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Sentimental Business Cycles

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We estimate the dynamic causal effects of consumer sentiment shocks in the US. We identify autonomous changes in survey evidence on consumer confidence using fatalities in mass shootings as an instrument. We find the instrument to be significant for an aggregate index of consumer expectations and also back up the identification scheme with micro evidence that exploits the geographical variation in mass shootings. Sentiment shocks have real macroeconomic effects. A negative sentiment shock is recessionary: It sets off a persistent decline in consumer confidence and induces a contraction in industrial production, private sector consumption and in the labour market, while having less evident nominal effects. Finally, sentiment shocks explain a non-negligible part of the cyclical fluctuations in consumer confidence and real macroeconomic aggregates.

Key words: Consumer confidence; Instrumental variables; Dynamic causal effects of consumer sentiments.

JEL Codes: C36, E0, E32

1. INTRODUCTION

An extensive empirical literature in macroeconomics has investigated the sources of impulses to the business cycle. The large majority of papers on this topic has provided causal evidence on the impact of shocks related to economic fundamentals, such as monetary and fiscal policy shocks, technology and investment-specific shocks, oil price shocks, among others (see the comprehensive survey of Ramey, 2016). However, under a variety of conditions, the economy may also be affected by shocks unrelated to economic fundamentals, such as expectational errors or "animal spirits", but there is very little—if any—*direct* evidence on the impact of such shocks on the aggregate economy. This article contributes to the literature by providing empirical estimates of the causal effects of autonomous changes in consumer confidence, changes that we will refer to as consumer "sentiment shocks". We find that deteriorating consumer confidence due to sentiment shocks is recessionary, especially in terms of its impact on the labour market, and that sentiment shocks account for a non-negligible part of cyclical fluctuations in the economy.

The editor in charge of this paper was Kurt Mitman.

The central challenge to estimating the causal effects of shocks that are unrelated to fundamentals is the translation of such shocks into functions of observables. We address this issue by extracting an autonomous component from consumer confidence data based on survey evidence on household expectations about the future outlook for the economy. The idea is that such consumer confidence data may reflect both households' views on the economy's future path based on their knowledge about economic fundamentals *and* an autonomous component unrelated to fundamentals, "consumer sentiments". We argue that one can extract this latter component by means of an instrumental variables (IV) approach. In particular, we adopt an instrument that reflects news about events that are arguably unrelated to economic fundamentals, but which we show impact on consumer confidence. This allows us to identify the consumer sentiment shocks and estimate their causal effects.

Operationally, we follow an extensive literature that has examined the Index of Consumer Expectations (ICE) produced by the University of Michigan in its Survey of Consumers. The ICE summarizes, at a monthly frequency, an aggregation of around 500 survey respondents' expectations regarding the future outlook for their own personal finances and for the overall US economy. The ICE is attractive for our purposes because it is available for a long sample period, allowing us to study the extent to which the identified consumer sentiment shock has an impact on the wider economy.

The instrument for sentiment shocks that we propose is the number of fatalities in mass shootings in the US. The idea of the instrument is that these tragic events, while not having any direct macroeconomic costs, may impact on consumer sentiments through the general populations' psychological well-being and via this channel affect the broader economy. Validity of the instrument hinges on the survey respondents being aware of mass shootings. Supporting this, mass shootings receive considerable press coverage and therefore constitute a source of bad news to a broad cross-section of the US population. It is well-established in the literature that mass shootings can impact on psychological well-being despite individuals not directly being under threat themselves, see e.g. Hughes, Brymer, Chiu, Fairbank, Jones, Pynoos, Rothwell, Steinberg and Kessler (2011) or Clark and Stancanelli (2017) and, more recently, Soni and Tekin (2020). The idea that we pursue is that such psychological effects may also show up in survey evidence on consumer confidence and thereby allow us to identify the sentiment shocks. Finally, mass shootings occur, unfortunately, with a regular frequency in the US implying that these events offer an instrument for which there are more than just a few treatments over the sample period that we examine.

We focus on mass shootings with seven or more fatalities that occurred in a public space and were unrelated to gang crime and common place circumstances (e.g. armed robbery, criminal competition, insurance fraud, argument, or romantic triangle). The reason we select more severe mass shootings is to increase the likelihood that the bad news from such events has reached a broad cross section of the population. We exclude shootings that took place purely in a private space or were related to gang crimes and common place circumstances because these events are less likely to be seen as a threat to those not directly involved. In order to have a good quality instrument, we provide a new database on mass shootings, which we build on the basis of information gathered from three existing sources: Duwe (2007), MotherJones (2020), and The-Violence-Project (2019). Our database uses a consistent definition of mass shootings across events and covers the sample period January 1965 to November 2018. During this 54-year span, there were no less than 581 fatalities in such shootings stemming from 42 separate events, with the most lethal one being the 2017 Las Vegas Strip massacre (58 fatalities), and other notorious incidents include the Columbine High School massacre in April 1999 (13 fatalities) and the Virginia Tech massacre in April 2007 (32 fatalities). Notably, the frequency and severity in terms of victims have both increased over time; almost one-fifth of mass shootings (that resulted in over 30% of total fatalities) occurred in the last 3 years of the sample.

In order to estimate the dynamic causal effects of sentiment shocks, we adopt the Mertens and Ravn (2013) proxy SVAR estimator. We also examine the results when using a Local Projection IV (LP-IV) approach. These estimators enable us to study how autonomous changes in consumer sentiments affect macroeconomic outcomes under two key identifying assumptions that the instrument (i) impacts on the ICE and (ii) is unrelated to other structural shocks. In line with the exogeneity assumption, there is no compelling evidence that mass shootings are triggered by economic circumstances, see e.g. Pappa, Lagerborg and Ravn (2019).

We study monthly data and initially focus on the sample period spanning 1965:1 to 2007:8, which excludes the period when shootings become very frequent as well as the Great Recession. However, results are shown to be robust to extending the sample to 2018:11, although the instrument becomes weaker as we add data for the last part of the sample. Our benchmark VAR consists of the following seven variables: the ICE, industrial production, the unemployment rate, the consumer price level, the short-term nominal interest rate, a measure of macroeconomic uncertainty and real stock market prices. However, we also look at other outcome variables such as private sector consumption, a broader selection of labour market indicators, as well as productivity.

We show that the number of fatalities in mass shootings is a significant instrument for the ICE and use it to extract the sentiment shock. According to our estimates, after a negative sentiment shock, consumer confidence declines persistently, and significantly so, for around 12–15 months. Importantly, we find that autonomous changes in consumer confidence affect the wider economy. Deteriorating consumer confidence has a recessionary impact reflected in declining aggregate activity as measured by industrial production. The decline in consumer confidence is also accompanied by worsening labour market outcomes as shown by a rise in the civilian unemployment rate, which remains significantly elevated for around 2 years, and by reductions in labour market tightness and in firms' vacancy postings. We also find that a fall in consumer confidence induces a reduction in private sector consumption of both non-durable and durable goods. Furthermore, our estimates indicate that negative consumer sentiment shocks are accompanied by a decline in the short-term nominal interest rate, which points towards some leaning against the wind on the part of monetary policy. Perhaps because of this, we find only a small and short-lived effect on the consumer price index (CPI). Stock prices drop (but mostly insignificantly) after the shock, while total factor productivity (TFP) does not react significantly to the shock in sentiments at any time horizon. Macroeconomic uncertainty rises on impact when measured by Jurado, Ludvigson and Ng (2015) index, while no impact is found on other indicators such as the VIX and economic policy uncertainty measures. We show that our results remain robust when considering, as an alternative instrument, a dummy indicator for mass shooting events that resulted in at least seven fatal victims (instead of the total number of fatalities) and also present a placebo exercise to illustrate the workings of our identification approach.

Since the Proxy SVAR estimator may potentially be subject to non-invertibility concerns, we also estimate the dynamic causal effects by employing a local projection instrumental variable (LP-IV) estimator. The impulse responses based on this estimator are shown to be very similar to those generated by the Proxy-SVAR estimator. Using the forecast variance ratio (FVR) statistic proposed by Plagborg-Møller and Wolf (2019), we find that confidence shocks explain a significant fraction of cyclical fluctuations in consumer expectations, labour market indicators, and industrial production, while they appear to be less relevant for variations in asset markets and in inflation. In particular, the point estimates of the upper bounds of the identified set of the FVR of the ICE fluctuate around 20% at most forecast horizons and goes to 30–40% at forecast horizons below 1 year. At the 90% level, we can only reject that sentiment shocks account for more than 30–35% of the FVR of the ICE at forecast horizons beyond 2 years, and 55% at the 6 months horizon. The point estimates of the upper bounds of the identified set of the FVR of industrial production depend on the forecast horizon, but reach (or go above) 20% for the 6 months to 1 year horizon and after 2 and a half years. On the basis of the 90% confidence intervals for the identified set for those horizons, we can rule out a contribution of consumer sentiment shocks to the FVR of this variable above 30–40%. At forecast horizons below 6 months, the contribution of sentiment shocks to the FVR of industrial production is instead estimated to be low. As regards unemployment, the point estimates of the upper bounds of the identified set indicate than sentiment shocks explain around 20–30% of the FVR during horizons between 6 months and 1 year, and 15–20% for forecast horizons above 1 year.[1]

We also provide micro evidence that backs up the idea about mass shootings impacting on consumer confidence. Under our hypothesis that mass shootings influence consumer confidence, one might expect these events to impact particularly strongly on respondents who reside close to where they occur. To investigate this, we draw on individual-level responses to the University of Michigan's Survey of Consumers and map mass shootings to US counties. We estimate the impact of local mass shootings on *individual* consumer sentiments when controlling for aggregate US conditions through time fixed effects. We show that mass shootings lead to a deterioration in individuals' sentiments about the outlook for the aggregate US economy for individuals living in the county where they occur (relative to outlook expectations of individuals residing in other counties), even after controlling for respondent fixed effects, reported changes in individual financial conditions and changes in county-level unemployment. The micro evidence also allows us to further back up the exogeneity assumption. In particular, we show that local unemployment conditions have no predictive power for mass shootings.

Our work is related to studies that have estimated the impact of sentiment shocks using either cross-sectional or panel data. Mian, Sufi and Khoshkhou (2015) also study the University of Michigan Survey of Consumers and identify sentiment shocks at the county level, using the outcome of presidential elections interacted with ideological predisposition as an instrument but find limited impact of the identified shock on consumer spending. In contrast, studying state-level data but also using political outcomes as an instrument, Benhabib and Spiegel (2019) find a large and positive effect of sentiment shocks on the level of economic activity of the state. Nearest to our article, due to similarity of the instrument and estimation approach, Lagerborg (2017) examines individual- and county-level data using school shootings as an instrument for consumer confidence and finds that declining consumer sentiments reduce individuals' appetite for consuming durable goods and induce higher unemployment at the county level.

A number of previous papers have also considered the impact of sentiment shocks at the aggregate level. Barsky and Sims (2012) also study data from the University of Michigan Survey of Consumers and use a timing approach embedded in a VAR set-up to estimate the innovation to consumer confidence. They then construct a DSGE model and conclude that much of the variance of this innovation reflects responses of consumer confidence to news shocks about TFP growth. Similarly, Fève and Guay (2019) adopt a VAR framework and identify sentiment shocks from assumptions about their (lack of) correlation with their measured fundamental shocks and the contribution to the variance of consumer confidence. They find that sentiment shocks contribute little to business cycle fluctuations. In contrast, using a structural approach where information on confidence from the Survey of Professional

1. The lower bounds of the identified sets of the FVR for all the variables that we inspect tend to be very small. The lower bounds correspond to the case in which the instrument is perfect (i.e. when it is identical to the unobservable structural shock). Given the outcome of weak instrument tests we report, we do not expect the instrument to be perfectly correlated with the unobserved structural shock (because of both the nature of the instrument and measurement errors). Hence, the lower bounds of the identified sets underestimate the importance of the consumer sentiment shock due to attenuation bias.

Forecasters is included in a structural estimation of a DSGE model, Faccini and Melosi (2019) find that sentiment shocks are important drivers of boom–bust cycles. Similarly, Lorenzoni (2009), Beaudry, Nam and Wang (2011), Forni, Gambetti, Lippi and Sala (2017), Chahrour and Jurado (2018), and Enders, Kleemann and Muller (2020) conclude in favour of considerable impact of noise and sentiment shocks on aggregate outcomes using a variety of approaches.

Our analysis differs from these papers in the use of an external instrument for sentiment shocks and, therefore, in our attempt to provide direct evidence on the causal effects of this type of shock without hard wiring the identification to a specific DSGE model. Our article comes closer in nature to papers that have adopted external instruments for the identification of other shocks such as Mertens and Ravn (2013) who estimate the impact of tax shocks using a tax narrative as an external instrument, Gertler and Karadi (2015) who estimate monetary policy shocks using high-frequency identified monetary news as an external instrument, or Miranda-Agrippino, Hoke and Bluwstein (2019) who identify technology news shocks using patent applications as an instrument. In an interesting recent contribution, Chahrour and Jurado (2022) also use a VAR approach to estimate a TFP noise shock which moves expectations about future TFP but not TFP itself. They find this source of expectational shocks to account for a significant fraction of GDP fluctuations in the US at business cycle frequencies.

The remainder of the article is organized as follows: Section 2 describes the identification approach, while Section 3 describes the data. Section 4 presents our empirical methodology and discusses in detail our instrument choice. Section 5 presents the main results and robustness exercises and, finally, Section 6 concludes.

2. THE IDENTIFICATION APPROACH

Our approach to the identification of a consumer sentiment shock is to extract an autonomous component from the time series of the University of Michigan's Survey of Consumers by use of an IV approach. We present the econometric approach below, but first outline the broader idea.

The consumer confidence time series that we study, and discuss below in detail, measures a cross-section of the US population's views about the current state and future outlook of their own and the aggregate US economic conditions. Numerous studies have found that (contemporaneous and lagged) indicators of consumer confidence are statistically significant in explaining and predicting aggregate outcomes. For example, Carroll, Fuhrer and Wilcox (1994) show that the Index of Consumer Sentiment (ICS) has predictive power for consumption growth (controlling for income); Matsusaka and Sbordone (1995) report that the ICS Granger causes GDP, after controlling for a broad set of indicators of the state of the economy as well as other predictors of GNP; and Ludvigson (2004) finds that consumer confidence has predictive power for consumer spending, when controlling for the consumption–wealth ratio.

