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The Impact of Connecticut's Clean Election Law: An Empirical Quick Look

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Abstract:

The State of Connecticut's General Assembly passed a Clean Elections Law in 2005. In this paper we conduct a preliminary appraisal of the law's performance based on recently published data on the voting results of the 2010 and 2008 state-wide office elections. The Clean Elections Law was considered among the most stringent in the nation at the time of its passage. It established full public financing for all elections to state offices, including the state legislature. The law applied to primaries as well as general elections. It allowed for supplemental monies in unbalanced contests pitting a privately-financed candidate against a publicly-financed one. The law also contained provisions banning campaign donations from lobbyists and state contractors.

Our study is similar to the 2009 one prepared by the Office of Legislative Research but with the benefit of additional data drawn from the 2010 election cycle. Importantly, we conduct our examination using statistical tests with significance thresholds at conventional 95 percent levels. We also add additional performance metrics to provide a wider lens to the appraisal. We use resampling methods to draw multiple simulated samples to calculate statistical significance. Resampling techniques provide a non-parametric determination of a statistic's distribution and a measure of effectiveness that is not sensitive to deviations from the assumptions underlying most parametric procedures.

Based on the results derived from statistical tests of the assembled metrics it is difficult to conclude that the public funding of elections in the State of Connecticut is an unqualified success, or for that matter, a qualified success. It appear that the one conclusion that we can unambiguously draw is that the effusiveness and optimism of the various commentators supporting clean election laws has not yet come to be realized in the State of Connecticut.

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Introduction

> *It is morally as bad not to care whether a thing is true or not, so long as it makes you feel good, as it is not to care how you got your money, as long as you have got it.*
>
> *Edwin Way Teal (Circle of the Seasons, 1953)*

Clean election laws aim to fund races for state assemblies with public monies, simultaneously proscribing any number of practices including limiting or altogether eliminating private funds expended in support of any candidate (General Accounting Office 2003, Zagaja 2009). Clean elections laws, in the view of advocates, constitute a remedy for a variety of social ills including government corruption, excessive interest-group influence, wasteful and excessive campaign spending, minimal electoral competition and lethargic individual voter participation (Mayer, Werner and Williams 2005).

In 2005, the State of Connecticut's General Assembly passed a Clean Elections' Law, possibly the most stringent in the nation at the time (Nyhart 2006). The Citizens Election Program established full public financing for all elections to state offices, including the state legislature (Mayer and Werner 2007, Zagaja 2009). The law applied to primaries as well as general elections. It allowed for supplemental monies in unbalanced contests pitting a privately-financed candidate against a publicly-financed one. The law also

contains provisions banning campaign donations from lobbyists and state contractors.

There are only three formal appraisal of Connecticut Citizens' Elections Program (to our knowledge): an analysis by Sullivan, of the Office of Legislative Research (Sullivan 2009) and a study by Zagaja (Zagaja 2009). In addition, Parnell conducts an interesting albeit limited study examining whether the election law has altered voting patterns of legislators (Parnell 2010).

Sullivan examined the effect of the legislation on (i) voter choice, (ii) electoral competition, (iii) voter participation and (iii) program participation data by examining changes between the 2006 and the 2008 election cycle. Although not testing for statistical significance the findings of the Sullivan's Office of Legislative Research study were charitably inconclusive at best, noting that: "it is too early to draw any causal linkages to changes, if any, that resulted from the public financing programs (Sullivan 2009)".

Zagaja examines quantitative and qualitative changes in several metrics between 2004 and 2008: (i) electoral landscape, (ii) participations, (iii) electoral competition, (iv) diversity, (v) decreasing the actual or appearance of influence by interest groups and candidates, limited excessive campaign spending, allowing candidates to spend less time fundraising, and, (vi) increasing voter confidence and participation. Zagaja's results are decidedly mixed. Although Zagaja does not rely on statistical testing in arriving at

conclusions his results are instructive nevertheless perhaps because of the added perspective provided by the various qualitative measures attempted. Zagaja's findings suggest that the electoral law's goals were met in only two of the seven metrics he examines: (i) participation, and, (vi) allowing candidates to spend less time fundraising. As for the remaining six measures, they were either inconclusive or conferred no support for the electoral law claim. Specifically: scrutiny of (ii) electoral competition, (iii) diversity, (iv) decreasing the actual or appearance of influence by interest groups on candidates, and, (v) limiting excessive campaign spending, proffered no support for the electoral law thesis.

