Perceptions of Economic Conditions and Mental Health

Lukas Kauer[‡,∨] Lukas Schmid[¶,§] Valentina Sontheim[¶]

December 11, 2023

Abstract: This paper explores how perceptions of economic conditions affect mental health. We study the sudden, substantial, and persistent appreciation of the Swiss Franc in 2015 and document that this shock was highly salient and deteriorated perceptions about job security, while actual unemployment remained constant. Using individual-level panel data from the largest Swiss health insurance company, we compare psychotherapist visits of individuals who were more affected by the shock to individuals who were less affected. We find that deteriorated perceptions of economic conditions increased the likelihood of a psychotherapist visit, particularly for individuals most affected by changes in perceived job security.

JEL Classification: F31, I12, I15, J60
Keywords: Mental health, perceptions of economic conditions, exchange rate shock, psychotherapist visits, psychotherapeutic drugs consumption

[‡]University of Lucerne, Faculty of Health Sciences and Medicine, Alpenquai 4, CH-6002 Lucerne, Switzerland;
[∨]CSS Institute, Tribschenstrasse 21, CH-6002 Lucerne;
[¶]University of Lucerne, Faculty of Economics and Management, Frohburgstrasse 3, CH-6002 Lucerne;
[§]University of St.Gallen, Department of Economics, Bodanstrasse 8, CH-9000 St.Gallen, Switzerland.

1 Introduction

A large literature in economics has explored the relationship between economic conditions and health.[1] This literature has primarily focused on the effect of the *actual* economic conditions on health indicators. There is, however, limited evidence on the impact of the *perceived* economic conditions on health outcomes. Estimating this impact is difficult because perceived economic conditions are heavily intertwined with actual economic conditions. We address this empirical challenge by exploiting a natural experiment that increased the fear of an economic downturn but did not lead to an actual recession. This allows us to estimate the impact of economic perceptions on mental health care consumption in Switzerland.

For empirical identification, we use the exchange rate shock on January 15, 2015, which led to a sudden, substantial, and persistent appreciation of the Swiss Franc relative to the Euro. This policy change was unexpected and increased the value of the Swiss Franc against the Euro by an average of 10.7% in the period from January 2015 to December 2016 compared to the period from January 2013 to December 2014. We first document that the discontinuation of the minimum exchange rate was highly salient in the national parliament and in the popular media and led to spikes in related Google searches. The high salience of the exchange rate in Switzerland is no surprise. First, the country is a small open economy, and countries from the European Union account for a share of 37.9% in total trade (FSO 2023a;2023b). Second, the appeal of the Swiss labor market rises with the value of the national currency, particularly for foreign workers who constitute a significant portion of the Swiss workforce. In 2012, cross-border commuters accounted for 7.8% of the total workforce.

In our main analysis, we explore the impact of the exchange rate shock on objective measures of mental health care by analyzing detailed panel data on psychotherapist visits. Our data comes from one of the largest Swiss health insurers, covering approximately a sixth of the entire Swiss population. We compare individuals who are more affected by the currency shock, those living close to the eurozone border, to individuals who are less affected by the currency shock, those living farther from the border, in a difference-in-differences setting. We measure the exposure of an individual to the exchange rate shock by calculating the shortest travel distance from an individual's residence to a eurozone border crossing.

1. See Ruhm (2000), Deaton (2003), Sullivan and Wachter (2009), Browning and Heinesen (2012), Black, Devereux, and Salvanes (2015), and Avdic, New, and Kamhöfer (2021).

The results from our difference-in-differences estimation suggest that the exchange rate shock had a substantial impact on mental health care consumption. It increased the likelihood of having at least one session with a psychotherapist per month by roughly 16.3%. It's worth noting that individuals are referred to a psychotherapist by a medical doctor, and the costs are covered by the mandatory insurance scheme. We present evidence indicating that our observed effect results from a rise in demand rather than a region-specific supply effect by accounting for the time-varying number of psychotherapists in the vicinity of an individual. Our findings are robust to several checks. First, we use an alternative definition of our treatment, namely the continuous distance measure. Second, we probe the sensitivity of our results by varying the sample and controlling for observed and unobserved changes over time. Third, we use an alternative measure of mental health care consumption and find that the likelihood of purchases of prescribed psychotherapeutic drugs also increased following the currency shock. Finally, we explore sample attrition of individuals moving between treatment and control regions and perform a randomization inference approach to assess the uncertainty of our estimates.

In our mechanism section, we establish that the perceptions of economic conditions are the key driver of why mental health deteriorated following the exchange rate shock. We first document that *actual* labor market conditions at the municipality level, such as unemployment rates as well as the shares of temporary residents and cross-border workers, continued to follow prior trends. We then show the exchange rate shock affected *perceived* economic conditions by increasing expected unemployment and deteriorating job security while leaving the expected financial situation unchanged. Our heterogeneity analysis provides evidence that the negative impact on mental health is most pronounced among working-age individuals, consistent with the notion that the labor market threat is higher in this age group. In addition, the impact of the exchange rate shock is substantially higher for working-age women who are more likely to be negatively affected by increased pressure from competition. Furthermore, we account for the fact that individuals may not work. We classify individuals into treatment and control groups based on the distance of their municipality's labor market center to the nearest border crossing. All previous results are robust to this complementary treatment definition. We find little support for an alternative mechanism that works in the opposite direction, namely that a stronger Swiss Franc increases individuals' purchasing power and thereby improves their mental health.

Our findings build on a larger literature estimating the effects of actual changes in individual or general economic circumstances on mental health. The first set of studies has analyzed the health

effects of individual economic conditions with a focus on homeowners, retirees, lottery winners, and unemployed individuals. Most of these studies have analyzed the impact of negative economic shocks and find a deterioration of psychological health when wealth decreases, acting through increased stress (Yilmazer, Babiarz, and Liu 2015; Schwandt 2018).[2] In a similar vein, recent findings suggest that hospital admissions for psychological conditions increase through negative daily stock returns, working through expectations over future consumption (Engelberg and Parsons 2016). Further, there is evidence that job loss increases public health care expenditure for mental health (Kuhn, Lalive, and Zweimüller 2009). Fewer studies have focused on the impact of positive economic shocks and found that an increase in wealth through lottery winnings is beneficial for mental health and acts through overall life satisfaction (Lindqvist, Östling, and Cesarini 2020). The second set of studies has analyzed the relationships between general economic conditions and health outcomes via the labor market or the stock market. These papers have documented that unemployment and mortality are positively correlated (Ruhm 2000; Ruhm 2015) and that the Great Recession increased excess suicide rates (Chang et al. 2013) as well as feelings of depression and the use of antidepressant drugs (McInerney, Mellor, and Nicholas 2013). Previous studies have also documented that young women suffer most during economic downturns (Black, Jackson, and Johnston 2022). We advance this literature by documenting that not only changes in *actual* economic conditions affect health outcomes, but that the *perceived* economic conditions have an impact on mental health.

The results of our study are most closely related to the literature that explores how the actual and perceived labor market situation affects mental health. A recent paper documents that an increase in unemployment insurance duration decreases the prescription of antidepressants and opioids but only for women, acting through lower pressure of finding a new job (Ahammer and Packham 2023). This is in line with our finding that working-age women exhibit larger negative effects than working-age men when perceptions about job security deteriorate. The only study, to our knowledge, that sheds light on a similar mechanism acting through perceived job security is conducted by Johnston, Shields, and Suziedelyte (2020). They analyze mental health data of highly commodity price-dependent Australian mining workers and find that commodity price increases lead to a rise in perceived job security and better mental health. While these two studies explore the impact of lower pressure on the labor market and an increase in perceived job security, which are very similar to the main mechanism in our paper, they differ in several dimensions from the setting we study. First, Ahammer and Packham

2. In contrast to these findings, Fichera and Gathergood (2016) find no effect of greater wealth on psychological health.

(2023) analyze the effect of a decrease in the actual pressure of finding a new job through longer unemployment benefit duration, while we focus on the impact of a decrease in perceived job security and the associated increase in perceived pressure on the labor market. Second, we complement the mental health outcomes analyzed by Ahammer and Packham (2023) and Johnston, Shields, and Suziedelyte (2020) prescription of antidepressants and opioids as well as perceived job security and other survey measures of mental health, by focusing on visits with psychotherapists. Third, our sample is comprised of the general population, while Ahammer and Packham (2023) study unemployed individuals and Johnston, Shields, and Suziedelyte (2020) focus on mining workers.

In sum, we advance the existing literature in several ways. First, we address the identification challenges arising through the correlation between *perceived* and *actual* economic conditions. Second, we use visits with psychotherapists as the main outcome measure that does not suffer from measurement error due to misreporting and complements previous findings on drug prescriptions. Third, we are able to control for changes in the supply of psychotherapists in the vicinity of individuals. Fourth, our detailed panel data allows us to study the general population of interest and enables us to include individual-level fixed effects to capture unobserved heterogeneity.

2 Institutional background

2.1 Switzerland's exposure to the eurozone

As a small landlocked country in Europe, Switzerland has traditionally had strong international economic ties. The export of goods and services accounts for 65.8% of GDP during our sample period 2013-2016. The main trading partners were the countries from the European Union (EU) with a share of 37.9% (FSO 2023b; FSO 2023c). Despite its strong economic ties with EU member countries, Switzerland has never been part of the EU or its predecessor organizations and has never introduced the Euro as a currency. However, it has signed several agreements that have fostered trade and market access for labor, goods, and services. One important example of these agreements is the free movement of labor that came into force in 2002 and gives Swiss and EU citizens the right to live and work in the territory of the other party.

These agreements were controversial because both the wage and price levels in Switzerland have always been higher than those in the EU countries. The wage differential makes the Swiss labor

market attractive for cross-border workers. In 2012, a total of 265,000 individuals (7.8% of the total workforce) worked as cross-border commuters in Switzerland (FSO 2014). Consumer prices in Switzerland were 55% higher than the EU average in 2013 (FSO 2022). This led to pressure on prices in the Swiss border regions (Auer, Burstein, and Lein 2021) and to increased attractiveness of cross-border shopping.[3]

2.2 The 2015 currency shock

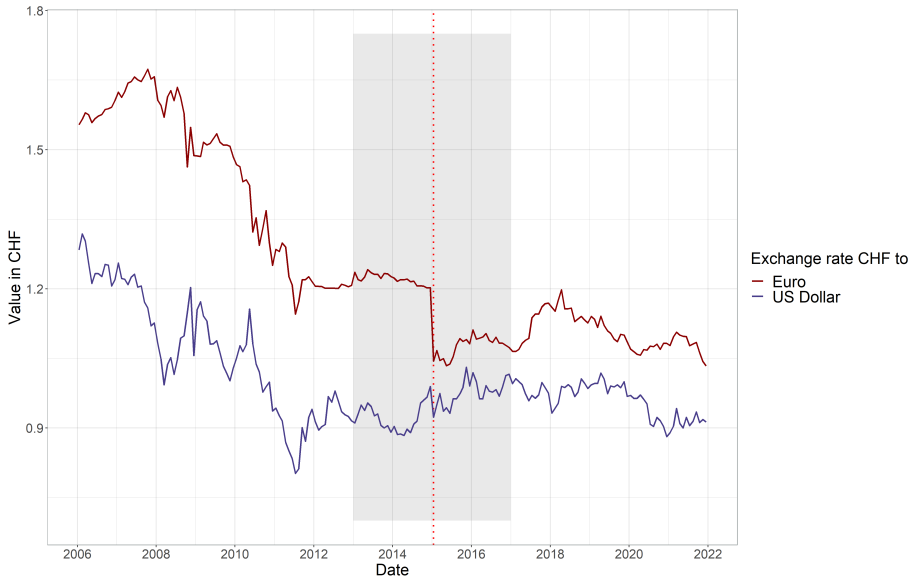
The Euro was introduced as an accounting currency on January 1, 1999, and physical Euro coins and banknotes came into circulation in 2002. The goals of the new European currency were to deepen the single market for goods and services and to create financial institutions that allow the free flow of capital across European countries (Brunnermeier and Reis 2023). Following the debt crisis in poorer periphery countries, including Greece, Ireland, Portugal, and Spain, the Euro faced enormous pressure against other currencies starting in 2009. This pressure led to a historically unprecedented appreciation of the Swiss Franc, which has traditionally been known as a safe haven currency (Habib and Stracca 2012). At first, the Swiss National Bank (SNB) tried to dampen this effect by increasing the liquidity of the Swiss Franc (CHF). Because this attempt was unsuccessful, the SNB fixed a minimum exchange rate of CHF 1.20 per Euro in September 2011.[4] At the end of 2014, the situation in the financial markets changed. It seemed that the monetary policy in the United States became less expansionary, while there was evidence that the Euro countries would further ease their monetary policy. The Swiss Franc then weakened against the US dollar but gradually approached the minimum exchange rate of CHF 1.20 per Euro. According to the SNB, the minimum exchange rate remained effective until the end of 2014 because Switzerland's economy recovered more rapidly from the Great Recession than did the economies of other countries. Then, however, it became clear that a long-term

