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A Historical Deconstruction of W. A. Marianne Boelen's Revisit to Cornerville

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Abstract

The purpose of this article is to demonstrate the value of using fieldnotes and supporting research to deconstruct refutations made by both researchers and lay writers. In particular, I use William Foote Whyte's unpublished fieldnotes and supporting research to deconstruct W. A. Marianne Boelen's misrepresentative refutation of *Street Corner Society* (1943), published in the *Journal of Contemporary Ethnography* in 1992.

To debunk Boelen's rebuttal, I divide the appraisal into *disciplinary themes* and *eccentric peculiarities*. Disciplinary themes include issues related to specific scholarly debates, such as whether Whyte should have included a study of the Italian family or whether the conclusions in *Street Corner Society* were influenced by ideas from the Chicago School of Sociology. Eccentric peculiarities, by contrast, concern Boelen's more arbitrary claims, for example, that the corner boys chose bowling teams differently from what Whyte had reported or whether gangsters offered Cornerville men beer. I found that none of the 20 refutations by Boelen that I dismantled were valid.

Introduction

Fieldnotes as historical relics for deconstructing research

Fieldnotes are usually seen as the fieldworkers intellectual property, which helps explain why they are rare, even in historical archives. I have access to William Foote Whyte's complete collection of more than 2,500 pages of fieldnotes for *Street Corner Society* (*SCS*) (1943), written during his three-and-a-half years of fieldwork from 1937 to 1940 in Boston's North End (XX; William Foote Whyte Additional Papers. Kheel Center for Labor-Management Documentation and Archives, Cornell University Library).

The most obvious reason for reanalyzing fieldnotes is to validate and compare them with the finished ethnographic text.[1] Another important but underexplored reason is that fieldnotes can help critically dispute assumptions and statements made by both researchers and lay writers about an ethnographic study.

While writing the introduction to the Swedish translation of *SCS* in 2009–2010, I developed the intuition that W. A. Marianne Boelen's refutation of *SCS* rested on questionable grounds (XX). There were simply too many strange aspects to her claims.

My subsequent in-depth historical research into Whyte's fieldnotes, which resulted in an anthropological-historical monograph in 2022, confirmed my initial impressions (XX).

It is time to debunk the many fabrications in Boelen's article. Many researchers and writers have repeated its misrecognition's without scrutiny. Of course, classic ethnographies should be able to withstand close scrutiny. Indeed, such ordeal is part of what makes them classics. In my view, however, this should be done in an honest and rigorous manner.

Journal of Contemporary Ethnography's special issue on Whyte and *SCS*

My reading of the introduction in *Journal of Contemporary Ethnography* (*JCE*) by Patricia A. Adler, Peter Adler, and John M. Johnson to the 1992 special issue suggests that they regarded Boelen's rebuttal of Whyte and *SCS* as a significant scoop, comparable in impact to the way Derek Freeman in 1983 challenged the reputation of Margaret Mead. Adler and Adler also served as editors of the special issue.

In a footnote, Adler, Adler, and Johnson (1992: 3) thank several prominent sociologists, as well as, more generally, "several Cornerville people who confirmed the authenticity of Boelen's research."

They likely relied on Boelen's account without directly interviewing the Cornerville people she mentions. In the article, they praise Boelen's "important assertions about William Foote Whyte's factual misrepresentations during the study" (Adler, Adler & Johnson 1992: 7).

One might ask what they actually knew about Whyte's alleged factual misrepresentations in *SCS*.

Adler, Adler, and Johnson (1992: 8) quote Boelen as saying that Whyte did not tell his informants that he wrote a book about the people of the North End. Whyte's fieldnotes show he told the informants he was writing a book about the neighborhood. He even had his main informant, Doc (whose real name is Ernest "Dean" Pecci), read and comment on various drafts of the manuscript before *SCS* was published. Later in the research process, Whyte (1995) began a lifelong collaboration with his other main informant, Sam Franco (whose real name is Angelo Ralph Orlandella). Whyte was far ahead of his time when it came to ethnographic collaboration and openness, as is shown by the fact that field researchers in the United States up until the 1960s often conducted their studies covertly (Dalton 2013 [1959]; Rose 1987).

Adler, Adler, and Johnson strangely make no distinction in the introduction between Whyte's three and a half years of fieldwork, during which he lived in two locations in the North End, and Boelen's brief, undocumented, and sporadic visits to the neighborhood between 30 (1970) and 50 (1989) years later. The longest period Boelen spent in the North End (Cornerville is the fictional name in *SCS*), if we are to believe what she writes, was three months in 1989; otherwise, her visits lasted one to two weeks. The time gap between 1970 and 1989, during which Boelen claims to have visited the North End about 25 times, is completely undocumented (Boelen 1992: 13). It is reasonable to expect that extensive, long-term field research spanning three and a half years, comparable to Bronisław Malinowski's (1978 [1922]) seminal stay in the Trobriand's, would yield more robust and substantiated insights into the customs of local inhabitants than those derived from Boelen's shorter, sporadic visits.

Adler, Adler, and Johnson also do not seem to have investigated the fact that Boelen, at Columbia University, had already written a Master of Arts thesis in political science on *SCS* in 1971, with a similar title to that in *JCE*. This is at least not apparent from their introduction.

Marianne Boelen's anachronisms, factual errors, and cacophony of dishonesty

Boelen's article contains many repetitions, mixed lines of argument, incoherent sentences and paragraphs, and validity problems regarding respondents. Most evidence indicates that the people in *SCS* whom Boelen claims to have interviewed or met were either deceased or very old at the time. The article also contains anachronisms, out-of-context reasoning, and several incorrect sources. For example, Herbert J. Gans' book *The Urban Villagers* about Boston's West End is stated by Boelen as having been published in 1965, when it was in fact first published in 1962, with a second expanded edition appearing in 1982.

What is presented under the various headings of the article is not rigorous scholarship but rather a set of incoherent and made-up claims. The factual errors are particularly problematic because they require the reader to be familiar with *SCS* and other sources in order to detect them. The article thus becomes a hodgepodge of inaccuracies.

