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The Effect of Sunday Closing Laws on Drinking and Driving: Evidence from Colorado

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Abstract

Laws requiring liquor stores be closed on Sunday could potentially reduce drinking and driving by reducing access to alcohol. They could also induce consumers to substitute toward other activities, such as drinking at a bar or restaurant or using illicit drugs, which could increase DUI activity on Sundays. This paper uses a difference-in-difference approach to test the extent to which DUI citations issued by the Colorado State Patrol changed after Colorado's Sunday closing law was repealed in 2008. While aggregate statewide effects on DUI volume are insignificant, I find that this is the result of two significant effects working in opposite directions: a significant reduction in Sunday DUI citations after the repeal in more populous areas (both the Denver MSA and the subset of Colorado counties with populations over 100,000) and a significant increase in Sunday DUI citations in less populous areas (counties with populations below 100,000). These results suggest that substitution effects related to these types of laws differ between more populous areas and less populous ones.

Keywords: Drinking and driving, alcohol, blue laws

JEL Codes: K30, K42

I. Introduction

Laws prohibiting commerce on Sundays, commonly known as "blue laws," were prevalent in the United States through the middle of the twentieth century. Even though laws restricting broad categories of commerce have been repealed by states since then (Gruber and Hungerman, 2008), laws specifically banning Sunday alcohol sales remained in many states. While these bans on Sunday alcohol sales (which, for the remainder of this paper is what I mean by "blue law" or "Sunday closing law") have been repealed in many states in recent years, there are still twelve states who still maintain them in some form.[1] Colorado is one state that recently repealed its ban on Sunday liquor store sales, allowing such sales to begin on July 1, 2008. Prior to the repeal, the only options for purchasing alcohol on Sunday were to patronize a bar or restaurant or to buy 3.2% alcohol-by-volume beer in a grocery store (Yu and Kaffine, 2011). This paper utilizes the policy change in Colorado to measure the effect of Sunday closing laws on drinking and driving activity as measured through DUI citations.

Repealing this law can theoretically affect drinking and driving in a number of ways. An increase in alcohol access could lead to more drinking and, thus, more drinking and driving. Allowing these stores to open could also induce people to substitute one behavior for another. Substitutions between distilled spirits and illicit drugs or between distilled spirits and beer may change. There is a possible substitution toward drinking distilled spirits at home on Sundays and away from drinking them at bars and restaurants, a phenomenon that could decrease the amount of drinking and driving activity. For individuals wishing to consume distilled spirits, the extent to

[1] According to prohibitionrepeal.com, 16 states have removed the ban on Sunday liquor sales since 2002. The 12 states that still prohibit Sunday sales are Alabama, Indiana, Minnesota, Mississippi, Montana, North Carolina, Oklahoma, South Carolina, Tennessee, Texas, Utah, and West Virginia.

which this substitution occurs likely depends on the variety, relative proximity, price levels of bars and restaurant options, and their own incomes, factors that certainly vary by location.

This paper employs a difference-in-difference approach to test the extent to which observed Sunday drinking and driving activity changes in response to this policy in comparison to observed drinking and driving levels on other days of the week. While I find no significant of evidence of changes in drinking and driving activity when DUI counts are aggregated to the state level, this aggregate effect is a consequence of two countervailing effects within the state. When restricting the analysis to subsets of more populous counties (the Denver metropolitan statistical area and the set counties with populations in excess of 100,000), I find that Sunday DUIs significantly decrease after the repeal of the Sunday closing law. Conversely, I find the amount of DUIs issued on Sunday increases after the repeal in the subset of counties with populations below 100,000. I further conclude that the effects that these laws have on drinking and driving are confined to Sundays, finding no consistent evidence of effects on other days of the week. Possible explanations for these findings center on how substitution patterns related to drinking distilled spirits differ between more populous and less populous areas.

The rest of this paper is organized as follows. Section 2 details the relevant literature and how this analysis fits within it. Section 3 describes the theoretical issues relating to drinking and driving and blue laws. Section 4 discusses the data and identification strategy employed. The empirical model and results are given in Section 5, and section 6 concludes.

II. Literature Review

There have been a number of papers written on Sunday closing laws, examining their effects in a variety of settings. In their broadest form, these laws restrict general retail activities on Sunday. McNiel and Yu (1989) show that these laws affect the distribution, but not the level,

of retail activity. Gruber and Hungerman (2008) find that repealing such laws reduces church attendance and leads to increased use of drugs and alcohol. Lee (2013) finds that repealing these laws leads to lower educational attainment. Other studies have focused on the specific type of blue law considered here, the ban on Sunday liquor store sales. The results of these studies have been mixed. McMillan and Lapham (2006) show that repealing blue laws in New Mexico increased alcohol-related accidents on Sundays. Yu and Kaffine (2011) find no significant effect on alcohol-related accidents or citations, Lovenheim and Steefel (2011) find little evidence that repealing blue laws affects the volume of fatal accidents, and Stehr (2010) finds no significant change in traffic fatalities for states other than New Mexico after they repealed their blue laws. Heaton (2012) finds that repealing blue laws did not affect DUI levels but did lead to an increase in minor crime and alcohol-related serious crime. In a study of a similar type of policy change, in which bar closing times were liberalized, Green, Heywood, and Navarro (2014) find an associated reduction in traffic accidents.

The nature of the substitutions induced by the presence of laws restricting access to alcohol are not empirically clear either. For instance, Gary et al. (2003) find circumstantial evidence of individuals living in counties where alcohol is prohibited increasing their impaired driving exposure by driving to counties where there is no such prohibition. Sulfridge (2012) finds support for this type of mechanism, citing statistics on DUI crashes in counties that have these laws in place. Conversely, Brown, Jewell, and Richer (1996) find that these county-level prohibitions reduce fatal accidents, and this effect is more pronounced when accounting for the endogeneous manner in which these laws come into effect. Furthermore, as discussed in Colin et al. (2005) and Fernandez et al. (2015), there is evidence that substitutions between alcohol and

other types of illicit drugs can occur as a result of laws prohibiting alcohol sales in a given county.

By focusing on Colorado's blue law repeal, this paper uses the same source of variation as Yu and Kaffine (2011). While a section of that paper employs a regression discontinuity design to consider changes in weekly counts of alcohol-related traffic citations, this paper makes use of daily counts of citations, allowing me to employ a difference-in-difference approach to account for time trends in the data (for instance, those related to possible variation in DUI enforcement levels over time). Through this methodological distinction, this paper complements and extends that analysis. Furthermore, through the separate considerations of these issues in more populous and less populous settings, this paper emphasizes the underlying tradeoffs that drive the results in this literature and highlights the presence of within-state variation in the effects of these policies.

III. Theoretical Considerations

There are a number of channels through which a repeal of Colorado's Sunday closing law could affect alcohol consumption and drinking and driving activity, and in this section I elaborate on these theoretical considerations. If one were to assume that consumers who wish to drink distilled spirits at home on Sunday are not forward looking enough to purchase them ahead of time, the Sunday closing law would give these individuals less access to those distilled spirits and, as a result, make them less likely to drink and drive on Sundays. Of course there is reason to believe that individuals are forward looking, and if they want to drink liquor at their homes on Sunday, they could buy it in advance.[2] If people had perfect foresight about their consumption of spirits,

[2] In fact, this is one reason some liquor store owners did not want the blue law repealed: they thought weekly volumes would be similar, but spreading those volumes out over an extra day would bring with it additional costs (Quenqua, 2008).

then these types of advance purchases would mean the law change would have no effect on liquor consumption or where it is consumed. However, there are reasons to think that consumers may find themselves with less than the desired stock of liquor in their homes at times when the Sunday closing law serves as a binding constraint. For instance, idiosyncratic shocks to one's stock of liquor (e.g., surprise guests) could leave an individual in this position. If some fraction of the population finds themselves with a lower than intended stock of liquor in the face of a mandated store closing, individuals in Colorado who would simply go to the liquor store on Sunday (before drinking anything) might adjust their behavior in one of the following ways:

1. They choose to accept their lower than intended stock of liquor and take no other action to procure additional alcohol. This would lower alcohol consumption and reduce the possibility of drinking and driving.

2. They purchase 3.2% alcohol beer at the grocery store instead of making the purchase from the liquor store they would prefer to make. Since they are presumably not modifying the location of their consumption and are consuming beverages with lower alcohol content, this would likely reduce the possibility of drinking and driving if it had any effect at all.

3. Individuals who realize on Sunday that their stock of liquor is low choose to drink at a restaurant or bar rather than staying home with a lower than expected stock of liquor. This substitution would likely mean more drinking and driving on Sunday than would occur if they could replenish their liquor stock at a liquor store on Sunday and drink that newly purchased liquor at home.

4. They choose to consume a substitute product, perhaps illicit drugs (a possibility research has suggested), in place of alcohol. The extent to which this affects the level of impaired driving is unclear. [3]

While this list does not exhaust all possible reactions to this type of policy, it indicates that because of the range possible reactions, the theoretical effect that this law change will have on impaired driving is ambiguous.[4] Furthermore, it is certainly possible that the relative attractiveness of these options differ between more populous and less populous areas. For instance, if on average, individuals who live in more populous areas live closer to establishments that serve alcohol than do individuals from a less populous area, it may be a less costly option for them to go to a bar or a restaurant, and thus they may be more likely to modify their behavior in that way in response to the Sunday closing law. Income levels and bar and restaurant price levels likely differ across locations as well, a factor that could affect the willingness of individuals in these respective locations to patronize a bar or restaurant. It is also possible that the cost and availability of illicit drugs could differ by location. Thus, though there are reasons to think that these issues may differ in more populous versus less populous settings, there is no clear theoretical prediction regarding where these effects should be most pronounced. The empirical

[3] Citations in the data do not differentiate between impairment due to alcohol or drugs, so drivers who are impaired as a result of either drugs or alcohol should appear in the data. According to the CSP, a "vast majority" of these citations are related to alcohol. The relative impairment levels associated with consuming illicit drugs as opposed to liquor is unclear, however.

