.545
GO vs. baseline	z = 2.45; p = 0.014**	z = 2.00; p = 0.045**
GP vs. baseline	z = 0.74; p = 0.456	z = 0.45; p = 0.656

Note: see note in table 3.

Table 4: Post-estimation pairwise comparisons of smallest numbers – change in fact

Consider first the influence of optimism/pessimism revealed in Table 3. GO(2) has a significantly higher smallest number than GP(2) in both the 1st round and all rounds (1 percent and 5 percent, respectively). BO(2) also has a higher smallest number (5 percent) than BP(2) in all rounds. Result 1(2) follows in relation to *H1* using these Stage 2 enhanced cross section, cross treatment comparisons: i.e. our second source of evidence (2).

Result 1(2) (in support of *H1*). There is evidence from cross treatment comparisons in Stage 2 that the smallest number in the 1st round and all rounds are higher in GO than GP (GO>GP) and higher in all rounds in BO than BP (BO>BP).

Turning to the influence of facts in Table 4, GO(2) has a higher smallest number in the 1st round and in all rounds than BO(2). Further GP(2) has a significantly (5 percent) higher smallest number in the 1st round than BP(2). GO(2) is the only Treatment that is significantly different from the Baseline. Result 2(2) follows in relation to *H2* from these enhanced cross section, cross treatment comparisons in Stage 2.

Result 2(2) (in support of *H2*). There is evidence from cross treatment comparisons in Stage 2 that the smallest number is higher in GO than BO (GO>BO) in 1st round and all rounds and that they are higher in GP than BP (GP>BP) in the 1st round.

Table 5 and 6 present our third, time series, within subject, source of evidence, indexed by (T). In Table 5, we run a regression on the change in individual number choices between Stage 1 and Stage 2 with dummies for each of the possible transitions. The omitted change is the baseline. The only significant coefficient (5 percent) is on the transition BP(1)→BO(2): it is positive consistent with *H1*.

In Table 6, we run a similar regression on the change in smallest number from Stage 1 to Stage 2 for each individual transition in isolation.

We note that there is no significant change in the Baseline between Stage 1 and Stage 2. There are, however, significant changes in Stage 2 as compared with Stage 1, for all changes/transitions in explanatory styles (columns 2-5). Three are in the direction predicted by *H1*: BP(2)<BO(1), GO(2)>GP(1) and BO(2)>BP(1). One is in the opposite direction to *H1*: GP(2)>GO(1). In contrast, only one of the four fact

transitions/changes is significant (column 8) and the direction of the change is consistent with *H2*: BP(2)<GP(1). Results 1(T) and 2(T) follow.

Result 1(T) (mainly in support of *H1*). There is evidence in the time series comparison of Stage 2 with Stage 1, consistent with *H1*: with individual level choices, BO(2)>BP(1) and with the smallest number, BP(2)<BO(1), GO(2)>GP(1) and BO(2)>BP(1). There is only one transition in the smallest number that is not consistent with *H1*, GP(2)>GO(1).

Result 2(T) (partial support for *H2*). There is evidence in the time series comparison of the smallest number in Stage 2 with Stage 1, consistent with *H2*, that BP(2)<GP(1). There is no evidence in support of *H2* in the individual level regressions.

	Change in number
GO(1)→GP(2)	0.189
	(0.158)
BO(1) →BP(2)	-0.218
	(0.165)
GP(1) →GO(2)	0.0325
	(0.162)
BP(1) →BO(2)	0.315**
	(0.153)
GO(1) →BO(2)	-0.112
	(0.209)
BO(1) →GO(2)	0.339
	(0.224)
GP(1) →BP(2)	-0.00435
	(0.157)
BP(1) →GP(2)	0.0549
	(0.158)
November sessions	0.0503
	(0.115)
Sessions with 12 subjects	-0.0697
	(0.105)
Age	0.00646
	(0.0104)
Female	0.0171
	(0.0682)
LOT	0.00740
	(0.0109)
Newsreports	-0.0139
	(0.0237)
Economics	-0.00773
	(0.0203)
Observations	2700

*Note: Regression is mixed-models ordered probit. This is with random intercepts at both the session and subject level. Period dummies are included (but not reported). Errors clustered at session level in parentheses. *** p<0.01 ** p<0.05 * p<0.10*

