of the phallus. If the same hospitalized patient would rather die or lose the penis than to reveal his homosexuality, it is ludicrous to suggest that this information could have been obtained before an elective circumcision in the office. How was an HIV test obtained when law requires written and informed consent from this patient who denied high risk behavior?

Respectfully,
John T. Hotter
1085 Fairhaven Boulevard
Elin Grove, Wisconsin 53122

RE: HISTOPATHOLOGICAL AND CYTOPATHOLOGICAL CORRELATIONS OF PERCUTANEOUS TESTIS BIOPSY AND OPEN TESTIS BIOPSY IN INFERTILE MEN

D. N. Kessaris, P. Wasserman and B. C. Mellinger

To the Editor: While there is no question that testicular biopsy can be reduced to a percutaneous method that will provide excellent histopathological and cytopathological information, it is also true that by obtaining the biopsy percutaneously the opportunity to examine the epididymis with magnification (loupes) is lost. Performance of a testicular biopsy with a spermatic cord block using intravenous sedation with diazepam through a small transverse scrotal incision in the office affords the opportunity to evaluate the epididymis visually in addition to obtaining biopsy material. The appearance of the epididymis provides extremely valuable information to exclude obstruction as the etiology of infertility and it also can help define obstruction involving the vasa efferentes. This is 1 instance when I argue against oversimplifying a technique.

Respectfully,
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Reply by Authors. The visual appearance of the epididymis may often suggest obstruction. However, visual inspection is certainly not conclusive. An obstruction can only be demonstrated adequately at surgical exploration. An obstruction is present when the testis has been determined to produce mature sperm and when no sperm can be identified in the vasal fluid distal to the site of obstruction. A small incision into the hemiscrotum, which would permit adequate visual inspection, may also subsequently lead to significant adhesions that may make reconstructive microsurgery somewhat more difficult. Most infertility specialists also advocate using the "window" technique when performing open testicular biopsy, since this may lessen the incidence of postoperative adhesions. The purpose of testicular biopsy is only to determine whether there is relatively normal spermatogenesis. Ductal obstruction is conclusively demonstrated at subsequent surgical exploration.

RE: AUTOMATED PROSTATE VOLUME DETERMINATION WITH ULTRASONOGRAPHIC IMAGING


To the Editor. We congratulate the authors on their perseverent work to develop an automated program measuring total prostate volume from ultrasound images. The accuracy of this computerized planimetric method was excellent compared to off-line hand-drawn planimetric volumetry using the identical ultrasonic images as the computer. The planimetric volume described as the transverse volume, which is the usual method of planimetry in the literature, was less accurate due to the clinical on-line situation of the ultrasonographer.

The authors chose planimetry as a standard because of its accuracy in the literature. Step-section planimetry, however, appears to be the most reproducible of the various volumetric methods, especially when used transrectally instead of the suprapubic method described by Feigr and Knönnagel. We really wonder about the reproducibility of the various volumetric methods applied by the authors, since their data show that the ultrasonographer in their study interpreted the volume of a prostate on an identical set of ultrasonic images to be approximately 40% smaller and with a large variation compared to the off-line situation in which the reference volumes were created. In addition, it will be of interest to learn whether the automated volume is reproducible when an identical prostate is measured from a second or third set of ultrasonic images because these ultrasonic slices will hardly ever be identical to the first set. In step-section planimetry it has been illustrated that various volumetric errors may be induced due to small changes in position of the ultrasonic probe in relation to the prostate. Moreover, the interobserver variation between urologists, possibly depending on their expertise, using the automated system compared to on-line planimetry in a standard clinical situation needs further evaluation.

Hopefully the authors will continue to create a similar program measuring the volume of the adenomatous tissue. Although the total prostate volume has been used to illustrate response to therapy, an even more important feature of prostate volume is the volume adjustment of serum prostate specific antigen (PSA) values. Various articles have shown that the transition zone is far more important than the prostate volume with regard to the increase of specificity to detect prostate carcinoma by PSA adjusted for volume of the transition zone compared to PSA density. 1-3


Reply by Authors. Bangma and Schröder are concerned about the reproducibility of various volumetric methods in sets of images of identical prostates, especially the reproducibility of the methods used for planimetric volumetry, since step-section volumetry appears to be the most reproducible method. The differences described in our article occur because different methods were used to obtain the volumes. The clinical and transverse volumes were obtained at the clinic using the built-in volumetry method of the Kretz Combison ultrasound scanner. The differences reported between the longitudinal and transverse volumes indicate the limitations of this method. The area for capturing ultrasound volumes is limited to 150 cc. Contour following with the track ball can easily lead to displaced contours. At our clinic routine outlining has been performed in the longitudinal plane to overcome the "salami" effect that may occur in the transverse plane (reference 3 in Letter). Interpretation differences may be introduced using different planes. The clinical inter-section distance (or step size) was not fixed but selected manually and errors may occur in clinical outlining due to time pressure. These reasons will lead to a large variability for the built-in volumetry of the echo scanner. The results were compared to the off-line outlining.
by an experienced ultrasonographer in images stored in the computer. The reproducibility of this off-line outlining will be equal to or better than the reproducibility found by Stone et al in a clinical application of step-section volumetry. They found a variability of 5% in longitudinal measurements in 15 patients with planimetric volumetry using transrectal ultrasound with 5 mm. intervals. Also, Hendriks et al indicated in a cadaver study that a good correlation could be obtained between the volume with transrectal ultrasound and the prostate volume measured after prostatectomy when the investigator is making an effort to outline exactly.2

