The Economic Effect of Discrimination: Evidence from Restaurant Sector*

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Abstract

Discrimination is a persistent phenomenon and often results in economic consequences for affected groups. This study investigates the economic impact of consumer discrimination on Chinese restaurants in the U.S. during the COVID-19 pandemic's onset. Using cell phone signal data, restaurant revenue records, and the pandemic's unexpected onset as a natural experiment, the study identifies a marked and early decline in visits to Chinese restaurants, culminating in a -10% relative drop in April. A back-of-the-envelope calculation based on restaurant revenue data indicates a loss of approximately 35 million dollars for Chinese restaurants in the U.S. from February to April 2020. Additionally, this study finds that supply-side factors, such as restaurant closures and online platform transitions, had limited impact in explaining the relative decline in visits, suggesting a substantial role for demand-side factors. This includes observable county-level variations linked to political affiliation, racial diversity, and Asian population ratio.

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Discrimination is a persistent social phenomenon. Its presence becomes more pronounced during certain public crises and leads to the affected group experiencing not only social repercussions but also economic consequences. For example, following the 9/11 attacks, individuals of Arab and Muslim descent in the U.S. faced discrimination, resulting in economic hardships such as lower wages in the labor market (Kaushal, Kaestner, and Reimers, 2007). Similarly, the internment of Japanese Americans during World War II led to the entire community facing unwarranted suspicion and immense economic losses (Chin, 2005). These examples highlight the need to understand the economic effect of discrimination, which is the policy-relevant parameter that policymakers would be interested in when considering policies to stabilize the affected social groups.

In this paper, I ask the following questions relevant to understanding the economic effect of discrimination: how do affected groups that are unfairly blamed suffer economically? what are the relative roles of factors contributing to such a phenomenon? For example, given that local social characteristics, such as local political affiliation and local racial diversity, could greatly influence and shape human behaviors and outcomes (e.g. Agan and Starr, 2020; Allcott et al., 2020; Ba et al., 2021), how is the economic effect of discrimination tied into local social charactersitics across regions?

I answer these questions by leveraging a combination of recently available cell phone signal data, restaurant revenue data and a unique natural experiment: the unanticipated arrival of the Covid-19 pandemic in the U.S. For three reasons, this triad offers a unique opportunity to gain valuable insights into the economic effect of discrimination. First, the Covid-19 pandemic witnessed the economic impact of discrimination towards a well-defined social group: Chinese (or broadly Asian) restaurants. News stories (e.g. The Washington Post, 2020; SUNY-Binghamton, 2020) claim that Chinese or Asian restaurants suffered from consumer discrimination: consumers discriminated in deciding to visit restaurants of different cuisines.[1] Anecdotally, these cuisine restaurants suffered much earlier and more than other

[1] As discussed later, consumer discrimination in this setting may not necessarily motivated by only racial discrimination. There is a difference between racial discrimination towards a group, and consumer discrimi-

cuisines in the U.S. at the onset of the pandemic, for reasons including the irrational fear that eating Chinese food leads to Covid infection. To the extent that the discrimination was towards a clear-defined group, it allows researchers to measure the economic effect of discrimination by comparing the business outcomes of affected groups (i.e. Chinese cuisine) to its counterpart (i.e. American and European cuisines).

Second, the cell phone signal data used in this study offer a novel way to measure discrimination in the context, providing sufficient statistical power for econometric analyses. While traditional metrics, such as hate crime reports, are crucial for understanding overt forms of discrimination during public crises, they often offer limited statistical power due to their relatively rare occurrence. Moreover, while hate crimes represent the more extreme end of the discriminatory behavior spectrum, many subtle forms of discrimination, which are just as impactful but less overt, are not captured by these traditional metrics. For example, consumers deciding not to patronize a business based on race constitutes a form of discrimination that, while morally questionable, is not a crime under current law and therefore often goes unreported (Bartlett and Gulati, 2016). The cell phone signal data utilized in this research overcome these limitations by enabling the measurement of a more subtle form of discrimination with sufficient statistical power — consumer discrimination in restaurant visits. By constructing a comprehensive weekly panel of individual restaurant customer visits from December 2019 to April 2020[2], encompassing nearly all restaurants in the U.S., this study can track relative changes in visits to Chinese cuisine compared to American and European cuisines. Furthermore, I complement this restaurant visit panel by a newly available restaurant revenue dataset with revenue information for a subset of the restaurants in the visit panel. This complementary dataset allows me to quantify the economic effect of

nation motivated by unfounded fear towards a cuisine; one could have no prejudice towards Chinese people but have an irrational fear that eating Chinese food leads to Covid infection so that practice consumer discrimination.

[2]The choice of the time frame is motivated by the observational fact that discrimination during public crises is the most prominent at the onset of crises when uncertainty looms large. Besides, the cell phone signal data provided by SafeGraph go back furthest to 2018, so the fact that the Covid-19 pandemic is one of the most serious public crises since the availability of the novel data makes it a good setting to study the effect of interest.

consumer discrimination towards Chinese cuisine in dollar terms.

Third, the unanticipated arrival of the Covid pandemic serves as a natural experiment that overcomes the identification issue in the discrimination literature. Discrimination often correlates with unobservable confounding factors that cloud identification (e.g. a survey by Pager and Shepherd, 2008). The arrival of the Covid pandemic, arguably conditionally uncorrelated with factors that change cuisine-specific trend of custom visits, serves as a natural experiment to bypass the identification challenges.

I use a difference-in-differences framework to approximate the effect of the consumer discrimination by the causal effect of early pandemic events on Chinese cuisine visits relative to American and European cuisine visits. My baseline empirical strategy, which employs week fixed effects for each county and individual restaurant fixed effects, relies on the cuisine-by-week variation in restaurant visits to identify the causal effect. Additionally, because takeout is an important feature of traditional Chinese restaurants in the U.S., my empirical specification directly accounts for the possibility that consumers might switch to takeout restaurants at the onset of the pandemic within counties. Conditional on this extensive set of controls, the model tracks the relative change of Chinese cuisine visits before and after the national first Covid case to provide suggestive evidence of consumer discrimination.

My empirical analyses uncover several key results. Echoing the news claims, Chinese cuisine was the only cuisine that experienced early relative drops in visits immediately after the national first case, and the size of the relative decline accumulated to about -8% by March. This baseline results are subject to robustness checks that rules out certain other factors potentially biasing the estimation, including some measurement issues and seasonality. A complementary analysis on restaurant revenue using a subset of restaurant reveals the same dynamics with Chinese cuisine revenue experiencing early relative declines. However, the magnitude of the effect on Chinese cuisine revenue is even larger than those on restaurant visit, with a relative drop in revenue of about -13% in February and amplifying to over -20% in March. A back-of-the-envelope calculation suggests that the Chinese cuisine in the U.S.

cumulatively lost about 35 million dollars in restaurant revenue from February to April 2020 relative to American and European cuisines.

I further assess the relative contributions of supply-side and demand-side factors in driving the observed decline in visits to Chinese cuisine restaurants, a proxy for the effect of consumer discrimination, which is primarily a demand-side phenomenon. My findings indicate that supply-side factors, such as voluntary restaurant closures and the rate of transition to online platforms, had minimal impact. This underscores a more significant role for demand-side factors in the observed changes in Chinese cuisine visits. This includes observable county-level variations linked to political affiliation, racial diversity, and Asian population ratio. For example, in counties with a higher proportion of Republican votes, there was an additional relative drop of -10% in visits to Chinese restaurants. Conversely, counties with greater racial and ethnically diverse populations saw an additional relative increase of +8% in such visits. Additionally, I observe a spillover effect impacting non-Chinese East Asian cuisines, rather than all non-Chinese Asian cuisines, suggesting likely influences by perceived culinary, cultural or geographical proximity.

This paper's contributions to the literature on the effect of discrimination are threefold. First, this paper contributes to the literature on the impact of discrimination towards minority groups during public crises. The uncertainty and fear at the onset of public crises often cause a common impulse to assign social responsibility to the crises and worsen discrimination (Hoppe, 2018). Examples of previous studies has explored various contexts, such as war (e.g. Chin, 2005), terrorism (e.g. Kaushal, Kaestner, and Reimers, 2007; Shayo and Zussman, 2011), extreme weather events (e.g. Anderson, Johnson and Koyama, 2017), economic or political instability (e.g. Anderson, Crost and Rees, 2020; Grosfeld, Sakalli and Zhuravskaya, 2020), and health crises, including the Covid-19 pandemic (e.g. Voigtlander and Hans-Joachim, 2012; Jedwab, Johnson and Koyama, 2019; Auer, Ruedin and Van Belle, 2023; Fasani and Mazza, 2023; Huang et al., 2023; Uruci, 2024). These studies have investigated outcomes during the crises or longer-term outcomes like persecution, violence, hate crimes, labor market outcomes, judicial bias, and housing market outcomes. My paper adds to this body of work by examining the economic impact of subtler forms of discrimination – specifically consumer discrimination against cuisine restaurants – at the onset of the Covid-19 pandemic.

In relation to Huang et al. (2023), who also used restaurant visit data to assess consumer discrimination in the restaurant sector, my study differentiates from their paper in the following key aspects: First, this paper utilizes both restaurant visit data and restaurant revenue data, and Huang et al. (2023) utilize only visit data. This dual-data approach enables a more comprehensive quantification of the economic impact of discrimination. Specifically, by incorporating restaurant revenue data, I not only quantify the economic impact of consumer discrimination in monetary terms but also provide insights into the performance of online business for Chinese cuisine, which saw less relative decline compared to in-person transactions.

Secondly, my analysis leverages both visit and revenue data to dissect the relative roles of supply-side factors in the relative decline of Chinese cuisine visits, an aspect not addressed by Huang et al. (2023). While Huang et al. (2023) do engage with some demand-side factors, my research uniquely delves into the pertinent supply-side factors and significantly extends the scope of the demand-side analysis. My findings suggest limited impact from supply-side factors like restaurant closures and online platform adoption, pointing towards a more pronounced influence of demand-side factors. Additionally, I expand on the role of political affiliation and uncover the significance of local racial diversity in explaining geographic variations in cuisine visit changes. I also examine the nature of the consumer discrimination, and the results suggest that it may be a mixture of taste-based and fear-driven statistical discrimination.

Thirdly, unlike Huang et al. (2023), who group all non-Chinese Asian cuisines into one category and report a generalized negative spillover effect, my study distinguishes between these cuisines and finds that the negative spillover effect is primarily concentrated in non-

Chinese East Asian cuisines, as shown by a negative impact on Japanese and Korean cuisine visit but the positive impact on Indian cuisine, a South Asian cuisine. This finding is crucial because it suggests that discriminatory behaviors and biases may be more specifically targeted and influenced by perceived culinary, cultural or geographical proximity.

Last but not the least, the discrepancy in the magnitude between my estimates and those of Huang et al. (2023) – mine being substantially smaller – can be attributed to my inclusion of county-by-week fixed effects and consideration of restaurant types (takeout versus sit-down). This discrepancy underscores the importance of accounting for local, time-varying factors unrelated to consumer discrimination that could significantly alter estimations and interpretations in this context. My paper also utilizes an event study specification that enables a dynamic examination of the effect and reveals that Chinese cuisine visit and revenue experienced early declines immediately following the national first case.

This paper also contributes the literature on consumer discrimination. A substantial strand of the literature focus on consumer discrimination in labor markets where it directly reduces the minority workers' productivity and influence their labor market outcomes. Examples includes Nardinelli and Simon (1990); Neumark, Bank and Van Nort (1996); Holzer and Ihlanfeldt (1998); Combes et al. (2016); Bar and Zussman (2017). Another substantial strand of the literature focus on consumer discrimination in consumer markets. In consumer markets, researchers have studied situation in which minority consumers receive lower-quality services, higher prices or differential treatment relative to their counterparts (e.g., Yinger, 1998; Cook et al., 2022, 2023). Conversely, another aspect of consumer markets that has garnered attention is the discrimination by consumers against sellers of varying demographic backgrounds, impacting the business performance of these sellers. Examples include Doleac and Stein (2013); Bar and Zussman (2017); Tjaden, Schwemmer and Khadjavi (2018); Huang et al. (2023); Zussman (2023). This paper contributes to the latter strand of literature finding that cuisine restaurants classified by race and ethnicity experienced differential treatment from consumers. The results suggest that, beyond individual sellers, racially themed small businesses were also affected by consumer discrimination. Note that the consumer discrimination in the context of the current paper could arise from taste-based discrimination where consumers punish Chinese cuisine for the Covid pandemic or statistical discrimination due to belief that going to Chinese restaurants may increase the infection risk. This paper abstracts from disentangling these channels and focuses on quantifying the economic impact of consumer discrimination. Uruci (2024) disentangles these channels by looking at relative changes in visits to Chinese restaurants with differential dining intensity, finding that the consumer behavior is consistent with statistical discrimination but hard to reconcile with a taste-driven mechanism.

Lastly, this paper suggests a novel measurement of discrimination that is well-powered for statistical analyses and may open a new door for researchers to measure the effect of more subtle types of discrimination. Discriminatory behaviors vary in intensity and subtlety, making some forms difficult to detect and measure. Recent research have been employed innovative methods to address this challenge. Examples include but not limited to, Google Trends search of racial slurs (e.g., Stephens-Davidowitz, 2014; Anderson, Crost and Rees, 2020); resume-correspondence study in labor market (e.g., Bertrand and Mullainathan, 2004); email-correspondence in rental market (e.g., Hanson, Hawley and Taylor, 2011); machine learning and textual analysis (e.g., Burn et al., 2022), experimental methods in lab settings (e.g. Neumark, 2018). This paper's approach, using comprehensive public location data and cell phone signals to track consumer visits, complements these methods by suggesting a tool for measuring more nuanced discriminatory behaviors.

1 Data

The primary data sources include (1) SafeGraph visit data, (2) SafeGraph spending data, (2) Covid case data from The New York Times (2021), (3) county race and ethnicity compositions from the 2019 5-Year American Community Survey (ACS), and (4) county voting data in the 2016 presidential election from the MIT Election Data and Science Lab (2018).

1.1 SafeGraph Restaurant Visit Panel

1.1.1 SafeGraph Visit Data

SafeGraph randomly samples 10% anonymous cellphone users in the U.S., then tracks and matches the signals to a collection of public locations (point-of-interest, POI) to measure visits of each POI on daily frequency. Public locations include restaurants, retail shops, etc. This paper focuses on restaurants.[3] According to SafeGraph (2022a), the restaurant sector in the U.S. is well covered in their POI records. For each POI, SafeGraph reports a range of information including its name, address, census block groups, the North American Industry Classification System (NAICS) code, and the key mobility measure used in this paper — the number of visits that are made by the sampled cellphone users each day.

The methodology for counting a cellphone signal as a restaurant visit involves two criteria. Firstly, SafeGraph assigns a geometric shape, predominantly a polygon, to represent a restaurant's space.[4] This polygon acts as a geofence, determining if a cellphone signal enters the restaurant's area. These polygons are fine-tuned and validated against Google Maps satellite imagery, which can detect discrepancies as small as 2 meters, ensuring that the polygon's size closely matches the actual size of the restaurant.[5] Secondly, to register as a visit, SafeGraph stipulates that the cellphone signal must remain within this polygon for a minimum of 4 minutes. This criterion ensures that mere proximity or passing by does not count as a visit. Therefore, a POI visit, as defined by SafeGraph, requires that the cellphone signal both enters and stays within the restaurant's polygon for over 4 minutes (SafeGraph, 2022b).

The cellphone user sample is representative of the U.S. population. Previous research and the internal analysis of SafeGraph (e.g. Squire, 2019) have found the cellphone sample is

[3]Hereafter, I will use the terms "restaurant" and "POI" interchangably

[4]SafeGraph uses two geometric shapes: "polygon" and "point." The latter is less common and more prone to measurement errors; only 6 out of over 1 million U.S. restaurants in SafeGraph's database are assigned a "point." Hence, this study focuses on restaurants represented by polygons.

[5]Google Maps is widely used by industry and academia as the benchmark data (e.g. Zamir and Shah, 2010).

well representative at the county level and highly correlated with key demographical features: population (Pearson correlation coefficient, r=0.97), education (r=0.99), and household income (r=0.99).

1.1.2 Sample Construction and Summary Statistics

The individual restaurant visit panel is formed by POIs with NAICS code "722511: Full-Service Restaurants" or "722513: Limited-Service Restaurants". I aggregate the restaurants' daily visits to weekly visits from December 2^{nd} 2019 to April 26^{th} 2020. For each restaurant, SafeGraph provides descriptive tags to describe the cuisine category that the restaurant belongs to. Example tags are "Chinese food", "American food", etc. Moreover, a number of restaurants are associated with multiple tags, and these restaurants are manually checked and coded in obvious cases. For example, "Chinese food; Soup" is classified as "Chinese food" without ambiguity. This information enable me to group individual restaurant into several different cuisines.

