

that the Internet causes social isolation. This *t* test design would have had considerably more statistical power than the correlational design that was used, in addition to eliminating the question of causal direction. I hope that such an experiment will be conducted in the near future.

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## Internet Use and Ties That Bind

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Our research reported in the *American Psychologist* (Kraut, Patterson, et al., September 1998; see also Kraut, Scherlis, Patterson, Kiesler, & Mukhopadhyay, 1998) showed that as participants in a field trial used the Internet more, they became reliably less socially engaged and more lonely and depressed. These changes occurred even though the participants' dominant uses of the Internet were social—electronic communication with family, friends, acquaintances, and strangers (Kraut, Mukhopadhyay, Szczypula, Kiesler, & Scherlis, in press).

Silverman (1999, this issue) argues that the online community she belongs to has contributed to the psychological well-being of its members. Her online group began a few years ago in the real world, when a subset of mental health practitioners at a meeting decided to go online. Silverman quotes the members' strong sense of group identity and support in their online communications.

Although our research did not explicitly study online groups, it is most surely true that some people create meaningful and fulfilling relationships online, both through electronic groups and through individual connections. Yet our own data (Cummings, Kiesler, Kraut, Mukhopadhyay, & Scherlis, 1999) and those of Parks and Floyd (1996) have shown that people typically feel less close to online communication partners than to those with whom

they have formed real-world relationships. Most online relationships formed by the participants in our field trial resulted primarily in informational support rather than emotional support, tangible aid, or companionship. When emotional or tangible support was exchanged, the partners were almost always friends or family who had preexisting real-world ties in addition to their communication online.

Rierdan (1999, this issue) notes that the Center for Epidemiologic Studies Depression (CES-D) Scale we used to measure depressive symptoms also taps anxiety, demoralization, and other aspects of distress. She notes that the mean scores of our participants showed low levels of distress and argues that we did not demonstrate that our participants became clinically depressed as a result of using the Internet. She argues that given small effect sizes, there should be no personal or public policy implications drawn from the findings.

We agree that our participants, as a group, did not become clinically depressed. Our claim was not that the Internet causes depression but rather that more use of the Internet can lead to small declines in social and psychological well-being. We used the CES-D scale along with five other indicators of psychological well-being (loneliness, stress/hassles) and social involvement (family communication, social support, size of social circle). The importance of our results is not in the size of the effects, which were admittedly small, but in their direction. We were shocked that a technology we and others had expected to be positive or at worst benign would instead predict negative impact on psychological and social well-being. Even small negative changes experienced by the tens of millions of people using the Internet can be socially significant.

Every day one hears public demand and policy support for putting every citizen, student, patient, school, or business online. It is certainly prudent to gather and discuss evidence relevant to the effects of these actions. Moreover, as businesses invest billions to merge television, telephones, interactive services, and the Internet, we need to better understand how people will use these new technologies, for better or worse. Ours is only one study contributing to this discussion.

Shapiro (1999, this issue) offers an alternative explanation for our results, which hinges on the high degree of social connectedness among some participants in the sample. Her explanation is that participants in our Internet trial consisted of families with teens on a school newspaper or an adult on the board of a community development organization. With a sample selected to have higher than average social connectedness, one would

expect regression toward the mean. That is, some people will drift toward less social connectedness. However, without counteracting drifts upward, because the sample was already at the top of the distribution, one would expect to see the sample as a whole become less social with time. Also, by the second year of the study, some of the high school students had left home to attend college or a job, leaving a possibly bereft family. Although these explanations may explain why the sample as a whole or individuals within some households might have grown more lonely and depressed, they do not address why individuals who used the Internet more would experience more declines.

To fill this gap in her story, Shapiro (1999) hypothesizes that those who were losing social contacts or were becoming more lonely or depressed started filling their now empty hours with Internet use. Although this causal route is plausible, it is inconsistent with our time-series data. In particular, people who were more lonely, depressed, and isolated at one time period did *not* subsequently use the Internet more, either in the month or two following the measurements of well-being or during the full duration of the field trial. We also looked at what happened to parents of teens who went off to college. (Once they left the household, the teens themselves were dropped from the sample because we could no longer measure their Internet use.) These parents made up a small group, but it does not look like the associations of well-being with Internet use were stronger in that group than in others.

Shapiro (1999) argues that we could have avoided these uncertainties at small cost had we included a control group in a true experiment. We agree that our study would have profited from a control group, even though we believe that our panel design allowed stronger causal inferences than are typical in survey research. We did, in fact, attempt to recruit and follow a matched control group and found the costs to be greater than our resources allowed. Selecting a sufficiently large matched group of families, monitoring them over time, and paying them for staying in the study proved to be beyond our capabilities when we started the research. With support from the National Science Foundation, we are now conducting a true experiment, in which people who recently bought a computer are randomly given an Internet account or not given one; these households are compared with a matched group who recently bought a television. However, even this research will not rule out alternative explanations because some of those who are given Internet access will not use it, and others in the control group will subscribe to an Internet service provider on their own.