Such evidence indicates a correlation between consumer confidence data and aggregate outcomes. It does *not*, however, reveal whether consumer confidence variations reflect information about shocks of various sorts to the economy, which may have predictive power for consumption and other variables, or whether *autonomous* shocks to consumer confidence influence the state of the economy. The IV framework that we propose aims at telling apart these two possibilities.

To see how one may go about this, it is instructive to relate our approach to a general equilibrium framework. Traditionally, in the macroeconomics literature, interest has been fixed on considering the impact of surprise or news shocks related to economic "fundamentals", such as changes in productivity, fiscal and monetary policy shocks, trade shocks, etc. Many economic theories, however, also open up for the possibility that expectations unrelated to economic fundamentals can generate economic fluctuations due to coordination failures, increasing returns, incomplete information, etc. To the extent that survey-based measures of consumer confidence contain information about such non-fundamental components, one might therefore extract autonomous shocks from these data.

At an abstract level, consider a state-space representation of a dynamic stochastic macroeconomic model:

$$\mathbf{y}_t = f(\mathbf{s}_t, \epsilon_t, \mathbf{n}_t), \tag{2.1}$$

$$\mathbf{s}_{t+1} = g(\mathbf{s}_t, \epsilon_t, \mathbf{n}_t), \tag{2.2}$$

where $\mathbf{y}_t$ is a vector of endogenous controls, $\mathbf{s}_t$ is a vector of endogenous and exogenous states related to economic "fundamentals" (such as capital stocks, debt, levels of technology, taxes, etc.), and ϵ_t is a vector of fundamental shocks to the exogenous state variables which agents may or may not observe. We denote $\mathbf{n}_t$ extraneous variables, unrelated to $\mathbf{s}_t$ and ϵ_t, which may affect agents' behaviour through expectations or other mechanisms that make the economy susceptible to non-fundamental shocks.

To draw inference on $\mathbf{n}_t$ and their impact on the economy, one could in principle consider estimating the mappings $f(\cdot)$ and $g(\cdot)$ with likelihood based methods or other approaches. However, this will in general require the complete specification of a structural model including all the relevant fundamental shocks impacting on the economy, a task that is very demanding. Moreover, the measurement of shocks, in particular those that are harder to model directly, becomes a function not only of the economic structure, but also depends on parametric assumptions.[2] This does not mean that such an approach would not be an insightful way of estimating non-fundamental shocks, but indicates that other pieces of evidence are valuable.

For that reason, we take an alternative approach by suggesting an IV method. The key idea is to assume that survey evidence on consumer confidence can be considered an empirical measure of one of the components of $\mathbf{y}_t$, and then extract the autonomous component from this variable by means of an instrument. We do this by focusing on consumer confidence that we think of as a component of the general equilibrium model reflecting household expectations. Let $\mathbf{ic}_t$ denote an empirical measure of consumer confidence, an unknown function $h(\cdot)$ of $\mathbf{s}_t$, ϵ_t, measurement error, $\mathbf{m}_t$, *and* one of the extraneous components representing $\mathbf{n}_{ic,t}$:

$$\mathbf{ic}_t = h\left(\mathbf{s}_t, \epsilon_t, \mathbf{m}_t, \mathbf{n}_{ic,t}\right). \tag{2.3}$$

The notation here indicates that the autonomous shock may impact on the economy through its impact on the survey measure of confidence but also that confidence contains both endogenous responses to $\mathbf{s}_t$ and ϵ_t, and an autonomous component, $\mathbf{n}_{ic,t}$. Our interest lies in identifying the latter.[3]

One leading example of such an autonomous component are stochastic sunspots, which can arise in models where the equilibrium is either locally or globally indeterminate. Sunspots are an example of extrinsic shocks to expectations that select the equilibrium as in e.g. Mertens and Ravn (2014) where waves of optimism and pessimism select the equilibrium in a New Keynesian model with a zero lower bound or in Pappa, Ravn and Sterk (2021) where the sunspot selects high vs. low unemployment equilibria in a Heterogeneous Agents New Keynesian setting. In such sunspot theories, the extrinsic shock coordinates agents' expectations but is not related to

2. As an example, Chahrour and Jurado (2018) show that the very large differences in the estimates of Barsky and Sims (2012) and Blanchard, L'Hullier and Lorenzoni (2013) of the importance of noise shocks derive from different assumptions on the extent of wage rigidities.

3. Fuhrer (1993) suggests a similar framework for thinking about the information incorporated in survey evidence on consumer confidence.

fundamentals as such. An alternative mechanism arises in models with imperfect information where noise shocks about fundamentals can generate economic fluctuations. As demonstrated by Chahrour and Jurado (2018), models with news shocks about fundamentals also have noise representations. Thus, one can think of our formulation as implementing the Chahrour and Jurado (2018) procedure to separate shocks into ϵ_t and n_t. Along these lines, Lorenzoni (2009), Barsky and Sims (2012), Blanchard *et al.* (2013), and Pappa *et al.* (2021) study models in which pure noise shocks generate aggregate fluctuations because agents confuse them with (current or expected future) changes in fundamental shocks.[4]

In this case, if one can find some instrument that correlates with $\mathbf{ic}_t$ and can be argued to be unrelated to $\mathbf{s}_t$ and ϵ_t, the fundamentals, one can derive an estimate of the shock $\mathbf{n}_{ic,t}$. Suppose that the mapping $h(\cdot)$ is linear and that there exists a proxy z_t with the structure:

$$z_t = D_t\left(\gamma n_{ic,t} + \nu_t\right), \tag{2.4}$$

where ν_t is a measurement error associated with the proxy, which satisfies $E(\nu_t)=0, E(\nu_t^2)=\sigma_\nu^2, E(\nu_t\nu_s)=0$ for $s \neq t$, and D_t is a random indicator variable (taking on (0,1) values). With this structure, the proxy z_t can potentially be used to recover the autonomous innovation to the survey measure of consumer expectations as long as $\gamma \neq 0$. This can be extended to the case where there are other sources of variations in $n_{ic,t}$ which are not reflected by z_t and the formulation allows the indicator variable to filter $n_{ic,t}$ so that only larger shocks are reflected in z_t, for example.[5]

The econometric estimators (described below) that we will employ should be interpreted in the light of this structure. In this respect, it is important to keep in mind that the survey measure that we will study (the ICE), will naturally be affected by the state of the economy (through s_t) and by fundamental shocks (through ϵ_t) and that there might possibly be other dimensions of autonomous shocks to confidence that our proxy does not recover (i.e. the proxy is not supposed to be thought of as a perfect instrument).

3. DATA

3.1. *Consumer confidence*

At the centre of our attention are consumer confidence data. For this purpose, we draw on subjective expectations collected by the University of Michigan's Survey of Consumers. This survey has been conducted since the late 1940's initially at an annual frequency, becoming quarterly in 1952, and since 1977 it has been conducted on a monthly basis. The long time span and the monthly frequency make these data attractive for our purposes. We start our sample in 1965 and linearly interpolate the consumer confidence data prior to 1977 to produce a monthly series.[6]

Each month, the University of Michigan interviews by phone approximately 500 individuals randomly selected across the US, such that the sample is constructed to be nationally representative. The survey respondents are asked a variety of questions regarding their own personal finances as well as the economic and financial situation of the US economy.

4. There are also shocks studied in the literature which lie somewhat in between these two examples. This includes "savings" or discount factor shocks which have factored prominently in models of liquidity traps, see e.g. Christiano, Eichenbaum and Rebelo (2011), or risk appetite shocks studied both in the macroeconomics literature and in international finance.

5. As in Mertens and Ravn (2013), the instrument that we will use takes on values at discrete events which is emulated by the presence of the indicator variable D_t in the above equation.

6. In the Supplementary Appendix, we show also that our results are robust to excluding data prior to 1977.

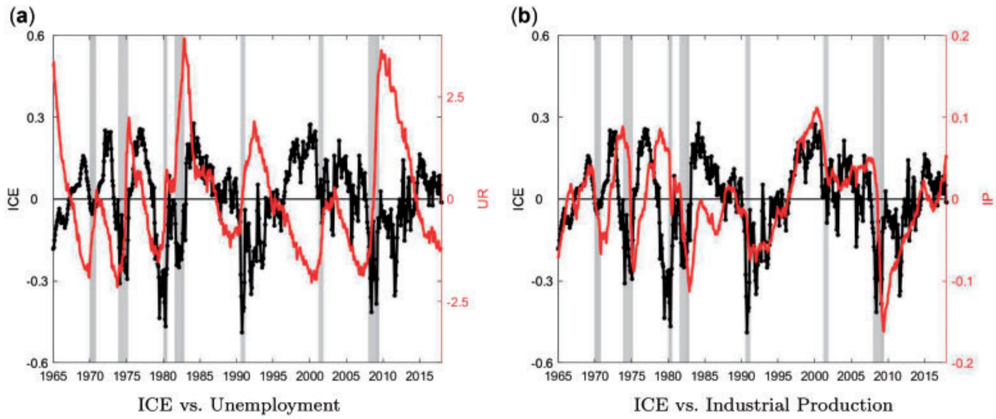

FIGURE 1

Consumer confidence vs. industrial production and unemployment

Notes: The graph presents time series of ICE against industrial production (left panel) and unemployment (right panel) from 1965:1 to 2018:11. All series have been detrended with fourth-order time polynomials. ICE is indicated by lines with circles (black online) using left scale. Unemployment (left panel) and industrial production (right panel) are indicated by lines without circles (orange online) and using right scales.

While we also exploit the information contained in individual-level responses, our main analysis focuses on time-series variation in an index that reflects the common component across households. The University of Michigan Survey of Consumers produces three such broad indices that are aggregated across respondents and across questions: the Index of Consumer Sentiment (ICS), the Index of Current Economic Conditions (ICC), and the Index of Consumer Expectations (ICE). The ICC focuses on questions that concern the *current* state of the respondents' own financial situation and of the US economy, while the ICE is based upon *forward-looking* questions also regarding own and aggregate US outcomes, and the ICS is a broad index covering respondents' views about both current and expected future conditions. We focus on the ICE because of its expectational nature.

The ICE summarizes responses to the following three questions:

1. "Now looking ahead—do you think that a year from now you (and your family living there) will be better off financially, or worse off, or just about the same as now?";
2. "Now turning to business conditions in the country as a whole–do you think that during the next 12 months we will have good times financially, or bad times, or what?";
3. "Looking ahead, which would you say is more likely—that in the country as a whole we'll have continuous good times during the next 5 years or so, or that we will have periods of widespread unemployment or depression, or what?"

For each of these three questions, commonly referred to as PEXP, BUS12, and BUS5, respectively, the survey subjects choose between positive, neutral, or negative answers. The index is then computed as 100 plus the difference in the percentages of positive and negative respondents and the scores are normalized relative to the 1966 base period.

As mentioned earlier, it is well documented that such consumer confidence data fluctuate with macroeconomic conditions. We visualize this in Figure 1, which shows the (detrended) time series of ICE alongside industrial production and the unemployment rate. The ICE is correlated with industrial production and unemployment (the correlation coefficients are 0.33

and −0.28, respectively) and tends to peak, but not always, at the late stages of expansionary phases, reaching its trough just prior to economic recoveries. Thus, there is correlation between consumer confidence and the state of the economy but, as discussed, one cannot make any causal conclusions from this, which is why we propose the IV approach.

3.2. *Mass shootings*

We produce a new dataset of mass shootings in the US for the sample period January 1965 to November 2018. The dataset is constructed by combining and augmenting information from three existing sources, MotherJones (2020), Duwe (2007), and The-Violence-Project (2019). The MotherJones magazine, published by the Foundation for National Progress, maintains an open-source database documenting mass shootings in the US since August 1982. We use MotherJones (2020) as the primary source of mass shootings data from August 1982. To extend the data back to 1965, we use as a primary source an alternative dataset collected by Grant Duwe, an American criminologist; the dataset was first published in his book, "Mass Murder in the United States: A History" (Duwe, 2007). Grant Duwe has supplied us with an updated dataset spanning 1902–2016. We use the overlapping parts of the samples of these two primary sources to cross-check information. The-Violence-Project (2019), a non-profit, non-partisan research centre dedicated to reducing violence in society, has collected a third alternative database on mass shootings, containing data for the 1960–2020 sample. We exploit this dataset to cross-check the pre-August 1982 incidents obtained from Duwe (2007) and as a second cross-check on the MotherJones (2020) database.

The three datasets disagree marginally on the definition of mass shootings. We adopt MotherJones' definition and record public mass shootings in which the motive appeared to be indiscriminate killing, satisfying the following criteria:

(i) the incident was associated with minimum *four* fatalities (excluding the perpetrator) in a single geographical location;

(ii) the killings were carried out by a lone shooter[7]; and

(iii) the shootings occurred in a public place. Crimes primarily related to gang activity, armed robbery, criminal competition, insurance fraud, arguments, or romantic triangle are not included, nor are mass killings that took place in private homes (often stemming from domestic violence).

We first cross-check mass shootings reported in the three aforementioned databases, in order to make sure that they fulfil the criteria mentioned above. Next, we verify the circumstances of the shootings and details such as the number of victims and fatalities using information drawn from news coverage and from court reports. Our dataset is available on our websites and the Supplementary Appendix describes in detail the construction of our series.

The random nature of these events (the fact that they occurred in a public space and were unrelated to gang activity and robberies) is important because it relates to the channel through which they impact on well-being, which may spill over to consumer confidence. Figure 2 shows the locations of the shootings on a map of the US (we exclude Alaska to make the map clearer), along with the number of fatalities (depicted by the size of each circle) and their timeline (depicted by the colour scheme). We have also singled out some of the most lethal events. The map illustrates the geographical dispersion of the incidents, although it is also clear that some states, such as California, Florida, and Texas, have witnessed many more events than other states. Secondly,

7. We include along with MotherJones (2020) the Columbine massacre and the Westside middle school killings both of which have two perpetrators.

