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Abstract

This article 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, the article examines how this monetary shock affects individuals' evaluations of well-being. This is done by using microlevel survey data, which—incidentally—were collected in the days surrounding the devaluation. The chance occurrence of the devaluation during the time of the survey enables us to use pretreatment respondents, surveyed before the devaluation, as approximate counterfactuals for post-treatment respondents, surveyed after the devaluation. 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.

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

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Few tasks are more important in the social sciences than discovering the sources of human well-being. While this remains a contested issue (Frey and Stutzer 2000; Clark et al. 2008; Frey 2008; Bjørnskov et al. 2010; Deaton 2012), 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 banking and financial crises (Deaton 2012; Bjørnskov 2014; Montagnoli and Moro 2014) and macroeconomic factors like inflation, unemployment, and gross domestic product (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 article, 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 microlevel 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 devaluation creates a clear demarcation between respondents surveyed before the devaluation and respondents surveyed in the days following the devaluation.

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

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, that is, 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 article contributes to the broader literature on the determinants of individual happiness and well-being (Oswald 1997; Dolan et al. 2008; Frey 2008; Bjørnskov et al. 2010). It is also closely related to contributions linking macroeconomic variables like GDP and inflation to 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 macrolevel 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 article adds to the small literature using large-scale exogenous shocks to study changes in subjective well-being (e.g., Frankenberg et al. 2003; Frijters et al. 2004; Montagnoli and Moro 2014).

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.

I. Devaluation and Subjective Well-Being

The response of individuals to the news of a devaluation might depend on at least two different mechanisms—price responses and a signaling mechanism.

Price Responses and Expectations

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, for example, South Africa (Rakotoarisao et al. 2011), the economic costs of the currency devaluation mainly accrued to consumers, at least in the short 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 price effects are likely to affect individuals and households to approximately the same extent. However, household reactions to the shock can differ substantially, as documented by Frankenberg et al. (2003). In particular, one might expect that households directly engaged in the production of near-substitutes to imports may benefit in the medium-run, as demand patterns react

to the changing relative prices. Conversely, all households would be harmed by a general drop in aggregate demand and an increase in uncertainty, making it difficult from a theoretical angle to make any systematically heterogeneous predictions (see Montagnoli and Moro 2014).

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). If prices of imported goods increase, price changes will take place almost instantly. Changes in economic expectations can therefore occur very rapidly given that individuals rely on consumption of imported final goods. If the general price level increases, 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. The extent to which people form and internalize expectations of how the economy is likely to develop in the longer run also depends on their cognitive sophistication, as well as information obtained from past experiences with similar policy shocks.

Signaling and Uncertainty

Another type of mechanism may also affect individuals' well-being. As stressed by Graham (2011), well-being is not only affected by individuals' current status and expectations of the immediate future but also their perceived uncertainty. A devaluation announcement might therefore have two additional effects.

First, relatively well-informed individuals are probably able to assess the direction of the price effects that we described above but may only have a vague idea about their magnitude. Policy changes with complex consequences, such as devaluations, may thus release a perceived demand for insurance of some kind, which in all forms must reduce current consumption possibilities. Whether this demand can be covered in actual insurance markets is questionable in middle-income countries. A likely consequence of a perceived and unanticipated increase in uncertainty is therefore likely to be an increase in either current savings or changed savings behavior in the near future. In either case, expected consumption and economic welfare is likely to decrease (Graham 2011).

Second, the announcement of a devaluation can easily be taken as a signal that the economy moving in a direction that is inconsistent with individuals' prior information. With limited information on the state of the domestic economy and less knowledge and information about that of major trading partners and the general world economy, governments' policy decisions may work as signals of the direction of economic change. Changes such as devaluations can therefore be perceived as signals of future economic slowdown—particularly by more well-informed citizens—that induce changes to expectations and financial plans.

These nontechnical 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 and signals from government policy changes may be complex, 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.

II. 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, May 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

[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. (2005) for descriptions of the *Afrobarometer*, and Mattes (2007) for a discussion of survey research in developing countries.

