Economic Shocks and Subjective Well-Being: Evidence from a Quasi-Experiment

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Abstract

This paper examines how economic shocks affect individual well-being in developing countries. Using the case of a sudden and unanticipated currency devaluation in Botswana as a quasi-experiment, we examine how this monetary shock affects individuals' evaluations of well-being. We do so by using micro-level survey data, which – incidentally – was collected in the days surrounding the devaluation. The chance occurrence of the devaluation during the time of the survey enables us to use pre-treatment respondents, surveyed before the devaluation, as approximate counterfactuals for post-treatment respondents, surveyed after the devaluation. Our estimates show that the devaluation had a large and significantly negative effect on individuals' evaluations of subjective well-being. These results suggest that macroeconomic shocks, such as unanticipated currency devaluations, may have significant short-term costs in the form of reductions in people's sense of well-being.

1. Introduction

Few tasks are more important in the social sciences than discovering the sources of human well-being. While this remains a contested issue (Bjørnskov et al. 2010; Clark et al. 2008; Deaton 2012; Frey and Stutzer 2000; Frey 2008), the question of whether 'money buys happiness' attracts particular attention, no doubt because of the seemingly paradoxical finding – first reported by Easterlin (1974, 1995) – that income growth is not associated with corresponding increases in happiness and well-being (Clark et al. 2008; Easterlin et al. 2010). However, recent work has emphasized that subjective well-being does seem to fluctuate with financial crises (Deaton 2012; Bjørnskov in press) and macroeconomic factors like inflation, unemployment, and GDP (Oswald 1997; Di Tella et al. 2001, 2003; Stevenson and Wolfers 2008; Kahneman and Deaton 2010; Sacks et al. 2012a), providing some support for the claim that income is correlated with happiness and well-being.

In this paper, we contribute to this literature by examining how macroeconomic shocks affect individual well-being. Using the case of an unanticipated and rapidly implemented currency devaluation in Botswana – a middle-income country in sub-Saharan Africa – we examine how individual evaluations of well-being respond to such a monetary policy shock. We do so by analyzing micro-level data from the *Afrobarometer*, which happened to be in the field conducting interviews for a survey at the time when the citizens of Botswana were exposed to the news of the national currency devaluation. Specifically, two days into the survey – late in the day on May 29, 2005 – the central bank of Botswana and the Ministry of Finance and Development Planning issued a public statement saying that the national currency would be devaluated by 12 percent, with effect from the following morning.[1] Our analysis exploits the fact that the chance occurrence of the

[1] Press Release, 17:00 hours, Sunday 29 May 2005, issued by the Ministry of Finance and Development Planning.

devaluation creates a clear demarcation between respondents surveyed before the devaluation and respondents surveyed in the days following the devaluation.

The incidental occurrence of the central bank's intervention during the time of the survey provides us with a quasi-experimental research design allowing us to examine the effect of a monetary shock on subjective evaluations of well-being. However, the fact that the devaluation was an unanticipated shock – a claim we will validate later – is not sufficient to treat it as exogenous. Identification of the causal effect on subjective well-being requires that the devaluation – the treatment – is orthogonal to the error term, i.e. uncorrelated with other factors that may affect the outcome. As we discuss in detail below, this assumption may not be satisfied unconditionally due to geographically imbalanced sampling of respondents in the pre- and post-treatment groups, caused by a shift in the sampling of respondents from urban to rural areas in the days surrounding the devaluation. However, since we can identify and measure the source of nonrandom treatment assignment with relative precision, the exogeneity of the devaluation is plausible conditional on adjusting for the urban-rural shift.

On this assumption, our results show that the devaluation caused an instant and observable discontinuity in the data. The change in reported levels of well-being occurred literally overnight, reflecting that individuals' responses were immediate and most likely based on expectations about the future consequences of the devaluation. Thus, respondents in the treatment group – surveyed after the devaluation – report levels of well-being that are both substantially and significantly lower than respondents in the control group – surveyed immediately before the devaluation. This result is robust to adjusting the data for nonrandom treatment assignment in various ways and to centering the sample on the discontinuity in the data created by central bank intervention. However, we also report evidence that respondents with more education and larger consumption of media news react

more strongly to the policy shock, suggesting that the effect of monetary shocks may be conditional on individuals' information and cognitive sophistication.

The paper contributes to the broader literature on the determinants of individual happiness and well-being (Oswald 1997; Frey 2008; Dolan et al. 2008; Bjørnskov et al. 2010). It is also closely related to contributions linking macroeconomic variables like GDP and inflation to developments in subjective well-being (Di Tella et al. 2001, 2003; Stevenson and Wolfers 2008; Deaton 2012). In particular, our results support the conclusion of Di Tella et al. (2003: 823) that 'macroeconomics matters', at least with respect to monetary shocks. However, the quasi-experimental nature of our design distinguishes it from standard correlational studies, which mostly regress well-being or life satisfaction on some potentially endogenous micro- or macro-level explanatory variable. The 'shock nature' of the currency devaluation allows us to avoid most of the problems caused by the usual endogeneity of macroeconomic and policy variables like GDP and inflation (Besley and Case 2000; Di Tella 2003). In this respect, our paper adds to the small literature using large-scale exogenous shocks to study changes in subjective well-being (e.g. Frijters et al. 2004).

The rest of the paper is structured as follows. Section 2 outlines theoretical mechanisms linking currency devaluations to subjective well-being. The research design and the experimental situation are described in section 3. Section 4 introduces the data and section 5 provides empirical estimates of the effects of the devaluation. Section 6 concludes.