Table 5: Regression results on the change in individual number choices between stage 1 and stage 2

	(1)	(2)	(3)	(4)	(5)
	Baseline	GO(1)→GP(2)	BO(1) →BP(2)	GP(1) →GO(2)	BP(1) →BO(2)
Stage 2	0.0397	0.293***	-0.570***	0.0874***	0.558***
	(0.373)	*(0.0693)*	*(0.0310)*	*(0.0283)*	*(0.0425)*
Observations	220	220	200	200	200

	(6)	(7)	(8)	(9)
	GO(1) →BO(2)	BO(1) →GO(2)	GP(1) →BP(2)	BP(1) →GP(2)
Stage 2	-0.328	0.183	-0.0571***	-0.274
	(0.239)	*(0.435)*	*(0.00563)*	*(0.410)*
Observations	180	200	200	180

*Note: Regressions are mixed-models ordered probit. These are with random intercepts at the session level. Errors are clustered at session level. *** p<0.01 ** p<0.05 * p<0.10*

Table 6: Regression results on change in smallest number from stage 1 to stage 2 for each individual transition

4. Discussion and Conclusion

Our results are similar in the baseline to previous experiments on a minimum effort game with stranger matching (see Goeree and Holt, 2005, Chaudhuri and Paichayontvijit, 2010, Chen and Chen, 2011). For example, our average smallest number is the middle option (3.54) in the 1st round, there is no convergence to either 1 or 7 and coordination slowly improves over the rounds in our Baseline. This is what is typically found. This is reassuring and we turn now to the purpose of this experiment: the test of whether either the explanatory style of the narrative linking the facts and/or the facts themselves that are in a contextual media report affects behaviour in a minimum effort game.

We have clear evidence that the explanatory style of the narrative influences decision making (H1).[7] All three sources of evidence have BO>BP (Result 1(1), 1(2) and 1(T)). We also have evidence from Stage 2 enhanced cross section, cross treatment comparisons and the time series comparison that GO>GP (Result 1(2) and 1(T)). The only contrary piece of evidence is from the time series evidence when GO transitions to GP, where GP>GO (cf. the reverse transition from GP to GO has GO>GP, as predicted by H1). In short, the choice of optimistic/pessimistic

7 The same evidence can be found on numbers. We reproduce this evidence in Appendix E, where we repeat the same analysis from the Results section on individual choices on numbers

explanatory style always matter in the manner predicted by H1 for Bad facts and also mostly for Good ones.

The evidence on H2 and the influence of the character of the facts is mixed by comparison. When there is optimism, BO>GO on the basis of Stage 1 cross treatment comparison (Result 2(1)) but we have the contrary GO>BO from the Stage 2 enhanced cross treatment comparisons (Result 2(2)). Further, there is no time series evidence that a change in facts has an effect when there is optimism (Result 2(T)). In short, there is no systematic pattern to the effect that a change in character of the facts has under optimism.

The evidence on the influence of the character of the facts under pessimism is stronger. GP>BP in both the Stage 2 enhanced cross section data (Result 2(2)) and in the time series data (Result 2(T)). But this evidence is relatively weak and there is no corroboration in the cross section Stage 1 data (Result 2(1)). It is relatively weak in Stage 2 because the evidence of a difference only appears in the 1st round; and in the time series it is relatively weak because only the transition of GP(1) to BP(2) appears to be significant. The change of facts in the reverse direction under pessimism of BP(1) to GP(2) is, however, not significant.[8]

On balance, therefore, the evidence on the influence of explanatory styles is qualitatively stronger than that of the facts. To give this qualitative assessment of the evidence a quantitative dimension, the smallest number increases by 17 percent as BP(1) changes to BO(2) and when the reverse happens and BO(1) changes to BP(2), the smallest number falls by 13 percent. In comparison, the change in the facts under pessimism, where there is the qualitatively strongest evidence for the influence of facts, there is no significant change in the smallest number when BP(1) changes to GP(2), and in the reverse case where GP(1) changes to BP(2), the smallest number only decreases by 2 percent.

