Funding of the Essential Synergy between Small and Large Telescopes

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Abstract:

Since the 1956 Flower and Cook Symposium on the future of moderate-sized telescopes, such telescopes have contributed a substantial fraction of the important scientific results in astronomy. In addition, some of the significant science and discoveries produced by astronomers at large telescopes could have been made at smaller instruments at that time. When considered on a per dollar basis, the scientific productivity of smaller telescopes dominates the field. Some small telescope myths, such as "small telescopes are best dedicated to large survey projects" are examined. If "goodness" of science is defined in any terms other than its cost, smaller telescopes easily hold their own. Astronomy has a history of an essential synergy between small and large telescopes. This synergy can be maintained only if there are a reasonable number of well-maintained, well-instrumented smaller-sized telescopes. Recommendations for a small telescope budget and a refurbishment of the Schmidt telescopes at the national observatories are made. Two percent of the Astronomy and Astrophysics Survey Committee decadal budget would provide well-instrumented facilities to the same number of astronomers who use the existing or approved large ground-based optical and infrared telescopes and add 50% to the amount of quality astronomy produced.

Key words: research productivity, small telescopes, large telescopes, decadal survey

> *"In publishing the original Letter by H.L. Johnson and W.L. Richards (Ap.J.[Letters],* ***160****, L111, 1970) the Editor did not sufficiently realize that the subject is a highly controversial one in which divergent views are*

> *strongly held. It does not appear that any useful purpose will be served by publishing further correspondence on this matter in the pages of the Journal. Accordingly, with the publication of Dr. Okes's Letter stating a different point of view the subject is closed"*[1]

The subject was the optimum size of optical telescopes and it was hardly closed; only banned from the pages of the Astrophysical Journal. The purpose of the current contribution is to provide some thoughts on an optimal expenditure of funds in astronomy with respect to the issue of telescope size. This purpose is narrower than the issues addressed by the Astronomy and Astrophysics Survey Committee (AASC, 2001, hereafter AASC) but integral to its general charge of how to allocate resources in the coming decade. In order to address this issue, we must consider the relative contribution to astronomy of the various components and their costs.

In 1956, the dawn of the post-photographic era and its effects on the relative importance of small telescopes to the advance of astronomy was heralded by a two-day symposium in honor of the opening of the Flower and Cook Observatory: *The Present and Future of the Telescope of Moderate Size* (Wood 1958). As Wm. Hiltner observed to open the meeting, " Most astronomers are faced with the problem of having a telescope smaller than the largest." Although CCDs were not yet on the horizon, techniques by Lallemand, Hiltner, and Fellgett in electronic image detection led to the observation that modest-sized telescopes using such detectors could perform as well as or better than the gold standard 200-inch telescope in limiting magnitudes while using photographic plates.

The promise of that meeting has now, nearly 50 years later, been fully realized as back-illuminated CCDs with nearly perfect quantum efficiency and almost no read-out noise are within financial reach of almost all astronomers. Such detectors make their modest-sized telescopes equivalent to one ten times their size in the photographic era, usually exceeding the performance of a 5-meter telescope[2]. Of course, large

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[1] S. Chandrasekhar (1970).

[2] E.g., Deeg and Ninkov (1996) performed V, R, and I photometry to 22.5 magnitude with a 36-inch telescope. Better detectors on the same telescope have now lowered the threshold fainter than 23rd magnitude. This is comparable to photographic results reported by Baum (1962) for the 5-meter telescope. Fry et al. (1999) reached 27.5 R magnitude arcsec^{-2} with a 24-inch aperture. Low-resolution spectroscopy of 19th and 20th magnitude objects is not unusual with 1 – 2 meter telescopes (e.g., Meusinger & Brunzendorf 2001). This compares favorably to a limiting magnitude of 17 reported for comparable dispersions with the 5-meter (e.g., Greestein and Sargent 1974)

telescopes have comparable or better detectors but, in this 50 years grace period, the 5-meter telescope certainly did not exhaust the important astrophysics then available only to it.