To the extent that Asia is a geographically vast area and is home to numerous different ethnicities with different cultures and cuisines, I choose **not** to lump all non-Chinese Asian restaurants together as a group, which facilitates the interpretation of spillover effect of consumer discrimination to other non-Chinese Asian restaurants. At the same time, it is important to balance between focusing on multiple non-Chinese Asian cuisines and having sufficient statistical power (and brevity). Specifically, I classify restaurants into 7 categories: "American and European cuisines", "Chinese cuisine", "Japanese and Korean cuisines", "Indian cuisine", "Asian fusion and other Asian cuisines", "Mexican cuisine and other Latin cuisines", and "other cuisines". "American and European cuisines", which is the reference group in the analyses, consists of American cuisine and major European cuisines including French, Italian, Greek, Portuguese, Spanish, German, Irish, and Mediterranean cuisines. I tried separating American cuisine from European cuisines, the baseline results are similar by either using American restaurants as the reference group or using European restaurants. For brevity and the context of this study, I group them together as the reference group. Similarly, I group "Japanese and Korean cuisines" as one group because they are geographically and culturally close to China and have a reasonably number of restaurants in the U.S. In addition, a popular South Asian cuisine, "Indian cuisine", is considered because while being an Asian cuisine, it is not as closely related to Chinese cuisine as Japanese and Korean cuisines. "Asian Fusion and other Asian cuisines" covers Asian fusion restaurants and other Asian cuisines that have fewer restaurants than Chinese, Japanese, Korean and Indian cuisines (e.g. Thai, Burmese cuisines). "Mexican cuisine and other Latin cuisines" consist of all cuisines from Latin America with Mexican cuisine being the majority. "other cuisine" contains restaurants without a tag and many other cuisines associated with other geographic areas, and they each have a small number of restaurants in the U.S. (e.g. Ethiopian cuisine).

The main dependent variable of my analyses is the individual restaurant weekly visits over the period of interest. Figure 1 panel (a) provides the trend of the average weekly visits for each cuisine group averaged over county and weighted by county population from December 2^{nd} 2019 to April 26^{th} 2020 using the raw data.

In my restaurant visit panel, 2.76% of the observations are weeks with zero visits, providing an opportunity to examine impacts on both the extensive and intensive margins. For the extensive margin analysis, I define a restaurant closure as a month with zero visits, rather than use weekly frequency. This choice accounts for SafeGraph's sampling of only 10% of U.S. cellphone users, recognizing that an occasional week with zero visits doesn't necessarily indicate a closure. Meanwhile, for the intensive margin analysis, all zero-visit observations are excluded to examine the change in visit to different cuisines conditional on positive visits.

In addition, as consumers might switch to mainly takeout restaurants at the onset of the pandemic, it is important to take this dimension into account. SafeGraph provides the dwelling time of each visit for each restaurant in the panel, so I use each restaurant's average customer dwelling time in 2019 as the approximation of the restaurant type that is either a mainly takeout restaurant or a mainly sit-down restaurant. Thus, I construct a binary variable, *takeout* , which equals to one if the restaurant's average consumer median dwelling time in 2019 was less than 30 minutes, and 0 otherwise as a mainly sit-down restaurant.[6]

Overall, the panel covers 642,220 restaurants in 3,098 out of 3,108 counties and county equivalents in the contiguous United States in a 21-week period from December 2^{nd}, 2019 to April 26^{th}, 2020. Appendix Table A1 provides the summary statistics for the number of restaurants associated with each cuisine.

1.1.3 Measurement Issues, Limitation, and Remedies

There are a few potential measurement issues that are important to discuss, along with discussions on potential remedies. First, according to SafeGraph (2022b), SafeGraph acknowledges limitations in the accuracy of the visit data for POIs enclosed in a bigger POI. An example is restaurants within an indoor mall where multiple "child polygons" (restaurants) are enclosed by the "parent polygon" (indoor mall). In this case, the cellphone signals inside the "parent polygon" tend to be jumpy hence are likely to be misattributed to different POIs within the bigger polygon. To address this issue, I will drop all restaurants in the constructed panel that are enclosed by bigger polygons (2.38% of all restaurants in the constructed panel). This exclusion does not significantly alter the baseline results, as will be demonstrated in the results section.

Second, in highly dense urban areas, people may live or work above the restaurants, which may result in misattribution of signals from residents or workers of firms upstairs. Fortunately, SafeGraph provides the number of visitors whose home or daytime census block group is the same as the restaurant's among all visitors of the restaurant. According to SafeGraph (2022b), the visitor's home (or daytime) census block group is determined by analyzing data from a base period of 6 weeks during nighttime from 6 pm to 7 am (or daytime from 9 am to 5 pm). SafeGraph will assign a home (or daytime) census block group to a visitor if the data provide evidence that the visitor's cellphone signal spent sufficient time

[6]Results are robust to using other thresholds like 15 and 20 minutes.

for most days in the corresponding census block group during the nighttime (or daytime). Utilizing this data attribute, the sample used in this study excludes those residents or workers from the total visit metrics.

Third, the potential for misattribution of cellphone signals in urban areas remains a concern, despite the precision of SafeGraph's polygon measures and efforts to filter out local residents and workers. To assess the extent of this issue, the analysis later divides the sample by USDA county rural-urban status, allowing for a comparison between different area densities.

Fourth, the visit data do not capture the restaurant revenue. If consumers showed sympathy by tipping more or ordering extra dishes from Chinese restaurants, the data do not capture this behavior. To help understand the financial implications beyond visit counts, I bring in SafeGraph spending data, which will be explained in details in the next subsection.

1.2 SafeGraph Spending Data and Restaurant Revenue Panel

Despite the comprehensive coverage of the U.S. restaurant sector by SafeGraph visit data, a notable limitation in the context of this study is its inability to measure restaurant revenue, particularly the revenue changes in Chinese cuisine relative to the reference cuisines. To address this gap, I leverage a newly introduced SafeGraph dataset known as SafeGraph Spending. This dataset leverages anonymized debit and credit card transactions from a large financial company, offering monthly aggregated data on each restaurant's revenue and transaction metrics.

The SafeGraph Spending data enrich the analyses by providing a monetary dimension to the mobility patterns observed. While visit data reveals customer foot traffic, the spending data bridges the gap by showing how this traffic translates into actual financial outcomes for restaurants. This addition is crucial for a more holistic understanding of the economic impact, especially in gauging the financial implications for Chinese restaurants during the onset of the Covid pandemic.

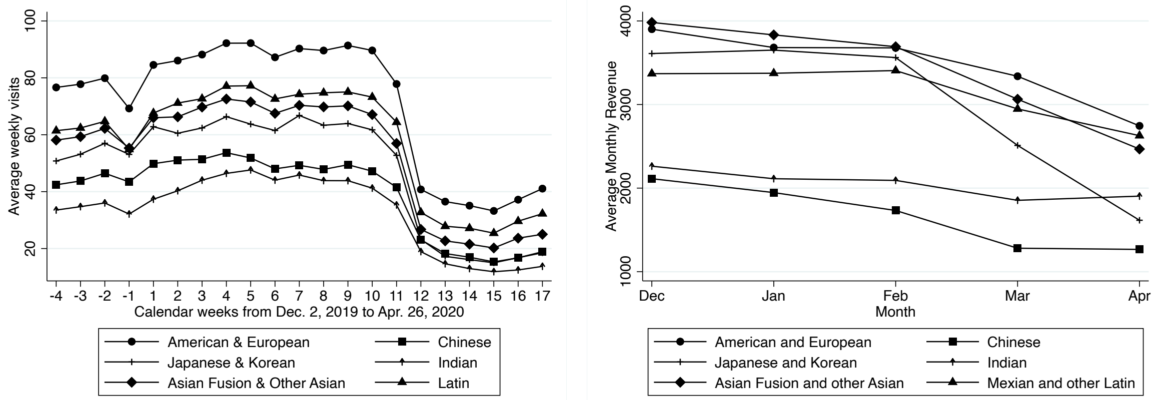
However, it is important to acknowledge a limitation of the SafeGraph Spending dataset: it covers approximately 40% of individual restaurants featured in the constructed restaurant visit panel. This compositional change necessitates a careful approach to ensure the robustness of my findings. Consequently, I will later demonstrate that the baseline results derived from this 40% subset, focusing on cuisine visits, align closely with those obtained from the full sample. This alignment supports the validity of using the SafeGraph Spending data as a complementary source to enrich my analyses, despite its smaller sample size.

The SafeGraph Spending data provides monthly revenue information aggregated to individual restaurants. To align with the sample period of the restaurant visit panel, I construct a 5-month restaurant revenue panel using data from December 2019 to April 2020. The main dependent variables are monthly revenue and monthly number of transactions. Figure 1 panel (b) provides the trend of the average monthly revenue for each cuisine group averaged over county and weighted by county population from December 2019 to April 2020 using the raw data.

A noteworthy feature of SafeGraph Spending data is its ability to differentiate between standard in-person and online platform transactions, which is identified before aggregation by transaction notes like "Doordash", "Uber Eats", etc. During the Covid pandemic's early stages, many restaurants shifted towards takeout and delivery services. This dataset allows me to analyze the online platform performance of different cuisines on both extensive margin and intensive margin. For the analysis on the extensive margin, I define a restaurant as utilizing an online platform if it records positive online revenue in a given month. For the analysis on the intensive margin, I examine the change of online revenues and online customer volume of Chinese cuisine relative to the reference cuisine. Thus, this data provide insight on how different cuisines fared on these online platforms.

Figure 1. Trend of Cuisine Weekly Visits and Monthly Revenue

(a) Trend of Average Weekly Visits by Cuisines (b) Trend of Average Monthly Revenue by Cuisines



The left panel shows the trend of average weekly visits for each cuisine in the panel from December 2^{nd} 2019 to April 26^{th} 2020. On the x-axis of the left panel, {-4,...,-1} denotes the last four calendar weeks of 2019, and {1,2,..,17} denotes the first 17 calendar weeks of 2020. The right panel shows the trend of average monthly revenue for each cuisine in the panel from December 2019 to April 2020.

1.3 County Political Affiliation and Diversity Index

To shed light on the demand-side factors related to the consumer behaviror in the context, I examine the heterogenous impact of consumer discrimination along county social characteristics. I define county political affiliation as the proportion of total votes in the county received by the Republican Party in the 2016 presidential election using data from the MIT Election Data and Science Lab (2018). The higher the measure, the more republican supporting the county is. I normalize the continuous measure to be mean zero and unit standard deviation to facilitate interpretations later. Appendix Figure A2 panel (a) shows the distribution of the standardized county political affiliation variable.

U.S. Census (2021a) calculates the county diversity index in race and ethnicity using Equation 3 in Appendix B. Subtly different from the dissimilarity index, which measures the evenness with which two race and ethnicity groups are distributed across component neighborhoods that make up the county (U.S. Census, 2021b), the diversity index measures the probability of two random draws of residents from a county being from different race and ethnicity groups. As consumers can easily travel within the county to visit restaurants, I choose to use the diversity index. The higher the diversity index, the more diverse the county is in race and ethnicity. I calculate the county diversity index in race and ethnicity using county race and ethnicity compositions from the 2019 5-Year ACS. Appendix Figure A2 panel (b) shows the distribution of the standardized county diversity index.

2 Empirical Strategy

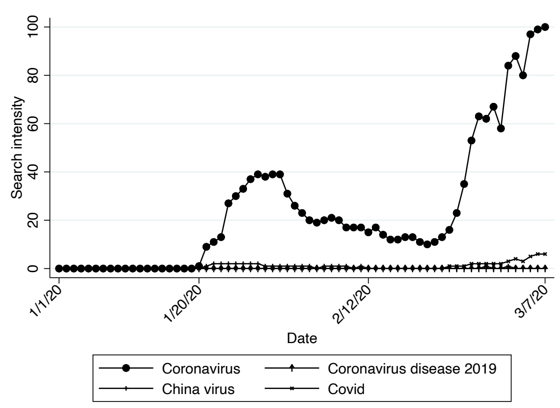
2.1 The National First Case as the Exogenous Shock

The selection of the reference week is pivotal to accurately capturing the impact of Covid-19 on restaurant visits. The first confirmed Covid-19 case in the U.S. was reported on January 20^{th}, 2020, in Washington state. Subsequently, five states reported their first cases in January and eight in February, with the remaining 37 states reporting in March. Notably, the early cases until late February were primarily among individuals who had traveled from abroad and were quarantined upon their return to the U.S. The Centers for Disease Control and Prevention only acknowledged community spread towards the end of February, raising questions about public awareness and concern following the initial cases.

To gauge public awareness, I analyzed Google Trends data for Covid-related search terms from January 1^{st} to March 7^{th}, 2020. As depicted in Figure 2, the search intensity for "Coronavirus" surged immediately after the first U.S. case, reaching about 40% of the peak search intensity by early March (normalized as score 100), when 22 states and Washington, D.C. reported their first cases. This trend suggests that the national first case triggered significant public concern, making it a potential exogenous shock for this natural experiment. Thus, the week before the first national case is selected as the reference week in this study.

An alternative reference week could be the week before the week of March 16^{th}, 2020, when former President Donald Trump first tweeted the term "Chinese Virus." However, I opt for the week before the first national case for two reasons. First, Google Trends data indicate public awareness of the virus well before the President's tweet. Selecting the week before the week of March 16^{th} could introduce an anticipation effect, failing to capture the early impact of consumer discrimination against Chinese cuisine.[7] Second, the week of March 16^{th} was surrounded by several significant events, including the national emergency declaration on March 13rd and various state lockdowns. These concurrent events make it challenging to isolate the impact of the President's tweet from other factors. Therefore, the week before the national first case offers a clearer and earlier reference for the dynamics of consumer behavior in response to the pandemic.

Figure 2. Google Trend: Attention of the U.S. Public to the Covid Shock



This figure shows the daily search intensity of four terms that were closely associated with Covid. The x-axis of the right panel denotes the days from January 1^{st}, 2020 to March 7^{th} 2020. The y-axis denotes the relative search intensity, and the value 100 is assigned to the day with the highest search intensity over the displayed period.

2.2 Baseline Specification

To estimate the causal effect of early Covid events led by the national first case in the fourth week of 2020 on cuisines' visits relative to American and European cuisines, I compare the visits of a cuisine to American and European cuisine visits before and after the national

[7] News and anecdotal evidence suggest that Chinese cuisine suffered from consumer discrimination as early as the late January(e.g. The Washington Post, 2020).

first case. This causal effect is an approximation to the effect of consumer discrimination toward Chinese cuisine[8]. The identification assumption is that the arrival of the national first case is conditionally uncorrelated with other time-varying factors that may cause the cuisine-specific visits changes.

The primary estimation method is Poisson pseudo-maximum likelihood method (Correia et al., 2020).[9] There are a few advantages of using this estimation method. First, the coefficient has an interpretation of semi-elasticity similar to the coefficient using ordinary least squares (OLS) with a logarithm transformation of the dependent variable. Moreover, since there is a small portion of zeros in the dependent variable, the Poisson pseudo-maximum likelihood method explicitly handles these zeros. Second, the Poisson coefficients represent the natural log of the conditional mean of the dependent variable, rather than the conditional mean of the natural log of the dependent variable, which is what is estimated by OLS with log-transformed dependent variable and may introduce bias by violating Jensen's inequality (Santos Silva and Tenreyro, 2006). Lastly, the Poisson pseudo-maximum likelihood method explicitly handles the case in which the dependent variable is a count variable like restaurant visit.

My baseline specification is:

$$Y_{i,g,c,t} = exp\{\sum_{t=-4,t\neq 3}^{t=17} \beta_{t,g} 1(g) D_t + \lambda_{c,t} * type_i + \lambda_i\}\varepsilon_{i,g,c,t} \tag{1}$$

[8]One thing to note is that, although there was a movement advocating support of Chinese restaurants to reverse the decline in patronage, depending on the relative strength, the discrimination-based declines in patronage could potentially lead to the exit of otherwise productive firms, making this phenomenon counter to both efficiency and justice. The model that I estimate in this study is likely to pick up the effect of the consumer discrimination toward Chinese cuisine, net of the effect of the counteraction by the support movement. This net effect is relevant to the survival of the affected small businesses and is the policy-relevant parameter that policymakers would be interested in when considering policies to stabilize the affected social groups.

[9]In situations where the dependent variable deviates from a count variable, such as a binary variable indicating restaurant closure or online platform usage, a linear probability model will be employed for estimation. Additionally, for analyses involving restaurant revenue as the dependent variable, an ordinary least squares (OLS) model will be used, with the dependent variable undergoing an inverse hyperbolic sine transformation.

where the dependent variable $Y_{i,g,c,t}$ is the weekly visits of individual restaurant i of cuisine g located in county c in week t. D_t is the week indicator for calendar weeks from December 2^{nd} 2019 to April 26^{th} 2020 where {-4, ...,-1} denotes the last four calendar weeks of 2019, and {1,2,..,17} denotes the first 17 calendar weeks of 2020, and the reference period $t = 3$ is the week before the week when the national first case occurred. $1(g)$ is the cuisine indicator. $type_i$ is an indicator variable that is 1 if the restaurant i's average median dwelling time in 2019 was below 30 minutes (i.e. a mainly takeout restaurant as defined), and 0 otherwise (i.e. a mainly sit-down restaurant). $\lambda_{c,t} * type_i$ is a vector of county-by-week fixed effects, which differ by the restaurant type, to flexibly control for the time-varying county-specific macroeconomic changes and Covid-related changes that may trigger customers to substitute among cuisines, while allowing the possibility that a mainly takeout restaurant has a different trend compared to a mainly sit-down restaurant. Furthermore, the vector of λ_i contains individual restaurant fixed effect to control for each restaurant's time-invariant characteristics that would influence visits. Conditional on this extensive set of controls, my application relies on the cuisine-by-week variation in visits. The coefficients of interest are $\beta_{t,g}$'s identifying the causal effect of early pandemic events led by the national first case on the cuisine g visits relative to American and European cuisine visits. $\varepsilon_{i,g,c,t}$ is an idiosyncratic error. I cluster all standard errors at the state level to allow arbitrary correlation of errors within a state. To the extent that many Covid policies during the period of interest were state-wide and that the state level is the highest possible level of clustering in the context, clustering all standard errors at the state level may provide more conservative confidence intervals.