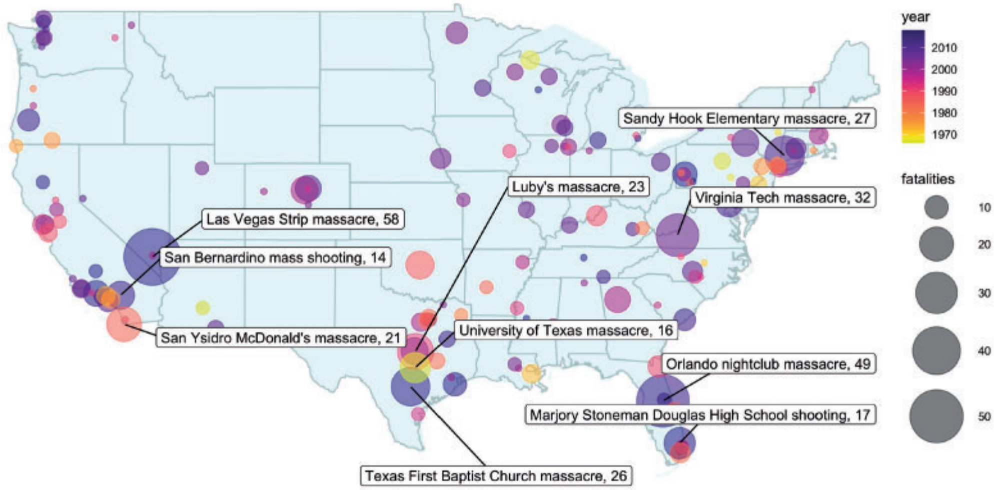

FIGURE 2

Map of mass shootings and fatalities

Notes: The map shows mass shooting incidents since 1965, indicating the number of fatalities by the size of each circle, and their timeline by the colour scheme. It also singles out the most lethal events.

there are many events with a large number of fatalities. From January 1965 to November 2018, there were no less than 42 mass shooting events with seven or more fatalities with a total of 581 fatalities corresponding to an average of approximately 14 fatalities per incident. Perhaps the two most notorious incidents are Columbine High in April 1999, where 12 students and one teacher were murdered, and the Virginia Tech Massacre in 2007, when an undergraduate student murdered 32 people on campus. The single worst mass shooting is the 2017 Las Vegas Strip Massacre in which 58 people were killed and 546 people were injured, followed by the Orlando Nightclub Massacre in June 2016 when 49 people lost their lives and 53 were seriously injured. The most serious incidents are listed in the Supplementary Appendix.

The frequency of mass shooting events has increased over time. Prior to 1990 there was on average one shooting with seven or more fatalities approximately every 970 days. Between 1990 and 2000 the frequency increased to on average one such shooting every 600 days, further escalating to one every 390 days between 2000 and 2015, and finally to one every 160 days in the last 3 years of the sample (see the Supplementary Appendix for a timeline of mass shootings over the whole sample). The number of fatalities in mass shootings per month has also increased. Prior to 2015, each shooting had on average 11 fatalities, a figure which has increased to 24 per shooting since 2015.

Given the increase in the severity of shootings, we control for a trend in mass shooting fatalities in our regressions. We do this by estimating a fourth-order trend in time for the mass fatalities and dividing the raw series by the trend.[8] We show in the Supplementary Appendix that results are robust to leaving out such a trend.

8. The trend is estimated for the observations with non-zero mass fatalities, thus treating periods with no shootings as missing observations. Results do not change if periods with no shootings are treated as zeros (also capturing the increase in frequency of shootings).

3.3. *Macroeconomic aggregates*

We study the impact of sentiment shocks on a wide range of macroeconomic aggregates. The key observables that we examine are the civilian unemployment rate, industrial production, the consumer price index, the federal funds rate, the short-term (12-month) uncertainty index of Jurado *et al.* (2015), and real stock prices (the Standard & Poor's 500 index divided by the CPI). We also look at private sector consumption of non-durables and durables and labour market indicators, such as vacancy postings and labour market tightness. Finally, we investigate the impact of the sentiment shock on total factor productivity and economic policy uncertainty. The Supplementary Appendix includes precise definitions and sources of the data.

Our benchmark sample spans January 1965 to August 2007, but we also report results when including post-2007 data. We focus on the shorter sample for two main reasons. First, as highlighted above, the frequency of mass shootings increases significantly towards the end of the sample which could lead agents to pay less attention to individual mass shooting events than in the earlier part of the sample.[9] Indeed, the instrument becomes weaker when we include post-2007 data yet it remains significant (and none of the key results change). Second, the 1965:m1–2007:m7 sample period leaves out the Great Recession and its aftermath because of concerns about the impact of this episode on the transmission mechanism. In particular, the depth of the downturn is likely to have changed both the behaviour of agents and the conduct of economic policy relative to other periods (due to e.g. issues related to the effective lower bound on short-term nominal interest rates). Our results are, however, shown to be robust to considering alternative sample periods.

4. EMPIRICAL METHODOLOGY

4.1. *Econometric methodology*

We implement the proxy SVAR estimator developed by Stock and Watson (2012) and Mertens and Ravn (2013) for the identification and estimation of the dynamic causal effects of consumer sentiment shocks. The central idea is to use external instruments for the structural shocks of interest in a VAR setting. We also show robustness of the results to using an alternative local projection IV approach (see e.g. Fieldhouse, Mertens and Ravn, 2018; Ramey and Zubairy, 2018; Stock and Watson, 2018). In both cases, the introduction of a rich set of control variables is important because it allows one to capture the dynamics induced by state variables and control for other structural shocks.

Let $\mathbf{Y}_t$ be an $n\times 1$ vector of endogenous observables perturbed by an $n\times 1$ vector of structural shocks, $\mathbf{e}_t$, that are mutually orthogonal. $\mathbf{Y}_t$ is assumed to be second-order stationary and can be represented as:

$$\mathbf{A}(\mathbf{L})\mathbf{Y}_t = \mathbf{u}_t, \tag{4.1}$$

where $\mathbf{A}(\mathbf{L}) = \mathbf{I} - \mathbf{A}_1\mathbf{L} - \mathbf{A}_2\mathbf{L}^2 - \ldots$, and $\mathbf{L}$ is the lag operator, $\mathbf{L}^i\mathbf{x}_t = \mathbf{x}_{t-i}$. The innovations $\mathbf{u}_t$ are linear combinations of the structural shocks:

$$\mathbf{u}_t = \Theta_0\mathbf{e}_t, \tag{4.2}$$

where Θ_0 is invertible. Under the stationarity assumption, this implies that:

$$\mathbf{Y}_t = \Gamma(\mathbf{L})\Theta_0\mathbf{e}_t, \tag{4.3}$$

9. If mass shootings happen frequently, they may become a part of "normality" and have little impact on survey respondents' psychological well-being.

where $\Gamma(\mathbf{L}) = \mathbf{A}(\mathbf{L})^{-\mathbf{1}}$ is square summable. We are interested in characterizing the causal impact of a single shock and therefore in obtaining a single column of Θ_0. Without loss of generality, we order consumer confidence first in the vector of observables. Let $\mathbf{s}_t$ be a proxy for $\mathbf{e}_{1t}$, the structural shock of interest (we use the notation $\mathbf{s}_t$ for $S_t - \text{proj}(S_t|\mathbf{W}_t)$ where S_t is the proxy, $\mathbf{W}_t$ is the history of Y_t, and proj$(x|z)$ denotes the projection of x on z). The proxy SVAR imposes the following identifying assumptions:

$$\mathbb{E}(\mathbf{s}_t\mathbf{e}_{1t}) = \phi \neq 0 \tag{4.4}$$

$$\mathbb{E}(\mathbf{s}_t\mathbf{e}_{it}) = 0, \ i > 1. \tag{4.5}$$

The relevance condition in (4.4) requires correlation between the proxy and the unobserved structural shock of interest, while (4.5) imposes orthogonality with other structural shocks. If the identifying assumptions hold, it follows that:

$$\mathbb{E}(\mathbf{s}_t\mathbf{u}_t) = \begin{pmatrix} \phi\Theta_{0,11} \\ \phi\Theta_{0,i1} \end{pmatrix}, \ i > 1$$

where $\Theta_{0,ij}$ denotes the (i,j)'th entry of Θ_0.

We scale the impulse responses so that the sentiment shock corresponds to a 1% decline in the consumer confidence index, i.e., $\Theta_{0,11} = 1$. The remaining structural coefficients of interest in the first column of Θ_0 are then obtained as:

$$\frac{\mathbb{E}\left(\mathbf{s}_t\mathbf{u}_{i,t}\right)}{\mathbb{E}\left(\mathbf{s}_t\mathbf{u}_{1,t}\right)} = \Theta_{0,i1}.$$

We implement the estimator with a 2SLS procedure and estimate the coefficients above by regressing $\widehat{\mathbf{u}}_t$ on $\widehat{\mathbf{u}}_{1t}$ using $\mathbf{s}_t$ as the instrument. To see how the identification approach works, consider the following representation of the forecast errors:

$$\begin{aligned} u_{1,t} &= \mathbf{b}_1\mathbf{u}_{2:n,t} + B_1 e_{1,t}, \\ \mathbf{u}_{2:n,t} &= \mathbf{b}_2 u_{1,t} + \mathbf{B}_2\mathbf{e}_{2:n,t}, \end{aligned}$$

where $\mathbf{u}_{2:n,t}$ denotes the forecast errors of the second to n'th variables in the vector of observables. Assumptions (4.4)–(4.5) allow us identify the main objects of interest, $\mathbf{b}_1, \mathbf{b}_2$ and B_1 (see Mertens and Ravn, 2013) which determine the first column of Θ_0 and therefore the response of the observables to the identified shock to consumer confidence.[10] With these coefficients at hand, the impulse responses can be computed from equation (4.3). One can also derive an estimate of a scaled version of the structural shock of interest, $e_{1,t}$, by projecting the instrument on $\mathbf{u}_t$, see Mertens and Ravn (2013) or Montiel-Olea, Stock and Watson (2021).

The parameters of interest can, as discussed by Mertens and Ravn (2013), be derived in three stages by first estimating the reduced form VAR, next regressing the forecast errors on the proxy, and third imposing the identifying assumptions. Alternatively, one can enter the proxy in the VAR in the first position and then impose a recursivity assumption as explained in Plagborg-Møller and Wolf (2021).

10. The parameters in $\mathbf{B}_2$ are not identified without further assumptions that allows for identification of the other structural shocks.

To be conservative, we use weak instrument robust methods to compute standard errors.[11]

4.2. *SVAR specification*

The benchmark specification of the vector of observables is:

$$\mathbf{Y}_t = [ic_t, u_t, ip_t, cpi_t, ffr_t, unc_t, sp_t], \tag{4.6}$$

where ic_t is the natural logarithm of the ICE, u_t is the civilian unemployment rate, ip_t is the natural logarithm of industrial production, cpi_t is the natural logarithm of the consumer price index, ffr_t is the federal funds rate, unc_t is the natural logarithm of Jurado *et al.* (2015)'s 12-month macroeconomic uncertainty index, and sp_t represents the natural logarithm of real stock prices.

Data for the macroeconomic variables were obtained from the Federal Reserve Economic Data (FRED), made available online by the Federal Reserve Bank of St. Louis. Data for the unemployment rate and consumer price index for all urban consumers are both produced by the Bureau of Labor Statistics. The industrial production index and the effective federal funds rate series are produced by the Board of Governors of the Federal Reserve System. The measure of macroeconomic uncertainty is constructed by Jurado *et al.* (2015). Stock market prices are deflated using the CPI index and also come from the FRED database. In the robustness analysis also study consumption of durables and non-durables, vacancy postings, labour market tightness, the VIX uncertainty index, economic policy uncertainty (EPU), and total factor productivity. The data on consumption of durables and non-durables as well as the VIX index of S&P 500 stock price options volatility are also obtained from the FRED. Data on vacancy postings, based on the Conference Board's "Help Wanted" index, were obtained from the replication filed of Stock and Watson (2012). The economic policy uncertainty index is constructed by Baker, Bloom and Davis (2016). For TFP, we use the latest vintage of the estimates by Fernald and Wang (2016). These series are all expressed in logs.[12]

The VAR includes a constant and the lag length is set to 18 months.[13] We detrend all variables apart from the federal funds rate with fourth-order time polynomials. The Supplementary Appendix reports further details on dta sources and contains results for alternative measures of confidence, no detrending of the data and controlling for seasonality in shootings.

4.3. *Mass shooting fatalities as a proxy for sentiments*

Our instrument for consumer sentiment shocks are fatalities in mass shootings. As already mentioned, the idea is that such events constitute a source of bad news, which may impact on consumer confidence, but in itself should not derive from economic fundamentals. There is direct evidence that mass shootings impact on psychological well being. For example, Hughes *et al.*

11. With strong instruments, inference can be carried out using a Delta method estimator of the covariance matrix. In the weak instrument case, a variety of covariance estimators are available, see e.g., the discussion in Mertens and Ravn (2019) for a discussion.

12. All the data we use in our analysis are seasonally adjusted, except for shootings and the federal funds rate. Those that were not already seasonally adjusted by the data source provider, were seasonally adjusted using the Census Bureau's X13 tool, namely all confidence indices (ICE, ICS, ICC, BUS5, and BUS12), asset price and uncertainty measures (S&P 500 Stock Price Index, Jurado *et al.* (2015)'s uncertainty index, the VIX, EPU), vacancy postings and utilization-adjusted TFP.

13. This lag length is chosen to maximize the first stage F-statistic, i.e., the relevance criterion of our proxy instrument.