The current trend, in most countries, is to close smaller telescopes in order to support fewer, larger telescopes. At the American national observatories, this represents an important shift from their original charge of providing telescope access to many astronomers from institutions without reasonable observing facilities and building unique equipment for all the astronomers of the country[3]. The recommendations of the AASC support the continuation of that trend. Even with the large telescopes absorbing well over 90% of the astronomy funding in the U.S., their proponents have appealed to users of smaller telescopes to support an increase of that fraction![4]

1. SCIENCE EFFECTIVENESS OF SMALL TELESCOPES

The difficult point to consider is what is the relative science effectiveness of small telescopes. Obviously, for observations at the limits of practical observational patience, large telescopes are essential. Also, unless the telescope has unique properties other than its size, it is difficult to justify instrumentation that is orders of magnitude more expensive than the telescope.

First, it seems worth stating that the *cost of a scientific result is not a measure of its scientific value*[5]. In astronomy, where serendipity is common, it is probably more difficult than for most sciences for expensive projects to rise above the mediocre because, understandably, funding agencies are reluctant to risk large sums of money on projects with low or uncertain probability of success.

Are citations a reasonable measure of scientific importance?

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[3] Supporting 700 different astronomers and students and producing 350 papers annually (Abt, 1985)

[4] Lowell Observatory Fall Workshop: The Role of Small Telescopes in Modern Astronomy, 1996.

[5] My favorite version of this is due to an anonymous NASA referee who asked why, if the science in the proposal was as good as it seemed, it was not proposed for a larger telescope?

Counting citations has been criticized as unfairly favoring smaller telescopes because they are more likely to be involved in survey projects which, in amassing large amounts of data, are likely to be frequently cited. It is not clear that this bias exists. Certainly, the greater pressure on larger instruments means that projects with shorter telescope runs are favored. Thus, a wider variety of papers are produced per night on a larger telescope. This produces a higher paper count per night and provides a greater opportunity for high citation papers.

The argument that survey projects unfairly favor small telescopes is based on certain assumptions. First, that the large amount of effort that goes into a major survey is somehow less deserving of its citation count than some different form of effort that goes into other research projects. Second, that the final product of a large survey is of less scientific importance than other research.

Surveys are performed when little is known about what a new observational opportunity will reveal (e.g., first x-ray satellite) or when the scientific question requires statistically complete data (e.g., Galactic structure, distribution of mass in the Universe). In the latter case, this usually reflects a maturing of the discipline; enough of the science involved is understood well enough that the extensive use of resources is warranted to produce the desired results.

On a scientific basis, the decision to commit resources to a major survey seems to be more dependent on the state of the scientific understanding than the size of the required instrument[6]. Although survey work has been frequently given as a possible justification for small telescopes, surveys are now becoming more prevalent on large and expensive telescopes as well. For example, the DEEP (The Hubble Deep Fields and Deep Extragalactic Evolutionary Probe) survey proposes to use nearly 200 Keck nights.

Surveys are the prime example of a distinction made by Abt (1996, 1999) between papers that are heavily cited because of their fundamental nature and those that are heavily cited because of their usefulness. Such a distinction is important in selecting papers for a Centennial volume (Abt 1999), but useful papers, which are often papers tabulating the results of surveys, are certainly as important to our ultimate understanding of how things work as those that we acclaim as fundamental. Abt (1996) lists the 15 papers from 1954 that were the most cited in the following 40 years.

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[6] On a cost and political basis, it is obviously more difficult to commit a very expensive instrument to long projects.

Roughly half were in the fundamental category and half were in the useful category.

Another difficulty with using citations as the measure of scientific value is that while, in the full course of history, a set of scientific results or ideas may be judged of great scientific importance, its citation rates in its time may be low because of slow recognition of its significance in the community[7], a lack of current popularity in that specialization, or an inability of the current technology to address the ideas[8].

Alternative methods of ascribing scientific worth might include the practical importance to mankind. In that case, the search for Earth-crossing asteroids would have to take first priority and SETI projects would probably be second[9].

It is so difficult to provide a quantitative method, outside of citation counts, to assess the scientific value of any research that one must consider *ab initio* to assess all valid scientific inquiry of equal value. This may be a more reasonable approach in astronomy than in many other sciences considering the substantial interdependence across specialties and the strong role of serendipity. For example, it is hard to imagine a field of astronomy that does not critically depend on positional astronomy as an essential building block to its results. This approach is difficult to quantify because of differing authors publishing styles and the nature of the results (e.g., survey versus discovery).

In short, citation count is certainly an imperfect method for determining scientific productivity but seems to be the most easily quantifiable and least controversial. However, using either numbers of papers or numbers of citations produces the same results: the majority of research is accomplished on small telescopes. Abt (this volume) finds those telescopes 2.5 meters in aperture or smaller produce 62% of the papers and are responsible for 55% of the citations. He finds that 75% of the citations are accorded to research performed with telescopes of less than 4-meter aperture.

