Abstract: The first edition of Howard Becker’s influential volume, Outsiders, was printed 50 years ago. Arguably its most important chapter, “Becoming a Marihuana User” (BMU), was originally published as an article a decade earlier in the American Journal of Sociology. Based on an interview conducted with Becker in 2002, strategic comparisons with contemporary publications, and a review of institutional evidence, this article documents the variety of academic meanings that BMU took on during its first half century. As other research would suggest, the widespread adoption of the work depended on readers treating it as theoretically and methodologically robust. Several historical and institutional contingencies also facilitated BMU’s success, including the publication of Outsiders, the changing organization of American higher education, and Becker’s ties to important sociology departments at the Universities of Chicago and California. Additionally, much of its success can be attributed to its multifaceted aesthetic appeals to readers. BMU was taken as symbolic of 1960s counter-culture; presented Becker as an author who participated, “up close,” in a marginal culture; and evoked imagery of physical and social interaction in research that appealed deeply to student sensibilities and American fieldworkers’ institutionalized tastes. The reception and changing meanings of BMU, Outsiders, and Becker’s other, closely related work, highlight some of the most important factors in how American sociology reached its current shape.

1 Thanks to the following for comments on versions of this article and/or the interview: Jack Katz, Robert Emerson, Melvin Pollner, Jonathan Turner, Scott Jacobs, Sally Jackson, Robert Garot, and Simon Gottschalk; and, of course, to Diane Hagaman and to Howie for graciously opening their home and sharing the story.
Fifty years ago, the first edition of Howard Becker’s (1963) volume *Outsiders: Studies in the Sociology of Deviance* was published. Its flagship Chapter 3, “Becoming a Marihuana User” (1953a [henceforth BMU]), was reprinted from his *American Journal of Sociology* article of the same name, published a decade earlier. For the last half century academics have considered BMU, in particular, to be a classic case study and one of the most influential works on the sociology of deviance and drug use ever written.¹

Becker’s article rose to prominence in part because it appealed to a loosely structured but powerful set of aesthetic and cultural values that emerged in late 20th century American academic culture. BMU was successful largely because audiences interpreted it as symbolic of a widely embraced, generalized social rebellion and counter-culture in the 1960s; interpreted Becker as a researcher willing to put himself in a marginal social world; and understood BMU as an evocative story about interacting with members of a deviant culture.

BMU was a fieldwork study based on interviews with fifty marijuana users. In the 1950s, what publications existed denounced marijuana as addictive and highly dangerous. Instead of focusing on personality traits that might somehow cause marijuana use—much less demonizing the users—Becker explained how people came to “use marihuana for pleasure” in “noncompulsive and casual” ways. The title word “becoming” indicated a subtle but radical departure from how most people thought about marijuana use at the time. The implied shift in perspective derived from an empirical finding that was highly unusual for the time: In his interviews Becker found that users do not automatically get “high” upon smoking marijuana; some felt no effects at all during their initial attempts (a
durable finding, informally but frequently replicated in undergraduate lectures, any time one teaches BMU and asks students about “their friends’” experiences).

The drug alone could not explain how the user experienced marijuana. The explanatory problem for Becker was to say how it is that people who initially felt nothing came to experience pleasure later. Becker explained that users had to learn to (1) “smoke it in a way that will produce real effects,” (2) “recognize the effects and connect them with drug use,” and (3) “enjoy the sensations” (Becker 1953a: 235). Social scientists in a variety of fields eventually treated that explanation as a landmark finding—but they did not do so immediately or universally. The reception of BMU and Becker’s other work reveals some of the most important currents that shaped American sociology in the second half of the 20th century (see also Sica and S. Turner [eds.] 2005).

Previous sociological case studies posit that two conditions must be met before any published research rises to prominence in a field: (1) readers must treat the epistemological content of the research, including its substantive relevance and theoretical robustness, as important; and (2) the author must successfully mobilize socionstituional resources like academic appointments, professional networks, and the publication process (e.g., Camic 1992; Lamont 1987; Latour1987).

Beyond these two, I suggest a third, unappreciated condition: BMU’s success was also critically shaped by its broad appeals to different university audiences’ tastes, and it only rose to prominence when it was recognized as symbolic of those aesthetic dimensions (see also Becker 1982; Curtin 1982; Griswold 1993; Marcuse 1978).² The history of BMU’s rise to prominence suggests that the aesthetic appreciation of written scholarship should also receive considerable attention in the sociology of ideas.
The content- and institutional dimensions were evidently crucial to BMU’s success: Sociologists defined it as having met standards of empirical adequacy and theoretical importance, and they discovered it at a time when Becker had achieved institutional visibility. The process through which BMU became widely known, however, indicates that readers were drawn to it largely because they felt strongly attracted to the aesthetic dimension embodied in the text. I do not mean only that readers felt it met disciplinary criteria of suitable format and rhetoric or that readers judged the writing style to be “good” (Gusfield 1976; Brown 1976, 1990). I mean also to bring attention to the processes by which readers made pre-reflective, aesthetic judgments about the attitude it represented; about the author’s presented self, temperament, and cultural disposition; and about the narrative experience of doing the research implied by the written document.  

BMU is particularly amenable to a study in the history of sociological ideas. It was first published sixty years ago and then, again, 10 years later in Outsiders, early in Becker’s unusually long academic career. Thus, Becker has since produced a long series of articles documenting his own understandings of how others have understood his work, including published interviews (Ben-Yehuda, Brymer, Dubin, Harper, Hertz, and Shaffir 1989; Debro 1970). Further, he was willing to collaborate in developing new data: I traveled to San Francisco to interview him at his residence in the summer of 2002 and corresponded with him several times by email.

THE CHANGING MEANING OF MARIJUANA

In 1951 Becker received his doctorate from the University of Chicago. He did not immediately find academic work, but instead made his living playing piano for a time. He
went to the American Sociological Association convention in Chicago that year, where
his teacher Alfred Lindesmith introduced him to Clifford Shaw and Henry McKay.
Heroin was a major national scare at the time and Shaw and McKay had a large grant to
study drug use.

Becker approached them with an idea. “Listen,” he recounted proposing, “I want
to do this study of marijuana use. Why don’t you guys hire me?” They agreed, and did so
with funding from the Institute for Juvenile Research in Chicago. Since he worked only
half time, he explained, “It didn’t cost much for them. . . . They never thought it was
important. In fact, nobody thought it was important. Because marijuana was trivial. It
wasn’t a big problem.” Fifty years later he recalls that the article was initially “treated as
a curiosity, nothing more. . . . They just thought it was weird. Why would you study
that?” When he said that, in 2002, Becker was not just being modest. A search of
contemporary journals revealed virtually no recognition of BMU.

