

Ethnography: Bridging the Qualitative-Quantitative Divide

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Abstract

This analytic autoethnographic and autobiographical essay addresses several interrelated questions regarding the use of ethnographic and otherwise ‘qualitative’ research methods in the study of contemporary urban society. The testy relationship between qualitative and quantitative research has historical as well as logico-deductive roots that continue to haunt the social sciences. As to hermeneutics, the debate parallels my academic career journey from Indiana University to Brooklyn College by way of New York University during which I learned that the normative practices of the social and not so social sciences come in a myriad of different competing, and occasionally conflicting, pre- and proscriptions. My intention in this essay is not to construct a fine, or even a crude, philosophical discourse but to argue for more attention to be paid to what social scientists do best as opposed to the labels they apply to each other and their trades. As the positivist founders of sociology would agree, social science is not exempt from the laws of social science. At the conclusion, an example will be given of the kind of knowledge accessible only through direct observation and best conveyed by thick description.

Keywords

Ethnography, qualitative research methods, disciplinary labelling, thick description.

For almost a century the degrees of heat of battle over the use of quantitative as opposed to qualitative methods have risen and fallen. Most recently it has been referred to as a ‘Paleozoic Debate’ (Follari, 2014). This analytic autoethnographic essay addresses several questions regarding the use of ethnographic and otherwise ‘qualitative’ research methods in the study of contemporary urban society. It also proceeds in an autobiographical manner, as for social scientists as well as for those they study, biography explains much about the academic life worlds in which we exercise our scientific craft. The relationship between qualitative and quantitative research is more than logical, it is social. When we conduct our research we do it within shared social life worlds, with and among,

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as well as for our peers as part of social organizations. I am not the first to argue that social research is itself a social act (see Cicourel, 2004).

I emphasize in all my work the sociological *verstehen* (understanding) method pioneered by Weber (1991[1921]). He argued that human society is made possible when social actors can imagine themselves in the place of the others with whom they interact, and thereby correctly anticipate the others' behaviour. We might think of society as dependent on such common, or shared, 'text'. Social scientists are no less confined by these strictures. Weber defined Sociology as:

[...] the science whose object is to interpret the meaning of social action and thereby give a causal explanation of the way in which the action proceeds and the effects which it produces. By 'action' in this definition is meant the human behavior when and to the extent that the agent or agents see it as subjectively *meaningful* [...]

The 'meaning' to which we refer may be either (a) the meaning actually intended either by an individual agent on a particular historical occasion or by a number of agents on an approximate average in a given set of cases, or (b) the meaning attributed to the agent or agents, as types, in a pure type constructed in the abstract. In neither case is the 'meaning' to be thought of as somehow objectively 'correct' or 'true' by some metaphysical criterion. This is the difference between the empirical sciences of action, such as sociology and history, and any kind of *a priori* discipline, such as jurisprudence, logic, ethics, or aesthetics whose aim is to extract from their subject-matter its 'correct' or 'valid' meaning. (1991 [1921]: 7)

The German verbs *kennen* and *wissen* also provide insight into Weber's Neo-Kantian sociological understanding (Rutgers & Schreurs, 2004). *Wissen* is factual knowing while *kennen* has to do with acquaintance with and working knowledge of something. While it is generally accepted that qualitative research, and especially ethnography, is best suited for *kennen* as it brings the researcher into the world of the subjects, I believe it provides access to *wissen* as well in the sense that it produces scientific data as well as generating and testing theories.