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), 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 nonequivalent—at the outset. This means that even a macroeconomic shock, for example, 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 are 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. Overall, 216

[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).

respondents—corresponding to 18 percent of the sample—were surveyed before the devaluation (the control group), while 984 respondents were surveyed after (the treatment group). However, 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, for example, 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 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, for example, 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

[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.

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

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 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. In Appendix S1, we provide further tests of the equivalence of the treatment and control groups on socio-economic background variables. Appendix S2 and S3 also shows results from regression and matching for observations that are on common support on the propensity score. These results do not change the main conclusions below.

III. 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

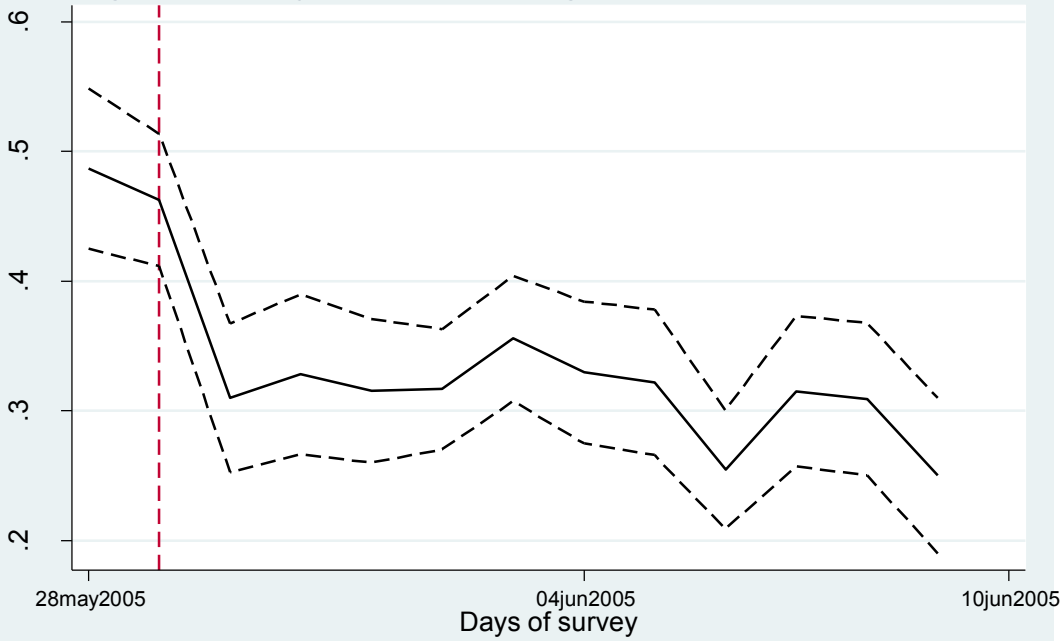
because it illustrates the most plausible mechanism connecting the currency devaluation to individuals' evaluations of well-being.

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" (Deaton 2008, 2012; Bjørnskov et al. 2010; 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.[6] We therefore use this question to measure subjective well-being.[7]

[6] We note that although the wording is not identical to most surveys asking about the satisfaction with life as a whole, the two questions tend to produce quite similar results. Using the 2011 wave of the World Values Survey in Ghana, we note the similarity between the regular life satisfaction question and a question specifically on satisfaction with one's financial situation. Less than 5 percent of respondents who declare themselves satisfied with their financial situation (rating it 8–10 on a 1–10 scale) declare themselves unsatisfied with their life as a whole.

