# Opening of hotels and ski facilities: impact on mobility, spending, and Covid-19 outcomes

**Abstract**

This paper investigates how reopening hotels and ski facilities in Poland impacted tourism spending, mobility, and COVID-19 outcomes. We used administrative data from a government program that subsidizes travel to show that the policy increased the consumption of tourism services in ski resorts. By leveraging geolocation data from Facebook, we showed that ski resorts experienced a significant influx of tourists, increasing the number of local users by up to 50%. Furthermore, we confirmed an increase in the probability of meetings between pairs of users from distanced locations and users from tourist and non-tourist areas. As the policy impacted travel and gatherings, we then analyzed its effect on the diffusion of COVID-19. We found a significant association between tourist movements and the severity of a major pandemic wave in Poland. In particular, counties with ski facilities experienced more infections after the reopening. Moreover, counties strongly connected to the ski resorts during the reopening had more subsequent cases than weakly connected counties.

Keywords: Covid-19, Tourism, Risky behaviors, Mobility, Spatial Spillovers

## 1 Introduction

Regulating risky behaviors often requires balancing competing policy goals. Maximizing utility or income through risky activity comes at the expense of the health of risk-takers and individuals not engaged in the risky behavior. Such negative externalities present a particular challenge in designing an efficient policy, as decision-makers often lack information on the magnitude of the social costs of risky behaviors.

The COVID-19 pandemic offers a unique setting to investigate the trade-off between individual freedoms and the negative externalities they generate (Stiglitz [2021], Stoddard et al. [2021]). This study opportunity arises as authorities attempted to balance stimulating economic activity and preventing infections (Alvarez et al. [2020], Acemoglu et al. [2021], Caulkins et al. [2021]).

The reopening of tourism during a pandemic is an instrumental case. Engaging in tourism has considerable potential for negative spillovers during a pandemic. Tourism creates long-distance movements of the population (Mangrum and Niekamp [2020]) and, as such, can contribute to the spread of infectious diseases (Belik et al. [2011], Bajardi et al. [2011], Findlater and Bogoch [2018]). Simultaneously, tourists generate significant income for local economies. Therefore, any decisions concerning tourist activity require balancing the trade-off between economic and public health outcomes. However, there is currently no evidence quantifying the impact of tourist mobility on the diffusion of infections. This paper aims to fill this gap by analyzing the effects of the reopening of Polish tourism on tourist consumption, mobility, and the spread of COVID-19.

Our study design takes advantage of a unique policy that reopened all hotels and ski lifts in Poland. On February 12th, 2021, the Polish government reopened ski lifts and hotels at 50% capacity, with food supplied through room service only. At the same time, authorities reopened only cinemas, theatres, and operas at 50% capacity with mandatory mask usage [1]. The hotels and ski lifts remained open until the 20th of March, when the second wave of infections ravaged the country.

[1] Such entertainment facilities rarely operate in ski resorts

This setting is particularly relevant as ski resorts play an essential role in the local economies and the Polish tourism industry. While on the national scale, only 2.2% of working Poles are employed in businesses related to hospitality or recreation (H&R), areas with ski resorts rely heavily on income from these sectors. Almost 10% of all businesses active in towns with more than three ski lifts are related to H&R[2].These locations are vital for the tourism industry. Nearly 9% of the country's H&R businesses were located in communes with some ski facilities, constituting 3% of all communes. Moreover, these communes were home to 15.5% of all hotel beds in the country. Hence, the policy of reopening hotels and ski resorts could strongly affect economic activities related to tourism.

We show that the policy indeed caused a significant increase in tourism spending and movements of tourists. First, we relied on the usage of government travel subsidies data to document a swift growth in the consumption of tourist services at ski resorts after the reopening. Second, using aggregated and anonymized geolocation data from Facebook (FB) and an event study framework (similar to Dave et al. [2021a]), we found that the policy's implementation increased the number of FB users in ski resorts by 25%–50%. Moreover, there was a surge in the probability that users from tourist and non-tourist areas were able to meet, as well as those normally living far from each other.

While the number of travelers and tourist gatherings increased due to the reopening policy, its effect on COVID-19 outcomes is not apparent. On the one hand, visitors could carry the disease from their homes to tourist locations and back. On the other hand, they could only gather in their rooms or outdoors as restaurants were closed. To learn more about the impact of the policy on COVID-19 cases, we collected granular infection data and used the uncofoundedness approach from Callaway and Li [2021]. We utilized this newly developed method because traditional fixed effect models have performed poorly in the presence of non-linearities inherent to patterns of COVID-19 diffusion (Gauthier [2021], Goodman-Bacon and Marcus [2020]). We found that counties with ski resorts saw additional cases within the third week of the reopening. Such an early effect is absent in other counties. Moreover, counties strongly interacting with ski resorts during the reopening had a higher incidence of infections than counties with weak interactions. This result is consistent with a secondary spread from tourists bringing the virus back home.

The epidemic has sparked a large body of literature related to COVID-19; however, relatively little research has been dedicated to the effect of tourism on infections. Researchers have shown that industry closures and stay-at-home orders have had a limiting impact on both mobility and subsequent COVID-19 outcomes (Fang et al. [2020], Gupta et al. [2020], Lyu and Wehby [2020], Beria and Lunkar [2021], Courtemanche et al. [2020], Abouk and Heydari [2020], Badr et al. [2020], Lau et al. [2020], Morley et al. [2020], Xu et al. [2020], Goolsbee and Syverson [2021]). Fewer papers analyzed reopening. An exception is Nguyen et al. [2020], who found that lifting restrictions led to a 6–8% increase in mobility.

Opening the tourism industry can lead to travel and large gatherings, and there has been some evidence relating these phenomena to viral spread. One of the seminal papers on this topic is Adda [2016], which demonstrated that school vacations and transportation strikes disrupted viral transmission. More recently, Chernozhukov et al. [2021], Andersen et al. [2021], Courtemanche et al. [2021], Bravata et al. [2021] , Goldhaber et al. [2021] linked school and college operating schedules to the local prevalence of infections. However, the reopening of schools has substantially different effects than tourism reopening, as children are less likely to suffer severe consequences of COVID-19 (Castagnoli et al. [2020], Dong et al. [2020]).

Closer to our population of interest, i.e., adults, are studies analyzing sports, social, and political gatherings. Large sporting events such as hockey, basketball, and football games can lead to higher COVID-19 prevalence (Carlin et al. [2021], Alexander et al. [2020], Breidenbach and Mitze [2021]). Similarly, smaller gatherings such as birthdays or bar meetings increase the likelihood of subsequent infections (Harris [2020], Whaley et al. [2021]). The evidence in the case of political gatherings is mixed. Palguta et al. [2021] found an increase in the growth rate of COVID-19 in areas with elections, while Dave et al. [2020] concluded that a political rally in Tulsa did not affect local COVID cases. They note, however, that the local population enhanced social distancing, which could offset the effect of the gathering. This compensatory behavior is unlikely for the reopening of tourism since local population will interact with incoming tourists by providing

[2]According to the data from December 2019 from the National Register of Business Entities (REGON). On the national level only 3% of businesses are in H&R.

them with hospitality services. Economic and educational gatherings also enhance the local diffusion of infections. Taylor et al. [2020] showed that proximity to livestock plants is associated with higher COVID transmission. Rufrancos et al. [2021] provided evidence that COVID-19 cases spilled from universities to surrounding neighborhoods.

Tourism can also encourage long-distance travel, and it has been shown that travelers contribute to the diffusion of infections. Mangrum and Niekamp [2020] provided evidence that college students returning from spring break trips accelerated the local spread of COVID-19 and the related mortality. Burlig et al. [2021] analyzed how the length of the travel ban mattered for subsequent COVID-19 outcomes and showed an empirical association between migrants traveling home and the following number of cases in their home area. Finally, people attending large events with little protective behavior, such as the Capitol Riot (Dave et al. [2021b]) or Sturgis Motorcylce Rally (Dave et al. [2021a]) bring the disease when traveling back to their home counties. As the literature shows, long-distance travel and large gatherings are associated with the diffusion of COVID-19. The reopening of tourism can encourage travel and gatherings and is highly relevant for the viral spread.

Our main contribution is using a unique quasi-experiment to identify the causal effect of tourism on mobility and infections. In particular, we leverage an interaction of two complementary policies which produced a significant shock to tourist movement: (1) a limited-time policy that allowed the reopening of hotels and ski facilities in Poland during the COVID-19 pandemic and (2) the availability of travel subsidies. In addition, our study relied on novel geolocation data from FB, which permitted us to measure mobility at a very granular scale in time and space. Furthermore, we provide quantitative answers to policymakers who seek to understand the health and economic consequences of reopening tourism in their locations. We first show that reopening ski resorts and hotels increased tourist spending and contributed to long-distance travel and gatherings. Secondly, we provide evidence that reopening tourism accelerated the spread of COVID-19. Finally, we show that the costs of the policy exceeded the benefits.

In the remainder of the paper, we first explain what data we use throughout the paper. The following section (3) discusses empirical methods and results regarding impact of the policy on mobility. Analogously, section 4 examines the effect of the policy on Covid-19 outcomes. The conclusion closes the paper.

# 2 Data

We compiled a unique dataset featuring mobility patterns of FB users, usage of government travel subsidies, and administrative data on COVID-19 related outcomes from the Polish Ministry of Health.

## 2.1 Mobility

The data on mobility came from FB's Data for Good initiative[3]. Since the start of the pandemic, it has been used for various studies mapping human mobility in countries such as the UK (Shepherd et al. [2021]), the USA (Kissler et al. [2020]), or Italy (Shtele et al. [2022], Pieroni et al. [2021], Spelta and Pagnottoni [2021], Beria and Lunkar [2021]). The data originates from FB users who enabled the location services on their devices. Note that manual location tagging is not required from the user. The location is captured when FB or any other app using GPS is active. Users' trajectories are aggregated and anonymized to show patterns of spatial movements. We used two measures of mobility: population and colocation probabilities. Basic information on the construction of these measures is presented below. A more detailed and technical discussion can be found in the appendix. The reliability of the data naturally depends on FB's penetration of the social media market and geolocation usage. In the case of Poland, FB is the most popular social media platform by far exceeding the competition (Hootsuite [2022], Statcounter [2022]). In early 2021, approximately 78% of the traffic generated by social media to other websites in Poland was from FB. Pinterest, with only 7%, was a distant second (Statcounter [2022]). In our data, we found about 1.9 million users with their geolocation services turned on, just over 5% of the Polish population. This number was

[3]Data provided by the Facebook's Data for Good Initiative: https://dataforgood.fb.com/. We thank Alex Pompe for his help with the data

relatively stable during the study period [4]. It is incomparably larger than most traditional datasets providing insights into mobility such as surveys or flight traffic data.

Nonetheless, the representativeness of such data is debatable, as specific demographics may be more likely to use social media or geolocation services. Sloan and Morgan [2015] showed that Twitter users who enable geolocation had different characteristics than users who do not. FB users may better represent the underlying population, as Gibbs et al. [2021] showed that the number of FB users correlated strongly with the local census estimates in the UK. Moreover, they found no specific relationship between age, ethnicity, or poverty and FB usage. In the case of Poland, we saw an uneven distribution in the share of the population feeding FB colocation data. As shown on map 16b in the appendix, FB has higher penetration in western counties, which are more prosperous. Overall, the predictors of FB usage seem orthogonal to the location of ski resorts, and we do not expect it to change in a short time around the policy. Moreover, colocation is defined as a share of possible interactions among available users, so the measure is robust to changes in the number of users.

It must be acknowledged that our estimates of mobility are specific to the population using FB in Poland. Unfortunately, FB does not share the demographic structure of its base. However, in appendix 7 we identified economic and demographic factors which correlates with consistent FB geolocation usage. Counties with high usage of geolocation tend to be more female, younger, and urban, although the differences are minor. It is also reassuring that FB mobility data has been shown to correlate well with other mobility sources, such as geolocation from mobile operator O2 (Jeffrey et al. [2020]) and Google mobility measures (Pérez-Arnal et al. [2021]). On the other hand, Desiderio et al. [2022] used open source data, e.g., train and flight traffic, to argue that FB may underestimate long-distance movements. Such bias would make estimated effects on long-distance travel conservative.

Overall, we are confident that FB's data provided unique and reliable insights into mobility. Below we discuss two measures used throughout the study.

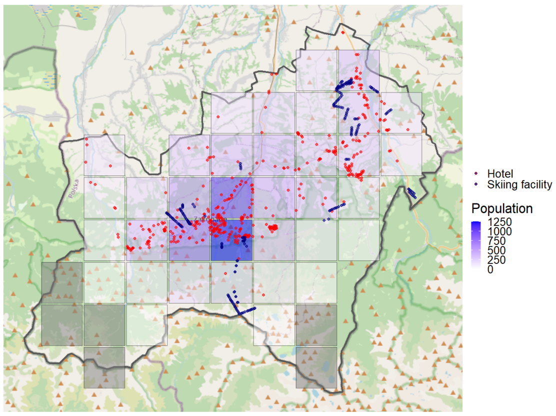
**Population** The population at time window t and tile A is defined as the number of users who were logging mostly from the tile A during the time window t. There were three time windows per 24 hours (with breaks at 00:00, 08:00, and 16:00 UTC) and tiles were approximately 3km x 3km. Observations with less than ten users are omitted for privacy reasons. We assigned tiles to counties based on their centroids. As an example, consider figure 1a. It represents the population logging on the tiles covering Tatrzanski County, a popular tourist destination, on the afternoon (17:00–01:00 ETC) of February 14th, 2021. The red dots and blue dots indicate the location of hotels and ski facilities, respectively. We used hotels and ski facilities' locations[5] to classify tiles as tourist or non-tourist. Figure 1b illustrates the time series of the number of users in Tatrzanski County. After the reopening of hotels, there was a clear uptick in the FB population. See appendix for technical details, and appendix table 1a and figure 14a for the population data summary statistics.