Parnell takes aim at the special interest rationale of the Connecticut election law (Parnell 2010). He measures changes in the voting patterns of legislators who served in the Connecticut General Assembly during the 2007-08 session and accepted taxpayer dollars for their 2008 reelection campaign. Specifically, he argues that by identifying significant interest groups and comparing their legislative priorities to voting patterns, a finding of a noticeable change in voting since the beginning of election law constitutes support for the argument that freeing legislators from private, voluntary contributions has indeed made legislators more responsive to citizens and less responsive to so-called special interests (Parnell 2010). Parnell's more limited and more focused study finds "no evidence to support the contention that providing taxpayer dollars to legislative candidates

reduces the likelihood that a legislator will vote with an interest group"

In this paper we conduct an appraisal similar to the Office of Legislative Research (Sullivan) study but with the benefit of additional data drawn from the 2010 election cycle. Importantly, we conduct our examination using statistical tests with significance thresholds at conventional 95 percent levels. We also add other performance metrics to provide a wider lens to the appraisal.

There as two important qualifications regarding our results. First, there are neither theoretical nor standard metrics that can be invoked *ex ante* in the examination of legislation such as the election law of Connecticut. Although one can conceivably assemble a significantly large number of informative metrics appraising the impact of election laws generally, several authors consistently examine the same few variables. We examine the Sullivan metrics and a few others popular with researchers. However, and for purposes of establishing criteria for success - few of these metrics are outcome metrics; all are instrumental ones. Thus, success is in terms of the particular realization of the instrumental metric and not necessarily in terms of ultimate outcomes – however defined.

For instance, consider the metric 'voter participation.' If the number of voters increases after the adoption of the election law, all else equal, one could attribute the increase to the law and

thereby argue that the law has been shown to be a success. However, no such conclusion can be established if the ultimate outcome of interest is the *economic* fortunes of the state – gauged in terms of 'state product per capita,' 'the unemployment rate,' or, 'construction permits' or any other representative result.

The second concern is that no one metric is privileged and therefore several instrumental metrics are examined. Similarly, there is no conceptually distinct and a priori aggregation weighting scheme. Thus, to the extent that the examined metrics convey different and possible contradictory inferences there can be no definitive way to conclude as to whether the election law was successful – even if we examine only instrumental metrics.

Our results reflect those of Sullivan, Zagaja and those of Parnell. Our efforts at determining the significance of the law are mixed: some metrics do suggest statistically meaningful differences whereas many others don't. No results are evident in the 2008 cycle. With token exceptions, all of the minute differences observed, when they are in fact observed, occur by the 2010 election cycle.

Ironically, a law aiming to enhance electoral competition in the state of Connecticut strengthened the position of the Democrats in a robustly blue state: the results of several metrics appear to have bestowed a slight edge to Democrats, at the expense of Republicans. That the legislation may have benefited Democrats

may be ironical but it is also unsurprising. Notwithstanding house republican leader Larry Cafero's observation that "the CEP made it easier for his party to recruit candidates to run for office in uncontested districts,"[2] one would think that election law proscriptions handicapping the ability of potential candidates to raise money would most likely affect the generally more affluent republicans.

Some caveats: despite the poor statistical showing of the election law performance metrics in Connecticut it is distinctly possible that the election law succeeds in "cleaning-up" the observed qualitative electoral-related ills, as has been argued. On the other hand, because of remaining porosity in the financing system the current version of the law may not sufficiently curtail the continuing practice of indirect flow of private funds to benefit favored candidates. And given the curtailing presence of *Citizens United* it is not clear whether there can be any further tightening (Citizens United v. Federal Election Commission 2010). Thus, we cannot *conclusively* claim that the law does not work or that it works badly; and we do not claim as much. Nevertheless, it does appear that any benefits of clean election laws are more evident in the telling than in reality.

[2] Cited in (Zagaja 2009).