2. Devaluation, prices, and subjective well-being

The response of individuals to the news of a devaluation might depend on at least two different mechanisms. First, following a devaluation, the prices of imported consumer goods will increase. If contracts are written in foreign currency – in the case of Botswana most likely South African Rand

or US Dollars – the price increase will be virtually immediate. If contracts are denoted in Botswana Pula, the price correction may occur gradually as import contracts are renegotiated over a period of weeks or months to reflect the new exchange rate. Depending on the price elasticity of the good, the degree of competition in the product market, and the availability of domestic substitutes – all of which would reduce the price response – some (or all) of the price increase will be reflected in proportionately increasing consumer prices. The devaluation thus makes imported goods more expensive and therefore reduces real wages for the population at large. Since Botswana is a net importer of food and other consumables like fuel and energy from, e.g., South Africa (Rakotoarisao et al. 2011), the economic costs of the currency devaluation mainly accrued to consumers, at least in the shorter term.

Second, the general price level is also likely to increase following a devaluation for two reasons associated with the price of domestically produced goods and services. One is that the devaluation affects final goods through its effect on import prices of raw materials and intermediate goods. By increasing input prices in production, the devaluation affects the prices of final goods that are produced domestically but relies on imported raw materials or intermediates. The second reason is that an import price increase is likely to cause an increase in the demand for domestically produced substitutes (or near-substitutes). As such pairs of goods tend to have substantial cross-price elasticities, the price of substitutes is also likely to increase proportionally to the devaluation. These effects are likely to affect individuals and households to approximately the same extent.

2.1. The role of expectations

Although price increases may occur immediately following the news of a currency devaluation, they do not adjust fully or instantly to their new equilibrium. Subsequent changes in subjective well-being are therefore likely to at least partially reflect expectations about the future (Graham

2008; Guriev and Zhuravskaya 2009; Sacks et al. 2012b). With the first mechanism – increases in prices of imported goods – price changes will take place almost instantly. Changes in economic expectations can therefore occur very rapidly given that individuals rely on the consumption of imported final goods. With the second mechanism – increases in the general price level – the inflationary effects of the devaluation are likely to spread over time to most goods and services, and lead to changes in expected and actual economic well-being for larger segments of society. However, the speed of adjustment of expectations is likely to depend on individuals' economic and cognitive sophistication. If individuals have little information about the economy, their economic expectations are likely to adapt gradually as the consequences of the devaluation become observable in prices, real wages and unemployment risk. In contrast, if individuals have sufficiently sophisticated mental models of the economy, a devaluation enables the formation of rational expectations (Muth 1961; Phelps 1967) that change rapidly after the *news* of the devaluation but presumably before the *actual* changes in absolute or relative prices. In this case, individuals with more sophisticated mental models of the economy will be better at foreseeing the consequences of devaluation and thus change their expectations earlier and more precisely.

These non-technical theoretical considerations lead us to expect the following: First, people's evaluations of subjective well-being will on average decrease following a devaluation, all else equal. Second, however, since price effects may not materialize immediately, we also expect that individuals with more sophisticated mental models and more complete information are able to form more accurate predictions of the consequences of a devaluation, and that their self-reported well-being will therefore respond more strongly to the news of a devaluation. Against this background, we proceed by describing the quasi-experimental design.

3. A quasi-experimental research design

Late in the afternoon on May 29, 2005, the Bank of Botswana – the country's central bank – and Botswana's Ministry of Finance and Development Planning issued a press release stating that the national currency – the Pula – would be devaluated by 12 percent against a basket of international currencies, with effect from the following morning, Monday 30, 2005. The central bank's decision to devaluate the Pula came as a shock to the general public, the business community, and currency markets in Botswana, as we will show in more detail below. Our research design exploits this sudden and unanticipated intervention to examine the effect of economic shocks on individuals' subjective well-being. We are able to do so because, incidentally, the devaluation occurred during the period where the *Afrobarometer* – an independent research project conducting surveys of political and social issues in Africa – was interviewing a representative sample of citizens in Botswana.[2] The chance occurrence of the devaluation two days into the survey demarcates the sample of respondents into a treatment group surveyed after the intervention and a control group surveyed immediately before the intervention.[3]

The terms 'natural' and 'quasi' experiments are often used in an imprecise and interchangeable sense. However, we advertently refer to the Botswana devaluation as a *quasi*-experiment and distinguish it from natural experiments. While a common feature of natural and quasi experiments is that an intervention generated by some force outside the control of the researcher assigns subjects into treatment and control groups (Meyer 1995; Robinson et al. 2009),

[2] The data are published as part of the third round of the *Afrobarometer*. Technical details on the sampling of respondents and the methodology of the survey are available on the *Afrobarometer* website http://afrobarometer.org/. See also Bratton et al. (2004) for descriptions of the *Afrobarometer*, and Mattes (2007) for a discussion of survey research in developing countries.

[3] The survey started on May 28 and ended on June 12, 2005. Since the devaluation was announced late in the afternoon (17:00 hours) on May 29, no interviews started after the announcement of the devaluation (the final interview began at 16:57 hours).

the defining characteristic of natural experiments is that treatment assignment occurs in a random or 'as-if' random way (Dunning 2008, 2012). However, as emphasized by Cook and Campbell (1979) and Achen (1986), what distinguishes quasi-experimental designs from natural and controlled experiments is that assignment to treatment is nonrandom, which means that the treatment and control groups are imbalanced – or non-equivalent – at the outset. This means that even a macroeconomic shock, e.g. a surprise devaluation, may not be strictly exogenous because nonrandom treatment assignment may make treatment status correlated with other factors that affect the outcome. In a regression framework, nonrandom assignment to treatment may therefore imply that treatment status is not statistically independent of the error term – at least not unconditionally – and that confounding is a potential challenge to a causal interpretation of the estimated treatment effect.