Boelen's article is closer to a defamatory pamphlet than it is to a scholarly scientific paper. She makes numerous claims that are not supported by empirical facts. Boelen does not report who she interviewed, what questions were asked, what answers were given, or provide quotations in context that can be independently verified. Only her own interpretations are presented. The frequency of factual errors and anachronisms is so high that it requires considerable effort to disentangle them. The editors of *JCE*, Adler and Adler, should have required Boelen to correct these inaccuracies. Researchers, such as Augusto Ferraiuolo (2006, 2009) and Steven Lubet (2018), who rely on Boelen's article to question Whyte's *SCS* are, in this regard, recklessly gullible and careless.

Boelen writes in a footnote (1992: 50) that the article was read and approved by Doc's sons, Doc's brother, Doc's niece or nephew, Chick Morelli (Christopher Iannella Sr.), Dr. Merluzzi, the Nortons, Nutsy, Frank Bonelli, Angelo Cucci, Bill Fopiano, Joe Gennusi, Tom Scala, Pat Russo, Mr. Kendall (Frank Havey, social worker and later director of the North End Union Settlement House), George Ravello Jr. (Frederick C. Langone, politician in the Democratic Party), the restaurant family, and social worker Clara D. Gommes.

As a reader, one never learns from Boelen's interviews who said what, or what responses were given. The interview questions and answers are not reported. It is therefore unclear which members of the Nortons —beyond possibly Nutsy, Frank Bonelli, and Angelo Cucci—actually read and approved the article. Danny and Mike, who were Doc's deputies and closest friends in the Nortons, are not mentioned. It is uncertain whether Boelen attempted to locate the gangster Tony Cataldo (Jeremy "Jimmy" Spats). Julia (Mama) and Fredo (Papa) Orlandi in the restaurant family were, according to the 1938 census, more than 60 years old and could hardly have been alive in 1970 (City of Boston 1938). Their son, Averaldo Orlandi, would have been 80 years old in 1989, if still alive.

Given that Boelen does not present her interview material, it is difficult to determine whether anyone other than Doc's sons, Doc's brother, Doc's niece or nephew, Christopher Iannella Sr., and Frederick C. Langone actually read and approved the article. It is unlikely that those who gave approval, unlike the editors Adler and Adler, were familiar with *SCS*, able to evaluate the sources critically, or well-versed in

the broader historical context. Much suggests that Boelen misrecognized Whyte's work while presenting herself in a favorable light to Doc's family.

The structure of my article

To create a conspicuous, more coherent structure from the unsystematic and scattered content in Boelen's article, I have created new headings that assemble material from different parts of her text. Under these headings, I have included lines of reasoning that can be traced both to *disciplinary themes* and to *eccentric peculiarities*. Some of the paragraphs are rather short, but I believe that, taken together, they create a more complex and composite depiction of both Boelen's refutation and my deconstruction.

The disciplinary themes are organized around the following ten questions: Was it a weakness for *Street Corner Society* that Whyte was not of Italian heritage? Did the Italian North Enders know that Whyte wrote a book? Should Whyte have included a study of the Italian family in *Street Corner Society*? Was calling the North End a slum wrong? Has the concept of social disorganization, as developed by the Chicago School of Sociology, been incorrectly dismissed? Was the Mafia overemphasized? Was gang properly defined as a concept? Is the emergence of gangs in the United States the result of the disintegration of European peasant culture? Is it unproblematic to compare men hanging on street corners across cultures? Did the Chicago School of Sociology's ideas about the slum influence Whyte's conclusions in *Street Corner Society*?

The eccentric peculiarities include the following ten assumptions and statements: "Two settlement houses of the North End", "State Senator George Ravello did not take bribes from the Mafia", "Chick Morelli was elected to the Boston City Council", "Boelen listened to the voices of the Italian North Enders", "Characters in *Street Corner Society* answered the same questions in the same way", "The North End was both a *Gemeinschaft* and a transient hotel", "The Nortons gathered in Frank's kitchen", "Whyte was wrong about how the Nortons chose bowling teams", "Gangsters did not offer Cornerville men beer", and "Academics of Italian heritage kept quiet".

I have separated the questions from the assumptions and statements in Boelen's article to illustrate how difficult it is to identify all the unusual instances of dishonesty. This structure also helps explain many, though not all, of the peculiarities that emerge in her work, since additional assertions remain. My analysis suggests that Boelen was dishonest and misrepresented her statements about *SCS*.

The other articles in *Journal of Contemporary Ethnography's* special issue

When the *JCE* special issue was published in 1992, William F. Whyte and his main informant Angelo Ralph Orlandella published harsh replies to Boelen's article. Arthur J. Vidich provided a disciplinary review, while Norman Denzin and Laurel Richardson cast doubt on the possibility of conducting a representative naturalistic ethnographic study. I have created a new, independent, and critical analysis of Boelen's article. Although there may be some overlaps with my own analysis, I have not referenced or addressed any of the other articles mentioned, regardless of their merit.

What I want to emphasize, however, is that my article, based on Whyte's fieldnotes and supporting research, demonstrates that a critical deconstruction of other researchers' texts and sources can play an important role in debunking imposture and dishonesty.

In the future, in the undulating wake of artificial intelligence, there is an imminent risk that it will matter less whether research findings are true or false, and more how they are packaged.

Was it a weakness for *Street Corner Society* that Whyte was not of Italian heritage?

Boelen does not seem to understand the basics of fieldwork. It was more common for early fieldworkers, at least in sociology, to work surreptitiouslyin order to become insiders, whereas since the 1960s most researchers have sought to work overtly. An insider risks losing distance and objectivity, that is, "going native." It is often, though not always, advantageous for the fieldworker to come from a different culture than the group being studied, since this can make it easier to observe what group members take for granted (Sluka & Robben 2012 [2007]). Max Gluckman's (1956) outline of witchcraft is a seminal case of disparate but not incommensurable cultural beliefs.

Notable exceptions are Nels Anderson (1923) and Francis A. J. Ianni (1972), who, in different ways, studied the cultural environments from which they originated. Anderson observed homeless men in Chicago, while Ianni studied a family within the Italian Mafia.