When using the SafeGraph restaurant revenue panel as a supplementary dataset, the frequency of observation for individual restaurants will switch to be monthly. The main dependent variables are revenue metrics including total monthly revenue, total monthly number of transactions, etc.

I do not weight this regression by county population. Population-weighting aims to give more weights to highly populated counties, and less weights to sparsely populated counties. Because the regression uses observations of *individual* restaurants' weekly visits, the regression is implicitly weighted by the number of restaurants each county has in the panel. However, county population in the 2019 5-Year ACS is almost perfectly correlated with the number of restaurants each county has in the panel, where the correlation coefficient is 0.99. Therefore, I do not weight the regression by county population.

2.3 Accessing Relative Roles of Supply-Side and Demand-Side Factors

Understanding the observed relative decline in visits to Chinese cuisine restaurants, and its role as an approximation of consumer discrimination, necessitates an examination of both supply-side and demand-side factors. On one hand, it is important to gauge the role of supply-side factors before attributing these declines to consumer discrimination, a primarily demand-side phenomenon. On the other hand, an assessment of various demand-side factors can illuminate the underlying mechanisms deriving the consumer discrimination. Disentangling the relative roles of supply-side and demand-side factors is challenging, in part due to data limitation. For instance, I do not directly observe certain supply-side information including restaurant closure, which requires inference using visit data, and I do not observe consumer demographics. Nevertheless, I provide a series of analyses to shed light on the relative importance of the supply-side and demand-side factors.

On the supply side, I examine two key factors that might explain changes in restaurant visit patterns. Firstly, one possibility is that the decrease in visits to Chinese cuisine might be linked to a higher incidence of business closures for Chinese restaurants, restricting consumer access. Such closures could be voluntary, driven by the restaurant owners, or involuntary, a consequence of reduced consumer demand. Such closures could be voluntary, driven by the restaurant owners, or a consequence of reduced consumer demand. The relative higher rate of the voluntary closure for Chinese cuisine may mask the effects of consumer discrimination. The analysis aims to provide an upper bound estimate of voluntary closures during the onset of the Covid pandemic, thus offering insight into the extent of voluntary restaurant closures as a factor in the decline of Chinese cuisine visits.

To infer business closures, I define restaurant closure in a month as a month with all zero visit weeks for the restaurant, as previously discussed in Section 1. This approach mitigates the impact of SafeGraph's 10% sampling on interpreting occasional zero visit weeks. This inferred variable is likely to pick up both types of restaurant closure thus may overestimate the voluntary restaurant closures initiated by the owners, so the estimated likelihood of voluntary closure by cuisine may overestimate the true likelihood. The dependent variable in Equation 1 will be replaced with a binary variable, *closure*, indicating whether a restaurant experienced a closure. A linear probability model will be applied for estimation.

Secondly, the other key consideration for the supply side factors is whether Chinese restaurants transitioned to delivery mode more rapidly than American and European cuisines at the pandemic's onset, potentially resulting in fewer physical customer visits. Although traditional phone orders followed by on-site pickups could still be captured by SafeGraph's visit data (provided the customer's signal remained on-site for over four minutes), the use of online delivery platforms often involves drivers collecting multiple orders simultaneously, which might not be recorded as individual visits.

To explore the rate of transition to online platforms across different cuisines, I turn to the SafeGraph spending data, which distinctly tracks transactions conducted through these platforms before aggregation. This data allows me to test the hypothesis regarding differential adoption rates of online platforms. A restaurant's usage of an online platform is inferred based on whether it recorded positive online revenue for the month. Analyzing this aspect will help understanding if a faster move to online platforms by Chinese restaurants contributed to the observed decline in physical visits, compared to their American and European counterparts.

On the demand side, my analysis focuses on two main areas: examining the spillover effects of consumer discrimination on various cuisines, and conducting heterogeneity analyses based on county social characteristics.

Firstly, I explore the spillover effect of consumer discrimination beyond Chinese cuisine. As outlined in Section 1, not aggregating all non-Chinese Asian cuisines allows for a more nuanced comparison. I pay particular attention to two East Asian cuisines — Japanese and Korean cuisines — due to their geographical and cultural proximity to Chinese cuisine and popularity in the U.S. Additionally, a popular South Asian cuisine — Indian cuisine — is considered; while it is also an Asian cuisine, it is not as closely related to Chinese cuisine in terms of culinary traditions and cultural perceptions as Japanese and Korean cuisines. Lastly, Mexican and other Latin cuisines, which predominantly include takeout-heavy Mexican restaurants and are not closely associated with Chinese cuisine. This comparison will illuminate how consumer discrimination might differentially affect these groups. However, at the same time, it's crucial to consider the potential differences in supply-side factors like restaurant closure rates and online platform usage among these cuisines in the discussion below.

Secondly, I conduct a series of heterogeneity analyses along the county social characteristics including county political affiliation, county Asian population ratio, county racial and ethnic diversity. For the heterogeneity analyses, my approach differs based on whether the variable in question has a continuous or non-continuous distribution. In cases where the variable has a non-continuous distribution, I will split the sample for separate analyses. Conversely, for variables with a continuous distribution, I perform the heterogeneity analyses using the following specification:

$$Y_{i,g,c,t} = exp\{\sum_{t=-4,t\neq 3}^{t=17} \beta_{1,t,g} 1(g) D_t + \sum_{t=-4,t\neq 3}^{t=17} \beta_{2,t,g} 1(g) D_t Z_c + \lambda_{c,t} * type_i + \lambda_i\} \varepsilon_{i,g,c,t} \tag{2}$$

where I normalize the continuous social characteristics to be mean zero and unit standard deviation and interact the standardized continuous social characteristics Z_c with the leads and lags D_t. In other words, the model differs from Equation 1 by the additional interactions between D_t and Z_c. The set of $\beta_{1,t,g}$'s measures the causal effect on cuisine g visits relative to the base group in the *average* counties (the counties with $Z_c = 0$) on the spectrum of the standardized social characteristics, and the set of $\beta_{2,t,g}$'s measures the *marginal* effect of a "one-standard-deviation" increase in the standardized county social characteristics variable on cuisine g visits.

3 Results

3.1 Results on Restaurant Visits

I first estimate the causal effect of early Covid events led by the national first case on Chinese cuisine visits relative to American and European cuisines, as an approximation to the effect of consumer discrimination toward Chinese cuisine. In Figure 3, I plot the event study coefficients of interest in the baseline Equation 1 for each cuisine. I also highlighted the simple DiD estimate along with the standard error for each cuisine. On all x-axes, {-4,...,-1} denotes the last four calendar weeks of 2019, and {1,2,..,17} denotes the first 17 calendar weeks of 2020. The corresponding calendar months and important events are highlighted above x-axes. The dashed vertical line highlights the reference period, which is the the week before the week of the national first case.

The onset of the Covid pandemic led to differential impacts on consumer visits across ethnic cuisines, relative to American and European cuisines. Notably, in panel (a), Chinese cuisine experienced a significant early relative decline in visit. A difference-in-differences (DiD) analysis reveals a statistically significant -4.27% reduction in visits (standard errors of 1.12%) over the sample period. Prior to the national first case, the trend of visit for Chinese cuisine largely mirrored those of the reference group, apart from a notable surge during Christmas week 2019. Given the popularity of Chinese and other Asian takeouts during the Christmas season, it reassures the accuracy of the cuisine classification in the study.

Following the national first case, Chinese cuisine uniquely experienced early relative declines in visits, with a relative 4.23% drop two weeks after the first case and intensifying to about -8% by March. An intriguing temporary recovery for Chinese cuisine visit occurred in the 12th week of 2020, coinciding with 40 states' restaurant restriction policies (Fullman et al., 2021). This rebound of Chinese cuisine visit was likely due to a more pronounced drop in American and European cuisine visits during that week due to the restaurant restrictions and subsequent pandemic-related measures. As illustrated in Figure 1 panel (a), the reference group experienced a large drop in visits on the 12th week and remained at a lower level afterwards. In Appendix Figure A3, I split the sample based on whether states had imposed a lockdown policy (which usually contains restaurant restrictions) and estimate Equation 1 separately, and it reveals that Chinese cuisine visits in non-lockdown states did not had the transitory recovery and declined much more compared to those in lockdown states,[10] supporting that the temporary relative recovery of Chinese cuisine visit was because of a larger decline in reference group due to the lockdown policies. Nonetheless, after this ephemeral uptick, Chinese cuisine visits resumed the relative decline and stabilizing at around -10% relative to the reference group in subsequent weeks.

In terms of other cuisines, Figure 3 panel (b) to (e) reveals distinct patterns. First, while Japanese and Korean cuisines didn't show the immediate relative declines seen in Chinese cuisine, their visits dropped significantly starting in late March, averaging around -14% relatively, surpassing the -10% relative decline observed for Chinese cuisine in April. One explanation for this spillover effect may be attributed to the fact that Japanese and Korean cuisines may have more sit-down restaurants than Chinese cuisine, but the baseline specification has taken into account for the differential time trends for sit-down versus takeout restaurants. As discussed below, supply-side factors like voluntary restaurant closure and online platform transition speed cannot fully explain this spillover effect, either. Another explanation is that this larger spillover effect could be attributed to a combination of two factors: consumers' misclassification of Japanese and Korean restaurants as Chinese restaurants, and the unintended effect of the support-Chinese-restaurant movement since March

[10]During the sample period, there were 7 states that never imposed a lockdown policy, and most other states imposed the policy within a span of three weeks.

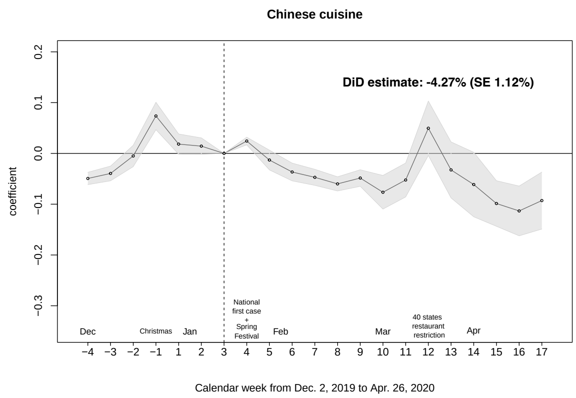
2020. On one hand, as indicated in Huang et al. (2023)'s restaurant misclassification survey, there is a tendency for U.S. consumers to erroneously categorize Korean restaurants as Chinese, which partly explains the decline in consumer visits for Japanese and Korean restaurants relative to the reference cuisine; On the other hand, as the group of progressive consumers rallied to reverse the decline in Chinese cuisine patronage, Japanese and Korean cuisines may have been overlooked, assuming these consumers correctly distinguished between the cuisines. This may in part explain why the spillover effect is larger than that of Chinese cuisine. However, this view should be read with a great deal of cautions because the early stages of the pandemic were marked by a plethora of concurrent events and changes, making it challenging to pinpoint the exact reasons for the larger relative decline in visits to Japanese and Korean cuisines.

Indian cuisine, interestingly, appears to have experienced a relative increase in visits. However, this trend is preceded by a pre-existing upward trend in visits relative to the reference group, which complicates the interpretation. Moreover, Asian fusion and other Asian cuisines show a marginal decline in visits with a DiD estimate of -0.97%. Notably, Mexican and other Latin cuisines, encompassing predominantly Mexican restaurants in this study, did not exhibit any significant relative decline in consumer visits throughout the observed period.

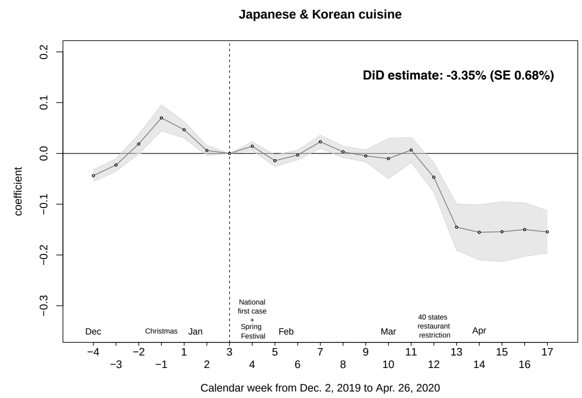
Three key insights can be drawn from comparing the consumer visit patterns across cuisines. First, Chinese cuisine indeed faced a significant and immediate relative decrease in visits following the national first case, confirming anecdotal evidence of a downturn. Second, differing from Huang et al. (2023) who do not distinguish between sub-Asian groups, my analyses suggest that the spillover effect appears to be concentrated in non-Chinese **East Asian** cuisines, rather than across all non-Chinese Asian cuisines claimed by Huang et al. (2023). This pattern may be attributed to the culinary closeness of these cuisines to Chinese cuisine, or simply the geographical and cultural proximity of Japan and Korea to China. This contrasts with Indian cuisine, a prominent South Asian cuisine, and Mexican and other Latin cuisines, which did not show significant relative declines in consumer visits, suggesting a more localized impact within the category of East Asian cuisines. Third, the concern that the specialization of Chinese cuisine in takeout might bias the estimates seems less significant. This is inferred from the observation that Mexican and other Latin cuisines, which anecdotally also feature heavily takeout services, did not experience a relative decrease in visits. This indicates that the ex-ante specialization in takeout business may not fully explain the observed trends in Chinese cuisine visit.

Figure 3. Baseline estimated effects of consumer discrimination on cuisine visits

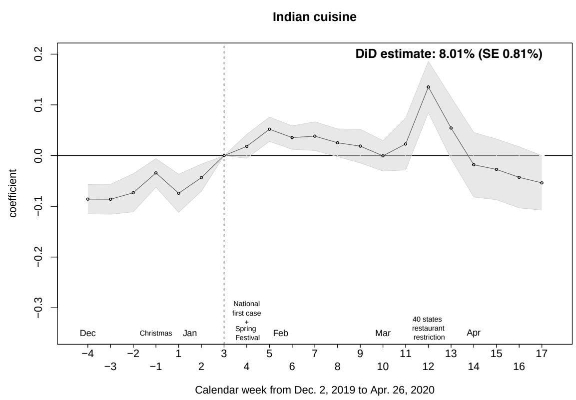
(a)



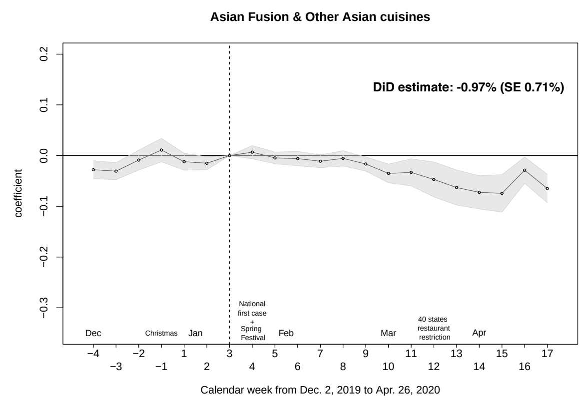
(b)



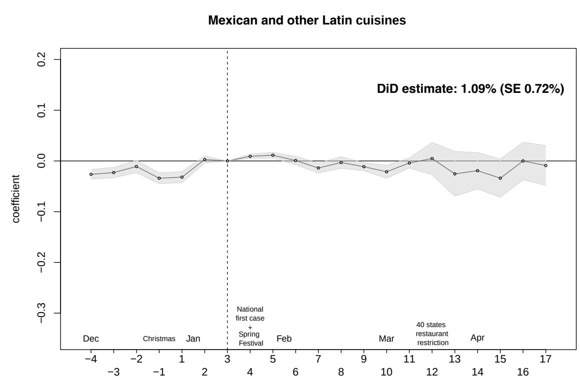
(c)



(d)



(e)



This figure shows baseline estimated effects of consumer discrimination on cuisine visits. Each panel shows the coefficients $\beta_{t,g}$ of the corresponding cuisine in Equation 1. The dotted line indicates the reference period. On all x-axes, {-4,...,-1} denotes the last four calendar weeks of 2019, and {1,2,..,17} denotes the first 17 calendar weeks of 2020. The corresponding calendar months and important events are denoted above the x-axis. The grey area shows the 95% confidence intervals constructed using standard errors clustered at the state level. The simple DiD estimates with the corresponding standard errors are shown in the graph.

Last but not the least, I show how the baseline DiD estimates change when gradually adding various set of control variables in Table 1 and compare my estimates to those reported in Huang et al. (2023). Notably, my estimates with the full set of control variables are about 30% of the magnitude of Huang et al. (2023)'s estimates. This discrepancy highlights the importance of accounting for different time trend across U.S. counties over the sample period and the differential impacts based on restaurant types (takeout vs. sit-down).

Before delving into the specifics of Table 1, two clarifications are pertinent. Firstly, since Huang et al. (2023) categorizes Asian restaurants into two broad groups (Chinese and non-Chinese), the most meaningful comparison here is with respect to Chinese cuisine. Secondly, to align with Huang et al. (2023)'s reference week (the week before the national emergency), Panel A of Table 1 adjusts accordingly. This adjustment explains why estimates in Panel A are generally larger than those in Panel B, which uses the week before the national first case as the reference. This is because Panel A's reference week is set later than that in Panel B, which mechanically put more weight on larger declines observed in the event study graphs in March and April.