While the survey data we use is a random and representative sample of 1200 adult citizens in Botswana, the key source of nonrandom assignment to treatment and control is that the sampling of respondents before and after the devaluation is geographically imbalanced. Pre-treatment respondents are predominantly from the capital of Botswana – Gaborone – and from urban areas more broadly. Specifically, 63 percent of the pre-treatment respondents were from Gaborone; 85 percent were from urban areas. In the two days following the devaluation, only 10 percent of the respondents were from urban areas. Therefore, the treatment coincides with a shift in the sampling of respondents from urban to rural areas, which is also likely to correlate with respondents' evaluations of their living conditions and well-being. Part of the treatment effect might therefore be due to preexisting differences in subjective well-being between people in rural and urban areas, or may arise if, e.g., more confident, optimistic, or resourceful individuals self-select into cities and urban areas (cf., Cook and Campbell 1979; Achen 1986).

Despite this initial imbalance between the control group and the treatment group, there are at least two reasons to believe that we can plausibly mitigate the consequences of nonrandom assignment. First, since we can identify the source of nonrandom treatment assignment – geographically imbalanced sampling – with relative precision, we can also go a long way towards making the treatment and control groups comparable by adjusting for the relevant covariates. As we explain in more detail below, we do so in a number of ways; most importantly by controlling for whether respondents live in urban or rural areas; by excluding respondents in the Gaborone area; and by zooming in on the discontinuity in the data generated by the devaluation. Second, since the imbalance between the pre- and post-treatment groups is a result of the fact that the *Afrobarometer* simply *happened to* conduct interviews mainly in Gaborone and urban areas prior to the devaluation, we can rule out other sources of nonrandom treatment assignment caused by the actors generating the data. First, it is highly implausible that the *Afrobarometer*'s timing of the survey was related to the central bank's decision to devaluate in any way, or vice versa. Second – and more importantly – there is little reason to believe that respondents could somehow sort or directly self-select into treatment or control, since they did not have the information, incentive, or capacity to do so (cf., Dunning 2012: 236). Indeed, qualitative evidence suggests that people in Botswana did not have any prior information about the central bank's decision to devaluate. For instance, media reports by the *Mmegi* (The Reporter) – an independent Botswana newspaper – and the BBC in the days following May 29, 2005, consistently refer to the devaluation as a 'surprise' or 'shock'. One report notes that the reduction of the value of the Pula 'has taken consumers by surprise'.[4] In another report, a woman being interviewed in the wake of the devaluation said that 'this information should be disseminated while we can act. This was a pre-emptive action'. These statements clearly

[4] 'Labour Slam "Surprise" Pula Devaluation', *Mmegi*, May 31, 2005. 'Botswana devalues the Pula', *BBC News*, May 31, 2005. 'Consumers Shocked at Effect of Pula Devaluation', *Mmegi*, May 31, 2005.

suggest that the central bank's intervention was a surprise move to citizens. Indeed, even business actors in currency markets – who should, *a priori*, be among the most likely candidates to be well-informed about a monetary policy intervention – expressed great surprise at the news of the devaluation. For instance, a BBC report stated that 'Botswana has surprised the currency market by devaluating the Pula by 12%'. On May 31, 2005 – the day after the devaluation became effective – the *Mmegi* newspaper quoted a chief executive officer of Stockbrokers Botswana – a registered member of the Botswana Stock Exchange – for saying that 'the move has taken the market by surprise, particularly the magnitude of the devaluation and the timing.'[5] A few days later, Stockbrokers Botswana (2005) issued a briefing paper commenting on the devaluation. While the company acknowledged the potential benefits of the devaluation to import-competing domestic producers and export companies, e.g. the mining industry, it also stated that '…we take issue with the brute force of the devaluation. It may have been more appropriate to introduce the new mechanism, explain it, and then take steps to devalue to the desired level in a more measured fashion. This would allow corporates and investors to plan for the adjustments and reduce the shock premium that the move will command. The danger is that where the market is shocked it will overreact…' (Stockbrokers Botswana 2005: 1).

This qualitative evidence supports two important points: First, neither the devaluation nor its timing was anticipated by the general public, and not even by businesses operating in currency markets. In that sense, it was an 'exogenous' economic shock to citizens and the outcome we study, subjective evaluations of well-being. Second, although citizens are able to self-select into categories (like living in an urban area) that are correlated with treatment assignment, neither respondents nor the *Afrobarometer* had information, incentive, or capacity to decide whether respondents were interviewed before or after the devaluation, making direct self-selection into treatment highly

[5] 'Devaluation Hits Low-Income Earners, *Mmegi*, June 6, 2005.

improbable. Rather, the currency devaluation by the Bank of Botswana was an event that demarcated the respondents of the *Afrobarometer* survey into two groups, not because of the knowledge or decisions of respondents, but simply by chance.

4. Devaluation and well-being: Simple pre- and post-treatment comparisons

To get a sense of the differences between pre- and post-treatment groups, this section shows the simple relationship between exposure to the devaluation and subjective well-being, as well as the development in food prices in the months surrounding the devaluation. The latter is important because it illustrates the most plausible mechanism connecting the currency devaluation to individuals' evaluations of well-being.