Boelen's claim that Whyte, after three and a half years of field research, based his ethnographic descriptions on prejudice and insufficient knowledge of Italian North End customs is unconvincing. In the 1930s, the expectation among anthropologists was generally that fieldwork should last about one year; three and a half years was exceptionally long (Keesing 1981 [1971]: 6). Whyte was persistent and wrote more than 2,500 pages of fieldnotes, indexed by group rather than by subject, which allowed him to trace the development, regrouping, and disintegration of four or five specific groups over time.

The significance of being an outsider has been discussed by more than anthropologists and sociologists. The British philosopher Stephen Toulmin (1961: 13) similarly argued that researchers who study their own scientific discipline risk focusing only on familiar details while missing the larger and less customary picture.

Boelen repeatedly questions whether Whyte was enough of an insider to understand Italian language and customs. However, the North End was very different in the 1930s, when about 90 percent of residents were of Italian heritage, than it was in 1970 or 1989, when approximatelyhalf of the residents had Italian ancestry and relatively few first-generation immigrants from before the 1930s remained.

One example Boelen gives is Whyte's account of looking for a man in a part of the North End where residents did not know who he was. That Italian residents would be suspicious of an Anglo-American searching for someone in the neighborhood is not surprising. Locals would probably have been suspicious of anyone they did not know, regardless of ethnicity. Boelen seems to assume that the North End remained unchanged between the 1930s, 1970, and 1989.

Boelen's claim that she went around the North End knocking on doors in search of gang members from *SCS* sounds implausible. One wonders whether the nicknames she claims to have used were the same fictitious names Whyte used in *SCS*. Which gang does she mean? What were the members' nicknames? In which blocks did they live? Boelen claims that everyone knew one another, yet she provides no concrete or specific information about who knew whom or where they lived.

Boelen also seems convinced that Italian Americans in the North End lived in exactly the same way as people in Italy. She does not appear to recognize that Italian Americans had long since adapted to American social conditions. Italians who arrived between 1880 and 1924, after which immigration from Italy largely stopped, did not necessarily identify with Italians who arrived after 1961, when immigration from Italy increased again.

Did the Italian North Enders know that Whyte wrote a book?

Boelen rhetorically asks whether Whyte committed a cardinal sin by failing to tell North End residents that he was writing a book. However, she does not specify which interviewees complained that Whyte had concealed this fact.

Contrary to Boelen's claims, Whyte's unadulterated fieldnotes make it indisputable that Doc and other North Enders knew Whyte was writing about the North End. This becomes especially obvious in the fieldnotes from 11th of November 1937 concerning the social distinction between corner boys and college boys, as well as the social organization of the Mafia:

I [Whyte] dropped in on Pecci [Doc] this afternoon. He was laying on the bed in his room, reading. First I went over my discussion with Mr. Cushman with him, then I asked him if he read my paper on the corner boys and college boys and on the organization of the rackets. He said he had and found nothing to object to in it except certain references I made: that I looked out from my room on the corner where the racket men hang, that a representative from this district is backed by the racketeers. He said such things in print could start an investigation. I reassured him that I wouldn't put such things in print (Whyte /4087, Box 4, Folder 2).

According to Boelen, Doc's sons believed that *SCS* consisted largely of fantasies. However, it is not clear from quotations or other empirical evidence what the sons themselves actually thought. Boelen reports only her own interpretations of their responses, which significantly reduces the validity of her claims.

The more important question is what Boelen told Doc's sons, if she met them at all, about Whyte and *SCS*. It is possible that she suggested Whyte had made a fortune from the book and that Doc, as its rightful co-author, had been cheated out of royalties (Whyte /4163, Box 2A, Pecci estate vs. Whyte). There is good reason to suspect this, because in 1992, 22 years later, the sons threatened Whyte with a lawsuit demanding royalties. Although the lawsuit was never filed, it suggests that Doc's sons believed Whyte had exploited their father.

Doc's sons probably did not know what the procedures and conventions were in academic publishing, and it was Boelen's responsibility to clarify this. Even in 1970, it was unusual for anthropologists and sociologists to share royalties from books with individual informants or groups. Exceptions include R. Lincoln Keiser (1979 [1969]: 87) and James P. Spradley (1980: 21–22). My view is that Boelen after their father died in 1967 worsened an already strained situation for the two sons.

There is no evidence that Whyte made a fortune from *SCS*. On the contrary, he was unusually honest and generous in acknowledging the contributions of Doc and also Sam Franco. It was precisely this openness that Boelen later exploited and turned against him.

The fieldnotes are a historical relic that has remained unaltered since Whyte wrote them in the North End between 1937 and 1940. They show that he discussed drafts of the manuscript with Doc, in what would now be named respondent validation. Later in the research process, Whyte also employed Sam Franco as a field assistant.

When *SCS* was published in 1943, no field researchers, as far as I know, had yet developed the practice of requiring their ethnographic work to be approved by the group being studied. There had been collaborations between anthropologists and indigenous informants, but not an expectation that the entire study had to be approved before publication (Wert et al. 2011).

Anthropologists often documented cultures that lacked written languages. What made Whyte's study unusual at the time was that he examined an ethnic group in which second-generation immigrants spoke the same language as he did; few anthropologists had done so before. Nels Anderson (1923) had studied homeless men in Chicago's Hobohemia, Frances R. Donovan (1929) had worked covertly as a saleslady in New York City, and Frederic M. Thrasher (1927) had mapped the territory of Chicago gangs, but their ethnographies did not focus as concretely as *SCS* on a single social group.

Since the 1960s, ethical concerns about the relationship between fieldworkers and the people they study have received greater attention, and some ethnographers have practiced respondent validation (Spradley 1980: 21–22). As the quotation above suggests, Whyte discussed drafts and ideas with Doc, though they did not always agree.

Respondent validation has nevertheless not become a widely used practice. There are several pitfalls, including deciding who within the group studied should have the authority to read, revise, approve, or reject fieldnotes and ethnographic analysis. Another potential pitfall is that consent may be influenced by the friendship between the researcher and the informant rather than by considerations of representativeness. Certain field researchers, seeking to ethically validate their research, occasionally acknowledge colonial guilt in relation to the people they study (Emerson 1983b: 107; Wert et al. 2011).