As will be obvious at some point, rather than taking the usual defensive posture for ethnography and otherwise 'qualitative' methods vis-à-vis their allegedly superior quantitative siblings, I argue for the unique values of close-up research. Only by close, intense study can we get access to subjective worlds and thereby make possible a deeper understanding of the social life of others. Since my own approach is a visual one, it will begin with comments on urban ethnography as a close-up visual practice:

When we pass through urban spaces such as a residential neighborhood we haven't visited before, we are like tourists using our eyes to decipher the clues and cues that loudly and quietly surround us. We might ask ourselves, Is this a safe or a dangerous place? Am I welcome here or should I leave before it is too late? What kind of neighborhood is it? Are the people who live here rich or poor? What is their race, ethnicity, or religion and how (or why) does it matter? Some things are easy to tell on a street, such as whether there are things for sale. Legitimate merchants make it obvious that they are seeking customers with signs that compete for attention, but for the sale of illicit goods, the signs vendors give off are subtler. Yet it seems that for the knowledgeable customer they are in plain view. (Krase, 2012: 1)

It appears at times that the socially constructed division between anthropology and sociology have created parallel universes in which 'Culture' reigns in one while in the other its Siamese Twin 'Social Structure' dominates. Ethnographers are condescendingly seen as the griots of social science; the commentators on urban life that the quantitative 'big boys' analyse. Often our writings are dismissed as mere journalistic utterances or at best, worthy of further, quantitative investigation. De Certeau might have written a statement similar to mine while creating the city in the act of walking (1985: 129). Ethnographers weave critical ideas into narratives of the places through

which they pass where mundane spatial practices of ordinary people make their social agency visible by changing appearances.

Ethnography provides what Psathas might call a phenomenological bridge between what ordinary people do and social scientists say about it (1973: 16). Through a syncretistic approach, the directly observable in urban theory can be discovered in semiotics, phenomenology, and symbolic interactionism. Ethnography, or the 'thick' descriptions required in our scientific empirically-based practice, or other 'thinner' qualitative and quantitative methods, must be explainable in their own terms. According to Collier, Seawright, and Brady '[o]ne type of thick analysis is what Geertz (1973) calls "thick description," that is, interpretive work that focuses on the meaning of human behaviour to the actors involved. In addition to thick description, many forms of detailed knowledge, if utilized effectively, can greatly strengthen description and causal assessment' (2004: 72).

As in all socially organized systems, social science research is hierarchical. Consequently qualitative researchers in general, and ethnographers in particular, feel the need to 'justify' their own practices with reference to those seen as of higher order. Within ethnography itself there is a rank order ranging downward from classical, through autoethnography, to short-term autoethnography. I imagine at the bottom of the barrel is the short-term visual auto-ethnography in which I often engage.

Even though it is often criticized for lack of rigor, today autoethnography is common practice in qualitative research. Anderson noted that most of it is evocative or emotional, and its advocates draw 'upon postmodern sensibilities' and 'distance themselves from realist and analytic ethnographic traditions.' For more solid grounding in social science methods, he proposed an 'analytic' version of the research practice in which the researcher is (1) a full member in the research group or setting, (2) visible as such a member in the researcher's published texts, and (3) committed to an analytic research agenda focused on improving theoretical understandings of broader social phenomena (Anderson, 2006: 375). This practice 'is consistent with qualitative inquiry rooted in traditional symbolic interactionism' (Anderson, 2006: 374). An interesting application of this approach was done by Schlichtman and Patch who, quoting Burnier (2006: 412) combined their personal and scholarly stories to create an account that 'is not strictly scholarly because it contains the personal, and [...] not strictly personal because it contains the scholarly' (2013).

For Pink and Morgan (2013: 352), short-term ethnography, such as used in visual anthropology and visual ethnography, is not an inferior, but an alternative route to knowing. Citing Ingold, they agree that ethnography alone is *not* anthropology, but a different scholarly endeavour whose objective is the description of 'the lives of people other than ourselves, with an accuracy and sensitivity honed by detailed observation and prolonged first-hand experience' (2008a: 69).