[4] I ignore the possibility that consumers can move across jurisdictional boundaries to places where liquor stores are open (the sort of variation in policy used in studies like Winn and Giacopassi (1993), Baughman et al. (2001), and Brown, Jewell, and Richer (1996)). Note that the possibility that consumers would circumvent blue laws in this way would only serve to diminish any effects from repealing them.

analysis is designed to illuminate the practical implications of the policy and to suggest possible explanations.

IV. Data and Identification

The empirical analysis uses data from the Colorado State Patrol on DUI citations issued between January 2006 and December 2010. While the Colorado State Patrol issues a large number of DUI citations, they are not the only agency that can arrest individuals for this, so this sample represents a subset of all DUI citations issued.[5] There are 35,585 individual DUI citations in the sample.[6] The sample provides roughly two and a half years of data for both the period before the law change and the period after the law change. It should be noted, however, that 3 months of data between late September 2009 and December 2009 are missing due to a technical issue with the way the data was reported. I have no reason to assume that this technical issue is correlated with enforcement patterns or drinking and driving activity in any way, and thus it should not affect the results.

I aggregate counts of citations by day for four groups of counties: all counties within the state, the ten counties comprising the Denver MSA,[7]counties with an average population during

[5] Local police and sheriff's departments can issue these citations as well, so this is not an exhaustive collection of all drinking and driving arrests in Colorado. In Yu and Kaffine (2011), it is stated that during 2007 through 2009, there were an average of 74.6 alcohol-related citations issued per day. During the 2006 through 2010 time period considered here, the CSP issued an average of 20.63 per day, which would be roughly 28% of the number of daily citations referred to in Yu and Kaffine (2011). It should also be noted that the CSP issues a relatively high percentage of its citations in counties with populations below 100,000: these counties comprise roughly 16.4% of Colorado's population, but over 35% of CSP citations are issued in these counties. Given the nature of the analysis and the ability to control for date, time, and location of citations, the fact that this is a subset of DUI arrests in the state should not bias results. Indeed, if the CSP spends a greater proportion of its time on the highways than do local law enforcement and a majority of drinking and driving occurs on local roads, the results estimated from the CSP citations would actually understate the true effects of the policy.

[6] This number is net of citations excluded from the analysis due to being issued on a day for which citation data is incomplete.

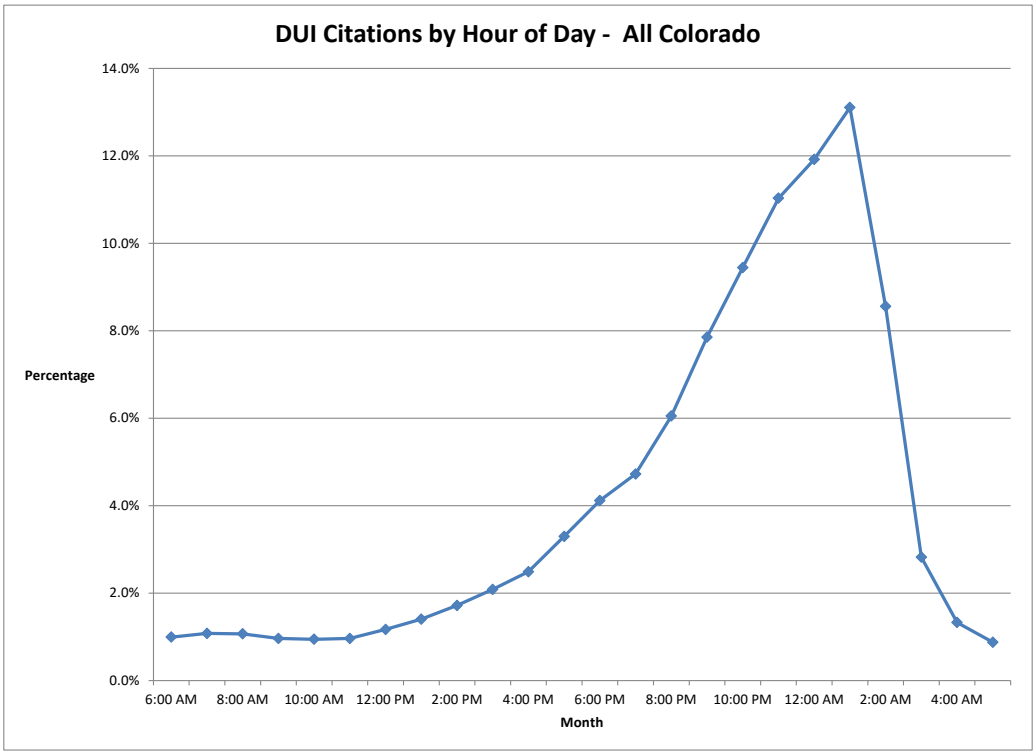
[7]This includes Denver, Arapahoe, Jefferson, Adams, Douglas, Broomfield, Elbert, Park, Clear Creek, and Gilpin Counties.

this time period over 100,000,[8] and counties with an average population during this time period under 100,000.[9] While somewhat arbitrary, using a population of 100,000 as the dividing line between "more populous" and "less populous" counties is convenient because it highlights a natural discontinuity in county population distribution: the smallest county over with a population above 100,000 (Mesa County) has an average population above 140,000 during this time period, while the largest county with a population under 100,000 (Broomfield County) has an average population below 55,000.

As Figure 1 indicates, citations tend to increase in the overnight hours, with peak DUI activity during the 11 p.m. to 2 a.m. window. With this peak spanning parts of two days, it seems appropriate to aggregate the citations in such a way where all overnight citations count toward the same observational unit. Thus, for the purposes of this paper, I choose 6 a.m. as time when a new day begins. So, for example, the count of citations for a Tuesday in the data includes citations between 6 a.m. Tuesday morning and 6 a.m. Wednesday morning. Table 1 displays summary statistics on the citation counts for the full data set, the Denver MSA subsample, the subsample of counties with populations exceeding 100,000, and the subsample of counties with populations below 100,000. For each group of counties, citations are lowest on Monday and increase throughout the week, reaching a maximum on Saturday, and then on falling on Sunday to a level that is close to Thursday levels.

[8] This set of 11 counties includes Denver, Arapahoe, Jefferson, Adams, Douglas, El Paso, Larimer, Boulder, Weld, Pueblo, and Mesa Counties. Data on population comes from the Colorado's State Demography Office's estimates, retrieved from http://www.colorado.gov/cs/Satellite/DOLA-Main/CBON/1251593346834.
[9] This includes the other 53 counties in Colorado.

Figure 1. DUI Citations by Time of Day



Data on DUI citations issued from 2006 to 2010 is from Colorado State Patrol.

In addition to the average within-week trends described in Table 1, DUI counts follow several other trends, which I show graphically in Figures 2 and 3. Figure 2 shows a seasonal effect within a given year. Citations tend to increase somewhat in the summer (and there is a spike in May and September due to the Memorial Day and Labor Day holidays) and decrease during the winter. Figure 3 shows average daily citations by month over the entire five-year sample, where an overall downward trend in statewide citations is clear even though it is obscured to an extent by within-year seasonal variation. The trends in the three subsamples are not as pronounced as the full sample trend, but they do not show any increase in DUI citations over the time period in spite of the fact that population for each of these groups of counties rose between 2006 and 2010.

Table 1. Summary Statistics

All Colorado - Daily Citation Counts

	Observations	Mean	SD	Median
All Days	1725	20.63	11.98	17
Sunday	247	17.12	7.61	16
Monday	247	11.83	5.23	11
Tuesday	246	12.57	4.53	12
Wednesday	246	15.10	5.16	15
Thursday	246	17.92	5.33	17
Friday	247	33.90	10.50	32
Saturday	247	35.96	11.44	35

Denver MSA - Daily Citation Counts

	Observations	Mean	SD	Median
All Days	1725	6.28	4.03	6
Sunday	247	5.26	2.81	5
Monday	247	3.28	2.05	3
Tuesday	246	3.58	1.99	3.5
Wednesday	246	4.97	2.54	5
Thursday	246	6.65	2.77	6.5
Friday	247	9.98	3.95	9
Saturday	246	10.22	4.45	10