Should Whyte have included a study of the Italian family in *Street Corner Society*?

Boelen repeats her criticism that Whyte did not study the Italian family on pages 41–42 and again at the bottom of page 43. However, the fact that fieldworkers may have a particular interest in a given

phenomenon does not necessarily mean that their research is biased, predetermined primarily by theoretical concepts (Emerson 1983c: 184–189) or ideological assumptions (Hobsbawm 1987: 273).

Good research, which often leads to unexpected discoveries, is usually driven by curiosity rather than by what others think should be investigated or by predictable intellectual trends (Moore 1963). Research also requires separating one's own moral assumptions from those of the group being studied.

Whyte did not study the Italian family largely for reasons of delimitation and, I suspect, because he came to know the bachelor Doc. Doc was almost 30 when Whyte settled in the North End in the spring of 1937, and had it not been for the Great Depression, he probably would have started a family. However, Doc was unemployed, living with his sister, and unable to support a family, which was more or less an unspoken rule for Italian men at the time.

Rosalie H. Wax (1971: 46–47) makes clear, referring to Whyte, that anthropologists often study groups that maintain a strict social separation between the sexes and generations. Whyte socialized only with men because, in the Italian American cultural setting, it was unthinkable to get to know women of the same age on a one-to-one basis unless one intended to propose marriage.

Whyte therefore had to adapt to the cultural customs that prevailed among Italians if he wanted to conduct the study at all. Jack Katz (2015: 138–139) posits that the way in which a group reactively assigns an identity or role to a field researcher reveals significant aspects of its social organization.

Rather than studying the Italian family, Whyte made groundbreaking discoveries in what was then virtually unexplored research territory. Many of his observations remain important today, including his work on participant and direct observation, the social organization of the slum, ethnicity and minorities, the social distinction between corner boys and college boys, the political machine, action research, the relationship between gangsters and politicians, and patterns of interaction in small groups.

Was calling the North End a slum wrong?

Whyte was far from the only researcher in the 1930s and 1940s to argue that the North End was a slum. Walter Firey (1968 [1947]: 173–174), an authority on the subject, stated in 1947, four years after the publication of *SCS*, in his respected ecological study of Boston neighborhoods that the North End could be defined as a slum.

According to Firey, a slum is characterized by substandard housing, high population density, a declining population, and a population that is either foreign-born or highly heterogeneous. The North End met all of these criteria except for ethnic heterogeneity; instead, it was ethnically homogeneous.

Residents may later claim, 30 or 50 years afterward, that they do not want their neighborhood in the 1930s to be described as a slum. However, it is important to remember that the North End had by then undergone gentrification (Jacobs 1992 [1961]: 8–10), and that minority groups from the early 1960s

onward, during the rise of identity politics, often celebrated and embellished the history of their own culture (Shibutani & Kwan 1965).

The North End was not the same neighborhood in 1970 or 1989 as it had been in 1937–1940. By the 1980s, the neighborhood's population had been cut in half, and the proportion of Italians had fallen to under 50 percent, compared to 90 percent in the 1930s and 1940s (XX).

Has the concept of social disorganization, as developed by the Chicago School of Sociology, been incorrectly dismissed?

Boelen references Whyte (1968), but this source is not listed in the article's references. Patricia A. Adler and Peter Adler, as editors of *JCE*, should have pointed this out.

The editors should also have read Bernard J. Newman's (1937 [1934]: 93–98) contribution, "Slums," in the *Encyclopaedia of the Social Sciences*, which Boelen refers to in the article.

They would then have discovered that, according to Newman, slums can be understood in terms of three general dimensions: economic, social, and governmental. I also checked the 1968 edition of the *International Encyclopedia of the Social Sciences*, in which "slum" does not appear as an entry.

Contrary to Boelen's assertion, Newman does not include the Chicago School of Sociology concept social disorganization as an additional criterion for defining slums.

Since the Whyte (1968) source Boelen cites cannot be located, I instead checked what Whyte writes in *SCS* about his doctoral defense, when Chicago sociologist Louis Wirth asked him to define the slum:

"I replied that a slum district was simply an urban area where there was a high concentration of low-income people living in dilapidated houses and under poor sanitary and health conditions. Wirth objected that this was not a sociological definition, but I refused to satisfy his conceptual appetite, replying simply that the conditions I stated were what had led me into the study of the North End and that I considered it an empirical problem to determine how people lived under those conditions" (Whyte 1993 [1943]: 356).

Boelen is correct that Wirth was dissatisfied with Whyte's definition. As the quotation shows, Wirth objected to Whyte's conclusion of the slum as socially organized rather than socially disorganized.

Whyte's central argument in *SCS* was precisely that the slum was socially organized and not, as the Chicago School of Sociology and others often assumed, socially disorganized.

Boelen also makes a strange and anachronistic comparison between the American urban slum and life in the rural Italian region of Abruzzi (or Abruzzo) during the 1950s. Had she instead compared the slums of Boston and Chicago with those of Naples, Rome, or Palermo during the 1930s, the comparison would have been more convincing.

Was the Mafia overemphasized?

Whether the Mafia played a dominant role in the North End during the 1930s must be understood in its social and historical context. After the repeal of Prohibition in the United States in 1933, gambling and the lottery replaced illegal alcohol sales as the Mafia's most important sources of income, although alcohol remained profitable for some time.

Local lottery agents who accepted bets from North End residents could be grocery store owners, barbers, restaurant proprietors, club owners, or private individuals. There were also bookies, speakeasies, and taverns where residents could gamble on lotteries, slot machines, horse and dog racing, prizefights, craps, billiards, and cards.

The Mafia also profited from arson, truck hijackings, extortion, prostitution, violence, and loan sharking, while the drug trade was growing. Although illegal, the lottery was tolerated by authorities who benefited from it. Local politicians and Mafia figures sometimes helped residents who could not afford rent, clothing, or food by providing money or services.