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[7] E.g., Practical microlensing (Paczyński 1986) or Zwicky(1957); although one of the most famous cases of the 20th Century is probably that of biologist Barbara McClintock.

[8] E.g., Kuiper belt objects

[9] Historically, although it may not have seemed practical at the time (or even now), the source of solar energy fits this category.

2. COST EFFECTIVENESS OF SMALL TELESCOPES

The cost effectiveness of small telescopes, in terms of cost per citation, was established by Abt (1980). He found that the initial cost at KPNO for buildings, dome, mounting, and optics was equal to \$362,000 $A^{2.37}$ for the 0.4 through the 4 meter telescopes where A is the aperture. For the years 1973 – 78, Abt found that the citation rate was proportional to $A^{1.5}$. Thus, roughly, *the dollar cost per citation is directly proportional to the aperture*.

A current estimate, based on slightly different parameters, provides a result even more favorable to smaller telescopes. Figure 1 shows the current cost (Melsheimer 2001) of only the telescope as a function of aperture of the telescope. Note that the exponent is nearly the same as that found by Abt for both telescope and facilities but somewhat smaller than the 2.6 found for just telescope costs by Meinel in 1980 (Robinson 1980). The cost per collecting area increases with the aperture such that the cost per unit collecting area of a 10 meter telescope is nearly twice that of a 1 meter telescope. In this volume, Abt found that telescopes of apertures from five to ten meters produced only twice as many citations per paper as those based on data from telescopes of one to two meter aperture. This suggests a cost per citation proportional to about $A^{1.5}$.

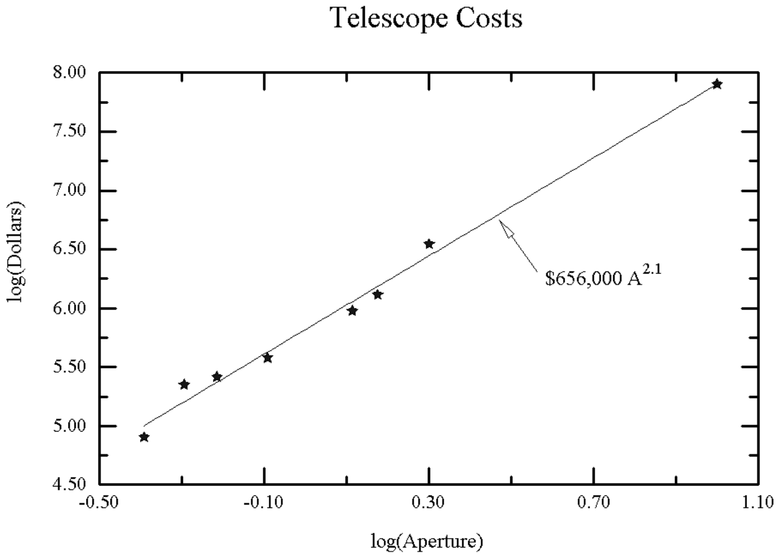

Figure 1. Current telescope construction costs (from Melsheimer, 2001.) Related costs, such as building or instruments, are not included.

Including the operating cost of the telescopes does little to change this latter relationship. Operating costs for one to two meter-class telescopes are of the order of $100,0000 while those of 8 to 10 meter-class telescope are about 50-100 times as much. This again suggests a cost per citation proportional to about $A^{1.5}$.

Tables A.1 and A.2 in the Appendix, are taken from Ringwald et al. (2001) who examined the productivity of 22 ground-based optical telescopes for papers published in 1995 and cited in 1998. After the HST, the typical size for the 22 most productive telescopes in terms of citations/area is about 0.9 meters. Since cost per unit area rises only slowly with aperture, we can take the citations/area to be a good approximation of citations/dollar. The average citations/area of the 22 most productive small ground-based telescopes is four times that of the large telescopes.

Thus, the financial considerations coincide with what most astronomers should feel intuitively: use the smallest telescope practical for the observation. This is possible only if such telescopes, properly instrumented, are available.

3. THE QUALITATIVE PRODUCTIVITY OF SMALLER TELESCOPES

The quantitative productivity has been covered by Abt (1980, and, more currently, this volume). These contributions cover both the publication rates by telescope size and the citation rates to those papers. Is there some additional qualitative aspect that is overlooked by this statistical approach?

