

[**Editor's Note:** This response to Rick Doblin's follow-up to the Leary Prison Experiment was originally published in the *Journal of Psychoactive Drugs*, Oct-Dec 1998 issue.]

Reflections on the **Concord Prison Project** and the Follow-Up Study

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Rick Doblin has provided a valuable service to the field of psychedelic research by conducting critical follow-up studies to the Good Friday study of religious experience as well as the study discussed here on a "behavior change" program for convicts in a maximum security prison. I was probably more deeply involved in the project and in the writing-up of the results than anyone besides Leary. I spent the better part of two years out of my four-year graduate program on this project, and though it was not the area in which I did my thesis research, I did also do a clinical internship in Concord prison, supervised by Dr. Madison Presnell, who was also the supervising psychiatrist for the psilocybin project.

It is disconcerting, of course, to discover, 35 years after the fact, that a research project I was involved in and wrote about, made quantitative errors and reported erroneous conclusions. As I read Rick Doblin's findings, and re-read our original papers, it did give me occasion to reflect on that period, and what was called the Harvard Psilocybin project. Leary was, at that time, a respected, experienced and committed research scientist, who had spent a decade doing statistical evaluations of psychotherapy — and coming to the depressing conclusion that none of it did any better than chance (as far as one could tell from the tests). The graduate students in the project, such as Gunther Weil and myself, were equally committed to doing exacting and rigorous demonstrations. Weil and myself, for example, spent most of a summer in the archives of the State Department of Corrections, poring through hundreds of prisoner files and assembling the data we needed to calculate an appropriate recidivism base-rate, against which any behavior change program could be tested.

The two dominating paradigms in the Department of Social Relations at Harvard, of

which the Center for Research in Personality was a part, were behaviorism and Freudian psychoanalysis. For this reason, Tim Leary was particularly pleased with the prospect of trying the psychedelic insight treatment with convicts. Having just shown that the subtle personality changes that psychotherapists feel sure happen, can't be demonstrated to happen, he pointed out that in the recidivism rate (the rate of return to prison after being released on parole) we had the perfect incontrovertible behavioral index of personality change. Personality tests would also be given before and after the therapy, but they were secondary.

It was therefore with some dismay that the members of our project realized, after almost a year of running dramatic, seemingly life-changing psilocybin sessions with convicts in the prison, that we (and the prisoners) didn't have a clue as to what to do for them once they got out of the prison and how to help them make it in society. In the 1965 paper published in *Psychotherapy: Theory, Research and Practice*, which I mostly drafted, I remember sharing the disappointing conclusion that our group's recidivism rate was not different from the base-rate. We did find and report significant changes in several scales on the California Psychological Inventory, and one on the MMPI; as well as some inconclusive behavioral ratings changes. And that is the way I have held the study in my mind ever since — that deep personality changes occurred, but in order to maintain changed behavior outside of the prison, some kind of half-way house or rehab program is essential.

This conclusion was stated clearly in our reports. Leary actually devoted a tremendous amount of time and energy to trying of ways that our project could somehow help the paroled convicts who had been through our program, "make it" on the outside, trying to help them find jobs, places to live, offering

them companionship. I remember trips to Boston bars to meet with some of the men, just to keep in touch. Leary has described (in *High Priest*, ch. 10) his extreme and almost comical efforts on behalf of an uneducated, unskilled, lower class, alcoholic petty thief who was the first to graduate from our program — including eventually giving him a job at the Center for Personality Research (the “job” being to find another job) and renting him a room in his family home, with his kids.

Thirty-five years is a long time from which to recall details of a statistical research project. Although I wrote up the results of the final and longer follow-up in the 1965 paper, I have to say at this point I have no idea how Leary came up with the “finding” that the return rate for parole violations was up and for new crimes down (hence the overall rate unchanged). This “finding,” which has now turned out to be erroneous, was of course the kind of result we wanted to find — it enabled us to maintain a positive, enthusiastic attitude in talking about this project. We fell victim to the well-known “halo effect,” by which researchers tend to see their data in as positive a light as possible. I have myself, in later years, sometimes forgotten the basically negative result we reported in the study, and talked about the project as if we lowered the recidivism rate.

In this sense, I’m grateful to have this late opportunity to acknowledge a chastening correction. Rick Doblin’s analysis of the situation shows that most prisoners who were actually returned for parole violations, had also committed new crimes — so that the distinction itself is an artifact.

Similarly, I have at this time no idea where the 10-month follow-up figure of 32% recidivism in our group came from. In the article in the *British Journal of Social Psychiatry*, which he apparently wrote before the project was completed though it was only published in 1968, Leary (erroneously) compared this 32% figure to the 30-month base rate figure — thereby arriving at a significant overall reduction. He stated these as “extremely tentative” results, subject to further analysis. Nevertheless, they are clearly inconsistent

with our own results reported in the 1965 article of no difference in overall rate of return. As Doblin points out, Leary was able to get this seemingly positive finding only by using the wrong control figure. In the paper from *Psychodelic Review*, which was excerpted from the forthcoming *High Priest*, he repeated the same mistake — and I, as editor, didn’t catch it. Clearly, we were both under the “halo effect.” Leary was also by this time no longer playing what he called “scientist game.”

Whether Leary made these mistakes consciously, faking the results that he wanted, or whether they were unconscious mistakes of carelessness, motivated by over-enthusiasm — is impossible to say at this point. I tend to favor the latter alternative, if only for the reason that our own results clearly show the inconsistencies. Our basic finding remains, then as now: that with psychedelics (and other programs) profound experiences of insight and personality change can be brought about, but criminal behavior patterns take a much more concerted system of rehabilitation and community support to change. The 30-year later interviews with two of the men document this in a direct and touching way.

The one statement of Doblin’s that I would still question is the need for a “higher standard” or “highest ethical standards in order to regain a measure of trust with regulators.” In my opinion, the existing accepted standards of honesty and truthfulness are perfectly adequate. We have those standards, not to curry favor with regulators, but because it is the agreement within the scientific community that observations should be

reported accurately and completely. There is no proof in any of this re-analysis that Leary unethically manipulated the data. Careless mistakes were made, no doubt, mistakes that made the data look more like we wanted them to look. But to make mistakes is neither unethical nor unscientific. It’s an integral part of the scientific method that when mistakes are found, they are reported and corrected. In this sense, I appreciate the better understanding that comes from this more complete and accurate review of the Concord prison project. •