This is an important conclusion. Economic models tend to assume people update beliefs as if they consulted the relevant tables from the latest issue of Official Statistics: i.e. they just get the new facts and use Bayesian or some other form of updating for beliefs. However, in practice most people in the economy do not receive

8 This asymmetry in effect between the direction of the change in the facts is consistent with other evidence (see Legg and Sweeny, 2014, where the ordering of good and bad facts operates in this way).

new information in this form. Instead, they frequently get contextual/background information (rather than information that is directly relevant to their decisions) and it comes packaged in a news story. In this context, our conclusion is important because this difference in how people actually acquire information proves not to be innocuous. The explanatory style used in the narration of the facts in a media report is a significant determinant of individual behaviour in a setting where optimism/pessimism can play a role; and furthermore, the explanatory style appears more influential than the character of the contextual/background facts in the story themselves.

Of course, it is an important qualification to this conclusion that the evidence from the experiment applies to a decision setting where there are multiple Pareto rankable Nash equilibria and so any feelings of optimism/pessimism engendered by the media reports might influence equilibrium selection. This is not evidence that explanatory styles always influence individual behaviour. Nevertheless, the experiment does provide specific support for Shiller's more general and recent claim that narratives can influence economic outcomes. His claim, too, is not that narratives always influence individual decision making. His illustrative arguments for the part played by narratives typically come from macroeconomic turning points, where, like Keynes when he invokes animal spirits, Shiller believes there is scope for narratives to impart optimism/pessimism and so influence decision making. Of course, our decision problem involves 3 people playing a minimum effort game and this can scarcely claim to be a macroeconomic interaction of the kind Shiller has in mind. So, in this respect our evidence is not directly relevant to his claims about narratives and the behaviour of the macroeconomy. However, we do identify a particular aspect of a narrative, its explanatory style, and by developing the psychological theory of explanatory styles and focusing on 3 person decision problem we are able to test specific hypotheses regarding the influence of different explanatory styles in the lab. This both addresses the issue of causal identification that troubles Shiller's illustrations of the influence of narratives and, since the minimum effort game is often thought to capture Keynesian-like macro interactions (see Cooper and John, 1988), it can also be regarded as a building-bloc in understanding macro interactions.

Thus, our results lend support to Shiller's argument that the influence of narratives in determining economic outcomes deserves more attention in economics. Our results may also have some relevance for policy makers.

Policy makers have often engaged in 'jaw-jaw' activities designed to influence future expectations---usually to engender greater optimism. Roosevelt famously did when asserting during the Depression that 'the only thing we have to fear…. is fear itself'. More recently central bankers have also become particularly attentive to how they phrase their assessments for fear that the odd word may have unintended consequences (e.g. see Haldane and McMahon, 2018). The salutary lesson in this regard is often taken to be the Federal Open Markets Committee (FOMC) on January 28 2004. There was no surprise change in the key 'facts': e.g, the Federal Funds rate did not change. Nevertheless, there was one of the biggest reactions to this news in 2-5 year Treasury bill markets since records of such announcement effects began. The interpretation of this extreme reaction in the Wall Street Journal was that it arose from the FOMC's change in the form of wording in this policy announcement concerning the continuation of policy accommodation into the future: the previous statement said that 'policy accommodation can be maintained for a considerable time' and the new statement said that 'the Committee believes it can be patient in removing its policy accommodation' (see Gürkaynak *et al,* 2005).

Of course, Wall Street Journal may not be right in this attribution---it does seem a bit of a stretch for so much to turn on such a small change in wording. The point, however, is that we do not really know because we do not know much, if anything systematic, about 'how the way we say things' as opposed to 'what we say' may predictably engender more or less optimism/pessimism. In this context, our result with respect to the influence of explanatory styles is important for policy makers. This is not because it is likely to offer secure ways of manipulating optimism/pessimism. The evidence of our experiment is too preliminary and who knows whether the theory could survive such a use of its own insights by policy makers. That is not the point. The importance of the result is that it suggests that the theory of explanatory styles could be useful starting point for a systematic enquiry into how 'how we say things' has effects and this looks rather more promising, for instance, than trying to discern a reproducible source of difference between 'being patient in removing policy

accommodation' and 'maintaining the policy accommodation for a considerable time'.

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APPENDIX A

In this Appendix we provide the experimental instructions.