(2011) evaluate the impact of the Virginia Tech shooting in 2007 on post-traumatic stress disorder (PTSD) symptoms amongst Virginia Tech students in the months following the tragic event and find that PTSD symptoms were elevated for an extended period even amongst students who were not under direct threat during the shooting. Clark and Stancanelli (2017) document a decline in subjective well-being across the US in the aftermath of the 2012 Sandy Hook School shooting. Soni and Tekin (2020) examine the impact of mass shootings on community well-being and emotional health (relating various dimensions of satisfaction with the local community, safety concerns, and indicators of stress) and find a significant negative impact on these indicators. Fox and DeLateur (2013) show that, while mass shootings account for the fewest loss of lives compared to any other type of homicide, these events induce the most fear in people due to their seemingly random nature and the inability to predict and prevent incidents. Starting from Elster (1998), several studies, especially in finance, highlight the role of emotions in economic behaviour (see e.g. Hirshleifer and Shumway, 2003; Edmans, Garcia and Orli, 2007; Glimcher and Tymula, 2017). The evidence on the link between mass shootings and psychological well-being and between optimism and economic choices, offers a potential channel through which mass shootings impact on survey evidence on consumer confidence about the US economy.

The use of mass shooting fatalities as an instrument for consumer sentiment shocks rests on the assumption that they can be considered exogenous to other economic factors. Given the random nature of mass shootings, this is a plausible assumption. There is no compelling evidence that these events are triggered by prevailing conditions in the economy. Pappa *et al.* (2019) show that mass shootings are not predictable by current economic conditions, measured by the unemployment rate. In line with this, more than 50% of perpetrators in the dataset that we construct were diagnosed with signs of severe mental illness *prior* to committing the mass shootings.[14]

One might also consider whether mass shootings could impact on macroeconomic aggregates directly, i.e., not through consumer sentiments only. Sadly, despite their tragic nature, mass shootings occur on a regular basis and each shooting is unlikely to trigger direct intervention (such as increased spending on security) which could question the exclusion restrictions that we impose.[15] Further, supporting our assumption that fatalities in mass shootings impact on the economy through consumer sentiments, we find fatalities to be an insignificant instrument for uncertainty, stock prices, and TFP.[16] Finally, our analysis below of the impact of mass shootings at the individual level offers another check on the exogeneity assumption by exploiting geographical variation. We find no evidence that questions the exogeneity assumption.

Since the ICE is constructed to represent consumer confidence for the US population, it is important that a broad cross-section of the US population can be considered as having been aware of the events. Supporting this, mass shootings receive significant news coverage, reaching a large portion of the US population. For example, according to LexisNexis (2020), a provider of electronic access to legal and journalistic documents, main national news sources in the US printed no less than 182 articles on the Fort Hood Massacre in Texas in 2009 (13 fatalities) and

14. Some studies link economic recessions to mental health problems. An in-depth literature review by Parmar, Stavropoulou and John (2016) concludes that many studies suffer from biases and their results should be taken with caution. Yet, even if such a link exists, effects on mental health were found primarily for women, while the vast majority (98% in our sample) of mass shooting perpetrators are men.

15. Abadie and Gardeazabal (2003) examine the impact of a related instrument, terrorism. Such events, however, are likely to impact directly on national security spending and would therefore not be appropriate as an instrument in our setting.

16. This result also addresses the concern that mass shootings may impact on measured TFP through worker productivity (e.g. due to their effects on mental health).

TABLE 1
F-statistics for instrument relevance tests

Part A: Benchmark VAR			
Sample	Proxy	F-statistic (HOM)	F-statistic (MOP)
1965:1–2007:8	MassFat_7	11.6	19.3
1965:1–2018:11	MassFat_7	10.5	5.2
1965:1–2018:11	MassFat_7 excl. Las Vegas	14.8	8.5
1965:1–2007:8	$\text{MassFat}_7\text{Dummy}$	12.1	17.0
1965:1–2018:11	$\text{MassFat}_7\text{Dummy}$	8.4	5.6
1965:1–2007:8	MassFat_4	6.8	6.2
1965:1–2007:8	MassVictims	8.9	12.9
Part B: Alternative VAR specifications, 1965:1–2007:8			
Confidence	Observables	F-statistic (HOM)	F-statistic (MOP)
ICC	Benchmark	5.9	8.5
ICS	Benchmark	11.3	13.7
BUS5	Benchmark	7.6	13.3
BUS12	Benchmark	7.7	11.4
ICE	CPI inflation	10.8	16.7
ICE	no SP500	8.8	13.1
ICE	no U12	9.6	15.7
ICE	no SP500, U12	7.2	10.9

Notes: The table records the outcomes of F-tests for the null hypothesis that the instrument coefficient is zero in the first-stage regression for consumer confidence. HOM and MOP respectively denote the F-statistics for the null of standard conditional homoscedasticity and no serial correlation, and for the Montiel-Olea and Pflueger (2013) HAR-robust F-test.

156 articles on the Sandy Hook shooting in Connecticut in 2012 (28 fatalities).[17] Lankford (2018) studies news coverage of the perpetrators of seven mass killings in the 2013–17 period and finds that mass shooters in many cases received more news attention than even celebrities such as sports stars.[18]

4.4. *Relevance*

To evaluate the use of fatalities in mass shootings as an instrument for consumer confidence, we first examine whether it satisfies the relevance condition. Table 1 reports the outcomes of the first-stage F-statistics for the null hypothesis that the instrument has no explanatory power for consumer confidence. We report (non-robust and HAC-robust) weak instrument F-test statistics for a variety of specifications. Since we have a single instrument, the robust version of this test statistic is equivalent to the Montiel-Olea and Pflueger (2013) efficient first-stage F-statistic, and the standard rule-of-thumb of a critical value of 10 can be applied, see Montiel-Olea *et al.* (2021).

We first check the outcome of the weak instrument tests for our benchmark 1965:1–2007:8 sample and also examine its value for the sample ending in November 2018. For the benchmark

17. These news sources constitute three of the highest-circulation national newspapers in the United States (Wall Street Journal, USA Today, and Washington Post) and one of the highest circulation newspapers in all four US census regions, including the Northeast (New York Times), South (Atlanta Journal Constitution), Midwest (Chicago Tribune), and the West (Los Angeles Times).

18. Given the mechanism we want to highlight, we could use media coverage instead of mass fatalities as the instrument for shootings. We have opted for the latter, since this measure is arguably more objective and consistent throughout the sample period. Instead, media coverage data (e.g. LexisNexis (2020) or Vanderbilt (2020) on tv coverage) are very noisy.

sample the standard non-robust F-statistic is equal to 11.6, while the robust version is 19.3. Extending the sample to November 2018, the standard F-statistic falls marginally to 10.5 while the HAR-robust F-statistic declines more substantially to 5.2. However, this drop in the F-statistic is more contained if we drop the Las Vegas shooting in October 2017, which stands out as the most fatal shooting in our sample (58 fatalities); excluding this incident, the F-statistics for the extended sample period until November 2018 are 14.8 and 8.5 for the non-robust and HAR-robust versions, respectively. In either case, the instrument remains significant but inference may require weak-instrument robust approaches.

We also report the weak instrument test when replacing the number of fatalities with dummy variables which equal one if a mass shooting with seven or more fatalities occurred and zero otherwise. In this case, the F-statistics are 12.1 and 17.0, respectively. When we extend the sample and include the period with more frequent shootings, as for the case of mass shooting fatalities, the F-statistics are reduced to 8.4 and 5.6, respectively. Our database also contains information on mass shootings with between four and six fatalities. Making use of this alternative instrument with four or more fatalities, the F-statistics decline but the instrument still passes the relevance condition. The weaker correlation between this instrument and consumer confidence is plausibly due to the less serious incidents attracting less nationwide media attention. Finally, we also make use of data available on total victims in mass shootings, including both fatalities and injuries, and find that F-statistics remain high at 8.9 and 12.9 using this measure as an instrument.

The second block of Table 1 examines the relevance of the instrument for alternative measures of consumer confidence. We consider the ICC, ICS, BUS5, and BUS12 indices which were discussed in Section 3.1 above. On the basis of the robust F-test statistics, mass shootings appear to be a strong instrument for the ICS, BUS5, and BUS12 while the test statistic is smaller for the ICC. Yet, no matter the index, the instrument remains significant thus indicating an impact of these tragic events on consumer confidence.[19]

The next rows in Table 1 report F-test values when we consider alternative specifications of the vector of observables, using CPI inflation instead of the CPI level, and when we exclude real stock prices (S&P500) or uncertainty (U12) or both from the observables. None of these modifications alter the conclusions about the relevance of the instrument.

Figure 3 displays the point estimate (black line) as well as 68% (dark grey) and 90% (light grey) confidence intervals for the ICE response to the identified sentiment shock. Given the weak instrument test outcomes for our baseline specification, we could use the Delta method for computing confidence intervals. We opted to be more conservative and use the procedure suggested by Montiel-Olea *et al.* (2021) for inference. This method is asymptotically valid in the face of weak instruments which is the case in some of the robustness exercises. To further gauge robustness, Figure 3 also shows point estimates (blue lines) of the impulse response function for confidence from specifications in which we exclude one-by-one each of the 21 mass shootings with seven or more fatalities in our baseline sample. This helps understand that our results are not driven by particular events.

The point estimate is highly robust and shows that the ICE falls persistently after a negative sentiment shock. Eight months after the drop in confidence, only 50% of the initial drop has dissipated and it takes around 16 months before the point estimate returns to zero. Taking sampling uncertainty into account, the decline in consumer confidence is significant for 13 months at the

19. We have also investigated whether mass fatalities can be used as an instrument for the Survey of Professional Forecasters' (SPF) GDP forecast but found no evidence of this. Such different properties of the forecasts contained in the University of Michigan's Survey of Consumers and the SPF are well known in the literature on disagreement, see Reis (2020) for a recent example.

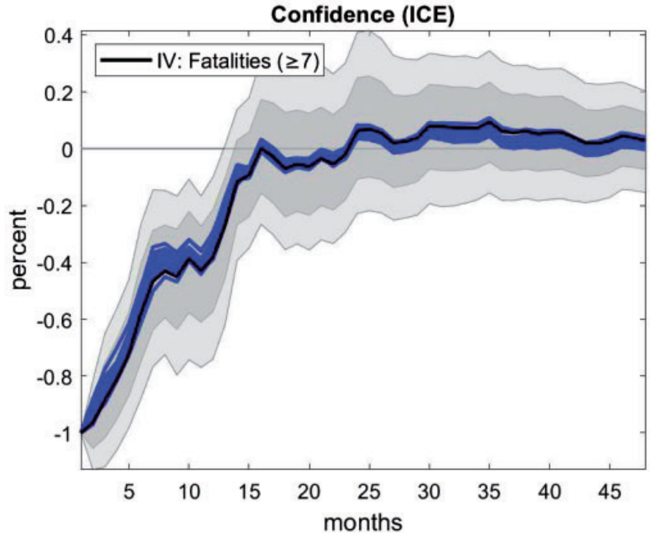

FIGURE 3

Confidence response to the instrument

Notes: The graph plots the responses of ICE to a sentiment shock. The continuous black line depicts the point estimate of the impact of the identified sentiment shock on the ICE in our benchmark specification. Dark grey and light grey areas represent 68% and 90% confidence bands based on the Montiel-Olea *et al.* (2021) weak IV robust standard errors, respectively. Blue lines depict point estimates of the impulse response functions from specifications in which we exclude each of the 24 mass shootings with 7 or more fatalities, one at a time. The sample period is 1965:1–2007:8.

90% level and for 14 months at the 68% level. The point estimates of the response of the ICE to the sentiment shock are essentially unchanged when excluding any of the individual mass shooting events in the benchmark sample.

4.5. *Mass shootings and individual sentiment*

One way of evaluating the credibility of our results about the impact of mass shootings on consumer confidence is to exploit the geographical variation in these events. In particular, one would think that mass shootings should impact particularly strongly on the consumer confidence of households that live close to the events. To investigate this, we examine sub-national variations in consumer confidence by looking at individual-level survey responses mapped to US counties.

We obtain individual-level data on confidence from the University of Michigan's Survey of Consumers, the same source used in the analysis of the aggregate effects above. We note that the survey is nationally representative with approximately 500 randomly selected individuals interviewed across the US each month. Given the small number of interviews for each county-month, a simple measure of average local confidence would inherit substantial noise from differences across individuals in their confidence levels and perceptions. To address this, we make use of the fact that two thirds of the survey subjects have a follow-up interview 6 months later and compute the 6-month change in confidence for the same individual, a feature of this survey which has not yet been explored in the related literature (e.g. Mian *et al.*, 2015; Benhabib and Spiegel, 2019). This enables us to reduce noise stemming from individual fixed effects, constituting a considerable improvement in the measure of variations in local sentiment. During the sample considered, which spans from January 2000 to June 2017 (based on data availability since individual-level data is only available starting in 2000), a total of 105,533 interviews were

TABLE 2

Exogeneity of mass shootings: linear probability model

Counties in sample	(1) ≥ 1 shooting P(shooting)	(2) All counties P(shooting)
Unemployment rate (−1)	−0.034	−0.001
	(0.071)	(0.001)
Constant	0.001	0.000
	(0.010)	(0.000)
Observations	13,020	659,370
R^2	0.013	0.000
Number of counties	62	3,144
Time FE	✓	✓
County FE	✓	✓
SE cluster	State	State

Notes: The table records the estimates of the linear probability model where the probability of mass shootings occurring in a given county is regressed against a constant and the lagged unemployment rate in the county.
Standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1.

conducted, of which 40,239 respondents were interviewed twice. Mapping individuals to their counties of residence and excluding those who have migrated counties, leaves 40,009 observations in the sample.

We focus on the survey question that regards expectations about the aggregate US economic outlook 5 years out in the future, BUS5, which was discussed earlier. A main reason that we focus on BUS5 is that this measure of confidence have been used in the other studies of sub-national effects (e.g. Mian *et al.*, 2015; Benhabib and Spiegel, 2019). Another reason for focusing on the outlook for the aggregate US economy (without considering questions relating to households' personal finances), is that controlling for time fixed effects (thought of as capturing aggregate US fundamentals common to all individuals), the residual component of confidence can be attributed to sentiments that are orthogonal to national economic fundamentals. Responses take discrete values between 1 and 5, which we re-order as increasing in confidence.