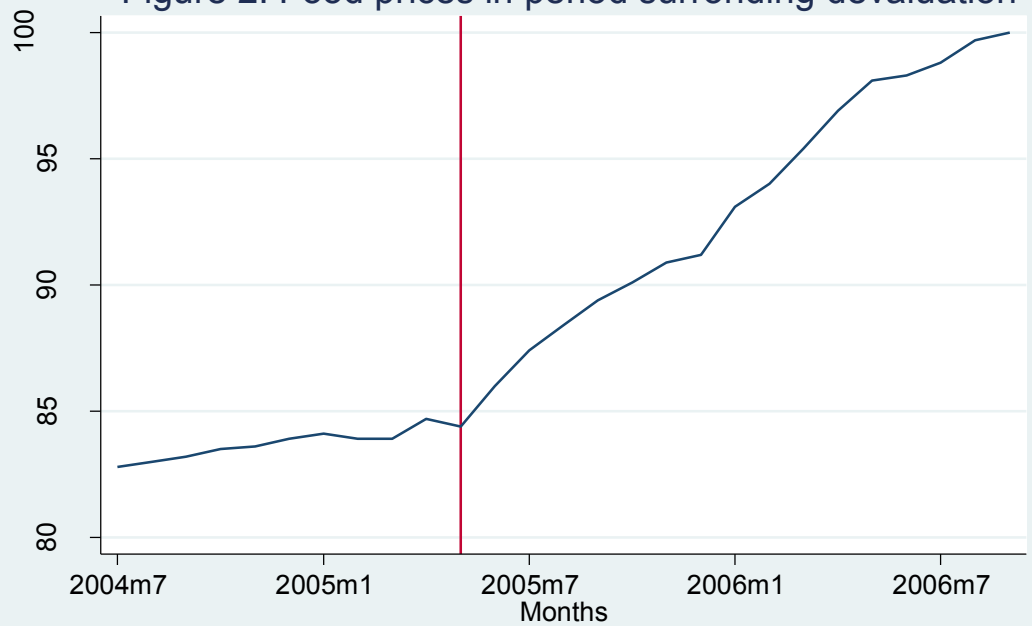
[7] The *Afrobarometer* also contains a related question, asking respondents to evaluate their living conditions relative to other people. Replications using this variable—evaluations of relative living conditions—does not change our findings substantially or statistically. Detailed results are available upon request. We do not think the two variables are sufficiently distinct to treat them as alternative measures, partly because questions asking people to rate their situation relative to others may pick up absolute and not relative differences (Karadja et al. 2014), and partly because the two living conditions questions are asked immediately after each other, which may make responses quite similar.

Figure 1: Subjective well-being around time of devaluation



Vertical line shows timing of devaluation. Dashed curves are 95%-confidence bounds.

Figure 2: Food prices in period surronding devaluation



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

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 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

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

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 pretreatment 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 Appendix S4.

IV. Empirical Results

To estimate the effect of the currency devaluation on subjective well-being, our econometric analyses use models for continuous and categorical data. First, we 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.[9] 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 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

[9] 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.

X is respondents' rural-urban status. Throughout, standard errors are regionally clustered to allow for arbitrary correlation among respondents living in the same region. Table 1 shows the results.

Main Results

Panel A in Table 1 show results obtained using OLS 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.

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***	-0.07***
	(0.035)	(0.035)	(0.028)	(0.025)	(0.032)	(0.027)	(0.042)	(0.059)	(0.028)	(0.030)	(0.025)
Botswana economic condition									0.11***		0.09***
									(0.012)		(0.013)
Own past economic situation										0.08***	0.05***
										(0.007)	(0.008)
Male dummy											-0.03*
											(0.012)
Urban dummy		0.05***									0.00
		(0.022)									(0.015)
Gaborone dummy			0.12***								
			(0.026)								
Poverty											0.25***
											(0.024)
Age											-0.01*
											(0.002)
Age squared											0.00*
											(0.000)
Unemployment											-0.02*
											(0.012)
No children											0.02
											(0.029)
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	1,109
R-squared	0.053	0.061	0.061	0.105	0.105	0.113	0.070	0.068	0.267	0.150	0.362
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***
	(0.206)	(0.207)	(0.167)	(0.212)	(0.202)	(0.158)	(0.230)	(0.314)	(0.174)	(0.178)	(0.195)

Source: Authors' analysis based on data sources discussed in text. 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 clustered at the region level in parentheses. *** $p < .01$, ** $p < .05$, * $p < .1$.

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.[10]

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.

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

[10] 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.

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.[11] 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.

