The response of CO_2 emissions to the business cycle: New evidence for the U.S.

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Abstract

This paper investigates the response of CO_2 emissions to the business cycle for the U.S. on a monthly basis between 1973-2015. Using a rolling-regression approach, we find that the emissions elasticity with respect to GDP is not constant over time, irrespective which filtering method, such as the Hodrick-Prescott, the Baxter-King, the Christiano-Fitzgerald or the Butterworth filter has been employed. In order to check whether or not emissions react differently during normal and recession times, next, we employ a Markov-switching approach. We find, first, that emissions are significantly more elastic during recessions than in normal times. Second, depending on the filtering method, we also obtain parameter estimates of the emissions elasticity above one in recession times and below one in normal times. The results are also robust against including monetary policy also in times of the zero lower bound. Thus, environmental policy instruments not turning out to be sub-optimal should account for this asymmetric response of emissions due to changes in GDP.

Keywords

Business cycle, CO_2 emissions, Rolling-regression, Markov-switching

JEL Classifications

Q54, Q58, E32

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1 Introduction

Emerging literature has directed attention towards the question how business cycles affect optimal pollution and related climate policy endeavors (among others, see Fischer and Heutel (2013) for a superb overview). Heutel (2012) within an environmental DSGE framework, makes the crucial point that emissions taxes or quotas are pro-cyclical. Within a sensitivity study, Heutel (2012) also shows that the elasticity of pollution with respect to GDP¹ directly drives the optimal abatement policy, a fact that points to the central role the emissions elasticity plays in the calibration of Environmental DSGE-models.

For U.S. quarterly and monthly, Hodrick-Prescott filtered data, Heutel (2012) also provides an estimate of the emissions elasticity for a sample period from 1981-2003. He finds that emissions react inelastic with respect to GDP. This elasticity estimate has been since used in several succeeding contributions for calibration purposes of dynamic, environmental macro models (see Heutel (2012), Doda (2013, 2014) Grodecka and Kuralbayeva (2014)). In these frameworks, the inherent assumption is that the emissions elasticity is constant over time. Thus, the emissions elasticity cannot adjust to a change of the economic regime. On contrary, in recent contributions, Sheldon (2017) and Cohen et al. (2018) find a asymmetric behavior of the emissions elasticity for the U.S. and China, respectively. Hence, the findings of the latter contributions suggest that the emissions elasticity is not constant over time but changes when the economy changes its regime, e.g. a change from a recession to normal times et vice versa.

While the focus of the latter mentioned contributions is on emissions forecasting, our focus is on the sensitivity of the emissions elasticity estimate by employing competing data-filtering methods like the Hodrick-Prescott filter. These filters are routinely employed to obtain parameter estimates for the calibration of modern macroeconomic models. Focusing on a single country, Canova (1998) point to the fact that using different filtering methods might produce different outputs. Hence, it is surprising that this issue has not been subject of a comprehensive discussion when it comes to an estimation of the emissions elasticity. Besides that and from a policy maker's point of view, it is important to understand whether or not the choice of a specific data-filtering method influences the parameter estimate of the emissions elasticity. Thus, one focus of this paper is to distill a potential sensitivity of the estimation of the emissions elasticity by employing different filtering techniques. The other focus is to check whether or not the emissions elasticity significantly differs in different economic regimes.

Using monthly U.S. data from 1973m1-2015m11 and a rolling-regression approach, this paper finds that the emissions elasticity is not constant over time and this result is robust against using different data-filtering techniques². In particular, we find that emissions rise less sharply when GDP in-

¹In the following we use the expressions "elasticity of pollution with respect to GDP" and "emissions elasticity" interchangeably.

²According to de Hahn et al. (2008), frequently used, non-parametric filters comprise the Hodrick-Prescott (HP), the Baxter-King (BK), the Christiano-Fitzgerald (CF) or the Butterworth (BW) filter. For details refer to Hodrick and Prescott (1997), Baxter and King (1999), Christiano and Fitzgerald (2003), and Butterworth (1930). Sensitivity comparisons of

creases than they fall when GDP decreases. This makes sense as during the period from 1973-2015 several empirical contributions show that the U.S. economy was hit by occasional sudden breaks, which for example "divided" the economy in regimes of high or low growth phases³.

Thus, there is no reason to assume that, ex-ante, emissions respond to this asymmetric behavior of the GDP in a symmetric way. This motivates us to check whether or not emissions react differently during normal and recession times. Employing a Markov-switching approach, we find, first, that emissions are significantly more elastic during recessions than in normal times. Second, depending on the filtering method, we also obtain parameter estimates of the emissions elasticity above one in recession times and below one in normal times. The results are also robust against including monetary policy also in times of the zero lower bound.

These findings have not only direct consequences for deriving optimal climate policies and evaluate their welfare consequences within standard (DSGE) dynamic environmental models, which often cannot sufficiently handle (dramatic) breaks in economic time-series. The results also have direct consequences for forecasting future emissions, since an appropriate design of mitigation or adaption policies depends on an accurate forecast of future emissions (see Sheldon (2017)).

The plan of the paper is as follows. Section 2 provides a first view on the data. On the basis of a rolling-regression and a Markov-Switching-Dynamic-Regression framework, section 3 and section 4 investigate whether or not (i) the emissions elasticity is constant over time and, (ii) significantly differs in different economic regimes. Section 5 summarizes the main findings and discusses the results. Section 6 concludes.

