Ecstasy Use Among Club Rave Attendees

The “rave” phenomenon—loud music, flashing lights, and frenzied all-night dancing—has been a major element in the resurgence of psychedelic drug use in Western society.1 Purportedly central to raves is the use of “club drugs,” including 3,4-methylenedioxy-methamphetamine, also called MDMA or ecstasy.1 The use of ecstasy seems to be increasing worldwide, with rave attendees being a high-risk population. Because these drugs have potentially serious physical and psychological consequences, such as anxiety, memory loss, paranoia, depression, cognitive impairment, cardiac complications, and kidney failure,2,3 the rave phenomenon has sparked the attention of health officials and policy makers. However, to our knowledge, no studies have collected self-report or objective drug use information from rave attendees in the United States.

Our study estimated the prevalence of ecstasy use among a sample of rave attendees. Self-reported drug use information and saliva specimens were collected from attendees of raves at 5 nightclubs in the Baltimore-Washington corridor between 1 AM and 4 AM during the fall of 2000. A total of 148 club rave attendees were approached for interviewing. The interviewer’s introduction described the purpose and sponsorship of the study and included informed consent provisions. Seventy-seven percent of persons leaving raves agreed to and completed the interview. Ninety percent of those who completed the interview provided a saliva specimen that was subsequently analyzed by a laboratory (PharmChem Laboratories, Menlo Park, Calif) for the presence of MDMA. Results are based on the 96 individuals who completed the interview and provided a saliva specimen. Of the original 102 respondents who completed interviews, 6 reported being younger than 18 years and were excluded from further analysis.

Most of the sample (65%) was male and between the ages of 18 and 21 years (76%). Ninety-four percent were white, 84% were employed at least part-time, and 88% had completed the 12th grade. Eighty-nine percent reported lifetime ecstasy use, 48% reported 30-day ecstasy use, and 18% reported using ecstasy within the 2 days preceding the interview (Figure). Twenty percent tested positive for ecstasy by saliva analysis.

The Table describes the drug use of the entire sample and compares ecstasy users with nonusers. Current ecstasy users were more likely than nonusers and past users to have used marijuana in the past 12 months (81% vs 36% and 33%; P < .001) and more likely than nonusers to have used powder cocaine in the prior 12 months (51% vs 0%; P < .01) (Table).

Estimates of lifetime ecstasy use obtained from this sample (89%) far exceeded those from our earlier study of juvenile offenders in Maryland (18%)4 and those from a study of a group of college students of comparable age (8.4%).5,6 Our findings, therefore, corroborate previous studies outside the United States7 that found rave attendees to be at high risk for ecstasy use. Our study also revealed that current ecstasy users were more likely than nonusers and past users to have used marijuana and pow
Some Pitfalls of Computer Modeling

The article recently published in the ARCHIVES titled “Computer Simulation of Stair Falls to Investigate Scenarios in Child Abuse” is a troubling example of irresponsible “computer simulation” without scientific foundation and experimental basis. The intention of the study was to demonstrate that computational tools may be used to aid clinicians, scientists, and engineers in determining injury mechanisms—a noble and important goal in objectively evaluating injury potential and formulating prevention, therapeutic intervention, and rehabilitation strategies. However, the authors demonstrate a naive approach to computer modeling and draw tenuous conclusions about the importance of stair fall events. The authors also take the additional, unsubstantiated step of postulating that their computer model will eventually differentiate between “accidental” and “nonaccidental” trauma, the latter implying intent typically on the part of a parent or caregiver.

The published results of the authors’ study demonstrate that the model achieves none of these results with any scientific basis or biomechanical rigor.

Although the authors mention the age-old caveat that their model is yet to be validated scientifically, the mere mention of the lack of objective support is only one of several shortcomings of the study and does not absolve the authors of drawing unsubstantiated conclusions about injury mechanism and intent. None of the model’s results are explained by theory or demonstrated with experimental evidence or discussion of case study. No comparisons are made between the model’s “outcome measures” and biomechanical tolerance data for various modes of femur failure. None of the plots demonstrating the model’s output are given a statistical indication of error and no sensitivity analysis of initial conditions and parametric variation is presented to indicate the true statistical significance of each parameter tested. None of these critical points are addressed in the study, and many of the readers unfamiliar with modeling may not even be aware that they are critical points.

While the authors’ formulation of a model using existing and powerful technology to aid in the determination of injury mechanism is well intentioned, the execution of such a study and the publication of its results are not substantiated simply by the use of a computer. The computer is only solving kinematic equations based on the laws of physics, and without the proper input and the crucial scientific and experimental validation of the output, the model is naive at best and misleading and potentially dangerous at worst. No computer model can “detect” intent.

In reply

In response to Dr. Thibault’s letter regarding our article, we would like to offer the following.

Dr. Thibault states, “the intention of the study was to demonstrate that computational tools may be used to aid... in determining injury mechanism. . . .” and that our conclusions about injury mechanisms and intent are unsubstantiated. Our study objective, as stated in the article, was to demonstrate the usefulness of computer simulation techniques for investigating the influence of stair characteristics on injury biomechanics. We concluded that stair characteristics play an important role in the likelihood of injury, highlighting the need for documenting details of the environment in which a fall occurs. The goal of our work was not to determine injury mechanism, but rather to better understand specific contributors to injury risk. We drew no conclusion about injury mechanism or intent.