[4]See figure 15 and the discussion on the spatial-temporal trends in FB usage in the appendix

[5]We use "hotel" for any accommodation facility. We found hotel and ski facility coordinates from the OpenStreetMaps project (OpenStreetMap contributors [2017]). The location of ski facilities were scraped from www.narty.pl and validated through own search

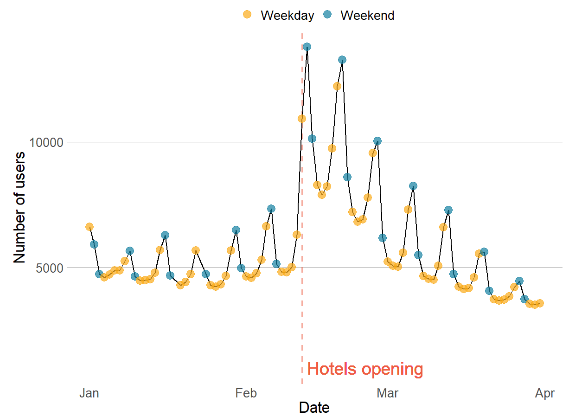
Figure 1: FB Population data

(a) Hotels and population in Tatrzanski county on the afternoon of the 14th February 2021




Note: The color of a tile represents the population, i.e., the number of users logging from the tile. Grey tiles correspond to no records. Red dots represent coordinates of hotels, and navy dots represent coordinates of skiing facilities. Source: OpenStreetMap and own elaboration based on Facebook data

(b) Number of FB users logging in between 17:00 and 01:00 in Tatrzanski county




Note: The count of users in Tatrzanski county represents the sum of users from tiles with centroids in the county. Users logging in multiple tiles during the 8-hour window are assigned to the modal tile. Source: Own elaboration based on Facebook data

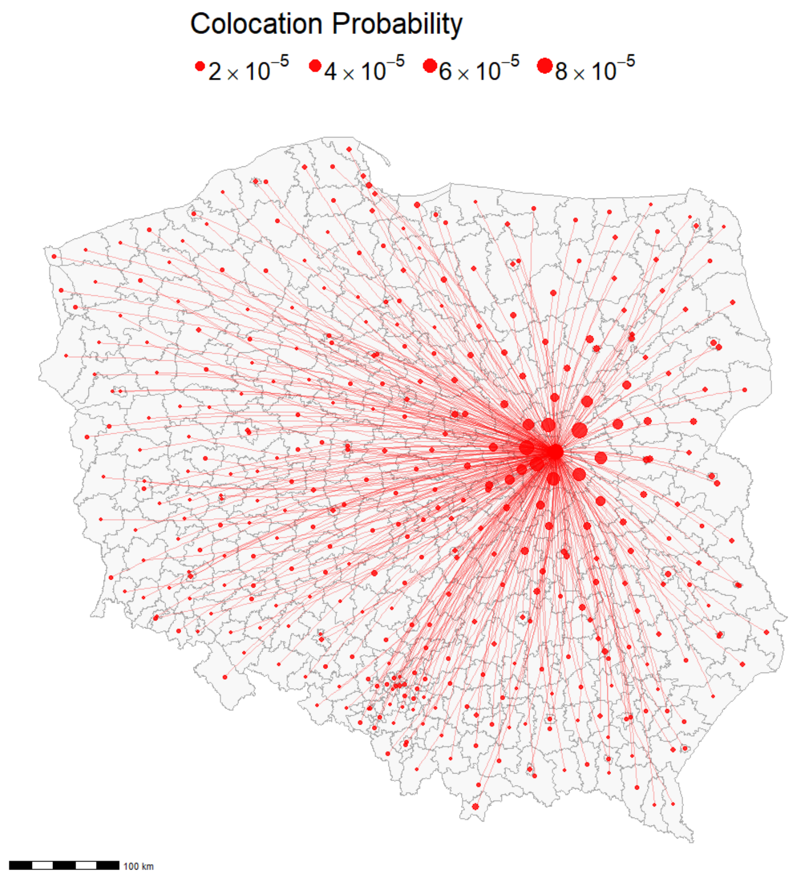
**Colocation** Colocation data aim to approximate how often users from different regions meet. Technically, it measures the probability that two randomly chosen users from county[6] i and j were within the exact location, i.e., the same 0.6km × 0.6km tile [7] at a randomly chosen 5-minute interval of a given week (Wednesday to Tuesday) [8]. We did not consider colocation between users from the same county. Note that the colocation probabilities are small since the denominator is the number of possible pairs of users from the two counties multiplied by the number of 5–minute intervals in a week. The user's county of residence is derived from a consistent history of night-time locations. The map in figure 2 illustrates the colocation probabilities of users from Warsaw with users from other counties in the week ending on February 16th, 2021. The dot size and link transparency are proportional to the colocation probability between Warsaw and the given county. See appendix for technical details, and appendix table 1b and figure 14b for summary statistics of the colocation data.

[6] County (*Powiat*) is an administrative unit larger than a commune. There are 380 counties in Poland

[7] Smaller tiles than in the case of population data

[8] Formally, and ignoring the week index, let $X_{tir}$ be the number of users from region $r$ at tile $t$ in the 5 minute time interval $i$. Then let $m_{rs}$ to be the sum of meetings between pairs of individuals from region $r$ and $s$ across all tiles and time intervals, that is $m_{rs} = \sum_{ti} X_{tir} X_{tis}$. The colocation probability is then the ratio of all actual meetings and all potential meetings, that is: $Pr(Colocation_{rs}) = \frac{m_{rs}}{2016 n_r n_s}$, where $n_r$ is the number of users from region $r$ and 2016 is the number of all five-minute intervals in a week. See Iyer et al. [2020] for more details

Figure 2: Colocation of users from Warsaw and other counties in the week ending on the 16th of February 2021




Note: The size of red dots and the transparency of curved links are proportional to the colocation probabilities between users from Warsaw and a given county. Source: Own elaboration based on FB data.

## 2.2 Spending on Tourism

We approximated the tourism spending by the usage of funds from the *Bon Turystyczny* (Touristic Voucher) governmental program subsidizing travel. This voucher program was initiated in 2020 to revive the tourism industry. Each family is entitled to one voucher (approximately \$130) per child under 18 years old[9], which can be spent on anything related to tourism, such as transportation, accommodation, or organized activities. The voucher can be spent on anything related to tourism, such as transportation, accommodation, or organized activities. This amount is a not insignificant given that Poles spent, on average, \$122 per trip in the first quarter of 2021[10], although skiing usually required higher spending. The data provided by the government specified the voucher amounts paid to businesses. In particular, it showed the total payments received by businesses each week and each *gmina* (commune). A commune is the smallest administrative unit in Poland, and they usually correspond to a town or a couple of villages. There are 2,477 communes in Poland with a median population of 7,486. The commune of the business is the commune where the headquarter is

[9] The value of the voucher doubles for children with disabilities

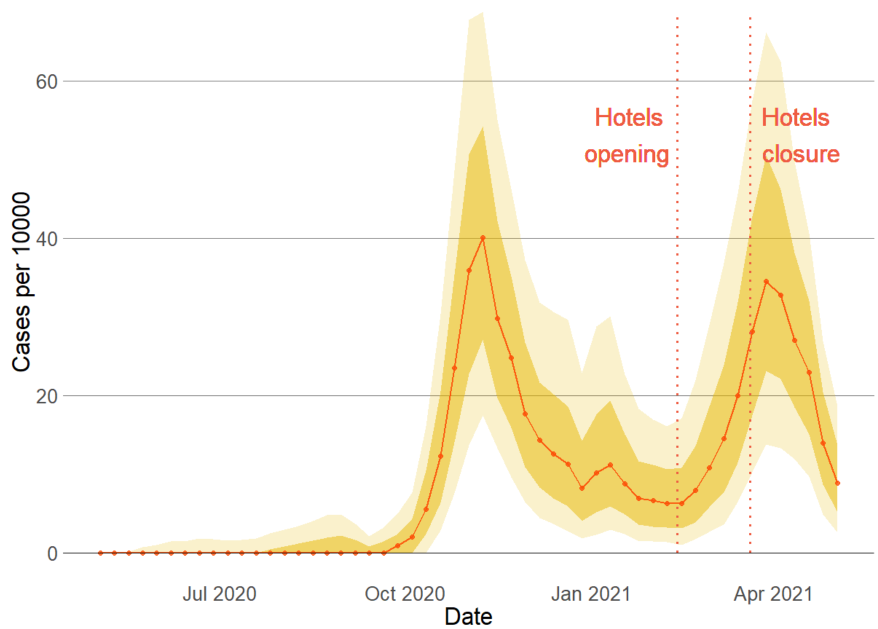
[10] Information obtained from the Polish Ministry of Sport and Tourism through the FOIA request

located. Data showed that the program was particularly beneficial for ski resorts. During the study period of January to April 2021, 26% of all payments made with vouchers (approximately \$5.5 million) were directed to businesses located in 89 communes with ski facilities.

### 2.3 Health Outcomes

The Polish Ministry of Health provided the data on health outcomes. It contained weekly observations at the commune level for the number of COVID-19 cases, deaths, tests taken, and people vaccinated with two doses. Our data cover May 2020–April 2021 for cases and tests, while the remaining variables concern January 2021–April 2021. During this period, Poland experienced its second significant wave of infections. Figure 3 illustrates the evolution of the pandemic in Poland by showing the median, $10^{th}$, $25^{th}$, $75^{th}$ and $90^{th}$ quantiles of weekly cases per 10,000 inhabitants across the communes. As one can see, the number of new infections varied considerably in the temporal and cross-sectional dimensions. Interestingly, the second wave's timing coincided with the reopening of hotels.

Figure 3: Quantiles of weekly cases per 10 000 inhabitants in Poland




Note: The lighter shaded area corresponds to the $10^{th}$ and $90^{th}$ quantiles. The darker area corresponds to the $25^{th}$ and $75^{th}$ quantiles. The red line and points represent the median. Source: Own elaboration based on the data from the Ministry of Health

## 3 Mobility and Spending Outcomes

This section contends that reopening hotels raised tourist spending in ski resorts and significantly increased mobility, especially at long distances. A sharp influx of travelers to tourist areas raised the frequency of meetings between inhabitants of tourist and non-tourist counties.

## 3.1 Empirical Framework: the Impact of the Policy on Tourist Spending

We first investigated whether people responded to the policy by increasing their consumption of tourist services in areas with ski resorts. We conducted an event study comparing spending from the Touristic Voucher program in tourist vs. non-tourist areas. In particular, we grouped all communes by the number of hotel beds per 100 inhabitants and the presence of ski facilities[11]. This strategy resulted in three categories aiming to approximate tourist appeal: communes with fewer than four hotel beds per 100 (non-tourist), communes with more than four hotel beds but no ski resort (tourist), and communes with more than four hotel beds and a ski resort (very tourist). We chose four beds as it roughly corresponds to the 90th percentile of the distribution of hotel beds per 100 inhabitants. Hotels and ski facilities could not operate before February 12th, so the spending trends should be parallel across categories. Then we estimated the following regression:

$$Spending_{kw} = \sum_{c \in C} \sum_{\substack{W \in \{\{01/03:01/31\},\\ \{02/14:05/02\}\}}} Tourism_c^k I(w = W)\beta_c^W + \lambda_k + \gamma_w + \epsilon_{kw} \tag{1}$$

The event study in equation 1 analyzed the change in payments from the "Touristic Voucher" program in communes of category $c \in C$[12] in week $w$ compared to an analogous change in communes with fewer than four hotel beds per 100. The baseline period was the week ending on February 7th, which was excluded from time dummies. The outcome variable was the vouchers' total value (USD) spent in businesses located in commune $k$ in week $w$. The dummy $Tourism_c^k$ took value 1 if the commune $k$ belonged to the category $c$. The parameter of interest was $\beta_c^W$ which measured the impact of the policy on spending in areas of type $c$ in week $W$ compared to non-tourist areas. We expected the coefficients to be 0 before the policy and higher in areas with ski resorts after the policy as these were more appealing during winter. We allowed for the commune $\lambda_l$ and week $\gamma_w$ fixed effects. We clustered the standard errors at the commune level.

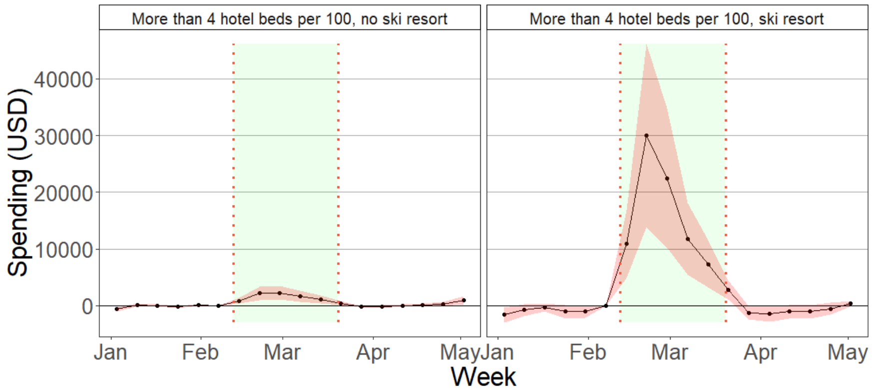
## 3.2 Hotels Reopening Led to Higher Consumption of Tourist Services at Ski Resorts

The reopening of the tourism industry increased tourist spending, especially in the ski resort areas. Figure 4 displays $\beta_c^W$ coefficients. Note $\beta_c^W$ was 0 before the policy date (dashed line), confirming that trends were parallel before the reopening. However, after February 12th, we found a dramatic rise in the payments received in communes with ski facilities (right panel). The increase was smaller in tourist communes without ski resorts (right panel). The effect of the policy at ski resorts peaked in the second week and reverted to 0 as hotels closed again at the end of March. Overall, the interaction of subsidies and the reopening of hotels increased touristic consumption at ski resorts.

[11] Data on hotel beds come from the Polish Statistical Agency for the year 2019. Data for ski facilities came from scraping the database of the website narty.pl. See figure 18 in the appendix for the spatial distribution of ski resorts

[12] Where $C$ contains the two non-excluded categories $C = \{$*communes with more than four hotel beds but no ski resort, communes with more than 4 hotel beds and a ski resort*$\}$

Figure 4: Event study: Hotels and ski resorts reopening and touristic spending




Note: Lines and points correspond to the estimates of $\beta_c^W$ from equation 1. The excluded category is *Communes with fewer than 4 hotel beds per 100 inhabitants*, and the excluded date is the 7th of February.The panel on the left represents the estimates for tourist communes without ski resorts, and the panel on the right shows estimates for the tourist communes with ski resorts. The red shaded area plots 95% confidence bands, which allows for clustering at the commune level. The green rectangle represents when the hotels and ski resorts were open. Source: Own elaboration based on administrative data.

Some caution, however, is required in interpreting this result. Firstly, estimates present the lower bound on total spending as they account only for the payments with the vouchers. While we cannot track the expenditures from other sources, the aggregate national spending on travel in the first quarter of 2021 was 50 times higher than the amount from the vouchers only[13]. Therefore, the reopening likely incentivized spending among people not using the subsidy. Nonetheless, unsubsidized groups may have experienced a different effect as they differed from the voucher holders in terms of age, income, and frequency of skiing, and their travels are not subsidized.

Second, it is worth considering if the results are robust to weather variations related to climate change. For example, warming temperatures may decrease snow coverage and make skiing less appealing. Nevertheless, ski resorts are popular tourist destinations throughout the year, and larger ski lifts serve hikers in the warmer months. Thus, we believe that climate variation would not fundamentally alter our results.

Third, our findings do not allow us to assess what would be the impact of tourism on spending in the pre-COVID-19 period. On the one hand, domestic expenditures could be lower due to the lack of travel subsidies and the ease of border crossing before the pandemic. On the other hand, they could be higher as potential tourists did not experience adverse income shocks or face a risk of infection. Therefore, a pre-COVID effect could vary in either direction.