Constructing a fully satisfactory list of the most important astronomical research since the publication of *The Present and Future of the Telescope of Moderate Size* is not a feasible task for this author. But, for the purposes of our current discussion, such a list should include not only those scientific works attributed to the smaller telescopes of their time but should also include those observations that could reasonably have been made by such telescopes.[10] For example, the discovery of the nature of 3C 273, was made with a 400 A/mm resolution spectrum obtained with the 200-inch telescope; however, a spectrum of that resolution of a 12.8 magnitude object was clearly within the range of modest-sized telescopes of that vintage.

One can, however, examine lists made by others and determine the size of the instruments used by their authors.

The AAS Centennial Committee selected 53 papers for republication in the *Centennial* issue of the American Astronomical Society (Abt 1999). I assigned papers primarily as supported by large or small[11] telescopes or strongly theoretical. If a primarily theoretical paper was based on data from

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[10] A clear example of such a case is the identification of Sco X-1. Stephenson identified the object with objective prism plates taken in May 1965 through July 1966 with the Burrell 24/36-inch Schmidt telescope for the purpose of identifying the x-ray source. Johnson confirmed this identification with spectra from the KPNO 84-inch in June 1966 (Johnson & Stephenson 1966). Also in June and July, 1996, Sandage et. al (1966) observed the object with the 200-inch and the Tokyo Observatory 74-inch. Although the papers were submitted within 4 days of each other in August 1966, the discovery is generally attributed to the 200-inch (109 citations vs 16 citations for the smaller telescope-based results).

[11] This required a floating definition with time. For example, the Mt. Wilson 60-inch was taken as a large telescope until the institution of the 200-inch telescope; all the Mt. Wilson solar telescopes were taken as large; and all Schmidts were taken as small.

an identifiably sized telescope, credit was given to that telescope size. Large telescope papers, the observations of which could have been done by small telescopes, were tabulated in both in the large telescope row and its own row.

Table 1. Number of Papers by Telescope Size in *Centennial*

Type	**Number**
Large Telescope	15
Small Telescope	16
Theoretical	19
Large, could have been small	2
Space-based	3

The AASC (p.18) provides a list of 12 accomplishments of the 1990s worthy of being included in a list of the important discoveries of the decade. The first was doubtlessly the most important as, if for no other reason, it answered a pressing astronomical question of over a century: the discovery of planets orbiting stars other that our sun. The first two telescopes that made these discoveries were certainly small and they continue to lead this research. Small telescopes also play significant roles in several others on their list, including microlensing, observations of the impact of Comet Shoemaker-Levy 9, and the optical identification of gamma-ray bursters.

While I can't create a personal list that is complete in any way, I would like to add a few of my favorite small telescope projects to those listed elsewhere in this volume. The resolution of the nature of Herbig-Haro objects depended on a telescope with a large enough scale to capture the wide nature of these jets. The discovery of both radio and optical pulsars are the work of small telescopes. The detection of 23 planetary or brown dwarf objects with a 1.2-meter telescope that produces radial velocities with 7 – 8 m/s accuracy (Naef et al. 2001).

There are numerous lists of potential research suited to small telescopes. These lists are either soon made obsolete by increased capabilities of small telescopes by improved instrumentation technology (e.g., Percy 1980) or attempt to avoid the problem by being impracticably vague. The many contributions presented at the Lowell Observatory Workshop on Small Telescopes, IAU Colloquium 183, and this volume describing either current or very near future research are probably the most reliable guides to such topics.

In some cases, small telescopes have specific advantages over large telescopes. For example, spectroscopy-resolution spectrophotometry (e.g., Torres-Dodgen & Weaver 1993) requires that all the light from the object pass through the spectrophotometer aperture. This becomes increasingly difficult with the increasingly longer focal lengths of larger telescopes. Appendix 2 shows that, for telescopes larger than about 1 to 2 meters, optimum spectrophotometer design becomes problematic.

4. THE INSTRUMENTATION AND MAINTENANCE OF SMALL TELESCOPES

> *A telescope lacking good instrumentation is like a person without all their senses* -- NOAO Director Jeremy Mould (2001)

Any small telescope without reasonable instrumentation is of little or no use to astronomers. An assured way to reduce demand for a telescope is to limit its capabilities with inadequate instrumentation. There are many examples of this at national and private observatories.