In Panel A, Column (1) mirrors Huang et al. (2023)'s control variables - week fixed effects, restaurant fixed effects, and local weekly Covid case rates. The estimate for Chinese cuisine visit, -10.84%, closely matches Huang et al. (2023)'s estimate of -11.5%. Next, as different counties faced diverse macroeconomic and pandemic-related challenges, I introduce county-week fixed effects in Column (2), reducing the estimate to -6.62%, a 38.93% decrease. Further, Column (3) differentiates the county-week fixed effects for mainly takeout restaurants, the estimate for Chinese cuisine visits drops to -3.55%, a 67.25% reduction from Column (1). Intriguingly, with the full set of control variables in Panel A, Column (3), the DiD estimate for Chinese cuisine becomes statistically insignificant. This is because Chinese cuisine visit has already declined before early March, which reinforces the appropriateness of using the week of the national first case as the reference week, avoiding the biases from anticipatory effects not captured when using the week before the national emergency.

Table 1. DiD Estimates with Various Sets of Control Variables

	(1)	(2)	(3)
Panel A. Reference Week: the Week before National Emergency (March 13, 2020)			
Chinese	-10.84%	-6.62%	-3.55%
	(3.11%)	(3.15%)	(2.08%)
Japanese & Korean	-27.72%	-20.29%	-9.32%
	(2.30%)	(2.67%)	(1.83%)
Indian	-19.19%	-5.99%	3.06%
	(3.11%)	(2.98%)	(1.95%)
Asian Fusion & Other Asian	-17.88%	-11.21%	-3.98%
	(1.95%)	(1.91%)	(1.05%)
Mexican & Other Latin	-3.97%	-3.31%	-0.03%
	(2.24%)	(2.63%)	(1.51%)
Week fixed effects	Yes	No	No
Restaurant fixed effects	Yes	Yes	Yes
Weekly Covid case rate	Yes	No	No
County-Week fixed effects	No	Yes	No
County-Week-Takeout fixed effects	No	No	Yes
Observations	12,321,344	12,321,283	12,321,162
Panel B. Reference Week: the Week before National First Case (January 20, 2020)			
Chinese	-6.67%	-4.95%	-4.27%
	(1.57%)	(1.41%)	(1.12%)
Japanese & Korean	-9.61%	-5.60%	-3.35%
	(0.78%)	(0.90%)	(0.68%)
Indian	-1.17%	6.27%	8.01%
	(0.60%)	(1.02%)	(0.81%)
Asian Fusion & Other Asian	-5.87%	-2.47%	-0.97%
	(1.03%)	(0.83%)	(0.71%)
Mexican & Other Latin	-0.69%	0.29%	1.09%
	(0.77%)	(1.01%)	(0.72%)
Week fixed effects	Yes	No	No
Restaurant fixed effects	Yes	Yes	Yes
Weekly Covid case rate	Yes	No	No
County-Week fixed effects	No	Yes	No
County-Week-Takeout fixed effects	No	No	Yes
Observations	12,321,344	12,321,283	12,321,162

Note: this table displays the DiD estimates when adding various sets of control variables. Panel A and Panel B utilizes different reference week. Penal A following Huang et al. (2023) uses the week before the national emergency declaration, and Panel B uses the week before the national first case as the reference week. Column 1 utilizes control variables used in Huang et al. (2023), while column 3 utilizes the control variables used in this paper. Standard errors are clustered at the state level and shown in the parentheses under the estimates.

3.1.1 Robustness

I performed three robustness checks for the baseline results in consumer visits. First, there may be differences in visits among cuisines for reasons unrelated to the arrival of the virus. For example, the relative surge in Chinese cuisine visits during the Christmas week substantiates the problem of seasonality. This observation raises the question of whether Chinese cuisine visits would have declined in the absence of the Covid pandemic. To account for seasonality, I use data from the nearby pre-pandemic year (i.e., the last 4 calendar weeks of year 2018 and the first 17 calendar weeks of year 2019[11]) to estimate the difference in visits among cuisines and subtract this from the estimates of the effect obtained using the 2019-2020 data. The result is reported in Figure 4. Reassuringly, after differencing out the estimates obtained using the 2018-2019 data, the pre-trends observed for Chinese cuisine and Japanese and Korean cuisines in Figure 3 are largely eliminated, especially in Christmas week, but the effects during the post-treatment period are very similar to those in Figure 3. Therefore, the relative declines observed are not driven by the cuisine-specific seasonality.

Second, as discussed in Section 1, I redo the baseline analysis after dropping restaurants that are enclosed in a bigger polygon in SafeGraph data, as measurement error may be of a serious concern in these cases. Such restaurants encompass 2.38% of all restaurants in the constructed panel. Appendix Figure A4 shows that dropping these restaurants do not change my baseline results.

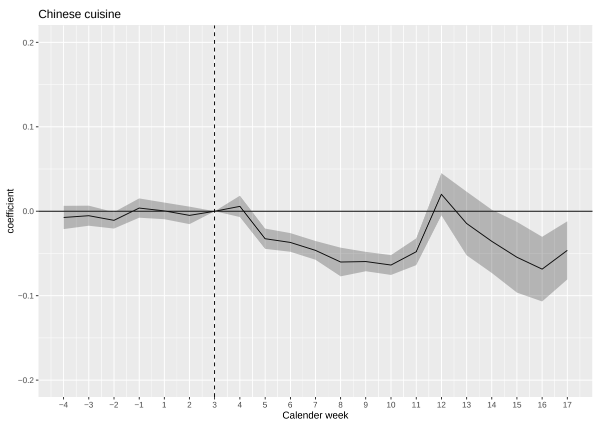
Third, despite the fine-tuned polygon size in SafeGraph data and efforts to filter out residents and workers in the same census block group, concerns remains about the measurement error in dense urban areas. To put this into perspectives, I split the sample by USDA county rural-urban status.[12] Appendix Figure A5 shows that the baseline effect is larger in magnitude for rural areas relative to urban areas. If naively assuming the difference in the effect between rural and urban areas is solely due to measurement errors, it infers that the measurement error would attenuate the effect towards zero. Of course, this heterogeneity analysis picks up the effect due to a bundle of differential factors across rural and urban areas.

[11]SafeGraph only provides data starting 2018, so the only available data from the nearby pre-pandemic years is the 2018-2019 data

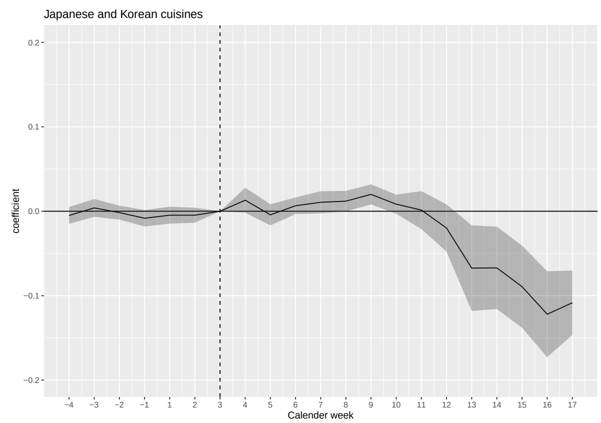
[12]USDA's rural-urban continuum codes classify counties into nine groups. Commonly, literature simplifies this into a binary categorization: metropolitan (top three groups) and non-metropolitan (bottom six groups) counties. My analysis follows this conventional approach.

Figure 4. Estimated effects of consumer discrimination on cuisine visits: using both 2018-2019 and 2019-2020 data to account for seasonality

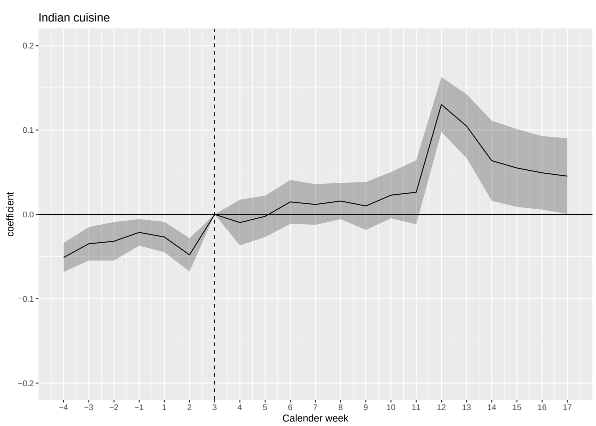
(a) Chinese cuisine



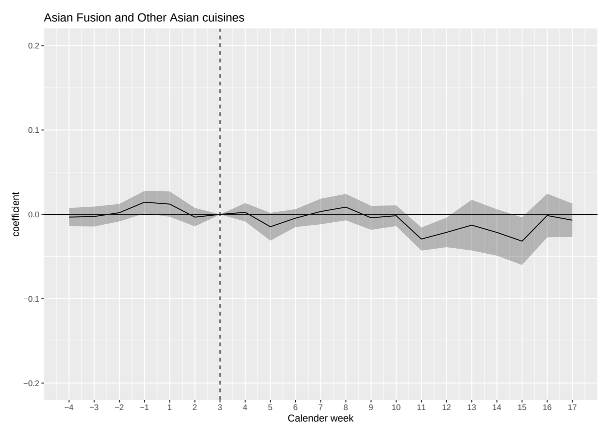
(b) Japanese and Korean cuisines



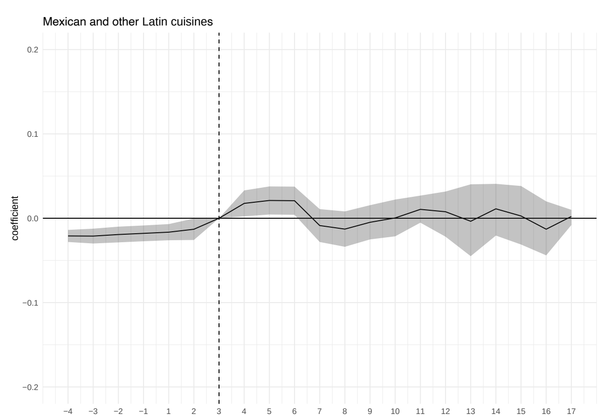
(c) Indian cuisine



(d) Asian Fusion and Other Asian cuisines



(e) Mexican and other Latin cuisines



This figure shows the estimated effects of consumer discrimination on cuisine visits, using both 2018-2019 and 2019-2020 data to account for seasonality. Each panel shows the calculation results of the estimates obtained using the 2019-2020 data minus the estimates obtained using the 2018-2019 data. The dotted line indicates the reference period. On all x-axes, {-4,...,-1} denotes the last four calendar weeks of 2019, and {1,2,..,17} denotes the first 17 calendar weeks of 2020. The grey area shows the 95% confidence intervals constructed using standard errors clustered at the state level.

3.2 Results on Restaurant Revenue

As discussed in Section 1, a major limitation of the restaurant visit panel is its inability to measure restaurant revenue, despite its comprehensive coverage of the U.S. restaurant sector. To address this gap and adding a monetary dimension in my analysis, I leverage on SafeGraph's newly introduced spending data. This data, sourced from debit and credit transactions by a large financial company, aggregates monthly revenue for individual restaurants.

Before presenting the results, it's important to note that the SafeGraph spending data covers only about 40% of the restaurants in the visit panel. To ensure consistency, I replicated the baseline visit results with this subset, as shown in Appendix Figure A6. The similarities between these results and those in Figure 3 confirm the reliability of using this smaller sample to gain insights into the pandemic's impact on restaurant revenues.

Overall, Figure 5, Panel A, illustrates that the pattern in monthly restaurant revenue by cuisines mirrors that of restaurant visits in the previous section. January 2020 serves as the reference month, aligning with the visit analysis. Chinese cuisine shows an early revenue decline in February (approximately -13%) compared to American and European cuisines, which intensified to -28% in March before rebounding to around -10%. The rebound is likely due to the reference group revenue dropping substantially in April when the pandemic-related measures came in place, as depicted in Figure 1 panel (b), rather than Chinese cuisine revenue's actual recovery. This is consistent with the rebound pattern in restaurant visit described in the section above. Next, Japanese and Korean cuisines did not exhibit an economically meaningful change in revenue initially but experienced a notable decrease from March onwards, with April seeing a relative drop of about -27%, surpassing the relative decline in Chinese cuisine. Other cuisines display a marginal relative revenue decline in March and April, but to a lesser extent than Chinese, Japanese, and Korean cuisines. Additionally, as shown in Panel B, the trends in monthly transaction volume closely reflect those of monthly revenue across different cuisines.

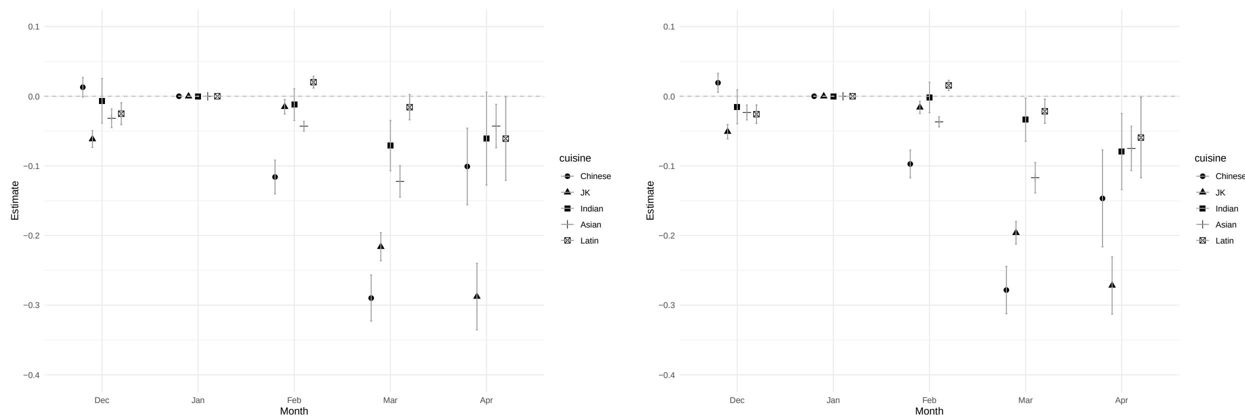
Utilizing spending data provides an advantage in directly quantifying the economic impact of consumer discrimination on specific cuisines. I performed a back-of-the-envelope calculation for Chinese cuisine using the difference-in-differences estimate and information in SafeGraph Spending data. Setting December 2019 and January 2020 as the pre-period, the simple DiD estimate gives a relatively -18.54% decline in Chinese cuisine monthly revenue. According to SafeGraph Spending data, Chinese cuisine's total monthly revenue over the pre-period was approximately 69.73 million.[13] A relative -18.54% reduction translates to a monthly revenue loss of around 11.8 million.[14] Consequently, from February to April, this calculation suggests a cumulative revenue loss of about 35.4 million for Chinese cuisine. Similarly, applying this method to Japanese and Korean cuisines indicates an estimated revenue loss of 43.6 million over the same period. One thing to note that those calculations are likely to be underestimating the true loss in revenue, as the SafeGraph spending data only utilizes debit and credit card transactions from one financial company.

[13] This number takes into account the fact that spending data only cover about 40% of all restaurants in the visit data, so I scale up the total monthly revenue to match the sample size in the visit data.

[14] This -18.54% relative change, derived from an OLS model with an inverse hyperbolic sine transformation of the dependent variable, equates to roughly -16.92% in linear terms.

Figure 5. Baseline estimated effects of consumer discrimination on restaurant revenue

(a) Monthly total revenue by cuisine (b) Monthly total number of transactions by cuisine



This figure shows baseline estimated effects of consumer discrimination on restaurant revenue and transaction volume. The spending data is of monthly frequency. For monthly total number of transactions, the model Equation 1 is estimated using poisson pseudo-maximum likelihood method. For monthly total revenue, the model is estimated using OLS, with the dependent variable undergoing an inverse hyperbolic sine transformation. Each panel shows the coefficients $\beta_{t,g}$ of the corresponding cuisine in Equation 1, with 95% confidence intervals constructed using standard errors clustered at the state level.

3.3 Results on Relative Roles of Supply-Side and Demand-Side Factors

Understanding the relative decrease in visits to Chinese cuisine restaurants, as a potential indicator of consumer discrimination, requires a thorough examination of both supply-side and demand-side factors. Assessing supply-side factors is key in determining whether these declines can indeed be attributed to consumer discrimination, primarily a demand-side issue. Concurrently, exploring various demand-side elements will shed light on the underlying mechanisms driving this discrimination.

3.3.1 Supply-Side Factors

I consider two important supply-side factors and their contribution to the observed relative declines in Chinese cuisine.

Voluntary Business Closure As discussed in Section 2, unlike demand-induced restaurant closure, voluntary closures initiated by the owners is particularly concerning in masking the identification of consumer discrimination. To address this, I utilize the inferred restaurant closure variable, as detailed in Section 2, as the dependent variable in Equation 1. Because this measure may pick up both voluntary and consumer-induced restaurant closure, this approach aims to provide an upper bound estimate of voluntary closures during the onset of the Covid pandemic.

