

Maximizing *Kepler* science return per telemetered pixel: Searching the habitable zones of the brightest stars¹

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¹A *white paper* submitted in response to the *Kepler* Project Office *Call for White Papers: Soliciting Community Input for Alternate Science Investigations for the Kepler Spacecraft* released 2013 August 02 (<http://keplergo.arc.nasa.gov/docs/Kepler-2wheels-call-1.pdf>)

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1. Executive Summary

Primary Recommendation: In Paper I (Hogg et al.) we propose image modeling techniques to maintain 10-ppm-level precision photometry in *Kepler* data even with only two working reaction wheels. While these results are relevant to many scientific goals for the repurposed mission, all modeling efforts so far have used a toy model of the *Kepler* telescope. Because the two-wheel performance of *Kepler* remains to be determined, **we advocate for the consideration of an alternate strategy for a 1 year program that maximizes the science return from the “low-torque” fields across the ecliptic plane.** There are considerable benefits of such a strategy which make this design a viable approach for *Kepler* in any scenario, but especially one in which, for any reason, the field analyzed in the primary mission cannot be used moving forward.

- By an analysis of planetary candidates previously detected by *Kepler* we project that, if we can achieve photometry equal to that of the primary mission, we conservatively expect to detect 800 new planetary candidates (after a thorough vetting for false positives). Even if our photometry is degraded by a factor of two from the primary mission, our conservative estimate decreases only to 400 new planetary candidates.
- The first scientific goal¹ of *Kepler* is to **determine the frequency of terrestrial and larger planets in or near the habitable zone of a wide variety of spectral types of stars.** Not only does this recommendation not detract from this strategy, it may provide the best chance to answer this question moving forward.
- In Hogg et al. we argue for a shorter target list. By culling the target list such that we are limiting ourselves to bright stars, the *Kepler* long cadence integrations of 29.4 minutes can also be shortened, which has the benefit of allowing for more sensitive observations of transit timing and transit duration variations. These stars will be later observed by the Transiting Exoplanet Survey Satellite (TESS Brown & Latham 2008), but only for 30 days; there will not be enough transits observed in this mission to detect variations due to planet-planet interactions. Therefore, by observing bright stars that can be followed up by TESS, this will enable dynamical studies that could not be undertaken with either telescope alone.
- Similarly, shorter integrations may allow for asteroseismic studies of more stars. Many asteroseismic targets have giant stars, for which the period of oscillations are long enough so that they can be observed in long cadence data (Chaplin & Miglio 2013). By

¹<http://kepler.nasa.gov/science/about/scientificgoals/>

decreasing the integration time, solar-type oscillations can be observed on subgiants and other stars nearer the main sequence. On these stars, asteroseismic signals are smaller, but by focusing on bright stars we expect the signals to be observable on stars nearer the main sequence.

- Bright stars are also advantageous in that they allow radial velocity follow-up observations to be carried out. By selecting brighter stars, we assure interesting objects can be characterized from the ground *before* the launch of JWST.

This white paper is organized as follows. In §2, we outline our target selection and observing strategies. In §3, we project the expected number of planets detected by this strategy. In §4, we explain how this strategy will enable completion of the primary scientific goal of the *Kepler* mission. Section 5 outlines the advantages of such a strategy for transit timing and transit duration variation studies, especially in concert with TESS. Section 6 discusses other advantages of such an observing strategy. We summarize and conclude in §7.

2. Target Selection

Simulations suggest pointing errors from a two-wheel *Kepler* will be minimized by pointing in the ecliptic plane, where the torque exerted about the spacecraft’s X and Y axes by solar pressure is approximately zero. Such areas are expected to be stable for approximately six months, so that the expected drift is negligible over a 30 minute or shorter integration. We propose to observe fields in the ecliptic plane for this reason. To avoid the Earth crossing the field of view, it is recommended the telescope point only in the direction opposite motion, meaning each field is stable and observable for only three months. **Fortunately, the ecliptic plane is ideal for the *Kepler* telescope.** Recall the *Kepler* field is situated 8-18 degrees out of the galactic plane (Gilliland et al. 2011). This location provides a compromise between having a sufficient quantity of $K_p < 16$ stars to observe and a lack of crowding in the telescope’s 4 arcsecond pixels. The ecliptic plane and galactic plane cross twice, at RA of approximately 7 and 19. It is therefore possible to choose four fields, each separated by approximately 6 hours in RA, that are both on the ecliptic plane and approximately the same distance from the galactic plane as the *Kepler* field. These are located near RA of 3, 9, 15, and 21.