Instructions – Part 1

Welcome to this experiment. This is an experiment about decision-making. You will receive £3 for your participation. You can earn more money depending both on your decisions and the decisions of others.

During the experiment you are not allowed to communicate with any other participants or with anyone outside the laboratory. Please switch off your mobile phone now. If you have any questions at any time during the course of this experiment, please raise your hand. An experimenter will assist you privately.

Your decisions will be recorded privately at your computer terminal. Your identity will not be disclosed to other participants.

During the experiment all decisions are made in tokens (more details below). Your total earnings will also be calculated in tokens and, at the end of the experiment will be converted to Pounds at the following rate:

100 tokens = £0.40

You will be paid individually and privately in cash at the end of the experiment.

The experiment consists of three parts. Your total earnings will be the sum of your earnings from Part 1, Part 2 and Part 3 plus a participation fee of £3.

You will receive instructions for Part 2 after Part 1 is completed.

At the beginning of Part 1, you will receive a brief news report. Every participant will look at the same news report. There are some questions about the report to check you have understood it. You answer each question by clicking on the button with the correct answer. The answers remain private. If you need to, you can always go back to the news by clicking on the button "Back to the news". Once everyone has answered all questions correctly, the experiment will continue.

Part 1 of the experiment consists of ten consecutive decision rounds. Your total earnings in Part 1 will be the sum of your earnings from all these rounds. **You will be randomly divided into groups of three participants at the beginning of each round.** This means that you will not always interact with the same participants.

In each of the 10 rounds, each participant has to choose a number from 1 to 7 (1, 2, 3, 4, 5, 6, 7). In each round, the smallest number chosen in each group will be identified.
The payoff for each round is given by the formula below where x is the number you chose, and $min(x)$ is the smallest number chosen by members of your group:

$$Earnings = 60 + 20 * min(x) - 10 * x$$

The following table reproduces the formula for all possible combinations of your number and the smallest number chosen by members of your group:

	Smallest Number in your Group						
Your Number	7	6	5	4	3	2	1
7	130	110	90	70	50	30	10
6	-	120	100	80	60	40	20
5	-	-	110	90	70	50	30
4	-	-	-	100	80	60	40
3	-	-	-	-	90	70	50
2	-	-	-	-	-	80	60
1	-	-	-	-	-	-	70

Here are some examples.

Example 1: Assume that you choose 3, the second group member chooses 3, and the third group member chooses 3, then in the table your number is "3" and the minimum number in your group is "3", your earnings are therefore 90 tokens.

Example 2: Assume that you choose 5, the second group member chooses 6, and the third group member chooses 4, then in the table your number is "5" and the minimum number in your group is "4", your earnings are therefore 90 tokens.

Example 3: Assume that you choose 4, the second group member chooses 1, and the third group member chooses 7, then in the table your number is "4" and the minimum number in your group is "1", your earnings are therefore 40 tokens.

The payoffs for all members of the group are calculated in the same way based on their choice and the smallest number in your group. At the end of the round, you will be informed about the smallest number in your group and your own earnings. The same process will be repeated for a total of 10 rounds.

Parts 2 and 3 have identical instructions but different news reports, as explained in the main text. There are 4 possible news reports: BO and BP news reports are given in the main text and GO and GP follow. We sketch the connection to the theory of explanatory style after the reports. Of course, the subjects just received the news report.

GO

Job creation accelerates

Job creation accelerated last quarter as output grew by 0.5%. The increase in jobs was 220k, up from 140k in the previous quarter. Domestic investment and consumption expenditure continued to fuel growth and private sector confidence seems not to have been affected by the rise in interest rates earlier in the year. With exports also at record levels, predictions for output growth next quarter are likely to be revised upwards.

Unemployment fell to 6% but there is no sign of inflationary pressure developing. With labour force participation so low, there is scope for entrants into the labour force to boost the supply of labour as demand increases. Baby boomers will likely delay retiring and discouraged workers will probably start looking for work again as the demand for labour continues to increase. There should be no need for the central bank to tighten policy significantly.

GP

Job creation accelerates

Job creation accelerated last quarter as output grew by 0.5%. The increase in jobs was 220k, up from 140k in the previous quarter. The good news is provisional. Exports were at record levels and the rise in interest rates earlier this year have not yet affected domestic investment and consumption expenditure.