Figure 6, Panel A shows the likelihood of restaurant closure by cuisine relative to American and European cuisines, with January 2020 as the reference month. The findings indicate that Chinese cuisine was not significantly more likely to close than the reference group throughout the sample period. Even in April, when the likelihood of closure for Chinese cuisine slightly increased (about 0.25 percentage point more likely), the estimate was statistically insignificant and considerably smaller than the observed decline in visits. Given that this measure likely overstates voluntary closures, it suggests that voluntary business closure is unlikely to explain the estimated declines in Chinese cuisine visits.

Additionally, as the analysis on the likelihood of business closure can be viewed as the effect on the extensive margin on restaurant visit, I also provide the estimated baseline effect on restaurant visits on the intensive margin (though this is not a sole supply-side aspect) by dropping all zero visits in the data (about 2% of all observations). Appendix Figure A7 shows similar results compared to Figure 3. Thus, the results of restaurant visits on both extensive margin and intensive margin suggest that the effect of consumer discrimination is concentrated on the intensive margin.

Online Delivery Platform Usage The other analysis on supply-side factors aims to determine whether Chinese restaurants transitioned more rapidly to online delivery platforms than American and European cuisines, potentially impacting physical visit counts. Using SafeGraph spending data, I inferred a restaurant's online platform usage from its positive online revenue in a given month. The results in Figure 6, Panel B indicate that Chinese restaurants did not significantly shift to online platforms earlier than their American and

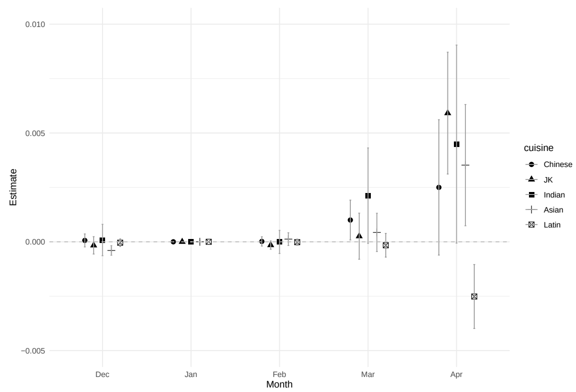
European counterparts. Though the magnitude is small, the estimate for Chinese cuisine suggests that Chinese restaurants were about 2.5% percentage points less likely to use online platforms in March but caught up in April.[15] This finding suggests that the observed decline in physical visits to Chinese restaurants is not primarily due to an accelerated adoption of online delivery services, reinforcing the likelihood that other factors, such as consumer behavior changes, played a more substantial role.

As the likelihood of online platform usage is about the extensive margin of online business performance for restaurants, I also conducted an analysis on the intensive margin (though this is not a sole supply-side aspect) using two variables provided by SafeGraph spending data — monthly online revenue and monthly online number of transactions. The results are shown in Figure 6, Panel C and Panel D. Notably, although the patterns for monthly online revenue and transactions observed here are similar to those for total monthly revenue and transactions shown in Figure 5, the scale of the changes is considerably smaller. This implies that, compared to in-person transactions, online revenue and transaction volumes experienced a less pronounced decline. Such a trend suggests that while physical visits to restaurants decreased significantly during the pandemic, the impact on online business activities was relatively milder.

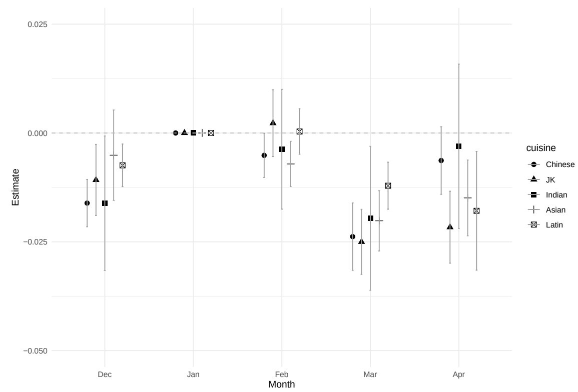
[15]This analysis only speaks to the technology adoption of delivery platform across cuisines. Chinese restaurants may still engage with "call by phone and pick up later" for their takeout services, which may be captured by the visit data, or "self-delivery" service.

Figure 6. Accessing the Contribution of Supply-Side Factors to the Changes in Cuisine Visits

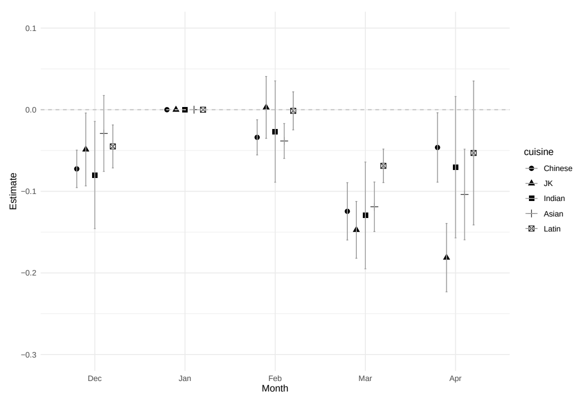
(a) Business closure by cuisine



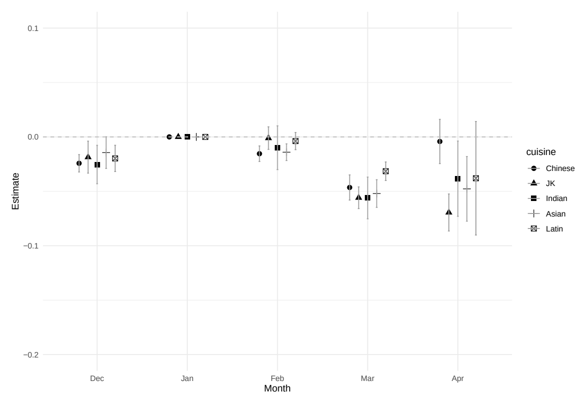
(b) Transition to online platform by cuisine



(c) Monthly online revenue by cuisine



(d) Monthly number of transactions by cuisine



Panel (A) shows the estimated likelihood of business closure by cuisines relative to American and European cuisines. Panel (B) shows the estimated relative likelihood of online platform usage by cuisines. All 95% confidence intervals are constructed using standard errors that are clustered at the state level. Panel (C) shows the estimated effect for monthly online revenue by cuisines. Panel (D) shows the estimated effect for monthly online number of transactions by cuisines.

3.3.2 Demand-Side Factors

Spillover Effect on Non-Chinese East Asian Cuisines Having analyzed the supply-side factors, it appears unlikely that they are the primary drivers of the baseline results observed in consumer visit patterns. Specifically, aspects like voluntary restaurant closures in Figure 6 Panel A and differential adoption of online platforms in Figure 6 Panel B do not fully explain the trends, especially the early decline in visits to Chinese cuisine. This leads us to revisit the significant spillover effect, where non-Chinese East Asian cuisines, notably Japanese and Korean, experienced a marked decline in visits. In contrast, Indian cuisine, a South Asian cuisine, along with Mexican and other Latin cuisines, did not experience similar declines. Given the limited influence of supply-side factors, it appears that this spillover effect is closely tied to demand-side factors.

Supporting this notion is Huang et al. (2023)'s survey findings on restaurant misclassification, indicating a propensity among U.S. consumers to mistakenly identify different Asian cuisines, such as misclassifying Korean as Chinese restaurants. This confusion could contribute to the heightened spillover effect observed in East Asian cuisines, underscoring the role of consumer perceptions and potential biases. The resilience of Indian, Mexican, and other Latin cuisines further highlights the specificity of this spillover effect, reinforcing the idea that changes in consumer behavior played a significant role in shaping the visit patterns across these diverse culinary categories.

County-level heterogeneity in the effect To provide further insights in the contribution of demand-side factors in explaining the changes in consumer visits across cuisines. I conducted heterogeneity analyses on the effect of consumer discrimination along three county-level social characteristics. Given that local social characteristics, such as local political affiliation and local racial diversity, could greatly influence and shape human behaviors and outcomes (e.g. Agan and Starr, 2020; Allcott et al., 2020; Ba et al., 2021), it is interesting to examine how the effect on cuisine visit is tied into local social charactersitics across regions. This analysis aims to uncover patterns that might explain the differential effects across regions, further contributing to our understanding of the demand-side influences during the pandemic.

Heterogeneity along County political affiliation In Figure 7, I look at the heterogeneity in the causal effect along the standardized political affiliation measure Z_c. For brevity, I show the results for Chinese and Japanese and Korean cuisine visits here and report the results for other three cuisine groups in Appendix figure A9. I normalize the county political affiliation measure to be mean zero and unit standard deviation due to its continuous distribution and for interpretation convenience.

Panels (a) and (b) of Figure 7 depict two distinct series of estimates. The dot series ($\beta_{1,t,g}$'s in Equation 2) represents the effect on cuisine g visits in average counties (where $Z_c = 0$) on the political affiliation scale. In contrast, the triangle series ($\beta_{2,t,g}$'s) indicates the marginal effect or the 'partisan gap'—how a county being 'one-standard-deviation' more Republican-leaning influences cuisine g visits. It's important to note that this series does not represent the effect in the more Republican counties but rather the marginal change due to increased Republican support relative to the average county. To illustrate the effects in counties that are 'one-standard-deviation' more Republican or Democratic, I use the dot and triangle series in panels (c) and (d), respectively. These are computed by adding (for Republican) or subtracting (for Democratic) the triangle series from the dot series in panel (a) and (b) and adjusting the standard errors accordingly.

There are two main points from Figure 7. First, there were sizeable declines in Chinese cuisine visits in the *average* counties. In panel (a), the dot series indicate that, in the *average* counties, Chinese cuisine experienced early relative declines in visits immediately after the national first case, and the magnitude of the decline accumulated to about -8% by March. As time went by, the relative declines in Chinese cuisine visits continued to amplify to about -20% in the average counties.

Second, there are substantial geographical variations in the causal effect on Chinese cuisine visits tied into county political affiliation.[16] Panel (a) reveals a substantial partisan gap in Chinese cuisine visits, emerging in early March and stabilizing at around -10%. This indicates that counties 'one-standard-deviation' more Republican-leaning experienced an additional 10% relative decline in Chinese cuisine visits. Notably, this trend's emergence coincides with certain public comments by Republican leaders associating the virus with Chinese

[16]According to Appendix Figure A9, Chinese cuisine led in the size of the partisan gaps over the period of interest.

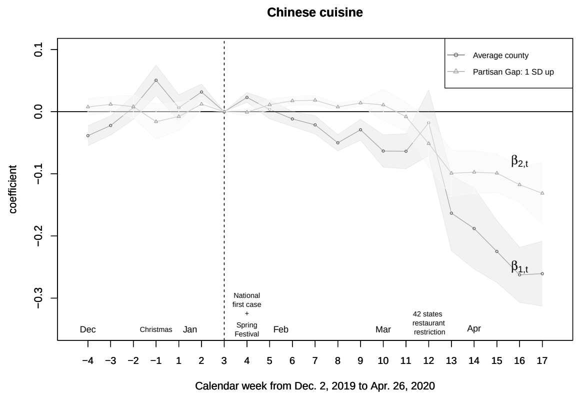
ethnicity (Yam, 2020). Panel (c) contrasts the effects in counties 'one-standard-deviation' more Republican versus Democratic, showing a remarkable difference: while the relative decline in Chinese cuisine visits was around -10% in more Democratic counties, it reached approximately -30% in more Republican counties. Considering the political affiliation distribution's range, this implies a striking contrast from one end of the political spectrum to the other, with Chinese cuisine gaining about 10% more patronage in extremely Democratic counties and facing a -40% relative decline in extremely Republican counties. Lastly, for Japanese and Korean cuisines (Panels b and d), the pattern is similar, with similar relative declines in average counties and a noticeable partisan gap of about -6% starting in March. These results collectively suggest that political affiliation can lead to meaningful differences in consumer behavior and the economic performance of small businesses during public crises, underscoring its significance in the context of the pandemic.

Heterogeneity along County Diversity in Race and Ethnicity In Figure 8, the heterogeneity of the causal effect along the standardized county diversity index in race and ethnicity is examined, focusing on Chinese and Japanese and Korean cuisines, with results for other cuisines presented in Appendix Figure A10. While the geographical variations in response to diversity are less pronounced than those related to political affiliation, they remain significant.

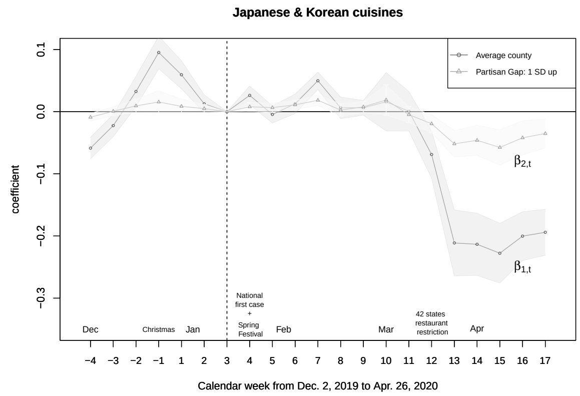
Figure 8 Panel (a) shows the relative declines in Chinese cuisine visits in the *average* counties on the diversity spectrum (dot series). A notable finding is the significant positive diversity gap (triangle series) of about +8% for Chinese cuisine starting mid-March. This suggests that counties 'one-standard-deviation' more diverse were associated with an 8% relative increase in Chinese cuisine visits. Panel (c) further illustrates a 14% gap in relative declines during March between counties 'one-standard-deviation' more and less diverse. Considering the maximum range of the diversity index distribution, the results imply almost no relative declines in extremely diverse counties versus a 27% relative decline in extremely homogeneous counties for Chinese cuisine.

Figure 7. Heterogeneity in the estimated effects of consumer discrimination on Chinese and Japanese and Korean cuisine visits: by county political affiliation

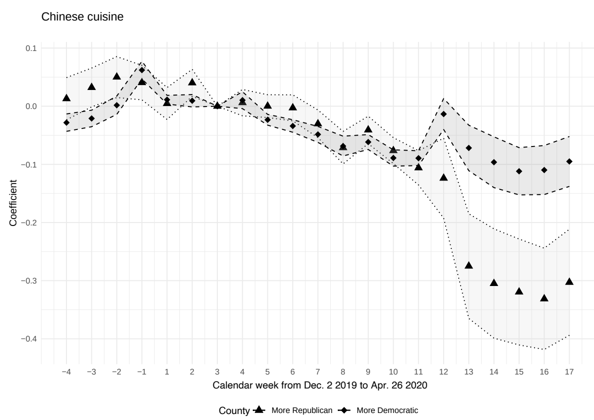
(a)



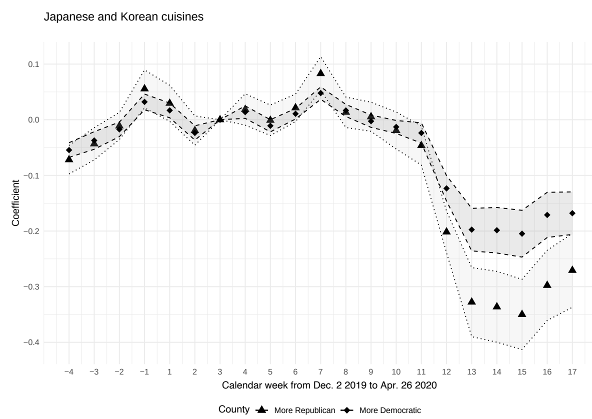
(b)



(c)



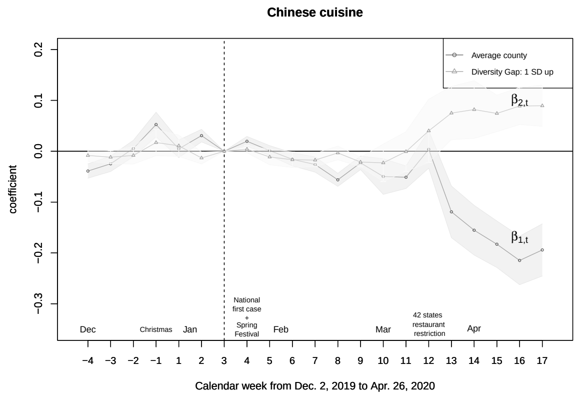
(d)



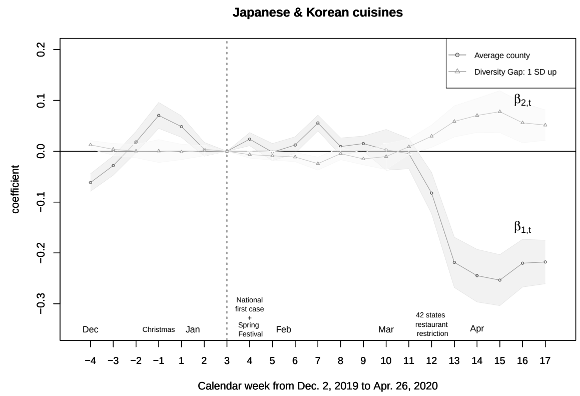
This figure shows the heterogeneity in the estimated effects of consumer discrimination by county political affiliation on Chinese and Japanese and Korean cuisine visits. In panels (a) and (b), the dot and triangle series respectively show the coefficients $\beta_{1,t,g}$'s and $\beta_{2,t,g}$'s of the corresponding cuisine in Equation 2. The triangle series ($\beta_{2,t,g}$'s) are **not** the causal effect in the "one-standard-deviation" more republican supporting counties. In panels (c) and (d), the dot series shows the causal effects for the corresponding cuisine in the "one-standard-deviation" more republican supporting counties by adding up $\beta_{1,t,g}$'s and $\beta_{2,t,g}$'s adjusting the standard errors, and the triangle series show the causal effects in the "one-standard-deviation" more democratic supporting counties by subtracting $\beta_{2,t,g}$'s from $\beta_{1,t,g}$'s adjusting the standard errors. The 95% confidence intervals are constructed using standard errors clustered at the state-level. The results for other three cuisine groups are in Appendix figure A9.