2 A first view on the data

Monthly data for the U.S. CO_2 emissions are taken from the U.S. Energy Information Administration⁴ and show the total carbon emissions from U.S. fossil fuel combustion. As our data set comprises the period of the zero-lower bound (ZLB), we rely on the shadow rate proposed by Wu and Xia (2016) that measures the overall stance of monetary policy without imposing a lower bound constraint on the nominal interest rate. When it is greater than zero, this shadow rate is identical to the federal funds rate and negative under the binding ZLB constraint. However, this rate is only available until 2015m11⁵. The monthly data for the seasonally adjusted, real GDP stem from the Federal Reserve

filters in a different, but applied economic context are, unfortunately, rare. Closest to our study are the *cross-country* studies produced by Calderon et al. (2007), Massmann and Mitchell (2004), and Artis and Zhang (1997) who found that a particular filtering technique does not affect their conclusions regarding business cycle synchronization. In contrast, Canova (1998, p. 475) found that *within* a country, filtering techniques "extract different types of information" and, hence, different filtering techniques might lead to different results.

³See Doornik (2013) for instance.

⁴Emissions data are obtained online from the Monthly Energy Review, release date February 24th, 2017. See: <https://www.eia.gov/totalenergy/data/monthly/>.

⁵The data can be directly accessed from: <https://sites.google.com/view/jingcynthiawu/shadow-rates>.

Economic Database (FRED)⁶. We seasonally adjust the emissions data by employing the X-13-ARIMA methodology⁷. Our data-set comprises the time span between January 1973 and November 2015. Both series are normalized to the January 1973 levels so that the starting value is equal to one.

We follow Heutel (2012) and employ different filter methods to remove the trend from the data. Besides the Hodrick-Prescott (HP) filter which is commonly used to remove the trend from a time-series, the relevant literature proposes alternatives, such as the Baxter-King (BK) bandpass moving average filter, the Christiano-Fitzgerald (CF) random walk filter or the Butterworth (BW) rational square wave filter. These filters can be generally distinguished in high-, low-, or band-pass filters. The HP filter is designed as a high-pass filter which suppresses low-frequency fluctuations and which results are sensitive to the smoothing parameter λ . Since data are monthly, we set the smoothing parameter $\tilde{\lambda}$ at 129,600 as recommended by Ravn and Uhlig (2002)⁸. A major drawback of the HP filter is that the cyclical component is contaminated by (very) high frequency noise. Thus, Baxter and King (1999) argue that a combination of a filter that removes high frequencies (low-pass filter) and a high-pass filter should generally outperform the HP filter. This combination, the BK (band-pass) filter, produces a cyclical component that does not contain any fluctuations above or below predetermined cut-off points. The CF filter is an approximate band-pass filter which is built on the same principles as the BK filter. Both filters only differ in the approximation of an ideal band-pass filter: In contrast to the BK filter, the CF filter does not assume symmetric filter weights. In empirical applications, the BK (CF) filter produces more accurate results for shorter (longer) business cycles and is less (more) accurate when ends of data series are of particular interest. If it is further unclear whether the trend is stochastic or deterministic, it is advised to employ the BK filter, because this automatically removes the stochastic as well as the deterministic trends. For the BK as well as for the CF filter, we set the minimum and maximum periodicities at 18 and 96 months, respectively. Finally, the BW filter is a two parameter, square-wave high-pass filter. The two parameters are the cut-off period and the order of the filter. Gómez (1999) and Pollock (2000) point to the fact that the BW filter produces a better estimate of the cyclical component than the HP filter because the former ensures more flexibility. Moreover, the BW filter shares the symmetry property of the BK filter. However, the coefficients of the BW filter do not need to sum to zero. In this contribution, we rely on the computational method suggested by Pollock (2000) implementing the BW filter.

It is clear that the quarterly frequency is pervasive in the business cycle literature and constructing monthly GDP series might come along with a measurement error. However, as one impetus is to

⁶Real GDP data are available only on a quarterly level. We follow Heutel (2012) and converted them into monthly values. Quarterly level data are available from the U.S. Bureau of Economic Analysis (BEA). We retrieved them from FRED, see <https://fred.stlouisfed.org/series/GDPC1>. The Unit is: Billions of Chained 2012 Dollars.

⁷See <https://www.census.gov/srd/www/x13as/>.

⁸Hodrick and Prescott (1997) set $\lambda = 1,600$ for extracting business cycles in quarterly data. For monthly data, Ravn and Uhlig (2002) show that λ should be multiplied by the fourth power of the observation frequency ratio s . Hence, we have that $\tilde{\lambda} = 1,600(s^4)$ with $s = \frac{12}{4} = 3$. Thus $\tilde{\lambda} = 1,600(3^4) = 129,600$.

Table 1: Descriptive statistics

<i>Descriptive statistics</i>					
A. HP-Filter	Mean	Std. dev.	Skewness	Kurtosis	corr(e,y)
Cyclical component GDP	-1.60e-11	.0149	-.5011	3.7368	0.5338***
Cyclical component CO ₂	4.65e-11	.0275	-.3147	3.7193	—
B. BK-Filter	Mean	Std. dev.	Skewness	Kurtosis	
Cyclical component GDP	.0004	.0130	-.6420	4.2476	0.7309***
Cyclical component CO ₂	.0008	.0190	-.8398	3.8716	—
C. CF-Filter	Mean	Std. dev.	Skewness	Kurtosis	
Cyclical component GDP	-5.40e-5	.0129	-.3361	3.2552	0.6950***
Cyclical component CO ₂	-.0001	.0187	-.4077	3.3489	—
D. BW-Filter	Mean	Std. dev.	Skewness	Kurtosis	
Cyclical component GDP	-2.12e-11	.0125	-.5777	3.7990	0.4586***
Cyclical component CO ₂	-8.04e-12	.0258	-.2745	3.7750	—

Note: *** indicates a $\alpha = 0.01$ level of significance.

compare our results with Heutel (2012), we focus on monthly data. Further, a former version of this paper focuses on HP filtered, quarterly data. At least for this case, even if a measurement error exists, it does not qualitatively alter our results⁹.