Small telescopes can be well-instrumented at a cost in proportion to the telescope cost. The Panel on Ground-Based Optical and Infrared Astronomy (GBOI Panel, 1995) proposed, for the National Research Council, that a new facility-class instrument be built for existing telescopes every five years at a cost of about 25% the cost of the telescope.

As emphasized in the AASC, telescope facilities must be maintained. This is as true for small telescopes as for large. What we have seen above is that these costs scale in a similar manner to telescope costs and, hence, provide the same cost-effectiveness in terms of science produced. Gemini operations, facilities, and maintenance costs are 10% annually of their construction costs. This is similar to costs for telescopes in the 1 to 2.5 meter class.

These two factors, quality instrumentation and reasonable maintenance, are probably the most significant factors limiting the scientific productivity of small telescopes. Limited instrumentation and telescope controls reduce the capabilities, effectiveness, and attractiveness of the telescopes. Astronomers often maintain smaller telescopes. This is not a job for which they are necessarily well suited; so, not only does it reduce the productivity of the telescope by preoccupying their astronomers with engineering rather than astronomical research, but the maintenance is often not accomplished well either.

The fact that smaller telescopes are less well maintained and instrumented raises an interesting question about their productivity. If they were instrumented and maintained at a level comparable to that of large telescopes, but at the lower cost proportional to their size, how much would their already impressive productivity increase? *What would keep them from becoming as scientifically productive as are their larger brethren, only with an emphasis on different research topics?*

Perhaps nothing. The KPNO 2.1-meter telescope is an excellent example of a small telescope that has received large telescope support. Ringwald et al. (2001) examined the productivity of 22 ground-based optical telescopes, including 13 in the 3-meter or larger class, for papers published in 1995 and cited in 1998. They showed that the 2.1-meter was 4th in total paper production, 6th in citations, and higher in citations per surface area than any telescope larger than it.

Trimble (1995) examined the productivity of telescopes larger than 2 meters for an 18-month period in the early 1990s and their citations in 1993. In general, her results concerning the relative productivity of private and national observatories confirm those of Abt (1985). She shows that the 2.1 meter telescope (there were no smaller telescopes in the 16 that she considered) was among the top three telescopes in terms of papers and pages of research produced and seventh in citations.

5. THE SELLING OF NEW TELESCOPES

The motivation for larger or unique telescopes is easier to express than that for more modest instruments. These instruments *will* be able to make observations difficult or impossible for their predecessors and, while their greatest scientific value may come from discoveries impossible to predict, one can, with a fair amount of confidence, make predictions about what new science can be addressed if the instrument is constructed.

Proposers of a non-unique telescope of an aperture that will not exceed the world’s largest telescope have a more difficult case to make in a national forum. They can only assert that the telescope will enable the research of a comparable number of scientists as its larger cousin, at a fraction of the cost per scientist, and its greatest scientific value will probably come from discoveries impossible to predict. But they cannot claim to be able to observe what no other instrument could observe[12].

This approach to the justification of the astronomical budget has become strikingly reminiscent of that of the particle physics community and, if continued, will probably suffer the same fate.

6. AN EXAMPLE CASE FOR A SPECIALIZED SMALL TELESCOPES: SCHMIDT TELESCOPES

Many of the large instruments recommended by the AASC are specialized telescopes. An example of a general purpose specialized small telescope is the Schmidt telescope. With the Michigan Curtis 24/36-inch Schmidt at Cerro Tololo now closed and its twin Warner and Swasey Burrell Schmidt at KPNO and the Palomar 48-inch Schmidt out of general use*, there are no significant Schmidt telescopes available to American astronomers.*

How important are Schmidt telescopes to modern astronomy? Using the ADS citation lists, I counted the number of citations to refereed papers published in 1994 and 1995 using Schmidt telescopes. These lists are incomplete so these results should be viewed as lower limits. Figure 2 shows the distribution of the citations to these 90 articles. Abt (1981) showed that the citation rate peaked about 5 to 7 years after publication and that papers averaged one citation per year over their lifetimes. I chose the 1994-95 interval to be as close as possible to current date to accurately as possible represent the effects of Schmidts with modern instrumentation while giving their citation rates time to mature. In fact, many of the articles still referred to data gathered with photographic emulsions. The citation rate is twice that of the average paper from Abt’s study.