Counties with Population Over 100,000 - Daily Citation Counts

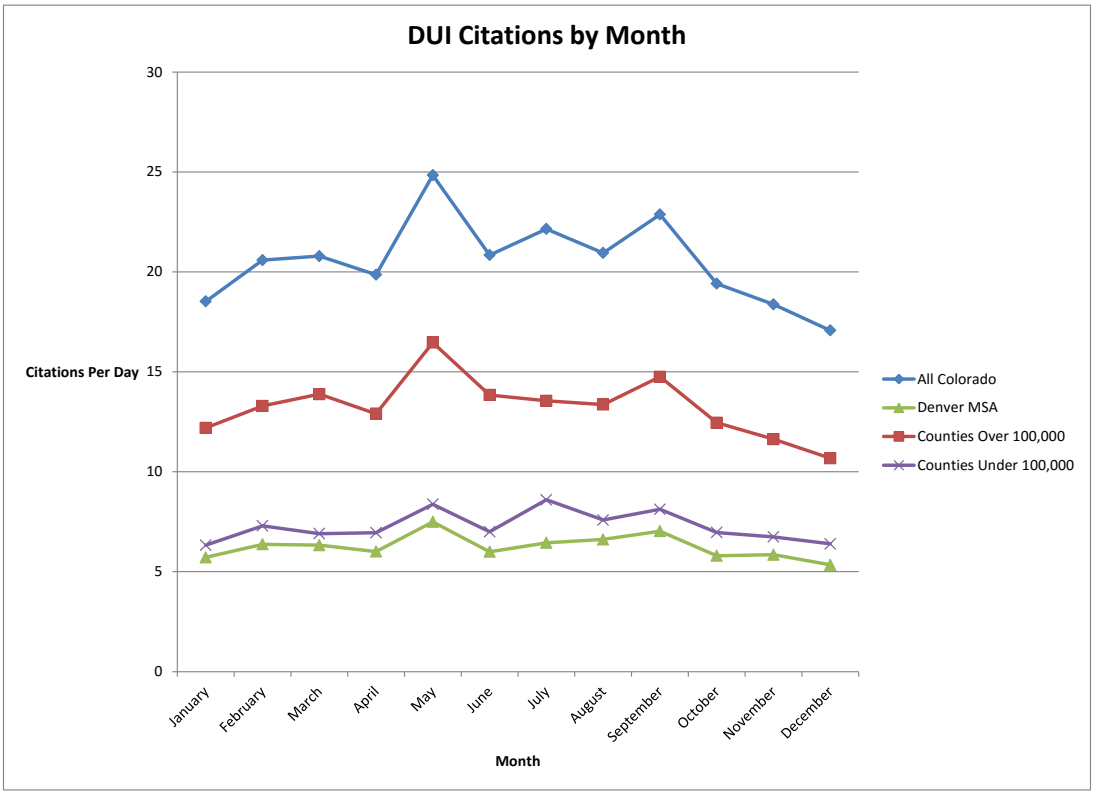
	Observations	Mean	SD	Median
All Days	1725	13.33	7.68	11
Sunday	247	11.48	5.25	11
Monday	247	7.64	3.76	7
Tuesday	246	8.38	3.53	8
Wednesday	246	10.09	3.66	10
Thursday	246	12.46	4.32	12
Friday	247	21.15	7.20	20
Saturday	246	22.12	8.08	21

Counties with Population Under 100,000 - Daily Citation Counts

	Observations	Mean	SD	Median
All Days	1725	7.30	5.28	6
Sunday	247	5.64	3.47	5
Monday	247	4.19	2.55	4
Tuesday	246	4.20	2.37	4
Wednesday	246	5.01	2.80	5
Thursday	246	5.46	2.57	5
Friday	247	12.74	5.01	12
Saturday	246	13.85	5.35	13

Data on DUI citations issued from 2006 to 2010 is from Colorado State Patrol.
Citation counts presented are aggregated for the defined groups of counties.

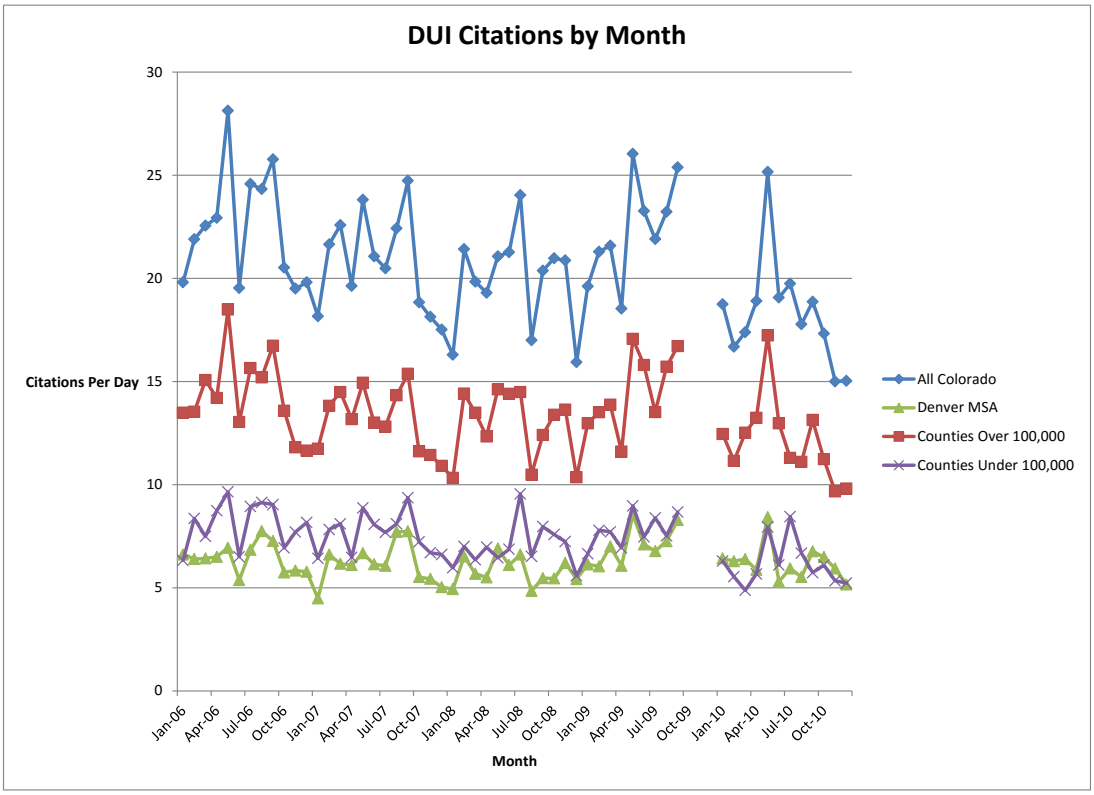
Figure 2. Average Monthly Trend in DUI Citations



Data on DUI citations issued from 2006 to 2010 is from Colorado State Patrol. Citation counts presented are aggregated for the defined groups of counties.

DUI citation volume essentially boils down to two things: the amount of drinking and driving activity that is actually occurring (much of which may go undetected by enforcement agencies) and the intensity with which drinking and driving laws are enforced. Both the enforcement level and the amount of drinking and driving likely vary over time, so absolute changes in the number of citations do not have a clear interpretation as it relates to changes in drinking and driving activity (it could be interpreted as a change in drinking and driving, enforcement intensity, or both). Since this paper is primarily concerned with how the law change affects drinking and driving activity on Sundays, I compare DUI citation counts from those days to DUI citations counts from a variety of subsets of the other days of the week, with time fixed effects to account for variation in levels of enforcement intensity over time and for changes over

Figure 3. DUI Citations by Month



Data on DUI citations issued from 2006 to 2010 is from Colorado State Patrol. Citation counts presented are aggregated for the defined groups of counties.

time in the level of drinking and driving activity. I compare Sunday citation counts to 3 separate control groups: all other days (Monday through Saturday), weekdays (Monday through Thursday), and weekends (Friday and Saturday). Since Sunday is part of the weekend, but Sunday night is the night before a weekday, it has elements of both the work week and the weekend, and the control groups are chosen to reflect that dichotomy.

The identifying assumption I make to compare the respective counts of Sunday citations to these control groups is that the relative enforcement intensity between the treatment and control groups is constant over time.[10] Because I break the sample into several subsamples, I am

[10] The enforcement intensity here relates to the Colorado State Patrol. Thus, I am implicitly assuming that if there is a shift away from CSP enforcement and toward local law enforcement, such a shift does not occur in a way that disproportionately affect Sunday citations after the blue law was repealed. The data do not allow me to directly test this assumption.

implicitly assuming that this assumption holds within each subsample. Thus for the full-state sample and all subsamples, I assume that there is no change in the intensity of enforcement on Sunday relative to other days of the week after the blue law was repealed. I have no reason to believe that this is an unreasonable assumption, but the fact that I cannot directly test it is certainly a limitation of the data.[11]

A variety of factors could affect enforcement intensity over time, though it seems unlikely that these factors would disproportionately affect Sunday enforcement. One such source of variation in enforcement intensity over time is Colorado's "The Heat is On" campaign, which involves increased DUI enforcements and a public awareness campaign during 12 enforcement periods during the year.[12] These increased enforcement periods involve multiple days or even weeks, and so they are unlikely to disproportionately affect the level of citations on Sunday, and furthermore, they often are scheduled for holiday weekends for which the models used in this analysis explicitly control. Issues like changing budgetary situations for enforcement agencies (either local agencies[13] or the CSP itself) or the effects of economic changes on consumer decisions should theoretically be accounted for with time controls. Furthermore, the temporal variation in the intensity of DUI enforcement on the weekends relative to the work week are accounted for, at least in some of the specifications of the control groups.

[11] One conceivable issue here is the possibility that the recession of 2008 affected less populous counties differently than the more populous counties. In examining the annual unemployment rates for the two groups of counties, I find that the trends after 2008 are similar to the trends in the preceding years. In 2006 and 2007, the less populous counties had an average annual unemployment rate that was 0.39 percentage points lower than the full-state average. In 2009 and 2010, those counties had an average annual unemployment rate that was 0.43 percentage points lower than the full state average. Because of those similar trends, I would not expect this to motivate any differential within-week patterns of drinking (and by extension drinking and driving) activity. Thus, I assume that this is not an issue that is materially affecting the results.