Table 1 shows that the standard deviation of the cyclical component of emissions e is larger compared to the standard deviation of the cyclical component of GDP, y irrespective of which filter is employed. Hence, cyclical carbon emissions are more volatile than GDP, an observation that is consistent with Heutel (2012), Doda (2014) and Khan et al. (2015). Depending on the employed filtering method, the correlation coefficient between the cyclical component of emissions denoted as $\text{corr}(e,y)$ lies significantly between 0.4586 and 0.7309. The corresponding p -values are less than 0.0000. Thus, emissions are significantly pro-cyclical during the entire time span (see for instance Khan et al. (2015)).

The key purpose of this paper, however, is to check whether the elasticity of emissions with respect to GDP is stable over time. The next two sections deal with this issue.

3 The stability of the elasticity of emissions with respect to GDP over time: A rolling-regression approach

This subsection is concerned with the question whether the elasticity of emissions is stable over time. To shed light on this question, we first employ a rolling-regression approach, followed by a regime switching analysis, pushed forward by Hamilton (1990)¹⁰. For the rolling-regression analysis, we set the window size to 276 observations¹¹. We perform this analysis for each employed data filtering

⁹Also Heutel (2012) uses both, monthly as well as quarterly data and does not find qualitatively different results.

¹⁰For an introduction to the class of Regime-Switching models refer to Hamilton (1994), ch. 22.

¹¹We use this window size to mimic the length of the monthly data-set used by Heutel (2012).

technique by running the following basic regression:

$$e_t = \beta_0 + \beta_y y_t + \gamma z_t + \epsilon_t, t \in \mathbb{Z} \quad (1)$$

with ϵ_t as an independent and identically (iid) normal error with mean zero and variance σ_t^2 . z_t contains the shadow rate as a control variable for monetary policy in first differences¹². Accordingly, γ quantifies the reaction of emissions due to changes of monetary policy. The motivation for the inclusion of monetary policy stems from the fact that besides productivity shocks, nominal monetary policy shocks affect the real economy (and, hence pollution), as prices are rigid in the short-run (inter alia, see Woodford (2003) or Galí (2008)). In this way we control for different sources of the business cycle. Moreover, the inclusion of monetary policy is important when it comes to a design and evaluation of a specific environmental policy (see Fischer and Heutel (2013) and Annicchiarico and Dio (2015)). From this point of view, ex-ante it is relevant to know whether or not the estimation of the emissions elasticity with respect to output b_y is significantly affected by the inclusion of monetary policy¹³.

Figure (1) presents the estimates of the emissions elasticity with respect to output over time for different filtering techniques with (b_{my}) and without the inclusion of monetary policy (b_y). Each panel of Figure (1) shows the point-estimate of the emissions elasticity as well as 95%-bootstrapped-confidence intervals (each with 5,000 replications). For every sub-sample used in the rolling-regression analysis, the x-axis indicates the end of the data-set. For the purpose of comparison, the red-dashed, vertical line shows the end of Heutel's (2012) data-set which is the end of the year 2003.

The analysis shows the following: First, irrespective of which filtering-method is employed, the rolling-regression indicates an unstable behavior of the point as well as for the interval estimator of the emissions elasticity. Further, the estimates are robust against the employed data filtering method. For instance, during the years 1995 until 2003, the point estimate of the emissions elasticity indicates a pronounced elastic behavior. Second, only for a relatively short period between the years of 2005 and 2008 - the period that also comprises the start of the recent financial crisis - our analysis shows that emissions are indeed significantly inelastic with respect to GDP, irrespective of which filter method is employed. This particular observation justifies the use of the Markov-Switching-Dynamic-Regression framework, because we might obtain the impression that emissions react differently in different economic regimes. Third, as indicated by the vertical dotted lines in each panel of Figure (1), our results also show that the finding made by Heutel (2012), namely that the emissions are inelastic with respect to changes in GDP irrespective of which data filtering method is employed only holds true for a relatively short period of time and is therefore not representative for the behavior of emissions

¹²To exclude spurious regression estimates, we have to work with stationary variables.

¹³It should be noted that there are additional rigidities and frictions which might affect the business cycle and, hence, potentially our estimates, such as labor adjustment costs or wage rigidities. The inclusion of further controls might provide an answer to the question, which shocks are most important for explaining the behavior of emissions at the business cycle frequency. However, this is clearly beyond the scope of this paper. The contribution of Khan et al. (2015) is devoted to answering this question.

due to changes of the GDP for a longer time span. Moreover, as also shown with Figure (1) and Figure (3) in Appendix 7.1, our results are qualitatively unchanged if we further include the shadow rate of Wu and Xia (2016) as a measure of monetary policy stance in both the recent pre- and (post-) crisis periods¹⁴. Finally, Figure (4) and Figure (5) in Appendix 7.2 show that the results are also qualitatively robust against changing the rolling regression's window size. As each rolling-regression run comprises different compositions of boom and bust times of an economy per construction, we conjecture that the emissions elasticity is not invariant against boom and bust times. We want to explore this issue further with the help of a regime-switching approach in the next subsection.