Most North Enders appear to have accepted the lottery as part of common sense. Extended families and local communities were held together through moral obligations, patron-client ties, and forms of clientelism in which it was often difficult to distinguish between legal and illegal activity. These practices frequently occurred simultaneously and were socially intertwined.

Only a small number of North Enders were in direct contact with leading Mafia figures. More residents worked for lower-ranking intermediaries or simply placed small lottery bets while shopping at the local grocery store or visiting a neighborhood bookie. Whyte discusses these patterns in *SCS*, and they are supported by the fieldnotes (XX; Whyte 1993 [1943]).

Boelen makes the issue too simple by sharply separating legal from illegal activity in the North End, detached from its historical and social context. Only long-term fieldwork makes it possible to observe what urban villagers take for granted; short visits and direct questions do not.

Whyte also had the advantage of being an attentive outsider who could observe aspects of social life that residents themselves may have viewed as ordinary or unremarkable. To understand what people unconsciously practice and believe, participant observation is often the most appropriate method (Becker et al. 1977 [1961]).

By the time Boelen visited the North End in 1989, the Mafia had largely disappeared from the district (Lehr & O'Neill 2012 [2000]). In 1970 and 1989, community spokespersons were more likely than in the 1930s to celebrate Italian cuisine, film, fashion, and musical talent than to discuss topics that could damage the neighborhood's reputation, such as the Mafia or fascism.

Donald R. Cressey (2008 [1969]: xxiii), a leading authority on American organized crime, described the Mafia's role in the United States in the late 1960s as follows: "Their activities continue to be viewed as

harmless little games. But the game is neither harmless nor little. Organized criminals are gradually but inexorably stealing our nation. And still no one cares."

Was gang properly defined as a concept?

Frederic M. Thrasher's (1927: 29) classic definition of gangs, which has been used by distinguished gang researchers such as R. Lincoln Keiser (1979 [1969]) and by James F. Short Jr. and Fred L. Strodtbeck (1965), is as follows:

"The majority of gangs develop from the spontaneous playgroup. As the boys or older fellows of a block or neighborhood come together in the course of business or pleasure, a crowd, in the sense of a mere gathering of persons, is formed."

However, Boelen instead uses Thrasher's definition of gangs from the *Encyclopaedia of the Social Sciences* (1937 [1931]: 564):

"A study of the formation of the modern gang reveals that the juvenile gang emerges from play groups and similar aggregations of boys which form most readily in the congested and disorganized portions of the community."

Whyte's definition (1955 [1943]: 255) of street gangs in Boston's North End corresponds more closely to Thrasher's definition in *The Gang*, whereas Thrasher's definition in the *Encyclopaedia* differs by adding the idea that gangs emerge territorially in the disorganized spaces of society:

"The corner-gang structure arises out of the usual association of the members over a long period of time. The nuclei of most gangs can be traced back to early boyhood, when living close together provided the first opportunities for social contacts."

Thrasher did not provide a single, entirely distinct, or easily discernible definition of gangs in either *The Gang* or the *Encyclopaedia*, because gangs are a complex and context-dependent social phenomenon. Thrasher uses the concepts of disorganization and interstitial in both texts. Whyte's research is primarily limited to Italians in the North End, whereas Thrasher's broader and particular gang research includes many ethnic groups as well as lawless gangs in the American frontier. Thrasher's ethnography *The Gang* is largely about boys and men who move through the slum's interstitial spaces and territorial no-man's-land.

It is however not surprising that Whyte's and Thrasher's ethnographic descriptions of the opportunistic and unstable alliance between the Mafia, street gangs, and politicians independently overlap in several respects. Both fieldworks are grounded in the gambling, the lottery, liquor sales, and ward politics of the United States during the 1920s and 1930s.

The key difference between them, however, is that Thrasher relied on Robert E. Park's and the Chicago School of Sociology's quite normative concept social disorganization. Whyte, by contrast, was, through

Conrad M. Arensberg and William Lloyd Warner, trained in A. R. Radcliffe-Brown's structural functionalism, in which the concept of social organization played the central role. This is a distinction that Boelen completely overlooks (XX).

Even if it were true, which has not been confirmed, that, as Boelen (1992: 22) writes, "the target population felt hurt" by the fact that corner boys in the 1930s were described as street gangs, Whyte at least grounded his definition in three and a half years of field observation.

Once again, Whyte conducted his field research in the 1930s, whereas Boelen visited the North End only sporadically 30 to 50 years later. By then, the housing stock had been renovated, the population had declined, and the Italian share of the population had fallen substantially. It was, quite literally, a different neighborhood (XX; Jacobs 1992 [1961]: 8–10).

Is the emergence of gangs in the United States the result of the disintegration of European peasant culture?

Boelen's assertion that Whyte concluded that gangs emerged from the disintegration of European peasant culture is incorrect.

One of Whyte's central arguments in both the 1943 article Boelen refers to and in *SCS*, which I have discussed elsewhere (XX), is a criticism of the Chicago School of Sociology concept of social disorganization (Blumer 1979 [1939]: 62–69; Thomas & Znaniecki 1958 [1918–1920]: 1127–1133). In brief, this concept refers to the breakdown or disintegration of customs within the peasant primary group and, simultaneously, to the process of individualization. This cultural and social transformation illustrates the transition from a traditional rural peasant culture to a more turbulent, emancipatory, and individualistic urban way of life (XX). Whyte (1943, 1993 [1943]) argues the opposite of what Boelen claims. He does not suggest that gangs lacked social organization because immigrants had been uprooted from European peasant culture. Rather, he argues that gangs were a product of immigrants' adaptation to urban life in the United States, an argument supported by Cressey (2008 [1969]: 25–26).

According to Whyte, the gang was a new and modern form of social organization, not simply a consequence of emigration from Europe.

During the 1960s, heated debates took place over whether the Mafia, then also known as Cosa Nostra, was an American creation or a branch of the Italian or Sicilian Mafia (Cressey 2008 [1969]: 8–71).

Is it unproblematic to compare men hanging on street corners across cultures?