[12] This campaign was in place for the entirety of the sample period.

[13] If average economic conditions are not changing in a differential way between more populous counties and less populous ones, as the unemployment figures suggest, it seems unlikely that differences in the budget situations of the local enforcement agencies should be a factor that biases the results.

Having made these assumptions about appropriate treatment and control groups, the remainder of the analysis involves comparing the citation counts of the treatment and control groups in periods before the Sunday was repealed and in periods after it was repealed. This is done through standard difference-in-difference regression analysis.

V. Empirical Model and Results

Table 2 tabulates these differences between the counts of Sunday citations and the counts for the relevant control groups, both for the period before the law was repealed and the period after the law was repealed. While this table is descriptive in nature and does not account for all the possible time trends affecting these counts, it does show several clear patterns (columns 5, 6, and 7 give the relevant differences as well as the difference-in-differences). First of all, differences in statewide citation counts do not change very much after the Sunday closing law is repealed. None of these differences for the full sample change by more than 0.6 citations. The second key feature is that in the two subsamples from populous counties, the differences indicate that Sunday DUI citations in these areas fell after the blue law was repealed, and the level of this decline was between 1 and 2 citations on any given Sunday. The effect is reversed for the subsample of counties with populations below 100,000. In the counties, we see an increase in the Sunday DUI citations relative to the control groups. These rough estimates put the increase at DUIs in these counties between 0.79 and 1.6 citations for any given Sunday.

The differences in differences that are descriptively shown in Table 2 are more carefully estimated using the following linear regression model:

$$Citations_{mdt} = \beta_0 + \beta_1(Sunday * PostRepeal_{mdt}) + + \boldsymbol{\beta_2}(\boldsymbol{holiday_{mdt}}) + \boldsymbol{\beta_3}(\boldsymbol{day\ of\ week_d}) + \boldsymbol{\beta_4}(\boldsymbol{yearmonth}) + \varepsilon_{mdt} \quad (1)$$

Table 2. Comparison of DUI Citations by Day, Before and After Blue Law Repeal

	Sunday	All Day Avg.	Fri./Sat. Avg.	Weekday Avg.	(Sunday - All Day Avg.)	(Sunday - Fri./Sat. Avg.)	(Sunday - Weekday Avg.)
All Colorado							
Ban (Before July 1, 2008)	17.88	21.25	35.77	14.85	-3.37	-17.90	3.03
No Ban (After July 1, 2008)	16.27	19.93	33.99	13.80	-3.67	-17.72	2.47
Difference-In-Difference					**-0.30**	**0.18**	**-0.56**
Denver MSA							
Ban (Before July 1, 2008)	5.84	6.22	9.86	4.50	-0.38	-4.02	1.34
No Ban (After July 1, 2008)	4.60	6.34	10.37	4.75	-1.73	-5.77	-0.14
Difference-In-Difference					**-1.35**	**-1.75**	**-1.48**
Counties Over 100,000							
Ban (Before July 1, 2008)	12.36	13.65	21.85	9.89	-1.29	-9.49	2.47
No Ban (After July 1, 2008)	10.48	12.97	21.40	9.36	-2.49	-10.92	1.12
Difference-In-Difference					**-1.20**	**-1.43**	**-1.35**
Counties Over 100,000							
Ban (Before July 1, 2008)	5.52	7.60	13.93	4.96	-2.08	-8.41	0.56
No Ban (After July 1, 2008)	5.78	6.96	12.59	4.44	-1.18	-6.80	1.35
Difference-In-Difference					**0.90**	**1.60**	**0.79**

Data on DUI citations issued from 2006 to 2010 is from Colorado State Patrol. Citation counts presented are aggregated for the defined groups of counties.

The unit of observation is the day, which can be indexed by its year-month (m), day of the week (d), and individual observation (t). The coefficient on the difference-in-difference term, β_1, is the one of primary interest. The other terms are dummy variables for individual holidays potentially related to drinking and driving activity (one dummy each for days that are part of Memorial Day Weekend, days that are part of Labor Day Weekend, July 4, and New Year's Eve), individual days of the week, and the year-month of the citation.[14] The data included in the models depends on the specification of the control group: when all other days are specified as the control group, no days are omitted from the data; when Friday and Saturday constitute the control group, all days other than Friday, Saturday, and Sunday are omitted. Since count data is being used, I also run several Poisson specifications analogous to the linear specification given in equation 1 as a robustness check.

[14] It should also be noted that when I use year-week dummies rather than year-month dummies to account for the time trends, the estimates are similar. I use year-month dummies rather than year-week dummies in part because, in the specifications that only use weekend data, the sparseness of the year-week indicators makes estimating the robust standard errors problematic.

A. Main Results

The results of specifications involving the statewide citation counts as the dependent variable are given in Table 3. The results reflect what the descriptive differences in Table 2 suggest: after accounting for trends in enforcement and drinking and driving activity, there is no significant change in the statewide volume of Sunday DUI citations (relative to the control groups) after the Sunday closing law was repealed. This is true for all control groups and for both the linear and the Poisson specifications.

Table 3. Effect of Blue Law Repeal, All Colorado Counties

	(1)	(2)	(3)	(4)	(5)	(6)
*Sunday*Post-Repeal*	-0.318	0.390	-0.0827	-0.0571	-0.954	-0.371
	(0.705)	(1.056)	(0.712)	(0.593)	(1.197)	(0.808)
	-0.450	0.370	-0.116	-0.0964	-0.797	-0.459
Specification:						
OLS	X	X	X			
Poisson				X	X	X
Control Group:						
Monday through Thursday	X			X		
Friday and Saturday		X			X	
Monday through Saturday			X			X
Observations	1,232	740	1,725	1,232	740	1,725
Adjusted R-squared	0.448	0.634	0.719			
Pseudo R-squared				0.154	0.371	0.426

Robust standard errors in parentheses. T-stats reported beneath standard errors. In Poisson specifications, marginal effects are reported. Dependent variable is daily count of citations. All specifications use year-month dummies to account for time trend. Day of week and specific holiday dummy variables are also included as covariates. *** p<0.01, ** p<0.05, * p<0.1. Data on DUI citations issued from 2006 to 2010 is from Colorado State Patrol.

Tables 4 and 5 and replicate this analysis for more heavily populated counties: the Denver MSA subsample the subsample of counties with populations over 100,000, respectively. In both of these subsamples, there is clear evidence of a reduction in Sunday DUIs after liquor

stores were allowed to operate on Sunday. In the Denver MSA subsample all estimates are significant at the 1% level, with point estimates of the effect ranging from -1.2 to -2.2. That range corresponds to a reduction between 23% and 41% in the average number of Sunday citations. For the counties over 100,000 people, the point estimates of the linear specifications suggest this reduction is between 1.2 and 1.4 citations per day (these numbers correspond to a range of 10% to 12% of the average number of Sunday citations in this set of counties). The marginal effects of the Poisson estimates reported in Table 5 are in line with the estimates from the linear model, though they vary more (the lowest magnitude is -0.9, while the highest is -2.5). Five of the six estimates related to the change in Sunday citation counts in Table 5 are significant at either the 5% level or the 1% level, and the other one is significant at the 10% level.

Table 4. Effect of Blue Law Repeal, Denver MSA

	(1)	(2)	(3)	(4)	(5)	(6)
*Sunday*Post-Repeal*	-1.413***	-1.733***	-1.519***	-1.194***	-2.173***	-1.571***
	(0.341)	(0.469)	(0.345)	(0.252)	(0.447)	(0.308)
	-4.140	-3.692	-4.405	-4.744	-4.857	-5.100
Specification:						
OLS	X	X	X			
Poisson				X	X	X
Control Group:						
Monday through Thursday	X			X		
Friday and Saturday		X			X	
Monday through Saturday			X			X
Observations	1,232	740	1,725	1,232	740	1,725
Adjusted R-squared	0.291	0.419	0.509			
Pseudo R-squared				0.102	0.179	0.219

Robust standard errors in parentheses. T-stats reported beneath standard errors. In Poisson specifications, marginal effects are reported. Dependent variable is daily count of citations. All specifications use year-month dummies to account for time trend. Day of week and specific holiday dummy variables are also included as covariates. *** p<0.01, ** p<0.05, * p<0.1. Data on DUI citations issued from 2006 to 2010 is from Colorado State Patrol.

The results for the set of counties with fewer than 100,000 people are given in Table 6. They show significant increases in Sunday DUIs in these counties after the Sunday closing was repealed. The estimated coefficients range from 0.9 to 2.0 (which corresponds to a range of 15% to 36% of the average number of Sunday citations in these counties). For both the linear and Poisson specifications, all estimates are significant at the 5% level, with most of them significant at the 1% level.

Table 5. Effect of Blue Law Repeal, Counties with Population over 100,000

	(1)	(2)	(3)	(4)	(5)	(6)
*Sunday*Post-Repeal*	-1.178**	-1.415*	-1.271**	-0.899**	-2.457***	-1.510***
	(0.545)	(0.790)	(0.550)	(0.441)	(0.809)	(0.553)
	-2.161	-1.792	-2.310	-2.038	-3.036	-2.732
Specification:						
OLS	X	X	X			
Poisson				X	X	X
Control Group:						
Monday through Thursday	X			X		
Friday and Saturday		X			X	
Monday through Saturday			X			X
Observations	1,232	740	1,725	1,232	740	1,725
Adjusted R-squared	0.362	0.534	0.619			
Pseudo R-squared				0.122	0.270	0.314

Robust standard errors in parentheses. T-stats reported beneath standard errors. In Poisson specifications, marginal effects are reported. Dependent variable is daily count of citations. All specifications use year-month dummies to account for time trend. Day of week and specific holiday dummy variables are also included as covariates. *** p<0.01, ** p<0.05, * p<0.1. Data on DUI citations issued from 2006 to 2010 is from Colorado State Patrol.