3. According to a survey by Rudolph, Nagengast, and Nitsch (2015) using data on more than 4,000 individuals, more than 60% of respondents stated that "from time to time" they buy grocery and drugstore items in foreign supermarkets and 30% of respondents said they do so for sports and furnishing items. These findings are consistent with customs data analyses from Credit Suisse that around 4-5 billion Swiss Francs were spent abroad through cross-border shopping in 2012 (Credit Suisse 2013).

4. The main reasons for this strong intervention were to support the large Swiss exporting sector in these uncertain times and to counter the risk of a deflationary development. The evidence on the position of the SNB regarding the minimum exchange rate and its discontinuation in this paragraph is based on a speech by the president of the Swiss National Bank, Thomas J. Jordan, at the Ordinary General Meeting of Shareholders of the SNB on April 24, 2015. The document is available online: Thomas J. Jordan: SNB monetary policy after the discontinuation of the minimum exchange rate, https://www.snb.ch/en/mmr/speeches/id/ref_20150424_tjn/source/ref_20150424_tjn.en.pdf, accessed on October 6, 2023.

commitment to the minimum exchange rate would have required permanent currency interventions of substantial magnitude. Therefore, the Governing Board of the SNB communicated on January 15, 2015, that a "minimum exchange rate of CHF 1.20 per Euro was no longer sustainable [...] from a monetary policy point of view" and discontinued the minimum exchange rate.

Figure 1: Evolution of the Exchange Rate

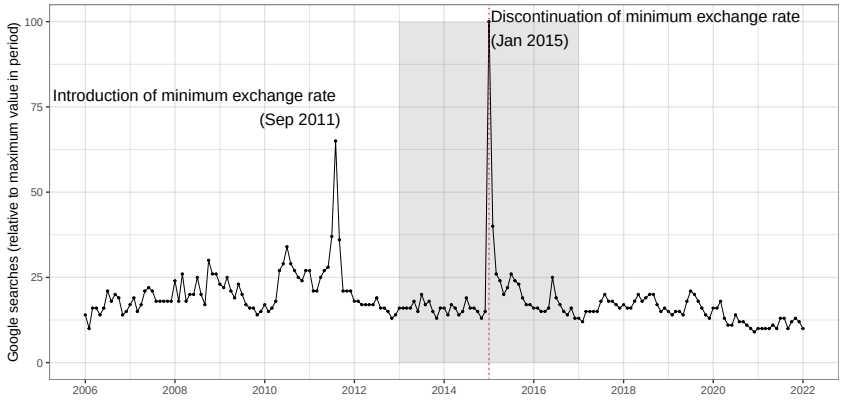


Notes: This figure shows the monthly exchange rate of the Swiss Franc to the Euro and the US Dollar. The gray area represents our observation period. The dotted red line marks the month of the currency shock. *Source:* Own visualization based on data from SNB.

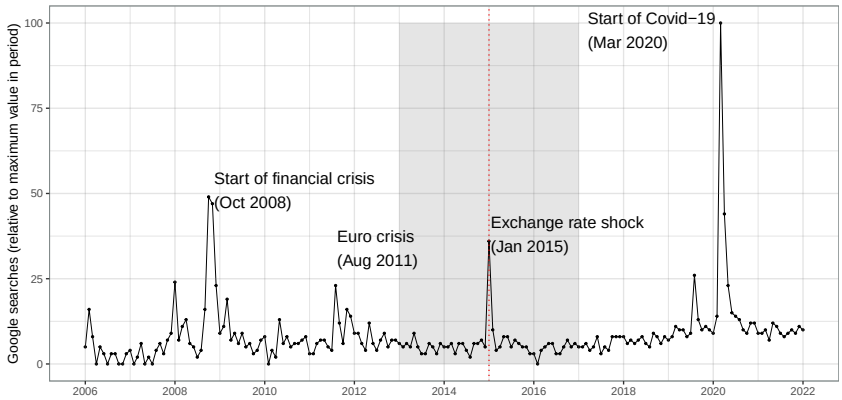
Figure 1 shows the evolution of the exchange rates of the Swiss franc against the Euro and the US dollar for the period 2006-2021 with our observation period 2013–2016 shaded gray. It depicts the substantial appreciation of the Swiss Franc against the Euro following the discontinuation of the minimum exchange rate, corresponding to 10.7% over the average in the years 2015 and 2016 relative to the average value in the years 2013 and 2014. An increase in the value of this magnitude was unprecedented in the history of the Swiss Franc after World War II and surprised financial markets, firms, workers, and consumers (Baltensperger 2016). The SNB's decision was heavily criticized by Swiss export firms who feared intensified international competition and by labor unions who feared import competition and higher international pressure on the Swiss labor market. It is thus no surprise that the exchange rate shock had a substantial impact on the political discourse, media coverage, and Google search patterns.

Figure 2: Google searches

(A) Search term "exchange rate"



(B) Search term "recession"



Notes: This figure depicts Google searches from Swiss IP addresses for two different search terms from January 2006 until December 2021 at the monthly level. On the y-axis, we depict the share of Google searches for a specific search term rescaled by the maximum number of searches in a month during the search period. Panel (A) reports the results for the search term "exchange rate" (German: *Wechselkurs*) and Panel (B) provides the results for the term "recession" (German: *Rezession*). Because Google does not provide the absolute number of searches, we cannot combine the search term results from different languages. We restrict our attention to the language spoken by the majority of Swiss citizens, which is German. 65.5% of Swiss residents list German as their first language in 2010, see https://www.bfs.admin.ch/bfs/de/home/statistiken/bevoelkerung/sprachen-religionen/sprachen.html, last accessed on January 24, 2023.

The national parliament organized a special debate during its ordinary spring session in March 2015. Some politicians proposed a permanent currency peg to the Euro, others proposed to reduce corporate taxes to increase their competitiveness.[5] Based on the parliamentary debate, we conclude that political actors from the entire party spectrum agreed that the exchange rate shock was a significant challenge to the Swiss economy. In addition, the federal government stated that the situation was particularly difficult in border regions because these labor markets faced increased pressure. To monitor that Swiss labor market standards were not undermined, the federal government offered financial support to border regions.[6] Popular media outlets also covered this debate about the exchange rate shock. We used the Dow Jones' Factiva service and searched for the terms "exchange rate" and "recession" in the two weeks before and after the exchange rate shock in Swiss media outlets. While only 30 articles included the term "exchange rate" before the shock, a total of 672 articles did so after the shock. For the term "recession", the increase was from 15 articles to 258 articles. The coverage of these topics in the media is consistent with Google search patterns. Figure 2 depicts the Google search patterns for the terms "exchange rate" and "recession" from January 2006 until December 2021 with our observation period 2013-2016 shaded gray. On the vertical axis, we depict the Google hits in a specific month relative to the maximum value in the full period.[7] The exchange rate shock date is indicated by the vertical red line. Panel (A) shows that Google searches for "exchange rate" peaked during the month of the exchange rate shock. Panel (B) documents that Google search patterns for "recession" in the month of the exchange rate shock were higher than at the height of the Euro crisis but lower than at the start of the financial crisis and at the beginning of the Covid-19 pandemic. These individual search behaviors suggest first that the exchange rate change was salient in the Swiss population and second that individuals connected the appreciation of the Swiss Franc with its potential economic consequences, namely the threat of a recession.

2.3 Mental health care provision and insurance coverage in Switzerland

All Swiss residents need to buy basic health insurance from competing private insurers (Schmid, Beck, and Kauer 2018). This mandatory basic health insurance covers a fixed set of services,

5. Source: https://www.parlament.ch/de/ratsbetrieb/amtliches-bulletin/amtliches-bulletin-die-verhandlungen?SubjectId=30550, in German, last accessed on February 9, 2023.

6. Source: https://www.parlament.ch/de/ratsbetrieb/suche-curia-vista/geschaeft?AffairId=20153030, in German, last accessed on February 9, 2023.

7. Google does not provide the absolute number of searches.

including mental health care. Visits with a psychotherapist are covered if the therapy is delegated by a physician.[8] Psychotherapists need to have studied psychology and graduated from psychotherapy training to bill under the basic coverage. They are not allowed to prescribe any psychotherapeutic drugs.[9] One visit is restricted to last up to 90 minutes. After 40 visits, the delegating physician needs to justify the continuous treatment with the insurer.

Demand-side cost sharing in mandatory basic insurance starts with a deductible of at least CHF 300. Once health care costs exceed the amount of the deductible, a copayment of 10% applies to the next CHF 7,000. The maximum cost sharing per year with the default deductible is therefore CHF 1,000. To reduce their premium, individuals can choose a higher deductible level up to CHF 2,500.[10]

3 Data

We have access to individual-level data from the largest Swiss health insurance, from which around one in six Swiss residents buy their mandatory basic coverage. The data include the customer's residence address and socio-economic information on age and gender. Unfortunately, the data does not include information on employment status and wages. From the plan data, we know the chosen level of the deductible. Further, we observe individual mental health care consumption, specifically visits with psychotherapists, and aggregate it to a monthly level. For our alternative outcome variable, we use a monthly indicator for whether an individual purchased any psychotherapeutic drug in a given month.[11]

We complement our individual-level insurance data with two additional variables. We argue that individuals who live close to the border are more exposed to the exchange rate shock than those who live farther away and measure this exposure by proximity to the border. For this first variable,

8. This applies to our study period. Since July 1, 2022, psychotherapists can bill under basic coverage without a delegation from a physician. If individuals have voluntary supplementary insurance, visits with a psychotherapist are often covered without a referral by a physician. We restrict our analysis to services covered by the mandatory basic insurance.

9. Psychiatrists can prescribe psychotherapeutic drugs and can also provide mental health care in terms of in-person consultations. We exclude visits with psychiatrists in our main outcome variable since psychiatrists often treat more severe cases and combine their treatments with psychotherapeutic drugs.