Unemployment fell to 6% raising concerns over future inflation. Labour force participation remains stubbornly low. Unless baby boomers stop retiring and benefits for those not in work become less generous, there will be no increase in the supply of labour to meet any further increase in demand. With the resultant pressure on prices, the central bank may have to further raise interest rates.

***GO: Internal** (your responsibility = personal), **global** (part of general improvement, = pervasive), **stable** (temporal continuity = permanent)*

*GP: **External*** (outside your control)***, Specific*** (isolated, not part of general improvement), ***unstable (***temporary)

Consistently with explanatory style theory, GO has internal (e.g. 'domestic investment and consumption expenditure'), global (e.g. 'baby boomers will likely delay retiring') and stable (e.g. 'predictions for output growth next quarter are likely to be revised upwards') attributes, while GP has external (e.g. 'exports were at record levels'), specific (e.g. 'unemployment fell to 6% raising concerns over future inflation') and unstable ('the good news is provisional') attributes.

APPENDIX B

The Amazon MTurk trial was run in March 2017. It exposed 94 subjects from the US (60 males, 39 females) to the same news framings than our subjects in the lab and asked them, after the same control questions, to state their level of confidence in the economy. The precise question was "How pessimistic or optimistic do you feel about the state of the economy?" and the answer was, exactly as the choice over numbers in the MEG, on a scale from 1 to 7. We paid them 0.20$ each for their time.
Table 7 has the statistics on the average and on the standard errors by treatment. For the report on the bad event, we clearly see a difference between the optimistic and the pessimistic framings (4,32 *vs* 3,67), which is almost significant at 10 percent level (p = 0.1108). An ordered probit regression disentangling between events and styles finds that indeed optimism boosts sentiment more than pessimism at 10 percent level (Table 8).

	Good event	**Bad event**
Optimistic	4.000	4.316
	(1.567)	*(1.493)*
	N=23	N=19
Pessimistic	3.773	3.667
	(1.716)	*(1.295)*
	N=22	N=30

Note: Standard deviation in parenthesis.

Table 7: Descriptive statistics on Amazon MTurk trial

	Sentiment
Good event	0.176
	(0.307)
Optimistic	0.519*
	(0.274)
Good event*Optimistic	-0.401
	(0.443)
Female	-0.173
	(0.219)
LOT	0.0620***
	(0.0230)
Observations	94

Note: Regression is ordered probit model. Standard errors in parentheses.
$^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$

Table 8: Regression results on sentiment in the Amazon MTurk trial

APPENDIX C

Table 9 reports results from regressions on the baseline both on 1st round and all rounds. In terms of marginal effects, an increase in dispositional optimism implies at least a 12 percent higher probability to choose a number higher than 3 in the 1st round. However, over all rounds this effect disappears, as it does when there are treatments (see Appendix E).

	(1)	(2)
	1st round	All rounds
	Number	Number
Age	0.0855***	0.0763
	(0.0258)	*(0.0961)*
Female	-0.138	0.112
	(1.001)	*(0.440)*
LOT	0.0187**	0.0778
	(0.00888)	*(0.0880)*
Newsreports	0.0627	-0.199***
	(0.0669)	*(0.0175)*
Economics	-0.0485*	0.115
	(0.0257)	*(0.145)*
Observations	33	990

Note: Regression(1) is ordered probit. Regression (2) is mixed-models ordered probit. This is with random intercepts at both the session and subject level. Period dummies are included (but not reported). Errors clustered at session level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 9: Regression results on Numbers in the baseline

APPENDIX D

Tables 3 and 4 in the ***4. Result*** section report Wald post-estimations statistics over pairwise comparisons of coefficients. The underlying regression is displayed in Table 10. The model is the same as for the regressions on the 1st stage, so a mixed-model ordered probit with intercepts at session level, and the omitted category for the regression is the baseline.