To study how mass shootings affect local confidence, we map mass shootings to the counties in which they occurred (shown in Figure 2). We use four fatalities as the cutoff since this implies that we have a reasonable number of treatments but show also robustness to minimum seven fatalities in line with the main instrument discussed earlier for the aggregate US results. Between January 2000 to June 2017, there were 71 mass shootings with four or more fatalities which occurred in 62 different counties, providing substantial cross-sectional variation. The Supplementary Appendix presents details for the distribution of fatalities and victims during these episodes.

As mentioned earlier, Pappa *et al.* (2019) examine the distribution of shootings across time and show that US unemployment and the number of mass shootings are contemporaneously unrelated over the period 1990–2013. We present further evidence confirming these results for our data sample. First, examining the cross-sectional distribution of shootings across counties, simple t-tests for differences in means do not reject that counties that experience mass shootings have on average equal confidence levels and unemployment rates as those that do not (see the Supplementary Appendix). Second, exploiting the panel dimension of our data, we estimate a linear probability model and find no impact of lagged unemployment rates on the probability of mass shootings occurring in a given county (Table 2). Taken all together, we find no evidence that the distribution of mass shootings across counties and time is related to underlying economic conditions, confirming the exogeneity restriction.

TABLE 3
Local impact of mass shootings on individual confidence

	(1) Δ Confidence	(2) Δ Confidence	(3) Δ Confidence	(4) Δ Confidence	(5) Δ Confidence
Total fatalities	-0.016^{**}				
	(0.007)				
Total fatalities (min. 7)		-0.016^{**}			
		(0.007)			
Dummy for min. 10 fatalities			-0.740^{***}		
			(0.148)		
Total victims				-0.008^{***}	
				(0.003)	
Dummy for min. 10 victims					-0.430^{**}
					(0.192)
Δ Personal finances	0.063^{***}	0.063^{***}	0.063^{***}	0.063^{***}	0.063^{***}
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Δ County unemployment	−0.010	−0.010	−0.010	−0.010	−0.010
	(0.018)	(0.018)	(0.018)	(0.018)	(0.018)
Constant	0.153	0.153	0.153	0.153	0.153
	(0.137)	(0.137)	(0.137)	(0.137)	(0.137)
Observations	36,276	36,276	36,276	36,276	36,276
R^2	0.027	0.027	0.027	0.027	0.027
Time FE	✓	✓	✓	✓	✓
SE cluster	MSA	MSA	MSA	MSA	MSA
F-statistic	4.9	5.0	25.0	7.8	5.0

Notes: The table presents estimates on regressing the 6-month change in consumer confidence, $\Delta_6 C_{i,j,t}$, on county-level mass shootings fatalities accumulated over those 6 months, $\sum_{k=1}^{6} S_{j,t-k}$. Column (1) presents results for total fatalities; Column (2) presents results when we use shootings with more than seven fatalities; Column (3) the case in which we use dummies for shootings with minimum 10 fatalities; Column (4) when we use cumulative fatalities and injuries in the regressions and Column (5) the case in which we use a dummy for shootings with more than 10 victims (including both fatalities and injuries).
Robust standard errors in parentheses *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Finally, we estimate how mass shootings affect sentiments for residents in the county of the shooting by regressing the 6-month change in individual consumer confidence ($C_{i,j,t}$) on county-level mass shooting fatalities ($S_{j,t}$) cumulated over those 6 months. This nets out time-invariant factors such as individual and location fixed effects. We also control for time-varying individual and local economic conditions—namely, changes in the individual's reported personal financial conditions ($Y_{i,j,t}$) and county unemployment rates[20] ($UR_{j,t}$),—as well as aggregate US fundamentals by including time fixed effects (α_t). Note that the time fixed effect implies that residual confidence is orthogonal to national fundamentals. To correct for heteroskedasticity, standard errors are clustered at the MSA-level.

We estimate the following specification:

$$\Delta_6 C_{i,j,t} = \beta \sum_{k=1}^{6} S_{j,t-k} + \lambda \Delta_6 Y_{i,j,t} + \gamma \Delta_6 UR_{j,t} + \alpha_t + u_{i,j,t}, \tag{4.7}$$

where i denotes the individual, j the county, t the year–month time period, and $\Delta_6 x_t$ denotes the 6-month change in x_t.

We present evidence that mass shootings significantly affect individual confidence in Table 3. On average, per fatality in a mass shooting, confidence drops significantly for individuals residing

20. Monthly data on county unemployment rates is obtained from the Local Area Unemployment Statistics of the Bureau of Labor Statistics. We seasonally adjust the data using the Census Bureau's X13 procedure.

in the county of the shooting relative to other counties, albeit by a small amount (approximately 0.02 points). Results are robust whether we consider all shootings (minimum four fatalities) or only larger shootings (minimum seven fatalities), respectively Columns (1) and (2). This result is not driven by a single large shooting event, as shown by considering a dummy for events with at least 10 fatalities (Column 3)[21]. Results are also robust to considering the number of victims, defined as including both fatalities and injuries. Per victim, confidence drops by a approximately 0.01 points (Column 4) and again, results remain significant when considering a dummy for events with at least 10 victims (Column 5). As such, mass shootings are shown to robustly generate significant drops in confidence about the aggregate economic outlook for individuals residing in the county where they take place (relative to individuals residing in other counties). Results are also robust to a variety of sensitivity tests that consider alternative mass shooting instruments and changes in the baseline specification presented in the Supplementary Appendix.

These findings provide additional microeconometric evidence, at the subnational level, that mass shooting events can trigger autonomous fluctuations in consumer sentiments that are orthogonal to fundamentals. The results are also in line with Lagerborg (2017), who uses a similar instrument—school shootings—and identification approach to show that these incidents also induce a significant drop in confidence for individuals residing in the county where they took place, and also trigger recessionary impacts on the economy.[22] Taken altogether, the significant effects of mass shootings on sentiments at the local level is in line with effects identified at the national level, giving additional credibility to the instrument used in our Proxy VAR and LP-IV estimations.

4.6. *Sentiment shocks*

Turning back to the Proxy SVAR results, Figure 4 depicts the historical realizations of the identified sentiment shocks together with the NBER recessions. To make the plot easier to digest, we also depict the 5-month centred moving average of the shock series.

The identified shock appears to turn negative prior to or at the very start of NBER recessions, a finding in line with Faccini and Melosi (2019) who attribute several US recessions to deteriorating "beliefs". Our identified shock appears particularly important for the early 1990's recession where the sentiment shock turns very negative from the onset of the recession until the late summer of 1990. Consistently with this, McNees (1992) attributes this recession partially to loss of consumer and business confidence as a result of the 1990 oil price shock. Similar drops in confidence lead the recession in the early 1980s. According to Seymour and Schneider (1987), this period was characterized by a decline in consumers' confidence in institutions. For instance, in mid-summer of 1979 Jimmy Carter called attention when he lectured the nation about the existence of a "crisis of confidence" that "stroke at the very heart and soul and spirit of the national will." We also notice a sustained period of negative confidence shocks in the aftermath of the 9/11 attacks in 2001 and many economists and institutions (e.g. Lenain, Bonturi and Koen, 2002) attribute an important role to confidence after this episode.

21. The F-stat is much larger when we consider only very large shootings, but we are reluctant to draw too strong conclusions given that there are a limited number of these events in the sample.

22. While we find that mass shootings induce significant drops in confidence, we note that F-statistics do not pass the Stock and Yogo (2005) weak instrument test rule of thumb value of 10 (except for Column 3 where we also note a very small treatment sample). In contrast, school shootings are shown to be a stronger instrument for confidence at the local level, according to Lagerborg (2017). We note that this could be because there are fewer mass shootings than school shootings during this sample period.

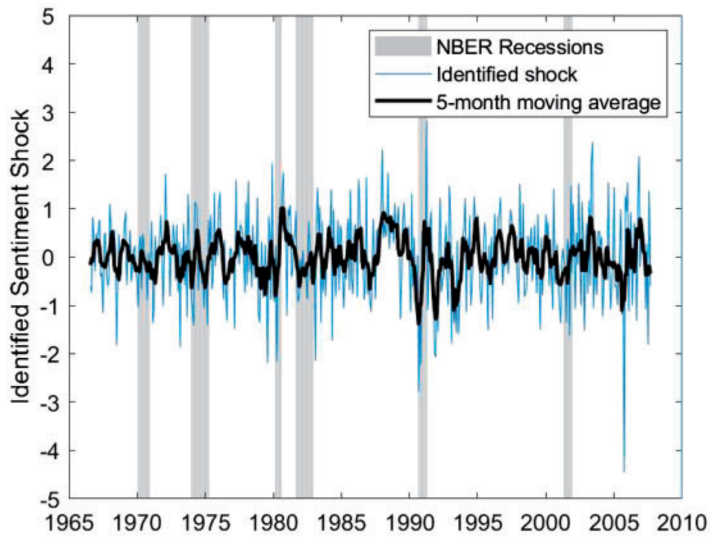

FIGURE 4

Historical realizations of sentiment shocks

Notes: The graph plots our identified shock series (blue line) and its 5-month moving average (black line) for our benchmark specification, where we use shootings with 7 or more fatalities as an instrument for consumer confidence. Grey shaded areas show NBER recessions.

5. DYNAMIC CAUSAL EFFECTS

Given the evidence on the instrument relevance, we now proceed to evaluate how sentiment shocks impact on the economy.

5.1. *Impulse responses*

Figure 5 plots the impulse responses for the baseline specification. An autonomous decline in consumer sentiments sets off a persistent deterioration in the state of the economy. As discussed above, consumer confidence falls for around 12–15 months. In parallel with this, industrial production falls gradually but persistently, with the maximum decline occurring 7–12 months after the consumer sentiment shock.[23] The decline in output is significant at the 68% level for just above 2 years and at the 90% level for around a year and a half.

The recessionary impact is also reflected in the labour market. The unemployment rate rises after the decline in consumer sentiments reaching its peak 13–16 months after the drop in sentiments, after which it starts to recover. The increase in unemployment is very persistent and is significant at the 90% level for 2 years. As we show in the subsequent analysis, the increase in unemployment is accompanied by a general weakening of labour market conditions.

On the monetary side, the negative consumer sentiment shock leads to a persistent rise in prices which is, however, significant at the 90% level only in the first couple of months (and at the 68% level for approximately a year). The short-term nominal interest rate declines with a lag and remains significantly below its initial level for a year and a half to 2 years. The reduction in the nominal interest rate may either be due to a strong policy response to the drop in output and the

23. In results not presented here for the sake of brevity, we find that the impulse response functions of variables relating to the intensive margin of factor input use, such as hours worked per worker and capacity utilization, follow similar paths to the responses of industrial production.

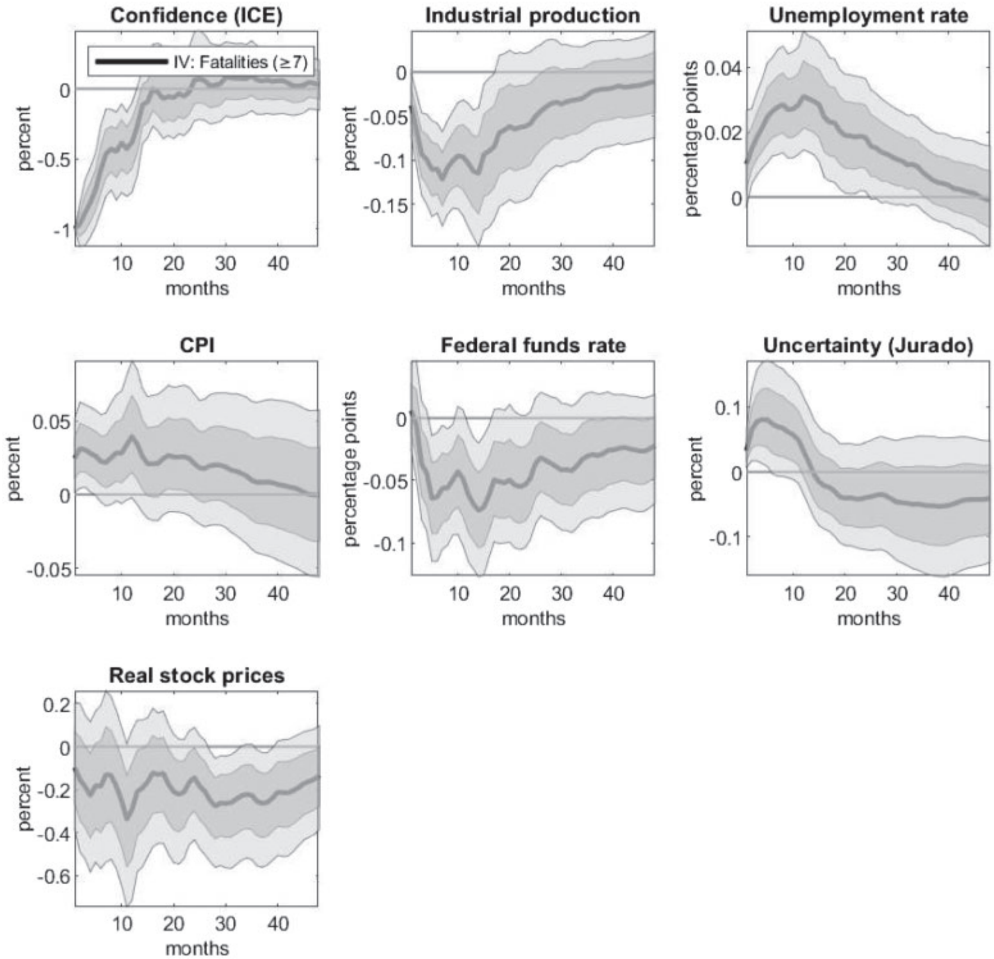

FIGURE 5

Consumer sentiment shock impulse responses—benchmark

Notes: The graph plots impulse response functions to a sentiment shock, for our benchmark specification. The continuous line depicts point estimates of the impact of the identified sentiment shock while dark grey and light grey areas represent 68% and 90% confidence bands based on the Montiel-Olea *et al.* (2021) weak IV robust standard errors. The sample period is 1965:1–2007:8.

worsening labour market conditions, or to the Fed responding directly to consumer confidence. Our approach is not able to tell these two options apart, but the results indicate some leaning against the wind on the part of monetary policy.