In the early 1950s marijuana was still largely unknown to middle-class white
populations, including college students and professors, except, rarely, through the media.
Instead the drug was confined to particular Southwestern Mexican and black cultures,
including those of the Chicago dance musicians Becker wrote about (1951, 1953b).

He had self-consciously set out do a piece of oppositional research that refused to
accept prevailing psychological claims, he recalled, “to prove that it wasn’t really the
result of personality problems.” Yet academics in the 1960s and 1970s would interpret
that paper to symbolize an oppositional and rebellious ethic of a different kind, as a
political rather than social scientific form of mischief. Over time Becker himself
reinterpreted the importance of the study, “that what I was really studying was the
interpretation of bodily experiences. I didn’t know that for about fifteen, twenty years, ‘til I wrote those other papers [i.e., Becker 1967a, 1973, 1974]…. Really, I didn’t have any ideas about marijuana liberation, or anything.”

In the years just before Becker’s article gained popularity, the civil rights and other social movements spread rapidly, especially on college campuses. Movements to claim legitimacy and redefine “deviant” statuses—most notably for ethnic and racial minorities, homosexuals, women, and people with various disabilities and stigma—were becoming highly politicized, often on campuses where Becker’s work received most of its exposure (Kitsuse 1980; Pfohl 1994; Sica 2005). Marijuana became a cause of its own, but more importantly a vivid symbol of a loosely-connected but broad counterculture.

Marijuana had not been personally relevant or particularly compelling to academic readers in the 1950s. During the 1960s, however, mainstream youth cultures, including white college student culture, began to embrace it. The new meaning of marijuana on campus—that it was “social,” leisurely, and mischievously unconventional—prepared the student audience to welcome BMU’s and Outsiders’ “normalizing” images, describing familiar interactions and scenes in detail (Katz 1997). Becker’s work on deviance throughout the 1950s and 1960s continued to take that same normalizing perspective, appealing to both students and faculty.

One of the most fateful qualities of Becker’s research was that it was about marijuana. The meaning of marijuana in the United States began to change dramatically about a decade after the 1953 article was published. Marijuana was ignited as a powerful symbol, embodying not only explicit political ideals, but also an oppositional ethic and skeptical posture toward what was seen as legal repression. Enough members of
American universities developed an appreciation of the subversive that they were willing to accept Becker’s study as an aesthetically powerful symbol.

It is worth noting here the comparison with Lindesmith’s book *Opiate Addiction* (1947). Many readers, including Becker, have interpreted BMU and *Opiate Addiction* to contain the same theoretical and political “content-fit” (Camic 1992). Indeed, academics drew many of the same inferences from both texts: They indicated that drug use had to be learned and that the interpretation of bodily and “inward” states was socially organized. It is arguable that, like Becker’s article, Lindesmith’s book was reasonably well written. Reviewers in the 1950s, at least, praised its writing (e.g., Lemert 1948). (In contrast, reviewers praised *Social Pathology* [Lemert 1951]—which also presented ideas similar to Becker’s—on grounds wholly other than style.) But Lindesmith was writing about heroin, a drug that still meant “addiction,” “darkness,” and “the streets” to people on campus, as opposed to “socializing,” “enlightened politics,” and “counter-culture.” The tastes of academic readers were not oriented to heroin, either as a drug, sociological topic, or disciplinary symbol.

The story of how BMU was singled out as a classic text in the 1960s provides an instructive illustration of how a broad intellectual habitus and related tastes are both expressed within academic institutions, and become institutionalized within them (Bourdieu 1984; Kurzman and Owen 2002). The cultural appreciation of marijuana is an initial element to the story. A second element in BMU’s rise to prominence was the massive transformation of the organization of the American university system during and after the 1950s.
THE INSTITUTION OF DISCIPLINARY TASTE

Fundamental organizational changes in American universities, especially in the field of sociology, were underway at the same time as the cultural changes. Enrollments rose exponentially. Sociology expanded faster than any other social science discipline and became a leader in degrees-granted. The number of Bachelor’s degrees granted in sociology rose from about 5,700 in 1954 to nearly 11,000 in 1964, finally peaking at nearly 36,000 in 1974 (Cappell 1974). Sociology in particular disproportionately attracting the kinds of politically progressive students for whom Becker’s work held potential appeal (Cappell 1974; see also Burton 1961; Sica and S. Turner 2005: xi; Turner and Turner 1990).

During a period of two decades, sociology had captivated a plentiful and progressive audience. However, well into the course of these changes—as late as 1964—BMU was still being ignored in academia. Much of its recognition came with a restructuring of university teaching.

The making of a classroom classic. BMU received very little attention during the first ten years following its publication. Only in 1963—upon the republication of BMU as Chapter 3 in Outsiders—did sociologists begin to herald Becker’s work as a model for research and theory. Notably, Outsiders’ acclaim was based in large part on the so-far ignored content of Chapter 3. Shortly after, the original article enjoyed a newfound readership and was reinterpreted to have been a work of tremendous significance all along.
The book format gave it publicity, but that is not the whole story. It was published on a popular press (The Free Press) and books generally receive more notice than articles. Thus, the publication of *Outsiders* is best understood as a “tipping point” in the longer process leading up to BMU’s popularization. In addition to the quantitative changes in university teaching there was a crucial qualitative one: the introduction of edited collections of readings in the classroom.

Increased enrollments demanded the production of new collections of readings. The enormous increases in funding for sociology between the 1950s and 1970s supplied the empirical studies needed to fill the pages (Turner and Turner 1990). Sociologists who reviewed such edited collections at the time suggested that undergraduate courses in sociology—especially on “social problems”—were growing in demand and that teachers were increasingly replacing textbooks with compilations of readings (e.g., Davis 1968; O’Donnell 1972; Quicker 1972). Becker’s piece was ideally suited for the new organization of college teaching. Its style was accessible to non-specialists, it described a topic likely to interest students, and it provided a distinctly sociological theory in only eight journal pages. BMU was widely reprinted in edited collections, including several considered notable enough to be reviewed in major sociological journals (Manis and Meltzer 1967; Rubington and Weinberg 1968; Grupp 1971). Further, BMU’s new hosts spanned the diverse fields of social psychology, deviance, and marijuana research, to name a few.