In this paper, we do not attempt to choose specific fields, but instead simply show that there are sufficient observable stars such that our proposed observations are feasible. In part, this is because (as Hogg et al. discuss) our image modeling techniques may be more successful when the drift is larger. With degraded pointing and additional drift, the diversity of stars

that touch different combinations of pixels is increased. More simulations are required to determine the optimal positioning of the telescope with respect to the ecliptic so that the drift is large enough to maximize our abilities to model pixel sensitivities but small enough that the stars stay within their apertures between telescope repointings. Fortunately, it is quite simple to select four fields that are both near the ecliptic and well separated such that each can be observed for approximately three months.

We recommend shorter integration times than the long cadence observations, as we discuss more fully below. As a result, fewer targets are available for observations than have been previously observed. We aim for approximately thirty thousand targets per field, with preference given to brighter stars. Here, “brighter” is intentionally left as an ambiguous term. If our goal is to detect Earth-sized planets, then M dwarfs are allowed to be fainter than G dwarfs because of their deeper transit signal.

If our image modeling techniques are successful, then a field similar to the original *Kepler* field with respect to the galactic plane will contain a sufficient number of viable solar-type and smaller targets. To verify each field will contain enough stars, we simulate the population of the nearby galaxy with TRILEGAL (Girardi et al. 2005).

We first consider the current *Kepler* field. TRILEGAL is limited to fields of 10 square degrees. To combat this, we simulate three regions of 8 square degrees across the *Kepler* field orthogonal to the galactic plane. We then estimate the extinction in the galaxy by calibrating the extinction value at infinity using the values found in the extinction map created by Schlegel et al. (1998). We return the *Kepler* magnitude of each star in our field.

Fig. 1 shows the distribution of stars as a function of effective temperature and apparent magnitude. Ideally, stars in the lower left region of the figure would be chosen. The number of simulated stars found is comparable with the true number of observable stars in the *Kepler* field.

There are sufficient hypothetical fields near both the ecliptic and galactic planes to develop four similar fields from which 30,000 stars could each be selected. It is of considerable importance, however, that **if new fields are selected, they should be located a similar distance from the galactic plane**. We provide evidence in support of this claim in the form of Fig. 2. This figure was developed in an identical manner to Fig. 1, but the numbers correspond to the expected yield in a region of the sky far away from the galactic plane (0:00:00, +0:00:00, blue) and very close to the center of the galactic plane, pointing away from the galactic center (6:00:00, +23:30:00, red). Out of the galactic plane, solar-type stars are an order of magnitude less common; in it, these stars are a factor of two more common, meaning crowding (and background eclipsing binary false positives) would be even more

a concern than in the original mission². Therefore, if new fields are selected we strongly recommend selecting four fields near the ecliptic plane and approximately 15 degrees from the galactic plane.

3. Expected Planet Detections

To determine the number of planets we would expect to find with this new observing strategy, we consider the best source of information on transiting exoplanets, the *Kepler* list of planet candidates found on the NASA Exoplanet Archive³ (Akeson et al. 2013). The number of detections depends on the level of success of our image modeling efforts, which has not yet been precisely quantified. By our mission design strategy, we would stare at each of four fields for one quarter of a year, or 93 days. To observe three transits of a planet, we would then be limited to planets with periods of 30 days or smaller. We then search the Q1-Q12 data release for planets with periods shorter than 30 days. To simulate one quarter of observations and a 7.1σ signal to noise cut, we only select objects in the Q1-Q12 catalogue with signal to noise larger than $7.1\sqrt{(12)}$. We limit ourselves to objects with $K_p < 13$, of which there are 28,000 in number, to simulate our recommendation of searching 30,000 bright stars. Finally, since we plan to observe four such fields, we multiply the number of “detections” by four. We find that we expect to detect **836** candidate planets in a one year mission. As we are analyzing the *Kepler* planet candidates, our number is the number of candidates we expect to find after considerable vetting efforts; known false positives have been excluded from this sample.

We repeat these experiments with multiple signal to noise tolerances, simulating hypothetical scenarios where our best achievable photometry was degraded by some factor relative to the photometry of the prime *Kepler* mission. The results are shown in the table below.

Degrading Factor	1	2	3	4	5
Number of Detections	836	428	284	216	176

Thus, even if our photometry is a factor of three worse than the original *Kepler* mission, we still expect to detect nearly 300 new planets.