10. The explicit options for the deductible level are CHF 300, 500, 1,000, 1,500, 2,000, and 2,500.

11. We include drugs that are indicated for sleeping, anxiety, and panic disorders in Switzerland (Plag, Hägele, and Ströhle 2012) based on the Anatomical Therapeutic Chemical (ATC) classification issued by the WHO (www.whocc.no/atc, accessed on February 9, 2023), which is comparable to the National Drug Codes (NDC) classification in the United States.

we geocode the addresses of individuals' residences using the ArcGIS service and use geocoded border crossing data published by the Federal Office of Topography (swisstopo) on roughly 300 border crossings consisting of streets that are wide enough to be passed by car. To calculate the travel distance of individuals' addresses to border crossings we use the routing service of the Open Street Map server and focus on travel minutes by car.[12] This results in travel times from each individual residence to each border crossing. Finally, we select the minimum travel time in minutes, leaving us with a continuous distance measure at the individual level. Higher mental health care consumption might also be driven by an increased supply of mental health care practitioners which we aim to control. For this second variable, we use addresses from all psychotherapist offices who provided services for our insurance company in each month and geocode them as above. Then, we calculate the travel distance by car in minutes from each residential geocode to each psychotherapist geocode. Finally, we count the number of practicing psychotherapists in the vicinity of each individual in each month of our observation period within a driving distance of up to 20 minutes by car.

In our regression analysis, we control for a binary indicator that captures whether an individual lives in an urban or rural municipality. This variable is based on data from the Federal Statistical Office (FSO 2017).[13] In our robustness tests and in the mechanism section, we use the same data from the FSO which assigns every municipality to one of the 92 different labor market centers based on the commuting patterns of the population. Using this definition, we categorize each individual into a labor market center based on their municipality of residence. In the robustness section, we incorporate dummy variables for the 92 labor market centers. For our mechanism section, we calculate the shortest distance from this center of the labor market to the next border crossing and use this measure to build a new binary treatment variable. We use two additional sources of data in our mechanisms section. The first source is data on consumer sentiments about the economic situation from the Swiss State Secretariat for Economic Affairs (SECO) published as the Swiss Consumer Sentiment Index. This is a quarterly conducted survey including a nationally representative sample of 3,300 individuals.[14] The second data source includes information on registered unemployed individuals, temporary residents holding a work permit, cross-border workers, and the number of refugees and provisionally admitted

12. We used the R library "osrm" that is publicly available and can be downloaded from the CRAN server. See https://cran.r-project.org/web/packages/osrm/index.html, last accessed February 11, 2023.

13. 22% of Swiss municipalities are categorized as urban. 63% of the Swiss population live and 75% of the workforce work in these municipalities.

14. The survey design is comparable to the Consumer Confidence Index published by the EU.

foreigners at the municipality level.[15] Data on unemployed individuals and cross-border workers are available at the monthly level, while other population data are available at the yearly level. We express all shares in terms of the total population of a municipality of residence of an individual and aggregate the unemployed individuals and cross-border workers to the quarterly level to avoid large fluctuations in small municipalities.

4 Estimation method

We argue that individuals living closer to the eurozone border are more affected by the exchange rate shock than individuals living farther away from the border. On the one hand, competition with foreign workers in the labor market is higher in bordering regions than in regions less affected by the presence of cross-border workers. On the other hand, individuals living closer to the border might commute across the border more easily and more often to consume goods and services than individuals living farther away.

4.1 Difference-in-differences design

We follow a difference-in-differences design in which we compare the difference in the probability of at least one visit with a psychotherapist in a month of individuals living close to the border before and after the currency shock with the respective difference of individuals who do not live close to the border. In a regression framework, this can be implemented as follows:

$$Y_{it} = \mu_i + \eta_v + \kappa_j + Post_t\beta_1 + D_i \times Post_t\tau + \boldsymbol{X}_{it}\beta_2 + \varepsilon_{it} \tag{1}$$

where Y_{it} is our outcome variable of interest, whether an individual has at least one psychotherapist visit in a given month, i is the index for individuals, and $t \in \{1, \ldots, 48\}$ indexes all months across our four-year-sample period. We include individual fixed effects μ_i to account for unobserved individual-level heterogeneity. Further, we control for the seasonality in medical treatments with a fixed effect for the calendar month, η_v, indexed by $v = \mod(t/12)$ as well as for regional-specific differences

15. Temporary residents holding a work permit live in Switzerland and have to renew their temporary status each year. Cross-border workers live abroad and commute to work in Switzerland, mostly daily. Their permit is conditional on being employed. Refugees and provisionally admitted foreigners have labor market access but low employment levels, particularly in the first few years after arrival (Schmid 2023).

by adding a canton fixed effect, κ_j, indexed by $j \in \{1, \ldots, 26\}$.[16] $Post_t$ is an indicator for all periods after the currency shock and D_i is an individual-specific treatment indicator.[17] Our main parameter of interest is τ, which captures the impact of the exchange rate shock on mental health care consumption of individuals living close to the border. $\boldsymbol{X}_{it}$ is a set of time-varying individual control variables, namely a person's age, a dummy for having a high deductible, and an indicator for living in an urban area. In our preferred specification, we also include the time-varying supply of psychotherapists within 20 minutes of driving distance by car from the residence of an individual as an additional control variable. The idiosyncratic error term is captured by ε_{it}.

4.2 A binary measure for the exposure to the exchange rate shock

In our main analysis, we leverage a difference-in-differences analysis for which we need to define a binary treatment variable. The choice of the threshold along our continuous distance measure to define treatment and control group involves a tradeoff. If we choose a cutoff value close to zero, individuals in the treatment group have high exposure to the currency shock but we suffer from a relatively low number of treated individuals. If we choose a cutoff value far from zero, we increase the number of individuals in the treatment group, but the treated individuals differ more in terms of their treatment exposure.

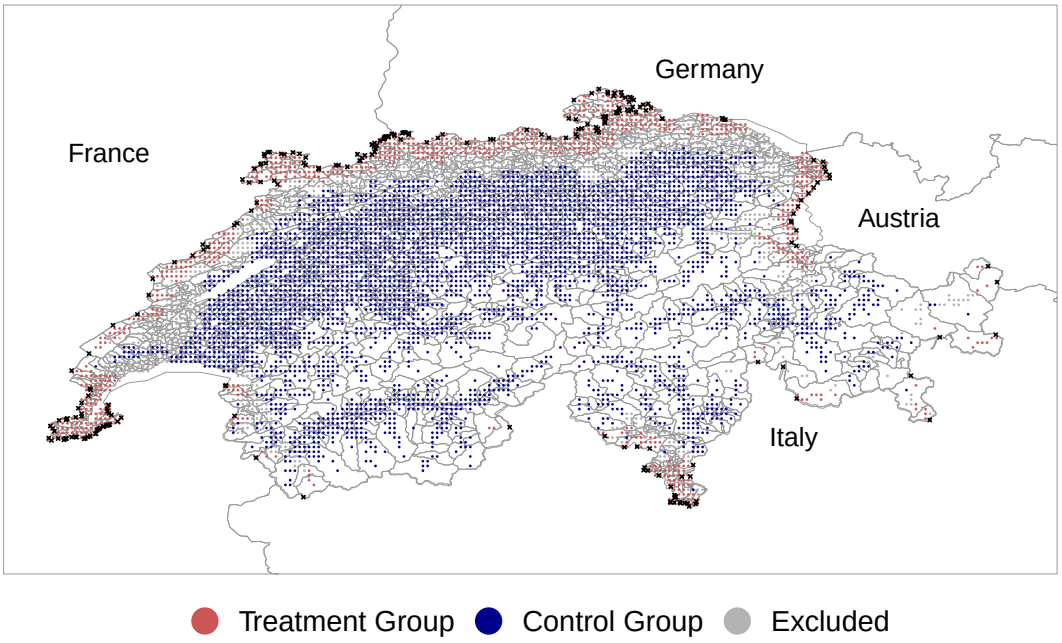
In our main estimations, we classify all individuals who live within 15 minutes of the next border crossing as treated, and all individuals who live more than 30 minutes of the next border crossing as control. The reason for this cutoff is that the labor market competition stemming from cross-border workers strongly decreases for individuals who are more than 15 minutes away from the next border crossing. This is documented in Figure A.1 in the online Appendix and consistent with the findings of Beerli et al. (2021). The reason why we exclude individuals who live more than 15 but less than 30 minutes from the next border crossing is to ensure that our results do not depend on the exact distance cutoff. In our robustness section, we document that our main results also hold when using a continuous distance measure.

16. Switzerland is a federal country with 26 federal units, the cantons. The cantonal fixed effect is identified from the individual fixed effect because we drop individuals only if they move between the treatment and the control region but not when they move to a different canton within the treatment or control region.

17. Because we exclude individuals moving between treatment groups, the individual fixed effect also includes the information about the treatment group, D_i, and therefore we do not include the baseline D_i in our regression.

Figure 3 shows the regional variation in treatment groups. For illustrative purposes, we calculate average travel distances in minutes to the next border crossing across all individuals in each grid point. We then classify all grid points into three groups. Grid points with average travel distances up to 15 minutes are classified as the treated group, grid points with average travel distances higher than 30 minutes are classified as the control group, and all other grid points are classified as excluded.

Figure 3: Exposure to the currency shock



Notes: This figure illustrates the geographical distribution of the treatment and the control group as well as the excluded group. For each grid point, we calculate the average travel distance to the next border crossing for all individuals and then classify them into the three respective groups. The size of each grid point is approximately 1.75×1.75 kilometers.

4.3 Sample restrictions

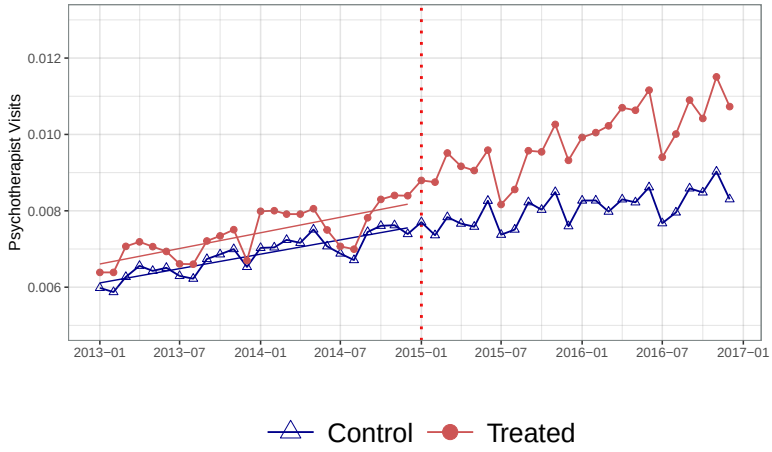
Our observation period lasts from January 2013 to December 2016. To work with a balanced panel, we exclude customers who join or leave the insurer, either due to switching to a different insurer, death, immigration, or emigration, during our observation period. We restrict our sample to customers older than 25 because younger people often live in a location different from the one registered with the insurer, and because basic insurance can be suspended during compulsory military service. Further, we exclude customers who reside outside of Switzerland. Finally, we exclude customers who move

during our study period if they move across treatment, control, or excluded group. We discuss the sensitivity of our results concerning these sample restrictions in the robustness section.

4.4 Validity of the parallel trends assumption

Central for the validity of the difference-in-difference design is the assumption that outcomes in the treatment and control group would have followed the same trend in the absence of the treatment. One way to assess the plausibility of this assumption is to compare the time trends in the two groups before the treatment. If these trends are similar, the assumption that the treatment group would have followed the trend in the control group in the absence of the treatment is plausible. Figure 4 depicts the evolution of the share of individuals with at least one monthly visit with a psychotherapist. While the level of the outcome variable is higher in the treated than in the control group in the pre-treatment period, the time trend is almost identical.

Figure 4: Evolution of Average Monthly Visits with a Psychotherapist



Notes: This figure depicts the share of individuals who had at least one visit with a psychotherapist at the monthly level. The treatment group (red circles) includes individuals living within 15 minutes driving distance by car of the next border crossing. The control group (blue triangles) includes individuals living 30 minutes or more from the nearest border crossing.