B. Other Specifications and Robustness Checks

If the proximity of bars and restaurants is a key determinant of how individuals respond to blue law changes, then population density might be a more appropriate means of distinguish "large" and "small counties." As it turns out, the most populous counties in the state tend to have

the highest population density. In fact, there are eleven counties in Colorado with an average population density above 60 people per square mile during the time period considered in this study. Ten of these eleven are counties with a population over 100,000. Broomfield is the only county with less than 100,000 people that has a population density over 60 (while Mesa county is the one with a population above 100,000 with a population density below 60). Table 7 reports results for the subset counties with a population density above 60, and the results are similar to those reported in Table 5 (in fact, the point estimates are marginally stronger in magnitude).

Table 6. Effect of Blue Law Repeal, Counties with Population under 100,000

	(1)	(2)	(3)	(4)	(5)	(6)
*Sunday*Post-Repeal*	0.861**	1.805***	1.188***	0.940***	2.025**	1.444***
	(0.367)	(0.524)	(0.367)	(0.364)	(0.793)	(0.533)
	2.344	3.444	3.239	2.584	2.552	2.707
Specification:						
OLS	X	X	X			
Poisson				X	X	X
Control Group:						
Monday through Thursday	X			X		
Friday and Saturday		X			X	
Monday through Saturday			X			X
Observations	1,232	740	1,725	1,232	740	1,725
Adjusted R-squared	0.297	0.553	0.634			
Pseudo R-squared				0.0891	0.276	0.315

Robust standard errors in parentheses. T-stats reported beneath standard errors. In Poisson specifications, marginal effects are reported. Dependent variable is daily count of citations. All specifications use year-month dummies to account for time trend. Day of week and specific holiday dummy variables are also included as covariates. *** p<0.01, ** p<0.05, * p<0.1. Data on DUI citations issued from 2006 to 2010 is from Colorado State Patrol.

Most papers that analyze Sunday liquor store closing laws focus on observed changes in drinking and driving activity on Sundays. While such a focus is logical, it is certainly possible that these laws could lead to changes in drinking and driving activity on other days of the week,

on Saturday in particular.[15] The findings in Carpenter and Eisenberg (2009) suggest that repealing these laws can change the distribution of drinking activity throughout the week, with a substitution away from Saturday drinking. Furthermore, any substitution activity induced by the law would be predicated on the law's restriction binding in some sense (someone, who absent the law, would go to the liquor store on Sunday). It stands to reason that an individual would be able to anticipate whether they would want to make a Sunday liquor store trip more clearly on Saturday than on earlier days of the week. For this reason, I test whether changes in Sunday closing laws affect Saturday DUIs using a similar framework as in the main analysis. I use two control groups: one that includes all other days of the week and one that only includes Friday.

Table 7. Effect of Blue Law Repeal, High-Density Counties

	(1)	(2)	(3)	(4)	(5)	(6)
*Sunday*Post-Repeal*	-1.423***	-1.799**	-1.568***	-1.116***	-2.740***	-1.750***
	(0.509)	(0.724)	(0.514)	(0.400)	(0.721)	(0.495)
	-2.795	-2.485	-3.051	-2.793	-3.799	-3.536
Specification:						
OLS	X	X	X			
Poisson				X	X	X
Control Group:						
Monday through Thursday	X			X		
Friday and Saturday		X			X	
Monday through Saturday			X			X
Observations	1,232	740	1,725	1,232	740	1,725
Adjusted R-squared	0.368	0.519	0.604			
Pseudo R-squared				0.122	0.248	0.291

Robust standard errors in parentheses. T-stats reported beneath standard errors. In Poisson specifications, marginal effects are reported. Dependent variable is daily count of citations. All specifications use year-month dummies to account for time trend. Day of week and specific holiday dummy variables are also included as covariates. *** p<0.01, ** p<0.05, * p<0.1. Data on DUI citations issued from 2006 to 2010 is from Colorado State Patrol.

[15] One paper that considers this to an extent is Lovenheim and Steefel (2011), who find that blue laws have an insignificant effect on the average of Friday and Saturday fatal crashes.

The results are given in Table 8, and are largely insignificant. The one significant effect is a reduction in Saturday DUIs relative to all other days of the week in the less populous counties. The sign of this single significant estimate is in line with the findings of Carpenter and Eisenberg (2009) and would counteract to an extent the observed increase in Sunday DUIs in these counties after the law was repealed.

In Table 8, I also test whether the law change has a measurable spillover effect on drinking and driving on Mondays. I find that the law change has no significant effect on Monday DUI citations in the full state sample, the large county sample, and the small county sample. There is does appear to be a small reduction in Monday DUI citations in the Denver MSA after the repeal, but the lack of a clear and significant pattern in these estimates suggests that any intertemporal substitutions in an individual's drinking and driving activities are not a central issue in terms of the effect of this type of law change.

Table 8. Effect of Blue Law Repeal on Saturday and Monday DUI Citations

	All Colorado			Denver MSA			Counties Over 100,000			Counties Under 100,000		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
*Saturday*Post-Repeal*	-0.421	-0.802		-0.433	0.225		0.146	0.452		-0.567	-1.254**	
	(1.636)	(1.164)		(0.677)	(0.500)		(1.178)	(0.883)		(0.814)	(0.588)	
	-0.258	-0.688		-0.640	0.449		0.124	0.512		-0.697	-2.132	
*Monday*Post-Repeal*			-0.215			-0.627**			-0.344			0.129
			(0.685)			(0.298)			(0.506)			(0.360)
			-0.313			-2.100			-0.679			0.359
Control Group:												
Friday	X			X			X			X		
Sunday through Friday		X	X		X	X		X	X		X	X
Observations	493	1,725	1,725	493	1,725	1,725	493	1,725	1,725	493	1,725	1,725
Adjusted R-squared	0.335	0.719	0.719	0.223	0.504	0.505	0.293	0.618	0.618	0.247	0.634	0.632

Robust standard errors in parentheses. T-stats are reported beneath standard errors. Dependent variable is daily count of citations. All specifications use year-month dummies to account for time trend. Day of week and specific holiday dummy variables are also included as covariates. *** p<0.01, ** p<0.05, * p<0.1. Data on DUI citations issued from 2006 to 2010 is from Colorado State Patrol.

I also run falsification tests, considering the effect that the policy has on mid-week (Tuesday, Wednesday, and Thursday) DUI citations, with the assumption that these days sufficiently removed from Sunday in a temporal sense to be unaffected by the policy. Thus, I expect insignificant estimates. The results are reported in Table 9, showing the law change has no significant effect on DUI citations on any of these days in the full state sample, the large

county sample, and the small county sample. While there are measured increases in DUIs in the Denver MSA on Wednesdays and Thursdays after the ban is repealed (in contrast with the reduction in Monday DUI citations in these counties after the ban was repealed), the overall results of these tests are in line with expectations.[16]

Table 9. Falsification Tests Related to Mid-week DUI Citations

	All Colorado	Denver MSA	Counties Over 100,000	Counties Under 100,000
*Tuesday*Post-Repeal*	0.442	-0.393	0.0199	0.422
	(0.759)	(0.323)	(0.578)	(0.384)
	0.583	-1.217	0.0344	1.100
*Wednesday*Post-Repeal*	0.929	1.063***	0.705	0.224
	(0.744)	(0.373)	(0.574)	(0.380)
	1.249	2.850	1.229	0.591
*Thursday*Post-Repeal*	1.159	1.217***	0.976	0.182
	(0.793)	(0.397)	(0.631)	(0.390)
	1.461	3.069	1.548	0.467
Observations	1,725	1,725	1,725	1,725
Adjusted R-squared	0.719	0.505	0.618	0.632

Robust standard errors in parentheses. T-stats are reported beneath standard errors. Dependent variable is daily count of citations. All specifications use year-month dummies to account for time trend. Day of week and specific holiday dummy variables are also included as covariates. *** p<0.01, ** p<0.05, * p<0.1. Data on DUI citations issued from 2006 to 2010 is from Colorado State Patrol.

VI. Conclusion and Discussion

The results in previous studies of the effects of Sunday closing laws on drinking and driving are varied, and this analysis shows that a factor in explaining this is a heterogeneity in the effect of such laws—the effect depends upon the population of the area, for instance. Looking at statewide DUI counts in Colorado, the relationship between Sunday closing laws and DUIs is

[16] In an unreported analysis, I re-ran the linear regression analysis under the assumption that the policy came into effect one-year earlier than it actually did and one year later than it actually did. I expect the results to be dampened when these two false temporal cut-points (July 1, 2007 and July 1, 2009) are used instead of the true cut-point. Since these false cut-point regressions still correctly code many of the observations as pre-policy change and post-policy change respectively, these regressions in all likely capture some fraction of the actual effect of the policy change but will presumably bias the effect toward zero. The magnitude of the point estimates are smaller in a large majority of these specifications, suggesting that the policy is affecting drunk driving activity. While typically smaller, none of the point estimates from the models using false cut-points is significantly different (at the 5% level) from the analogous specification using the true cut-point. This is likely due to the fact that many of the observations are correctly classified as either pre-policy or post-policy in these alternative specifications.

insignificant, but this masks the fact that two separate effects are working in opposite directions. When various subsets of counties are considered, the results suggest that Sunday closing laws do affect the individual's decisions about where and how much to drink on Sundays, but that nature of these effects are very different for more populous settings than they are for less populous settings.