²This is not a problem for a future mission focused on the study of M dwarfs: because the stars are intrinsically faint, most M dwarfs brighter than 16th magnitude are within 100 parsec and distributed approximately isotropically across the sky.

³<http://exoplanetarchive.ipac.caltech.edu>

These estimates are *conservative and approximate*. First off, they assume a flat magnitude limit of $K_p = 13$, which would bias our sample towards massive stars. To observe a planet with a given S/N ratio around a massive star, a massive planet would be required. By preferentially selecting smaller stars, we are more sensitive to smaller planets, which are far more common (Morton & Swift 2013). Therefore, a more careful target selection will likely increase our number of detections.

Additionally, the Q1-Q12 planet search was carried out by the *Kepler* team using a now-dated planet search pipeline. Recent improvements to this pipeline are likely to improve the sensitivity of the search, boosting our planet detection numbers even more.

Moreover, this thought experiment assumes a one year new mission. If the remaining two reaction wheels encounter no problems, we would expect to be able to push our detections to longer periods. As an added bonus, we would be biased toward planets with approximately one year periods due to our unique observing cadence. The proposed cadence is both a blessing and a curse for finding planets in the so-called habitable zone, as we explain in the next section.

4. The Frequency of “Habitable-Zone” Planets

Goal 1 of the *Kepler* mission is to determine the frequency of terrestrial and larger planets in or near the habitable zone of a wide variety of spectral types of stars. For M dwarfs the habitable zone varies considerably by spectral type, but for the majority of M dwarf objects in the habitable zone have periods shorter than 30 days (Selsis et al. 2007; Kopparapu et al. 2013). Thus, in a one-year mission we would expect to determine the frequency of M dwarf habitable systems. This number is presently poorly constrained due to the small number of M dwarfs observed in the primary mission; we hope this is given prime consideration in the future plans for the telescope.

The true frequency of habitable zone planets is less constrained for solar-type stars. If our image modeling techniques are successful, such a strategy as the one proposed here will enable an estimate of this value in a multi-year extended mission. Because we return to the same field every year, we will be biased towards planets that transit once per year. Planets that transit exactly twice (or three, or N) times per year will also appear in our sample as habitable zone “impostors.” Such impostors can be accounted for in a statistical sense by an analysis of transit durations (which increase as a function of period), and for individual highly interesting systems by future space-based follow up. Therefore, we do not expect such “impostors” to be a significant hindrance.

For each field, because of our cadence, we expect to miss a significant fraction (approximately 75%) of the transiting habitable zone planets. This is an unavoidable effect from this observing strategy, but should not affect our results. Because *Kepler* would always be staring at one of the four proposed fields, it will always be on the lookout for some transiting, potentially habitable systems; this missed systems can be accounted for statistically.

Thus, from this observing strategy, we expect to increase both the number of Earth-like planet candidates and the precision to which planet occurrence rates are measured.

5. Dynamics with *Kepler* and TESS

5.1. The Flatness of Transiting Exoplanetary Systems

Despite the tremendous success of *Kepler* at finding planets, questions remain as to the flatness of the exoplanetary systems uncovered. While the vast majority of transiting systems must have inclinations of a few degrees or fewer, it is unclear how flat is “flat.” Moreover, in special cases we may expect inclined companions to transiting planets. For example, hot Jupiters may be formed by early dynamical interactions with a mutually inclined perturber, initially exchanging inclination for eccentricity and then circularizing through tidal effects. If this scenario is accurate, then we would expect inclined companions to slowly perturb transiting planets. As the inclination of a transiting planet changes, so does the chord the planet cuts across the face of its star. This also necessarily changes the duration of the planet transit, especially for large impact parameters when a small change in inclination significantly affects the length of the transit chord.

Due to its four year mission lifetime, *Kepler* is not optimal for observing these secular effects, which occur on timescales of order

$$\tau \sim \frac{M_\star}{M_p} P_{tr} \quad (1)$$

with M_\star the mass of the host star, M_p the perturber’s mass, and P_{tr} the period of the transiting planet. For a planet in a 10-day orbit perturbed by a $2M_J$ object, this cycle is of order 10 years. However, a repurposed *Kepler* working in concert with TESS or a future TESS-like mission would be the ideal instrument for this study. *Kepler* will be able to measure the transit durations of large, warm transiting planets in a few fields in 2014. Moreover, with an increased cadence the precision of observations will be enhanced over the work currently accomplished in the *Kepler* field. Observing transits each year will enable a search for transit duration variations (TDVs). When these fields are revisited in 2018-2019 with TESS, we will immediately have at least a five year baseline to compare