Figure 1: Rolling-regression estimates of the emissions elasticity with (blue line) and without (green line) the inclusion of monetary policy for different filter methods. 95% bootstrapped confidence intervals (5,000 replications) are shaded light (with monetary policy) and dark (without monetary policy) gray.

Note: b_{ym} (b_y) represents the emissions elasticity estimate with respect to output with (without) monetary policy control. 95% CI_{ym} (95% CI) stands for the 95% confidence interval for the emissions elasticity estimate with respect to output with (without) monetary policy control.

¹⁴Indeed, Figure (3) in Appendix 7.1 shows that for some periods and depending on the filtering method, the inclusion of the shadow rate significantly affects the parameter estimates of the emissions elasticity with respect to output for some periods. However, as indicated by Figure (1), these changes, if significant, are relatively small, so that our results regarding the (elastic or inelastic) behavior of the emissions elasticity with respect to output remain *qualitatively* unchanged over time.

4 The stability of the elasticity of emissions with respect to GDP over time: A Markov-Switching-Dynamic-Regression (MSDR)

The so-called Markov-Switching-Dynamic-Regression (MSDR) model allows for shifts, first, in the intercept, second, in covariates, and third, in the variance. We use this approach to check whether the elasticity of emissions with respect to GDP is state-dependent. For example, with this approach we can check whether emissions drop more during recessions than in normal times. As mentioned before, an answer to this question is not only relevant for the design of an optimal environmental policy, but also begs the question whether standard solutions methods of DSGE models assuming constancy of parameters are the suitable framework for discussing the design of an optimal environmental policy. Hence, if we find evidence for a significant regime-switching behavior of the emissions elasticity, we should better rely on a Markov-Switching-Linear-Rational-Expectations (MSLRE) framework as proposed by Foerster et al. (2016), which explicitly allows parameters such as the emissions elasticity to change over time.

4.1 The theoretical model

The Markov-Switching-Dynamic-Regression (MSDR) can be formally written as

$$e_t = \beta_0^{s_t} + \beta_1^{s_t} y_t + \gamma^{s_t} z_t + \epsilon_{s_t}, \quad t \in \mathbb{Z}, \quad (2)$$

with e_t as the cyclical component of CO₂ emissions. y_t represents the cyclical component of GDP and z_t is a vector of regime-dependent, monetary policy control. Denote further the number of regimes by N with $i = \{1, 2, \dots, N\}$. The state variable s_t cannot be observed and evolves according to a Markov-Chain on $\mathbb{S}$. The corresponding $(N \times N)$ matrix of transition probabilities is given by $\mathbf{P} = [p_{ij}]_{N \times N}$, with $p_{ij} = \Pr(s_t = j | s_{t-1} = i)$, $i, j \in \mathbb{S}$, where columns entries must sum to unity. The diagonal probabilities p_{ii} can be used to measure the persistence of each state i . Thus, $\beta_k^{s_t}$ for $k = \{0, 1\}$ shows the vector of state-dependent coefficients. We are mainly interested in the point estimate of $\beta_1^{s_t}$, which gives us the emissions elasticity in state s_t . ϵ_{s_t} is an independent and identically (iid) normal error with mean zero and state-dependent variance $\sigma_{s_t}^2$. Finally, we assume that $\{\epsilon_t\}$ and $\{s_t\}$ are independent.

The likelihood of the MSDR model can be evaluated with Hamilton's (1990) filtering and Kim's (1994) smoothing method¹⁵. We numerically maximize the corresponding log-likelihood as a function of the parameter vector $\theta = (\beta_k^{s_t}, \gamma, p_{ij}, \sigma_{s_t}) \forall i, j$. Inter alia, we obtain time-varying, smoothed probabilities for model selection.

¹⁵For technical details, see Hamilton (1994, Chapter 22) and Krolzig (1997, Chapter 5).

4.2 Model selection

For the following reasons, we restrict the number of regimes to $R = 2$: First, for $R > 2$ and for some model specifications, it turned out that the EM-algorithm did not converge. Second, the recent contribution of Sheldon (2017) which also deals with emissions in the business cycle context describes the U.S. economy sufficiently with two regimes.

For each proposed data filtering method we estimate five different MSDR models. They can be distinguished as follows: *Model 1* assumes that both regimes exhibit the same degree of volatility ($\sigma_1 = \sigma_2 = \sigma$) but GDP is regime-dependent ($y_{1t} \neq y_{2t}$), *Model 2* assumes a regime-specific volatility but a regime-independent GDP ($y_{1t} = y_{2t} = y_t$) and, third, *Model 3* is a combination of *Model 1* and *Model 2*, and thus assumes a regime-dependent volatility and a regime-specific GDP. Across *Model 1-Model 3*, we further assume a regime-dependent monetary policy. *Model 3a* assumes a non-switching monetary policy, and finally, *Model 3b* completely neglects the influence of monetary policy.

When it comes to the decision which model is the best, we have to take into account the fit of the model to the data as well as the quality of regime classification. If we go through Table 2, it is not surprising that the model selection depends on the data-filtering method. If the information criteria are inconclusive, we prefer to work with more general models and models which include monetary policy as a control. We use standard t- and Wald-tests to check the significance (regime-dependent role) of the monetary policy control. Hence, if we focus on the HP filtered data, we chose *Model 3a* instead of *Model 3* as the first also controls for monetary policy. For the BK filtered data, we prefer *Model 1*, whereas for the CF filtered data *Model 3b* is chosen¹⁶. Finally, we work with *Model 3a* for the BW data.