While the interaction of vouchers and hotels reopening makes it difficult to generalize the effect on spending, it also provides a unique setting to investigate the impact of tourism on infections. As both policies are complementary, they likely produced a substantial spike in tourist movement, which we can leverage to investigate its effect on public health.

## 3.3 Empirical Framework: The Impact of the Policy on Population Movements

Along with the increase in tourism spending, we expected a significant increase in tourist movement after the policy. An upsurge in national railway and passenger car traffic provided suggestive evidence for such movements (see figure 13b in the appendix). To investigate this formally, we used differential tourist accommodation capacity and proximity to ski resorts to conduct an event study evaluating whether the reopening

[13] The aggregate spending from vouchers in Q1 2021 was $17,359,888, while aggregate national spending on travels in the same period was $877,385,506 according to the Ministry of Tourism

of hotels increased the inflow to tourist locations. In particular, we analyzed whether there was a more significant increase of users on tiles with many hotels after the policy's implementation than on tiles with no hotels. Furthermore, we stratified the analysis by whether the tiles were close to ski facilities. We hypothesized that the policy induced a large influx of tourists to ski resorts, noting that places with many hotels attracted more tourists due to their greater capacity. Moreover, stratifying the analysis by the proximity to ski resorts helped to alleviate the concern that the number of hotels proxies high urbanization as ski resorts are usually located within small towns.

To implement this strategy, we located all accommodations and ski facilities and assigned them to tiles. We then calculated the number of hotels in each tile. We grouped tiles into five categories: 0 hotels, 1 hotel, 2 to 9 hotels, 10 to 19 hotels, and 20 or more hotels. Next, we defined tiles as *in proximity to a ski resort* if they were within 25km of the closest ski lift in the mountains. Then we estimated the following regression separately for the tiles in proximity and not in proximity to ski resorts:

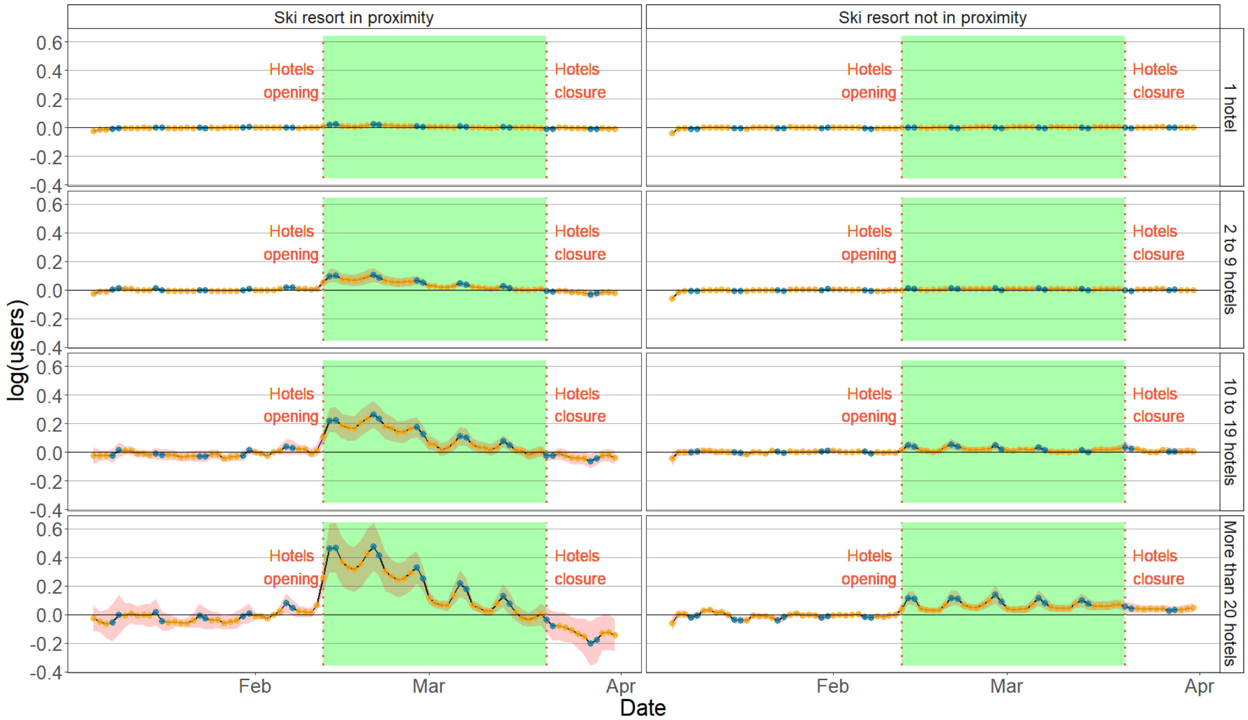
$$log(population)_{jtp} = \sum_{\substack{h \in \{\{1\},\{2:9\} \\ \{10:19\},\{20+\}\}}} \sum_{\substack{T \in \{\{01/06:02/03\}, \\ \{02/05:03/31\}\}}} Hotels_h^j I(t=T)\beta_h^T + \lambda_{jp} + \sum_{dw \in DW} \alpha_{i(j)}^{dw} + \gamma_{tp} + \epsilon_{jtp} \tag{2}$$

The event study in equation 2 analyzes the change in the population on tiles with $h$ hotels at date $t$ compared to an analogous change in tiles with 0 hotels. The baseline period was February 4th, which was excluded from time dummies. The outcome variable was the natural logarithm of the population at tile $j$, date $t$, and time window $p$. The dummy $Hotels_h^j$ takes value 1 if the tile $j$ has a number of hotels in the bin $h$. The parameter of interest was $\beta_h^T$, and we expected it to be 0 before the policy ( February 12th), and positive after the policy. Moreover, $\beta_h^T$ should increase with the number of hotels. The increase should be considerably larger in proximity to ski resorts if the movements were tourism-related. We allowed for the tile×time-window fixed effects $\lambda_{jp}$, weekday×county fixed effects $\alpha_{i(j)}^{dw}$, and date×time-window fixed effects $\gamma_{tp}$. We clustered the standard errors at the county level.

### 3.4 Hotels Reopening Led to an Increase in Tourist Area Mobility

The reopening of hotels increased the population present at ski resorts considerably. Figure 5 displays $\beta_h^T$ coefficients. We see that $\beta_h^T$ was 0 before the policy date (dashed line), confirming that trends were parallel before the reopening in all types of tiles. However, after February 12th, we see high growth in the number of users in tiles with hotels, especially in the proximity to ski resorts (left panel). The growth is also higher for tiles with more hotels. For tiles with more than 20 hotels and in proximity to ski resorts, we see about a 50% increase in the population during weekends and a 30% increase during weekdays. The effects subside with time as Poland entered its second wave of the pandemic. While there is a significant increase in population at tiles with many hotels and not in proximity to ski resorts (right bottom panel), its magnitude is small. We conclude that there was a large influx of tourists to ski resorts after the opening.

Figure 5: Event study: Hotels and ski resorts reopening and Facebook users




Note: Lines and points correspond to the estimates of $\beta_h^T$ from equation 2. The excluded category is *tiles with 0 hotels*, and the excluded date is the February 4th. The panel on the left represents the estimates for tiles in the proximity to ski resorts, the panel on the right shows estimates for the remainder of the tiles. Estimates of $\beta_h^T$ for each bin $h$ are plotted separately, starting with the lowest bin $h$ at the top and the highest bin $h$ at the bottom. Blue points correspond to weekends and yellow to weekdays. Red shaded area plots 95% confidence bands, which allows for clustering at the county level. The green rectangle represents the time when the hotels and ski resorts were open. Source: Own elaboration based on Facebook data

## 3.5 Empirical Framework: The Impact of the Policy on the Frequency of Meetings

In this section, we discuss whether the policy affected the frequency of meetings between users from different counties. Such meetings are essential from an epidemiological perspective because they can transform local outbreaks into a national wave. Our hypothesis was that the reopening policy made population flows at long distances and flows from non-tourist to tourist counties more likely. To test this, we performed two analyses. The first analysis investigated whether the frequency of long-distance meetings increased relative to short-distance meetings after the policy was enacted. We classified each link into five distance bins based on the distance between centroids of the counties: $d \in D = 0-100km, 100-200km, 200-300km, 300-400km, 400+km$. Next, we regressed the log of colocation probabilities on the interaction of the week dummies with the distance bins:

$$\begin{aligned} log(P(colocation))_{klw} = & \sum_{\substack{W \in \{\{01/05:02/02\}, \\ \{02/16:04/13\}\}}} \sum_{d \in \underline{D}} Distance_{kl}^{d} I(w=W)\beta_d^W \\ & + \phi_{kl} + \chi_w + v_{klw} \end{aligned} \tag{3}$$

Where $log(P(colocation))_{klw}$ is the log of the probability of colocation between users from county k and l in the week $w$. A dummy $Distance^d_{kl}$ was equal to 1 if the distance between counties $l$ and $k$ was in the bin $d$.

The bin with the shortest distance was excluded as a reference. The dummy $I(w = W)$ was equal to one if the week of the observation corresponded to the week $W$. The excluded week was the last week before the reopening, that is, the week of February 9th. We allowed for link $\phi_{kl}$ and week fixed effects $\chi_w$, and we clustered the standard errors at the link level. The parameter of interest was $\beta^W_d$ which was a difference-in-differences estimator: the first difference measured the percentage change in the colocation probabilities for counties at a distance $d$ in the week $w$ compared to the week of 02/09. The second difference took this change and compared it to an analogous change for counties at a distance of 0-100km. We expected $\beta^W_d$ to be positive for weeks when the policy was in place as people started to travel long distances.

The second analysis tested the hypothesis that meetings between tourist and non-tourist counties increased after the policy was implemented. We relied on the assumption that counties with extensive skiing and accommodation capacities would have greater tourist appeal (see the derivations of the theoretical impact of policy on colocation in the appendix). Therefore, we defined the exposure to tourists by using the total length of skiing trails and the number of hotel beds in the county[14]. In particular, we first classified counties as below ($few\ hotels$) or above ($many\ hotels$), the third quartile of the distribution of hotel beds. Second, we classified counties with 0 skiing trails as 0 $trials$. Finally, for counties with some skiing trails, we grouped them based on whether they were below ($few\ trails$) or above ($many\ trails$) the third quartile of the distribution of the total length of skiing trails[15]. In this way, we obtained five possible exposure statuses $es \in ES$=\{*0 trails & few hotels beds, 0 trails & many hotel beds, Few trails & few hotel beds Few trails & many hotel beds, Many trails & Many hotel beds*\} [16]. See figure 19 for the spatial distribution of the exposures.

We expected the probability of colocation between inhabitants of tourist and non-tourist regions to have increased after the policy's implementation. The most substantial effect should exist for pair of counties with and without ski resorts. To measure such an effect, we conducted our analysis on the link level (links between counties) by running the following regression:

$$\begin{aligned} log(P(collocation))_{klw} = & \sum_{\substack{W \in \{\{01/05:02/02\},\\ \{02/16:04/13\}\}}} \sum_{s \in \underline{ES}} \sum_{q \in \underline{ES}} Exposure^s_k \times Exposure^q_l I(w = W)\beta^W_{sq} \\ & + \delta_{kl} + \gamma_w + \epsilon_{klw} \end{aligned} \tag{4}$$

A dummy $Exposure^s_k$ was equal to 1 if the county $k$ belonged to the exposure category $s$ and 0 otherwise. The excluded combination of classes was the one between counties which both belong to *0 trails & few hotels beds*. Analogously to equation 3, we excluded the week of Febryary 9th, and we allowed for link $\delta_{kl}$ and week fixed effects $\gamma_w$, and we clustered the standard errors at the link level. The parameter of interest $\beta^W_{qs}$ estimated the percentage change in the colocation probabilities between users from counties of types $s$ and $q$ in the week $w$ compared to the week of February 9th and relative to an analogous change for users from two different counties both belonging to the type *0 trails & few hotels beds*. Note that the links were undirected, hence we used $\beta^W_{qs}$ independently of which county was in $q$ and which was in $s$. We expected $\beta^W_{qs}$ to be positive after policy implementation for pairs $s$ and $q$ such that one had ski trails and hotels while the other did not. Moreover, the effect should have grown in the difference between the counties' tourist appeal and thus should have seen the most prominent effect for pair *0 trails & few hotels beds* and *Many trails & Many hotel beds*.

[14] Data from the Polish Statistical Agency for the year 2019

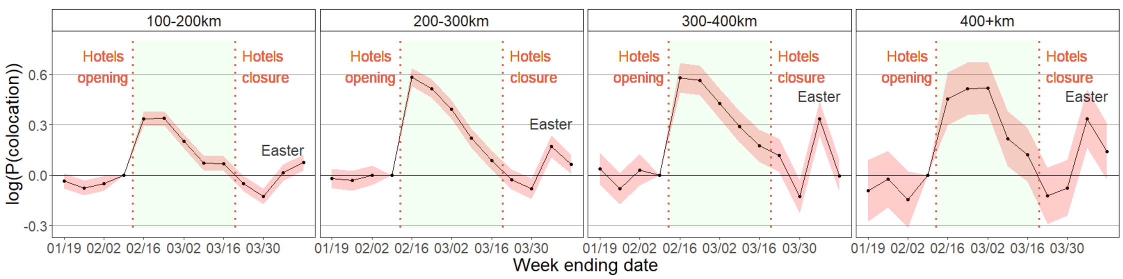
[15] Among counties with any skiing trails

[16] All counties with long skiing trails have many hotels beds, hence exposure *Few hotel beds & Many trails* is missing

### 3.6 Policy Increased Likelihood of Long-Distance Meetings and Those Between Locals and Tourists

The frequency of long-distance meetings and meetings between non-tourist and tourist counties increased after the reopening. Figure 6 shows the parameter of interest from the equation 3 and displays a well-defined increase in colocations at long distances compared to counties within 100km after the policy was enacted. Moreover, the increase was greater for distances above 200km, consistent with tourists going farther as they can stay the night in a hotel. The parameter of interest decreased after the initial surge, which may be related to the rising number of COVID cases in late March. Furthermore, figure 6 reveals a spike in the first week of April, which likely corresponds to Easter festivities.