**DISCIPLINARY AESTHETICS**
Myths, Institutions, and Fieldwork Appreciation. Readers of any text construct not only a substantive sense of the research, but also a narrative sense of the author’s self and personal experiences in producing it. Ethnographic, symbolic interactionist, and similar audiences have never really thought of Becker as exclusively, or even mainly, a drugs, deviance, and “labeling theory” scholar. His dozens of varied writings in and on participant observation are used as models for instruction. His research on marijuana, though, has been treated as the prime example of a research style with distinctive appeals. BMU and Becker’s (1955) article, “Marihuana Use and Social Control” (later, Chapter 4 in Outsiders), were read partly as stories about a researcher viscerally validating the humanity of a contested culture, by getting “up close” and verbally and physically interacting with its members.

The most enduring disciplinary legend about sociological fieldwork is that it grew from generations of innovative scholars at the University of Chicago shortly after the First World War. Becker was a student in what some call the “Second Chicago School” of ethnography in the years following WWII (Fine 1995). His most influential models were the classic authors and teachers Everett Hughes, Herbert Blumer, and Alfred Lindesmith. The research strategy most closely associated with the Chicago tradition has been participant observation (see, e.g., Adler and Adler 1987: 8-19). Early calls for this style did not simply expound the methodological virtues of fieldwork but also evoked narratives of bodily commitment to research. Thus sociologists, especially ethnographers, continue to hear echoes of Robert Park’s legendary instruction to “get the seat of your pants dirty” (McKinney 1966: 71; cited in Emerson 1983). Empirical and theoretical criteria were always elements of how readers judged fieldwork (Camic 1992). But so
were the viscerally appealing implications of carrying out such work. These included the rights to present a hero-scholar identity and to invoke in the reader’s imagination stories about being part of unfamiliar and possibly dirty, dangerous, or adventurous social worlds.⁵

Until the late 1950s and early 1960s the University of Chicago remained at the center of academic sociology and graduate training. At that time, though, the discipline and the American university system began expanding, members of the Chicago department moved around the country. Goffman and Blumer set up shop at the University of California – Berkeley in the late 1950s and others of their colleagues helped to establish prestigious sociology departments around the country—for instance, Hughes at Brandeis, where the Chicago tradition was propagated. These people either had a hand in Becker’s scholarship or personally knew him from Chicago, and placed his work alongside other ethnographic and symbolic interactionist works in the core curriculum of graduate fieldwork training. The legend of the Chicago ethnographers spread and became institutionalized in graduate training across America. BMU, among Becker’s other works, has been required reading, served as a model, and been widely discussed in the education of practically every American sociological ethnographer since. BMU has been noted for presenting a causal analysis based on in-depth interviewing (Lofland 1971), providing a clear example of how symbolic interactionism combines sociological and actors’ points of view (Denzin 1978), and depicting ethnographic imagery that politicized the status of its subjects (Katz 1997). More recent ethnographic studies continue to cite it as an example of how to study drug worlds up close (e.g., Bourgois, Lettiere, and Quesada 1997; Jackson-Jacobs 2001, 2004; Lalander 2003).
Evaluations of Becker’s work appeal to conventional methodological arguments, but also conjure up a deep disciplinary narrative. Unlike researchers who surveyed or experimented on marijuana users, Becker invited readers to imagine him physically present in their world, “up close.” He presented himself both as a sociologist and also the kind of person who would, and could, “hang out” with marijuana smokers. Though he did not write much about how he got in and out of the field or what he did there, his texts beg the reader to question, “What was it like?” and “What did he do there?”

Readers who knew Becker drew on their knowledge of his biography. He was a young, hip, piano-playing, boyish-looking white kid in his early twenties, an enticingly out-of-place character in the world of older, slightly disreputable, black jazz musicians. The mystique and allure must have been even greater for those who did not know him. Becker filled in few details on his fieldwork, leaving readers to imagine his extracurricular life, smoking dope and playing piano in Chicago clubs. Further, he normalized and honored the culture described by being there and sharing social and bodily experiences with its members, at the same time conjuring imagined scenes of thrill and excitement. Though rarely discussed explicitly, this aesthetic of visceral affiliation with research subjects is deeply appreciated in the culture of American fieldworkers.

Drug Research. As Becker’s work became known on campuses, he soon found himself being recruited to work as a “marijuana expert.” His first faculty job came in 1965 at Northwestern University at the time when considerable numbers of middle-class kids and college students in the Midwest had begun using marijuana. Marijuana was being transformed from something “trivial” to a public concern. Growing panic stimulated the
institutional, emotional, and economic bases for what would become the current interdisciplinary field of drug research. Though Becker would step away from the subfield shortly, he played a pivotal role in its cultural and intellectual formation, leaving an impression grossly disproportionate to the modest section of his vita relating to drugs.

Soon after he joined the faculty at Northwestern there was a publicized marijuana arrest of some students. The university hurriedly organized a panel to discuss the “marijuana problem,” though in a necessarily ad hoc way. A panel organizer had asked the head of the sociology department if anyone might know something about marijuana. He suggested Becker. Becker promptly gathered with therapists, psychologists, and medical doctors who equally rapidly concluded that, in fact, marijuana itself was not a problem. The panels kept springing up, though, and Becker and his fellow drug experts began to recognize each other, develop friendships, and work out the culture from which the academic drug research community would develop. For the academic world it was a rare kind of clique, since they were organized neither by departmental arrangement nor by a traditional substantive concern. Instead they congregated at panels and criminal trials where they brought their disciplinary sensibilities to bear on policy toward “the marijuana problem.”

“Well,” Becker mused, “the government paid for us to get to know each other.” The official crackdown, it turned out, created and formalized its own opposition.

Many of these people left the academic world, a number of them exploiting the government’s investment in their social capital to develop counter-cultural networks. They included David E. Smith, who organized the Haight-Ashbury Free Clinic, the well-known medical doctor Andrew Weil, and the influential psychiatrist Norman Zinberg.
Members of the organization branched out to make new connections in increasingly distant fields, though all shared a common sense of irreverent yet socially progressive creativity.