4.3 Results

Panel A in Table 3 reports the estimation of the emissions elasticity with respect to GDP for the afore selected model specifications. In Panel B of Table 3, regime-specific volatility estimates are reported¹⁷. Moreover, Table 3 also presents the transition probabilities (panel C) as well as the regime-specific durations the economy stays in (panel D).

At a first glance, we observe significantly elastic as well as inelastic behaviors of the emissions with respect to changes in output. This outcome obviously depends on the filtering method employed. For instance, if we refer to the HP-filtered data, *Model 3a*, which assumes a regime-specific behavior of the GDP, indicate an inelastic behavior of the emissions with elasticity values of 0.93 (first regime) or 0.68 (second regime). Moreover, the estimates significantly differ between the regimes. These

¹⁶We also estimate *Model 3a* to check whether or not the inclusion of monetary policy changes the results obtained from *Model 3b*. We find no statistical significant differences.

¹⁷Estimates for the remaining parameters are available upon request from the authors. Due to space limitations we refrain from reporting these results.

Table 2: Model fit

<i>A. MSDR model with HP filtered data and switching monetary policy</i>				
Model specification	<i>AIC</i> ^a	<i>HQIC</i> ^b	<i>SBIC</i> ^c	$\mathcal{L}$ ^d
Model 1: Switching GDP ($y_{1t} \neq y_{2t}$)	-4.8410	-4.8119	-4.7668	1253.1475
Model 2: State-specific variance ($\sigma_1 \neq \sigma_2$)	-4.8381	-4.8122	-4.7721	1251.3983
Model 3: $y_{1t} \neq y_{2t}$ and $\sigma_1 \neq \sigma_2$	-4.8372	-4.8048	-4.7546	1253.1492
<i>MSDR model with HP filtered data and with non-switching (Model 3a) or without monetary policy (Model 3b)</i>				
Model 3a: $y_{1t} \neq y_{2t}$ and $\sigma_1 \neq \sigma_2$	-4.8410	-4.8119	-4.7668	1253.1491
Model 3b: $y_{1t} \neq y_{2t}$ and $\sigma_1 \neq \sigma_2$	-4.8365	-4.8106	-4.7705	1250.9872
<i>B. MSDR model with BK filtered data and switching monetary policy</i>				
Model specification	<i>AIC</i> ^b	<i>HQIC</i> ^c	<i>SBIC</i> ^c	$\mathcal{L}$ ^d
Model 1: Switching GDP ($y_{1t} \neq y_{2t}$)	-6.6716	-6.6388	-6.5883	1483.4309
Model 2: State-specific variance ($\sigma_1 \neq \sigma_2$)	-6.6330	-6.6038	-6.5589	1473.8885
Model 3: $y_{1t} \neq y_{2t}$ and $\sigma_1 \neq \sigma_2$	-6.6672	-6.6307	-6.5747	1483.4617
<i>MSDR model with BK filtered data and with non-switching (Model 3a) or without monetary policy (Model 3b)</i>				
Model 3a: $y_{1t} \neq y_{2t}$ and $\sigma_1 \neq \sigma_2$	-6.6669	-6.6340	-6.5836	1482.3813
Model 3b: $y_{1t} \neq y_{2t}$ and $\sigma_1 \neq \sigma_2$	-6.6695	-6.6403	-6.5955	1481.9655
<i>C. MSDR model with CF filtered data and switching monetary policy</i>				
Model specification	<i>AIC</i> ^b	<i>HQIC</i> ^c	<i>SBIC</i> ^c	$\mathcal{L}$ ^d
Model 1: Switching GDP ($y_{1t} \neq y_{2t}$)	-6.4622	-6.4331	-6.3880	1669.7938
Model 2: State-specific variance ($\sigma_1 \neq \sigma_2$)	-6.4677	-6.4418	-6.4016	1670.1909
Model 3: $y_{1t} \neq y_{2t}$ and $\sigma_1 \neq \sigma_2$	-6.4634	-6.4311	-6.3809	1671.0938
<i>MSDR model with CF filtered data and with non-switching (Model 3a) or without monetary policy (Model 3b)</i>				
Model 3a: $y_{1t} \neq y_{2t}$ and $\sigma_1 \neq \sigma_2$	-6.4652	-6.4361	-6.3909	1670.5508
Model 3b: $y_{1t} \neq y_{2t}$ and $\sigma_1 \neq \sigma_2$	-6.4685	-6.4426	-6.4025	1670.4070
<i>D. MSDR model with BK filtered data and switching monetary policy</i>				
Model specification	<i>AIC</i> ^b	<i>HQIC</i> ^c	<i>SBIC</i> ^c	$\mathcal{L}$ ^d
Model 1: Switching GDP ($y_{1t} \neq y_{2t}$)	-4.8492	-4.8201	-4.7749	1255.2430
Model 2: State-specific variance ($\sigma_1 \neq \sigma_2$)	-4.8471	-4.8212	-4.7811	1253.7016
Model 3: $y_{1t} \neq y_{2t}$ and $\sigma_1 \neq \sigma_2$	-4.8457	-4.8134	-4.7632	1255.3502
<i>MSDR model with BW filtered data and with non-switching (Model 3a) or without monetary policy (Model 3b)</i>				
Model 3a: $y_{1t} \neq y_{2t}$ and $\sigma_1 \neq \sigma_2$	-4.8496	-4.8205	-4.7753	1255.3415
Model 3b: $y_{1t} \neq y_{2t}$ and $\sigma_1 \neq \sigma_2$	-4.8454	-4.8195	-4.7793	1253.2573