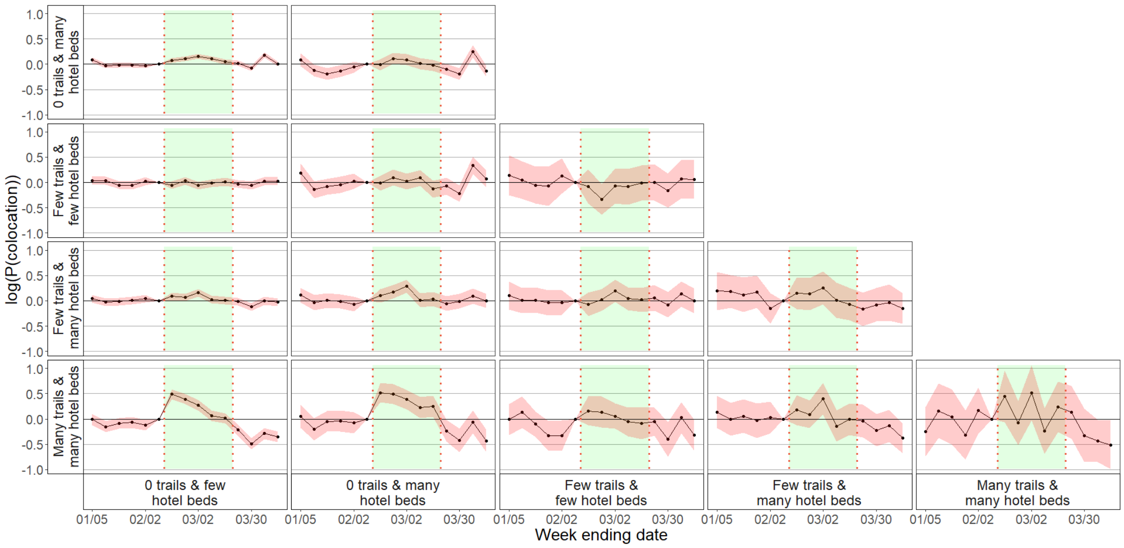
Figure 6: Event study: Hotels and ski resorts reopening and long-distance colocation




Note: Lines and points correspond to the estimates of $\beta_d^W$ from equation 3. The excluded category is *Distance < 100km* and the excluded week is February 9th. Each panel represents estimates of $\beta_d^W$ for a different distance bin $b$ starting with the lowest distance on the left. The red shaded area plots 95% confidence bands, which allowed for clustering at the link level. The green rectangle represents the time when the hotels and ski resorts were open. Additionally, an annotation was added to mark the week of Easter. Source: Own elaboration based on Facebook data

Regarding equation 4, figure 7 shows increase colocation between tourists and locals. Each panel in figure 7 corresponds to estimates of parameters $\beta_{qs}^W$ for different $s$ and $q$. The row label represents category $s$, and the column label represents category $q$. For example, the top-left panel represents the change in the colocation probabilities between counties with *0 trails & few hotels beds* and with *0 trails & many hotels beds*. As expected, we did not see significant changes in the frequency of meetings between pairs of counties that were either both tourist or both non-tourist. While the null effect among non-tourist counties was accurately estimated, we obtained noisy estimates for links between counties that both had trails. This was due to a lower number of tourist counties and fewer connections among them. Importantly, we saw a significant increase in the probability of meetings between counties with 0 trails and counties with many trails immediately after the reopening. The magnitude of this increase was approximately 50% for the week following the reopening and stayed positive for the next three weeks.

Figure 7: Event study: Hotels and ski resorts reopening and touristic colocation




Note: Lines and points correspond to the estimates of $\beta^W_{sk}$ from equation 4. The excluded category is one with both counties belonging to *0 trails & few hotels beds* and the excluded week is Februray 9th. Each panel represents estimates of $\beta^W_{sk}$ for a different pair of $s$ and $k$ types. Note that the ordering of types does not matter because the links were symmetric. The types are described in the strips on the left and on the bottom. For example, the bottom left panel represents $\beta^W_{sk}$ where one county belongs to *0 trails & few hotels beds* and the other to *many trails & many hotels beds*. The red shaded area plots 95% confidence bands, which allows for clustering at the link level. The green rectangle represents the time when the hotels and ski resorts were open. Additionally, we add an annotation to mark the week of easter. Source: Own elaboration based on Facebook data

We conclude that the policy increased the frequency of long-distance meetings and that of meetings related to tourism. As such, it could have had a significant impact on COVID-19 outcomes.

# 4 COVID-19 Outcomes

## 4.1 Conceptual Framework

Since the policy encouraged gatherings and travel, it should have impacted the number of infections due to an increase in the number of contacts between individuals. However, the communes should have been affected differently depending on their participation in the tourism industry. We exploit this exposure heterogeneity when designing our identification strategy.

Conceptually, we can distinguish between three levels of the exposure due to the policy. First, communes hosting ski facilities (type 1) were highly exposed. They experienced a large influx of tourists and increased local interactions due to amplified economic activity. We expected new cases arising among locals who come in contact with a higher number of individuals. Second, the policy directly affected communes sending tourists to ski resorts (type 2). Tourists came into contact with locals and other tourists while staying at the resorts; consequently they were at a higher risk of getting infected. Third, some communes did not send tourists to ski resorts after hotels reopened (type 3). They were the least exposed to the policy as their contact patterns have not changed.

While the contact patterns of type 3 communes were not affected, these communes could still experience treatment effect in terms of new infection. Policy-induced cases in communes of type 1 and 2 could have produced secondary infections that spread through the existing networks (Chang et al. [2021],Kuchler et al. [2022],Fritz and Kauermann [2021]). They could flow to communes not sending tourists to ski resorts through the connections pre-existing the policy. Note that these additional secondary cases would not have happened in the absence of the policy; hence they were part of the treatment effect. Although every commune could be potentially affected by the policy, we expect that the timing of the treatment effect differs by type (Shtele et al. [2022], Thomas et al. [2020]). In particular, policy induced infections should first appear in communes hosting ski resorts or directly sending tourists, and only later in communes not sending tourists.

Motivated by this reasoning, our identification strategy leveraged differential exposure to the policy by type of the commune and time after the opening. In particular, it relied on comparing the dynamic of infections in communes directly exposed to the policy (type 1 and type 2) to the analogous dynamic in the communes not sending tourists to ski resorts.

## 4.2 Empirical framework

Translating this conceptual framework to an empirical one posed three main challenges: We needed to identify communes sending tourists, adjust for differences in the pre-policy pandemic outcomes across communes, and find a valid counterfactual in the realm of a national policy.

### 4.2.1 Tourists' Origin Communes

We used colocation data to identify communes sending tourists to ski resorts. In particular, we summed all meetings between individuals from county $i$ and any county containing a ski resort [17] during the first three weeks of the policy. Mathematically, the strength of connections, which we call *exposure* is:

$$exposure_i = \frac{\sum_{w \in \{w*, w*+1, w*+2\}} \sum_{j \in ski\_resort} Meetings_{ijw}}{N_i}$$

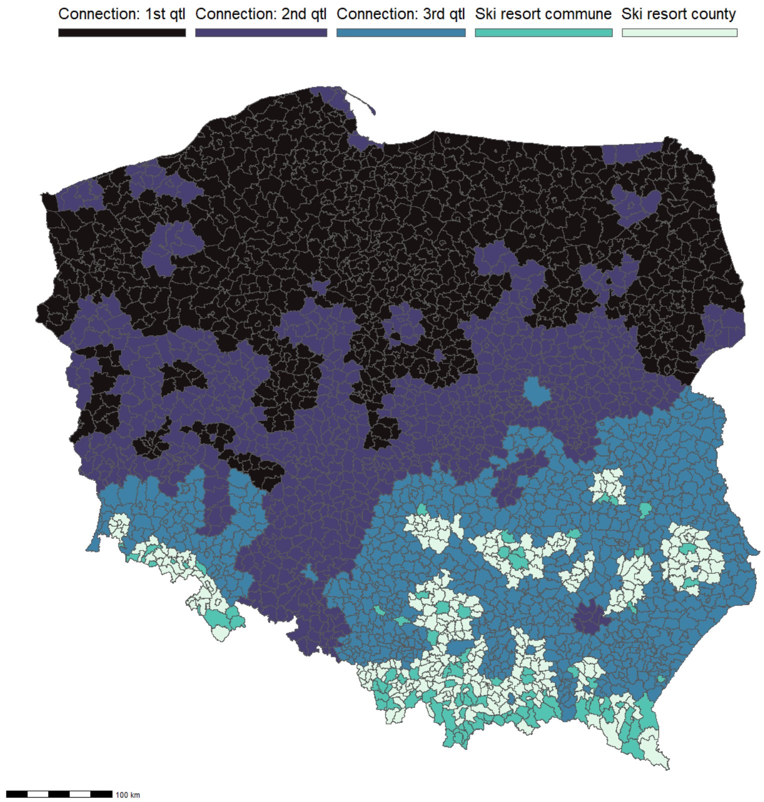
Where $N_i$ is the number of FB users in county $i$ and $ski\_resort$ is the set of counties with ski resorts. Note that the connection measure is at a lower geographic granularity (county) than the outcome (commune). Thus, we assume that all communes in a county had the same *exposure*. We divide communes into five categories which corresponded to types. Communes with ski resorts are classified as *Ski resort commune*. Communes without ski facilities but in the counties with ski resorts are classified as *Ski resort county*[18]. The rest of the communes are classified according to the tertiles of their county's *exposure* (similarly to Dave et al. [2021b]). We assume that the counties in the first tertile did not send any tourists because they had fewest interactions with inhabitants of ski resorts during the policy. We further assumed that counties in the third tertile sent more tourists than counties in the second tertile. The map on figure 8 shows the spatial distribution of the treatments [19].

[17] We excluded ski facilities not located in the mountains

[18] This distinction is necessary because we do not know connections within the county

[19] Note that the connections are at the county levels, but the outcomes are at the commune level. Hence, communes in the same counties will have the same exposure

Figure 8: Exposure to ski resorts




Note: Each color corresponds to a different exposure category. Communes within the same county belong to the same exposure unless the county contains a ski resort. Communes containing ski facilities are in the category *Ski resort commune* and communes without ski facilities but in counties containing ski resort communes are in the category *Ski resort county*. Source: Own elaboration

For the preliminary empirical evidence on the impact of the policy, we compare new cases in communes with different treatments. Figure 9a plots the average number of new cases per 10 000 inhabitants in communes by their exposure to the policy. First, one notes that the pre-policy infection trends differ between the treatment arms. Second, we do not see a significant difference in the number of new infections right after the enacted policy (first dotted line). However, such a simple comparison may be insufficient to uncover the impact of the policy. The diffusion is affected not only by the number of contacts but also by the number of previous infections and susceptible individuals. Different treatments seem indeed to be at a different stage of the pandemic before the policy, as evidenced in figure 9a. This by itself is enough to render a simple comparison unreliable.

#### 4.2.2 Balancing Communes by Pre-Policy Outcomes

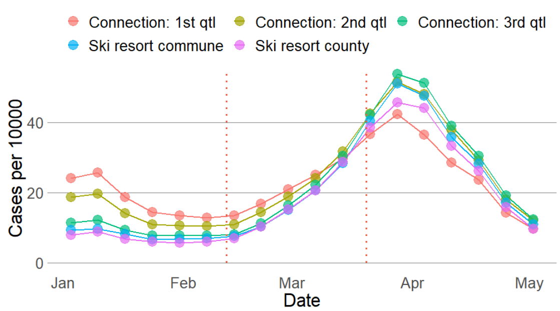
To analyze the policy, we need to compare locations at a similar pandemic stage before the policy. As SIRD has Markovian properties, the current pandemic situation should depend only on the indices in the previous period. The variables representing number of infections, share of those susceptible, and number of infections among connected units in the current period act as sufficient statistics for the outcome in the next period. Consequently, communes with similar pre-pandemic outcomes and characteristics should evolve in the same way in the absence of the policy. Therefore, our analysis conditions on a set of pre-policy variables $F_{k,w*-1} = \{X_{k,w*-1}, Z_k\}$. In particular, $X_{k,w*-1}$ represents pandemic outcomes in the last period before the policy. It includes the number of cases, deaths, and tests per 10 000, their growth rates, and their cumulative numbers since the beginning of the pandemic to proxy for share susceptible[20]. It also contains the weighted sum of cases among neighbors where the weights correspond to the number of commuters per capita. Moreover, $X_{k,w*-1}$ includes the squares and interactions of all these variables. The controls $Z_k$ represent observed location-specific characteristics that may influence the diffusion. In particular, $Z_k$ includes population, population density, the type of the commune (urban or rural), unemployment rate at the end of 2020, the number of sport objects per capita (swimming pools, stadia, courts), and the number of theatres and cinemas per capita. The remaining analysis relies on the assumption that in the absence of the policy and conditional on these variables, the potential outcomes would evolve in the same way across all the types:

**Assumption 1** *-Ignorability* : $Y(0)_{k,w\geq w*} \perp\!\!\!\perp Treatment_k^{tp} | X_{k,w*-1}, Z_k$
*for* $tp \in \{ski\ resort, sending\ tourists, not\ sending\ tourists\}$

Figure 9b plots the average number of new cases by type in the sample matched on the conditioning variables. We noticed a considerable improvement in the similarity of the pre-policy outcomes, suggesting that the matching was successful. However, the outcomes diverge in the weeks after the opening. In particular, the number of cases was higher in the first weeks after the reopening in locations with ski resorts and communes strongly connected to ski resorts.

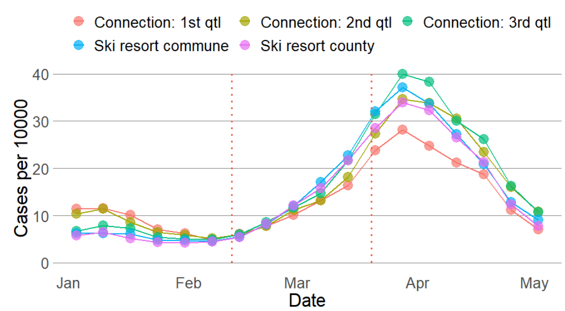
Figure 9: Covid cases in ski resorts

(a) Covid cases: full sample




Note: The averages were calculated on the full sample. Dotted lines represent the reopening and closure of hotels. The date corresponds to the last day of the week. Source: Own elaboration based on the Ministry of Health Data

(b) Covid cases: matched sample




Note: The averages were calculated on the matched sample. Each treated unit was matched with one unit from the first tertile of connection. Units were matched by the distance in the propensity scores computed based on the conditioning variables. Dotted lines represent the reopening and closure of hotels. The date correspond to the last day of the week. Source: Own elaboration based on the Ministry of Health Data

#### 4.2.3 Identifying control communes

The policy was nationwide; however, the exposure to the policy differed between the communes. We argue that the least exposed communes can act as controls to identify the *on impact* effect of the policy. Since the level of interactions did not change in communes not sending tourists, their infection dynamic was not affected at the onset of hotels reopening. Consequently, they could serve as controls in the initial period of the new policy. Thus, we could identify the *on impact* treatment effect in communes with ski resorts and analogously in communes sending tourists. The identification was more challenging in future periods. As time passed, infections induced by the policy flowed through the existing network of connections. Due to these spillovers, the treatment effect was no longer null in communes not sending tourists in later periods. In particular,

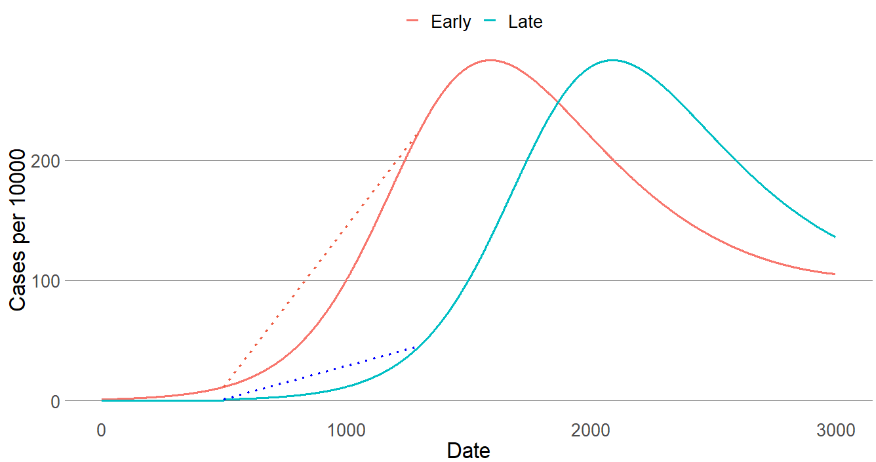
[20]Since the beginning of 2021 in case of deaths, as the number of deaths is only available for 2021

they were affected by higher levels of infections in communes to which they were connected. Hence, the comparison was biased in later periods, and the bias was more significant if control communes were well connected and experienced large spillovers. However, the treatment effect in communes not sending tourists was presumably non-negative as the policy should have increased the number of new cases. Therefore, we could identify lower bounds on the treatment effect in the remaining communes.