Tackling Nonrandom Treatment Assignment

As mentioned above, there are systematic differences between pretreatment 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 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 (or rural) areas in general from the sample.[12]

[11] The poverty index is based on the work of Bratton et al. (2005) 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 2014). 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).

[12] 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).

Table 2. Robustness Tests

	(1)	(2)	(3)	(4)	(5)
Panel A: Least Squares	*Dependent variable: Subjective evaluation of living conditions*				
Treatment	-0.09***	-0.09**	-0.15***	-0.14**	-0.13**
	(0.028)	(0.02)	(0.024)	(0.021)	(0.44)
Excluding Gaborone	Yes	Yes	No	No	No
Sample centered on discontinuity	No	Yes	No	Yes	No
Excluding urban respondents	No	No	Yes	Yes	No
Excluding rural respondents	No	No	No	No	Yes
Observations	1,063	176	679	128	519
R-squared	0.008	0.027	0.016	0.045	0.051
Panel B: Ordered logit					
Treatment	-0.63***	-0.64**	-1.06***	-0.89***	-0.61*
	(0.173)	(0.091)	(0.129)	(0.114)	(0.317)

Source: Authors' analysis based on data sources discussed in text. 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 clustered at the region level in parentheses. *** $p < .01$, ** $p < .05$, * $p < .1$.

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 pretreatment 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.

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.

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. Column (5), finally, omits all rural respondents, focusing only on respondents from urban areas. This also leaves conclusions unchanged. For all model specifications, we find very similar results using ordered logit instead of OLS (as reported in Panel B).

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 Appendix S5, 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.[13] 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

[13] 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.

respondents. We did similar placebo tests using as treatment the first two days of the survey from Round 4 (Appendix S5). This reveals that in Round 4 there was no discontinuity in respondents' evaluations of living conditions after two days of surveying.

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 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 informed respondents as those who report getting daily news from the radio, television, or newspapers (coded 1). News consumption must be on a daily basis to moderate 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 S4 for details).

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**
	(0.025)	(0.029)	(0.027)	(0.023)	(0.020)	(0.026)
Daily news consumption	0.16***	0.15***	0.13***			
	(0.007)	(0.014)	(0.015)			
Treatment-news interaction	-0.08***	-0.08***	-0.07**			
	(0.018)	(0.024)	(0.023)			
Education				0.04***	0.03***	0.03***
				(0.003)	(0.003)	(0.005)
Treatment-education interaction				-0.01**	-0.01**	-0.01*
				(0.005)	(0.004)	(0.005)
Urban dummy		0.03	0.02		0.02	0.02
		(0.021)	(0.019)		(0.020)	(0.019)
Own past economic situation			0.07***			0.07***
			(0.006)			(0.006)
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*
	(0.164)	(0.203)	(0.203)	(0.157)	(0.133)	(0.189)
Daily news consumption	1.11***	1.03***	0.97***			
	(0.082)	(0.100)	(0.101)			
Treatment-news interaction	-0.54***	-0.56***	-0.48**			
	(0.174)	(0.203)	(0.193)			
Education				0.28***	0.22***	0.20***
				(0.022)	(0.017)	(0.038)
Treatment-education interaction				-0.08**	-0.08***	-0.09**
				(0.035)	(0.026)	(0.044)

Source: Authors' analysis based on data sources discussed in text. 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 clustered at the region level in parentheses. *** $p < .01$, ** $p < .05$, * $p < .1$.