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[12] This situation might seem clearer if applied to a different type of scientific instrument, say, electron microscopes. Why would one build any microscope that would not exceed the resolving power of the world’s best?

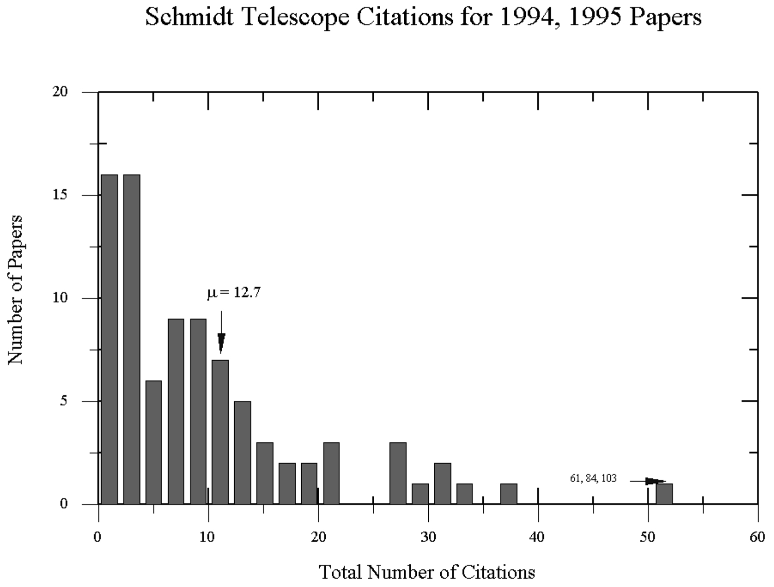

Figure 2. Total number of citations listed in ADS to refereed papers in 1994 and 1995 using Schmidt telescopes.

The Ringwald results in the Appendix show that the citation rate/(area or cost) of the Burrell Schmidt to be over four times that of the large telescopes. Three Schmidt telescopes are in the top 22 effective ground-based telescopes and their average citation rate/(area or cost) is about three times that of the large telescopes.

The 24/36-inch Schmidts, located at two excellent sites in both hemispheres and well-equipped with a range of objective prisms can perform absolute spectrophotometry over a 25 square degree-field to limiting magnitudes of 20. A large CCD array could cover a large fraction of that field. Accurate, automatic two-dimensional stellar classification by artificial neural networks (e.g., Bailer-Jones et al., 1998; Weaver & Torres-Dodgen, 1997) will be an essential component to the exciting next generation of Galactic structure and dynamics research enabled by deep-reaching satellite-based parallaxes and proper motions. Such instrumentation would also permit more complete, much deeper surveys of star formation regions or extragalactic emission-line objects (e.g., the KISS project, Salzer et al. 2000). Surface photometry of galaxy disks by Fry et al. (1999) reached 27.5 R magnitude arcsec^{-2} with the Burrell Schmidt.

Other Schmidts have been very productive lately with deep QSO surveys (e.g., Croom et al., 2001; Meusinger & Brunzendorf, 2001), detecting young objects near the Galactic center (e.g., Dufton, et al., 2001), searches for very low mass stars and brown dwarfs (e.g., Barrado y Navascués, et al., 2001), and Trans-Neptune Objects (Ferrin, I. et al., 2001).

The 24/36-inch Schmidts were not as oversubscribed while they were available at the national observatories as were the 4-meter telescopes, although both typically were oversubscribed in the late 1990s by 20 to 30%, ranging as high as an oversubscription factor of 2.9. This lower subscription rate may be because a typical night at a Schmidt telescope can generate enormous amounts of data (hundreds of spectra per hour and ten to a hundred times as much photometry) and because the telescopes have been poorly instrumented compared to their full capacity and compared to the large telescopes.

Currently, these telescopes sample only a small fraction of their 5-degree fields with single CCDs and are not generally available to American astronomers because of lack of national funding. These telescopes should be equipped with large CCD arrays and, as funded through the national observatories, be made available to general use.

Paving the focal planes of these telescopes with modern CCDs is an expensive project compared to the original costs of these telescopes but extremely inexpensive compared to the cost of projects listed in the AASC for the amount of astronomy that they could produce. This has already been proposed for the Burrell Schmidt by GBOI Panel.

7. THE SUPPORTING ROLE OF LARGE TELESCOPES

The relationship between large and small telescopes is synergistic, each providing support for the other. The role of small telescopes in support of larger telescopes is often assumed and discussed; but the role of large telescopes in support of small ones needs further recognition.