Those pursuing both scientific and artistic work shared a similar cultural orientation: Becker noted that “one of the people who helped us get together was Allen Ginsberg.” Through this network, currents in thought about marijuana, ranging from the pharmacological to the literary, were circulated. The National Organization for the Reform of Marijuana Laws (NORML) was founded in 1970, drawing much of its legitimacy from members of that community. (Becker sat on the board for five years.)

Throughout the 1970s, when Becker himself had moved on to other topics, drug research became a more developed substantive discipline and policy focus, influenced by members of his old community of marijuana experts. A generation of respected researchers arose directly from those meetings in the 1960s, including the original community members, their students, and colleagues who worked in university departments, philanthropic centers, and policy research organizations. Although the field became increasingly disciplined by scientific convention, the influence of their culture persists within the sphere of American drug experts, evidenced by continuities in the ideas they developed.

The most commonly cited of BMU’s ideas are that inward states (e.g., being “high”) are interpreted socially and that techniques of using must be learned through interaction. Researchers from medicine, pharmacology, psychology, psychiatry, and sociology have used diverse methods to generalize these ideas, including both laboratory experiments (Sicé, Levine, Levin 1975) and survey methods (Orcutt and Briggs 1975;
Orcutt 1978). Other fieldworkers, though, have constructed the strongest link between BMU and current drug research.

Crucial to this link was the work of Becker’s old friend Norman Zinberg, the Harvard psychiatrist. He adopted Becker’s focus on social groups and their norms and processes of use. Zinberg (1984) used interviews with hundreds of marijuana, hallucinogen, and opiate users to develop a theory that continues to be widely influential, even in the most conventional circles of drug research (e.g., the National Institute on Drug Abuse). He claimed that three factors explain the experience of taking a drug: the drug itself, the psychological “set” of the user, and the social “setting” of use. Other drug researchers cited BMU and Zinberg’s work alongside one another, treating them in combination as providing an empirically testable theory of group practices, individual careers, and subjective experiences of drug use.

In one influential research partnership, Waldorf, Reinarman, and Murphy (1991) used interview methods to test whether BMU’s three necessary and sufficient conditions for becoming a regular marijuana user also applied to becoming a cocaine or crack user. Cocaine sniffers, they found, had to learn the physical techniques to use cocaine and also to recognize, interpret, and enjoy the high. Yet crack smokers, unlike marijuana or cocaine users, had only to learn the physical technique. Once the physical technique was mastered users invariably recognized a powerful high with no further instruction. Later fieldworkers have since applied BMU’s ideas to ingesting hallucinogens (Zinberg 1984), injecting heroin (Bourgois et al. 1997; Zinberg 1984; Zinberg, Harding, and Winkeller 1977), snorting powder cocaine (Waldorf, Reinarman, and Murphy 1991), and smoking

The drug research community has been most interested in Becker’s idea that the interpretation of psychological or bodily experience is social. How they imputed this idea to Becker is revealing. BMU only implicitly touched on the topic of the social bases of the experience of being high. Becker, as indicated earlier, only consciously considered the topic fifteen or twenty years later. He has written that he regretted not recognizing or developing it in the 1950s (1986), though his 1967a, 1973, and 1974 articles on hallucinogen use stated the idea more clearly. Nonetheless, drug researchers usually cite BMU as the idea’s exemplar—the substantive claims of the 1967a, 1973, and 1974 papers are retrospectively read into the 1953 one.

Contemporary writing on drugs cites BMU either as a historical document or theoretical guide, giving the impression that the empirical or theoretical content alone explain its continued relevance. Although academic judges usually use the term “robust” to describe theoretical strength, it might be used more accurately here to describe the range of appeals that later scholars would discover in BMU. The article robustly appealed to drug experts across historical periods both as a piece of disciplined, rigorous, empirical research, and also as symbolic of a skeptical perspective toward conventional wisdom about drugs. Although it first appealed in the context of a fledgling intellectual and cultural movement, a later generation of drug researchers would reincarnate it on new grounds, as a piece of their specialized substantive knowledge.
Criminology, Deviance, and “Social Problems.” Qualitative and drug-related researchers greeted Becker’s work with consistent admiration. Researchers in the more mainstream fields of crime and deviance, though, varied widely in how they evaluated and interpreted it. Various scholars for decades have treated Becker as a symbolic proponent of a number of perspectives on drugs and deviance, some of which he intended and others for which he claims no personal responsibility. The former included, most importantly, an anti-psychological approach to drug use, a “normalizing” perspective, and a focus on the social learning behind the experience of being “high.” The latter included, especially, his association with the various shades of controversial ideas often referred to as “labeling theory” and “the underdog perspective.” Published debates over those concepts in the late 1960s and early 1970s reveal a deep and previously unspoken discord over an emerging system of tacit aesthetics within crime and deviance research.

The most obvious early influence on Becker was Lindesmith. BMU is an unmistakable application of Lindesmith’s (1947) inductive method and understanding of the social bases of bodily experience, as Becker readily acknowledges. Like Lindesmith’s work BMU was a reaction against psychological, medical, and criminal justice-oriented interpretations of marijuana use.

However, the oppositional ethic of other authors’ works took different shapes. Lindesmith was quite willing to blur the boundaries between academic writing and policy critique, fully participating in a dramatic, decades-long, and widely publicized dispute with Harry Anslinger, the first director of the Federal Bureau of Narcotics. Lindesmith wrote articles explicitly critical of drug policy, most notably his (1940) “Dope Fiend Mythology” in The Journal of Criminal Law, Criminology and Police Science, and rallied
the American Bar Association and the American Medical Association (Joint Committee 1961) to his perspective (Reasons 1975). Anslinger publicly attacked Lindesmith, even going so far as to try to intimidate Lindesmith and his publishers (sometimes successfully [Galliher, Keys, and Elsner 1998]).

Becker, in contrast, stayed out of political debate, though he did testify for the defense in a number of criminal trials and affiliate himself with NORML. Sociologists and criminologists bickered over the imagery he suggested, the perspective he implied, and the oppositional ethic his research embodied, though on more “academic” grounds and less explicitly than in Lindesmith’s case. Toward the end of the 1960s, sociologists indicated in published debates that they were increasingly contesting those features of BMU, though they often continued to couch their stances in terms of “scientific” issues.

When Outiders was published the most influential criminologists of the early 1960s, also with ties to the department at Chicago, immediately recognized the imagery and novel perspective as important (Erikson 1964; Sykes 1964). Drawing heavily on BMU, they noted particularly that Becker’s book presented a “sequential” alternative to psychology’s “simultaneous” causal explanations, stressed the role of interaction with enforcers in constructing deviance, and demonstrated the need for direct observation of deviants’, enforcers’, and moral entrepreneurs’ practices.