According to Boelen, the gangs that gathered on street corners in the North End during the 1930s, 1970, and 1989 practiced cultural customs that had been transferred unchanged from Italy, where men also gathered on street corners.

Boelen claims that she bases her conclusions on her observations in the Italian Abruzzi (or Abruzzo) region, where she lived between 1955 and 1959, as well as on interviews with prominent Italian academics (Boelen 1992: 25–26). If those academics knew how Boelen used their answers, I suspect they would have objected.

Boelen does not present any empirical evidence that would justify a comparison between Italy and the North End. Comparative research across countries and continents, without a detailed empirical and historical foundation, is problematic because an outsider such as Boelen may misrecognize a social activity that appears similar on the surface.

For example, there were, and still are, major differences between the customs of northern and southern Italy, as Denis Mack Smith (1997 [1959]) points out. Moreover, by the time Boelen conducted her observations, 30 to 50 years had passed since Whyte's fieldwork.

Men gathering on street corners in the United States was common not only among Italians, but also among workers and gangs from other ethnic groups. Later ethnographic studies of African Americans, workers, and other ethnic groups in the United States often refer to *SCS* (Anderson 2003 [1976]; Keiser 1979 [1969]; Liebow 2003 [1967]; Suttles 1968).

Furthermore, Whyte's field research coincided with the Great Depression, when mass unemployment prevailed among workers in both the North End and the United States (Anderson 1974 [1940]; Trout 1977).

Boelen also argues that Italians in the North End, Frankfurt, and Rotterdam gathered on street corners for the same reasons as people in Italy. However, the assumption that Italians in the North End in both the 1930s and 1970 gathered on street corners for the same reasons is unlikely.

Men who gather on street corners, regardless of ethnicity, class, nationality, or historical period, usually do so for different reasons (Anderson 2003 [1976]; Keiser 1979 [1969]; Liebow 2003 [1967]; Whyte 1993 [1943]).

Boelen's own assumptions about Italians, together with her ambition to rebut Whyte and *SCS*, seem to blind her to these differences.

Did the Chicago School of Sociology's ideas about the slum influence Whyte's conclusions in *Street Corner Society*?

Boelen was wrong claiming that the Department of Sociology at the University of Chicago, headed by Robert E. Park, advocated social reforms for slum dwellers. Park, who left Chicago after retiring in 1933–1934, instead broke, together with William I. Thomas, with earlier traditions of social reform and Christian sociology. They aimed for an objective, value-free sociology modeled after natural sciences, concentrating on urban life, human ecology, migration, personal documents, cultural conflict, and social institutions. That the Chicago School of Sociology did not advocate social reform should not, either, be

conflated with the idea that its sociological perspectives influenced Whyte's conclusions in *SCS* about the slum.

Ernest W. Burgess, Herbert Blumer, Louis Wirth, Everett C. Hughes, and William L. Warner, who shaped the Departments of Anthropology and Sociology at the University of Chicago during the 1930s and 1940s, mostly shared Park's view of social science. Warner was Whyte's supervisor in Chicago from 1940 to 1943. (XX).

Two settlement houses of the North End

When Boelen visited the settlement house in 1970, Mr. Kendall was still working there; he retired in 1974 (Whyte 1993 [1943]: 344). What makes it difficult for the reader is that Boelen mixes things up, making it hard to determine which parts of her account are accurate.

Mr. Kendall (Frank Havey) was a social worker and later director at North End Union Settlement House on Parmenter Street. However, the settlement house most frequently mentioned in *SCS* is the North Bennet Street Industrial School on North Bennet Street, where George C. Greener (Mr. Bacon in *SCS*) served as director between 1915 and 1954, which is far from evident in Boelen's article.

It becomes even more peculiar when she claims, in connection with her outline of the settlement house, that Chick Morelli was the leader of a gang, when in *SCS* he is portrayed as a college boy. One of Whyte's main points in *SCS* is that corner boys were part of informal street corner gangs, while college boys belonged to more formal clubs based on role models and patterns they had learned in school and in the settlement house.

Boelen's constant congeries of factual errors and misrecognitions render her entire article unreliable and substandard. It places a heavy burden on the reader to sift through the material. The fact that researchers and lay writers have treated her refutation of *SCS* as reliable is therefore problematic.

State Senator George Ravello did not take bribes from the Mafia

Boelen reports interviewing seniors who recalled politician George Ravello. Ravello's real name was Joseph A. Langone Jr. (1893–1960) (Puleo 2007; Wheeler, Boston Public Library). Boelen does not mention the interview dates. Was it 1970, 1989, or sometime in between? Nor does she mention where the interviews took place, how long they lasted, or which seniors she interviewed. She could have described the seniors' relationship to Ravello without exposing their identities. She does not specify what questions she asked or what the interviewed seniors answered. The empirical data are completely lacking. One gets the impression that Ravello was alive in 1970 or 1989, but he died in 1960.

Ravello was an undertaker, and while his political machine was not as organized as those of some other bosses, Whyte's fieldnotes show that he depended on Mafia support to win his election.

Political corruption was not primarily, as Boelen asserts, about bribes. Politics in Boston and the North End during the 1930s were based on favors and reciprocity: votes in exchange for construction contracts, licensing agreements, information about when and where police would conduct raids, political appointments of police chiefs and attorneys, or bail. The politician needed votes to get elected, and the Mafia needed favors to circumvent the law and make money.

Between 1932 and 1940, Ravello was a colorful State Senator in the Massachusetts Congress; however, his attempt to win a seat in the United States Congress was unsuccessful. The fact that Italian American spokespersons after World War II did not want to discuss the importance of the Mafia follows a pattern found in the identity politics of other ethnic groups, where history is often embellished so as not to tarnish the group's reputation (XX; Cressey 2008 [1969]; Shibutani & Kwan 1965).

Chick Morelli was elected to the Boston City Council

Boelen's anachronistic assertions lead the reader to believe that Chick Morelli was elected to Boston City Council at the same time Whyte was conducting field observations in the North End. In fact, Morelli was elected to the Massachusetts House of Representatives and Boston City Council after World War II.