Yet, short-term ethnography as we have developed it is not disassociated from its academic roots in anthropology. It draws from contemporary renderings of anthropological ethnography, originating in the late twentieth century reflexive turn of the 'writing culture' debate (Clifford & Marcus, 1986) and its legacy (James, Hockey, & Dawson, 1997) the idea anthropological ethnography involves doing research *with* rather than about participants (Ingold, 2008b). Thus, short-term ethnography as we define it differs from its uses in other disciplines in that it is shaped by, and contributes to, distinctly anthropological ways of understanding and being in (and with) the world. (Pink & Morgan, 2013: 359)

I take a less defensive tack, and believe that the value of a method ought not be dependent on its service to another, even related, research enterprise. It is like the difference between an astronomer and an astronaut who examine the same celestial body. It is not merely a matter of degree of closeness that matters as much as the kind of knowledge one is capable of acquiring from the different standpoints. Although qualitative methods might recapitulate quantitative ones, they do not

require their confirmation. The tension between the two, often presented as dichotomous, might be explained by some attention to simple hermeneutics.

According to Kaltenbacher, Pardo, and Prato in this volume: ‘Contemporary urban anthropology goes [...] well beyond classical anthropology, for it adopts – and adapts – methods from other disciplines according to the specific research perspective, aims and geographic location. Common to anthropological research in the city is the interest in the complexity of urban life and the application of ethnographic methodology. Combined with a third element – the specific research objective, this leads to a great variety of approaches and to new paradigmatic challenges.’ In particular they note the past dominance of sociological research on Western urban, and, despite a commitment to interdisciplinarity, a need to ‘argue the difference between urban sociology and urban anthropology.’

My own experience has been that academic disciplines and sub-denominations serve more political as opposed to hermeneutic ends. That is, I need not be a certified anthropologist or sociologist to employ the methods, theories, and techniques of either discipline. Although I abhor the term ‘hermeneutics,’ the phenomenological sociology in which I engage emphasizes, if not exaggerates, the requirement that understanding social events requires an understanding of how the participants/creators themselves understand them. Therefore, the question of how ethnographers understand their own activities is critical.

Gadamer argued that ‘truth’ and ‘method’ were in conflict because approaches to humanities were in conflict. One approach to understanding a particular text was modelled upon the natural sciences, and the other implied that its interpretation required knowledge of the original intention of its author. For him, although meaning cannot be reduced to the author’s intentions, it is however dependent on the context of the interpretation. For Gadamer people have ‘historically-effected’ consciousness and are embedded in the particular history and culture that shaped them. These ‘prejudices’ affect their interpretations, but rather than being a hindrance they are prerequisites to interpretation. That is, the scholar interprets the history of a text by connecting it to his own background. According to Malpas, Gadamer’s work, in conjunction with that of Heidegger, was ‘not a rejection of the importance of methodological concerns, but rather an insistence on the limited role of method and the priority of understanding as a dialogic, practical, situated activity’ (2013).

From perspective of the Association for Objective Hermeneutics:

the standard, nonhermeneutic methods of quantitative social research can only be justified because they permit a shortcut in generating data (and research ‘economy’ comes about under specific conditions). Whereas the conventional methodological attitude in the social sciences justifies qualitative approaches as exploratory or preparatory activities, to be succeeded by standardized approaches and techniques as the actual scientific procedures (assuring precision, validity, and objectivity), we regard hermeneutic procedures as the basic method for gaining precise and valid knowledge in the social sciences.

(<http://www.objective-hermeneutics.com/>. See also: Oevermann, 1987)

The social construction of my own academic life world is informative in this regard. I was introduced to the Siamese twin sister disciplines of anthropology and sociology in 1961 at the Anthropology-Sociology, or Sociology-Anthropology Department at Indiana University. In the freshman year sequence the first introductory course was Anthropology and the second was Sociology. The primary distinction was that anthropologists studied culture, such as norms, or ways of doing while for sociologists it was the statuses, or positions, and their relations in social structures. Incidentally, when the department split, the defining characteristic of the Sociology Department was its quantitative, statistical emphasis and for Anthropology it was ethnography.