Becker entered the field at a time when criminology was changing. Up until then the classics of the field included other interactionist, processual, and comparative ethnographies, such as Thrasher’s (1927) The Gang, Shaw’s (1930) The Jack-Roller, and Cressey’s (1932) The Taxi Dance Hall. Around the time of Becker’s early work, though, the field started to become divided. As noted above, sociologists were beginning to
attract larger sums of money from federal and other government agencies. Newer theories that were more easily tested quantitatively, including Cloward and Ohlin’s (1960) *Delinquency and Opportunity* and Hirschi’s (1969) *The Causes of Delinquency*, started to replace the older generation of classic texts.

In criminology quantitative studies focusing on pressing policy matters and theoretical programs in sociology were funded (e.g., Short and Strodtebeck 1965). Agencies also began to fund practitioners with academic appointments, especially in the growing area of gang research (Katz and Jackson-Jacobs 2004). Even when such researchers used field methods, they presented themselves as corrective agents—“gang program workers” and “social workers”—that is, outsiders gathering data to assist formal agencies (e.g., Klein 1971). By the end of the 1970s much of the interactionist and constructionist bent of mainstream criminology had largely disappeared.

“Social problems” and deviance perspectives splintered off from the discipline of criminology in the 1960s. Criminology was becoming institutionalized in many university departments while the deviance/social problems approach was becoming institutionalized in new sociological associations, journals, and courses. Whereas criminology took on criminal justice system-, control-oriented interpretations of the problems and substance of the discipline, interactionist interpretations of deviance and the construction of social problems stayed closer to central sociological issues of culture and subculture.

Becker was not a passive participant in this process. He was one of the first editors of the journal *Social Problems* (1961-1965) and presidents of the Society for the Study of Social Problems (SSSP; 1965-1966). While still early in its development, he
helped direct the future of the field at this historically critical period. He took major editorial control in shaping the direction and tenor of *Social Problems*, especially the chapters reprinted in his *The Other Side* (Becker 1964).

When he took over as editor his predecessor, Erwin Smigel, implored Becker to solicit articles. Taking that advice, Becker recalled that he “went and hustled up articles from all my friends.” It is important to recognize that, while “hustling up” these articles had the effect of promulgating a particular perspective, it was also a practical strategy for “getting the journal out. . . . It doesn’t matter which side of anything you’re on,” he pointed out. Publishing deadlines create demands of their own.

Becker strongly influenced the contemporary definition of a “social problem” by using that editorial process. He reported, “The rubric had been ‘social disorganization.’ And that meant things that everybody knew were social problems. Alcohol, crime, poverty. . . . In order to [get the journal out on time] I was hitting up all my buddies who were doing this kind of work.” These articles, like his own, deepened the perspective that individual-level definitions of deviant behavior were contingent, produced in concrete instances of social interaction, and that general cultural definitions of deviance were also socially constructed.

At the same time Becker’s contemporaries were taking similar stances toward deviance. Szasz (1961), Goffman (1963, 1971), and Scheff (1966, 1974) had initiated influential critiques of the definition and treatment of psychiatric patients and other stigmatized groups. There was a similar movement in literature, as writers like Ken Kesey (1962) and Tom Wolfe (1968; despite a conservative personal politics) took sympathetic or normalizing approaches to “deviants.”
Throughout the 1960s, scholars became increasingly heated in their debates about “conventional” and “unconventional” ways of thinking about the substance of criminology and deviance studies. Yet, except in rare and highly instructive cases, they continued to phrase their arguments as contests over empirical findings. Published statements by “social problems” and “societal reaction” authors almost never explicitly attacked “official” perspectives or ways of doing things, but instead wove the ethic into empirical claims and theoretical arguments. Kitsuse and Cicourel’s work on “official statistics,” for example, took the form of empirical claims about the practices of turning events into numbers representing criminal acts (Cicourel 1968; Kitsuse and Cicourel 1963). They argued that official agency statistics about crime, or any survey research for that matter, be understood as socially produced artifacts, textual objects for sociological study in their own right, rather than reflections of the objects they claim to measure.

Yet Becker—like many other readers of various methodological bents—understood those studies as, in his words:

a direct attack on ways of doing business that were very well entrenched. Like the whole attack on official statistics Kitsuse and Cicourel had, which I bought into one hundred percent. That was an attack on all the people who did research with official statistics, which was the way it was “supposed to” be done.

The sentiment Becker expressed—that a confrontation was brewing—was implicit in sociology throughout most of the 1960s. Deviance researchers across the divide had looked on their opponents with growing suspicion. Until then end of the
decade, though, they found ways of dismissing each other’s work by developing alternative substantive and causal research questions or simply by ignoring the other side.

Two debates broke out in the late 1960s, though, consuming the field of deviance and receiving widespread attention from the whole of sociology: the “underdog perspective” and “labeling” theory controversies. Many sociologists focused quite explicitly on the generally unconventional aesthetic dimension of what they perceived as the threatening, frustrating, and thoroughly distasteful trend in the new field of deviance/“social problems.”

BMU remained fundamentally inseparable from the tangle of interactionist perspectives at the center of controversy. Along with Outsiders, it had been the best-known piece of research from the so-called “labeling” or (as Becker preferred) “interactionist” camp, within which critics made few distinctions once the debate began to intensify. Trying to disentangle the specific role of BMU, or of Outsiders, might be impossible and is probably not desirable, since critics themselves felt cumulatively frustrated with the budding deviant intellectual habitus in the midst of their field and in academia more generally, rather than specifically upset by any single publication.

TWO SOCIOLOGICAL CONTROVERSIES

The Underdog Perspective. One exchange was transacted with a surprising contingent of sociologists. The criticism came not from “conservative” sociology, but from “radical” sociologists. They accused interactionists—Becker, most of all—of writing in ways that unwittingly supported the conservative status quo. Here the final straw was laid when Becker asked out loud, “Whose side are we on?” (Becker 1967b; his 1967 presidential
address to the SSSP). What readers made of that paper indicates that they had been reading empirical studies for their implied political postures toward the moral status of deviants and control agents all along.