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- the 1980s" (p. 717). They also asserted, "As we have noted and one reviewer recently observed, 'For over a decade, virtually every contemporary commentary on corrections in the United States has reminded us that the system [is] in crisis' (Cullen, 1995, p. 338)" (p. 718).
- What Haney and Zimbardo (1998) failed to acknowledge is that these conservative developments were clearly a response to liberal sentencing and parole policies and a widely held belief among the citizenry that the rights of victims of violent crime and the potential future victims of reoffenders were not being given appropriate consideration against the rights of those convicted of such offenses.
- There was substantial sentiment among front-line and supervisory law enforcement officials and among prosecutors that our sentencing and parole policies constituted a "revolving door" in which criminals were too often returned to the streets after little or no incarceration time and with high rates of reoffense. The move toward "truth in sentencing" laws that have restricted parole was a response to frustration on the part of juries or judges giving 10 to 20 year sentences while the convicted criminals often served only a fraction of the sentence.
- The tightening of parole and the introduction of grid-sentencing systems, which take into account various factors like previous offenses and the use of firearms, were stimulated by citizenry demands to rein in the heavy use of discretion by judges; lenient judges were giving slaps on the wrist, whereas hanging judges threw the book at defendants, creating a "luck of the draw" situation, where assignment of a case to a particular judge could mean either an extremely liberal or an extremely conservative sentence. We believe that determinate sentencing (e.g., replacing a 7-20 year sentence with a 14-16 year sentence with restricted opportunity for parole and time off for good behavior) was, in fact, a reaction to the excessive discretion exercised by some judges at each extreme end of the political spectrum. We oppose determinate sentencing because it gives inmates little incentive to be on their best behavior (e.g., skills acquisition, education, therapy, drug treatment, and community service). We have advocated what we call "truth in minimum sentencing," which combines a mandatory minimum sentence with a wide range of possible sentence lengths (e.g., instead of a 10-12 year sentence with 15% maximum time off for good behavior, we would support a 14-20 year sentence with 40% maximum time off for good behavior). This would provide strong incentives for self-betterment and good behavior while retaining the opportunity to keep refractory inmates incarcerated for longer periods. We have confidence that an appropriate balance will ultimately emerge between the need for some uniformity in sentencing and the exercise of judicial discretion.
- Similarly, we view the introduction of mandatory minimum sentences as another response to a perceived excess in judicial discretion. A number of widely publicized incidents of rapists being sentenced to probation shocked the nation and led to calls for mandatory minimums. We believe that the need for mandatory minimums was well-established (e.g., it deeply concerns us that any convicted violent rapist might ever be allowed to roam freely in society without a prison sentence or with a mild prison sentence). Whether the current mandatory minimums have been set too high is another question. We believe there is a strong argument for lowering many of these minimums (particularly in drug offenses), whereas in the case of rape, mandatory minimum sentencing might in fact still be too lenient.
- Whether these conservative trends in sentencing and corrections are collectively wise is indeed questionable in our judgment, and we do not champion conservative views in corrections. We agree with many of Haney and Zimbardo's (1998) criticisms of the nation's correctional and economic policies. However, these laws appropriately embody democratic legislatures' responses to the strongly held views of citizens that violent criminals with several serious convictions should not be released after serving only short sentences and that the rights of violent felons need to be appropriately balanced against the rights of victims and potential victims. Many conservative policy analysts view the recent declines in U.S. crime largely as a direct result of tougher crime and corrections policies (see Reynolds, 1998, for a compelling empirical argument in favor of this view). This is not an unreasonable hypothesis; in fact, psychological science would predict a drop in crime as the penalties for specific behaviors become more severe. A front-page news article in *The New York Times* (certainly not known as a conservative newspaper) recently explained the recent drop in American crime thus: "Among the specific reasons for the continued growth in the prison population, . . . experts said, are longer sentences, reduced use of parole, increased arrests of parole violators, sending them back to prison, and improved efficiency by the police in solving crimes as there are fewer crimes to solve" (Butterfield, 1998, p. 1).

## A Balanced Approach for Corrections Policy Needed

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Haney and Zimbardo's (July 1998) treatment of the past 25 years of corrections policy suffers from politicization and suggests an ideological freighting inappropriate to the advance of psychological science. Haney and Zimbardo argued that "national prison policy has become remarkably punitive" (p. 718), described the 1996 Prison Litigation Reform Act as "this remarkable legislation" (p. 718), and said that "a sharp right turn away from the possibility and promise of the Warren Court's view became evident at the start of

## Opportunities for Behavior Scientists

We applaud Haney and Zimbardo's (1998) call for more involvement by psychologists in correctional policy formation and execution.