#### 4.2.4 Estimation

To estimate the effects, we turned to the unconfoundedness approach suggested by Callaway and Li [2021]. It has been shown that fixed effects estimation is unlikely to produce reliable results if outcomes are generated by a non-linear model (Gauthier [2021], Callaway and Li [2021]), and the infections are likely a product of such a non-linear model[21] (Keeling and Eames [2005], Brauer [2017], Caccavo [2020]). This issue occurs because the treated units could be at a different pandemic stage than the control units. For example, suppose that the treated units experienced their first case earlier than the control units. Then, their outcomes would not evolve parallel to the outcomes in the control areas, even without the policy. Figure 10 illustrates this problem. It plots the results of a simple simulation of the Susceptible-Infected-Recovered-Deceased model in two identical areas except for the timing of their first case. The area represented with the red curve started the pandemic earlier. Dotted lines show linear trends in the number of cases between two points in time. Despite identical parameters, the trends are not parallel because these areas are at different pandemic stages. Therefore, differencing trends would introduce a bias rather than remove it.

Figure 10: Non linearities implied by SIRD model




Note: figure plots the results of a simulation of SIRD model in two areas. The parameters are identical in both cases, however the timing of the first case is different. The area represented by red curve had the first case earlier than the area represented by the blue curve. Dotted lines measure linear trends in cases between two points in time. Despite identical parameters, areas experience non-parallel trends

The unconfoundedness approach alleviated the above issue in two ways. First, it did not rely on fixed effects. Second, it ensured that the control units were at a similar pandemic stage before the policy. This is achieved by conditioning on the pre-treatment covariates related to the pandemic. Thus, this approach was compatible with a case in which pandemic-related parameters varied over time and with commune-specific characteristics. It was, however, not compatible with a general unobserved heterogeneity in parameters

[21] We perform traditional event studies on the full (figure 17a) and matched samples (figure 17b), see the results in the appendix

by location. Hence, the estimation presented below was valid under the assumption that the parameters changed across locations only due to the variation in the conditioning controls $F_{k,w*-1}$.

The unconfoundedness approach computed a weighted difference of outcomes in treated communes versus control communes with similar pre-treatment characteristics to the treated units. The larger the similarity, the higher the weight for the control unit. In particular, denote the pre-treatment characteristics as $F_{k,w*-1} = \{X_{k,w*-1}, Z_k\}$ and $C_{w,k}$ as the number of new cases in week $w$ and commune $k$. Following the notation from Callaway and Li [2021], we estimate:

$$ATT_w^c = E[w(treatment_k^c, F_{k,w*-1})(C_{w,k} - m_{0,w}^C(F_{k,w*-1}) \tag{5}$$

where $ATT_w^c$ corresponded to the average effect on the treated by treatment $c$, and the weights correspond to:

$$w(treatment_k^c, F_{k,w*-1}) = \frac{treatment_k^c}{E[treatment_k^c]} - \frac{\frac{p(k,F_{w*-1})}{1-p(k,F_{w*-1})}(1 - treatment_k^c)}{\left[\frac{p(F_{k,w*-1})}{1-p(F_{k,w*-1})}(1 - treatment_k^c)\right]}$$

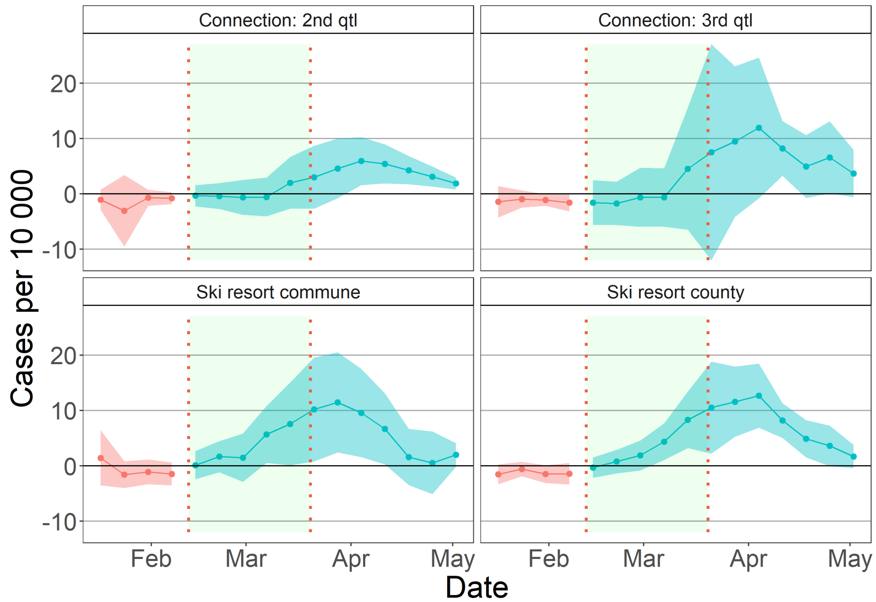
Finally, the untreated potential outcomes correspond to

$$m_{0,w}^C(F_{w*-1}) = E[C_w | F_{k,w*-1}, treatment_k^c = 0]$$

.

The untreated potential outcome of a treated unit $k$ in a week $w$ came from outcomes of untreated units similar to $k$ in week $w$. Their expectation was unbiased for the untreated potential outcome under assumption 1. Concretely, we implemented this method by estimating propensity scores $p(F_{k,w*-1})$ with logit and outcome regression $m_{0,w}^C(F_{k,w*-1})$ with OLS. The method was double robust because it was robust to the misspecification in either propensity scores or counterfactual regression of potential outcomes. The method assumed that the pandemic trajectory would be similar in the treatment and control groups after conditioning on the $F_{k,w*-1}$. We argue for the validity of this assumption in section 4.2.5. Note that we estimate four effects: one for each treatment versus the control group of communes in the first tertile of $exposure$. Thus, we estimated the effect in four samples where each sample contained units from the control and one of four treatments. Figure 11 plots the results of the estimation together with 95% confidence bands calculated with multiplier bootstrap.

Figure 11: Event Study: Covid-19 and tourism opening




Note: The estimates come from the unconfoundedness approach by Callaway and Li [2021]. Red points correspond to the estimates for pre-treatment periods. The reference point in a pre-treatment period $t$ is the previous period $t-1$. Blue points correspond to the estimates for post-treatment periods. The post-treatment periods' propensity scores and outcome regression were based on the last period before the policy $w * -1$. Control units are communes in the first tertile of *exposure*. The shaded area represents simultaneous 95% confidence bands with clustering at the commune level. The date corresponds to the last day of the week.

The results are consistent with the hypothesis that the policy precipitated the wave of infection and that tourists brought the disease back to their home counties. Communes in counties with ski resorts experienced more infections after the reopening. They had about five additional infections per 10,000 in the third week after the reopening compared to the control. The effect was statistically and economically significant as it represented a 70% increase in the pre-policy average in these communes. We interpreted this as the *on impact* treatment effect. It is not apparent why communes without ski facilities but in counties with ski resorts experienced impact equivalent to ski resort communes. There may have been many interactions between the communes with ski resorts and without ski resorts in the same county. For instance, people may have gone skiing or to work in nearby ski resorts. Figure 20 in the appendix shows that commuting to ski resort communes was stronger than across no ski-resort communes, but the magnitude of commutes was not large.

The early increase in cases was absent in communes in the second and the third tertile of *exposure*. However, communes with medium and strong connections saw an increase in infections compared to the control starting in the fourth week after the reopening. This could have been the result of secondary infections from tourists bringing the disease back home and spillovers from other communes. Moreover, we saw a monotonicity of this effect in the strength of connection: communes in the third tertile had a higher increase in cases than communes in the second tertile. While this was consistent with the story of tourists contributing to the diffusion of the virus, these differences were not statistically significant.

The above analysis provides suggestive evidence that the reopening of hotels contributed to the diffusion of COVID-19 through tourist gatherings and travel. In particular, the wave of infections arrived earlier in the counties with ski resorts. Moreover, there were more infections in counties with ski resorts and counties strongly connected to ski resorts than counties weakly connected to ski resorts. Note, however, that this exercise does not allow us to conclude whether the reopening of tourism caused or did not cause the second wave of the pandemic in Poland. While we found differential trends in infections by exposure to tourism, all units would have been affected by the policy in the medium or long term. Therefore, there was no plausible counterfactual which would allow for the evaluation of what would happen in the absence of the policy in the medium or long term.

#### 4.2.5 Robustness

The The unconfoundedness method developed by Callaway and Li [2021] relies on the assumption that applying the propensity score weights and the regression to control units can predict the counterfactual outcomes for treated units. In other words, control-based predictions should replicate the potential trajectory of infections in the treated communes that would have happened in the absence of the policy. While it is impossible to test the counterfactual's performance during the intervention, one can look at periods before the policy's enactment. In particular, conditional on $F_{k,w*-1}$, there should be no difference in the outcomes between treated and control communes before February 12th.

For the first check, we extended the study period until November 2020 to examine the pre-policy differences. We compared the counterfactual prediction[22] and the treated outcomes before the implementation date and we found no considerable differences as shown in figure 22 in the appendix.

As a second check, we performed a placebo exercise, setting the treatment timing to start a month before the actual implementation date. We expected no differences between the "treated" trajectory and the control-based predictions as the actual policy had yet not started [23]. Figure 23 confirms this intuition showing no significant differences between the treated and control communes after the placebo date. While there were some deviations from 0, they were small and opposite to the policy effect. Therefore, we believe the method provided a reasonable counterfactual trajectory for the treated communes.

In addition to reopening of hotels and ski resorts, the government changed some other restrictions during the study period. We took steps to ensure that these additional changes were not driving our results. Firstly, simultaneously to lifting the closure of hotels, Polish authorities allowed theatres, cinemas, swimming pools, and outdoor sports venues to resume their activity. For this reason, the main specification included the per capita number of theatres, cinemas, and sports venues [24] as conditioning variables. Consequently, treatments and control groups should be balanced with respect to the availability of these venues; thus, this should not affect their infection dynamics deferentially. Second, the Polish government introduced stricter measures in four regions [25]: on February 27th in Warminsko-Mazurskie, and then on March 15th in *Pomorskie*, *Mazowsze*, and *Lubuskie*. The measures included closing hotels, theatres, sports venues, and malls, and instituting remote learning for primary schools. These measures covered the entire country starting on March 20th. Note that none of these regions contained ski resorts. As a robustness check, we repeated our analysis but excluded these four regions from the sample. The results (presented in the appendix in the figure 21) were qualitatively unchanged, albeit less precise. Finally, some nationwide changes were implemented, such as mandatory quarantine for certain international travelers and the use of mandatory medical grade facemasks (as opposed to bandanas). As these measures were national, we did not expect they would affect our treatment and control groups differently.

## 5 Cost-Benefit Analysis

While reopening the hotels and ski lifts revived the tourism industry, it also produced public health costs. We evaluate the policy by quantifying its costs and contend that they were larger than the benefits.

We assumed that the primary costs stemmed from increased usage of healthcare resources and deaths as in equation 6:

[22] Note that the pre-treatment counterfactual predictions are based on the preceding period, while post-treatment predictions are based on the last period before the policy, which results in a longer prediction horizon

[23] The main difference with the previous check is a longer prediction horizon for the counterfactual outcomes trajectory

[24] As reported by the Central Statistical Office.

[25] There are in total 16 regions in Poland.

$$\text{Costs} = \text{Increase in Healthcare Costs} + VSL * \text{Number of Additional Lives Lost} \tag{6}$$

where VSL stands for the Value of Statistical Life that translates death into financial terms.

Consider first the hospital costs. Accounting for the probability of hospitalization from COVID-19, its length, and fees in Poland (based on Orlewska et al. [2021]), each case had an expected healthcare expenditure of $275. Next, consider the cost of Covid related deaths. Computing the cost of lives lost requires identifying fatality likelihood and assigning a value to each life. The fatality rate of COVID-19 in the period of interest in Poland was 2.7% [26]. We used the information provided in Robinson et al. [2021] to monetize the value of life. In particular, they combined the constant value per statistical life-year (VSLY) and age distribution among COVID-19 deaths to calculate the cost of years lost due to a COVID infection. They assigned $4.47 million per life lost to COVID, which results in an expected death cost of $120,690 per infection. We chose this measure for two reasons. First, COVID-19 deaths were concentrated among the elderly, and the VSLY measure accounted for their older age. Second, this technique gave the lowest estimate among other methods proposed by Robinson et al. (2021), and therefore helped construct a lower bound on costs. Summing up the healthcare expenditures and the value of life lost, each case was associated with an expected cost of $120,965.

The policy caused 134,886 additional infections throughout the country. We obtained this number by summing the estimated treatment effect across populations of all affected areas in the periods after the implementation. Given the 2.7% fatality rate, we calculated 3,642 deaths were due to the reopening. The policy's total cost was the product of infections times their expected cost, which was $16.316 billion.

We assumed that benefit of the policy was its contribution to the GDP through an increase in tourism-related activity.