Repealing these laws leads to less drinking and driving in more populous counties but more drinking and driving in less populous counties. Possible explanations depend on the incentives consumers have to drink away from home in the face of this constraint. If on balance, drinking distilled spirits in restaurants and bars offer a relatively attractive option to consumers in higher population areas (due to their higher incomes, increased quality, increased variety, or more convenience, for instance), these findings make sense. If consumers in less populated areas find more substitutability between distilled spirits and low-alcohol beer that is available for purchase on Sunday than do consumers in more populated areas, it could lead to substitutions that are consistent with the results. Substitution between distilled spirits and illicit drugs may change in different ways in these locations as well, leading to differential policy effects. Future research that empirically tests these possible explanations against one another would be useful in illuminating the nature of these effects.

This is essentially a study that focuses on the level of a particular social cost—the harm caused by drinking and driving—that is affected by this policy. This policy likely affects consumer surplus in the distilled spirit market and potentially brings with it other costs as well. The findings along this dimension suggest that these policies can impact different parts of the state in different ways, and that heterogeneity in the effect should be a consideration when crafting these policies.

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Appendix

The data used in this analysis is from the records of the Colorado State Patrol, and it contains information on individual DUI citations issued between January 2006 and December 2010. There are 35,585 individual DUI citations used in the analysis. For each citation, there is information on the time and location at which the citation was issued, and that information is used to generate the citation counts that form the basis of the analysis. Three months of data between late September 2009 and December 2009 are missing due to a technical issue with the way the data was reported, and thus no citations from this time period are included in the analysis.

Data on county population (which was used to divide counties into subsamples) comes from the Colorado's State Demography Office's estimates, which were retrieved from http://www.colorado.gov/cs/Satellite/DOLA-Main/CBON/1251593346834.

Social Choice and the American Revolution

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Abstract

The stated goals of the American Revolution were the protection and promotion of inalienable rights. Life, liberty, and the pursuit of happiness were purportedly to be maximized, if the colonists could throw off their colonial chains. The long term effects of the American Revolution can still be seen today. The US population greatly exceeds that of other similar British offshoots (for example: Australia, Canada, and New Zealand), but the US lags behind these same countries in measures of life expectancy, economic freedom, wealth, and happiness. This study illustrates that the success or failure of the American Revolution at maximizing life, liberty, and happiness for generations of Americans can only be measured by examining social welfare. Furthermore, it highlights the need for an examination of social welfare effects for future war and policy decisions.

Keywords: General Welfare, Wellbeing, National Security and War, Comparative Economic History

JEL Codes: I31, H56, N10

This study examines the very act that established the US as a country in order to show the importance of social welfare construction and consideration for evaluating the impact of public policies. Using a social choice framework to compare outcomes in the US with those in other British offshoots, the paper demonstrates that whether the revolution is deemed a relative success or failure is wholly dependent on how, and for whom, welfare is counted. The first section examines the impact of the American Revolution on two prominent social welfare functions constructed from income and inequality. The examination then extends to measures of life, liberty, property, and happiness. Most importantly, this study illustrates the importance of investigating the social welfare effects of future war and policy decisions for varying definitions of society. As far as whether or not the American Revolution was a success at maximizing social welfare, the answer depends on how, and for whom, welfare is counted.

I. Constrained Social Welfare Functions

The Founding Fathers argued that revolution should only be engaged in if it were in the best interest of American colonists. The US Declaration of Independence states in its third paragraph,

> We hold these truths to be self-evident, that all men are created equal, that they are endowed by their Creator with certain unalienable Rights, that among these are Life, Liberty and the pursuit of Happiness. --That to secure these rights, Governments are instituted among Men, deriving their just powers from the consent of the governed, --That whenever any Form of Government becomes destructive of these ends, it is the Right of the People to alter or to abolish it, and to institute new Government, laying its foundation on such principles and organizing its powers in such form, as to them shall seem most likely to effect their Safety and Happiness (1776).

The following constraining assumptions are made in order to understand the Founding Fathers' perception of a social welfare function:

1. All men (and women) are created equal. As such, every person's utility should be counted toward social welfare as long as the US government maintains governing jurisdiction over him or her. This means that the utility of persons not governed by the US government is not considered. This applies to such people as Native American tribes as long as they were governed as independent nations. The utility of Native Americas who assimilated into American society and were subject to the US government must be counted. The utilities of citizens of foreign lands (even those lands that would later be annexed into the US) are only considered once they become subject to rule by the US government.

2. All men (and women) have the right to select a government such that it maximizes their safety and happiness subject to the protection of inalienable rights.

Inalienable, according to Webster's Dictionary means "incapable of being alienated, surrendered, or transferred" (2014). That is, even if the acts of taking an innocent life, depriving an innocent man of liberty, or taking the property of an innocent man without just compensation increase total utility, they are not allowed.

It is important to note that this is a strict interpretation of the Declaration of Independence. Obviously, the fact that some signers owned slaves is proof that they did not believe that these rights truly extended across all racial bounds. This study confines its analysis

solely to maximizing the literal interpretation in order to examine in what way, according to the principles put forth in the Declaration of Independence, the War of American Independence improved social welfare. Whether revolution was optimal for the signers thereof is a political economy question for further research.[17]

As J.C.D. Clark noted, "It is not surprising that so few American historians have ventured seriously to question the 'manifest destiny' of the United States with counterfactual enquiries. Those few writers who have imagined American history without independence have tended to treat the idea as a joke" (1999, p. 126). And yet, "Because decisions about the future are-usually-based on weighing up the potential consequences of alternative courses of action, it makes sense to compare the actual outcomes of what we did in the past with the conceivable outcomes of what we might have done" (Ferguson 1999, p. 2).

The United States is just one of many former British colonies that now engage in self-rule. Other former British colonies with similar histories but no revolutions serve as a useful benchmark for social welfare analysis in order to understand the American Revolution's impact on life, liberty, and the pursuit of happiness[18]. Table 1 shows that, when measured today, the US trails Australia, Canada, and New Zealand in measures of life (life expectancy), liberty (economic freedom), property (median adult wealth), and happiness. At first glance, it appears that British offshoots that opted against a violent revolution served to increase their average levels of life, liberty, property, and happiness to higher degrees than did the United States. The US compares more favorably under alternative social welfare functions.

[17] Sawers (1992) suggests narrow economic interests of the patriots relative to the Navigation Acts may be the chief cause of the Revolution. Baack (2001) outlines a more thorough overview of the economics of the American Revolution.

[18] While many former British commonwealths could be chosen for comparison, the present analysis is restricted to Australia, Canada, and New Zealand because they exhibited similarly large ratios of European settlers relative to the slave and indigenous populations.

Table 1

Comparison of Life, Liberty, Property, and Happiness among British Offshoots				
Country	**2010 Life Expectancy**	**2014 Rank Economic Freedom**	**2013 Median Adult Wealth**	**2010 Gallup Happiness Rank**
Australia	81.63	3rd	$219,505(1st)	8th
Canada	81.23	6th	$90,252 (13th)	8th
New Zealand	80.36	5th	$76,607 (15th)	6th
United States	78.11	12th	$44,911 (27th)	14th
Sources: *The World* Fact Book, The 2014 Heritage Foundation Rank of Economic Freedom, Credit Suisse 2013 Global Wealth Report, and 2010 Gallup Poll				

In order to measure social welfare, one must first define society (Strow and Strow 2011). In this case, one must determine whether society refers to the founding fathers, white male 1776 inhabitants of thirteen American colonies, all 1776 inhabitants of the thirteen American colonies (including slaves and Native Americans), all 1776 inhabitants of the land that would later become the domain of the United States of America, or all 1776 members of the human race. The justification for slavery or wars against Native Americans would depend largely on how broadly society is defined. Society must likewise be defined by time. Does society refer to just those alive in 1776 or to those who were yet to arrive in America, by birth or immigration, as of that date?

To measure social welfare, one must also specify the social welfare function. Atkinson suggests that normative economic claims regarding social welfare must be examined through varying social welfare specifications (2009). Two prominent competing social welfare functions have been offered by Jeremy Bentham (1789) and Amartya Sen (1982).

II. Social Welfare as Measured by Sen and Bentham

The utilitarian social welfare function, as proposed by Jeremy Bentham, can loosely be described as seeking to maximize the greatest good for the greatest number of people. It seeks to maximize the summation of individual utility over all members of society. Under a utilitarian framework, the success of the American Revolution would be dependent upon achieving the greatest good for the greatest number of people over time. In a strict utilitarian framework, institutions (such as slavery) or actions (such as the annihilation of Native Americans) could be justified if they were shown to increase overall social welfare.

In contrast, Amartya Sen's social welfare function takes both income and income inequality into account. It is a function of per capita income multiplied by (1-Gini coefficient). Richer societies with lower levels of income inequality have higher levels of social welfare under Sen's definition than poorer societies or societies with more income inequality. The successfulness of the American Revolution, according to Sen's definition, would be measured by examining its effects on per capita income and income inequality.

Sen's social welfare maximization would take the following form:

$$Max \sum_{j=1}^{t} ((Per\ Capita\ Income\ (1 - Gini))\ \mathrm{j}$$

subject to the protection of inalienable rights, where j= years.