transit durations against. **This is a science objective that could not be carried out by TESS or *Kepler* alone.** A search for TDVs will enable key outstanding questions about the architecture of exoplanetary systems to be answered; there are not any missions current or planned that will be able to study the flatness of exoplanetary systems as well as the combination of a repurposed *Kepler* and TESS. Additionally, a shorter cadence which is more sensitive to transit duration variations might aid in the Hunt for Exomoons with *Kepler* (Kipping et al. 2012), which requires sensitive transit timing and duration observations. Finally, an increased sensitivity to transit timing variations would provide for an increased ability to confirm planetary systems without radial velocity follow-up, as the presence of dynamical effects can immediately cause false positive scenarios to be discarded.

5.2. Masses of Transiting Planets

An unexpected success of the *Kepler* mission has been the discovery of many systems with tightly-packed inner planets (hereafter STIPs, e.g. Boley & Ford 2013). Ten percent or more of stars appear to have planets a few Earth radii in size with periods smaller than 20 days. These systems appear to form around stars of a variety of spectral types, from Solar-type stars (Kepler-11, Lissauer et al. 2011) to mid M dwarfs (Kepler-42, Muirhead et al. 2012). While their existence is unquestioned, their formation is uncertain. Boley & Ford (2013) propose *in-situ* formation via aerodynamic drift, and Chiang & Laughlin (2013) also suggest *in-situ* formation is reasonable. Meanwhile, Swift et al. (2013) and Cossou et al. (2013) propose migration of planetary embryos to form such systems. To better understand the formation and evolution of these systems, it is imperative to understand their mass (and thus density) distributions. The most effective method to determine masses of small transiting planets to date has been the characterization of transit timing variations (TTVs) (e.g. Fabrycky et al. 2012), the effects of gravitational perturbations between planets near each conjunction.

Far and away the most common type of TTV signals observed with *Kepler* are variations that occur on the timescale of planetary conjunctions for near-resonant systems (the ones described above). A mission that observes systems for approximately one month per year is suboptimal for detailed characterization of this type of TTV for specific systems. However, all hope is not lost. In the most commonly analyzed case where both near-resonant planets transit, the period of the TTV signal is known and equal to the period of conjunctions, leaving the only free parameters the amplitude of the TTV signal and its phase, assuming free eccentricity in the system is negligible (Wu & Lithwick 2013). As the typical period of these conjunctions is 1-2 years, one quarter of observations is not enough to determine the

masses of the transiting planets. However, return observations of these systems over multiple years with *Kepler* and eventually with TESS can enable a full cycle to be observed, allowing for masses to be estimated. Even when a full cycle can not be observed, strong upper limits can be placed on the masses of the planets from TTV nondetections.

Kepler was, fundamentally, a statistical mission. A study such as this will help us better understand the statistics of TTV systems. While we will likely not be able to characterize systems as well as can be presently accomplished (e.g. KOI-142, Nesvorny et al. 2013), statistical analyses that would benefit from an increased number of dynamically interacting systems can be undertaken. If the updated *Kepler* mission collects four pointings each of 30,000 stars, then of the 120,000 stars observed, 12,000 would be expected to host STIPs and approximately 600 of these systems would be expected to transit, allowing for the discovery of hundreds of near-resonant systems. Adding numbers such as these to the current sample will allow a significant increase in our understanding of the properties of these systems and provide insight into their formation.

6. Other Benefits

In some ways, the proposed mission is similar to a mini-TESS. There are considerable benefits to such a strategy. Many have been outlined above; the potential to open up TESS to searching for TDVs and TTVs over 400 square degrees of the sky cannot be overstated. TESS will also have the ability to search for longer-period planets in these fields, when its data is combined with *Kepler* data. It is worth mentioning that this proposed survey will only cover one percent of the sky, and will thus not impinge on TESS’ discovery space in any significant way. It is of our opinion that the benefits to observing these fields in the first half of this decade as a precursor to TESS only serves to enhance the future mission, as opposed to detracting from it.

Observing brighter stars at a higher cadence will allow asteroseismic studies of more stars than were observable with the completed *Kepler* mission. Long cadence data can be used to characterize global properties of giant stars that display asteroseismic oscillations. The Nyquist frequency for a cadence of 29.4 minutes is 283 μ Hz, which corresponds to stars at the base of the red giant branch on the HR diagram. Increasing the cadence would impact the population of stars available for asteroseismic analysis, bringing bright sub-giants into the asteroseismic regime (Chaplin, W., J., private communication). Since the magnitude of stellar pulsations decreases with increasing stellar density, brighter sub-giant targets would need to be selected in order to reach the current measurement precision for giant stars.