We also use statistical tests to explore whether the outcomes of the treated and control groups evolve in parallel before the exchange rate shock. These pre-existing differences in trends can be tested by regressing the outcome of interest on the time trend and the treatment-group-specific time trend

before the currency shock. We estimate the same model as in equation (1) for the pre-treatment period only but include treatment-group specific time trends.

$$Y_{it} = \mu_i + \eta_v + \kappa_j + t \times \gamma + t \times D_i\delta + \boldsymbol{X}_{it}\beta_3 + \varepsilon_{it} \tag{2}$$

Our estimate for γ is positive and significant, in line with the visual inspection of Figure 4 showing an increasing probability for psychotherapist visits over time. The estimated coefficient for δ, however, is not statistically significant and very close to zero (see Table B.1 in the online Appendix), implying that the pre-trend for the treatment group does not differ from the one of the control group. In sum, both the visual inspection and the estimates of the pre-trends lend support to the plausibility of the parallel trends assumption.

5 Descriptive statistics

Table 1 provides descriptive statistics for individuals in the treatment and the control group. The variables are grouped into five panels. Panel (A) of the table documents that individuals in the treated region exhibit higher levels of mental health care consumption compared to the control group. About 0.9% of individuals in the treatment group have at least one monthly visit with a psychotherapist compared to only 0.7% of the control group. The higher mental health care consumption is also reflected in the higher probability of purchases of prescribed psychotherapeutic drugs. Panel (B) reports the average travel distance by car from individuals' residence to the next border crossing which is 7.66 minutes in the treatment group and 50.93 minutes in the control group. We further report travel distances from the labor market center of an individual's municipality of residence to the next border crossing. Panel (C) reports control variables. Age and the dummy for having a high deductible are comparable across treatment and control groups. In contrast, the table documents that individuals in the treatment group have more psychotherapists within a 20-minute driving distance and are more likely to live in an urban municipality. Panel (D) reports two variables used for our heterogeneity analysis, the share of females and the share of individuals of working age, which are very similar across groups. Finally, Panel (E) documents that the share of registered unemployed in terms of the total population is very low with 1.9% in the treated group and 1.4% in the control group. Further, the share of cross-border workers is 12.1% in the treatment group and very close to zero in the control

group, while the share of temporary residents is 9.2% in the treatment and thus about 3 percentage points higher than in the control group.

Table 1: Summary statistics

Treatment category	Treated	Control
(A) Outcome variable		
Any psychotherapist visit	0.009	0.007
Any psychotherapeutic drug purchase	0.009	0.007
(B) Treatment variable		
Distance from residence to border crossing	7.661	50.928
Distance from labor market center to border crossing	9.020	48.200
(C) Control variables		
Age	56.288	55.476
High deductible	0.338	0.356
Urbanity	0.414	0.180
Number of Psychiatrists within 20 min	110.323	58.620
(D) Heterogeneity variables		
Female	0.540	0.531
Working-age population	0.633	0.655
(E) Labor market variables		
Share of cross-border workers	0.121	0.004
Share of temporary residents	0.092	0.066
Share unemployed of total population	0.019	0.014
Share refugees of total population	0.006	0.007
Number of observations	6,477,120	18,498,768
Number of individuals	134,940	385,391

Notes: The table reports means per group. The groups in columns are split based on the distance in minutes from the next border crossing. All variables are binary except distances to the nearest border crossing, age, the number of psychotherapists within 20 minutes of driving distance, and all shares in Panel (E).

6 Results

6.1 Main results

In our main analysis, we estimate the impact of the exchange rate shock on an indicator variable for at least one psychotherapist visit in a month. In column (1) of Table 2, we find that the exchange rate shock increased the probability of a psychotherapist visit by 0.12 percentage points when accounting for individual fixed effects only. In column (2) we add month and canton fixed effects, and in column (3), we add time-varying individual control variables, namely a person's age, a dummy for

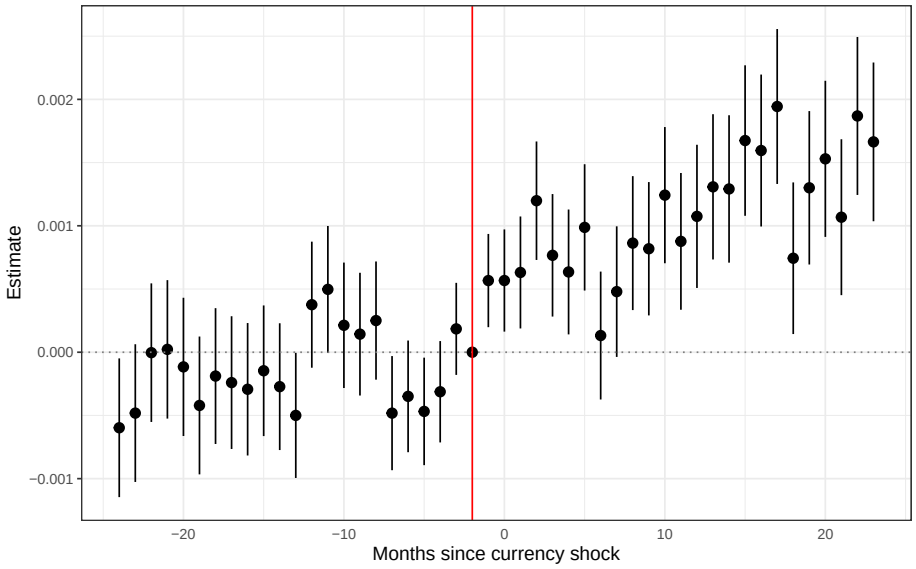
having a high deductible, and an indicator for living in an urban area. Our coefficient of interest remains virtually identical, and the explained variation only marginally increases, pointing to the fact that most individual-level heterogeneity is accounted for when including the individual-level fixed effects. In our preferred specification in column (4), we add a time-varying supply control for the number of psychotherapists in the neighborhood of the individual, which does not affect the treatment effect. In relative terms, the treatment effect is equivalent to an increase of 16.3% in the probability of at least one monthly visit with a psychotherapist relative to the mean in the treatment group during the pre-treatment period.

Table 2: Effect of the currency shock on psychotherapist visits

Dependent variable:	**Any psychotherapist visit**			
	(1)	(2)	(3)	(4)
Treatment × Post	0.0012	0.0012	0.0012	0.0012
	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Baseline	0.0074	0.0074	0.0074	0.0074
Effect in Percentages	16.26%	16.26%	16.26%	16.26%
Observations	24,975,888	24,975,888	24,975,888	24,975,888
R^2	0.4404	0.4405	0.4405	0.4405
Individual FE	✓	✓	✓	✓
Month FE		✓	✓	✓
Canton FE		✓	✓	✓
Individual Controls			✓	✓
Supply Control				✓

Notes: This table reports regression coefficients from estimating equation (1) with clustered standard errors (by individual) in parentheses to account for within-individual serial correlation in outcomes. We use data at the monthly level from January 2013 to December 2016. The dependent variable is an indicator of having had any psychotherapist visit in a month. In column (1), we include individual fixed effects, in column (2) we add month and canton fixed effects. In column (3) we add individual controls including age, a dummy for having a high deductible, and an indicator for living in an urban area. In column (4), we additionally control for the supply of psychotherapists in the vicinity of an individual. The entries in the row "Baseline" report the mean of the dependent variable in the pre-treatment period in the treatment group. The entries in the row "Effect in Percentages" express the treatment effect relative to this baseline.

Figure 5: Dynamic Treatment Effects

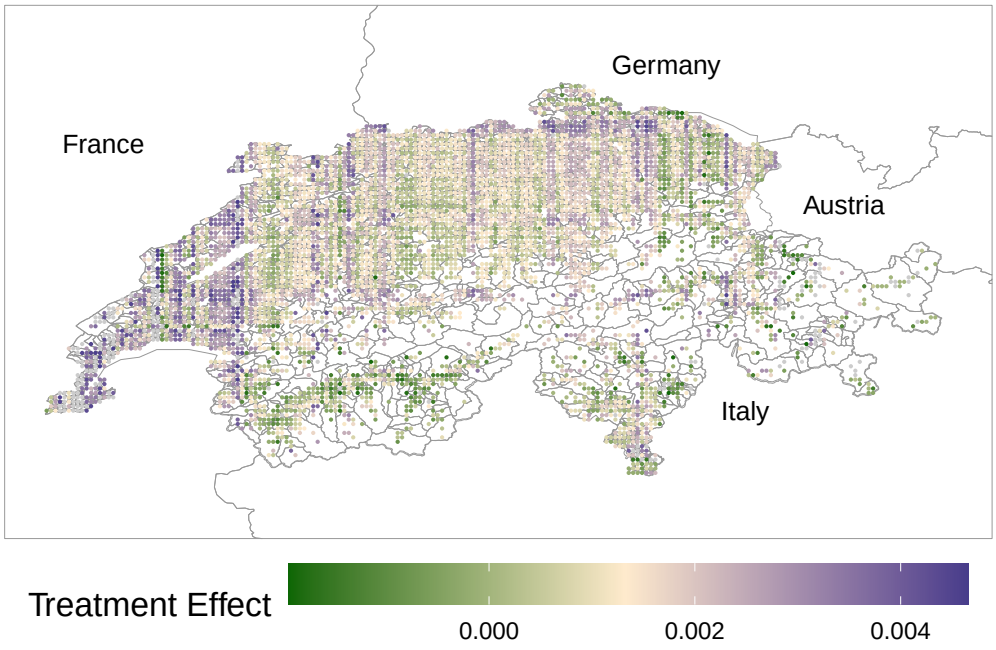


Notes: This figure depicts monthly estimates including 95% confidence intervals from estimating equation (1). We use data from January 2013 to December 2016. The dependent variable is an indicator of having had any psychotherapist visit in a given month. In the regression, we include fixed effects for the individual, the month, and the canton as well as individual controls including age, a dummy for having a high deductible, an indicator for living in an urban area, and a control for the supply in psychotherapists in the neighborhood of an individual. As the reference period, we use November 2014 to avoid seasonal dependence when picturing dynamic effects caused by incentive effects concerning the deductible usage in December.

To explore the dynamics of our treatment effect, we use an event study design with November 2014 as the reference period.[18] Figure 5 depicts the estimated parameters for each month and documents two main insights. First, there are no visible pre-trends in the months before the exchange rate shock. Second, the impact of the exchange rate shock seems to set in during the second half of 2015. This suggests that individuals have a reaction period to the shock of about half a year. This behavior is in line with individuals first having to consult a general practitioner and only then being referred to a psychotherapist.

18. The reason for this choice is that December is not a good reference period because health care consumption is distorted by the holidays. Both demand and supply are significantly lower than in other months.

Figure 6: Exposure to the currency shock



Notes: This figure illustrates the geographical distribution of the treatment effect. We abstract from the treatment groups of the main results and calculate average treatment effects substituting the treatment group indicator by new regional bins when estimating our main equation (1). For each grid point, we calculate the average treatment effect for all individuals. The color scale is such that the median of the resulting treatment effects is the midpoint, which is 0.0013. The size of each grid point is about 1.75×1.75 kilometers.