As dependent variable, we use respondents' answers to the following question: "In general, how would you describe: Your own present living conditions?" Answers are given on a scale consisting of the categories 'very bad', 'fairly bad', 'neither', 'fairly good', and 'very good', where high values denote good living conditions. While the literature often uses questions concerning 'life satisfaction' (Bjørnskov et al. 2010; Deaton 2008, 2012; Kahneman and Deaton 2010; Asadullah and Chaudhury 2012), the question we use asks respondents to evaluate their present living conditions on a scale from 'very bad' to 'very good', which is clearly a constitutive feature of subjective well-being. We therefore use this question to measure subjective well-being. The *Afrobarometer* also contains a related question, asking respondents to evaluate their living conditions relative to those of other people.[6] However, we emphasize that replications using this

[6] The wording of the question is: 'In general, how do you rate: Your living conditions compared to those of other Batswana?' Answers are given on the categories: 'Much worse', 'Worse', 'Same', 'Better', 'Much better'.

variable – evaluations of relative living conditions – does not change our findings substantially or statistically.[7]

[Figures 1 and 2 about here]

Figure 1 shows a simple time-series plot of respondents' average evaluations of their present living conditions (subjective well-being) for each day of the survey. Figure 2 shows a plot of the development in an index of food prices from July 2004 to September 2006, with the value of September 2006 indexed at 100 (Central Statistics Office 2008). The vertical lines indicate the timing of the devaluation. As is clearly visible in Figure 1, upon the devaluation of the Pula, there is an immediate and substantial drop in respondents' average evaluations of living conditions in the magnitude of 0.16 on a scale from 0 to 1. Compared to individuals surveyed prior to the devaluation, the subjective well-being of people surveyed after the devaluation was much lower. The immediacy of this drop in well-being is important too, as prices are unlikely to have adjusted very much already on the first day after the devaluation. While there were media reports of upward re-pricing by retailers and a consequent 'shock of skyrocketing prices'[8] shortly after the devaluation, the price level of consumables did not fully adjust to its new equilibrium within the short period where the survey data were collected. As shown in Figure 2, food prices developed as expected in the months following the devaluation. While the food price index was relatively stable in the year preceding the devaluation, it increased dramatically in the year after the devaluation. This suggests that people's reaction to the devaluation – the drop in their evaluations of subjective well-being shown in Figure 1 – is in large part driven by (qualitatively correct) expectations about

[7] Detailed results are available upon request.

[8] 'Consumers Shocked at Effect of Pula Devaluation', *Mmegi*, May 31, 2005.

the effects of the devaluation. Indeed, while the price effect of the devaluation materialized over months, there are good reasons to believe that people in Botswana knew what to expect, because 16 months earlier – in early February 2004 – the Bank of Botswana also implemented a 7.5 percent devaluation of the Pula. While this did not make the May 2005 devaluation any less of a shock to people in Botswana, the prior experience with the consequences of a sizeable currency devaluation means that people may have rationally updated their expectations concerning the effects of the devaluation rapidly, even though the consequences of the May 2005 had not fully materialized at the time of the survey.

Figure 2 illustrates a second important point, namely that the Pula devaluation increased the price of imported food products and consumables in general, making consumers the major losers of the devaluation. A likely causal mechanism linking the currency devaluation to subjective well-being is therefore (expectations about) the development in prices, particularly the price level of food and consumables. During the time of the *Afrobarometer* survey in Botswana in late May and early June 2005, this was a very salient feature of the devaluation to the Batswana. In a report in the *Mmegi* newspaper, several people being interviewed who were employed in various low-wage jobs expressed concern at the consequences of the devaluation. A taxi driver reportedly stated that the expected price increases'…will have a devastating impact on our business and the economy at large'. In the same report, another employee is quoted for saying that 'putting food on the table will empty wallets' and that 'I am concerned and feel impoverished'. These examples suggest that people in Botswana had clear expectations about what consequences the devaluation would have for the price level of consumables and, therefore, for their own well-being. They also suggest that the expectations of increasing prices could be an important factor driving individuals' feelings of being impoverished, and are therefore the most likely causal mechanism linking the currency devaluation to the drop in subjective well-being we observe in Figure 1.

Although the relationship between the Pula devaluation and subsequent drops in subjective well-being is clear in Figure 1, we can use pre-treatment observations as approximate counterfactuals for post-treatment observations only on the assumption that the devaluation is a plausibly exogenous shock to the citizens of Botswana. Given the imbalanced sampling of the pre- and post-treatment groups, the plausibility of the exogeneity assumption of course requires that we successfully condition on relevant confounders, most importantly by adjusting for rural-urban differences between the two groups as discussed above. However, as we show in the next section, neither the urban-rural shift nor a range of other potential confounders can fully account for the observed drop in subjective well-being following the devaluation. Detailed descriptions of all variables used in the econometric analyses along with summary statistics are available in Table A1 in the online appendix.[9]

5. Empirical results

To estimate the effect of the currency devaluation on subjective well-being, our econometric analyses use both linear and non-linear models. The first treat the dependent variable as continuous by converting the categorical responses into a variable that assigns a number to each response. Following this strategy, we construct a variable, which holds the values 0, 0.25, 0.5, 0.75, and 1 corresponding to the five response categories, and use this as our dependent variable in a series of linear regressions.[10] As an alternative, we maintain the categorical nature of the data and estimate an ordered logit model, using the appropriate link function. In what follows, we report the

[9] The appendix is available at https://sites.google.com/site/mkjustesen

[10] This effectively amounts to a rescaling of the numerical values assigned to each response in the *Afrobarometer* survey such that our variable runs in the interval from 0 to 1.

coefficients of interest using both estimators to show that the results are qualitatively identical. Our starting point is the following linear regression model.

$$(1) \qquad y_i = a + dT_i + bX_i + e_i,$$

where the dependent variable, y_i, is respondent i's evaluation of her present living conditions; T_i is the devaluation treatment indicator; and X_i is a vector of controls. The identifying assumption in (1) is that that T and e are orthogonal, $\text{Cov}(T, e)=0$, conditional on X, where the most import element in X is respondents' rural-urban status. Table 1 shows the results.