An additional benefit to focusing on brighter stars is the increased ability for radial velocity (RV) followup. If our proposed observing strategy is undertaken, there will be four new *Kepler* fields, two near a declination of +20 degrees and two near -20 degrees. These are ideally placed for follow up radial velocity work by existing telescopes in Hawaii and Chile, as well as future 30-meter class telescopes. The faintest stars in the *Kepler* field are too faint to be observed efficiently on existing telescopes, and with observing time as competitive as it will likely be on the proposed giant telescopes, this is unlikely to change. Therefore, a focus on brighter stars (perhaps $K_p < 15$ for solar-type stars) will enable more efficient and successful RV follow up.

Finally, and perhaps most significantly, such a mission will allow potentially interesting objects to be found before the launch of the James Webb Space Telescope (JWST). Scheduled to launch in 2018, JWST will be placed at the inaccessible L2 Lagrange point, making it necessarily a fixed-length mission. Moreover, by this time the *Spitzer* telescope will have drifted too far from Earth to provide useful data. TESS is scheduled to launch at approximately the same time as JWST, but space telescopes do occasionally run into delays for various reasons⁴. If TESS were to be delayed, and JWST ran for only its nominal five year mission, it is conceivable that many of TESS’ most interesting discoveries will occur at a time when there are no available infrared space facilities available for follow up work. Certainly, even if both telescopes proceed according to plan, time will be limited between publication of TESS’ results and the end of the JWST mission. This proposal, which intends to find interesting transiting systems across the sky, will accomplish this feat well before the launch of JWST, allowing considerable time to plan observations with the future observatory before launch and assuring that JWST will have an abundance of planets to characterize.

7. Summary and Conclusions

Our strategy outlined here is one possible observing strategy. Of course, there are considerable benefits to continuing to observe the current *Kepler* field. We assume these benefits will be discussed in other white papers. Unfortunately, there is no guarantee our methods (described fully in Hogg et al.) will be able to be applied to this field because of the large torque induced on the telescope along the Y axis by solar radiation pressure. In the case where new fields must be chosen, we recommend selecting four fields subject to the constraints of §2. This strategy, in addition to uncovering 800 or more new planets, will provide the best chance to improve constraints on the frequency of habitable zone planets as a

⁴see also: JWST

function of stellar spectral type, one of the key goals of the *Kepler* mission. It will also enable new transit timing and duration studies, allowing us to probe the flatness of exoplanetary systems with better precision. This strategy will also turn TESS into a transit timing machine in these fields, creating a 5 year baseline for dynamical studies where otherwise none would exist. Moreover, the detection of 500 new planets well before the launch of JWST is significant as it allows time to plan follow-up analyses of transiting exoplanets with this telescope across the sky well before its launch. If our image modeling techniques are successful, we feel the strategy outlined herein provides a unique opportunity for the contributions of *Kepler* to continue unhindered for the next decade, through a combination of its observations and those planned in the future by TESS.

8. Acknowledgments

Much like its namesake, *Kepler* has been instrumental⁵ in ushering in an astronomical revolution. It is our great pleasure to have used data from the *Kepler* telescope since its launch; we acknowledge the hard work put in by this team both in building the telescope and providing data and support since 2009. BTM is supported by the NSF Graduate Research Fellowship grant DGE-1144469. DWH, RF, and DFM are all partially supported by NSF grant IIS-1124794. MH acknowledges support from the European Research Council in the form of a Starting Grant with number 240672.