We explore the regional distribution of the treatment effect in more detail by estimating our main equation (1) but replacing the treatment indicator, D_i, with two indicator variables that capture an individual's latitude and her longitude. Each latitude indicator is about 1.75 kilometers wide and each longitude indicator is about 1.75 kilometers long. Based on the results of this model, we predict the difference in probability of a psychotherapist visit before and after the exchange rate shock for each 1.75×1.75 kilometers grid point.[19] These probability differences are depicted in Figure 6 where the mid-point of the color scale is set to the median of the predicted treatment effect differences. Areas shaded in blue have higher treatment effects, while areas shaded in green show treatment effects that are close to zero or even slightly negative.[20] From this exercise, two interesting results emerge. First, the median of the average treatment effects is 0.0013 and thus very close to our estimated treatment effect in Table 2. Second, we observe a large treatment effect in urban centers, particularly those close

19. For the covariates, we take the mean values in the respective grid point.

20. We cannot estimate treatment effects for each separate grid point due to computational reasons. The consequence of this is that treatment effects are heavily correlated within the same longitude and the same latitude.

to the border. Geographically, the effect is most pronounced in the Western part of Switzerland, which is close to the French border, and a bit less pronounced in the Northern and Southern parts. These findings lend support to the notion that our empirical findings do not depend on the exact definition of treatment, control, and excluded groups, but that the treatment effect for the grid points is strongly correlated with the driving distance to the next border crossing as depicted in Figure 3.

6.2 Robustness

We probe the sensitivity of our main findings by conducting six robustness tests relative to our main results reported in column (4) of Table 2. First, we use alternative definitions of our treatment group. Second, we vary the sample and control for observed and unobserved changes over time. Third, we use an alternative measure of mental health care consumption. Finally, we check for sample attrition.

Continuous treatment measure — In our main analysis, we argue that individuals who live up to 15 minutes from the next border crossing to the eurozone are more affected by the exchange rate shock than those who live more than 30 minutes away. The binary treatment definition and the respective cutoff are discretionary but important decisions of our empirical design. In our first robustness test, we include the driving distance by car to the next border crossing as a continuous variable and interact it with our post-treatment indicator. To facilitate the comparison of this estimate with our main estimate, we invert the distance variable and express it in hours instead of minutes for the same sample as for our main analysis.[21] Column (2) of Table 3 reports the results and documents that the exchange rate shock increased psychotherapist visits. Together with Figure 6, this result provides support for the notion that our main results are not driven by the specific functional form of defining the treatment and the control group.

Sample of cantons with treatment variation — Another concern for the interpretation of our results might be that our control group is not a valid counterfactual for our treatment group in the post-treatment period. One important difference is that our treatment group comprises cantons with a large share of urban centers, while our control group includes cantons with a larger share of rural municipalities. As long as the heterogeneity between the two groups remains constant across time, it will be absorbed in the individual and the cantonal fixed effects and thus does not pose a problem

21. If we extend our main analysis sample by adding the excluded group of individuals, the estimate of this regression is 0.0014, and thus virtually identical to the estimate reported in Table 3 in column (2).

Table 3: Robustness tests

	(1)	(2)	(3)	(4)	(5)	(6)
Treatment × Post	0.0012	0.0015	0.0010	0.0006	0.0012	0.0008
	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0002)	(0.0003)
Observations	24,975,888	24,975,888	18,668,747	24,787,371	24,905,814	24,975,888
R^2	0.44049	0.44049	0.43907	0.44035	0.44083	0.44778
Individual FE	✓	✓	✓	✓	✓	✓
Month FE	✓	✓	✓	✓	✓	✓
Canton FE	✓	✓	✓	✓	✓	✓
Individual Controls	✓	✓	✓	✓	✓	✓
Supply Control	✓	✓	✓	✓	✓	✓

Notes: This table reports regression coefficients from estimating equation (1) with clustered standard errors (by individual) in parentheses to account for within-individual serial correlation in outcomes. We use data at the monthly level from January 2013 to December 2016. The dependent variable in columns (1) to (5) is an indicator of having had any psychotherapist visit in a given month. In all regressions, we include fixed effects for the individual, the month, and the canton, and we additionally control for a person's age, a dummy for having a high deductible, and an indicator for living in an urban area. In column (1), we report our main estimate from Table 2 column (4). In column (2), we estimate equation (1) using the continuous measure of the distance to the closest border crossing in hours. In column (3), we only keep individuals who live in cantons that have both treated and control observations. In column (4), we control for labor market-specific time trends. In column (5), we add a control for the share of refugees in terms of total population in a municipality of residence of an individual. In column (6), we report the coefficient with a different dependent variable, namely an indicator for purchasing at least one psychotherapeutic drug in a month.

for our difference-in-differences estimations. If, however, our outcome variable follows differential trends across cantons, the fixed effects will no longer be able to capture this heterogeneity and our main estimate would suffer from a bias. To account for this possibility, we assess the sensitivity of our main estimates by analyzing a sample that includes only individuals from cantons where both treated and control individuals are present. This robustness test drops all observations from the cantons of Schaffhausen and Basel-Stadt with only treated individuals and from the cantons of Glarus, Lucerne, Nidwalden, Obwalden, Schwyz, Uri, and Zug with only control individuals. The estimates in column (3) of Table 3 are almost identical to the estimates in the main results.

Labor market-specific time trends — Figure 4 suggests that the pre-treatment trends for both our outcome measures are comparable between the treatment and the control group. Despite this similarity, trends in labor markets might evolve differently over time. We address this concern by adding labor market-specific time trends to our main equation (1). The entries in column (4) of Table 3 indicate that our results for psychotherapist visits decrease but remain statistically significant when including these time trends.

Refugee crisis — A potential concern about the validity of our estimates is that other developments that may affect mental health care consumption happened at the same time as the exchange rate shock. If these developments have a differential impact on the treatment and the control groups, they may confound our estimates of the exchange rate shock. One such important development is the increased influx of refugees in 2015 and 2016 into Switzerland. This increase was predominantly a consequence of the ongoing wars in Syria, Iraq, and Afghanistan (SEM 2015, 2016). If the influx of refugees had a differential impact on the mental health of individuals in our treatment and control groups, our estimate of the impact of the exchange rate shock on mental health could be biased. We explore this concern by controlling for the number of refugees and provisionally admitted foreigners as a share of the total population in a municipality interacted with a binary indicator for the time after the exchange rate shock. Column (5) in Table 3 shows that the inclusion of this potentially omitted variable does not change our coefficients for the exchange rate shock.

Psychotherapeutic drug consumption — In our main analysis, we have explored the impact of the perceptions of economic conditions on psychotherapist visits. These in-person visits are one method to treat mental health problems, but therapists may rely on different treatment methods. Most prominently, physicians can prescribe psychotherapeutic drugs to their patients. In column (6) of Table 3, we use an indicator variable that captures whether an individual has purchased a prescribed psychotherapeutic drug in a given month as the dependent variable.[22] We find that the exchange rate shock increased the purchase probability by 0.08 percentage points. Relative to the baseline of 0.0775, the mean during the pre-treatment period in the treatment group, the estimate corresponds to an increase of 1.0%.

Sample attrition — In our estimations, we use a balanced panel of individuals observed throughout the entire study period who do not move across treatment, control, and excluded regions. A potential concern is that individuals might strategically move to or from the treated border region. This would mean that we exclude people who might be more adaptive and less affected by changes in macroeconomic conditions. To probe the validity of this concern, we explore the moving patterns using the unbalanced panel of our data. Figure A.2 in the online Appendix documents the share of individuals moving for each month in our data. First, we find that the share of movers is very low. Second, we do not observe a pattern of increased movements after the exchange rate shock.

22. Figure A.3 in the online Appendix documents support for the validity of the parallel trends assumptions for this alternative dependent variable.

Randomization inference — In our main results, we cluster the standard errors at the individual level. Since the impact of the exchange rate shock differs by region, it may create dependencies across several other dimensions, such as within households. These dependencies would render traditional asymptotic inference invalid. An alternative way to conduct statistical hypothesis testing is randomization inference. Therefore, we conduct a test that is related to traditional randomization inference (Fisher 1935). We keep the numbers of post-treatment periods identical to our main analysis but draw a vector that randomly assigns each month to either the pre-treatment or post-treatment period. We then estimate our main equation with the same specification as in equation 1 but with the modified indicator variable for $Post_t$. The results of 1,000 iterations of this exercise are depicted in Figure A.4 in the online Appendix. As expected, the distribution of treatment effects under the null hypothesis is centered around zero. More importantly, our estimated treatment effect of 0.0012, marked by the red line, is far to the right of the null distribution. We conclude that we reject the null hypothesis of no impact of the exchange rate shock on the probability of at least one monthly visit with a psychotherapist.

6.3 Mechanisms

From a theoretical perspective, the impact of the exchange rate shock on mental health is a composite of two effects that point in opposite directions. On the one hand, we expect that the exchange rate shock increases job insecurity and expected unemployment, both due to the threat of a recession, and due to the increased job competition from foreign workers, as the Swiss Franc appreciates. This effect is more pronounced in border regions because cross-border workers are almost exclusively working in those regions and not in the central regions of the country as documented in Figure A.1 in the online Appendix. Therefore, the labor market mechanism postulates that individuals in border regions are more likely to have a visit with a psychotherapist compared to individuals in non-border regions. On the other hand, the purchasing power of the Swiss Franc increases with its appreciation after the exchange rate shock. Individuals living closer to the border are those who benefit most from this increase, both through the higher pressure on Swiss prices in bordering regions and their vicinity to cross-border shopping opportunities. The consumption mechanism thus posits that visits with a psychotherapist should decrease for individuals living closer to the border relative to individuals in non-border regions. In our main analysis, we find that psychotherapist visits decrease by 16.3% relative to the baseline. This evidence suggests that the labor market effect is larger than the consumption

effect in absolute terms. In this subsection, we will provide several pieces of evidence to support this conclusion.

In the first step, we explore whether the exchange rate shock changed *actual* labor market conditions or only *perceived* labor market conditions. We estimate equation (1) with the share of unemployed individuals and the share of temporary residents in a municipality of residence as dependent variables. Table B.2 in the online Appendix presents the results. The effect of the exchange rate shock on unemployment shares is slightly negative but very close to zero. This means that if anything, the exchange rate shock has slightly decreased unemployment rates by 0.05 percentage points. The effect on temporary residents is positive but also very close to zero. For those variables, the parallel trends assumption is plausible as documented in Panels (A) and (B) of Figure A.5 in the online Appendix. For the variable that captures the number of cross-border workers in an individual's municipality, the parallel trends assumption is not plausible given the graphical evidence in Panel (C). This is because there are virtually no cross-border workers in the control region. Because of the lack of parallel trends, we do not perform difference-in-differences regressions for cross-border workers. Instead, we provide graphical evidence that the share of cross-border workers steadily increases both in the pre-treatment and the post-treatment period, but there is no difference in trends between the two periods, as suggested in Panel (C) of Figure A.5 of the online Appendix. Overall, our analysis provides evidence that the exchange rate shock led to no major differences in terms of actual labor market conditions in the treated versus the control group.

In the second step, we shed light on changes in consumer sentiment after the exchange rate shock by analyzing questions from the Swiss Consumer Sentiment Index, a survey representative for the Swiss population. Unfortunately, the survey does not provide data on individuals' residence municipality, preventing us from separately exploring consumer sentiments for the treated and control groups. Figure A.6 in the online Appendix reports monthly averages of four different survey questions. We observe that the anticipated economic situation had deteriorated before the currency shock and persisted in a declining trend until the end of our study period. The expected personal financial situation remains very stable over the entire study period. When focusing on the labor market perception, we find a substantial increase in expected unemployment and a decrease in perceived job security after the currency shock. The analysis thus far provides evidence that the exchange rate shock negatively affected Swiss residents' perceptions about the labor market, but that these fears of an economic downturn did not materialize.