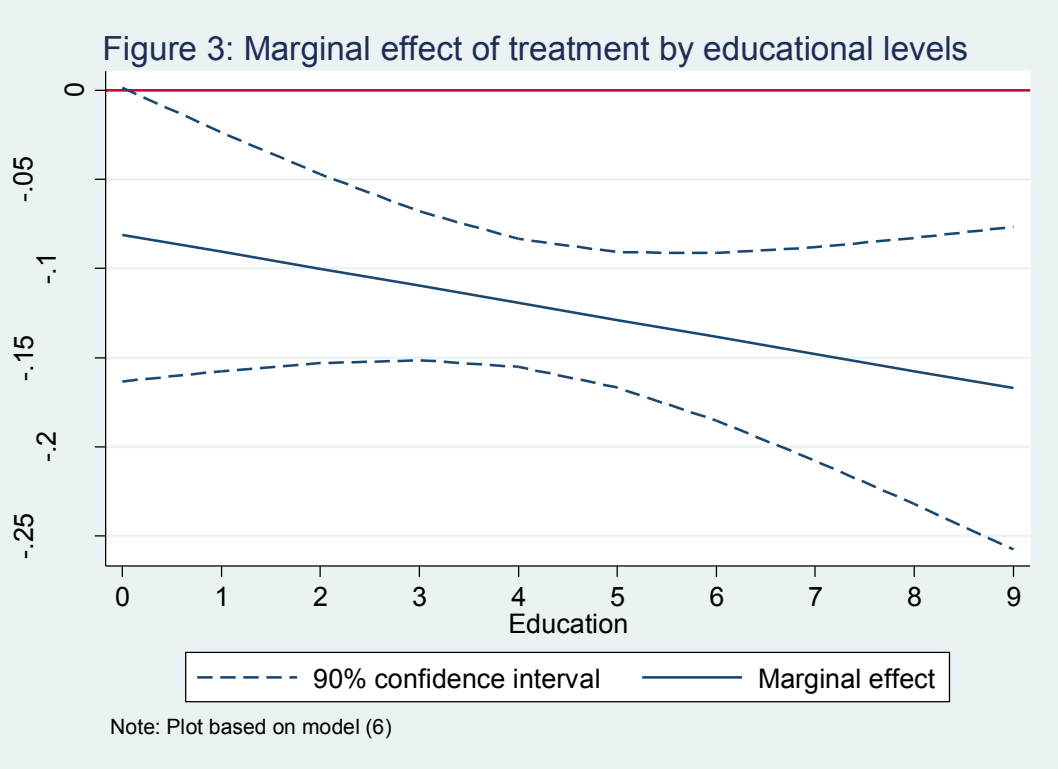
To examine whether information and cognitive sophistication condition the relationship between the currency devaluation and subjective well-being, we augment the regression model (1) with, first, an interaction of the treatment indicator and our measure of information and, second, an interaction of the 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.

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 that individuals with higher levels of information more quickly update their perceptions of well-being.[14]

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

[14] The devaluation might plausibly affect rich and poor individuals differentially. Within occupational groups, however, there are no signs of a heterogenous treatment effect between rich and poor (results available on request).

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.



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.

V. Conclusions

This article 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 of 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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Appendix

Appendix S1: Modelling nonrandom selection into treatment

A potential worry in our design is that the control and treatment groups are dissimilar not only in terms of urban/rural difference but also on other relevant characteristics. While we discuss this in the main text, Table S1 provides an additional test of the extent to which the control and treatment groups are balanced. Specifically, in Table S1 we use treatment assignment as the dependent variable in a logit regression (treatment=1; control=0). The covariates used to test treatment-control imbalances are similar to those used in Table 1, plus education. This regression therefore specifies a selection equation (Robinson et al. 2009, 350) that enables us to examine potential differences between the treatment and control groups on a series of pre-determined socio-economic variables. The results in Table S1 show that there are few significant differences between the treatment and control groups. The data are somewhat imbalanced in terms of urban/rural differences, poverty (albeit only with $p<0.1$), and unemployment. However, in terms of gender, age, having children, education, and perceptions of the country's and one's own economic situation, there are no significant differences between the treatment and control groups. These results provide evidence in favor of the quasi-experimental nature of our design.