The Backdrop

Corruption scandals culminating with the one surrounding then Governor John Rowland left an unsightly blemish on Connecticut state politics at the beginning of the new century. John Rowland announced his resignation in June 2004. He was sentenced to federal prison in 2005, charged with receiving improper gifts and campaign contributions. Convicted around this time were two sitting-majors, Joseph Ganim of Bridgeport and Phil Giordano of Waterbury as well as the State Treasurer Paul Sylvester. And with the turmoil came calls for action, for reform, despite the fact that reformers such the Connecticut Citizen Action Group and the Connecticut chapter of Common Cause had spent a decade attempting to move the issue onto the legislative agenda. Rowland's subsequent resignation from office finally hastened the passage of legislation, resulting in the *Act Concerning Comprehensive Reform for Statewide Constitutional and General Assembly Offices* (Nyhart 2006).

Public Financing of Elections

A primary objective behind the boxing-in or outright removal of private money in campaigns is to lessen influence peddling in government outcomes: the ubiquitous "pay-to-play" influence of interest groups in public policy. Ostensibly, in substituting state monies in lieu of private monies elected officials will no longer be beholden to the special interests represented by their contributors. In turn, the implicit outcome of this cleansing would lead to legislative outcomes that clearly reflect, or better reflect, the interests of the majority.

Relatedly, the laws also attempt to reach the persistence of incumbency. Before there existed any public monies made available by clean election laws, aspiring officeholders were compelled to either raise money from citizens or interest groups or self-finance their campaigns. In the face of a seemingly monolithic incumbent few potential donors were willing to support a challenger. Few candidates had the wherewithal to pay their own way. The perception of invincibility associated with incumbency in effect translated into a full-employment act for sitting legislators (Mayer and Werner 2007).

Election law reformers felt that the seeming sense of entitlement brandished by sitting legislators created by the lack of a credible threat of removal rendered them ineffective and inattentive to the concerns of their constituents. Public monies and limits on donations were considered an effective way to overcome the

"barriers to entry." Small donations pose a less serious threat because the individuals who make them are in no position to extract quid-pro-quo type of concessions from legislators. Yet despite the "breath-of-fresh-air" quality and the bona fide innovations intended by the clean election laws several inconsistencies can be noticed.

In Connecticut, the clean elections law adopted in 2005 was not a voter initiative as they are in the few other states who have embraced such reform. Rather, it was a legislative act. But if individual state legislators knew that influence peddling was endemic across their ranks – why not simply refuse to do the special interest's bidding? Why chose the more elaborate process of assembling legislation to address this problem? Opting for legislation – an approach which binds all – appears to be a solution to a run-of-the-mill social dilemma problem (Elster 2007, Huberman and Glance 1994). Every legislator was aware that collectively they would all be better off if they refused special interest monies. And getting elected – even in state legislature races – requires money. Yet, to individually refuse to accept money from lobbyists was impractical because the money would simply be funneled to a more willing state assemblyman; after all, at some level assemblymen are a fungible lot. By collectively agreeing to a common course of action via legislation to avoid accepting special interest monies the legislature found a solution to their "commons" problem.

A second puzzling observation logically suggests itself. It's not clear that refusing private monies will lead to better social outcomes. To our knowledge no meaningful evidence linking the provision of the state funding of candidates to improved legislative performance was provided. In fact, some would argue that the impact on legislation has been nil. Voting by legislatures in the 2010 assembly was virtually indistinguishable from the previous legislative voting patterns (Parnell 2010). Nor was there any evidence linking the proposed measure or to any other performance metric, for that matter. In fact and to the contrary, one can envision any number of scenarios in which the resulting will of the majority, unencumbered by private monies, could have serious *negative* economic repercussions. Thus, one has to wonder why the focus of the clean elections legislation on what are merely instrumental measures-rather than on measures that would guarantee desired social outcomes – to the extent that it was possible?

We conjecture that with implicit instrumental measures the central concern of the public funding of elections law adopted by Connecticut is its own reward. Indeed, the literature around the "fairness heuristic" finds compelling evidence that most people resort to perceived procedural fairness when information on the trustworthiness of an authority is unavailable or corrupted (van den Boos, Wilke and Lind 1998). Like Pompeia, the legislature must

appear to be beyond reproach. The seeming impropriety of being seen as beholden to private interests, especially after the uproar resulting in the Governor Rowland's impeachment, was diminishing their moral stature in the community – and their re-election chances.