The North End, South End, and West End constituted Ward 3 during the 1930s. When Boelen refers to Chick Morelli being elected to Boston City Council after World War II, the South End had shifted too wards 4, 8 and 9, while Chinatown and the Financial District, which had been part of the South End during the 1930s, together with the North End and West End, had established their own precincts in ward 3 (XX; City of Boston n.d.; Serino 1950).

The geographical redrawing of precincts and wards was usually done to manipulate election results and is called gerrymandering (The American Heritage Dictionary of the English Language 2022).

Boelen listened to the voices of the Italian North Enders

Boelen's statement that she listened to the voices of Italian North Enders lacks empirical support in the article, which is a validity problem. This is especially important because she criticizes Whyte for not listening to the voices of the Italian population, despite his concrete and elaborate account in *SCS* of which groups and individuals he observed. Had Boelen truly intended to give Italians in the North End their own voices, she should have learned from Clifford R. Shaw's life history of Stanley in *The Jack-Roller* (1966 [1930]).

Characters in *Street Corner Society* answered the same questions in the same way

Boelen contends that she interviewed most of the characters in *SCS*. She interviewed one character at a time and asked each person the same questions in order to compare the answers with one another and with what is written in *SCS*. Presumably, this was meant to convince the reader that her study was valid.

It seems improbable that people connected to *SCS*, interviewed 30 or 50 years later, would respond to Boelen's questions about *SCS* and Whyte in exactly the same way. As a reader, you do not learn which characters were interviewed, what questions were asked, what the respondents answered, where the interviews took place, or how long they lasted. You do not find out who said what. The quotes are detached, out of context, and lack respondent sources. Her study consistently lacks sources and context.

The North End was both a *Gemeinschaft* and a transient hotel

Boelen writes on pages 39, 41, and 45 that, for Italian immigrants, the North End was both a *Gemeinschaft* and a transient hotel: a *Gemeinschaft* because everyone knew each other, and a transient hotel because many Italians moved to other neighborhoods after a couple of years.

According to Boelen, Whyte did not understand that residents of the North End in 1937–1940—despite the fact that immigration from Italy had essentially ceased more than ten years earlier—continued to practice the same customs as villagers and peasants in the Italian Abruzzi (or Abruzzo) region.

A neighborhood cannot, as Boelen claims, be both an unchanged transplanted Italian village and, for immigrants, a transient hotel on the path to becoming American. This argument does not hold.

Boelen points to a much-debated paradox in ethnicity and immigration studies, dating back at least to the golden age of the Chicago School of Sociology in the 1920s. Studies of ethnic communities present conflicting narratives and demographic findings: on the one hand, residents are described as being tightly embedded in close-knit, inextricable networks, in which everyone is related to or friends with one another; on the other hand, many move away from the district within a couple of years (XX; Ianni 1972; Thernstrom 1973).

The problem is that Boelen appears to lack the analytical capacity to reconcile these two contradictory portrayals of the North End as both a *Gemeinschaft* and a transient hotel. As a result, the text, like in several other places, becomes confusing, contradictory, and incoherent.

If Boelen had at least read William M. DeMarco's (1980) historical dissertation on North End enclaves from 1899 to 1929, or examined the migration process and cultural adjustment from peasant life in Italy to urban working-class life in the United States (Nelli 1967), her argument would still not have been entirely convincing, but it would have been stronger than the awkward comparisons she makes.

The Nortons gathered in Frank's kitchen

Boelen writes that Nutsy, rather than Doc as Whyte states in *SCS*, was the leader of the Nortons. She does not explain who Frank was in *SCS*, but presumably she means Frank Bonelli.

If Boelen met Frank in 1988, he would have been approximately 73 years old. Nutsy would have been about 80. Boelen claims that everyone from the Nortons except Doc, who died in January 1967 (The

Boston Globe, January 31, 1967), gathered in Frank's kitchen in 1988. Yet she only mentions Frank and Nutsy. This sounds like a tall story.

Frank's wife, according to Boelen, offered coffee and doughnuts. Boelen suggests that Italian customs prevented her, as a woman, from participating when the Nortons gathered on the same street corner in the North End in 1988 as they had in the 1930s.

Boelen asserts that in Frank's kitchen she could observe the same group patterns of interaction that Whyte had observed in the North End during the 1930s: "I could observe the small group dynamics so well described in *SCS*" (Boelen 1992: 28). This is a ridiculous statement.

We do not find out what questions Boelen asked the Nortons or what Nutsy or Frank answered. She writes nothing about Danny or Mike, who according to Whyte were deputy leaders of the Nortons and Doc's closest friends. If the Nortons, except for Doc, really did gather in Frank's kitchen, it would have been particularly valuable to know what Danny and Mike had to say in comparison with Whyte's account in *SCS*.

If Boelen had included the corner boys' responses, the reader would have gained a more reliable account of their views on Whyte and *SCS*. Direct questions are not as useful as observations, but Boelen presents no empirical evidence at all, not even from Frank or Nutsy. Neither is Frank's wife given a chance to speak.

Whyte was wrong about how the Nortons chose bowling teams

The bowling competition is a key event in *SCS*. Boelen is right about the connection between bowling results and group ranking, but she is wrong about how the Nortons chose the order of play in the competition.

In the famous bowling competition in April 1938, the Nortons competed individually, not in teams. It is true that the bowling competition probably was arranged by Whyte in order to test whether his hypothesis about bowling and group ranking was correct.

Bowling was a real social event involving friendship, group ranking, leadership, and social status. One could say that the informal social dynamics of the bowling competition are a microscopic social situation that can be extrapolated to the broader Italian community's complex and sometimes contradictory social organization.

In theory, the activity could have been something other than bowling, but bowling was the most prestigious social activity for corner boys when Whyte conducted field research in the North End. The connection between bowling results and group ranking was, with a few exceptions, correct. Some of the boys played baseball, though not as often or as regularly with the group. Instead, they played for clubs both within and outside the North End.