There are at least a couple of natural ways in which this occurs. Often a research project can be largely accomplished with smaller telescopes but require the photon gathering power of a large telescope for project completion or a critical part of the data. An example of the latter, deconvolution of monitored images of gravitational lenses is substantially improved with HST images providing accurate size and position of the components.

A second way in which this supporting role is apparent reflects the continuing nature of astronomy as an exploratory science, still filled with frequent surprise discoveries. Astronomy is a science where observational results have a longer citation half life (35 years) than theoretical papers (22 years, Abt 1996). As has been noted above, many discoveries are made with small telescopes where research with lower probability of success can be economically persued. Once the discovery is made and the technique is vetted, the research, continued on small telescopes, is often supplemented or extended by large telescopes. The prime example must be the discovery of extra-solar planets and brown dwarfs but many others, such as gravitational lenses, rotation curves of galaxies, or Herbig-Haro jets are available as examples.

Conversely, discoveries made at large telescopes can often be most effectively exploited on smaller telescopes.

Small telescopes cannot uphold their half of the synergistic relationship with large telescopes unless they exist and are well-instrumented and well-maintained.

8. CONCLUSIONS

1. By any measure, small telescopes produce the bulk of quality astronomical research.
2. Astronomy produced on small telescopes is much more cost effective than that produced on large telescopes.
3. The funding required to maintain the impact of small telescopes on the progress of astronomical research is a tiny fraction of the overall proposed budget for the current decade.

Astronomy is done by astronomers. Although many observational research projects can be accomplished in less time at larger telescopes, the extreme expense of the world’s largest telescopes, which is their intrinsic nature, strongly limits the observing time available to much less than could be profitably used by all but a small fraction of the world's astronomers. With any balanced approach to telescope size distributions the cost of telescope construction and maintenance will be dominated by the larger instruments; however, it seems apparent that the amount of astronomy accomplished will be maximized if the cost of users of lesser-sized telescopes is a significant fraction of the total cost of astronomers.

Both quantitative and qualitative arguments demonstrate the continuing importance of small telescopes to the astronomical endeavor. The quantitative arguments show that it is significantly less expensive per citation to use the smallest telescope that will accomplish the research. Both the quantitative and qualitative arguments show that the research accomplished by small telescopes is of continuing and lasting significance to the discipline as witnessed by their non-diminishing contribution to astronomy over the last century and the persistence of their citation histories.

To continue the recent trend of closing many smaller telescopes to fund a decreasing number of very large telescopes assures a diminishing scientific productivity in astronomy. There has been a long history of synergy between large and small telescopes and breaking this highly productive tradition for a small fraction of the decadal funding proposed in the AASC would be very unfortunate.

A well-instrumented 1.5 meter telescope could be installed at an established site for about $2.5 M. A decade of maintenance is of the same order. *Two percent of the AASC $4,670M decadal budget would fund construction and maintenance of 17 such telescopes*, a number equal to the number of existing and approved large ground-based optical and infrared telescopes in the world (AASC, Table 3.3). This would double the number of astronomers with access to good telescopes and add something of the order of 50% to the amount of quality research (citations).

A more practical approach is suggested in Table 2.

Table 2. A modest decadal budget for small telescopes

Number	**Item**	**Cost**	**Total Cost**
6	Instrumented 1.5 m telescope	$2.5M	$15M
3	Instrumented 2.5 m telescope	$6M	$18M
	Maintain above for decade	= construction cost	$33M
2	Schmidt Focal Plane CCD arrays	$2M	$4M
	Upgrade & maint. existing telescopes		$30M
		Total	**$100M**

The suggested instrumentation for the proposed nine telescopes might be a relatively standard spectrograph and direct imaging CCD for each plus one relatively unique instrument for each of the telescopes.

The upgrading of existing small private and public telescope control systems and instruments is included with some maintenance support for these existing telescopes. This is similar to the $50M proposed by the AASC in their Telescope System Instrumentation Program, aimed at the upgrading of instrumentation of *large*, private telescopes. As the AASC states, such funding should be accompanied by maintenance funding. This is especially true for smaller telescopes, which are much more likely to be under-maintained.

If well instrumented and maintained, I'm sure that telescopes constructed for the national observatories or constructed at private observatories and made generally available would be well-subscribed. They would be further over subscribed if TACs aggressively allocated observations to the smallest practical telescopes.