Turning to stock market prices, we find that the decline in consumer sentiments gives rise to a persistent drop in real equity prices which is, however, statistically insignificant at the 90% level apart from some short periods that occur with more than a 2 years delay (at the 68% level, significance occurs for some short periods 1 year after the shock and more persistently 20 months after the decline in consumer sentiments). Macroeconomic uncertainty rises in the first year at the 68% level and at the 90% level only in the first 3 months after the sentiment shock.

In order to explore impacts on macroeconomic aggregates in more detail, we introduce other variables into the VAR one at a time. Given the considerable impact of the consumer sentiment shocks on unemployment, we first take a deeper look at other key labour market variables. Of particular interest is the impact on firms' hiring activities and on the overall state of the

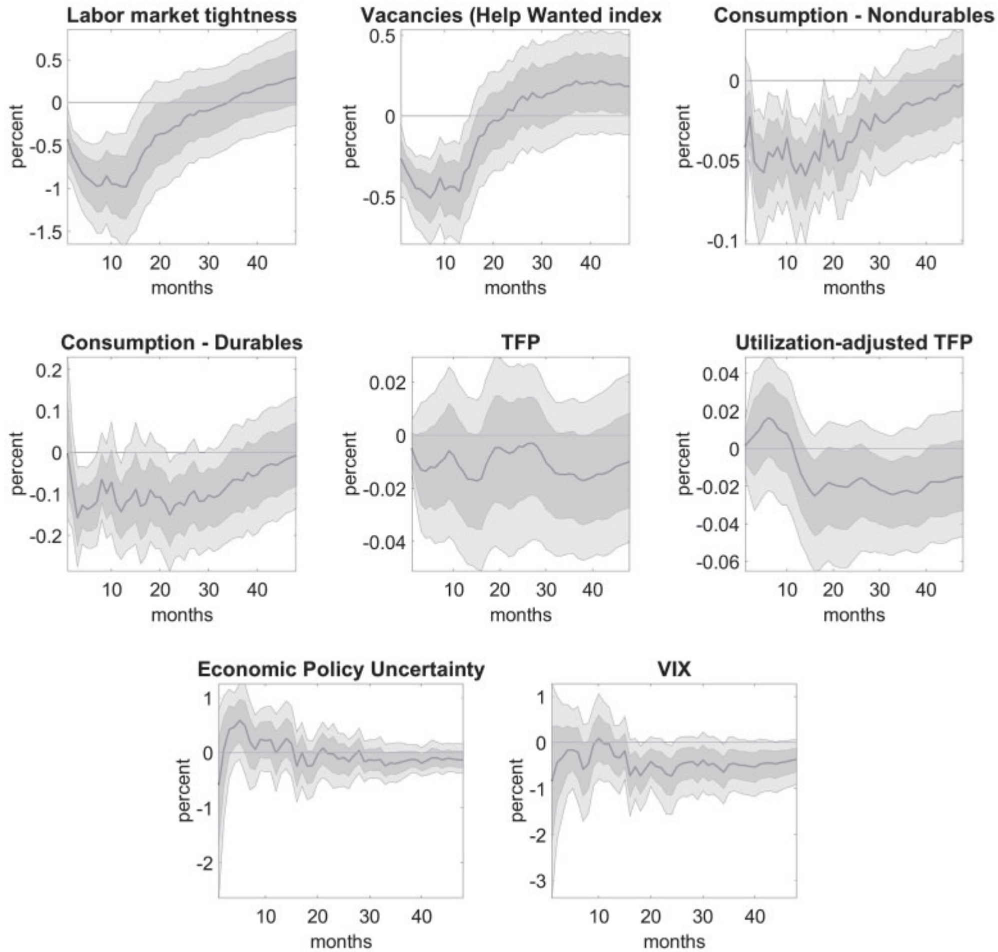

FIGURE 6

Consumer sentiment shock impulse responses—additional variables

Notes: The graph plots impulse response functions to a/sentiment shock. The continuous line depicts point estimates of the impact of the identified sentiment shock while dark grey and light grey areas represent 68% and 90% confidence bands based on the Montiel-Olea *et al.* (2021) parametric bootstrap. The sample period is 1965:1–2007:8.

labour market. Figure 6 shows that labour market tightness, the ratio of vacancy postings to unemployment, declines very persistently and statistically significantly so for 15 months at the 90% level (and almost for 2 years at the 68% level). The worsening of labour market tightness derives from the combination of higher unemployment discussed above and a large decline in firms' vacancy postings which is significant at the 90% level for 16 months.

Figure 6 also reports the impact on household real spending on non-durable and durable consumption goods. The decline in aggregate activity produced by deteriorating consumer sentiments is accompanied by a contraction in private sector consumption. Non-durable consumption spending falls on impact and the decline is statistically significant at the 90% level for over 2 years. Spending on durable consumption goods also declines and does so in a more elastic way but the standard errors of the impulse responses of this variable are quite large. The decline in industrial production and in private sector consumption in combination with the worsening labour market conditions would be consistent with the idea that autonomous changes in consumer sentiments are related to “demand shocks”.

Turning back to the benchmark VAR, a priori, one would perhaps expect the sentiment shock to induce significant downward pressures on prices of goods and equities. However, our estimates indicate no evident decline in the CPI, and while we do find a decline in stock prices, the standard errors are large. One possible explanation comes from the monetary accommodation. The decline in the nominal interest rate and the weak response of prices imply a decline in the real interest rate which, through a standard discounting effect, stimulates asset prices. Thus, the net effect on the stock market will depend on whether the direct recessionary effects of the sentiment shock dominate or not the discounting effect.[24] We have investigated whether the behaviour of the Fed can potentially explain the stock market responses by performing a counterfactual exercise in which we compute the impulse responses of stock prices when we combine the negative consumer sentiment shock with a series of contractionary monetary policy shocks computed such that there is no change in the short-term nominal interest rate at all.[25] Such an exercise is at best indicative and subject to the Lucas critique but may still suggest whether the monetary policy response is potentially important for the lack of a strong stock price decline. The results of this exercise suggest that, in the absence of the monetary policy response, stock market prices would have fallen 2–5 times more in the medium run (6 months to 2 and a half years horizon) in response to the sentiment shock.

Finally, it is well-recognized that there is some instability in the impact of identified macroeconomic shocks over time. Ramey (2016), reports, for example, instability in the impact of monetary policy shocks when comparing samples going up to the mid-1990s with late sub-samples. While the sources of such instability are unknown, they may also matter for our analysis. For this reason, in Figure 6 in the Supplementary Appendix, we report results when estimating the proxy SVAR for the sample period 1965:1–1995:6. For this earlier sample period, we find a negative impact on both stock prices (on impact) and on the CPI (with a lag) in response to the identified sentiment shock, which would be more in line with the standard intuition about the impact of negative demand shocks.[26]

In summary, our findings indicate that the autonomous component in consumer confidence that we identify with the external instrument, has important aggregate consequences in particular on the labour market but also on industrial production and private sector consumption. The severe labour market ramifications of sentiment shocks are shown by Pappa *et al.* (2021) to be consistent with an incomplete markets model with labour market frictions in the style of Ravn and Sterk (2021) when modelling sentiments either as responses of the economy to noise shocks (in an imperfect information setting) or as stochastic sunspots (in a multiple equilibria environment).

24. Although there is some evidence that mass shootings affect stock returns of firearms manufacturers (see e.g. Gopal and Greenwood, 2017), when we look at the S&P500 we do not find any significant reaction of stock market prices to mass shootings. If we use mass shootings as an instrument for movements in stock market prices the F-statistics indicate that mass shootings do not affect the stock market prices significantly, alleviating possible concerns also about the exclusion restriction.

25. In particular, we identify a monetary policy shock through a timing assumption as in Christiano, Eichenbaum and Evans (2005) (ordering it as last in the baseline VAR). We then compute a series of monetary policy shocks over the forecast horizon so that the combined effect of the sentiment shock and the monetary policy shocks on the short-term nominal interest rate is zero.

26. We note that despite stock prices falling on impact in this sub-sample, the response of TFP (measured with or without adjusting for utilization) is insignificant, confirming that our structural sentiment shock is not capturing TFP news shocks.

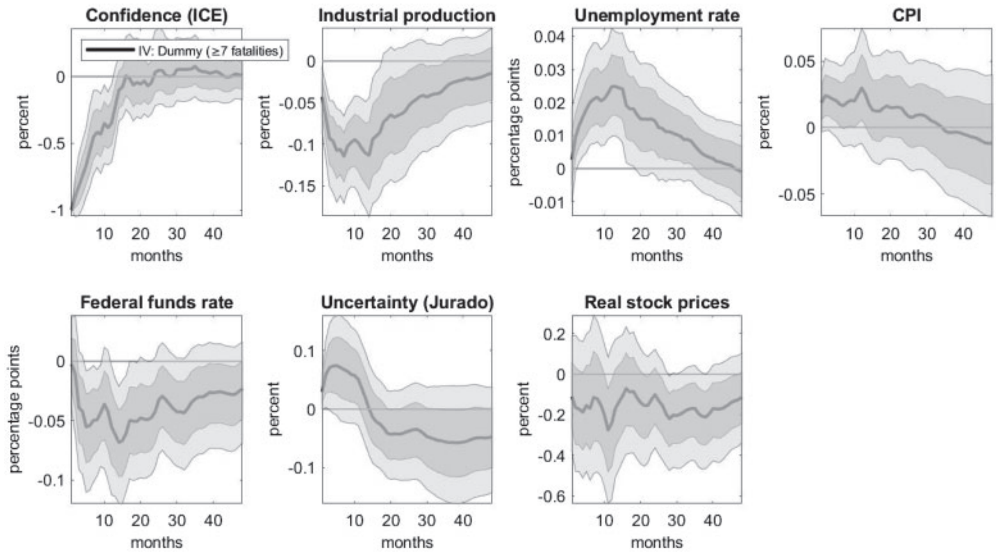

FIGURE 7

Alternative IV: mass shooting dummies, 1965m1–2007m8

Notes: The graph plots impulse response functions to a sentiment shock, for the specification in which we use dummy variables instead of fatalities in mass shootings as an instrument for ICE for the sample period 1965m1–2007m8. The continuous line depicts point estimates of the impact of the identified sentiment shock while dark grey and light grey areas represent 68% and 90% confidence bands based on the Montiel-Olea *et al.* (2021) weak IV robust standard errors.

5.2. *Robustness to measurement of the instrument*

Our baseline estimates rely on the use of detrended fatalities in mass shootings with seven or more casualties as the IV. We now investigate the robustness of results to alternative versions of our instrument.

To test for robustness to shooting outliers, we use a dummy for the months in which mass shootings with seven or more fatalities occurred as the IV. As discussed earlier, the weak instrument tests for this alternative instrument are very similar to the baseline instrument, see Table 1. The impulse responses presented in Figure 7 are nearly identical to those for the baseline results. Figure 8 illustrates the impulse responses estimated for the longer sample up to November 2018 using this instrument. The results are again very similar to the baseline results, but confidence bands widen due to the decline in the instrument strength plausibly resulting from the increased frequency of mass shootings towards the end of the sample.

One might wonder whether the results depend at all on the external instrument. To investigate this, Figure 9 shows the impulse responses when we carry out a placebo test in which we randomly reshuffle the instrument. To be precise, we draw the dates of the mass shootings from a uniform distribution and estimate the impulse responses with 10,000 replications. We then plot the median of the point estimates of the impulse responses at each forecast horizon together with the 90% and 68% estimates of the impulse responses over these simulations. In this case, not surprisingly, the instrument is insignificant and, as indicated by the impulse responses, we get no significant effects. Although this is difficult to see in the graph due to the wide confidence bands, the medians of the point estimates of the impulse responses are very similar to those generated by identifying consumer confidence shocks with a timing assumption implemented through a Cholesky decomposition of the covariance matrix that we discuss below. As discussed

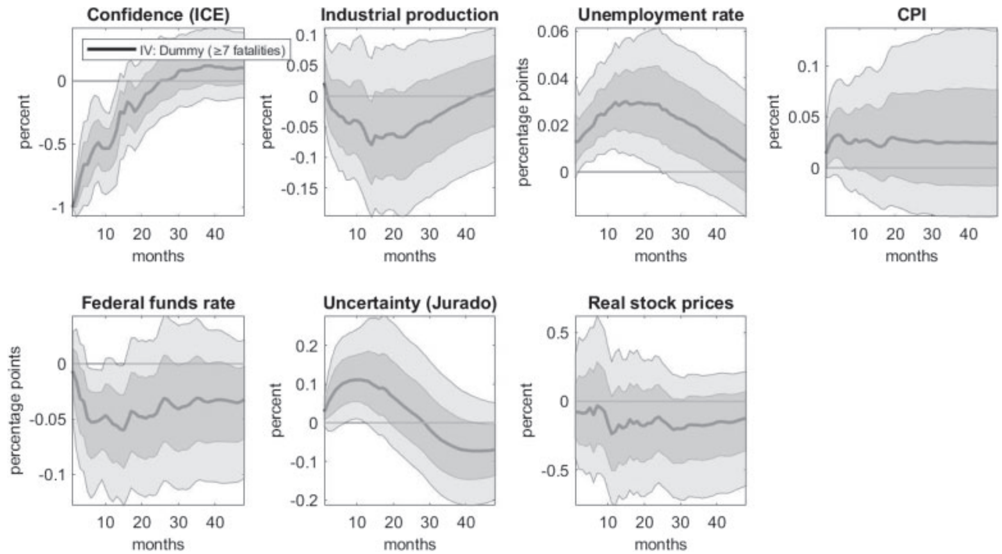

FIGURE 8

Alternative IV: mass shooting dummies, 1965m1–2018m11

Notes: The graph plots impulse response functions to a sentiment shock, for the specification in which we use dummy variables instead of fatalities in mass shootings as an instrument for ICE for the sample period 1965m1–2018m11. The continuous line depicts point estimates of the impact of the identified sentiment shock while dark grey and light grey areas represent 68% and 90% confidence bands based on the Montiel-Olea *et al.* (2021) weak IV robust standard errors.

by Montiel-Olea *et al.* (2021), this is due to the insignificance of the instrument in the placebo exercise.[27]

5.3. *Other shocks*

An important check on our results is the extent to which the identified consumer sentiment shock may be confounded with other shocks. Barsky and Sims (2012) study the impact of innovations to consumer confidence using a Cholesky decomposition of the covariance matrix on quarterly US data and argue, on the basis of a DSGE model, that the responses are consistent with consumer confidence innovations mainly reflecting news about future TFP.