We cannot answer; we can only surmise. But we can examine the performance of several instrumental measures and thereby provide a basis for tentative answers. This is our task in this paper.

To some extent, the ultimate impact of Connecticut's efforts will depend on of the evolving resolution of the recent controversial U.S. Supreme Court ruling in *Citizens United v. Federal Election Commission*. In *Citizens United* the courts signaled its intention to roll-back even long-standing limits on corporate campaign contributions (The Hartford Courant 2011).

Empirical Methodology, Data Sources, Performance Metrics, Limitations & Scope

We examine whether there is any statistically significant change in the levels of various metrics between the periods before and after the implementation of the legislation. Specifically, we scrutinize those variables originally examined by Sullivan:

(i) voter registration data

(ii) party registration data

(iii) election results

We do so for the 2006 state assembly elections and compare them to voter and party registration data and state assembly election results in 2008 and in 2010, respectively (Sullivan 2009). In principle, any observed statistically significant change in a given metric *is consistent with* a hypothesis attributing causality to the public elections law (Imbens and Wooldridge 2008). Put simply: any effects thereby attributable to the election laws could be observable in the 2008 and 2010 election results.

We use resampling methods and draw multiple simulated samples to calculate statistical significance (Good 2001). Since the distribution of any statistic is attainable using resampling methods it is possible to test any number of performance metrics.

In this instance resampling conveys three advantages over parametric and non-parametric approaches. Resampling techniques provide a non-parametric determination of a performance metric's distribution and a measure of effectiveness that is not sensitive to deviations from the assumptions underlying most parametric procedures. The simulated samples are drawn from extant elections outcomes data instead of draws from a theoretical data-generation process. The heteroskedasticity and the small-numbers characteristics of the elections outcome data are not consistent the desired error distributions and thereby greatly limit the applicability of stochastic data models such as the

traditional logistic or multiple linear regression models (Breiman 2001).

Potentially useful non-parametric tests such as the Wilcoxon signed rank test and the sign-test are not constrained by the *a-priori* data assumptions required by stochastic data models. However, their flexibility comes at a loss of statistical power. It is usually more difficult to reject the null hypothesis when non-parametrics are used, which in turn increases our chances of incurring a type-II error (a failure to reject a null hypothesis that is false). Thus, avoiding non-parametric methods would tend to enhance the chances of a finding in favor of observable effects of the elections law.

Last, we analyze statistical constructs in our analysis – e.g. measures of diversity such as the gini coefficient, the herfindahl index, and vote sums or totals, *inter alia* – for which the theoretical statistical behavior is not known. In these instances the observed sample statistic is compared with the null resampling distribution derived from our resampling protocol discussed below.

We set forth our null hypothesis of no difference in the levels of the examined statistics. Generally:

$$\boldsymbol{Ho: \Phi_2 - \Phi_1 = 0}$$

$$\boldsymbol{Ha: \Phi_2 - \Phi_1 \neq 0}$$

Where: $\boldsymbol{\Phi i}$ represents the realization of a particular statistic in the given election-cycle year `i' and where year `1' is always 2006.

We generate via monte carlo simulation the permutation distribution of the test statistics. We run 10,000 iterations using Stata. We calculate the observed difference between the simulated 2008 and 2010 election (or 2006 and 2010, as the case might be) assuming a null hypothesis of no difference in election results. We compare the frequency of occurrence of this simulated statistic with the observed difference between the levels of the statistic. We reject the hypothesis and accept the alternative if the value of the test statistic for the observations is an extreme value in the permutation distribution of the statistic. We use 95 percent significance.

Data Sources and Data Treatment

The data used in examining the election law performance were obtained from publicly available data for the years 2006, 2007, and 2008. Specifically, we culled data on the 'vote for state representatives' for all 'assembly districts' reporting. The data is published online by the Connecticut Secretary of State. (Secretary of the State 2006) (Secretary of the State 2008, Secretary of the State 2010).

The table below offers a comparative look at the metrics across the three extant election cycles.