In the third step, we explore the labor market mechanism with a heterogeneity analysis of our estimation sample. To shed light on the effect acting through the pressure on the labor market, we split our sample by a dummy for whether an individual was of working age for the entire observation period and estimate the effect of the discontinuation of the minimum exchange rate separately for each sample. The results documented in Table B.3 of the online Appendix show that the positive effect on the probability of having at least one psychotherapist visit in a month is driven by working-age individuals, while we do not find an effect for retirement-age individuals. This result is in line with the consumer sentiment evidence. Individuals do not worry increasingly about the general economic situation or their personal financial situation after the currency shock, as the effects of a threat of a recession and the increase in real purchasing power of the Swiss Franc work in opposite directions for individuals. However, individuals fear the labor market effects, which could act through increased competition caused by an influx of foreign workers or by the pressure on the prices of Swiss goods, caused by the appreciation of the Swiss Franc.

To explore whether increased competition might be the determinant of our results, we build on a large body of laboratory and observational studies that have shown that women shy away from competitive environments and are more likely to be negatively affected by increased pressure (Buser, Niederle, and Oosterbeek 2014; Niederle and Vesterlund 2007; Black, Jackson, and Johnston 2022; Ahammer and Packham 2023). We therefore expect working-age women to be more affected by the shock than their male counterparts. To empirically test this conjecture, we split our sample of working-age individuals by gender and report the results in Table B.3 of the online Appendix. The point estimate for working-age women is almost double the point estimate for men and also substantially larger when expressed relative to their baselines.

In the final step of this mechanism section, we account for the fact that individuals may not work where they live. In our main analysis, we use the distance between an individual's residence and her closest border crossing to measure the exposure to the exchange rate shock. If individuals commute for work, this might introduce measurement error. As an alternative exposure measure, we employ the distance of labor market centers to the next border crossing. In Table B.4 in the online Appendix, we report the findings of estimating our main regression equation for the full sample and for the sample splits described above to further explore the labor market mechanism. In sum, we document that the estimates are of similar magnitude as when using our main treatment definition based on the distance between individuals' residence and their closest border crossing.

7 Conclusion

We estimate the effect of perceived economic conditions on mental health in Switzerland. We use an exchange rate shock that takes place in a stable macroeconomic environment. This shock deteriorated individuals' unemployment expectations and their sentiments about job security but did not lead to a recession or an increase in unemployment. We compare individuals living close to the eurozone border who are most affected by this currency shock to individuals living farther away who are less affected and serve as a control group in our analysis. Using our difference-in-difference setting, we find a large and positive effect on the probability of at least one monthly visit with a psychotherapist. In our mechanism section, we first document that *actual* labor market conditions were unaffected by the currency shock, while *perceived* labor market conditions changed. To further explore these mechanisms, we provide split sample estimates, which suggest that the population most at risk of these changes in perceived labor market conditions exhibits the largest treatment effect. Our results provide evidence for a causal impact of perceived economic conditions on mental health via changes in labor market perceptions. Future research may explore how perceptions about macroeconomic conditions other than those related to the labor market affect mental health. From a policy perspective, policymakers should be advised to pay more attention to the *perceived* labor market conditions of the population, rather than relying solely on *actual* economic indicators to decrease health care costs caused by psychological distress.

Acknowledgments and disclosure We thank Demid Getik, Bryan Graham, Sandro Heiniger, Thomas G. McGuire, Fanny Puljic, Christian P.R. Schmid, Nicolas Schreiner, and Hannes Schwandt as well as participants of the 38th meeting of the European Economic Association, of the Conference of the European Association of Labour Economists 2023, of the 11th Annual Conference of the American Society of Health Economists, of the 15th iHEA Biannual World Congress, of the Conference 2022 of the European Health Economics Association, of the 14th Annual Congress of the German Health Economics Association, of the Annual Congress 2023 of the Swiss Society of Economics and Statistics, and seminar participants of the University of California Berkeley, Lucerne, Lund, and Technology Sydney for their valuable comments and suggestions. We thank CSS Insurance for providing us the data for this research project. This project did not receive any funding.

References

Ahammer, Alexander, and Analisa Packham. 2023. "Effects of Unemployment Insurance Duration on Mental and Physical Health." *Journal of Public Economics* 226:104996.

Auer, Raphael, Ariel Burstein, and Sarah M. Lein. 2021. "Exchange Rates and Prices: Evidence from the 2015 Swiss Franc Appreciation." *American Economic Review* 111 (2): 652–686.

Avdic, Daniel, Sonja C. de New, and Daniel A. Kamhöfer. 2021. "Economic Downturns and Mental Health in Germany." *European Economic Review* 140:103915.

Baltensperger, Ernst. 2016. *Der Schweizer Franken - Eine Erfolgsgeschichte.* Zürich: NZZ Libro.

Beerli, Andreas, Jan Ruffner, Michael Siegenthaler, and Giovanni Peri. 2021. "The Abolition of Immigration Restrictions and the Performance of Firms and Workers: Evidence from Switzerland." *American Economic Review* 111 (3): 976–1012.

Black, Nicole, Angela Jackson, and David W. Johnston. 2022. "Whose Mental Health Declines During Economic Downturns?" *Health Economics* 31 (1).

Black, Sandra, Paul Devereux, and Kjell Salvanes. 2015. "Losing Heart? The Effect of Job Displacement on Healthlosing Heart? The Effect of Job Displacement on Health." *ILR Review* 68 (4): 833–861.

Browning, Martin, and Eskil Heinesen. 2012. "Effect of Job Loss Due to Plant Closure on Mortality and Hospitalization." *Journal of Health Economics* 31 (4): 599–616.

Brunnermeier, Markus K., and Ricardo Reis. 2023. *A Crash Course on Crises.* Princeton: Princeton University Press.

Buser, Thomas, Muriel Niederle, and Hessel Oosterbeek. 2014. "Gender, Competitiveness, and Career Choices." *Quarterly Journal of Economics* 129 (3): 1409–1447.

Chang, Shu Sen, David Stuckler, Paul Yip, and David Gunnell. 2013. "Impact of 2008 Global Economic Crisis on Suicide: Time Trend Study in 54 Countries." *British Medical Journal* 347 (7925): 1–15.

Credit Suisse. 2013. "Retail Outlook 2013 – Fakten und Trends." *Credit Suisse Economic Research.*

Deaton, Angus. 2003. "Health, Inequality, and Economic Development." *Journal of Economic Literature* 41 (1): 113–158.

Engelberg, Joseph, and Christopher A. Parsons. 2016. "Worrying about the Stock Market: Evidence from Hospital Admissions." *Journal of Finance* 71 (3): 1227–1250.

Fichera, Eleonora, and John Gathergood. 2016. "Do Wealth Shocks Affect Health? New Evidence from the Housing Boom." *Health Economics* 25 (2): 57–69.

Fisher, Ronald Aylmer. 1935. *The Design of Experiments.* Oliver Boyd.

FSO - Swiss Federal Statistical Office. 2023a. *Foreign Trade by Goods and Imports by Trading Partners (Countries).* Neuchâtel.

———. 2023b. *Gross Domestic Product: Expenditure Approach.* Neuchâtel.

———. 2023c. *Imports by Trading Partners (Countries).* Neuchâtel.

FSO - Swiss Federal Statistical Office. 2014. *Labor Market Indicators 2014.* Neuchâtel.

———. 2022. *Price Level Indices in International Comparison (38 European Countries).* Neuchâtel.

———. 2017. *Raumgliederungen der Schweiz - Gemeindetypologie und Stadt/Land-Typologie 2012 (Spatial Divisions of Switzerland - Community Typology and Urban/Rural Typology 2012).* Technical report.

Habib, Maurizio M., and Livio Stracca. 2012. "Getting Beyond Carry Trade: What Makes a Safe Haven Currency?" *Journal of International Economics* 87 (1): 50–64.

Johnston, David W., Michael A. Shields, and Agne Suziedelyte. 2020. "Macroeconomic Shocks, Job Security, and Health Evidence From the Mining Industry." *American Journal of Health Economics* 6 (3).

Kuhn, Andreas, Rafael Lalive, and Josef Zweimüller. 2009. "The Public Health Costs of Job Loss." *Journal of Health Economics* 28 (6): 1099–1115.

Lindqvist, Erik, Robert Östling, and David Cesarini. 2020. "Long-Run Effects of Lottery Wealth on Psychological Well-Being." *Review of Economic Studies* 87 (6): 2703–2726.

McInerney, Melissa, Jennifer M. Mellor, and Lauren Hersch Nicholas. 2013. "Recession Depression: Mental Health Effects of the 2008 Stock Market Crash." *Journal of Health Economics* 32 (6): 1090–1104.

Niederle, Muriel, and Lise Vesterlund. 2007. "Do Women Shy Away From Competition? Do Men Compete Too Much?" *Quarterly Journal of Economics* 122 (3): 1067–1101.

Plag, Jens, Claudia Hägele, and Andreas Ströhle. 2012. "Pharmakotherapie der Angststörungen." *Psychotherapie* 17 (1): 62–72.

Rudolph, Thomas Wirtschaftswissenschaftler, Liane. Nagengast, and Frauke. Nitsch. 2015. *Einkaufstourismus Schweiz 2015: Eine Studie zu den aktuellen Entwicklungen des Einkaufstourismus.* Universität St. Gallen - Forschungszentrum für Handelsmanagement.

Ruhm, Christopher J. 2000. "Are Recessions Good for Your Health?" *Quarterly Journal of Economics* 115 (2): 617–650.

Ruhm, Christopher J. 2015. "Recessions, Healthy No More?" *Journal of Health Economics* 42:17–28.

Schmid, Christian P.R., Konstantin Beck, and Lukas Kauer. 2018. "Health Plan Payment in Switzerland." In *Risk Adjustment, Risk Sharing and Premium Regulation in Health Insruance Markets: Theory and Practice,* edited by Thomas G. McGuire and Richard van Kleef, 453–489. Elsevier Publishing.

Schmid, Lukas. 2023. "The Impact of Host Language Proficiency on Migrants' Employment Outcomes." *American Economic Review: Insights, forthcoming.*

Schwandt, Hannes. 2018. "Wealth Shocks and Health Outcomes: Evidence from Stock Market Fluctuations." *American Economic Journal: Applied Economics* 10 (4): 349–377.

SEM - State Secretariat for Migration. 2016. *Asylstatistik 2015.* Technical report. Bern.

———. 2017. *Asylstatistik 2016.* Technical report. Bern.

Sullivan, Daniel, and Till von Wachter. 2009. "Average Earnings and Long-Term Mortality: Evidence from Administrative Data." *American Economic Review* 99 (2): 133–138.

Yilmazer, Tansel, Patryk Babiarz, and Fen Liu. 2015. "The Impact of Diminished Housing Wealth on Health in the United States: Evidence From the Great Recession." *Social Science and Medicine* 130.