Table S1. Modelling selection into treatment

Model	(1)
Dependent variable	Treatment status
Urban	-2.13*
	(1.149)
Male	0.03
	(0.041)
Poverty	-1.52*
	(0.901)
Age	0.01
	(0.013)
Age squared	0.00
	(0.000)
Unemployment	0.68**
	(0.312)
No children	0.49
	(0.334)
Education	0.01
	(0.051)
Botswana economic condition	-0.14
	(0.147)
Own past economic situation	-0.09
	(0.079)
Constant	3.80***
	(1.093)
Observations	1,106

Source: Authors' analysis based on data sources discussed in text. Dependent variable is treatment indicator: 1=treated; 0=controls. Results obtained using logit regression. Coefficients are log(odds) with robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Appendix S2: Regressions with common support on the propensity score

To further test the robustness of the results in Table 1, Table S2 replicates the results from Table 1 in regressions where only observations on common support are included. The results in Table S2 are obtained in two steps. In the first step, we use the logit model from Table S1 above to obtain propensity scores, i.e. the predicted probability of being treated, $Pr(T=1)|\mathbf{X}$, given the covariates, **X**, in the model. The propensity score is used to define the interval of common support, i.e. the interval on the propensity score where there is overlap between observations in the treatment and control groups (Morgan and Winship 2007; Persson and Tabellini 2003). This ensures that for every treated observation, there are comparable observations in the control groups, which should increase the homogeneity of observations in the two groups (Ho et al. 2007). Specifically, we follow Persson and Tabellini (2003, 143) and define common support as the interval between the minimum propensity score (*ps*) for the treated (*min* T=1) and the maximum propensity score for the controls (*max* T=0). In our case, this interval is given by $0.375 \leq ps \leq 0.975$. In the second step, we replicate the results from Table 1 in regressions that are restricted to observations on common support. Hence, in Table S2 observations that are outside the interval of common support are discarded, meaning that the number of observations drop compared to Table 1. However, while the coefficients in Table S2 are slightly lower than in Table 1, the general pattern is similar and corroborates that the devaluation tends to decrease subjective assessments of well-being.

Table S2: Replication of Table 1: Observations on common support

Model	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Dependent variable	*Subjective evaluation of living conditions*										
Treatment	-0.13***	-0.12***	-0.05*	-0.03	-0.13***	-0.10***	-0.15**	-0.09	-0.10***	-0.11***	-0.07**
	(0.037)	(0.038)	(0.025)	(0.045)	(0.038)	(0.028)	(0.043)	(0.066)	(0.030)	(0.032)	(0.025)
Botswana economic condition									0.10***		0.09***
									(0.015)		(0.013)
Own past economic situation										0.08***	0.05***
										(0.011)	(0.012)
Male dummy											-0.03*
											(0.015)
Urban dummy		0.01									0.00
		(0.022)									(0.020)
Gaborone dummy			0.12***								
			(0.023)								
Poverty											0.30***
											(0.025)
Age											-0.01**
											(0.003)
Age squared											0.00**
											(0.000)
Unemployment											-0.03**
											(0.013)
No children											0.03
											(0.033)
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	958	958	958	958	958	958	362	185	958	958	958
R-squared	0.037	0.037	0.047	0.085	0.086	0.094	0.060	0.023	0.209	0.110	0.310

Source: Authors' analysis based on data sources discussed in text. 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 clustered at the region level in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Appendix S3: Matching

As a final test of the robustness of our results, Table S3 report results where we use nearest neighbor matching rather than the OLS estimator. Specifically, we use the nearest neighbor matching algorithm developed by Abadie et al. (2004). That is, every observation in the treatment group is matched to its closest twin in the control group, and based on these matched comparisons, the average treatment effect (for the treated) is calculated (cf. Justesen 2012). We use this algorithm to calculate the effect of the devaluation shock (models 1-3) and for observations on common support only (models 4-6). The covariates used for matching are those shown in Table S1. Models 1 and 4 use one matched control per treated observation; models 2 and 5 use two matches per treated observation; and models 3 and 6 use three matches per observation in the treatment group to calculate the treatment effect. However, regardless of the number of matched observations and whether we restrict the data to observations on or off common, the results are quite clear: The devaluation has a significantly negative effect on people's subjective well-being.