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Dr Thibault states that our model does not achieve the results of identifying intent with any scientific basis or biomechanical rigor, and that without proper input and validation, models can be misleading and dangerous. We agree that any model is only as accurate as its input. He states, “The computer is only solving kinematic equations based on the laws of physics . . .” Our model is based on the laws of physics, which govern fall dynamics. Simulations were based on anthropometrics of a 3-year-old and actual stairs; the biomechanical measures were derived from physics-driven equations. Computer simulation techniques allowed us to systematically explore the effects of stair characteristics on biomechanical outcomes by isolating one parameter at a time. He also states “no comparisons are made between the ‘outcome measures’ and biomechanical tolerance data . . .” As stated in our article, “Without experimental validation, absolute values of biomechanical measure should not be relied on and simulation results should only be used to study relationships between model parameters and outcome measures.” It would not be appropriate to make comparisons with tolerance values, since our findings relate only to the relationships between stair characteristics and biomechanical measures.

Dr Thibault concludes that nonaccidental trauma implies intent, that “no computer model can ‘detect’ intent,” and that we have postulated our computer model to do so. We agree that no computer model can detect intent, but do not agree that abusive trauma implies intent, and we did not indicate that our model could detect intent. Intent is a psychological aspect of injury that is neither objective nor biomechanically based. We do hope, through medical and bioengineering collaboration, to develop a more objective model to aid in more accurate injury assessments. Computer simulations are only one small first step toward this goal.

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**Medical Neglect: A Child-Focused View**

Wolf et al presented an interesting approach to situations in which parents reject interventions to reduce postnatal transmission of the human immunodeficiency virus (HIV). We wish to offer an alternative perspective. Wolf and colleagues provide a framework focused on parental behavior or omissions in care. The multiple and interacting factors that contribute to children’s needs not being met have been underscored by recent reports exploring issues of noncompliance and neglect. These factors include aspects of the disease, the treatment, the child, family functioning, and parental behaviors. There are clear advantages to view neglect from a child’s perspective—occurring when a child’s basic needs are not met—and to acknowledging the many risk factors that may be responsible. After all, our goal is to ensure children’s well-being, not to blame parents.

From the child’s perspective, even a small risk (eg, 0.5%) of contracting a potentially fatal disease is a serious concern. Indeed, there are many health measures we employ for far smaller and less consequential risks. Wolf et al present the choice as whether to pursue a court order (to mandate adherence to health recommendations) or not. This dichotomous situation should be rare. There is an array of intermediate, less intrusive measures that often help. Physicians can address misconceptions, allay parental fears, and develop a plan that both protects the child and acknowledges parental concerns, obviating court action. Perhaps the authors assumed such steps had been tried, but it was not clear.

Judgments of neglect must be grounded on the best available information regarding risks and benefits. In this regard, we must note that data on reduction of perinatal transmission with prenatal zidovudine monotherapy has been supplanted by the observation that the risk of perinatal transmission is virtually zero when prenatal combination therapy maintains an undetectable viral load.

The first vignette involving the schizophrenic mother is clear: treatment is mandatory. We think that in the second case intervention is also clearly required—not merely “permissible” as suggested by the authors. The Nairobi randomized trial showed a 16% decrease in HIV transmission with randomization to artificial feeding (even though adherence to the assigned group was not good), along with an increase in HIV-free survival at 2 years. Although the risk of transmission through breast milk is related to maternal viral load, other factors, especially the presence of mastitis, have profound effects. Transmission from women with lower viral loads than the scenario presented by Wolf et al is well documented. Thus, although in this case the viral load was not high, there is compelling evidence that significant risk of catastrophic consequences remains. We strongly disagree with the statement that one “may reasonably judge that the risk to the infant is not high enough to override parental authority.”

The third vignette is more equivocal. There are no published data yet regarding the effect of highly active antiretroviral therapy on transmission through breast milk (or on the effectiveness of prolonged neonatal prophylaxis on preventing transmission through breast milk). However, the reduction of perinatal transmission to virtually nil when viral replication is undetectable on therapy and the suggestion that HIV in breast milk may represent leakage from serum make it very likely (but unproven) that the effect of maintaining an undetectable plasma viral load on treatment may reduce risk to an extremely low level.

The authors rightly point to the need to clarify the social benefit and harm. However, one citation on foster care is not representative of the foster care system, which can benefit some children. Does the usual dogma regarding breastfeeding apply when a mother knows she may be transmitting a fatal disease to her newborn? Complex medical decisions with profound consequences need to be made by informed professionals. We greatly re-
spect mothers (and fathers) and seek to work with them, but the child’s health, and life, is our priority.

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In reply

We agree with Dubowitz and colleagues that pediatricians must consider the multiple factors that may affect an infant’s well-being. We also agree that a court order should only be sought after attempts to resolve the dispute with less intrusive measures have failed. Accordingly, pediatricians have an obligation to work with parents to understand the parent’s position, to explain their recommendations to the parents, and to explore alternatives that can mitigate the risk to the infant and be acceptable to the parents and the pediatrician. In most cases, such discussions will result in an agreement on a plan of care.

While the medical risk to the infant is an important factor to consider, it is not the deciding factor. We cannot eliminate the risk of infection even when prevention measures are complied with fully, and other risks, such as violence or loss of housing and support, may present a more immediate and certain harm to the infant and mother. While foster care can be beneficial in some circumstances, disrupting the parent-infant relationship has its own harmful consequences and should not be undertaken lightly. Physicians who believe that they can best protect the infant’s interests by maintaining a good working relationship with the parent should be permitted to act accordingly.

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