Table 2 shows that if measurements were taken today, the US would rank third out of these four British offshoots in terms of their Sen social welfare score. This comes despite the fact that the US leads the four offshoots in terms of per capita GDP. Income inequality in the US is higher than in any of the other offshoots. Table 3 shows that during the 19th century, the US

trailed Australia and New Zealand in per capita income. The US fared even less favorably under Sen's social welfare function during the 19th century relative to other British offshoots.

Table 2

Bentham and Sen's Social Welfare Scores					
Country	**Population (2014)**	**Per Capita GDP (2012 $)**	**Gini (2011)**	**Bentham Score (billions $)**	**Sen Score (thousands $)**
Australia	23,427,253	41,954	30.5	983	29,158
Canada	35,344,962	42,317	32.6	1,496	28,522
New Zealand	4,519,640	29,481	36.2	133	18,809
United States	317,746,000	51,704	45.0	16,429	28,437
Sources: IMF 2012 per capita income and The World Bank 2011 Gini Index					

In contrast, the utilitarian social welfare maximization would take the following form:

$$Max \sum_{i=1}^{n} \sum_{j=1}^{t} \text{Uij}$$

where U = individual utility, i = individuals, and j = years.

If per capita income is used as proxy for individual utility, then the US (by far) has created the highest level of social welfare of any country in world history. Table 2 shows that not only does the US have the highest level of per capita GDP of any of the British offshoots under consideration, it also contains a much larger percentage of the world's population.

Table 3

Per Capita Income $1990			
	1820	1870	1900
Australia	$1,528	$3,801	$4,299
Canada	$893	$1,620	$2,758
New Zealand	----	$3,115	$4,320
US	$1,287	$2,457	$4,096
Source: Angus Maddison 1995.			

Two facts are indisputable regarding the development of the US economy relative to other British offshoots. The first is that since its founding, the US has provided more people with life, liberty, and happiness than other British offshoots. Given that most immigrants (not slaves or prisoners) to the former British colonies went freely, the fact that so many chose to immigrate to the US serves as a revealed preference for where they thought their happiness would be maximized relative to transportation costs. The second is that the US developed with higher levels of inequality than other British offshoots. In contrast to other British offshoots, the US has served as the land of opportunity rather than the land of guaranteed success.

While it is helpful to use other British offshoots as proxies for growth and development without revolution, factors other than revolution can explain differences between British offshoots. Geography, climate, availability of natural resources, the use of slavery, and presence of natives are just a few other factors that affected growth and development. Nevertheless, there are key development differences between the US and other British offshoots that likely stem from the nature and timing of the American Revolution.

III. The Impact of the American Revolution on Life, Liberty, and Property

Jeremy Bentham understood that social welfare comes from more than just income or income inequality. So too did the founding fathers of the US. In writing of the inalienable rights of life, liberty, and the pursuit of happiness, the signers of The Declaration of Independence drew from Locke's natural rights of life, liberty, and estate (1690). Concretely, social welfare can be considered to be a function of life, liberty, and estate (property). Life is a function both of the quantity and quality of life a person attains. One's liberty is a function of whether or not one is enslaved or has civil, political, and economic freedom. One's property is a function of the amount and security of one's personal wealth taking into account the public obligation of one's wealth. Thus,

$$\text{Social welfare} = \text{f}(\text{Life}, \text{Liberty}, \text{Property})$$

$$\text{Life} = \text{f}(\text{life expectancy}, \text{health})$$

$$\text{Liberty} = \text{f}(\text{bondage}, \text{civil liberties}, \text{political freedom}, \text{economic freedom})$$

$$\text{Property} = \text{f}(\text{wealth}, \text{security of property rights}, \text{taxes})$$

where the above measures are applied to all people under the jurisdiction of the US government throughout time.

The next step is to systematically analyze each of the subcomponents of social welfare to examine how the American Revolution impacted them. The purpose of doing so it to see how social welfare was affected by the American Revolution in order to illustrate the importance of social welfare considerations in public policy.

A. Life

Life expectancy at age ten in the American colonies rose steadily from 52 years in 1730 to a peak of 56.7 years in 1790 (less than 10 years following the end of the American

Revolution). "…they [colonists] reached levels of life expectancy that were not attained by the general population of England or even by the British peerage until the first quarter of the twentieth century" (Fogel 1986, p. 466). However, this was followed by a steady decline in US life expectancy such that by 1855, life expectancy at age ten had fallen to 47.8 years (Ibid.). The American Revolution was a negative turning point for US life expectancy.

This fall in life expectancy is often attributed to a decline in the environmental conditions in which Americans lived. Increased industrialization and urbanization initially resulted in higher levels of pollution and greater incidence of disease. Increased immigration by less healthy immigrants may also have decreased average US life expectancy.

Additionally, the American Revolution, the War of 1812, and the US Civil War resulted in thousands of American casualties, lowering average US life expectancy. While the first two wars were a direct result of actions to declare independence from Great Britain, the latter was an indirect result. Great Britain ended slavery in its empire without war. The American Revolution and subsequent US Civil War accounted for well over six hundred thousand Americans who were killed or wounded.

On the other hand, if the colonies had remained part of the British Empire, they would have entered into WWI and WWII earlier, which could have increased their casualties from those conflicts. In entering the war much later, the US only lost .13% of its population fighting WWI as compared to losses of 1.64% in New Zealand, 1.38% in Australia, and .92% in Canada. While the casualty rates for WWII remained lower for the US than other British offshoots who still viewed the British Monarch as their figurehead of state, the gap was smaller than it had been in WWI. The US lost .32% of its population in WWII compared to New Zealand's .72%, Australia's .57%, and Canada's .40%. Membership in the British Empire would possibly have

raised some US war casualty rates, particularly in WWI. Even so, the US would likely have suffered fewer casualties from WWI and WWII combined than they suffered in the US Civil War.

Not only did life expectancy fall during the first half of the 19th century, so too did the average height of Americans. Cliometricians have long used height data as a measure of physical and economic health. At the time of the American Revolution, the average height of male Americans was 173.2 cm. Americans were the tallest population in the world. Their taller stature was due to enhanced nutrition, (higher levels of protein), better health, and the taller heights of their parents and grandparents (Atack and Passell 1994).

Height data is a lagged measure of well-being since the data reflects not only the nutritional level of the person whose height is being measured, but also the nutritional level of that person's parents and grandparents. For instance, US born Union Army recruits in 1860 whose parents were foreign born were .4 inches shorter in final height than native born recruits of native born parents (Fogel 1986).

Average US male height fell from 173.5 cm in 1830 to 168.9 cm in 1882 (Ibid). Given the generationally lagged nature of height data, this data is consistent with the US life expectancy data. American health was in steady decline for the time period between the American Revolution and the US Civil War. Both height and life expectancy started increasing again thereafter. Part of the decrease in life expectancy and decrease in average height is due to immigration from shorter immigrants. It is also possible that US independence led to decreased life expectancy and health if independence accelerated industrialization.

Great Britain's policies toward her colonies discouraged industrialization. Paul David (1970) and others have suggested that Protective tariffs promoted by Alexander Hamilton and

implemented in the first half of the 19th century (hitting their peak with the Tariff of Abominations in 1828) may have increased the rate of industrialization and urbanization in the US above where it would have been had the colonies remained part of the British Empire. However, this contradicts the idea that international trade encourages the transfer of knowledge between regions. Thus, it is not obvious that industrialization under British rule would have been slowed.

British policies that restricted immigration and possibly slowed industrialization had helped to make the population of the thirteen colonies the tallest and longest living in the world. These same policies led other British offshoots to increase their average life expectancy and health relative to the US. Rejection of these policies through independence led to health declines for the average American. However, independence extended longer and taller lives to waves of immigrants and their descendants who were more freely able to immigrate to the US. Thus, the American Revolution lengthened life expectancy for some while it decreased it dramatically for others.

B. Liberty

The American Revolution did not prevent the US from adopting British common law. As such, the rule of law generally differed little from either colonial America or other British offshoots (notwithstanding a nominal British monarchy). Republican legislatures were used in the colonies to oversee most of the operation of government. The American Revolution did succeed in replacing appointed governors with elected executives. While other British offshoots gained political independence from Great Britain peacefully, this likely was a by-product of the American Revolution. While the US did not invent democracy, it did move the idea of

democracy forward on a world scale. Still, the US lagged other British offshoots in extending the right to vote to women.

As US regulatory law generally followed British common law, the only major economic regulatory differences between colonial rule and US rule were trade restrictions. British mercantilist policies had clearly annoyed many American colonists, particularly in New England. However, bounties and preferential treatment for some American goods, such as indigo, had diminished the negative impact of British mercantilist policies.

Robert Thomas placed the annual per capita burden of colonial membership in the British Empire at $0.26 from 1763-1772, which was less than one percent of colonial incomes (1965). He includes the cost of not exporting tobacco and other products directly to their final markets and the benefits of bounties and preferential treatment towards American products. What he fails to include is the true opportunity cost of being on the outside of British mercantilist policies. Independent of British mercantilism, colonial goods lost subsidization and also faced increased trade barriers and import duties to British North America and the Caribbean.[19]

American independence generated more trade barriers than it lowered. Thus, colonial independence was the enemy of freer American trade and so, ran counter to that form of economic liberty. The Tariff of Abominations in 1828 was seen by many as being so opposite the cause of liberty or wealth creation that the new country faced a nullification crisis. As Table 4 illustrates, US per capita imports and exports were retarded by colonial independence and did not reach pre-revolutionary levels until the eve of the US Civil War (with the exception of the brief period of US neutrality during the Napoleonic Wars. Reductions in international trade not

[19] Today, Canada is the largest trading partner of the US. Further research should examine the full extent of lost wealth creation caused by trade barriers between the US and Canada that resulted from American independence.

only meant less specialization of labor, it also meant a reduction in the exchange of ideas and knowledge across borders.