Table S3. The Effect of the Devaluation on Perceived Living Conditions: Matching Estimates

Model	*(1)*	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
Outcome variable	*Subjective evaluation of living conditions*					
Treatment (ATT)	-0.093***	-.091***	-0.089***	-0.076**	-.0748***	-0.073***
	(2.64)	(2.92)	(3.53)	(2.22)	(2.70)	(2.87)
Matches per observation	1	2	3	1	2	3
Common support definition	All observations	All observations	All observations	min(T=1), max(T=0)	min(T=1), max(T=0)	min(T=1), max(T=0)
Region of common support	-	-	-	0.375≤*ps* ≤0.975	0.375≤*ps* ≤0.975	0.375≤*ps* ≤0.975
# Observations	1105	1105	1105	1105	1105	1105
# Obs. on common support	-	-	-	958	958	958

Source: Authors' analysis based on data sources discussed in text. Reported estimates are average effect of treatment on the treated (ATT), obtained using nearest-neighbour matching with the inverse of the sample variance as weighting matrix. Matching is done on covariates shown in Table A.4. Estimates are obtained in Stata 12 using the *nnmatch* command developed by Abadie et al. (2004). Matches per observation = number of matched controls per treated observation. Observations in control group are allowed to be matched to more than one observation in the treatment group (matching with replacement). Region of common support is defined as: $0.375 \leq pr \leq 0.975$, where ps=propensity score. Absolute values of heteroskedasticity-consistent z-statistics are shown in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Appendix S4: Descriptive statistics

Table S4. Variable descriptions and summary statistics

Variables	**Survey question and variable coding**	**Data source**	**Mean**	**Std.dev.**	**Min.**	**Max.**	**Obs.**
Treatment (currency devaluation)	Indicator variable that takes the value 0 for survey responses before the devaluation (May 28 – May 29, 2005) and 1 for survey responses after the devaluation (May 30 – June 9, 2005).	Authors' coding	0.82	0.38	0	1	1200
Evaluation of living conditions (subjective well-being)	Respondents' evaluation of their own present living conditions. Question Q4B: *In general, how would you describe: Your own present living conditions?* Answers given in categories: 'very bad', 'fairly bad', 'neither', 'fairly good', and 'very good'. Coded categorically on scale from 0 to 1.	Afrobarometer, round 3	0.34	0.27	0	1	1198
Evaluation of living conditions II (subjective well-being) This variable is used for robustness tests. Results are reported in tables below in this appendix.	Respondents' evaluation of their own present living conditions. Question Q5: *In general, how would you rate: Your living conditions compared to those other Batswana?* Answers given in categories: 'much worse', 'worse', 'same', 'better', and 'much better'. Coded categorically on scale from 0 to 1.	Afrobarometer, round 3	0.40	0.25	0	1	1172
Urban or rural residence of respondent	Urban or rural primary sampling unit (variable *urbrur*). Coded as 1 for respondents in urban areas, and 0 for respondents in rural areas. Answered by interviewer.	Afrobarometer, round 3	0.43	0.50	0	1	1200
Gaborone indicator	Indicator variable coded as 1 if respondent resides in Gaborone area, and 0 otherwise.	Afrobarometer, round 3	0.11	0.32	0	1	1200
Region fixed effects	Binary indicator variables for the 15 geographical regions registered	Afrobarometer, round 3	*				

	by the Afrobarometer in Botswana. Answered by interviewer.						
Tribal fixed effects	Binary indicator variables for the 23 tribal/ethnic groups registered by the Afrobarometer in Botswana. Question Q79: *What is your tribe? You know, your ethnic or cultural group*.	Afrobarometer, round 3	*				
Occupational fixed effects	Binary indicator variables for the 25 occupational categories registered by the Afrobarometer in Botswana. Question Q95: *What is your main occupation?*	Afrobarometer, round 3	*				
Evaluation of country's present economic conditions	Respondents' evaluation of the country's present economic conditions. Question Q4B: *In general, how would you describe: Your own present economic conditions of this country?* Answers given in categories: 'very bad', 'fairly bad', 'neither', 'fairly good', and 'very good'. Coded categorically on scale from 1 to 5.	Afrobarometer, round 3	2.80	1.17	1	5	1153
Evaluation of past living conditions	Respondents' evaluation of own living conditions compared to 12 month ago. Question Q6B: *Looking back, how do you rate the following compared to twelve months ago: Your living conditions?* Answers given in categories: 'much worse', 'worse', 'same', 'better', and 'much better'. Coded categorically on scale from 1 to 5.	Afrobarometer, round 3	2.90	0.99	1	5	1190
Gender	Question Q101: Respondents gender. Q101. Binary (0-1), with 1=male	Afrobarometer, round 3	0.50	0.50	0	1	1200
Poverty.	Index based on following questions	Afrobarometer, round 3	0.77	0.21	0	1	1179