As Confucius might say, the decade of the 1960s was an ‘interesting time’ to be a budding sociologist. According to Turner, logical positivism ‘served as a source for claims to the status of “science”, and the logical positivist model of theory came to be invoked, or, in its own way, followed’ (Turner, 2007: 27. See also: Haney, 2008: 26–27; Wiley, 1979). While C. Wright Mills criticized Parsonians and other ‘grand theorists,’ Pitirim Sorokin took on quantitative sociologists such as Franklin H. Giddings, and George Lundberg. ‘These texts reflected the bitterness of the division in American sociology over quantification and its scientific status, a division which is crucial to understanding the later reception of logical positivism’ (Turner, 2007: 26). Sorokin wrote in *Fads and Foibles of Modern Sociology*:

The younger generation of sociologists and psychologists explicitly claims that nothing important has been discovered in their fields during all the preceding centuries; that there were only some vague ‘arm-chair philosophies’; and that the real scientific era in these disciplines began only in the last two or three decades with the publication of their own researches and those of members of their clique. Claiming to be particularly objective, precise, and scientific, our sociological and psychological Columboes tirelessly repeat this delusion as scientific truth. Accordingly, they rarely make any references to the social and psychological thinkers of the past. When they do, they hardly veil the sense of their own superiority over the unscientific old fogies. (Sorokin, 1956: 3–4. Citation from Haney, 2008: 129)

In addition to the too often referenced Qualitative-Quantitative divide, another methodological dichotomy that is often misrepresented is that between Descriptive as opposed to Analytic studies, with the latter afforded greater ‘scientific’ status than the former as it implies the need for quantification; that is, its validity and reliability are dependent on the employment of formulae and/or numbers. Analytic meant creating new knowledge from data as opposed to merely describing it. Deduction and Induction are also commonly presented as mutually exclusive dichotomies without taking note of its valuable synthesis of Analytic Induction.

The amicable divorce between anthropology and sociology mirrored in some ways the evolution of Znaniecki’s version of analytic induction as at first a quantitative (enumerative) methodological and theoretical innovation into Glaser and Strauss’s thoroughly qualitatively ‘Grounded Theory.’ At the time, quantitative testing of hypotheses logico-deductively drawn from established theories was becoming *de rigueur* for doctoral candidates; with minor adjustments necessary for those willing to employ those of Merton’s ‘middle range.’ The end result was that social scientists were defined more by the methods that they used than the subjects that they studied. It was at Indiana University (1966–1967) under the brief mentorship of Alfred Lindesmith (1937; 1968) that I was exposed to the theory-generating method of analytic induction established by Znaniecki, and that Lindesmith had refined in his work on opiate addiction.

There have been many definitions provided for analytic induction; simply put it is a process by which hypotheses are generated about a phenomenon and tested against successive observations. If a hypothesis fails, either the phenomenon is redefined or the hypothesis revised to include the exception (Turner, 1953; Tacq, 2007; Pascale, 2011). However, ‘[e]xactly what is meant by analytic induction remains something of a mystery though more than forty years have passed since Robinson and Turner’s attempts at clarification. This has allowed both well-informed critics and proponents to shift their positions to suit their interests, while “outsiders” who are looking for a fair assessment are left confused and frustrated at the end of the day’ (Goldenberg, 1993: 162).

According to Oktay, analytic induction, or the constant comparative method, evolved into a classic ethnographic approach that produced rich understandings of social life. The intent was not to test theories as much as develop them. It blends Strauss’s symbolic interactionism and qualitative descriptive approach with Glaser’s analytic interests and quantitative methods. The first was

the development of ‘middle range theory.’ They were critics of the ‘traditional Chicago-style urban ethnographies because they provided detailed descriptions but did not generate theories that would be useful for practice. They also criticized the type of “logico-deductive” theory that was popular at the time (1967) because it was based primarily on speculation and deduction, and was not empirically based’ (Oktay, 2012: 13; also Glaser & Strauss, 1967).