What Boelen does not mention, or perhaps did not know or understand, is that the relationship between bowling results and group ranking was, according to Whyte's hypothesis, only valid if most of the gang or peer group participated. That is why Whyte, with the presumably support of the leaders—Doc, Danny, and Mike—gathered as many Nortons as possible for the bowling competition.

If only two corner boys bowled against each other, it did not have the same social prestige. In that case, a lower-ranked member could defeat a higher-ranked one without the group's hierarchy being questioned. The stakes were much higher when most of the gang participated. If the followers defeated the leaders, the ranking order in the gang could potentially be overturned.

Boelen contends that she interviewed all members, except Doc, of the Nortons separately in 1970, after interviewing Doc's sons, and that they unanimously explained how bowling teams were chosen. Yet we do not learn which Nortons she interviewed, what she asked them, or how they responded. Boelen aimed to rebut *SCS*, but distorting and omitting many facts in support of what I believe to be a contrived thesis is dishonest.

Gangsters did not offer Cornerville men beer

One may get the impression that by "Cornerville men" Boelen mean the Nortons. But this allusion probably refers to the gangster Tony Cataldo and the "Cornerville S. and A. Club" discussed in Chapter 5 of *SCS*.

Cataldo paid the rent for the men's club and exercised partial control through his henchmen. The club consisted mostly of mature family men who met regularly to play games, hold meetings, fraternize, and talk about life. Several of the men worked as lottery agents for Cataldo.

The club organized bingo nights, beer and liquor sales, and election rallies. Cataldo shared the profits from lottery sales with the neighborhood Mafia boss. His main purpose in sponsoring the club was to invite local politicians to election rallies in order to demonstrate that he could deliver votes.

Voters functioned as a kind of social capital during election periods: the gangster could supply them, and the politician needed them. The gangster and the politician thus formed a fragile, temporary pact.

Boelen's assertion that alcohol was not served in clubs probably came from Chick Morelli, who in *SCS* is chairman of the "Italian Community Club." Chapter 2 of *SCS* is about Chick Morelli's club. My conclusion is that Boelen confuses the "Cornerville S. and A. Club" with the "Italian Community Club."

Academics of Italian heritage kept quiet

Boelen contends that academics of Italian heritage whom she interviewed did not publicly question *SCS* because no one would have listened to them. It is not clear who these academics were, which universities they worked at, what questions Boelen asked them, or what they answered.

According to Boelen, the academics unanimously claimed that they did not track down people from *SCS* or write a book defending the North End because an American audience would expect such a book to be about the Mafia and corrupt politicians rather than about a harmonious, close-knit urban village where everyone knew each other.

Jane Jacobs in *The Death and Life of Great American Cities*, published in 1961, romantically celebrates Boston's North End as a utopian urban village with lively street life and a close-knit community. This contradicts the argument that books describing the North End without a focus on the Mafia and corrupt politicians would lack an audience in the United States.

Boelen's refutation of *SCS* ultimately lacks empirical support and, in my opinion, reflects more of her own prejudices and preconceptions than the results of a valid empirical restudy conducted 30 or 50 years after *SCS* was published.

Conclusions

When I began to critically dismantle Boelen's rebuttal of Whyte's *SCS*, my idea was to distinguish between understanding/situational analysis (Gluckman 1958) and pre-understanding/hermeneutic interpretation (Betti 2021 [1962]). Situational analysis highlights the value of grounding analysis on fieldnotes and ethnographic texts, instead of relying too much on predetermined theories (Emerson 1983a&b) or ideological doctrines (Hobsbawm 1987: 266–277).

Gradually, however, I came to realize that a philosophy-of-science discussion about understanding and pre-understanding would not work because the substance of Boelen's article is largely built on fabrications and tall stories. It is one thing if researchers debate and disagree about whether the focus should be on situational analysis or hermeneutic interpretation; it is another, I argue, if the article is based on mostly dishonesty and misrecognition. Therefore, my analysis of Boelen's refutation has required a source-critical deconstruction based on Whyte's fieldnotes and supporting research.

Two concrete examples of how the fieldnotes have been indispensable in debunking Boelen's fabrications are Whyte's manuscript discussions with his informant, Doc, and the fact that he was primarily influenced by A. R. Radcliffe-Brown's structural functionalism rather than by the sociology of the Chicago School. The fieldnotes have also made my knowledge, both articulate and inarticulate, of how *SCS* came into being considerably more historically grounded.

In order to create a new structure for Boelen's article, I have, as previously mentioned, made a rough distinction between *disciplinary themes* and *eccentric peculiarities*. What distinguishes the disciplinary themes in Boelen's article is that they can be related to recurring discussions in anthropology and sociology about fieldwork och *SCS*. For instance, whether the researcher disclosed his real intentions to his informants, whether the study was properly delimited, whether the group received an undeservedly bad reputation after the study was published, what theoretical framework was used, how the fieldnotes

and interview material were collected and analyzed, and whether concepts should have been defined more rigorously.

What characterizes the eccentric peculiarities in Boelen's article has been more difficult to pinpoint, but they cannot be omitted because they occupy so much space. Examples include Boelen's arbitrary claim that she conducted interviews with corner boys whom Whyte had observed, that Whyte was wrong about how the Nortons chose bowling teams, that the Nortons gathered in one of the corner boys' kitchens to drink coffee and eat doughnuts, and that she there could observe the same interaction patterns that Whyte had observed on street corners in the North End. She also asserts that beer was not served in Italian men's clubs.

Another thing that strikes me is that Boelen seems to think that Whyte derived most of his ideas and conclusions from the Chicago School of Sociology, when in reality he worked within the tradition of Radcliffe-Brown's structural functionalism.

In summary, when I deconstruct all 20 questions and assumptions or statements, both disciplinary themes and eccentric peculiarities, presented in Boelen's article, Whyte's fieldnotes and supporting research show that none of Boelen's claims can be validated.

Declarations

Author Contribution

O.A. wrote the article.

Acknowledgement

Martin K. Whyte

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Footnotes

1. With the introduction of digital tools such as laptops, tablets, and smartphones, where fieldnotes can be continuously revised, fieldnotes no longer possess the same historical value as relics or remains as they did when they were written by hand or on a typewriter.