[Table 1 about here]

5.1. Main results

Panel A in Table 1 show results obtained using linear regressions. Panel B shows the treatment coefficient from identical specifications obtained using ordered logit regressions. Throughout all models in Panel B, the ordered logits confirm the basic conclusion from the linear models of a negative association between the devaluation and respondents' evaluations of their living conditions. Since the results are substantially similar, we comment only on the results in Panel A.

Column (1) in Panel A shows the unconditional association between the treatment and respondents' evaluation of their living conditions. The point estimate of the treatment effect is negative and with a magnitude about 16 percentage points corresponds to the finding in Figure 1. The association is highly significant and corresponds to 60% of a standard deviation. In columns (2) and (3), respectively, we include an urban dummy and a capital (Gaborone) dummy. This serves to immediately alleviate concerns that our results are in fact driven by a shift in the sampling of

respondents from urban (predominantly Gaborone) to rural areas. In column (2), the urban dummy barely changes the estimated association. In column (3), the Gaborone dummy does attenuate the association somewhat, but it remains sizeable and statistically significant. In the next section, we tackle the fundamental problem of nonrandom assignment in more depth.

In column (4) we proceed to include a full set of dummies for the geographical regions of Botswana registered by the *Afrobarometer* to remove as much idiosyncratic geographical variation as possible in how respondents evaluate their living conditions. The association drops marginally to 0.12 and remains negative and highly significant. In columns (5) and (6), we included fixed effects for respondents' tribal affiliation (column (5)), and for each of the 25 occupational categories available in the *Afrobarometer* survey (column (6)). In both cases, the association between the devaluation treatment and subjective well-being remains substantively and statistically significant.[11]

In columns (7) and (8) we zoom in on the discontinuity in the data, i.e. the days immediately surrounding the devaluation. We do so to minimize the likelihood that some unobserved event occurring after – and close to – the treatment is confounding the results. In column (7), we focus on the four days surrounding the devaluation (two days before, two days after); in column (8) we focus on the first day before and the first day after the devaluation. This drastically reduces the sample size, but it does not change the main result: The size of the treatment coefficient is virtually unaffected as is its level of statistical significance. That is, zooming in on narrow bands around the discontinuity generated by the devaluation does not change the negative association between the devaluation treatment and subjective well-being.

[11] In addition, we have experimented with categorizing particular occupations as export-exposed. However, we cannot know whether individuals within those occupations are indeed engaged in export activities or not. Furthermore, for any clear theoretical implication to hold, we would need to know whether the Marshall-Lerner condition holds in the short run for the particular occupation. As results are as mixed as the theoretical prerequisites, we refrain from showing them.

In column (9), we control for respondents' assessments of the country's economic conditions, since this could plausibly affect how they perceive their own living conditions by supplying a signal of the existence of an overarching macroeconomic problem. The treatment coefficient barely changes, however, and remains highly significant. In column (10), a control has been added for how respondents perceive their own past personal economic situation. This shows that even after removing the effect of respondents' past economic situation, there is a very sizable and significantly negative change in the perception of living conditions following the Pula devaluation.

Finally, in column (11) both of these controls have been included together with the urban-rural indicator variable, gender, age and age squared, as well as a measure of poverty.[12] While this lowers the coefficient of interest to 0.08, it is still highly significant and substantive, corresponding to approximately a third of a standard deviation. Since these observable variables are unable to account for the negative effect of the devaluation, we do not suspect that equally important unobservables are driving the estimated effect.

5.2. Tackling nonrandom treatment assignment

As mentioned above, there are nevertheless systematic differences between pre-treatment and post-treatment responses since the former group was predominantly from urban areas (particularly the capital, Gaborone). This provides reason for caution because the shift from urban to rural respondents could plausibly coincide with a drop in evaluations of living conditions if, for example, more confident or optimistic individuals self-select into urban areas. While we dealt with this issue

[12] The poverty index is based on the work of Bratton et al. (2004), and measures poverty as respondents' experience with lack of access to five basic types of household necessities: food, water, medicine, fuel to cook food, and cash income (Justesen and Bjørnskov 2012). The index comprises the sum of these five survey items. A principal component analysis show that all five items load onto the same component (alpha=0.74).

above, this section provides further tests that tackle the issue of nonrandom treatment assignment in more detail. We do so in Table 2 chiefly by removing respondents from the Gaborone area and respondents from urban areas in general from the sample.[13]

[Table 2 about here]

In column (1), we report the basic unconditional association after omitting all respondents from Gaborone, which reduces the sample from 1,198 to 1,063 respondents. In absolute terms, the coefficient is reduced from 0.16 to 0.09, but it remains highly significant and shows that the relationship between the currency treatment and subjective well-being cannot be accounted for by the presence of respondents from the Gaborone area in the pre-treatment group. In column (2), we continue to exclude respondents from Gaborone but also zoom in on the two days surrounding the devaluation (the first day before; the first day after). This does not change the results substantially either.

Column (3) shows the basic unconditional association, this time omitting all urban respondents. The familiar conclusion obtains also in a sample of rural respondents, which shows that our results are not driven by differences in evaluations of living conditions between urban and rural respondents. The model in column (4) again omits urban respondents and zooms in on the two days surrounding the devaluation, with little impact on the treatment effect. For all model specifications, we find very similar results using ordered logit instead of OLS (as reported in Panel B).

[13] A separate issue is that the treatment divides the sample between weekend and weekdays. If subjective evaluations were, for some reason, more positive during weekends, our results would be biased (Helliwell and Wang, 2011). However, in further estimates (available upon request) we show that this is not the case in the present sample or the subsequent fourth round of the *Afrobarometer* survey in Botswana.