Notes: ^a*AIC* stands for the Akaike Information Criterion (*AIC*). ^b*HQIC* stands for the Hannan-Quinn Information Criterion (*BIC*). ^c*SBIC* stands for the Schwarz or Bayesian Information Criterion (*SBIC*). ^d $\mathcal{L}$ reports the value of the log-likelihood.

estimates are in line with Heutel's (2012) findings. The duration in regime 1 is seven months compared to regime 2 with 15 months. Moreover, we do not find statistical evidence for a regime-specific volatility of the error term. Based on the BK filtering method, we can tell a different story: *Model 1* suggests that the emissions elasticity in regime 1 is elastic with a value of 1.05, whereas it is inelastic for regime 2 with an estimate of 0.80. A Wald-test clearly rejects the null hypothesis of an emissions elasticity equality at a 1% significance level. The duration in regime 1 is 16 months compared to regime 3 with 26 months. We do not find significant, regime-dependent, volatility differences between the regimes. In contrast to the HP and BK filtered data, for the CF-filtered data we estimate a significantly elastic behavior of emissions with respect to output. However, the emissions elasticity does not significantly differ between the states. Consequently, the duration in regime 1 and 2 is nearly the same for both models with 18 months on average. Finally, the results for the BW filtered data point neither to an elastic behavior of the emissions nor to a significant regime-specific volatility of the error term. Hence, the results are similar to the ones we obtain for the HP filtered data.

In this context, it is important to point out that the main impetus of this paper is on the estimation of the emissions elasticity. The estimation strategy above does not claim a causal relationship. Section 5 of this paper is devoted to a discussion of a potential estimation bias of the emissions elasticity and its direction. Before we turn to this issue, what follows next is an economic interpretation of the obtained regimes.

Table 3: Estimation results for selected MS specifications

MSDR with N=2 states	Data-filtering methodology					
	HP-filtered		BK-filtered		CF-filtered	
	Model 3a	Parameters	S.E. ^b	Model 1	Parameters	S.E. ^b
A. Emissions elasticity						
State 1		b_y^1	.9310815****	1.051451****	1.134313****	.0571373
State 2		b_y^2	.6847554****	.7975936****	1.185169****	.0549753
Monetary policy control		γ_y^1	✓	✓	X	✓
$\mathcal{W}^\alpha (H_0 := \beta_y^1 = \beta_y^2)$			3.41*	21.23****	0.88	2.67*
$\mathcal{W}^\alpha (H_0 := \gamma_y^1 = \gamma_y^2)$			—	2.93*	—	—
B. State-specific volatility						
State 1		σ_1	.0184429	.0074236	.0086252	.0004234
State 2		σ_2	.0185370	—	.0077258	.0004388
$\mathcal{W}^\alpha (H_0 := \sigma_1 = \sigma_2)$			0.00	—	1.96	0.13
C. Transition probabilities						
State 1		p^{11}	.8588696	.9379913	.9482520	.0087328
State 2		p^{22}	.9315972	.9620083	.9380996	.0149077
D. Estimated Duration						
State 1			7.085645	16.12676	3.811407	3.261121
State 2			14.61929	26.32152	5.453697	3.890666
Obs.			514	442	514	514

Notes: ^a $\mathcal{W}$ stands the Wald test statistic. The Wald test statistic is $\chi^2(1)$ distributed with $df = 1$. ^b Auto-correlation and heteroscedasticity-consistent standard errors (S.E.) are reported. ***, **, * indicates the $\alpha = 0.01$, $\alpha = 0.05$ and $\alpha = 0.10$ level of significance.

4.4 Are the regimes correlated with the business cycle?

In order to provide an economic meaning of the identified regimes, we check whether or not the identified regimes are correlated with the business cycle. Table 4 presents the correlation between various j lags of the ex-ante probabilities $p_{t-j+1} := \pi(s_t = i | \mathcal{I}_t)$ and the NBER recession indicator for the US business cycle. Table 4 implies that the first regime ($s_t = 1$) is significantly associated with an economic downturn, whereas the second regime ($s_t = 2$) represents an economy in normal times. However, the correlation is not perfect, but becomes stronger over time. This might indicate that the duration of the U.S. emission cycle differs from the duration of the U.S. business cycle. Section C and D of Table 3 suggest that state 1 will typically persist for about 12 months, whereas state 2 will persist for about 17 months on average across all filters. Moreover, both states are highly persistent (see Section C in Table 3). Thus, U.S. emissions cycles tend to be more short-lived than what is officially reported for U.S. business cycles, where the duration of expansions/recessions is 59/12 months on average between 1949-2016¹⁸.

This result is graphically supported by Figure 2, where we have employed the smoothed transition probabilities and regime classifications to reproduce the cyclicity of emissions. Based on the smoothed transition probabilities, we follow Chevallier and Goutte (2017) and plug the identified regimes back to the data: Red dots represents a recession (regime $s_t = 1$), whereas green dots show normal times (regime $s_t = 2$)¹⁹. The grey shaded areas highlight the NBER recessions²⁰. Again, we find that first/second regime ($s_t = 1/s_t = 2$) is associated with economic downturns/normal times. However, the correlation is not perfect supporting our findings in Table 4. In a nutshell, we find that emissions are (i) procyclical, (ii) more volatile than the business cycles, (iii) on average more elastic during economic downturns than during expansions, and, depending on filtering method and regime, (iv) lead to an elastic or inelastic behavior of emissions during the business cycle. Finally, (v) emissions tend to be more short-lived compared with the U.S. business cycle.