Figure 8. Heterogeneity in the estimated effects of consumer discrimination on Chinese and Japanese and Korean cuisine visits: by county diversity index in race and ethnicity

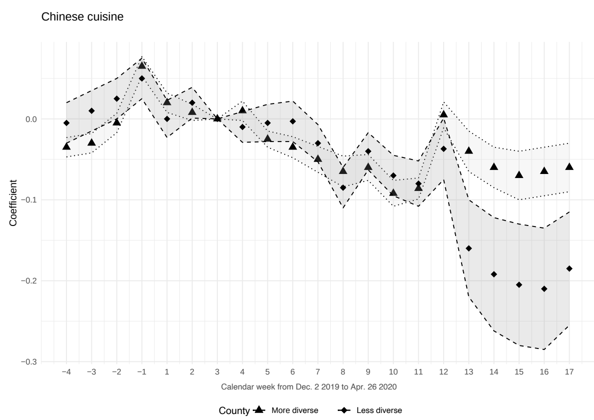
(a)



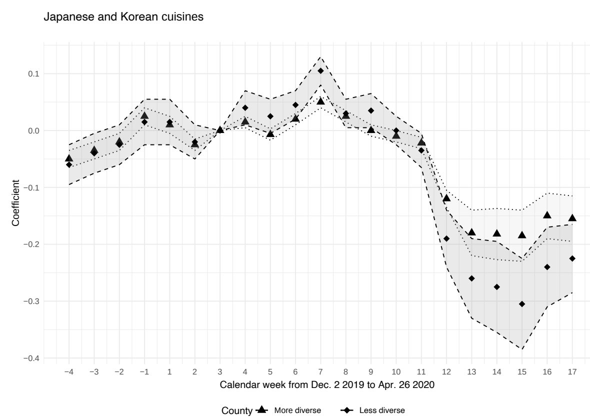
(b)



(c)



(d)



This figure shows the heterogeneity in the effects of consumer discrimination by county diversity index in race and ethnicity on Chinese and Japanese and Korean cuisine visits. In panels (a) and (b), the dot and triangle series respectively show the coefficients $\beta_{1,t,g}$'s and $\beta_{2,t,g}$'s of the corresponding cuisine in Equation 2. The triangle series ($\beta_{2,t,g}$'s) are **not** the causal effect in the "one-standard-deviation" more racially and ethnically diverse counties. In panels (c) and (d), the dot series show the causal effects in the "one-standard-deviation" less racially and ethnically diverse counties by subtracting $\beta_{2,t,g}$'s from $\beta_{1,t,g}$'s adjusting the standard errors, and the triangle series shows the causal effects for the corresponding cuisine in the "one-standard-deviation" more racially and ethnically diverse counties by adding up $\beta_{1,t,g}$'s and $\beta_{2,t,g}$'s adjusting the standard errors. The dotted line indicates the reference period. The 95% confidence intervals are constructed using standard errors clustered at the state-level. The results for other three cuisine groups are in Appendix figure A10.

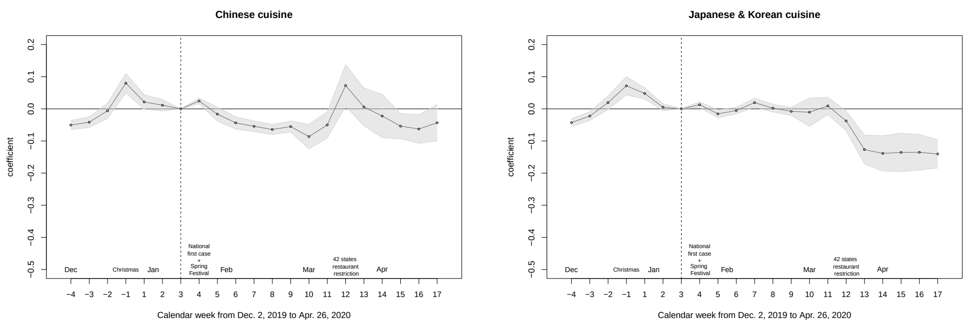
Heterogeneity along County Asian Population Ratio Asians are more likely to be the patrons of Asian restaurants, so it is possible that Asian customers drove the relative declines in East Asian cuisine visits, in this study, Chinese, Japanese and Korean cuisines. The hypothesis is that Asians might be more alert to the virus at the onset and voluntarily reduce visits to public establishments. Although I cannot observe the cellphone users' race and ethnicity, I could look at the effects in highly Asian-populated counties versus less Asian population counties to get a sense of how plausible this hypothesis is. I split the sample by the 75 percentile of the county Asian population ratio distribution. Figure 9 panel (a) and panel (b) respectively show the estimates from Equation 1 for Chinese cuisine and Japanese and Korean cuisines using counties above and below the 75 percentile of the distribution. For brevity, the estimates for other three cuisine groups are reported in Appendix Figure A8.

The comparison between panels (a) and (b) uncovers interesting dynamics. In highly Asian-populated counties, the early relative declines in Chinese cuisine visits before March were more pronounced, partially confirming the hypothesis. However, this trend reverses post-March, with counties having fewer Asian residents experiencing more substantial relative declines of about 30% in Chinese cuisine visits, compared to nearly no relative decline in highly Asian-populated counties. This indicates that while the hypothesis may hold for the initial period, it does not fully explain the later declines. Furthermore, both groups of counties saw declines in Japanese and Korean cuisine visits, albeit less so in highly Asian-populated counties.

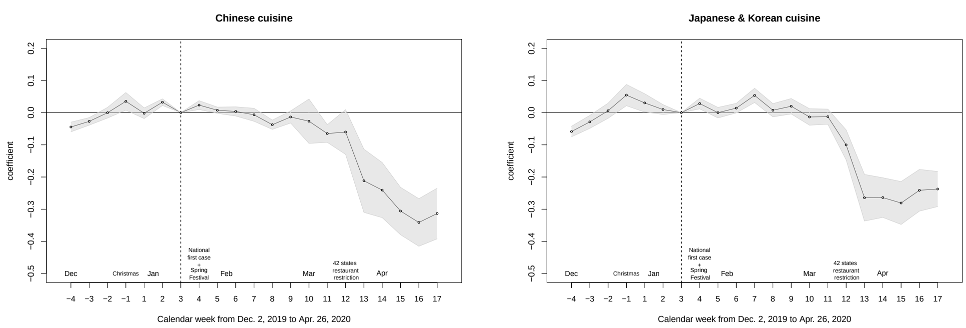
This heterogeneity suggests that the observed consumer discrimination may not solely stem from Asian residents' cautionary behaviors. The early decline in Chinese cuisine visits in counties with larger Asian populations aligns with the narrative of a more informed and cautious response from Asian residents at the pandemic's onset, choosing to reduce restaurant visits. However, the subsequent dynamics, particularly in less Asian-populated counties, imply that other factors, possibly including broader consumer perceptions and reactions to the unfolding pandemic, played a significant role.

Figure 9. Baseline estimated effects of consumer discrimination on Chinese and Japanese and Korean cuisine visits: by county Asian population ratio

(a) Counties above the 75 percentile of the county Asian population ratio distribution



(b) Counties below the 75 percentile of the county Asian population ratio distribution



This figure shows the baseline estimated effects of consumer discrimination on Chinese and Japanese and Korean cuisine visits by the 75 percentile of the county Asian population ratio distribution. Each graph shows the coefficients $\beta_{t,g}$ of the corresponding cuisine in Equation 1. The dotted line indicates the reference period. The 95% confidence intervals are constructed using standard errors clustered at the state level. The estimates for other three cuisine groups by the 75 percentile of the county Asian population ratio distribution are reported in Appendix Figure A8.

4 Conclusion

This paper studies the overarching question of the economic effect of discrimination by asking two relevant questions: how do affected people suffer economically facing discrimination?

What are the relative roles of contributing factors in explaining this effect? For example, how does the economic effect of discrimination differ across regions with different social characteristics? Specifically, this paper sheds light on these questions using a combination of recently available novel data and the Covid-19 pandemic as a setting to quantify the effect of consumer discrimination on Chinese restaurants. I employ a difference-in-differences framework to approximate the impact of consumer discrimination on Chinese cuisine restaurants during the early stages of the COVID-19 pandemic.

Key findings from the empirical analysis reveal a significant and early decline in visits to Chinese cuisine restaurants, aligning with anecdotal reports. This decline, reaching approximately -8% by March, is further substantiated by a parallel drop in revenue for these restaurants, which was even more pronounced than the decrease in visits. Notably, the revenue of Chinese cuisine restaurants fell by about -13% in February, escalating to over -20% in March. A back-of-the-envelope calculation suggests an estimated total revenue loss of around 35 million dollars for Chinese cuisine compared to American and European cuisines during the February to April 2020.

The study also delves into the roles of supply-side and demand-side factors in explaining this observed decline. The analysis indicates that supply-side factors, such as restaurant closures or a shift to online platforms, had little influence, highlighting the prominence of demand-side factors. A closer examination of these demand-side elements uncovers significant county-level heterogeneity related to political affiliation and racial diversity. This is evident from the additional -10% decline in visits to Chinese restaurants in counties with a higher proportion of Republican votes, and an +8% increase in counties with greater racial and ethnic diversity. Furthermore, the study observes a spillover effect on non-Chinese East Asian cuisines, suggesting a nuanced impact driven by perceived similarities in culinary, cultural, or geographical aspects.

In interpreting these results, caution is necessary. Firstly, the estimates are proxies for consumer discrimination, representing the causal effects of early pandemic events on selected cuisines. While the empirical model incorporates numerous controls to clear potential confounding factors, it's important to recognize that these estimates are not perfect representations of the causal parameters. Therefore, the model's estimates should be viewed as suggestive, not definitive, evidence of the economic impact of consumer discrimination. Secondly, the nature of the observed consumer discrimination may not be solely racial. I am not able to observe the cellphone users' individual characteristics due to confidentiality, which does not allow me to see if any social group was more prone to practice consumer discrimination. Notably, the early decline in Chinese restaurant visits driven by Asian-populated counties hints at informed caution, while later declines driven by less Asian-populated areas suggest a mix of fear and possible racial bias. This complexity highlights the multifaceted mechanisms driving consumer behaviors in times of crisis.

Overall, this study emphasizes the need to understand the economic effects of discrimination, particularly during public crises, and provides valuable insights for policymakers seeking to address and mitigate the challenges faced by marginalized social groups. By recognizing the substantial social and economic repercussions of discrimination, policymakers can formulate targeted policies that promote inclusivity and stability for affected communities.

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A Appendix Tables and Figures

Appendix Figure A1. Number of restaurants by cuisine groups in the panel

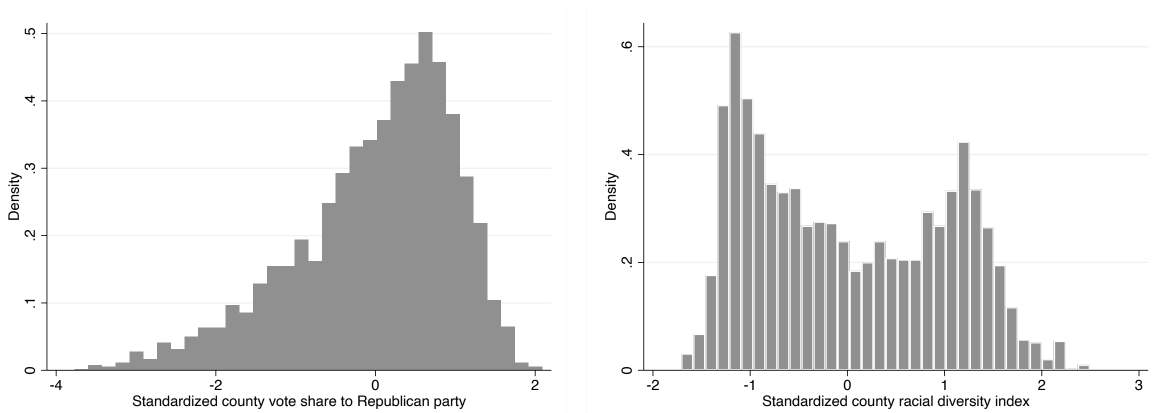
Table: Number of restaurants associated with each cuisine

	Freq.	Percent	Cum.
American and European cuisines	410897	63.98	63.98
Asian fusion and other Asian cuisines	32222	5.02	69.00
Chinese cuisine	34231	5.33	74.33
Indian cuisine	7166	1.12	75.44
Japanese and Korean cuisines	23009	3.58	79.03
Latin cuisine	96211	14.98	94.01
Others	38484	5.99	100.00
Total	642220	100.00	

Appendix Figure A2. Distributions of County Social Characteristics

(a) County political affiliation

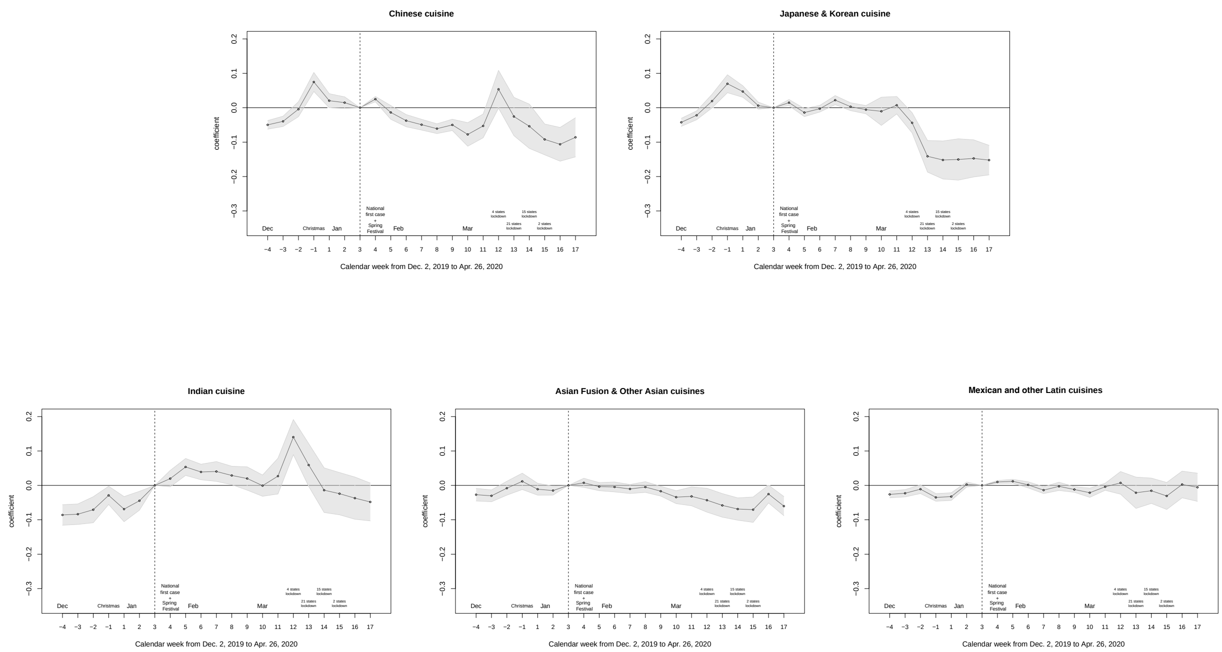
(b) County diversity in race and ethnicity



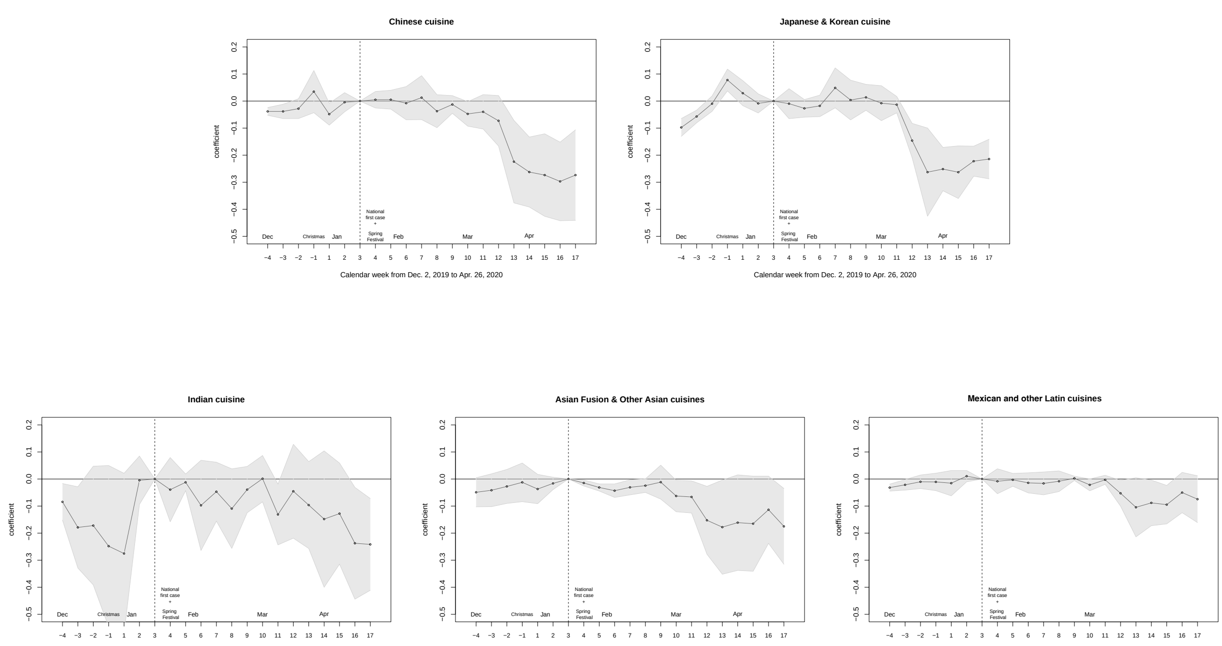
The left panel shows the distribution of the standardized county-level proportion of total votes received by the Republican Party in the 2016 presidential election from the MIT Election Data and Science Lab (2018). The right panel shows the distribution of the standardized county-level diversity index in race and ethnicity calculated using county-level racial and ethnicity compositions from the 2019 5-Year ACS following the method of U.S. Census (2021a).