We now augment the vector of observables with the (latest vintage of the) TFP series of Fernald and Wang (2016).[28] We find that TFP, both measured with or without adjusting for utilization, is unresponsive to the identified consumer sentiment shock. The responses are insignificant at the 90% level for both series at all forecast horizons, and at the 68% level for all horizon for TFP at all horizons and at all but a few sporadic horizons for utilization adjusted TFP (see Figure 6). Hence, the sentiment shock identified with the external instrument does not appear to be a news shock about TFP.

Along the same lines, we study the responses of two additional measures of uncertainty (on top of the Jurado *et al.* (2015) measure of aggregate uncertainty that we include in the baseline VAR). In Figure 6, we report the impact of sentiment shocks on a news coverage-based indicator

27. Also, as we demonstrate in the Supplementary Appendix, there is no correlation between mass shootings and recessions, as well as no signs of seasonality.

28. Note that these data are only available at a quarterly frequency, and to perform this exercise, we linearly interpolate into monthly frequency.

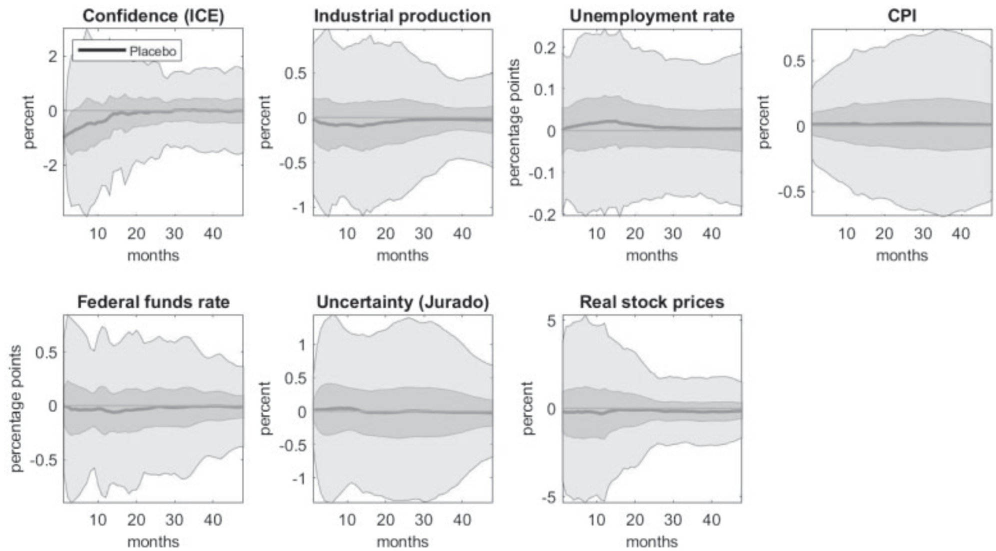

FIGURE 9

Proxy SVAR: placebo with reshuffled shootings

Notes: The graph plots the median with the 90% and 68% estimates at each forecast horizon of the impulse responses over 10,000 replications in which we carry out the placebo test by randomly reshuffling the instrument by drawing the dates of the mass shootings from a uniform distribution.

of economic policy uncertainty (EPU) constructed by Baker *et al.* (2016). Our estimates indicate a negative but insignificant impact of sentiment shocks on the EPU measure. Figure 6 also reports the impact of the identified shock on an asset price-based uncertainty measure, the VIX index of US stock market option price volatility. We find a negative but insignificant short-term impact, while for longer forecast horizons the asset price uncertainty drop is significant (but only at the 68% confidence level). Thus, we find no evidence of sentiment shocks being confounded by uncertainty shocks.

Hypothetically, mass shootings might be perceived to impact on future taxation due to an increase in spending on policing and security. In the Supplementary Appendix, we show that our identified shock does not Granger cause the exogenous tax changes series of Romer and Romer (2010) (nor is it Granger caused by the tax changes) which would appear to alleviate such concerns.

5.4. *Cholesky decomposition*

In order to assess the benefits of our identification procedure, we present the responses to a sentiment shock identified through a timing assumption (instead of the IV), implemented by imposing a triangular structure on the covariance matrix as in Barsky and Sims (2012).[29] Although responses of the macro variables look qualitatively similar to the ones estimated using the Proxy SVAR, there are significant differences.

Figure 10 shows that when using the Cholesky decomposition, real stock prices decline contemporaneously with the impact of the shock. Figure 11 also reveals that TFP falls significantly

29. We show results for ordering the ICE first in the VAR. Ordering it last, as Barsky and Sims (2012) do, produces the same macroeconomic effects and a significant decline in utilization-adjusted TFP with a year's delay, indicating that the shock identified with a timing assumption reflects partially a TFP news shock.

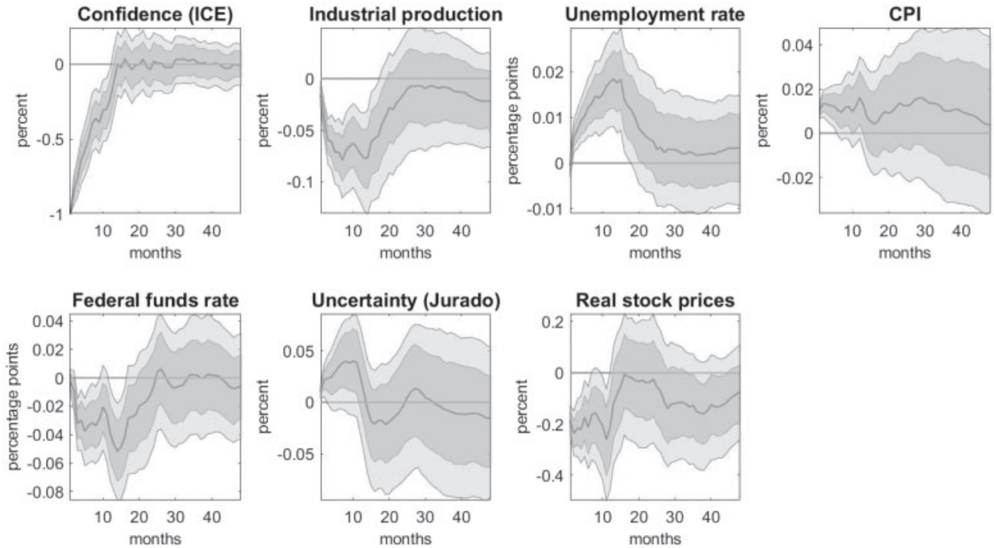

FIGURE 10

Cholesky SVAR—baseline variable responses

Notes: The graph plots the impulse responses to a confidence shock identified by imposing a triangular structure on the covariance matrix as in Barsky and Sims (2012). The continuous line depicts point estimates of the impact of the identified sentiment shock while dark grey and light grey areas represent 68% and 90% confidence bands.

reaching a peak decline around a year after the shock (or 1.5–2 years in the case of the utilization-adjusted TFP series, although this effect is only borderline significant). This would be consistent with the idea that identifying the sentiment shock with a timing assumption produces an innovation that confounds the sentiment shock and TFP news. These findings, thus, go some way towards reconciling our results with those of Barsky and Sims (2012) because of the differences between our identified shock and the innovation to consumer confidence that these authors study. In addition, Figure 11 also shows that the Cholesky decomposition implies an immediate and significant increase in the VIX and in economic policy uncertainty indicators in response to a negative sentiment shock. This indicates that, using a Cholesky decomposition, possibly induces some confounding of the identified shock with uncertainty shocks.

5.5. *Local projections*

A major advantage of the proxy SVAR estimator adopted above is that it provides a parsimonious description of the data, where the dynamic causal effects are functions of $\mathbf{A}(\mathbf{L})$ and the identified column of Θ_0 only. On the other hand, the VAR model imposes invertibility so that the shocks can be derived from current and past values of the observables. Invertibility may be an issue for our analysis to the extent that consumer confidence reacts to news about future fundamentals. One concern in this respect is that we find hump-shaped responses of both industrial production and unemployment to the identified shock.

For robustness analysis, we therefore also derive dynamic causal effects on the basis of a local projection estimator, which imposes less restrictive assumptions. In particular, we apply the LP-IV estimator (previously used by Fieldhouse *et al.* (2018), Ramey and Zubairy (2018), and Stock and Watson (2018)). Plagborg-Møller and Wolf (2019) show that this estimator can be used to derive forecast error variance decompositions bounds under a recoverability condition (which

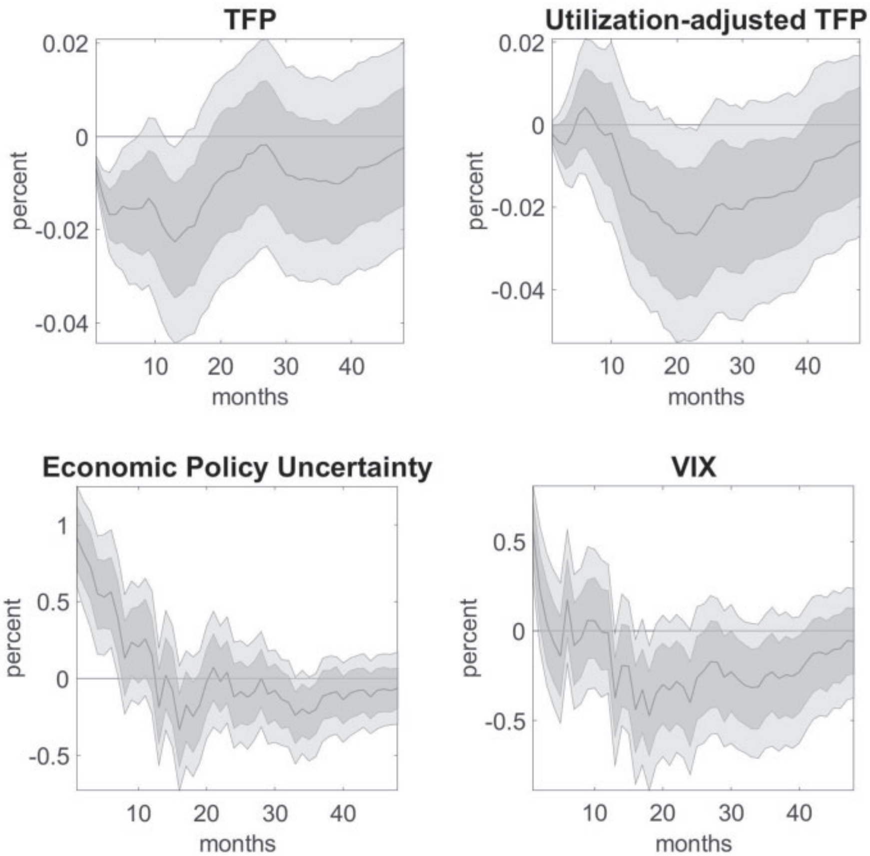

FIGURE 11

Cholesky SVAR—other variables

Notes: The graph plots impulse response functions to a sentiment shock, identified by imposing a triangular structure on the covariance matrix as in Barsky and Sims (2012). The continuous line depicts point estimates of the impact of the identified sentiment shock while dark grey and light grey areas represent 68% and 90% confidence bands.

is weaker than the invertibility condition) which allows the shock of interest to also depend on future values of the observables.

For identification, we now add to condition (4.5):

$$\mathbb{E}\left(\mathbf{s}_t \mathbf{e}_{it+\tau}\right) = 0, \ \forall i, \tau \neq 0 \tag{5.1}$$

which states that the proxy should be orthogonal to both leads and lags of the structural shocks.

The impulse response functions for a horizon going up to H periods are derived as the estimates of $(\gamma_h)_{h=0}^{H}$:

$$y_{i,t+h} - y_{i,t-1} = \alpha_h + \gamma_h ic_t + \varphi_h(\mathbf{L})\mathbf{Y}_{t-1} + \varepsilon_{i,t+h}, \ h = 0,..,H \tag{5.2}$$

where y_{it} is the i'th variable of $\mathbf{Y}_t$. This relation is estimated using $\mathbf{s}_t$ as an instrument for ic_t using a two-stage least squares procedure. We specify the control variables, $\mathbf{Y}_{t-1}$, exactly as in the proxy SVAR application. Hence, the first stage is identical to above. Notice that since the control variables are the same as in the Proxy SVAR application, the LP estimates of the impulse responses would, under some standard regularity conditions, be identical to those of the Proxy SVAR in an infinite sample. However, the two sets of estimates may still differ in a small sample.