The estimated switching probabilities and regime durations can be also compared with the U.S. business cycle literature²¹. Reflecting the more recent literature, Chauvet (1998) finds relatively long periods of output increases and relatively brief periods of sharp declines in output along with a duration of a recession/expansion of 15/39 months. Many researcher have also employed three-state Markov-switching models, extending the two-state setting by a high growth phase or severe recession regime. Using monthly U.S. data from January 1985-November 2002, Ferrara (2003) estimates a three-state Markov-switching model (recession, normal times, high growth phase). According to Ferrara's (2003) estimates, the duration of an expansion is 11 months (for normal times)

¹⁸See <https://www.nber.org/cycles.html>.

¹⁹The relatively poor results for the CF filtered data are not surprising, as we find no statistical evidence for a regime-dependent behavior of the emissions elasticity based on this filtering method (see Table 3).

²⁰The recessions data can be freely accessed from <https://www.nber.org/cycles.html>.

²¹There is a large strand of literature employing Markov-switching models for the U.S. Among others, for an overview see Carstensen et al. (2017).

and 8 months for a recessionary state. In contrast, Hamilton (2005) estimates a three-state Markov-switching model (severe recession, recession, normal times) and finds that an expansion last about 36 months, whereas the duration of a recessionary state is less than 6 months for the period January 1948-March 2004. Hence, the duration of the recessionary state is adequately estimated with our model, whereas the duration of the expansionary state is at the lower bound of what can be found in the recent literature.

Figure 2: Regime-dependent cyclicity of emissions.

Note: Red dots are associated with a recession (regime $s_t = 1$), whereas green dots show normal times (regime $s_t = 2$). The grey shaded areas highlight the NBER recessions.

Table 4: Markov-Switching regimes and the US business cycle

Markov-Switching-Dynamic-Regression (MSDR)						
MSDR with N=2 states ^a						
Model	Correlations Months Ahead j					
	1	2		4		6
	$\rho(p_{t-j+1}, rec_t)$	$p\text{-value}^b$	$\rho(p_{t-j+1}, rec_t)$	$p\text{-value}^b$	$\rho(p_{t-j+1}, rec_t)$	$p\text{-value}^b$
HP filtered (Model 3a)	0.0818	0.0000	0.1689	0.0000	0.2327	0.0000
BK filtered (Model 1)	0.0975	0.0000	0.2028	0.0000	0.2570	0.0000
CF filtered (Model 3b)	0.0417	0.3454	0.0830	0.0607	0.1221	0.0010
BW filtered (Model 3a)	0.0948	0.0317	0.1950	0.0000	0.2697	0.0002

Note: ^a $\rho(\cdot, \cdot)$ stands for the correlation coefficient.

5 Summary of findings and discussion of results

This section summarizes and discusses the main findings. In section 3, a rolling-regression approach clearly indicates a pronounced unstable behavior of the elasticity of emissions with respect to GDP, irrespective – and that is important – of which filtering technique is employed. For all employed filters we also observe that the instability is less pronounced between 2005 and 2009 (see Figure (1)). This implies a robust inelastic behavior of the emissions with respect to GDP across employed filtering methods²².

For this time span, these results imply good news for most of empirical studies in this direction of research employing either the popular HP or the BK filter. Let us speculate about this behavior of the emissions elasticity between 2005 and 2009. First note that Figure (1) shows data-end points on the x-axis. Thus, the time span between 2005 and 2009 partly contains the aftermath of the recent financial crisis period at the end of the time span as well as times of economic expansions at the beginning. Now assume that energy intensity reacts more elastically during bad times than during good times (see Sheldon (2017)). According to this assumption, we expect that emissions react less elastically at the beginning of the time span and more elastically at the end. This explains why the emissions elasticity first decreases and then increases between 2005 and 2009. This latter finding is also supported by the Markov-Switching-Dynamic-Regression (MSDR) approach which allows for shifts in the intercept, covariates and the variance. Inter alia, we have shown that the identified regimes either classify economic downturns or normal times and that emissions respond more elastically to changes of the GDP during bad times than they do in normal growth times. However, looking at the entire time-span of our data that also includes the years 2005-2009, our results show that depending on the data filtering methodology, emissions might react *elastic* to GDP changes during downturns, whereas they react *inelastic* in expansionary phases. Hence, the period between 2005 and 2009 is not representative for the behavior of the emissions elasticity over a longer time period.

Hence, these findings have direct implications for theoretical modeling. In contrast to a standard DSGE setting that assumes a constant emissions elasticity, our results suggest that Markov-switching DSGE (MSDSGE) models should be the natural framework for discussing the design of an optimal environmental policy²³.

Moreover, these findings also have direct consequences for forecasting future emissions, since an appropriate design of mitigation or adaption policies depends on an accurate forecast of future emissions (see Sheldon (2017)). Assuming a constant emissions elasticity with respect to GDP may therefore lead to biased emissions forecasts and consequently to wrong environmental policy

²²Irrespective of which filtering method has been employed, between 2005 and 2009, Figure (1) clearly shows that the 95% confidence interval of the emissions elasticity estimates (with and without the inclusion of monetary policy) is between zero and below one, suggesting an inelastic behavior (see Figure (1)).

²³Of course the solution of MSDSGE models is more challenging than solving non-switching DSGE models. Setting up such a model is clearly beyond the scope of this paper. See Foerster et al. (2016) for an insightful introduction (to higher-order approximations) to the solution of MSDSGEs.

designs. For instance, one explanation why the Kyoto protocol is less successful than intended is also due to poor emissions forecasts (Sheldon (2017)). In particular, Böhringer (2003, p. 460) annotates that the Kyoto protocol "is doing less than perfect on the cost-efficient implementation of emissions reduction targets which would require full "where"- and "when"-flexibility". It is therefore not surprising that countries with emissions targets accounted for nearly 64% of carbon dioxide emissions in 1996 (Cooper (2001)). Hence, the implication for the policy maker is that the design of environmental policy instruments should account for this significantly asymmetric behavior of emissions due to changes in the GDP.