Table 1

State Assembly Election Results
Sullivan Metrics

Metric	2006	2008	2010
Average Number of Candidates per District Race	1.66	1.64	1.79
Challengers	99	98	119
Incumbents	151	151	151
total number of candidates	250	249	270
total number of races	151	151	151
Percentage of Uncontested Races	27.8%	29.1%	20.5%
Voter Participation	943,710	1,378,631	1,074,318
Minor Party Affiliation	36.0%	37.3%	23.2%

For expositional purposes, we construct a second table. Table 2 contains the realized difference in the levels of each of the metrics listed in Table 1. The figure in italics under each measure of realized change is the p-value obtained from our statistical test. The asterisks over each particular p-value indicate whether the difference is statistically meaningful at a 95 percent level of significance.

Table 2

Realized Changes in the Sullivan Metrics:
Base Year 2006

Metric	2006-08	2006-10
Change in the Number of Participating Voters	434,921	30,608
p-value	*(0.0001)**	*(0.0017)**
Change in the Number of Challengers per District Race	(2)	20
p-value	*(0.8059)*	*(0.0146)**
Change in the Percentage of Uncontested Races	1.3%	-8.6%
p-value	*(0.5649)*	*(0.0146)**
Minor Party Affiliation	1.3%	-12.8%
p-value	*(0.6158)*	*(0.0135)**

We observe the following. The direction of change in the levels between the 2006 and 20008 election cycles were to the contrary of what one would expect. However, but for the number of participating voters, no change in the levels of the Sullivan metrics was statistically significant. And given the impressive ability to energize voters by the 2008 Obama campaign it is not clear whether the observed statistically significant increase in the number of participating voters can be attributed in part – or at all – to the election law.

Additional Metrics

We examine several additional metrics to capture the relevance of other dimensions of the reach of the elections law. Specifically, we examine the following:

(i) Change in the margin of victory for each particular state house race, both in absolute terms and as a percent of the total vote. We hypothesize that the increased competitive vigor brought about by the increased funding would reduce the margin of victory as measured by both metrics.

(ii) Electoral races are considered competitive if the average outcome of races is less than or equal to 60%. We hypothesize that the electoral law should increase competitiveness.

(iii) Another measure of competitiveness of vigor is the total number of votes cast by the opposition. We hypothesize that the electoral law should unequivocally result in an increase in the number of votes cast by the opposition.

(iv) Diversity is considered a desirable outcome. We examine whether there has been an increase in diversity with the Hirschman-Herfindahl Index of Concentration (HHI). We look at the share of party

presence in the various races and also at the share of total votes garnered by each party. The HHI is a sample statistics and therefore its sampling variance can be calculated. Formally, the index is calculated as follows:

$$HHI = 10{,}000*$$

Where: Si is the relevant share of the either party presence or share of votes. The higher the index the less diversity is present. The maximum is HHI = 10,000, representing a one party outcome.

The data on the realized value of these other metrics are presented in Table 3.

Table 3

Metric	2006	2008	2010
Average Winning Margin of Races (Levels)	3142	4352	2297
Winning Margin of Votes Cast (% of total votes cast)	57.5%	54.8%	41.0%
Competitive Vote Margin (is the average outcome less than or equal to 60%?)	60.0%	55.0%	71.5%
Total Number of Votes Cast by Opposition	233,864	361,352	367,999
Diversity Index of Opposition (Herfindahl on Share of Party Presence in the Various Races)	3588	3562	3995
Diversity Index of Opposition (Herfindahl on Share of Votes Garnered by Party)	5844	5402	5849

Results: Additional Metrics

Procedurally we use the same methodology described above. Formally we test for any change at the 95 percent significance level assuming a null hypothesis of no change. We again use

permutation methods to test our hypothesis and to generate our p-values. The results are presented in Table 4.

Table 4

Metric	2006-08	2006-10
Change in the Average Winning Margin of Voters	1,209	(845)
p-value	*(0.561)*	*(0.487)*
Difference in the Winning Margin of Votes Cast (% of total votes cast)	(0.03)	(0.17)
p-value	*(0.657)*	*(0.0417)**
Difference in the Competitive Vote Margin (is the average outcome less than or equal to 60%)	(0.05)	0.12
p-value	*(0.053)*	*(0.0017)**
Change in the Total Number of Votes Cast by Opposition	127,488	134,135
p-value	*(0.031)**	*(0.0432)**
Change in the Diversity Index of Opposition (Herfindahl on Share of Party Presence in the Various Races)	(27)	407
p-value	*(0.671)*	*(0.450)*
Change in the Diversity Index of Opposition (Herfindahl on Share of Votes Garnered by Party)	(442)	5
p-value	*(0.759)*	*(0.562)*