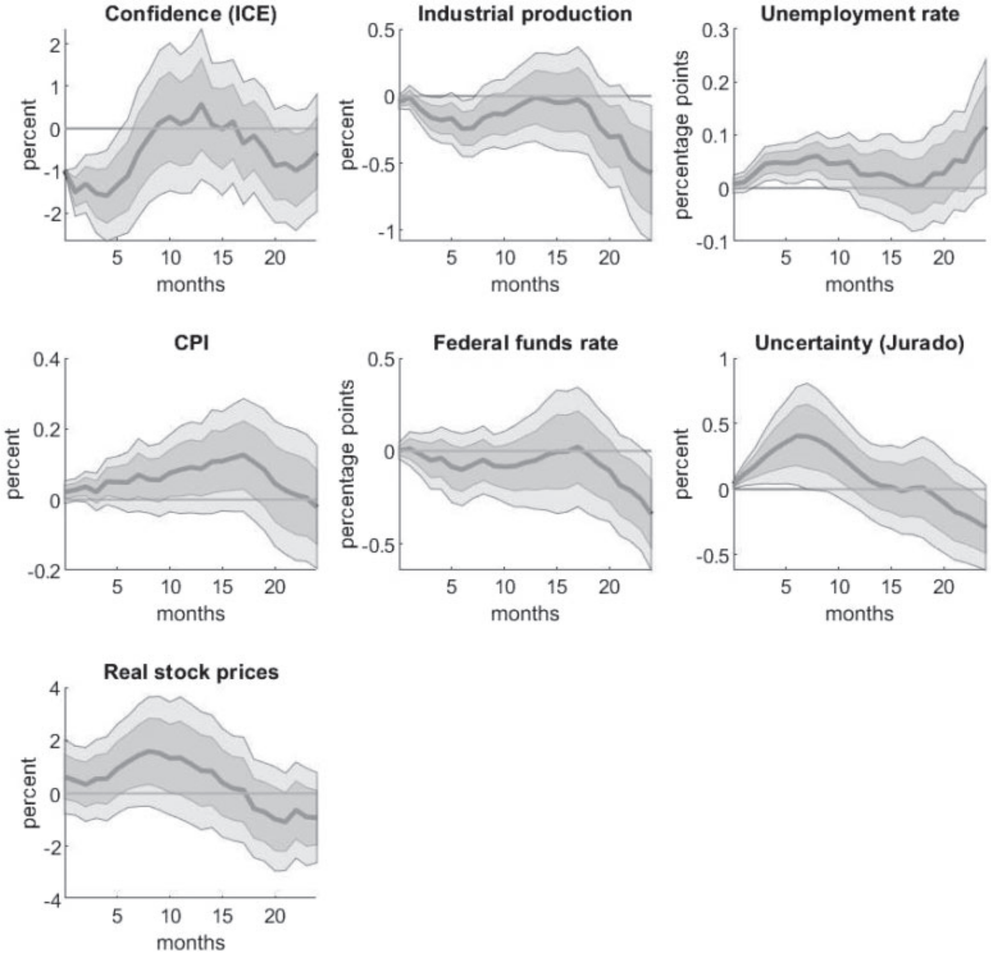

FIGURE 12

Consumer sentiment shock IRF—using LP-IV

Notes: The graph plots impulse response functions to a sentiment shock using the LP-IV methodology. The continuous line depicts point estimates of the impact of the identified sentiment shock while dark grey and light grey areas represent 68% and 90% confidence bands based on the Newey–West estimator. The sample period is 1965:1–2007:8.

Figure 12 illustrates the impulse responses of the macro variables included in the benchmark VAR, estimated using the LP-IV estimator with 18 lags of the observables as controls as above (with 68% and 90% Newey–West confidence bands). We show the impulse responses for a horizon (H) up to 24 months.

The results are qualitatively very similar to those in Figure 4 but somewhat less significant. Confidence bands are expected to be wider since with the LP-IV approach parameters are estimated for each projection horizon, while with the Proxy SVAR parameters are estimated only once for the entire projection horizon. We find that, in response to a negative sentiment shock, consumer confidence falls for about 8 months, and significantly so for the first 6 months. Thereafter, confidence recovers. Industrial production also falls for around a year and significantly so for 6–8 months in response to the negative sentiment shock. While the impact on aggregate activity is less persistent than the one obtained from the VAR, the peak decline estimated with local projections is actually larger than what we found using the VAR estimator, hence, illustrating the sizeable impact of consumer sentiments on the economy. We also confirm that a negative consumer sentiment shock induces a weakening of the labour market. In particular, the unemployment rate rises significantly for almost a year and we show in the Supplementary Appendix that also vacancies and labour market tightness fall significantly. Similar to the results for industrial production, the labour market responses estimated with LP-IV are larger at peak impact but less persistent than the ones we derived using the proxy SVAR estimator.

Similar to the Proxy SVAR estimation, the LP-IV estimation also produces a decline in the nominal interest rate, though this decline is insignificant at the 90% confidence level. Furthermore, in line with the earlier results, we find an increase in consumer prices in response to the decline in consumer confidence, but standard errors are large and do not rule out no response at any forecast horizon at the 90% level. Likewise, the response of stock prices remains insignificant, while uncertainty increases with a lag and its response is significant 2–5 months after the shock. In the Supplementary Appendix, we also present impulse responses using the LP-IV methodology for the rest of the variables we present in the SVAR exercise.

5.6. *Business cycle contributions*

We now examine the extent to which sentiment shocks may matter for business cycle variations. Using the LP-IV estimator, we evaluate the contribution of the shocks by imposing the associated weaker assumption of recoverability, using the FVR statistic developed by Plagborg-Møller and Wolf (2019):

$$\mathbf{FVR}_{i,h} = 1 - \frac{\mathbf{var}\left(y_{i,t+h} | (\mathbf{Y}_\tau)_{-\infty \le \tau \le t}, (\mathbf{e}_{1\tau})_{-\infty \le \tau \le t}\right)}{\mathbf{var}\left(y_{i,t+h} | (\mathbf{Y}_\tau)_{-\infty \le \tau \le t}\right)},$$

where $\mathbf{var}(x_t|z_s)$ denotes the variance of x_t given z_s. The $\mathbf{FVR}_{i,h}$ statistic thus measures the reduction in the forecast variance of variable i at horizon h induced by (hypothetically) knowing the sequence of the identified sentiment shocks. Plagborg-Møller and Wolf (2019) demonstrate that the $\mathbf{FVR}_{i,h}$ statistic is set identified for a scale parameter α (which is related to the absolute impulse response). By relying only on recoverability of the shock, the FVR metric is robust to invertibility concerns that are inherent to VAR estimators.[30]

Plagborg-Møller and Wolf (2019) show that the lower bounds of the identified sets correspond to the case of having a perfect instrument. Measurement errors induce attenuation, in which case the true reductions in the forecast variances are larger than those indicated by the lower bounds. The weak instrument tests and the nature of the instrument leads us to expect attenuation to be relevant. For that reason, the lower bounds of the FVR will underestimate the true reductions in the forecast variances that would be attained by knowing the sequence of the sentiment shock. Instead, as highlighted by Plagborg-Møller and Wolf (2019), the upper bounds are monotonically decreasing as one adds variables (and lags) to the set of variables in the controls. Our rich set of controls, therefore serves also for providing conservative estimates of the upper bound on the FVR.

Figure 13 presents the point estimates of the identified sets and the 90% confidence bands for the FVR statistics for the consumer sentiment shock that we identify for forecast horizons going

30. In fact, the R_l^2 measure of invertibility proposed by Plagborg-Møller and Wolf (2019) indicates that invertibility may be an issue for low values of l but not for $l \ge 4$ months and that the estimated FVR upper bounds are "informative" after 4 months.

FIGURE 13

Identified sets and confidence bands of FVRs

Notes: The graph plots the point estimates and the 90% confidence intervals for the identified sets of FVRs, across different variables and forecast horizons. Bias-corrected estimates/bounds are set to lie in the [0, 1] interval. The sample period is 1965:1–2007:8.

up to 5 years. For the ICE measure of consumer confidence, we find that the point estimates of the upper bounds of the identified set of the FVR are close to 20% for most forecast horizons apart from shorter ones, when it reaches around 30–40%. The 90% confidence bands for the identified set are quite wide, though. At the 90% level, we can rule out that sentiment shocks account for more than 30% of forecast variance of the ICE at forecast horizons beyond 1 year, while we can only rule out a contribution above 55% at the 6-month horizon. Thus, the evidence points towards the ICE being sensitive to non-fundamental shocks especially at short forecast horizons. The point estimate of the lower bounds are close to 5% for the forecast horizons that we consider. As previously mentioned, the nature of the instrument and measurement error leads us to expect that the lower bound underestimates the true contribution of the sentiment shock to variations in consumer confidence.

For industrial production, the sentiment shock contributes little at very short horizons below 4 months. At forecast horizons between 6 months and 1 year, the point estimates of the upper bounds of the identified set exceed 20% and the 90% confidence band allow us to rule out that the confidence shock account for more than 30% of the forecast variance of industrial production. At longer forecast horizons, the point estimates of the upper bounds of the identified set of the FVR statistic hover at levels close to 20–25%, and we can only rule out that a forecast variance reduction of industrial production from knowledge of sentiment shocks above 35–40%. The results for unemployment are similar to those for industrial production. The point estimates of the upper bounds peak at close to 30% at the 1 year horizon where we can only rule out a contribution of the sentiment shock to the FVR above 40%. For forecast horizons beyond 2 years, we can to rule out that sentiment shocks account for more than 25–30% of the forecast variance of the unemployment rate. The lower bounds of the FVR statistics are small for both industrial production and unemployment, for reasons already explained.

Additional results presented in the Supplementary Appendix reveal that sentiment shocks also may account for a significant fraction of the fluctuations in consumption, labour market tightness, and firms' vacancy postings. For consumption of both durable goods and non-durables, the 90% confidence bands of the FVR statistic peak at around 35–40%, and remain above 30% for forecast horizons beyond 20 months. For vacancies and labour market tightness, the 90% confidence band peak at 26% and 30%, respectively, at the 6-months horizon. At longer horizons, the sentiment shock appears less relevant for vacancies, while for labour market tightness we can only rule out a forecast variance reduction due to sentiment shocks above 20–25% at the 90% level.

In contrast, it appears that the identified shock does not matter much for real stock prices. In particular, at short forecast horizons going up to 6 months, this shock is irrelevant for the stock market. At longer forecast horizons, we can rule out that it accounts for more than 15–18% of the FVR. Similarly, there is very little contribution of the sentiment shock to the CPI, with the point estimates of the upper bounds rarely exceeding 10% and the 90% bands allowing us to rule out contributions to the forecast variance of CPI above 15% at most horizons. The results for the short-term nominal interest rate indicate little importance of sentiment shocks for this variable for forecast horizons up to 30 months. Beyond this horizon, the 90% bands for the identified allow us only to rule out a contribution above 20–30%. Yet, taken as whole, the results indicate that sentiment shocks are not an important source of variation in nominal interest rates.

We conclude that these FVR results suggest important contributions of sentiment shocks to business cycle variations in the real economy. This is also consistent with the Forecast Error Variance Decomposition based on the Proxy SVAR that we report in the Supplementary Appendix.

The finding that sentiment shocks contribute significantly to macroeconomic fluctuations in real output and unemployment is consistent with other papers such as Blanchard *et al.* (2013), Chahrour and Jurado (2022), and Levchenko and Pandalai-Nayar (2020), who all find a significant contribution of pure expectational shocks to macroeconomic fluctuations even if these contributions do not agree quantitatively about the importance of this source of volatility. Blanchard *et al.* (2013) find a much larger contribution to consumption fluctuations at short forecast horizons than our estimates; Chahrour and Jurado (2022) find overall much larger contributions of noise shocks to macroeconomic volatility at business cycle frequencies; and Levchenko and Pandalai-Nayar (2020) find a larger contribution to output fluctuations at short horizons than we do. These differences could be due to our use of higher frequency data or, more likely, due to differences in the identification strategy. Regardless, each of these contributions agree on the fact that shocks unrelated to economic fundamentals appear to be an important source of impulses to the US business cycle. We add to this the insights that consumer sentiments appear important as a source of fluctuations in key labour market aggregates.

6. CONCLUSIONS

Aggregate fluctuations in the economy derive from many different sources. A large and important literature has produced causal evidence on the impact of a large number of shocks, such as monetary policy shocks, various sources of fiscal shocks, trade policy shocks, productivity shocks, financial shocks etc. These types of shocks all share the common feature that they are "fundamental" in terms of relating to primitives that impact on the economy through production possibilities, impacting on constraints that agents face when making decisions, or through policy variables that constrain or influence behaviour.

Much work in macroeconomic theory has also addressed the possibility that fluctuations may derive from other sources relating to expectations that impact on behaviour beyond those related to changes in fundamentals. This is, for example, the case in models where agents' information sets are coarse so that households and firms face signal extraction problems when drawing inference on the fundamentals and open up the possibility that they respond to "noise". Expectations also matter in models where there is either local or global indeterminacy, in which case sunspots (and other phenomena) may impact on choices.

For this second type of impulses to the business cycle, there is comparatively little empirical evidence, perhaps due to challenges associated with measuring such sources of fluctuations from observational data. In this article, we have tried to address this issue by means of an external instrument for autonomous fluctuations in survey evidence on consumer expectations. We draw on the ICE data produced by the University of Michigan's Survey of Consumers and propose to use a source of pessimism—that we argue is unrelated to economic fundamentals—as an instrument. We show that our instrument, namely fatalities in mass shootings, impacts significantly on consumer expectations, and use it to identify an autonomous component of the ICE. Using dynamic estimators, we then show that this component, which we call *consumer sentiments*, has significant macroeconomic impact particularly on real output and labour market outcomes.

Providing further support for our instrument, we also present evidence that the significant effects of mass shootings on sentiments identified at the national level are confirmed using individual level data. Consumer expectations are shown to drop for individuals residing in the county where the mass shooting takes place, relative to individuals living in other counties, controlling for both time and individual fixed effects.

It would be interesting, in future work, to extend the idea underlying our identification approach to other sources of news unrelated to economic fundamentals and focus on other indicators of expectations, such as those relating to professional forecasters, which may impact differently on the economy. It would also be interesting to extend our analysis to other countries for which similar types of dramatic events permit the use of the IV strategy. Such analysis could help establish external validity of our results.

A key question not addressed in the current article regards the relationship between the empirical evidence on consumer sentiment shocks and economic theory. Pappa *et al.* (2021) make some progress on this, showing how incomplete information combined with labour market frictions and lack of insurance against unemployment generate a dynamic response to noise shocks that closely resembles those estimated in the current article. Equally important is the question of how to design stabilization policy to insulate the economy from such non-fundamental sources of aggregate fluctuations.

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Supplementary Data

Supplementary data are available at *Review of Economic Studies* online. And the replication packages are available at https://dx.doi.org/10.5281/zenodo.6798963.

Data Availability Statement

The data and code for this article are made available through the Zenodo digital repository at https://doi.org/10.5281/zenodo.6798963.

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