

## Notes and Comments

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NOTES AND COMMENTS

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Articles appearing in this journal are abstracted and indexed in HISTORICAL ABSTRACTS and AMERICA: HISTORY AND LIFE.

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COVER PHOTOGRAPH

The cover photograph labelled "City Buildings, Kingston, Ont., Canada" is taken from a circa 1912 postcard held in Queen's University Archives.

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No. 3-72 (November, 1972)	No. 3-73 (February, 1974)
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<u>1976</u>	<u>1977</u>
No. 1-76 (June, 1976)	No. 1-77 (June, 1977)
No. 2-76 (October, 1976)	No. 2-77 (October, 1977)
No. 3-76 (February, 1977)	No. 3-77 (February, 1978)

. . . . .

INDEX  
URBAN HISTORY REVIEW  
1972-1977

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HISTORY OF CANADIAN RESOURCE TOWNS

A special issue of Plan Canada, published by the School of Urban and Regional Planning, Queen's University, Kingston, is entitled "Canadian Resource Towns: Their History and Development," and has been published as Volume 18, No. 1 (March 1978). The contents are as follows:

\* \* \*

## Editorial

## Abstracts/Resumes

## Articles

"Canadian Resource Towns in Historical Perspective"  
Gilbert A. Stelter and Alan F.J. Artibise

"The Development of an Unplanned Community: Cobalt, 1903-1914"  
Douglas Baldwin

"Planning and Development in Whitehorse, Yukon Territory"  
Paul M. Koroscil

"The Changing Internal Structure of Canadian Resource Towns"  
L.D. McCann

## Book Reviews/Comptes rendus

"New Communities in Canada: Exploring Planned Environments," a Special Issue of CONTACT, Journal of Urban and Environmental Affairs, Vol. 8, No. 3 (August 1976), Norman E.P. Pressman, Editor/O.W. Saarinen.

To Preserve and Defend: Essays on Kingston in the Nineteenth Century, Gerald Tulchinsky, Editor/David G. Knight.

"Canadian Urban Studies," Communique: Canadian Studies, Vol. 3, No. 3 (April 1977), Alan F.J. Artibise/John Weaver.

Choosing Canada's Capital: Jealousy and Friction in the 19th Century, David B. Knight/L.D. McCann.

## Books Received/Nouvelles acquisitions

## Contents of Other Journals/Sommaires d'autres revues

## Contributors/Auteurs des articles

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FIRE INSURANCE PLANS IN THE  
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This publication is an inventory of the fire insurance plans, both Canadian and Foreign, preserved in the National Map Collection, Public Archives of Canada. Dating from 1878 to 1975, these large-scale maps, now numbering more than two thousand, graphically document the structure of some fourteen hundred cities, towns and villages. Fire insurance plans, with information concerning streets, lot lines, individual structures, construction material, all of which are designated by colour and symbols, offer the greatest detail of any comparable urban map.

An introductory essay on the history of fire insurance plans, emphasizing Canadian developments, precedes the listing. Entries are arranged

alphabetically and chronologically by community within each province or country. For each plan, the name of community, date, author/publisher, scale and number of sheets is given. Over thirty plans or details are reproduced.

These cartographic works can form an integral part in many studies in urban history. Researchers interested in morphologic change, urban land use, architecture or urban demography will find the publication an indispensable reference aid.

Copies of the publication may be obtained free from the National Map Collection, Public Archives of Canada, 395 Wellington Street, Ottawa, Ontario, K1A 0N3. Telephone (613) 995-1077.

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PLANS D'ASSURANCE-INCENDIE DE LA  
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La présente publication constitue un répertoire des plans d'assurance-incendie, canadiens ou étrangers, conservés par la Collection nationale de cartes et plans, Archives publiques du Canada. Ces cartes à grande échelle, au nombre de plus de deux mille et couvrant la période 1878-1975, illustrent l'organisation de quelque quatorze cents villes et villages. Parmi les cartes de villes, ce sont les plans d'assurance-incendie qui se révèlent les plus instructifs, car ils fournissent des renseignements concernant les rues, les limites des terrains, les différentes constructions, les matériaux employés, tous indiqués par des couleurs et des symboles.

Le répertoire comprend une brève introduction où l'on raconte l'histoire des plans d'assurance-incendie, en particulier au Canada. Les entrées sont disposées en ordre alphabétique et chronologique, selon les différentes collectivités de chaque province ou pays. Pour chaque plan, on donne le nom de la collectivité, la date, l'auteur/éditeur, l'échelle et le nombre de feuilles. La publication comprend des reproductions de plus de trente plans ou détails.