Becker seems to have meant the presidential speech both as sociology of knowledge and as a methodological argument that taking some perspective is a necessary and justifiable approach to empirical research. He discussed the relationship between whose perspective a researcher takes along “hierarchies of credibility” and the political and value perspectives that will likely be attributed to the researcher depending on that choice. Becker affirmed that view of the address in our 2002 interview. But the paper has rarely been read that way. What became of it was most crucially shaped by Alvin Gouldner’s (1973 [1968]) widely publicized critique of “underdog sociology.”

Gouldner and most readers took Becker’s article and address as a call to do “underdog sociology,” meaning something like sociology in defense of the morally marginal. Having noted other authors’ attribution of that perspective to Becker, I had at first assumed that he did explicitly advocate “taking the side of the underdog.”

After describing his research to him in those terms by email, Becker wrote back expressing puzzlement that his work had been stuck with that tag so for so long. He noted in particular, “I don't think I ever used that expression.” Returning to the paper, somewhat embarrassed but still slightly skeptical that I had made such a misreading, I found that in fact he used the word “underdog” only once in that paper, and no instances of “underdog perspective” or any other such phrase. Furthermore, in that one instance, he used it in a way that suggested no real attachment to the word, using it in a chatty style to
characterize the political perspective of most sociologists: “We usually take the side of the underdog” (1967b: 244).

That readers characterized the work as an explication of the “underdog perspective” or “underdog sociology,” even though those phrases themselves do not appear in the text, and despite what seems to be a very causal use of the word “underdog” when it does appear, suggests that something else was at issue. A sizeable aggregate of readers seized on “underdog,” interpreting it as a growing perspective among a particular faction of sociologists. Gouldner (1973 [1968]) managed to both introduce the phrase “underdog perspective” and successfully attribute it to Becker. One of the more fascinating elements of how the “underdog perspective” developed was that later authors who played off it one way or another cited the phrase (or similar ones) to Becker’s (1967b) paper (e.g., Taylor and Walton 1970; Huber 1973; Gusfield 1976; Posner 1980). Readers interpreted Gouldner to have critiqued Becker’s explicit recommendation that sociologists take the side of the underdog, even though Becker never made that recommendation.6

“Labeling” theory’s opponents, as discussed below, found it distasteful that researchers defied convention and took the deviants’ perspectives too seriously. The critics of “underdog sociology,” however, attacked from the opposite side, accusing practitioners of being duped by convention and failing to take the “correct” oppressed groups seriously.

Gouldner wrote of a growing uneasiness that “came to a head” with Becker (1967b), and suggested Becker was
a leading spokesman of a viable coterie of sociologists specializing in the study of social deviance, whose members include such able men as Howard Brotz, Donald Cressey, John Kitsuse, Raymond Mack, David Matza, Sheldon Messinger, Ned Polsky and Albert J. Reiss; and this coterie in turn overlaps with a larger network that essentially comprises the ‘Chicago School’ of sociology. (1973 [1968]: 27-29).

That such a wide network was associated with the “underdog perspective,” and with such simmering vitriol, following a single, rather offhand use of the word, reveals the latent, unspoken demand for a suggestive label to categorize the approach taken by sociologists like Becker.

Gouldner explicitly critiqued Becker for failing to say what side he was on, for inconsistency between theory and method, and for working with a “collector’s aesthetic” that romanticized the exotic (1973 [1968]: 38). He suggested instead a “humane sociology” on the side of people who “suffer,” especially political underdogs and those suffering at the hands of the power elite. As indicated by Gouldner’s invocation of Becker’s colleagues, his focus on the phrase “underdog,” and his solution, the debate was about something other than the possibility of value-free sociology, or even what the values of sociology should be.

A volatile contest of institutionalized aesthetics and “sentimental disposition” (Gouldner 1973 [1968]: 32) was bubbling close to the surface. Their opponents did not appreciate Becker and company’s studies of deviance up close as narratively rich embodiments of a normalizing researcher-subject relationship. Instead the new deviance
researchers’ opponents saw their work as the “glib” expressions of trendy researchers who were “at home in the world of hip, drug addicts, jazz musicians, cab drivers, prostitutes, night people, drifters, grifters and skidders: the ‘cool world’” (Gouldner 1973 [1968]: 29). Gouldner interpreted that approach to be “smug,” and to essentially see deviants as the “overdogs” do. The alternative to the cool, hip, “collector’s aesthetic,” was a “critical,” “radical” approach, positively judging wise researchers who humanely discovered new forms of oppression and hidden suffering. The presented selves of the authors, narratives of their experiences in the field, and professional missions were at the heart of the conflict.

That Gouldner’s well-known position has been treated so uncritically can be explained, in part, by Becker’s own reluctance to respond. When I spoke to him he did not have much to say about it, except that he wished he “could get rid of this description of my stance.” Given Becker’s non-response, though, it was also critical that many sociologists already tacitly interpreted Becker’s style of research that way and that there was a demand for a useful phrase with which to categorize it.

The end result has been that the “underdog” label stuck. At the time, and still today, both supporters and critics of that position believe Becker himself to be its originator. Becker’s work, interpreted holistically by both supporters and detractors, came to symbolize a powerful clash of aesthetic dispositions going on both inside and outside the university during the 1960s and 1970s. Indeed, the clash only intensified in the following years, as a second controversy broke out.
Labeling Theory. Not long after *Outsiders* was published, sociologists of all stripes had begun to see Becker’s major career contribution to be “labeling” theory. Commentators routinely attributed to him the claim that people who are labeled deviant will become even more deviant as a result of labels, the phenomenon dubbed “secondary deviance” (e.g., Gove 1970, 1975). Becker has tried to resist that attribution ever since sociologists started making it, often pointing out that the term does not appear in his own writing and that there is only a slight hint that he ever gave much thought to any idea like it.

The term “secondary deviance” actually came from Edwin Lemert’s (1951) book *Social Pathology*—a book that Becker tells me he never saw until *Outsiders* was already at the printers. The point here, though, is not to elucidate the proper reading of Becker’s texts. Instead, it should be a sociological question in its own right how such an easily discreditable interpretation became the dominant view of Becker’s research. The point is that readers interpreted Becker’s version of labeling theory in ways that allowed them to handle it with the very same kinds of conventional data and methods that Becker had sought to avoid with his own theoretical formulation. Those ways of interpreting the labeling perspective were grounded in scientific justifications, yet they preserved the stance that criminology was about placing blame, though allowing for the blame to be shifted to control agencies, should ever there be a significant finding in that direction (there was not, to my knowledge). 7

Becker’s work on deviance suggested focusing on the interactional process by which people manage and construct deviant identities. However, the typical rendering of Becker’s theory was that he saw “deviant labels” as an independent variable and secondary deviance as a dependent variable. That interpretation made for easy tests of
experimental and survey-research data, suitable in format for publication in the most popular sociological research journals. Interpreted thusly, the labeling or “societal reaction” theory was often rejected on empirical and criticized on theoretical grounds (e.g., Gove 1970). Throughout the 1970s opponents most commonly attacked labeling theory by falsifying the secondary deviance hypothesis with statistical tests (see Gove 1975).