The overall results remain consistent with the overall results obtained from testing the Sullivan metrics. First, few of the metrics exhibit statistically significant change between the 2006 and 2008

election cycle. Thus, the observed improvement in the winning margin of votes cast as a percent of total votes cast is not statistically significant. The same findings emerge for the two diversity indexes estimated. Second, there is discernible change in the metrics between 2006 and 2008 that is contrary to what one would expect – but the change is not statistically significant. For example, there is an *increase* in the average winning margin (levels) of votes between 2006 an 2008 (from 3142 votes to 4352) rather than the anticipated decrease.

There are more significant changes in the comparison with the 2010 election cycle. But the results are contradictory. We find positive improvements in the winning margin of votes cast but a deterioration of competitive vote margin. Both are statistically significant. Importantly, the diversity indexes show no statistically discernible improvement at all, whether from the comparison to the 2008 or the 2010 cycle. A somewhat troubling indicator is the fact that party and opposition diversity appears to have *deteriorated* by 2010 when compared to the party and opposition layout present in the 2006 election, although the difference is not statistically meaningful.

Interpretation of Results and Concluding Comments

Our results examining the impact on metrics aimed at appraising the impact of the law are inconclusive: some metrics do suggest

statistically meaningful differences whereas many others do not. Hardly any statistically significant results are evident in the 2008 cycle. With token exceptions, all of the minute differences observed, when they are in fact observed, occur by the 2010 election cycle.

Ironically, a law aiming to enhance electoral competition strengthened the position of the Democrats in a robustly blue state: the results of several metrics appear to have bestowed a slight edge to Democrats, at the expense of Republicans. That the legislation may have benefited Democrats may be ironical but it is also unsurprising. To the extent that proscriptions handicap the ability of potential candidates to raise money it is most likely to affect the generally more affluent republicans.

Given the inconclusiveness in the information elicited by our examination of the various metrics assembled it is difficult to conclude that the public funding of elections in the State of Connecticut is an unqualified success, or for that matter, a qualified success. First, no one metric takes precedence over another. Second, any weighted combination of metrics must necessarily rely on subjective weights. Third, performance is essentially multidimensional: superior performance against one objective cannot easily be traded off against modest or inadequate performance on another. Fourth, several of the proposed 'success' criteria may be specified inadequately. Fifth, and at any rate,

`success' in this instance refers to the performance of the chosen metrics. Put differently, our examination scrutinized instruments, not final outcomes.

It should be clear that the lack of conclusive discernible effects is not evidence against the proffered benefits of the Connecticut clean election law. Our inconclusive results may be an artifact of the data, in other words, we could have an instance in which the hypothesis is true but our metrics are poor representations of the hypothesis, resulting in false negatives. The result may also be a consequence of low statistical power, or the fact that the "administration" of the law was faulty, or even that the existing law left several gaping loopholes. In fact, one could realistically argue that because of remaining porosity in the system the current law may not sufficiently curtail the privately-directed flow of funds to favored candidates. And given the presence of *Citizens United* it is not clear whether there can be any further tightening. A critical limitation of our study is the lack of covariates designed to hold exogenous influences constant. It may very well be that there is a vigorous effect ascribable to the election law but that is not noticeable because it is eroded by broader confounding influences – for which we don't control. Last, given the historical importance of local town and municipal elections in Connecticut a law aimed at alleviating the ills of the state electoral system may miss the well known point that all elections are *really* local, leading one to think

that we are looking for lost keys where the light is and not where we dropped them.

It is distinctly possible that election law has succeeded in "cleaning-up" the observed intangible ills – the lingering sense of corruption, the bothersome belief that only the more affluent are listened to - as was argued. There is some support for that perception. But more accurate or more specific tests will have to wait a different occasion.

Thus, we cannot conclusively claim that the law does not work or that it works badly. It appear that the one conclusion that we can unambiguously draw is that the effusiveness and optimism of the various commentators supporting clean election laws has not come to be realized in the State of Connecticut.

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