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⁵Pun not intended

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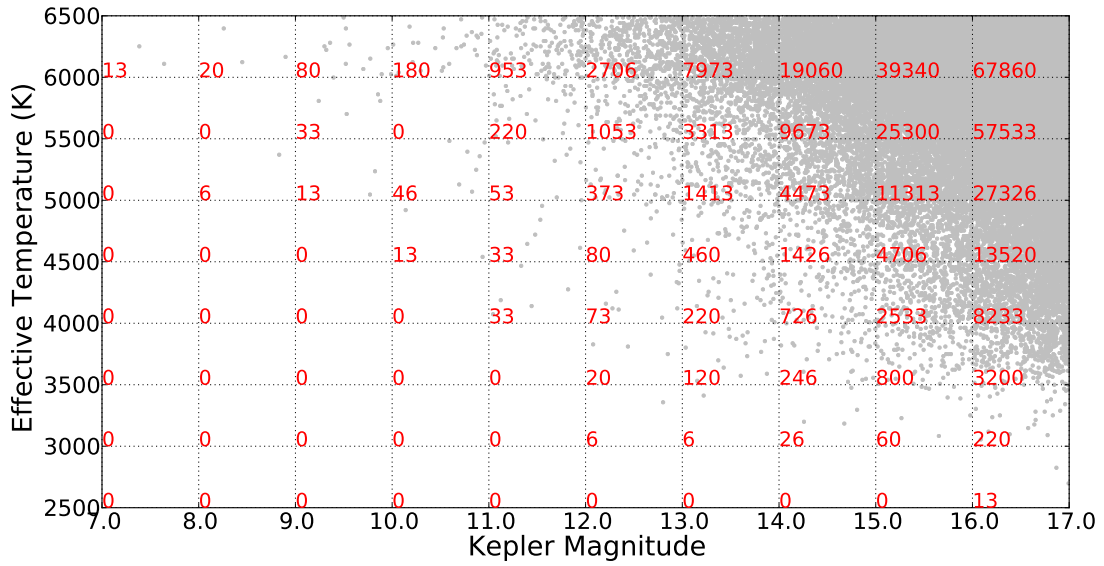


Fig. 1.— Estimated number of visible targets in the *Kepler* field as a function of apparent magnitude in the *Kepler* bandpass and stellar effective temperature, simulated using TRILEGAL. As might be expected, a plurality of targets are found in the top right corner, where luminous, distant stars exist. There are a sufficient number of bright targets equal to or smaller than the size of the Sun, around which small planets in the habitable zone could be detected.

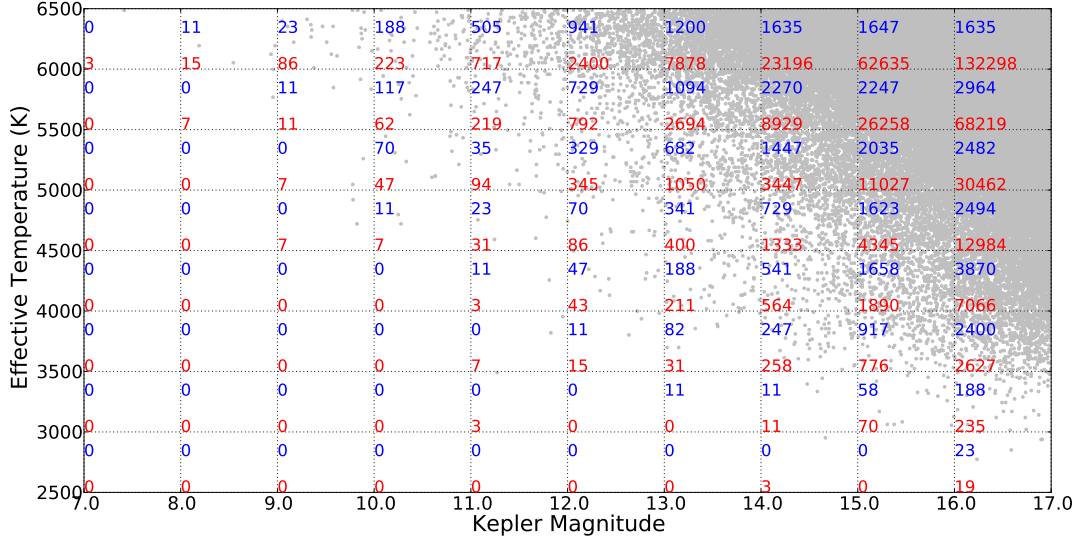


Fig. 2.— Same as Fig. 1, but for *Kepler* field-sized patches of sky well out of the galactic plane (blue) and in the galactic plane, opposite the galactic center (red). Far away from the galactic plane, there are not a sufficient number of stars in one 100 square degree field of view. In the plane, crowding is expected to be a significant concern, inflating the false positive occurrence rate. Therefore, we recommend selecting four fields 10-20 degrees from the galactic plane, similar to the current field. The optimal distance from the ecliptic plane with respect to the telescope drift rate has not been precisely quantified, but it is expected that four appropriate fields, approximately 6 hours apart in right ascension, should easily be selectable. For a survey of M dwarfs, target selection is much less of a concern. Note all three fields have similar numbers of low-mass stars. As M dwarfs are significantly underluminous relative to solar mass stars, bright M dwarfs must be located within tens of parsecs and are thus approximately isotropic over the sky.