Besides the intra-disciplinary conflicts over research methods in sociology itself, I was introduced by anthropology to the ‘quantitative’ logic of the Human Area Relations Files at Indiana University. HRAF essentially translates one methodological language to another. It is a global-wide database of full-text ethnographies on nearly 400 different cultural groups that can be mined and used to make cross-cultural comparisons by a unique method designed for rapid and accurate retrieval of specific data on given cultures and topics (<http://www.yale.edu/hraf/guides.htm>). The ideal document is a detailed description derived from prolonged residence among the subjects. Pages are indexed and coded according to the *Outline of Cultural Materials (OCM)* (Murdock et al., 2008). The *OCM* consists of 710 subject categories plus a category numbered ‘000’ for unclassified materials. The categories are grouped into seventy-nine major subject divisions, each assigned a three-digit code ranging from 100 (Orientation) to 880 (Adolescence, Adulthood, and Old Age). Within each major subject division, up to nine more specific categories are defined. Thus ethnography can be used for both qualitative and quantitative research.

The phenomenology and ethnomethods I was later to embrace were, at the time, emerging on the fringe. The best example of this internecine conflict was the *Proceedings of the Purdue Symposium on Ethnomethodology* (Hill & Crittenden, 1968). These transcripts of exchanges between ethnomethodologists such as Harold Garfinkle and quantitative sociologists like Karl Schuessler (who taught me statistics at Indiana University) read like a conversation between people speaking two different languages, along with thinly veiled insults.

At my next stop, New York University, the schizoid experience of Indiana continued. There structural theory and quantitative methods were impressed on all students. Robert Bierstedt (1970) taught McIver (1947), Merton (1968), and Parsons (1951) to the exclusion of all else in his theory classes. There I was rewarded with an essay in which I blasted Erving Goffman’s behaviourism as being as dangerous to our discipline as Skinnerian psychology. In a year of statistics (descriptive and analytic) I was chastised for equivocating, at the level of logic, qualitative and quantitative methods. In the yearlong research (quantitative and qualitative) methods sequence there was hardly mention of ethnography. Most graduate sociology departments today offer separate courses on qualitative methods in which ethnography is a major topic.

My final quantitative methods paper at NYU argued that the choice made by investigators between quantitative and qualitative methods was a ‘matter of taste.’ I put social science in the context of a society and wrote that we should see research orientations as cultures and subcultures. In response to my question regarding choice of methods, my research methods professor, Herbert Menzel, replied: ‘It depends on what side your bread is buttered,’ however I believe I still only got a ‘B’ on the paper. Often overlooked in scholarly discussions of contrasting research methodologies are their economic and political implications as each has created their own distinct spaces in the field. The ghettoization of qualitative studies created an opportunity for academic entrepreneurs and as a result today also full-fledged qualitative separate journals, conferences, funding sources, and honours competitions as well as teaching and administrative positions.

These rather crass quantitative–qualitative and structural–cultural dichotomies continue today in the theoretical debates between proponents of the New versus Old Urban Sociology, or as described by Flanagan, culturalists who ‘explore the cultural, organizational, and social psychological consequences of urban life,’ and structuralists who ‘are concerned with the wider economic and political impact of the city’ (1999: 385–398). This is central to the urban culturalist perspective

in sociology; as Borer observes: '[t]he ways that people make sense of the world they live in, once lived in, or hope to build are tied to the places where they *practice* their culture' (2006: 175, emphasis in original). Borer critiqued American 'Schools' of urban sociology (Chicago, Urban Political Economy, Los Angeles), and created a new one composed of 'Urban Culturalists [who] explicitly investigate the symbolic relationship between people and places and the ways that persons invest places with meaning and value in order to make sense of their world. In fact, they have looked at the development and redevelopment of the built environment as a means for understanding cultural values, ideas, and practices' (2006: 180).