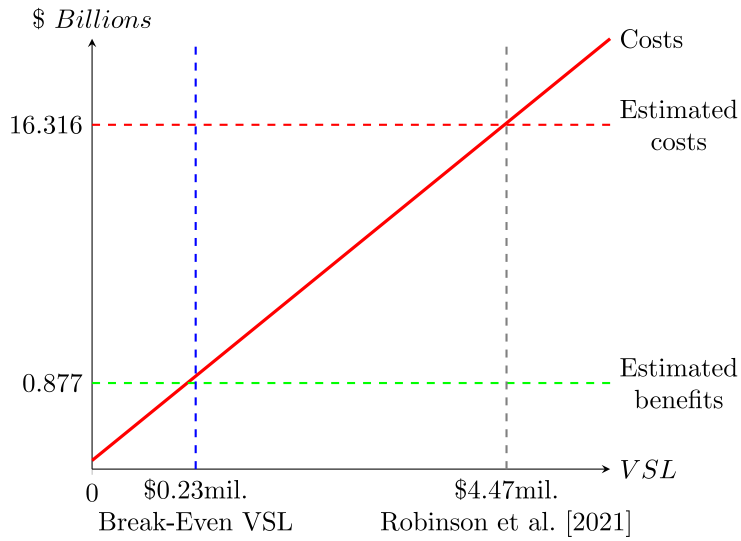
$$\text{Benefits} = \text{Increase in the Consumption of Tourism Related Services} \tag{7}$$

Estimating the benefits is more challenging because we only have granular data on spending from the vouchers. While vouchers are just a government transfer, they might had a multiplier effect: people may dedicate additional spending on items not covered by the vouchers. Furthermore, people not qualifying for vouchers could also increase their touristic consumption. Nevertheless, we could identify the upper bound of the policy's contribution to the GDP. According to the Polish Ministry of Sport and Tourism, tourist expenditures in the first quarter of 2021 totaled $0.877 billion. This is an upper bound because likely only part of this amount was due to the policy.

Costs and benefits are schematically demonstrated on the figure 12. The X-asis represents VSL. We present costs as a function of the VSL (red line representing equation 6), so the reader can evaluate the policy for a range of plausible VSLs. The grey dashed vertical line represents the VSL estimate from Robinson et al. [2021] and the black dashed horizontal line shows the corresponding lower bound of the cost ($16.316 billion). It is considerably above the green dashed horizontal line representing the estimated upper bound on the benefits ($0.877 billions). Even if all this spending stemmed from the policy, it would still be just over one-twentieth of the lower bound of the cost. Thus, we determined that the policy's cost vastly surpassed its benefits. Only at the VSL equal to $0.23 mil. would the policy stop being harmful. Nonetheless, such low estimate is below any value of life considered in the literature.

[26] Calculated as the ratio of deaths to cases lagged by two weeks

Figure 12: Cost-Benefit Analysis as a Function of VSL




Note: The X-axis represents the Value of Statistical Life (VSL). The red line corresponds to the cost of the policy as a function of the VSL. As VSL increases, the cost of the policy goes up. The grey vertical line points to the VSL from from Robinson et al. [2021] and the black horizontal line indicates the corresponding cost. The green horizontal line shows the upper bound on the benefit of the policy. Finally, the blue line shows the maximum value of VSL that does not make the policy harmful (break-even VSL).

The policy benefited only tourists coming to the ski resorts. By the revealed preferences, we conclude that they have enjoyed a positive surplus because they chose to go skiing despite the risk of infections. On the other hand, people outside ski resorts who are not tourists were worse off because they experienced negative externalities of the policy without any benefits. A priori, the results for the ski resorts' inhabitants are ambiguous. From our results it is clear that they did not benefit from the policy. It caused 13,448 new cases in communes with ski facilities which led to a cost of $1.626 billion. This number is still higher than the upper bound on the benefits. The costs would surpass policy's benefits as long as the number of produced infections exceeded 7250, or alternatively as long as there were more than 197 deaths (keeping the VSL constant). Note that a wider access to vaccinations and treatments could potentially make the policy beneficial. However, at the time of the opening only 1.4% of Poles were fully vaccinated.

Tourism during the pandemic a was risky behavior. It exposed tourists to the virus and subsequently contributed to a broader diffusion of cases among the general population. This negative externality produced costs that vastly outsized the benefits of hotels reopening. Even the ski resorts were worse off, despite the policy's intent. The only beneficiaries were tourists, particularly those with children, who could enjoy subsidized travel.

# 6 Conclusion

Tourism plays a vital role in providing income for many local economies. However, while vital for economic reasons, it also encourages long-distance travel and gatherings. Moreover, tourist services often require risky in-person interactions. These features make tourism a transmission vector for various infectious diseases.

We hope that our analysis will provide some guidance for policymakers struggling to balance the trade-off between economic and public health goals related to reopening the tourism industry.

In this paper, we investigated how the reopening of hotels and ski facilities impacted tourist consumption, mobility, and COVID-19 outcomes. These reopenings were followed by large movements of tourists to locations with ski facilities. Areas with many hotels near the ski trails experienced an exceptionally high influx of visitors and spending. Travel often originated from distant locations, so the probability of meetings between individuals residing far from each other increased after the policy. Additionally, there had been an increase in meetings between pairs of individuals such that one person lived in a tourist location and one in a non-tourist location. These observations point out the strong impact of the reopening on mobility.

Travelers have a high potential to carry disease between distant locations. This potential is dangerously elevated when they also participate in gatherings. Visitors of ski resorts could only gather in their hotel rooms and on trails as the restaurants were closed. Nonetheless, we presented suggestive evidence that they impacted the COVID-19 trajectory. We showed that counties with ski facilities were correlated with an increase in infections after the policy. Moreover, counties with frequent meetings with ski resorts during the reopening had more infections than counties with few such meetings.

We believe our results can be extrapolated to other settings involving mass tourism. We think that travel and gatherings are the main factors driving additional infections related to tourism reopening. As long as these two elements are present, one may expect an increase in the number of cases, although of different magnitude depending on the circumstances. While the effects may be more substantial during winter because people spend more time indoors, there is still a considerable amount of close interactions in tourist activities during other seasons. For instance, travelers still share public means of transportation, and locals engage in repeated interactions with tourists when providing them services; thus, additional opportunities for transmission still arise.

Our study shows that engaging in tourist activity can generate negative externalities as it contributes to spreading infections. Hence, it might be reasonable to impose additional costs, such as post-travel quarantine, for people involved in tourism.

We note that the policy was enacted before the full distribution of COVID-19 vaccines. These could potentially mitigate the impact of tourism on COVID-19. Nonetheless, long-distance travelers have a high potential to carry novel variants to new locations. Future research could explore whether tourism activity is associated with a faster arrival of new variants.

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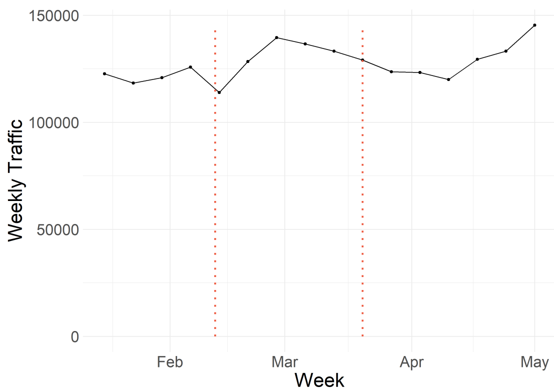
# 7 Appendix

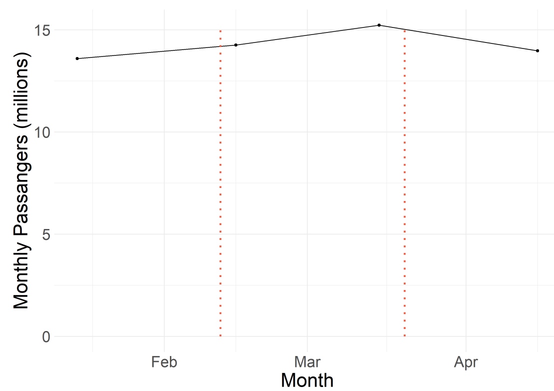
Figure 13: National traffic

(a) Average Weekly Car Traffic




Note: Figure shows average weekly number of passenger cars passing through traffic control points. There are 35 traffic control points equipped with high accuracy cameras distributed throughout the main Polish roads monitoring constantly. For visualization purposes, each point is assigned to the last day of its week (Sunday). Dotted lines represent the start and the end of the hotels opening. Source: Own elaboration based on the data from General Directorate of Roads and Motorways

(b) Monthly Number of Railway Passengers




Note: Figure shows the number of passengers transported nationwide by railway in each month. For visualization purposes, each point is assigned to the 15th day of its month. Dotted lines represent the start and the end of the hotels opening. Source: Own elaboration based on data from the Railway Transportation Authority

Table 1: Summary statistics

(a) Population data

| | |
|---|---|
| Number of unique tiles | 34592 |
| Number of tiles with any hotels | 4203 |
| Number of tiles of in proximity to ski resorts | 3316 |
| Minimum number of users on tile | 10 |
| Maximum number of users on tile | 8693 |
| Average number of users per tile 01:00-09:00 | 70 |
| Average number of users per tile 09:00-17:00 | 81 |
| Average number of users per tile 17:00-01:00 | 78 |

Note: Each observation counts the number of FB users on a tile in an 8-hour window. Data is omitted for privacy reasons if there are fewer than 10 users. If a user was on several tiles during the 8-hour period, they are assigned to the tile from which they were logging the most often (modal tile). The period covered is January 6th 2021-March 31st 2021 Source: Facebook Data for Good

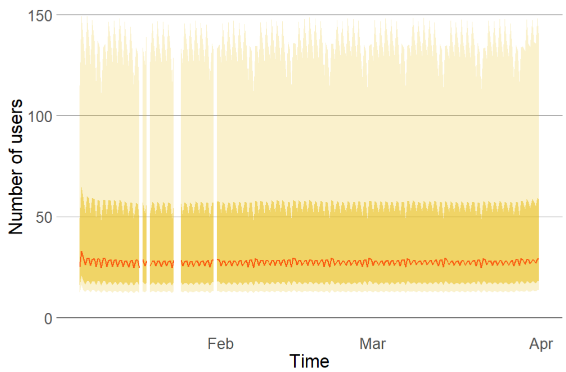
(b) Colocation data

| | |
|---|---|
| Number of unique links | 68820 |
| Average number of users with a consistent home per county | 768 |
| Average colocation probability | $2.84 * 10^{-6}$ |
| Minimum colocation probability | $9.89 * 10^{-11}$ |
| Maximum colocation probability | 0.00156 |

Note: Each observation corresponds to the probability that two randomly drawn users from two chosen counties meet in a randomly drawn 5-minute interval in a given week. A meeting is defined as being present on the same tile (0.6km × 0.6km) during a 5-minute interval. A user's home county is defined as one where she/he spent at least 6 nights in 10 days intervals around the date considered. User is discarded from computations if there is no consistent night location. Data is omitted for privacy reasons if there are fewer than 10 users. The period considered begins on the third week of January (with the last day 01-09-2021) and ends with the second week of April (with the last day 04-13-2021). Source: Facebook Data for Good

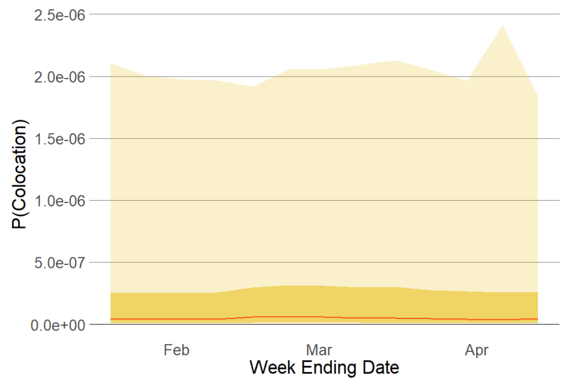
Figure 14: Data summary statistics

(a) Quantiles of FB tile population




Note: The lighter shaded area corresponds to the $10^{th}$ and $90^{th}$ quantiles of the population on tiles in a given 8 hour period. The darker area corresponds to the $25^{th}$ and $75^{th}$ quantiles. The red line represents the median. White breaks show missing data. The X-axis shows time which comprises the date and the 8-hour window. Source: Own elaboration based on Facebook data

(b) Quantiles of colocation probabilities




Note: The lighter shaded area corresponds to the $10^{th}$ and $90^{th}$ quantiles of colocation probability in a given week. The darker area corresponds to the $25^{th}$ and $75^{th}$ quantiles. The red line represents the median. The X-axis shows the last day of the week. Source: Own elaboration based on Facebook data

## Facebook data construction and spatial-temporal trends

Facebook geolocation data comes from users who have a Facebook app installed on their phones, and their location history is turned on. Their spatio-temporal records are used to calculate various measures, including Population and Colocation datasets.

### Population

Population measures the number of users at a location $A$ in a time-window $t$. It is computed in the following way.

**Assignment of a user to location** Map of a country is divided into 3km x 3km tiles. Each 24h is split into three time-periods with breaks 00:00, 08:00, and 16:00 UTC. User is assigned to tile $A$ during time window $t$ if they were pinging from a location on tile $A$ in the time window $t$. If the user pinged from more than one tile during the time window, they are assigned to their most frequent tile.

**Aggregation** The population at time window $t$ and at the tile $A$ is the number of users who logged from $A$ in time window $t$.

### Privacy concerns

For privacy reasons, FB does not show data based on fewer than ten users. Given a small size of tiles, there may be many with fewer than 10 users. This is especially true for areas without towns or villages. Mobility in such sparsely populated places may be difficult to estimate. Nonetheless, it is easy to identify when data is missing due to few users present which alleviates the problem.

**Spatial-temporal trends** Figure 15a shows trends in the daily number of users present in the Population dataset during the study period. The number is relatively stable at about 1 900 000, constituting about 5% of the Polish population. Weekends usually see fewer users than weekdays. There were five days in late January when the number of users was undercounted due to technical difficulties. Nonetheless, these dates are considerably before the policy and do not threaten our strategy. The map on the figure 16a shows the average baseline spatial distribution of the users. The baseline number of users was calculated over 90 days before the data was launched (April 2020). Data relatively well reflects the geographical structure of the Polish population. The tiles with the highest number of users correspond to Poland's large population centers. There are some tiles without enough users to cross privacy threshold in northern Poland which correspond to sparsely populated areas[27].

### Colocation

Colocation measures the probability that two randomly chosen users from county $r$ and $s$ were within the same location. It is constructed in the following process.

**Assignment of home counties to users.** First, a map is divided into administrative units (counties in the case of Poland). Next, each user is assigned a home county based on their nighttime location. Only users who pinged at least three times for each date are counted. The modal location between 8 pm and 6 am is then registered as the user's nighttime location. User is assigned a home in county $r$ if they spent at least 6 out of 10 nights in county $r$. Let $n_r$ be the number of all users assigned home in county $r$.

**Intersecting users trajectories** The goal is to check whether users were at the same place simultaneously. Only users who have location updates sufficiently often are taken into account [28]. A week is divided into 5 minutes intervals, and the map is divided into small 0.6km x 0.6km tiles[29]. Two users met or co-located if their application pinged from the same tile within a 5-minute interval.