To further document that the effect of the devaluation on subjective well-being cannot be reduced to the shift in the sampling of respondents from Gaborone to rural areas, we have performed a series of placebo tests, repeating some of our analyses using data from Round 4 (2008) of the *Afrobarometer*. In these tests, we define a placebo treatment indicator as living outside Gaborone (or urban areas more generally). If our results were in fact driven by differences in evaluations of living conditions between respondents in the capital (or urban areas) and elsewhere, the coefficient on this placebo treatment indicator should be similar in size to the coefficient on the treatment indicator reported above. However, as we document in the appendix (Table A2), across various model specifications the difference between Gaborone and the rest of Botswana is never more than 0.07 in Round 4 of the survey. And in some cases it is both statistically and substantively indistinguishable from zero.[14] With the Round 3 data we use here, in contrast, the coefficient of interest is consistently significant and negative, in the magnitude of -0.16. This provides additional confirmation that our results are not driven by nonrandom treatment assignment of survey respondents.

5.3. Conditioning effects of information and cognitive sophistication

So far we have documented a strong effect of the shock devaluation on subjective well-being. However, as mentioned earlier, there may be reason to expect that people with higher levels of information and cognitive sophistication display stronger and more immediate responses to the news of the devaluation. Specifically, individuals with more informed and sophisticated mental models of the economy may make more accurate predictions of the consequences of the devaluation

[14] Identical results (both in terms of size and significance of coefficients) follow when we use the distinction between urban and rural rather than Gaborone as distinct from the rest of Botswana. We also checked whether there were significant differences between urban and rural areas by adding a rural-treatment interaction. As we found no indications of heterogeneity, we refrain from any further discussion.

and update their expectations about the future more rapidly. In Table 3 we examine whether the association between subjective well-being and the macroeconomic shock depends on respondents' level of information and cognitive sophistication. To operationalize information we construct a dummy variable where we treat as informed respondents who report getting daily news from the radio, television, or newspapers (coded 1). News consumption must be on a daily basis to mediate the observed drop in subjective well-being already on the day following the devaluation. If respondents do not follow the news on a daily basis, we treat them as uninformed (coded 0). As a proxy for cognitive sophistication, we use respondents' level of education (see appendix for details).

To examine whether information and cognitive sophistication condition the relationship between the currency devaluation and subjective well-being, we augment the regression model (1) with, first, an interaction of the treatment indicator and our measure of information and, second, an interaction of the treatment and education, our proxy for cognitive sophistication. Panel A in Table 3 shows results from linear regressions, while Panel B shows coefficients from identical ordered logit models. As in Tables 1 and 2, across specifications the conclusion that follows from these models confirms the OLS models in Panel A.

[Table 3 around here]

Consistent with our expectations, the coefficients in column (1) show that the association between the devaluation and subjective evaluations of living conditions is stronger if respondents are well informed. Thus, while the coefficient on the treatment indicator remains significantly negative at 0.08, treated respondents with daily news consumption evaluate their living conditions to worsen by an additional and significant 0.08. Similar conclusions follow from the specifications in columns

(2)-(3), where controls for urban residence, respondents' perceptions of their past personal economic situation, and occupation fixed effects are added. This suggests individuals with higher levels of information more quickly update their perceptions of well-being.

In columns (4)-(6), we interact the treatment indicator with respondents' education. Here we find that higher levels of education strengthen the association between the treatment and respondents' negative evaluations of their living conditions. We show this in Figure 3 by plotting the marginal effect of the currency treatment at different values of education (cf. Brambor et al. 2006) along with 90 percent confidence intervals (indicated by the dotted lines). While the devaluation shock causes a drop in subjective well-being even for people with no formal education (values of zero on the education variable), Figure 3 clearly shows that the negative effect increases and becomes more significant as respondents' educational level increases.

[Figure 3 about here]

The conditioning effects of information and education are both intuitive. In order to understand the effect of a devaluation on (future) living conditions, people must be reasonably informed about the devaluation and have mental models that allow them to predict the future consequences of the devaluation. Even so, the fact that respondents who follow news on a daily basis give more negative responses following the devaluation need not reflect cognitive sophistication, but can also reflect respondents' ability to mimic and absorb the evaluation of experts reported in the news. However, higher levels of cognitive sophistications in the form of education also seem to strengthen the effect of the devaluation on respondents' subjective well-being. This probably reflects both increased consumption of daily news among this group of respondents and that education increases individuals' knowledge about the future consequences of the devaluation and their consequent

ability to form rational expectations. Overall, these results suggest that the devaluation shock did on average result in drops in subjective well-being for all citizens of Botswana, but that the negative effect is conditional in nature and larger for people with higher levels of information and education.

6. Conclusions

This paper documents a strong and significantly negative effect of monetary shocks on subjective well-being. Using the case of a central bank devaluation in Botswana as a quasi-experiment, our results show that people's subjective well-being dropped immediately after the news of the devaluation was released in the public. As we have documented, this result is extremely robust and persists even when plausible sources of nonrandom treatment assignment are dealt with. The results therefore provide robust evidence that monetary shocks in the form of unanticipated currency devaluations have a strong and negative causal effect on how people rate their living conditions and personal well-being.

Moreover, people who are well-informed through higher levels of news consumption and people with higher levels of education respond more strongly to the news the devaluation. This suggests that the effect of monetary shocks on subjective well-being is conditional on individuals' levels of information and cognitive sophistication and not merely an effect of real economic change in the very short run. Given the short time period for which we have data – the days in which the survey was conducted in Botswana – we cannot say anything about how quickly well-being might recover following an economic shock like the one we study. However, our results strongly suggest that macroeconomic shocks, such as unanticipated currency devaluations, may have significant short-term costs in the form of reductions in people's sense of well-being.