	(1) Stage 2 Smallest number
GP(2) - change in style	-0.169
	(0.903)
BP(2) - change in style	0.0316
	(0.926)
GO(2) - change in style	0.624
	(0.910)
BO(2) - change in style	0.955
	(0.936)
BO(2) - change in event	0.110
	(1.208)
GO(2) - change in event	2.010**
	(1.003)
BP(2) - change in event	-0.756
	(1.248)
GP(2) - change in event	0.549
	(1.233)
November sessions	0.310
	(1.112)
Sessions with 12 subj.	-0.290
	(0.480)
Observations	900

Note: Regressions (1) is mixed-models ordered probit. This is with random intercepts at the session level. Period dummies are included (but not reported). Errors clustered at session level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 10: Regression results on the Smallest Number, 2nd stage

APPENDIX E

All analyses of the ***4. Results*** section are here replicated over numbers and regressions, apart from treatment dummies, have individual controls (age, gender, number of newspaper reports read in a week, self-reported knowledge in economics and LOT). The main results are replicated.

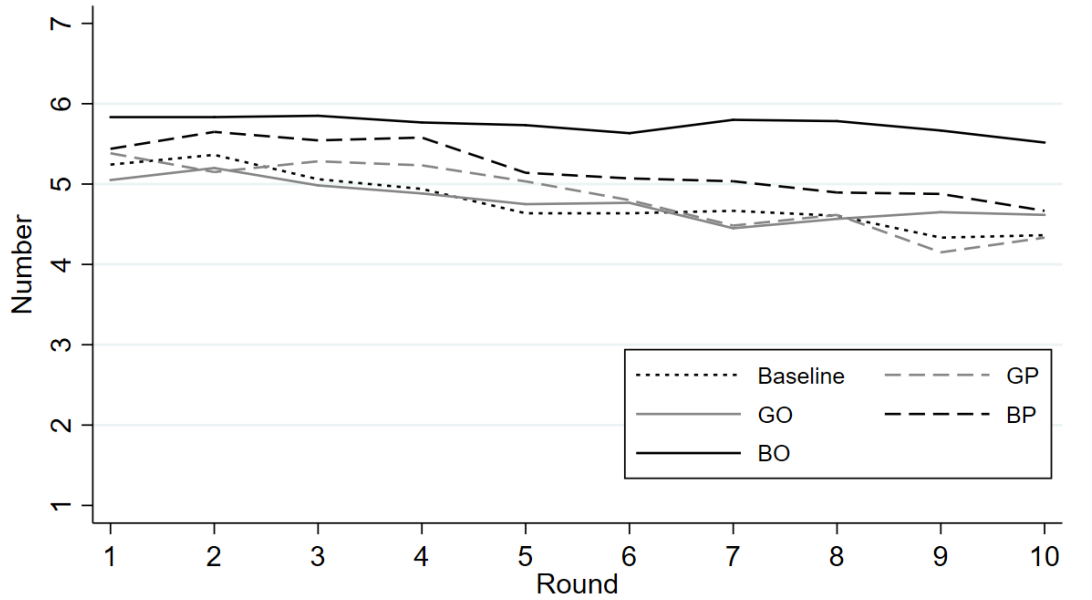

Figure 5: Average number for each period, 1st stage

		1th round	All rounds
Comparison of style	BO vs. BP	z = 1.32; p = 0.188	z = 2.45; p = 0.014**
	GO vs. GP	z = -1.26; p = 0.209	z = -0.36; p = 0. 723
Comparison of facts	BO vs. GO	z = 2.31; p = 0.021**	z = 3.98; p = 0.000***
	BP vs. GP	z = -0.40; p = 0.688	z = -1.25; p = 0.212
Comparison of treatments to the baseline	BO vs. baseline	z = 1.56; p = 0.120	z = 3.19; p = 0.001***
	BP vs. baseline	z = 0.44; p = 0.658	z = 1.11; p = 0.267
	GO vs. baseline	z = -0.42; p = 0.675	z = -0.29; p = 0.769
	GP vs. baseline	z = 0.72; p = 0.469	z = 0.24; p = 0.812

Note: Observations on number at subject level. * p < 0.1, ** p < 0.05, *** p < 0.01

Table 11: Wilcoxon ranksum tests on numbers, 1st stage

	(1)	(2)	(3)	(4)