Table 4

American Terms of Trade 1770-1860 (in 1790 $)			
Year	**Per Capita Exports**	**Per Capita Imports**	**Per Capita Trade Surplus**
1770	$7.20	$8.89	$-1.69
1790	$5.14	$5.91	-$0.77
1800	$9.74	$12.50	-$2.76
1810	$6.71	$8.51	-$1.80
1820	$6.61	$6.99	-$0.38
1830	$5.75	$5.43	$0.32
1840	$6.81	$5.38	$1.43
1850	$5.63	$6.96	-$1.36
1860	$9.40	$9.96	$0.56
Source: US Bureau of Labor Statistics			

Other British offshoots also responded to independence with larger trade barriers. As such, revolution itself seems to carry very little weight as an argument for why economic liberty fell in the US. Rather, protectionists in all British offshoots found it easier to capture local politicians than politicians far off in Great Britain.

The complaints toward Britain regarding civil liberties were the following: Britain's limitations on the number of immigrants allowed into the colonies, their quartering of armies in peace time, their protecting soldiers from murder charges, and their depriving some people of a trial by a jury of their peers. US independence did initially bring with it easier immigration into the Americas, an end of forced quartering of armies in peace time, and judicial guarantees to a

trial by one's peers. Civil liberties, however, were not extended to slaves, and were in many ways deprived from freed slaves for generations following emancipation. Civil liberties were also compromised by the use of military drafts by both sides in the US Civil War and the use of white conscript slave patrols to monitor slave mobility.

The most notable difference between the US and other British offshoots (outside of the Caribbean) was its continued reliance on slavery as an economic institution. The 1833 Abolition of Slavery Act set in motion the end of slavery in the British Empire. This act freed slaves less than 6 years of age in 1834 and stipulated a four-year apprenticeship for all other slaves. Slave owners in the British Empire were paid a total of £20 million to compensate them for their losses. In contrast, the United States didn't end slavery until 1865, and did so without compensating owners for their losses[20]. The presence of a sizable slave population in the Americas may have pushed back the emancipation date in the British Empire beyond 1838, but it is unlikely to have pushed it back to 1865, since that was well beyond the European norm[21].

While the abolition of slavery eventually came to the Americas, it took even longer for civil liberties to be granted to former slaves. Furthermore, the presence of slavery retarded immigration to slave states. According to the 1860 US Census, only 4.4% of the population in slave holding states was foreign born compared to 18.7% in free states.

And so, the American Revolution likely delayed or decreased liberty for slaves, women desiring the right to vote, and those engaging in international trade. At the same time, it

[20] Compensated emancipation in the US would have been far cheaper in 1834 than in 1865 for three reasons. First, the number of slaves needing emancipation would have been 2 million less. Second, slave prices appreciated greatly during this time. The average slave price went from $974 (1831-1835) to $1,596 (1856-60) (Atack and Passell 1994, p 332). Third, the tax base of Great Britain (if the US colonies were included) was 36.7 million in the early 1830's vs. a population of only 31.2 million in the US in 1860.

[21] As the present social welfare function only applies to those under US jurisdiction, the welfare loss of a later emancipation to other British slaves outside of the US is not considered here.

expanded democracy and potentially increased liberty for those wishing to immigrate to the US, for those who otherwise would have been forced to quarter armies, and for those benefiting from a jury trial.

C. Property

The American Revolution caused a reduction in property rights to finance the war, created a new country that was taxed more heavily than the British taxation of the 13 colonies, and lowered US 19th century per capita GDP below that of comparable British offshoots. The US government funded the Revolutionary War largely through inflation and confiscation of loyalist property. The Continental Congress printed money in mass quantity. While the Continental dollar originally traded at par with specie in 1775, by 1781 the Continental dollar was worth less than one percent of its face value. (Calomiris 1988). Colonists who traded property for Continental dollars suffered great loss.

If a person chose to be loyal to the government of their youth, their property was subject to confiscation by American authorities to be sold at auction to finance the war. Approximately 70,000 loyalists, or roughly 3% of the population left the US during the American Revolution. Approximately 350,000 to 400,000 colonists were loyalists. Many other colonists were neutral in the American Revolution. The Patriots did not even garner the active support of a majority of colonists in the thirteen colonies (Calhoon 2000). Following 1781, the colonial army began to scavenge what they could find from farmers who lived in their path.

As Table 5 illustrates, citizens of the American colonies were taxed at a much lower rate than their British counterparts. Colonial legislatures controlled the levels of taxation in each colony, and chose low levels of taxation. These low levels of taxation were subsidized by British citizens. That is, colonists were able to enjoy national defense, civil rule, and a judicial system

with economies of scale and partially subsidized by British taxpayers. In 1778, Parliament, under the leadership of Lord North, offered the 13 colonies dominion status along with the repeal of all revenue measures from 1763 onward. Such status would have given colonial legislatures complete control over their taxation, trade, and foreign affairs. John Adams, Thomas Jefferson, and James Wilson had, in 1774, argued that dominion status was what they wanted for the colonies, yet America declined the offer.

Table 5

Index of Per Capita Tax Burdens in 1765	
Country	**Index (Great Britain =100)**
Great Britain	100
Ireland	26
Pennsylvania	4
Maryland	4
Massachusetts	4
New York	3
Connecticut	2
Virginia	2
Source: Gerald A. Gunderson (1976) p. 89	

National defense is cheaper when a country is on friendly terms with the world's military superpower rather than openly antagonistic toward it. Further, military protection benefits from economies of scale. By declaring independence from Great Britain, the American colonists ended the centuries-long free riding of national defense expenditures. Thus, independence from Great Britain carried with it a greater cost for military protection that was reflected in the higher

tax burdens for US citizens. This may be in part explain why tax rates in the US immediately following independence were higher than those imposed under British rule (Perkins, 1988).

Not only were tax rates affected, but output was likely affected as well. It wasn't until WWI that US per capita GDP caught up to that of Australia – another British colony who opted to take the route of dominion status. Not until the 20th century did the US surpass New Zealand in terms of per capita GDP. Likewise, Canada's per capita GDP grew at a faster rate than that of the US between 1820 and WWI (Walton and Rockoff 1998). While the 20th Century can certainly be called the American Century, the 19th Century cannot. The 19th Century belongs to Great Britain. Per capita incomes in Great Britain and her loyal offshoots of Australia and New Zealand were greater than in the US for the duration of the 19th Century. The American System of high tariffs, military animosity toward Great Britain, and the inability to avoid a costly civil war cost the US economy dearly in the 19th Century. The 19th Century economic malaise of the US might have been avoided if the colonists hadn't declared independence in 1776.

And yet, the United States has extended a higher standard of living to more people than any other British offshoot. Restrictive immigration laws as enforced by Great Britain and later adopted by other British offshoots themselves greatly limited access to wealth creation for those not fortunate enough to be born in their political borders. Where would immigrants who chose the US have lived had borders been kept relatively closed as they were in other British offshoots? Arguably these individuals benefited immensely from being able to immigrate to the US instead of to their next best option. In this way, the American Revolution may well have facilitated the world's largest wealth creation by a nation to date. It also worked to increase income inequality by attracting more impoverished people to immigrate.

IV. Conclusion

The answer to whether or not the American Revolution improved social welfare largely depends on how society is defined and which components of social welfare receive the highest weight. At over 300 million residents and the highest level of per capita GDP among British offshoots, the United States has created more income for more people than has any other country in world history. With the lowest tax burdens among British offshoots, these same people in the US are able to hold onto a larger percentage of their property. Wave after wave of immigrants over the years have chosen to move to the US based upon their belief that doing so would increase their welfare more than going elsewhere. The ability of the US to take in poor immigrants on a continual basis from around the world and allow them to create their own wealth has to be the US's largest contribution to social welfare.

However, other British offshoots that did not engage in violent revolution from Great Britain have longer average life expectancies, encounter greater economic freedom, experienced a quicker emancipation of slavery, face more secure property rights, have a higher median adult wealth, and are happier. With fewer immigrants from the world's poorest countries, the other British offshoots chose to create a higher average standard of living mainly for themselves and a small number of immigrants from elsewhere in the British Empire.

Had American colonies not declared independence in 1776, the social welfare of African Americans and Native Americans would likely have been higher. Emancipation of slaves in the British Empire occurred a generation ahead of when it took place in the US. Great Britain also limited American colonists territorial claims on Native American lands. By only allowing a limited number of specific immigrant groups access to their American colonies, British rule would have slowed immigration to the US which would have created lower levels of income inequality.

A utilitarian social welfare function defining the society as those who would ultimately become under the control of the US government suggests that the American Revolution was a resounding success. It brought income and liberty to the world's masses who immigrated to the US. A social welfare function constructed by Sen defining society as those who existed in the physical boundaries of what would become the US suggests that the American Revolution lowered social welfare over where it would have been had the revolution not occurred. In the end, the US can claim a utilitarian victory while other British offshoots can claim victory using Amartya Sen's vision of social welfare.

Was the American Revolution a welfare increasing event? It depends how social welfare is measured. Different measures provide different answers to the same question. Likewise, as economic policies and wars are considered in the future, policymakers must consider carefully not only which social welfare function they are using to measure the costs and benefits of the policy but also whose utility they are attempting to maximize in their definition of society.

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