One way of considering the labeling controversy is to see it as a debate over the appropriate way of conducting scientific tests. But we can also observe a dispute over a system of tastes and aesthetics, residing below the surface of explicitly contested empirical and theoretical claims. The normalizing perspective and the culture of affiliating with morally marginal people clearly was not universal in the United States or its universities. It was strong enough in some fields for Becker’s work to become institutionalized, but not strong enough to remain uncontested. By the end of the 1960s the competition between what Matza (1969) called “appreciative” and “corrective” approaches was becoming fairly explicit.

When Matza described an appreciative stance he meant the kind of research done by people like Becker, Goffman, and Kitsuse. They had all developed strong epistemological justifications for “taking the perspective of the actor,” which often meant the perspective of the deviant. To Becker and company “taking the perspective of the deviant” meant describing the world as seen and understood by the deviants, a method of verstehen.

Many critics read that advice not as methodological but as moralistic. However, they remained unsure of the exact nature of this moral advice. Surely it was not really a
call for researchers to accept and condone deviant values? The “secondary deviation” interpretation made sense of what might otherwise have struck some readers as an implausibly offensive stance toward deviance. *Aha—these guys are just trying to blame crime on “society”!* It also made sense of a second widely confusing feature of labeling and societal reaction theory, that the proponents were trying to explain the causal processes through which deviant labels were applied and sanctions enforced. The processual style of causal thinking was familiar to phenomenologically and symbolic interactionally informed sociologists, but not to administratively oriented criminologists.

Thus, opponents of labeling theory, much more so than labeling theorists themselves, treated as a serious empirical question, “Who is causally responsible for criminality: control agents or criminals?” Becker (1963), Erikson (1962), and Kitsuse (1962) had written about topics that almost completely ignored such etiological hypotheses about deviance (much less rendering “cause” as “blame”), describing instead causal conditions for the successful application of the label “deviance.” A few others, especially Lemert and Scheff, did make some statements about the etiology or causes of deviant behavior (see also Schur 1969; Gibbs and Erickson 1975).

The diversity of approaches on that “side,” however, did not get in the way of defining a single school of thought. Becker and others’ ideas were implicated throughout the debate. Walter Gove, the leading critic of labeling theory in the 1970s, wrote an introductory chapter to a book summarizing a labeling perspective purportedly shared by Tannenbaum, Lemert, Garfinkel, Becker, Erikson, Goffman, Kitsuse, and Cicourel: Primary deviance “may cause someone to be labeled as a deviant, and secondary deviance, which is behavior produced by being placed in a deviant role” (1975b: 3-4).
There is no evidence Gove was at all concerned that none of those authors, except possibly Lemert, would have agreed with his assessment of their work. His version, though, made sense of the vague unease and confusion that critics felt, and it soon became popular.

Labeling theorists had created confusion by inverting traditional causal questions, looking for conditions for the application of the label “deviance,” not conditions for doing deviant behavior. Opponents resolved the confusion by modifying the causal issues into a third question, one that practically no one on either side seems to have actually taken seriously: Does social control cause people to do deviant acts they otherwise would not have done? Besides ethnomethodologists (Pollner 1978), none of the “labeling theorists’” critics seem to have grasped their major point, even those who made apparently good-faith attempts at sober critique (e.g., Tittle 1975). As long as the debate remained only mildly contentious, and nothing particularly provoked the critics, it continued to look like confusion over the theoretical questions really was the only serious disconnect. The most successful critics of labeling theory, including Gove, tended to write with a tone of bemused skepticism rather than outright moral indignation. But there were also periodic and sociologically fascinating episodes of academic panic.

Extreme opponents only rarely voiced their more heated suspicions, but it happened often enough to be a prominent feature of sociological journal publishing of the period. On occasion, opponents would wildly breach the usual standards of polite, academic argumentation, lashing out at labeling theorists. One event did not focus on Becker in particular, though the critics took enough jabs at BMU, Outsiders, and Becker’s other work to suggest they considered him a primary target. The explicit focus
was an interactionist interview study by Robert Lejeune (1977), called “The Management of Mugging,” published in Urban Life. He had conducted several dozen interviews to document the perspectives of young robbers in New York City, and had analyzed the act of mugging as a series of interactionally managed steps.

The journal ran a symposium on the article in 1980. Judith Posner’s response, “On Sociology Chic”—a title meant ironically—represents several common features present in a number of such minor battles. The labeling paradigm had been coming under fire often enough by that point that there was precedent for an additional, focused attack on this new and especially troubling piece of research. As Posner saw it, a trend had originated with Becker’s case study of marijuana users and, in recent years, reached such a state that she felt she must speak up: “But Lejeune’s study goes one step further in the relativist paradigm. Management of a Mugging focuses on the actual criminal act, one which everyone would agree is both illegal and repugnant” (1980: 105). To focus on the criminal act itself, she suggested, was to blur the line between appreciating it in the methodological sense and appreciating it in the moral sense.

For a number of academics and non-academics alike the “relativist paradigm” Posner referred to was one of the most disturbing aspects of how the university had transformed after the 1960s. For professors, the problem must have been inescapable and thus unbearably frustrating. As Becker and his collaborator-friend, Irving Louis Horowitz, commented in 1972, many faculty members found the “collective upheaval” within universities intolerable, especially as demonstrated by the conduct of radical students and disturbingly unconventional colleagues. Imagine the frustration “straight” sociology teachers must have felt at having no option but to assign collections of labeling
readings to their classes, both because that was what students wanted and simply because that was all that was being published then. Prominent criminologists sometimes professed such exasperation, befuddled by the throngs of undergraduate and graduate students who became enchanted by the irreverent perspective: As Travis Hirschi once complained, “Students are enthralled …” (Hirschi 1975: 181).