Appendix Figure A3. Baseline estimated effects of consumer discrimination on cuisine visits: by state lockdown policies over the sample period

(a) States that imposed a lockdown policy



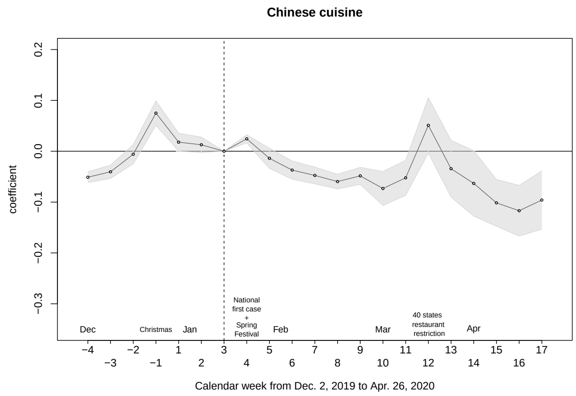
(b) States that did not impose a lockdown policy



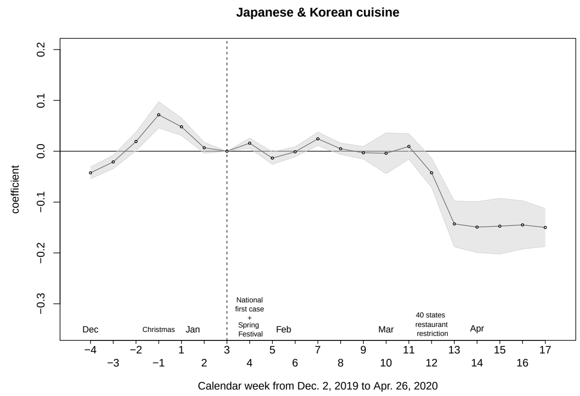
This figure shows the baseline estimated effects of consumer discrimination on cuisine visits by state lockdown policies over the sample period. Each panel shows the coefficients $\beta_{t,g}$ of the corresponding cuisine in Equation 1. The dotted line indicates the reference period. The 95% confidence intervals are constructed using standard errors clustered at the state level.

Appendix Figure A4. Baseline estimated effects of consumer discrimination: dropping those restaurants that are enclosed by a bigger polygon

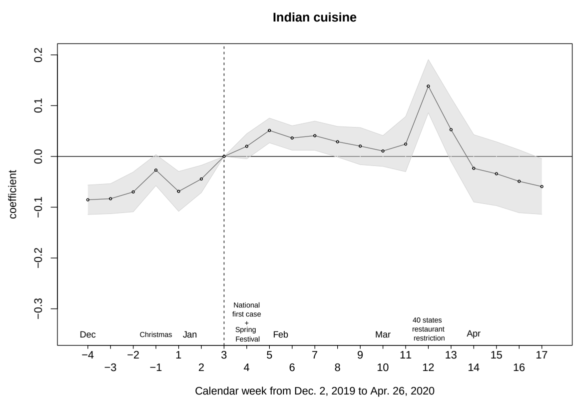
(a)



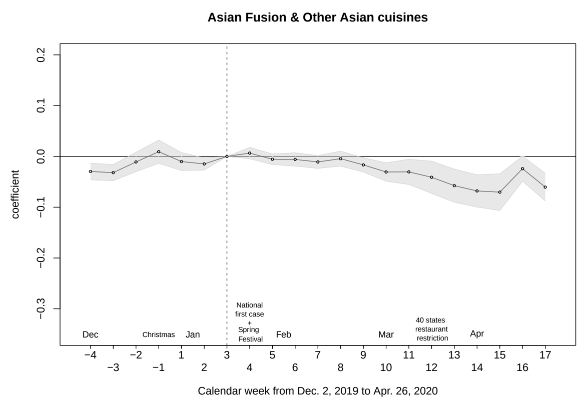
(b)



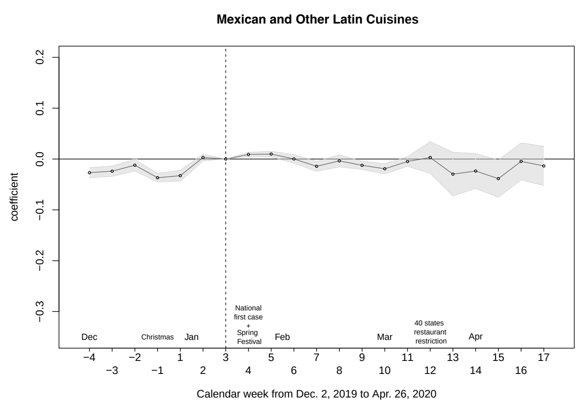
(c)



(d)



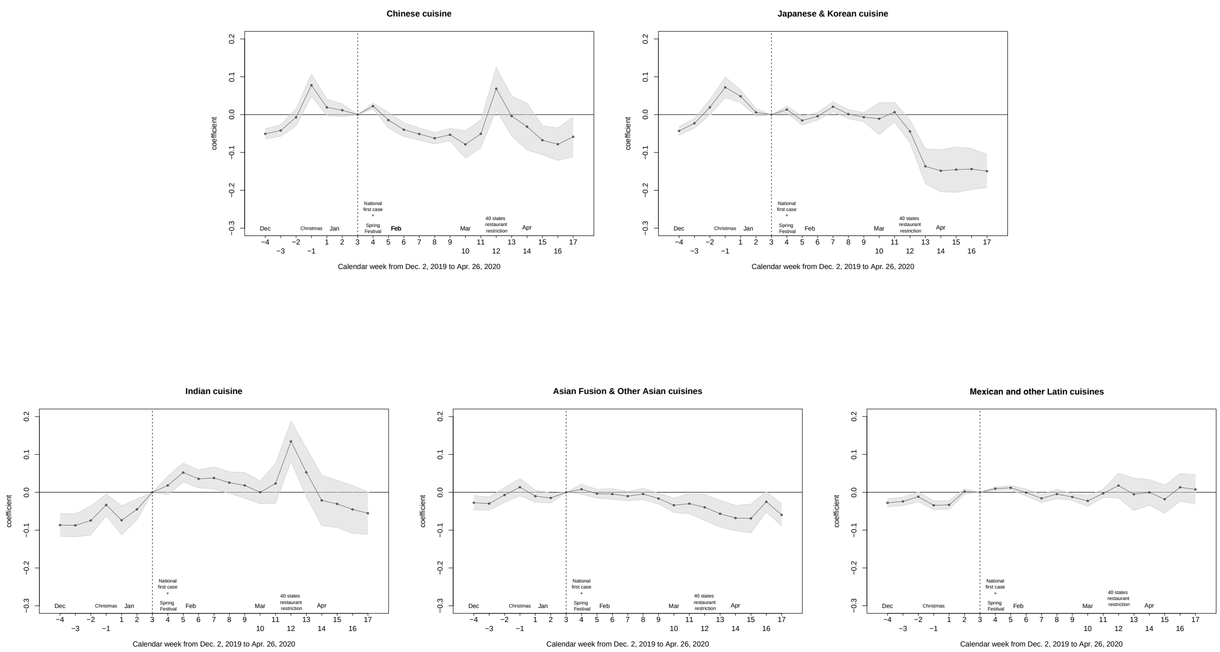
(e)



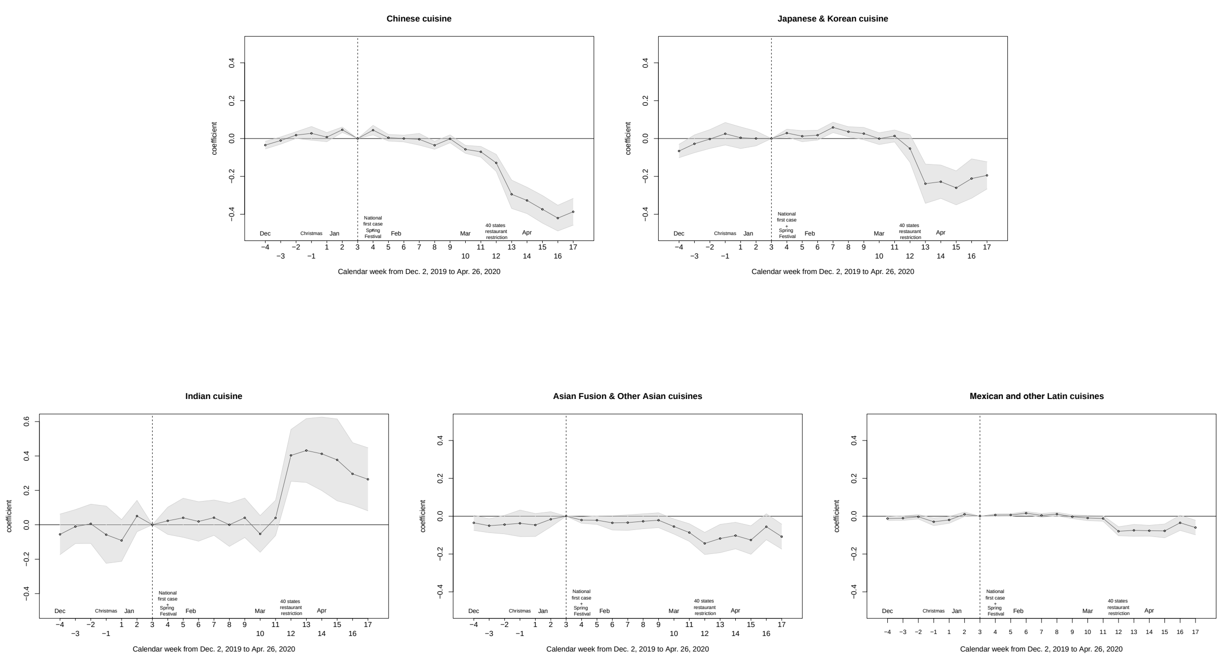
This figure shows baseline estimated effects of consumer discrimination after dropping those restaurants that are enclosed by a bigger polygon. The dotted line indicates the reference period. The x-axis denotes the last 4 calendar weeks of 2019 and the first 17 calendar weeks of 2020. The corresponding calendar months and important events are denoted above the x-axis. The grey area shows the 95% confidence intervals constructed using standard errors clustered at the state level.

Appendix Figure A5. Baseline estimated effects of consumer discrimination on cuisine visits: by USDA county rural-urban status

(a) Counties with urban status



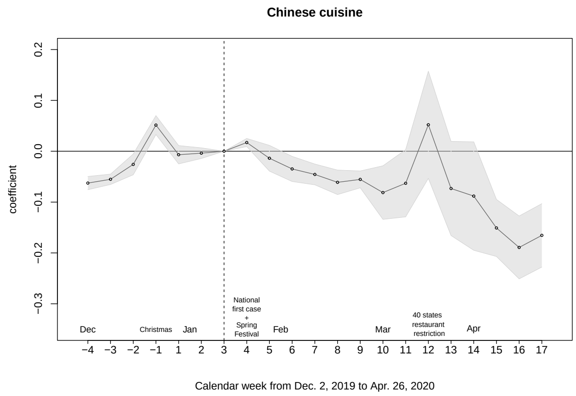
(b) Counties with rural status



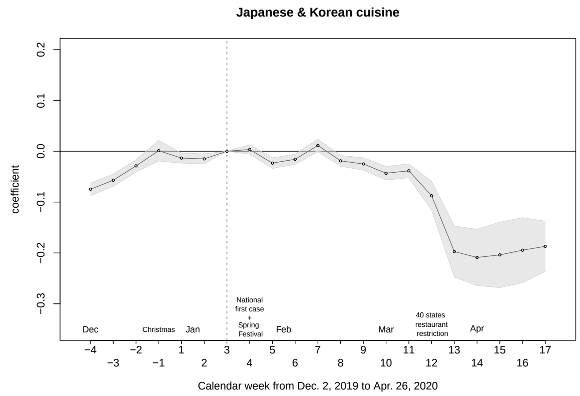
This figure shows the baseline estimated effects of consumer discrimination on cuisine visits by USDA rural-urban status. Each panel shows the coefficients $\beta_{t,g}$ of the corresponding cuisine in Equation 1. The dotted line indicates the reference period. The 95% confidence intervals are constructed using standard errors clustered at the state level.

Appendix Figure A6. Baseline estimated effects of consumer discrimination on cuisine visits: using the sample of restaurants covered in SafeGraph spending data

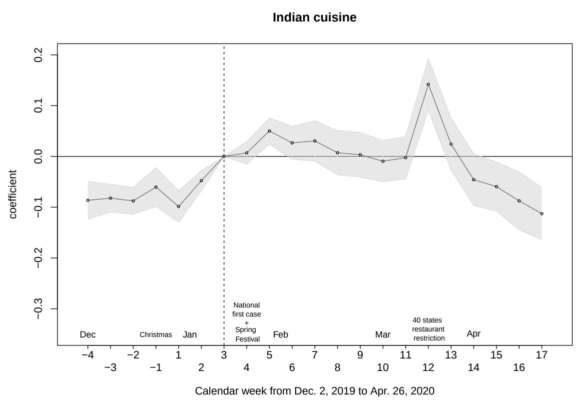
(a)



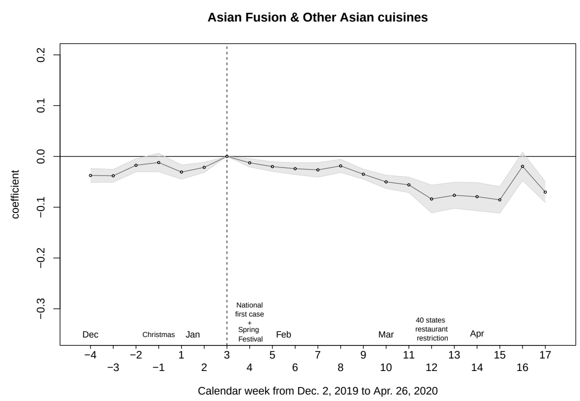
(b)



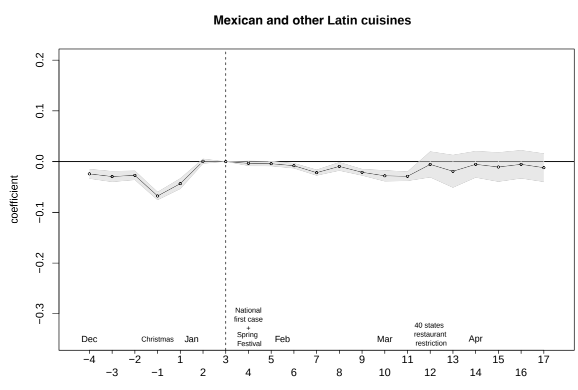
(c)



(d)



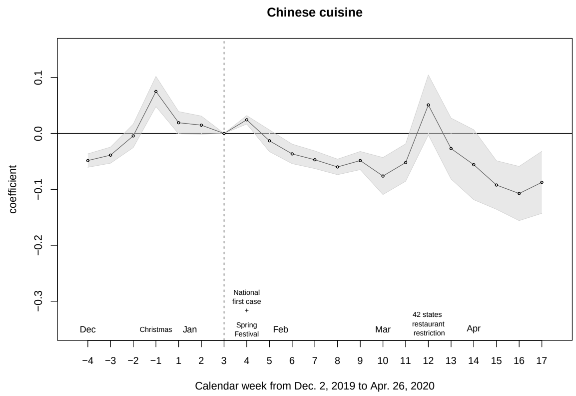
(e)



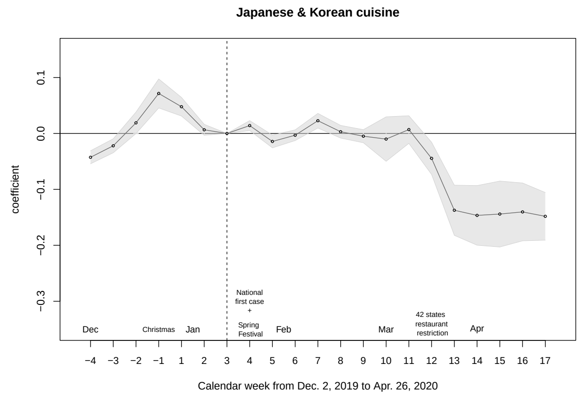
This figure shows baseline estimated effects of consumer discrimination on cuisine visits using the sample of restaurants covered in SafeGraph spending data. Each panel shows the coefficients $\beta_{t,g}$ of the corresponding cuisine in Equation 1. The dotted line indicates the reference period. On all x-axes, {-4,...,-1} denotes the last four calendar weeks of 2019, and {1,2,..,17} denotes the first 17 calendar weeks of 2020. The corresponding calendar months and important events are denoted above the x-axis. The grey area shows the 95% confidence intervals constructed using standard errors clustered at the state level.