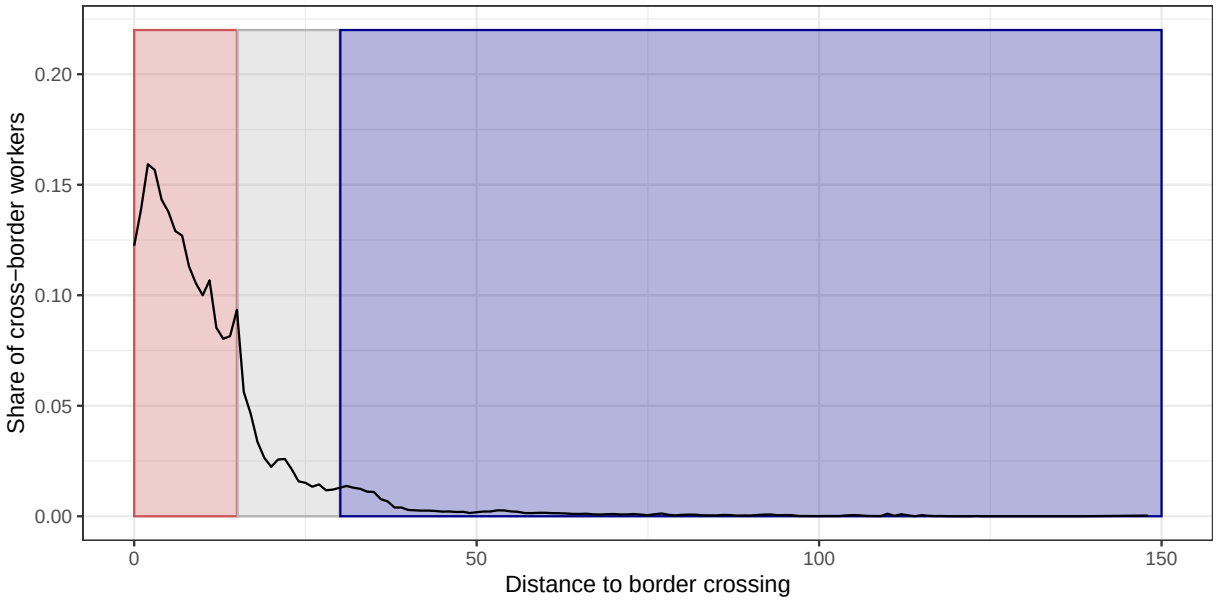
Online Appendix for

Perceptions of Economic Conditions and Mental Health

Lukas Kauer, Lukas Schmid, Valentina Sontheim

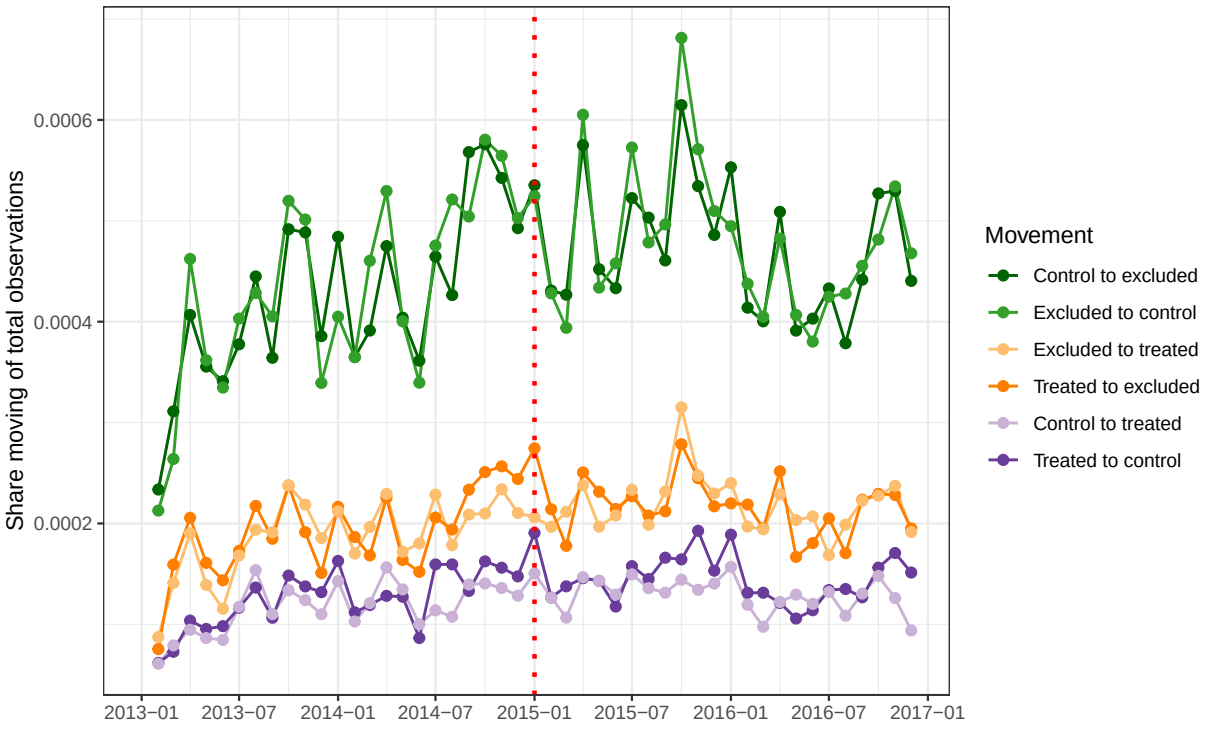
A Additional Figures

Figure A.1: Share of Cross-Border Workers by Distance to the Border



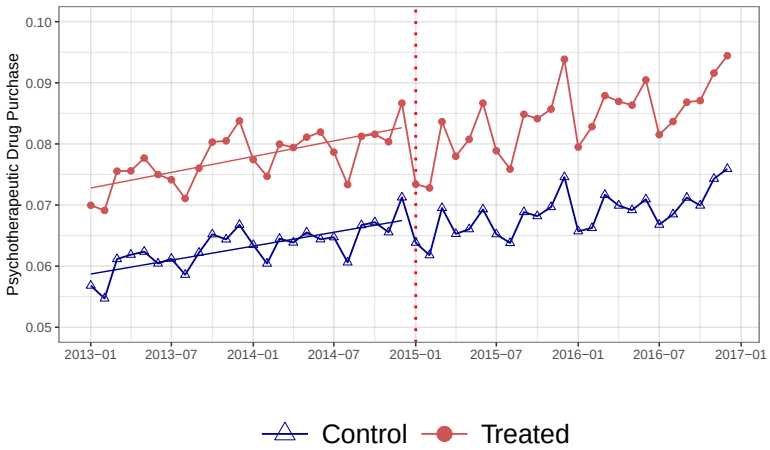
Notes: This figure shows the share of cross-border workers in an individual's municipality of residence per minute of driving distance by car from an individual's residence to the next border crossing in the pre-treatment period. The red shaded area depicts our treatment region, the grey shaded area depicts our excluded region, and the blue shaded area is our control region.

Figure A.2: Individuals Moving across Treatment Groups



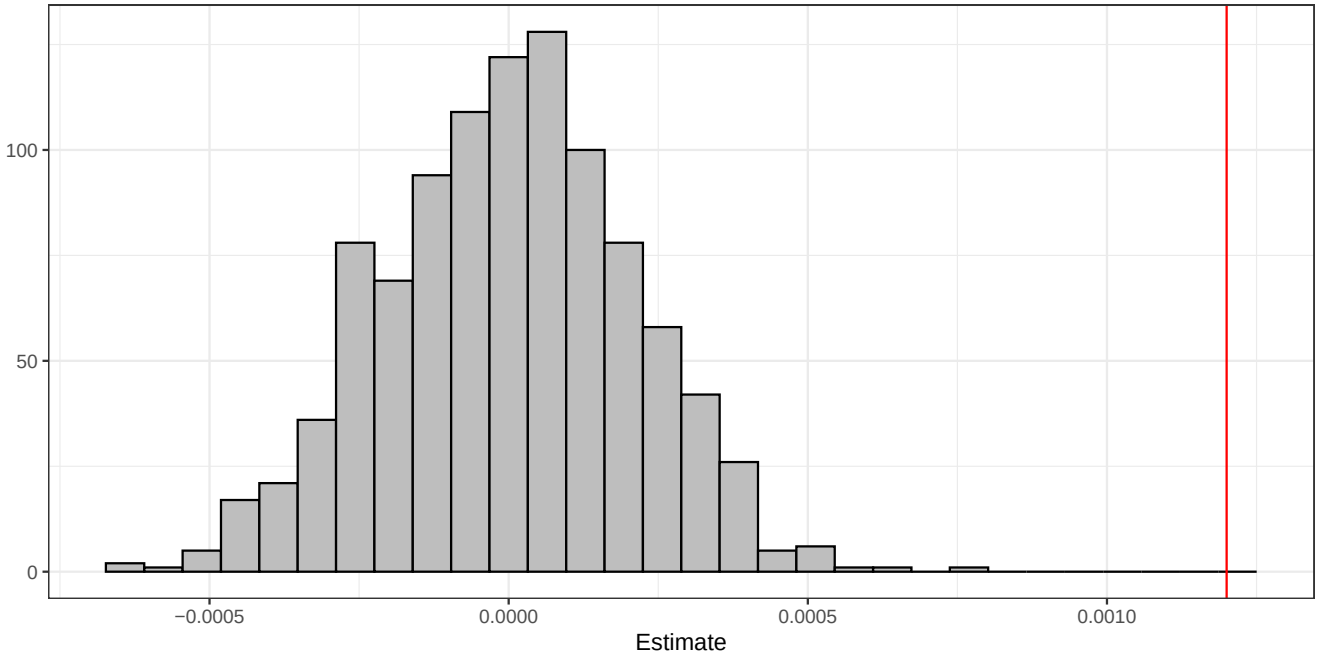
Notes: This figure shows the share of individuals in our data changing their residential address per month by direction of movement across treatment regions for the years 2013 to 2016.

Figure A.3: Evolution of Average Purchases of Prescribed Psychotherapeutic Drugs



Notes: This figure depicts the monthly averages of our alternative dependent variable that captures whether an individual purchased at least one psychotherapeutic drug in a month. The treatment group (red circles) includes individuals who live within 15 minutes of driving distance of the next border crossing by car. The control group (blue triangles) includes individuals who live 30 minutes or more from the nearest border crossing.

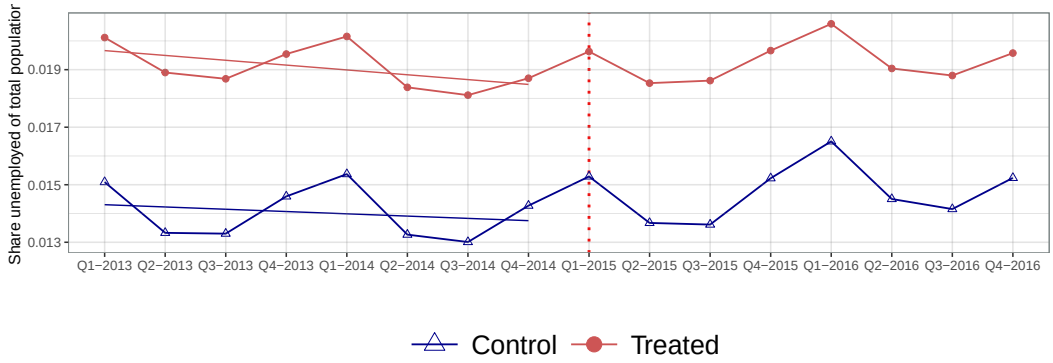
Figure A.4: Results from Randomization Inference



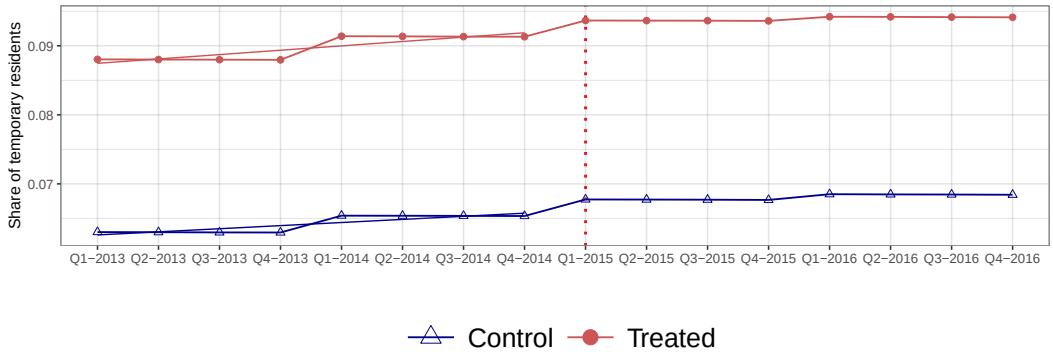
Notes: This figure shows the results from our randomization inference. The number of post-treatment periods is 24, as in our main specification, but we randomly assign each month to the pre- or post-treatment period and estimate our main equation (1) with the modified indicator for the post-treatment period, $Post_t$. We run this regression 1000 times and plot the frequencies of the resulting estimated coefficient of interest, $\hat{\tau}$. The red line indicates our treatment effect of 0.0012 reported in column (4) of Table 2.