Ces oeuvres cartographiques pourraient servir de base à de nombreuses études sur l'histoire urbaine. Les chercheurs qui s'intéressent aux changements morphologiques, à l'exploitation du sol urbain, à l'architecture ou à la démographie urbaine y trouveront un instrument de recherche très précieux.

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## WORKERS AND TORIES: AN EXCHANGE

Editor's Note: *The following two items concern an article published in U.H.R., No. 3-76 (February, 1977). The article was Michael J. Piva, "Workers and Tories: The Collapse of the Conservative Party in Urban Ontario, 1908-1919," pp. 23-39. The Review welcomes comments from readers concerning material published in the journal.*

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## WORKERS AND TORIES: A COMMENT

Professor Michael Piva's recent paper on working class support for the Ontario Conservative party in the years 1908-1919 raises important issues heretofore given short shrift in the literature on that important period. Unfortunately, Professor Piva's methodology leaves the reader with grave reservations as to the validity of his conclusions. Although I believe that his interpretations are essentially correct, I should like to comment briefly on his use of statistical techniques. These criticisms are offered in a constructive spirit from one who has encountered many of the same problems in attempting to analyse Ontario electoral history.

The first point concerns Professor Piva's failure to acknowledge the horrendous methodological difficulties inherent in inferring individual level relationships from aggregate, ecological data.<sup>1</sup> Although he cannot be expected to solve these problems, for indeed most are insoluble, discussion of them is more than ritualistic genuflection to the literature. That these difficulties cannot be satisfactorily resolved does not mean that we should not be aware of the ways in which they may affect our analysis and constrain our conclusions.

As many readers of the Urban History Review will be aware, social scientists have for many years debated the merits of geographically-aggregated data for studying voting behaviour and kindred topics. The extreme position, propounded in W.S. Robinson's well-known article, is that, due to the distortions inherent in the aggregation process, such data simply should not be used for these purposes.<sup>2</sup> With the wealth of census, electoral and other data available only by geographic tracts, this is a disconcerting proposition, and any number of analysts have duly noted Robinson's admonitions and then proceeded to ignore them. A number

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<sup>1</sup>The general problems and prospects of aggregate data are well set out in Yehuda Grunfeld and Zvi Griliches, "Is Aggregation Necessarily Bad?" Review of Economics and Statistics, Vol. XLII (February, 1960), pp. 1-13.

<sup>2</sup>W.S. Robinson, "Ecological Correlations and the Behaviour of Individuals," American Sociological Review, Vol. XV (June, 1950), pp. 351-7.

of (generally less than satisfactory) alternatives to ecological correlation have been put forward,<sup>3</sup> but a recent study, which re-analysed Robinson's own data, suggests that the essential problem lies not so much with ecological correlation, but with what statisticians refer to as improperly specified models.<sup>4</sup> Without becoming enmeshed in detailed proofs, it can be readily shown that even the sign of the simple (i.e., one independent variable, here occupation) correlation need not be in the same direction as the correlation between that variable and the dependent variable (here vote share) in a properly specified, multivariate model. This difficulty is directly relevant to Professor Piva's analysis.

Professor Piva's comparison of the strength and significance of the simple correlation (Pearsonian  $r$ ) of Tory voting and a) concentration of manufacturing workers and b) concentration of various ethnic and religious groups leads him to conclude that "class was far more important in determining voting behaviour than ethnicity or religion" (p. 33). This is simply not a statistically supportable conclusion. Were he to find that, with appropriate statistical controls introduced for religion, ethnicity and a host of other variables on which data are lacking, class, as measured in occupational terms, is still strongly and positively associated with Conservative support, then such a statement would be justified. By way of illustration, according to the 1911 census, Anglicans and persons of English origin were disproportionately concentrated in urban Ontario. A plausible argument might therefore run that 'Anglicanism' and 'Englishness', rather than class, underlay urban Tory voting. Without taking such variables into account via multiple regression or other multivariate techniques, we must be very leery of attributing substantive meaning to the positive class correlation. Given the immensely complex nature of the social and political universe, the need for multivariate techniques is a troublesome but unavoidable fact of life.

My own research, which employs fairly unsophisticated multiple regression analysis (and is itself beset by very serious methodological shortcomings), suggests that once the ethnic and religious composition of the electorate is taken into account, the concentration of wage earning workers (obtained from the 1911 census) was all but unrelated to levels of Conservative voting.<sup>5</sup> On several grounds, this finding

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<sup>3</sup>Leo A. Goodman, "Some Alternatives to Ecological Correlation," American Journal of Sociology, Vol. LXIV (May, 1959), pp. 610-25; and W. Phillips Shively, "'Ecological' Inference: The Use of Aggregate Data to Study Individuals," American Political Science Review, Vol. LXXIX (December, 1969), pp. 1183-96.