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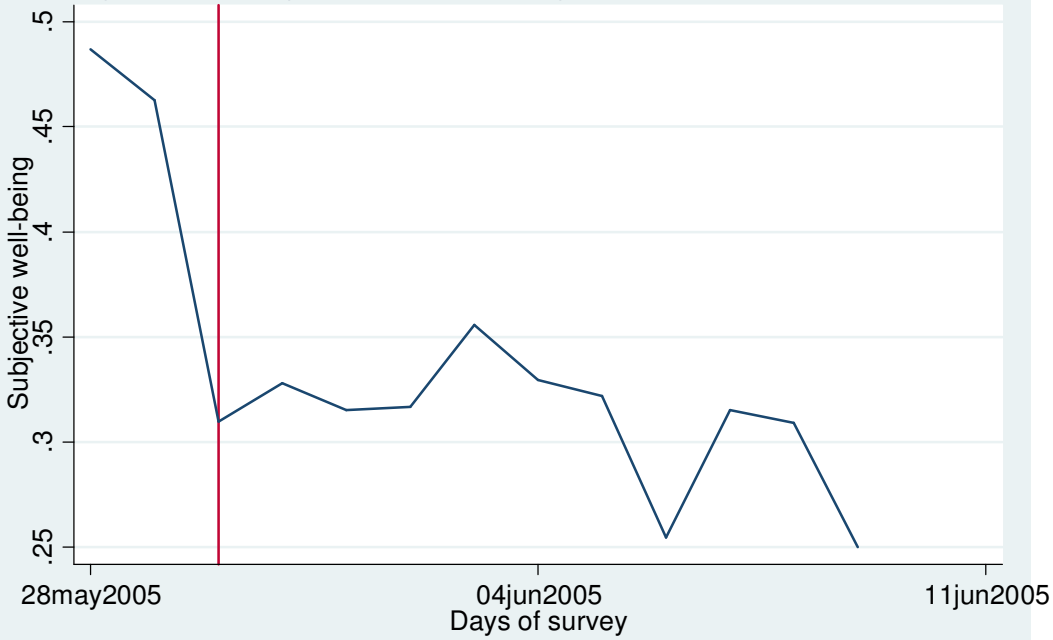
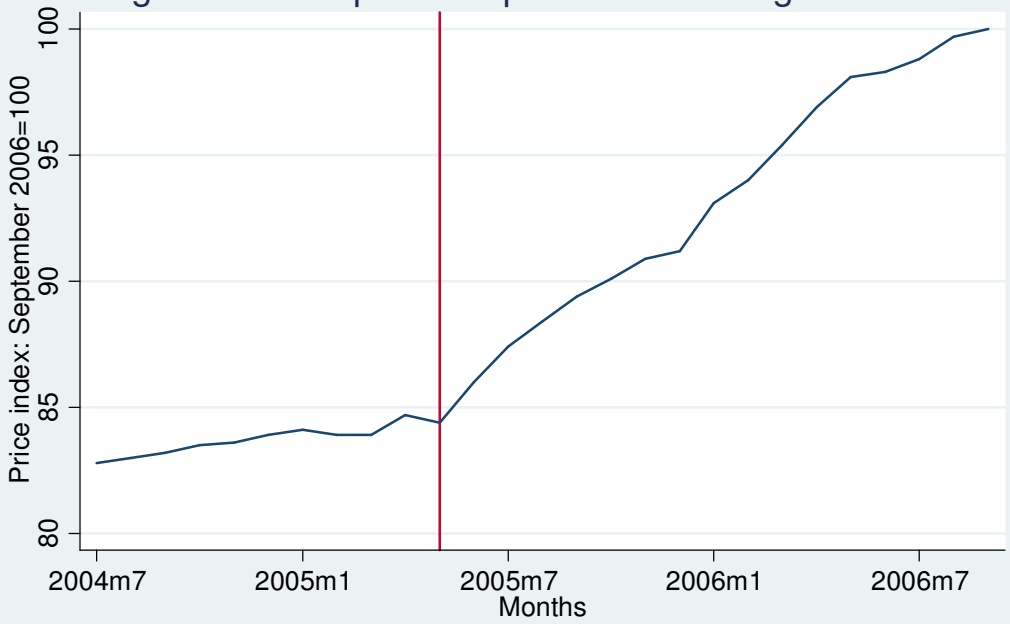
Figure 1. Subjective well-being around time of devaluation



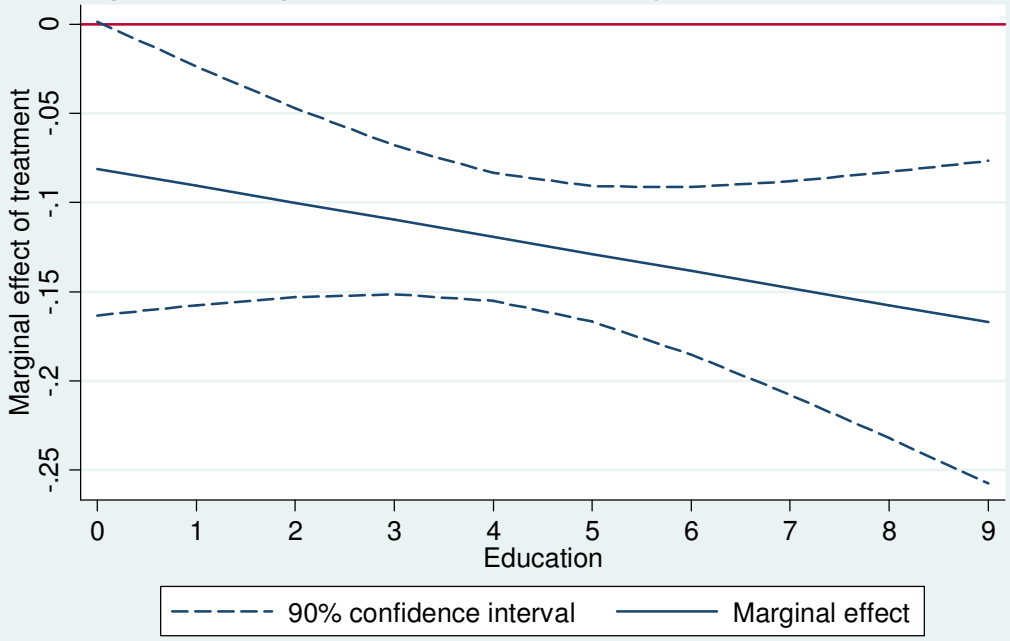
Note: Vertical line shows timing of devaluation