Figure A.5: Evolution of Actual Labor Market Conditions

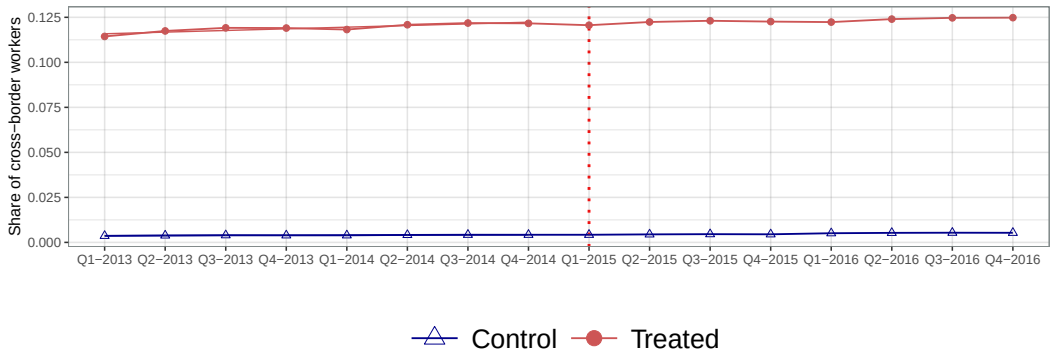
(A) Share of Unemployed in municipality



(B) Share of Temporary Residents in Municipality



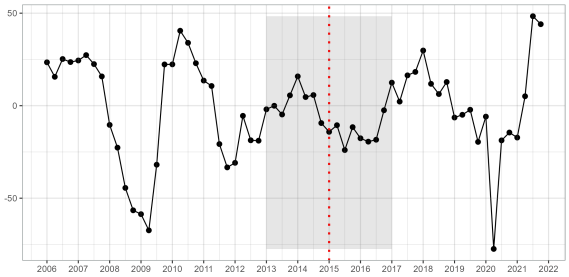
(C) Share of Cross-border Workers in Municipality



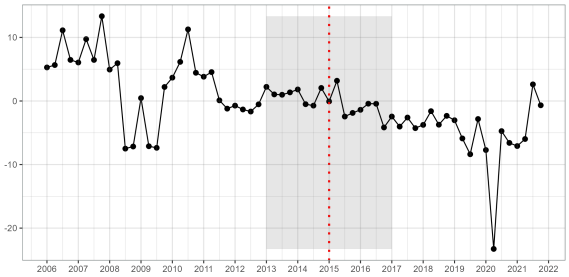
Notes: This figure depicts quarterly averages of our indicators for actual labor market conditions at the residence municipality level of the individuals in our sample. Panel (A) depicts the average for the unemployment share. Panel (B) depicts the average for the share of temporary residents. Panel (C) depicts the average for the share of cross-border workers. The treatment group (red circles) includes individuals living within 15 minutes of driving distance of the next border crossing by car. The control group (blue triangles) includes individuals living 30 minutes or more from the nearest border crossing. Note that the yearly shifts in Panels (A) and (B) are due to limited frequency of some variables. *Source:* SECO, FSO, and own calculations.

Figure A.6: Consumer Sentiment Analysis

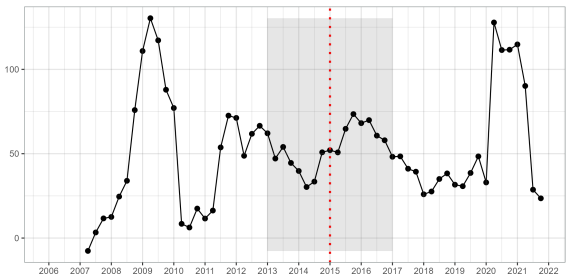
(A) Expected Economic Situation



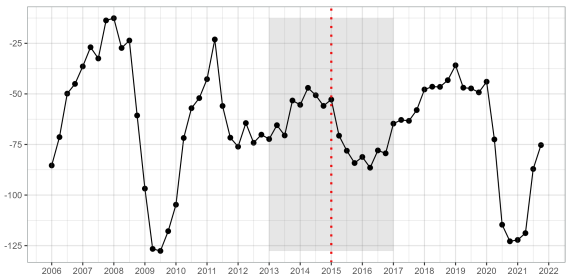
(B) Expected Financial Situation



(C) Expected Unemployment



(D) Perceived Job Security



Notes: This figure shows answers to four questions that are part of the quarterly consumer sentiment index. Panel (A) shows the answer to the question was "How do you think the general economic situation will evolve in the next 12 months?" The mean of all answers is then multiplied by 100 and reported in the figure. Panel (B) shows the answer to the question "How do you think the financial situation of your household will evolve in the next 12 months?" In both questions, respondents can choose from a 5-point Likert scale which is scaled from -2 (substantially deteriorate) to +2 (substantially improve). In Panel (C), the question is "How do you think the number of unemployed in Switzerland will evolve in the next 12 months?" Respondents can choose from a 5-point Likert scale which is scaled from -2 (substantially decrease) to +2 (substantially increase). In Panel (D), the question is "How do you think has the security of jobs evolved?" Respondents can choose from a 5-point Likert scale which is scaled from -2 (substantially more insecure) to +2 (substantially more secure). In all questions, the mean of all answers is then multiplied by 100 and reported in the figure.

B Additional Tables

Table B.1: Test for Pre-Trends

Dependent variable:	**Psychotherapist visits**			
	(1)	(2)	(3)	(4)
Treatment × Time Trend	5.23e-6	5.14e-6	4.71e-6	4.33e-6
	(1.22e-5)	(1.22e-5)	(1.22e-5)	(1.23e-5)
Observations	12,487,944	12,487,944	12,487,944	12,487,944
R^2	0.53773	0.53775	0.53775	0.53775
Individual FE	✓	✓	✓	✓
Month FE		✓	✓	✓
Canton FE		✓	✓	✓
Individual Controls			✓	✓
Supply Control				✓

Notes: This table reports regression coefficients of an interaction term between the linear time trend and the treatment indicator with clustered standard errors (by individual) in parentheses to account for within-individual serial correlation in outcomes. We only use the data in the pre-treatment period at the monthly level from January 2013 to January 2015. The dependent variable is an indicator of having had any psychotherapist visit in a month. In column (1), we include individual fixed effects, in column (2) we add fixed effects for the month and the canton. In column (3) we add individual controls including a person's age, a dummy for having a high deductible, and an indicator for living in an urban area. In column (4) we additionally control for the supply in psychotherapists in the neighborhood of an individual.

Table B.2: Effect of the Currency Shock on the Share of Unemployment and the Share of Permits for Temporary Residents

Dependent variable:	**Unemployed**	**Temporary residents**
	(1)	(2)
Treatment × Post	-0.0005	0.0003
	(8.19e-6)	(3.21e-5)
Baseline	0.0191	0.0897
Effect in Percentages	-2.62%	3.34%
Observations	8,260,826	8,315,314
R^2	0.90580	0.97101
Individual FE	✓	✓
Canton FE	✓	✓

Notes: This table reports regression coefficients from estimating equation (1) with clustered standard errors (by individual) in parentheses to account for within-individual serial correlation in outcomes. We use data at the quarterly level from January 2013 to December 2016. The dependent variables are the share of registered unemployed individuals per municipality in column (1) and the share of temporary residents with work permits per municipality in column (2). In all regressions, we include fixed effects for the individual and the canton. The entries in the row "Baseline" are the means of the respective dependent variable in the pre-treatment period in the treatment region. The entries in the row "Effect in Percentages" express the treatment effect relative to this baseline.

Table B.3: Heterogeneous Effects

Sample:	Working Age	Working Age Female	Working Age Male	Retirement Age
	(1)	(2)	(3)	(4)
Treatment × Post	0.0018	0.0023	0.0013	0.0003
	(0.0003)	(0.0004)	(0.0003)	(0.0002)
Baseline	0.00983	0.01110	0.00853	0.00318
Effect in Percentages	18.31%	20.72%	15.24%	9.43%
Observations	16,208,325	8,123,808	8,084,544	8,767,536
R^2	0.44883	0.44264	0.45748	0.38511
Individual FE	✓	✓	✓	✓
Month FE	✓	✓	✓	✓
Canton FE	✓	✓	✓	✓
Individual Controls	✓	✓	✓	✓
Supply Control	✓	✓	✓	✓

Notes: This table reports regression coefficients from estimating equation (1) with clustered standard errors (by individual) in parentheses to account for within-individual serial correlation in outcomes. We use data at the monthly level from January 2013 to December 2016. The dependent variable is an indicator of having had any psychotherapist visit in a month. In columns (1) and (4), we split our sample by a dummy for whether an individual was of working age during our entire sample period. In columns (2) and (3), we split the sample of working-age individuals by gender. All regressions include fixed effects for the individual, the month, and the canton, and individual controls including a person's age, a dummy for having a high deductible, and an indicator for living in an urban area. Further, we control for the supply of psychotherapists in the vicinity of an individual. The entries in the row "Baseline" report the mean of the dependent variable in the pre-treatment period in the treatment region of the respective sample. The entries in the row "Effect in Percentages" express the treatment effect relative to this baseline.

Table B.4: Heterogeneous Effects with Labor Market Treatment

Sample:	Full Sample	Working Age	Working Age Female	Working Age Male	Retirement Age
	(1)	(2)	(3)	(4)	(5)
Treatment × Post	0.0010	0.0016	0.0019	0.0011	0.0002
	(0.0002)	(0.0003)	(0.0004)	(0.0003)	(0.0002)
Baseline	0.00738	0.00983	0.01110	0.00853	0.00318
Effect in Percentages	13.55%	16.28%	17.12%	12.90%	6.29%
Observations	24,447,926	15,847,395	7,928,066	7,919,329	8,600,531
R^2	0.44223	0.45062	0.44515	0.45822	0.38648
Individual FE	✓	✓	✓	✓	✓
Month FE	✓	✓	✓	✓	✓
Canton FE	✓	✓	✓	✓	✓
Individual Controls	✓	✓	✓	✓	✓
Supply Control	✓	✓	✓	✓	✓

Notes: This table reports regression coefficients from estimating equation (1) with clustered standard errors (by individual) in parentheses to account for within-individual serial correlation in outcomes. We use data at the monthly level from January 2013 to December 2016. The dependent variable is an indicator of having had any psychotherapist visit in a month. In all columns, we use an alternative treatment definition based on the distance of the labor market center of the residence municipality as defined by the FSO to the next border crossing. In columns (3) and (4), we split our sample by age, in columns (5) and (6) we split the sample of working-age individuals by gender. All regressions include fixed effects for the individual, the month, and the canton, and individual controls including a person's age, a dummy for having a high deductible, and an indicator for living in an urban area. Further, we control for the supply of psychotherapists in the vicinity of an individual. The entries in the row "Baseline" report the mean of the dependent variable in the pre-treatment period in the treatment region of the respective sample. The entries in the row "Effect in Percentages" express the treatment effect relative to this baseline.