<sup>4</sup>Eric A. Hanushek, John E. Jackson, and John F. Kain, "Model Specification, Use of Aggregate Data and the Ecological Correlation Fallacy," Political Methodology, Vol. II (Winter, 1974), pp. 89-107.

<sup>5</sup>"Social Change and Political Stability in Ontario: Electoral Forces 1867-1977," Ph.D. thesis in progress, McMaster University, Chapter VIII.



is not comparable to those of Professor Piva and therefore cannot be viewed as a clear contradiction of them, but it does call them into some question.

In discussing the relationship between Tory strength and low turnout in pre-War Ontario, Professor Piva suggests "low participation tended to work to the advantage of the Conservative party" (p. 35). It seems to me that this view of low turnout causing Tory success is no more inherently plausible than the view that Tory strength by disheartening opponents, caused the low turnout.

On page 36 Professor Piva concludes "clearly the major change in voting habits between 1914 and 1919 in urban Ontario was a general increase in the number of voters who exercised their rights." Here he seems to be making what has been called the 'assumption of minimal change', which entails assuming only that change visible as net change in aggregate data.<sup>6</sup> Great undercurrents of change may lie below apparently similar aggregate vote totals. As with so many problems of ecological data, the analyst can only rarely resolve this problem, but he should at least be aware of it.

The burden of these methodological cavils has been that Professor Piva has not exercised appropriate caution in analysing and interpreting his data. I should also like to offer some comments on one or two substantive issues.

In his discussion of participation rates, he comments that "it seems unreasonable to argue that women demonstrated markedly different voting behaviour than men in light of the consistency in patterns between 1914 and 1919" (p. 36). Aside from finding this logic slightly dubious, I would suggest two reasons why female voters might well have behaved differently, especially in 1919. First, having never voted before--at least provincially--women would not have acquired the psychological attachment to the old line parties that many men would have built up over several elections. Secondly, one of Hearst's innumerable political gaffles was to schedule a prohibition plebiscite and the election on the same day. This could well have attracted the more ardently prohibitionist female electorate to the polls in greater numbers than would otherwise have been the case. Moreover, given the Tories' record of vacillation on temperance and the unabashedly wet stance of the new Grit leader, Hartly Dewart, avid female prohibitionists had little reason to support the established parties (but then the ILP was also distinctly, if discretely, wet).

Finally, Professor Piva's discussion of the electoral failure of independent labour politics prior to the Great War is apt, but does not seem to give due weight to organizational factors. As the Italian political sociologist Giovanni Sartori has put it,

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<sup>6</sup>Philip E. Converse, "The Problem of Party Distance in Models of Voting Change," in M. Kent Jennings and L. Harman Zeigler, eds., The Electoral Process (Englewood Cliffee, N.J.: Prentice-Hall, 1966), p. 177.

large collectivities become class structured only if they are class persuaded. The most likely and apt "persuader" is the party (or the union) playing on the class appeal resource. In any case, ideological persuasion requires a powerfully organized network of communications.<sup>7</sup>

If the impediments to class politics have differed somewhat in Europe and in Ontario, still a good deal of working class politics is explicable in Sartori's organizational terms.

To repeat, my feeling is that Professor Piva's interpretation of the electoral proclivities of Ontario's urban workers is essentially accurate. However, this view rests more on his skill and perception as a traditional historian than on his statistical analysis.

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<sup>7</sup>Giovanni Sartori, "The Sociology of Parties: A Critical Review," in Party Systems, Party Organizations and the Politics of the New Masses (International Conference on Political Sociology, Berlin, 1968), p. 16.

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#### WORKERS AND TORIES: A REPLY

The historian confronts innumerable methodological pitfalls as a consequence of his inability to freely generate new data. He is largely dependent upon evidence which survives much of it in a form inappropriate for his specific purposes. John Dales' comment that "in the final analysis, all forms of intellectual enquiry are essays in persuasion"<sup>1</sup> reminds us that the conclusions drawn in our research are always tentative. My article claims only to suggest one approach to a much more complex problem. As such I take some comfort in Professor White's support for the overall interpretation advanced in the article, but all interpretations are subject to further tests. For the moment, however, I remain confident that my method was suited to the available evidence and that the hypothesis is consistent with that evidence.