Another point of discussion is the reproducibility of the automated method when an identical prostate is measured using another set of images. The quality of planimetric volumetry can be expressed by the accuracy and reproducibility. We are aware of the limitations of numerical integration influencing the accuracy and reproducibility. Possible influences are first step effect, step size and "salami" effect. Testing the reproducibility of the automated method is also a test for the reproducibility of prostatic ultrasound. From a computer analysis, we concluded that in theory the accuracy error for 4 mm. step size is -2.3 to 1.8%, depending on the selection of the first section and the length of the prostate.1 Also, rotational movements leading to a tilted prostate axis compared to the probe axis can be an influence. From this analysis, we can conclude that several sets of images from 1 patient will have a theoretical accuracy of more than 97%. The objective of our study, however, was to compare the computer outlining in prostatic images to the manual outlining by an experienced ultrasonographer. For the computer it makes no difference which set of images is taken from a new or former patient. The computer has no memory nor has it learned from other images. Since the reference volume is obtained from the corresponding set of images, new data points are obtained. We can assume that the accuracy of these points will be in the same range as the accuracy presented in the article for 55 patients.

A question raised by Bangma and Schröder concerns the inter-observer variation when different urologists use the automated method. The expertise of the examining urologist can be of influence in the results of the automated method but this influence is limited to the first and last step-sections, and to the quality of the images used as input to the automated method. Leaving out images at the beginning or the end of the prostate will, of course, lead to an underestimation of the prostate volume. Also, the image quality is important for the correct assessment of the prostate boundary location, which means that the protocol definition is important: store the first image at the base in which the prostate is visible, and retract the probe until the apex is reached and the next image contains no prostatic tissue. For imaging of the prostate, standardized settings were defined. These settings were determined by echo scanning leading to sufficient image quality. Because of the promising results presented in our article an image processing system was located at our urology clinic and is used on a routine basis by several urologists in patients with lower urinary tract symptoms. Evaluation of the first 260 patients showed results comparable to those presented in the article, and so we can conclude that the results of the automated method are not influenced by different users.

As a final result, we agree with Bangma and Schröder that the volume of the transition zone is important for the interpretation of PSA levels. Since the prostate has a nonhomogeneous structure within the gland, no local information can be used to detect this zone with the edge detection tools described in the article in contrast to the outside border of the prostate. Other image processing tools, such as texture, describing parameters for classification of prostatic tissue,5 may be capable of detecting the transition zone automatically. However, the possibilities of these techniques for a clinical application need further evaluation.


RE: EDITORIAL: BENIGN PROSTATIC HYPERPLASIA

H. Lepor

To the Editor. Lepor maintains that in our pressure-flow diagram for precise analysis of voiding function the division of obstruction grades and the "assignment between obstructed and unobstructed is strictly arbitrary," and he arbitrarily derives a number of questions from this contention. Lepor continues with a discussion that mainly exploits the fact that the traditional terminology, specifically the term obstruction, is vague, clinically difficult to qualify and impossible to quantify, and thus, scientifically undefined. A scientific definition of obstruction with quantification by objective measurement can only be derived from urodynamics. The difficulty then arises that such objective urodynamic measures of obstruction do not closely relate to any clinical definition of obstruction, so that urodynamic obstruction and clinical obstruction are not compatible but in competition. Therefore, we cannot calibrate the urodynamic measure of obstruction with the traditional clinical obstruction data but we must decide on using a direct and objective urodynamic measure or indirect and subjective clinical information.

Consequently, a consistent urodynamic definition must be built on urodynamic data alone. Therefore, we have assigned the term unobstructed to those outflow conditions that cannot be improved by surgery (transurethral resection of the prostate) and we grade as obstructed outflow conditions those urodynamic values that can be improved by surgery. I do not agree that such a definition is strictly arbitrary but I believe it is a theoretically meaningful definition with practical applicability. This definition as well as answers to all other questions that Lepor then discusses are included in the original report.1 Of course it is true that the clinical significance of urodynamic obstruction is not clear and can be derived only from longitudinal studies, including the natural history. However, is the clinical significance of using any other objective definition and fine grading of obstruction known? Where are these studies?

Urodynamics for many years has provided precise tools, that is the scientific methodology, to investigate the complex of bladder outflow obstruction, bladder voiding and storage symptoms, and benign prostatic hyperplasia.2 We will not know what these tools can do before we have used them but we know that the future of urology will depend on such scientific methodology. I would wish that more of the prominent American researchers and clinicians would start to use urodynamics to assess bladder outflow obstruction in benign prostatic hyperplasia. My impression from the literature and as a regular attendee at the American Urological Association meetings, as well as from this editorial, is that some American attitudes toward urodynamics have concentrated on identifying the missing answers in urodynamics more than contributing to provide these answers.

Respectfully,

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Reply by Author. Schäfer acknowledges in his Letter that "the clinical significance of urodynamic obstruction is not clear and can only be derived from longitudinal studies, including the natural history." This was the essence of my editorial comments. I am delighted that we finally agree. I have nothing against Schäfer's grading system for obstruction relative to other definitions and grading systems. Hopefully, future studies will define the clinical relevance of these measurements.