Appendix Figure A7. Baseline estimated effects of consumer discrimination: using the subsample without zero visit observations

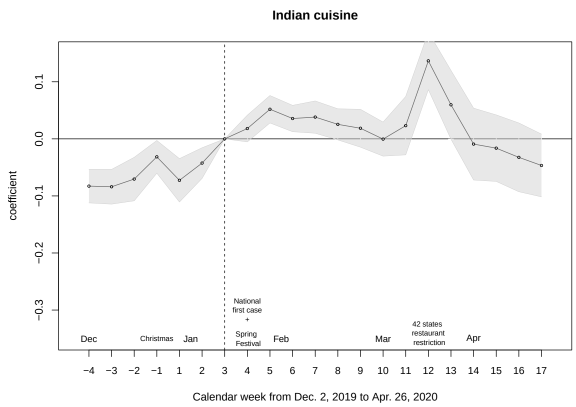
(a)



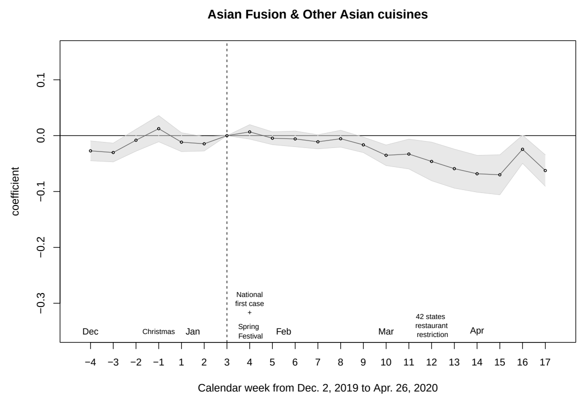
(b)



(c)



(d)



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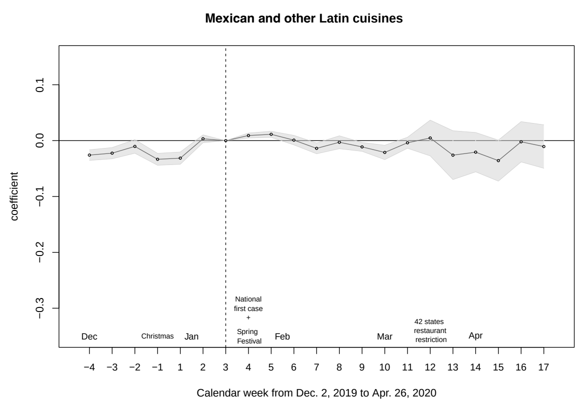
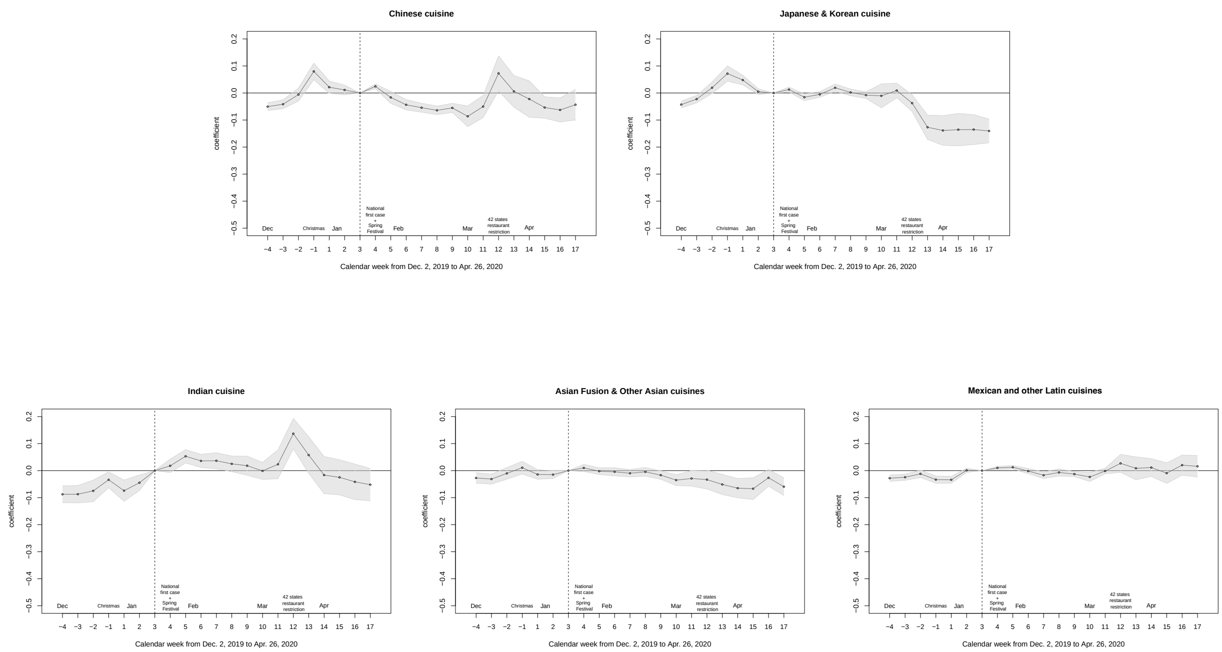


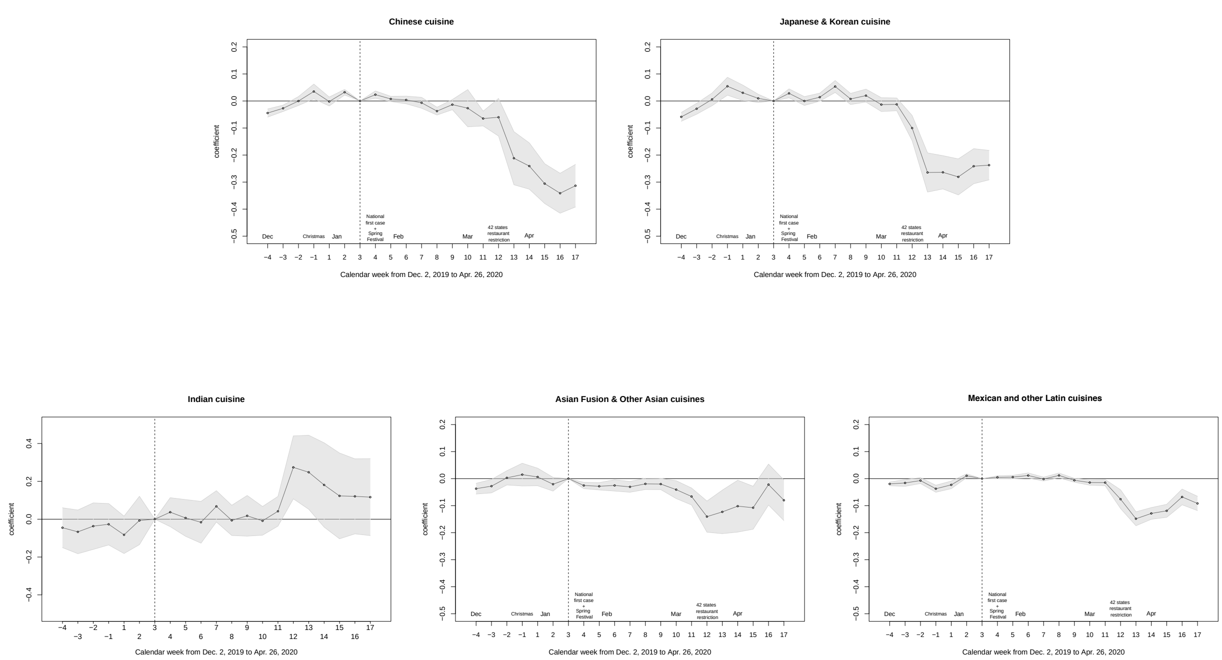
Baseline estimated effects of consumer discrimination using the subsample without zero visit observations. The dependent variable is the log transformation of restaurant foot traffic. Each panel shows the coefficients $\beta_{t,g}$ of the corresponding cuisine in Equation 1. The dotted line indicates the reference period. The x-axis denotes the last 4 calendar weeks of 2019 and the first 17 calendar weeks of 2020. The grey area shows the 95% confidence intervals constructed using standard errors clustered at the state level.

Appendix Figure A8. Baseline estimated effects of consumer discrimination on cuisine visits: by county Asian population ratio

(a) Counties above the 75 percentile of the county Asian population ratio distribution



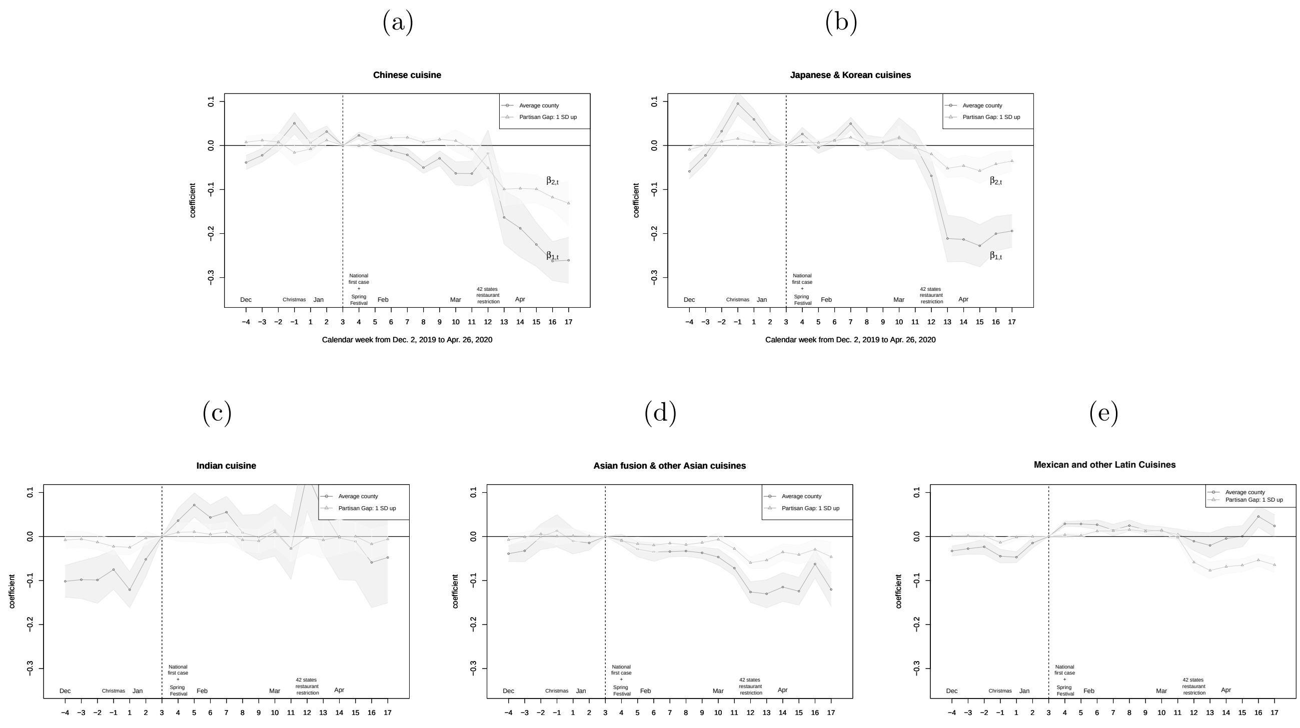
(b) Counties below the 75 percentile of the county Asian population ratio distribution



This figure shows the baseline estimated effects of consumer discrimination on cuisine visits by the 75 percentile of the county Asian population ratio distribution. Each panel shows the coefficients $\beta_{t,g}$ of the corresponding cuisine in Equation 1. The dotted line indicates the reference period. The 95% confidence intervals are constructed using standard errors clustered at the state level.

Remarks on Figure A9: Besides the discussions on Chinese cuisine and Japanese and Korean cuisines in the main text, I would like to point out that Indian cuisine visits were relatively similar to American and European cuisines, with no significant partisan gap; Asian fusion and other Asian cuisines visit patterns mirrored these of East Asian cuisines but with smaller magnitudes; interestingly, though Latin cuisine visits in the *average* counties trended similarly as the base group, there was a roughly -7% partisan gap for Latin cuisines starting March.

Appendix Figure A9. Heterogeneity in the estimated effects of consumer discrimination by county political affiliation

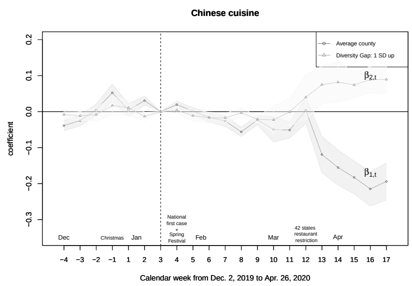


This figure shows the heterogeneity in the estimated effects of consumer discrimination by county political affiliation. In each panel, the dot and triangle series respectively show the coefficients $\beta_{1,t,g}$'s and $\beta_{2,t,g}$'s of the corresponding cuisine in Equation 2. The triangle series ($\beta_{2,t,g}$'s) are **not** the causal effect in the "one-standard-deviation" more republican supporting counties, which is measured by adding up series $\beta_{1,t,g}$'s and $\beta_{2,t,g}$'s adjusting the standard errors. The dotted line indicates the reference period. The 95% confidence intervals are constructed using standard errors clustered at the state-level.

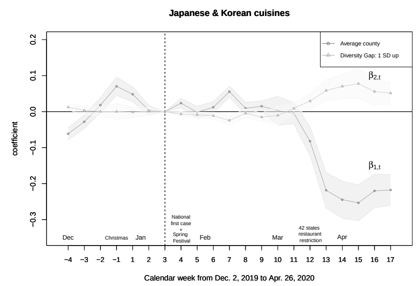
Remarks on Figure A10: "Asian fusion and other Asian cuisines" had smaller but significantly positive diversity gaps; Indian cuisine did not have a statistically significant diversity gap; Latin cuisines had a diversity gap about 7% starting early March.

Appendix Figure A10. Heterogeneity in the effects of consumer discrimination by county diversity in race and ethnicity

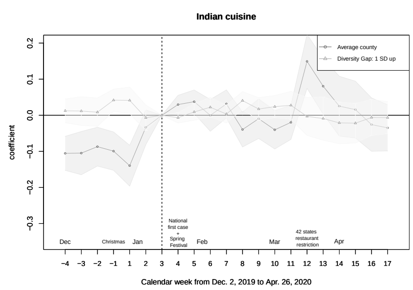
(a)



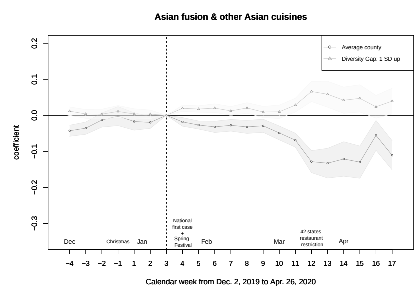
(b)



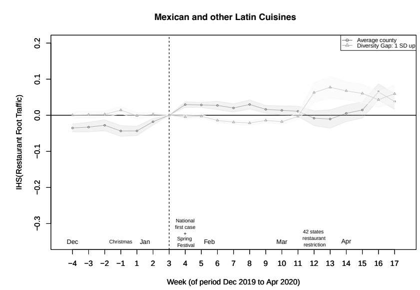
(c)



(d)



(e)



This figure shows the heterogeneity in the effects of consumer discrimination by county diversity index in race and ethnicity. In each panel, the dot and triangle series respectively show the coefficients $\beta_{1,t,g}$'s and $\beta_{2,t,g}$'s of the corresponding cuisine in Equation 2. The triangle series ($\beta_{2,t,g}$'s) are **not** the causal effect in the "one-standard-deviation" more racially and ethnically diverse counties, which is measured by subtracting series $\beta_{2,t,g}$'s from $\beta_{1,t,g}$'s adjusting the standard errors. The dotted line indicates the reference period. The 95% confidence intervals are constructed using standard errors clustered at the state-level.

B Appendix One

Equation for county diversity Index in race and ethnicity (U.S. Census, 2021a)

$$DI_c = 1 - \left(H_c^2 + W_c^2 + B_c^2 + AIAN_c^2 + Asian_c^2 + NHPI_c^2 + SOR_c^2 + Multi_c^2\right) \tag{3}$$

where H_c is the proportion of the population in county c who are Hispanic or Latino; W_c is the proportion of the population in county c who are White alone, not Hispanic or Latino;B_c is the proportion of the population in county c who are Black or African American alone, not Hispanic or Latino; $AIAN_c$ is the proportion of the population in county c who are American Indian and Alaska Native alone, not Hispanic or Latino; $Asian_c$ is the proportion of the population in county c who are Asian alone, not Hispanic or Latino; $NHPI_c$ is the proportion of the population in county c who are Native Hawaiian and Other Pacific Islander alone, not Hispanic or Latino; SOR_c is the proportion of the population in county c who are Some Other Race alone, not Hispanic or Latino; $MULTI_c$ is the proportion of the population in county c who are Two or More Races, not Hispanic or Latino; All race and ethnicity compositions come from the 2019 5-year American Community Survey.