From Alan Blum I learned phenomenology and ethnomethods in an Urban Sociology course. Blum, and his collaborator Peter McHugh's, reflexive analytic social inquiry was a subversive version of sociology. In his course I was especially impressed by the work of the anthropological sociologist Erving Goffman who was at the time anathema in the discipline of sociology, yet later became, ironically, President of the American Sociological Association. My first professorial appointment was in the Brooklyn College Sociology Department, which has recently separated from the Sociology-Anthropology department. In some cases, faculty chose their own affiliation. As a result, Brooklyn College has long been dominated by sociologists with qualitative and ethnographic pedigrees starting with Hylan Lewis, Oscar Glantz, and Feliks Gross.

According to Key:

Qualitative research is a generic term for investigative methodologies described as ethnographic, naturalistic, anthropological, field, or participant observer research. It emphasizes the importance of looking at variables in the natural setting in which they are found. Interaction between variables is important. Detailed data is gathered through open-ended questions that provide direct quotations. The interviewer is an integral part of the investigation [...]. This differs from quantitative research which attempts to gather data by objective methods to provide information about relations, comparisons, and predictions and attempts to remove the investigator from the investigation. (1997)

In the 'hard' Sciences such as analytic chemistry that the social sciences wish to emulate, things are a bit more direct. Qualitative analysis is designed to identify the elements or compounds in an unknown substance; 'What is in this sample?' answers usually simple yes/no questions. Quantitative analysis determines the quantity of particular chemicals in a substance. It asks 'How much?' The modelling of qualitative analysis on the quantitative norms is typical and even the best arguments for ethnography – such as that by Small (2009) who playfully asked 'How many cases do I need?' – are defensive. However, Small strongly cautioned ethnographers against retreating toward models designed for statistical descriptive research and enjoined them to enhance their own.

[...] ethnographers in many fields of study [...] have no intellectual engagement with quantitative researchers, no expectation that the latter will ever be reviewers, no need to assess the work against a larger body of quantitative studies asking similar or related questions. [However, in] urban sociology [...] ethnographers must contend, explicitly or implicitly, with scholars trained in radically different traditions who claim expertise on the same questions and may well assume a unity of method.

Generally, the [qualitative] approaches call for logical rather than statistical inference, for case rather than sample-based logic, for saturation rather than representation as the stated aims of research. The approaches produce more logically sensible hypotheses and more transparent types of empirical statements. Regardless of the method, ethnographers facing today's cross-methods discourse and critiques should pursue alternative epistemological assumptions better suited to their unique questions, rather than retreat toward models designed for statistical descriptive research. (Small, 2009: 28)

Most qualitative research prolegomena seem to be excuses, or apologies for being qualitative. In contrast, Cicourel (2004) had shown decades ago that the findings of quantitative researchers are also impacted by the social and psychological contexts in which the craft is practiced. ‘I am NOT Opposed to Quantification or Formalization or Modeling, But Do Not Want to Pursue Quantitative Methods That Are Not Commensurate With the Research Phenomena Addressed.’

In response to a critique of his *Method and Measurement in Sociology*:

My concern has been with the way social scientists often ignore biases introduced by the variations in the way different research analysts USE methods. There is no way to avoid such biases. The best we can do is to try and identify such biases and take them into account when we discuss our results. The book was not an attempt to reveal how we should go about creating measures using different methods. I can only defend my position by reference to the many empirical studies I have conducted. The book *Method and Measurement in Sociology* was deliberately programmatic. The subsequent research I published and continue to publish attests to what I think can or should be done. (2004: 5)

Summary and discussion

From the earliest urban ethnographies of the Chicago School to those of today, in the search for reliably valid research findings qualitative and quantitative methods have been mutually beneficial. In my experience, most of the ‘intergroup’ related problems have been matters of interpretation of findings caused by ecological and atomistic reasoning errors. As described by Russo:

The ecological fallacy consists of inferring individual behaviours from aggregate measures. Robinson pointed out, for instance, that correlations between two characteristics measured on a binary basis among individuals (e.g. being black and illiterate in the US), or by proportions in regions (e.g. proportions of black and illiterate people in the population) were generally not identical and could even carry opposite signs. Conversely, the atomistic fallacy arises when, analysing individual behaviours, the context in which such behaviours occur is neglected. (2006: 102, note 7. Also Robinson, 1950)

As to the contrasting views of astronauts and astronomers, a particular problem of large-scale quantitative research has been its misapplication at local levels. For example, in the analyses of election data and surveys, ethnographic research can expose ecological fallacies; erroneously deduced conclusions about individuals (or small groups) solely on the basis of an analysis of group (larger group) data. Such was the case during my fieldwork for *Ethnicity and Machine Politics* (Krase & LaCerra, 1991).

In 1977, political experts and pundits, relying on large-scale voter surveys and the opinions of other experts, had predicted a easy victory of Stanley Steingut, one of the most powerful leaders in New York State, in a minor Democratic Party primary election. He was upset however by an unknown last-minute substitute candidate. From a distance the experts couldn’t see what was happening locally. Only close-up study could discover the reasons for the loss, which had to do with the structure and operation of the political club itself, as well as interpersonal and intergroup relations. An excerpt of my field experience during the ‘Last Campaign’ is illustrative.

My first assignment during the campaign was ascertaining our candidate’s voter appeal and creating a list of voters to ‘pull’ during the election. I was given a list of registered Democratic voters by voting address. Mine was a large territory of single and two-family dwellings showing signs of deterioration and racial transition. My job was also to note issues and other voter concerns. This information enables campaign strategists to fine-tune efforts. Although the registered voter list was only one year old, less than half remained in the community, and their replacements were not registered voters. Many newcomers were immigrants ineligible to vote, and almost all were black.

More important was the fact that those who were registered as Democrats and seemed likely to vote were anti-Steingut. These were older Jewish, Italian, and Irish long-term residents of the community. They cited a recent, highly publicized, 'Nursing Home Scandal' and 'Capital Punishment' as reasons for their vehement opposition. I tried to be polite and offered to discuss the issues but to no avail. Generally, those who seemed in favour of the Madison club candidates were newly located black families. The only good point for the club was the paucity of voters in the area.

The best time to canvass is in the early evening when people are at home, or on Saturdays in Gentile areas and Sundays in Jewish areas. Ideally, the canvasser is not the first and only contact with the voters. The canvasser should also be someone who knows the voters well, and is liked by them. One incident is illustrative; one canvasser was assigned to an election district in which he no longer resided. Unfortunately, his estranged wife still lived in the neighbourhood. The friends and relatives of his spouse were very unhappy with him and consequently the club candidates suffered. In another instance, novice canvassers were 'waylaid' by a resident in the hallway of a large apartment house with many registered voters. The resident, who was working for the opposition, used the opportunity to create a scene and when people came out to see what was happening, he went into a loud anti-Steingut diatribe. A person familiar with the building would have known whom to avoid.

When I returned to the club, after several evenings of canvassing, things seemed to be worsening. People in the social area talked more openly about problems and resentment toward the outsiders who were running the show. In the campaign room, there were no signs of difficulty. When I came in I was told to turn in my canvass to a staff member, but before I left the room to join my friends, Steingut, who was standing with some advisers, came over and asked how things were going. Still not yet aware that I should not be a bearer of bad news, despite my first experience, I replied that he would be 'lucky' if he got 30% of the vote. His smile turned to a frown. I felt uncomfortable and feebly tried to put some positive light on the situation by saying that at least there were not very many voters there. This did not help, and my conversation, which was overheard by the advisers, was quickly interrupted and Steingut was ushered away. (Krase & LaCerra, 1991: 199–200)

It wasn't until after the election, and for some analyses the publication of our book, that the experts began developing alternative explanations for the loss.

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