Perhaps Professor White is correct to admonish my failure to even genuflect to the methodological debate on the appropriateness of using

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<sup>1</sup>John Dales, The Protective Tariff in Canada's Development (Toronto, 1966), p. 90.

aggregate data. My problem with Robinson's argument is that to accept his position would be equivalent to saying that no analysis is possible--at least not until the year 2021 when the manuscript census becomes available. I accept the argument that the methodological problems preclude definitive proof in historical analysis, but I see enquiry as legitimate. Like Professor White, I accept the limitations but still attempt an analysis.

The real point of his criticisms, however, concerns the more concrete problem of an "improperly specified model." During the course of my research and analysis I toyed with a multivariate statistical technique but found it both awkward and cumbersome without adding anything to the analysis. In the end I decided that the correlation coefficient (the Pearson  $r$ ) was the most suitable technique for my purposes. That purpose was to suggest one approach to the complex problem of class formation, in this case the emergence of a working class. As I pointed out in the introduction, I support the Hobsbawm position that "class and class consciousness" are inseparable. Other evidence not pursued in this article indicates that the unions articulated a working-class ideology which was more reformist than radical. The question, however, is whether or not this minority--the leadership of organized labour--accurately reflected the attitudes of workers in general. In an effort to throw some light on the subject one needed to find if there was evidence of collective behaviour and, if there was, whether or not the tendency was to support opposition-reformist political options.

My second purpose was to challenge two assumptions in the literature which I felt were misleading: 1) that the working class switched its affiliation between 1914 and 1919 and 2) that the class variable in 1919 had no significant pre-war roots. I argue that 1) class was a significant variable in 1919 and 2) this development occurred prior to the war. I feel the analysis forwarded shows that this hypothesis is both reasonable and consistent with the available evidence. Certainly it indicates that the previous assumptions in the literature are inadequate. Professor White is correct to point out that the use of the Pearson  $r$  does not constitute statistical proof but neither would other statistical techniques. It does, however, provide us with some evidence. The credibility of this evidence is reinforced by its consistency with other types of evidence. If the hypothesis is incorrect, let us see the evidence which would show it to be so.

Let us approach the problem by posing the four alternative hypotheses. If we accept the hypothesis that the working class supported Whitney prior to the war then we would expect to find a positive correlation between concentrations of manufacturing workers and Tory electoral support in 1908, 1911 and 1914. This is not borne out by the evidence. If we accept the hypothesis that the class voting patterns in 1919 were conditioned by the war and had no significant pre-war roots then we would expect to find no correlation between concentrations of manufacturing workers and Tory electoral support prior to 1919. Again this is not borne out by the

evidence. If we accept the hypothesis that ethnicity and religion were important variables in 1919 we would expect to find a correlation, either positive or negative, between concentrations of particular ethnic groups or religious groups and Tory electoral support. The evidence indicates that this hypothesis is applicable only to Irish Protestants and French Canadians but not for other ethnic and religious groups. This brings us to my hypothesis: that class was the major variable, that workers voted against the Conservative government, and that this trend is discernable in the pre-war period. If we accept this hypothesis we would expect to find 1) a strong negative correlation between concentrations of manufacturing workers and Tory electoral support and 2) at the minimum, a negative correlation at least by 1914 and a longer term trend in this direction. This hypothesis is the most plausible because it is consistent with the evidence.

I also remain convinced that class was more important than ethnicity or religion in 1919. I would like to point out that my argument refers only to 1919; I would not, and do not, make such a claim for the pre-war elections. More importantly, the analysis argues that class was not a variable in 1908 and was, at best, only marginally important in 1911. Since I argue that class was not a significant variable I could not reasonably conclude that it was more important than ethnicity or religion. But I do not claim that it was. If Professor White's illustration refers to the 1911 election I can see no implied, let alone clear, contradiction between my hypothesis and what I take to be his position with regard to the importance of Anglican and English as opposed to class variables.

If, however, the illustration is meant to refer to the 1919 election, there is a conflict in our two views. But if the reference is to the 1911 election I would suggest that Professor White has a very serious problem with his data because the analysis employs the 1911 census. The relative concentration of manufacturing workers between urban centres changed significantly between 1911 and 1919. The \$1 billion in war contracts awarded by the Imperial Munitions Board had a substantial impact. Ranking, from high to low, all cities of 5,000 or more according to the percentage of population employed in manufacturing demonstrates this. Welland moved from rank 21 to rank 1 between 1911 and 1919. Toronto slipped from 13th to 19th. Guelph moved from 10th to 15th. I would argue that the 1911 census is a wholly inadequate data base for an analysis of the class voting patterns in the 1919 election.