**Computing colocation measure** Let $X_{tAr}$ be the number of users assigned to home county $r$ who pinged on tile $A$ in the 5 minute time interval $t$. Similarly, let $X_{tAs}$ be the analogous number of users assigned to home county $s$. Then, the number of collocations produced on tile $A$ at time $t$ between users from counties $r$ and $s$ is the product $X_{tAr}X_{tAs}$. Data is aggregated across all tiles on the map and across all 5 minutes intervals in a week $w$ to compute the number of colocations in the week $w$: $m_{rs,w} = \sum_{tA} X_{tAr}X_{tAs}$. The colocation probability is then the ratio of all actual meetings and all potential meetings in that week, that is: $Pr(Colocation_{rs,w}) = \frac{m_{rs}}{2016 n_r n_s}$, where 2016 is the number of all five-minute intervals in a week. The procedure is then repeated weekly.

**Spatial-temporal trends** Figure 15b shows the total number of weekly users (with complete trajectories) available to calculate colocation probabilities. The number varies between 230 000 and 260 000. It is considerably smaller than the Population dataset. This is expected as the requirements to include someone in colocation data are more stringent than for the population data (consistent home, relatively complete trajectories). There is a very slight downward trend during the study period. Map 16b illustrates the average share of county population used to calculate colocation probabilities. These shares are usually between 0.25% and 1% of the total county population. A clear trend arises where a higher share of the population is available in western counties. This follows approximately economic patterns. The divide seems, however, orthogonal to the location of ski resorts.

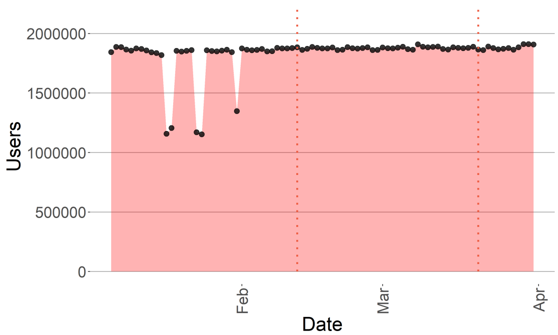
### Usage Predictors

We check whether the main demographic and economic variables correlate with the population share using Facebook geolocation. We pull a set of characteristics at the county level from the Polish Statistical Office for the year 2000. The variables are summarized in table 2. Next, we regress the average share of the

[27] Note that we cannot calculate penetration rate as we do not know true population at a tile level
[28] See Iyer et al. [2020] for technical details
[29] Note that these are smaller tiles compared to population dataset

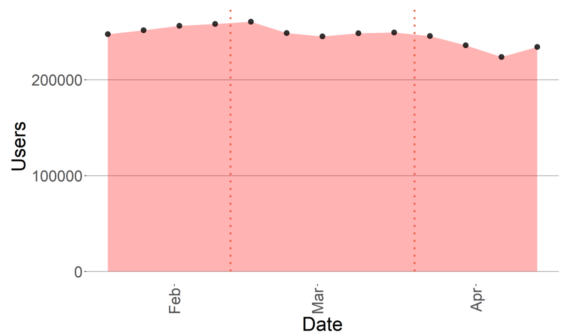
Figure 15: Temporal trends in Facebook base

(a) Population data




Note: Dots represent the daily number of FB users present in the Population dataset in Poland. Number of users on a given date is an average across three time windows of that day. Vertical dotted lines show the timing of the policy. Note that there were technical issues with data generation on five dates: January 17,18,23,24, and 31.

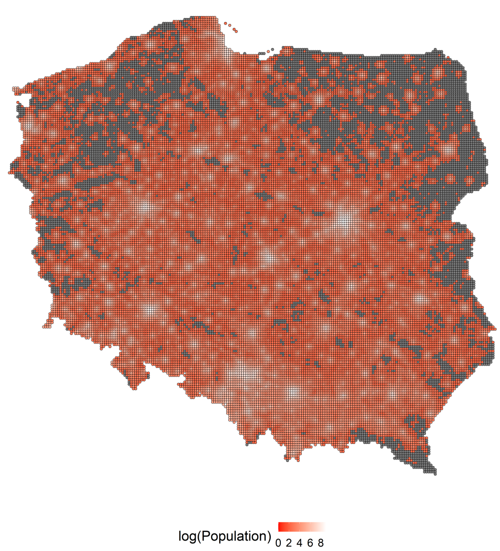
(b) Colocation data




Note: Dots represent the number of FB users in each weak whose data were used to calculate colocation probabilities in Poland. Vertical dotted lines show the timing of the policy.

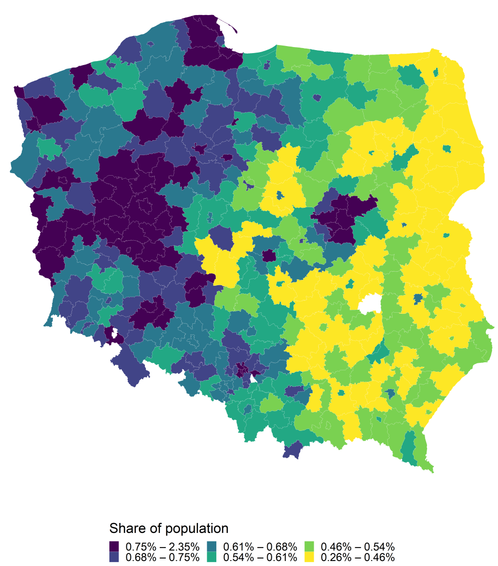
Figure 16: Spatial trends in Facebook base

(a) Population data




Note: Map shows the average baseline number of FB users on each tile. The baseline is calculated as the mean number of users on a given day-of-the-week and time-window combination across 90 days prior to April 2020 (before pandemic). The map takes average across all day of the weeks and time windows. Grey areas have no data available. The color becomes lighter as log(Population) increases.

(b) Colocation data




Note: Map shows the average share of county population used to calculate colocation probabilities. The average share in county $i$ is calculated as the mean number of users with home at county $i$ taken across all weeks divided by the total population in this county. The shares are divided into 6 quantiles with dark blue being highest shares and light yellow being lowest shares. White color correspond to no data.

population feeding colocation data (pre-policy) on the county's characteristics. The results are presented in the table 3.

Results suggest that counties with high Facebook geolocation usage are younger, more female and more urban. Moreover, they tend to do better economically as evidenced by the negative coefficient on the share unemployed. On the other hand, they seem to have slightly worse infrastructure in terms of access to healthcare, roads density, and cinemas. Facebook users with geolocation seems to also be more prevalent in counties with more hotel beds. Hence our estimates may put more weight on the movements in these populations. Note, however, that these patterns do not introduce bias in the strategy, as that would require interaction between users' characteristics and the policy timing.

Table 2: Demographic and Infrastructure Variables

| Statistic | Mean | St. Dev. | Min | Max |
|---|---|---|---|---|
| Share in colocation data | 0.006 | 0.002 | 0.003 | 0.024 |
| Population | 100,920.100 | 120,344.900 | 19,689 | 1,794,166 |
| Population per $1km^2$ | 362.398 | 647.498 | 19 | 3,690 |
| Share male | 0.488 | 0.009 | 0.456 | 0.510 |
| Share age≤14 | 0.152 | 0.016 | 0.110 | 0.225 |
| Share age≥65 | 0.182 | 0.023 | 0.118 | 0.284 |
| Share living in urban areas | 0.503 | 0.272 | 0.000 | 1.000 |
| Share of university students in the population | 0.011 | 0.031 | 0.000 | 0.197 |
| Share unemployed | 0.052 | 0.022 | 0.016 | 0.145 |
| Average monthly salary | 4,772.427 | 582.086 | 3,872.060 | 8,920.410 |
| Roads per capita | 0.009 | 0.005 | 0.001 | 0.033 |
| Doctors per capita | 0.002 | 0.001 | 0.0001 | 0.008 |
| Hotel beds per capita | 0.025 | 0.061 | 0.0002 | 0.419 |
| Libraries per capita | 0.0003 | 0.0001 | 0.00003 | 0.001 |
| Cinema seats per capita | 0.005 | 0.006 | 0.000 | 0.033 |

## Impact of opening on colocation

Suppose that the number of tourists who would come from a county s to a county r is proportional to the length of the trails in the county r ($LP_r$) and the number of people in the county s. Hence, we have $\alpha LP_r n_s$ tourists from s potentially coming to visit r (where $\alpha$ is a proportionality factor). Now, we want to know the number of additional meetings that will occur once the trails are open. In order to have a meeting, individuals need to be in the same space within a five-minute interval. For the moment, assume that every visitor from s to r stays in r for the same amount of time and that space aspect does not matter. That is, suppose that the probability that a tourist meets a local during their stay is $\delta$. So a tourist meets on avarege $\delta n_r$ locals during their stay. Consequently, the additional number of colocation events is $\alpha\delta LP_r n_s n_r$. Hence, the probability of colocation after the opening is:

$$Pr(Colocation_{rs}|After) = \frac{m_{rs0} + \alpha\delta LP_r n_s n_r}{2016 n_r n_s} = \frac{m_{rs0}}{2016 n_r n_s} + \alpha\delta LP_r$$

where $m_{rs0}$ is the default number of meetings before the opening captured by the fixed effects. Taking logs we have that

$$log(Pr(Colocation_{rs}|After)) = log(m_{rs0} + \alpha\delta LP_r n_s n_s) - log(2016 n_r n_s)$$

Taking the difference between after and before the policy implementation we obtain:

Table 3: Geolocation usage predictors

| Dependent Variable: | Share in colocation data |
|---|---|
| Model: | (1) |
| *Variables* | |
| (Intercept) | 0.0292*** |
| | (0.0099) |
| Population | $3.21 \times 10^{-10}$ |
| | $(8.03 \times 10^{-10})$ |
| Population per $1km^2$ | $-2.63 \times 10^{-7}$ |
| | $(2.03 \times 10^{-7})$ |
| Share male | -0.0513*** |
| | (0.0184) |
| Share age≤14 | 0.0242*** |
| | (0.0093) |
| Share age≥65 | -0.0077 |
| | (0.0078) |
| Share living in urban areas | 0.0037*** |
| | (0.0006) |
| Share of university students in the population | 0.0069 |
| | (0.0043) |
| Share unemployed | -0.0150*** |
| | (0.0038) |
| Average monthly salary | $-8.89 \times 10^{-8}$ |
| | $(1.49 \times 10^{-7})$ |
| Roads per capita | -0.0384 |
| | (0.0248) |
| Doctors per capita | -0.4296*** |
| | (0.1054) |
| Hotel beds per capita | 0.0028** |
| | (0.0012) |
| Libraries per capita | 1.369 |
| | (0.8603) |
| Cinema seats per capita | -0.0337* |
| | (0.0185) |
| *Fit statistics* | |
| Dependent variable mean | 0.00628 |
| $R^2$ | 0.38799 |
| Observations | 377 |

*IID standard-errors in parentheses*
*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

$$
\begin{aligned}
&log(Pr(Colocation_{rs}|After)) - log(Pr(Colocation_{rs}|Before)) = \\
&log(m_{rs0} + \alpha\delta LP_r n_s n_r) - log(2016 n_r n_s) - \\
&(log(m_{rs0}) - log(2016 n_r n_s)) = \\
&log(\frac{m_{rs0} + \alpha\delta LP_r n_s n_r}{m_{rs0}}) \approx \frac{\alpha\delta n_s n_r}{m_{rs}}
\end{aligned} \tag{8}
$$

Now let us add the hotel beds to the analysis. Assume that the hotel beds attract some additional tourists from county s and that tourists stay longer in ski resorts. In particular, suppose that the number of new visitors is proportional to the number of beds available for them. Hence we have $\tau H_r n_s$ new visitors from s to r (in addition to those who would come just for open trails) where $H_r$ is the number of hotel beds in r and $\tau$ is a proportionality constant. Additionally, visitors coming for skiing can now stay longer. Assume again that a share of them proportional to the number of beds stay longer. Hence more meetings can take place. Suppose that the share $\zeta H_r$ of tourists who stay longer produce $\kappa$ more meetings than a tourist who does not stay in a hotel. Let us some up all the new terms. First, we have tourists who come skiing but don't stay for the night: $(1 - \zeta H_r)\alpha LP_r n_s$. Second, we have tourists who come because hotels opened: $\tau H_r n_s$. Third, we have tourists who come skiing and stay for the night: $(\zeta H_r)\alpha LP_r n_s$. In total, we obtain the following expression for the colocation probability after the opening of hotels and trails:

$$
\begin{aligned}
Pr(Colocation_{rs}|After) &= \frac{m_{rs0} + (\delta\alpha LP_r n_s n_r)\zeta H_r + \delta\tau H_r n_s n_r + (\kappa\delta\alpha LP_r n_s n_r)(1 - \zeta H_r)}{2016 n_r n_s} \\
&= (\delta\alpha LP_r)\zeta H_r + \delta\tau H_r + (\kappa\delta\alpha LP_r)(1 - \zeta H_r)
\end{aligned} \tag{9}
$$

Taking again the difference of logs before and after the policy we obtain:

$$
\begin{aligned}
&log(Pr(Colocation_{rs}|After)) - log(Pr(Colocation_{rs}|Before)) \approx \\
&\frac{(\delta\alpha LP_r n_s n_r)\zeta H_r + \delta\tau H_r n_s n_r + (\kappa\delta\alpha LP_r n_s n_r)(1 - \zeta H_r)}{m_{rs}}
\end{aligned} \tag{10}
$$

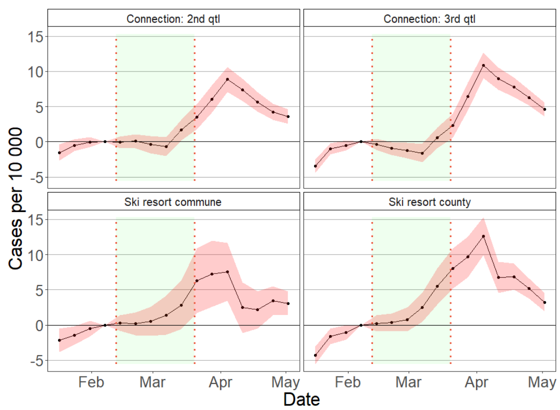
**Event studies in the number of Covid-19 Cases** Figures 17a and 17b show the coefficients from the health outcomes event study in the full and matched sample, respectively. We estimated the following regression to obtain the coefficients:

$$
y_{kw} = \sum_{\substack{W \in \{\{01/17:01/31\},\\ \{02/14:05/02\}\}}} Ski\_resort_k I(w = W)\beta^W + X_{kw}\delta + \zeta_k + \pi_w + \epsilon_{klw} \tag{11}
$$

Where $y_{kw}$ represents the number of cases per 10 000 in a commune $k$ and a week ending at date $w$. The dummy $Ski\_resorts_k$ takes value 1 if the commune k contains a ski resort, and the indicator $I(w = W)$ is one if the week at hand is equal to $W$. The interaction between these two terms measures the differential trend in the cases per 10 000 in communes with versus. without ski resorts. $X_{kw}$ contains controls for the share of fully vaccinated two weeks ago and the number of negative tests per 10 000. We allow for time $\pi_w$ and commune $\zeta_k$ fixed effects, and we cluster the errors at the commune level. Figures 17a and 17b plot $\beta_W$ coefficients from either estimation on the full sample or matched sample. The results are consistent with the unconfoundedness approach and suggest that the opening of hotels sped up the arrival of second-wave to communes with ski resorts.