	1st round Number	1st round Number	All rounds Number	All rounds Number
GO	-0.105		0.0493	
	(0.101)		*(0.410)*	
GP	0.242*	0.352**	0.0928	0.0455
	(0.135)	*(0.154)*	*(0.465)*	*(0.381)*
BO	0.412***	0.529***	0.926**	0.854***
	(0.131)	*(0.140)*	*(0.364)*	*(0.276)*
BP	0.169	0.269**	0.427	0.383
	(0.127)	*(0.107)*	*(0.491)*	*(0.413)*
November sessions	-0.0786	-0.0847	-0.0795	-0.103
	(0.204)	*(0.206)*	*(0.894)*	*(0.888)*
Sessions with 12 subjects	-0.132	-0.123	-0.0900	-0.0844
	(0.154)	*(0.154)*	*(0.226)*	*(0.222)*
Age	-0.0234	-0.0273	-0.00353	-0.00898
	(0.0243)	*(0.0244)*	*(0.0105)*	*(0.00880)*
Female	-0.141	-0.0898	0.00570	0.0143
	(0.133)	*(0.108)*	*(0.139)*	*(0.101)*
LOT	0.00538	0.00113	0.00705	-0.00290
	(0.0152)	*(0.0177)*	*(0.0218)*	*(0.0221)*
Newsreports	-0.0289	-0.0411	-0.0577	-0.0410
	(0.0421)	*(0.0495)*	*(0.0472)*	*(0.0515)*
Economics	0.0311	0.0456	0.0135	0.00567
	(0.0410)	*(0.0476)*	*(0.0470)*	*(0.0515)*
Observations	270	237	2,700	2,370

Note: Regression (1) is ordered probit. Regressions (2) and (3) are mixed-models ordered probit. These are with random intercepts at both the session and subject level. Period dummies are included (but not reported). Errors clustered at session level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 12: Regression results on Numbers, 1st stage

	1st round	**All rounds**
BO vs. BP	z = -0.28; p = 0.778	z = 2.61; p = 0.009***
GO vs. GP	z = 2.19; p = 0.028**	z = 1.31; p = 0.189
BO vs. baseline	z = 1.61; p = 0.107	z = 1.33; p = 0.184
BP vs. baseline	z = 1.54; p = 0.123	z = 0.36; p = 0.717
GO vs. baseline	z = 1.79; p = 0.073*	z = 0.96; p = 0.339
GP vs. baseline	z = -0.28; p = 0.776	z = -0.02; p = 0.982

Note: These are post-estimation Wald tests between treatments. Regressions are mixed-models ordered probit. These are with random intercepts at both the session and the subject level. Period dummies are included (but not reported). Errors are clustered at session level * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 13: Post-estimation pairwise comparisons of numbers – change in spin

	1st round	All rounds
BO vs. GO	z = -3.34; p = 0.001***	z = -3.13; p = 0.002***
BP vs. GP	z = -2.26; p = 0.024**	z = 1.07; p = 0.286
BO vs. baseline	z = 0.43; p = 0.665	z = 0.34; p = 0.738
BP vs. baseline	z = 0.15; p = 0.879	z = -0.54; p = 0.592
GO vs. baseline	z = 3.15; p = 0.002***	z = 2.98; p = 0.003***
GP vs. baseline	z = 1.48; p = 0.140	z = 0.49; p = 0.622

*Note: See note in Table 13 * p < 0.1, ** p < 0.05, *** p < 0.01*

Table 14: Post-estimation pairwise comparisons of smallest numbers – change in event

	(1)	(2)	(3)	(4)	(5)
	Baseline	GO(1)→GP(2)	BO(1)→BP(2)	GP(1)→GO(2)	BP(1)→BO(2)
Stage 2	-0.328	-0.0949***	-0.675***	-0.313***	0.00670
	(0.242)	*(0.0171)*	*(0.0742)*	*(0.0150)*	*(0.0234)*
Observations	660	660	600	600	600

	(6)	(7)	(8)	(9)
	GO(1)→BO(2)	BO(1)→GO(2)	GP(1)→BP(2)	BP(1)→GP(2)
Stage 2	-0.505***	0.178	-0.320***	-0.289***
	(0.104)	*(0.613)*	*(0.0631)*	*(0.0846)*
Observations	540	600	600	540

*Note: Regressions are mixed-models ordered probit. These are with random intercepts at both the session and subject level. Errors clustered at session level in parentheses. * p < 0.1, ** p < 0.05, *** p < 0.01*

Table 15: Regression results on change in number from stage 1 to stage 2 for each individual transition

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