	(Q8A-Q8E): *Over the past year, how often, if ever, have you or anyone in your family gone without: a) Enough food to eat; b) enough clean water for home use; c) medicines or medical treatment; d) enough fuel to cook your food; e) a cash income?* Each question is answered on a five-point scale from 'never' to 'always'. The index is the sum of all five items recoded to scale from 0–1, where high values indicate wealth/no poverty and low values indicate severe poverty (i.e. frequent or permanent lack of basic household necessities).						
Age	Respondent's age in years. Question Q1. *How old are you?*	Afrobarometer, round 3	37.6	16.4	18	99	1187
Age squared	Square of variable Q1	Afrobarometer, round 3	1681.2	1572.2	324	9801	1187
Education: Proxy for cognitive sophistication	Question Q90: *What is the highest level of education you have completed?* Answers given on 10-point scale from 0 (no formal schooling) to 9 (post-graduate degree).	Afrobarometer, round 3	3.42	2.02	0	9	1196
News consumption: Proxy for information	Binary indicator variable based on questions Q15A-Q15C: *How often do you get news from the following sources: Radio (Q15A); Televison (Q15B); Newspapers (Q15C)?* Variable is coded as 1 (informed) if respondents get news from either the radio, television, or newspapers on a daily basis; and 0 otherwise.	Afrobarometer, round 3	0.54	0.50	0	1	1198

Source: Data sources are listed in table. * Summary statistics not reported due to large number of categories on region, tribal, and occupational fixed effects.

Appendix S5: Placebo tests

Table S5. Placebo tests using Gaborone as treatment indicator in *Afrobarometer* Round 4

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Least Squares	*Dependent variable: Subjective evaluation of living conditions*					
Gaborone dummy	-0.07***	-0.07***	-0.05	-0.06***	-0.06***	0.02
	(0.015)	(0.012)	(0.031)	(0.015)	(0.012)	(0.008)
Botswana economic situation					0.08***	0.06***
					(0.009)	(0.008)
Own past economic situation				0.06***		0.03***
				(0.010)		(0.009)
Age						-0.00***
						(0.000)
Age squared						0.00***
						(0.000)
Male dummy						-0.01
						(0.017)
Urban dummy						-0.01
						(0.017)
Poverty						-0.42***
						(0.025)
Tribal fixed effects	No	Yes	No	No	No	No
Interview day FE	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,199	1,199	1,199	1,198	1,168	1,149
R-squared	0.01	0.06	0.06	0.05	0.11	0.27
Panel B: Ordered Logit						
Gaborone dummy	-0.47***	-0.50***	-0.39*	-0.42***	-0.44***	0.13
	(0.103)	(0.088)	(0.229)	(0.101)	(0.086)	(0.064)
Panel C: Least Squares						
Dummy for survey day 1-2	0.03	0.02	0.21***	0.03	0.01	0.01
	(0.033)	(0.023)	(0.055)	(0.031)	(0.028)	(0.015)
Panel D: Ordered Logit						
Dummy for survey day 1-2	0.18	0.16	0.91**	0.17	0.02	0.05
	(0.226)	(0.174)	(0.383)	(0.216)	(0.211)	(0.122)

Source: Data are from the round 4 of the *Afrobarometer*. All models contain a constant term (not reported to save space). Robust standard errors clustered at the region level in parentheses. *** p<0.01, ** p<0.05, * p<0.1