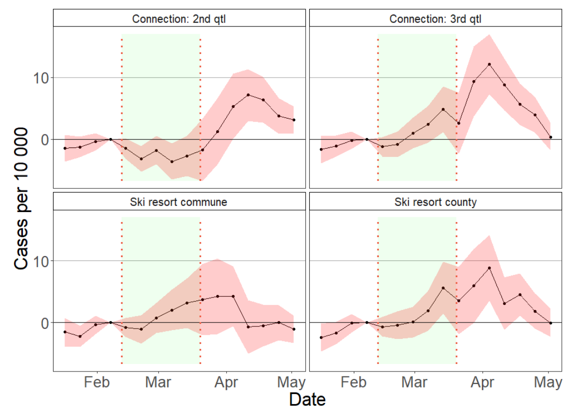
Figure 17: Event study: Covid-19 cases and hotels opening

(a) Covid cases: full sample



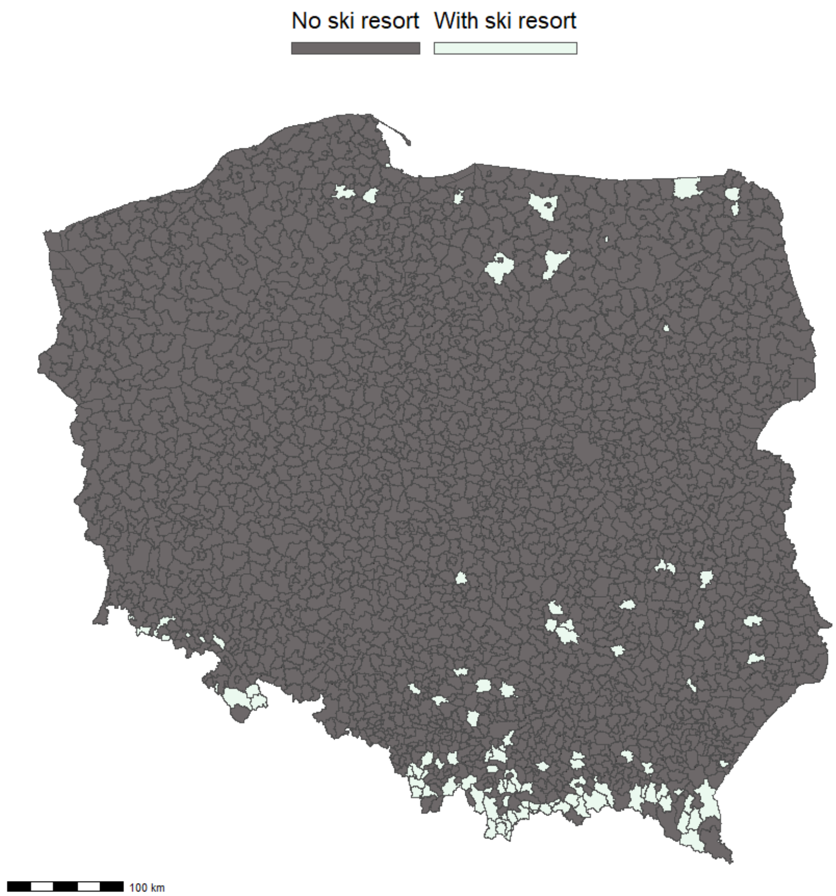

Note: The regression coefficients were calculated on the full sample. The date corresponds to the last day of the week. Source: Own elaboration based on the Ministry of Health Data

(b) Covid cases: matched sample



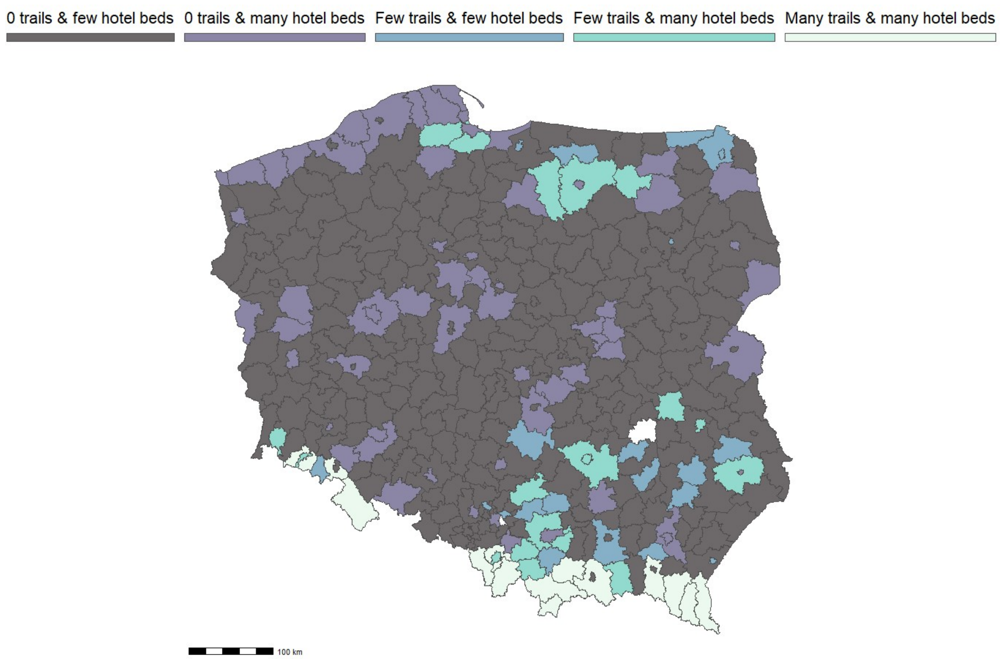

Note: The regression coefficients were calculated on the matched sample. Each treated unit was matched with one untreated units (first quantile of connection). Units were matched by the distance in the propensity scores computed on characteristics in the last period before the policy. The date corresponds to the last day of the week. Source: Own elaboration based on the Ministry of Health Data

Figure 18: Spatial distribution of communes with ski resorts



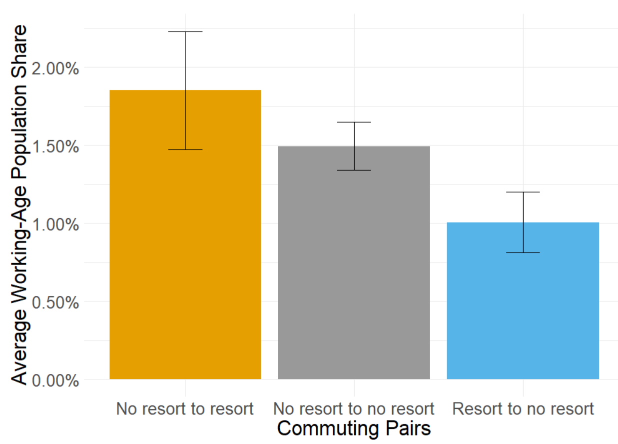

Note: The communes colored in white contain ski resorts. Source: Own elaboration based on data collected from internet

Figure 19: Spatial distribution of the touristic appeal



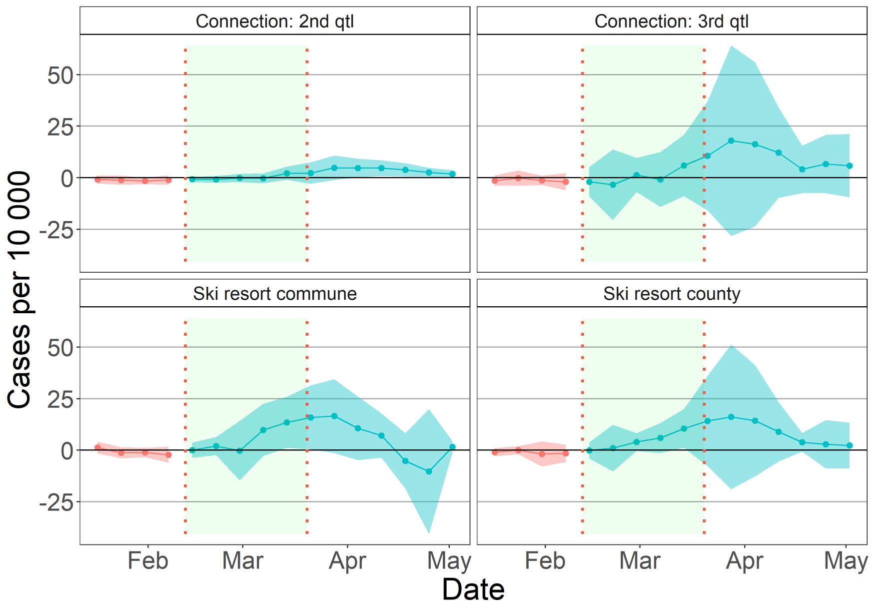

Note: Colors correspond to the touristic appeal. Source: Own elaboration based on data from Polish Statistical Office and own data

Figure 20: Commuting in counties with ski resorets



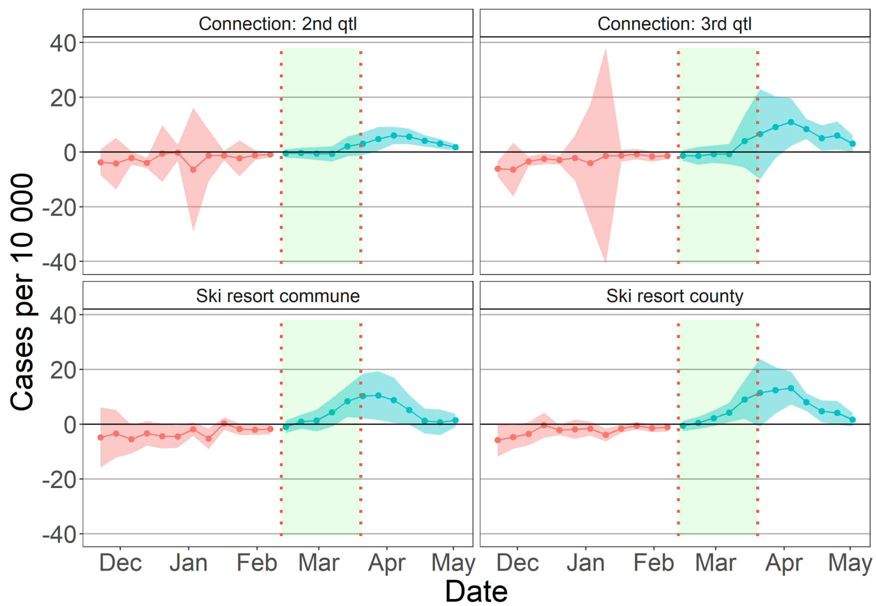

Note: The shares are calculated from data on commuting between communes in 2014 (the most recent available data). A share of working age commuters corresponds to the share of working age population in a commune $j$ commuting to work in a commune $i$. The averages are taken across types of pairs. No resorts communes are communes without ski resorts and resort communes are communes with ski resorts. The sample is restricted to counties containing ski resorts. Source: Own elaboration based on data from Polish Statistical Office

Figure 21: Event Study: Covid-19 and tourism opening restricted sample



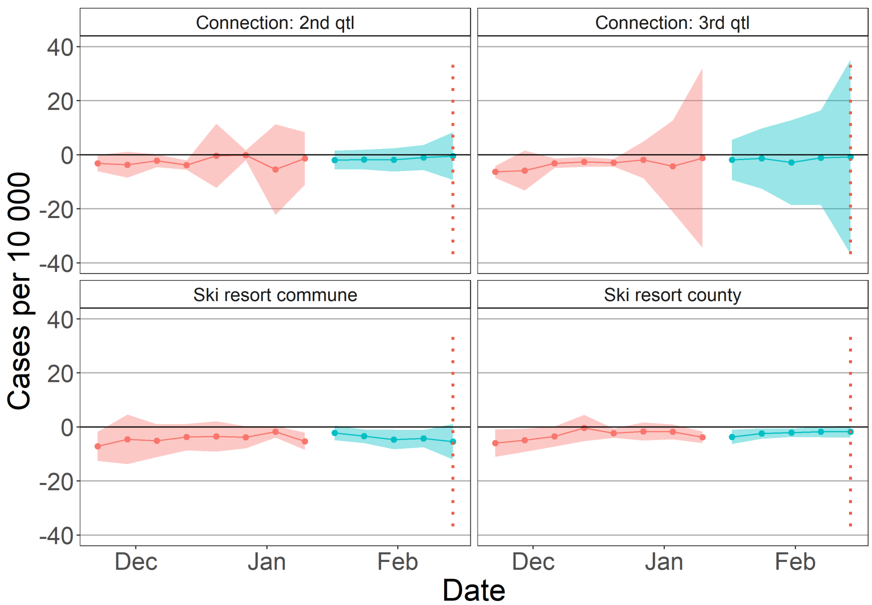

Note: This figure replicates figure 11, but excluding 4 regions which changed restrictions during the study period. As the sample size decreased, some covariates could no longer be used for conditioning due to convergence issues in estimating propensty scores. In partiular, the following covariates were excluded: *population size*, *population density*, and *whether the commune is a county*. The estimates come from the unconfoundedness approach by Callaway and Li [2021]. Red points correspond to the estimates for pre-treatment periods. The reference point in a pre-treatment period $t$ is the previous period $t-1$. Blue points correspond to the estimates for post-treatment periods. The post-treatment periods' propensity scores and outcome regression are based on the last period before the policy $w*-1$. The shaded area represents simultaneous 95% confidence bands with clustering at the commune level. The date corresponds to the last day of the week.

Figure 22: Event Study: Covid-19 and tourism opening; extended pre-period




Note: This figure replicates figure 11, but it extends the preperiod until November 2020. Vaccinations and deaths variables can no longer be used for conditioning as the data starts in 2021. The estimates come from the unconfoundedness approach by Callaway and Li [2021]. Red points correspond to the estimates for pre-treatment periods. The reference point in a pre-treatment period $t$ is the previous period $t-1$. Blue points correspond to the estimates for post-treatment periods. The post-treatment periods' propensity scores and outcome regression are based on the last period before the policy $w*-1$. The shaded area represents simultaneous 95% confidence bands with clustering at the commune level. The date corresponds to the last day of the week.

Figure 23: Event Study: Covid-19 and tourism opening; placebo timing




Note: This figure replicates figure 11, but it sets a placebo treatment date on the 14th of January. Vaccinations and deaths variables can no longer be used for conditioning as the data starts in 2021. The estimates come from the unconfoundedness approach by Callaway and Li [2021]. Red points correspond to the estimates for pre-treatment periods. The reference point in a pre-treatment period $t$ is the previous period $t-1$. Blue points correspond to the estimates for post-treatment periods. The post-treatment periods' propensity scores and outcome regression are based on the last period before the policy $w*-1$. The shaded area represents simultaneous 95% confidence bands with clustering at the commune level. The date corresponds to the last day of the week. The dotted line represents the actual start of the actual.