9. FINAL THOUGHTS

The projects proposed in AASC are meritorious; I'm sure most astronomers would rearrange a priority or two and/or substitute a program or two that didn't make the final list for one that did. It is difficult to believe, however, that either the stated individual budgets or the significance of the final decadal budget is accurate to the two percent discussed above. The substantial additional scientific productivity that would result from a small-telescope budget similar to the one proposed here or, conversely, the substantial loss of astronomical productivity if the proposed AASC zero budget for small telescopes were adhered to, is much more certain.

APPENDIX 1. PRODUCTIVE TELESCOPES

Table A.1. Top telescopes in terms of productivity per collecting area., solar and radio telescopes are omitted. From Ringwald et al., (2001, Table 7) who examined the productivity of 22 ground-based optical telescopes for papers published in 1995 and cited in 1998. Since cost per unit area of telescopes rises slowly with aperture, the citations/area is also a good representation of citations/dollar.

Table A.1. Top Telescopes in Productivity per Collecting Area

Telescope	***Aperture (m)***	***Papers/area***	***Citations/ area***
IUE*	0.45	106	312
Mt. Hopkins APT	0.25	51	41
HST*	2.4	28	105
CTIO	0.9	18	37
Burrell Schmidt	0.6/0.9	18	28
KPNO	0.9	15	50
KAO	0.9	14	39
CBA West	0.36	13	13
CTIO Lowell	0.6	13	26
CBA	0.32	12	50
Goethe-Link	0.4	12	20
KPNO Coude feed	0.9	12	27
U.Missouri	0.35	10	21
Las Campanas Swope	1.0	10	32
CTIO Yale	1.0	10	14
Mt. Laguna	1.0	9.4	12
CTIO Curtis Schmidt	0.6/0.9	9.2	10
Uhawaii	0.6	9.2	11
Palomar	1.5	8.9	24
KPNO	1.3	7.7	20
KPNO	2.1	7.1	18
Landis	0.2	7.0	10
CTIO	1.5	6.9	18
Palomar Schmidt	0.45	6.3	13
	Average of ground-based telescopes		***24.8***

* These two space-based telescopes are included for completeness and interest but are not included in the average. The IUE is somewhat outside the scope of this article but it certainly ranks as a small telescope/inexpensive science satellite that has produced enormous amounts of quality science.

Table A.2. Productivity for Other Telescopes (from Ringwald).

Telescope	***Aperture (m)***	***Papers/area***	***Citations/area***
CTIO Blanco	4	2.6	8.5
KPNO Mayall	4	2.5	11
CFHT	3.6	2.7	10
Palomar	5	1.2	3.9
MMT	4.5	1.4	5.2
Keck I & II	10	0.3	1.3
Steward	2.3	4.3	12
Lick Shane	3	2.5	7.4
IRTF	3	2.2	6.1
AAT	3.9	1.2	4.2
WHT	4.2	0.9	2.2
UKIRT	3.8	1.0	5.0
	average		***6.4***

APPENDIX 2. SPECTROPHOTOMETER EFFICIENCY

The scale at the focal plane of a grating spectrograph is $S_{tel}EF_{tel}/F_{cam} = (D_{tel}\, F_{cam})^{-1}$, where S_{tel} is the scale of the telescope, F_{tel} is the F ratio of the telescope, D_{tel} is the diameter of the telescope, and F_{cam} is the F ratio of the spectrograph camera. Thus, to collect all the light from an object,

$$D_{tel}\, F_{cam}\, P\, W_d \,/\, \exists(\cos 2_i/\cos 2_d),$$

where W_d is the linear width of the spectrograph entrance aperture at the at the detector, ∃ is the angular size of the area of the sky being imaged onto W_d , and $\cos 2_i/\cos 2_d$ is the ratio of the cosine of the angle of incidence to the cosine of the angle of diffraction of the dispersing element, which we take as unity. For good seeing and tracking, and allowing for the differential refraction of typical hour angles, we take ∃ as about 5 arcseconds and W_d, ideally, as two 25: pixels (= 50:) so

$$D_{tel}\, F_{cam}\, P\ 2,$$

where the diameter of the telescope, D_{tel}, is expressed in meters. Thus, as the F-ratio of spectrograph cameras are rarely lower than 2, telescopes larger than 1 meter must use more pixels than that suggested by the sampling theorem or adopt other strategies to accomplish spectrophotometry.

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