Further, we tackle the issue whether our results are robust to endogeneity concerns. Two major sources of endogeneity might affect our estimation results. The first is a possible simultaneity bias. In good times, GDP rises, which in turn increases pollution. At the same time, a rising level of pollution may negatively affect the health status of people, thereby reducing GDP (see Klarl (2016)). The second source arises from a potential omitted variable bias. Environmental policy endeavors, cultural or geographic factors jointly determine GDP as well as the pollution level. Naturally, the question arises whether these issues have implications for our results. First of all, due to these endogeneity concerns, alike Heutel (2012), Sheldon (2017) or Doda (2013, 2014), our estimates of the emissions elasticity should not be interpreted as causal. In fact, none of these contributions claim that. Nevertheless, we can speculate about the sign of the potential bias coming along with our estimates. For example, there is evidence for the U.S. that the health status during a boom is decreasing²⁴See Klarl (2016), Antony and Klarl (2019) and Ruhm (2000, 2003, 2005, 2007).. Further, the health status might also suffer from pollution. Hence, neglecting the health status from the regression leads to an underestimation of the emissions elasticity. Hence, we conclude that our estimates provide conservative measures of the emissions elasticity at the business cycle frequency.

6 Concluding comments

The elasticity of emissions with respect to GDP is a central parameter for environmental DSGE models. Heutel (2012) was the first who provided an estimate of this elasticity for the U.S. He concludes that emissions are inelastic with respect to GDP irrespective of which data filtering technique is employed. This contribution is mainly concerned with the question whether or not this elasticity measure is stable over time. For this purpose, we follow Heutel (2012) and apply standard data filtering techniques identifying the cyclical component for emissions and GDP. Next, by employing a rolling-regression approach, we find an unstable behavior for the emissions elasticity estimates, switching between less and more elastic or even inelastic estimates. These results are robust to the inclusion of monetary policy.

Next, we employ a Markov-Switching-Dynamic-Regression (MSDR) approach to identify a possible source of this instability. We identify two regimes, one for normal times and one for economic

²⁴black

downturns. In general, we find that in contrast to normal times, in times of an economic downturn emissions tend to be more elastic. Depending on the filtering method, we also obtain parameter estimates of the emissions elasticity above one in recession times and below one in normal times.

Finally, we discuss a possible endogeneity problem coming along with our regression approach. We argue that our estimates are potentially biased downward, implying that this paper reports conservative measures of the emissions elasticity at the business cycle frequency.

The general lesson we can learn from this paper is that policy makers should be aware of the asymmetric influence of the business cycle on emissions. Emissions forecasts which do not satisfactorily capture this effect will be biased, policy endeavors will turn out to be suboptimal, and, hence, the welfare ranking of these endeavors might be wrong.

Compliance with Ethical Standards

The author declares that he/she has no conflict of interest.

7 Appendix

7.1 Robustness Check I: Inclusion of monetary policy stance measure

Figure (3) shows the difference between the rolling-regression estimates of the emissions elasticity with and without the inclusion of monetary policy. Although the difference is statistically significant for HP and BK filtered data particularly before 2009, the role of monetary policy tends to be to insignificant or only slightly significant after 2010. Thus, from a *qualitative* point of view, the emissions elasticity estimate is not affected by the inclusion of monetary policy as shown with figure 1.

Figure 3: Difference between rolling-regression estimates of the emissions elasticity with and without the inclusion of monetary policy (Δb_y) for different filter methods (blue line) for different filter methods. 95% bootstrapped confidence intervals (5,000 replications) are shaded light gray.

7.2 Robustness Check II: Rolling regression with different window sizes

The choice of the window size in a rolling regression context is usually fixed by an arbitrarily chosen number of past experience. In our context, we choose the window size to match Heutel's (2012) period of investigation. Nevertheless, and although not the focus of this paper, in a forecasting context this strategy might turn out to be problematic, since Inoue and Rossi (2012) point to the fact that different window sizes might lead to different empirical forecasting results and good results might be obtained by chance. In this section, we show that our regression results are locally robust to changes of the window size around our benchmark with a window size of 276 calendar dates that matches Heutel's (2012) data set length. Figure (4) reproduces the analysis with with 300 instead of 276 calendar dates, whereas Figure (5) uses a window size with 200 instead of 276 calendar dates.

It seems that the role of monetary policy slightly decreases by decreasing the number of calendar dates. However, as shown in Section 7.1 of this paper, the inclusion of monetary policy does not qualitatively affect our main result that the emissions elasticity is not constant over time (see Figure (1)). This finding is also supported by our MS-switching regressions.

Figure 4: Difference between rolling-regression estimates of the emissions elasticity with and without the inclusion of monetary policy (Δb_y) for different filter methods (blue line) for different filter methods and for a larger window size (with 300 instead of 276 calendar dates). 95% bootstrapped confidence intervals (5,000 replications) are shaded light gray.

Figure 5: Difference between rolling-regression estimates of the emissions elasticity with and without the inclusion of monetary policy for different filter methods (blue line) for different filter methods and for a smaller window size (with 200 instead of 276 observations). 95% bootstrapped confidence intervals (5,000 replications) are shaded light gray.

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