I assume that Professor White's reference to "appropriate statistical controls introduced for religion, ethnicity and a host of other variables" is meant to suggest that a methodological technique which attempted to hold some variables constant would be preferable. I agree but do not consider such an approach feasible in the context of this paper. The data is in the aggregate and the manuscript census will not be available for some time to come. Census districts, meanwhile, do not correspond to provincial ridings, and manufacturing statistics for 1919 are available for cities rather than census districts. This means that until the manuscript census becomes available the smallest working unit (in the context of the questions posed in the article) is the city. Holding variables constant

would limit comparisons to cities with, for example, comparable ethnic and religious populations but differing class structures. This I found impractical.

Turning to the plausibility of my argument concerning abstentions, we can again approach the question by posing the null hypothesis. If perceived Tory strength caused low turnout, I think it would be reasonable to expect low turnout in 1919. Nearly all political commentators expected the Conservatives to win the 1919 election; even E.C. Drury was surprised by the results. Sir Adam Beck, for example, was considered to be unbeatable by that most optimistic ILP paper, the Industrial Banner. And yet participation increased in spite of the almost unanimous contemporary opinion that the Government remained strong. But Professor White does have a valid point, although I would pose it somewhat differently. A more appropriate formulation of the hypothesis would be that Tory strength was a function of opposition weakness. The Liberals were very weak indeed as can be seen in their continuing leadership problems. The ILP was simply not a creditable party before the war with the exception of the local ILP organization in Hamilton. This formulation is I think consistent with my argument that by 1914 there was measurable opposition to the Conservative government, but the lack of a creditable opposition party produced a high level of alienation reflected in voter abstentions. In this formulation one would expect greater participation if the opposition was creditable--and this is what occurred generally in 1919. At the same time one would expect to find that if in a particular city the ILP was sufficiently divided so as not to constitute a creditable alternative participation would not increase significantly and this would work to the advantage of the Conservatives. This is precisely what occurred in Toronto where the labour movement was badly divided, where participation remained exceptionally low, and the Tories carried six seats. I think my hypothesis is reasonable and consistent with the evidence, although I must acknowledge that the formulation of the argument in the article could have benefitted from greater specificity and precision.

The posing of the null hypothesis is again, I think, the best approach to respond to the criticisms of my comments on the role of women. My comments do acknowledge that the granting of the vote might have caused some of the increase in participation, yet I do not think we can take this point very far. If we accept the hypothesis that the scheduling of the prohibition plebiscite on the same day as the provincial election resulted in an increase in participation because it brought prohibitionists to the polls, we could expect the increase in different cities to be relatively comparable. This, however, was not the case as can be seen in the very marked differences between Toronto and other cities.

The other hypothesis is I think equally suspect. If we assume that women had a greater proclivity to vote for a third party because they had not developed psychological attachments to the traditional parties we are left with the implausible assumption that ILP success in 1919 was based to a remarkable extent on the parties appeal to women. This is inconsistent with the pre-war situation in Hamilton, the only city where the ILP became

a creditable opposition party and sent Allan Studholme to the legislature from 1906 until his death in 1919. If the hypothesis is accepted one would also expect the correlation between class and Tory electoral weakness to be relatively weaker since women voters would be drawn from all classes. I also find it difficult to accept the plausibility of the assumption that women were not subject to the political pressures in their society exerted by the traditional parties because they could not vote.

In addition I find the hypothesis that prohibition was somehow linked to Tory troubles implausible for precisely the reasons cited by Professor White. The "wet" Tories were led by a prohibitionist, the "dry" Liberals were led by a "wet" candidate, and the supposedly "wet"--or at least damp--ILP had many prohibitionists, such as James Simpson, in their ranks. Moreover all of these parties adopted the same line--they would abide by the plebiscite decision.

This brings me to Professor White's final point. I must say that I could not agree with him more--I was simply unable to pursue the issue because of the limitations on length. The importance of organizational factors is what I tried to argue in my comment that "although there was close cooperation between city centrals like the DLC and the ILP, these Councils too failed to affiliate. In the long run this proved a fatal weakness" (p. 38, n. 18). I would argue that the trade unions were the institutions which could have provided, in Sartori's words, an "organized network of communications." The failure to solidify the organizational link between the unions and the party was a critical weakness. These points will be pursued at greater length in a paper I hope to submit for publication in the near future.

In conclusion, I would like to say that although I do not agree with many of the points raised by Professor White, his criticisms are both constructive and thought provoking. I greatly appreciate the time he has taken to consider my work as well as the spirit in which he has tendered his comments. I hope that the debate will continue and that it will lead to more research and analysis of these and other issues. Investigation, research, and analysis is a continuing process. I hope this exchange has contributed to the debate, and I feel confident that Professor White will in the future be able to contribute even more.

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