It is understandable, then, that many critics exposed their utter dissatisfaction when dealing with labeling theorists in print. Labeling theory, as they understood it, represented the culmination of a series of horrifying new turns in the field, especially the “decay” of sociology into an utterly “relativistic,” phenomenological, and symbolic interactionist paradigm (Posner 1980: 109). Critics like Posner often read examples of labeling scholarship as not just academically relativistic, but as potentially condoning the deviance being described. Posner made a series of accusations of types and intensity uncharacteristic of scholarly discourse. In her most intense criticisms she suggested that what was really at issue, what had really been at issue all along, had been that the labeling perspective flaunted a perverse emotional logic, moral offensiveness, and a totally distasteful hedonistic ethic.

Indeed, Posner based her most outraged responses on her own professed revulsion at the aesthetics of the field. Labeling theory in general and Lejeune’s article, in particular, were becoming something like “a kind of bizarre sociological voyeurism” (Posner 1980: 104). One criticism of Lejeune’s work that she leveled was that he seemed all too comfortable with such material. How could he not be morally and sensually repulsed? Why, she asked pointedly, would he want to study this kind of topic?
(His interest, he replied, was sparked upon being traumatized as the victim of a mugging [Lejeune 1980].)

Referring specifically to Becker, Goffman, and others, Posner indicted the new trend on almost exclusively moral and aesthetic grounds. Ethnographers of deviance, Posner suggested, were getting “a kick out of studying” outlandish and “chic” characters (1980: 112). The field was becoming “a form of entertainment,” she felt, “ludicrous at best—and sadistic at worst”—“grotesque,” “bizarre,” and generally “repugnant” (Posner 1980: 109-111).

Besieged from both left and right, and hardly attempting a defense, one might expect Becker and his colleagues to have been obliterated. Instead, BMU, Outsiders, and related work, was virtually canonized.

CONCLUSION

How could an unassuming eight-page paper written by a young research assistant in his early twenties rise to such prominence and stir such powerful debate? What might superficially seem to be just a case of “the right place at the right time”—a curious anecdote for a historian of science—in fact opens a complex and important sociological story. The accessibility of the ideas, methods, and claims in BMU made it easy for the earliest audiences to treat it as “trivial.” Yet those same qualities allowed the article to fly below the political radar, at least for a time.

From the standpoint of both qualitative sociologists and sociologists of ideas, BMU is a marvelous piece. Like other major scholarly successes, it became famous partly because its explicit content met scientific criteria and because its author secured
socioinstitutional power (Camic 1992). Unlike other prominent social scientific works that have been studied, though, BMU has gone largely uncontested on scientific grounds. Because of its success on those grounds, we can see more clearly the role that the aesthetic dimension played in its reception.

BMU will likely remain a paradigmatic exemplar in undergraduate and graduate education due to its tacit appeals. An ironic shift in meaning, though, may have already begun. New readers continue to appreciate the surface aesthetics of the text—the interesting topic, accessible diction, and fluid grammar, for example. However, the deeper aesthetic dimensions—the self presented and the narrative research experiences evoked in the pages—are historically contingent. The persona of the young pianist-ethnographer and the narrative of the young kid from the University of Chicago mixing in with the deviant fringes of hip society were vivid several decades ago, but may be lost on those not familiar with those times and scenes. And, finally, to the degree that the movement to transform marijuana into the realm of the “normal” and the conventional succeeds (of which BMU was certainly a part), new generations of readers will no longer recognize just how irreverent and mischievous that article was for the earlier generations.

NOTES

1 In a non-exhaustive search of the Social Science Citation Index I found 208 citations to BMU since 1974. As discussed below, BMU became most popular after re-publication as chapter 3 in Becker’s Outsiders: Studies in the Sociology of Deviance (first published 1963). Citations to Outsiders, excluding those that indicated reference to other than chapter 3, totaled more than 2000 citations for the same period. Perusing these references it is clear that a sizable minority meant one or more of the chapters on marijuana,
especially BMU. However, most citations leave unclear to what part of the book in particular, if any, the author is referring.

2 The metaphor of written science as multi-dimensional aesthetic text—extending off flat pages into the world of culture, projecting embodied selves of authors, and invoking narratives of human relationships—reflects a growing interest in the tacit dimensions of knowledge, interaction, and the diffusion of ideas (Bourdieu 1984; Garfinkel 1967; Katz 1999; Latour 1987; Sacks 1992; Polanyi 1958, 1966). The aesthetic dimension is important in the explanation of why some research is accepted and other work overlooked (cf. Camic 1992); how to understand the relationship between culture and scholarship (Kurzman and Owens 2002; Sica and S. Turner 2005); and how to characterize scientific writing (Gusfield 1976; Brown 1976; Brown 1990).

3 Nothing here attempts to discredit or to debunk science. Descriptively speaking, aesthetic judgement is unavoidable in scientific writing and reading, as in any other medium. In a variety of ways aesthetic conduct and interpretation in scholarship may well be a tacit response to more highly esteemed scientific aims. Consider Katz’s (2001, 2002) claim that ethnographic “terms of appreciation” indicate practical knowledge of causal logic, residing beneath reflective awareness.

4 Becker felt he was being somewhat “mischievous.” Similarly, Gusfield (1976: 31) reports enjoying a sense of self-as-“smartass” when he described “the literary rhetoric of science.”

5 For example the disciplinary virtue and charisma credited to the Erving Goffman-style “hero anthropologist” identity (see Collins 1985).

6 The regularity of the “underdog perspective” interpretation is particularly astonishing when readers have read both texts in succession. Consider an example from a graduate seminar at UCLA in 2000. Of twelve students who turned in one-page reaction papers after reading the Becker – Gouldner exchange, nine commented on “underdog” (one
other student mentioned Becker but not the phrase “underdog”). At least three of them explicitly suggested that Becker recommended doing underdog sociology. One of them attributed the phrase “sociology of the underdog” to Becker. Only one of the ten who mentioned Becker at all seemed to have understood his paper as sociology of knowledge/ideas. She was also the only one who “read Becker’s comments on this in a different way” from the rest of the class and Gouldner. None noted that the word “underdog” was totally superfluous to Becker’s arguments.

However, in psychology and health-related disciplines the well-known phenomenon, “iatrogenic labeling,” means something close to “secondary deviance,” and has a body of “positive” findings, both quantitative and qualitative (Sartorius 2002; Whitney 1981).

REFERENCES