Figure 2: Food prices in period surronding devaluation



Note: Vertical line shows timing of devaluation
m1=January; m7=July

Figure 3: Marginal effect of treatment by educational levels



Note: Plot based on model (6)

Table 1. The Effect of the Devaluation on Perceived Living Conditions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Panel A: Least Squares	*Dependent variable: Subjective evaluation of living conditions*										
Treatment	-0.16***	-0.14***	-0.09***	-0.12***	-0.16***	-0.13***	-0.16***	-0.15***	-0.12***	-0.14***	-0.08***
	(0.021)	(0.023)	(0.029)	(0.046)	(0.023)	(0.022)	(0.029)	(0.039)	(0.019)	(0.020)	(0.019)
Botswana economic condition									0.11***		0.09***
									(0.007)		(0.007)
Own past economic situation										0.08***	0.06***
										(0.007)	(0.008)
Male dummy											-0.02*
											(0.013)
Urban dummy		0.05***									0.00
		(0.016)									(0.015)
Gaborone dummy			0.12***								
			(0.038)								
Poverty											0.25***
											(0.038)
Age											-0.01***
											(0.002)
Age squared											0.00***
											(0.000)
District fixed effects	No	No	No	Yes	No	No	No	No	No	No	No
Tribal fixed effects	No	No	No	No	Yes	No	No	No	No	No	No
Occupational FE	No	No	No	No	No	Yes	No	No	No	No	No
Sample centred on discontinuity	No	No	No	No	No	No	Yes	Yes	No	No	No
Observations	1,198	1,198	1,198	1,198	1,198	1,198	375	216	1,152	1,188	1,113
R-squared	0.053	0.061	0.061	0.105	0.105	0.113	0.070	0.068	0.267	0.150	0.363
Panel B: Ordered logit											
Treatment	-1.07***	-0.89***	-0.62***	-0.91***	-1.12***	-0.91***	-0.97***	-0.97***	-0.93***	-0.97***	-0.65***
	(0.148)	(0.154)	(0.186)	(0.330)	(0.164)	(0.153)	(0.189)	(0.254)	(0.148)	(0.148)	(0.161)

Note. Treatment denotes the Pula devaluation. Days before devaluation are coded as 0; days after devaluations are coded as 1. All models contain a constant term (not reported to save space). Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 2. Robustness tests

	(1)	(2)	(3)	(4)
Panel A: Least Squares	*Dependent variable: Subjective evaluation of living conditions*			
Treatment	-0.09***	-0.09**	-0.15***	-0.14***
	(0.029)	(0.040)	(0.044)	(0.052)
Excluding Gaborone	Yes	Yes	No	No
Sample centered on discontinuity	No	Yes	No	Yes
Excluding urban respondents	No	No	Yes	Yes
Observations	1,063	176	679	128
R-squared	0.008	0.027	0.016	0.045
Panel B: Ordered logit				
Treatment	-0.63***	-0.64**	-1.06***	-0.89***
	(0.193)	(0.273)	(0.276)	(0.330)

Note. Treatment denotes the Pula devaluation. Days before devaluation are coded as 0; days after devaluations are coded as 1. All models contain a constant term (not reported to save space). Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 3. Information, Education, and the Effect of the Treatment

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Least squares						
Treatment	-0.08**	-0.05	-0.05	-0.08*	-0.07	-0.06
	(0.036)	(0.037)	(0.034)	(0.047)	(0.051)	(0.046)
Daily news consumption	0.16***	0.15***	0.13***			
	(0.041)	(0.041)	(0.039)			
Treatment-news interaction	-0.08*	-0.08*	-0.07*			
	(0.045)	(0.044)	(0.041)			
Education				0.04***	0.03***	0.03***
				(0.009)	(0.010)	(0.009)
Treatment-education interaction				-0.01	-0.01	-0.01
				(0.010)	(0.011)	(0.010)
Urban dummy		0.03*	0.02		0.02	0.02
		(0.017)	(0.016)		(0.017)	(0.016)
Own past economic situation			0.07***			0.07***
			(0.008)			(0.008)
Occupational fixed effects	No	Yes	Yes	No	Yes	Yes
Observations	1,196	1,196	1,186	1,194	1,194	1,184
R-squared	0.083	0.135	0.211	0.103	0.133	0.205
Panel B: Ordered logit						
Treatment	-0.57**	-0.34	-0.37	-0.56	-0.44	-0.36
	(0.232)	(0.250)	(0.252)	(0.341)	(0.374)	(0.353)
Daily news consumption	1.11***	1.03***	0.97***			
	(0.275)	(0.285)	(0.293)			
Treatment-news interaction	-0.54*	-0.56*	-0.48			
	(0.296)	(0.305)	(0.312)			
Education				0.28***	0.22***	0.20***
				(0.067)	(0.072)	(0.068)
Treatment-education interaction				-0.08	-0.08	-0.09
				(0.073)	(0.079)	(0.075)

Note. Treatment denotes the Pula devaluation. Days before devaluation are coded as 0; days after devaluations are coded as 1. All models contain a constant term (not reported to save space). Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.